II. *Observations at Sea.*

Under this head three distinct investigations have been made, as follows;

(*a*.) From an examination of the results obtained by chronometric longitude expeditions, we find that for a voyage of 15 days the average error is 5.3^s; the range between the greatest and the least results in each series is 18.0^s; the latter value has a range between 1.5^s and 55.0^s, and the coefficient is 3.4.

(*b*.) The longitudes of 36 stations have been determined by various British naval expeditions. The chronometers were rated at the Greenwich Observatory before starting, and the observations for time at the terminal stations were made in the usual way with the sextant. Evidently more than usual care was taken both with the observations and reductions, We find that the average difference between the results obtained by different chronometers is 4.4 miles with a range of 15.1 miles. The average range between the different results for longitude is 5.0 miles with a range of 31.6 miles. The average number of chronometers was 11, and the average duration of voyage was 11 days.

(*c*.) During the spring and summer of 1880 Officer W. H. Bacon, of the Cunard steamer "Scythia," kindly undertook for me a series of systematic observations from which the relative errors could be determined with considerable certainty. A complete series for a single day consisted of five sights at intervals of fifteen minutes, about 8 o'clock in the morning, five sights in the neighborhood of 11 o'clock, and five sights at the corresponding hours in the afternoon. Observations were also made when the ship was in known positions as often as possible.

This series of observations has an exceptional value on account of the conscientious fidelity with which the programme was adhered to and of the skill with which they were made. The relative errors were determined by comparing each position with the mean of the series, the rate being determined both from the morning and afternoon observations and from the log.

The results obtained are found in the following table:

Limits in Miles.	Average Error from Observations at 9^h and 3^h.	Average Error from Log at 9^h and 3^h.	Average Error from Observations at 11^h and 1^h.	Average Error from Log at 11^h and 1^h.	Difference between Observation and Log at 9^h and 3^h.	Difference between Observation and Log at 11^h and 1^h.
	No. Cases.	No. Cases.	No. Cases.	No. Cases.	No. Cases.	No. Cases.
0.0- 0.5	1	0	0	0	7	6
0.5- 1.0	9	6	2	3	1	2
1.0- 1.5	8	13	3	5	3	3
1.5- 2.0	4	5	3	3	3	2
2.0- 2.5	6	4	6	5	2	3
2.5- 3.0	2	1	3	4	1	0
3.0- 3.5	2	2	6	5	7	2
3.5- 4.0	4	1	4	5	1	2
4.0- 5.c	1	3	6	5	4	4
5.0- 6.0	0	0	2	1	1	5
6.0- 7.0	0	0	2	1	2	2
7.0- 8.0	1	1	0	1	1	1
8.0- 9.0	2	0	1	1	0	2
9.0-10.0	0	1	0	0	1	2
10.0-11.0	0	0	0	0	1	1
11.0-12.0	0	0	0	0	2	1
12.0 +	1	1	0	0	0	0

QUERY.

A SUBSCRIRER would like to know the best method of mounting Triple phosphate crystals (dry) so as to tack them to the slide without interfering with definition. —Replies invited.

ON THE ACTION OF BACTERIA ON VARIOUS GASES.*

By F. Hatton.

