Kensington, Fulham, Finsbury, Shoreditch, Stepney, Poplar, Battersea, and Greenwich. The 81 deaths from measles were 75 below the corrected average number; among the various metropolitan boroughs this disease was proportionally most fatal in the City of Westminster, St. Pancras, Finsbury, Shoreditch, Stepney, and Battersea. The 31 fatal cases of scarlet fever showed a decline of 36 from the average number in the corresponding periods of the ten preceding years; the greatest proportional mortality from this disease occurred in Paddington, Kensington, Finsbury, Bermondsey, and Deptford. The 40 deaths from diphtheria were less than one-third of the corrected average number; this disease was proportionally most fatal in Kensington, Hackney, Bethnal Green, Poplar, Deptford, and Woolwich. The 55 fatal cases of whooping-cough showed a decline of 101 from the average number in the corresponding periods of the ten preceding years; among the various metropolitan boroughs the highest proportional fatality of this disease was recorded in Shoreditch, Stepney, Bermondsey, Battersea, and Wandsworth. The 21 deaths referred to "fever" were 32 below the corrected average number; the "fever" death-rate was highest in Fulham, St. Marylebone, Stoke Newington, and Hackney. The 599 fatal cases of diarrhoea were rather less than half of the average number in the corresponding periods of the ten preceding years; among the various metropolitan boroughs this disease was proportionally most fatal in Hammersmith, Fulham, Shoreditch, Stepney, and Greenwich. In conclusion, it may be stated that the aggregate mortality in London last month from these infectious diseases was nearly 55 per cent. below the average.

Infant mortality in London during August, measured by the proportion of deaths among children under one year of age to registered births, was equal to 144 per 1000. The lowest rates of infant mortality were recorded in St. Marylebone, Hampstead, Stoke Newington, Holborn, Lewisham, and Woolwich; and the highest rates in Kensington, Fulham, the City of London, Shoreditch, Stepney, and Greenwich.

THE SERVICES.

ROYAL NAVY MEDICAL SERVICE.

THE following appointments are announced:—Fleet Surgeons: W. H. Patterson to the *Dædalus* (dated Sept. 9th, 1903); H. Meikle to the *President*, lent to the *Spartan* (dated Sept. 12th, 1903); and C. W. Sharples to the *President*, additional, for temporary service at the Royal Navy Rendezvous. Staff Surgeons: W. Hackett to the *Northampton*, for the *Cleopatra*, J. E. Coad to the *Audacious* (dated Sept. 9th, 1903), and R. Miller to the *President*, for three months' course of hospital study (dated Sept. 15th, 1903); G. A. Dreaper to the *Fox*; E. Corcoran to the *Boscawen*, additional, for Portland Sick Quarters. Surgeons: R. D. Jameson to the *Racer*, for Royal Navy Cadets' Sick Quarters, Osborne (dated Sept. 9th, 1903); and A. Davidson to the *Partridge* (dated Sept. 10th, 1903).

ROYAL ARMY MEDICAL CORPS.

Lieutenant-Colonel W. L. Chester to be Colonel, vice Colonel R. Exham, retired (dated August 19th, 1903). Major G. Moir will proceed for service in Madras; Captain W. G. Beyts will proceed for service in the Punjab; Captain G. A. Moore will proceed for service in the Punjab; Captain N. H. Ross has been transferred from the Poona District to the Mhow District; Lieutenant J. M. H. Conway will proceed to India for duty. Captain H. Herrick has been appointed to the command of the Station Hospital, Satara. Captain A. Crichton Lupton is on temporary duty with the Coldstream Guards, pending embarkation for service abroad. Colonel W. McWatters has resigned the post of Principal Medical Officer, Southern District. Major J. R. Mallins has joined at Woolwich. Lieutenant R. B. Black is seconded for service with the Egyptian Army.

ARMY MEDICAL RESERVE OF OFFICERS.

Surgeon-Lieutenant William H. Vickery to be Surgeon-Captain (dated Sept. 12th, 1903). The promotion of Surgeon-Lieutenant-Colonel E. Luke Freer, V.D., is antedated to Dec. 24th, 1899.

ROYAL ARMY MEDICAL CORPS (MILITIA).

Lieutenant H. Fox is seconded for service under the Foreign Office (dated August 13th, 1903).

INDIAN MEDICAL SERVICE.

The following Lieutenants to be Captains (dated June 28th, 1903):—James Drummond Graham, Cuthbert Allan Sprawson, Maxwell MacKelvie, William Lapsley, William Henry Cazaley, Percy Alfred Browne, Walter Valentine Coppinger, Alfred Spitteler, James Charles Stewart Oxley, Henry Richard Macnee, Leonard Joseph Montagu Deas, William Mitchell Houston, William David Acheson Keys, George Joseph Grafton Young, James Good, Alexander Chalmers, William Gavin Hamilton, and Samuel Robert Godkin.

