

Interactive comment on “Past and future scenarios of the effect of carbon dioxide on plant growth and transpiration for three vegetation types of southwestern France” by J.-C. Calvet et al.

Anonymous Referee #3

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This paper is a pure modeling study for 3 vegetation types (wheat, corn, coniferous forest), applied to 3 scenarios of CO₂, and with climate from observed hot and dry years in 2000–2004. They predict biomass, LAI and water variables under changes in stomatal conductance due to CO₂ increase (2050 scenario). However, a weakness of the study is that there is very little comparison with data. Even for the recent period, where data are available, all we are presented with are simulated results (table 2). Are the results in table 3, for example, consistent with actual measurements in comparable years? The only observational data relevant to 2050 are FACE and enclosure data,

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and these are not particularly useful because the results from the two approaches are quite different. Hence the value of the paper all hinges on whether their biophysical model is trustworthy enough to make meaningful conclusions about a changed set of circumstances, and there does not really seem enough evidence here to do so with confidence.

Some important information needed to interpret the paper is missing.

The most important aspect of this is that nowhere do the authors define the population over which they are defining distributions and statistics such as medians and quartiles.

Is it some random variation in the model parameters, for example? Without this being clearly described, interpreting the rest of the paper is problematical. For example, what does it mean to say that the distribution is bimodal? (l.186).

Section 3.3 is very confusing, with the text having little apparent connection with the Figs. Fig. 2 is quoted in support of values like +540 % increase in g_c and -4.4 decrease in temperature, but these clearly aren't on that fig. Similarly, the highest increase is around 1430, not noon, as stated. The largest decrease in g_c is around 10:30, not in the afternoon. The discussion of Fig. 3 is in terms of g_c , but that is not on the Fig.

On a more mundane level, the presentation of the paper could be improved. It is annoying to find g_c introduced at l. 59, but not defined until l. 142. Similarly, but less extreme, FACE (l.228). Some use of acronyms is gratuitous and hinders the reading. Why not corn, wheat and conifer, rather than IRC, MTM and CNF? The vertical scale of Figure 2 should be increased.

The most useful aspect of this paper is that it extends the debate on the link between carbon and water, and makes clear that any meaningful model seeking to predict either must adequately represent the full range of vegetation and soil processes. In doing so they call into question the conclusions of the Gedney et al (2006) study.

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