HYGIENE AND PREVENTIVE MEDICINE DURING 1905.

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In attempting to review the chief features of the past year under this subject, it must be admitted that the period has not been remarkable for any startling advances or discoveries. As in previous years, it will be convenient to consider the chief points under the two main heads of (I.) Sanitary legislation and (II.) Matters of scientific interest.

I. SANITARY LEGISLATION.

Few years have been more barren of sanitary legislative effort than 1905. Out of the twenty-three public general statutes, which represent the total legislation of the year, only two—the Aliens Act and the Unemployed Workmen’s Act—can be said to have any relation to public health. In spite of it having been prominent in the King’s Speech in February, 1904, we are still without the much-needed Bill to amend and consolidate the law of public health. There is reason to believe that such a Bill had been prepared by the Local Government Board, but it has not yet been laid before Parliament or published.

THE ALIENS ACT.—This measure, to amend the law with regard to aliens, is based on the recommendations of the Royal Commission on Alien immigration. The Act consists of ten Sections, and, broadly, provides for the regulation of alien immigration into, and for the expulsion of undesirable aliens from, the United Kingdom. Section 1 provides that an immigrant shall not be landed from an immigrant ship except at a port at which there is an immigration officer appointed under this Act, and shall not be landed at any such port in the United Kingdom without the leave of that officer given after an inspection of the immigrants in company with a medical inspector. When leave to land is so withheld, the master, owner, or agent of the ship, or the immigrant, may appeal to the Immigration Board of the port, who have power to give leave to land, if satisfied; such permissions operating as the leave of the immigration officer. For the purposes of this Section an immigrant is to be considered undesirable: (a) If he cannot show that he has, or is in a position to obtain, the means of supporting himself and
his dependents; (b) if he is a lunatic or an idiot, or owing to any disease or infirmity is likely to become a charge upon the rates, or otherwise a detriment to the public; (c) if he has been sentenced in a foreign country for an extraditable crime of a non-political character; (d) if an expulsion order under the Act has been made in his case. The lack of means shall not be a bar to the admission to this country of an immigrant who can prove that he is so seeking admission solely to avoid persecution or punishment on religious or political grounds; similarly, leave to land shall not be withheld in the case of an immigrant who shows to the satisfaction of the immigration officer or the Board concerned with the case that, having taken his ticket in the United Kingdom and embarked direct therefrom for some other country immediately after a period of residence in the United Kingdom of not less than six months, he has been refused admission in that country and returned direct therefrom to a port in the United Kingdom; likewise, want of means shall not entail refusal of permission to land in the case of an immigrant who can prove that he was born in the United Kingdom, his father being a British subject.

Section 2 provides for the appointment of Immigration Boards for ports. Under Section 3 the Secretary of State has power to make expulsion orders requiring criminal aliens to leave the United Kingdom within a specified time. The expenses entailed by the expulsion of undesirable aliens is provided for in Section 4, and the Section also provides for the masters of immigrant ships being liable in these cases to afford the expelled aliens free passages to the original port of embarkation. The requirements as to statistical returns and the appointment of immigration officers is given in Sections 5 and 6. The next Section contains provisions as to penalties for infringement of the Act, and Section 8 contains definitions of immigrants and other terms. The last two Sections make the Act applicable to the United Kingdom, causing it to come into operation on January 1st, 1906, and repeals the Registration of Aliens Act, 1836—an Act which has been long a dead letter.

In the main, the new Act will be administered by the Home Office, but the Local Government Board, from the public health point of view, is also concerned, and doubtless the officers of the various port sanitary authorities will be the executive agents of the Act. The enforcement of the provisions of this Act cannot but help towards an amelioration of the conditions prevailing in certain parts of London and other great seaports; at least the Act must tend to improve the quality, even if it fail to reduce the quantity, of aliens immigrating to this country.
THE UNEMPLOYED WORKMEN ACT.—This is an Act to establish organisation with a view to the provision of employment or assistance for unemployed workmen in proper cases. Broadly speaking, the Act provides for the establishment throughout the country, more especially in municipal boroughs and certain districts having a population of 50,000 or upwards, by order of the Local Government Board, of representative and responsible distress committees, whose duty it is to make themselves acquainted with the condition of labour within their areas, and to inquire into and discriminate between the applications made to them from persons unemployed. The action of the various local distress committees is co-ordinated by a central body in London, working under the Local Government Board. It is contemplated that the expenses of this scheme will be met by funds contributed privately and voluntarily, but provision is made for meeting any deficiencies out of local rates. No such contribution by a council shall, however, in any year exceed the amount which would be produced by a rate of one halfpenny in the pound calculated on the whole rateable value of the borough, or such higher rates, not exceeding one penny, as the Local Government Board may approve. The Act is made applicable, with certain necessary modifications, to Scotland and Ireland, and is to continue in force for three years from August 11th, 1905, but no longer, unless extended by Parliament. This Act has been frankly criticised by some as faultily conceived on socialistic lines; this may be true or not, but, regarded as an indirect means of coping with, or, at least, ameliorating, some of the worst sanitary evils which follow invariably in the wake of poverty and distress, whatever its defects, it must be recognised as an enactment conceived in the best interests of preventive medicine.

Among the unsuccessful Bills, or those which failed to pass, were the following, having a definite hygienic interest:—The Milk Depôts (London) Bill, proposing to empower Metropolitan borough councils to establish and maintain depôts for the sale of pure and sterilised milk for the use of infants. The Sale of Butter Bill was a measure introduced by the Government to restrict the amount of water allowable in butter, and to provide for the inspection of butter manufactories by officers of the Board of Agriculture. The Sale of Whisky Bill, which sought to secure the differentiation of whisky distilled in a pot still from spirit distilled in a patent still. The Infectious Diseases (Ireland) Bill, which proposed to make the Infectious Diseases (Notification) Act of 1889 apply generally throughout Ireland. Also the Alkali Works Bill, which, though
mainly a consolidation Bill, contained an important proviso that
the owner of every alkali works shall use the best practicable means
for preventing the escape of noxious or offensive gases by the exit
flue of any apparatus used in any process, and for preventing the
discharge of such gases into the atmosphere, whether directly or
by a chimney or other outlet, and for rendering such gases where
discharged harmless and inoffensive. All these Bills had to be
withdrawn for want of time, but it is to be hoped that all will be
re-introduced and passed.

II. MATTERS OF SCIENTIFIC INTEREST.

Although no startling discovery can be chronicled in the domain
of preventive medicine for the year 1905, still, in several depart­
ments of study, notably those of epidemiology and bacteriology,
there are matters of interest to record.

YELLOW FEVER.—A severe epidemic of this disease has recently
occurred in New Orleans, and, in the Report of Working Party
No. 2, Yellow Fever Institute (Washington, 1905), Beyer, Francis,
Parker and Rosenau conclude that the micro-parasite of this affec­
ton is still unknown. They further state that no abnormal elements
can be discovered in the blood, that the infective principle can pass
through the pores of a Pasteur-Chamberland B filter, and that the
incubation period, after bites with infected stegomyia, is usually
three days, but may be five days occasionally. In any attempts to
screen the sick from these mosquitoes, it appears to be essential
that wire gauze of not less than twenty meshes to the inch should
be employed; if of wider mesh than this the stegomyia may pass
through. The importance of recognising the serious nature of the
mosquito as a factor in the dissemination of yellow fever has been
emphasised by the recent epidemic in Panama. Here the earlier
cases of the disease were concealed, and it was not until a serious
outbreak was in progress that severe measures were taken by
appointing a special Governor endowed with autocratic powers. The
chief danger lay in the demoralisation of public spirit, as shown by
a condition of utter panic on the part of some and the lapse into
a state of cynical bravado on the part of others. These latter
professed an utter contempt for the mosquito theory of disease
dissemination, and refused to obey the preventive rules which had
been formulated, exposing themselves to mosquito bites and wilfully
destroying the netting placed over the windows of hotels and
offices. The result of this attitude on the part of the larger number
of inhabitants was the incidence of a very severe epidemic. By
spear disciplinary measures and the adoption of a strict sanitary
police, especially in regard to mosquitoes, the Governor changed
the condition of affairs, with the result that there was soon a rapid
diminution of yellow fever incidence, until finally, in three months,
no further cases occurred.

PLAGUE.—This disease still holds sway in the centres which, for
several years, we have been forced to associate with its prevalence.
Of these centres India is undoubtedly the chief, and, although the
lessened incidence during the latter part of 1905 serves to keep the
total death-rate from the disease lower than was anticipated,
the total deaths from plague in India during the last year approxi­
mate to 1,000,000, as against 1,034,787 deaths in 1904. The
Bombay presidency remains still the chief focus of the disease,
but it prevails practically now from Karachi to Burmah, and from
Kashmir to Mysore. Although plague has persisted over this large
area for ten years, the disease must be considered still as an
epidemic, and possibly we shall have to wait another ten years
before we can say definitely which areas may be regarded as future
endemic, instead of epidemic, centres of the disease. As to what
is the essential factor in endemicity we know as little as we did a
year ago. Some still attribute the endemicity to infected grain, but
it is more probable that animals are the hosts. It is true that rats
and mice are infected with plague to an extent in excess of any
other class of animal, but it is doubtful if, with these rodents, as
with man, the disease is anything but epidemic in character, while
there is much to suggest that in some other animal plague finds a
permanent home; but everything points to the spread of this
disease being no simple process. The much-discussed question
of the carriage of plague by fleas has not yet been settled, but
Hankin reports that he found plague bacilli imbedded as clusters
in the tissues of the stomach of a flea caught upon a dead rat. From
the rat itself plague bacilli were not recoverable. There is much,
in our later knowledge, to suggest that the rat plays an indefinite
part in the dissemination of plague, as we have records of plague
in man without the rats in the neighbourhood being affected, while,
again, the disease may prevail in rats without man being attacked.
Recently much attention has been given to the difference between
the common long-tailed black rat of India (mus rattus) and the
brown or Norway rat of Europe (mus decumanus), as a possible clue
to the discrepancy in opinion concerning the part which the rat
plays in plague, and it may be that the different habits of these
rodents account for the peculiar limitations of plague epidemics;
but, in the light of our present-day knowledge, it is impossible to dogmatise. For an interesting, original and suggestive conception of the etiology and spread of this disease we are indebted to our brother officer, Lieutenant-Colonel Skinner (Brit. Med. Journ., 1905, i., p. 994; ii., pp. 622, 926, 1453), some of whose ideas seem worthy of serious study, even if unconfirmed.