INDIAN MEDICAL SERVICE, BENGAL.

Lieutenant-Colonel Zalnoor Alee Ahmed, M.D. Glasg., retires from the service (dated July 19th, 1903) and Lieutenant-Colonel Sorabshaw Hormasji Dantra, M.D. Aberd., retires from the service (dated July 10th, 1903).

VOLUNTEER CORPS.

Royal Engineers: 1st Lancashire: Surgeon-Lieutenant H. Halton to be Surgeon-Captain (dated Sept. 12th, 1903).

VOLUNTEER INFANTRY BRIGADE BEARER COMPANY.

5th London: Surgeon-Captain W. N. Evans, from 1st London Volunteer Infantry Brigade Bearer Company, to be Captain (dated Sept. 12th, 1903).

ROYAL ARMY MEDICAL CORPS (VOLUNTEERS).

The following announcement is substituted for that which appeared in the *London Gazette* of June 26th, 1903, under the heading "Gordon Volunteer Infantry Brigade Bearer Company":—The Aberdeen Company: The under-mentioned gentlemen to be Lieutenants: James Smart (dated June 27th, 1903) and John Wallace Milne (dated June 27th, 1903). The transfer of Lieutenant F. Kelly from the Royal Army Medical Corps (Volunteers) the Aberdeen Company, to the Gordon Volunteer Infantry Brigade Bearer Company, which was announced in the *London Gazette* of August 14th, 1903, is cancelled. The Woolwich Companies: Lieutenant A. H. Minton to be Captain (dated Sept. 12th, 1903).

Under the new order which provides for a special sanitary officer at the headquarters of each military district Major C. J. W. Tatham, R.A.M.C., has been appointed in that capacity to Devonport.

Correspondence.

"Audi alteram partem."

MEDICAL DOCTRINES OF HEREDITY.

To the Editors of THE LANCET.

SIRS,—May I trespass upon your space to draw attention to what seems to me to be one of the important lessons to be drawn from the present keen interest which is being manifested in the subject of the hereditary nature of alcoholism? Whatever may be the views held by particular individuals as to the opinions of Dr. Archdall Reid, expressed in his books and in the present correspondence in THE LANCET, there can be but one opinion as to the enormous importance of the subject and the thanks of the profession are due to Dr. Reid for forcing it upon our attention.

I do not wish to intrude at present in the precise point under discussion; I rather wish to note one of the sidelights of the question, which I think should be attended to in the future. It is impossible to have read the criticisms of Dr. Reid's paper at the recent meeting of the British Medical Association and the subsequent correspondence in THE LANCET without being struck with the extraordinary variety of opinions held upon the biological aspect of the matter. One is forced to the conclusion that heredity and evolution are somewhat nebulous terms to many, and perhaps it is not easy to be very precise in the meanings attached to the former at any rate. The real reason for this is that no systematic attempt is made to teach these subjects in the medical curriculum. I am, of course, aware that every student learns more or less of such problems in biology, but he does so during his first session, when he is frequently a mere school-boy, and whatever little knowledge is then gained is speedily forgotten in the more practical business of the strictly professional subjects for his subsequent examinations. The same remark applies to embryology. How many

men on the day they take their qualification or degree have anything but the vaguest notion of modern scientific research and thought on such subjects as heredity, immunity, and evolution? From personal experience in teaching I should say the number is extremely few. The fault is not the student's; it is simply that he is not taught. And yet nothing is more certain than that the future of medicine is becoming more and more a biological matter and that it is to biological research we must look for the explanation of the most important problems we are called upon to face.

What is the remedy? The defect is in the curricula and the remedy must be applied there. My own opinion is that instead of compelling the student to acquire an enormous number of facts in natural history and botany, which are of no earthly use to him afterwards, a course of lectures on the great *problems* of biology should be substituted, and these lectures should have a special reference to the connexion of these problems with disease. Instead of dismissing biology at the end of the first session with a mere smattering of superficial knowledge, defer the consideration of these important questions until the student has become familiar with some of the facts of medicine and then teach him thoroughly what is known as to the nature of these biological phenomena. In that way he will come to have a really scientific conception of medical science and the knowledge will come to him at a time when its bearing on medicine will be evident. Under the present system these great questions are dealt with too soon or not at all and the student forgets them as soon as possible without ever realising anything of their true significance. Is it any wonder that after some years in medical practice the average man is found to know little or nothing about the most vital questions of his own profession and science?

