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Biogeosciences Discussions
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Interactive comment on "Measurements of CO_2 exchange with an automated chamber system throughout the year: challenges in measuring nighttime respiration on porous peat soil" *by* M. Koskinen et al.

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We wish to heartily thank the referees for their positive comments and their valuable contributions to our manuscript. We feel priviledged to have had their effort and time used to improve our manuscript. We agree with most of their comments and will alter the manuscript accordingly. We also contest several points and justify each countered comment in the following replies.

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Anonymous referee #1

General comments

1. C5203 paragraph 3 We will add detailed information on the tree stand and the EC measurements to the final version of the paper.

2. C5203 p 4 We compared calculations with H2O correction and without it, and the differences were minuscule even during summer daytime when respiration is the highest. We will mention this in the final version.

3. C5203-C5204 p 5 We started releasing the sample air into the measurement cabin after noticing that the return air tubes froze in late 2011. After this, we adopted the practise as standard procedure for preparing the system for winter. As we also detached the return air tubes from the chambers at the same time, no under pressure condition was induced. We will clarify this in the text.

4. C5204 p 1 We recognize that the shallow insertion of the collar into the surface moss poses a risk of wind disturbance. However, as we wanted the vegetation on the measurement plots to remain undisturbed by the chamber and inserting a collar deeper than this would necessarily cut the roots of shrubs present on the plot, we had no alternative.

Specific comments

1. Page 14197, lines 2-3: CH4 and N2O fluxes are more straightforward than CO2 exchange in that as the microbes producing and consuming them are heterotrophic, they are not affected by light conditions in the same way as CO2 exchange is. However, as the sentence does not add greatly to the informational value of the manuscript, we will remove it as suggested.

2. Page 14197, lines 7-8: This was not the interpretation we intended. We will rephrase the sentence.

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3. Page 14201, lines 13-19: We will describe the research period on page 14201 as suggested

4. Page 14202, line 11: We will add measurements of the fan-induced wind speed at different voltages to the final version.

5. Page 14202, lines 12-14: Thank you.

6. Page 14203, lines 26-29: The delay was assessed visually from dozens of measurements. Around 30 seconds was the time it took for the concentration change to become somewhat clear in the data. The air tubes are rather narrow so the air flow can be assumed to be mostly laminar which discourages air mixing in the tubes.

7. Page 14204, line 15: This was indeed done to lessen the instrument noise. As all the referees have commented on this, we have now changed the calculation so that no averaging of CO2 values takes place.

8. Page 14204, line 23: g CO2 m-2 h-1 is, while not an SI unit, a unit used in many papers concerning ecosystem or forest floor CO2 flux. It is also easier to grasp the general level of flux as grams per square meter than mol units. Therefore we see no reason to change the unit.

9. Page 14204, lines 23-26: We will take samples of the snow in the coming winters as suggested. We will also add an analytical assessment of the possible error in the winter measurements due to the snow pack contributing to the effective chamber height.

10. Page 14205, line 5: An "e" will be added to the "th".

11. Page 14205, lines 12-13: This exclusion was done in addition to the 30s removed in preprocessing. It will be clarified in the final version.

12. Page 14206, lines 20-22: We acknowledge this as a problem and have changed the filtering criteria accordingly. The zero and negative fluxes during wintertime proved upon closer inspection to be due to the return air tubes being frozen and they could

be filtered by monitoring the Licor cell pressure: When the return air tube of a chamber was blocked, the pressure was higher during measurement of that chamber than between measurements when the air source was ambient air. Under normal operation, the pressure was always lower during measurement due to the air tube conveying ambient air being significantly shorter than the air intake tubes from the chambers.

13. Page 14207, lines 9-11: Correct, we will make it more obvious in the final version.

14. Page 14208, lines 12-16: The air temperatures between ambient and chamber were compared and no significant differences were found between measurements. During daytime measurements the air temperature rises were generally under 2.5 degC and no accumulation of fog was observed visually or by the PAR sensors. Please see accompanying figure 1.

15. Page 14214, lines 18-28: This manuscript concerns respiration data and the difficulties and challanges posed by low-turbulence conditions during certain summer nights. Net exchange will be the subject of another manuscript and the calculation method for assessing the PAR response of the photosynthesis will be different. We will add a sentence to the text regarding this.

