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Biogeosciences Discussions



# *Interactive comment on* "A model for simulating the timelines of field operations at a European scale for use in complex dynamic models" *by* N. J. Hutchings et al.

**Anonymous Referee #2**

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General comments: The title reflects well the content of the paper. The abstract gives an overview on what was done and achieved in a very general way. The quality of language is good. The publication address a topic which is relevant and of brought interest to readers of BG. Automatic generation of field operations related to meteorological variables are very useful for large area simulations where data availability is limited. This is especially necessary for climate change impact studies where the climatic boundary conditions are altered. However, the study presents results which seem to be at a preliminary stage. The description of the methods is clear although the role of some models within the framework becomes not always clear (see specific comments). As mentioned by the authors the methodology is very much simplified.

The concept using thermal sums for such purposes is not new and the simplified way it was done here turned out to be insufficient to reflect the reality appropriately. There are examples from agro-ecosystem models where automatic sowing and harvest is already included (e.g. DSSAT). These approaches should be considered and discussed in the paper. In the discussion it becomes obvious that many assumptions are not valid and results are not sufficiently good. Some of the deviations are not surprising from the aspect of an agronomist, e.g. too late date for first application of N fertilizer for winter crops. A main merit is that data for testing the time predictions of field operations were only available for one year which is not sufficient to test the climate related variation of timings and model sensitivity within the landscapes. It should be proved if data from phenological observation networks within European countries cannot be used to improve the data base. In conclusion the results are not sufficiently good to justify the use of the model. It seems that the authors themselves have some doubt if the concept of the timeline model is feasible and it is a positive point that they discuss the alternative to implement automatic rules directly into the dynamic models. Another weak point is mentioned by the authors themselves (line 11/12, page 10597): "the current model does not take into account any interactions between the mineral fertilizer and manure application." Moreover, it does not take into account soil mineral nitrogen which is the base of N fertilizer recommendations for many field crops to ensure an environmentally sound fertilization. Especially if trace gas emissions (or water pollution) is the focus of the simulations this is an essential aspect and a further argument to implement the rules into the dynamic models.

Specific comments: To reproduce the findings of the authors it would be desirable that they publish also the values of the thermal sums which were estimated for the specific crops and field operations. Regarding the thermal sums the question arises why the authors have not used crop specific base temperatures, e.g. cardinal minimum temperatures from the literature. Within the section "specifications of the timelines model" there is some confusion as the role and function of CAPRI is not well explained. When the authors mention "the model", e.g. in line 20 on page 10588 it is not clear



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if they refer to CAPRI or the timelines model. At line 17, page 10593 the authors mentioned that “estimated yield was required by a number of ecosystem models”. This requires some more explanation as the impression for the reader is that the estimation of the timeline is used for crop growth modelling as an important part of the carbon budget. Regarding the pure effect on long term soil carbon sequestration timing seems to my opinion of minor importance except if it is used for dynamic crop growth modelling influencing biomass formation. On page 10595, lines 25/26 the authors mention that there is a better relationship between predicted and measured sowing dates for autumn sown crops than for spring-sown crops. However, if I understand right, the autumn sowing dates were taken from the data base and not predicted using the temperature sum method.

Required corrections: Page 10587, line 27: “Wattenbach, 2012” should be “Wattenbach et al., 2012” Page 10592, line 21. the citation of "EMEP, 2009" is not in the reference list. Page 10593, lines 4 and 5 causes some confusion as the landscape names are not well related to the coordinates in brackets. “Turew” should be moved to the next line (between “and” and “Poland”. Line 4, page 10595: the reference to table 1 is not clear. I assume it refers to table S1 in the supplement. Same holds for, e.g table 2 in line 10 at the same page and probably others within this paragraph. Line 16, page 10595: please add “small” between “there are” and “errors”. Line 5 page 10596: Delete the first appearance of “later” in the sentence. Page 10601, line 22 ff.: European Commission. European Soil database is not cited in the text. Page 10602, line 16 ff.: Sutton et al. 2007 is not cited in the text. Fig. 3: the size of the characters is far too small

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