

descendants, whereas modifications are not. If then, the enfeeblement is a true variation, it is impossible to understand how racial evolution can result from a cause which renders offspring degenerate. But if it is only an acquirement, if the germ cell merely is enfeebled, while the germ plasm contained within it is not radically altered—as seems to be the case with the sperms that remain in the body—then here would be an explanation of the mystery. It would be possible in that case for racial evolution to occur even though parental disease enfeebled offspring, since the enfeeblement would not be transmitted to descendants. Only direct experiment can decide this point. Professor Ewart does not appear to have experimented with the offspring of the enfeebled individuals; but it is to be hoped that he will do so. I offer this suggestion for what it is worth but the results of asexual reproduction render me extremely sceptical.

Leaving aside for the time being these very obscure and abstruse problems, I may be permitted to express my pleasure that none of your correspondents now appear to doubt that human evolution really has resulted from disease and alcohol. When first I raised the point it was received with amazement and amused incredulity. The next step, I hope, will be a recognition of the tremendous consequences of that evolution. The political history of man is important but it is a little thing, even as regards political results, when compared to his natural history.

I am, Sirs, yours faithfully,

Southsea, August 23rd, 1903.

G. ARCHDALL REID.

*To the Editors of THE LANCET.*

SIRS,—Dr. G. Archdall Reid and Dr. C. R. Niven require no assistance in meeting the arguments of Dr. J. Wigglesworth; one can only marvel at Dr. Reid's patience in meeting and disposing of the same class of opponent year after year. I beg a small space to comment upon the report of the discussion on Dr. Reid's paper before the British Medical Association which appeared in THE LANCET of August 15th, p. 469.

I cannot believe that your reporter is romancing; I cannot believe that he is indulging his sense of humour, which would be sadly out of place in an account of the annual professional exhibitions of mental meteorism; I can only believe that his report is too true and represents the solemn opinions of a number of medical men whose education, presumably, included a study of biology. The position of these champions of *post hoc* reasoning appears to be as follows: Statistics prove that drunken parents often have defective children (or, rather, defective children in asylums have often had drunken parents), therefore parental drinking is a cause of filial degeneration.

Is it really necessary to point out the fallacy to gentlemen who have passed an examination in Jevons or Berkeley? Have they checked their conclusions by ascertaining to what extent non-drunken parents have defective offspring or to what extent the defective children in asylums are the offspring of defective people? Mental instability (whatever that means) is, they tell us, a cause of drunkenness. Then may not the shortcomings of the child be due to the parental "instability" and not to the parental drunkenness which accompanied it? Surely there is a great gulf here between the *post* and the *propter*. And is there not reason for less dogmatic assertion when one recollects that biologists—men who understand the real difference between inborn and acquired characters—have never yet ascertained (*pace* Dr. A. W. Gilchrist and his Neapolitan hearsay), in spite of immensely prolonged investigations, that it is possible to modify the germ plasm.—I am, Sirs, yours faithfully,

Florence, Italy, August 21st, 1903.

H. LAING GORDON.

*To the Editors of THE LANCET.*

SIRS,—I was somewhat struck on reading the account in THE LANCET of the meeting of the Psychological Section of the British Medical Association with the ignorance displayed by Dr. G. Archdall Reid's opponents of his previous works on the subject and of the grounds upon which he based his arguments. As one who has taken a certain interest in the subject I must confess my surprise at Dr. J. Wigglesworth's remarks. I fancy that had he been acquainted with Dr. Reid's "Alcoholism, a Study in Heredity," he would have considerably modified the statements which he made. The same applies to the remarks made by Dr. A. W. Gilchrist and others. Had these seen the work that I have mentioned,

together with "The Present Evolution of Man" written by the same author some little time back, they would have found that there their arguments had been completely forestalled and refuted. These works are the most interesting and lucid amongst those I have had the pleasure of reading and they throw an entirely new light upon the vexed question of heredity, a subject upon which medical men unfortunately seem to dogmatise without first studying the actual facts as they are seen in nature.

I may be allowed possibly to quote the remarks of your own reviewer on "The Present Evolution of Man": "The result is a monograph which is crowded with illustrative data and well-reasoned arguments, a work that requires to be read and studied many times before its full import is grasped, and one which deserves in its place as much attention from the thoughtful as any of the numerous writings that have arisen from this most fruitful doctrine."<sup>1</sup> Dr. Reid may be right or he may be wrong. It is not for me to say. At any rate, to my mind he has advanced arguments and proofs which in the various discussions that have taken place have never been refuted. I, and I think many others, would follow with the very greatest interest a discussion of his theories by those who have made a careful study of his works and have not, as happens with so many, jumped to immature conclusions after what has evidently been a most perfunctory perusal of his arguments.

