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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.

A. Leip et al.

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We would like to express our thanks to the three anonymous referees who have on one hand acknowledged our work as an ambitious and carefully conducted study, and on the other hand have provided a detailed analysis of issues that helps to improve our manuscript. We have scrutinized each of the specific and technical comments and considered them in the revised version of the manuscript as indicated below. Nevertheless, there are a few general comments that we like to address here:

1. The manuscript was in several places too long and required additional proof-reading

2. It appeared that it remained somewhat unclear how our study distinguishes itself from other studies
3. It was questioned whether the identified possibilities for improvements of the work imply that our study is not mature enough for publication

Regarding the first point, we have completely revised the paper and shortened the manuscript in many places. In doing so, we considered not only the many suggestions made by the referees, but based the revision on an additional external proof reading and own cleaning-up. The introduction has been completely revised adding explicitly objectives at the end of the section. One referee suggested the use of an appendix to shorten the overall paper while maintaining the level of detail. We decided that for the section on the validation of the land use and the livestock density maps the appendix is indeed the more appropriate location, as this section is very detailed. The main results of the validation are now included in the discussion of the land use map in section 4.2

Regarding the second issue, we like to emphasize again that we see the strength of our approach in the combination of a Europe-wide coverage and a high spatial and thematic resolution on one hand, and maintaining internal consistency between the economic and the mechanistic model on the other hand, for example by accounting of fodder production, feed purchases and animal products versus manure excretion and nitrogen application. Further we are not aware of a study that has put similar efforts into the generation of an EU-wide agricultural land use map, combining consistency with EU-wide statistics and the spatial distribution of land use shares at a detailed level based on a large number of observations obtained from the LUCAS survey. Hence, we think that the above makes clear that there are important differences between our work and for example EFEM-DNDC, which has been mentioned by the referees. Other EU-wide assessments, focusing on long term trends and are linked to the well defined IPCC scenarios (Smith et al., 2005b; Schröter et al., 2005) or aiming at a multi-sectoral integrated assessment (IMAGE, RAINS) based on empirical relationships, have clearly

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a different scope and approach than the one presented in our study. Studies with a similar scope (Schmid et al., 2006) are discussed at length in the manuscript.

Regarding the third point, the needs or rather possibilities for improvements identified must bee seen in relation of the ambition of the study. The European soil map is the basis of other EU-wide studies (e.g. Smith et al., 2005b) and we describe how we dealt with the problem to improve soil initialization. We regard our approach of calculating manure and mineral fertiliser application rates individually per crop and HSMU in dependence of economic constraints (livestock density, manure availability etc.) and crop need as a function of the environmental conditions as an important novelty of our study. Further improvements will be possible if CAPRI will be trained to predict simulated responses of DNDC. This, however, goes beyond the scope of the present manuscript and will be a separate study. Simulated yields are rarely compared to statistical productivities, mainly because the aim of a regionally closed N-balance in agriculture is far beyond the scope of most studies. A "quick and dirty" solution to the discrepancies we found would be to apply a fixed correction factor to the potential yield data offered to DNDC. We decided against this solution, as such an approach would not be according to the standards we set ourselves and we expect this problem to be largely solved once the above-mentioned training of CAPRI is accomplished, which, as said before, cannot be part of the present study. In conclusion, even though we termed our N and C flux results as "preliminary" we are certain that the quality of these data is at least as good as that of any other similar study. In writing the manuscript we let us guide by the maxim of maximum transparency.

1 Anonymous Referee #1

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The manuscript demonstrates the potential of integrating process models and economic models to improve the inter-

pretation 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 benefit 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.

The examples brought forward are Sleutel et al. (2006), Kesik et al. (2006) and Neufeldt et al. (2005), which are all discussed in the manuscript. There are important differences/improvement between the mentioned studies and the presented work. The study of Sleutel et al focused on Flanders in Belgium (ca. 4,125 km2), where very detailed soil and crop data were available. The study of Neufeldt et al. investigates the federal state of Baden-Württemberg in Southwest Germany (14,681 km2). The 'downscaling' approach used by Neufeldt et al. is based on Corine data (for cropland, grassland, vineyards, and orchards) solely implying a correction factor where the total area does not match with the statistics. Our approach covers an area of 4,261,000 km2 and further includes additional elements that were downscaled such as potential yield, mineral fertilizer and manure application. The study by Kesik et al. simulates nitrogen oxides fluxes from forest soils.

Fusion of biogeochemical models with human dimension would be generations-long efforts for quantitatively linking anthropogenic activities to the physical processes occurring at the landscape, regional or global scale. We are at the very beginning of the efforts. The relevant papers (e.g., Sleutel et al. (2006), Kesik et al. (2006) and Neufeldt et al. (2005)) just appeared in publications. That in no means implies the integration between process models and economic models has been accomplished. In fact, due to the complexity of either economic models or biogeochemical models, the methodologies for the integration should be highly variable from case to case. This manuscript is to make a special contribution to the methodologies of the fusion.

