

and that I published both the detailed and average results. He seems to think that I started, not with the object of ascertaining the truth, but to justify some preconceived opinions and that for this purpose I used an unsuitable stove "probably badly fitted to the flue." What right has Mr. Brearley to say of a fixture he has not seen that it is probably bad, unless, indeed, he implies that all gas fires are badly fitted? In that case my results are still of value.

Mr. Brearley is surprised that I did not record the temperature of the room during the experiments. This did not come within the scope of my investigation, which was directed entirely to the effect produced on the composition of the air by various methods of heating and lighting. He also objects to the size of room I used, forgetting that gas fires are often used in small rooms and, still worse, gas-cooking stoves in still smaller sculleries.

Finally, I object to Mr. Brearley's interpretation of my motives. It is true that he says he would be the last to "impute other than the highest motives" to me, but that is quite neutralised by such statements as the following: "I must say that if you had been endeavouring to get the percentage of carbonic acid in the atmosphere as high as possible you could not have adopted better conditions." Mr. Brearley forgets that I hold no brief either for gas or electric lighting companies, that I should have published the results in any circumstances, whether in favour of gas or coal fires, and I may tell him that as a consequence of the experiments I at once introduced the electric light in my own house. I am sorry to find that the editor of the *Electrical Engineer* by expressing his confidence in the accuracy of my work is sneered at by Mr. Brearley—his "wish is father to the thought."

When Tennyson found his poems adversely reviewed by Christopher North he replied somewhat as follows (I quote from memory):—

You did once review my lays,
You did mingle blame with praise,
When I learned from whom it came
I forgave you all the blame,
I could not forgive the praise,
Crusty Christopher.

So I feel with regard to Mr. Brearley, I cannot forgive his praise when he accompanies it with attacks on my good faith in carrying out the investigation on the air of rooms.

I am, Sirs, yours faithfully,

FRANCIS JONES, M.Sc., F.R.S.E., F.C.S.

Manchester, Jan. 19th, 1907.

SPECIFIC GRAVITY OF URINE.

To the Editors of THE LANCET.

SIRS,—Although many may be aware of the abstract fact that the specific gravity of all liquids varies with the temperature it may not be generally appreciated, as a concrete example, that the specific gravity of urine is very materially influenced by temperature. Probably in the great majority of cases specimens of urine are examined when it has become cooled—i.e., when it is at the temperature of the surrounding air, since these specimens generally reach one in bottles, which are allowed to stand some little time, but I have frequently had occasion to examine urine which has been freshly passed, notably in cases of examination for life insurance, when it is passed at the time of examination.

I have often been struck with the low specific gravity of such freshly passed urine in many cases of young and healthy lives and was disposed to attribute it to the influence of the ingestion of fluid previous to the examination. On one occasion, lately, a specimen showing a specific gravity of 1015, when freshly passed, was left standing over night, with the urinometer *in situ*. On looking at it next day, before emptying the test glass, I was surprised to find the specific gravity to be 1020, thus showing a rise of five points. My attention being thus directed to this discrepancy, I made a series of observations, noting the temperature and specific gravity of the urine when it was freshly passed, and, later, when it had cooled down to the temperature of my surgery. As a result I find that the specific gravity of urine varies inversely with the temperature and that it rises, on an average, one point for every fall of eight degrees of temperature. I find no allusion to this in any text-book of physiology or clinical medicine, and I consider the recognition of the fact is of

importance, and it is a fact which can be easily verified, inasmuch as it should conduce to the prevention of fallacy in estimating the specific gravity of urine. Urine which at 60° shows a specific gravity of 1020, will at 96° show a specific gravity of only 1015.

I am, Sirs, yours faithfully,

ALEX. THEODORE BRAND, M.D., C.M. Aberd.

Driffield, East Yorks., Jan. 19th, 1907.

MEAT DIET AND THE TEETH.

To the Editors of THE LANCET.

SIRS,—In connexion with the correspondence which has appeared in your columns upon this subject over the signatures of Dr. J. Sim Wallace, Dr. D. Chalmers Watson, and Mr. J. H. Gibbs, I should like to state that in the specimens shown at the Pathological Society on Dec. 18th, 1906, a distinct difference was apparent between a tooth germ in the meat-fed and one in the bread-and-milk-fed animal. The section of the head of a meat-fed rat, thrown upon the screen in order to demonstrate the bone changes, showed a developing tooth which was not a normal one. The vascular papilla, the odontoblasts, and the ameloblasts were poorly and irregularly developed. The contrast between this germ and that of a normal rat was well displayed in Dr. Chalmers Watson's excellent preparations. This specimen may, of course, have been from one of the three animals referred to in Mr. Gibbs's letter in which the erupted teeth were of the honeycomb variety. Nevertheless, it would be of interest to know whether the enamel organs and dental papillæ were found to be normal in the other meat-fed rats.

