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Interactive comment on "Ecological Controls on N_2O Emission in Surface Litter and Near-surface Soil of a Managed Pasture: Modelling and Measurements" *by* R. F. Grant et al.

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1 General comments

1.1 Topic and modelling approach

This is an interesting paper about the difficult issue of explaining spatio-temporal variation in N2O-emission from soils. The authors mostly focus on temporal variation, in particular regarding emission-events following fertilization of grassland (a grass-clover mixture). Some attention is given to variation in the vertical direction, between different soil layers. Horizontal heterogeneity is not investigated.

The authors use the model ecosys as their analysis tool. The model is highly complex.

It has a detailed representation of plant physiology, soil composition, microbial populations dynamics, diffusive and mass-flow transport processes, and chemical transformations - and all that with vertical spatial variation represented in the form of multiple canopy and soil layers.

Modelling N2O-flux variability is a notoriously difficult problem, and there are arguably no great success stories yet, so it is good to see another attempt. Some of the modelling results shown here look promising indeed: the model is able to account for some of the differences in N2O emission factors between years and fertilization-events, as observed at the Oensingen site in Switzerland over a period of 8 years. But the use of the highly complex ecosys model is the stand-out feature of this study, and much of the following discussion is about whether or not the complexity was shown to be essential.

1.2 Is the model complexity needed?

Is all the complexity built into the ecosys model needed for simulation of N2O-emissions? The authors claim so. They claim that process-based models for N2O-emission "must" follow several prescripts (lines 77, 79, 95), one of which reads:

"These models must also explicitly represent the effects of mineral N, Ts and theta on the demand for vs. supply of O2 and alternative e- acceptors NO3-, NO2- and N2O, and the oxidation-reduction reactions by which these e- acceptors are reduced. However earlier process models have . . .".

This is a bold claim about the required modelling detail. But such statements on what models "must" look like are subjective and never fully defensible. Even complex models are (over)simplifications and other model structural choices could have been made. Despite the complexity of ecosys, it is still simplified compared to reality (e.g. plant hormones are ignored, as are within-plant variation in N-C ratio and chlorophyll fraction, the soil is assumed to be horizontally homogeneous etc. etc.). So the authors have made their own choice as to what fraction of known system complexity to represent in their model, and others might justifiably make different choices. For example, the modelling results reported here - and real emissions likely too - depend heavily on how fast the sward can regrow after harvesting. Fast plant regrowth requires much nitrogen, thereby leaving little substrate for N2O-emission. But we know that post-harvest regrowth in grass swards suffers delays because of the weak photosynthetic machinery in uncut leaves at the bottom of the sward (a grass field after cutting is more yellow than green). However, this is not represented in ecosys - it assumes constant nitrogen and chlorophyll concentrations. Should we now say that ecosys "must" be changed to incorporate that? Many more such examples could be given.

The fact that the model ignores horizontal spatial variation is a huge simplification given what we know about small-scale spatial variability for N2O-emission. The spatial simplification seems at odds with the enormous detail in process representation as those processes operate in 3D not 1D. Indeed, large variation was observed between chambers regarding their annual total fluxes (Table 3), but this information was not used in the model-data comparison (chamber-data were averaged). The highest measured annual fluxes per chamber were nearly four times larger than the smallest values in several years, with presumably orders of magnitude larger relative differences at shorter time scales. Averaging environmental conditions is justified in a linear model, not in a nonlinear model like ecosys. So, given that the horizontal spatial heterogeneity was averaged away, can there be any justification for using a complex nonlinear model?

1.3 Performance of ecosys

We can also ask: Does the model teach us anything about N2O-fluxes from grasslands that simpler models can't? That question is not directly answered in this paper (because no other models - or model versions - are considered) but the authors do compare their model results to observations.

One strength of the analysis here was that little site-specific calibration was carried out. Parameters were - apparently - kept at generic values, except for site-specific soil properties. It would be good though if more detail were provided about the parameterisation

procedure: which parameters exactly were informed by knowledge of the local system and which were not? An advantage of working with little or no site-specific calibration, was that the data could be considered as independent from the modelling and thus used to assess the quality of the modelling.

Some of the ecosys results are very good, but not all of them. One good result, not even pointed out by the authors, was the fact that ecosys ranked the 8 years of annual fluxes in approximately the correct order (Table 3). The four years with lowest measured annual fluxes (2003-2006) were also the ones with the lowest simulated fluxes, and likewise for the two years with the highest fluxes (2008-2009) even though those two years had low fertilization (Table 3). Given the universal problems with modelling N2O-fluxes, this is a good result. So years are in the right order, but interannual variation was underestimated. The simulated magnitude of interannual flux-variability (standard deviation across the 8 years) was about three times too low compared to the data, but even that is a typical modelling result: models often underestimate ecosystem variability.

Very good results are also mentioned on p. 13, with the model correctly predicting differing emission factors for selected fertilization events (with higher emissions in some events despite lower fertilization levels). That is good, but why was the analysis not carried our for all emission events in the data? Could you expand the analysis to include all events, perhaps using the concept of the event-specific emission factor (see Flechard et al. 2007)? Why not for every observed emission event show the measured and modelled start/end dates, peak flux, cumulative flux? That would give a far more comprehensive picture of the quality of the simulations.

The timing of emission-events seems to be reasonably well predictable using a simple empirical model (Smith & Massheder 2014 Nutr Cycl Agroecosyst 98:309-326): does the ecosys model improve on that?

