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Comment

***Interactive comment on “Foliar photochemical processes and carbon metabolism under favourable and adverse winter conditions in a Mediterranean mixed forest, Catalonia (Spain)” by D. Sperlich et al.***

**Anonymous Referee #1**

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GENERAL COMMENTS

This paper reports measurements of several parameters related to photochemistry and C metabolism in leaves of three co-occurring Mediterranean woody species during winter. Results are compared between (i) three species (*Quercus ilex*, *Pinus halepensis* and *Arbutus unedo*), (ii) two sampling periods (“mild” and “frost”), and (iii) two positions in the tree crown (sunlit and shaded). All measurements were made in the laboratory, on twigs recently collected from the field. This paper adds important data to the available information on winter performance of Mediterranean evergreen species. However,

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it presents some significant flaws:

1. The two periods are not well defined: the authors are apparently comparing a “mild winter period” with a “frost/cold period” when it seems to me (from their own description) that they sampled a “frost period” and a “post-frost cold period”. As a consequence, no real “mild period” was assessed. This compromises interpretations based on the comparison between “mild winter” and “cold winter”.
2. Most of the Introduction focuses on the importance of different adaptive strategies and interspecific competition as determinants of plant community trends, particularly under climate and land use changes. However, the Discussion does not satisfactorily address these issues!
3. The paper is burdened with theoretical details concerning the measured parameters, which were many. In fact, only those parameters with the most relevant (and not redundant) results should be presented, and discussed in view of the primary aims of the study. I get the impression the authors lose themselves in a “forest” of parameters and data and miss the purpose of the whole study.

### SPECIFIC COMMENTS

The TITLE reflects the core contents of the work, but the word “favourable” should be checked/corrected (cf. item 1, in General Comments).

The ABSTRACT should indicate that measurements were made in the laboratory (on twigs collected from the field). The reference to the spring values should be removed since they were not obtained from the present study; the sentence in line 24-25 should also be removed because no results are presented for the weeks “after” the cold period (the cold period corresponding to 14-24 Feb, as described in section 2.3). Line 6-8: how does this relate (or not) with the outcompetition of *P. halepensis* by *Quercus* spp. you describe in section 2.2? How does this competitive disadvantage of *A. unedo* relate with the current forest trends and/or the predicted trends?

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The INTRODUCTION is too long and/or not adequate for the kind of measurements and results presented afterwards (or vice versa. . .).

MATERIALS AND METHODS is an excessively long (but incomplete!) section. Examples of missing information:

1. Although the reasons for studying *P. halepensis* and *Q. ilex* can be deduced from the description of stand history (2.2), the choice of *A. unedo* is not explained.
2. How many trees/species were sampled on each occasion? How many leaves or sets of needles were measured for each parameter (e.g., the means presented in the Figures correspond to how many leaves?). In other words, provide some information about the representativeness of your samples and measurements.
3. Why was 25 °C the selected temperature for measurements?

Sections 2.4-2.11 are unacceptably long in this sort of paper. Although most of the laboratory details could be important for the correct interpretation of the results, the degree of such detail is excessive in comparison with the little or no information provided about other aspects of the methodology (cf. paragraphs above). Formulae that are of general knowledge or have been proposed by other authors (e.g.  $F_v/F_m$ , gm) could be avoided. In fact, most of these sections could be presented as an appendix to the main paper.

RESULTS about shoot growth (pg 9715, ln 21-22) refer to the 3 studied species? Where and how was this radiation measured (pg 9715, ln 25), and what was its value during the mild period? Where is Fig. 5 mentioned, in the text? In 3.4, please rephrase “representing the health of a leaf” when referring to  $F_v/F_m$ . Why are values from shaded leaves during the “mild period” not shown for *P. halepensis* and *A. unedo*? The paper contains too many tables and figures; table 3 should certainly be removed. The mild and frost winter periods should be clearly indicated in Figs. 1-9 (and the indication of the sampling periods would also be welcome in Fig. 1).

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DISCUSSION needs shortening and focusing. Since no actual field measurements were performed (as far as I can understand from the present manuscript), it is not correct to imply that the present study combined both field and laboratory measurements (Pg 9719, In 23-25). Moreover, the contents of most of this same paragraph should be moved to Materials and Methods! Section 4.2 does not discuss the presented results. You did not show that leaf position has species-specific effects because you only showed the results for one species (Pg. 9726, In 15-18). What is an investment in life cycles (Pg. 9727, In 17)?

The REFERENCE list is too long.

#### TECHNICAL CORRECTIONS

Replace Treitach et al. by Tetriach et al. Pg. 9702, In 23 – indicate which century Pg 9716, Ins 19 and 22 – please check the grammar Pg 9718, In 9 – “most strongly pronounced” is awkward. Pg 9719, In 21 – “though” is not appropriate (therefore?) Figs. 10, 13, 14 and 15 -  $\mu\text{mol}$  and not  $\mu\text{mols}$  The whole text should be revised for minor corrections (grammar, missing words?, punctuation)

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