At that meeting of the Pathological Society I suggested, as reported in THE LANCET of Dec. 29th, 1906, that Dr. Chalmers Watson was mistaken in ascribing many of the effects observed by him to a meat diet and that they were probably due to a deficiency of lime and other bases, as no bone or bone dust was given with the meat. The perusal of Dr. Chalmers Watson's letter in THE LANCET of Jan. 12th leads me to add that until this point is settled any deductions from the experiments as to the effects of meat-eating habits upon the physique of the present generation seem to me to be premature. Indeed, whether the results produced in rats are due to the want of lime and other bases or whether they are an expression of the failure of a naturally omnivorous animal to rapidly adapt itself to a purely flesh diet it is clear that they cannot be directly applied to the case of civilised man, who never in ordinary circumstances lives upon a diet of meat alone. A mixed diet may contain a very large proportion of meat and yet be not wanting either in carbohydrate or fat or in the necessary inorganic elements present in vegetables, fruits, milk, and in the drinking water of many districts.

I am, Sirs, yours faithfully,

London, W., Jan. 21st, 1907.

EDMUND I. SPRIGGS.

To the Editors of THE LANCET.

SIRS,—In his letter of Jan. 19th Dr. W. G. Creswell asks whether the artificial feeding of infants prejudicially affects the teeth. For several years, during which I have examined about 5000 hospital dental patients annually, I have directed special attention to this subject and have satisfied myself that the artificial feeding of children as generally carried out does exert a harmful effect on the development of the teeth, a view which is in accord with that generally held by dental surgeons. This harmful effect is shown in structural defects of the tissues and in irregularity of the teeth, conditions which favour the lodgment of food and subsequent caries.—I am, Sirs, yours faithfully,

Edinburgh, Jan. 21st, 1907.

J. H. GIBBS.

To the Editors of THE LANCET.

SIRS,—In his letter to THE LANCET of Jan. 19th Dr. W. G. Creswell asks two important questions which have not been answered by all authorities in the same way. Nevertheless the evidence is sufficiently strong, I think, to justify us in saying that the artificial feeding of infants *per se* does not prejudicially affect the development or structure of the teeth. The teeth of children, whether breast fed or otherwise, are almost invariably perfectly good to all external appearances when they cut the gums. Only about 6 per cent. have somewhat defective enamel; but this defective formation of the temporary teeth cannot be attributed to artificial feeding, as

the enamel is largely formed before artificial feeding is commenced. Mr. C. S. Pomes in his "Dental Anatomy" writes, "the first temporary molars are complete as to their masticating surfaces" at birth. As these are perhaps the teeth of the temporary set which succumb first and most rapidly to dental caries, it is impossible to attribute this to defective development or to any predisposition to decay in the tooth itself as a result of artificial feeding. Now this is important, as about 99 per cent. of children at the age of five or six years have dental caries in several teeth, of which the first temporary molar is almost invariably one. Again, children who are fed physiologically after the artificial feeding of the first year invariably, so far as my observations go, have teeth free from dental caries. I have come across a few examples of this, although as physiological feeding, having regard to the consistency of the food and the arrangement of the meal, is so generally unknown such examples are rarely to be found. With regard to the permanent teeth which may be affected by artificial feeding, it would appear that the evidence is but slight in support of the supposition that it has an injurious effect on the development or structure of the teeth. It would appear rather that some severe constitutional disturbance, the result of mal-environment, is generally responsible for the defective structure when it exists, though occasionally grossly improper feeding and consequent ill health may be presumed to have a like effect. Mr. Sidney Spokes has made a valuable contribution to this subject. He finds that in one of our public schools 4.6 per cent. of the boys had hypoplasia—i.e., defective development of the enamel in the permanent teeth—while in the Poor law schools the percentages were 7.1 for the boys and 7.5 for the girls. The statistics are not mentioned as to the relative number of breast-fed children in either group, but notwithstanding their greater freedom from hypoplasia it may be presumed that the boys of the public school were less frequently breast fed than the boys and girls of the Poor-law schools. Corroborative evidence of like nature is, I think, to be found in the investigation undertaken by Mr. J. H. Gibbs and Mr. G. W. Watson, recently recorded by Dr. Chalmers Watson in THE LANCET. With regard to Dr. Creswell's second question, it is no doubt the case that the teeth of bottle-fed children do decay more early and rapidly than those of breast-fed children, not, as a rule, because the teeth are in the least defective, but because such children are usually kept longer and more exclusively on the bottle than breast-fed children are kept at the breast. Moreover, having acquired in all probability a delicate digestion the result of artificial feeding, they are kept more strictly on a diet in which a more or less starchy artificial food is substituted for, or added to, cows' milk, in order to prevent curdling and the dyspeptic troubles which might follow. Such bottle-fed children are also likely at a later age to be kept more strictly to a diet in which the bread or rusks, &c., are carefully "well soaked" in the milk for like reason. To this may be added the fact that an artificially fed baby occasionally develops rickets after it is a year or two old, and then, instead of being given a little fish or chicken, or vegetable, or fresh fruit to supplement its daily dietary, the rickety child probably has more frequently its milk soaked diet supplemented by raw meat juice and perhaps fruit juice. Lastly, there is no doubt that on the average the breast-fed child thrives best, the teeth are put to better use and are not bathed in vitiated secretions. The jaws grow normally, the teeth become less crowded, and altogether they have a better chance of being subjected to those natural processes which normally prevent dental caries.

