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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Eclipse effects on field crops and marine zooplankton: the 29 March 2006 Total Solar Eclipse" *by* G. Economou et al.

G. Economou et al.

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Response to Reviewers

We would like to thank the reviewers and the editor for their insightful comments that have really helped us to improve this final version. Their suggestions have been taken into account and all raised issues are answered one by one. Technical corrections are also implemented in the manuscript. Below is a point by point answer to the comments:

1. The two systems the authors analyzed are very different and there is no attempt to bridge them whatsoever. In other word there is no specific added value in discussing the two experiments together.

We agree with the reviewer that the two systems are discussed separately. However, within the scope of the Special issue which is more focused on atmospheric research we believe that a paper including two biota systems under the approach of "eclipse effects on ecosystems", it is valid and integrates the overall effects of the specific eclipse event on the environment, as the initial scope of the Special Issue. In any case, a special paragraph discussing the difficulties of associating the two systems has been added in the Introduction (last paragraph).

2. They noted that carbon uptake decreased sensibly. But it is not discussed whether the decrease was simply a response to reduced photon flux or was also conflicting with biochemical processes that should have run at a different rate at that time of the day and couldn't. The comments on page 8 of the manuscript suggest that the former looks more likely. The second part of section 3.1, which discusses this aspect, is too vague and a little inconclusive.

A more detailed treatment of the data is included in this version of the manuscript, in order to assess more effectively the pathways leading to the observed decrease in carbon uptake. Thus, the new Tables added (namely Tables 4 and 5) provide information on intercellular CO_2-concentration and the quantum yield for CO_2-uptake, respectively. The study and discussion of these new data gives more information on the mechanisms involved.

3. Once stated that there was no eclipse induced depression of CO_2 uptake, why to invoke possible endogenous controls that would depress photosynthesis, if the decrease of photon flux might fully explain that. I suggest plotting not only the absolute carbon uptake but also the carbon uptake normalized by irradiance to test whether this is true or not.

The implication of endogenous rhythms is restricted only in stomatal behaviour and is connected with the calculated rate of change in PAR during the eclipse, which was higher than that normally observed at dawn or dusk. Endogenous controls on photosynthesis are not invoked any more, in view of the findings mentioned in the previous point #2.

ACPD

8, S3634–S3640, 2008

Interactive Comment

4. It would also be interesting that authors compute the amount of Carbon that they think has been taken by the plants and figure out what could have been its fate.

It is not possible to speculate on the fate of the carbon assimilated during the eclipse, because there is no adequate experimental information. From this point of view, there is no reason for an assessment of the total carbon taken up by the crop plants.

5. It was also noted that the time derivative of irradiance during an eclipse is anomalous but it is not said if the authors detected some anomalous response in the plants.

The time resolution of the measurements on plants unfortunately did not allow us to investigate thoroughly any anomalies in plants response.

6. The authors also mentioned that, especially on the land, sharp decrease of light are frequent because of clouds, wind induced leaf movements, etc. Each varies in a specific frequency and amplitude range. Also dawn and dusk display a predictable rate of change in the illumination, which depends only on the latitude and time of year. I would suggest to compare the rate of change of irradiance due to the eclipse to those more "familiar" changes.

We have added the following paragraph in the text: "The rate of solar irradiance change during the eclipse has been calculated and compared to the corresponding rates during other frequent illumination changes such as dusk, dawn and clouds. This way eclipse conditions are put into context with more familiar illumination changes. Thus, during the eclipse, and excluding periods around first and last contact and near totality, the rate of irradiance change varies between 10-15 Watt/m2/min, within a period of about 2.5 hours. The corresponding rates during dusk and dawn were calculated to be in the range 1-3 Watt/m2/min. The situation is more complex for clouds depending on their type and thickness. Calculations on clouds appearing at the site the day after the eclipse, have shown rates of irradiance change varying between 5-30 Watt/m2/min with a median of 15 Watt/m2/min, but these changes are much quicker. Thus, during the eclipse the irradiance change rate appears to be much faster than during dusk

ACPD

8, S3634–S3640, 2008

Interactive Comment

and dawn, whereas clouds even though can disturb incoming light at similar rates, is not expected to be as sufficient due to shorter duration and the parallel effect of light scattering by atmospheric particles."

7. I also wonder why the authors did not start their observations before the dawn and continued them after the dusk. They rightly assumed that some responses mimic night-time behavior. It would have been better producing data on night time behaviour for both systems. I imagine that for the crops, being an experimental site, those data are probably available and might be used. For the marine site it is not straightforward to compare the observed behavior to another night behavior of a different system, because of the change in species composition, environmental conditions, history of the community, season, etc.

