The authors analyze the human influence on the harsh frost weather in early April 2021 after a unusually warm March over central France, which pose great damages on the grapevine and fruit trees. The results show that human-induced climate change has significant impacts on such extreme events. The topic is interesting and the workload is also relatively heavy. However, I think there is a framing issue and some methodological issues, which may pose a question about the coherency and credibility of the study.

We thank the reviewer for the effort and constructive comments. We provide here a short reply on how they will be addressed.

At first sight, I think it is a attribution study according to the title. But when I read the whole paper, I feel like it is a evaluation and projection study. Thus, I think the authors should first clarify the main purpose and storyline. In terms of the framing, it is usual to first describe data and methods used in one paper, and then elaborate the results. The results can be organized as observation, model evaluation and attribution. In this paper, the data and methods are combined with the results, which add difficulties to reading and understanding. This study used five model ensembles with different resolutions, and the future scenarios include RCP8.5 and SSP2-4.5. But what is the purpose to use all of them ?

It is true that when carrying out an attribution study, it is necessary to evaluate the models, especially regarding the specific aspects of the extreme event in question. This is why such model evaluation is performed each time. And in addition to the attribution itself, we often add future projections of probability change for the models, which brings important information for future adaptation. To account for this remark we will add a paragraph in the introduction explaining this.

Regarding the different model ensembles and scenarios, we can not say one model or scenario is the best, they are all possible. Taking different scenarios is necessary to explore the possible evolution of climate related to mitigation options, whereas taking several models is necessary to consider the uncertainty in our knowledge on the climate system. Single models usually do not give a reliable description of the probability distribution of trends. To span the range of possibilities and to get an indication of the model uncertainties we use as many models that pass the validation tests and as many scenarios as possible.

Regarding the article structure itself, we will follow the reviewer's remark and restructure it by presenting first the definition of the event, then present all data, observations and models, followed by methods, and then describe the results.

The paper involves too many unclear and inaccurate descriptions as well as the inappropriate choices of the methods. The following is a list of some specific comments:

(1) In terms of model evaluation, the Kolmogorov–Smirnov nonparametric test is often applied to determine whether two probability distributions are well-distinguished. In addition, the observed and simulated time series can also illustrate whether the models have the ability in reproducing the observation.

Indeed, a KS-test would probably be interesting for checking the agreement between a model and observations. This would require a comparison with the method of Philip et al, 2020 which only compares inferred coefficients in order to validate a model. Since this paper is about the attribution of a particular extreme event, and not the methodology used, we prefer to use the already validated method of Philip et al 2020, and test this approach in another research paper.

Philip, S., Kew, S., van Oldenborgh, G. J., Otto, F., Vautard, R., van der Wiel, K., King, A., Lott, F., Arrighi, J., Singh, R., and van Aalst, M.: A protocol for probabilistic extreme event attribution analyses, Adv. Stat. Clim. Meteorol. Oceanogr., 6, 177–203, https://doi.org/10.5194/ascmo-6-177-2020, 2020.

(2) There are too many data tables. It is more visual to draw figures like boxplot or bars.

There are currently 4 data tables. We will transform 2 of the tables into boxplots (the second [results for stations] and the third [evaluation])

(3) Section 4.5 is not necessary in this paper, and the results derived from two ensembles with different scenarios are not comparable. The author can do more literature research and write another projection paper.

Given the paper restructuration future trends are now included in the results section. We emphasize the importance of providing future assessments as an extension of the attribution, as this uses the same data and methods and provides a quick overview of how probabilities will change in the future, offering important insights for future adaptation, hence we prefer to keep it here, even though it is a small section.

We don't know which scenario will be true. But as we use the different scenarios and analyse results for the same warming level of climate change rather than for a given year we can compare these results.

(4) The paper only focus on the trend of regional mean, and it is insufficient to know the pattern distributions.

The paper focuses on regional mean but also provides some results for individual stations. For stations, results for the GDD indices bear large uncertainties as can be seen from Table 2. We think patterns within the domain will not be significant and interpretable due to uncertainties. Considering patterns at a larger scale, for areas with different climatologies, would drive the analysis too far from the event itself, its impacts, which could be done in another paper but is beyond the scope of this current study.