Finally, I will call attention to a point which should, perhaps, have guided us to the recognition of the serious import of the case, which was early presented, namely, the restless, anxious, and agitated mental condition of the patient, which seemed at first out of proportion to the severity of his symptoms.

THE VALUE OF MERCURIC CHLORIDE AS A PRACTICAL DISINFECTANT.

A REPLY TO DR. V. C. VAUGHAN.

BY WILLIAM B. HILLS, M.D.,

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THE Committee on Disinfectants of the American Public Health Association, in their report published in 1885, recommended, among other substances, mercuric chloride as a suitable agent for certain processes of disinfection. As a result of their conclusions this substance has since occupied a prominent place in the list of disinfectants employed in this country, replacing to a large extent all others for certain purposes. At the annual meeting of the Massachusetts Medical Society, held in June last, I presented, under the above title, a paper which subsequently appeared in the pages of the JOURNAL,¹ in which I called attention to certain chemical properties of mercuric chloride which had been disregarded by the committee in their investigations: properties which were likely to render this substance unreliable for some of the purposes for which it had been recommended by this committee: and I further characterized the report of the committee as contradictory, obscure, and, in general, unscientific, so far as it related to mercuric chloride. In my paper I suggested that the failure to take into consideration the chemical changes this substance undergoes when it is added to the material to be disinfected was due to ignorance regarding any such changes or to forgetfulness arising from hasty investigation. But a recent article² by Dr. V. C. Vaughan, of Ann Arbor, Mich., a member of the committee, in which he criticises my paper, and defends the conclusions reached by the committee, suggests very strongly that, while ignorance or forgetfulness were probably in part responsible for their omissions, there may have been in addition a willingness to suppress or misrepresent such facts as might have an unfavorable bearing on the conclusions published by them. I am well aware that this is a severe criticism. To show that it is a just one, I propose to notice the points raised by Dr. Vaughan in his paper, considering them as nearly in their order as possible.

In my paper I stated that corrosive sublimate "undergoes chemical changes, when brought into contact with organic matter. It is immediately converted by albumen to the insoluble albuminate of mercury. For this reason, albumen is recognized as the most efficient antidote in cases of poisoning by corrosive sublimate." These statements Dr. Vaughan denies. He maintains first, that the albuminate of mercury is soluble in solutions containing organic matters. "I endeavored," he says, "to show in the report, which Dr. Hills criticises, that the albuminate of mercury is soluble in solutions con-

taining organic matter," and he then states that I place my opinion regarding this question against his (Dr. Vaughan's) experience. I have looked carefully through the report of the committee in search of the experience which Dr. Vaughan refers to, and find that it comprises *one* experiment in which he shook up mercuric oxide with albumen, filtered, and detected mercury in the filtrate. This is all very well so far as it goes. Dr. Vaughan does not, however, tell us *how much* mercury the filtrate contains; but as he says a little later that the albuminate is freely soluble in an excess of albumen, he evidently intends his readers to infer that the amount was large. If Dr. Vaughan will investigate this matter quantitatively he will find that the amount of albuminate of mercury thus dissolved in albumen is comparatively small. Dr. Vaughan says that the albuminate of mercury in solution is diffusible. Very likely, so long as it undergoes no change: but as I said in my first paper, it is likely, in masses of decomposing excreta, to be at once converted to the insoluble sulphide of mercury by the sulphuretted hydrogen present. This last statement, which Dr. Vaughan also denies, I shall refer to later.

Dr. Vaughan then quotes Merck, and Nothnagel and Rossbach, and gives the formula of Filehne's preparation to support his own statement that the albuminate is soluble in an excess of albumen. Filehne's solution is not, however, a solution in an excess of proteids. The albuminate in this is held in solution chiefly by ammonium chloride, in which it is soluble. Dr. Vaughan says "Filehne's solution contains more than two and a half drachms of the bichloride. This amount would hardly be called very small." These statements, which are well calculated to deceive the careless reader, furnish a good illustration of the peculiar methods employed by Dr. Vaughan in the discussion of this subject. Why does not Dr. Vaughan complete the statement, instead of leaving it in such a form that it may create a misunderstanding in the minds of his readers, — and say that Filehne's solution contains two and a half drachms *in each* 2250–4500 *cubic centimeters*, approximately (assuming that he gives the strength correctly), and that it is a solution of albuminate in ammonium chloride essentially? Evidently because these facts, when fairly stated, do not support his assertion that the albuminate is freely soluble in an excess of albumen. That they may *appear* to do so he suppresses a part of the truth, trusting apparently that such suppression may escape the reader's notice. Such methods, other instances of which appear in Dr. Vaughan's article, do not appear well in the discussion of scientific questions. Dr. Vaughan says: "When Dr. Hills says that albumen is recognized as the most efficient antidote in cases of poisoning by corrosive sublimate, *on account of the insolubility of the albuminate of mercury*, he teaches a doctrine which, I must admit, is wholly new to me." Dr. Vaughan has, presumably, never investigated the solubility of the albuminate of mercury himself, but has accepted the statement of Nothnagel and Rossbach without any question. This idea that the albuminate of mercury is freely soluble in excess of albumen is one the origin of which I am not quite certain of. Christison, Tardieu, Wharton, and Stillé, and Taylor all accept it, but they give no

