

## ***Interactive comment on “The Nexus Land-Use model version 1.0, an approach articulating biophysical potentials and economic dynamics to model competition for land-use” by F. Souty et al.***

### **Anonymous Referee #2**

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The manuscript presents the rationale and applications of new land-use model (‘NEXUS land use model’) that is aimed to model impacts of policies (e.g. forest conservation, energy ) or rising energy prices on land-use change and agricultural intensification. The model is documented in an extensive, open way. Specific innovations address the integration of biophysical and economic information into one framework, a better representation of decision making processes in land use, improved mechanisms of the livestock system(s), including fodder production, as well as agricultural intensification – all “neglected children” of land-use models.

This is certainly a heroic task, and the principal merit of the paper lays in the fact that

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it is attempted and so a process is started. In particular, it has some holdings to use a production function to discern between land use intensity classes and to base the land-change model on such an approach.

However, some severe caveats relate to the implementation of these elements in the model.

First of all, the paper fails to make explicit the purpose of the model, and does not discuss or elaborate on the strengths of this particular model over other models or older model generations. The paper creates the impression that here an innovative, bath-breaking new approach is presented- but the expectations raised get harshly disappointed. A more modest language revision is strongly recommended in order not to disappoint the readership and to save the merits of the paper. Particular attention should be paid to (a) explicit discussion of the purpose of the model and the innovations, and (b) an illustration – at least on an exemplary basis – of the strengths of new model elements over alternative implementations (e.g. other models).

In overall terms, the text is not an easy read, which may at least to a big part result from the “jumping” structure of the text. In particular, the introductory part on the modelling philosophy should be improved, the general principles of the model remain vague and in consequence, it is difficult to follow the many and complex elements of the model and to understand implications of specific assumptions and approximations. An illustrative, meaningful graph on the different modelling steps) would be an asset (Fig 3 is unfortunately not sufficient); following a pyramidal principle in the introducing paragraphs of each chapter certainly would help.

Contentwise, the Nexus-model in its current state has some crucial limitations: - decision making in land use does NOT exclusively follow Riccardian laws, the diversity of decision making is completely ignored in the model. Land-use decisions are only in the exception of industrial, large-scale agriculture based on cost minimization strategies, but much more driven by incentives such as risk minimization, minimum income,

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minimum leisure time (due to the strong labour input - marginal output link), and thus depending on variables such as alternative income possibilities, commodification status, access to markets, availability of capital. The authors are aware of this (“The theoretical basis governing the Nexus Land-Use does not completely match the reality” p606), but this insight remains without consequences for the modelling strategy and the paper.

Indeed, the approach of the nexus model to use potential yields for land allocation is more or less the mainstream in land-use allocation models, and should not be used as the innovation hook of the paper. A further legitimation of this approach is required. The argument that certain fraction of land use / cover (extensive pastures) are beyond the modelling scope and thus represent these “economic imperfections” (see reply to reviewer #1) is not valid, as it is the modelling representation that is interesting, and not the static assumptions derived from observations. The question must be: how will this change when drivers change?

- in the current version, cropland expansion is an exogenous variable, a function of deforestation. It is stated that this will be changed in a next version. This limitation questions the purpose of the model altogether, and it should be made explicit why, without this implementation, the nexus-model is worth to be published. The main innovation, to model “a generic representation of agricultural intensification mechanisms” will not be achievable if land expansion is not an endogenous option in the model.

- The same holds true for the fact that the substitution effects between chemical inputs, labour and capital cannot be modelled – they are at the core of intensification (yields can be increased with higher fertilizers, but also higher labour, weeding, for example, mechanization is also important, etc.)

- It remains unclear why "dynamic cropland" relates only to 50% of the cropland area (and 75% of production). The Maps in Fig 4 and 5 seem to cover the entire global cropland area, and pot yield and act yield are both calculated for all grid points. Apparently,

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the method description is not explicit enough at this point. The implications and limitations that result from this approach, that only 50% of the cropland area dynamically modelled, remain unclear.

- The principle of splitting intensive-extensive land use does not become clear, the caption of Fig 11 (and 12?) is almost content-less. Furthermore, the cropland seems to be treated as an entity, only discerning the part which is modelled with LPJmI, and one that is not. But is there a individual production function at the cultivar (CFT) level, and not only for each region?

- The text fails to convincingly argue on the advantage of an assumption of a fixed mix of CFTs (for the year 2000) for calculating one aggregate representative yield, and not to work with rotation constraints etc. This is a crucial difference to magpie, but not legitimated further than to state that the error might be small. But the magpie approach has it holdings, as it is based on agricultural principles (such as rotation restrictions), and not on a snapshot distribution for the year 2000, which is a result of potentials AND actual implementation of farmer decisions at the same time.

- extensive pasture production is assumed not to follow production potentials. This is simply wrong. Also, referring to the permanent pasture class, derived from FAO (and in particular in the Ramankutty 08 interpretation which reduces the extent of grazing land further) is an underestimation of land currently used for subsistence agriculture, because permanent pastures refer to land used for more than 5 years for grazing. This definition leaves out rangelands and other grazing land, which nevertheless sustain a signification fraction of the world population. Such a restriction can affect the model results and their interpretation considerably.

- the exemplary application section is almost meaningless.

Conclusion: the paper presents a start - limitations of the model make it necessary that the text elaborates specifically about the added value of this approach in its current implementation in order to justify publication. Contextualisation is the weakest point

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- the discussion of the scope of applicability and limitations of the approach is poor. Clarity of the text and referencing should be improved.

Minor remarks: - make clear that the NEXUS land-use model is the name of the model, and not referring to the nexus between things

- Why is the land-expansion area pot yield modelled with a “without irrigation” assumption? Future land change might entail the expansion of irrigation.

- the term "land classes" for depicting land units with similar potential yield is misleading; land classes usually denote classes such as tree cover, cropland, urban, etc.

- the description of the use of the Bouwman et al. dataset is not described adequately

- No link to Fig. 12 in the text

- referencing in general is poor

- pg 573 the differences of the approaches have to be presented in detail, this is too much insider-speak

- pg 574 2 cropland classes, but the figure 2 holds 3: agrofuels, dynamic, other

- pg567 – the plus 4% relates to pasture area, but does not say anything about land under grazing

- pg 577 data source for chemical inputs?

- pg 578 I am not convinced that omitting monogastrics-trade is a wise decision (see Galloway, J.N., Burke, M., Bradford, G.E., Naylor, R., Falcon, W., Chapagain, A.K., Gaskell, J.C., McCullough, E., Mooney, H.A., Oleson, K.L.L., others, 2007. International trade in meat: The tip of the pork chop. *AMBIO: A Journal of the Human Environment* 36, 622–629.)

- pg 579 revise paragraph, clarity should be improved

- pg 585, ln 16 The sentence “This rained . . . .” is unclear

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- pg 588 unclear how you treat fodder land
- pg 593 quote for the Lagrange multipliers method

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Interactive comment on Geosci. Model Dev. Discuss., 5, 571, 2012.

## GMDD

5, C290–C295, 2012

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