

it seems that Dr. Pavy and Professor Halliburton agree that in the form of diabetes due to phloridzin there is a breaking down of the albuminous molecule with formation of sugar. Is it not possible to extend this explanation so as to cover all the phenomena of diabetes? If we postulate the existence of a poison acting somewhat similarly to phloridzin and splitting off a saccharine radicle from the protoplasmic molecule, such a substance will first attack loose combinations of sugar such as exist in the hepatic cells, which probably hold their glycogen attached by some mechanism analogous to Ehrlich's side chains and readily allow it to be split off. The poison would equally attack at an early stage a loose combination of sugar and protoplasm such as Dr. Pavy suggests (in the lymphocytes) as the vehicle for the carriage of sugar to the tissues. As, however, the disease advances and more of the poison is formed, its activities will not be confined to these loose compounds, but it will attack the other cells of the body, breaking off from them too a saccharine radicle. Now we know that the hepatic cells can give up and resume their glycogen without injury to themselves: such is their function. But to extract a molecule of sugar from other cells is probably impossible without destroying them. Thus it comes about that in the later stages of diabetes there is a destruction of protoplasm, with formation of those acetone bodies which are so characteristic of grave diabetes. I venture to think that the theory which I have sketched involves little hypothesis and harmonises many, at all events, of the phenomena of the disease.

I am, Sir, yours faithfully,
W. CECIL BOSANQUET.

HOUSE FLIES AND ENTERIC FEVER.

To the Editor of THE LANCET.

SIR,—Referring to the comments of your own correspondent in last week's issue, I may say that the windows of the dairy at the City Hospital for Infectious Diseases here have always been guarded with wire netting. As far as possible the milk bowls are kept covered with gauze, but at times these must be exposed, and when the doorway to the kitchens is opened then flies, like certain proverbial persons, *will* "rush in."

I am, Sir, yours faithfully,
HENRY E. ARMSTRONG,
Medical Officer of Health.

Health Department, Town Hall, Newcastle-upon-Tyne,
Dec. 22nd, 1908.

* * We regret the oversight by which this letter was not published last week.—ED. L.

FLIES AS A NUISANCE AND FLIES AS A "DANGEROUS NUISANCE."

To the Editor of THE LANCET.

SIR,—In an annotation in THE LANCET of Dec. 19th, 1908, p. 1834, you quote from Dr. W. H. Hamer's latest report his view that with regard to the carriage of the infection of diarrhoea "importance attaches, not so much to the number of carriers of diarrhoea organisms as to the extent of distribution of the organism itself."

Surely, Sir, both factors are of equal importance. My position is as follows, and I think it a logical one; I found it on various premisses.

1. *Diarrhoea is but a symptom*; it is an intestinal flux due to a variety of irritants or toxins. These irritants or toxins are chemical in nature and may be derived from various sources. They may be derived from mineral sources or they may be the result of biological processes—whether as pure vegetable alkaloids, or whether as living vegetative bacteria producing their toxins within the body itself. At any rate, diarrhoea is a prominent symptom in Asiatic cholera which is associated with Koch's spirillum or allied spirilla just as much as it is in summer diarrhoea. It is also an uncertain symptom in enteric fever, influenza, and other diseases. I have more than once expressed my belief that there is no one and only specific organism of summer diarrhoea. At one time or place Shiga's bacillus may be playing a prominent part; at another time the cholera bacillus, or the Gärtner bacillus, or the bacillus typhi abdominalis, *et hoc omne genus*.

2. The infection of diarrhoea is conveyed by way of the mouth (and almost invariably through contaminated food), but as shown by (1) the nature of the infection may vary.

3. The food must be infected by means of some agent or agents. (We have to decide whether infection most readily reaches food by way of dust or by means of flies or otherwise.)

4. Epidemic diarrhoea is distinctly a seasonal disease. Therefore the particular agent of infection must also be of seasonal prevalence. Dust is very prevalent in the spring, diarrhoea not so.

5. In local outbreaks after a case or two of diarrhoea have occurred a specialised form of organism is undoubtedly present in large numbers, having greater infective properties than prior to the development of any cases. On the assumption that these propositions cannot be denied, Dr. Hamer will find himself on the horns of a dilemma, for the extent of distribution of the diarrhoea-causing organisms must be greatest when the number of cases is greatest, and yet there follows a diminution in cases and mortality which cannot be explained, as he himself has shown in his previous report, by an "exhaustion of material" theory.

6. The fly curve as regards numerical prevalence and the diarrhoea curve as regards numerical mortality may not exactly correspond, but it cannot be gainsaid that they closely approximate.

