Atmos. Chem. Phys. Discuss., 8, S571–S575, 2008
www.atmos-chem-phys-discuss.net/8/S571/2008/
© Author(s) 2008. This work is distributed under
the Creative Commons Attribute 3.0 License.

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.

Anonymous Referee #2

Received and published: 6 March 2008

General comments The paper reports and discusses the observations carried out during the total solar eclipse occurred in March 29, 2006 on biota response to the unexpected progressive light decrease-increase causes by an eclipse. The organisms monitored were terrestrial plants (cereals and legumes) in an experimental Greek field and microzooplankton and mesozooplankton in the surface layers of an oligotrophic site of Eastern Mediterranean sea. The most evident responses were a decrease in carbon assimilation rates by plants, though not due to stomatal closure, and a redistribution of both micro- and meso-zooplanktonic organisms in the water column with an apparent upward displacement during the darkening phase for many species and stages.

I believe that the questions stimulating the study are relevant, as mentioned by the

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

authors in the introduction. In particular, given that the light plays the role of both source energy ('photoenergetic') and source of information ('photoregulatory') for the plant an eclipse is a very good 'natural experiment' to discern among them. A similar argument holds for heterotrophic plankton, for which the light plays certainly the role of being a source of information. Though, I am not convinced that the experimental setup was optimal for addressing the above issues, that the responses shown by the data are significant to the extent the authors claim, that they fully discussed their results and that they improved our knowledge on the processes they studied. Before giving a detailed account on the above comments, I just want to note that 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.

Specific Comments

Considering the light as a source of information for plants, it is well established that the change of light at dawn and dusk are felt by higher plants also as change in band ratio in the red/near-infrared region by a suite of phytochrome photoreceptors. Because a change in band ratio during an eclipse is unlikely, one would expect that some of the responses occurring at dawn or dusk would not manifest during an eclipse. In addition to this, one would expect that physiological responses regulated also by the circadian rhythm of the plant would not manifest either, or would be disturbed by the unexpected (from the point of view of the circadian rhythm) strong decrease of light. The authors noted that stomatal conductivity did not respond to fluctuation of light due to the eclipse, probably following an internal rhythm. They noted that carbon uptake decreased sensibly. But it is not disussed 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. Once stated that there was no eclipse induced depression of CO2 uptake, why to invoke possible

ACPD
8, S571–S575, 2008

CC BY

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. 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 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 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. 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 behavior 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. 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. 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. 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

ACPD

8, S571–S575, 2008

Interactive Comment

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. 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. 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 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? 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.

Technical corrections

The plot on relative humidity is not needed since it covaries with the air temperature and no analysis is carried out on its impact

Perez and Dolan (1995) (pag 11) is not listed in the References. It is probably Perez et al (2000)

ACPD

8, S571–S575, 2008

Schulze and Hall (1982) is not listed in the references, on the other hand there is Schulze (1994) never cited

Sherman and Honey (1972 (pag. 12) is probably Sherman and Honey (1970)

Sathaiah et al. 1994 is never cited in the text

Vecchione et al. (1987) is listed two times

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