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Interactive comment on "Partitioning of canopy and soil CO_2 fluxes in a pine forests at the dry timberline" *by* Rafat Qubaja et al.

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Detail response to two Reviewers comments, BG-2019-291

Ref1

This paper uses soil and canopy fluxes with stable isotopes (13C) and radiocarbon(14C) measurements of a 50-year-old dry pine forest over one year to partition the ecosystem's CO2 flux into gross primary productivity (GPP) and ecosystem respiration(Re) and soil respiration flux into autotrophi (Rsa), heterotrophic (Rh), and inorganic(Ri) components. The measurements and data are valuable. The topics are be of great interest.

Response: Thank you, it is the most important point. But as we clarify better in the revisions (see also below) the paper goes beyond "one year of partitioning" as this year of measurements allowed us to combine it with our own study about 10 years earlier at the same site to provide a longer-term perspective on the changes in such partitioning (and as the saying goes, with the "whole is greater than the sum of its parts" . . .)

But the writing is very confusing.

Response: We are sorry this is the case and made a serious effort to fix this by better streamlining the paper (as also noted below), careful proof-reading and, considering that the authors are not native English speakers, also sending it out to professional editing.

Abstract: The abstract lacks critical information.

Response: We are not sure what is the missing critical information. We added in the Abstract key information on the site and the study itself. We concluded that this is likelly the missing information after we carefully checked the summary in the Abstract and the Result. The results are presented in 3 figures and 4 tables, and carefully comparing the detail summary of the results in the Abstract, indicates that all the main points, including our observed values of GPP, Re, NPP, CUE, the partitioning (the main effort in this study) of respiration Rs to Rsa to Rh, Ri, the seasonal changes, the main long-term changes when combined with our earlier study, and the main occlusions.

Introduction: The introduction should be rephrased. There are too much pieces of information on general knowledge. The Introduction needs a better flow. The scientific significance should be addressed more. A description that explain why the paper is needed following the previous studies (i.e. the 2001-2006 study) would be very informative for the readers. Furthermore, I suggest the authors cite more relevant papers on the Mediterranean climate zones and add one or two hypothesis.

Response: Done. We shortened the introduction by nearly 30%. The original Intro had 6 paragraphs, and we removed two non-essential ones, and combined the one on isotopes into the paragraph on partitioning. We also improved the Motivation (the importance of the combination of the two studies at our site in 2001-6 and 2015-16 to assess changes over time) and we provide clearer working hypotheses, as requested. We checked the literature and added missing references on Mediterranean studies.

Site description: provide more information about the vegetation (e.g. root depth). Flux and meteorological measurements: How did the gaps in NEE and GPP are filled? How many missing data points are there due to instrument failure and quality control?

Response: Done. We added the requested information, including detail on root depth and on the vegetation at the site (overstory and understory, indicating the main species). Specifically, regarding the requested information root depth we note that our paper published earlier this year (Preisler et al. 2019, Functional Ecology; cited) provides detail information on root depth, distribution and microsite effects (see SI Fig. 1).

Soil CO2 flux: I'm really confused. How many data did the author used in the paper? Just one year? Using just one-year measurements can not identify the long-term temporal changes in the soil–atmosphere CO2 fluxes in this environment.

Response: As noted above, we now clarify this issue straight-out in the Intro and again in the Methods and in the Discussion. Briefly, note that all the figures and tables indicate that the new data were obtained during 2015-2016 (one full year). But both the figures/tables and Discussion show that impotent virtue of the paper is in combining these new data with our earlier study at the same site (2001-2006) looking at the same parameters and obtaining a long-term perspective of the change in the flux component (i.e. in the partitioning) over a time window of about 10 years. Many of the studies reported in this journal aim at assessing change, especially in response to global change. While some parameters are monitored continuously (our flux tower operated continuously for 20 years), other measurements required for the Partitioning of soil fluxes cannot be, practically, made continuously and here we combine the continuous measurement with the periodical campaign to assess sufficiently long-term changes.

