

undoubtedly starts with the well-known conclusion that a Calorie in the food absorbed in a mixed diet from whatever source, protein, fat or carbohydrate is of equal dynamic value. Previous work amply justifies this.

She was not foolish enough to attempt to draw any conclusion from her investigations as to the relative value of animal and vegetable food in the diets on the physical development of the individuals.

Professor Pearson seems entirely unable to grasp the fundamental fact that the physical development of the individual depends largely upon his past conditions of life. To co-relate it with the special constituents of the food which he habitually eats will require not only an enormous series of studies, but a full investigation of the character of the various food stuffs and of the mode of cooking.

These points I tried to explain to him when I wrote to him in summer. He did not write to me as, in his criticism, he says he did. Miss Lindsay forwarded to me a letter from him to her, and I wrote a reply to Professor Pearson which he did not acknowledge.

In conclusion I would say that before he expects his criticism of a physiological problem to be taken seriously, he had better make some attempt to understand the nature of the problem. Certainly it is not my intention to waste time in replying further to his criticism unless in the future it is more pertinent than is his present contribution.

II. The Statistical Study of Dietaries. A Rejoinder.

By KARL PEARSON, F.R.S.

I PUBLISH Professor Noel Paton's reply because it is very typical of the type of difficulty which we meet with at present, when we assert that what is really statistical work must be undertaken only by the adequately trained statistician and that when it is not, then the investigation cannot be considered as falling into the field of science.

Professor Paton states that the following question given on p. 4 of the Report formulated its object: "Do the working classes of this city get such a diet as will enable them to develop into strong, healthy, energetic men, and as men, will enable them to do a strenuous day's work; or are the conditions of the labouring classes such that a suitable diet is not obtainable?"...

Now Professor Paton either assumes that the sample taken of the diet of the individual family was their customary diet, or he does not. If he does, then the question: Was the diet such as would enable the working classes "to *develop* into strong, healthy, energetic men"? has meaning. If he does not, not only is it idle, but the section dealing with the physique of the children on the basis of a sample diet taken as a rule *for a week* (occasionally for a fortnight), is beside the point.

But anyhow, I ask how he can possibly ascertain how the working classes will "develop into strong, healthy, energetic men," if he does not take an adequate anthropometric survey of the families subjected to the dietaries recorded? He says that it is accepted and proved that "If a family diet...gives a yield of energy of less than 3500 calories per man per day it is insufficient for active work; and if less than 3000, it is quite inadequate for the proper maintenance of growth and normal activity." He further assumes with Miss Lindsay that calories from animal and vegetable foods have equal "dynamic value." I assert that neither of these conclusions,

which he accepts, are based on adequate research and they are in fact refuted by Miss Lindsay's own material. For, if it can be shown that animal and vegetable calories have different results on the physical development of the children, it is clear that the first statement as to how many calories are needful for the proper maintenance of growth has no significance until a statement is made with regard to the source of the calories. Professor Paton cites no evidence for his statements; from what I have read on the subject of calories, I feel convinced that most of the data on the matter would not stand for five minutes any adequate statistical analysis. The *Report*, Professor Paton tells us, shows "very markedly the relationship between the physique and the food." Yet in a previous paragraph he says "that the physique is determined by the whole previous condition of life and by the influence of heredity, and that it is absurd to attempt to relate it *solely* to the diet."

Now the only way to ascertain whether there was a *marked* relationship between the food and the physique of the children was to correlate the two *for a constant age* and investigate whether the correlations were such, *having regard to their probable errors*, that they could be considered significant. I did this with the result that the total calories in the food and the girls' weight for constant age was not definitely significant with regard to the probable error, while in the case of the boys the probable error was so large that it was impossible to say whether the relationship was really considerable or not. In fact no *marked* relationship could be deduced from Miss Lindsay's data, they were too inadequate. If Professor Paton's statement as to the influence of heredity is to be trusted, then even my correction for age was inadequate, and the data ought to be corrected also for physique of parent! If so, why was the parent not measured?