16. Page 14215, lines 6-7: See respose to specific comment 1. Of particular importance is that we refer here to CO2 respiration, not net exchange or carbon dioxide flux, which have a whole different set of dynamics. We will clarify this in the final version.

17. Page 14215, lines 9-13: See response to specific comment 4 above.

18. Page 14215, lines 14-16: This is apparently not clearly stated in the text, and we will clarify it in the final version. There was a space of an hour and a half between the high- and low-fan speed measurements, which is ample time for the CO2 gradient to build up again.

19. Page 14224, Table 1: Carelessness on our part. We will change the number of significant digits in the final version.

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20. Page 14228, Figure 3: The "dC/dt" values are actually dCO2/dt values. The text in the figure will be corrected.

21. Page 14234, Figure 9: The meaning of "high" and "low" will be clarified in the article body.

22. Page 14235, Figure 10: The u* was measured at Lettosuo. 30-90s. fits were used to achieve comparable results between 2011 and 2012. During summer 2011 the chamber closure time was only 180 seconds and therefore 120-240s. fit from that period was not available. We will replace figures 8 and 10 with a table containing parameter values of Lloyd-Taylor respiration models with and additional u*-parameter. The table will more clearly convey the message of these two figures.

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Anonymous referee #2

Specific comments

1.(C5719 p1) This is a good point and we will clarify which part of the ecosystem was measured in the current study.

2.(C5719 p2 - C5720 p1, Flux calculation) This was done to lessen the instrument noise. We have ceased doing it; please see reply to specific comment 7 of Anonymous referee #1

3.(C5720 p2, Flux filtering) We acknowledge the problem; please see reply to specific comment 12 of Anonymous referee #1

4.(C5720 p3, Respiration modelling 1) We excluded the night-time respiration measurements from the models because they were potentially subject to the CO2 storage problem which causes the respiration to appear higher when temperatures are lower. If the night-time respiration measurements were reliable, we could use only them without resorting to shaded measurement campaigns.

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5.(C5720 p4 - C5271 p1, Respiration modelling 2) We were unable to make the b-parameter (controlling the temperature sensitivity) of the Lloyd-Taylor-model to have a value above zero in many cases if we did not either combine the chambers or use a single model for the whole summer.

6.(C5271 p2, Respiration modelling 3) This manuscript is written from the point of view of methodology for measuring CO2 exchange with automatic chambers and in particular the problems stemming from CO2 storage during still nights. We have not presented methods for calculating net forest floor CO2 exchange, which will have to accommodate the peculiarities of daytime CO2 exchange measurements (eg. changing PAR levels). Therefore yearly estimates are not presented here, only mention that the system in principle produces enough data to make such estimates.

7.(C5271 p2, Effect of fan speed) As this figure, which is intended to be viewed as a pair of figure 10, has caused confusion among more than one of the referees, we will remove them both and replace them with a table of parameter values of Lloyd-Taylor respiration models with and additional u*-parameter. The table will more clearly convey the message of these two figures.

8.(C5271 p3, Sensitivity to u*) Yes, the “real” in-situ fluxes are indeed sensitive to u* (at least if this is deduced from the eddy covariance data). However, with chambers the situation is opposite to EC flux measurements: with low u* we observe huge fluxes because of the chamber itself disturbing the strong gradient and causing an artificial flush of CO2 out from the close-to-surface air and top peat layer. So, actually far from the “in-situ” situation. Moreover, because we want to use the data for parameterizing the temperature response model, we want to have data which is not distorted by a strong physical phenomena, but which represents the biological activity as precisely as possible. Using the flush-out data does definitely not represent in-situ conditions or biological processes, but is caused by the measurement system itself.

9.(C5271 p4 - C5272 p1, Conclusions) See reply to Specific comment 6 above.

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Technical comments

1-5. We will alter the manuscript as suggested

6. (p. 14208 ll.15f.) The sites our chamber systems are installed on are forested peatlands, which can explain the low temperature rises we observed. However, our original wording may have been misleading as we did indeed observe rising temperatures in the chambers during measurement. The temperature rises during high-PAR situations were usually less than 2.5 degC during the 960-second chamber closure, although singular measurements with temperature rises of over 10 degC were also observed (see attached figure 1). These will not pose a problem since for our net CO2 exchange calculations we are probably going to use data from as early as possible during the measurement, but it is not in the interest of this manuscript. We will alter the wording in the manuscript to reflect this.