The experiments were made to ascertain the nature of the action exerted by various gases on the life and increase of bacteria, and to observe what influence the bacteria had on the percentage composition of the gases. The bacteria were obtained by shaking fresh meat with distilled water. The aqueous extract was filtered and exposed to the air for twenty-four to thirty-six hours; it was always found to be full of bacteria. A small flask was half filled with mercury, filled up with the bacteria solution, and inverted in a mercury trough. The gas under examination was then passed up, a small glass vessel was introduced under the mouth of the flask, and the whole removed from the trough. The liquid was examined daily as to the condition of the bacteria, the sample being removed by a piece of bent glass tubing having an india rubber joint. After about a week the gas was pumped out by means of a Sprengle and analyzed. Atmospheric air was first tried. The bacteria lived well during the fifteen days of the experiment (T. 15° to 22°). A large absorption of oxygen took place, but it was not replaced by carbonic anhydride; in a second experiment (T. 25° to 26·50) 20 per cent. of the oxygen disappeared, and only 17 per cent. of CO_2 was formed. Pure hydrogen after fourteen days had no action on the bacteria; the gas contained 0.34 per cent. CO_2, 98.94 per cent. H. Pure oxygen after ten days was converted into CO_2 29.98 per cent., O 70.02 per cent. A mixture of CO 46.94 per cent., CO_2 1.27, O 1.27, N 50.51, was next tried after fourteen days; the gas contained CO_2 17.77, CO 0.55, H 7.58, CH_4 2.50, N 71.57. In all of the above cases the bacteria flourished well. Cyanogen was next tried. The solution of meat turned gradually to a thick black fluid. On the fifth day very few bacteria could be seen. From this time, however, they increased, and on the twelfth day were comparatively numerous. On the fifteenth day the gas was analyzed; it contained CN 5.35, CO_2 57.59, O 2.24, N 34.79; a second experiment gave similar results. It appears, therefore, that cyanogen is fatal to bacteria as long as it exists as such, but that it soon decomposes into ammonic oxalate, &c., and that the bacteria then revive, especially in sunlight. Sulphurous anhydride was next tried; the bacteria lived during the fifteen days: the gas contained CO_2 7.87, O 0.00, N 2.13, SO_2 90.10. Similar results were obtained with nitrogen, nitrous oxide, nitric oxide, carbonic anhydride, a mixture of H and O obtained by the electrolysis of water and coal gas. In all cases the bacteria lived well during the experment. The author next experimented with a solution of urea (0.98 per cent.) and phosphate of potash (0.4 per cent.), sowing it with bacteria. The bacteria lived well during the fourteen days of the experiment; small quantities of gas were evolved containing 0.53 per cent. CO_2, 2.64 per cent. O, and 96.82 per cent. N. An experiment was made with spongy iron, air, and bacteria. On the fourth day, all the bacteria had vanished; the air was analysed on the fifth day, and consisted of CO_2 0.26, O 0.00, and N 99.74 per cent. Experiments were also made with acetylene, salicylic acid, strychnine (10 per cent.), morphine, narcotine, and brucine; none of these substances had any effect on the bacteria. On the other hand, phenol, spongy iron, alcohol, and potassium permanganate were very destructive to these microscopic growths.

Mr. W. M. Hamlet said that these experiments confirmed some observations of his own. He had found that bacteria could exist in almost anything—in carbonic oxide, hydrogen, 1 per cent. creosote, phenol, methylamin, methylic alcohol, chloroform. Moreover, Crace-Calvert had shown that they could live in strong carbolic acid. In

* Read before Chemical Society, March 3, 81. This paper obtained for the author the Frankland Prize of £50 at the Institute of Chemistry.

reply to Mr. WARINGTON the speaker said that the acetic acid fermentation went on in the presence of chloroform.

Mr. KINGZETT called attention to the fact that the oxygen was completely used up when the meat infusion was placed in contact with air. He did not think the experiments represented the action of bacteria on gases or of gases on bacteria, but rather the effects of various gases on the mode and extent of ordinary putrefaction.

Dr. FRANKLAND expressed his satisfaction with the results obtained by the author in his laborious research. He must confess that these results had surprised him not a little. The fact that bacteria, which were real organisms and could not be shielded under the term putrefaction, lived and flourished in SO_2, CO, CN, &c., seemed to him very extraordinary, and the question arose whether the germs to which infectious diseases were probably due were not similarly endowed with a power of great resistance to ordinary influences.

Mr. F. J. M. PAGE said that Dr. Baxter had proved that with some fever-producing liquids, their virulence was destroyed by chlorine and sulphuric acid, and that he had seen some experiments at the Brown Institution which led to the same conclusion; so it seemed that, at all events in some cases, the virulence of infective liquids was due to organic matter, essentially different from the bacteria observed by Mr. Hatton.

NOTES ON CHICKEN CHOLERA.

We observe in a recent number of the *Chemical News* that C. T. Kingzett, F. C. S., points out, that, in explaining the protective influence of repeated inoculations with the attenuated virus of chicken cholera, against the more virulent forms of this disease, Pasteur finds it "impossible to resist the idea that the microscopic germ which causes the disease, finds in the body of the animal conditions suitable to its development, and that to satisfy the necessities of its life the germ alters certain substances, or destroys them, which comes to the same thing, whether it assimilates them or whether it consumes them with oxygen borrowed from the blood."

So, again, in cases where complete immunity has been attained, the birds "no longer contain food for the germ."

