

measurements which take 8 min. to collect for each child, longer in fact than is spent on each child in the routine school medical examinations in this country (Stuart & Meredith, 1946; Meredith & Stuart, 1947). If any set of measurements can provide what is asked of them, then the time spent collecting them would indeed be time well spent.

REFERENCES

- Meredith, H. V. & Stuart, H. C. (1947). *Amer. J. publ. Hlth*, **37**, 1435.
 Stuart, H. C. & Meredith, H. V. (1946). *Amer. J. publ. Hlth*, **36**, 1365.
 Yudkin, J. (1948). *Brit. J. Nutrit.* **2**, 158.

Summing up

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These comments are based on a study of papers submitted to me by the authors. The papers, on the authors' admission, were not in final form, and it is possible that substantial alterations may have been made in the meantime. In this event some of my comments may appear pointless or irrelevant, but I hope not impertinent. If, perchance, I have committed any of these faults, I apologize.

Marrack (1948) rightly points out that laboratory data have the great advantage that they are quantitative and free from personal errors or bias, but he is evidently not certain whether all such data are reliable or that their meaning is clear.

He compares the results of haemoglobin determinations during the war with pre-war values. His comparisons show that on the whole from 1943 there has been no falling off in haemoglobin levels. Indeed, there was an improvement in women. He asks why agricultural workers and others engaged in strenuous pursuits had lower values than sedentary workers such as civil servants. I asked the same question 2 years ago (Magee, 1946). My colleagues and I are at present carrying out investigations on this matter.

Marrack's review of the vitamin A data shows that some observers during the war found quite low values, under 70 i.u./100 ml., in blood, and low values also in the livers of people dying from diseases in which the liver was not involved. On the other hand, he himself and others found surprisingly high values in the blood and also in the liver. He quotes dark-adaptation tests, but concludes that the data are insufficient to throw any precise light on the state of nutrition in regard to vitamin A.

He deplores the paucity of data on blood phosphatase which is a sensitive index of the state of metabolism of calcium and phosphorus.

He refers also to the survey of Kon & Mawson (1947) which shows that the vitamin A content of the milk of nursing mothers did not vary with the amount eaten in the food. On the other hand, the vitamin C and vitamin B₁ contents varied with the amounts of these vitamins in the diet. An interesting finding is that the level of vitamin B₁ rose significantly in 1942 after the introduction of 85% extraction flour.

Yudkin (1948) in his paper is less concerned with the results of anthropometric and performance tests in determining nutritional status than with the appraisal of the

methods used. It would have been more in conformity with the title of the conference if the order of emphasis had been reversed. He discusses the significance of anthropometric and performance data mainly to indicate what they do not, rather than what they do, signify. It is not the first time that this line of argument has been taken. I think it would have been of greater interest if he had compared data obtained during and since the war with pre-war findings.

On performance tests he stresses the importance of controlling psychological factors and mentions incentive as one. This is probably the same thing, or almost the same thing, as what the Americans call motivation, but psychologists might not agree. By control he presumably means elimination as far as possible. But do we really want to eliminate motivation in dynamometer and endurance tests? These are to a great extent a measure of what is commonly called 'guts', i.e. will-power and determination, as well as of neuro-muscular efficiency. It is will-power which sets the neuro-muscular apparatus in motion and a test, to be of any value, ought, as I see it, to measure both. What we really want to find out is what the individual is capable of; this is what is important. We know, too, that the malnourished person is not as fit to perform muscular work as the well-nourished person. For instance, the findings obtained from undernourished people in Europe and elsewhere have shown clearly that lethargy, i.e. lack of guts or will-power or motivation, is an early and prominent symptom of insufficient food. Anyhow, the only way to eliminate guts or will-power or motivation is to decerebrate the person. I do not, however, accuse Prof. Yudkin of wanting to go as far as this.

Sinclair (1948), like Yudkin, has paid scant attention to the general title of the conference. He has dealt with methods and general principles, and criticizes in some detail publications by my colleagues and myself. He deplors the paucity of recent nutritional survey data but almost ignores those of the Ministry of Health except where they have provided an opportunity for criticism, and he makes no mention at all of the results of his own surveys carried out in this country since 1941, results which, I am sure, would have made an important contribution to this conference. Indeed, I venture to think that they would have been welcomed by many. I must confess to a personal feeling of disappointment at this important omission.

Sinclair's criticism of the papers of myself and my colleagues would seem to have arisen out of a series of misunderstandings which I feel I must correct. In the first place there is no essential difference between the procedure of Sydenstricker and of his successors in the clinical surveys of the Ministry of Health. Sydenstricker lighted the torch, and the same lighted torch was carried in succession by Stannus and Hawes, by Adock and Fitzgerald, and later by Milligan and Townsend. Sydenstricker's task was to ascertain whether rationing was adversely affecting the nutritional state of the people. We did not want him to ascertain the effects of past nutritional conditions or of heredity. Had he insisted on doing this we should have told him to go home. He, in fact, determined to the best of his ability the existing nutritional state of samples of the population. I say 'to the best of his ability' deliberately, for the imperfections in our present methods are well known and have been discussed many times in the past, I must confess with little profit. As I see it, Dr Sinclair's discussion of the subject has

been equally profitless; for the procedure which he advocates in his paper is quite impracticable for departments of public health, local or central. To suggest, as he has done, a new name, nutriture, for nutritional state does not take us anywhere. Moreover, it is somewhat tiresome to pronounce.

The paper by Adcock, Hammond & Magee (1947) is based on all the survey data available at the time the paper was written and includes those by Sydenstricker, Hawes and Stannus, as well as those of Adcock and Fitzgerald. Dr Sinclair's paper gives the impression that Sydenstricker's observations were not included; they were. One objective of the statistical analysis was to find out whether there was any correlation between certain signs such as folliculosis and the nutritional state. We found none. The other objective was to find what relationship existed between certain clinical signs, such as posture, and the nutritional state, signs which are normally taken into account by the physician but not always recorded. We found a high degree of correlation. In order to avoid confusion we attached special labels to certain terms and we made use of current statistical terminology. Sinclair has made the mistake of taking these terms out of their statistical setting and of attaching to them a clinical connotation. This has led him to imagine, for instance, that the Ministry's clinicians could miss a case of scurvy because it did not show signs such as poor posture or poor musculature.

Finally, I would stress that clinical medicine is partly art as well as science. We can express some of it in words and figures, but not all of it; we can learn some of it from books and lectures, but not all of it. The part of it that cannot be put into speech or writing is the main cause of controversy. The physician with much experience is generally more competent than the physician with little, and the physician who has had much experience of deficiency diseases is more competent for nutritional work than the physician who has not. The physicians whom the Ministry of Health selected for its clinical surveys are men with such experience.

REFERENCES

- Adcock, E. W., Hammond, W. H. & Magee, H. E. (1947). *J. Hyg., Camb.*, **45**, 65.
Kon, S. K. & Mawson, E. H. (1947). *Proc. R. Soc. Med.* **40**, 900.
Magee, H. E. (1946). *Brit. med. J.* **i**, 475.
Marrack, J. R. (1948). *Brit. J. Nutrit.* **2**, 147.
Sinclair, H. M. (1948). *Brit. J. Nutrit.* **2**, 161.
Yudkin, J. (1948). *Brit. J. Nutrit.* **2**, 158.