7-9. We will alter the manuscript as suggested

10 This is true for nighttime fluxes. We do not comment on daytime net exchange measurements in this manuscript. We will stress this in the manuscript.

11-13 We will alter the manuscript as suggested

14 We will alter the manuscript; please see reply to comments 1 and 16 of referee #1

15 This is correct

16-19 We will alter the manuscript as suggested.

20 Fig. 8 The figure will be replaced; please see reply to Specific comment 7 above

21 Fig. 9 The caption in this figure is erroneous: the data in fact does include both night- and daytime measurements. We will correct this in the final revision and modify the figure as suggested.

22 Fig. 10 The figure will be removed as suggested; please see reply to Specific

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comment 7 above.

23 The linear fit is included as a visual aid only in order to highlight the similarity of the temperature responses of the night- and daytime data; we will clarify this in the caption. The P-value does not reflect the goodness of the fit; it is the significance of difference between the night- and daytime fluxes. This is stated in the caption. We will add a similar figure of the Kalevansuo data to this figure.

24 The manual respiration measurements are those used in Badorek et al. (2011) as cited by us in the text body. For clarity, we will add a citation into the figure caption and describe the measurements better in the text body. The parameters for the different Lloyd-Taylor-based models will be displayed in the table we refer to in our reply to Specific comment 7 above.

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Anonymous referee #3

General comments

1 (C5797 - C5798 p1) We thank the referee for the compliments. On the subject of potential bias and empirical or subjective decisions, we will comment on this later on in our reply.

2 (C5798 p2) We will clarify this as suggested

3 (C5798 p3) We will do as suggested. Figure 1 will be removed and its information will be incorporated into the text. Also figures 8 and 10 will be removed and their information will be converted into a table of model parameter values.

Specific comments and technical corrections

1 (P14196/25) We will do as suggested

2 (P14197/3) We will remove the sentence from the manuscript. Please see reply to

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Specific comment 1 of referee #1.

3-4 (P18197/8 -> P14197/8, P18197/13 -> p14197/13) We will act as suggested

5 (P18197/25ff -> P14197/25ff) This paragraph is important in pointing out the need for high temporal resolution measurements of other gases than CO2 and in further justifying our efforts to build our chamber system

6 (P14198/1) This is true, but they still influence the actual measurement less than the presence of a manual chamber operator. Also manual chamber measurements often require structures such as collars.

7 (P14198/6+7) We will make the sentence more specific or remove it altogether.

8 (P14198/15) We will correct as suggested

9 (P14199/4ff) We will add the suggested references to the manuscript

10 (P14199/26) We will correct as suggested

11 (P14200/10) The systems were installed. We will make the suggested additions

12 (P14200/14) Outcomes of the waterlevel drawdown, as described in the following sentence. We will add a colon after the word "outcome" in the text to connect the sentences.

13 (Table 1) This data is important in describing the differences between the sites and explaining why the night-time problem existed on one site and didn't on another. Therefore we prefer to keep it in the main article.

14 (P14201/13ff) The EC measurements are used for assessing the effect of u* on the observed flux. The mention of previous EC measurements on the Kalevansuo site adds to the importance of our choosing of the site for installing the automated chamber system. It also complements the picture of the whole research setup: that we have two sites at which both the whole-ecosystem NEE has been measured for several

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years, and now we can compare the forest floor fluxes at these two sites. The EC measurements also highlight the differences between the sites: the Kalevansuo site has been a sink for CO2 (Lohila et al. 2011), whereas the Lettosuo site has been a source of CO2 due to the carbon sink of the tree stand being canceled by the carbon source of the soil (unpublished data). We will state this in the manuscript.

15 (2.2 Description of the chamber system) We will add a technical drawing as supplementary material

16 (P14201/27f) We recognize this as a possible problem (please see our reply to General comment 4 of referee #1). The moss was packed over the connection between collar and soil

17 (P14202/27) Yes, but ambient air is also an air source. We will clarify this in the text

18 (P14203/4ff) We will do as suggested

19 (P14203/22) See above

20 (P14204/23-25) This is true, due to instrument limitations

21 (P14204/15ff) We will use unaveraged data for our calculations (see reply to Specific comment 7 of referee #1). We will use the suggested mass flow calculation method in our future studies concerning net CO2 exchange, but as the temperature changes in nighttime or shrouded measurements were negligible, we will not use it in this article (see attached figure 2 for a histogram of temperature changes during shrouded respiration measurements).

