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Biogeosciences Discussions

Interactive comment on "Interactions among vegetation and ozone, water and nitrogen fluxes in a coastal Mediterranean maquis ecosystem" *by* G. Gerosa et al.

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The paper focuses on measurements of ecosystem- scale fluxes of o3, water vapour, and nox from a coastal maquis that were part of a summer measuring campaign where a broader suit of phenomena were simultaneously recorded. The paper also offers calculations of canopy-scale stomatal and non-stomatal ozone fluxes, as well as exposure and dose metrics of ozone risk assessment. Accompanying measurements such as soil water content, sap flow, and meteorology assist the interpretation of the data.

The partition of the ozone fluxes and the mechanisms behind the processes are relevant topics in the study of ozone fluxes in the field. In addition, as the authors discuss,

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also the understanding of the fluxes is a necessary step tot the understanding of the risk to ozone exposure. The contents of the paper are of interest and suitable for the scope of Biogeoseciences.

There are an increasing number of papers that describe ecosystem-scale fluxes; the modest novelty of this paper lays in the type of ecosystem being measured and the combination of different types of measurements together. As it stands now it is rather a descriptive paper. The results serve to illustrate the behaviour of the ecosystem fluxes, the discussion considers several possibilities, but the paper does not aim to reach substantial conclusions. I feel the authors are loosing a chance to present a more analytical study where previously formulated hypothesis could be tested. The readers will have to wait for future analysis of the dataset in that direction.

I find the following points would need revision or reformulation:

1. The treatment of the relationship of the non-stomatal ozone deposition with air humidity.

It is stated in the conclusions that the non-stomatal ozone deposition is positively influenced by air humidity. This is surely the case, but considering the wealth of data presented for other topics, the data presented in relation to this is disproportionately small. I miss a mention -or better a graph- about the general humidity regimes of the site. Please add.

I read from other papers in this special issue: Fares et al in "The ACCENT/VOCBAS campaign...": the proximity of the site to the sea led to a high humidity regime (with RH rarely below 60%). Mereu et al in "A whole plant...": Mean daily RH remained in the range 60-80% throughout the campaign. See also their Fig 1.

The humidity regime in this site is thus quite characteristic as a humid coastal location. This is very relevant for the analysis of the non-stomatal ozone flux. It has been discussed before that somewhere between 60 and 70% RH there is water film formation

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on the plant surfaces. This means water films would be present more or less all the time, and the presence of salts in this coastal site makes this even more likely. It has also been reported that the presence of films enhance the deposition. For an extended discussion see Altimir et al, 2006 BG, which attempts an explanation on some of the observation reported in the earlier Altimir et al, 2004 that you kindly consider in 1470 line 24.

I would recommend to use the relative humidity instead (or in addition of) of the absolute humidity. In part because I believe it connects more directly with the processes behind the deposition phenomena (see previous point) but also because it will be better comparable with results of all previous works that have used RH in similar analysis.

In relation to this, the criteria for dry canopy could be revised or better explained. In previous works, for the definition of dry canopy the potential existence of films has been taken into account and dry conditions had been not only absence of liquid drops but also RH$<$60 o 70%. According to this the canopies in your site would never be dry.

I miss a better description of the surface wetness sensor. Please add details about the calibration, the presence of paint coat and type, the orientation of the plate, and the location of the sensor. This are all quite critical to determine the sensitivity and representativity of your measurements. More explanation on the relative units in fig 9 is needed. I also believe that you can not talk about immediate drying (1468 line 18) when it actually takes more than 2 hours according to fig 9.

Please clarify all these points.

2. Ecosystem structure: description and significance.

A short paragraph on the type of ecosystem and its structure is needed in addition to the reference to the accompanying paper of Fares et al. Please add.

Actually, the lack of detailed description of the area seems not justified provided the exhaustive structural measurements done for the campaign. I read in Fares et al that

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the projected area of each plant in the experimental plot was measured and thereafter other samplings were also performed to asses the LAI etc. Therefore, it seems that it would be possible to even construct quite a detailed spatial map of the density and composition of the vegetation in the plot.

It is also said in Fares et al that the plot is made out of patches of maquis and garigue communities. This would imply patches of higher vegetation including Quercus and patches of low-schrubs species. From the point of view of a resistance analogue this site could seem to be closer to a dual-sink system. In general the adequacy of the one-layer big-leaf approach at this site is not discussed.

Also, the title refers only to maquis (I suppose because garrige in this site is interpreted as a degradation of maquis) and without a better description leaves the reader to believe the site is somewhat homogeneous

All these points need clarification and correction.

3. Title and Introduction consistency

As I have understood it, this is a paper about ozone fluxes. The title is slightly misleading because water, o3 and nox are not approached equally in the paper. Consider a change.