More striking still is the following passage in reference to chickens which are born proof against cholera:—"Animals in this condition may be said to be born vaccinated for this disease, because the fœtal evolution has not placed in their bodies the proper food of the parasite, or because substances which would serve as such food have disappeared while they were yet young.

Now whether or not we may be prepared to regard the said parasite as the direct cause of the disease, it is remarkable that the reasoning of Pasteur should have culminated in the conclusion upon which Liebig insisted with considerable power.

If we turn to Gregory's (3rd) edition of Liebig's "Animal Chemistry" (p. 205) we find the following passage:—"The condition which determines, in a second individual, his liability to the contagion, is the presence in his body of a substance which by itself, or by means of the vital force acting in the organism, offers no resistance to the cause of change in form and composition operating on it. If this substance be a necessary constituent of the body, then the disease must be communicable to all persons; if it be an accidental constituent, then only those persons will be attacked by the disease in whom it is present in the proper quantity and of the proper composition. The course of the disease is the destruction and removal of this substance: it is the establishment of an equilibrium between the cause acting in the organism which determines the normal performance of its functions and a foreign power by whose influence these functions are altered."

I repeat that to me it seems somewhat remarkable that the investigations and reasoning of two such eminent (and in many matters diametrically opposed) thinkers should have culminated in the same conclusion as regards the conditions of the living body which subject it to, or protect it from, infection.

While, however, it can be readily understood how a profuse growth of parasites could quickly alter or destroy a comparatively large amount of substance—as, for instance, happens in ordinary putrefaction—it does not appear to me so easy to accept Pasteur's reasoning as to his so-called vaccination.

In this inflicted process an attenuated virus is introduced into the body of a chicken which becomes ill but does not die. It does not die because, if Pasteur be correct, the parasites do not sufficiently multiply. Why do they not multiply? It cannot be on account of the insufficiency of the pabulum, for in the large majority of cases where death results this seems to arise from the very profusion of the growth of the parasite when more freely introduced.

Can it be expected, therefore, that even, say, in three successive inoculations the substance which I have here spoken of as pabulum can be entirely removed or destroyed by the very limited number of parasites which are introduced by the inoculations, and which so soon perish in the body? I think this cannot be expected; but if it may be, then the particular substance or substances upon which the parasites prey must be extremely limited in quantity. After all, we are faced with the enormous difficulty of ascertaining the nature of such substance, and the further equally great difficulty of understanding why an undiscovered and undetermined substance should be entirely absent from the bodies in some animals and present in varying proportions in others.

Here we come in contact with the weakest point in the parasitic theory. The immunity from a second attack of an infectious disease of the class in question is simply inexplicable under the parasitic theory. We are forced back to an alternative theory, and that is one of which we at present only recognize the beginnings.

A NEW CORTICAL CENTRE.*

By GRAEME M. HAMMOND, M.D., NEW YORK.

Physician to the Department for Diseases of the Nervous System in the Metropolitan Throat Hospital.

Some six years ago there appeared in the *Centralblatt*, Nos. 37 and 38, a short communication by Betz, embodying an account of certain nerve-cells found by him in the cortex of a region of the brain which he newly named the paracentral lobule. This paper has probably aroused more general attention among neurologists than any other paper of recent times dealing with the structure of the cerebral hemispheres, and this, on account of the anatomical confirmation which the discovery seemed to furnish, of the localization doctrine based on the electrical stimulation of the cortex carried out by Hitzig and Fritsche.

After localizing these cells chiefly in the paracentral lobule and the upper ends of the pre- and post-central gyri of man, stating them to be very few in number in the lower halves of these gyri, Betz proceeds to say, "the constancy of the occurrence of these cells, not only as regards the cortical layer, but also the special convolutions in which they are found, led me to direct my attention to that portion of the brain of animals, and particularly of the dog, on which latter Hitzig and Fritsche obtained such brilliant physiological results. I refer to that lobule which bounds the sulcus cruciatus. Now I found in this very lobule in the dog, cells in similar nests and of a similar shape. With the dog as in man they are distributed in the fourth layer."

Engaged in a study of the ganglionic masses of the forebrain of the cat, an animal on which the experiments of Hitzig and Fritsche have been repeated, and in which

* Read before the New York Neurological Society, February 1, 1881.