It cannot be much satisfaction to professors and lecturers on botany and natural history to have to give an elaborate course of lectures to a number of students just through their preliminary examination, knowing, as they must do, that the men have no chance of getting a real grasp of the subject. It would be a real satisfaction to these same teachers to teach the great principles of these subjects to advanced students who could appreciate the bearing of them upon their own special studies. In such a system the practitioner would recognise that the problems of heredity, immunity, and evolution, as applied to disease, faced him at every turn of his work and were indeed the most fascinating of the problems which engaged his attention.

I am, Sirs, yours faithfully,

GERALD R. LEIGHTON, M.D. Edin.,
Professor of Pathology, Royal Veterinary College,
Edinburgh.

Sept. 7th, 1903.

To the Editors of THE LANCET.

SIRS,—I deplored in my recent letter that so many of Dr. Archdall Reid's opponents should by their remarks show that they were quite ignorant of that author's works. Amongst these must, from the sixth and seventh paragraphs of his letter of August 29th, be classed Dr. Charles Mercier. Had this gentleman read Dr. Reid's "Alcoholism" these two paragraphs would, I suspect, have never been penned. Surely the least that can be asked of Dr. Mercier is that at any rate he should first make himself acquainted with the works of the gentleman whose views he is apparently so anxious to belittle.

Dr. Mercier's conclusion that Dr. H. Laing Gordon and I support Dr. Reid's views because we are Scotsmen is quite on a par with other of his scientific deductions.

I am, Sirs, yours faithfully,

Sept. 8th, 1903.

A. MEARNS FRASER.

To the Editors of THE LANCET.

SIRS,—There are several matters in Dr. Niven's letter dated Sept. 3rd¹ which I should like, with your permission, to notice next week; but as to one matter Dr. Niven makes an appeal in terms so solemn, so urgent, so piteous, that it would be discourteous to postpone the answer, especially as Dr. Niven makes no secret of his opinion that I have no answer to give and that no answer is possible. But for Dr. Niven's impassioned appeal I should certainly not have entered upon this subject, for I think it unlikely that you would care to occupy your correspondence columns with an exposition of the rudiments of biology. If I had to

explain the matter to a biology class I should tell them to clean their slates (but not on their pinafores) and take down the following statement:—

Every sexually produced organism is the offspring of two parents. One of these parents is male and is called the father, the other is female and is called the mother. In so far as the characters of the father and mother are alike, the corresponding characters of the offspring almost always reproduce those of both the parents. For instance, if the father and mother each have four legs and one tail the offspring will almost certainly possess four legs and one tail. But as to those characters in which the parents differ, the corresponding characters of the offspring obviously cannot faithfully reproduce those of both parents, and in such cases the characters of the offspring may follow one of several courses. In respect to these characters (these in which the parents differ) the characters of the offspring may (1) resemble either parent to the exclusion of the other (for instance, if the father be white and the mother black the offspring may be wholly white or wholly black); or (2) the offspring may at one time of life resemble one parent and at another the other (it may, for instance, be born white and subsequently turn black); or (3) the differing characters of the parents may be mixed in the offspring, as when the progeny of a white and a black parent is piebald; or (4) the offspring may present, with respect to these characters, a mean or average between the two parents, as though the differing characters were intimately blended together, as when the progeny of a white father and a black mother is a mulatto. Now apply this doctrine to the case of the offspring of a drunken father. The father is, *ex hypothesi*, a drunkard, but one of the counteracting circumstances which Dr. Niven demands, and of whose existence he is incredulous, is the circumstance that the offspring has *two* parents! and the mother may not be a drunkard. She may have as great an abhorrence of drink as Dr. Niven has of a neo-Lamarckian, whatever that may be, or as I have of a misquotation. In such a case the offspring may inherit solely the father's propensity for drink or solely the abhorrence of the mother; or he may take after his father at one time of life and after his mother at another; or he may have their qualities blended or averaged and have just a healthy relish for a glass of beer after a long day in the hayfield.

It is perhaps too much to expect that Dr. Niven's researches in heredity, which appear to have been of no very long standing, should have revealed to him a fact so obscure, so recondite, and so seldom recognised as that every child has a mother as well as a father; but if he perseveres he will in time come across another counteracting circumstance, to which I first called attention many years ago, which may save the offspring from being a drunkard and the race from extinction even if both his parents are drunkards. This is the remarkable and powerful tendency that the members of every race evince, in ordinary circumstances, to return to the norm from which their parents have deviated. The children of giants are never so tall nor are the children of dwarfs so short as their parents. This tendency has been investigated by Professor Karl Pearson in an immense number of cases and has by him been measured and expressed in figures as to certain characters. As he very aptly and justly says, "In the tenth generation a man has 1024 tenth great-grandparents. He is eventually the product of a population of this size, and their mean can hardly differ from that of the general population. It is the heavy weight of this mediocre ancestry which causes the son of an exceptional father to regress towards the general population mean; it is the balance of this sturdy commonplaceness which enables the son of a degenerate father to escape the whole burden of the parental ill."