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.

It is obvious that explicit modeling needs to be based on pre-defined units with well-defined characteristics. We do not claim that the idea of intersecting different maps to derive appropriate simulation units (be it HSMUs or HRUs) is very original. However, the specific value of our work is in the methodology of deriving the characteristics at this level, which goes beyond what we have found in literature. Also, most studies involving SWAT are done at the watershed-scale. The most similar approach that we found and which is based on the HRU is the work by by Schmid et al. (2006), which however is not yet published in a peer-reviewed journal (E. Schmid, March 2007, pers. communication). We dedicate several paragraphs to discuss the differences with our approach (section 4.1).

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 I8217;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 N_2 flux measurement and modelling. As far as I know DNDC is widely validated for field N_2O emissions measurement, but never for N2 emissions. I missed a discussion whether the N_2O/N_2 ratios calculated from the given N_2O and N_2 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.

Dinitrogen (N_2) is an end product of denitrification in soils. However data of observed N2 fluxes are sparse due to the difficulties in direct measurement of N_2 emissions in fields. The DNDC model was developed to meet the gap. In DNDC, the conversion of N_2O to N_2 has been explicitly parameterized based on the well-documented laboratory work done by Leffelaar and Wessel (1988), in which the growth rate, death rate and C consuming rate of N2O denitrifiers were precisely measured. In other words,

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DNDC has been built up upon the observed activities of not only nitrate denitrifiers, nitrite denitrifiers and NO denitrifiers but also N_2O denitrifiers that should, at least theoretically, have set a sound basis for the model to reasonably simulate N_2 production in soils (Li et al.,1992a). Although the directly measured N_2 data are sparse, efforts were still made to validate the DNDC-modeled N_2 fluxes. For example, a number of researchers conducted field experiments using acetylene to inhibit the conversion of N_2O to N_2 so that they quantified total N_2O+N_2 emissions. With this technique, N_2 was indirectly measured (Ryden 1983). These data have been utilized for validating DNDC, and some of the results have been published in the early papers of DNDC (Li et al., 1992b). In addition, N_2 emission is modeled by DNDC also regulated by the substrate mass balance. As a N-oriented biogeochemical model, DNDC is capable of quantifying soil ammonium, nitrate, DOC, temperature, moisture, Eh and pH dynamics at a daily time step that should minimize the modeled error for N_2 emissions.

In summary, DNDC is capable of estimating N2 flux or total denitrification rate with a sound scientific basis although more validations are required to further strengthen this feature.

2 Anonymous Referee #2

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This work is an ambitious study which attempts to develop the framework for a tool for determining the socio-economic linkages with environmental assessment over a very large region. The authors provide detailed technical aspects involved in allocating and disaggregating much of the data necessary for both the economic and environmental assessment portions of the model, however, weather variability and tillage intensity are important activity data that could be useful to the assessment. Greenhouses gas emissions are known to be highly variable from year to year depending on regional change in weather patterns. The grammar in the manuscript needs to be cleaned up, the paper should be shortened where possible, and the introduction does not provide a clear and concise background and objectives for the paper. As well the paper should place a more focus on the appropriateness of using DNDC versus another mechanistic model for this study. The manuscript shows promise but requires substantial work. I've included below several comments and specific changes which should be addressed.

We will address the specific comments below - for the technical comments another author's comment is required

2.1 Specific comments

1. The use of "agricultural soils" in the title will better accommodate livestock coverage in the study than "cropland soils"

Livestock is included in our methodology exclusively to estimate manure application rates per crop and HSMU. Even though nitrogen fluxes and greenhouse gas emissions from livestock production are indeed calculated within the CAPRI framework, they are not the focus of the present manuscript and hinted at only in Figure 1. We present and discuss only simulations of fluxes from arable soils, also excluding grassland and permanent crops. Therefore we feel that the term "agricultural soils" is too wide and even the term "cropland" (which include permanent crops, IPCC, 2006) might not be appropriate. We therefore propose to use the term "arable soils" in the revised version.

2. The abstract suggests broader coverage in terms of outputs to include GHGs and carbon fluxes whereas the title restricts coverage to N-fluxes. Some reconciliation is needed.

The abstract describes first the scope of the methodology that was set-up (the estimation of the nitrogen and carbon balances for cropland soil) and then restricts the scope of the present manuscript to the presentation of first results on the nitrogen budget, as possibilities for improvement have been identified. The title was referring only to the latter. We see that this differentiation may appear a bit awkward and suggest to change the title into " . . . to estimate nitrogen and carbon losses . . . ".