I will assume that the foregoing propositions will be admitted on all hands. I will now repeat some of the conclusions I have previously recorded as the result of my own observations, and I should like to know if the observations of others who have also studied the question do not confirm them and support my deductions. 1. The fly season is the diarrhoea season (as a corollary—very few flies very few cases of fatal epidemic diarrhoea). 2. Entirely breast-fed infants are almost exempt from fatal epidemic diarrhoea. (Can flies settling on or in the mouth of a breast-fed infant be absolutely excluded in the exceptional cases?) 3. Fatal epidemic diarrhoea is almost entirely an infantile disorder. 4. Epidemic diarrhoea is almost confined only to those who drink milk.

Dr. Hamer lays great stress on the fact that judging by the numbers of flies caught the diarrhoea curve rose during a stationary period of fly prevalence in 1907, while during a similar period in 1908 it declined. But surely the main point to decide is not whether the flies were equally numerous during the period of diarrhoea declension as whether they were equally attracted to human food. After a certain period of existence flies still numerically abundant pay less attention to (human) food and are more busily engaged in courtship and in obeying reproductive instincts. The meteorological conditions are of great importance. If unfavourable, whether by reason of wet or of cold or both, flies do not peregrinate to the same extent. Therefore, though they may be equally numerous in the house (as judged by the fly-catcher) few, if any, fresh importers of bacteria come into the house. Flies are also peculiar in some of their habits. At certain times they congregate for hours together in a warm corner, apparently indifferent to food. The relation of flies to diarrhoea cannot, then, be decided solely by means of fly papers, though the elaborate observations of Dr. Hamer are of great interest and importance. One thing I am certain of is that flies in London, which is a water-closet city, are not so dangerous as flies in Midland and other privy towns.

The place of development of *musca domestica*, which is the fly found in houses in over 90 per cent. of the observations made (my own observations give considerably over 95 per cent. and convince me that other flies such as *homolomyia canicularis* are almost negligible), is in my opinion of considerable importance. In order of importance in considering the fly as a carrier of dangerous germs I would place its chief breeding places in the following order (1) midden privies or night-soil deposits and (2) house refuse (because both often contain specific organisms); and (3) manure. As regards food, milk is undoubtedly the most important article, because a single fly falling into milk and having its whole body, wings, legs, and proboscis continually laved in the milk, assisted by its vigorous attempts to escape, must give rise to 20 times as much pollution as a dozen flies which merely settle on solid food. In fluid milk bacterial contamination proceeds apace, growth being undoubtedly slower in solid food.

The temperature of the air of the room is of great importance, controlling as it does to so marked a degree the multiplication of germs conveyed to milk by flies or otherwise in hot weather; one polluting fly may contaminate more in a

given time than two or three flies when the temperature is some degrees colder. This is a most important point to bear in mind, because the numerical prevalence of flies may be stationary and the number even visiting food may be approximately the same, yet the multiplication of bacteria is greatly lessened by a reduced temperature. If the reduction of the temperature is considerable (for instance, if the temperature of the room is below 50° F.) flies become very sluggish and congregate in the warmest corners, which they do not leave unless the temperature is raised. Nothing is more destructive of fly life than a sharp frost, and no measures of control hitherto devised will have so marked an effect in the reduction of diarrhoea or cholera as the advent of frosty weather. Now, germs are not destroyed by frost but the carriers are. The abatement of the epidemic influence by frost is not confined to cholera and diarrhoea but has been noted in connexion with the incidence of other diseases attributable to the agency of flies of some kind—such, for instance, as yellow fever.—I am, Sir, yours faithfully,

J. T. C. NASH, M.D. Edin., D.P.H.,

Dec. 23rd, 1908.

County Medical Officer of Health of Norfolk.

EPSOM COLLEGE.

To the Editor of THE LANCET.

SIR,—I have just read your remarks about Epsom College and also Mr. Henry Morris's letter and I feel sure now that the urgent needs are so ably and concisely put before the profession on behalf of the pensioners and foundation scholars that more of the 40,000 on the Medical Register will certainly send their names in to the treasurer or secretary and become annual subscribers and bring the number up to at least 5000 or more, and not leave it to the insignificant and paltry list of only, as at present, a little over 2000 annual subscribers. The council ought to be relieved of the great anxiety it must cause them to get the necessary amount, £7000 a year. It would be indeed a great pity and a disgrace to our profession if we cannot manage to keep up this annual sum and try yearly to increase it and do all we can to help our poorer members in their distress. I do hope Mr. Morris's urgent letter will touch their hearts and bring in fresh annual subscriptions. I myself am going to commence with £1 1s. and hope others will follow.

I am, Sir, yours faithfully,

W. CUNNINGHAM CASS.

Jan. 4th, 1909.

To the Editor of THE LANCET.

SIR,—The apathy of the profession to Epsom College and other charities is too deep to be ascribed by the most charitable mind to "lack of thought or need of being reminded." For many years I have worked hard; for at least ten years I myself sent appeals on post-cards to every medical practitioner in my neighbourhood, many hundreds in all, and the response amounted to something less than 10 guineas, and in place of medical contributors my usual list consisted during that time of a parson, a widow, a retired general practitioner, his wife, and myself; and even now my most keen supporter is a devoted monthly nurse who persuades contributions from her patients to help the orphan of one of her former employers.

