

Geosci. Model Dev. Discuss., author comment AC2
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Reply on RC2

Tobias Conradt

Author comment on "Choosing multiple linear regressions for weather-based crop yield prediction with ABSOLUT v1.1 – Initial tests for the districts of Germany and an over-confidence trap in statistical modelling" by Tobias Conradt, Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-21-AC2>, 2021

Many thanks also to the second anonymous referee for valuable input! Here are my replies, after the referee's comments highlighted in bold:

In the "Introduction" section, author firstly provide the research gap that "the entirety of crop and landscape specific weather effects is hardly captured by the existing models" but the "crop and landscape specific weather effects" is not clear because weather effect consist of extreme adverse weather effect, weather trend and fluctuation effect, and the current linear regression model can capture the effect of annual and seasonal mean weather (Peng et al 2018). More specific information about "weather effect" is helpful to emphasize the question aimed to be addressed.

Agreed. Especially yield losses caused by weather extremes are hardly mirrored in modelling yet, and this gap is indeed not targeted by the present study. I propose to change the sentence in question – lines 21f, page 1 – into: "The full spectrum of potentially yield-relevant meteorological averages in varying seasonal time windows is however rarely scrutinized by the existing models; the same holds for landscape-specific weather response patterns of different crops."

If the key question is to optimize the current linear regression model, the innovation of the study is controversial. As numerous studies have found the linear regression performs not as well as machine learning algorithms in reproducing the spatio-temporal pattern of crop yield (Leng and Hall 2020, Cao et al 2021, Cai et al 2019, Zhang et al 2020), I think optimized current linear regression is not able to improve the yield prediction accuracy significantly. Thus, I suggest to revise the research gap.

In the comparison of the model performance to a linear model with pre-defined input weather aggregates the performance gain seemed to be massive, see Section 4.6.2 with Figs 7–9. As I will show in my final comment, this was, as Reviewer no. 1 had already suspected, largely owing to using out-of-sample data for the automated regression variables selection. I wonder whether less constrained machine learning is prone to fall into a similar trap as the highly specific, nonlinear patterns it identifies are probably even harder to uniformly reproduce from different sets of input years. Anyhow, my hypothesis for this paper is that the full potential of linear regression models is regularly not maxed

out because of sub-optimal input factor selections. I will clarify that in the introduction and also set my exhaustive testing approach in perspective to the recent machine learning studies – thanks for the references!

After introducing the research gap, nine questions related to yield prediction were provided but the key question aimed to be addressed in this paper is still not clear for me. Also, I think the literature review is not strongly associated with the question this paper intended to address. This study focused to on weather-based yield prediction but there were papers uniting remote sensing data and weather data to predict yield, which may be related to the question 4 but considering two much non-meteorological factors will departure the goal of weather-based yield prediction.

The nine questions were not meant to define the research question, they should just illustrate how many decisions are left to the modeller applying “multiple linear regression” making any resulting model setup highly individual. The formatting of the questions as numbered list lets them however stand out in an unfortunate way. I will blend them in the normal text paragraphs and emphasize the research question(s) instead.

In the “Materials” and “Method” section, it is necessary to provide some information about the weather relevant covariates and their correlated relationship with crop yield, which can help us understand the primary response of crop yield to climate change established in the model.

This could only be done in a very general way, because there are dozens of candidate variables (time-averages of meteorological variables over different seasonal periods), and their predictive power varies between crops and regions. The setup described in the Methods section is deliberately designed to start without any prior information about variable–yield correlations.

Some observations from the Germany example can however be discussed in the following sections: The overhauled version of the model shows a clear difference in predictive power between Western and Eastern Germany. It only works satisfactory in the east where crop performance is much more limited by water availability. However, not only precipitation but also temperatures and sunshine are correlated to water availability through evapotranspiration. High temperatures in early summer negatively affect crop fertility, this is relevant in the south-west but not so in Northern Germany.

Reproducing the impact of climate extremes is becoming increasingly important in yield prediction models. As the weather data are monthly, I wonder whether the ABSOLUT model can predict the effect of climate extremes, which is a major source of yield loss in Europe (Trnka et al 2014). From the results of Figure 5, the predicted model underestimates the impact of drought in Northern Germany and the district-level performance is not good.

Drought, unlike extreme precipitation, is very well represented in monthly weather data. It is true that the errors in individual districts are high – which is related to the high variability in soils and landscape characteristics. The regional focus of the drought-related yield losses was correctly determined though, and this has been reflected in the text, cf. lines 431–434 on page 19 and 452–454 on page 20. The general underestimation of the drought impact is more likely caused by the absence of a comparable drought situation in the other years on whose observation the 2018 prediction is based on, cf. lines 458f on page 21.

The comparison of other prediction approaches should not be limited to the yield forecast in Germany but some other yield prediction across the major

breadbaskets of the world (Cai et al 2019, Li et al 2021), making the readers understand whether the ABSOLUT is an advanced yield prediction method across the globe. Also, more information about uncertainty of the model should be addressed.

I do agree, but such an extensive evaluation would not fit any more into this model description paper which already seems a bit long. As GMD state in their guidelines for model description papers (https://www.geoscientific-model-development.net/about/manuscript_types.html#item1): "Where evaluation is very extensive, a separate paper focussed solely on this aspect may be submitted."

Specific comments:

Line 22: the full name should be added when the ABSOLUT first mentioned.

OK, no problem.

Line 26: the equation of the ABSOLUT should not be moved to "Method" section.

Should be or should not be? I guess there was originally something like "should not appear in the Introduction" in the editing process, so I will move the equation to the Methods where it indeed seems better placed.

Figure 2 can demonstrate the model performance at country level but why not provide the results of maize simultaneously.

Good idea which I will follow. I will also juxtapose the ABSOLUT results to the ones from the Gornott and Wechung approach for direct comparison in Figure 7.

Line 160: why use some ellipsis?

The ellipsis is meant to supplement the original German title in italics with the English translation. The same format is used in line 158.

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