in the twenty-eight English towns. The fatal cases of diarrhoea, which had been 32 and 49 in the preceding two weeks, declined again last week to 35, of which 12 occurred in Glasgow, 9 in Edinburgh, and 5 in Dundee. The deaths from scarlet fever, which had increased from 2 to 8 in the previous four weeks, further rose to 11 last week, and included 6 in Glasgow and 2 in Aberdeen. The 8 fatal cases of measles corresponded with the number in the preceding week, and were all recorded in Glasgow. Of the 5 deaths from diphtheria, 2 occurred in Aberdeen; and the 6 fatal cases of "fever" included 5 in Glasgow. The deaths referred to diseases of the respiratory organs in these towns, which had been 53 and 80 in the preceding two weeks, declined again last week to 66, and were 18 below the number in the corresponding week of last year. The causes of 39, or more than 8 per cent., of the deaths in the eight towns last week were not certified.

HEALTH OF DUBLIN.

The death-rate in Dublin, which had been 21.0 and 27.5 per 1000 in the preceding two weeks, declined again to 26.6 during the week ending Oct. 3rd. During the thirteen weeks of last quarter the death-rate in the city averaged 21.1 per 1000, the rate for the same period being 17.4 in London and 16.1 in Edinburgh. The 177 deaths in Dublin during the week under notice showed a decline of 11 from the number in the preceding week, and included 17 which were referred to diarrhoea, 5 to "fever," 4 to whooping-cough, and not one either to small-pox, measles, scarlet fever, or diphtheria. In all, 26 deaths resulted from these principal zymotic diseases, equal to an annual rate of 4.1 per 1000, the zymotic death-rate during the same period being 2.1 in London and 2.6 in Edinburgh. The fatal cases of diarrhoea, which had been 14 and 29 in the preceding two weeks, declined again to 17 last week. The deaths referred to different forms of "fever," which had been 4 and 3 in the preceding two weeks, rose to 5 last week. The 4 fatal cases of whooping-cough were within one of the number in the previous week. The 177 deaths in Dublin included 50 of infants under one year of age, and 41 of persons aged upwards of sixty years; the deaths of infants showed a decline, while those of elderly persons exceeded the number in any recent week. Three inquest cases and 5 deaths from violence were registered; and 52, or nearly a third, of the deaths occurred in public institutions. The causes of 24, or nearly 15 per cent., of the deaths in the city were not certified.

CALCUTTA AND ITS SUBURBS.

Dr. W. J. Simpson, the health officer for Calcutta, in his memorandum for the month of June last, states that the total number of births in that month was 541, against 622 in the preceding month, giving an annual ratio of 13.9, against 17.2 per 1000 of the population. This is less than in seven years of the corresponding figures of the past decade, and is also below the decennial average by 3. The total number of deaths (exclusive of stillbirths) registered in June was 770, against 839 in the preceding month, showing an annual ratio of 19.9, against 23.2 per 1000 of the population. The monthly total exceeds seven of the corresponding figures of the past decade, and also surpasses the decennial mean by 16. There were 33 deaths from cholera, against 68 in the preceding month. This is less than all the corresponding numbers of the last ten years, excepting 1886, and is also less than the decennial average by 55. The number of deaths from fever and diarrhoea was in excess of the corresponding figures of the decennial average. With regard to local distribution of diseases, Hastings, Shampooker, and Puddopooker were the sections showing the highest death-rates, while the death-rates in the sections Park-street and Waterloo-street were below the average. In the amalgamated area of suburbs the birth-rate for the month of June was in the annual ratio of 13.4, against 22.6 in the preceding month per 1000 of population; and the total number of deaths registered was 476, against 617 in the preceding month, giving an annual ratio of 26.6, against 44.0 per 1000. Of these deaths there were 31 from cholera, 188 from fevers, 64 from bowel complaints, 39 from tetanus. According to the latest weekly return in our possession—viz, that for the week ending Aug. 22nd last—the total number of deaths in the town of Calcutta was 192, against 201 and 187 in the two preceding weeks. Among the deaths

there were 3 from cholera, 1 from small-pox, 16 from tetanus, and 88 from fevers and bowel complaints. The general death-rate was 21.5 per 1000 per annum, against 21.1, the mean of the last five years. The total number of deaths registered for the amalgamated area of suburbs during the same week was 137, as against 116 and 106 in the two preceding weeks, and somewhat higher than the corresponding week of last year. There were 7 deaths from cholera, none from small-pox. The general death-rate for the week was 33.2 per 1000 per annum, against 30.7, the mean of the last five years. The general death-rate of the combined area is equal to 25.1.

