

Interactive comment on “Combined effects of altered N:P stoichiometry and trees on Mediterranean savanna root dynamics” by Richard Nair et al.

Anonymous Referee #2

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Studies on root systems are extremely labor intensive and thus data on fine root growth dynamics are very limited, especially in Mediterranean systems. The manuscript presents a thorough assessment of fine root dynamics in a Mediterranean savanna and shows that above and belowground growth are coupled and respond sensitive to seasonal moisture changes. The data clearly contributes to our knowledge on the belowground system, but in its current state the manuscript is strongly limited by (1) the poor experimental set-up with unrepeated fertilization treatments whose impact on ecosystem stoichiometry remains uncertain, (2) an unclear and puzzling presentation of data. (3) Including auxiliary data (N-P contents, soil moisture, soil properties) would greatly improve the quality and the interpretability of the manuscript and in such a large

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scale study, these data should also be available. (4) Finally, the language is somewhat sloppy, see many examples in the Specific comments. Based on these points, the manuscript does not meet the standards of Biogeosciences and requires a major input of additional data, a rewriting of large sections and thus should be submitted as a new manuscript.

(1) The overall experimental set-up is not ideal for a root study as there are no real replicates for the treatments. I can understand that the sampling design has been adjusted to the overall design of the eddy-covariance measurements (not allowing more replications), but the manuscript profits very little from these additional measurements. I would recommend to state this statistical flaw openly in the Statistics section (and readers can judge by themselves if they accept the design as pseudo-replication or not). Moreover, site and soil of the different sites have to be presented if they are comparable among sites. Also, by analyzing N-P contents in fine roots or by presenting other indices of N-P fertility, the authors could provide evidence that the differences among sites can be indeed attributed to fertilization. In the Discussion, it is written that “plants exploring shallow soils in the P-limited N treatment” – which actually indicates that soil conditions were apparently not similar among treatments or sites – this definitely has to be documented! P fertilization is particularly prone to become sorbed in soils and thus without additional documentation of the effectiveness of the treatment, the discussion of P effect or P deficiency remains speculative. In this sense, also the title is misleading as the authors did not study stoichiometry but fertilization.

(2) The data presentation remains unclear as it is extremely difficult to follow what has been measured at what time as the description in the method section differs from what is shown in the Figures and Tables. Figures 2, 4 and Table 1 only show subsets of the data differing among themselves and it remains unknown why. The same holds true for the soil core and ingrowth measurements, where I presume that the labelling in Figure 4 was simply mixed up. (3) The manuscript would greatly profit from auxiliary data. Indices of N-P-fertility (ideally in root tissues) to document if treatment effects are

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real (or simply related to site differences), soil properties to document micro site effects (highlighted in the Introduction), and seasonal changes soil moisture contents (to discuss habitat effects and potentially also critical moisture contents). Specific comments: (1) Data presentation. Figure 2 shows data from March 17, May 17, Sep 17, Dec 17 while in the methods it is written that RLD has been measured on six dates (as shown in Figure 6). Figure 4 shows other dates. Why are the data from the other dates not shown?

Comments to Figures:

Figure 4 start y –axis at 0!

First sentence of Figure Caption: Root biomass in top 13 cm from ingrowth cores (all installed Dec. 2016 and removed then in Nov 16 and Mar 17): how can you install a core in Dec 2016 and sample it in Nov 2016, hence a month earlier? The method text on page 6 reads: We collected ancillary measurements of root biomass from two non-concurrent methods, sampling within 1m of the minirhizotron tubes; soil cores (Dec 2016, March 2017) and two rounds of ingrowth cores installed in December 2016 (removed May 2017, December 2017) and December 2017 (removed March 2018, May 2018).

