Sternberg, in a recent article copied in the *Canadian Practitioner* from the *Brooklyn Medical Journal*, has admirably condensed these into a practical form, that we should all bear in mind: (1) It not only does not increase, but is rapidly destroyed by desiccation, a very short time depriving it of vitality; (2) Although it thrives in warm air yet it will not survive exposure to high temperature, its death point being below 140°; (3) That it is easily destroyed by comparatively weak solutions of various disinfectants, such as hydrochloric acid, 1-1300; sulphuric acid, 1-1000; methyl violet, 1-1000; and carbolic acid, 1-400, a period two hours' exposure being sufficient.

The important fact to be deduced from Koch's and Bolton's experiments would seem to be that, provided any of these means of disinfection are thoroughly applied, the destruction of the contagium is not difficult nor uncertain, but whatever method be employed it is essential that all parts of the material to be disinfected be effectually exposed. If heat (dry), steam, or boiling water be used, clothing, for example, must be so arranged that all parts of it are penetrated, hence it should not be thrown into a mass, but separated as much as possible; if a disinfectant solution used for digestious discharges, etc., the quantity of the solution should be sufficient to completely immerse it.

The discovery of the comma bacillus and its acceptance by the profession as the active agent in the production of the characteristic symptoms of the disease have led naturally, in accordance with the views generally held with regard to specific diseases at the present time, to attempts to protect from the disease by means of inoculation either of the pure virus or of bacteria produced by artificial culture. These attempts, first made, I believe, by Forran, in Spain, in 1885, when they attained considerable notoriety on account of the contradictory reports with regard to the results obtained, and carried on extensively both in France and Germany during the epidemic of last year, have not so far proved very satisfactory. Injections, administered hypodermically, whether of the pure bacilli or cultures, have, as a rule, failed to induce the disease, and it seems probable that to produce its full effect, the bacillus must find its entrance into the stomach and be retained in the intestinal canal. Koch says that he failed to produce cholera in guinea pigs by simply introducing the poison into the stomach, unless at the same time he injected tinct. opii into the peritoneal cavity. If further experiments bear this out, it would have an important bearing upon the advisability of endeavouring to restrain the excessive secretion by means of astringents or opiates.

The diagnosis of cholera is made by the culture of the bacilli in various media, such as pure water gelatine and water, sugar and starch solutions, chicken albumin, sterilized milk, etc., in all of which they grow readily, and by the development of the so-called cholera red in the presence of free acids, of which according to Jadassohn, hydrochloric acid is the best. To obtain this reaction, the presence of oxygen is necessary, and it is also requisite that the culture be pure and unmixed with other bacilli which either prevent or delay the development of the colour. When these conditions are complied with and a sufficient quantity of the oxydizing agent added, a well-marked violet red colour is produced after a short time, due to the existence of a substance which is formed in peptone or albumin cultures, and supposed to be an indol derivative. There seems, however, to be a good deal of uncertainty still about this as a means of diagnosis, since the use of impure acids, or old and impure cultures, either masks the reaction by producing other colours, or delays or prevents it altogether.

I have adopted Sir George Johnson's view as to the causation of the change of the blood in cholera, because it seems to me at once reasonable and consistent with the results of treatment, so far as we can be said to have obtained any results. It is a matter of very great importance, for if the symptoms and *post mortem* appearances are due to chemical changes caused by the presence of the bacillus, it is clearly irrational and mischievous to endeavour to suppress the discharges and retain the poison within the system. If, on the other hand, the thickened blood, the sharpened, pinched, features, the cramps and collapse, are due simply and solely to the withdrawal of a large quantity of fluid from the body, then our chief efforts should be directed to the arrest of this loss. As the question of treatment is to be taken up by others, I will not dwell further upon it.

Kingston, June 20th, 1893.