

pointed our expectations, not because the author contends for the unity of continued fever, but in consequence of the manner in which he has treated this subject. Purporting to be a work devoted to statistics of typhus and typhoid fever, embodying the experience of several years at a large hospital, we had hoped to find the question of the identity or non-identity of these affections tested anew by a comparison of the results of the numerical analysis of a series of cases of each fever separately; and we were anxious to see whether the results of observations made in the capital of Sweden would correspond or differ from those obtained in France, England, and America. Instead of this, the work, so far as this question is concerned, only gives the opinions of the author, backed by whatever authority may be derived from the fact that he has seen and treated a large number of cases of both forms of fever. The force which belongs to such opinions different persons will undoubtedly estimate differently. We do not, however, hesitate to attach to them small importance when brought into antagonism with conclusions reached by the analytical investigation of recorded cases. The statistics relating to both typhus and typhoid fever indiscriminately possess a certain value, provided these affections are identical; otherwise they are comparatively valueless.

With the portion of the work devoted to treatment, we are less dissatisfied. Although there is here room for criticism, the therapeutical views which the author advances, and the pathological notions on which they are founded, are, we believe, in the main, in accordance with the opinions and practice of the most intelligent and judicious practitioners of the present time.

A. F.

ART. XII.—*Chimie Appliquée à la Physiologie et à la Thérapeutique.* Par M. le Docteur MIALHE, Pharmacien de l'Empereur, Professeur Agrégé à la Faculté de Médecine, &c. &c. Paris, 1856.

M. MIALHE'S book is a reproduction, in great measure, of separate essays published by him at various intervals between 1840 and 1850; and it is remarkable to see how many of his physiological doctrines, whether adopted or original, which have been either modified or entirely superseded by the advance of the science since the date of their first appearance, are retained by the author, and re-stated with as much confidence as ever. After passing through, in the introductory pages, with some general considerations on the character of the chemical changes taking place in the healthy body, and the proper mode in which they are to be studied, he takes up, in the first chapter, the consideration of the phenomena of oxidation and nutrition—commencing with those of oxidation. He makes a division of all the alimentary substances according to Liebig's celebrated hypothesis, into *plastic* and *respiratory* elements. Of these, the former are mostly used in the nutrition of the tissues, and the latter—such as sugars and oils—almost completely destroyed by direct oxidation in the blood; terminating, therefore, soon after their ingestion, in the production of water and carbonic acid. The very important objections to this doctrine, which have recently been acknowledged by the leading physiologists, seem to have nearly or quite escaped the author's notice. He speaks constantly of those substances which, on being taken into the system, do not reappear in the excretions, as necessarily "burnt" in the

circulation; and regards the natural heat of the body as the result of the same combustive process. These are all opinions which were adopted some time since, and are now modified or abandoned in consequence of their resting too evidently on insufficient data; several very important links in the proof being entirely wanting, or supplied only by inference. There is one theory, however, advanced by M. Mialhe, in this connection, which is, we think, a novelty, and at the same time a striking instance of the common, but very dangerous practice of inferring a chemical reaction to take place in the body without any direct proof, and simply because its materials exist there. It is in regard to the mode of action of various poisonous substances, and particularly of hydrocyanic acid. The author, after speaking of the constancy and rapidity with which the "intravascular oxidation" goes on in the body, and its importance to the continuance of life, goes on to remark, that any substance which should interfere with this oxidation, would, by that fact alone, become poisonous. Some substances actually do, as he thinks, have such an effect. Chloroform and sulphuric ether, for example, introduced into the blood, are thought to displace its oxygen in this manner, and to suspend life, during the continuance of their anæsthetic effect, by simply arresting the process of combustion. The volatile oils are considered as liable to exert the same influence by reason of their avidity for oxygen; and sulphuretted, seleniuretted, and arseniuretted hydrogen also act by depriving the blood of its oxygen, while they are at the same time converted into other compounds, poisonous by themselves.

"We can very easily understand, then," says M. Mialhe (p. 29), "how rapidly fatal would be the operation of any substance which should have the power of seizing at once upon all the oxygen contained in the blood, and destined for the supply of respiration and nutrition. Such would be the action of phosphorus, provided it were possible to administer this body in a gaseous condition. The same effect would be produced by any substance endowed with the property, without itself becoming oxidized, of suddenly putting an end to the process of intravascular combustion. Such a poison would act on the living body like a stroke of lightning. Now we know there is a poisonous substance, whose operation is in reality like that of a stroke of lightning, viz: hydrocyanic acid. It results from the researches of M. Millon that this acid has a remarkable tendency to interfere with certain phenomena of oxidation, or combustion; and that, even in very small quantity, it prevents entirely the powerful combustive action of iodic acid upon oxalic acid. Considering the relation which exists between the two phenomena of respiration and oxidation, we may conclude that the only effect of hydrocyanic acid, introduced into the organism, is to cut short the process of vital oxidation, and produce in that way an instantaneous death."