Professor Paton places before the readers of *Biometrika* two tables on which this "marked" relationship is asserted by him to rest. One of the cases in his Table A, No. 32, is erroneously placed in this table; the details show that the number of calories was 2949 and not 3822*; it should be in Table B. These tables contain 16 boys' weights and 20 girls' weights. Professor Paton takes the British Association measurements, which are, of course, wholly inadequate as a test of Glasgow children, and making no real correction for age† considers whether the children in the two tables were or were not above the quite arbitrary limit of 5 lbs. below standard. He gives us no measure at all of the significance of the result, which is based on the vagaries of sampling 16 boys of ages from 3 to 11, and 20 girls from 5 to 13; and he supposes in some way that this treatment can possibly refute the correlation coefficient, r_{wC_p} , of weight and food calories for constant age with its probable error! I can, however, throw more light on the matter. Owing to the great courtesy of Dr Chalmers, Medical Officer of Health for Glasgow, I have been able to more than treble the number of weights of the boys and girls subjected to the dietaries. The results for total calories in food, C_p , now are‡:

Girls, 69	Boys, 55
$r_{wC_p} = +.21 \pm .08,$	$r_{wC_p} = +.05 \pm .09.$

Thus the relation for boys is now quite insignificant, and for girls may well be insignificant also. At any rate although both correlations are positive, there is no "marked" relationship between the physique and the dietary. Of course, it may be said that these weights (w) have been taken at some interval after the dietaries were recorded, but unless we assume the dietary to be a rough measure of the permanent feeding of the family, whose physique has been gradually built up for years before the dietaries were recorded, the observations must be discarded as of no value at all for testing physique, or as Professor Paton phrases it "development."

* In the Appendix V of *Rickety Families*, it is given again; this time as 2329 calories.

† The deviation at each age would have to be measured in terms of the standard-deviation of weight at that age; naturally the deviations are larger for older children.

‡ I have to thank Miss B. M. Cave for the present series of correlations.

But the most interesting point ascertained from the new material is the confirmation of the result that the higher the proportion of animal to vegetable calories the greater the weight. In *Biometrika*, Vol. ix, p. 533, we had for 16 boys and 20 girls:

$$\text{Boys:} \quad {}^a r_{w, c_v/c_A} = -\cdot 23 \pm \cdot 16,$$

$$\text{Girls:} \quad {}^a r_{w, c_v/c_A} = -\cdot 12 \pm \cdot 15.$$

We now have for 55 boys and 69 girls:

$$\text{Boys:} \quad {}^a r_{w, c_v/c_A} = -\cdot 30 \pm \cdot 08,$$

$$\text{Girls:} \quad {}^a r_{w, c_v/c_A} = -\cdot 24 \pm \cdot 08.$$

These results seem to indicate that Miss Lindsay and Professor Paton, who supports her view, are in error when they consider a calory the same whether it be from animal or vegetable food. On the other hand, our larger numbers now indicate that:

(i) For a constant age the *expenditure* on vegetable or on animal food has no sensible relation to weight.

(ii) For a constant age the number of calories in vegetable food has no sensible relation to weight.

(iii) For a constant age the number of calories in animal food has a positive correlation with weight for both girls and boys, being definitely significant in the first case ($+\cdot 32 \pm \cdot 07$) and not so in the second ($+\cdot 08 \pm \cdot 09$).

(iv) For a constant age the correlations of weight with ratio of expenditure on vegetable and animal foods are for both boys and girls quite insignificant as compared with their probable errors.

I am extremely obliged to Dr Chalmers for doing his best to supply additional material. As far as it goes, it tends to show that calories are of far more importance than expenditures, but that calories from animal food are more closely related to physique than are calories from vegetable food*. The new material supports my criticisms that the failure to distinguish between animal and vegetable calories stultified the advice given by Miss Lindsay, i.e. to spend money on oatmeal rather than on eggs. It also indicates that no safe conclusions with regard to dietaries can be drawn until a reasonable anthropometric survey accompanies the record of dietaries, and the whole is reduced with adequate statistical knowledge.

One point I can allow Professor Paton. It was an oversight on my part, when I said that I had written to both Miss Lindsay and to himself; the letters in which Miss Lindsay and he stated that to follow up the families now would be impossible were both replies to one and the same letter of mine addressed to Miss Lindsay. The additional facts I desired were in their opinion unascertainable, and further correspondence did not seem to me likely to be of any service in achieving the end I had in view, namely to render of real service to science a piece of recording work from which in my opinion then and in my opinion still, very misleading conclusions had been drawn, and which conclusions in their turn had been exaggerated in the press résumés of the paper. I do not think any such work as that done on dietaries by Miss Lindsay and Professor Noel Paton will be of real value until (i) these dietaries are accompanied by a thorough anthropometric survey of the whole families of the dieted and (ii) the equality of animal and vegetable food calories ceases to be considered as a dogmatic truth.

* Of course the results show that on such data as are available, the food has relatively little relation to the weight, there is no "marked" relationship.