Detailed Comments

Page 1 & 2 : No annotation of the statements made.

Page 5 and other pages : Many punctuation, spelling and grammatical errors on page 7, 10, 11, 17, 18, 22, 23, 25, to mention only a few 45, 50, 54, 58, 66, 78, 83, 85, 89, 92, 108, 110, 112, 116, 118, 140, 153, 185.

Page 15 line 3 & 4 : "However", Is this appropriate in the context?

Page 15 line 10 from bottom : What is meant by cold injury ?

Page 16 : With regard to maternal size and birthweight, have the authors cited gone no further than indicating the possibility of a relation ?

Page 17 first few lines : Do all the references cited implicate diarrhea as the cause of undernutrition ? This is typical of the kind of imprecise referencing in many other places i.e. p. 25. Where more than one factor is involved, the references for each should be identified up separately.

Page 19 : Criteria for classification not specified properly wt/age, 60%
What do these mean?

Page 20 : line 8 from bottom : When this statement is contradicted subsequently, why such a categorical statement ? This should have been preceded by some statement such as that this is the case in most studies. Also, is the reference cited the most critical one for this ? When a number of references are available to support a statement, what are the criteria for selecting a few ?

Page : Inappropriate generalization

Page 23 : Middle para. Are these statements applicable to Kwashiorkor ?

Page 24 : line 5-8 : References ? (e.g. Sheila, Perera, Whitehead)

Page 26 : line 7 : Quite a few of the references make no reference to the statement made.

Page 29 line 4-5 : Is a small stature necessary for work capacity or do some individuals manage to perform well inspite of it ?

Page 30 : Greater physical fitness and low energy intakes.
line 1-3 : Statement seems hardly credible (Edmund 1977, 1979) check reference for statement - Work output not affected by calorie intake? Unbalanced presentation.

Page 35 - para 1 : Again use of references eg: Lee and Chow or Chow & Lee, 1965? found not only impaired x growth but also impaired feed efficiency, but only the former is attributed to them.

Last sentence in this paragraph the observations have been made in earlier studies the references cited are not the only ones northe first ones. (e.g. Miller, 1965) Hasten worked on animals

Second para : Is the reference cited only one or even the major one relevant for opening sentence.

Pg 36 middle para How come no comment or critical made regarding examination has been made ton the remarkable finding attributed to Knittle (1972) that maternal protein deficiency does not produce observable effects at weaning this contradicts several other studies? ~~Inflammation~~ 'effects' on what ?

36 last para repetition : In some places, blanket statements are made and references selected at random, appropriate otherwise cited. In other places, one specific study not necessarily more crucial in others, is gone into in great detail For instance, the first and last paras of this page in contrast with the second para.

Page 39 : Again some (e.g. Radhakrishnan 1966), references discussed in very great detail and others are catalogued together.

Page 37 : Does the study of Knittle (1972) include behavioral aspects ? A great deal of repetition. Contradict statement attributed to knittle on page 36.

Page 38 : The subtitle is undernutrition and the very first sentence deals with protein deficiency. Again, references clubbed together for variety of indices. Levitsky and Barnes 1972 Barnes et al 1970. Which are the effects among those specified not found to be reversed by rehabilation by these authors ? e.g. (Radhakrishnan 1966).

Page 43 : A large part irrelevant. Lines 1-7 from bottom could have been greatly condensed.

Page 60 : Para 2 : The references cited. Are they all specifically concerned with the effects of hytate? What about the cambridge studies on whole wheat breads ?

Page 62 last para : Is the statement attributed to Leitch (1964) correct? First sentence Increased prevalence with age - i.e. in the elderly ? reference ?

Page 66 : line 12 - Do 50 g dark green vegetables contain only 650 ug of carotene contradict the figure in the previous sentence.

Page 73 line 3 from bottom : In this and other places ad libitum being words of foreign origin should have been underlined throughout.

Page 75 : line 5 6 & 7 in first para - with respect to or 'as judged by'?