In your next issue I expect to see that Dr. Niven credits me with the belief that every offspring has three parents, or that giraffes lengthen their necks by stretching, or that parents are produced by their own offspring, or something equally absurd. I appeal, therefore, to your readers to take the beliefs of Dr. Niven's opponents, not from what Dr. Niven says, but from the statements of those who best know what these beliefs are—the opponents themselves.

I am, Sirs, yours faithfully,

Catford, Sept. 14th, 1903.

CHARLES MERCIER.

To the Editors of THE LANCET.

SIRS,—Will you allow me to apologise to Dr. J. Wigglesworth? I have wrongly represented him as saying that parental drinking causes filial intemperance. Need I

¹ THE LANCET, Sept. 12th, p. 786.

say that this was not willingly done. I remember after reading his first letter turning to "The Foundation of Death" to see how far his views agreed with those of other alienists—e.g., Krafft-Ebing—and must, I suppose, in the process have mixed their opinions. I am deeply grieved and I can only say I am very sorry. I should have liked to be able to leave the above standing by itself, but Dr. Wigglesworth also represents me as saying that I charged him with holding the belief that acquired traits are transmissible. That I never did. I believe if Dr. Wigglesworth will again read the passage where he thinks I make this charge he will at least do me the justice to say that he also has made a mistake.

With regard to the third point where I use the word "special" instead of "general": I do not think that I thereby gain any advantage in this discussion nor do I think that Dr. Wigglesworth loses any. In trying to account for his belief that parental drinking causes filial mental instability, Dr. Wigglesworth represents the germ cell as containing or being composed of numbers of molecules which subserve the development of the various tissues of the body; those for the cerebral centres being the latest formed in time, and therefore more unstable than the others, suffer first from any injurious agent. I stated in my reply that an alteration such as this was simply a variation, or rather that the offspring varied in a *definite* direction; also as the cerebral centres suffered first it did not seem to matter much what term we used. I thought, and I do think, that I was justified in saying a variation was the result and that Dr. Wigglesworth would be in perfect agreement with me, for in THE LANCET of August 1st, p. 340, he says: "If as a result of the development of the germ cells in a vitiated nutrient medium the child resulting therefrom is modified in any direction, however slight, then a definite germinal variation has been produced through the agency of the poison in question." If a variation is produced I can only look upon the resulting offspring as having varied in a definite or special direction. There is, however, no need to labour the point. It is a matter of indifference to me what qualification is used—i.e., whether the influence be general, special, or definite. All I insist on is that a variation has been produced, and variations, as we know, tend to accumulate and to be transmitted. The Darwinian doctrine, so far as I understand it, depends upon (1) increase of organisms in a geometrical ratio; (2) struggle for existence; and (3) variation. Let anyone destroy all that is meant by variation and for me Darwinism is destroyed.

May I add to Dr. Wigglesworth's summary of this discussion that only beliefs have been adduced to support the contention that parental intemperance causes filial mental instability. These have been called in question and demands have been made for proof but without success. It seems that all that we have heard in the past (I am not now referring to Dr. Wigglesworth) as to the transmission of this or that acquired trait has simply been so much chatter. Dr. Wigglesworth has propounded a new theory of evolution; he has also endeavoured to show *how* poisons can produce variations in germ cells from which offspring arise. If he can prove the existence of his molecules and also prove the rôle he makes them play, he will have accomplished an astounding piece of work, the results of which will be far-reaching.

In THE LANCET of Sept. 5th, p. 731, Dr. C. Mercier, in one of his characteristic letters, condescends to indicate one of the "counteracting circumstances which would prevent the giraffe's neck growing as high as the cross of St. Paul's." He has "ballyragged" others for not defining their terms and for not giving proof of their assertions, and yet here we have Dr. Mercier himself in delightful simplicity asserting that the neck would crush the animal. Is this not a flourish without proof? I wonder if Dr. Mercier has ever heard of the correlation of growth or of the co-adaptive variation of all the structures that subserve any given condition? Is there anything more certain than that co-adaptive parts vary together and as natural selection has developed in the structures an immense power of varying under fit stimulation it is absolutely certain that Dr. Mercier, with all his fancied precision, is wrong. Mr Darwin, in "Animals and Plants," vol. ii., p. 329, quotes from Mr. Herbert Spencer the case of the "Irish elk with its gigantic horns over 100 pounds in weight" and refers to the "coördinated structures necessary—namely, a thickened skull, strengthened cervical vertebræ, with stronger ligaments, enlarged dorsal vertebræ, powerful fore-legs and feet," &c., indeed, there