The color code on Figure 4 indicates 2 for ingrowth cores (Nov 16 and Mar 17 on the left) and 4 sampling dates for soil cores – I presume that these have been mixed up

Figure 5 Caption: Control Treatment response to rain pulse in May 2017 (Section 3.5). Response of??? Please keep correct and self-explanatory in Figure captions. As a reader I would also prefer if you would avoid abbreviations here

Figure 6: What is the unit of y-axis: % of maximal value, only 1%. Probably you mean 'fraction'. Explain the abbreviations please

Table 1: Why don't you report the data from the September and December 2017 measurements?

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Comments to text

Introduction

Page 2 : 12-15 sentence read awkward

Page 2 L. 17 'ecological attention' – please clarify

Page 2 L. 25ff is a listing of global change buzzwords that are not linked to this study – instead the authors should develop how N and N-P additions may affect root growth. This appears not before introducing the experimental set-up and phrasing the hypothesis.

P. 3L.10 rephrase 'a 2015 meta analysis'

P. 3 L. 14, L. 22 'fewer investigating global change factors' - buzzwording, which one did the authors? The specific one of this study needs to be developed

P. 4 L. 2 'standard resource limitation theory' - explain what standard is

Results:

P. 7L. 22 cover-based markup is slang

P 8 L. 3 ff in the most abundant period – reads sloppy to me

P. 8 L. 4 peak shallow RLD – please explain

P. 8 L. 5 "shallow soil" - which soils were shallow? – this needs to be described in the methods

P. 8 L. 9 less roots in TC than in OP – what is the statistical significance? While the experimental design does not allow an analysis of treatment effects it allows the identification of tree canopy effects.

P. 8 L. 17 "limited the potential for variation" please explain – variation should be independent from intensity

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P. 8 L. 20 “While these” clarify what “these” are!

P. 8 L.25 “as following transformation to fit the assumption of linear models” – requires explanation

P.9 paragraph 3.5. While a higher temporal resolution of root growth is highly appreciated, 2 time points are still providing a poor picture and the reader is left how significant the findings are.

P. 9 L. 16 sentence is awkward and unclear – “indicate” is somewhat strong

P. 9 L. 20 introduce RSR, try to avoid a abbreviations, the vegetation layer meant is grassland or?

P. 9 L. 22: 3-4 statements in one sentence is too much

P. 9 L. 23/24 statement is unclear. Do you really want to compare the differences between roots and shoots as compared to nutrient treatment effects?

Discussion

Overall, the Discussion is somewhat lengthy partly due to the repeating of methodological issues.

P. 9 L. 27 clarify what the different directional effect

P. 10 L. informed by installation and sampling effort: clarify what is meant by “informed”

P. 10 what is an “Effect on root measurements between methods” Provide a proper title of this paragraph!

P. 11 L.1 “inversion of directional effect” – requires explanation

P. 11 L. 8 the sentence on NP effect interrupts the discussion on habitat effects

P. 11 L. 11-17 methodological issues have already been discussed in the first paragraph of the discussion

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P. 11 L. 25 plants exploring shallow soils in the P-limited N treatment – here we learn that soil conditions were apparently not similar among treatments or sites – this has to be documented

P. 11 L. 27 the discussion on P availability effects has to be backed up with some measures of P availability

P. 12 L. 2 “elevation of N and NP biomass – slang and unclear if above- or belowground biomass - explain!

P. 12 L.10 the discussion on plant trait effects could be more straightforward by simply presenting species composition of the grassland in your study

P. 12 L. 15 “under P deficiency” – speculation

P. 12, L. 29 “ so this relative decrease in RSR was still a large absolute increase in root biomass” – needs further explanation – is it not evident that a ratio changes with its numerators and denominators. . .

P. 13L 5 what are the sampling depths of the other studies, frequently they are limited to surface soil, but the data of this study indicate also that roots are present in the deeper soil.

P. 14 L. 1 “C status” requires explanation or omit

P. 14 L. 7 “short-term events are highly coupled” – awkward wording you probably mean above and belowground growth in response to an event and not the event as such. . .

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