

the three preceding weeks. The deaths referred to diseases of the respiratory organs in London, which had been 132, 110, and 107 in the three preceding weeks, rose again last week to 120, but were 11 below the number in the corresponding period of last year. The causes of 55, or 1.2 per cent., of the deaths in the 76 towns were not certified either by a registered medical practitioner or by a coroner. All the causes of death were duly certified in West Ham, Bristol, Salford, Bradford, Newcastle-on-Tyne, and in 46 other smaller towns; the largest proportions of uncertified deaths were registered in Liverpool, St. Helens, Warrington, Rochdale, Sunderland, and South Shields.

HEALTH OF SCOTCH TOWNS.

The annual rate of mortality in eight of the principal Scotch towns, which had been 15.4, 14.7, and 14.6 per 1000 in the three preceding weeks, rose again to 15.4 per 1000 during the week ending August 15th, but was 0.3 per 1000 below the mean rate during the same period in the 76 large English towns. The rates in the eight Scotch towns ranged from 12.2 in Edinburgh and 13.4 in Dundee to 17.6 in Paisley and 18.8 in Greenock. The 505 deaths in these towns included 45 which were referred to diarrhoea, 12 to whooping-cough, five to measles, three to scarlet fever, three to "fever," and one to diphtheria, but not one to small-pox. In all, 69 deaths resulted from these principal infectious diseases last week, against 62, 63, and 60 in the three preceding weeks. These 69 deaths were equal to an annual rate of 2.1 per 1000, which was 1.0 per 1000 below the mean rate last week from the same diseases in the 76 large English towns. The fatal cases of diarrhoea, which had been 34, 28, and 34 in the three preceding weeks, further rose last week to 45, of which 24 were registered in Glasgow, five in Dundee, four in Leith, four in Greenock, three in Aberdeen, three in Paisley, and two in Edinburgh. The deaths from whooping-cough, which had been 11, 15, and five in the three preceding weeks, rose again to 12 last week, and included eight in Glasgow and two in Edinburgh. The fatal cases of measles, which had been seven, six, and five in the three preceding weeks, were again five last week and included four in Glasgow. The deaths from scarlet fever, which had been four, three, and six in the three preceding weeks, declined again last week to three, and all occurred in Edinburgh. The deaths referred to diseases of the respiratory organs in these towns, which had been 79, 69, and 54 in the three preceding weeks, further declined last week to 50, and showed a decline of 23 from the number in the corresponding period of last year. The causes of 17, or more than 3 per cent., of the deaths in these eight towns last week were not certified.

HEALTH OF DUBLIN.

The death-rate in Dublin, which had been 17.5, 18.3, and 18.2 per 1000 in the three preceding weeks, further declined to 18.0 per 1000 during the week ending August 15th. During the past four weeks the death-rate has averaged 18.0 per 1000, the rates during the same period being 13.4 in London and 13.6 in Edinburgh. The 131 deaths of persons belonging to Dublin registered during the week under notice were one less than the number in the preceding week and included seven which were referred to the principal infectious diseases, against nine, five, and eight in the three preceding weeks; of these, two resulted from "fever," two from diarrhoea, and one each from measles, from diphtheria, and from whooping-cough, but not one from either small-pox or scarlet fever. These seven deaths were equal to an annual rate of 1.0 per 1000, the death-rates last week from the same diseases being 2.3 in London and 1.3 in Edinburgh. The fatal cases of diarrhoea, which had been four and five in the two preceding weeks, declined again to two last week. Two deaths were referred to "fever" last week, against none in either of the two preceding weeks. The 131 deaths in Dublin last week included 28 of children under one year of age and 35 of persons aged 60 years and upwards; the deaths of infants showed a decline, while those of elderly persons were slightly in excess of the number in the preceding week. One death from violence and four inquest cases were registered and 54, or more than two-fifths, of the deaths occurred in public institutions. The causes of 13, or nearly 10 per cent., of the deaths registered in Dublin last week were not certified.

Correspondence.

"Audi alteram partem."

MEDICAL DOCTRINES OF HEREDITY.

To the Editors of THE LANCET.

