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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Chilling accumulation in temperate fruit trees in Spain under climate change" *by* Alfredo Rodríguez et al.

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Alfredo Rodríguez et al. did an extensive and rigorous job on trying to quantify future developments on chilling accumulations for Peninsular Spain and the Balearic islands. They did a major effort in modelling and validation of input data and consider a highly relevant aspect of local fruit production that is vulnerable to climate change (Campoy et al., 2011; Luedeling, 2012). In this sense, and in my opinion, this regional study has its relevance and its place in this journal. This study does also contribute to a better understanding in this domain, by improving the methodology with regards to previous studies through the use of state of the art climate models and scenarios, although it does not stand out for the novelty of the used approaches. To increase the value, that the paper brings to the scientific community as well as to end users, a couple of

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revisions are suggested below, which, if taken into account, would make this paper more suitable for publication.

Major remarks regarding the content

With regards to the methodology and scope of the paper, I agree in most points with Eike Luedelings review comment (RC1):

(1) First of all, combining models that have been found to be inadequate (Luedeling, 2012) is not innovative, and the fact that the models were apparently applied without calibration to local conditions is in my eyes the biggest shortcoming of the paper. To my knowledge, there is no evidence that a model that was tested for North Carolina (Latitude range ∼36.5°N-33.8°N, Köppen-Geiger classification 'Warm temperate with hot summer climate' (Peel et al., 2007)), can be transferred to Spain (Latitude range ∼43.5, 36.0, major Köppen-Geiger classification 'Arid steppe cold' climate (Peel et al., 2007)); nor can be safely assumed, that the cultivars in all regions have the same physiology, which is implied by using the same model, despite the mention of this fact on p.3, l. 11.

(2) At this point of the introduction, a better contextualization and reference for the obtained values would be highly appreciated. Only on p.3, ll. 14-17, an exemplary chilling requirement is given, and this for apricot which is not considered in this study. Without a knowledge of local requirements of apple, olive and vineyard, the severity of the change in chilling units is hard to grasp. Also, with the quoted requirements at hand ("631 chill units [Utah model, 'Palsteyn' variety), the observed difference between models ("less than 500 chill units", p.7 l. 25) can be substantial, and the outcome of Figures 7-8 more alarming than described in the paper. Later, on p.10, ll. 11- 17, exemplary requirements for an apple and an olive variety is given, which are at risk of not being fulfilled according to the 'far future' predictions. For better understanding of the key findings of the paper, more such values should be given.

(3) In my opinion, estimations of concrete, crop or variety related shortcomings in chill-

ing have highest relevance for planning applications and various end users, so if this is possible, it would be very interesting to find in this paper indications which zones under cultivation of a given crop will become unsuitable in terms of chilling for major varieties.

(4) Obviously, the diversity of species cannot be fully covered in this paper, but, joining the suggestion of RC1, with open source code and output maps, interested parties could quickly assess these zones following an example. It might be a subject of discussion in this stage of the paper, if these findings would be improved or not by considering the agreement of different chilling models. A priori, there is a major concern with this methodology, that I share with the author of RC1, because of the unjustified comparison of chill units among models and the mentioned inadequacy of some of them.

(5) Potentially reducing the number of models and increasing the documentation (equations, parameters) of the models should help overcome the, in my view, given uncertainty about how the different models can be understood considering the three studied fruit crops mentioned in the paper. In p.5 ll.22-24, the North Carolina model is introduced as being developed for apple trees, the De Melo-Abreu method for olive trees and the Dynamic method for peach trees. I would wish for more elaboration on how these choices have been justified and on how to make use of the findings presented in the Figures 3-8. The codes should be open access, too, since I totally agree with RC1, a research should be reproducible and with the given information this is not of application.

Remarks regarding the form

Title

In p.2, ll.1-22, the authors state “Vineyard, apricot trees, olive trees and almond trees could be also included in this last subgroup [of temperate fruits], although some of their climatic requirements are nearer the subtropical fruit trees” p.2,ll 6-7). Bearing this in mind, the mention ‘temperate fruit trees’ in the title of the paper is in my opinion a bit misleading, although reference handbooks do classify olives and grape as temperate

(Schaffer, 2018).

Abstract

The abstract could be more concise and feature more detail about the findings of this study than the context.

