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Comment

## ***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 #1**

Received and published: 3 September 2012

1. Does the paper address relevant scientific questions within the scope of BG? Yes
2. Does the paper present novel concepts, ideas, tools, or data? Yes
3. Are substantial conclusions reached? Yes, even though these are that the model needs substantial revision.
4. Are the scientific methods and assumptions valid and clearly outlined? The methods and assumptions are clearly outlined but it is clear that not all the assumptions are valid. The authors acknowledge the weakness of some of their assumptions and indicate how some of these can be overcome. In many respects this is a difficult paper to judge. The modelling approach has a number of considerable weaknesses which, the results

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clearly indicate, show that much of the model output is unreliable. However, this is openly discussed and there is merit in attempting modelling of this kind using simple approaches and then iteratively revising the model to make it more reliable. Publication should facilitate a dialogue that can contribute to a useful revision of the model.

5. Are the results sufficient to support the interpretations and conclusions? Yes

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Yes

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes

8. Does the title clearly reflect the contents of the paper? Yes

9. Does the abstract provide a concise and complete summary? The abstract is concise but I don't think it fully summarizes the paper. Having read the paper, more than once, I do not think it can be said to be 'broadly capable of simulating the timing.....', since there are some very large differences between modelled and actual results. A difference of two and a half months in the mean estimate of the sowing date of spring barley, which may have a growing season of six months is a very large error. An error of almost 1.5 months in the date of the first fertilisation of winter barley is also a significant error, given that top dressing of nitrogen fertilizer for this crop would not often exceed 2 months. It would be more consistent with the findings of the paper to write something like 'while for some crops a reasonable agreement was obtained in the prediction of the times of field operations there were some very large differences which need to be corrected'.

10. Is the overall presentation well structured and clear? Yes, it is very clearly presented and well written.

11. Is the language fluent and precise? Yes

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and

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used? Yes, apart from one possible instance detailed among the editorial comments.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Some more detailed comments are made.

14. Are the number and quality of references appropriate? Yes

15. Is the amount and quality of supplementary material appropriate? Yes

an initial paragraph or section evaluating the overall quality of the discussion paper ("general comments"),

Given some of the assumptions made I was not convinced the output from this model would be reliable and the results confirm this. However, the model output will be more objective, transparent and useful for scenario evaluation than expert judgement or using activity data collected 30 years ago. And this is a discussion paper so readers can see the difficulties in detailed modelling of agricultural systems when we do not have current, robust and spatially disaggregated data to use. In many respects this is a difficult paper to judge. The modelling approach has a number of considerable weaknesses which, the results clearly indicate, show that much of the model output is unreliable. However, this is openly discussed and there is merit in attempting modelling of this kind using simple approaches and then iteratively revising the model and introducing other inputs to make it more reliable. Publication should facilitate a dialogue that can contribute to a useful revision of the model.

followed by a section addressing individual scientific questions/issues ("specific comments"), and by a compact listing of purely technical corrections at the very end ("technical corrections": typing errors, etc.).

Page 10586, lines 4-7. Is this a very good example? Surely little fertilizer nitrogen is now applied before sowing. A better example would be the application of N fertilizer to silage ground in early spring.

Page 10586, lines 9-13. These are sensitive to short-term weather conditions to

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some extent but isn't long-term weather more important when assessing annual emissions? With respect to nitrate leaching the prime meteorological consideration is hydrologically-effective rainfall (HER) over winter. Apart from perhaps once in a century events HER is a cumulative response to rainfall over the winter months which is a long-term weather impact.

Page 10586, line 28. The mid 1990's are relatively recent but the cereal varieties grown then will now have been largely replaced and there has been a trend toward earlier harvesting so can the author's be confident data from that era are still representative. Plus sowing date is somewhat opportunistic, being related to rainfall and soil conditions rather than temperature (page 10587, line 1). Were there any unusual autumn weather patterns in those years that might make the results untypical?

Page 10589, lines 1-3. I consider this to be a major weakness since neither sowing nor harvest are particularly associated with temperature, although there may be a broad correlation. For example, sowing of cereals. There is a very good correlation between sowing date and yield, as sowing is delayed in the autumn yield tends to decrease. Hence farmers will drill as soon as they can depending on the cereal type and rotational position. They will not be delayed by unusually warm or cool weather, but they may be delayed by rain. I appreciate it would be very difficult to include rainfall in the model, given its unpredictability in the maritime regions and near impossibility of forecasting medium term deviations from the long term mean. However, I do think the authors must acknowledge that their assumption is weak, albeit it may be the only one that can be reasonably made. I appreciate the text goes on to acknowledge 'these were gross simplifications', but perhaps that caveat could be placed earlier in the paragraph (e.g. middle of line 3).

Page 10589, line 14. Minor point perhaps, but line 28 dated the observations to the mid 1990's.

Page 10589, lines 17-18. And for cereals at least major changes in the varieties grown.

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New crop varieties are extensively field trialled before becoming commercially available and so any tendencies toward earlier maturing, which can have an influence on the sowing date of subsequent crops as well as on harvest date, should be available.

Page 10591, lines 22-23. Is there a reference for this? It seems an unrealistically short interval given that many European soils have a significant clay content and will need further cultivations before a seedbed has been created.

Page 10591, line 28 - Page 10592, line 2. Again a reference for this would be useful. I think it is an unrealistically small gap which might be good practice but not common practice. If no reference can be provided for this assumption and the one above then the uncertainties of both should be recorded.

Page 10592, lines 10-11. In the arable regions of w Europe at least this assumption is wrong. It is not standard practice to apply 80% of the recommendation, which can be around 160 kg/ha, in one dressing, which is a large amount to apply in one go. Good practice is to apply about half the main dressing at early stem extension. The remainder is then applied once the first round of application has been completed. This reduces the risk, if the weather turns wet and halts spreading, of a delay in N application to some of the crop area.

Also the assumption of the application taking place after 20% of the growing season has elapsed could be improved upon. For combinable crops the main N applications are made at the time of specific crop growth stages. For cereals the first dressing would be at tillering (GS 21-25) the main dressing at the stem erect stage. Cereal growth stages can be related to accumulated temperature, there will be papers in the literature, and this is the approach that should have been used. This needs to be acknowledged in the text.

Page 10594, line 26. Mean differences between mean recorded and predicted dates are given in Table 2. Looking at Table 2, spring barley, given my comment above about ploughing often taking place well in advance of sowing I'm not not surprised at the large

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difference. However, since the predicted date tends to be close to the sowing date I would have expected it to be later in the year and hence a positive number.

Page 10599, line 6. Given the text in line 2 of the previous page I think the authors meant to write that 'the chances of such inconsistencies arising would decrease...'

Page 10594, line 4. It looks odd to give names of the locations in Denmark and France but only the grid reference for Poland.

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