"The contribution of respiration in tree-stems to the Dole Effect"
Angert et al. reply

We thank both reviewers for their comments, and for interest they found in this manuscript. Detailed response to the main comments appears below:

Severinghaus: 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.

Response: We have added consideration of this effect, in both the analytical and numerical model. RH in Jerusalem rarely goes as low as 30%, and the daily average values are usually >60%. In the tropical forest site the values are of course much higher. The model shows, that as the reviewer suggested, this effect do exist, but its implication are small.

Severinghaus: 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.

Response: Such sponge tree will only repeat an experiment already done at Severinghaus et al., 1996 GCA.

Severinghaus: A second reservation is that the status of O_2 as a non-trace constituent of air may need some careful evaluation. If one simply measures O_2/Ar to estimate the loss of O_2, one will underestimate the true consumption of O_2 due to the fact that O_2 is a major component. Perhaps the authors have already considered this, but I couldn't tell

Response: We have now included in the numerical model all the major gases in the chamber (N_2,O_2,Ar, CO_2, H_2O), and have also included mass flow derived by pressure gradients, that can be caused for example if O_2 consumption is not fully compensated by CO_2 increase. This modeling also includes now the effect of water vapor gradients.

We have found that all these effect, another wide range of possible conditions, will shift the discrimination we calculated by the analytical model by less than 0.1 permil.

Also, Indeed Fick's law is an approximation that work only for trace gas. However, we compared the ratio between the diffusion we calculated and the one that will be found by a Dust Gas Model (DGM, Webb, 1998, Eq, 22), and found that while the diffusion of individual gases may be quite different, the difference for gas pairs and especially isotope pair is very small.

Severinghaus: 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?)

Response: This is now dealt by the numerical model as explained above.

Severinghaus: 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?

Response: Yes, we used meteorological stations data to make sure that the variations during the experiments were small.

Severinghaus: Also, if temperature changes, diurnally or otherwise, there will surely be a viscous flow between outside
and inside.

Response: In the tropical site the diurnal cycle is small. However, this can pose a larger problem in Jerusalem. We have added this point to the text.

Severinghaus: 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".

Response: To do this correctly, there should be also oxygen consumption in the artificial trunk, with the same O2/CO2 uptake/release ratios, the "trunk" structure should be close as possible as to real one, there should be water flow in the "xylem", and the water should have the same composition as in real xylem, etc. Given this, it seems that results from such artificial "trunk" will always be questionable.

In addition, if the role of the artificial "trunk" is to study mainly the water vapor effect, then the model shows this effect is very small even for the Jerusalem experiments. For the tropical forest in which the RH is high this effect does not play any role.

Severinghaus: 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.

Response: This is the external precision we got between replicates (samples or outside air). The error includes the MS precision in peak-switching mode, and noise introduced by the perpetration vacuum line and offline GC. This precision is adequate for the needs of the current study.

Severinghaus: 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-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.

Response: This data is taken from previously published paper and described an experiment on one tree. Since we don't have data on diffusivity nor respiration rate, a model could be always fitted (or rather over-fitted) to these data. Hence, this experiment shows that diffusion was a dominate process for gas transfer, but does not prove it was the only one. However, the leak checks we describe in the text, which were preformed for all trees, provide independent evidence that air could enter the chamber only through the stem pores.

Severinghaus: 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.

Response: This sentence just states that stem respiration is an important contribute to the global O2 fluxes. We don't go into O2 modeling there. Anyway, 16% of forest photosynthesis is also ~16% of forest O_2 production (neglecting photorespiration).

Severinghaus: 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.

Response: We added the reference.

Reviewer# 2: …it is based on a rather limited survey, in terms of sites and species, for modifying the global scale discrimination value , D, and the global Dole effect.

Response: We clearly stated that "we observed large variability in discrimination values with season, between species, and within species, so this value cannot be assumed to be globally representative." Moreover, this first of its kind study did not aim to get a representative sampling of the >10^4 tree species, nor do we believe this is possible.

Reviewer# 2: Second, in parts it gives the sense of a Response to the paper of Severinghaus et al 2009 that also tried to improve the global Dole effect estimates. Interestingly, Severinghaus et al. used a previous paper of Angert et al reporting a limited soil survey that argues for modified soil D in the global Dole effect. And now, the authors use another limited survey of stems respiration and suggest that perhaps the earlier correction made by Severinghaus based on the earlier Angert paper... may be incorrect.

Response: We agree with the reviewer that only few groups are working on the Dole Effect. Severinghaus did the best interpretation possible with the data available then. We added now new data, that ice-core researchers, as well as modelers should take into consideration in their next studies.

Reviewer# 2: Methods: Israeli site with no met data as given to the Peruvian site.

Response: Data was added.

Reviewer# 2: Chambers used in Peru need diagram.

Response: We added a reference with a diagram.

Reviewer# 2: Not clear Checking CO2 for leaks No indication of flow through capacity, use of IRGA?

Response: Details were added.

Reviewer# 2: Chamber in Peru was incubated for 10 days. No info for sampling detail in Israel

Response: Info was in the table, now also mentioned in methods.

Reviewer# 2: Model: assumption of little dissolution or degasing does not say much about possible extent 'isotopic exchange' between chamber headspace and liquid flowing through its base.

Response: It is basically the same argument. Very little O2 is in the water compared to the headspace. So the expected effect is small.

Reviewer# 2: The jump from eq. 2 to 3 is not obvious enough and requires investigation of two references. This is not justified and more detail should be given here.

Response: This step is mostly algebraic, and already detailed in these two references. We don't see a room to repeat the derivation of an equation which is already published in few papers.

Reviewer# 2: It would help to spell out D-stem.

Response: Below equation 2 we state "D_{stem} is the overall ^{18}O discrimination of the O_2 uptake by the stem"

Reviewer# 2: Results: More visual presentation of the results, e.g. means by species/range/frequency, will be good (only a table is presented).

Response: We do not see room to that, given the lack of correlation with these parameters.

Reviewer# 2: Mass flow, means advection? The discussion of the test of the assumption of no mass flow into the chamber is not clear. The argument of a negative del18O value in the chamber is a qualitative one and does not indicate that getting values of around -3 are not a net effect of diffusion and mass flow (leaks..). Testing the 1st assumption in the paper is critical for the discrimination estimates and therefore critical to discuss in more convincing way.

Response: We answered that fully to Severinghaus , above.

Reviewer# 2: Eq. 4, 5 should also be combined, it's a linear system and series of steps in line, each with its D...

Response: True, but combining this to one big equation will not add any information, nor it will help to solve it.

Reviewer# 2: This will also highlight the duplication in labeling, such as Ci/Ca used in both equations for different parameters.

Response: Good point. We now updated this to Ci/Ca and C'i/C'a

Reviewer# 2: In this paragraph it is stated that D of 14 permil can only be obtained with liquid diff in the inner box. But this is not accompanied by the detail demonstrating this.

Response: We now added that this is given the D in respiration is >20 permil.

Reviewer# 2: Table 1 gives no error estimate. This is not normally acceptable, and especially critical when assessing the discussion on the AOX aspects.

Response: Error estimates were added.

All grammatical and other minor comments of both reviewers were corrected.