

last two years, and bacteriological examination of these cases has yielded a very interesting and rather surprising result. In each case I isolated from either the mouth or the blood, or from both, one organism only—namely, the *bacillus pyocyaneus*. One always remembers that the arm of coincidence is a very long one, but it could hardly be pure chance, one would think, that this organism should have occurred in these three cases without any other. Moreover, the presence of one of the organisms of suppuration, if it be the causal agent, throws a good deal of light upon the pathology of the condition. In the first place, it accounts for the infectious nature of the disease; in the second place, it accounts for the type of morbid growth which is produced; and in the third place, it might throw some light upon the fact of the owner in one of the cases above described becoming infected with a pustular eruption. Supposing that this organism, or any other organism of suppuration, is the cause of canker in serpents (which I am inclined to believe is the case), one need not be astonished that the disease takes the form it does rather than that of an acute abscess. We do not know very much about the physiological chemistry of cold-blooded animals, and still less about the pathological, but it seems reasonable to think that chemical processes in a reptile must be very much slower and less active than in a mammal. This is further indicated by the slowness of respiration and the pulse. I think it is, therefore, quite likely that an organism of this kind, acting upon the tissues of a snake, would produce not a suppurative effect, but merely an inflammatory and proliferative growth. The question could be more or less settled by direct experiments of inoculation, but this I have had no opportunity of doing. My opinion at present, therefore, is that canker in serpents is a sub-acute infective condition probably caused by one or more of the organisms of suppuration.

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#### OBSERVATION ON THE PRECIPITIN TEST.

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IF we inject the blood serum of any particular species of animal into an animal of a different species, the serum of the latter acquires the property of producing a precipitate when mixed experimentally in a test tube with serum of any individual of the former species. At the present day, as is of course well known, amongst other spheres of usefulness, this test is utilised in the medico-legal identification of blood stains, the detection of adulteration of sausages and minced meat with horse flesh, etc.

When referring to the serum of the "treated" animal (*i.e.*, the animal into which the several doses of serum have been injected), it is customary to use the term "precipitating serum" or "precipitant."

and this would lead one to infer that the homologous normal serum is precipitated by the addition of this "prepared" or "treated" serum.

Rodet, however, amongst other writers, has shown that this is not exactly the case. This observer conducted a number of experiments in which use was made of the serum of rabbits injected previously, some with doses of horse and others with sheep serum; consequently, a precipitate formed when such serum was added in a test tube, respectively, to horse or sheep serum.

Before proceeding further it may be as well to state that, as different terms have been used by various writers to designate the treated and ordinary normal serum, throughout the present short note we intend referring to the prepared or treated serum of the rabbit as the "*anti-serum*" and the homologous normal serum (untreated) as the "*primary serum*."

Rodet found that, when using a constant quantity of primary serum and adding varying quantities of anti-serum, the quantity of precipitate produced was in direct accordance with the quantity of anti-serum.

On the other hand, when constant quantities of anti-serum were used and varying quantities of primary serum, the amount of precipitum showed but slight variation.

A very strong dose of primary serum interfered with the reaction, consequently in this second series of experiments it was necessary to use doses decreasing from a certain optimum.

To state the case in a different way, we may say that a precipitum is soluble in excess of serum. This was shown by Nutall in experiments conducted some years ago.

To resume, then, it appears that variable quantities of anti-serum for a constant quantity of primary serum have much more influence on the values of the precipitates produced than have variable quantities of primary serum for a constant quantity of anti-serum.

Rodet next endeavoured to determine which of the two serums (anti or primary), *after a finished reaction*, had lost its power to furnish a new precipitate. He found with the serums utilised, even when the quantity of anti-serum was ten, twenty, or eighty times that of the primary, the liquid (after the finished reaction) still possessed the property of being again precipitated by the further addition of anti-serum, whilst the further addition of primary serum had no such effect.

Says Rodet: "This experiment shows that the active substance of anti-serum is entirely drawn into the precipitate already formed, and that it is the primary serum even in minimal quantity which provokes the formation of a new precipitate."

The present writer recently had the opportunity to conduct a series of experiments similar to those mentioned above. In these tests the *anti-serum* utilised was furnished by rabbits which had undergone periodic injections with normal ox serum. The primary serum of course was simply normal ox serum.

In the following tables the numerals refer to those on the test tubes shown in the accompanying photographs illustrating the various reactions.

P.S. means primary serum (normal ox serum diluted 1-20); A.S. means anti-serum (the serum of rabbits which had received injections of normal ox serum).

1. A.S. .5 cc. + P.S. .25 cc.
2. A.S. .5 cc. + P.S. .5 cc.
3. A.S. .5 cc. + P.S. .75 cc.
4. A.S. .5 cc. + P.S. 1 cc.
5. P.S. .5 cc. + A.S. .25 cc.
6. P.S. .5 cc. + A.S. .5 cc.
7. P.S. .5 cc. + A.S. .75 cc.
8. P.S. .5 cc. + A.S. 1 cc.