I am, Sirs, yours faithfully,

A. MEARNS FRASER, M.D. Edin.,  
Medical Officer of Health of Portsmouth.

Portsmouth, August 24th, 1903.

## THE USES OF SODIUM SALICYLATE IN THE TREATMENT OF MALARIAL FEVER.

*To the Editors of THE LANCET.*

SIRS,—At p. 95 of THE LANCET of July 11th, 1903, Mr. A. D. E. Kennard has an article on this subject, in the course of which he says: "I find that this drug is very little used in India for malarial fever, in fact the practitioners there pin their faith to quinine," &c. It is true that medical men in India use quinine in malarial fevers; but it is incorrect to state that they do not use sodium salicylate as well. It is a very common treatment indeed to employ sodium salicylate either alone or in combination with other diaphoretics for relief of the *symptoms* of pain and pyrexia, while quinine is used at the same time for the *cure* of the malarial attack. Speaking for myself, I have systematically used sodium salicylate on myself and on my patients in malarial fever for over a dozen years, and I know that many of my brother officers in the Indian Medical Service do the same.

I am, Sirs, yours faithfully,

July 31st, 1903.

D. M. MOIR, M.D. Edin.

## THE ADMINISTRATION OF SOMNOFORM.

*To the Editors of THE LANCET.*

SIRS,—In THE LANCET of August 15th, p. 496, is a letter under the heading "The Administration of Somnoform," in which the writer alludes to the description of an inhaler for somnoform by Mr. Vernon Knowles appearing in THE LANCET of August 1st, p. 323. The writer speaks of this apparatus as an excellent one, but goes on to say that he personally does not see the need of any special apparatus beyond the usual Clover's bag attached to the ordinary facepiece which he lines with cotton-wool, leaving the entrance to the bag free. Having used Mr. Knowles's inhaler almost daily for the last three months with excellent results I should like to point out that the most important features of this inhaler have been overlooked by Dr. W. Bernard Secretan and certainly do not obtain in his method. The fact that the supplemental bag of this inhaler is reversible and can also be sterilised is obviously a great improvement on the ordinary Clover's bag.

Again, the air valve obviates the usual though very objectionable procedure of the administrator inflating the bag with his own breath before presenting the anæsthetic. When once the facepiece has been applied there is no need to remove it until complete narcosis has been obtained, the air-supply being regulated by the valve. In using this

<sup>1</sup> THE LANCET, June 13th, 1896, p. 1647.

inhaler the period of induction is lessened and a smaller quantity of somnoform is required.

The lint cone, too, is a more convenient medium for conveying somnoform than cotton-wool, as the former can more easily be adjusted to the facepiece and can be kept in exact position by the four-armed spring and is so quite out of the way and cannot touch the face of the patient. In giving anæsthetics for dental practitioners plenty of space in the facepiece is very necessary, especially when a mouth prop of the Brunton variety is used, which projects one inch or more in front of the incisor region.

I find it preferable to give two and a half cubic centimetres or half the dose marked on the bottle, repeated if necessary, as with this quantity I have found no tendency to sickness, whereas with larger doses a feeling of sickness or actual vomiting sometimes occurs.

As somnoform is a very satisfactory anæsthetic for minor operations and as a preliminary to ether administration further correspondence from those who have had experience with it would be of interest.

I am, Sirs, yours faithfully,

ERNEST C. MAGUIRE, M.D. Aberd.,

Honorary Anæsthetist, Brighton Dental Hospital.

August 22nd, 1903.

## THE TREATMENT OF GOITRE BY THE USE OF DISTILLED OR RAIN WATER.

*To the Editors of THE LANCET.*

SIRS,—The interesting point is that Dr. C. A. Rayne finds that his patients are losing their goitres by the simple plan of avoiding the "cause," whatever it be, contained in drinking-water. This, of course, can only apply to the "acquired" disease. In the article "Thyroid" in the *Encyclopædia Medica* Dr. G. R. Murray states the same conclusion in practically similar terms. The *boiling* of all drinking-water, he says, may in early stages be followed by disappearance of the goitre. The connexion of the water-supply with endemic goitre is fairly generally recognised, and while one authority (Quain's "Dictionary," 1883) advises to abstain from drinking the water in goitrous districts, another (Heath's "Dictionary of Practical Surgery," 1886) says, "The patients and those living in goitrous districts ..... should drink only *boiled* water." Yet I venture to think that Dr. Rayne is correct in regarding this "old" advice about using only *boiled* or *distilled* or *rain* water as not insisted upon or rigorously carried out in practice. Dr. Murray further suggests that a living micro-organism is indicated as the probable cause of the disease from the fact that the water containing it is rendered harmless by boiling. If this be found equally as efficacious as distilled or rain water *boiling* would seem to be the least troublesome process.

I am, Sirs, yours faithfully,

Urmston, August 24th, 1903.