THE SERVICES.

ARMY MEDICAL STAFF.—Surgeon-Colonel Stewart Aaron Lithgow, M.D., C.B., D.S.O., to be Surgeon-Major-General, vice G. M. Slaughter, retired, Brigade Surgeon. Lieutenant-Colonel Arthur Charles Gaye to be Surgeon-Colonel, vice S. A. Lithgow, M.D., C.B., D.S.O., and Surgeon-Lieutenant-Colonel Charles White to be Brigade Surgeon-Lieutenant-Colonel, vice A. C. Gaye (all dated Sept. 16th, 1891); Surgeon-Captain Whitley Bland Stokes, M.B., F.R.C.S.I., from half-pay, to be Surgeon-Captain, vice E. McN. Woods, M.B., deceased (dated Sept. 26th, 1891).

ARMY MEDICAL RESERVE OF OFFICERS.—Surgeon-Major (ranking as Major) John Percy Waddy, having resigned his Militia appointment, ceases to be an Officer of the Army Medical Reserve of Officers (dated Oct. 7th, 1891).

ARMY MEDICAL SCHOOL.—Surgeon-Captain H. Whitehead, Army Medical Staff, has been appointed Assistant Professor of Surgery at the Army Medical School at Netley.

NAVAL MEDICAL SERVICE.—The following appointments have been made at the Admiralty:—Surgeon James H. Dawe, to the *Vernon* (dated Oct. 8th, 1891); John W. Gill to be Surgeon and Agent at Downerry, and Robert T. Thomas to be Surgeon and Agent at Looe (both dated Oct. 5th, 1891); Mr. William J. Mullaney to be Surgeon and Agent at Belmullet (dated Oct. 2nd, 1891).

VOLUNTEER CORPS.—*Rifle*: 1st Volunteer Battalion, the Suffolk Regiment: William Vernon, Gent., to be Acting Surgeon (dated Oct. 3rd, 1891).

Correspondence.

"Audi alteram partem."

"A THEORY OF SEX."

To the Editors of THE LANCET.

SIRS,—According to Mr. Andrew Wilson's theory in THE LANCET of Sept. 26th sex is determined by the condition of the ovule at the time when it comes into contact with the spermatozoon; and this he holds is greatly influenced by the time which elapses between the extrusion of the ovule from the ovary and its fertilisation. If insemination occurs before a menstrual period, so that the spermatozoon meets the ovule high up, the nutrition of the ovule is held to be still active (anabolic), and a male embryo results; if insemination occurs after the menstrual period, the ovule is met lower down when it has travelled further and become wayworn (katabolic), and a female conception follows; but should insemination happen (as can only rarely be the case) in the middle of the menstrual period, then the ovule is in "stable equilibrium," and a hermaphrodite is the correspondingly rare issue. There is something so captivating in the Aristotelian simplicity of this theory that physiologists would hail it with gladness were it only supported by results. It is difficult, as Mr. Wilson says, to collect sufficient evidence in the case of man, but we have no reason to suppose that man differs in this respect from other mammals. Stockbreeders adopt a pretty uniform rule in covering animals. A cow's season lasts about twenty-four hours, and it is the practice to endeavour to obtain the services of the bull about twelve hours after the season has come on. According to Mr. Wilson's law this

ought to result in a race of hermaphrodites, but such is not the case; male and female calves are fairly mixed. Occasionally it is found that the twin of a bull calf is a "freemartin," and then we have possibly the rare exception that proves Mr. Wilson's rule. Again, if a ram be turned into a sheepfold he will cover each ewe just at the beginning of her season; and stallions in the New Forest will take the mares in the same way, just as their season comes on; but the races of sheep and horses do not die out, as they would speedily do if the consequence of this uniformity of custom were to produce one sex only. The time of insemination in these cases does not support Mr. Wilson's theory, but in that theory does he not go somewhat unnecessarily out of his way in reversing the theory of Geddes and Thomson? Their view he thus summarises: in a katabolic habit, when waste exceeds repair, males preponderate; in an anabolic habit, with nutrition in excess of waste, we have a majority of females. Since the great body of facts support Geddes and Thomson, why should Mr. Wilson elect to put his theory against theirs, when, had he thought for a moment, he might have made it agree with their statistics? Surely when the ovule has had far to travel and has become wayworn, the spermatozoon can have had but a little way to come and must be still hale and vigorous. And, on the other hand, when the ovule is met high up and is fresh and hearty, the spermatozoon must have toiled far and have become tired and weary. Had Mr. Wilson based his theory on the nutrition of the spermatozoon, instead of on that of the ovule, he would have found himself in accord with Geddes and Thomson. A katabolic spermatozoon would beget a male, an anabolic one a female. But he forgets the spermatozoon altogether; he thinks only of Penelope, and leaves Ulysses tossing unheeded on his raft.

