

red and pink shades. The country rock depending on its iron content weathers cream, brown, or pink. Within the bioturbated sediment and sometimes in the country rock are pools of authigenic quartz such as may be formed when silicates are breaking down under biogenic attack or during weathering and are releasing large amounts of silicic acid. These are the 'quartz grains' referred to by Cloud.

If the burrows were made by termites, why did those making type A (ref. 2) burrows live only above the 2 m of strata at the base of the Orebody Member whilst the succeeding 2 m are characterised only by those making type B burrows²? And why is the unit containing the burrows overlain by hundreds of metres of identical lithologies which show no signs of bioturbation. I believe that the burrows are not those of termites nor are they lithologically controlled, but they are paleofacies-controlled. The transition type A to type B burrows reflects the transition from intertidal to lagoonal/lacustrine sedimentation, as indicated by facies studies³.

Total de-stratification, such as indicated by my Fig. 2 (ref. 2), produces a swirled fabric in the rock; this is explained only by invoking contemporaneous sedimentation and bioturbation. For while small tunnels of termite origin are at least conceivable, complete fluid mixing over a thickness of several centimetres is not. This fabric implies that the medium undergoing bioturbation was granular and wet.

The idea that type B burrows represent covered passageways is not feasible. The burrows are an integral part of the rock, are not confined to exposed bedding surfaces and can be revealed by excavating the strata. They have pellet-formed walls which may show disintegration at the burrow termination into equidimensional pellets. Again these are part of the rock and not surficial features.

As far as it is possible to tell, the burrows (unequivocally metazoan according to Cloud) were formed contemporaneously with the sedimentation of the containing units about 1,000 Myr ago.

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No paradox in the control of energy intake

As Rothwell and Stock¹ note their results refute the notion that a particular size of energy store precisely controls energy

intake in rats. Other reports²⁻⁴ agree with this. However, contrary to what is claimed, their results do not necessarily reveal a paradox or physiological enigma in the control of energy intake in the rat. They are clearly compatible with the theories of Kleiber⁵, Adolph⁶, Ugolev⁷ and Booth⁸, in which energy metabolism controls feeding and the energy store is only one component in the feedback system. Rothwell and Stock state that our model⁹ has yet to include effects of body energy increments. On the contrary, in a publication which they cite¹⁰, and elsewhere¹¹, we have included a representation of effects of energy store size on intake through energy metabolism. We also emphasised that although the energy store may settle at a particular size¹¹⁻¹³, there is no privileged size either achieved or used as a homeostatic set-point. In these simulations the precision of defence and (if a specific size is maintained) the value defended depends on a compromise between the small but persistent graded effect of store size on metabolism and all the other dietary and endocrine conditions affecting metabolism.

Rothwell and Stock also state that it is difficult to conceive how such a model could explain an increase in fat deposition rather than a decrease in intake when energy supply is in excess of requirements. However, our model probably provides several ways of increasing efficiency without altering intake. For example, a change in regimen might divert a higher proportion of absorbed energy into fat, while also reducing the faecal excretion of energy or the thermic effect of feeding. All these effects would be liable to increase the size of the energy store in the model. The extra metabolism from the increased retention of energy intake would help reduce intake, and the extra diversion after absorption would help to increase it.

The importance of Rothwell and Stock's data lies in the questions raised as to the exact mechanism(s) by which dietary changes can alter the relations among the processes of utilisation, storage and intake. Quantitative simulation is of similar importance.

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ROTHWELL AND STOCK REPLY—We apologise to Booth and Toates if we have neglected those aspects of their feeding model that take into account energy storage. Nevertheless, we were well aware (and said so) that the increased fat deposition of our animals results from an increased metabolic efficiency (almost certainly due to a decreased thermic effect of feeding), thus providing a larger fraction of total ingested energy available to metabolism. According to Booth and Toates, this excess energy would either reduce total energy intake or, if diverted to fat storage, help to increase intake. In fact, we found that total energy intake remained at precisely the same level as that in the free-feeding control animals.

We would also suggest that an energostatic approach fails to account for the differences between the two types of energy load used in our experiments, that is, fat and Complan. If one accepts Booth's proposal¹ that it is the availability of energy that determines intake, it is logical to assume that isoenergetic stomach loads will have similar effects on voluntary energy intake. In our experiments, however, rats tube-fed 34% of normal intake precisely compensate for the load when it is a balanced nutrient mixture, but over-eat by 15-16% when the load is comprised of fat. This suggests that an energostatic approach may be too simplistic and, since our original letter was published, Savory has written to us supporting this view. He obtained very similar results to ours by feeding quail with diets of varying energy density², and one of his conclusions from this work was that "... differences in energy expenditure were not balanced by differences in nutrient intake, so perhaps the quail were regulating food intake to meet their requirements for some dietary component other than energy".

There are many attractive aspects of the energostatic model of Booth and Toates, and we regret that they did not take advantage of their computer simulation to substantiate the claim that our results are compatible with their feeding model. We hope that they will do so and if necessary, as we suspect it will be, make appropriate alterations.

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