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Correspondence.

"Audi alteram partem."

"UNBOILED v. BOILED MILK."

To the Editors of THE LANCET.

SIRS,—In the letters which have been addressed to you on this subject I have been struck by the absence of any reference to experimental data. Yet there is a large amount of experimental evidence bearing directly upon the points at issue. May I ask for a little space to consider the subject from this point of view?

One must distinguish sharply at the outset between (1) the *digestibility* of the milk and (2) its *absorbability*. By the former I mean the ease with which it is peptonised in the stomach; by the latter the extent to which it enters the blood and is available for purposes of nutrition. Jessen,¹ experimenting on healthy men found that a pint of *raw* milk has completely disappeared from the stomach in three and a half hours; a pint of *boiled* milk in four hours. Verhaegen,² however, found that boiled milk required less time for digestion than that, while Reichmann³ even goes so far as to state that 300 cubic centimetres of boiled milk remains for an hour less in the stomach than an equal quantity of milk taken raw.

It is obvious from this that the results of experiments on the relative digestibility of boiled and unboiled milk are not unanimous, but at all events boiled milk has not been proved to be much *less* digestible. Any one, moreover, who has taken the trouble to compare the relative density of clot formed by raw and by boiled milk under the influence of rennet must have recognised that any slight difference there may be is certainly in favour of boiled milk and not the reverse. And yet it is the density of the clot which determines the ease or otherwise with which milk is disposed of by the stomach.

As regards the comparative *absorption* of boiled and unboiled milk experiments have also yielded rather discordant results. Randnitz⁴ found that dogs did not absorb the casein of boiled milk quite so well as that of raw milk, but the absorption of fat was the same in the two cases. Vassilief⁵ found that the nitrogen and fat of raw milk were better absorbed by healthy young men than the same ingredients after boiling, but this conclusion is disputed by Gaschibowsky.⁶ It has also been found in the case both of infants and of calves that sterilised milk which has been kept at or about the boiling point for more than an hour is absorbed quite as well as milk which had merely been boiled in the usual way.⁷

The supposed loss of nutritive matter in the "skin" which forms on boiled milk is not of any real importance. 100 cubic centimetres of milk when boiled for 15 minutes lose only 0.273 gramme of proteid.⁸ As a matter of fact the "skin" consists largely of phosphate of lime of which there is already too much in cow's milk.

It is my habit in out-patient practice at the Hospital for Sick Children to order that the milk shall be boiled in every case. Amongst several hundreds of infants so fed I have but rarely had any difficulty in obtaining the normal increase of weight. This is not a mere general impression, for I make a point of having these children weighed every week and a careful record is kept of the increase in weight and the exact

method of feeding. I am convinced that such satisfactory results as I find to be the rule could not be obtained if the nutritive value of milk really were, as Dr. Clement Dukes asserts, greatly impaired by boiling.

As regards the supposed risk of producing scurvy by the use of boiled milk it will be sufficient to quote the following paragraph from Holt's well-known text-book on the Diseases of Childhood (p. 210).

Since the introduction of the practice of heating milk used in infant feeding the question has been raised in many quarters whether this may not be a cause of scurvy. I have carefully investigated this question in the records of three institutions in which for five years "sterilised milk" was the standard food for all artificially-fed infants. The number of children under 18 months who have had this diet is nearly 1000. During this period but two cases of scurvy were observed, and in neither case had the child been upon a diet of sterilised milk. However, I have recently seen in private practice two cases of scurvy in which the cause seemed to be prolonged sterilisation at a high temperature—i.e., 212° F. for over an hour. In some of the cases in which the sterilised milk is supposed to have been the cause of scurvy it was undoubtedly the milk formula which was at fault and not the process of heating. In two patients under personal observation who developed scurvy while taking "sterilised" milk and a proprietary food the food was discontinued and the patient recovered although heating the milk was continued. In four cases observed by Winkes no other treatment was employed than the substitution of "sterilised" milk for the previous diet, which in three instances had been proprietary foods. All the patients promptly recovered. In these cases the milk was heated to 212° F.

In any case the risk of scurvy is one of quite secondary importance. All that one has to do in order to obviate it is to see that a little fresh fruit juice is given to the child occasionally.

Everyone will agree with Dr. Dukes that the ideal to be aimed at is a milk-supply of such assured purity that there can be no risk of disease being conveyed from the cow or the dairy to the consumer. But everyone will also agree that, whatever may be the case in such exceptional circumstances as those in which Dr. Dukes has the advantage to be placed, the milk-supply of the great bulk of the community is not yet in this ideal condition. As long as it can be truly said, as Dr. Dukes has himself admitted,⁹ that "milk has been shown to be a fertile cause and communicator of disease in communities," and as long as it remains true, as he has also remarked,¹⁰ "that between the cow's udder and the human stomach the milk may be so poisoned as to cause death to the recipient," so long, I am convinced, will the medical profession do wisely to urge that (again to quote Dr. Dukes): "No milk should be drunk until it has been boiled, even although its nourishing value is thereby diminished." That any real proof of this diminution of nourishing value has yet, however, been adduced I, for one, will not admit.—I am, Sirs, yours faithfully,

ROBERT HUTCHISON.

