disinfection must be correctly judged and examined precisely on this point, and that we have to distinguish between what only arrests putrefaction, and what really kills bacteria. The former may very possibly serve as a means of preserving infectious matters.

I will only mention the limit of the power other substances possess of arresting the development: alum, 1:100; camphor, 1:300. I had expected a stronger effect from camphor, but several careful experiments have shown that this substance possesses only a very slight influence on comma-bacilli. Carbolic acid, 1:400. This figure nearly agrees with what we know of the influence of carbolic acid on other bacteria. Peppermint-oil, 1:2,000. Sulphate of copper, 1:2,500. This substance has a very powerful effect. But if we want to calculate how much sulphate of copper must be given in order to check the growth of the bacilli in the intestinal canal, we should arrive at quantities which could not be given to a human being. Quinine 1:5,000; and corrosive sublimate, which is here again seen to exceed all other substances in power, 1:100,000.

In these experiments on the influence of substances for arresting the development of comma-bacilli, the striking fact was evident that comma-bacilli die off extremely easily when dried. These experiments were made by letting a very small drop of a substance containing bacilli dry on an object-glass, and a large supply of these object-glasses were immediately prepared for a series of experiments. A drop of the liquid which was to be examined was then placed upon such an object-glass, and left for development in the hollow object-holder. Having proceeded in this manner, in no single preparation did anything grow that had received meat-broth as nutritive fluid, nor in a striking manner in the test-preparations either. At first I did not know what caused the absence of growth, and thought that the broth must be the cause of it, for I had never met with anything like this before in the case of other bacteria. For instance, anthrax-bacilli can be kept in store for a long time dry on object-glasses; they retain vitality from half a week to nearly a whole week in this manner. As, however, the meat-broth on examination proved to be unexceptionable, we had to examine whether the comma-bacilli had not probably died off owing to being dried upon the object-glass. In order to obtain certainty on this point, the following experiment was made. A number of object-glasses were provided with a small drop of substance containing bacilli. The drop dried up in a few minutes. One object-glass was now diluted with a drop of meat-broth after an interval of a quarter of an hour, another after an interval of half an hour, another after an interval of an hour, and so on. Then it was seen (and I made several series of experiments) that the comma-bacilli did come to development on the dried glass-plates that had laid a quarter, a half, and a whole hour, but after two hours they sometimes died off; after three hours, I could not keep the bacilli alive in these experiments. Only when compact masses of bacilli-cultivations—for instance, when the pappy substance of a cultivation made on potatoes was dried—did the bacilli retain vitality for a longer time; clearly because in this case complete desiccation followed much later. But, also, under these conditions I have never succeeded in preserving the bacilli alive in a dried state longer than twenty-four hours.

This result was in so far important, as by its means it could easily be tested whether the bacteria have a permanent state. We know that other pathogenic bacteria—for example, anthrax-bacteria, which form spores, can be preserved for years in a dry state on an object-glass without their dying. We know also of other infectious substances, with whose nature we are not yet accurately acquainted for example, the infectious matters of small-pox and of vaccine, which can be kept in a dried state for several years, still retaining their power of infection. If now the comma-bacilli, which, as such, are unexceptionally speedily killed by drying, pass into a lasting condition under some circumstances, that would be very soon shown during the process of drying.

This is anyhow one of the most important questions for the etiology of an infectious disease, and especially so for cholera. The investigation of this point has therefore been made in the most careful manner possible, and in every possible manner, and I hardly think that anything more can be done on this point. Above all, cholera-dejecta and the contents of the intestines of cholera-corpses were left in a damp condition on linen, in order that the comma-bacilli might develop under the most favourable circumstances. After certain intervals of time, pieces of the linen were dried, for example, after twenty-four hours, after a few days, after several weeks, to see if, during this period, any condition of permanence had been established. For infection through cholera-linen affords the only undisputed example of the presence of an effectual infectious substance, which adheres to a special object. If there were a permanent state to be found anywhere, it must have been found on cholera-linen.

But in none of these cases was a permanent state discovered. When the dried things were examined, it was seen that the comma-bacilli had died off. Then, further, the dejecta were placed in earth, being either mixed with earth or spread on the surface, which was either kept dry or moist; they were mixed with marsh-water, and were also left to decay without anything being added to them. In gelatine-cultivations, the comma-bacilli have been cultivated up to six weeks, also in serum of blood, in milk, on potatoes, on which anthrax-bacilli are known to form spores extremely rapidly and in great abundance. But we have never obtained a permanent state of comma-bacilli. As we know that the majority of bacilli have a permanent state, this result must appear very striking. But I will remind you here, what I mentioned before, that we have most probably to deal with a micro-organism here, which is not a genuine bacillus at all, but is more allied to the group of screw-shaped bacteria, spirilla; but we do not know of any permanent state of spirilla as yet. Spirilla are bacteria which depend for their existence exclusively on liquids, and do not, like anthrax-bacilli, vegetate under certain conditions in which they have for once to endure a dry state. It, therefore, seems to me, as far, at least, as my experience goes, that there is no prospect of finding a permanent state of comma-bacilli. I shall later on explain that the absence of a permanent state perfectly coincides with the experiences of the etiology of cholera.

[To be continued.]

REMARKS ON THE PROPHYLACTIC POWER OF ARSENIC IN MALARIA.

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As to the prophylactic power of arsenic in enabling persons exposed to the infection of scarlatina, diphtheria, etc., to resist such poisons, referred to by Dr. Walford in the JOURNAL of July 5th, I am unable to speak from experience; but that it has such power against malaria seems to be established on more than merely *prima facie* evidence. Such an experiment is open to so many sources of fallacy, that only repeated observations made under the strictest safeguards can settle the question.

In a suggestive report made by Dr. Tommasi-Crudeli to the Italian Minister of Agriculture in March 1883, some very important facts were adduced. The trial of arsenic in this way was proposed by him in a communication to the Accademia dei Lincei, December 1880, and was carried out by the medical staff in charge of the Roman and South Italian railways, amongst the men employed, who are largely exposed to the infection of malaria. In 1881, Dr. Ricchi, chief of the staff, instituted such experiments, but the year was one of feeble malarial activity. In 1882, the malaria was much more active, and Dr. Ricchi caused arsenic to be administered to 455 persons, so exposed, commencing with one milligramme, gradually increased to eight milligrammes for each person daily. Of these 455, 338 were cured of fevers which they had, or prevented from contracting such. In 43 cases, the results were negative; in 74, doubtful. Amongst the negative cases were included those who did not take the remedy regularly, or only for a few days. The doubtful class alternated the arsenic with other supposed prophylactics. Dr. Ricchi, therefore, continues his observations, and is convinced that, "if arsenic is not always preservative against the malarious infection, it renders the human organism less and less susceptible to the ferment of malaria." Like conclusions followed from the use of arsenic amongst persons employed by Messrs. Piacentini, in the Roman Campagna; by Prince Corsini, in the Tuscan Maremma, amongst the men employed in the Royal Chases at Castelparciano. The results are borne out by experiments on animals, made by Marchiafava and Cuboni, who inoculated these with the blood of malarious fever.

Further researches were instituted, under the most careful safeguards against error, by preparing animals for inoculation with malarious blood; one-half being treated with arsenic, and the other half without. Owing, however, to the remarkable paucity of cases of pernicious fever, Professor Marchiafava, Drs. Celli, Ferrareri, and others, have not since been able to obtain the necessary supply of the blood of pernicious fever, and the experiments have not hitherto been able to be carried out in their integrity.