22 (P14205/5) The measure was gently placed on the snow surface on several positions on the plot and the average depth down from the collar was estimated. We will clarify this in the text. The level changes were coincided with snowfall and thaw events because those are the times the snow depth actually changes.

23 (P14205/10ff) Our wording here is careless. We should have said that we exam-

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ined the polynomial fit. We chose the polynomial (or quadratic) fit as an example of an alternative method to the linear fit as it has been widely used in respiration studies. Central to our choosing of the linear fit is the fact that our problem with the night-time respiration measurements is not that we could not accurately estimate the concentration change at the time of the chamber closure; the problem is that the concentration change at that time is unrepresentative of any biological process and the exchange of CO2 between soil and atmosphere, which is what we are trying to study. It rather represents the exchange of CO2 between an air layer of a high CO2 concentration and another of a low CO2 concentration when the previous is disturbed by the introduction of the chamber and the turbulence caused by the fan. Using a more accurate fit to the initial concentration change would not help, vice versa it would cause severe overestimation of respiration flux. We will, however, change the examination so that the shorter polynomial fit will be done to 120-240 seconds after chamber closure. We will also test the significance of the differences between the fits with Student's paired t-test

24 (P14205/20ff) We have never observed condensed water on the chamber walls during the measurements under normal operation.

25 (P14206/1) These tests are separate and a shorter fit time period was desirable in this test as it emphasizes the differences in calculated flux rates between time periods and eases choosing the most stable period.

26 (P14206/3ff) Little extra information would have been gained from moving the window at a shorter interval as the number of measurements used for this test was large. The concentration-time gradient was always calculated for the zero second point of each 60-s window. If we calculated the dCO2/dt for the moment when the chamber closed, we would greatly exaggerate the flux rate during still nights when the CO2 storage build-up discussed in reply to Specific comment 23 above and in the article occurred. We refer here to problems with certain night-time flux measurements on porous peat soils. The problem is not universal to all chamber measurements on all soils.

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27 (P14206/11) We alter the manuscript as suggested.

28 (PP14206/20ff) We have altered our filtering criteria. Please see our reply to Specific comment 12 of referee #1

29 (Equation 2) We will alter the manuscript as suggested.

30 (P14207/17ff) On effectively drained peatlands the water table level is generally quite low. In our case, it varied between ∼-20 and -60 cm at Lettosuo and ∼-25 and -45 cm at Kalevansuo during summer 2012. In Ojanen et al. (2012, cited by us in the manuscript), WT had no significant effect on the CO2 efflux on drained peatlands, probably because the water level is low enough not to limit the respiration or tree growth.

31 (P14208/15f) The observed temperature rises were generally low. Please see our reply to Specific comment 14 of referee #1 and to Specific comment 24 above.

32 (P14208/27) We will use the heating preemptively in the future as we now recognize the conditions most susceptible to air tube freezing. Please see attached figure 3 for a histogram of temperature changes during winter measurements.

33 (Fig. 2) The loess smoothins acts as a visual aid in the graph. It gives a general idea of the functionality of our system over time at a glance better than the individual points. We will remove the legend and the x-axis label.

34 (P14209/20f) This makes us happy

35 (Fig. 3) We will alter the manuscript as suggested.

36 It is possible there are also remains of air from the previous measurement in this figure (Fig. 3, right-side panel) due to some irregularity of the air pump or the solenoid valves. We will change the text in the manuscript accordingly.

37 (P14210/23ff) We believe that Fig. 4 is the least space-consuming way of showing why we concluded that the high measured CO2 respiration during summer 2011 on Kalevansuo was a measurement method artifact rather than a realistic picture of the

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underlying processes. We will recognize the possibility of a slightly leaking chamber causing the curvature in the concentration development in the text. We will add time of day to the caption of Fig. 5

38 (P14211/16ff) We will alter the manuscript as suggested

39 (P14212/14+15) We will change the word “made” to “conducted”

40 (P14212/16) Please see our reply to specific comment 26 above.

41 (Fig. 6) We disagree. Figure 6a shows very clearly the general decreasing trend of the changing dCO2/dt during the time since closure. Fig 6b shows the dynamics of the other variable used in the decision of the time range of data used for flux calculation. As the selected period of 120-240 s was a compromise between these two, we think the figure showing dynamics of both variables is essential.

