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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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Rodriguez et al. present an assessment of past and future winter chill in Spain, using an ensemble of climate scenarios and four chill models. It seems to me that the climate data processing was very well done; the way scenarios were prepared seems very reasonable. The authors' expertise in this field is evident. Unfortunately, the study has some shortcomings regarding the estimation of winter chill, which will have to be addressed. Major issues: 1) Similar work has been done before, for various countries and also at global scale. It remains somewhat unclear what the particular advantage of this new approach is. A (smaller) ensemble approach was already used 10 years ago (Luedeling et al., 2009a) for California and shortly afterwards at the global scale (Luedeling et al., 2011). In these studies, we used a weather generator rather than

just climate model outputs, which (in my view) makes the methodology used then more robust than what is presented here. Admittedly, some other elements of these assessments were not as well done as what is described in the current manuscript, and it's good to see a study using RCPs rather than SRES scenarios (though we did this here: Benmoussa et al., 2018, but not as a spatial analysis), but the novelty of the current methodology isn't sufficiently described. 2) Another innovation the authors point out isn't really a feature but rather a bug in my view. As highlighted on page 9, ll. 1-2, this may well be the first study that projected climate change impacts for these four chill models. However, there are good reasons for there not being more studies, in particular no recent studies. The reason is simply that most of these models can't be trusted to accurately describe chill accumulation. There have been a number of model comparisons over the years that have consistently found the Dynamic Model to be superior to the others (e.g. Benmoussa et al., 2017; Luedeling et al., 2009b; Ruiz et al., 2007; Zhang and Taylor, 2011; there are quite a few more). Adding old, obsolete models to such a study would be like adding a flat-earth model to a GCM ensemble – it makes little sense to consider models that have been shown to be inadequate. The situation with chill models is not the same as with GCMs – we do have a clear idea of which models are better, and there is really no rationale in my view to go for an ensemble approach. 3) Related to the previous points, we've done several studies to compare the response of various chill metrics to climate change. First, they differ greatly in their sensitivity to warming (Luedeling et al., 2009c). Second, they are not comparable, with the ratio between different chill metrics varying tremendously across the globe, especially along climate gradients (Luedeling and Brown, 2011). Especially at the warmest end of the climatic range for temperate fruit trees, most models fail (Balandier et al., 1993; Benmoussa et al., 2017a, 2017b; Linsley-Noakes and Allan, 1994). The Dynamic Model is the only model I know that has a chance of somewhat describing changes correctly across different climates. This is the reason why in our 2011 paper (Luedeling et al., 2011) we only report Chill Portions (we actually calculated other metrics too, if I remember correctly, but I consider the results meaningless).

This reasoning is actually described in several places in this paper and elsewhere (e.g. Luedeling, 2012). Just as an illustration, in the literature we found the chilling requirement of 'Ohadi' pistachios quantified at 1000+ CH in Turkey, but they grow well at 100 CH in Tunisia. This difference is not trivial at all and illustrates how badly off we can be if we use the wrong model. 4) One particular criticism of chill models has been that they are calibrated for a particular site and not necessarily generally valid. There is a reason why the North Carolina Model and the Utah Model are named after geographic areas, not after crops, and why researchers in various places saw the need to make adjustments. For example, in South Africa the Utah Model regularly produced negative chill totals at the end of the season. This was 'addressed' by removing the chill negation (resulting in the Positive Utah Model: Linsley-Noakes and Allan, 1994). The necessity of these 'empirical hacks' clearly indicates that these models can't be trusted across climatic gradients – which is critically important for a credible climate change assessment. 5) The presumably innovative outlook of possibly using estimates of the amount of chill that is exceeded 90% of the time (p. 10, l. 29) isn't so innovative after all. In fact, we already used this 'Safe Winter Chill' approach in several publications, dating back to 2009 (Luedeling et al., 2009a, 2011). It has also been picked up by others (though I don't currently remember who that was). 6) Another alleged innovation is the variable duration of the chilling period, which is determined by the minimum and maximum chill accumulation. Sure, this is new, but is it correct? The authors don't present any evidence for this. I realize that some authors have claimed that something like this makes sense (e.g. Cesaraccio et al., 2004 for their own model, but others have also said this for the Utah Model I think), but is there really any evidence? Actually, I strongly doubt that trees can make use of chill accumulation over the entire cold period. We've done a number of studies where we tried to statistically determine the chill-responsive period (Guo et al., 2015; Luedeling and Gassner, 2012; Luedeling et al., 2013a, 2013b), and we've always found periods that are much shorter than the full winter season. Now this may mean various things, including that trees are pretty safe from chill shortfalls in many places, but I suspect that it would make sense to end the

