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Discussions

Interactive comment on “CO_2 flux determination by closed-chamber methods can be seriously biased by inappropriate application of linear regression” *by* L. Kutzbach et al.

L. Kutzbach et al.

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Thank you very much for the careful review, constructive critique, the hints to weak points and open questions in our study, and the many very useful suggestions to improve the manuscript. Sorry for not using the Interactive Discussion tool for more intense and quicker exchange of ideas. The substantial delay of this response is due to many unexpected organisational problems during our field campaigns in Russia this summer and unexpectedly long field stays of our first author Lars Kutzbach. In the following, we will answer all comments of the reviewer #3. First, we will repeat the comment of the referee and then we will give the respective answers. We will submit a considerably revised manuscript in which much of the reviewers’ comments will be reflected.

Anonymous Referee #3

General comments:

“This manuscript deals with saturation of CO_2 concentration within the chamber headspace, pressure anomalies, leakage and their effect on the measured CO_2 flux. These problems seem to be still partly unsolved in closed chambers despite of advances in the measurement technology e.g. pressure vents and scrubbing of CO_2. The authors of the manuscript are evidently familiar with these problems based on their literature review presented in the introduction. The new calculation method presented in this manuscript adds a new tool for flux calculation in the closed chambers and therefore, the subject is relevant for publication. However, at its present form, the manuscript was quite tedious to read, especially pages 10 and 11. The modeling approach used should be presented more clearly, bearing in mind the readers who are not familiar with the statistical methods used (e.g. Taylor’s power expansion).”

–>Answer: We will remove the whole part about the Taylor power series expansion, i.e. the last part of chapter 2: from p. 2290, line 16 to p. 2291, line 18. We applied the Taylor power series expansion because this enables the estimation of the initial slope of the c(t) curve with less uncertainty compared to the application of the original exponential. More explanation why the Taylor power series was used is given above in the answers to reviewer #1. As the uncertainty estimates of the parameters is not the focus of this paper, we retract the mathematical formulation of the Taylor power series as it appears to confuse the reader. The parameters themselves, in particular the initial slope of the exponential curve are virtually identical (difference below 0.05%) for the exponential and the power series expanded to 17th order. Thus, the results of this study will not change. Removing the Taylor power series part, will help to shorten the manuscript somewhat.

“Moreover, the experimental methods should be presented in more detail. The chamber description is insufficient and more information should be presented from the study

BGD

4, S1718–S1727, 2007

Interactive Comment

site e.g. vegetation, porosity, and water table if available. All of these have an effect on the leakage term and now it is difficult to explain the differences between the study sites without further information."

–>Answer: Some more details on the experimental set-up will be given in Table 1 and Chapter 5: Chamber and collar dimensions are already given in Table 1. We will give more information in Chapter 5 on the methodology: All experiments were performed manually, robust boardwalks with vertical poles and chambers at least 1.5-2 m distance from the poles at Salmisuo and Vaisjeäggi and 0.5-1 m at Samoylov. Vegetation, porosity, and water table vary substantially within the respective ecosystems. To present and discuss all this site-specific data and would go beyond the scope of this paper. As stated in response to reviewer #1, we plan to prepare a second paper where we divide the different situations regarding day or night conditions, soil and vegetation characteristics, soil hydrology, micrometeorological conditions and different chamber set-ups. For this paper, we would like to stick to a more general overview of the appropriateness of linear and nonlinear models looking at the large database as a whole.

Specific comments:

"I recommend that the authors add a list of abbreviations. This would help the readers to understand the numerous equations. Now they are rather difficult to follow, because of large amount of symbols."

–>Answer: We will provide such a list and add it as an Appendix to the manuscript.