¹ August 25th, 1888.

² Boston Medical and Surgical Journal, January 3, 1889.

authority for it. Orfila has been quoted by some writers as an authority. But what Orfila says is this: "It dissolves slowly and *in small quantity* in albumen."³ In Storer's "Dictionary of Solubilities," Melsens is quoted as the authority. Melsens says, "This precipitate" (referring to the albuminate of mercury) "is soluble in albumen, etc." It is not quite clear whether he bases this statement on his own experiments or not, but apparently not. The idea is evidently one which originated many years ago, and which has been accepted by a few more recent writers without investigation. But Dr. Vaughan will doubtless admit that very many of the statements made by the chemists of forty or more years ago have failed to stand the test of more recent investigations. Examples are so numerous and the fact so well known that it is unnecessary to particularize any further than to say for the information of our readers that the statement that the albuminate of mercury is freely soluble in albumen belongs in this class. In my own investigations I have never found the albuminate more than slightly soluble in an excess of albumen. Possibly Dr. Vaughan may not be disposed to accept my conclusions. As he has given considerable prominence to the question in his criticism of my paper, I will call his attention to a few authorities. Woodman and Tidy⁵ say that their experiments do not confirm the opinion that the albuminate is soluble in excess of albumen.

Van Hasselt⁶ says albumen occupies the first place as an antidote, since it forms with corrosive sublimate an insoluble albuminate; and further, that the quantity of albumen need not be limited, since the albuminate is not so easily and quickly dissolved as has been supposed. Mulder⁷ says that a solution of the albuminate of mercury by albumen alone cannot be effected. Husemann⁸ says albumen is the most important antidote on account of forming an insoluble precipitate with corrosive sublimate. Miller⁹ says that the compound which albumen forms with corrosive sublimate is a sparingly soluble compound, and that it is for this reason that the white of eggs is the best antidote in cases of poisoning with corrosive sublimate. Wormley¹⁰ says: "Of the various antidotes that have been proposed in poisoning by corrosive sublimate, albumen in the form of white of egg seems to be much the most efficient." "It should be given in large quantity." "The solid coagulum resulting from the precipitation of the poison with albumen, *which is one of the most insoluble compounds of this kind, etc.*"¹¹ Dr. Vaughan will hardly question these authorities. If now he will investigate the subject for himself, he will doubtless find occasion to modify his present views somewhat, and will perhaps be willing to acknowledge that the well-known value of albumen as an antidote in cases of poisoning by corrosive sublimate is based on the comparative insolubility of the albuminate of mercury.

Dr. Vaughan says that I (Dr. Hills) have suggested the idea that the albuminate of mercury is

not absorbed by the stomach, and then proceeds to criticise this assumed suggestion. In fact I did not refer in any way to the question of its absorption, and there is no statement in my paper which can in any manner be interpreted as referring to it. I said that albumen is the antidote to corrosive sublimate in the way of illustration, and Dr. Vaughan doubtless so understood it. The after-treatment in cases of poisoning by corrosive sublimate, as well as the manner of absorption of mercurials, are wholly foreign to the question under discussion.

