Silverio, J. (1964). *Am. J. med. Sci.* **247**, 732.
Smallpeice, V. & Davies, P. A. (1964). *Lancet* ii, 1349.
Stearns, G. (1956). In *Infant Metabolism.* [I. H. Scheinberg, editor.] New York: The Macmillan Company.
Vahlquist, B. (1958). *Adv. Pediat.* **10**, 305.
Wennberg, R. P. Schwartz, R. & Sweet, A. Y. (1966). *J. Pediat.* **68**, 860.
Widdowson, E. M. (1963). *Proc. Nutr. Soc.* **22**, 121.
Widdowson, E. M., Slater, J. E., Harrison, G. E. & Sutton, A. (1960). *Lancet* ii, 941.

The Symposium concluded with a general discussion, opened as follows by Professor J. P. M. Tizard, MA, BM, BCh, FRCP, Institute of Child Health, Hammersmith Hospital, London, W12

The other guests and I are most grateful for being invited to this meeting of the Nutrition Society. We have been very impressed not only with the papers but by the standard of discussion; just because the discussion of individual papers has been so good, my task is all the harder, and I imagine that what is really wanted now are some provocative and speculative remarks.

Although our knowledge of the nutrition of the foetus and newborn could be so rich that we could afford to look at the subject from, shall I say, oblique angles, I suspect that it would be nearer the truth to say that we know so little directly about the subject that we are forced to glean what information we can from studies designed originally for another purpose. Perhaps we should consider what direct observations are possible and practical in the human and other species, and which one would like made soon; and we should consider the ultimate purpose of such studies. Of course, the truth in itself is part of the ultimate purpose, but is there a more utilitarian part? In the case of the human being great stress has, I think rightly, been laid on ultimate intelligence, and as Dr Davies has pointed out, quantity in the shape of a human infant, may not necessarily mean quality. Clearly one wants a good many different kinds of human being, but the Aldous Huxleyan view of the future, in which there would be some point in breeding a race of morons, has become less credible; he could hardly have been expected to have anticipated that to dig a hole in the ground would in the year 1968 demand a considerable technical knowledge of how to manipulate a large and complicated machine—a technical knowledge that could not be acquired by people with IQ's of less than, perhaps, 100. As the problem of how to employ those of low IQ will surely be one of the major world problems in the future, anything directed towards increasing the total level of intelligence in a population is well worth considering. With animals of course one is looking for quite simple attributes, like milk yield; but is it important for a race-horse, for example, to have a high IQ?—and is this looked for when it comes to breeding and to questions of early environment?

Turning now to the individual papers, I am prompted by Dr Widdowson's fascinating remarks about trace elements to wonder about lead. Studies of lead

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poisoning in children have shown that all individuals have a certain amount of lead in the blood; is this simply a reflection of some change in environment which has taken place in recent history, or has it always been the case? Because, if the latter, it seems to me unlikely that in the course of evolution the mammalian organism has not formed some positive use for lead, and some of us might perhaps be suffering from a condition of hypoplumbism of which we are completely unaware.

I noticed, in Dr Naismith's paper, that the number of foetuses in the litters born to the dams on the low-protein diet was greater than in those on the high-protein diets. Although this particular finding may have been statistically insignificant I wonder if there is evidence that undernutrition results in larger reproduction. Is there an analogy, in mammalian life, from the root pruning of an apple tree to bring it into fruit, or from what one sees in populations threatened with destruction? Is it the case that in the well-nourished mammalian mother with a multiple pregnancy there might be absorption of foetuses and preferential growth of a few, and that in the undernourished larger numbers are born?

Dr Wigglesworth's paper poses, I think, a new problem for the paediatrician. We have got used to the idea of distinguishing between babies who are small at birth because of a short length of gestation, and those who are small for their gestational age. Now, in the context of most of the papers we have heard in these two days, we must make a second distinction, between babies who are small because of loss of growth potential occurring very early in embryonic life, and babies who are small because of an interference with nutrition occurring in later pregnancy. As Dr Wigglesworth made clear, these situations are not mutually exclusive; there is a continuum, and the question of interest is the relative contributions of small numbers of cells and of small-sized cells, and the significance of this.