"I do not fully agree with the approach used in this study. I am not sure if the contributions of different flux components can really be separated with a complicated regression model with large number of parameters. Or maybe that is not the actual aim of the study. More likely the aim is to find a function which best describes the changes in CO2 concentration in the chamber headspace. The CO2 concentration in the chamber is a result of several processes (CO2 flux from the soil, respiration of plants, photosynthesis and leakage below the collar) as the authors state. These are dynamic processes and

BGD

4, S1718–S1727, 2007

Interactive Comment

should be analyzed with a dynamic process model rather than with a statistical model."

–>Answer: We agree with you. All the involved soil and vegetation processes are dynamic by nature and should ideally be analyzed with a dynamic process model rather than with a statistical model. However, such a dynamic process model would also have to incorporate many statistical regression components because many of the important parameters cannot be measured but must be estimated by "fitting" the dynamic process model to the data (inverse modelling). At the moment it seems unrealistic that all chamber experiments of the past and the future can be analysed by such an advanced dynamic process model. We try to evaluate which form of nonlinearity could be expected using our conceptual modelling approach – which is significantly simplified to include soil and plant processes. From our admittedly strongly simplified modelling exercise, we conclude that the nonlinearity should be exponential or near-exponential. We are aware of the simplifications and that an exponential function would probably still underestimate the predeployment flux as was shown for the soil-atmosphere system by Livingston et al. (2005, 2006).

"As I understand it now, the authors have parameterized these independent processes in a single regression equation (Eq 11), and the CO2 concentration in the chamber is solved in Eq. 13. Then the measured concentrations are fitted against parameters p1, p2 and p3 in exponential function (Eq. 14). Each of the parameters p1, p2 and p3 consists of numerous parameters, which are partly measured and partly estimated. Am I right? However, to me it is unclear which parameters are estimated or fitted and which are based on actual field measurements. For example, how did you determine the mean diffusivity of leaks (Dchamber) and mean diffusivity of soil (Dsoil)?. Or are they just fitted parameters? In general, the determination of parameters is very poorly presented. It remains unclear, which are measured and which are fitted parameters."

–>Answer: All parameters are just fitted parameters. Actually, we are really more interested on the general form of the c(t) curve than on the single parameters. We developed the whole model exercise to show that a nonlinear function has to be ex-

BGD

4, S1718–S1727, 2007

Interactive Comment

pected and that the form of this function can be expected to be exponential or near-exponential. The aim of this study is not to determine and discuss the parameters of the biophysical model, but we are here more interested in the estimation of the pre-deployment flux which we assume to be best estimated by the initial slope of the c(t) curve. And it can be shown that the estimates of the predeployment fluxes by linear and nonlinear regression partly strongly deviate from each other.

"On page 10, the authors state that "the parameters p1, p2 and p3 can not be interpreted physiologically and physically since they represent a mathematical combination of several physiological and physical parameters of the investigated soil vegetation system and the applied closed-chamber technique". I definitely agree to this and the statement that the initial slope of the curve should be used if the concentration in the chamber is changing exponentially. But still I am not quite sure if all these processes can really be reduced into a single regression equation. At least, the interpretation of such complex regression models is rather difficult."

–>Answer: see above. For this study, we do not aim to interpret the different parts of the model formula. We want to compare linear and nonlinear regression and had to develop some physically-based theory to determine which nonlinear function is reasonable to use. The simplified approach of our model exercise suggests that the c(t) curve should be reasonably fitted by an exponential function. The results of the residual analyses show that indeed an exponential curve is often much better suited to fit the observed data than the linear line even if chamber closure times are short. A quadratic function is not inferior or superior when comparing the residual statistics. However, a quadratic function would have no physical background at all, and yields partly different estimates for the initial slope of the c(t) curve, particularly when fluxes are large.

