

Guest editor report: nness-2018-392; original title “Chilling accumulation in temperate fruit trees in Spain under climate change”

The authors have undertaken an ambitious research in assessing winter chill across Spain, as derived from meteorological observations and climatic projections. Each of the two reviewers have provided an excellent and detailed revision of the manuscript, to which the authors have responded in a detailed manner.

The two reviewers reach consensus on a number of critical points. The interactive comments from both reviewers have been well documented and the authors have formulated solutions to take the manuscript further in two separate documents (RC1 and RC2). I agree with the solutions presented by the authors. Below I highlight some points of attention for the authors.

Overall the research can be documented better in the manuscript such that justice is done to the rigorous work undertaken. The processing of meteorological observations and climate scenarios, and their relation to the impact on the Spanish fruit trees uses state-of-the-art methodology. Therefore I would suggest that the authors revise the manuscript according to their documentation and replies to the reviewers.

The following major points require the authors' attention:

1. Avoid vague descriptions and formulate more precisely what has been done, certainly in the abstract. [an example: “near and far future” is vague; define the periods “2021-2050” and “2071-2100”] Overall a focus on precise findings will improve the readability of both manuscript and abstract.
2. A comprehensive review of chilling requirements for different species will be of enormous relevance and interest to an international audience. To this extent, the authors' suggestion of adding a table is excellent. References to the literature, as already documented in the authors' replies to the reviewers, could be extended to include research that is relevant to Spain or similar climatic environments (e.g. California).
3. An important outcome of the research relates to winter chill reduction. It would be useful to discuss the number of times chilling requirements are compromised for the different periods studied.
4. The choice of keeping the different chill model results separately is underpinned by the reviewers' preference for the dynamic model, and therefore I recommend to keep the results separately as currently done. Nevertheless, a better documentation of the different chill models and temperature thresholds will clarify the comments made.
5. I leave it to the authors to decide whether to share their code in the supplementary material or document the formulas used.

Since most of the above points have been documented in the replies to the reviewers, the revised manuscript can be reviewed by the handling editor.