Biogeosciences Discuss., 9, C1997–C2002, 2012
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Biogeosciences Discussions



# *Interactive comment on* "The contribution of respiration in tree-stems to the Dole Effect" *by* A. Angert et al.

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Received and published: 19 June 2012

This paper presents a series of carefully done chamber experiments, on to tree trunks, on a topic relevant and important to global carbon cycling and hydrological-biosphere interactions. The authors show that tree stem respiration fractionates atmospheric dioxygen isotopes less than expected from the typical respiratory enzymatic consumption of O2. The reason that the authors propose, familiar in several other contexts, is that diffusion of O2 to the site of consumption is partially limiting the effective, whole-system fractionation that ultimately occurs.

One way to visualize this diffusion effect is that a "back flux" of isotopically enriched O2 must exist in order for a sink process to leave a fractionation signal on the remaining gas in the reservoir. This is easily shown by considering the following thought exper-

iment: an evacuated flask is opened, drawing in air, then sealed. The oxygen in the flask is then totally consumed by respiration in a closed chamber, such that the O2 mole fraction becomes zero in the flask. Then the flask is re-evacuated and the process is repeated. It is obvious that no fractionation effect on the atmosphere can occur in this situation, because there is no "back flux" of isotopically heavy gas to the atmosphere - all of this heavy gas has been destroyed by respiration.

Diffusion-limitation of O2 transport to the consumption site similarly precludes a back flux from the enzymatic reaction site to the atmosphere. This discovery is important because the magnitude of the Dole Effect and its temporal variations could potentially be a powerful tracer of hydrology and biosphere-atmosphere interactions. But its full potential has not yet been realized because we do not understand it fully yet. This paper takes a small but important step toward that ultimate goal, and as such it is quite appropriate for GBC and it should be published with some major revisions, detailed below. The writing is excellent, the presentation very clear, and the organization is fine.

The authors have taken on a challenging technical problem, which is to make a seal on the rough and somewhat porous bark of tree stems. They employ a clever technique using hot glue and box modeling to deal with the unavoidable "leakage" by diffusion from the bark surrounding the chamber. At steady state, they show convincingly that the conductance of this bark-induced pathway drops out of the equation and thus makes the problem tractable, allowing for an estimation of the magnitude of the fractionation from the measured d18O of the O2 in the chamber.

I especially like the clever approach that the authors used to verify that molecular diffusion was the dominant transport pathway, versus viscous flow, between the chamber and the ambient environment: filling the chamber with pure N2 and then monitoring the transient approach to steady state to verify that fractionation indeed did occur as predicted by the box model. (it would be helpful to the reader to show a plot of d18O versus dO2/N2, with both model and the two measured points, to show how well the measured points fit the model prediction.)



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I have several reservations about the chamber approach, impressive as it is. One is that water vapor from the tree stem will likely cause the chamber's relative humidity to rise nearly to 100% at steady state. If the relative humidity of the ambient air is less than 100%, as seems likely in the desert environments in which the experiments were conducted, there will be a steady state flux of water vapor through the bark-induced pathway, out into the environment. This water vapor flux will oppose the dioxygen flux going into the chamber. In this situation there should be a water-vapor-flux-induced fractionation of the oxygen isotopes, which would be roughly equal to the ratio of the binary diffusivities of the two oxygen isotopologues into water vapor, times the gradient in mole fraction of water vapor. For example, if the mole fraction of water vapor is 0.03 inside the chamber, and 0.01 outside, and the ratio of the diffusivities is 1.0108 (light over heavy), then the effect would have a magnitude of (1.0108 - 1)0.02 = -0.22 per mil (Severinghaus et al., 1996 GCA). This is pretty small compared to the results but I think it should nonetheless be considered.

One way to deal with this would be to make an artificial "tree trunk" out of plastic or rock, that has no oxygen uptake, but can supply a steady flux of water vapor (such as a sponge- or quartz wool-lining that is wetted at the start of the experiment and always has some amount of liquid water present through the whole experiment). This "blank" chamber would then be allowed to come to steady state, and sampled, just as in the real chamber experiments on tree trunks. The expectation would be that the value measured in the chamber air would be -0.11 per mil for each 0.01 difference in the vapor mole fraction between inside and outside the chamber. This "blank" experiment would also serve as a check on the assumptions made in the box modeling exercise, and also perhaps reveal any unanticipated artifacts.