The determination of sex rests on a broader basis than the date of insemination, and is regulated entirely by the requirements of the species. The existence of the sexes is not essential to reproduction, and many highly organised species propagate themselves without sexual concourse. The *Cynips kollari*, for example, which produces the marble galls so abundant at this season on oak trees, has no male, and goes on propagating itself in unbroken succession in the female line. Other hymenoptera which have two generations in the year have a winter generation which is female only and a summer generation which is sexual; the *Spathogaster baccarum* emerges male and female from the currant gall of the oak in June; after the nuptial flight the female deposits her eggs in the under-surface of the young leaves, and over them rises a totally different gall, the common spangle, from which emerges next April the *Neuroterus lenticularis* in the female sex only; when she has pricked the young buds we have the currant galls formed again. These two species are linked together in one generation cycle. Others of the hymenoptera which propagate habitually by conjugation of the sexes, still retain the power of reproducing themselves in the female line when the life and continuance of the species is threatened and parthenogenesis is called for to save the race. The queen bee is fertilised by the drone only on the wing, and if fertilisation be prevented by depriving her of her wings just before the nuptial flight, she will nevertheless go on laying her eggs; but these eggs, instead of producing females, as they would have done for months if fertilised, will produce for a time only males—the need of males by the species having been in some way realised, males are produced.¹

But if sex is not actually essential, it is an extremely valuable aid to a species in the struggle for existence. When the germ plasmas in one species are derived from one parent, and in another species from two parents, it stands to reason that the variation in the descendants of the sexual generation will be infinitely greater than in the asexual; and the greater the variation in the progeny, the greater will be the chance of survival in the struggle for existence. It is therefore of importance to a sexual species to maintain both sexes; and just as in the case of the bee, if either sex is specially called for, that sex is reproduced in greater abundance, so as

to secure the continuance of the species. If a young ram be put to a flock of five-year-old ewes, 70 per cent. of the lambs will be female and 30 per cent. will be male, and there will be a goodly number of twin births. If an old ram be put to a flock of young ewes, 70 per cent. of the lambs will be male and 30 per cent. female, and there will be no twin births. But if the probability of life in both parents be equal, and a five-year-old ram be put to five-year-old ewes, the sexes will be about equally represented in the offspring. If to either of these conditions we add variations in the quantity of food and in the ease with which it is obtained, we have a further set of modifying conditions. When food is abundant and easily obtained, large flocks are possible and female issue preponderates; when mountain pasture is scarce and long distances have to be traversed the male sex is increased. A study of these facts appears to lead to the conclusion that anything in the parents or in the environment which declares the production of either sex in greater abundance to be necessary for the welfare or extension of the species, makes itself known through the parents, and so modifies the constitution of the male and female pronuclei as to produce more of the sex required. It is quite possible, where insemination at the outset of the menstrual period has long been the rule in a species, that delaying it unusually might give an impression of scarcity of males, and male births would result, which would accord with the Geddes, but not with the Wilson, theory.

It seems difficult to describe the mechanism by which constitutional weakness and the infirmities of age are stamped on the ovule and spermatozoon better than by the term "katabolism" and the opposite condition by that of "anabolism." It is clear from the facts I have given with regard to the age of rams and ewes that some physical changes are caused by age and environment, which affect equally the male and the female nuclei, and have been impressed upon each before insemination took place.

I am, Sirs, yours truly,

Wilton, Salisbury.

CHARLES R. STRATON.

To the Editors of THE LANCET.