Unfortunately, such data are not available and we should take the comment into account in planning similar experiments in the future.

8. In fact the authors mention that not all the species reacted in the same way. What the author observed was a temporary crowding of animals in upper part of the water column. I wonder if they measured directly or indirectly if this changed their feeding response.

This experiment was a first approach to study the effects of a total solar eclipse on marine micro and mesozooplankton. Unfortunately, time and facilities on board were limited, so we did not have the opportunity to measure feeding response to the changing light regime. It would be an interesting point to study in similar experiments during future eclipses.

9. The authors state that there was no change in the vertical structure of water column, though the chlorophyll increased by ca. 30%. (.14 to .19 mg m-3). It is not clear which chlorophyll (the depth integrated average, the value at the maximum?) and it cannot be derived by the plots that show only fluorescence.

We stated that chl-a concentration, plotted versus depth (vertical distribution), varied from 0.14-0.19mg m3 and showed a small chlorophyll maximum between 40 and 60m. We agree that it was not clearly written (text has been modified accordingly) that no significant changes in chl-a values were observed during the eclipse event.

10. Do they believe that the displacement of grazers during the two hours might have increased the net growth of phytoplankton? Looking at their figure 4 it seems that after the eclipse there was a significant upward displacement of the DCM, the thermocline and the pycnocline. Vertical displacements are also detectable between the other profiles. They could either be due to advection or to internal waves. It does not come out in the discussion whether the authors considered this additional element in their analysis.

As mentionned before there was no change in chl-a values during the eclipse, so the point mentionned by the reviewer that there was an increased net growth of phytoplankton is not true. However, looking at fig.4, after the eclipse we can observe an upward displacement of the DCM, the thermocline and the pycnocline (K7). This is probably due to possible action of physical processes (wind) that we believe did not affect organisms'vertical distribution.

11. Did the author take into account the physical dynamics in the spatial redistribution of animals? At least microzooplankton could be plotted versus density instead that depth. For mesozooplankton, whose numbers are depth integrated, they could try a similar analysis using the values derived from the interpolating splines they plotted in figure 7.

As recommended, we took into account physical dynamics. Microzooplankton was plotted versus density instead of depth but nothing different appeared, as density profile did not change during the eclipse.

12. I would also check if the mass is conserved. This especially for mesozooplankton where the different distribution from sample K1 to K2 and from K2 to K4 are hardly

ACPD

8, S3634–S3640, 2008

explainable by a vertical displacement of the animals, unless one assumes that they migrated from below the 100 m depth horizon. Then, did the composition of the community change? Did they found species living deeper? In the introduction it was mentioned that even deep migrants may be affected by the surface light variation. We know, and the model simulation confirmed this, that light below 150 m is very-very low. Proving that even at such low level animals can detect directly or indirectly its variation, which could be biologically plausible, would be an important result. Is in their data any evidence for this?

It is true that mesozooplankton depth integrated biomass was not concerved during the different sampling efforts and especially samples K4 K5 and K6 showed an increased biomass. However, community composition did not really change and there was no evidence that species living deeper migrated upwards, excluding possible consequences on biomass from deep migrants (lack of deep species in the samples). Probably this is due to physical processes and different water masses.

13. As a side comment I note that the authors are right in saying that Hydrolight is a state-of-the-art tool to model underwater light field but I feel it was a little oversized for the specific application. In fact they used a fully resolved spectral model only to model the attenuation of total downwelling irradiance. A simpler model would have satisfactorily done the job, especially considering that they had to make some approximations on the concentration/presence of optically active materials.

We used the Hydrolight model to estimate underwater fields in this work because as the reviewer mentions, it is a state-of-the-art tool, and also because of our previous experience with this model and the extensive testing (i.e. detailed optical closure experiments) we performed as part of our previous studies. Based on these studies we feel that this model provides accurate estimates of underwater light fields in waters of varying levels of optical complexity. Use of this model allowed us to include effects of inelastic scattering (i.e. both chlorophyll and CDOM fluorescence) and all orders of multiple scattering in our PAR calculations. Moreover, as part of this study, in addition to the results presented in this paper,we used Hydrolight to estimate a number of other optical quantities (e.g. profiles of underwater down-welling and up-welling radiances, as well as water leaving radiance and remote sensing reflectance spectra), which were not to include in the submitted manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 1291, 2008.

ACPD

8, S3634–S3640, 2008

Interactive Comment