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Interactive comment on “Decreased summer drought affects plant productivity and soil carbon dynamics in Mediterranean woodland” by M. F. Cotrufo et al.

Anonymous Referee #1

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The manuscript by Cotrufo et al reports results from a long-term precipitation manipulation experiment in Italy. The rationale for the experiment is good, as climate change induced shifts in precipitation are predicted to have significantly greater impact on Mediterranean ecosystem C exchange than temperature effects. This topic of research is clearly suited to the scope of Biogeosciences. The study includes most of the main components of the C cycle, and results show very interesting significant impacts on C input and CO₂ efflux in the soil according to moisture treatment. Overall, i think the results are worthy of publication, as they are very important for the development of ecosystem models that include biotic and abiotic drivers in soil C dynamics. The fact that root c input was overlooked is a shame, but I think that the remaining results are

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strong enough for publication. I do however have a number of queries that should be addressed before final acceptance of the manuscript.

My first query concerns the use and impact of drains for throughfall exclusion. These cover 20% of the area, so will have intercepted approximately 20% of leaf litterfall at the site. The methods do not indicate whether this was corrected for (e.g. by collecting litter in drains and re-distributing manually), but it has obvious implications for the carbon balance presented and has to be clarified. The nature of the throughfall exclusion using drains is that dry “strips” were created underneath drains, whilst areas between drains received normal amounts of throughfall. For the plants at the sites, I agree that this is an overall reduction of available water equivalent to a 10% reduction in precipitation. However, for heterotrophic respiration, as well as root abundance, the heterogeneous nature of this throughfall exclusion will have a much more localized impact. In order to evaluate the reduction in soil CO₂ efflux, it is important to know where SR chambers were positioned with regards to the drains, and also where moisture sensors were located. Again, this needs to be clarified.

The method for estimating root-derived C input (rhizodeposition) in in-growth cores containing C₄-soil is interesting. The use of exogenous soil has its limitations, as the authors clearly point out, but it is an innovative approach that here shows marked and highly significant differences. In order to assess belowground C allocation, it would however have been even more relevant to include belowground NPP, e.g. by quantifying the amount of root-ingrowth to the soil cores. The rhizodeposition gives a good indication of higher input in the ‘Wet’ treatments, but this C is likely to turnover at a high rate. Actual root biomass data for newly grown roots (even if all caveats for using exogenous soil apply) would have helped with the overall carbon balance for soil C in- and out-puts. As the roots had to be removed from the C₄-soil cores, I wonder if these root biomass data may in fact exist.

I’m not convinced that the modelling of CO₂ fluxes is that meaningful in this paper. The authors show results of a multivariate regression for abiotic drivers (temperature and

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soil moisture), but I can't see how this adds much to the results. I find the experimental results quite convincing, but could not learn much more from the abiotic modelling. Figure 7 appears to show higher CO₂ fluxes for identical abiotic soil conditions, which would indicate that differences are due to different substrate supply. The current interpretation of higher fluxes due to co-occurrence of higher moisture with higher temperature is not strong, as the regression should capture the different ranges of driver values. If only abiotic drivers could explain the respective results, the three regressions should be more or less identical, but the driver ranges over which the regressions are obtained should differ. If the authors think that modelling is significant in this study (and to my mind it is not critical for acceptance), they should consider the interpretations of the result more carefully.

Detailed comments

5959, 14: The hypothesis includes direct and indirect moisture effects on SR (meaning, presumably, control of moisture on microbial decomposition on the one hand, and impact of moisture of plant-derived C as a source of CO₂). However, these two aspects are not investigated independently, so I wonder if the hypothesis should reflect better that the approach investigates total soil CO₂ flux only. This does not invalidate the interpretation that there are different effects underlying this, based on results for plant C inputs, for example.

5960, 2: Replace "vegetation types" by "plants".

5960, 22-25: It seems from Figure 1 that moisture control in the 'Wet' treatment was at least as good in 2007 than in other years (but I do note a short data gap in late summer). From these data, the reduced irrigation performance seems to have minimal or no impact. In fact other years (2006) seem to show that moisture is below the target value more frequently and for longer than in 2007. 5960, 26: "wide", rather than "width".

5962, 21: "entry"

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5964, 24: Here and throughout the text, please remove “the” before years.

5966, 6: I assume “JAS” stands for “July, August September” – please clarify, it’s not that obvious.

5966, 8: “when”, rather than “were”

5968, 6-12: Figure 7 shows moisture limitation and exponential temperature responses for all treatments, not just wet. That highest fluxes are recorded in the ‘Wet’ treatment is clear from the previous graphs. The temperature scale differs between the panels of Fig. 7, and it is not clear whether for similar temperature and moisture the fluxes actually differ. If they do, this would indicate that the abiotic drivers don’t determine the flux magnitude, but that other factors (e.g. autotrophic fluxes) are significant, with higher substrate supply for identical abiotic conditions. For soil moisture above about 0.15 (or so), there is no impact of soil moisture in these graphs anyway.

5969, 5-5970, 7: The opening of the discussion does not seem ideal. Rather than re-stating the aims, and dealing with various caveats, I suggest starting with the discussion of key findings. The points you make in the first two paragraphs are important, but the impression I got from reading it was that there were a number of problems with this study, when I think these aren’t the main issue.

5970, 19: The point of measurement of soil moisture within the ‘dry’ treatment will have great impact on the measured soil moisture reduction. As for soil respiration and root growth (see above), depending on whether moisture is measured beneath drains, or in between drains will determine whether or not actual throughfall reduction is detected or not. This should be clarified in the methods in order for the reader to be able to assess this apparent lack of effect.

5970, 28: According to Table 3, leaf litter production is not significantly different between ‘Dry’ and ‘Control’ treatments.

5971, 1-3: What about effects of increased moisture in the litter decomposition study

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cited here?

5971, 29-5972, 1: If the moisture response is supposed to be linear, why do you use a negative-exponential curve for soil moisture dependence in your model! It certainly doesn't look linear in Fig. 7.

5972, 14: Inglima et al. (2009) is not in the reference list.

Fig. 5: These plots show that the model used is not well suited to capture soil CO₂ efflux. Both the visual match of measured and modeled in the main panel, and the very significant deviation from a 1:1 relationship in the inset graphs show that modelling results have to be treated with considerable caution here, and I suggest that the data are strong enough without the model.

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