In the tubes numbered 1 to 4 in fig. 1 it will be observed the quantity of precipitum shows little (if any) variation; whereas in

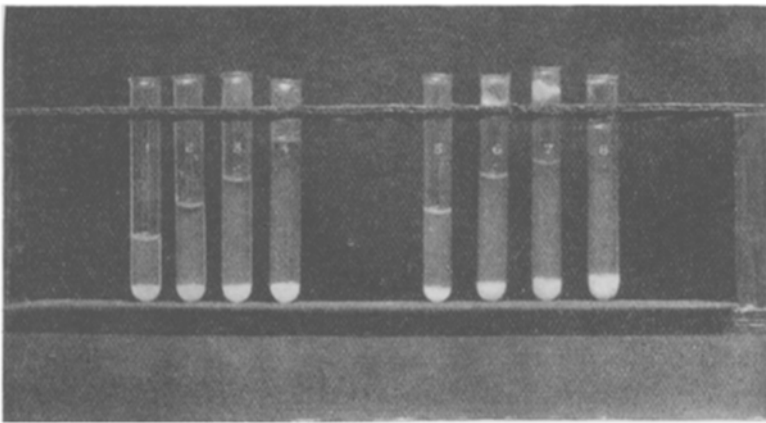


FIG. 1.

those numbered 5 to 8 the quantity shows a gradual but decided increase.

In the next experiments, after the finished reaction (*i.e.*, after a well-marked precipitum had resulted from the mixture of anti- and primary serum) the clear liquid above the precipitum was carefully pipetted into fresh sterile tubes, and to this—referred to in the tables as S.F. (supernatant fluid)—varying quantities of primary and anti-serum were added.

9. S.F. .5 cc. + A.S. .25 cc.
10. S.F. .5 cc. + A.S. .5 cc.
11. S.F. .5 cc. + A.S. .75 cc.
12. S.F. .5 cc. + A.S. 1 cc.
13. S.F. .5 cc. + P.S. .25 cc.
14. S.F. .5 cc. + P.S. .5 cc.
15. S.F. .5 cc. + P.S. .75 cc.
16. S.F. .5 cc. + P.S. 1 cc.

Reference to fig. 2 will show that in series 9 to 12 there is a distinct and gradually increasing quantity of precipitate; whilst in series 13 to 16 there is none whatever, the tubes remaining quite clear.

These tests, then, appear to confirm the conclusion arrived at by Rodet. It seems well established that the anti-serum (prepared serum) furnishes especially the *material* of the precipitate, whilst the primary serum (normal serum) *provokes* the reaction; consequently,

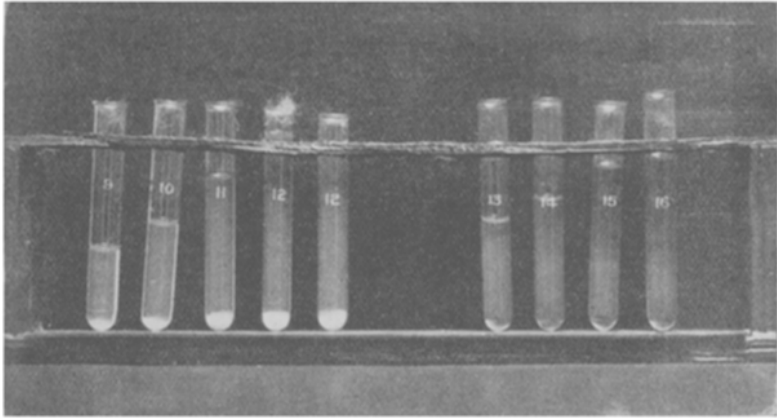


FIG. 2.

the normal serum appears to be the real precipitating serum (precipitant), since it is this serum which precipitates the specific substance (precipitin) of the prepared serum.

### THE ULTRAVISIBLE VIRUSES.

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DURING the last thirty years the application of the methods of research initiated by Koch and Pasteur has revealed the fact that the cause, formerly vaguely designated the "virus," of many of the contagious or infectious diseases is the presence within the body of animal or vegetable parasites (bacteria or protozoa), which, although minute, can be made manifest to the eye by use of the microscope. In all these cases the term "virus" has lost its former usefulness, but in a considerable number of cases that term is still the only one that is justified in speaking of the cause of the disease. These are the contagious, infectious, or inoculable diseases in which the ordinary bacteriological methods, so fruitful in other cases, have entirely failed to resolve the virus into a visible micro-parasite.

It is possible that in some of these cases further investigation may show that the causal micro-parasite has hitherto merely been overlooked, but in other cases there appear to be sufficient reasons for abandoning such an expectation, and for holding that the causal parasites have not been seen simply because they are too minute to