I am, Sirs, yours faithfully

Wimpole street, W., Jan. 19th, 1907.

J. SIM WALLACE.

SPIROGRAPHS OR NASAL "BREATH PICTURES."

To the Editors of THE LANCET.

SIRS,—The practice of testing nasal patency by breathing upon a prepared surface is by no means new, but its usefulness has been somewhat restricted by the want of a satisfactory material. Slate, glass, and polished metals all have their shortcomings, but I have now found that vulcanite with a medium polish gives a very reliable and faithful image. By placing the plate horizontally on the upper lip half an inch from the nostrils and giving one short and steady expiration a well-defined steam impression results, which on

evaporating affords reliable and striking evidence of the actual and relative patency of the nostrils. The image may be temporarily fixed or rendered more conspicuous for demonstration purposes by lightly powdering it with calcined magnesia or fine starch. Small sheets of vulcanite with a suitable surface and of a convenient size are supplied by the Medical Supply Association, 228, Gray's Inn road, London, W.C.

I am, Sirs, yours faithfully,

WYATT WINGRAVE, M.D. Durh.

Central London Throat and Ear Hospital, Jan. 19th, 1907.

THE PAST AND PRESENT INCIDENCE OF INFECTIOUS DISEASE.

To the Editors of THE LANCET.

SIRS,—I agree with Dr. H. E. J. Biss in saying we can never expect to see the full benefit resulting from notification and isolation till the whole country has gone through a systematic sanitary inspection. The correction of existing sanitary defects would mean the breaking up of the germ-breeding grounds, and we should thus be dealing with the prime cause of infectious disease. A supply of pure air, pure water, and pure food in any district cannot do otherwise than bring about a general increase of vitality, or, in other words, increased powers of resistance.

I am, Sirs, yours faithfully,

Bexhill-on-Sea, Jan. 21st, 1907.

F. P. ATKINSON.

MILK IN SMALL GENERAL SHOPS.

(FROM A CORRESPONDENT.)

II.¹

In my previous article dealing with the manner in which milk is retailed in small general provision shops, I described somewhat briefly a few shops of this kind into which I entered to observe the conditions. As pointed out, in none of these were the conditions favourable for keeping milk in a fit state to serve as a food for young children, and in some of the shops the surroundings were so insanitary as in certain circumstances to render the milk absolutely dangerous as a food for infants. It is not only in the East-End of London that small general provision shops in which milk is retailed are prevalent. They abound in every part of London in which the poor are massed, and undoubtedly are the means of supplying this class with the greater portion of the raw milk which they and their families consume. In the course of my limited tour of inspection I visited several localities in and about London, populated chiefly by the working classes, and found, as in the East-End, that public-houses and small general provision shops were everywhere conspicuous. A description of such a shop in Holborn or in St. Pancras, or even in a suburban district, is in most essential details similar to that of one in the East-End. They differ only in degree. Almost all are small, many by no means clean, and packed with a miscellaneous store of eatables and of articles for domestic use. In perhaps the majority of these establishments the milk is kept either uncovered or partially uncovered, the dipper is exposed to contamination by outside agencies, the door of the shop is frequently kept open in hot weather, always thrown back, permitting the dust and dirt from the street to blow in freely; in short, most precautions calculated to protect the milk from pollution are neglected. Again, the mode in which the milk when bought is conveyed from the shop to the home is, as a rule, characterised by great carelessness. In many instances a child is sent with a jug or other open receptacle in itself often unclean and to which dust and dirt of every description have ready access. Into the question of the extent to which milk is open to contamination in the homes of the poor there is no need to enter here, although undoubtedly this source of pollution is a potent factor in the causation of infantile mortality and one which must be grappled with ere the successful, or partially successful, solution of the problem of how to insure a comparatively pure milk-supply to the young children of the poor is in sight.

¹ The first article was published in THE LANCET of Jan. 19th, 1907, p. 193.