Biogeosciences Discuss., 4, S976–S977, 2007
www.biogeosciences-discuss.net/4/S976/2007/

Biogeosciences Discussions

BGD

4, S976–S977, 2007

Interactive Comment

Interactive comment on "Linking an economic model for European agriculture with amechanistic model to estimate nitrogen losses from cropland soil in Europe" *by* A. Leip et al.

Anonymous Referee #1

Received and published: 26 July 2007

The manuscript demonstrates the potential of integrating process models and economic models to improve the interpretation of agricultural or agri-environmental policy impacts on greenhouse gas emission or groundwater pollution. As such, it is a very important work with respect to tracing the human impact on element cycles in ecosystems on a landscape scale. The authors would focused the paper on the methodology which was developed in order to get a new policy impact simulation tool by linking the large-scale economic model CAPRI with the biogeochemistry model DNDC and would presented only some "preliminary" results. I think that the most interesting points within the manuscript are the data accumulation including the dis-aggregation procedure and the modelling results. However, the authors need to point out more precisely the ben-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU

efit for the reader with respect to this specific linking of the two models. In particular, what is the specific value for the scientific community considering the linking, since a linking of models was already done by a lot of other working groups in the past inter alia by the authors itself (e.g. SOIL USE AND MANAGEMENT 22 (4): 342-351 and 352-361, BIOGEOSCIENCES 2 (4): 353-375, and AGRICULTURE ECOSYSTEMS & ENVIRONMENT 112 (2-3): 233-240. In this form of the manuscript I am not able to find out the novelty of the presented approach, especially since advanced model couplings are already published. Furthermore, the definition of appropriate calculation units likes Homogeneous Spatial Mapping Units (HSMUs) is common, e.g. as the hydrologic response units (HRUs) in SWAT. Nevertheless, I clearly recognise that the linking of the model is a lot of work, but in the present form I can not accepted this part of the manuscript as the major topic. Alternative I recommend the authors to shorting the methodology part and refocus the current manuscript on the model results and the uncertainties of the results with respect to the quality of the used input data. This was already done in the manuscript, for example with the good discussion about the influence of the dis-aggregation on the input data. But I'm missing the information about the influence of the dis-aggregation on the model output. Another example is the presentation of the emissions from soil in Tab. 4, without any information about the uncertainty of this information and /or a discussion about N2 flux measurement and modelling. As far as I know DNDC is widely validated for field N2O emissions measurement, but never for N2 emissions. I missed a discussion whether the N2O/N2 ratios calculated from the given N2O and N2 emissions at country scale (range from 1.2 to 0.08) are really realistic. In my opinion the range is too wide at this high level of aggregation. If this will not be discussed in detail, the paper runs the risk to be interpreted in a wrong manner. I will give more special comments to this manuscript, if the authors reply to the above mentioned points.

Interactive comment on Biogeosciences Discuss., 4, 2215, 2007.