Discussion: The hypothesis should also be into Introduction. The present study used only one-year data, I suggest the authors using a tempered tone in the discussion part. The Discussion has the same problem with Introduction. There are many pieces of interesting information. But the discussion should be centered on several key aspects of your results. The Discussion should echoes the Introduction. I suggest rephrase the Discussion and make a better flow in the Discussion.

Response: Done. The working Hypotheses are included in both the Introduction and Discussion as noted above, and we streamlined the Discussion to improve the correspondence with the Introduction. As also noted above, we better focus on the link of the new one-year data of this study with our older study at the same site to assess changes over a period of about 10 years to addresses the issue of changes in the ecosystem and particularly in the soil carbon flux component as climate is changing (we also added in the SI the long-term records of temperature and precipitations).

Ref2

This manuscript describes the study partitioning of canopy and soil CO2 fluxes in a pine forest at the dry timberline using the measurements of isotopic signatures (δ13CandΔ14C) of CO2 emitted from bulk soils, fine roots, root-free soils, and carbonate fractions. The measurement and data are interesting. Then, scientific insights, which can be gained from this study, would significantly contribute for improving our under-standing the response of dry environment ecosystems to climate change.

Response: Thank you. It is important to see that this study is recognized as adding to understanding ecosystem response to change.

The writing, however, should be improved more and more as pointed out by Referee #1. Then, please refine every sentence in the manuscript more carefully, because there are substantial typos (e.g. “a pine forests” in the title, “Soil respiration from the atmosphere” in Line 29-30, “Reflux” in Line 369, and so on).

Response: We recognize that our failure to submit an appropriately proofed manuscript made some significant damage, although there were no errors in the science. This was the result of some unfortunate confusion in combining the different versions of the paper proof-read by different coauthors. It has been fixed and considering that the authors are not native English speakers the paper has been sent out for professional editing.

In addition to these concerns for writing, I have a technical concern about the estimating δ13C for CO2 emitted from bulk soils (i.e.δ13C RS in the manuscript). The authors estimated δ13C RS using the keeling plots for soil CO2 profile data at 0, 30, 60, 90, and 120 cm depth; however, theδ13C of soil organic matters, the major source of heterotrophic respiration, often change along with soil depth increase. Then, these vertical changes inδ13C of soil organic matters have significant potentials affecting theδ13C-CO2 profile. This means that the observed relationships betweenδ13C-CO2 and CO2 concentration profiles might be affected not only by the change in contribution of source CO2 and background CO2, but also by the changes inδ13C of source CO2. Therefore, in my opinion, the authors are needed to provide the reliable justification for their methodology, to quantify the uncertainty for estimatedδ13C RS, and/or to apply alternative methodology for estimatingδ13C RS.

Response: This is indeed an important point and has now been clarified in the revisions. The Ref is correct in noting that the Keeling plot approach is based on 2-end members mixing (as also explained in the Review one of us co-authored; Pataki et al., 2003), and in many cases this assumption does not hold in soils. However, it seems that the very dry conditions at our study site gave us an opportunity to avoid this caveat. As shown in the figure below, there is essentially no change in 13C of soil organics with depth (SD of the 12 samples=0.12 permil; see SI Fig. 2). This is likely because the dry conditions strongly constrain decomposition and probably also the range of microbial populations (and help explain the high soil carbon storage in this system as noted in the Discussion). It therefore seems that the soil CO2 samplings we carried out still represent predominantly the mixing of atmospheric CO2 with one integrated soil source signal. We must conclude of course that the variations among the contributions of Rsa, Rh, and Ri do not change significantly with depth and the single set of isotopic signatures in Table 2.

Finally, please consider to include the photographs showing conditions of each chamber site and the schematic diagrams describing three collars locations within a chamber site

Response: We are happy to oblige and agree this could help. We added to the SI diagram and photo (see SI Fig. 3).

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-291, 2019.

