

Letters to the Editors

Is diet-induced thermogenesis an experimental artefact?

The concept of diet-induced thermogenesis (DIT) really does seem to provoke some respectable scientists into making the most ill-considered and misleading statements. In a recent paper (Barr & McCracken, 1984), the authors attempted to repeat one of our experiments, using similar methods, and strain and age of rat. The fact that they did not obtain the same results may have been due to several differences in protocol and they recognized the need to conduct further studies to determine the factors responsible for interlaboratory differences. Nevertheless, they then went on to conclude at the end of the Discussion: 'It seems probable, therefore, that the results of Rothwell & Stock (1979), Andrews & Donne (1982) and Trayhurn *et al.* (1982) arise from one or more experimental artefacts and are unlikely to be of relevance to the study of human obesity or to the efficiency of energy utilization of farm animals'.

We consider this *ex cathedra* statement completely unwarranted, and certainly not substantiated by anything approaching a scientific discussion. Even if we disregard their assumption that only their experiments are free from experimental artefact, we would still like to know why they did not justify this statement, and why they completely failed to discuss or refer to other published energy balance studies. We would like to make the following points.

(1) We (Rothwell & Stock, 1982*a*) have shown large inter-strain differences in DIT and energetic efficiency in cafeteria-fed rats that depended on genetic strain, but were independent of the experimental methods employed (identical for all four strains studied). For some rats, the effects of the high-fat cafeteria diet were similar to those observed by Barr & McCracken, but for others they were entirely consistent with our earlier and subsequent observations of DIT. Presumably, the putative artefacts invoked by Barr & McCracken only apply to those strains exhibiting DIT; a novel dualistic approach. In other experiments (Rothwell & Stock, 1982*b*) it was found that young rats responded to cafeteria feeding with a 41% decrease in net efficiency, whereas there was no significant change in older animals on the same diet, and they rapidly gained body fat. Likewise, the paper by Trayhurn *et al.* (1982) unequivocally demonstrated high levels of DIT and decreases in efficiency in cafeteria-fed lean mice, but large increases in efficiency and body fat (as Barr & McCracken would predict) in their genetically obese siblings. Once again, it would require some form of intellectual schizophrenia to accept half the results and reject the other half.

(2) Very careful consideration has been given to the possibility of experimental error contributing to our observations of DIT, and in two substantial papers devoted to this problem (Rothwell & Stock, 1982*b, c*) we found errors of less than 3% on our estimates of energy intake, gain and expenditure, while demonstrating increases in heat production of 45–77% in the cafeteria groups. In a later study (Rothwell & Stock, 1982*d*), there was an 84% increase in heat production in very young rats given the cafeteria diet, but even more remarkable was the lower energetic efficiency (24% below stock-fed controls) observed when the cafeteria-fed animals were returned to the normal stock diet. Thus the metabolic adaptations to the cafeteria diet were still detectable when the animals were eating the same amount of the same diet as controls. What 'experimental artefacts' could be invoked to explain that observation?

(3) Perhaps rat cafeteria experiments have no relevance to human obesity or farm animal nutrition, but an experimental paper is hardly the place to make such far-reaching pronouncements, particularly on the basis of one contradictory study. Other workers appear to find these experiments of value in trying to elucidate the metabolic basis and treatment of obesity and, in the light of recent dietary guidelines for the UK, now might be the time to reintroduce the DIT and leanness that generations of farmers have bred-out of our farm animals.

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Reply to letter by Rothwell & Stock

We are grateful to have been given the opportunity to take part in this historic first exchange of correspondence in the new section on 'Letters to the Editors'. However, we are surprised that our paper has caused such a vehement attack on our scientific impartiality and judgement. We welcome the opportunity to set the record straight.

Contrary to the impression given in the opening paragraphs of the Rothwell & Stock letter, we did not set out to re-create the universe on the basis of one preliminary experiment. We would be quick to criticize such a course as being scientifically unsound and morally reprehensible. On the contrary, we were attempting to put our modest contribution and those of Rothwell & Stock (1979), Andrews & Donne (1982) and Trayhurn *et al.* (1982) into the perspective of 100 years of scientific research on farm and laboratory animals, and to make a balanced though tentative assessment of the probable importance of the DIT phenomenon. This we feel to be a valid and worthy objective of a scientific publication. Indeed we are puzzled that the authors of the claim 'we have now developed a new dietary regimen that induces voluntary overeating of a balanced diet in normal animals and unequivocally demonstrates the quantitative importance of DIT in energy metabolism' (Rothwell & Stock, 1979) should now consider that 'an experimental paper is hardly the place to make such far-reaching pronouncements'.

It would appear that some confusion has arisen over our use of the term 'experimental artefact'. Though it is unfortunately true that there were several discrepancies in the Rothwell & Stock (1979) paper, some of which have been discussed by Hervey & Tobin (1982), we (Barr & McCracken, 1984) dismissed the possibility that the reported differences could be solely attributable to experimental error. The experimental artefacts to which we were referring were primarily those fundamental criticisms of the cafeteria-feeding system which we had discussed at some length, namely individual variability in the balance of nutrients and the likelihood of complex dietary deficiencies which could vary in extent between and even within experiments.

It seemed to us a reasonable if somewhat elementary hypothesis that such dietary imbalances varying in nature and extent would explain the variability in response from one laboratory to another, between experiments and even between strains of rat in the same experiment. This hypothesis would inevitably undermine the relevance of such experiments to human and animal nutrition, since the primary aim of the nutritionist must surely be to define balanced diets in the interests of health and longevity in the human and these, coupled with efficient production, in farm animals. We are surprised that such elementary logic should be castigated as 'ill-considered and misguided'.

In our paper (Barr & McCracken, 1984) we did not discuss in detail the other published energy balance studies referred to in the Rothwell & Stock letter simply because they were conducted on younger animals and we wished to save such discussion for more relevant publications, still in the press. Obviously it would be impossible to enter into a detailed discussion in this letter, but since we have already reported some of the relevant information in abstract form (McCracken & Barr, 1982, 1983; Barr & McCracken, 1983; Barr *et al.* 1983) it is probably appropriate to point out that we have confirmed the variability of nutrient intake by young cafeteria-fed animals and the complex dietary deficiencies which can arise. Regretfully, we have been unable to find convincing evidence for the presence of the elusive DIT phenomenon in any of our experiments although we have demonstrated marginal improvements in nitrogen and energy utilization in response to mineral/vitamin enrichment.

It is obviously of more than academic interest that we should attempt to bring this unfortunate controversy to a satisfactory conclusion. We appeal to all those using cafeteria feeding or other similar methods of inducing hyperphagia to attempt to define all the experimental variables and to keep an open mind about the existence of experimental artefacts which could be responsible for the differences reported.

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