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Biogeosciences Discussions

Interactive comment on "Decadal variability of soil CO_2 NO, N_2O, and CH_4 fluxes at the Höglwald Forest, Germany" *by* G. J. Luo et al.

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Referee's report on BGD 2011-457 manuscript "Decadal variability of soil CO2 NO, N2O, and CH4 fluxes at the Hoeglwald Forest, Germany" by Guo et al.

General Comments

This paper reports the results of a long-term (15 years) soil CO2, N2O, CH4 and NO flux measurement exercise in a temperate coniferous forest and examines the environmental controls of trace gas fluxes and their seasonal and inter-annual variability. The high resolution, high quality flux data are well presented with a comprehensive discussion of flux variability coupled with empirical modelling based on multiple regressions. The dataset is certainly unique for trace gas diversity and longevity, and well worth publication in BG. One limitation of the multivariate analysis is that, although the authors describe very well the essentially exponential temperature effect for all gases, and the non-linear, bell-shaped control of soil moisture on NO and N2O fluxes, they then go on to develop multiple linear regression models to predict fluxes. I feel that the analysis would have much benefited from non-linear models, which likely would have explained more of the variance, especially for NO and N2O emissions which, being products of nitrification and denitrification, occur preferentially within certain ranges of soil moisture and whose intensity decreases on either side of an optimum. The moisture effect on NO and N2O cannot therefore be expected to be monotonic, neither linear nor exponential. Another limitation of this analysis, the paper only considers meteorological and soil physical state variables (namely, temperature and water) as drivers of fluxes and of their interannual variability, while there is no mention of the potential impact of interannual GPP variability, or of interannual variability of N deposition, which are both also meteorology driven and both have the potential to affect soil processes at interannual scales. The paper should be published after a consideration of the following comments for minor revisions.

Specific comments

p12203, l12: could the authors be a little more specific with respect to the non-linear algorithms they tested? eg Hutchinson & Mosier (1981) model? Non-steady-state diffusive flux model by Livingston et al. (2006) ? Intercept method by Kroon et al. (2008) ?

p12203, l18-23: please provide the dimensions of the chamber

p12204, l16-17: '...Gaps originating from instrumental failure were filled by linear interpolation between measured fluxes for calculation of cumulative annual emissions.' Was there a time limit for the data gap, eg a few hours up to a day, beyond which the linear interpolation method was not applied? A linear interpolation between two flux data points is fully justified if the two point are not too distant in time, but beyond several

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days this method can clearly bias the annual flux, especially for trace gases like N2O and NO, which are extremely dependent on instantaneous rainfall and short-term variations in soil water status. I would have found it much more logical to fill the flux data gaps using the regression algorithms developed in this paper (as is common practice in the CO2/NEE flux community and eg FLUXNET methodology). Why do all this work on environmental control of fluxes, and not apply this to gap-filling?

p12204, l19-20: I doubt that air temperature measured at the weather station, presumably at 2-3m above grass (?), can be considered representative of air temperature in the understorey of the forest.

p12205, l17-21: '...To develop and assess empirical models, weekly aggregated data, monthly aggregated data and data aggregated within comparatively larger temporal resolutions...': Why not establish empirical regression models on the basis of hourly or daily fluxes? I understand that one might wish to use easily measured state variables like T and SWC as predictors of trace gas fluxes, but surely the hourly and day-to-day dynamics are essential to capture the overall flux variability? Besides, I doubt that the straightforward application of the algorithms developed here would have much predicting power at other sites than Hoeglwald; at the very least that would have to be scaled for N deposition (and how?), and to account for soil type, structure, pH, C/N ratio, dominant tree species, etc. For the purposes of the present paper, which was to explain the observed variance in fluxes at seasonal and inter-annual time scales, the finer-resolution hourly data would provide a wider spread of flux values to stretch the range of the regression. I would suggest, if possible, to add to Table 3 the regression results using hourly and daily flux data; it may well be that the R2 values of the regressions are higher at weekly or monthly time scales than daily or hourly, but if that is the case then it should be demonstrated and used to justify the use of weekly/monthly fluxes.

p12205, l27-28: '...In addition, the under- or overestimation of the mean flux was compared to the measured mean flux...': this is a valid test only if the measured flux data capture during the comparison period is close to 100%; i.e. there are no flux data gaps during which environmental conditions and driver may lead to very different fluxes, which are represented in the model but absent in the measurements.

p12206, l1-3: How did the authors deal with bi-directional fluxes like N2O and CH4? Were the negative (uptake) N2O fluxes removed prior to regression, since the log value of a negative number can't be calculated? For CH4, presumably, the sign of (negative) uptake fluxes was changed to positive before log-transformation, but what happened to the few emission fluxes visible in Fig.2 ?

p12206, l13-14: Why show the comparison for 2000 to 2003 only? Soil moisture was unavailable in 1996-99 and in 2004-06 only. Please show all years 1994-2010 in Fig. 1.

p12206, l17: what is the unit of the RMSE? %?

p 12207, l4: Does the mean measured value of 932 mm include snow? If so, what is contribution of snow to total precipitation?

p12207, Section 3.3: the term 'aggregated' conjures up the notion of temporal integration. I would favour the use of 'averaged' instead of aggregated throughout this section and in Fig.2 as well as Table 3.

p12207, l28 to p12208, l3: For CO2, Fig.3 does not show any significantly increased flux in February. The difference is really marginal. There may have been large differences during individual years with pronounced freeze-thaw cycles, but the difference is averaged out when taking the 15-yr mean (unlike N2O, which is clearly visible in red on the figure).

p12209, l2-3: there is much too much scatter in Fig.6 for CH4 to argue that CH4 uptake is best described by a quadratic fit to soil temperature, and similarly for soil moisture on Fig. 5.

p12209, l6-12: I agree that the relationship of NO flux to temperature is, as one would expect, exponential, and that the impact of moisture is bell-shaped (Lorentzian ? in Fig.