The introduction contents do not appear to reflect the analysis and measurements presented in the paper. I understand the authors try to state the toxicity of ozone, the need for proper ozone risk, and the lack of information for certain ecosystems as the motivations behind the measurements. All this could be stated in one paragraph. I do not find a detailed description of the potential injury relevant here (and any review of such should be accompanied with a mention to the experimental exposures etc…). The reader could be better informed about the state-of-the-art in the understanding of ozone fluxes. The introduction now does not mention several topics that appear prominently in the later analysis: nox flux, comparison with the sap flow measurements, and

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non-stomatal flux. Some literature review paragraphs now placed in the discussion could be better placed in the intro, for example on the topic of the non-stomatal flux and why is it relevant to know it in the context of ozone risk assessment.

Please, modify accordingly.

4. Placement

I have a problem with figures 10, 11 and 12 appearing in the discussion. I think the result figures belong to the results section.

Figures 10 would be presented in connection with fig 5. Figure 11 is actually removable and replaceable by a reference to Mereu et al. The results on non-stomatal flux would logically follow the analysis of stomatal uptake fraction, thus Fig 12 would logically follow fig 6.

Other comments

1455 line 20 it is not clear why you refer to year 2000, is this the only estimation available?

1456 line 1: "stomatal regulation plays an important role" clarify in what. Suggestion: "because it regulates the passage..."

1456 line 14 "measurements of AFstY are difficcult"Please clarify. Are they difficult or are they scarce?

1456 line 15 "direct measurement" you might have meant "direct ecosystem-scale measurements"

1456 line 17 "not completely standardised complex equipment" please clarify. I would say the equipment is pretty well know by now

1456 line 17 "As a consequence" are we using models because we can not measure easily? would not modelled estimations exist in any case?

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1457 line 3 ";suitable for model calibration" please clarify to what model(type) you are referring.

1459 line 11 is it van or Van? 1460 line 13 "the canopy had to be completely dry" Please clarify what does this mean having into account the previous literature

1460 line 15 Title looks awkward, consider another title for this section such as : "calculation of the stomatal flux"

1461 line 17 please provide reference of the diffusivity ratio

1461 lines 23-26 this sounds bad for you, could you explain it better?

1462 line 1 this section is about gap-filling remove "data selection" from the title, you already have another section on data selection

1463 line 22 and 1464 line 2 2semi-hours" replace by half-hours

1463 line 24 "indicates alternatively" please clarify what this means, perhaps you had two analysers or you were switching between measuring modes within the same analyser

1464 line 4 "nearly all samples between 8 and 8:30 were excluded due to presence of dew". This exclusion is not appreciated in Figure 1, please clarify. Also it is not clear why you mention the exclusion between 8 to 8:30 when actually according to the leaf wetness sensors dew is present longer than that. See your own text of 1468 line 17-19. Please clarify.

1464 line 11 these division of periods is justified by the SWC change but it is not clear how are they justified by the meteorology. Please clarify

1464 lines22-23 there are around 14 days with high nightime values and rather than treating them as an exception it would be interesting to know the reason for that

1465 line 10 remove "hereafter"

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1465 line 13-16. I do not see the relevance of commenting on the energy fluxes an even showing a graph. Please, remove or clarify.

1465 line 25 onwards. It would be nice to mention exactly what is gained by performing such comparison. Also, perhaps an extra clarification is needed here that explains that sap flow measurements allows the calculation of plant-specific stomatal conductance. Fig 5 shows the largest discrepancy in the morning until about 10am. Please expand this point here. Is this reflecting the contribution of the other plant species or is it reflecting the fact that the canopy is not completely dry? This is treated by the authors in the discussion section with fig 10 and 11. I think no new result figures should be introduced in the discussion.

1466 line 10 the ")" after Fig 6 is missing

1467 line 6 would it be more correct to same "around a mean value"

1468lines 22-27 this paragraph sounds highly speculative, please rephrase. The obvious alternatives are, as already mentioned, missing contribution from other plants or wet canopy. The suspicion of hided stomatal opening would need here some references to back it up.

1471 line 5 correct "wais"

1471-1472 it is not clear if the halogenates and their reactions you refer throughout the paragraphs are in the air or in the surface films.

1472 line 20-23 and would this imply there is no role for the surface films?

1474 line 9-10 "this is what happened..in drawing figure 4" This sentence is confusing, please rephrase and also correct the figure number

Figures Please, harmonise the format of your graphs throughout. Use the same format to denote dates, remove unnecessary fraction digits from time units, add units to all axis, respect the subscripts and upperscrips of units and other symbols, enlarge the

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font of the axes labels and legends, respect the horizontal orientation of axes labels. Please, redo the panels to share the axes when possible, so that the minimum space is lost between figures.

fig 2 it is not clear whose standard deviations the bars refer to. It seems also Fstomdew has none. please clarify

fig 5 clarify whether it shows the average of the whole period or one representative day

fig 6 it is not clear whose standard deviations the bars refer to

fig 8 b, is it top or bottom? is it median or mean?

fig 9 clarify the units, are this means? how would the error bars look like?

Fig 12 i13 what period is included?

Interactive comment on Biogeosciences Discuss., 6, 1453, 2009.