SIRS,—Dr. Wigglesworth agrees that every lethal disease, by weeding out the weak against itself, has been a cause of evolution and that herein lies the explanation of the fact that every race is resistant to every disease strictly in proportion to its past experience of that particular disease. Apparently he admits, also, what indeed is a patent fact, that every race which commands a sufficient supply of alcohol is temperate strictly in proportion to its past experience of the poison. But because some victims of alcoholism perish after the child-bearing age he is inclined to attribute the evolution in this case, not to the elimination of the drunkards, but to the elimination of the offspring. According to him, then, it is not the generation which drinks but the generation which, owing to its youth, does not drink that is weeded out by alcohol. It will be seen that he enunciates a new and highly original theory of natural selection. I can only reply that while there is very massive and conclusive evidence that from the age of 25 years forwards alcohol is the cause among actual drunkards of a mortality greater than that caused by any disease, there is not a scintilla of real evidence that parental drinking is a cause of "inherited weakness" among children. I fear I must insist that the evidence set forth by alienists is not, as it stands, worth the paper on which it is written. It is asserted by them that "mental instability" and a tendency to alcoholism are apt to coincide in the same individual. It is also admitted that both "mental instability" and a tendency to alcoholism are transmissible to offspring. Why, then, they should attribute the filial defects to parental drinking and not to the parental defects passes my comprehension, especially in view of the evidence furnished by biologists of the extreme indifference of the germ plasm to external influences. If it be asserted that the offspring of drunkards are often more defective than themselves I can only reply that so are the children of non-drunkards. It is clear that an inquiry conducted on these lines should be stringently limited to the children of drunkards who were otherwise perfectly normal and even then—so complex and difficult is the matter—we should have to eliminate the personal equation of the observer. When I was a student one of my teachers who had a "purity" craze attributed all nervous complaints and a good deal more besides to "sexual excess" or venereal disease. Another—a temperance enthusiast—attributed precisely the same disorders to drink. It went very hard with either if they failed to find evidence of what they expected in the sufferers or their progenitors. At the present day I have the advantage of knowing personally more than one industrious observer who, I am sure, would discover "mental instability" in an archangel if only it were proved that the archangel's parents had "twined the vine-leaves in their hair."

May I beg Dr. Wigglesworth when considering his new theory of natural selection to remember that *all* the members of a race that has had no experience of alcohol are drunken to a furious and uncontrollable degree when given access to strong modern solutions. It cannot be doubted since every race which is now temperate was anciently drunken that this was the primitive condition of mind. The most temperate people on earth—the South Europeans and the West Africans—have had unlimited supplies of alcohol for thousands of years, and as there is no evidence of an effective temperance propaganda among them every member of these races must have been exposed to the influence of alcohol "of such intensity as to preclude the possibility of any stable elements being left." Under the conditions I can understand the operations of the scheme of evolution by natural selection as explained by Darwin, but I cannot understand it as explained by Dr. Wigglesworth. I must beg him besides to remember that West Africans have been exposed also to the influence of another poison, malaria, "of such intensity as to preclude," if his doctrine of germinal injury through parental poisoning be correct, "the possibility of any stable elements being left."

Dr. Wigglesworth declares that "it seems impossible to suppose that the delicate cells of which the germ plasm is

composed should be wholly uninfluenced by being placed in an adverse nutritional environment such as must exist when alcohol or other poisons are circulating in the blood." But really I have not declared the contrary. I know nothing about the point raised and I am inclined to believe that no one else knows more. I have merely insisted that alcohol and other poisons do not appear *so* to affect the germ cells that their very remote descendants (the body cells of the child) are rendered defective. I ask for evidence and after all science is founded solely on evidence. The method of inquiry followed by Dr. Wiglesworth, the arriving at important conclusions in the absence of evidence, or in despite of evidence, has led, I suppose, to more scientific error than all other causes combined.

At the meeting of the British Medical Association held lately at Swansea almost every speaker who commented on my paper on Human Evolution with Especial Reference to Alcohol, declared confidently that injurious influences acting on parents cause offspring to be degenerate. Dr. W. Lloyd Andriezen, for example, had seen "hundreds of cases." Dr. A. T. Schofield was amazed to find that I had not observed what others saw "every day," and scandalised that doctrines so flagitious should be introduced to the public weighted with all the prestige that attaches to a paper read before the Psychological Section of the British Medical Association. He was good enough to intimate that an hour's conversation with himself would convince me of the errors of my ways. But Dr. Schofield's surprise, however great, was not half so great as my own. Imagine the Psychological Section suddenly rediscovering and introducing to an appalled world the scientific common-places of the last 10 or 20 years. I am not unduly exalted or sanguine of the result. I fear the conversation between us is impossible, but let me quote for him an authority whom he will probably respect. Professor Clifford Allbutt stated at Bradford a few weeks back in his address to the Sanitary Institute: "My prejudices are against such inheritance. For example, in my young days the dalesmen of Yorkshire drank prodigiously; drank like the Scandinavian heroes whose descendants they are. By drink they damaged their own lives badly; those stalwarts whom nothing but the scythe of time could mow down lived to great ages in spite of the drink; they were bound for longevity and not rarely they made it; but the majority had poisoned themselves by threescore. Yet I noted in them no deterioration of race; generally their youths maintained the same promise of great stature, of energy, and of courage as their forefathers."

