Correspondence

Ischaemic Heart Disease

SIR—I read with great interest the two papers of Szanto & Yudkin (*Postgrad. med. J.*, 1969, 45, 602) and Yudkin, Szanto & Kakkar (*Postgrad. med. J.*, 1969, 45, 608) in your September issue. The main reason for the interest is, of course, the deep importance of any work which may be related to the cause of ischaemic heart disease. Professor Yudkin has already popularized the idea that social affluence is associated with an ever-increasing consumption of sucrose at the expense of starch. He has also popularized the idea that there may be a causal relationship between sucrose intake and ischaemic heart disease, quite apart from the validity of any association between the two. This is a much bigger step which may lead us astray. However, we cannot discount it until further evidence is forthcoming. A recent population survey (Welborn, Stenhouse, Curnow & Johnstone, in press) suggests that, in addition to cigarette-smoking, obesity, age, blood-pressure and diabetes, a brisk insulin response to an oral glucose load at an early age is associated with a high incidence of ischaemic heart disease. Since obesity, maturity-onset diabetes and hypertension predispose to hyperinsulinism, it seems reasonable to postulate that hyperinsulinism and ischaemic heart disease are associated.

It is, therefore, tempting, intellectually, to accept the concept of 'sucrose-induced hyperinsulinism' (pp. 602–607) and to indulge the authors of the second paper (pp. 608–611) when they report a significantly positive correlation between sucrose intake and plasma insulin concentration in patients with peripheral vascular disease. But, unfortunately, it is impossible to feel convinced by their data. Nineteen men (average age 28) were asked to take a diet for 2 weeks which was high in sucrose (440 g/day). 'As far as possible' the consumption of starch was reduced to maintain the same calorie intake. The insulin response to 50 g of glucose given orally was significantly higher in six of these nineteen young men as compared with the other thirteen, and also with a 14-day period on low sucrose (10 g/day). These same six were also the only ones to gain weight significantly (mean increase + 2.1 kg). The authors imply that the frequency distribution of their population is bimodal, but the data available is inadequate to establish this. They also seem to imply that the excessive weight gain relates to a tendency to obesity which, in turn, predisposes to ischaemic heart disease. Another, perhaps more realistic, interpretation (which they do not discuss) is the possibility that the reduction of starch during the high sucrose period was not carried out as conscientiously in six of their subjects as in the others, so leading to a higher carbohydrate intake, weight gain and, hence, insulin hyper-responsiveness. Even if the authors can reassure their readers convincingly that adequate precautions were taken to avoid this source of error, it is a pity that they do not analyse their results by the paired *t*-test. This would give a more valid indication of the difference of response between the groups. Since this difference is precisely the point they wish to emphasize, the absence of individual data, either in numerical or graphic form, is surprising.

In the second paper the conclusions are even more panoramic. People attending the Medical Centre of the Institute of Directors did not show a significant correlation between sucrose intake (assessed historically) and the insulin concentration 30 min after 50 g glucose orally, whereas patients attending the peripheral vascular clinic at King's College Hospital did so. The groups were admittedly not comparable with regard to age, occupation, economic 'class', etc., but nonetheless the authors claim that correlations are valid. There are so many factors, apart from sucrose intake, which may influence the insulin concentration 30 min after a glucose load (at best, a fickle measurement) that the biological as opposed to the statistical validity of a 'significant' correlation must be seriously questioned.

Sometimes the strength of a good idea can be convincing despite inadequate data but, at other times the practical importance of the problem demands such rigid scientific control that an exceptionally high standard of 'trouble-shooting' is mandatory. Such is the case with the problem of ischaemic heart disease.

The Middlesex Hospital Medical School,
London, W.1

J. D. H. SLATER

We have shown this letter to Professor Yudkin and his colleagues who reply as follows:

SIR—We are glad that Dr Slater is sufficiently interested in our paper to wish to see all the details of our findings. Nevertheless, not all of Dr Slater's queries need this further information. For example, there is no question of the results being due to a difference in the conscientiousness with which six of our subjects followed their diets, compared with the other thirteen. As the text states, the subjects had to follow general dietary instructions and record the amounts of the food they ate. We ourselves then calculated the intake of the different items with our computer program. The results we give showed that there was no difference between the diet of these six subjects and those of the remaining thirteen, either before or during the experiment. We cannot see how the subjects themselves could, unconsciously or consciously, have manipulated their dietary records so as to have produced these results.

We certainly believe that our population of nineteen men fell into two distinct groups ('bimodal', to use Dr Slater's term) in respect to weight changes, insulin response and platelet adhesiveness. In all these characteristics, there was no overlap between the values for the sub-group of thirteen subjects and the sub-group of six subjects. Thus, as Table 5 shows, the high-sugar diet resulted in a range of weight increase in the thirteen subjects from 0 to 0.9 kg and in the other subjects from 1.4 to 2.7 kg. As regards insulin levels, there was no difference in these two sub-groups in for example resting levels before the high-sugar diet; after the diet, the ranges were 17–27 units for the thirteen subjects and 32–48 units for the six subjects. A similar clear distinction between the two groups was seen for insulin levels at 30 and 120 min after the oral glucose test, and also for platelet adhesiveness.

We feel very strongly that statistical tests based on normal and related distributions (such as the paired *t*-test which Dr Slater suggests) are not the most appropriate when one is dealing with relatively small samples. Bradley (1968) effectively destroys some of the myths surrounding the indiscriminate use of classical statistics. We have used the non-parametric statistical method of Mann & Whitney (1947) which we considered more appropriate.

As to our second paper, may we recall our argument? It was that one deduction from our hypothesis, that sucrose-induced hyperinsulinism is an indication of proneness to coronary disease, was that 'healthy' subjects should show no correlation between habitual sucrose intake and insulin levels, whilst atherosclerotic subjects should show such a correlation. We then said that, in two such groups to which we had access, this prediction was verified. We spelled out that there was the possibility that these differences might have been due to differences in class and occupation, but gave as our opinion that this was less plausible than a difference resting on our hypothesis. Dr Slater is of course quite entitled to his own opinion as to the relative plausibilities of these two explanations.

Department of Nutrition,
Queen Elizabeth College,
University of London

JOHN YUDKIN
STEPHEN SZANTO AND
RALPH WATSON