"When measuring photosynthesis the decrease in CO_2 concentration is not always as regular as in Figure 2. For example, the cloudiness may change the irradiance during the measurements, which affects the shape of the curvature. Therefore, the exponential model may not always be the best. The authors have taken this into consideration in equations 15-19. However, it was unclear to me how the authors define irradiation-limited and non-irradiation limited situation. Do they have some threshold value for irradiation or what? This should be expressed more clearly. Fp(t) is highly dependent on irradiation, more than on CO_2 concentration, and I am not sure if this kind of simplification as presented in Eq. 4 is feasible. At least it should be explained in more detail."

–>Answer: Dealing with irradiation-limited and non-irradiation-limited situations was done because both situations exist. However, it is shown that during both situations the form of the c(t) curve can be expected to be exponential. Furthermore, we retracted our statement that changes of irradiation, temperature or humidity are less critical when applying nonlinear models compared to the linear approach in response to reviewers #1 and #2.

"The Taylor power series expansion (Eq.27) is also confusing. I think that the message in this manuscript becomes masked by the complicated presentation. I admit that the statistical methods used in this study are not familiar to me, and this is partly the reason for my confusion. Anyway, I think that the main message here is that the concentration change in time should be extrapolated to the beginning of the chamber deployment instead of using a linear regression over the whole deployment period. This is feasible, but now the reader gets confused with the rather complicated equations."

–>Answer: Yes, this is the main message. We will remove all parts of the manuscript dealing with Taylor power series expansion. More explanation at the beginning of the answers to this reviewer #3 and also in the answers to reviewer #1.

"Most of the problems presented in this study (the saturation of CO_2 concentration in the chamber headspace) originate from the chamber design. The saturation of the CO_2 concentration during respiration measurements could be partly avoided by designing the chamber dimensions so that the volume to surface area -ratio of the chamber would be as large as possible. The chamber dimensions should be presented in table 1 in

BGD

4, S1718–S1727, 2007

Interactive Comment

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order to allow for the reader the evaluation between the chambers used in different study sites. The installation depth of the collar and the soil porosity beneath the collar affects the leakage from the chamber and therefore they should also be presented."

–>Answer: The chamber dimensions and collar insertion depths are already added to Table 1. We do not agree with you that the nonlinearity problems in chambers can be easily circumvented by increasing the height of the chamber. This would have the same effect as shortening the chamber deployment time, which we here show to not always have the desired effect. In contrast, we agree with reviewer #1, who asked us to recommend low chamber heights and long chamber deployment times to emphasise nonlinearity which would allow for more precise estimation of the nonlinear parameters and thus also of the initial slope of the c(t) curve. As reviewer #1 states, "evidence is rapidly growing" that linear regression is not appropriate for closed chambers at all. Besides this study, more evidence against the reasonable use of linear regression is given by Hutchinson et al. (2000), Livingston et al. (2005, 2006), Nakano et al. (2004), Wagner et al., 1992, and others.

"Because these studies were conducted on a peatland where the water table level is high I, suspect that the leakage was not a major problem, in particular, if collars were used. The saturation in the CO2 concentration in the chamber during respiration measurements was probably mostly resulted from the decrease in concentration gradient between the chamber headspace and soil."

–>Answer: We also believe that leakage was not causing nonlinearity at least at the Salmisuo, Vaisjeäggi and Samoylov sites as here relatively deep collars were installed and water level was high. On the other hand, this is not so clear for Linnasuo where water level was deeper and no collars could be used due to the ongoing peat extraction activity.

"Most of the problems involved in saturation of the CO2 concentration when measuring respiration could be avoided by designing the chamber so, that the surface area is small compared to the volume when the fluxes being measured are high. However, this decreases the sensitivity of the chamber when measuring very small fluxes. I think that the chamber dimensions should also be discussed in the practical recommendations for closed chamber measurements. This would help the readers to reduce the saturation problem."

–>Answer: See above, we would rather recommend the opposite of what you would think to be optimal, at least for soil-atmosphere situations without vegetation. For situations with vegetation, nonlinearity should also be forced as promoted by reviewer #1 but the advantages of this approach must be carefully balanced with the risk of unpredictable plant responses due to strongly lowered CO2 concentrations or artificially high water vapour contents in the chamber headspace. Here, the best compromise must be found for each situation.

