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Interactive comment on "Modelling economic and biophysical drivers of agricultural land-use change. Calibration and evaluation of the Nexus Land-Use model over 1961–2006" *by* F. Souty et al.

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This paper documents an updated version of a global model of land-use change, Nexus (previously documented in the same journal, Souty et al., 2012), and provides an evaluation of its performance over the period 1961-2006. The paper starts with the observation that evaluation of land-use change models against historical data is not common practice in the community and undertakes this exercise by focusing on different model behaviors: intensification versus extensification of agricultural production, food prices, trade and fertilizer consumption.

1. General comments

My opinion on this paper is really mixed. On the one hand, the model presented is

very rich from a biophysical point of view, built upon datasets of reference (LPJmL, land use maps from Ramankutty et al. (2008), ORCHIDEE for vegetation growth). An impressive work has been done to put all together these different datasets into a single consistent framework. It certainly provides an interesting tool to derive some relationships on land use requirements in response to various forms of additional food demand and/or changes in management. And I suppose that, from a biophysical point of view, the paper provides the level of clarity on the modelling features that is required from a GMD paper. The paper is precise in its description of the model assumptions and well documented (27 figures). I appreciate this effort.

This being said, I am concerned by the economic part of this modelling approach and the evaluation method. The representation of agent behaviors proposed in this framework is quite incomplete, which questions the purpose of the approach and the type of scenario that can be assessed. In particular, the evaluation method is performed under a large set of restrictions and exogenous constraints that are key variables for land use development. Too many times, functions appear to be fit to historical data using ad-hoc adjustments and testing, whereas the relationship between variables would deserve a separate econometric estimation, or at least better account of the past econometric literature on these relations. Some assumptions are in particular quite problematic. For instance, it is assumed that agricultural land only expands into forest, which is a strong restriction. But more disturbing, later in the evaluation, forest cover is also taken as exogenous for all the evaluation period! This means there is not any longer endogenous expansion of agricultural land when testing the model... Other similar assumptions are found along the paper: food demand does not react to prices, trade is subject to self-sufficiency constraints and to an exogenous shifter, when not purely "prescribed" (not ruminant), etc... If some assumptions may be defendable at a model development stage, they seem much more an issue as soon as the model comes to an evaluation stage. The evolution of some economic variable of the model questions some of these assumptions (for instance, the price trend is in the wrong direction for the period until 2000, what does it imply for the intensification response? What about trade?). The link with the rest of the economic literature is too weak, we do not find for instance a discussion on value of supply elasticities. It would be more interesting if the paper was focusing on one more specific question, such as for instance agriculture intensification response - with the role of fertilizer and irrigation, but also other drivers only briefly mentioned in the paper such as technological adoption and R&D. The evaluation scope chosen for the moment is clearly too ambitious, which makes the overall evaluation process hard to follow.

Overall, this paper probably does a good job at documenting a complex model with a rich level of details on the biophysical side. The manuscript is from this respect well developed, and precise on what assumptions have been made. However, it makes no doubt that the model presented here needs to be strengthened on its economics foundations. Some key economic mechanisms are missing and the apparent good performance of the model can only be obtained with a large number of constraints and assumptions. The predictory power of this tool therefore appears relatively limited. The authors acknowledge it several times in their conclusion, which I find quite telling... Some more work remains to be done before the model can perform in a convincing way.

2. More specific comments

- Model evaluation/validation: the authors only refer to an article from Beckman et al. (2011) that focuses on CGEs, and they conclude that model evaluation is not common in economics. But economics is much wider than CGEs! The purpose of econometrics is usually to test models on past data. And model validation has been discussed in the linear programming literature (e.g. McCarl and Apland, 1986). It is noted however that the word "validation" is not used in the article. However, this seems strange, as evaluation of model performance on past data is a widely used approach in this literature on model validation. The article concludes that the model performs well. Does that mean the authors consider their protocol validates the model?

- Calorie metric: I am a bit skeptic about the capacity to predict land use change through a demand aggregated in calories. It seems to me that calories per ha are quite heterogeneous across crops and crop composition directly impact aggregated calorie yield. I did not find where the crop composition bias was controlled for in this exercise. Dynamic crop yield is related to level of input and also to irrigation but apparently not to the change in underlying crops in the production functional form. The crop mix has also a direct consequence on the potential yield. Did not understand how this was taken into account.

- Agricultural land expansion: p. 6980, l.10, one can read: "in each region, it is assumed that agricultural expansion (cropland and pasture) can only be made at the expense of forest". How to justify this? Agricultural land expansion into other areas than forest is common. See for instance in Brazil expansion of land into Cerrado. More worryingly, that also means that in the retrospective exercise, where forested area is taken as exogenous, agricultural land is also exogenous... That reduces significantly the challenge of reproducing cropland development, but also constrains the system to intensify by driving prices up!

- Trade sufficiency constraints (same page, l. 16): how is this empirically grounded? How is this parameterized? This is also a very strong assumption, and I suppose that is significantly restricts the trade development patterns.

- Ramankutty and Foley (1999) are used for annual observation based estimates for the period 1961-2006! Their paper title mentions 1700-1992. Not clear how we obtain the last 14 years of the time series. Also, a few words on how this time series of historical land use has been produced would be welcome.

- Agribiom: I did not succeed to access these data and underlying assumption. What is this database and why using that one rather than FAO, usually a common reference for all the parameters listed in the paper? In particular, some values in Agribiom seem to differ from FAOSTAT (for instance, in fig. 2, consumption per capita is higher for the

US and EU than level in FAOSTAT in 2006).

- Fixed cost: I don't think the assumptions made here to calculate FC make sense. The value added contains the profit margin that goes to the farmer and is usually considered associated to land rent. When agricultural prices vary, the value added changes but not labor and capital costs. Land rent change is here assumed to be a function of population change per hectare, without any consideration of agriculture profitability; it does not make sense to me. Additionally, the numbers show on Fig. 7 are a bit strange. Why would production cost per ha of an intensive system be more expensive in China than in the US? . . .

- Trade: I do not understand the modeling of trade. There is a large body of literature on trade modelling. How do the assumptions in Nexus relate to it? First the formula adopted impose imports to be a linear function of domestic price. That's a very strong assumption, one would expect a discussion on the right value of price elasticity of imports. The rest of the calibration does not make any sense to me. First, the alpha parameter value is shifted because trade increased over time: what mechanisms is it supposed to represent? We don't know. Second, as said before, the price trends goes in the wrong direction globally on the period 1960-2000. Regional price trends should inform on which regions export more over time, and which import more. The calibration of gamma can only make sense if the regional price trends are right (what an econometric approach would look at). Here, by taking simulated price trends rather than historically observed price trends, I don't see how the values of export elasticities can be rightly estimated, and then the trade behavior validated.

3. Some details: - Formula (3) is not very clear (check k indexes). - Fig 10. I find the x-axis changes of scale extremely misleading to compare calorie supply curves. For instance, see the difference between Africa and China. . . Having the same scale would allow to see the differences in potential yield assumptions, and where significant changes in intensification have been observed (not in Africa obviously). - Fig 13. See previous comment on time series from Ramankutty Foley (1999) going until 2006. - Fig

14. Not clear at all what the dashed line represents. It seems that the ration between two axis changes across regions, this requires explanation - Fig 25. Impossible to read. - Fig 27. Why showing this graph considering that this is exogenously constrained?

4. References

Baldos, U. L. C. & Hertel, T. W. (2013). Looking back to move forward on model validation: insights from a global model of agricultural land use. Environmental Research Letters 8 (3), 034024.

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