P. 78, P 83, p 85, p 89, p 92, 108, 112, 118, 126, 140, 15 ; ; ? punctuation & typographical errors.

Page 97 : The 1954 formulation has since been revised, to include zinc. Why was this not used?

Table 22 : The S-e's not shown for values in relation to body surface in the 45-50 g group UN animals seem to have consumed more food but also interms of surface area.

The above is also true of the statement in the next table. In the tables presented, a few values apparently not consistent etc. With the rest of the data found to be wrongly calculated on subsequent analysis of the basis of the individual data presented in the Appendix for instance Table 22 - Body weights. This implies that other tables necessitate correcting.

Page 146 : Statement (c) fails to specify this to be the case whether or not this was preceded by nionatal undernutrition.
No mention is made of the differences between animals from litters of 4 & 9 subject to the same degree of restriction in the neonatal period, viz, the 80% and 66% groups.

Page 147 : table 27 : The tabulation could have included the period of feeding.
If the candidate is talking of the degree of undernutrition to explain results, she should have indicated this in more specific terms for instance, body weight as of controls at the end of restriction (and perhaps at the time of testing as well)
In this and many other tables the use of 'F' ratio for statistical analysis would have been much more appropriate.

Page 149 line 2 : The word 'group' meaningless.

Page 154 line 10 : 'typical range' or 'expected range'? In most animal colonies the normal variation is more than this. Did mothers reared in smaller litters give rise to small litters ? If so, this trend has not been commented on by the candidate.
Line 7 from bottom : row means means ?
Last few lines : The candidate has again failed to note that the differences specified are associated with differences in the size of the litter in which the mothers were raised.

Page 155 Table 29 : The number of animals is not stated (this commission in typical of most tables). Presumably, they were small. The number of fmmm females successfully reproducing in each group is not stated even in the table on.

From the individual valuesg given in the Appendix the number seems less than five for many of the groups-it would have been better to combine the 100 and 80% groups on the one hand, and 66% and 50% on the other as the inconsistent pattern found could well be due to small sample size. Difficulties in monitoring weight differences with a reasonable degree of precision in animals of this size with ordinary balances makes it all the more essential to have an adequate sample size. Such errors seem likely on the basis of data for food intake. The discussion does not take into account all the differences which seem statistically significant in this table - e.g. the difference between groups fed the same post weaning diet with a different developmental history before weaning. This is also true of many other tables. The same comments also apply to the analysis and discussion of the data on lactation. Those consideration limit the value of the data on food utilization.

Page 157 : The weaning weights in this expt for pups raised in standard sized litters are more than what is indicated by the range of 35 - 45 g in the previous expt. The mothers were presumably on the same diet. Yet the candidate does not even comment in this difference.

Page 163 : line 6 from bottom : The first part of the statement inconsistent with the second part. This whole section needs to be revised in the light of the points raised.

In this expt. as well as in expt 2, values for the proportion of females with succesful gestation and lactation could have been given and the implications discussed. Also, were the animals which failed to conceive on their first mating opportunity allowed to mate again ?

Page 177 : Variation in the values and tissue produced per g for food intake not shown as in previous tables. Last column 82 or 8.2 ?
Last row : Net maternal weight gain What does ± S mean ?

Page 179 : The mean weaning weights in this experiment for animals fed HIG and are less than for those fed the stock diet in expts 1 & 2.
Is this expected on the basis of diet composition or do other factors account for the difference? e.g. the type of processing ?

Page 180 line 6-7 : A saight change of 2 with and s.e. (or s.d) of 4 g (Table 21) can hardly be considered as significantly different from zero and as representing weight again. Table 21 In this and many other tables, values presumably present mean ± s.e or s.d. This is not specified. In the absence of this information and the number of observations, it is difficult to assess the validity of the statements made. One should not have to go to earlier portions.
line 1 & 2 : The values for the two generations could have been combined if they are not significantly different from each other and composite picture examined for consistent trends.

Page 190 third para : The data comparing the relevant parameters or gestation, lactation and controls could have been presented in summary form as in Table 32.
Are the gains for lactation consistent in with those in the previous table. (Before mating) have been more specific than 'initially'.