London Hospital College, July 12th, 1901.

To the Editors of THE LANCET.

SIRS,—I believe that this controversy would have been summarily dismissed as rank heresy were it not for the fact that the heretic is such a distinguished man as Dr. Clement Dukes. If it were possible for the milk to be transferred *direct* from the gland to the human stomach there would have been no necessity for either boiling or sterilising it. But in practice we find that by the time milk is actually consumed it has been exposed to numerous foci of infection (both wilful and accidental) and is in consequence charged with countless bacterial forms.

Milk is an exceptionally good medium for the growth of organisms, and they thrive therein without causing any perceptible change in its appearance. With the appearance of acidification some organisms (notably cholera vibrios) are, no doubt, destroyed; but so much time usually elapses that milk in most cases is used up before this occurs.

All these risks can be easily avoided by boiling or sterilising; and there is no evidence on record to show that these methods cause any sensible lowering in the nutritive value of milk.

Dr. Dukes maintains that all that is really wanted is more efficient inspection of cowsheds and dairies. I might as well ask him to apply this reasoning to the water-supplies, to endeavour to prevent their contamination, and to do away with all sand or biological filters. No matter how stringent be the precautions taken, it is as impossible to prevent the entrance of microbes into milk as it is in our water-supplies.

⁹ School Diet, second edition, 1899, p. 89.

¹⁰ *Ibid.*, p. 90.

¹ Zeitschrift für Biologie, 1883, Band xix., p. 129.

² Physiologie et Pathologie de la Sécrétion Gastrique, Paris, 1898, p. 11.

³ Zeitschrift für Klinische Medizin, 1885, Band ix., p. 565.

⁴ Zeitschrift für Physiologische Chemie, 1890, Band xiv., p. 1.

⁵ Quoted by Cautley, "The Feeding of Infants," 1897, p. 214.

⁶ Maly's Jahresbericht, 1894, Band xxiv., p. 502.

⁷ See Bendix, Jahrbuch für Kinderheilkunde 1894; Band xxxviii., p. 393; Cautley, op. cit., p. 215; and Weber, Bulletin de la Société Médicale Pratique, 1892, p. 77.

⁸ Solomin, Archiv für Hygiene, 1897, Band xxviii., p. 43.

Dr. Fred. J. Smith observes, in THE LANCET of July 6th, p. 50, that he has seen children made "peevish and even seriously ill" by the effects of boiled milk. But is he quite sure that the cause of this is to be found in milk and not elsewhere? And what evidence is there to show that unboiled milk would have given more satisfactory results in the case of those children?

I am, Sirs, yours faithfully,

M. L. DHINGRA, M.D. EDIN.

Albemarle-street, W., July 8th, 1901.

"LEGISLATION AGAINST NATIONAL INTEMPERANCE."

To the Editors of THE LANCET.

SIRS,—I crave your courtesy so far that you will afford me space for a final reply to Dr. Archdall Reid. We have reached that stage in the controversy when the *tu quoque* argument is apt to be used, and I do not desire that this important discussion should flicker out, so far as I am concerned, with Dr. Reid's peculiar view both of logic and summation of facts. Dr. Reid has a little joke (not at my expense but at his own, of course) regarding the presence of a dragon at three minutes past midnight on Nov. 10th, 1900, curling his whiskers on the cross of St. Paul's. He says that if he asserted that the dragon occupied this ecclesiastical situation nobody could disprove the assertion. Now this very example delivers Dr. Reid and his peculiar ideas regarding the nature of proof into my hands. I shall not spare mine enemy. The case of the dragon would be settled for myself (and I imagine for all sensible men) by considerations connected, first, with proofs of the existence of such a creature; next with questions of its ability to scale St. Paul's (Dr. Reid's dragon would possess wings, no doubt); and, thirdly, with the state of Dr. Reid's mind—here I need not say that I imply nothing personal, of course—when he asserted that he saw the whisker-curling operation. If he merely alleged that a dragon did curl his whiskers, without saying that he saw the operation performed, his case would be less worthy of consideration. Now, judged scientifically, I say we have no evidence in this case that dragons exist; therefore, all the rest goes by the board into that sea of speculation in which Dr. Reid's natatory efforts are not likely to keep him afloat very long if he trusts to the dragon illustration as a species of lifebuoy. He asks me for evidence that "acquired" characters are never transmitted. I suggest in turn that such evidence does exist. This is a reasonable view enough, but Dr. Reid, with an eel-like gesture, slips out of his difficulty on the pretence that I ask him to "prove a negative." That strain of his has "a dying fall." I have heard that argument used many times when the task of proving his own case was beyond the powers of an opponent. Suppose I assert that it is impossible that a dragon should have "curled his whiskers on the cross of St. Paul's." Dr. Reid says that I cannot prove my view to be correct because I "cannot prove a negative." Am I to take it, then, as a corollary of this monstrous proposition, that Dr. Reid asserts that, say, a lunatic's ideas cannot be contradicted? The real case is, that Dr. Reid and myself are both face to face with certain facts of inheritance and each of us seeks to explain them. He chooses one explanation; I choose another. I do not deny that congenital characters are inherited, but I contend that acquired characters may also be transmitted. It is no question of "proving a negative" at all; it is a question whether Dr. Reid's exclusive view as regards "acquired" characters is to prevail. If he has no stronger argument to offer than his dragon illustration as against my contention he must be indeed in "a parlous state." One word more. Dr. Reid professes to scout Spencer, Eimer, Hertwig, Cunningham, and other advocates of the view that "acquired" characters may be handed on. Nobody asks him to "review the works of Spencer" in THE LANCET, but this rejoinder of his, I hope, none of your readers will regard as disposing of the fact that some of our most distinguished biologists are opposed to the Weismannian and Galtonian doctrines tooth and nail. Already I have referred Dr. Reid to cases reported in your own pages. He will find others in Eimer's and Hertwig's works. He says nothing, for example, of the experiments I alluded to on the division of ova, each division reproducing a perfect animal, and yet he avers there is no evidence on my side for him to criticise. In a word, Dr. Reid shuts