is hardly one structure in the body capable of varying that would not vary. If it be argued that it would be impossible for all these structures to vary favourably in one individual and that therefore there would be no selection, it must be remembered that superiority in one quality is commonly associated with superiority in other qualities and that selection depends on a general average superiority. Now, suppose a giraffe develops a longer neck under a given amount of stimulation than its progenitors did with the same amount of stimulation, then an extra strain is put on all co-adaptive structures and in them also a proportionate increase in size would result. The length of the neck therefore would depend upon the inborn power to vary under stimulation and the amount of fit stimulation to which it was submitted. As this is so and as the later development of nearly all structures in higher animals is an acquirement, and as what is transmitted is the power to vary in response to stimulation, it follows that there is hardly any limit to which a structure might not grow if required by natural selection. Thus, if it were required, say, for browsing purposes that the head of the giraffe should reach the top of the cross at St. Paul's there is absolutely no reason why it should not do so, and Dr. Mercier cannot disprove it.

Dr. Mercier defines a variation "as a difference between parent and offspring not due to post-natal action of the environment on the offspring." Here, again, there is no definition of terms, but I suppose he means by post-natal what it would mean in medical jurisprudence—live-birth. How delightfully simple and clear everything seems after one gets everything explained. How long must this truth have been "jumping up and hitting every Darwinian in the face" and they did not see it or feel it! Had they only been logicians! Just fancy how easy it all is now! If a man takes small-pox it is an acquirement, but if a child is born with the rash fully developed on him it is a variation; if a man has his arm amputated it is an acquirement, but an intra-uterine amputation is a variation; if a man has an eye gouged out it is an acquirement, but if a child has an eye dislocated by unskilfully applied midwifery forceps it is a variation. Leaving this, however, may I point out to Dr. Mercier that "the whole controversy is" *not* "on the question whether this difference has its root in the original constitution of the germ plasm or whether it may not be due to the action on the germ plasm of its environment."

As regards the legs of the kittens Dr. Mercier says that he used no argument and nothing is more distasteful to him after an argument than a mis-statement of fact. He says that I stated a universal negative and that his simple affirmative destroys it. Well, but does it? Two things which differ in every detail are in reality exactly the same. I am sure that no one but a logician like Dr. Mercier could have discovered that.

I have confined my reply to the things to which I might legitimately claim a right to reply and I may say, in conclusion, though it is always risky to give hostages to futurity, that I am confident that at the end of this controversy I shall have a little more scalp and a little more hair on that scalp left than Dr. Mercier will.

I am, Sirs, yours faithfully,

Liverpool, Sept. 13th, 1903.

C. R. NIVEN.

To the Editors of THE LANCET.

SIRS,—Dr. Reid repeats his argument, taking the case of malaria as an illustration, without meeting my objection. I can only suppose that my former letter was obscure. May I therefore explain precisely what I meant? My whole point was to show that "the mean of the whole race," as Dr. Reid puts it, is not necessarily lowered by the influence of alcohol or disease, *even if it does cause degeneracy*. It would be so if the dead counted, but they do not for purposes of reproduction. When a disease—malaria, for instance—attacks a community the elimination of the unfit by death may surely more than counterbalance the degeneracy of the survivors, leaving a higher mean vitality in spite of the injury to them individually. May I give an arithmetical illustration to make it clear? Take a few numbers at random to represent the vitality of individuals—3, 6, 11, 1, 13, 2. The average of these is 6. Subtract 3 from each to represent a diminution of vitality. Three of the numbers fall to zero, illustrating elimination of the unfit, and no longer count. We have left 3, 8, 10, the average of which is 7—higher than before we subtracted 3 from each! The conclusion to

my mind is therefore irresistible that it is at least possible for degeneracy and elimination working together to raise the mean vitality of the whole race and the more virulent the disease the more rapid and complete its evolution and conquest of that disease—not, as Dr. Reid says, its degeneration and extinction.

I am, Sirs, yours faithfully,

Sept. 12th, 1903.

C. E. SCUDAMORE.

To the Editors of THE LANCET.