3. In places it can be difficult to understand the intention of the authors. More proof reading is required. Also, attempts should be made to shorten the document, perhaps in some of the methods and results sections.

The paper has been proof read and shortened.

4. The author states they used statistical production data for yields as inputs values for the potential yields in DNDC. Would this not underestimate the maximum potential yield achievable for DNDC if it was based

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on average measured yield data?

The referee touches here one of the weakest point of the current methodology and this is also extensively discussed in the manuscript in sections 3.3.2 and 4.3.2. We make this issue very transparent in Figures 11 and 12. Indeed, Figure 12 suggests that higher N_2O emissions are produced the closer the simulated yield gets to the potential (= statistical) yield. One must be careful with the interpretation though and we conclude only that emissions tend to be higher on high-productivity sites. We considered it as in-appropriate to add a lump-factor (of say 20-25%) to the statistical yield when estimating the potential yield for DNDC. This approach would have closed the gap between **average** simulated and statistical yield, but only shifted the real problem which lies in the spatial heterogeneity of the relation "potential yield/simulated yield". As was outlined in the last paragraph of the conclusions, the "... second step will be to link the bio-physical model back to the economic model" in order to enhance the capability of the economic model for environmental impact assessment, but also to "increase further the consistent estimation of nitrogen fluxes in European agriculture."

5. One climate year does not encapsulate the climate variability necessary for estimating N2O emissions. (100 year spin up with one climate file + 30 year of climate data would be a better approach). Tillage intensity is also of importance whereby the authors should compare emissions from intensive, minimum and no-till agriculture. This is of importance within the economic model.

(Includes also response to technical comments on Page 2228, lines 21-23 and on Page 2229, lines 10-11)

There are several factors that are not considered in the present work that make the simulated nitrogen and carbon fluxes likely to be not completely realistic. The referee mentions two factors: the climate variability and the tillage intensity. We can add more: multi-cropping systems (relevant in southern Europe) and (very important) crop rotations and land use changes. Tillage intensity is implemented in the standard version, but statistical information on tillage intensity in Europe are not available. Our group is currently working on a time series of weather data for the period 1900-2005; we are cooperating with groups working on the estimation of land use change and we are also implementing crop rotations for dedicated simulations. Each of these pieces requires a lot of work and the scrutiny of a peer review. Even though it would be ideal if they were already in place for the current study, research must advance step-by-step and we firmly believe that the step presented in the current work is an important one towards a realistic high resolution and large-scale assessment of cropland soils in Europe. We emphasize in the manuscript that our aim is to offer the scientific community a detailed description of such an approach and highlight that the results in terms of nitrogen and carbon fluxes are preliminary.

6. Based on your methodology I assume a small amount of manure is applied across all land area. I realize that it is not possible to place manure on land where it is actually applied, however, a better strategy may be to apply manure at maximum recommended rates on some crops until all is applied then apply fertilizer to the rest of the land. Soil C change and GHG emissions results may not be as seriously compromised if this methodology was employed.

(Includes also response to technical comments on Page 2227, lines 16-28 and on Page 2234, line 12)

As we tried to explain in section 2.4.2, manure application across the land is not uniform, but a function of crop type and animal stocking density in the surrounding of the HSMUs. Also, upper bounds for manure application according to the nitrate directive are set. This results in very diverse application rates. Within a uniform region and on one crop-type, we do not differentiate (e.g. only half of the maize fields receive double dressings). It is certainly worth testing the impact of this different scenario on the nitrogen and carbon fluxes in a region.

7. In a quality control process the work would be strengthened if more comparison could be made between measurements and DNDC output within the study region.

This is certainly true – so far we have to rely on the several quality control exercises that have been done with DNDC, also for European field sites (summarized in Li et al., 2005). The fact that DNDC has already been successfully applied in many regions of Europe is also one of the main reasons why this model was chosen in the present study.

Only during the past decade, several biogeochemical models were accomplished and reported in various publications. We selected the DNDC model for this study based on a wide range of comparisons. Based on publications in 1990-2000, about 4-5 biogeochemical models were tested to be suitable for quantifying C and N dynamics for agroecosystems. However, most of the reported tests were conducted by the model developers. Since the beginning of this century, driven by the research demands, more researchers beyond the model developers independently tested these models with a diverse datasets observed worldwide. Based on the latest publications, DNDC is one of the few models, which showed its applicability for quantifying crop yields, soil C sequestration, trace gas emissions and nitrate leaching across climatic zones, soil types and management regimes in North America, Europe, Asia and Oceania. DNDC is currently adopted for several large regional projects sponsored by NASA, NSF and

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USDA in the U.S. In Europe, DNDC is being utilized as the central model to interpret, integrate and extrapolate field observations through the project NitroEurope. In China and India, DNDC has been used for rice production-induced greenhouse gas inventory and mitigation. DNDC was intensively tested through these projects with promising results.