Dr. Vaughan next takes exception to the statement made by me in my first paper that whatever albuminate of mercury is dissolved is likely to be converted at once to the inert sulphide by the sulphuretted hydrogen present. He says: "Here Dr. Hills is again wrong. *Sulphuretted hydrogen does not decompose the albuminate of mercury*" (italics mine). "Every toxicologist knows this, and destroys the organic matter before he attempts to precipitate mercury from solutions containing proteids. In the report of the committee, where I show that the albuminate of mercury is soluble, I state that the organic matter was destroyed by potassium chlorate and hydrochloric acid, *after which* the mercury was precipitated with sulphuretted hydrogen. Nothnagel and Rossbach say that from the albuminate of mercury one cannot precipitate the metal with sulphuretted hydrogen until the organic matter has been destroyed. If sulphuretted hydrogen precipitates mercury from proteid solutions the mercury so precipitated is not combined with albumen, and the occurrence of such a precipitation shows that the mercury exists in excess above that taken up in the formation of the albuminate. The albuminate of mercury is not easily decomposed." Dr. Vaughan is altogether wrong in his statement that sulphuretted hydrogen does not decompose the albuminate of mercury. It is true that the toxicologist destroys the organic matter of tissues and fluids before he attempts to precipitate metallic poisons with sulphuretted hydrogen. But it is equally true that organic matter does not necessarily interfere with the precipitation of mercury as sulphide. Christison, for instance, says that in mixed organic fluids the action of sulphuretted hydrogen in precipitating mercury is not liable to be prevented. Reichardt says it is unnecessary in testing the urine for certain poisons, mercury included, to destroy the organic matter before precipitating with sulphuretted hydrogen. In his own single experiment which he refers to, Dr. Vaughan could just as easily and much more quickly have detected mercury without destroying the organic matter. For the simple purpose of detecting the presence of mercury the destruction of organic matter was not necessary. The albuminate of mercury is immediately decomposed by sulphuretted hydrogen or ammonium sulphide. I have repeatedly subjected to the action of sulphuretted hydrogen pure albuminate of mercury suspended in water, a dilute solution in excess of albumen, prepared as Dr. Vaughan prepared his solution, by shaking mercuric oxide with albumen, and Merck's solution. In every case the decomposition was almost instantaneous. The last two solutions, it will be observed, are the same solutions which Dr. Vaughan has referred to in his paper as solutions of albuminate of mercury.

³ *Traité des Poisons*, 3rd edition, 1827, page 245.

⁴ *Annales de Chimie et de Physique*, 1851, page 180.

⁵ *Forensic Medicine and Toxicology*, page 202.

⁶ *Handbuch der Gifthehre*, page 306.

⁷ Quoted by Van Hasselt and Husemann.

⁸ *Handbuch der Toxikologie*, page 883.

⁹ *Inorganic Chemistry*.

¹⁰ *Micro-Chemistry of Poisons*, 2nd edition, page 336.

¹¹ *Ibid.*, page 353. Italics mine.

If he will subject them to the action of sulphuretted hydrogen or ammonium sulphide, he will find the results very convincing, though possibly not altogether satisfactory from *his* point of view.

"Again," to quote Dr. Vaughan, "Dr. Hills thinks that the alkalis in decomposing matter would precipitate the mercury." My statements with reference to this point were the following: "But such decomposing masses (referring to excreta) are constantly disengaging ammonia and sulphuretted hydrogen, both of which convert corrosive sublimate into insoluble compounds, etc.," and "decomposing excreta also contain alkaline carbonates, phosphates, urea, and doubtless many other substances which react with corrosive sublimate, and destroy its efficiency; for there is no reason for believing that any of the resulting compounds have any disinfecting power;" and "the excess, if any (referring to corrosive sublimate), is in turn converted to inert compounds by the albuminous material, ammonia, sulphuretted hydrogen, or other substances, etc." Dr. Vaughan does not, and cannot deny, the truth of these statements. They are well-established facts, which have been known for years. But to admit them would be to admit the existence of serious objections to the efficiency of corrosive sublimate. He therefore very ingeniously says "if common salt be added to an alkaline solution of albumen, mercuric chloride will then fail to produce any precipitate." That is, he endeavors to convey the idea that I made a statement wholly different from those actually made by me, and then criticises this assumed statement. The most amusing fact regarding this criticism is, that Dr. Vaughan assumes that fecal masses contain a comparatively large amount of an alkaline solution of albumen; while in the next paragraphs, in order to criticise me on another point, he quotes analyses to show that feces contain but small amount of proteids. Dr. Vaughan evidently intends to stand by corrosive sublimate even at the expense of consistency. We will give another instance of this peculiarity of his. In the report of the committee for 1885, Dr. Vaughan states that the fear that mercuric chloride may filter through the soil, when used as a disinfectant in privy vaults and cesspools, into wells, is groundless; for the soil, he claims, has great power of fixing the mercury, on account, probably, of the inorganic salts it contains; and yet he goes to the extent of denying chemical facts, in order to claim that excreta, which contain not only the same inorganic salts, but also organic matters and products of decomposition capable of decomposing corrosive sublimate, are not at all likely to injure its efficiency as a disinfectant.