The question of controlling plague by the induction of an acquired immunity is still being tested in India, where inoculation by Haffkine's method points to encouraging results. Taking the returns from several areas, it appears that where the procedure has been systematically carried out, 90.62 per cent. of the population were saved by inoculation; while from Aden the figures indicate an eight-fold degree of immunity among the inoculated, and a degree of protection among the attacked equivalent to rather more than double. A new prophylactic for plague has been put forward by Klein in a preliminary report to the Local Government Board (Paper No. 223), for which he claims advantages, over other prophylactics, of reliability and uniform efficacy of dosage. While working at the vitality of the plague bacillus in the tissues of animals dead of the disease, Klein dried preparations of the infected organs over sulphuric acid, and found that, after the bacilli originally contained in them had been killed by the drying, emulsions of the material, when injected in definite amounts, were capable of killing rats within a few hours, although no plague bacilli could be found in their tissues. When smaller amounts of material were injected the animals, though becoming ill temporarily, not only recovered, but were refractory against plague infection on further inoculation with virulent plague bacilli. The obvious inference from this was that the dried plague organs, though not containing any living bacilli, were imbued with a powerful plague toxin, which was capable, in appropriate dosage, of serving as a prophylactic. Arguing from these results and developing the idea, Klein has made a series of investigations which show that the necrotic nodules found in the buboes, spleen, lungs and liver of guinea-pigs which had died of plague, more particularly mild or sub-acute plague, the result of cutaneous injection, when finely minced with aseptic precautions and dried over sulphuric acid at 47° C., yield a material, which is at once readily prepared, of uniform and reliable efficacy, and superior to all other plague prophylactics. Thus, a guinea pig of 350 grammes in weight will yield from 5 to 7 grammes of dry powder, which is equivalent to some 500 protective doses for an adult rat, and which, arguing from the statistics from Haffkine's work in India, is equivalent to 1,000 human doses.
In preparing the prophylactic for use the desired amount of dried tissue powder is weighed out (say 5 to 7 milligrammes for a human adult), well rubbed down in a desired volume of sterile warm water, and the turbid emulsion thus obtained injected subcutaneously. Such material contains not only the acutely active toxin, but also the dead bodies of all the bacilli pestis originally present in large numbers in the necrotic organs (bubo, spleen, lung and liver), with probably other substances of an undetermined nature and action. This material, tested by cultivation, is found sterile. When it is borne in mind that (1) this dried prophylactic does not require more than twelve days for its preparation; (2) that a large volume can be prepared of uniform strength; (3) that its efficacy is easily standardised on the rat; (4) that it is dry, portable and preservable, and that the protection afforded by its injection lasts many weeks in the rat, its general superiority over other preparations is marked. If these claims of Klein’s can be substantiated, there can be no doubt of the far-reaching importance of his work, while the answer to the interesting and important question, whence is derived the especial efficacy of the new prophylactic, which contains not only the dead plague bacilli and associated tissue toxin, but also other tissue constituents, will have an important bearing upon the subject of immunity in general. For the present we are forced to remain sympathetically expectant that Klein’s sanguine anticipations in this field may be realised.

MALTA FEVER.—Under this head progress can be reported, notably as the result of the joint commission which has been investigating the disease in Malta. Whether this fever is as widely spread as some think, is perhaps open to doubt, but the evidence is overwhelming as to the serious extent of its incidence in Malta itself, while in some parts of South Africa and in the Philippines its prevalence is equally assured. The Commission recently at work find that the goats in Malta harbour the specific micrococcus, and that the milk from these animals contains the organism. As the milk supply of the island is derived mainly from goats, and the animals live in close proximity to man, this constitutes an observation of the highest etiological importance. It appears that, as hosts of the specific coccus, the goats do not suffer from illness, but it is only too likely that their milk when consumed is the medium by which it is conveyed to man. It is not quite clear how goats become infected originally, but Zammit, who first observed that the blood of goats fed with cultures of the Micrococcus melitensis, soon gave an agglutination reaction with the organism, calls attention to the
practice of driving goats about the streets in herds, and as the udders of many of these animals are abnormally large, often touching the ground, their liability to soiling is obvious. The reality of the danger attaching to goats' milk in endemic centres of this disease has been proved by our brother officer, Major W. H. Horrocks, who, on examining the milk of certain goats, the blood of which reacted, was able to isolate the micrococcus from it. To Horrocks we are indebted for some interesting facts regarding the saprophytic existence of the micrococcus; he finds that it survived for sixty-nine days in dry sterilised manured soil, for eighty days on dried fabrics, for seventy-two days in damp soil, for thirty-seven days in sterile tap water, and for twenty-five days in sterilised sea water. Further experiments by the Commission indicate that the inhalation or ingestion of infected dust gives rise to the disease in animals; that the ingestion of infected food gives rise to Malta fever in healthy monkeys; that the subcutaneous inoculation of the *M. melitensis* caused a typical attack in a monkey; and that the micrococcus when recovered from the urine of patients suffering from the fever, is capable of giving rise to the disease in healthy monkeys. If the experiments of Ross and Levick (*Brit. Med. Journ.*, April 1st, 1905) are confirmed, the question of a transmission of this disease by dust and infected urine becomes doubtful. These observers, it may be noted, inhaled cultures in a moist state as well as dried and finely powdered, and also drank water which had been mixed with the urine of a patient suffering from Malta fever without taking any ill effects.

The specific micrococcus has been recovered from the urine in a large number of cases, but not from the sweat, skin or faeces, though in guinea-pigs experimentally infected the organism is passed in the faeces. The organism was found to be present in the blood, though sparsely, or about one coccus in 4 cc. of blood; this fact militates against the idea that the disease may be conveyed by biting insects. For this connection, experiments made with *Stegomyia fasciata* have so far given unsatisfactory results, but before the final word can be said on this point, further experiments with other varieties of insects seem desirable. The year's work upon this disease has been undoubtedly good, but much remains to be done, notably the adoption of practical hygienic methods to establish the truth of conditions which experimental inquiry has shown to be highly probable.

**Oriental Sore and Kala-azar.**—The development of flagellated forms from the parasitic bodies associated with the names of
Leishman and Donovan, was referred to in my last review of Preventive Medicine in this Journal (vol. iv., p. 285), since then the observations have been confirmed and inferences as to their possible meaning in both oriental sore and kala-azar critically examined. The most important contribution to the discussion of the connection, if any, between these two diseases and malarial cachexia or splenomegaly, has been from James (Sci. Mem. Med. Off. Army of India, Nos. 13 and 19, 1905), who, writing of oriental sore, confirms Wright's view that many of the cells composing the new tissue contain parasites which cannot be distinguished from those obtained from the spleen and other organs in certain cases of splenomegaly and kala-azar. On the other hand, the parasites met with in the Punjab and Upper India are capable apparently of producing only the comparatively mild local disease known as oriental sore, while those in Assam produce only the dangerous general disease known as kala-azar. This suggests at once the question whether the parasites of oriental sore and kala-azar may not be different species, though belonging to the same class. No matter how many of these parasites the tissue of an oriental sore may contain, it is notorious that there results from it no general disease such as kala-azar, and assuming that the parasites in the two affections are identical it is antagonistic to the idea that the path of infection in kala-azar is by way of the skin; further, even if any biting insect were proved to be the infecting agent of oriental sore, it does not follow necessarily that this is the mode by which kala-azar is contracted. It is just possible that some have been too hasty in regarding all cases of enlarged spleen with malarial cachexia met with in the Punjab and Upper India, as being due to the parasite found in the spleen and other organs of some cases of like clinical features common in Calcutta, Madras and Southern India. As to kala-azar, James pleads for it to be regarded as a disease distinct from every other, maintaining that it has no connection with malaria; in support of this contention he points out that its geographical distribution in India is more limited, and its presence and conditions of spread quite different from those of malaria. The presumption is strong that the Leishman-Donovan parasite is both present in, and the cause of, every case of kala-azar, but complete proof is still wanting.

ANTI-VENOM SERUM THERAPY.—Some excellent work in this difficult field of study has been done by Lamb (Sci. Mem. Med. Off. Army of India, No. 16, 1905), in continuation of earlier investigation on the same lines. These researches do not lend themselves readily to analysis, but show conclusively that anti-venomous serum
is highly, if not absolutely, specific, and that, for the successful treating of any snake bite, the homologous serum must be used. The establishment of this fact, and it is in accord with Noguchi's conclusions, will be a disappointment to many, as it must limit seriously the possibility of combating the effects of snake-bites by serum treatment.

The Spirochaeta Pallida of Syphilis.—The discovery of the causal agent in syphilis has often been claimed, but the recent work of Schaudinn and Hoffmann suggests that we must now place syphilis among the rapidly increasing group of diseases due to protozoa. It may be regarded as certain that, in the primary and secondary lesions of syphilis, a special organism is constantly present: this is the Spirochaeta pallida, first identified by Schaudinn and Hoffmann (Berl. Klin. Wochsch., May 29th, 1905). This organism can be detected fairly readily by suitable methods in the primary and secondary manifestations of the human disease, and has been found to be present, moreover, in the syphilis produced in apes by inoculation with virus from chancres. In appearance the S. pallida resembles a thin spirally-twisted thread, moving backwards and forwards by rotating in a corkscrew fashion round its longitudinal axis; in addition to this motion its whole structure has been described as undulating to such an extent as to amount at times to a lashing movement. The S. pallida is distinguished from other members of the spirochaeta group by its minuteness and low refractive index, also by the fact that the coils of its spiral are numerous, deep and closely arranged. Its average length is 10 μ with a thickness of 0.6 μ; the number of its coils or twists varies from 8 to 18, each measuring about 1.1 μ. In some specimens a trace of what appears to be an undulatory membrane has been noted, and Schaudinn has observed in certain cases the existence of a single flagellum at one end with two at the other extremity. The organism is Gram-negative and peculiarly difficult to stain by ordinary methods; it is extra-cellular, but has been observed to be attached to a pus cell, having one end embedded in the cell body. Among other forms of spirochaeta are S. refringens, common in all inflammatory conditions of the glans and prepuce such as balanitis, also S. buccalis found in Vincent's angina, the S. obermeieri of relapsing fever, the S. anserina of goose septicaemia, and the spirochaeta of so-called fowl spirillosis. All this group of spirochaetae are extremely primitive forms and must not be confused with the vibrios which are true fission fungi. The chief organism from which S. pallida has to be distinguished is the larger S. refringens,
so common in genital ulcers of all kinds, and which further resembles the spirochae already known to occur in noma, phagedena, Vincent's angina, ulcerative stomatitis and hospital gangrene. Bearing in mind that Schaudinn's earlier work had shown that one species of spirochaeta (S. ziemanni) was merely the asexual stage in the life cycle of the trypanosome of the stone-owl, it is suggestive that the sexual disease of the horse, known as dourine, is due to a trypanosome, and that the infective agent is conveyed by direct contact, even through unbroken mucosa, without the assistance of the usual intermediate invertebrate host. Further, in kala-azar, a disease caused presumably by the Leishman-Donovan bodies, and which are also but a stage in the life cycle of a flagellate, there is some evidence of its communicability by the sexual act.