42 (P14212/25+26) In the case of the night-time measurements, the period of least potential bias from saturation (right after the chamber is closed) poses a high potential to be biased due to the physical process of releasing CO2 storage, already explained above, in responses to other referees, and in the manuscript. We must make some sort of compromise between these two biases, and the only reasonable way we could think of was to choose the period of the most stable flux when it could be thought that the two biases could cancel each other out.

43 (P14214/13ff) Indeed.

44 (P14214/24-26) We will alter the manuscript as suggested

45 (Fig. 8) The fan speed differs between the years, which is explained in the text body. However, see our reply to Specific comment 7 of referee #2. We will add measurements of the wind speeds induced by the different fan speeds to the text.

46 P14215/22) We will use RMSE in the final version.

47 (P14216/14) The decision to use the 120-240s. window was made using the data

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collected with 960s chamber closure times. We will clarify this in the text.

48 (P14216/29) We actually refer here to Table 1. We will correct this in the manuscript.

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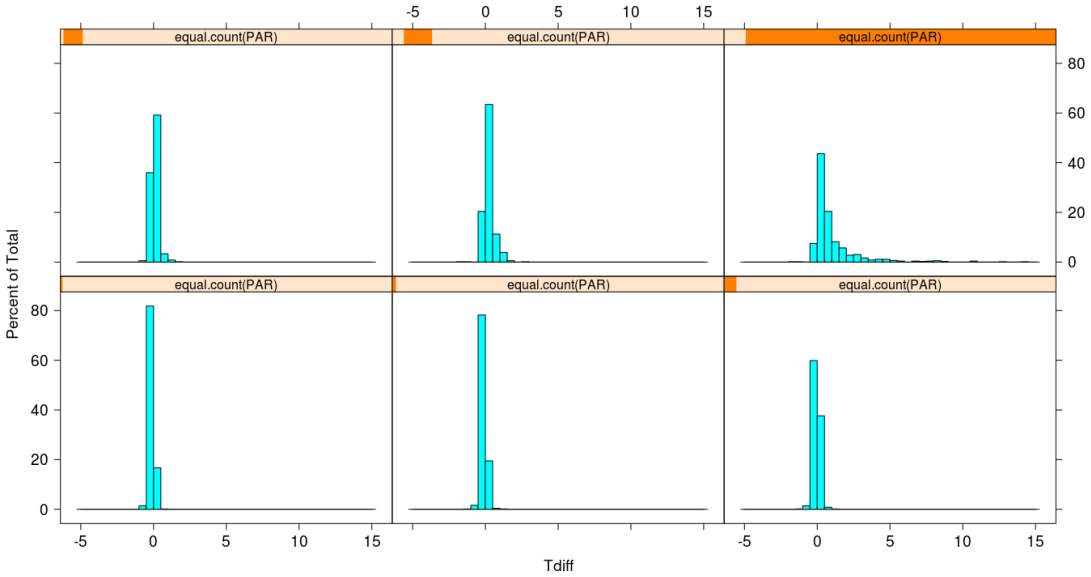

Fig. 1. Temperature changes in 120-950-second measurements in June 2012 at Kalevansuo, classified into equal counts according to PAR values. Lowest PAR values in bottom left panel, highest in top right panel.