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5). However, I wonder why NO emission is clearly highest at SWC=22% (and much reduced at 30%) in Fig. 7 , while it is clearly highest at SWC=28-30% in Fig. 5. Could there be a problem with the calculation of the contour plot? Indeed I wonder that flux observations for NO and for the other gases only cover the soil temperature range -2 to + 15°C only, while the contour plots for CH4, NO and N2O extrapolate up to 22°C. Are there flux measurements at soil temperatures higher than 15°C, which are not shown in Fig.5, but which were used to calculate the contour plots of Figure 7 (except for CO2, which stops at 15°C) ?

p12209, l13-22 and Fig.6: the temperature effect on N2O for non-freezing conditions is likely exponential, and should be shown as such on Fig.6, in which the quadratic fit is non-sensical and misleading, being driven by freeze-thaw events on the left-hand side. I suggest redrawing an exponential fit, discarding soil temperatures below say 1°C.

p 12210, Section 3.5: I wonder why, having shown clearly on Figs. 5,6 and 7 that the controls of temperature, but especially moisture, on gas fluxes are highly non-linear, the authors develop '...multiple linear regression analyses for predicting soil-atmosphere fluxes...'. The log-transformation of fluxes prior to regression does justify the treatment of temperature as first-order when fitting the log values (being equivalent to an exponential fit overall), but for nitrification/denitrification products like NO and N2O, bell-shaped functions could have been built into the equation. This was done in Fig. 5, but then abandoned later on for the models; why?

p 12212, l13: suggest change 'well within' to 'at the lower end of'. This value of 7.91 tC/ha/yr was calculated from the means of available measured flux data in years when the number of days with data was >292 (Table 2). It would be good to compare these estimates with values derived from gap-filled time series, assuming that input data (soil T, SWC) needed to run the regression models were available during periods with no flux data.

p 12214, l1-15: The hypothesis that long periods of winter freezing primes soil organic

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matter mineralisation and boosts annual soil respiration is an interesting and important one. However, the observation that the highest annual CO2 emission occurred during the year with the coldest mean temperature (1996 with 5.7°C) is not evidence enough to support this claim. For N2O the argument was much more convincing (although the processes involved are different), with cumulative winter N2O emission clearly linked to the duration of freezing, and many (14) contrasting measurement years, whereas CO2 measurements were available during 6 years only, and there may be confounding factors. Soil respiration has been shown to be strongly linked to gross primary productivity across sites (eg Migliavaca et al, 2010, GCB 17, 390-409), and interannual variations in GPP at one site may also result in different rates of soil heterotrophic respiration the following year. The high 1996 CO2 flux might have resulted from a high litter fall in autumn 1995, for example. The paper here would benefit from a discussion of other environmental controls of soil respiration, including interannual variations of fluxes of nutrients into the soil, such as in GPP (photosynthesis), and N deposition which can also vary by eg 30% from year to year, in relation to meteorology.

Further, what is the proposed uncertainty in the annual flux estimates? Can the 1996 CO2 flux be said to be significantly larger than the interannual mean?

p12220, l1-3: '...Our failure to demonstrate such a relationship between soil moisture and CH4 uptake rates is likely a result of the weekly and monthly aggregation of measurement data...'. I can only repeat my comment made above, about the opportunity of presenting regression results based on hourly and daily data, as opposed to and in complement of, weekly and monthly data as currently presented in the manuscript.

p12223, l26-27: "...This rather low predictive power showed that simple regression models using measured soil environmental parameters hardly work to simulate soil N2O fluxes." I certainly agree that predicting N2O emissions takes more than a bivariate model; however, as suggested above, and considering the bell-shaped response (Fig. 5) to soil moisture at this site (and similar pattern for NO), a non-monotonic approach (eg Lorentzian) would probably have yielded a better predictive power.

Technical corrections

Abstract, p12198, l14: delete 'high'

p12199, l6: strictly speaking, NO is not a greehouse gas; please rephrase.

p12200, p14: insert '...other than CO2...' after '...trace gas fluxes...'. There is a lot of literature on long-term datasets for CO2 emissions from soils. This paper is not unique in this respect.

p12200, l28: change 'even' to 'much'

Table 2, footnote: there would be no need for this footnote if fluxes had been gap-filled (using the regression models developed in the paper) to provide the annual-scale fluxes, instead of relying on the average and using N>292 days as a quality criterion.

Table 3: flux units for NO, CH4 and N2O: change 'h-2' to 'h-1'

Table 3: please provide confidence intervals for the intercept, temperature and moisture coefficients

Figure 1: please show the comparison of measured and modelled soil moisture data for the entire period 1994-2010

Figure 2: Could SWC be shown as water-filled pore space (%)?

Figure 3, caption: remove comma ',' after 'both'.

p12211, l8: change 'columns' to 'lines of Table 3'

p12212, l22: delete 's' at the end of 'suggests'

p12220, l25: delete 's' in 'reduces'

p12221, l24-27: '...Even though magnitudes of both fluxes were different by a factor of 1000, which showed that NO production and emission at our site was closely cou-

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pled to the microbial mineralization of organic matter and subsequent nitrification of mineralized N.' : this sentence is unfinished, please change.

p12223, l14-16: '...This result was most likely closely linked to the observation that at our site (Butterbach-Bahl and Papen, 2002), but also at other temperate forest sites (Smith et al., 2000).' : this sentenced is unfinished, please change.

p12225, l2: change 'in average' to 'on average'

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