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Rejoinder to the reviewer's comment on 'Bioenergetics, Thermodynamics and Plant Physiology'

As the author of the book, I think it would be prudent to clarify some of the mistaken notions about the book¹. The primary objective of the book is to establish the physical principles behind the plants' physiological transformations. In this connection it is to be mentioned that thermodynamics govern the basic principles of plant physiology. From this viewpoint, I am therefore giving a rejoinder for the review.

The objection regarding the interpretation of the energy transactions primarily in terms of entropy is not tenable here. It has never been interpreted as the energy transaction in terms of entropy. Rather, in chapter 2 of this book, the First Principle of thermodynamics on work and energy transformation has been dealt with; in chapter 3, the Second Law of thermodynamics on entropy and plant physiological processes has been included, and in chapter 4, plant metabolism in terms of entropy and free energy has been elaborated. The role of free energy has been explicitly described in the processes of energy transformation in

plant functioning. It is to be mentioned that free energy deals with the system only, while entropy deals with both the system and its surroundings. In addition, it has also been described that the transformation of a system from 'disorder' to 'order' cannot be explained by the change in entropy with the present perception about the entropy. It is explained in terms of the Law of Maximum Entropy enunciated by Rod Swenson in 1988, in which he stated that 'a system will select the path or assemblage of paths out of available paths that minimizes the potential or maximizes the entropy at the fastest rate given the constraints'. In my opinion, the basic principles written in the book have been largely overlooked by the reviewer.

The review reads 'The parallel to the Bible runs through and we understand that to begin with, all seas were red due to halobacteria...'. Nowhere in this book, has it been mentioned that the early sea was 'Red'. To the contrary, the relevant chapter (chapter 9) has emphasized the probability of the early sea being purple

due to abundance of Halobacterium with bacteriorhodopsin.

The 'sea was not saline to begin with' as mentioned in the review needs re-checking. Many accepted estimates of the early ocean's salinity provide that it ranged between 1.2 and 2 times present-day salinity.

The comment 'The author may not be familiar with the accepted notations like uniport, symport, etc.' is undesirable. Perhaps the reviewer did not go through the book properly. Any reader may refer to chapter 8 of the book in general and figure 8.9 in particular to get an idea of these proteins in ion transport.

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