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Biogeosciences Discussions

Interactive comment on "Effects of climate warming and declining species richness in grassland model ecosystems: acclimation of CO_2 fluxes" *by* S. Vicca et al.

Anonymous Referee #1

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General Comments This is a highly pertinent subject for Biogeosciences because of the continuing topicality of the idea that global greenhouse-effect warming will cause global ecosystem respiration to increase more than net primary production giving off even more CO2 to atmosphere thereby acting as a positive feedback onto global air temperature (eg Cox PM, Betts RA, Jones CD, et al. (2000). Acceleration of global warming due to carbon-cycle feedbacks in a coupled climate model. Nature 408, 750-750). The paper addresses the question of long term acclimation of ecosystem respiration to continuous atmospheric warming (+3C over 28 months) and compares it to long term acclimation of photosynthetic carbon gain. The authors conclude that both photosynthesis and respiration acclimated to continuous warming and did so to the

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same degree such that a change in whole ecosystem-respiration to photosynthesis ratio could not be detected. Additionally, the authors found that the number of plant species or functional types growing together in the model ecosystem microcosms had no impact on this ratio or on the above conclusion.

The paper has an appropriate title and adds important long term data to the topic, but fails to address effectively the unresolved issue of whether heterotrophic respiration (ie decomposition) in the ecosystem acclimates differently from autotrophic respiration (ie plant respiration). As such an opportunity for a definitive advance to this topic was foregone.

Substantial conclusions are added to the world literature but ones that will attract much debate. The methods are generally good but key observations were not made, leaving interpretation difficult for important matters (see below). This is a great pity and leaves the way open for doubts.

The language is fluent and manuscript structure fine, but leaves important information about procedure out (see below).

Specific Comments Although the Introduction distinguishes heterotrophic respiration from autotrophic respiration, the body of the experimental paper loses track of that important distinction. It is an important distinction because the mechanism and magnitude of each type of respiration might be quite different - one being a plant-process based on relatively recently fixed carbon, the other microbial oxidation of organic matter of a wide range of ages from seconds or minutes to hundreds or thousands of years. Section 2.1 describing the study site indicates that tubes (pots) were planted with various grassland species but does not say what rooting medium was used. This is critical. If an inert carbon-free potting medium were used then the ecosystem respiration would be dominated by plant respiration, heterotrophic respiration being confined to decomposition of only the new organic matter added to the soil by root and shoot litter and exudates during period of the experiment. If it was a high organic matter

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soil then heterotrophic respiration could exceed autotrophic respiration. Readers are guided to Leemans et al (2006) for further descriptions of the experimental setup. In that paper it indicates that the rooting medium used was a sieved loamy soil from a maize field. Unfortunately it does not state the soil carbon content, which is important in determining soil respiration rate. Specification of conventional SOM pools (eg fast, medium and slow turnover pools by a standard methodology) would have been even better for appreciating the significance of the results.

For these kinds of study where communities of plants are being studied it is critical to indicate the spacing between the pots. If there is a space then the leaves of edge-plants in each pot will spread (or flop) out beyond, possibly well beyond, the edges of the pots thereby harvesting more light than is appropriate for their number. The preferable procedure is for the pots to be packed together so that each micro-community (in each pot) has the light regime appropriate to soil surface area that it occupies. A diagram in Leemans et al (Fig 1) implies that they were packed close together but, if that were so, this should be stated explicitly for clarity.

The CO2 flux measuring system needs clarification. Leemans et al (2000), to whom readers are referred for details, indicates that single fully expanded leaves were measured. But the present ms refers to a cuvette that is 60cm high by 25cm wide. Only by implication, then, is it assumed that this is a whole pot cuvette that presumably clamps onto the top of a single pot wholly enclosing the shoots. If so it is unclear how the clamping was done and how the cuvette was installed over the plants if the pots within the growth chamber were in fact tightly packed together touching side by side. Assuming that the cuvettes were whole-pot cuvettes then, with sunlight exposure, the temperature would rise above that of the growth chamber in which they were installed unless there was some ancillary cooling system in the air circuit. What was done to control temperature in the measuring cuvette during measurement needs description? How long were the plants enclosed in the cuvette foe each measurement cycle?