SIRS,—Dr. C. Mercier has hardly grasped the spirit of my apology. I imagine any acquaintance with a sense of humour will enlighten him. By turning to the communication which opened this discussion the readers of THE LANCET will be able to gauge the amount of justification for all this indignation—for these quotations from Shakespeare, these references to Almighty God and worms and black-beetles, and so forth. It is perhaps necessary to add that Dr. Mercier had not entered my thoughts when I wrote it. If he really insists on knowing why I treat him so “haughtily” I suppose I must inform him, however much I may regret the necessity. It is because, in a scientific discussion, he has evaded the real points at issue and committed the unpardonable offence of limiting himself almost solely to small personalities and verbal quibbles. One need not go beyond his last letter to prove this. He denies that he asserted that “alcohol diseases, &c., affecting parents cause offspring to be degenerate.” He is quite right, but it is remarkable that he should call attention to the fact. What, then, is his position in this discussion? Has he come in solely to chop logic and for the laudable purposes I have indicated? After a dissertation on courtesy and dignity he proceeds to explain that he has scalped Dr. C. R. Niven and that Dr. H. Laing Gordon and Dr. A. Mearns Fraser agree with me only because we are all Scots. Much space in his letter is occupied by what, for his own sake, I can only hope is an intentional misreading of very plain meanings. Thus, when Dr. Niven stated that no two individuals were exactly alike in any particular he retorted that kittens are alike in having four legs and now asserts triumphantly that he has thereby destroyed Dr. Niven’s universal negative. I am surprised he did not add that kittens are alike in being kittens. When I wrote of South Europeans and West Africans as being very temperate the context showed clearly that I was alluding to the inhabitants of countries where alcohol was particularly abundant—the wine and toddy countries. He retorts with a list of all the peoples from Lisbon to Constantinople and Tangiers to Cape Town. I spoke of a patent fact. He announces that by patent I mean closed and asks for information which would about fill an issue of THE LANCET and which I have given elsewhere. I raised a discussion by declaring that medical men are mistaken in supposing that the effects of disease are transmissible and gave my reasons at elaborate length. He elegantly adjures me to abandon “storming, hectoring, and ballyragging,” and to try reasoning. No one attempts to refute those reasons, least of all Dr. Mercier, to whom, it appears, argument is abhorrent. His luminous retort is that I have committed a fault in logic. It seems that when I declare a thing to be true and no one is able apparently to controvert me I must not assume it is true. That is *petitio principii*. It is unnecessary to multiply examples, but I am now forced to explain to Dr. Mercier that if he cannot perceive that these ignominious personalities, these pretended misunderstandings of plain meanings, these merely verbal carpings, are not infinitely petty and irrelevant, I can very clearly. I shall be glad to deal with any scientific points that he may raise; indeed, I propose to deal immediately with two or three of these, but in the future I shall leave all questions as to my “haughtiness” and the like alone. I will only add that if he desires to bring my pride to a fall he has but to defeat me on real issues. He has only to prove that any influences, no matter what—disease, alcohol, privation, anything—acting on parents are causes of variations in offspring to render my position humiliating and absurd. After all this is only a preliminary skirmish. I hope before long to call the attention of the readers of THE LANCET to questions of wider interest and deeper import. Imagine my chagrin if I be overwhelmed at a vital point at the very beginning of the campaign.

Now for the scientific points which Dr. Mercier thinks will prove “disconcerting.” He believes apparently that the modern giraffe has a long neck because ancestral

giraffes stretched their necks and the stretching was transmitted, but that the giraffe’s neck is not now as high as the cross of St. Paul’s because the weight of it would crush the animal. Has anyone ever heard of a giraffe being crushed by the weight of its neck? We do not know that the individual giraffe can add a cubit to its stature by stretching, but we do know that the muscles, the bones, and the ligaments of an individual grow in size and strength when exposed to increased strain. If, then, the neck is capable of such facile evolution why not the supporting structures? The following is Dr. Mercier’s definition of a variation. “By a variation I mean a difference between parent and offspring not due to post-natal action of the environment on the offspring.” It appears, then, that if a child be bruised by forceps during delivery the injury is a variation, an inborn trait, but if he be circumcised immediately after the mutilation is an acquirement. The definition is interesting as explaining a good deal in Dr. Mercier’s scientific attitude, but unfortunately will be accepted by no one who has a knowledge of the subject. Let me then attempt a definition myself and Dr. Mercier can pick it to pieces if he is able. Without definitions that really define we cannot have clear ideas or a common basis for discussion. An inborn trait (including variation) is one which takes *origin* in the germ plasm. It arises because the germ plasm is so constituted that under fit conditions of fertilisation, shelter, and nutrition, it tends to proliferate into an individual having that trait. An acquirement, on the other hand, does not take *origin* in the germ plasm. It is a modification of an inborn trait caused by the action of the environment on that trait *after* it has begun to develop from the germ plasm.