Let me, then, implore these gentlemen, who certainly seem very confident and should therefore be able to justify their assertions, to afford us the opportunity of examining their evidence. Above all, let me implore them to present it in such a form that there can be no suspicion of confusion between *post hoc* and *propter hoc*. By way of incentive let me remind them of the tremendous importance of their labour if successful. It will revolutionise biological science. It will raise them to a pinnacle of fame. Many of the acutest minds in the world for a score of years have sought for and failed to find the evidence they have seen "every day" and "in hundreds of cases." In the, I fear, very probable event of the non-production of this evidence I am compelled, most reluctantly but still most categorically, to charge them with a total and by no means creditable neglect of the vast amount of definite work that has been accomplished by biologists, and with an intolerant disregard of conclusive evidence drawn from their own sciences, even when placed before them in the clearest light. I am perfectly aware of, and greatly regret, the arrogant and even offensive nature of this declaration, but I know no other way of shaking these gentlemen out of an attitude of too complacent confidence. I repeat, either they are in the possession of valuable evidence which will revolutionise the science of the last half century, or they are, scientifically speaking, in a backwater from which it is high time they emerged.

I am, Sirs, yours faithfully,

Southsea, August 9th, 1903. G. ARCHDALL REID.

To the Editors of THE LANCET.

SIRS,—It has long been a matter of comment that so very little of the logical faculty is sufficient for those who conduct the affairs of a nation. It would seem that even less is needed to conduct a discussion upon a scientific subject. I will not now enter upon the merits of the question at issue

between Dr. J. Wiglesworth on the one hand and Dr. G. Archdall Reid and Dr. C. R. Niven on the other, but if you will allow me I will show how Dr. Niven's letter in THE LANCET of August 15th (p. 494) appears to a humble student of logic.

If parental drinking led to increased filial drinking or filial insanity, and as these would increase generation after generation, we would expect that those races which have had the most prolonged acquaintance with alcohol would be the most drunken and would have a bigger death-rate from alcohol than those that have not had this prolonged acquaintance with it. Did parental drinking lead to filial insanity the same result would ensue—that is, extermination of the race.

By a parity of reasoning, if a long neck in a giraffe led to increased length of neck in the offspring every giraffe would now carry his head as high as the cross of St. Paul's. So easy is it to procure a *reductio ad absurdum* by ignoring essential conditions—in this case counteracting circumstances. The *non sequitur* is followed by a *petitio principii*.

Since the child varies somewhat from the parent the germ cell from which he is derived *must also* [the italics are mine] have varied somewhat from the germ cell from which the parent was derived.

The next is not among the recognised fallacies enumerated in books on logic but it is not of Dr. Niven's invention. It was used in Newton's time to refute the absurd theory of universal gravitation.

It is difficult to conceive that this variation in the offspring will be of the same kind as that in the parent. In other words, *it is difficult to believe* that because the parent developed his calf muscles the child will have better calf muscles than he otherwise would have had. [The italics are mine.]

"There has not been any evidence advanced worth the time spent in examination." As an argument I admit that this is irrefutable. As a statement of fact it is—well, it leaves something to be desired.

His statistics are supposed to prove that parental drinking will cause filial intemperance or filial mental instability. The foregoing part of this letter and my last letter prove [prove!] that for this contention there is not one iota of justification. Were this so, then we would expect every member of a litter of pups or kittens to resemble exactly one another, for the germs have been subjected to the same conditions of environment.

This appears at first to be a case of *ignoratio elenchi*, but on examination it is found to be a neat example of the *fallacia a dicto secundum quid et dictum simpliciter*, the *secundum quid*, that the environment of germs is not the sole cause of variation of the offspring, being ignored.