As there is little doubt that the spirochaeta of syphilis is the causal germ of the disease, a knowledge of its easy and rapid demonstration becomes increasingly important. A considerable number of papers have appeared discussing suitable methods; the most important, perhaps, is that by Giemsa (Deutsche Med. Wochschr., June 29th, 1905, p. 1,026) in which he recommends a mixture of azur ii.—cosin, 3 grammes, azur ii. 0·8 gramme, Merck's pure glycerine and methyl alcohol, 250 parts of each. This solution is said to keep well, even in the tropics. The film or preparation is first fixed in absolute alcohol for twenty minutes, then one drop of the staining solution is added to one cc. of distilled water and the diluted stain used for fifteen minutes. The spirochaeta stain better if from one to ten drops of a solution (1 to 1,000) of carbonate of potash be added first to the distilled water with which the stain is diluted. Herxheimer and Hubner (Deutsche Med. Wochschr., June 29th, 1905, p. 1,023) do not like Giemsa's stain, but recommend staining the preparation for twenty hours in a filtered 10 per cent. solution of a mixture of Nil and Capri blues. With this reagent they succeeded in demonstrating the parasites in a section of a syphilitic papule. As an alternative, Reitmann (Deutsche Med. Wochschr., June 22nd, 1905, p. 997) advocates that after fixation in absolute alcohol for ten minutes the preparation be placed for five minutes in a 3 per cent. solution of phosphotungstic acid, then washed in distilled water and 70 per cent. alcohol, then washed again in water. Now stain in hot carbol fuchsin, wash in water, pass rapidly through 70 per cent. alcohol, re-wash, dry and mount. By this method the cell protoplasm is very little coloured, the nuclei are dark and the spirochaetae come out a clear red colour.
In the *Comptes rend. Soc. de biologie*, Paris, June 24th, 1905, p. 1,044, Proca and Vasilescu recommend fixation for thirty minutes in alcohol, then in a mixture of phenol 50, tannin 40, and water 100 parts, to which is added 2.5 parts of basic fuchsin. Leave the film or specimen in this mixture for ten minutes, wash and stain for another five minutes in phenol gentian violet. From the diversity of methods suggested it is evident that the demonstration of the spirochaetæ is not difficult, although there may be still differences of opinion as to the best method. Bandi and Gimonelli (*Münch. Med. Wochsch.*, August 29th, 1905), maintain that the elaborate methods of Giemsa and others are unnecessary, and that good results can be obtained by means of the usual methods for staining tubercle bacilli, namely, by first fixing in alcohol or with heat, and then staining for a few seconds with hot carbol fuchsin. Leishman has obtained good results when using his own reagent in the proportion of 4 parts to 5 parts of water, after re-vitalising the film with blood serum and giving a contact with the stain of twenty-five minutes; while similar success in demonstrating an undulatory membrane in the larger spirochaeta of a monkey has resulted by the employment of Löffler's stain with an exposure of fifteen minutes at 50°C.

Whatever may be the final verdict as to the precise etiological significance of the *S. pallida* in syphilis, it is noteworthy that the only strong opponent to Schaudinn's views and theory is Thesing, who (*Münch. Med. Wochsch.*, July 11, 1905) makes the following criticisms: (1) The spirochaeta is a bacterium, not a protozoon, because it has neither nucleus, flagellæ, nor an undulatory membrane. (2) The alleged specific differences between *S. pallida* and *S. refringens* are inadequate. (3) Schaudinn's specimens showed so many other organisms as to suggest extraneous contamination. Whatever may be the value of the second and third points, independent observation of others indicates that the first criticism is based upon a misapprehension of facts.

**EXPERIMENTAL SYphilis.**—In amplification of their earlier work, Metchnikoff and Roux have published their latest researches on the production of experimental syphilis in anthropoids (*Ann. de l'Instit. Pasteur*, November 25th, 1905, p. 673), stating that the chimpanzee is the animal in which the closest analogy to the human disease can be reproduced. In twenty-two chimpanzees inoculated with syphilitic virus from various sources, not a single case of failure has been noted, on the contrary, the lesions produced in every case being typical. The incubation period averaged thirty days. Eight
out of the twenty-two animals presented secondary lesions; the period of incubation of the secondary lesions, that is to say, the time elapsing from the appearance of the primary lesions to the appearance of papules, varied from nineteen to sixty-one days, with an average of thirty-three days. Of the fourteen animals which did not develop secondary symptoms, some had been inoculated with attenuated virus and some died before the lapse of the requisite incubation period. In the case of one animal, which developed secondary symptoms, the disease assumed a severe type; the hair came off all over the body and the bare skin exhibited a very abundant crop of papules, later, these lesions ulcerated; finally, the animal fell into a condition of extreme cachexia and died. Bacteriological examination of the ulcerated papules revealed an abundance of various micro-organisms, particularly streptococci, and the authors consider it probable that death may have been due to a secondary non-syphilitic infection. In several of the animals paralytic symptoms appeared during the secondary stage, but gradually passed off. In the animals which died, death was due in most cases to broncho-pneumonia; but the autopsies failed to reveal any signs of tertiary syphilis. The efforts of the authors to devise a means of conferring immunity have not, so far, met with any marked success, but, as they are continuing the work, they are not without hope that such may be attained ultimately.

In connection with this research it is interesting to note that out of thirty-one cases of monkeys of various kinds in which syphilis after inoculation had been induced, 23, or 74 per cent., revealed the presence of the S. pallida of Schaudinn. Metchnikoff and Roux further state that in all their work upon these cases they have never found S. refringens, or any other spirilla save that of syphilis.

RECENT WORK ON PROTEID CHEMISTRY AND METABOLISM.—Although the chemical constitution of the proteid molecule is still a matter of doubt, our increased knowledge of its analytical cleavage products suggests that we may soon see the puzzle solved. For this knowledge we are indebted mainly to Kühne, Kossel and Emil Fischer. The earlier conception of the changes which result from the gastric and tryptic digestion of proteids was the formation first of primary proteoses or albumoses, followed by their further splitting up into secondary proteoses, which in turn broke into the still smaller molecules of peptone, a differentiation being made between the peptone produced in the stomach and that produced during pancreatic digestion. Recent work shows that there are no essential differences between the action of the gastric agent pepsin-hydro-
chlo-
ric acid and the pancreatic enzyme trypsin. These proteolytic ferments act by a process of hydrolysis, splitting the proteid molecule into proteoses, then peptones, and finally into simple products, such as the mono-amino acids glycine, alanin, leucin, asparagin and aspartic acid, or the di-amino acids ornithin, arginin and lysin, or aromatic amino acids like tyrosin and tryptophane, or certain nitrogenous derivatives of the benzene ring, like indol, scatol, cytosin and cystin. These substances represent various nuclei which exist preformed in the proteid molecule, and are then linked in more or less complicated groups. During proteolysis some of these groups can be detected and separated, notably the combinations of the amino acids which Fischer has termed polypeptides. The polypeptides are ultimately broken down into the amino acids of which they are composed, and practically occupy a position intermediate between the proteoses and peptones on the one hand and the final product on the other. Further, inasmuch as many of these polypeptides have been made synthetically there is great promise of the final synthesis of the larger proteid molecule.

Passing from these purely chemical considerations we find that simultaneous advances have been made in our knowledge of proteid absorption. Not long ago, leucin, tyrosin and similar substances were regarded as so much waste material passing to the liver for conversion into urea. We now know that the absorptive epithelium of the intestine can regenerate the more complex proteid molecule from the quite simple cleavage products. There is thus an analogy between what happens to proteids and what has long been known to happen to fat and carbohydrate. Fat during digestion is split into its fatty acids and glycerin; during absorption and assimilation it is once more synthesised from these simple molecules. So, too, during the digestion of starch, dextrins first appear as analogues of albumens and peptones; these dextrins are converted into molecules of maltose or analogues of the polypeptides, and finally, the simple glucose molecules are formed from the maltose. Ultimately, on absorption and assimilation the larger molecule of glycogen is built synthetically from the small molecules of sugar. Moreover, animal experiments indicate that it is possible to maintain weight, health and nitrogenous equilibrium for a considerable time by feeding upon the crystalline cleavage products resulting from pancreatic proteolysis; in other words, we are forced to form an entirely new conception of the origin of the most important chemical material of living tissue.

If, then, this theory or assumption of a complete breakdown of
the proteid food in the alimentary canal previous to its being built up into living tissue be true, what will be the fate of food proteids when introduced into the blood stream without the intervention of the alimentary digestive processes? Mendel and Rockwood's work (Amer. Journl of Phys., vol. xii., p. 336, 1905), throws some interesting light on this point. They find that proteids administered by intravenous or intraperitoneal injection are retained and apparently utilised, though rapid injection may cause toxic symptoms. Clinical experience with nutrient enemata indicates the possibility of a direct absorption of proteid without previous digestive changes. Although the usual form in which the body gets its proteid is by building it up from simple crystalline materials, the result of intestinal digestion, still, if these are not available, it can get proteid by absorbing the larger molecules presented to it. This is not inconsistent with the view that preliminary proteid cleavage is a necessity, though the locus of that cleavage need not be necessarily the alimentary tract; the fact being that the cells of the other tissues or the enzymes present in those cells are capable of a vicarious action, doing the work of trypsin and the erepsin of the intestinal juice. This view is supported by Vernon's recent discovery that every tissue of the body has an ereptic action. The importance of the tissue enzymes or erepsins in the metabolic cycle during life has long been suspected, and there can be little doubt that they are the means which enable the tissue cells to break down and then assimilate the proteids brought to them by the blood and lymph, as well as to initiate those subsequent katabolic changes which terminate in the excretion of waste materials.

