We thank the three reviewers for their interest in this paper, for their encouraging remarks, and for their comments. The revisions we made, and answers for the main comments are indicated below:

Reply to Reviewer #1, Daniel Metcalfe

We have corrected the typos, as suggested.

Reply to Reviewer #2

Based on the reviewer's comments it seems that the main questions this study addressed were not emphasized enough in the submitted version of the manuscript. We have corrected the manuscript so it now to emphasize our main questions:

1) Is internal CO_2 transport important in tropical trees?
2) If internal CO_2 transport is important, will it result in net release of CO_2 derived from root-respiration in the roots at the stem, or will it cause the opposite effect of net removal of stem-respired CO_2?

The data collected are an initial study and do not answer all of the questions raised by reviewer 2 (what is the source and ultimate fate of internally transported CO2?). However, we view this as a first step and our goals were to test whether what has been demonstrated in temperate trees by the group of Teskey et al. and inferred from observations in tropical trees by Cavaleri et al (2006) can also be demonstrated in tropical trees. We developed the ARQ method, which is especially suited for application in remote locations, specifically to test the question of net export versus net import of CO2 in stem. However, this is an inference based on the ARQ, and there are clearly details of exact mechanism for export (eg dissolved in organic versus inorganic form) that remain to be resolved in future studies

The second major comment by Reviewer 2 deals with the variability among individual trees, tree species and between dry and wet seasons. Given the logistical constraints associated with flask sampling for the ARQ measurements, we did not feel we had enough of a sample to address variations with species or season. Our measurements across all of these conditions (different species and during different seasons, outside and inside the stem) indicate that the conclusion of net removal of respired CO_2 from tropical trees stems (i.e. ARQ<1) is robust and may be a common feature for other tropical tree species. We did not want to make broader conclusions based on such a small subsample of tree species in tropical forests. Our work instead should be viewed as a challenge to make more – and more systematically targeted- measurements.

We agree with reviewer #2 that it will be interesting in future studies to combine the ARQ measurement with measurement of sap flow rates, and of the xylem carbon concentrations along the stem. However, in this first, exploratory study, we did not have the resources to do this.

With regard to the initial publication of the ARQ method by Alon and Sherer (2011) it should be noted that the current manuscript adds two new important theoretical considerations:

1) We added a numerical model of the stem chamber, which enables the exploration of important questions that were not addressed by the simple analytical model we initially reported. This led to our measurements of ARQ both in the initial, non-steady-state and in the steady-state

conditions. In our view, the agreement between these two measurements adds considerably to the robustness of the overall method..

2) We showed that O_2-based respiration rates can be calculated from two concentration measurements, if the second measurement is taken when the chamber air is in steady-state. We used O_2 measurement on 5 trees for proof-of-concept of this idea.

Both of these are important advances, given the difficulty of making measurements of small changes in O_2 under field conditions, and should help with design of future work by others. The ARQ method needs to be shown to be robust before it can be widely applied.

Reply to Reviewer #3, Nerea Ubierna Lopez

Reply to point #1-7 of the review:

1. The reviewer brings up an important point that we now address directly in the manuscript Indeed, the supply of O_2 to the inner sapwood by the transpiration stream has been discussed in the literature as an important process to keep cells alive (including the citations pointed out by the reviewer). However, given the low solubility of O_2 this supply (and its rate of consumption) must be small compared to the overall fluxes we observed, and thus can be important only in areas (such as the inner sapwood) where respiration rates are extremely low. Moreover, a considerable supply of dissolved O_2 in the transpiration stream would tend to lower the influx of O_2 (by lowering the gradient from outside to inside the stem). In other words, our observations would then tend to underestimate the influx of O2, and result in ARQ>1, which is inconsistent with our observation.
2. The revised version now includes the requested estimate of possible CO2 transport, as well as O2 transport in the transpiration stream.
3. We now deal with the issue of data collected in between the linear and steady-state portions of the model curve in the text. We conclude that the deviation is well within the analytical uncertainty and the differences observed from one tree to the next and conclude that its effect is within the precision of our estimates.
4. We used atmospheric CO2 and O2 concentrations as ambient. Given the large differences in concentrations found in the chambers and the soil, this approximation will have negligible effects (e.g. an error of the order of 50 ppm in an overall difference of several thousand to tens of thousands of ppm). We now address this directly in the text.
5. Although Table 3 hints at possible differences between tree species and between seasons, we did not really design our study to examine such trends (for example, we would need to know relative sap fluxes to interpret them, and do not have these data). However our data suggest strongly that follow-up studies could explore these questions in more detail.
6. The argument about drought is well taken. We now cite the Teskey et al. review paper on this subject – certainly the upward transport of CO2 may have had a more important role in low-CO2 time periods such as the last glacial. There could also still be a role if VPD is higher, in which case stomata could partially close without slowing evaporation/transpiration losses from leaves..
7. The reviewer argues that the transport of C in the stem does not change the C budget of the whole tree, it merely moves processes. In other words, C fixed in the stem and moved

upwards will get respired (not fixed) higher up in the tree. The recent paper by Bloemen et al. (2012) shows that stem-transported C can be fixed in branches. Also, Cavalero et al showed that tropical tree canopies respire C at much higher rates than the stem. Also, as we wrote:

"While a shift in the height in which CO_2 is emitted from the stem will not affect the entire tree carbon balance, this shift is important for the following reasons: First, measurements of stem CO_2 efflux are normally made at ~1.3 m height and then extrapolated to the surface area of the rest of the tree. The upward transport of CO_2 we report would create a systematic error in such extrapolated stem respiration rates."

All technical comments were corrected.