T. CARTER BOOTH.

*To the Editors of THE LANCET.*

SIRS,—Although, as Dr. L. E. Stevenson points out in THE LANCET of August 15th, p. 493, the treatment of goitre by means of distilled water is nothing new, yet I think the following case which occurred in my own practice a good many years ago may be of interest to your readers in connexion with the various letters which have appeared on the subject in your columns. The patient was a girl, aged about 15 years, who had a rapidly increasing parenchymatous goitre of short standing. The sole water-supply to the house in which she lived was rain-water collected from the roofs. The drinking of such water should not produce goitre, but the main collecting-pipe which led from the roof to the cistern crossed the bottom of a dung-pit and was cracked. The water consequently had been getting polluted with stable manure and when this defect was remedied and the girl drank plain unfiltered rain-water the goitre gradually disappeared. The contamination of surface water from farm and stable yards is quite recognised as a factor producing goitre. It has been pronounced to be due to the presence of ammonia salts or free ammonia, but I do not know whether the presumption is warranted. I am sure that goitre is much less common in this district than it was 15 years ago, probably owing to the disappearance of surface wells and unprotected spring supplies, although the water-supplies of this place are of rather more than average hardness, one supply—viz, that of the Kent Water Company from the Orpington

wells—being very hard indeed. But there is nothing new in all this, and I believe myself that the sole effect of rain or distilled water is that the imbibition of poisonous material, which produces a proliferating catarrh of the thyroid acini, is withdrawn.

I am, Sirs, yours faithfully,

Westerham, August 26th, 1903.

ARTHUR MAUDE.

## INTESTINAL ANASTOMOSIS FOR PROLAPSED SMALL INTESTINE.

*To the Editors of THE LANCET.*

SIRS,—I read with great interest the case of Intestinal Anastomosis for Prolapsed Small Intestine by Mr. R. W. Murray in THE LANCET of August 22nd (p. 524) and I think that all will congratulate him on the success of his operation. The point that I wish to refer to, and which is, I think, of great importance to bear in mind when operating on a similar case, is the fact that the boy died two years after the operation and at the post-mortem examination it was found "that a portion of intestine which had been occluded—namely, 18 inches of the ileum, the cæcum, and ascending colon—was enormously distended, filling the greater part of the abdomen. It was entirely shut off from the rest of the gastro-intestinal tract and filled with offensive mucopurulent fluid." Some years ago a German surgeon drew attention to the fact that the mucous membrane of the intestines secreted a fluid which had all the appearance of ordinary fæces notwithstanding that no food had been introduced into the intestine for some considerable time. This statement appeared at the time I was making some experiments in intestinal surgery at the laboratory of the Royal Colleges of Physicians of London and Surgeons of England and to test the accuracy of the statement I performed the following operation on a dog. I withdrew some 12 inches of small intestine and cut it across at each end. I then washed the loop thus drawn out thoroughly by allowing a current of weak carbolic solution to run through it. Having thus cleansed it of its contents I invaginated both ends and closed them with a few stitches. I then united the proximal end of the divided intestine with the distal, thus re-establishing the intestinal tract, the detached loop of intestine was then returned into the abdomen, and the parietes were closed in the usual manner. The dog experienced no perceptible inconvenience from the operation and took its food as usual. The abdominal wound healed quickly. After a few weeks there was perceptible distension of the abdomen and the dog was killed some short time afterwards. On opening the abdomen the occluded loop of intestine was found, as in Mr. Murray's patient, to be enormously distended and congested. On opening it it was found to be filled with a most offensive fluid of the consistence of gruel and having all the appearance of ordinary fæces as found in the small intestine.

The point of practical interest which arises from Mr. Murray's case and from my experiment is, I think, that in any case similar to Mr. Murray's in which it is found necessary to occlude a portion of the intestine, it would be wise to establish a fistulous opening between the occluded loop and another portion of the intestinal tract or with the abdominal surface. In Mr. Murray's case possibly, if instead of dividing the transverse colon and invaginating the cæcal end he had been content with performing a lateral anastomosis the same object might have been obtained.

I am, Sirs, yours faithfully,

August 24th, 1903.

FRED. BOWREMAN JESSETT.

## THE TITLE OF "DOCTOR."

*To the Editors of THE LANCET.*

SIRS,—A small addition to the Medical Acts Amendment Bill would remove a notorious and long-standing grievance. Clause 21 deals with registration after passing a State final examination. If to that clause were added the words "and such registration shall confer the title of 'Doctor,'" diploma holders would then be placed in a fair position and the public would no longer be puzzled by the registered medical practitioner, to whom they have gone for medical assistance, telling them that he is not a "doctor." If desired the actual degrees or diplomas can be easily shown, as "Dr. Jones, L.S.A.," "Dr. Brown, M.D.," or even "Dr. Smith, F.R.C.S." There is no suggestion here of conferring a degree, but of legalising