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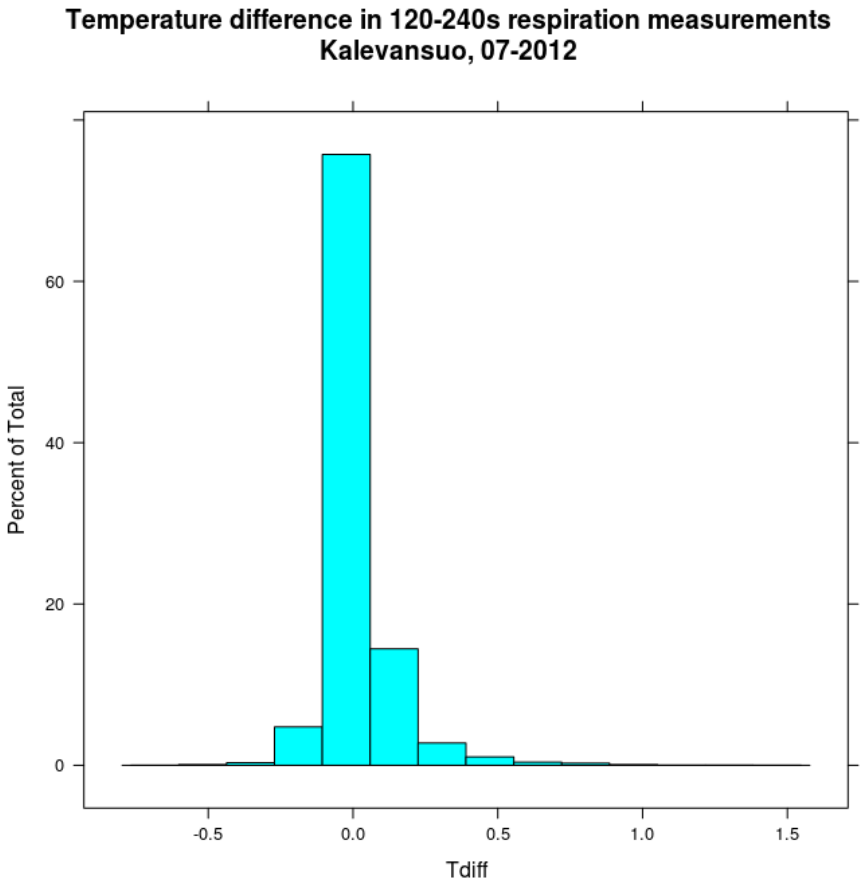

Fig. 2. Temperature changes in 120-240-second measurements in June 2012 at Kalevansuo. Shrouded respiration measurements.

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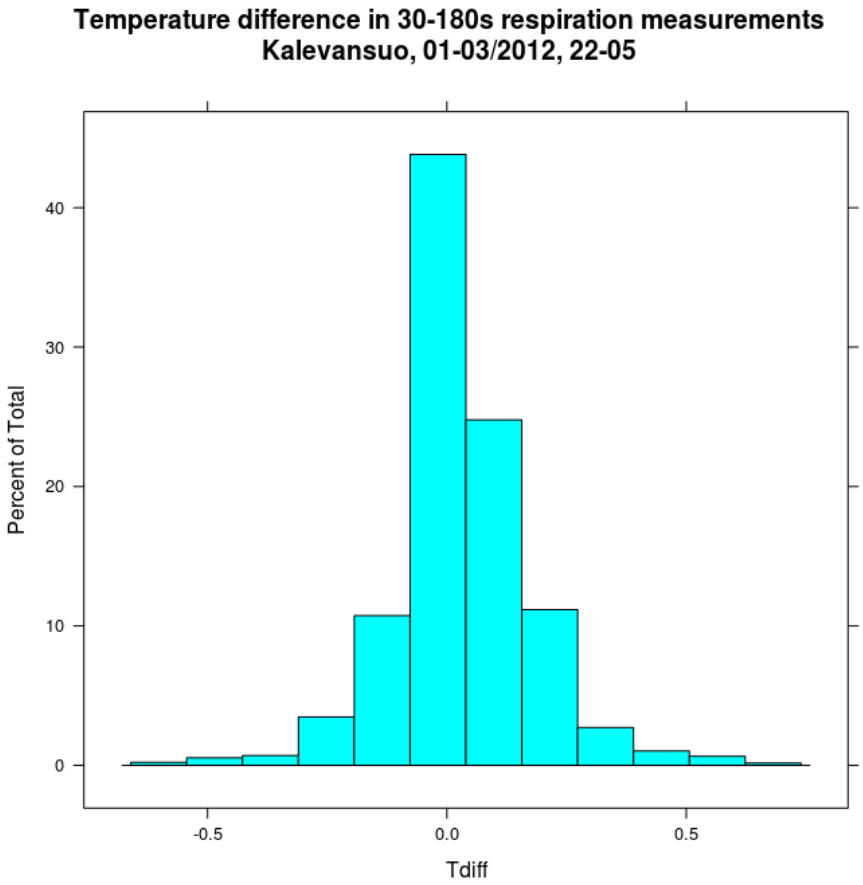

Fig. 3. Temperature changes in 30-180-second measurements in January-March 2012 at Kalevansuo. Night-time (22-05) respiration measurements.

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