Dr. Mercier mentioned “bud variations” at Swansea and now seems to imagine that I evaded this point through inability to meet it. As a fact my time was scanty and the objection had already been disposed of many times and thoroughly. Let us take the case of the nectarine. When a gardener has grown a good variety of peach from seed he propagates it by cuttings. A cutting is a detached portion of the tree whence it is taken, and trees thus grown, no matter how numerous or how many in succession, still remain detached portions of the original tree which sprang from a seed. A thousand peach trees in an orchard may each have a thousand twigs and branches. One of those branches may bear nectarines. Since the peach differs from the nectarine the difference must have been caused somehow. But how? Only two ways are conceivable. Either the branch was exposed to external influences (wind, weather, nutrition, &c.) so surprisingly different from what the rest of the branches in the orchard were exposed to that it produced a nectarine instead of a peach, or else that portion of the germ plasm from which that particular branch arose was different originally—i.e., in the seed—from the germ plasm from which the other branches arose. In short, the difference must have been either in external influences or in the germ plasm. Can anyone, except, perhaps, Dr. Mercier, believe that the difference was in the external influences? I cannot do better than quote from Professor Adam Sedgwick’s presidential address at Dover to the Zoological Section of the British Association. “In so far as these bud variations are not of the nature of acquired variations produced by a change of external conditions, and disappearing as soon as the old conditions are renewed, they are probably stages in the growth and development of the organism. That is to say they are of the same nature as those peculiarities in animals which appear at a particular time of life, such as a single lock of hair of a different colour from the rest of the hair, the change in the colour of the hair with growth, the appearance of insanity or epilepsy at a particular age. There is nothing more remarkable in a single bud on a tree departing from the usual character at a particular time of life than in a particular hair of a mammal doing the same thing.”¹

“If Dr. Reid means, as he appears to mean,” says Dr. Mercier, “disease and alcohol alone [produce evolution among civilised men], I not only doubt it, but think it preposterous.” Yes, I do mean it. But it seems that Dr. Mercier has discovered that religion, laws, languages, commerce, and many other influences also produce evolution. He is not inclined to “swagger” about these discoveries. They jumped up and hit him in the face. Will he then tell us how they produce evolution and what the

¹ Nature, Sept. 21st, 1899, p. 503.

precise nature of that evolution is? I do hope, however, that we shall not be treated to another example of that confusion between evolution and mere development, which depends on a confusion of the inborn with the acquired and which is such a nuisance in so many popular treatises.

The letter of "M.D. Edin." is a little puzzling. If he means to imply, as I think he does, that heredity lies at the root of every social and moral question I agree with him. Hence the importance of the study. But if he means to imply, but of this I am very doubtful, that we should suppress the truth for fear of disturbing some of the arguments which clergymen have founded on error then I disagree very strongly. That would be profoundly immoral.

To Mr. Sidney Vines I need only reply that I have "no practical suggestion whatever to make concerning the prevention of drunkenness" at present. We are discussing quite a different question.

I am, Sirs, yours faithfully,
Southsea, Sept. 12th, 1903. G. ARCHDALL REID.

OBSERVATIONS ON MASTICATION.

To the Editors of THE LANCET.

SIRS,—Dr. Harry Campbell's objections to the use of antiseptic tooth powders and mouth washes have been assailed as being "quite contrary to belief, acknowledged practice, and observation." We know that they are contrary to belief and acknowledged practice, but to say that they are contrary to observation is wholly unjustifiable. Indeed, clinical evidence of the value of antiseptic mouth washes is conspicuous by its absence and the absolute failure of acknowledged practice to check the ravages of dental caries at the present day is one of the most dismal facts which we are compelled to recognise. I do not know that any observations or tabulated results have been recorded to show that those people who use such antiseptic methods are in any way more free from caries than others. On the other hand, the evidence is overwhelming that peoples who consume food which compels efficient mastication are relatively free from dental troubles. In fact, dental caries is inversely proportional to the amount of mastication. I have particularly noticed that patients who carefully adopt the acknowledged practice are not only specially subject to "abrasion," "erosion," and recession of the gums, but are, as a rule, most liberally supplied with carious cavities or "fillings." I feel sure that the use of antiseptic mouth washes is only sanctioned by purely theoretical considerations. Dr. W. D. Millar showed that dental caries was a chemico-parasitical process and immediately he had done so he and the whole dental profession at once embraced the theory that we must destroy the micro-organisms and neutralise the acid in order to avoid the disease. It never was surmised that this chemico-parasitical process which causes the disintegration of the teeth was but an intensification or perversion of a normal process. Consider the nature of the micro-organisms of the mouth. Some are acid producers, some are liquefiers, and some produce acid or liquefy according to the medium in which they exist. It is obvious that those bacteria which liquefy gelatin and other albuminoid substances must be of great importance in rendering shreds of foodstuffs soluble and so helping to clean the buccal cavity, and within limits the acid producers may have a similar function. In fact it would seem impossible to do without the mouth bacteria as there is no ferment in the mouth capable of digesting the albuminous shreds and particles which otherwise would be bound to accumulate between and about the teeth. It may be said that at least we would be well to be without a considerable proportion of the acid-producing bacteria, for without acid it is impossible to have dental caries. No doubt this is so. But this result is best obtained by efficient mastication, for food which requires considerable mastication calls forth a corresponding flow of saliva which gets thoroughly incorporated with the food. The ptyaline converts the starch into a soluble form, while the mastication expresses it from the rest of the bolus. Thus, therefore, the starchy matter is more or less thoroughly swallowed, while the remaining part is being more completely masticated. By this means the smallest possible residue tends to lodge about the teeth and what little does remain, being almost devoid of starch, allows the bacteria which liquefy to predominate while preventing the multiplication of the acid-producing bacteria. If, however, food—especially starchy food—is taken in a soft form the

