

Commentary

A comment on: 'Effect of starvation on glucose transport and membrane fluidity in rat intestinal epithelial cells', by: P.D. Gupta and A.A. Wahead (FEBS Letters, 300: 263–267)

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General

In the introduction the authors indicate that no information is available on the structural and physiological aspects of the MV membrane during starvation and re-feed conditions. The authors cite in the 'Discussion' some papers (ref. 20–22); I do not understand why the authors do not refer to the 'classical' papers of Jared M. Diamond and colleagues, from which they could have obtained a significant input into their experimental strategy, interpretation of data as well as on their conclusions.

Furthermore, the influence of starvation on intestinal D-glucose transport has been studied earlier by many different groups (e.g. G. Esposito/Faelli; E. Scharrer; U. Hopfer; C.I. Cheesman; G. Wiseman; D.H. Smyth; R.J. Levin and others). From a detailed study of the literature the authors might have learned, that their selected experimental protocol might not have been an 'ideal' one because complete food removal for periods of up to seven/eight days might be rather extreme ('non-physiological') and involve a large panel of 'secondary' effects which make any conclusions difficult.

Experimental conditions

The authors describe the methods to isolate brush border membrane vesicles. They also describe a method to measure D-glucose transport, consisting of the incubation of inverted intestines in Hank's buffer containing radiolabelled glucose. Transport was apparently measured in the presence and absence of sodium, in the presence and absence of ethanol (rationale?) and at various temperatures. A method for vesicular D-glucose transport is not given!

Results

With respect to the morphology of intestinal epithelia, the authors might consult the review by Karasov and Diamond (Am. J. Physiol. 245: G445–G462) and offer an explanation for the differences in their findings as compared to the results of different groups! In the

second paragraph of results the authors talk on the EM structure of isolated vesicles without providing data: What is new and specific to the topic of the paper? With respect to the analysis of the membrane fluidity, the authors do not present the primary data; thus, it is impossible to assess the accuracy/validity of these data. The methods used to obtain the transport data given in Table II are not explained (vesicular transport; Na-gradient dependent (?); initial linear uptake or some other time-point (?)). It is just impossible to follow the conclusions given by the authors (especially on re-feeding) without additional information (including appropriate statistics). In Fig. 4, it is not clear, how this experiment was done. Is transport with and without (contamination!) sodium by the same pathway; the use of phlorizin under both conditions might have given an answer. Do the authors have some idea about the sodium-independent pathway?

Conclusions

I completely disagree with the author's statement, that the present study sheds some light on the mechanisms of higher uptake of glucose during starvation conditions. My only conclusion is that this study has the potential of adding confusion. However, it must be realized, that the quality of this study should not represent a basis for creating 'confusion': The experimental design, the quality of the data and especially also the knowledge of the specific literature is just too inappropriate! I am sorry that this study has been published in a journal with the scientific standards of FEBS Letters.

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