Not all results are equally impressive. Spring emission events (up to DOY 180) are generally missed by the model (Fig. 2). Why is that?

And peaks of emission events after DOY 180 are often overestimated (Fig. 2), again why? Exceptionally accurate simulations are shown for two emission events that were singled out for closer study in Figs 3 and 4 - it would be better to have a more representative (or comprehensive) choice of events.

It would also be good if at least one more site were included besides Oensingen, to test the modelling capacity for different soils and climates.

1.4 How much of model performance is due to its complexity?

In any case, it is not at all clear to what extent the model results shown are due to the complexity of the model being used. There was vastly more detail in the modelling than in the data, so no testing of underlying simulated processes could be carried out. And perhaps more importantly, no comparison of the performance of ecosys with other models, or simplified versions of ecosys itself, were carried out. All that leaves us unclear about which of the many processes and mechanisms represented in ecosys are essential for predicting N2O-emission rates.

1.5 A complex model as hypothesis generator

Of course, there is another line of reasoning to justify model complexity: complex models may help explore possible mechanistic explanations for observations.

In lines 399-451, the authors give us story lines, derived from the modelling, as to what happened in the soil leading up to various emission events. These are highly interesting and show the value of the model that was being used. However, in the final analysis the key mechanisms seem to be fairly simple. The authors provide a nice summary in lines 449-451: "model findings indicated the importance to N2O emissions of surface and near-surface theta after precipitation, and of plant management (intensity and timing of defoliation in relation to N application) and its effect on subsequent CO2 fixation". Another nice short summary is given in lines 686-697. Given these sim-

ple explanations, could the same results not have been achieved with a much simpler model? One that models these summary mechanisms without superfluous detail? For example, the authors do not mention microbial activity (or population dynamics) anywhere in lines 399-451, so do they need to be modelled at all if the aim is forecasting N2O-emissions?

Moreover, some of the results, e.g. those about the importance of the top soil layer, are already known from the experimental literature as cited by the authors (Neftel et al. 2000; van der Weerden et al. 2013; Pal et al. 2013). Also the model results concerning sensitivity to intensity of foliage removal at harvests (Ruzjerez et al. 1994; Imer et al. 2013). So what does the authors' modelling study add to that? Arguably, the model analysis did help formulate and evaluate hypotheses on the mechanisms underlying the phenomena. And the paper usefully builds on that with their interesting sensitivity analysis showing the possible importance of harvest intensity and harvest timing for emission rates.

1.6 Overall

Overall, this is interesting work, somewhat marred by the enormous complexity of the model without the reader being able to judge the necessity of that complexity. The absence of any uncertainty analysis, and the focus on just a few of the many emission events observed, and the use of data from just one site, also make it hard to evaluate the work.

I look forward to future modelling work from the authors in which they show how much of their model's complexity is essential, and how much can be stripped away without affecting model performance.

2 Specific comments

Some claims about the accuracy of the simulations are difficult to judge, as Figures 2 to 4 are much too small, and the latter two are much too crowded. The first line of the discussion states that "Most N2O emission events measured from 2004 to 2009 were simulated within the range of measurement uncertainty, estimated to be about 30% of mean values (Fig. 2)". Apart from the fact that no basis for the uncertainty level is given, the degree of model-data correspondence cannot be judged from the figures. Perhaps use fewer but bigger graphs and move some to the Supplementary Material where they also need not be so small?

Lines 250-251: "The soil [at the Oensingen field site . . .], key properties of which are given in Table 1". No, those are clearly not measured soil properties but assumed model inputs. It is impossible to measure exactly the same values, to three significant digits, of bulk density, field capacity etc. at five different depths. So how were the soil properties quantified in reality? Were they set to values that gave proper model performance?

The eddy covariance system: can you state the magnitude and location of its footprint relative to the positions of the chambers: do the N2O- and CO2-fluxes refer to the same part of the field?

Line 376: What is that uncertainty assessment of 30% based on?

The last footnote to Table 1 is incorrect. Soil composition (sand, silt, clay) cannot have been recalculated as a function of SOC and CF, because those latter variables varied with depth in the range 0.28-1.50 m, whereas your values for soil composition were constant over that same depth range.

3 Technical corrections

The list of References is not fully alphabetical: see Conant et al. (2005), and also the many Grant et al. references.

The paper is very well-written and I noticed only few minor language errors (lines 40, 108, 295 and the second caption lines of Figs 3 and 4).

There are unit errors on line 518.

Figures 3-4 require a microscope.

The Supplementary Material is very long but unnecessarily so: much of the text and many of the equations are irrelevant for the grassland modelling carried out here. There is information on modelling woody plants ["branches", "twigs", "coarse woody litter"], C4 plants, methane emissions, horizontal heterogeneity i.e. x- and y-variables in equations etc. etc. - none of which is used in the current study. Also, the within-text referencing is often wrong. For example, there is no section called "Energy Exchange" either "above" or anywhere else. Likewise for "Autotrophic Respiration and Growth" etc. etc. Either make - and carefully check - a document that is specific to this text, or refer to a proper general ecosys-document not linked to this paper. It would also be good if the model description began with some basic overall facts: How many state variables does the model have, how many input variables, how many parameters (and how many are deemed universal constants, functional type constants, site-specific parameters). Some diagrams of model structure would also be helpful.

Header of Table 2: replace "2004" with "2001".

Table 3: the NEP-values are redundant (NEP = GPP + Re).

Fig. 4e: "1-3 m" should be "1-3 cm".

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