flow of saliva is, relatively to the amount of starch, but inefficiently stimulated. The starch is not fully converted and swallowed but remains plastered, as it were, into all the crevices in and about the teeth. Furthermore, the chronic lack of efficient stimulation of the salivary glands engendered by relatively soft foods leads, no doubt, to a corresponding lack of development and output of ptyaline. Now if there is a relatively great amount of starch left about the teeth can we wonder that the acid-producing bacteria largely preponderate and play havoc with the teeth?

Of course, in exceptional circumstances, as during illness or after a very improper diet, the use of antiseptics may be advocated, "but under normal conditions their use is, I believe, to be deprecated."

I am, Sirs, yours faithfully,
Wimpole-street, W., Sept. 12th, 1903. J. SIM WALLACE.

To the Editors of THE LANCET.

SIRS,—The following experiment may be of interest during the controversy.

Eat a small piece of bread and notice the doughy feeling which it attains during the process. Brush the teeth with a solution of odol (I used the peppermint variety). Then eat another piece of bread and notice that the doughy feeling does not occur to anything like the same extent even after prolonged mastication. As a matter of fact, the gullet becomes aware that the bread is still quite granular when it is swallowed. This would seem to indicate an arrest in the production of ptyaline and a failure in the conversion of the starch, but the solution of that question I leave to other hands. It is, at least, a result of the use of a powerfully antiseptic tooth cleanser. I am, Sirs, yours faithfully,

Guernsey, Sept. 14th, 1903. J. AIKMAN.

UMBILICAL CALCULUS AND THE URACHUS.

To the Editors of THE LANCET.

SIRS,—The report published in THE LANCET of Sept. 12th, p. 747, of a Case of Umbilical Calculus by Mr. J. R. Harper and Dr. C. G. Seligmann is of considerable importance. It will interest those gentlemen to know that besides Shattock and Dean's cases about a dozen others have been collected by Ledderhose, Pernice, Volkmann, and Otto Hahn, whose article, "Ein Nabelkonkrement von Taubeneigrösse," was published three years ago in the *Beiträge zur klinischen Chirurgie* (vol. xxvi., p. 80). The umbilical calculus in Hahn's case measured 30 by 20 millimetres and was of the size and shape of a pigeon's egg. The patient, a man, aged 43 years, was of dirty habits—a common clinical feature, Hahn remarks, in all reported cases.

In relation to umbilical calculus is the question of urachal fistula, which I suggested in the article "Urachus" in the "Encyclopædia Medica." Although the usual cause is no doubt irritation from accumulation of dirt it is possible that in some instances the upper portion of the canal of the urachus remains open, communicating by a minute orifice easily overlooked with the surface of the integument in the umbilicus. The patent canal would then be liable to infection, so that it would discharge morbid material into the umbilicus which would mix up with epidermis and foreign matter. Spencer Wells found urinary (?) concretions in the canal of a urachus which was closed at both ends and Wutz twice detected suppurative inflammation in a minute cystic dilatation representing an unobliterated portion of the urachal canal. Such conditions being known to exist we can understand how, if the uppermost part of the canal remained open, it might inflame and cause the development of concretions in the umbilicus.

I have repeatedly observed, when removing abdominal tumours, incomplete closure of the urachal canal and therefore I suspect that the urachus is more often a source of local mischief than is generally supposed. No doubt a well-established urachal fistula gives rise to very prominent symptoms and a vesical calculus in the dilated urachal canal, as in the case of Paget of Leicester, would never be mistaken for an umbilical calculus. Still, it is possible that in exceptional cases the urachal canal remains patent, though extremely narrow, throughout, so that when the bladder is unusually distended a few drops of urine might occasionally pass up it and irritate the integuments of the umbilicus. The