For the last few years I have been able to collect more from the profession, perhaps because I am older and others are younger, but chiefly because my appeals have been personal more often than epistolary, so that a refusal is less easy; but it is disheartening work, and expensive too, as each donation obtained averages the outlay of two stamps, two envelopes, two sheets of notepaper, and an appreciable amount of work and time; whilst each refusal involves half that expenditure plus disappointment and vexation. No, Sir, it is neither "lack of thought" nor "need of being reminded," and were it not that our wealthier brethren are equally ungenerous I should believe it to be actual poverty. Can it be helped? After many years of consideration I believe that the method of distribution of the funds collected may be partly responsible; the subscribers have no certainty that their donations will help those they wish to benefit. It was shown, years ago, that the cost of an annual canvassing for votes often exceeds the cost of a year's schooling; while at the end, as Mr. Morris says, the candidate may fail and be left unaided, so

great is the competition, so terrible are the numbers and straits of the applicants!

Is it not possible that of these also the bulk of the profession is unconscious? Regular subscribers can, and do, learn something by a perusal of the voting papers, but, as they are never published broadcast, how many outsiders can dream of lady mothers who maintain their children by domestic and even more menial work? of worn-out widows, children, even medical men themselves barely kept from the workhouse? of the many who are there or in hopelessly worse plight? Cannot you, Sir, in these times of great charity to the hospitals arouse the worshippers at the *renaissant* shrine of Æsculapius to the fact that, while the temples are enriching, the priests, like those of most other religions, are starving? Cannot you point out that some help is needed by, and due to, those who have devoted their lives and given their not inconsiderable mites of wealth and health for the general good without thought of personal thrift or future gain? Cannot you point out that, in the words of the late Sir Andrew Clark, "the orphans of the profession should be the charge of the public"?—I am, Sir, yours faithfully,

Dec. 29th, 1908.

HONORARY LOCAL SECRETARY.

RELIGION AND TREATMENT.

To the Editor of THE LANCET.

SIR,—Surely the time has arrived when the medical profession should make some effective and organised protest against the ever-growing systems of so-called religious treatment which are now so much in vogue. Medical men are nowadays fully aware of the potentialities of suggestion in the treatment of functional disorders, and know quite well that suggestion can be brought to play through the medium of pseudo-religious formulæ, which accounts for the temporary relief sometimes obtained from these systems. Hence it is obvious that any case cured by such means could readily have been cured by a regular practitioner who understood the uses of suggestion in treatment. Would it not be possible to form a committee or association which should have for its chief object the instruction of the public in the fallacies of such systems of irregular practice, without the traditional principles of the medical profession being thereby violated?

I am, Sir, yours faithfully,

Harley-street, W., Dec. 28th, 1908.

EDWIN ASH.

HOMOGENEOUS IRRADIATION.

To the Editor of THE LANCET.

SIR,—In THE LANCET of Dec. 5th, 1908, p. 1679, under "New Inventions," there is a note to the effect that Dr. Holzknacht of Vienna is the inventor of a method of irradiating with Roentgen rays deep-seated tumours effectively without burning the skin. However, the fact is that he is not the inventor at all of this method of "homogeneous irradiation" as it was named by its real inventor—viz., Friedrich Dessauer. Dessauer, a physicist and director of a manufactory of x ray and electrical apparatus at Frankfort on the Main and Aschaffenburg, invented the method referred to above and in the note mentioned, entirely independently, and published it in a detailed article in the *Medicinische Klinik*, Nos. 21 and 22, as early as 1905!

It is true that at the last Roentgen Congress at Berlin last spring Dr. Holzknacht read a paper on the subject. But Dessauer promptly replied, claiming the priority for himself and, in fact, proving beyond all doubt that *he is the actual inventor*. This is an absolute fact which is easily shown as such by simply enumerating the literature on the subject. Moreover, in the controversy which ensued on Dr. Holzknacht's communication to the Roentgen Congress Dessauer remained ultimately unrefuted.

I give Dessauer's communications on the subject:— "Beiträge zur Bestrahlung tiefliegender Prozesse" (*Medizinische Klinik*, Nos. 21 and 22, 1905). "Eine neue Anwendung der Roentgenstrahlen" (*Verhandlungen der Deutschen Physikalischen Gesellschaft*, IX. Jahrgang, No. 3, 1907). "Eine neue Anordnung zur Roentgenbestrahlung" (*Archiv für Physikalische Medizin*, &c., Band. ii., Nos. 3 and 4, 1907). "Eine neue Anwendung der Roentgenstrahlen" (*Münchener Medizinische Wochenschrift*, No. 24, 1908). "Zur Frage der Homogenbestrahlung" (*Deutsche Medizinische Wochenschrift*, No. 40, 1908). F. Dessauer and Dr.