The fundamental laws governing proteid katabolism are closely associated with, if they do not actually dominate, those governing the composition of urine, and the recent work of Otto Folin (Amer. Journl of Phys., xiii., 1905, pp. 45, 66 and 117) prompts the thought that we must reconsider our ideas not only as to the nature of proteid metabolism but as to what is and what is not a normal urine. To appreciate the meaning of the newer ideas, one must be reminded that two main theories have long held the field, they are those of Voit and Pflüger. Voit's theory states that katabolism occurs only in "dead" or "circulating" proteid; while Pflüger enunciates that before katabolism occurs, all the proteid input must be transformed into living material. Folin suggests that neither of these extreme views is correct, but that nitrogenous katabolism is of two kinds—one is
Immediate, inconstant, varies with the food, and leads to the formation of urea and the inorganic sulphates; the other is constant, smaller in amount and largely represented by kreatinin, neutral sulphur compounds, some uric acid, aromatic sulphates, and possibly a little urea. This latter form of metabolism may be regarded as tissue or endogenous metabolism, whilst the other is exogenous. The endogenous metabolism marks the limit of the lowest level of nitrogenous equilibrium, and the proteid input sufficient to maintain it is the indispensable minimum. It is possible that the proteid which is metabolised exogenously is by no means necessary; there is certainly evidence to show that it can be replaced by non-nitrogenous food. In other words, the catabolism which ends in urea formation is of less fundamental importance than that which leads to the elimination of kreatinin; possibly the formation of ammonia and amino-acids as the result of intestinal activity is but a means of getting rid of any excess of nitrogen taken in. Or we may say that the organism requires only the small amount of nitrogen necessary for endogenous metabolism, and that yielded by an excess proteid input is unnecessary.

An interesting corollary to these purely theoretical considerations is afforded by Chittenden's recently published experiments (Physiological Economy in Nutrition, 1905, Lond., Heinemann) with reference to the minimal proteid requirements for healthy men. It is well known that physiologists have varied greatly in their estimation as to the true physiological necessities of the body for proteid food. The standard of diet generally accepted for an adult man of average weight (145 lbs.) doing a moderate amount of work is that suggested by Voit, namely, proteids 118 grammes, of which 105 grammes would be absorbable, fats 56 grammes, and carbohydrates 500 grammes, with a total fuel value of 3,000 calories. There is much evidence to show the possibility of a much lower standard of diet sufficing to meet the real physiological needs of the body. Thus, Kumagawa, studying the diet of the Japanese, found with a purely vegetable diet containing per day 54.7 grammes of proteid (equal to 8.75 grammes of nitrogen), 25 grammes of fat and 570 grammes of carbohydrate, that health and nitrogenous equilibrium were maintained; the average daily output of nitrogen by the urine being 6 grammes and in the faeces 2 grammes, or, of the total nitrogen-containing food, barely 70 per cent. was absorbed. Chittenden's singularly elaborate and fully detailed experiments, carried out during many months on professional men, on soldiers of the Medical Corps of the United States Army, and on trained
athletes of the universities, confirm these facts, and make it plain that our accepted dietary standards are too high, and prove conclusively that the physiological needs of the body can be met by a greatly reduced proteid intake as represented by some 60 grammes. This, moreover, can be accomplished without increase in the daily intake of non-nitrogenous foods. In Chittenden’s experiments the proteid intake was reduced to half, and in some cases to less than half, the number regarded as normal. After a variable initial drop in body weight, the deprivation was apparently followed by no untoward results; equilibrium was maintained, the health remained perfect or improved, the muscular force in the athletes was usually increased, mental activity was undiminished and desire for richer food disappeared. His results obtained with thirteen soldiers living for over five months on a prescribed diet and exposed to the ordinary stress of military service, are of peculiar interest to ourselves. A metabolism of less than 50 grammes of proteid per day, corresponding to 7 to 8 grammes of nitrogen, was quite sufficient for the needs of the body, and a fuel value of some 2,600 calories was ample to meet the requirements of these men. In the face of these carefully conducted observations, it may be asked why adhere to an input of 118 grammes with a metabolism of 105 grammes of proteid per day? The affirmative answer is not easy, especially when we recollect that the 18 grammes or so of nitrogen in the urine, resulting from this higher intake of proteid, reach the final stage of urea, &c., only by passing through a series of stages, each one of which means the using up of energy, to say nothing of that required in digestion, absorption, &c. It needs little imagination to picture the amount of physiological labour which the daily handling by the body of such amount of proteid food entails, or to fancy how the liver and kidneys must rebel at times at the excessive labour they are called upon to perform. An additional justification for moderation in proteid ingestion lies in the fact that many of the nitrogenous katabolites are toxic and that the evil results due to their accumulation are real. In a word, all this newer work is a suggestive plea for a revolution in our ordinary dietary and a reversion to what may be called a simpler mode of living.

To us, as practical soldiers, the whole question is of importance, especially in so far as it bears upon the dietaries of men serving in the tropics or on field service. To many of us the idea is not new that much preventable disease and possibly some of the excessive liability to certain types of fever, particularly among young Europeans in
the tropics, is to be attributed to faulty alimentation, mainly in the
direction of excessive consumption of proteid foodstuffs. But before
we can accept the newer doctrine in its entirety there is need of
further research, notably in the determination of precise facts as to
metabolism in the European on translation to a tropical climate.
All our hitherto accepted physiological teaching as to nitrogenous
balance needs to be put aside and the subject approached *de novo*.
We need to know what is a normal urine both at home and in
a tropical country. A vast field for research lies open before our
officers serving in all parts of the world. Work on the lines which
Statham of our Corps has so well sketched out in the pages of this
Journal will well repay the labour which it involves. Personally, I
am convinced that Chittenden's views are near the truth, and that
subsequent research and experience will both confirm and extend
his ideas: for the present, however, sympathetic caution should be
our attitude, coupled with every endeavour to observe and determine
facts bearing on this question; not the least important of these will
be evidence showing whether the power of resistance to disease is
diminished in any way by a low proteid intake. The problem
involved in the answer to the question—what is the minimal proteid
requirement for the healthy man?—is one that must attract the
attention not only of the physician and the physiologist, but also
claim the careful consideration of the economist and sociologist.
It is a question which concerns as much the man interested in
securing immunity to disease as the individual whose interests are
centred chiefly in the prevention of race degeneration.

THE INTER-RELATION OF TYPHOID AND PARATYPHOID INFECTIONS.—It is well known that in certain cases of continued fever,
presenting all the clinical symptoms of mild enterica, the Grüber-
Widal reaction is absent. The existence of this fact has suggested
the recognition of a new disease, called, for want of a better
name, paratyphoid; further, the majority of these cases have
been found to be associated with the presence in the body of a
bacillus culturally distinct from the bacillus of Eberth. Biologically
this bacillus occupies a position intermediate between the common
colon bacillus of Escherich and the enteric micro-organism of
Eberth and Gaffky (*Journal Royal Army Medical Corps*, vol.
ii., p. 241). The origin of these paratyphoid infections is as yet
imperfectly understood, though it is probable that, like enterica,

1 *Journal Royal Army Medical Corps*, vol. i., p. 191, also vol. ii., pp. 123 and
320.
they are transmitted by means of fouled food and water. A very suggestive paper bearing on this subject, and one which seems to have attracted much less notice than it deserves, was published last year by our brother officer, Captain A. B. Smallman (Journal Royal Army Medical Corps, vol. v., p. 137.) He treated some two hundred guinea-pigs with living and dead enteric bacilli, injecting pure cultures of the micro-organisms into the abdominal cavity; all died at varying periods following the injection. The micro-organisms cultivated from these animals after death were in the majority of cases true and typical enteric bacilli; but in twenty-two cases in which living enteric bacilli had been injected the micro-organisms found were of the intermediate or paratyphoid type. It is an interesting question, whence did they come? They were not found in the faeces of the control animals, but were present in the intestinal contents of the guinea-pigs which had been inoculated with enteric germs; so clear are the facts that it would seem possible that a transformation had been effected within the animal. This conception opens up a wide range of possibilities, so much so that one cannot resist hoping that this clue may be followed up to determine whether the paratyphoid bacillus in the lower animals is the analogue of Eberth’s bacillus of enterica in man, and whether the latter may not be regularly transformed into the former in passing through certain animals, notably pigs, dogs, cats, and rats, and so cause this infection in man. Many of the types of so-called swine-fever in pigs are eminently suggestive of this sequence of events, while many rats found in styes and yards frequented by infected swine are not free from similar lesions. Another point which suggests itself is, whether an attack of paratyphoid fever confers immunity against genuine enterica. If it does confer immunity, and if passage through one of the lower animals transforms the enteric bacillus into the less virulent intermediate or paratyphoid, there may here be a suggestion of protective inoculation against the more serious disease.