Measuring whole-system respiration by darkening the whole plant sometime during the

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day requires clarification and detail. Darkened plant respiration rate can vary through the day and night, and with recent irradiance. It also often declines progressively after darkening. On the other hand, it takes time for a large-volume cuvette system, such as that used, to stabilise after a change in condition from carbon uptake to carbon emission. A compromise has to be adopted as to how long to wait before making the measurement after darkening or after dusk. Thus a systematic, informed strategy has to be evaluated, adopted and stated to do such measurements. What was it? The results could vary with the approach adopted. There is no perfect approach, because we cannot be sure that respiration during photosynthesis by day is the same as after darkening or as the average during the night. But any differences in conclusion between investigators performing similar experiments may relate to differences in the approach adopted for measuring dark respiration rate.

P 1479 line 14-16. Again, in measuring green plant cover, there needs to be a description of what was done about leaves that extended beyond the edge of the pots if the pots were not deployed tightly packed.

P1480, line1-5. As a Q10 function was used to specify TER7, there needs to be indication of how well that function fitted the data over the temperature range used. Generally the Q10 function does not work well with respiration, usually declining with increasing temperature. This needs clarification for this system.

P1480, Equn 3. How well did this function fit the data. Exactly which data were used to calculate coefficients a and b? Was it the entire data set for the whole experiment across all communities and treatments? A figure of y against x for all data would be worthwhile.

I am having difficulty understanding how equation 3 was used. From the description it seems that the values of x used for calculating TER7(pc1) and GPP100(pc1) were as actually measured at the time of gas exchange measurement, rather than at some standard plant cover. In which case what was it that the equation is doing for the anal-

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ysis? And what is the meaning of the residual? Perhaps there is a need for clarification of exactly what is meant by “at the corresponding plant cover” on line 21: corresponding to what? The subsequent parenthetic comment says it is the “measured” plant cover, but surely that does not “correct for” variation in plant cover deriving from temperature effects on plant cover, as was indicated as the purpose of Equation 3. If these comments suggest to the authors that I have not followed the procedure, then perhaps it is indicative that others may have trouble too, and a clearer graphically-illustrated description is worth consideration.

Furthermore, for evaluating acclimation of ecosystem respiration and photosynthesis to long term warmer temperature, is adjustment in relation to plant cover an appropriate procedure? For the heterotrophic respiration component of respiration , I cannot see the logic of it.

P1481, line 21-22. This sentence that outliers were not included in the analysis sounds rather ominous. Not a problem if there were only one or two in the whole data set, but how many outliers were in fact discarded?

P1484, The authors seem very ready to jump to the conclusion that warming had a developmental effect of slowing the onset of leaf senescence to explain the higher plant cover. But no evidence is given for this. Generally plant and organ development is accelerated by higher temperatures rather than delayed or postponed. To me the more likely explanation is that plant growth rate was at a suboptimal temperature on its bell curve and that warming increased daily growth rate that was expressed in more and/or bigger leaves, possibly well before the GPP measurements were made. So to convince me, the authors need to present evidence to distinguish between these two options - more or bigger leaf production versus longer lived leaves.

P1485, line 6-15. When you have round pots with spaces around them it is indeed possible to have plant cover exceeding 100% due to the spread of leaves beyond the edge of the pot. Again, how this was dealt with needs to be explicitly addressed.

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P1485, line 16-18. This seems to be the primary conclusion of the paper. It needs to have a Table of data showing this effect giving TER7, GPP100 and their ratio for the various communities at each temperature.

Editorial comments and corrections. The manuscript is remarkably error free.

Interactive comment on Biogeosciences Discuss., 3, 1473, 2006.