SIRS,—Mr. Wilson's paper in your last issue is of extreme interest to those who have given this subject consideration. It must, however, remain a matter for conjecture whether or no the ovum is relatively the stronger germinal element; and even although it should be the stronger, I do not see why it should bias the embryo towards a male product. I should prefer to consider that the germinal elements (ovum and spermatozoon), after impregnation, had the effect of biasing the embryo towards their own sex, so to speak—the ovum towards the female, the spermatozoon towards the male,—and that the question of sex was really determined by their relative vitality at the time of impregnation, irrespectively of any definite time. Now this relative vitality will depend on a variety of circumstances; it will depend no doubt on the time elapsed since the escape of the elements from their respective producing organs, the ovary and testis. It must depend also on the state of nutrition of the parents. If either of the parents be diseased or in a low state of nutrition from any cause, his or her germinal elements are bound to suffer in proportion. Thus the spermatozoon of a healthy male may impregnate the badly nourished ovum of a sickly female, even shortly after its emergence from the ovary in Mr. Wilson's pre-menstrual epoch, and its vigorous strength will develop a male. And suppose impregnation to take place between these two elements, in Mr. Wilson's post-menstrual period there is more probability than ever of a male product resulting. Again, I should imagine, a healthy, vigorous ovum will carry all before it. Meeting an unhealthy spermatozoon (or one which has been some time about the female organs) pre-menstrually, a female will result from its superior vigour; very probably also had this same ovum (having by this time itself lost in vitality) met post-menstrually the same unhealthy male element it might still have retained sufficient vigour to determine a female product. Again, the relative vitality will depend on the state, healthy or otherwise, of the parts traversed by the respective elements. The question, however, is unfortunately involved in obscurity, and one can only speculate on such a difficult question. In conclusion, then, I will briefly state that in my opinion the question of sex is determined by the relative vitality of the germinal elements when impregnation takes place, irrespectively of

¹ These cases of agamous reproduction, either permanently or temporarily occurring in certain species, are very simply explained by Weissman. In them the second or reducing division does not take place in the female germ plasmas, and no second polar body is extruded from the ovule. When a reducing division takes place and half the germ plasmas of the female are extruded, their place is made good by half the germ plasmas of the male on the union of the male and female pronuclei. When reproduction is agamous, the whole of the female hereditary germ plasmas are necessarily retained.

menstrual epochs, and that each element will bias the embryo in the direction of its own sex. It is wrong, no doubt, to talk of these elements having a sex, but I am writing on the supposition that each element has an inherent power, by which, when the complementary element is met with, the sex is determined in favour of the stronger.

I am, Sirs, yours truly,

LOUIS E. STEVENSON, M.B.

Jedburgh, N.B., Sept. 28th, 1891.

"RECURRENCE OF SYMPTOMS AFTER GASTRO-ENTEROSTOMY."

To the Editors of THE LANCET.

SIRS,—In discussing this case clearly we are dealing merely with possibilities, which it would be idle and unprofitable to follow further, as the pathological specimen alone can demonstrate whether the artificial gastro-jejunal opening has closed or not. At a future date, however, I trust Mr. Larkin will communicate in your columns what actually has taken place in his most interesting case.

Mr. Larkin has, I fear, misunderstood me, as he says my main suggestion is that "the contraction occurred at the opening made in the peritoneal layers of the transverse meso-colon and great omentum; what I said was constriction—quite a different thing from contraction—of the jejunum may have taken place at the aperture in the transverse meso-colon. It is to this point I wish to refer, and the clearest way of expressing myself will be to give the reasons for my discontinuing to perform the operation by this method. By referring to Mr. Larkin's admirable diagrams my meaning will be more readily understood.¹ By drawing a loop of the jejunum through an opening in the meso-colon and great omentum we have two portions of intestine closely approximated, and firmly fixed by peritoneal cicatrization to an aperture in the meso-colon (necessarily of rather narrow dimensions) at the one end, and to the point on the stomach which the surgeon may select at the other extremity of the loop at the other end; in Mr. Larkin's case at the cardiac end and, judging from the diagram, rather high up. Between these two points the intestine passes through an opening in the great omentum. The two portions of intestine thus run nearly parallel to each other throughout the whole loop. The dangers of the operation performed in this manner are, I think, threefold:—1. There is actual danger of nipping of the intestine by peritoneal cicatrization and constriction at the aperture at the meso-colon. This evidently did not occur in Mr. Larkin's case, as the biliary and pancreatic secretion passed readily. 2. There is danger of kinking of the intestine at the gastric opening on account of the sharp curve which takes place. This accident did not apparently occur. 3. This is purely a mechanical danger. The loop of intestine is, as we have seen, fixed at both ends—viz., stomach and meso-colon. The physiological action of the stomach in health, we are taught, is, when empty, the greater curvature is directed downwards, but when the viscus is distended it rotates upon the lesser omentum, and the greater curvature is directed forwards, while the anterior surface is directed upwards. This action, then, must of necessity cause considerable traction upon the loop of intestine fixed at the two points, as I have described. The result of this is to temporarily contract, and possibly close, the opening between the intestine and stomach, especially when the loop is united high up on the cardiac end. The lumen of the intestine would be still patent for the flow of the biliary and pancreatic secretions. A similar danger may occur from a loaded transverse colon; if Fig. 1 is carefully studied it will be seen how readily constriction may occur from this cause. For these reasons I now invariably adopt the method of performing gastro-enterostomy by turning a loop of the jejunum round the transverse colon. I am not saying that any of these factors were responsible for the obstruction which occurred in Mr. Larkin's case, as it is impossible to decide upon what we have not seen, but I place them before the profession for what they are worth, and can only say they are conclusions drawn, not hurriedly or upon theoretical grounds alone, but upon actual practical experience.

