Geosci. Model Dev. Discuss., 5, C191–C194, 2012
www.geosci-model-dev-discuss.net/5/C191/2012/

**Geoscientific
Model Development
Discussions**

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 #1

Received and published: 23 April 2012

General comments: A new approach to modelling land change at global scales is very much welcomed. Current models either are dominated by economic rationale ignoring physical and environmental processes and feedbacks or are based on physical processes only. Model coupling has tried to resolve these issues (see e.g. (Eickhout et al., 2007;Van Meijl et al., 2006)), but not always sufficiently. I therefore very much welcome any attempt to make progress into this direction.

Upon reading the paper a certain disappointment is, however, obvious. Basically the same rationale used in large scale economic models (aggregated cost-minimizing agents) and that in physical models (a strong focus on potential crop yield) is taken.

Such approach ignores the social diversity underlying decision making structures and deviations from ricardian decision making. Moreover, the focus on potential crop production and the notion that actual crop production is just a result of the farm economic decisions ignores the fact that in many regions the options to move towards potential crop production levels are either not achievable due to socio-economic constraints or not optimal from an ecological perspective. A lot of the constraints to move to higher yields have to deal with risk and farm structure/farming system choices. A stronger focus on the current reasons underlying variations in actual crop production and the farming systems determining the large spatial variation in management intensities might have been a more rewarding and innovative approach.

The paper mentions that "yield variations induced by the possible expansion of croplands on less suitable marginal lands are modelled by using regional land area distributions of potential yields, and a calculated boundary between intensive and extensive production." as a main innovation. However, this is very much the same approach as taken by the coupling of the GTAP and IMAGE model a few years ago. The authors should better acknowledge that and should certainly not claim this as an innovation achieved in their paper.

A main constraint to the model structure is that 'The model external drivers are the calorie consumption per capita, the share of animal products in food consumption, agrofuel consumption and evolution of forest areas'. I guess that also demographic developments are external drivers. Why have consumption patterns been turned into exogenous parameters to the model? Consumption patterns, especially in developing economies are highly depending on economic growth and food/fuel prices. The advantage of general equilibrium models is that such feedbacks can be included. Making such drivers exogenous makes it difficult to apply this type of modeling for longer term scenarios. This is strange, as the authors indicate that their aim is to couple the model to climate change models. Similar considerations for fertilizer prices, these are likely to be demand dependent?

The model both models intensification of agriculture and/or area expansion or contraction. However, these two processes are strangely disconnected by an allocation of land cover by land intensity classes. The assumptions underlying this model architecture are not outlined specifically. Moreover, it is not clear why this somewhat strange approach was chosen out of the other possible architectures of the model.

A main problem of this type of models is that they minimize production costs of an artificial agent/farmer representing the whole world region. In reality, multiple, sometimes contradicting, land change trajectories happen at the same time. Abandonment and intensification are often happening in nearby areas. The representation of an aggregate agent in the decision making model may cause all kinds of scaling issues. At least, this should have been discussed in the paper.

The authors themselves indicate in the discussions to some extent the limitations of their ricardian approach. However, at the same time large claims are made of the potential of this model to make all kinds of predictions. I would rather see a bit more modest approach in which the model is used to see how, under the very strong assumptions made, the supply reacts to demand scenarios. The implications for reality and policy should only be made with enormous care since the model is not validated and some of the underlying assumptions are very questionable.

Specific comments: -The introduction does not clearly explain the differences of the proposed modeling approach to existing models and integrated assessments at global scale. Although MagPie as a model of the same group is mentioned the differences are not clear. The other competing efforts such as MiniCam, GTAP/IMAGE, GTAP/AEZ are ignored as well as the many continental scale approaches that take a similar approach such as CAPRI-DynaSPAT. -It is unclear what is the difference between exogenous variables and those variables (such as population) that are forced by the scenarios. -The referencing is very restricted, a lot of wealth on alternative models at global and regional scale is available that should have been better accounted for, e.g. the LandSHIFT model that takes, on the physical site, a similar approach.

References

1. Eickhout,B., Van Meijl,H., Tabeau,A., van Rheenen,T., 2007. Economic and ecological consequences of four European land use scenarios. Land Use Policy 24 (3), 562-575. 2. Van Meijl,H., van Rheenen,T., Tabeau,A., Eickhout,B., 2006. The impact of different policy environments on agricultural land use in Europe. Agriculture, Ecosystems & Environment 114 (1), 21-38.

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