his eyes conveniently to the evidence on the other side, and then, with his optic on the dragon seated on the cross of St. Paul's, asks me to desist from requesting him "to prove a negative." "I ask him for evidence," says Dr. Reid, and in turn I have suggested where he will find it. If he does not care to criticise it—he has criticised a good deal of evidence on his own side in your pages—that is his own affair. Meanwhile let me say that I believe that the whole question of heredity is still *sub judice*. It is sheer folly for Dr. Reid, as it would be folly on my part, to pretend that either of us has reached an adequate explanation of the varied phases of inheritance. But I protest forcibly at the same time against the idea that the advocates of Weismannian or Galtonian doctrines have proved their case as against the heredity of acquired characters. That case, as pushed to extremes, may, at present, be adequately represented by Dr. Reid's ecclesiastically minded dragon.

I am, Sirs, yours faithfully.

Edinburgh, July 6th, 1901.

ANDREW WILSON.

EPSOM COLLEGE.

To the Editors of THE LANCET.

SIRS,—I beg to thank you warmly for your admirable leading article in THE LANCET of July 13th, p. 90, on the position, aims, and difficulties of Epsom College—nothing could be better. But there is one error in your report of my observations at the dinner that needs correction. The amount collected and expended upon the pensioners and foundation scholars within the past 14 or 15 years has been between £70,000 and £80,000, not between £17,000 and £18,000.

I omitted to state that by the will of the late Mr. J. F. France, ophthalmic surgeon to Guy's Hospital, he has left about £10,000 to found additional pensions for *medical men*, to be awarded at the discretion of the council upon which he had served nearly 50 years. He had also arranged to complete the purchase of a tenth scholarship for girls at the schools of the Royal Asylum of St. Anne's. These also are entrusted to the council to award. Altogether my old teacher has expended £22,000 on the purchase of these scholarships for young girls at St. Anne's and in providing for these additional pensions for aged medical men.

I am, Sirs, yours faithfully,

C. HOLMAN,

27, Soho-square, W., July, 1901.

Treasurer of Epsom College.

"THE ARMY MEDICAL SERVICE."

To the Editors of THE LANCET.

SIRS,—Following your recent leading article on the above subject in which you express regret for the absence on the committee of reorganisation "of an army medical officer of high administrative rank and experience" and "trust that the committee will be empowered to call any witnesses whom they may desire to examine," and the reply of the Secretary of State for War in the House of Commons on July 4th that "the committee was a consultative one," only, that "the proceedings would be private" and that they "would not take evidence," one turned to your issue of July 13th in the hope of finding some reference to the position taken up by the War Office as narrated in the replies to the questions asked by Dr. Farquharson and Sir Charles Dilke, and in the absence of such will you allow me to make a few remarks?

I take it for granted that the department will concur in the views which you express in the article in question; and, indeed, without a member on the committee of high army medical rank "with a knowledge of military organisation and the requirements of field medical service," it is difficult to understand how the outcome of this scheme placed before the committee, emanating apparently from the War Office itself irrespective of the Army Medical Office, can adequately meet the army wants and solve the difficulties under which the Royal Army Medical Corps labours. It is true that this committee comprises the names of professional men of high civil standing, a lieutenant-colonel of the Royal Army Medical Corps who also represents Ireland and the professional head of the India Office, yet, with the one limited exception, its members cannot possess that practical knowledge of the medical needs and the details of army routine organisation and command bearing on the efficient performance of the duties placed upon the corps, which is