Dr. Vaughan thinks I base my condemnation of the committee's biological tests solely upon the incompatibility of mercuric chloride with albumen. This is a mere assumption on his part. As he chooses to misunderstand my remarks on this point, I will restate my criticism. "If we examine the experimental evidence upon which corrosive sublimate is recommended as an efficient agent for the disinfection of solid excreta, we find that it comprises ten or twelve biological tests made with pure cultures of micro-organisms, or with broken-down beef-tea, or with semi-solid feces. Some of these were successful, others were failures. It does not

appear to have occurred to the committee that the *nature* of the material acted upon had any influence on the result in the unsuccessful experiments, or that any further investigation was desirable for the purpose of discovering the cause of the failures. But it was assumed that the *quantity* of material was too great for the amount of disinfectant employed, and that if the latter had been added in larger quantity, or if the time of exposure had been longer, the experiment would have been successful. A liberal allowance was therefore made on the side of safety, and a recommendation made accordingly. The evidence upon which corrosive sublimate is recommended for disinfection of liquid discharges is the following: 'The liquid discharges from the bowels of patients with cholera, typhoid fever, advanced tuberculosis, septic diarrhoea, etc., may be fairly compared with our broken-down beef-tea, as regards physical and biological characters.' The amount required to sterilize a certain quantity of broken-down beef-tea was therefore determined, multiplied by two, and the product recommended as efficient for the sterilization of an equal amount of liquid fecal discharge. A recommendation based upon experimental work of this amount and character is not creditable to a committee of the leading sanitary association of this country, and is not entitled to the favor with which the one in question has been received."

In a word, the experiments of the committee were too few in number to warrant *any* conclusions regarding corrosive sublimate. Their methods of arriving at the quantity of the disinfectant to be used were wholly unscientific. The committee neglected altogether to take into account the chemical action of the material to be disinfected on the disinfectant. They should have from the first admitted that corrosive sublimate is a substance which has a long list of chemical incompatibles, and should have made investigations to determine just what changes it undergoes when added to material to be disinfected; if such changes are likely to injure its efficiency as a disinfectant; and if so, whether the injurious effect of such changes can in any way be overcome.

Dr. Vaughan, it will be observed, chooses to believe that my statement, in the paragraph just quoted, regarding the nature of the material to be disinfected, relates solely to albumen. A reference to my original paper will show that albumen is only one of a number of incompatibles of corrosive sublimate which I mentioned as having been disregarded by the committee. "In the latest report (1888)" Dr. Vaughan says, "carbolic acid has been given the second place, and mercuric chloride has not been recommended for this purpose. This change was made *because the carbolic acid was believed to be sufficient, and not because the mercuric chloride was believed to be inefficient.*"¹² I will quote from the report of 1888 the reasons given *there* for not recommending corrosive sublimate. The reader can judge for himself whether this statement of Dr. Vaughan's is a correct one. On page 160 we find the following: "We would, however, give the preference to the chloride of lime solution for the disinfection of excreta, *not only on account of the neutralizing action of albuminous mate-*

¹² Italics mine.

rials, but for the reasons, etc." On page 164: "The presence of ten per cent. of gelatine, or of ten per cent. of egg-albumen, in the material to be disinfected, has no marked influence upon the germicide power of this agent (carbolic acid), and gives it, *therefore*, a decided advantage over mercuric chloride or sulphate of copper for the disinfection of albuminous material." On page 166: "A consideration of the experimental data above given leads the writer to believe that carbolic acid possesses a decided advantage over mercuric chloride," etc. "*The fact that it is not decomposed or neutralized by putrefying material, and that it will exercise its antiseptic action throughout the mass, etc., gives it a decided advantage.*"¹³ In a word, the first and chief reason given by the committee for not recommending corrosive sublimate is that it is decomposed by albuminous material and not likely to diffuse. The committee even as late as the date of their last report had not apparently discovered that excreta contain other constituents than albumen which will decompose mercuric chloride. Still they have learned something since their report of 1885, and in their report of 1888 they distinctly admit, as the above quotations show, that mercuric chloride is inefficient for the disinfection of excreta. It is with some surprise that we now find Dr. Vaughan practically denying the statements made by the committee of which he was a member.