THE ENTERIC BACILLUS AND PULMONARY INFECTION.—That the specific bacillus enters the general circulation and is not infrequently found in the lungs during an attack of enteric fever is well established, but the precise effect of the presence of these micro-organisms in the lungs is still uncertain. Robinson (Journ. Inf. Dis., vol. ii., p. 498) reports observations which throw some light upon the significance of the enteric bacillus in pulmonary complication of enterica. One case, after being in hospital twenty-seven days with a typical attack of typhoid fever, suddenly develops the signs
of lung infarction, after which he passes into a marked toxemic condition, and dies a few days later. At the autopsy one lobe of the right lung is converted into an abscess cavity. A broncho-pneumonia is present in the rest of the right and throughout the left lung; a pure culture of the enteric bacillus is obtained from the abscess, and in both lungs bacilli alone are seen in sections stained with methylene blue, but no organisms are seen in sections stained by the Gram-Weigert method. The complete absence of any other causative agent suggests the conclusion that the enteric bacillus is capable of causing pulmonary abscess and gangrene in lung tissue. The existence of broncho-pneumonia in the lungs of those dying of enterica is frequent, and when the various inflammatory lesions produced by the enteric bacillus are considered, it is not unreasonable to suppose that this organism can produce broncho-pneumonia; the evidence, however, in support of the idea is astonishingly small. Chantemesse and Widal, who were the first to investigate the subject, drew the conclusion from certain cases that came under their notice that broncho-pneumonia found in enteric fever was to be considered as a specific manifestation of the disease. Finkler, Bruneau, Lepine and Bancel, have all reported cases of similar nature and expressed the same view. As to the lobar pneumonia complicating enterica, it is probable that the enteric bacillus plays a subordinate rôle. All investigators have found the pneumococcus present in such conditions, and regarded it as a secondary invasion. The \textit{B. typhosus} has been rarely isolated in these cases, though it can undoubtedly exist together with the pneumococcus in the circulating blood of patients suffering from lobar pneumonia as a complication of enteric fever. Although there is no doubt that the pneumococcus is the usual cause of lobar pneumonia in enterica, there is some evidence that such a lesion may be caused by the typhoid bacillus. Curschmann long ago held this view, although admitting this must occur very rarely. Other writers for the most part consider a true typho-pneumonia as unproved or even impossible. Robinson (\textit{op. cit.}) gives details of a case which is very suggestive, but it is noteworthy that the bacillus isolated was of the paratyphoid type \textit{B}. The experimental work upon this subject is small, and mainly negative; the best being that by Lepine and Lyonnet (\textit{Arch. de Med. Exp.}, 1899, vol. ii., p. 549). The fact that the enteric bacillus may be present in the lungs in the pulmonary complications of typhoid fever, makes it probable that it is not infrequently present also in the sputum of these cases. The most extensive study of this question has been made by Jehle (\textit{Wiener Klin. Wochsch.}, 1902, p.
271, who concludes that the enteric bacillus is present regularly in the sputum and bronchial secretions of enterica, complicated by pneumonia or broncho-pneumonia, and that it exists there only a little less regularly in the sputum and bronchial secretions of cases with simple bronchitis only. This is a fact which should be emphasised, in order that any spread of the disease by this means may be prevented. It would, further, appear that when either \textit{B. typhosus} or \textit{B. paratyphosus} \textit{B.} are the causative factors of a lobar-pneumonia in enterica, the pneumonia is of a hæmorrhagic character, clinically recognisable from the bloody nature of the sputum.

\textbf{Recent Work on Pneumonia.—} In October, 1904, a Commission was appointed by the health department of the city of New York to investigate the subject of pneumonia. A report of some of the work undertaken for this Commission by skilled laboratory workers has now been published (\textit{Journ. Exp. Med.}, vol. vii., No. 5, August 25th, 1905). The Commission decided to concentrate attention first upon the bacteriological and clinical aspects of lobar-pneumonia. Among the studies bearing on these points which it was decided to pursue were the following: (1) A study of the occurrence and virulence of the pneumococcus and organisms related to it in the human mouth in health and disease; (2) the evidence of variations in virulence of the pneumococcus; (3) the occurrence of pneumococci in hospitals, homes and asylums before and after outbreaks of pneumonia; (4) the vitality of the pneumococcus under various conditions; (5) a study of mouth disinfection. Although the various papers of these workers constitute the contents of the whole number of the \textit{Journal of Experimental Medicine}, it must be confessed that the results are disappointing, and little more than a reaffirmation of what has long been known. The pneumococcus seems to have been found very frequently in the mouths and throats of apparently healthy persons, and differing little in virulence for rabbits and mice from the same organism obtained from persons with pneumonia. An interesting fact is that recorded by Norris and Pappenheimer to the effect that pneumococci are present, not only in the secretions of the mouth and throat, but actually in the lungs of all autopsies, and this, whether the lungs were normal or were the seat of pneumonic lesions. The significance of this discovery is, however, weakened by control experiments, which make it probable that the organism was carried into the lungs by fluids collecting in the mouth of the dying person and flowing down the air-passages during the transfer of the body.
In the course of a comparative study of pneumococci and streptococci, by Longcope and Fox, attention was directed to Rosenow's statement, that acid is formed in considerable amount in cultures of pneumococci in pneumonia serum, but not when serum from normal persons is used. Longcope and Fox found that this phenomenon is dependent upon the appearance in the serum, under certain conditions, of a substance from which the pneumococcus is capable of forming large quantities of acid. This substance is almost constantly present in the serum during an attack of pneumonia, but disappears after the crisis; what it is, and what occasions its presence in the blood during the pneumonic process, they were unable to determine satisfactorily. The practical significance of the presence in the blood of this substance is not apparent, but it may be protective in intention, as acid is definitely inimical to the growth of the pneumococcus. Certainly the subject invites further investigation. Some experiments of F. C. Wood, made to determine the viability of the pneumococcus after drying, are of interest as confirming what is known regarding the vulnerability of this organism. It is tenacious of life when undisturbed in moist or dried sputum kept in the dark, but exposure to sunlight kills it rapidly; oxygen is also fatal to its prolonged existence, for it was found that the organisms perished in from one to four hours in powdered sputum, even when kept in the dark. In coughing, sneezing, or even talking, a person suffering from pneumonia expels many fine droplets of saliva or sputum containing great numbers of the specific pneumococci; this danger to others seems to be minimised by the rapid death of these micro-organisms under the influence of fresh air and sunlight.

Epidemic Cerebro-spinal Meningitis.—At the commencement of the year an epidemic of this disease broke out in America. In New York alone, during February, 149 deaths occurred from this disease, and so serious was the view taken by the City Board of Health that a Special Commission was appointed to investigate the disease, and to endeavour to devise means to prevent its spread; this report is awaited with interest. About the same time over 400 cases occurred in the neighbourhood of Kattowitz, in Siberia, with a mortality of 37 per cent.; while in this country, a small localised outbreak was reported simultaneously at Irthlingborough, in Northamptonshire. Of all the epidemic diseases attacking mankind this affection, or cerebro-spinal fever, as it is sometimes called, offers some interesting problems for elucidation, and the possibility of its epidemic spread is a serious question for those in charge of
the public health. Although it is one of the most fatal of the acute diseases, yet owing to the peculiar local distribution of the epidemic it does not figure largely in the death returns. On the other hand, sporadic cases are by no means uncommon.

About the bacteriology of the disease a good deal is known, though the subject cannot as yet be said to have been thoroughly elucidated. The organism most frequently associated with the infection is the *Diplococcus intracellularis meningitidis* first described by Weichselbaum, who found it in the purulent exudate from cases of the disease, and affirmed the constancy of its presence in sections of the brain and its membranes. Councilman and Osler have confirmed this work by an extended series of observations. From all the evidence there seems to be no doubt that this diplococcus is the causal agent in a large proportion of cases at least; other bacteria which have been found occasionally in association with the disease are the pneumococcus and Friedländer's bacillus. With regard to the detection of the organism during life, the fact that it has frequently been identified in the cerebro-spinal fluid, withdrawn by lumbar puncture, makes this method of examination an important aid to diagnosis. Little is known about the channel by which infection with the *D. meningitidis* takes place, but evidence exists in support of the hypothesis that it may gain access to the brain directly by the nasal, auditory or other passages. The distribution of the micro-organism in nature is not known. The disease does not seem to be directly contagious, and is probably not transmitted by clothing or the excretions. In the present state of our knowledge it is difficult to understand how any particular epidemic arises; but in view of the fact that most outbreaks are localised and rarely widespread, the risks of an importation of the disease into this country from America or elsewhere are not very great. Farrar, in his report on the Irthlingborough outbreak (*Rep. Loc. Gov. Board*, No. 218, 1905), observes that the multiple invasions of houses, together with evidence that in three instances the first person to be attacked in a household had been in personal relations with the sick or with other members of a family already attacked, suggest that the disease was transmitted directly from person to person or by the families of the sufferers. There were, however, other circumstances which do not favour the explanation of direct personal infection. The most striking of these is the time which elapsed before attack in the case of persons more or less continuously in relation with the sick or residing in an invaded house. Farrar gives several instances of this kind, and specially points to
the circumstance that four teachers at one primary school living in different parts of the village were attacked, while no cases (with one doubtful exception) occurred among any of the children attending this school. No evidence could be obtained as to the prevalence of similar diseases in the neighbourhood of Irthlingborough, or as to the manner in which it became introduced into the village.