"We know, however, that no two members of the same litter correspond in any one character or trait." True, indeed! When did ever two kittens or puppies have the same number of legs or tails?

Dr. Wiglesworth next refers to the Mohammedan races which show us millions of human beings who are perfectly sober, not by virtue of such an evolution, but in consequence of the operation of religious sanctions. If these races are Mohammedans and abstainers we must take into consideration the price they are paying in knowledge, belief, art, morals, law, customs, &c.

Here is the *ignoratio elenchi* with a vengeance. The bearing of these considerations upon the effect of parental intemperance upon the offspring is, indeed, obvious, but to make our view complete should we not also take into consideration the phases of the moon, the bank-rate, and the birds and the blossoms of spring, *tra la?*

And now, Sirs, let me leave the question of logical correctness and ask in all sincerity for information at the hands of Dr. Reid and Dr. Niven. We acknowledge freely and unreservedly their wealth of knowledge and their dialectical skill. We admit that in comparison with them we are but worms, but why do they address their opponents in the tone of Almighty God addressing a peculiarly ignorant and recalcitrant black-beetle? After all, if a man does hold that acquired qualities are heritable he is still one of God's creatures. I am, Sirs, yours faithfully,

Catford, August 15th, 1903.

CHAS. MERCIER.

To the Editors of THE LANCET.

SIRS,—It is not always an easy matter in a controversy to place oneself in the mental attitude of one's opponent, but the perverseness with which Dr. C. R. Niven continues to misrepresent my position is remarkable. In his last letter¹ Dr. Niven states that I believe and claim to have proved by my statistics "that parental drinking leads to increased desire for alcohol as well as to increased mental instability in the offspring." It would naturally be supposed from this statement that I had

¹ THE LANCET, August 15th, p. 494.

expressed an opinion to the effect that parental drinking leads to increased desire for alcohol in the offspring. Nevertheless for this statement of Dr. Niven there is not the smallest shadow of justification. I not only do not hold that opinion, but I have nowhere given any indication that I do so. There is not a line in either of my two letters² referring to any increased desire for alcohol being produced in the offspring by parental drinking. There is no suggestion to that effect. The subject is not even alluded to. What I did say was that, in my opinion, the molecules of the germ cells of the parent were capable of having their development modified by the circulation of alcohol in the blood in such a way as to cause the organism resulting therefrom to develop on abnormal lines in the direction of mental instability. This is a statement of a wholly different character from that which Dr. Niven represents me to have made. It implies a general influence on the germ plasm by the action of the poison and not a special or specific one in the sense that the changes produced in the offspring are the same as those produced in the parent, which I have nowhere argued in favour of, and the assumption that I have done so is quite unwarrantable on Dr. Niven's part. The reason why a general agent such as alcohol acting on the germ plasm may be capable of producing mental instability in the offspring is not far to seek. The molecules of the germ cells which determine the development of those portions of the cerebral centres which subserve the mental activities constitute the latest formed portions of the germ plasm; they are of comparatively recent development in the race and are, in consequence, the most unstable portions and are therefore the most liable to give way under adverse conditions. If germ cells are forced to develop in a vitiated nutrient medium, one sufficiently impure to retard their activities without destroying them, it scarcely admits of doubt that the most stable, most deeply organised, and most resistant portions of these germ cells are those which will suffer least, and that the stress will fall mainly upon the least organised, most specialised, and least resistant portions. That the higher cerebral centres then should have their development adversely modified under the conditions postulated is only what we might expect. But I note that Dr. Niven does not deny that changes may be produced in the offspring by the circulation of poisons in the blood of the parent, for he says: "It is not denied that the offspring developed from germ cells may be influenced in this or that possible or probable direction by injurious agents—for example, alcohol—circulating in the parent's blood." And again, "With regard to permanent alterations being produced in germ cells and the resulting offspring from poisons, &c., circulating in the blood, this is probably correct." But this really concedes the main point under discussion, the very thing I have been arguing in favour of. It is the point which Dr. Reid denies in his letter of July 4th, p. 56, which started this discussion, for in that letter Dr. Reid quite clearly gives it as his opinion that offspring are not permanently injured by the fact that the germ cells from which they arose having been developed in a medium vitiated by the presence of alcohol, opium, or other poisons. From the ardour with which Dr. Niven espoused Dr. Reid's cause I was led to suppose that he also held that opinion, but I note now that this is not so but that, on the contrary, he differs from Dr. Reid and agrees with me on this important point which is really the root and kernel of the whole discussion. I am, Sirs, yours faithfully,

Rainhill, August 16th, 1903.