Introduction

In p.2, ll.1-22: In line with RC1, I consider the description of the classification as too long and can be left out, especially in view of the ambiguity of the classification mentioned above. The section on bias adjustment (p.4 ll.1-9) could be slightly more elaborated, and precise how it is ensured that the change over time of the climate signal is not cancelled out, see also Michelangeli et al. (2009). The transition from this paragraph to the following is a bit sharp. At this point, an overview of similar (regional) studies on chilling requirements would be expected point.

Materials and methods

Regarding the selection of models and scenarios, although hardly done in literature, the choice of models could be better justified using methodologies as in (Mendlik and Gobiet, 2016), since there is evidence of high sensitivity of climate model selection (Wilcke and Bärring, 2016). However, the authors chose the two reasonable scenarios (RCP 4.5 and RCP 8.5), allowing for consistent comments on importance of mitigation in context of actual discussion. Key equations of the chilling models should be provided in the additional material. In the main text a comment on the validation of the models should be given, in the view of their applicability on future time series.

Results

With regards to the CV, MAPE and IQR, the classes > 20, >0.4. . . are in my view not informative enough. Also, in section 3.1, the MAPE values are declared as problematic above 20% for few grid points, without mentioning until how high they stretch. Thus, no conclusion can be made if the computation for these grid points can be trusted at all.

Discussion

The difference between the two researched scenarios could be expressed more clearly (p.10, ll.11-19).

References

I join the request made in RC1 for indented references. In the text, the reference in p.3, l.29 should be revised.

Figures

As stated in RC1, all figures need to be presented with a scale bar, north arrow, and (due to inconsistency between figures) the reference system. Preferably all maps would be shown in the same projection (or the stretch of the figures should be revised). The layout of subfigures could be optimized so as to allow for bigger figures. If the decision will be taken to not report on all models, this could be of great improvement of the readability.

Figure 1 shows a good overview of land use in Spain for the reader, exposing major growing areas for the considered crops. Values seem reasonable from my experience. However, the choice of the color map is unfortunate, <1%, which could be conceptually be negligible, is very hard to distinguish from the higher classes. I suggest to revise the classification to a lower number of classes, 5 being preferred. A clarification is needed whether the map shows the percentage from the total area or from area classified as cropland.

Figure 2 features a useful example output of the analysis, but it was not justified that this is a representative example. The most reliable model would have been preferred, the Dynamic model was judged as best performing (Luedeling, 2012). In subplot B, over the years, the chilling units decrease, a trend line could be interesting, next to the mean. Subplot C should highlight which model is used for subplots A and B. In subplot D, neighboring grid points expose substantial differences in this mountainous terrain.

With regards to the shortcomings mentioned in mountains areas, a further study could envision a more focused analysis on those areas.

Regarding Figures 3 -8 and as mentioned above, classes such as >20 are little informative. In this line, it would be of great value if the maps could either exclude or highlight less reliable outcomes. This could be done by keeping grid points white, or, if readability is not compromised, with a hatched overlay. From visual comparison, there seems to be a substantial part of the apple cultivation shown in Figure 1 in coastal and mountainous areas, those reported as with comparatively high errors.

Technical comments (additionally to those mentioned in RC1, to which I fully agree):

* P.2 l. 26 delete 'it

* P.2 l.18, production, not productivity (if productivity is meant, the reference i.e. area should be specified, and I agree it is not relevant in this paper, rather give the importance of other fruits in Spain, ideally with national statistics rather than FAOSTAT)

* P.3 l.34, add 'among other regions'

* P.5 l.23 inconsistent usage of Dynamic model / Dynamic method

* P.8 l.10, specify where the biggest change occurred

* P.9 l.16, Mediterranean','

* P.9 ll.17-18 reformulate

* P.9.l.21 a warmer scenario

* P.9 ll.28-29 'Nonetheless, few tree crops are grown [. . .]' – have these areas also be found as potential new cropping areas?

* P.10 l.17 are you comparing this value (469 chilling units, according to the De Melo-Abreu method) with all outputs? It should only be compared to the output of the analysis using the same method, which, in the case of the far future under RCP8.5, where

the map shows mainly values between 500-1000 chill units in the area coinciding with olive cropping.

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