Dr. Vaughan assumes throughout his paper that I have had no practical experience with the chemistry of corrosive sublimate, but that I merely express opinions founded upon what I deem to be well-established facts. I do not imagine that much importance will be attached by the readers of the JOURNAL to this criticism. I may be pardoned, however, for saying that my experience with corrosive sublimate has been in one way and another an extensive one, and that any statements made by me relative to its chemistry are based upon my own observation. Reference to the literature of the subject will show whether or not they may be regarded as "well-established facts."

I desire no further controversy with Dr. Vaughan on this subject. In fact, none is necessary. The albuminate of mercury is freely soluble in an excess of albumen, or it is not. It is decomposed by sulphuretted hydrogen, or it is not. Mercuric chloride has a long list of incompatibles, some of which are important constituents of excreta, or it has not. These are the only questions under discussion. Any one who is still in doubt regarding these can easily have his doubts removed by consulting standard authorities, or by the performance of a few simple tests, and without any further controversy between Dr. Vaughan and me.

REPORT ON PROGRESS IN THORACIC DISEASE.

BY F. C. SHATTUCK, M.D., AND G. G. SEARS, M.D.

DIAGNOSIS OF EARLY PHTHISIS BY THE MICROSCOPE.¹

TROUP emphasizes the diagnostic value of elastic fibres in cases of suspected phthisis, and says that "in no case will they be missed if sought for with

¹³ Italics in these quotations mine. The last quotation refers to the disinfection of masses of material.

¹ Edin. Med. Jour., 1888, July.

sufficient patience. They are to be found very early in the disease and in the most innocent-looking sputum. They will be discovered before stethoscopic or percussion sounds, even when listened to by skilled ears and interpreted by skilled brains, give any other than uncertain information as to what is going on." They may precede the bacillus often for a considerable time, and unlike it are never temporarily absent. They are not, however, pathognomonic, but if seen the supposition is strong that we are dealing with phthisis, and a supplementary search for the bacillus will complete the diagnosis. Conversely their absence will enable us to assert with confidence that cases are not phthisis, where signs and symptoms point strongly to a contrary opinion.

CONTRAINDICATION TO THE TREATMENT OF PHTHISIS BY HIGH ALTITUDES.

Sir Andrew Clark,² in a brief communication, states as a result of his experience that phthisical patients with albuminuria should not be sent to the high stations; and that, if albuminuria develops while there, they should be sent away immediately. As it were incidentally, he also says: "I am as sure as I can be about anything at present incapable of actual demonstration, that the recoveries from phthisis, judiciously treated at high altitudes, are much more numerous and much more lasting than those treated by any other method at any other place."

THE VALUE OF INHALATIONS IN THE TREATMENT OF LUNG DISEASE.³

A limited number of experiments made by the author on phthisical patients tend to show that inhalations of iodine, whether vaporized by the steam or the hand-ball atomizer, have no deep penetration, inasmuch as the iodine cannot be detected in the urine after the inhalation. Turpentine, on the other hand, when inhaled, imparts its characteristic odor to the urine, thus showing that it is absorbed into the blood. This may explain its efficacy in checking hæmoptysis.

The conclusions are as follows:—

1. That the success of inhalations as a mode of medication depends principally on the easy convertibility into gas or vapor of such substances as are clearly desirable for the purpose.
2. That, consequently, bodies which are volatilized at ordinary temperatures are more readily absorbed by the lungs than bodies which have to undergo combustion before conversion into gases.
3. That all moist inhalations, where steam, watery vapor, or spray is the vehicle of medication, are but slowly absorbed by the lungs, and enter the circulation in small quantities, and in some cases not at all, the slow rate of pulmonary absorption contrasting strongly with the rate of gastric absorption of the same medicines when swallowed, as proved by their detection in the urine.
4. That medicinal inhalations are more useful in diseased conditions of the pharynx, larynx, and larger bronchi than in those of the alveoli and lung parenchyma.
5. That in pulmonary disease the antiseptic respirators, while they lessen cough, and reduce ex-

² Lancet, 1889, i. p. 8.

³ C. Theodore Williams, Lancet, 1888, ii. p. 700.