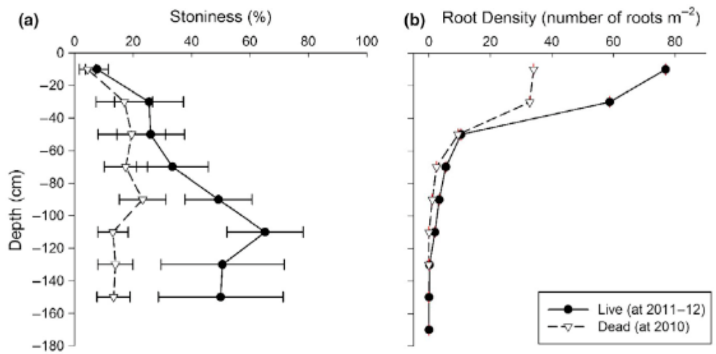

FIGURE 4 Stoniness percentage in L and D plots along the soil profile (left). Root density distribution in L and D plots along the soil profile. Error bars are included, but since their values are low, they are often obscured by the symbols

Fig. 1. Figure 1

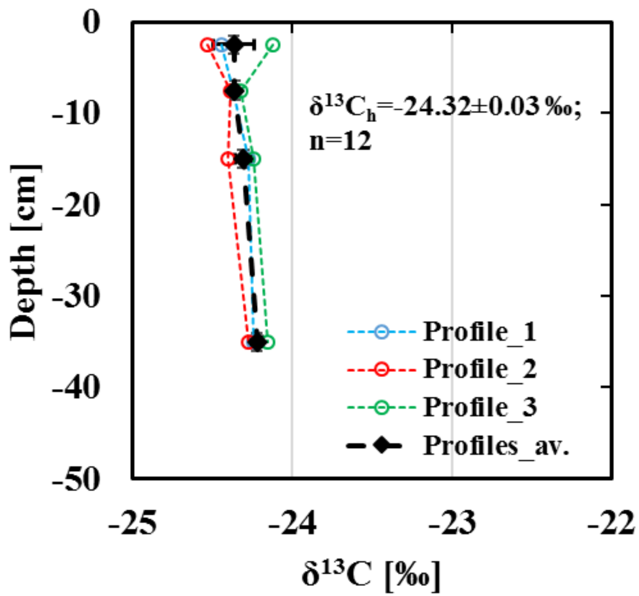

Fig. 2. Figure 2

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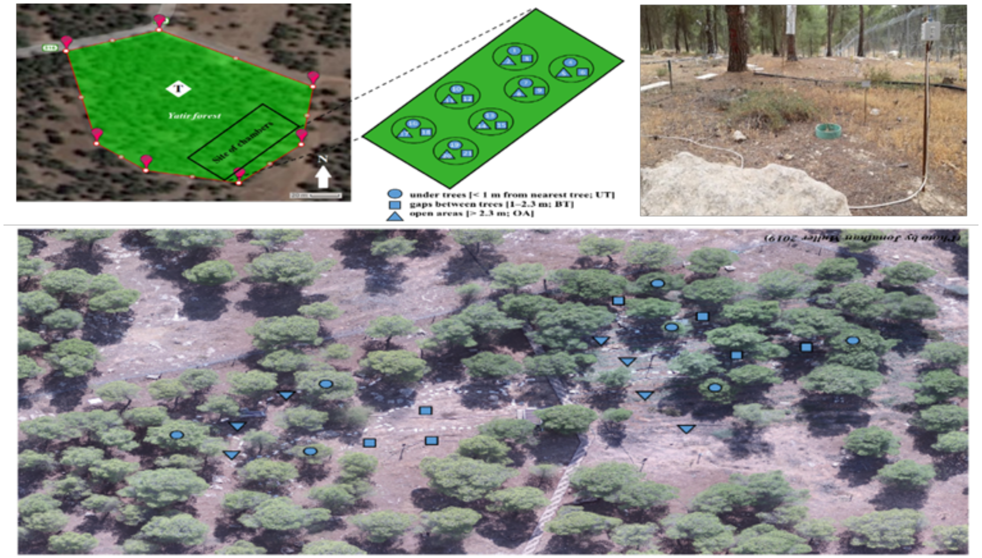

Fig. 3. Figure 3

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