Biogeosciences Discuss., 10, C7531–C7537, 2013
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Biogeosciences
Discussions
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Interactive comment on "An inverse modeling approach for tree-ring-based climate reconstructions under changing atmospheric CO_2 concentrations" *by* É. Boucher et al.

Anonymous Referee #1

Received and published: 21 December 2013

This is a well-written, scientifically sound, article that attempts to solve a fundamental problem recently identified by paleoclimatologists and ecophysiologists dealing with growth patterns and isotopic composition of tree-rings under changing atmospheric conditions. The authors argue correctly that the standard dendrochronological approach relies upon simple (linear) transfer functions to reconstruct historical climate, which could introduce an unanticipated bias due to a potential CO2-induced stimulation of tree growth. This confounding factor is expected to be particularly problematic in chronologies that extend through and beyond the beginning of the industrial era and has hindered accurate predictions of the effects of changes in climate and atmospheric composition on terrestrial ecosystems, as its reverberations on the ecophysiological

performance of tree species are typically not accounted for. To solve the problem the authors propose a novel approach, tested using data from the Fontainebleau forest (France). They conclude that their inverse modeling method represents a better alternative to the traditional transfer function technique, with advantages such as the ability to distinguish between climatic and CO2 effects on tree growth. While I think this modeling exercise is valuable and agree with most of the proposed methodology, I have a number of concerns regarding the use and interpretation of growth and isotopic data, which in my opinion need to be (at least in part) reevaluated to reflect ongoing discussions in the specialized literature. Below I provide specific comments that when addressed will greatly improve the relevance and impact of the study, as well as its future contributions for the attribution of causality in the analysis of CO2-driven changes in tree growth patterns.

General issues with growth patterns

It is important to note that the present study does not represent the first attempt to address modern biases in dendrochronological analysis. Approaches other than simple transfer functions exist and these should be acknowledge and discussed to emphasize the relevance and unique value of the method proposed here. To mention one example, a likelihood perspective on tree-ring standardization aimed at eliminating sampling bias (which would also encompass CO2 effects) has been recently published Cecile et al. (Climate of the Past 9: 4499-4551, 2013). These authors describe a new standardization technique, using fixed effects that contain both classical regional curve standardization and flat detrending. Their assessment of the traditional approach revealed a significant negative bias in estimated tree growth over time, largely concentrated in the last 300 years of tree-ring growth data, which poses serious questions about the reliability of commonly used standardization techniques. These include the regional standardization used here (e.g., the adaptive regional growth curve technique used to produce the average chronology shown in Fig. 4) and therefore should be addressed.

More importantly, a central assumption of the present manuscript is the existence of a measurable “CO2 fertilization” effect, which supposedly has systematically increased the productivity of trees since the beginning of the industrial revolution, an assumption that has been proven incorrect in recent studies. For example, Gedalof & Berg (Global Biogeochemical Cycles, 24: 1-6, 2010) analyzed the global record of annual radial tree growth derived from the international tree ring data bank, looking for evidence of CO2 fertilization effect. They concluded that only 20 percent of sites globally exhibit increasing trends in growth that cannot be attributed to climatic causes, nitrogen deposition, elevation, or latitude, showing very limited direct evidence of CO2 fertilization of forests over the 20th century. More recently, Silva & Anand (Global Ecology and Biogeography 22:83-92, 2013) used dendrochronological and isotopic records to evaluate the impacts of atmospheric changes on tree growth across forest biomes. They concluded that over the past 50 years, tree growth decline has prevailed despite increasing atmospheric CO2. Furthermore, they found that the impact of atmospheric changes on forest productivity is latitude dependent, but empirical data suggest that globally CO2 fertilization of trees will not counteract emissions.

It is therefore essential that the authors consider CO2 acclimation and warming- and drought-induced growth decline in their model. At least, they should discuss the implications of acclimation and growth decline evident in empirical datasets, addressing modeling scenarios other than CO2 stimulation of growth.

General issues with isotopic patterns

The inversion approach described here is largely based on the work of Danis et al (Can. J. Forest Res., 42, 1697–1713, 2012) and seeks at finding optimal combinations of input atmospheric data so that process-based simulations are as close as possible to observations of growth responses and isotopic composition (used to infer physiological responses to CO2) of tree rings. This approach ignores, however, ongoing discussion on limitations of the isotopic method. Particularly relevant here is the debate on whether the use of tree ring δ13C data has overestimated responses to rising CO2. A recent study by Silva & Horwath (PLoS One 2013 8(1): e53089) reproduces global

increases in water use efficiency using Monte Carlo simulations to show that autocorrelations in the derivation of physiological parameters from δ13C largely overestimate responses to rising atmospheric CO2. These authors show that random δ13C datasets lead to increasing water use efficiency, over time and across altitudinal gradients, of the same magnitude and direction as reported in recent field studies. This is explained by the fact that CO2 is used as an independent variable in the calculation, generating positive responses to rising CO2 levels regardless of actual physiological responses. The same calculation is used here (eq. 3), thus, a discussion on real versus artificial CO2 effects is necessary.

This issue is connected with the pattern of growth decline and acclimation described above. For example, Silva & Horwath consider physiological responses as those that cause changes in atmosphere to plant (source to product) 13C discrimination. Under this definition expected increases in photosynthesis or declines in stomatal conductance should lead to proportional changes in 13C discrimination, but they found that increases in water use efficiency occur independently of changes in discrimination or growth. They propose the use of a baseline (non-physiological) response curve, reflecting what would be observed if Ci increased proportionally with Ca, a conservative scenario compared with, for example, constant Ci which would represent strong increases in water use efficiency and growth. Stimulation of photosynthesis that characterize CO2 fertilization effects would tend to keep Ci constant and this has been empirically demonstrated by Linares & Camarero (Global Change Biology 2012 18:1000-1012), who tested temporal trends in water use efficiency under three theoretical scenarios for the regulation of plant-gas exchange at increasing CO2, using temperature and precipitation data to predict tree growth. In their study the theoretical scenario assuming the strongest positive response to CO2 (i.e. Ci constant) explained 66–81% of the water use efficiency variance and 28–56% of the observed tree growth variance, whereas climatic variables explained less than 11–21% of the growth variance.