chilling period earlier than an automatic algorithm would suggest (actually, if I could change one thing about our earlier studies, I would shorten the period we considered, which seems much too long now in hindsight). 7) The paper starts with a strange introduction about the classification of fruit trees, which I'm not sure I agree with and which is also not relevant here. This paper is only about temperate species, so no need for such a general take. The first two paragraphs should be deleted. 8) I strongly urge the authors to make their code public, either in a repository or as supplementary materials to this paper. This will make it much easier to understand what was done. For instance, the statement that the authors used the method by Fishman et al. (1987a, 1987b) is not sufficiently detailed – anyone who's seen these papers knows that this is not at all trivial to implement (and I wonder if this is really the authors' source of the algorithm). Ideally, a paper should be reproducible, meaning that the methods should be sufficiently detailed for readers to repeat an experiment. This is often not really achievable, but it is not difficult for a modeling study such as the one described here. Please share the code. The main reason for this is that the actual results of this paper are not particularly helpful – pretty much the same has been shown before. The innovation (for the chill modeling community) lies in the climate data processing, but if this isn't actually shared with readers, nobody can easily make use of this methodology. In my view, the offer that readers can contact the authors isn't sufficient. 9) Finally, I suggest that the authors compare their results (and maybe also their methods) with similar studies that have been done before. There have been quite a few, as the authors will realize if they do a systematic search, not necessarily on Spain, but on various other regions. 10) Even more finally, I suggest language editing. There is still some room for improvement in terms of language, and some statements are unclear. Minor issues: p. 1, l. 14: what are 'inner physiological factors'? p. 1, l. 14: 'accumulating cool temperatures to finish dormancy is unclear (at least in terms of what dormancy this is – I'd most likely associate finishing dormancy with bloom of leaf out, but that also requires heat). No need for "be broken" in quotation marks. This is commonly used and doesn't need to be identified as an odd term (or whatever the purpose of the quotation marks is). p. 1, l. 16: I don't think the chilling requirement is different for each variety (which means that no two varieties have the same requirement). They are crop and variety-specific, but not all different. p. 1, l. 28 – p.2, l. 10: irrelevant – delete p. 2, l. 12: income, not wealth p. 2, several places: for simplicity and reader-friendliness, I recommend replacing 10ˆ6 by 'million' p. 2, ll. 18-19: FAOSTAT doesn't directly provide such values I believe, so it would be important to state how this was determined (also note that there are all kinds of issues with this database). It is also not obvious that this sentence refers to the global scale, since the previous sentence talks about Spain. Overall, this isn't a very relevant statement in a paper that's just on Spain. p. 2, l. 24: I believe the thing trees are sensitive to is frost (not generally cold temperatures) p. 3, l. 1: 'accumulation of cold periods' is an unfortunate choice of words. Sounds like trees need, say, 5 cold periods to break endodormancy. p. 3, l. 3: not all models are based on temperatures between certain thresholds. The Dynamic Model works differently, and even the Utah-type models don't really follow this simple structure. p. 3, 12: I disagree that the chilling requirement corresponds to conditions where a tree is grown. It may rather correspond to conditions where it evolved/was bred p. 3, ll. 13-17: not sure what information is conveyed here. The initial statement is about considering a range, but then the examples are precise values, not ranges. If this is supposed to illustrate intra-specific variation, then please make sure to use the appropriate terminology (not sure what 'crop tree' refers to). p. 4, l. 9 (or elsewhere): Somewhere the authors need to mention the various chill assessments that have already been done by a number of people in a wide range of places. p. 4, l. 17: no, the models do not need hourly Tmin and Tmax. They just need hourly temperature, which can be derived (if no other information available) from daily Tmin and Tmax. p. 4, l. 22: not sure what 'freely distributed' means. Open-access? p. 4, l. 24: is this really an observational dataset? p. 5, l. 15: more details are needed on the temperature generation, especially since the source will be hard to find for most readers. What mathematical functions were used for constructing daily curves? The common method in horticultural studies such as this one is a methodology by Linvill (1990), which is based on a sine curve during