A second reservation is that the status of O2 as a non-trace constituent of air may need some careful evaluation. If one simply measures O2/Ar to estimate the loss of O2, one will underestimate the true consumption of O2 due to the fact that O2 is a major component. Perhaps the authors have already considered this, but I couldn't tell



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from the discussion. They may have a fortuitous situation in which water vapor in the chamber replaces, in some sense, the lost O2. If this is in fact the case then they could make a correction to the [O2] concentration term used in equation 3, to account for the water vapor present in the chamber. This would have the effect of lowering the O2 mole fraction that is used in equation 3, hence increasing the total inferred discrimination. In any case it would be useful for them to measure the water vapor mole fraction, or calculate it from a hygrometer measurement, in the chamber to verify that it is indeed at saturation. (I expect it would be but it should be measured since this is a complex system and many things are surprising - for example what is the vapor pressure over a highly concentrated sap solution? Lower due to Raoult's Law?)

Another worry is that if atmospheric pressure changes, there will surely be a viscous component to the exchange between outside and inside. Was barometric pressure continuously monitored during the course of the experiments? Also, if temperature changes, diurnally or otherwise, there will surely be a viscous flow between outside and inside.

The authors should address all these points, and I recommend strongly that they do the "blank" experiment using the "artificial tree trunk", preferably in the same groves of trees where the real experiments are done, at the same times as the real experiments, to capture the actual humidity that the real experiment sees. Then the sampling of chambers can all be done at the same time, both on "treatments" and "blanks", all using the same apparatus. The mean and standard deviation of the blanks should be reported in the paper, because it would also provide a valuable over-all estimate of the total measurement uncertainty (not just the analytical uncertainty). One might think of this as a "process blank".

Detailed remarks are given below.

page 2, line 5 add a space between semicolon and Gillon - also in whole paper, wherever multiple citations are made



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page 2, line 9 "....Effect has been suggested to be..."

page 3, line 1 the statement "...16% of forest annual photosynthesis" is perhaps a bit misleading to the reader, even though it may be correct. The relevant figure, in the context of discussions on the Dole Effect, is the fraction of gross oxygenesis, not annual photosynthesis. Of course it is difficult to estimate gross oxygenesis, but if the turnover time of O2 is taken to be 1000 years, this implies a gross oxygenesis of 37 Pmol per year. What fraction of 37 Pmol per year is the respiratory consumption of O2 by aboveground woody tissues? In carbon-equivalent units, this would be 12 x 37 = 444 Pg C per year.

page 5, line 11 the precision of O2/Ar is surprisingly low (1 per mil). Why? Some discussion would be helpful. If you are measuring isotopes you should be able to get comparable precision on O2/Ar as on isotopes.

page 6, line 5 "..take up or lose only a small amount..."

page 6, line 12 did you mean to say "stem" here instead of "soil"?

page 7, line 17 "...Equation 3, only from chamber experiments..."

page 7, line 20 "...stem chamber experiments.."

page 7, line 24 put an apostrophe after "trees" in "Mari-Mari trees' "(it is the possessive form for the plural)

page 7, line 25 same for Tanagrana trees

page 7, line 31 "...chambers by mass (viscous) flow. Entrance of O2 by mass flow should cause no..."

page 8, line 5 "...assumption of domination by gas-phase..."

page 8, line 14 these observed values, -3.15 per mil and -2.58 per mil, must be shown with their corresponding [O2] values and the numerical model prediction for the tran-



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sient, in order for the reader to judge meaningfully whether they indeed support the conclusion of no significant mass flow. A figure with d18O on vertical axis, and [O2] on horizontal axis, is probably the most efficient way to accomplish this.

page 8, line 27 "...first diffuses in the gas..."

page 8, line 28 "...then diffuses in the liquid.."

page 9, line 9 need a reference here, to back up the assertion that discrimination in liquid phase diffusion is close to zero (maybe the Knox, Quay, and Wilbur paper, JGR 97 20335-20343 (1992)) they get about 2.8 per mil for gas-liquid exchange, which is probably the same for liquid phase diffusion.

page 10, line 9 "This effect is of similar magnitude.."

page 10, line 12 "...diffusion limited and to have..."

page 10, line 27 "...which somewhat resembles the"

page 10, line 29 "..findings are in agreement with..."

Jeff Severinghaus

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