**AIR-BORNE INFECTION FROM SMALL-POX HOSPITALS.**—The recent reports to the Local Government Board, by Drs. Buchanan and Reece, upon the influence of small-pox hospitals in the dissemination of this disease in the respective towns of Gateshead and Liverpool, has given rise to much discussion during the past year as to the manner in which the infection of small-pox is carried, especially with regard to the air-borne theory. This is a very old controversy, dating back to the well-known report of Power on the Fulham Hospital, some twenty years ago, in which he, recalling familiar facts respecting the transit of floating particulate matter for considerable distances through the open air along lines of definite convection currents, suggested that in course of the many days in which a small-pox hospital is used during an epidemic, occasions now and then arise when infectious particles carried upwards from the hospital are conveyed by local air currents to places in the neighbourhood without undergoing any great degree of dispersion. The hypothesis of aerial convection depends on an assumption of the probability that it may occur, and, if this assumption be sound, affords a consistent explanation of the facts which exist apparently with regard to hospital influence, more particularly the recurrence and persistence of small-pox round the same hospital in different epidemics. The alternative theory is that all these phenomena of hospital influence result, in some way or other, from personal communication and traffic. Since Power's work, numerous instances of alleged hospital influence have occurred and been analysed, notably the case of the small-pox hulks in Long Reach, the Glasgow hospital at Belvedere, and more particularly the recent circumstances at Gateshead and Liverpool. Reece's report on the conditions prevailing at the last-named place is the most important contribution to the question which has appeared of late. In that city there were two old hospitals, Priory Road and Park Hill, and one new one at Fazakerly, outside the city boundary. According to Reece, the inhabited areas within a mile of each of these three hospitals have suffered more severely from small-pox than the city as a whole, the exceptional incidence of the disease within these
areas corresponding, in point of time, with the use of these hospitals for the treatment of acute small-pox cases; further, broadly speaking, within these hospital areas, the dwellings nearer to hospital have sustained a heavier incidence from small-pox than those further away. These conclusions have been vigorously challenged by Dr. Hope, who points out that if hospital influence was the cause of the greater incidence in certain areas, how is it that the area around the Fazakerly building was more heavily affected, when that hospital is surrounded by an open space of 130 acres, while the other hospitals are in crowded areas? Further, in the vicinity of the Park Hill Hospital twelve distinct outbreaks of small-pox occurred before the hospital was opened and nine after the reception of patients. He asks, pertinently, why should these latter be ascribed to the influence of the hospital, while the other twelve were evidently independent of it? Besides, four of the nine outbreaks were proved to have been infected elsewhere. In fact, Hope shows that considerably more than half of the cases occurring within a mile of one or other of the hospitals are traceable to infection in other places, fifty-four had been overlooked and treated as chicken-pox in private houses, others travelled in trams, worked in shops or factories, attended the out-patient department of hospitals, or roamed the streets. In short, Hope maintains that the effects of hospital influence has been exaggerated, and that, in view of these instances which he quotes, if only the facts could be fully known, the results of so-called hospital influence can be explained from personal infection of this kind. This view was developed and defended by Newsholme, in a recent discussion before the Epidemiological Society (Lancet, May 20th, 1905), wherein, adopting the argument that it is impossible to prove a negative, he argued that it is practically impossible, in the case of the best administered hospital, to say that no communications have taken place between patients or infectious members of the staff on the one hand, and persons living near the hospital on the other; and that the hypothesis that numerous and persistent communications of this kind have, after all, occurred, is always to be preferred to the hypothesis of aerial convection. One must admit the question bristles with difficulties, but the more recent cases of hospital influence are not altogether easy to reconcile with this view, and certainly the hospitals lately concerned have been those of cities which leave little to be desired in the way of hospital organisation, administration, and readiness for emergency. Certainly many of the Liverpool cases are explicable by causes other than the vicinity of the
hospitals, but the experiences of Glasgow and Long Reach are a strong indictment as to the reality of so-called hospital influence. As to Gateshead, it is noteworthy that the so-called hospital influence was mainly felt in the district of Felling, which neither sent cases to the Gateshead Hospital, nor had any traffic or communication with that institution. Neither can unknown personal contact explain some cases at Monsall, where, in three days, small-pox appeared in four separate wards of the fever hospital, which adjoined the small-pox hospital, coincidentally with an outbreak in the outside neighbourhood. The fullest inquiries failed to trace personal communication in either set of cases, while both outbreaks are readily explicable by aereal convection. It is, moreover, well known that no indication of malign hospital influence attaches to the case of institutions containing patients suffering from scarlet fever, diphtheria, or enteric fever; or in the case of hospitals receiving only convalescent cases of small-pox. Setting aside all bias, we must agree as to the reality of aereal convection at short distances, for all infection, other than by inoculation or ingestion, is through the air; we must further recognise that every outbreak of an infectious disease in crowded centres affords instances not only of aereal convection, but of personal infection and spread of disease from virulent concealed cases. We must be careful to maintain each of these factors in proper perspective, and, in all diseases, to admit and provide for every form of transmission, of which aereal convection is but one, though perhaps playing a more important part in small-pox than in others, and, judging from the analogy of other particulate matter, by no means confined to comparatively short distances. In any case, and whatever opinion may be held as to the merits or demerits of the air-borne theory, the building of small-pox hospitals in populous districts is probably a thing of the past.

The Cause of Return Cases of Infectious Disease.—An important and interesting report on this subject by A. G. R. Cameron has been issued by the Metropolitan Asylums Board, dealing mainly with scarlet fever and diphtheria. The scarlet fever return cases appear to be most numerous during the winter months, but no close relationship is found between high infectivity rates and a short average period of detention in hospital. When the largest number of return cases occurred, the infecting cases suffered from mucous discharges after leaving hospital more frequently than during the remaining months. From the statistics given, it appears that between the ages of 4 and 10 years patients carry home infection more frequently than at any other age period, and that return
cases contracted in this way have a higher mortality than ordinary cases. Cameron is disposed to attribute the infection of return cases to such complications as nasal discharge in the patient rather than to failure of disinfection in the home or duration of detention in hospital, and believes that such complications lead to a recrudescence of infection, though nasal or aural discharges may not be necessarily infectious per se. He concludes that return cases of scarlet fever are due to premature discharge from hospital, even though the infecting case may have been detained longer than usual; as a rule, this premature discharge cannot be foreseen. It is recommended that for patients, particularly those predisposed to mucous discharges, the usual warm bath should not be given immediately before discharge, but on the previous evening.

Similar general conclusions are made in respect of diphtheria, though return cases are less common after this disease than after scarlet fever, and a larger proportion of alleged cases are doubtful or explicable as coincidences. The value of this report is much enhanced by critical remarks made by the superintendents of the Board's hospitals; they make the significant observation that "in the present state of our knowledge it is difficult to see how the occurrence of return cases of either of these diseases can be prevented either in home-treated, or hospital-treated patients." These views are practically an endorsement of Cameron's conclusions. All modern experience goes to show that the evidence is strong that the infectivity of scarlet fever lies not in the desquamating cuticle, but in the throat and nasal cavities; the beneficial influence of hospital treatment in the incidence of either scarlet fever or diphtheria, cannot be realised to the fullest extent without cooperation on the part of parents, doctors and sanitary authorities.

The Problem of Tuberculosis.—The chief interest in this field of work during the year, has centred round efforts to secure a cure, or prevention, for the tuberculous process, rather than work directed to unravel the mysteries of its etiology. For this we are mainly indebted to Behring's communication to the International Congress on Tuberculosis. Preliminary to reviewing this question it may be advantageous to refer briefly to the characters and alleged specific actions of the more important tuberculins or so-called remedies for this affection. The original tuberculin of Koch was a clear brownish fluid, obtained by filtering through a porcelain filter a glycerine broth culture of tubercle bacilli which had been evaporated over a water-bath to one tenth of its volume. When injected in small doses into persons or animals suffering from
tuberculosis a marked rise in temperature resulted, accompanied by an inflammatory reaction around any tuberculous foci present in the body. This reaction involved a necrotic change in the tuberculous foci, leading to the disintegration of the diseased tissue and a liberation of any living tubercle bacilli contained in it. An appreciation of the risks attaching to this facilitated dissemination of the infective organisms from tuberculous foci, indicated that this tuberculin could not be used with safety for prophylactic purposes. As a result of further work, Koch adopted a new method of preparing tuberculin. Highly virulent cultures of tubercle bacilli were carefully dried and finely triturated in an agate mortar, whereby the bacilli were reduced to fragments, and the specific substances which they contained, being liberated from the fatty substance which surrounds the bacillus, rendered capable of passing easily into solution. To these finely triturated bacilli, distilled water was added and the mixture centrifugalised, the result being a separation into an upper opalescent layer containing no bacilli and a lower layer of deposit. This deposit, after drying and trituration, was mixed with water and centrifugalised; this process was repeated until the transparent layer after centrifugalisation became perfectly clear. Koch distinguished the opalescent upper layer obtained after the first centrifugalisation as T.O., while the clear upper layer obtained by further treatment he designated T.R. Owing to the fact that glycerine gives a precipitate with T.R. but not with T.O., the latter may be regarded as containing those elements of the bacilli which are soluble in glycerine and comparable to the old or original tuberculin. The T.R., on the other hand, is apparently free from the dangerous properties of the other two preparations, but contains valuable bacillary substances insoluble in glycerine. Moreover, many observations have shown that T.R. possesses distinct immunising powers, and, in spite of certain objections to its nature, constitutes the chief basis of the anti-tuberculous vaccines of the present day.

During the past year a new aspect has been given to the efforts towards a successful prophylaxis and cure of tuberculosis in man by Behring’s communication to the Paris Congress. The fundamental conception of his new method appears to be the production of a cellular rather than a humoral or anti-toxic immunity; this he maintains can be secured by a modified constituent of the tubercle bacillus. This substance he tentatively terms T.C. when it exists in its native state, but when it is modified by the cellular activity of the body he refers to it as T.X. Behring maintains that in the process of immunisation of cattle against tuberculosis the T.C. is
separated from other substances in the bodies of the tubercle bacilli, and exercises a specific action on the tissue cells of lymphoid organs. He regards this substance T.C. as the cause on the one hand of the reaction to Koch's original tuberculin, and on the other hand of the protective reaction against tuberculosis. In order to free the T.C. from the substances which hinder its therapeutic action, it is necessary to distinguish three groups of constituents in tubercle bacilli: First, a substance soluble in water which possesses a fermentative and catalytic action; this Behring refers to as T.V., and regards it as representing the toxic factor of Koch's tuberculin. Secondly, there is a globulin, called T.G.L., soluble in a 10 per cent. solution of sodium chloride; it is toxic also. Thirdly, there are several non-toxic substances which are soluble in alcohol, ether and chloroform. When the tubercle bacillus has been freed from these three groups of substances there remains only a body which he calls the "rest" bacillus, retaining the shape and all the staining reactions of the tubercle bacillus itself. This rest bacillus can be transformed into an amorphous substance capable of being absorbed by the lymphoid cells of animals; the cells then assume oxyphile or eosinophile characters, and coincidently with this change the condition of immunity develops. A point of great importance in Behring's utterances is that he believes that the T.C. can be elaborated in vitro in such a manner as to be capable of utilisation in the treatment of human tuberculosis. So far the evidence in support of this belief has not been published, but Behring's previous work in regard to the preparation of diphtheria antitoxin, has been so brilliant and successful that we entertain hopes that his confidence in this new work will be justified.

Leaving these more or less theoretical considerations as to the possible line of cure in tuberculosis, we may review the facts and principles which have led to practical results during the year. This mainly resolves itself into a reconsideration of tuberculin therapy, as originally put forward by Koch, and the adoption or recognition of entirely new conceptions as to its practical application. For this we are mainly indebted to the work of our old colleague, A. E. Wright, whose achievements in the treatment of the localised forms of tubercle is a dominant feature in the advances made during the past year in this branch of medicine. It has long been recognised that against bacterial diseases the mechanism of defence in our bodies consists probably in the production of protective substances which, entering the blood stream, are carried in this stream throughout the body. We need not inquire here where these substances
are formed; it is sufficient to know that these chemical protective substances can be demonstrated in the blood of healthy persons, and further, that they are created by the host. Whatever be the source and mode of origin of these protective substances they suffer remarkable changes in quantity when an infection takes place, or when a "vaccine" is introduced into the body artificially; these changes have been described by Wright as the "law of the ebb, flow and reflow, and subsequent maintained high tide of immunity." As readers of this Journal know, the study of immunity against bacteria has led to the expression of the greatest diversity of opinion as to what is the nature and action of the protective agencies, some claiming anti-toxic properties, others lytic, and some phagocytic attributes. By their work in 1903, Wright and Douglas (Proc. Roy. Soc., vols. lxxii. and lxxiii.) made a notable advance towards the solution of this question by showing that substances—opsonins—exist in the blood serum, and whose function is, in some way, to alter microbes so that they may fall an easy prey to the phagocytes or leucocytes. The general accuracy of this work is now recognised, but the exact nature of the opsonic action has not yet been worked out. All investigators, however, are agreed that where different bloods are compared the variable factor is the serum and not the leucocytes. The existence of Metchnikoff's hypothetical stimulins, or bodies which stimulate the leucocytes, still lacks exact demonstration in respect of tuberculosis, though some suggestive work as to their nature quâ enteric fever has been done by Leishman (Trans. Path. Soc. Lond., vol. lvi., pt. iii., 1905). The relation of the leucocytosis, which accompanies most bacterial infections, to the opsonic content of the serum is equally obscure, though Bullock and Ledingham, in a large number of experiments, failed to show that there is any close relationship between these factors.