I was fascinated by Professor Brambell's concept for the explanation of the transfer of immune globulins without the digestion of whole proteins, but I wonder if it has any significance in terms of nutrition. I think some teleological speculation would be interesting as to why some animals, particularly it seems herd animals, acquire nearly all their immune globulins through milk, whereas mainly nest-animals and mother-clinging animals appear to be born with their passive immunity. Arising out of Dr Shelley's paper I should like to have further discussion about enzymes in their adaptive significance and about the question of physiological anticipation in enzyme formation.

At this point I would like to question whether the extremely interesting relationship that has been shown between IQ's at 5 years of age and 'smallness for dates' in the newborn is necessarily due to asymptomatic hypoglycaemia. One has to reflect that hypoglycaemia is related to smallness for dates, or lightness for dates as Dr Neligan likes to put it; that lightness for dates is in itself correlated with size of mother, not just on a genetic basis but on an intrauterine environmental basis; that stature, certainly in adult life, is directly related to intelligence; and that intelligence of parents is related to intelligence of offspring, with I think a correlation coefficient of 0·5. So the difficulty in making cause and effect assumptions, and also in getting adequate controls, is evident.

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I thought that Dr Mount and Dr Hull perhaps oversimplified their discussion of thermal neutrality. It has been shown by Adamsons and his colleagues that it is not the environmental temperature with which oxygen consumption is related but the skin–environmental temperature differences. And at Hammersmith Dr John Grausz has shown that the situation is still more complicated; that the previous experience of an infant in relation to environmental temperature influences its oxygen consumption at a particular environmental temperature. Thus, if the environmental temperature of healthy newborn babies was increased from 30° to 35° to 40°, their oxygen consumption progressively fell; but if the temperature was then reduced to 35° the oxygen consumption would rise to a higher level than that which was found before at that temperature. The experiment was designed to ensure that the skin–environmental temperature gradient was similar on the two occasions. A homely analogy is that if one goes into a room at a set temperature on a hot summer day it seems cool, while going into the room at exactly the same temperature on a cold winter day it feels hot, and it seems that this kind of comparison is in fact possible in the newborn baby and influences its response. However, this effect may only be important in acute experiments rather than in the longer-lasting experiments described today.

Listening to Professor Wilkinson's paper I was astonished at the enormous weight loss that can be tolerated by babies undergoing the stresses he described, and still be compatible with their survival, and I wondered how this compares to the weight loss that can be sustained by an adult. Is it known in which body compartments this weight loss preferentially takes place? Is some of the large weight loss a consequence of the relatively great water content of newborn babies compared with older children? Have organ weights been studied and compared with those of normal babies who died without undergoing stress, say acute stillbirths of the same gestational age and birth weight? Another question which emerges from this paper is that of the selectivity of the gut, and the possibility of adaptation. For instance, can the infant who has lost the terminal ileum adapt to absorb vitamin B_{12} from the upper ileum or jejenum in a way that would be impossible for an adult who had undergone a similar operation?

Finally to Dr Davies' paper. I do think that members of The Nutrition Society should be interested in the decline of breast-feeding. The trouble about this subject is that it is naturally a highly emotional one, and statements about the decline of breast-feeding tend to be moralistic, even angry sometimes, in tone. I feel that a more dispassionate and scientific view is necessary. It is for instance possible that the decline in breast-feeding may be genetically determined, at least in part. Before this view is dismissed as absurd, reflect that throughout history there have been women who have been poor breast-feeders, and that there must be some genetic components in the ability to breast-feed. The disadvantages of not being a good breast-feeder in the past, in terms of biological fitness, are obvious: the infant died, and, as Dr Davies pointed out, still does so in underdeveloped countries today. The positive advantages in terms of biological fitness of not being a good breast-feeder I do not propose to discuss. But in this situation as in so many others we are

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probably dealing with a balanced polymorphism, which in western countries in recent years has been changed by the introduction of safe artificial feeding. When the balance of advantages and disadvantages is altered there is a quick re-establishment of a new equilibrium in a population, and it is possible that in respect of breast-feeding this is one of the things we are witnessing and one of the explanations for its decline. On the environmental side it might be the case that an important pre-condition for success in breast-feeding is for a woman to have been breast-fed herself. But whatever the reasons for the decline in breast-feeding it is quite clear that we must come to terms with it, and take seriously the question of the adaptation of other animal milks, or of 'humanizing' milk.

Thank you again for inviting me.

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