I am, Sirs, yours very faithfully,

FREDK. BOWREMAN JESSETT.

Upper Wimpole-street, Oct. 3rd, 1891.

THE USE OF ANTISEPTICS.

To the Editors of THE LANCET.

SIRS,—As a practitioner of perhaps the least important branch of surgery, it seemed hardly fitting I should take upon myself to challenge Mr. Lawson Tait to a full defence of the position which he has assumed on the above question; but, in common, I am sure, with a large number of your readers, I felt very much inclined on perusing his lecture to dispute the sweeping generalisations which are criticised by Mr. Adamson in THE LANCET of to-day. The surgery of the teeth, and the treatment of inflammatory conditions within and around them, only represent in miniature what is displayed on a larger scale in general surgery; but the relatively simple structure of the organs and their exposed position render the processes of dental disease and the effects of treatment more easily and correctly observable. I will simply affirm what will be corroborated by every dental practitioner: that the antiseptic method has rendered amenable to treatment a vast class of cases which before the introduction of this method were extremely unsatisfactory. Cases of teeth with putrid pulp canals and chronic abscesses or fistulæ through the alveolus and gum—cases entirely analogous to surgical conditions affecting wider areas in other parts—yield at once to antiseptic measures. Such cases before the antiseptic system was understood and practised formed the opprobrium of dental surgery; they are nowadays curable with almost complete certainty. Although there can be no question as to the success of Mr. Tait's practice in abdominal surgery, it is difficult to see how on scientific grounds he can support his views. Indeed there seems something contradictory in his statements. He insists upon perfect cleanliness. But scientific cleanliness cannot be produced or maintained without antisepticism. From the surgical point of view, the presence of micro-organisms constitutes uncleanness; without them putrefaction and fermentation—the production of septic conditions—are impossible. In order to preserve a wound scientifically clean, organisms must be excluded; to maintain it clean, they must if they enter be destroyed or rendered sterile. The only effectual way to do these things in the present state of our knowledge is by the use of antiseptics.

I remain, Sirs, your obedient servant,

Wimpole-street, Oct. 3rd, 1891.

HENRY SEWILL.

To the Editors of THE LANCET.

SIRS,—I am not disposed to reply in detail to Mr. R. O. Adamson's very numerous inquiries, because the space necessary would mean more columns of THE LANCET than you could place at my disposal; because all of the surgical incidents specified are such as have been outside my line of practice for more than twenty years; and specially because the late Mr. Sampson Gamgee, in his work on the "Treatment of Wounds," answered them many years ago. My condemnation of the antiseptic doctrines and the antiseptic practice came upon Mr. Adamson as "startling" only because he graduated in 1889. A very slight research into the literature of the subject for the fifteen years preceding that date will at least cause my views no longer to startle him.

Mr. Adamson's letter reminds me that I may have a communication to make to THE LANCET in a week or two concerning this much vexed question of antiseptics which, I venture to think, will probably be startling to the Court of Examiners of the Royal College of Surgeons of England.

I am, Sirs, yours obediently,

Birmingham, Oct. 2nd, 1891.

LAWSON TAIT.

THE USE OF ATROPINE IN THE ADMINISTRATION OF CHLOROFORM.

To the Editors of THE LANCET.

SIRS,—I was surprised to find that no one made any remarks at the Bournemouth meeting of the British Medical Association about the use of atropine, with a view to annul the dangers attending upon the administration of chloroform. So far back as 1868 Dr. Harley pointed out the applicability of atropine in all cases where there is lessening of the sympathetic influence, as in syncope, shock, collapse of cholera, and risk of the heart stopping under the influence of chloroform &c.

¹ THE LANCET, Sept. 19th, p. 667.