J. WIGLESWORTH.

To the Editors of THE LANCET.

SIRS,—In THE LANCET lately there has appeared repeatedly an argument which Dr. G. Archdall Reid sums up as follows: "If parental disease affected the offspring subsequently born then the morbid condition thus reproduced in the offspring would be a congenital one and such congenital morbid conditions would, according to biological law, be transmitted to the descendants of the said offspring. Under such conditions a race afflicted by any disease would undergo rapid degeneration and ultimate extinction. We know that this is not the case."³ Without further elucidation this is not a very convincing argument because it assumes that no other influence can counteract the ill effects of transmission. In order to maintain his ground Dr. Reid must show that

degeneracy due to an acquired morbid condition of the parent would be necessarily more potent than any counter-acting influence—e.g., the elimination of the unfit. For example, if 20 per cent. of a community were swept off by an epidemic is it impossible that such a removal of the weak and susceptible would have a more powerful effect on future generations than an injury to some of the offspring of the survivors? It certainly seems unsafe on such flimsy theoretical grounds to reject any positive evidence that may exist—e.g., that idiocy is common in the offspring of alcoholic parents.

I am, Sirs, yours faithfully,

Tollington-park, N., August 17th 1903. C. E. SCUDAMORE.

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

To the Editors of THE LANCET.

SIRS,—Whether this treatment is entirely new or not (it will be seen on reference to my letter in THE LANCET of July 18th, p. 185, that the opinion that it was new was given on much higher and more experienced authority than my own) is of much less importance than the fact, which is not open to question, that it has not heretofore met with any general recognition. Possibly "there is nothing new under the sun" and a reference to it may yet be found in some ancient Chaldaic or Egyptian manuscript. I see in the *British Medical Journal* of August 15th that the microbe of plague was apparently known 14 centuries ago! The treatment is not referred to in any general text-book in common use that I have seen (Roberts's *Medicine*, sixth edition; Gibson's *Medicine*, 1901; Whitt's *Dictionary of Treatment*, 1901; Heath's *Dictionary*, &c.). Moreover, in patients in my own practice who have gone elsewhere for special advice never has this treatment been suggested to them, but they have come back with varying prescriptions or a cautious recommendation regarding surgical interference.

Perhaps the present correspondence will draw more marked attention to the matter. It seems desirable that in every case this simple methods should be given a fair and thorough trial for a few months before recourse to drugs or more radical measures. Even in exophthalmic goitre the treatment should be given a trial. My experience teaches me that there is no very clear line of demarcation between the two classes of cases; all gradations are met with between the extremes and it may well be that the same agent is responsible for both, acting in different circumstances and on different constitutions.

I may say that there is, so far as I know, no iron or sulphide contamination in the water here. So many minerals are now occasionally used in therapeutics that one thinks any connexion of the disease with any one of them would ere now have been noted if it existed. Therefore one has to fall back upon a microbic theory as probable. I have at present another cases of long-standing goitre with distressing symptoms which came under my care only a month ago. Under the rain-water treatment carefully carried out, the patient is already much relieved and the bronchocele is reduced in size.

I am, Sirs, yours faithfully,

C. A. RAYNE, M.D. Lond., &c.

Lancaster, August 16th, 1903.

HUMAN AND BOVINE TUBERCULOSIS.

To the Editors of THE LANCET.

SIRS,—In the account of my paper introducing the above subject at Swansea in THE LANCET of August 15th, p. 473, there is a slight inaccuracy which I ask to be put right. The account says: "Dr. Nathan Raw stated that he agreed with Koch that human and bovine tuberculosis were distinct diseases, although a similar bacillus was found in each. He believed strongly that bovine tubercle could be transmitted to man, although human tubercle could not be conveyed to cattle."

My opinions, which may have been badly expressed, are—that whilst the experimental evidence is at present in favour of Koch in stating that human and bovine tubercle are separate and distinct diseases my own impression is that they are rather *different varieties of the same species* producing a different train of symptoms in the human body. Human tubercle can undoubtedly be conveyed to cattle but

² THE LANCET of July 18th, p. 186, and August 1st, 1903, p. 340.

³ THE LANCET, August 15th, 1903, p. 469