A theoretical assessment similar to that proposed by Linares & Camarero could be used here to distinguish physiological responses from artifacts of calculation. Alternatively, a simple response contrast analysis, based on the ratio between cumulative changes in growth and water use efficiency, could be used as proposed by Silva & Anand to integrate the effects of CO2 and climate variability on tree growth over time.

Specific comments

Abstract Line 4 CO2 levels have reached unprecedented levels in the past few centuries, but over geological timescales present levels are actually much lower than observed in the past (e.g. Veizer et al 2000. Evidence for decoupling of atmospheric CO2 and global climate during the Phanerozoic eon. Nature. 408: 698-701).

Introduction Lines 13-18 Soil-plant interactions and biotic (community level) responses, along with changes in resource (nutrient and water) availability, are among the most important factors controlling long-term effects of CO2 and should be mentioned here. For experimental examples see: Reich & Hobbie, Nature Climate Change 2012 3:278–282. For natural forest ecosystems see: Silva & Anand Community Ecology 2013 14(2): 208-218. Lines 20-23 This is correct but some important global patterns have already been described. See references discussed above. Line 26 I don't think "risky" is the best word choice here. Lines 25-28 "Variables such as oxygen and carbon isotopes in tree-ring cellulose are now routinely measured, so inverse modeling approaches should account for these proxies that present complementary (different) signals (sources of noise)." Somewhere in the manuscript expected relationships between δ13C and δ18O under drought stress and CO2 stimulation and including "sources of noise" should be discussed in detailed and references should be provided. In the current version of the manuscript these are not sufficiently described. Lines 10-15 All questions posed here, particularly number 3 ("Is the approach able to take into account and eventually isolate the effect of CO2 fertilization on tree ring growth and thus, attests of its impact on past climates reconstructed from tree ring series?"), have not been satisfactorily addressed in the discussion. I suggest including more direct answers to each of these questions in the first paragraph of the discussion.

MAIDENiso Lines 25-26 Consider other sources of mistaken ecophysiological interpretation based on suggestions made above. Inversion procedure Line 13 How are these "autocorrelations" addressed in the current model? Does the solution include concurrent changes CO2 and temperature? How can these effects be distinguished?

Tree ring data Line 3 The use of late wood measurements alone is rather unusual. Please explain why annual rings or early to late wood ratios were not used here. Given the fact that ring width declines with tree age many recent studies report growth patterns as changes in basal area increment over time (see references above). How would this conversion affect the output of the analysis?

Atmospheric data Line 24 Precipitation δ18O was "estimated statistically". How? Based on temperature and d18O enrichment relationships? Was total precipitation used as a factor in the temperature-driven enrichment calculation? How was covariance between these "independent" variables addressed? Were estimates compared with actual precipitation isotopic composition? Can these be shown as supplementary materials?

Results and discussion Line 15 Explain why summer temperature and precipitation are the best predictor of late wood growth. What would happen if annual ring width or basal area increments were used instead of late wood measurements? The selection of late wood as the main response variable needs to be justified here or in the methods section.

Comparison with modern records Line 23 How good and representative are these correlations? Patterns of recent widespread tree growth decline typically show better relationships with annual temperature and precipitation than these (e.g. Silva et al. 2010 PLoS ONE 5(7): e11543) Line 4 This discussion on "ecophysiological processes" needs to be expanded and improved to reflect ongoing discussions on artificial effects and actual responses to rising CO2. Line 11 What is the benefit of a seasonal resolution here?

Impact of CO_2 on the reconstruction Lines 3-4 “the A1 scenario produces a drier climate than the A2 scenario. These results might point out at a fertilization effect by CO_2.” This is incorrect. These results could potentially represent changes in water use efficiency and lower transpiration, which would not necessarily be accompanied by CO_2 stimulation of photosynthesis. Lines 24-28 “WUE calculations were performed for both A1 and A2 CO_2 scenarios (Fig. 9). An increase in WUE efficiency is depicted for the A1 scenario while WUE remained stable over time when the reconstruction is forced by the A2 scenario. Thus, our results suggest that anthropogenic CO_2 concentrations at Fontainebleau led to a more efficient utilization of water resources by the plant”. Yes but this could be an artifact of the calculation (eq. 3) as increasing Ca (independent variable) will always lead to progressively higher WUE over time independently of actual changes in A or g. The use of baselines curves, comparisons with Ci/Ca theoretical scenarios, or relationships between growth and WUE would clarify the cause of this observation, allowing differentiations between actual CO_2 fertilization and artificial responses. See references and discussion above. Lines 16-19 “The rate at which WUE augments with respect to CO_2 can be evaluated by plotting the difference in CO_2 concentrations and WUE for both A1 and A2 scenarios.” Again, this is a potential measure of the calculation artifact and should be explored under alternative scenarios. Lines 20-23 Progressive resource (water and nutrient) limitation is also important here.

Conclusion Line 25 This fertilization effect is not clear based on the data presented. Changes in WUE must be distinguished from CO_2 stimulation of photosynthesis, which would represent an actual fertilization effect.

Interactive comment on Biogeosciences Discuss., 10, 18479, 2013.