the day and logarithmic cooling at night (implemented in the chillR package; Luedeling, 2018). I'd be quite curious to learn how de Wit's method compares with this, but the authors provide insufficient information about their approach. p. 6, ll. 11-13: The authors compute a mean and then a median. Later in the paper they argue that one should calculate a 10% quantile. Why didn't they do this here? p. 6, ll. 16-17: As stated above, I'd prefer to have the code made publically available, for full transparency and usefulness. p. 6, l. 23: Is the full name of MAPE really 'mean percentage absolute error'? That would seem to lead to the acronym 'MPAE' p. 7, l. 19: 'similarly simulated' is awkward wording p. 7, ll. 23-27: All these models use different units, so they can't be compared (the fact that they're probably all called chill units doesn't make them equivalent). While it's obvious that the Dynamic Model can't be compared to the others (because values are much smaller than for the other metrics), the others are also not comparable! p. 8, l. 27: scenarios were averaged in this study, but we also provided information for determining the impact of climate model and emissions scenario. p. 9, ll. 1-2: As stated above, I don't consider it an asset to include outdated models in a study. . . p. 9, l. 22: not sure what 'discrete nature' means. And I also think that this may be an indication that these models are too sensitive for warm places. p. 9, ll. 26: this study didn't 'find' this, it just reported on it. Luedeling et al. (2011) sort of found this. p. 10, ll. 4: Yes, it would be great to have more datasets, but we actually already have a lot. Rather than call for collecting more data, I'd call for better use of such data for model development and validation. p. 10, ll. 11-12: 'crop varieties depending on the RCP' is unfortunate wording. First, crop varieties don't depend on RCP. Second, RCPs are theoretical pathways that will not be followed precisely. Better to say something like 'depending on how rapidly GHG emissions can be reduced' or something like that. p. 10, l. 23: not sure what 'low-limit chill requirements' are p. 10, l. 29: as mentioned above, this is exactly what the Safe Winter Chill metric achieves. p. 11, 2-4: It's obvious that RCP8.5 causes greater change, similar to the end vs. middle of century. Doesn't need to be mentioned or should clearly be marked as expected. p. 11, l. 6: why especially in warm regions? The impact depends not only on chill loss, but also on what is grown there and how much chill it needs. p. 11, ll. 17-18: confusing sentence. Reference list: It would be so much easier to look through this, if all but the first row of a reference were indented. Maps: maps should have a coordinate system, north arrow, scale bar etc. Fig. 1: I doubt that all the olive data are right. If so, some parts of Spain would be almost exclusively olives. Maps 3-7: very hard to compare changes, which is really the most important part of this paper, if the maps are scattered across various places. Fig. 5: is the scale used for the change useful.

In summary, I think this contribution has potential, since the way the climate data were processed is very robust. But the team should consider adding some chill modeling capacity to the study to make this more convincing. While chill seems like an easy application of a climate change projection framework (it's assumed to just depend on temperature after all), things are actually quite complicated due to the invisibility of chill-induced changes, which has precluded the development of convincing models so far. In consequence, there are many models, and most of them are not suitable for studies across climates. If the authors manage to adequately consider this, this manuscript may become publishable.

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