Were the weight gains similar in all the groups during gestation ? This is contrary to what is indicated by the data. Somewhere in the discussion, the LIG is identified as showing improvement in food effeciency during gestation. In a subsequent sentence both groups (LIG & HIG) are claimed to show this phenomenon. Are requirements of pups metabolism perhaps greater than those for fetal metabolism. The first sentence in the part - the latter necessarily imply former or vice-versa.

Table 36 : The value for N retention for gestation in the LIG group is not consistent with those for food and Fecal N. Even the observation that values for the two generations are markebly different from each other inspite of a lack of difference in other respects does not seem to have prompted a re-examination.

Similar errors in calculation are to be found in Table 36 and Table 39 in other respects and have similarly escaped a recheck.

They were detected only because of their apparent inconsistency?
Table 38 - What does 'Pre-lactation' mean? gestation ? of the other values presented?

Page 193 : Is the value for food N for HIG - G_1 group during lactation - correct ?

Page 199 : The mean value for LIG nor consistent with that given on P 194 nor with the individual values. Such affors seem common place e.g. see p 195 & 203. Is the statement about lack of differences during gestation quite valid? What if the value for N possibly due to calculation error ? Also, for the values for lactation differences in pup growth rate during 12-18 days not taken into account ?

The statements make no mention of the need for taking considering fetal tissue composition during gestation and tissue composition pup during lactation.

Page 205 : Data on p 195 do not support the statement on increase in apparent degistibility during gestation.
That Nitrogen retention during lactation was comparable in all the groups inspite of gross differences in food intake is not sought to be related the appreciable differences in urine Nitrogen and the observation cited by the author that urea N is increased during pregnancy. If all the values could have been calculated as percentages of those for the HIG group, a clearer picture would have emerged. One of the mean values is outside the range of individual values. This is also true of one of the means in Table 29.

Page 203 : The data of this and other tables have been presented in a summary ~~from~~ form so as to make the comparisons more clear.

In discussing N retention during lactation, some estimates could have been made of the amount of likely to have been transferred to the pups on the basis of weight gains during the period of balance studies. In the absence of this information and assumptions made, the statements in the second para do not seem warranted and are hardly consistent with the assumptions and calculations made on P 196.

- They do not even take into account the gross fact that a large part of the Nitrogen retained would be utilized for pup growth and the glaring probability that the differences observed may merely reflect differences in pup growth rate. A better picture might have emerged if the weight in the pups period of study had been considered in the calculations. It is surprising that this is taken into account in expt. 1 for calculating feed efficiency.

Page 206 : Discussion irrelevant in the light of the points raised/ Also no reference is made to the studies of Naismith regarding the composition of tissue gain during gestation.

Page 210 : Impaired utilization ?
Last line : Is such a categorical statement justified ?

Page 212 line 14-16 : Statement inconsistent with previous statements in this connection.

Page 214 : First link with previous experiments not clear.

Page 215 : Experimental details could have been indicated diagrammatically.

Statement not borne out Figure on P 221

Page 222 : The whole discussion shows lack of clarity and a few inconsistencies.

Page 223 : Not well defined - In what way ?

Page 224 : Middle, para last sentence statement regarding haemoglobin not consistent with data of Table 46 for the group fed 10% protein. It is a good to express the value as % of control values has been done in this table, those significantly different could have been marked with an asterisk - This also applies to similar presentations in other Tables. (Line 10 from bottom - reference ?)
No clear comment made on the differences between the 10-10% and 20-10% w.r.t. haemoglobin.

Page 240 : Was stature measured in the study cited? if so, how ?

Page 247 line 3 onwards : Statement differs from the blanket statement made elsewhere.

The examples given are corn, wheat and albumin for man and rat. The values could have been given in both cases for all these.

Page 249 : last para - last few lines
The discussion of the data not satisfactory
Table 55 - Body weight gains not consistent with the difference between initial and final body weight in many cases - This makes it impossible to evaluate the discussion.