"From my own experience with closed chamber measurements, the first 0-30 seconds period after chamber deployment is usually disturbed by the pressure pulse resulted from the chamber and the fan, and therefore it should be left out of the flux calculation. Of course, this depends on the chamber dimensions and how quickly the concentration starts to saturate inside the chamber."

–>Answer: We see increased noise levels at the experiment start more often and to a stronger degree in the data from Linnansuo and Samoylov and less often and to a lesser degree in the Salmisuo and Vaisjeäggi datasets. For the Linnansuo dataset, the first 3 measurement points (30 s) were discarded. For Samoylov, the first concentration measurement point was discarded (45 s). For Salmisuo, the first 10 measurement points were discarded (10 s). For Vaisjeäggi, no measurement points were discarded. We will include this important information into Table 1. However, we think that the need for discarding data at the beginning of closed chamber experiments is a major problem. The reason for it should be found and accounted for. Ideally, the regression should start at the real start of chamber closure time and not later as the slope of the c(t) curve is expected to be greatest both by the exponential and the FDME model proposed by

Livingston et al. (2006).

"Linear regression is not always useless and exponential curve may in some cases even increase noise in the flux measurements, because it may either overestimate or underestimate the initial slope of the concentration change in the chamber. Did you test if the noise in the flux values (Fnet(t0)) was increased as a result of using the exponential or quadratic models compared to the linear model? At least by looking at the standard error bars in figure 6, the measurement noise with exponential model seems to be higher than with the linear model. This "measurement noise" may reduce the accuracy when modeling the environmental responses of photosynthesis and respiration (e.g. light and temperature response)."

–>Answer: Here, the question arises how to measure the noise of the flux estimates? The variability is indeed higher for the flux estimates based on nonlinear regression. However, the fluxes estimated by linear regression have to be expected to reduce natural variability due to their underestimation bias. The error bars in Fig. 6 do not indicate noise but the uncertainty (standard error) of the parameter. This uncertainty is inherently higher for a three-parameter model like the exponential than for a two-parameter model as the linear model because the dependency of parameters is higher for the exponential one given our observed data. However, it must be stressed that the standard error of parameters cannot be taken as an indicator of model validity. We agree with reviewer #1 that the use of linear regression should not be recommend as it generally underestimates the initial slopes of the c(t) curves and thus the predeployment fluxes. If the assumption holds that the initial slope of the c(t) curve is the best estimator of the predeployment flux, then nonlinear regression can only improve the flux estimation compared to linear regression. However, nonlinear regression can also be strongly biased if the mentioned assumption does not hold due to the problems of changing turbulence or pressure disturbances. Considering the rather many experimental c(t) curves which showed curvatures that were not consistent with the exponential model, we see the urgent need for more research on the impact of turbulence and pressure

BGD

4, S1718–S1727, 2007

Interactive Comment

disturbances on the CO_2 fluxes below closed chambers.

“The comparison between exponential, quadratic and linear models starting on page 12 is feasible and the questions a, be and c on page 13 should be emphasized also in the aims of the study. The list of practical recommendations in the end of discussion chapter is good, but it overlaps with the conclusions. The manuscript is too long and requires some condensation. Maybe it would be good idea to combine the practical recommendations and conclusions.”

–> Answer: We will move questions a-c to the end of chapter 1. As we see it, the list of conclusions differs significantly from the list of practical recommendations. Thus, we find it difficult to combine both lists. We suggest condensing the manuscript by removing the part about Taylor power series expansion (page 2290, line 16 to page 2291, line 19) Furthermore we propose to remove the detailed description of the different measures of goodness of fit (page 2295, line 6 to page 2296, line 11.

Interactive comment on Biogeosciences Discuss., 4, 2279, 2007.