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Interactive
Comment

# *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.

**Anonymous Referee #3**

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General comments:

This manuscript deals with saturation of CO2 concentration within the chamber headspace, pressure anomalies, leakage and their effect on the measured CO2 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 CO2. 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). Moreover, the experimental methods should be presented in more detail. The chamber description is insufficient and more information should be presented from the study 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.

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.

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



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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.

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.

When measuring photosynthesis the decrease in CO2 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 CO2 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.

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.



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Most of the problems presented in this study (the saturation of CO2 concentration in the chamber headspace) originate from the chamber design. The saturation of the CO2 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 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. 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.

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.

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. 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))



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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).

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.

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