However, particularly the European project NitroEurope-IP, to which some of the authors are partners, will provide new field data and thus allow us to make additional quality control work.

8. In section 2.2 where spatial mapping units are defined, a fourth database, presumably a database of administrative units should be enumerated for completeness.

OK – see answer to technical comment nr. 39 on Page 2221, line 25.

9. Section 2.4.4: Number and timing of fertilizer and tillage applications were taken from defaults in DNDC farm libraries. This would seem to have an significant impact on the estimate of N2O. These values should be replaced with actual agricultural activity data from specific regions, where available.

We realize that estimation of field management operation was poorly explained. A possible reason could be that they were split into three 'one-sentence-sections' and the linkage among them was not clear. Indeed, sowing dates are obtained from Bouraoui and Aloe (2007) that calculated sowing and vegetation period for small administrative regions in Europe. This information is the basis also for determining tillage and fertilizer application scheduling. From the DNDC-library, only some expert knowledge about time lags between sowing and management practice is obtained and applied on the region-specific sowing dates. JRC holds a dataset of observed phenological data from some crops and a quality check against these data has been made by Bouraoui and Aloe.

10. In section 2.6.1 it is mentioned that with the default version of the DNDC model, the farm library is constant at provincial level. However, it is not made explicit how the province relates to either NUTS2/3 or the HSMUs. A flowchart for the linkages for the data down to the HMSU's would be helpful.

We admit that the reference to the 'province level' is unfortunate, as it is taken from the default DNDC-structure and has no relevance in DNDC-EUROPE. We would suggest modifying the sentence into a neutral wording: "In the default version of DNDC, the farm management data cannot vary at the level of the simulation units for a specific type of land use." Figure 2 already shows the structure for the set-up of DNDC-EUROPE. Note that there is no reference to 'regional' level, as all spatial information is linked to the individual simulation unit. How these data

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are derived is described at length in section 2.1 through 2.5.

Nevertheless, on suggestion of referee #4, the whole section 2.6.1 is very much related to “DNDC-insider” issues and we therefore propose to remove this section.

11. It will help to predefine all acronyms before they are used in a stand alone form in the rest of the text.

Done

3 Anonymous Referee #4

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The paper touches important topics and provides a high level of detail in coupling an economic agricultural sector model with a biophysical model focusing on nitrogen deposition. In the beginning the paper discusses well the issues on scale mismatches in available statistics and required model inputs. Also regional vs. local-level environmental problems and suitable modeling approaches are appropriately discussed. In my view the paper is mainly a validation paper on the model coupling procedure, technical aspects and data preparation, downscaling and management between the two models. As is pointed out in the end, the focus here is (not yet) on model application and scenarios. So in parts of the paper it seems a bit early for such a publication without having some scenario results at hand. The authors do well acknowledge further work needed in the discussion part. So it is up to the editors to decide whether this strong focus on validation is worth publishing. The title to me promises a bit more than the paper actually holds. Instead, the description of data handling, downscaling and model coupling is described in much detail, in parts too long from my perspective. Maybe some subchapters in chapter 2 and 3 could be moved to appendices (or supporting online material) to make the paper better accessible without dropping the details. To make it clear, I think this work has great potential and is carefully done. The question is, is it already mature enough for a paper which demonstrates the modeling approach, related data issues and first results. Two general points remain unclear to me:

(a) What is actually calculated by which model? As I understand, crop yield is taken from CAPRI and used as an input in DNDC. But then, DNDC is a process model calculating biomass and related nutrient flows. So, isn't yield of harvested organs also calculated within DNDC? For me the "workflow" between the models needs clarification here.

As we describe in section 2.4.1 DNDC calculates yield of harvested (and other) organs implicitly against a ‘potential yield’ which must be given to it externally. This is calculated in CAPRI. The results are presented in section 3.3.2 and further discussed in section 4.3.2.

(b) In the end, the paper talks about 99-year simulations, which confuses me. Are these just "synthetic" scenarios to calibrate DNDC (or bring the model into equilibrium, or something else)? At another point it is said, that future application of the coupled modeling framework will probably cover a time span of 10 years. So what input is taken from CAPRI for the 99-year simulations. Possible structural limitations of CAPRI for long-term projections are not discussed.

Correct, the model runs 98 years in order to remove bias from the soil map. We introduce this concept now already in section 2.5.2 (Soil data) to make the purpose of the 99-year run clearer. The “performance” of this approach is discussed in detail in section 3.3.3

The timeframe of CAPRI is bound to the timeframe agricultural projections are available. CAPRI calculates on the basis of these general projections a baseline and policy scenarios. In section 4.1 we mention that the CAPRI-DNDC framework is thus limited to the availability of agricultural projections, which is usually about 10 years.

We will address the detailed in a separate author’s comment

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