Page 253 : The value of 44 for 11-31 weeks hardly consistent with those of 190 and 438 g for 3-11 and 3-23 weeks for the 20% group. The last statement is based on this value.
The period specified for the last group (11-31 weeks) not consistent with the description given.

Page 254 : Data in Table not clearly discussed in the section following. What is this logic if leaving out the data for 11-15 and 21-26 weeks ? If the object is to compare 8 week growth in all cases, weight gains for both the complete phase and the eight week for selected.

Page 258
Table 57 : Similar values for initial body weights would have made the data more meaningful. Units should have been specified for the parameters.

Page 263 para 1 : Both the first and last sentence in this para seem inconsistent with the data.

Page 264 line 3 from bottom : 1500-2000 mg of what ?

line 2 from bottom - 7000-1000 ml of milk consumption from the value given for the diet of the HIG, elsewhere-

Page 267 : Footnotes - not clear. Is refreshing to find atleast table with identification of the measure following the ± sign and no. of animals. Why was this not done consistently in all the tables 0.005 ?

Page 268 table 60 : The parameter given has to be inferred as weight gain from the heading. This should have been indicated in the table specifying the units.

The significance of small differences in weight gains for animals weighing as much as 500 g is highly questionally, in view of the large errors in measurement which are likely to result with animals of this weight. Even a 3 % would result on a 15 g difference. Normal fluctuations in body weight could also be of this magnitude. Under the circumstances, the entire discussion on this part of the data loses relevance, specially as no direrences are found in either body weights or bone composition.

Page 271 lines 3-5 : Such a categorical statement is perhaps not justified. The most that could be said is that growth deficits found in the high calcium group at the end of phase II were reversed after the switch to a lower level. Here again, the body weight data have to be considered along with those on weight gains and bone composition. A scrutiny of the data in Table on P 268, does not suggest a significant difference in weight gains between the 2 groups fed 440 mg and 600 mg during phase I although they are marked significantly different from each other. The fact that the picture may change if the obvious calculations errors in this table are taken into account is quite another matter

line 13-14 : Is this an origin l inference resulting from the present observation?
line 9-8 from bottom : without significantly altering-who or what does the altering?

Page 281 : Have no other studies been made on this aspect ?

Page 282 : The levels of calcium should have been identified for Phase I and Phase II. Each table should be self-sufficient to the extent possible. That the levels indicate those in previous diets is not obvious.
Again, the discussion of the results for 52 week old animals is meaningless in the abscence of differences in bone composition. This would have been obvious had the candidate calculated the increments as percentages of initial values.
Some of the conclusions are also at fault because of these considerations.

Page 284 line 6 onwards : The statements in this para are indeed, amazing in view of the well-known prevalence of skeletal retardation in many poor areas. This has also been discussed in a monograph published from the candidates laboratory (Baroda J Nutr. Vol 3, 1977).
The references cited in support of this statement are probably concerned with calcium balance and not skeletal maturation. The candidate does not seem to realise a child with calcium etc. unless complete cessation of growth occurs.

Page 285 line 1 : Calcium retaining ability ? What does this mean ? The discussion on haemoglobin and iron is skatchy. Several studies including some in the candidate laboratory have shown to the beneficial effects of calcium supplementation to groups such as school boys. Overall that nutritional status.

No justification for the inference a factor on the basis of the data reported.

Page 288 : Refs for statements in paras 2 & 3.
Our previous impressions. Whose ? The candidate's ?
Weekly body weights could have been presented al least for the late depletion and early repletion periods.

Page 292 : Repletion with vitamin A ?

Summary

The summary, instead of being a good overview of the various experiments presenting them in an overall perspective and showing the links between them where appropriate is a cataloguing of the results.

Page 301 : line 8 and 7 from bottom : What is the justification for the statement made ? I can seem no connection between this and the objectives of the experiment as outlined in the previous paragraph and else. Again, this is the sort of statement that casts grave doubts in the maturity of the candidate.