The practical importance of these researches consists in their relation to the question of the treatment of infections by the inoculation of vaccines, especially that of tuberculosis by tuberculin. Here again Wright's work during the year is of importance, as he has shown (Lancet, 1905, vol. ii., pp. 1598 and 1677) that the chief defensive work against tubercle exists in the serum and not in the cells as was believed, and that the destruction of the bacilli by the body cells must be preceded by the action of the serum. He further shows that the opsonic content or index of individuals suffering from tuberculosis is lower than that of normal people, and that the tuberculous opsonic content of the serum can be increased by the inoculation of tuberculin. It is, further, noteworthy that
following the inoculation of tuberculin there is an ebb of protective substances as represented by tuberculous opsonins, or as Wright calls it, the negative phase. The negative phase seems to be variable; in some cases the ebb or fall is abrupt, in others the negative phase may persist over a number of days. The recognition of this negative phase is of vital importance in the administration of tuberculin, as shown by the observations of Lawson and Stewart. (Lancet, 1905, vol. ii., p. 1679), who point out that the administration of tuberculin during the existence of a negative phase tends to the production of more harm than good, as it means the releasing of numbers of tubercle bacilli from foci into the general circulation at a time when the serum of the blood is least able to destroy them. If this period be avoided, the general results of tuberculin inoculation in a considerable number of cases has been distinctly favourable. In fact, all the evidence goes to show that, provided it be given in very small doses, say from \( \frac{1}{100} \) to \( \frac{1}{1000} \) milligramme of the powder and with due regard to the absence of a negative opsonic phase, we have in tuberculin (T. R.) a substance which possesses remarkable curative powers in tuberculosis. It is obvious that the last word has not been said upon this matter, but so far as this year's work is concerned it must be conceded that a distinct advance has been made and in a direction in which the outlook was the least hopeful.

As to the relation between human and animal tuberculosis we await still the report of the British Royal Commission, but the year's discussions indicate that we are hearing less of the contention that bovine and human tuberculosis are not reciprocally infective; while recent research, including the report made this year by the German Commission, points to an opposite conclusion. Of recent work, some interesting experiments have been made by Calmette and Guérin (Ann. de l'Institut Pasteur, Oct. 25th, 1905, p. 601) upon goats to test the validity of Behring's assertion that adult pulmonary tuberculosis is not due to infection by the respiratory tract but of intestinal origin, and almost always of an intestinal infection contracted in early life. They selected healthy goats which were in an advanced stage of gestation, and by means of a tube inserted into the lacteal ducts, inoculated into the mammary glands of these animals cultures of bovine, human and avian tuberculosis. The goats which received the bovine bacilli became ill and all died in less than sixty days, though the tubercular disease did not in most cases show much extension beyond the mammary gland. Those inoculated with the human bacilli in the same doses
suffered much less severely, and in three months had regained their normal health. Those inoculated with the avian bacilli were even less affected. Each goat was kept with its own offspring and isolated, the young being given every opportunity to suck their mother's milk. The kids suckling milk containing bovine bacilli exhibited mal-nutrition very markedly, and those killed or which died, showed extensive tuberculous lesions in the mesenteric and other glands, as well as in the lungs. In the kids fed on milk containing human bacilli, although the mesenteric glands were affected the disease had not spread further. The kids fed with milk containing avian tubercle bacilli remained practically well. In addition to the foregoing, other experiments were made on young kids by introducing cultures of bacilli into the stomach by means of an oesophageal tube. All the animals fed with bovine bacilli developed acute disease of the lungs, mesenteric and thoracic glands. The animals fed with the other strains of tubercle bacilli remained unaffected. These results are clearly in support of the view that the intestinal tract, in early life, is the portal of entry for tubercle bacilli of bovine origin leading to the production of pulmonary tuberculosis; but, contrary to Behring's view, when similar feeding experiments were made upon adult goats, almost identical results were obtained. On these facts, Calmette and Guérin reject Behring's view that the pulmonary tuberculosis of the adult is due to an intestinal infection contracted in early life, but they insist, on the contrary, that the adult is, if anything, more susceptible to pulmonary infection by the intestinal route than the infant. In spite of the apparent success of these experiments, one must confess to difficulty in accepting the conclusions absolutely, though they constitute a valuable contribution to the study of tuberculosis. To a large extent in confirmation of the foregoing experiments on goats, are the results of some on calves made by Vallée (Ann. de l'Institut Pasteur, Oct. 25th, 1905, p. 653), from which we are forced to the conclusion that tuberculosis due to an infection through the alimentary tract is probably much commoner in man than is generally supposed.

As regards indirect means of preventing tuberculosis, the year has been rich in abundance of effort. On the value of sanatoriums as a means of reducing the prevalence of pulmonary tuberculosis, and as to the extent to which it is desirable to urge provision and organised effort out of public funds for the segregation on a large scale of more advanced cases of the disease, considerable differences of opinion exist, so much so that in the present state of our know-
ledge it is desirable to suspend judgment. On the other hand, reports from all parts continue to illustrate the value of the work which is being done to ameliorate the conditions of housing and living which foster tuberculosis, and urge the need for further effort by local authorities in this direction.

The Lincoln Outbreak of Enteric Fever.—Those of us who have been sufficiently impressed with "the tolerance of enteric in the Army," will appreciate the object lesson presented by the experiences of the city of Lincoln during the past year. Few outbreaks of enteric fever have afforded a more striking instance of catastrophe resulting from contamination of a public water supply. The outbreak began on January 22nd and continued until June 21st, 1905. In the first two weeks some 400 persons were attacked, during the next eight weeks about 500 other cases occurred, while, including a considerable crop of secondary cases, the total number of persons attacked in Lincoln during the first half of the year amounted to 1,021, constituting some 2 per cent. of the total population, with a death-roll of 120. In spite of much attention devoted to this outbreak, it is not clear how or where the infection was first introduced, but all the facts point to one common cause, restricted either in virulence or in distribution at the outset, and subsequently becoming more diffused and spreading with explosive violence about the third week in January. For more than twenty years Lincoln has had exceptional difficulty in finding sufficient quantities of safe water for its inhabitants. The only sources of water available are the River Witham and its tributaries, all of which have long been known to be constantly contaminated with sewage and other manurial matter contributed from manured land, cottages, &c., adjacent to the streams. The water drawn from these doubtful sources was submitted to sand filtration before distribution, a process which, if conducted under certain well-defined conditions, might be expected to have rendered even so doubtful a water reasonably safe. Unfortunately, the conditions essential to efficient filtration seem to have been wanting, certainly for a while during December, 1904, and early January, 1905, when the onset of frost and the undue hastening of the rate of filtration completely broke down the defences for safeguarding the public health. Undoubtedly the distribution of an imperfectly filtered water drawn primarily from sources open to persistent sewage pollution, was the direct cause of this epidemic. A critical study of the various concurrent circumstances in connection with the outbreak makes it clear that its magnitude and distribution were probably influenced by other factors.
For example, it is difficult to eliminate in a certain number of cases the possibility of infection by means of sewer air, as the epidemic prevailed not only where the Witham water was drunk, but where air from the sewers could more easily penetrate the dwellings. This arose from the fact that the sewers of Lincoln are much too small for the normal volume of sewage flowing through them: moreover, it is admitted that there was an increase over the normal quantity of sewage equal to about 25 per cent. for a few weeks preceding the outbreak. This means that for some weeks before the commencement of the outbreak the sewers were waterlogged to a larger extent than usual, and that there was extra back pressure and a forcing out of the sewers of a larger amount of sewer air and associated gases. In this connection it is noteworthy that those parts of the town where there were greatest facilities for the escape of sewer air suffered more than those localities having fewer sewer air outlets; these differences of incidence were quite unconnected with either quality and style of house or social grading of the people.

Another curious feature about this enteric outbreak is the remarkable age distribution of cases. Up to March 1st, among those under 15 years of age, there were 172 cases among males and only 115 among females. Among those over 15 years of age during the same period, there were 266 male cases and only 179 female; it may be legitimately asked, if the contamination of the water was the sole cause, why these extraordinary differences? It can hardly be explained by the idea that men having to do hard manual work drink more water than women do, as the greatest discrepancy occurred during the ages from 6 to 10 years, where there were only 38 cases among the girls and as many as 85 among the boys; at these ages boys do not do severe manual work. Apart from these interesting epidemiological features, this outbreak at Lincoln affords a convincing demonstration of the danger of taking water from contaminated sources and relying upon sand filtration to remove all primary risks; it suggests also the importance of sound minor sanitary arrangements in safeguarding the community from secondary risks; while the supineness and dilatoriness displayed by the local authorities towards putting their town in order, in spite of repeated warnings from their own health officer and the Local Government Board, give rise to considerable misgivings as to the efficiency of the disciplinary or administrative control exercised by the central over the local authority in matters of this kind.

Not the least interesting fact in connection with this epidemic
is the success which appears to have resulted from the attempt to purify the town water supply in bulk by chemical means. Sodium hypochlorite was the re-agent used, containing about 12 per cent. of free or available chlorine. This was applied in proportions of 1 part of the salt to from 10,000 to 1,000,000 parts of the water, but so far as one can judge a ratio of about 1 to 100,000, or, say, 7 grains to each 10 gallons of water, was the smallest amount yielding satisfactory results. By giving sufficiently small a dose and a prolonged contact, dechlorination of the water seems to have been unnecessary, though the treated water is said to have had a mawkish or so-called "spent" taste. The result of this treatment of the Lincoln water appears to have been distinctly successful, as over 75 per cent. of the treated water samples have contained no B. coli in 100 cc. of the water. The practical lesson of this gigantic experiment in civil life is not without meaning to some of us called upon to cope with similar facts under very dissimilar conditions.

THE CANCER PROBLEM.—The results of recent investigation into cancer are leading undoubtedly to a better conception of this subject, and the lines of inquiry promising the best results have been essentially biological in character. For the major portion of this good work we are indebted to Bashford and Murray, investigating on behalf of the Cancer Research Fund, while others, notably Farmer, Moore and Walker, have contributed largely to our knowledge of the cytological characteristics of malignant new growths in man. The occurrence in cancer of those types of cell division, which had hitherto been regarded as peculiar to the reproductive tissues, has been interpreted by these latter workers to signify that malignant new growths are virtually reproductive tissues. Bashford and Murray dissent somewhat from this view, and maintain that the occurrence of these peculiar mitoses or modes of cell division are to be regarded merely as a phase in the life-history of malignant new growths, which are not built up solely of cells comparable to those in normal reproductive tissue. They point out that the potentialities residing in cancer cells can be exemplified by observations on the transmissibility of cancer from one animal to another. It has been demonstrated that in successful transmission, the tumours which arise are the genealogical descendants of the cells actually introduced. Further, the artificial transplantation of a malignant tumour is not to be regarded as throwing any light upon the origin of cancer, but merely as demonstrating that the proliferative activity of its cells can persist for a long time; in other
words, cancer is an irregular and localised manifestation of a process otherwise natural to the life-cycle of all organisms.

Inasmuch as we know so little of the true meaning of the nuclear changes in cell division, we confess to considerable sympathy with those who question the theory of an etiology for cancer based upon the view that, because the mitotic changes in cancer resemble very closely the mitotic changes in reproductive tissue, there is necessarily an identity in the physiological processes of these two tissues. The proposition to be discussed or kept in view is rather that a somatic cell may undergo functional involution, and in so doing may resume the characters of a primitive cell, and that a colony of such cells forms what is called a tumour. Clinically, the evidence is strong in favour of tumour cells having lost their normal function, and that a cancer may flourish while all the other tissues are wasting; in old people a cancer arises frequently in an organ, or part which is undergoing involution. Involution is merely a phase of life synonymous with a lessened functional activity. A simple instance where a cell having lost its normal function involves to a primitive type, is seen when a fat cell undergoes atrophy; the fat slowly vanishes and the cell becomes connective tissue, and this in turn may organise into fibrous tissue. That involution is a frequent forerunner of cancer is explicable, because the cell has lost its functions and tends to resume the exuberant growths of the primitive protoplasm; just as a somatic cell has evolved from the general to the special, so a tumour or cancer cell has involved from the special to the general. There is, however, another aspect of the problem. Is it possible that the presence of nerves in certain tissues acts like a brake or check on cell production? There is much to emphasise the apparent relations between carcinoma and nerve or trophic areas. If we think of nerve and epithelium as two diverging lines arising from a common epiblast, it is conceivable that nerve tissue, either from over specialisation or lessened function, might so lose control over or fail to keep pace with the epithelial cell, which proliferating rapidly from irritation, frees itself from nerve influence. The presence of nerves in cases of certain slow-growing fibromyomata and their absence in certain rapid-growing tumours lends support to this view.

Quite apart from their cytological researches, the workers for the Imperial Cancer Research Fund have established that cancerous processes are identical in all vertebrate animals, and that both in short-lived and long-lived animals their incidence increases with advancing age. That this is true for sarcoma as well as for carcinoma, suggests
the view taken by Bashford, that these are manifestations in different tissues of an essentially similar process. Experiments in engrafting cancerous growths in mice, show that the processes by which cancer cells are transferred to a new individual are fundamentally different from all the known processes of infection. This necessitates either the abandonment of the view that cancer is due to an infective process, or the recognition of some new method of infection quite unknown. The age-incidence of cancer is a factor of the first importance, and the failure to recognise the frequency of the occurrence of sporadic cancer in aged animals vitiates the value of transplantation experiments when made on other than young animals. It has to be recognised, too, that two years of age in one species of animal may correspond to 60 years of age in another. Bashford's observations on mice and other animals render it highly probable that, when the true facts corrected for age-distribution of the animals in each species are known, the incidence of cancer in such animals as cattle, horses, mice, &c., will approximate to that in man. This being so it is clear that our outlook hitherto on the causation of cancer has been too limited. If cancer occurs, as it evidently does, in wild as well as in tame animals, in savage as well as in civilised man, the essential factors in its causation are probably remote from civilised life. The one dominant note in all this recent statistical work on cancer is the relationship between the process and the span of life, and this leads us back again to the reiteration of the view that in its essential nature cancer tissue is but an involution of special cells to primitive types. There is nothing in the investigations of the Imperial Cancer Research Fund which points to an actual increase in the death-rate from cancer; but they emphasise the fundamental importance of a knowledge of the proportionate number of individuals of different ages in any group of men or animals, when estimating the relative recorded frequency as distinct from the absolute incidence of cancer among them.

THE FALLING BIRTH-RATE.—In previous reports a reference has been made to this subject, but I offer no apology for returning to it, for its importance and scientific interest is very great. Everyone concerned for the greatness of our nation is deploiring the serious fall in the birth-rate which, setting in some years ago, has culminated this last year as the lowest on record, or 27·2 per 1,000 persons living. All agree that, if we are to hold our own with foreign countries, we must have plenty of children to furnish workers, and, if it be necessary, fighters on land and sea. So true
Hygiene and Preventive Medicine during 1905

is this, that unusual prominence has been given, both in the press and elsewhere, to the social and economic bearing of this dominant feature of our national vital statistics, many interpreting the facts not only as evidence of moral decadence but also as a symptom of physical deterioration. There is reason to think that possibly these pessimists take too narrow a view of the question; whether this be so or not, it is incumbent upon ourselves to consider the subject scientifically.

It will be conceded that the moral aspect of the question is one of unusual delicacy and, too, not to be ignored, but, recognising that it is not my province here to sit in judgment on the motives of individuals, I propose merely to recognise and inquire into the consequences of facts as they exist. Now, the first thing we have to grasp is the fact that the birth-rate is falling, not merely in this country, but in all civilised countries, with the exception of Russia and Japan. We may dismiss at once, as chimerical, the idea that this is due to any failure in vitality of civilised races or to the existence of any progressive inherited degeneration of peoples. The truth is that, in civilisation, the birth-rate is falling simply because the public desires that it shall fall. The significance of this may be readily over-estimated, but let us understand that a falling birth-rate is perfectly compatible with a rapidly increasing population and with an acceleration in the increase of the population, if the death-rate falls with sufficient rapidity. In our own case, the death-rate last year was 15·2 per 1,000 persons living, and, moreover, it has shown a steady diminution for many years, with the result that in the first half of the nineteenth century the population of England doubled; in the second half it increased 81 per cent.; during the decade (1891-1900) there were added to the population of England 3½ million people. This same figure applies roughly to the United Kingdom (3,721,600) as the decrease of Ireland was practically neutralised by the increase of Scotland. But during this time, we sent 898,000 emigrants to other lands, making the actual increase of the population during the decade over 11 per cent., and the natural increase, or excess of births over deaths, over 12 per cent. If the present birth-rate and death-rate continue and emigration increases proportionally to population, we may anticipate, about 1980, that the people of these islands will be quite 80 million souls, or about double our present-day population. One need not pause to discuss of what magnitude will be the housing problem in that day or what will be the density of population in the larger towns; the evils of our own day are sufficiently
embarrassing. A diminishing population, or even a stationary one, as in France, may be and probably is a grave national danger; but in our own case, so long as our birth-rate exceeds our death-rate in its present proportions, we have nothing to fear except lack of employment and want of support for our increasing population. If the actual rate of increase in our population be suggestive of gloomy forebodings to the statistician, would it not be better to dwell less upon the falling birth-rate, but rather dwell more upon the need of checking wasteful expenditure of life and, by so adding to our national income in this respect, secure a more favourable balance? The destruction of infant life in this country has long been a crying disgrace to our civilisation, and it would appear more appalling were it not that other countries can tell a worse tale. The figures are sufficiently striking. For the ten years 1895-1904 the percentage death-rate of children under one year of age was 15.6 of the birth-rate for the whole country. During 1904, the death-rate per 1,000 births in London was 146, while in Blackburn it was 191, in Liverpool 196, and in Birmingham 197, meanwhile, in Southampton it was only 115, and in Willesden 114, and even these relatively good ratios are far above the figure for Melbourne during the same year, namely 83. Surely it is not too much to expect that the infantile death-rate in our rural and urban communities should be kept down to something below 10 per cent. Were such the case it is easy to realise the increase in population which would result from the saving in infant life, a substantial part of which survivors might hope to reach adult age. There can be little doubt that if we could abolish the infantile mortality which now prevails we might allow the birth-rate to fall lower than it is at present and yet have nothing but gain in every direction.

There is, however, another aspect of the question which needs to be borne in mind, and that is the relative fertility of various classes of the community. Karl Pearson has been very explicit on this point, and goes so far as to say that the fall in the birth-rate is due to the relative infertility of the most valuable stocks in the race; he, moreover, attributes to this fact the present dearth of ability, or, in other words, the survival of the less able. Unfortunately, we are not in possession of precise data, but we know in general that the upper classes are less fertile than the middle classes, and these less fertile than the lower classes. A birth-rate of 11 in Mayfair is met with one of 60 in Poplar or Stepney, the figure for the general population being about 27. These are facts which must
Hygiene and Preventive Medicine during 1905

make us ponder and suggest the need of our attempting to improve the race by attention not to the quantity but to the quality of the births. The policy of the future must be to lessen the enormous wastage of life which now goes on amongst infants, for the birth-rate will certainly continue to fall as the natural result of human will and the outcome of certain knowledge. That knowledge is being diffused steadily downwards into the lower social grades, but when that knowledge, which is now the property of the relatively few, is common property, then the present disturbing factor in the proportion of the birth-rate contributed by different classes of the community will disappear and the average quality of the births rise.