It would be difficult to imagine a physiological theory more destitute of direct evidence than this; or one which depended for its support upon remoter analogies.

In his chapter on the *digestion of starchy substances*, M. Mialhe reproduces almost entirely the opinions which he professed ten years ago with regard to the importance of "animal diastase," or the organic substance of the saliva, and its identity with vegetable diastase, as well as with the organic matter of the pancreatic juice. He was among the first to extract this substance from the saliva, and to recognize its power of transforming starch into sugar. He still continues to attribute to it a very important part in the chemical processes of digestion, though this opinion is now, and has been for some years, generally abandoned, for experimental reasons which do not seem to be regarded by the author as having the importance which is usually attributed to them.

The question of the *origin of sugar* in the animal economy is disposed of in a still more loose and unsatisfactory manner. M. Mialhe contents himself with stating, in very concise terms, the important doctrine maintained by Bernard, that a certain quantity of sugar is constantly produced in the liver from metamorphosis of the elements of the organ itself; and then proceeds to express his dissent from it in the following brief terms:—

“For our part, we cannot share the opinions of M. Bernard. For us, the liver is not a secreting organ for the sugar, but only an apparatus of condensation, in which the sugar accumulates, after being taken with the food; in the same manner as the liver also condenses in its tissue certain metallic poisons introduced into the economy.” (P. 61.)

The many experiments performed and reported by Bernard, in opposition to this very objection, and the long contest carried on, during the past year, before the French Academy on the whole question, in which Longet, Figuier, Bernard, and Lehmann all took a part, and during which a special committee investigated the matter and reported favourably to Bernard, are all passed over in silence; though they ought certainly to have some weight in deciding so very interesting a physiological question.

In his concluding paragraph of this section the author also misrepresents Bernard's doctrine in a very singular manner:—

“If the sugar be not derived,” he says (p. 61), “exclusively from amylaceous elements of the food (since it is now demonstrated that muscular flesh and the white of egg contain a certain quantity of it), it is nevertheless an undeniable fact, admitted by M. Bernard himself, that the quantity of sugar existing in the organism is in direct proportion to the quantity of starchy matters absorbed from the digestive apparatus. So the liver, whether it be a secreting or simply a condensing organ, derives after all from the food the saccharine elements which it supplies to the circulation.”

This is certainly unjust, both to Bernard and his doctrine; for one of the points upon which he most positively insists is, that the liver is *not* dependent for its saccharine supply upon starchy matters in the food; that as much sugar is produced by it under an animal as a vegetable diet; that even after several months' strict regimen on food carefully deprived of amylaceous matters, it is still as abundant as when the food was of a mixed character; and that, finally, the sugar of the liver, instead of being derived from any starchy elements, is produced in the tissue of the organ itself, by a transformation of its own substance. (Bernard, *Nouvelle Fonction du Foie*, Paris, 1853.)

In treating of the *destruction of sugar* in the animal economy, M. Mialhe describes first the usual reactions of a saccharine substance when treated, in solution, with acids, alkalies, and metallic salts, and particularly its power of reducing the salts of copper, when heated in an alkaline solution. Sugar alone, he observes, will not produce any reduction of the oxide of copper; but in order to produce this effect it must be accompanied with an alkali or alkaline carbonate. The body which becomes oxidized, therefore, at the expense of the copper, is not the sugar itself, but the new substances (ulmic, formic, and glycolic acids) into which it has been previously converted by contact with the alkali. He regards the phenomenon of the reduction of metallic compounds which sometimes takes place in the body (as in the case of the salts of copper, red ferrocyanide of potassium, &c.) as owing to the presence of the formic and ulmic acids, &c. into which the sugar has been converted by the alkaline carbonates of the blood. It is in this way that he arrives at the following theory of the natural transformation of the sugars in the circulation:—

"On its arrival in the blood the glucose decomposes the alkaline carbonates, forming with their bases new compounds, *glycosates*, and setting free their carbonic acid; the glycosates, which are unstable compounds, are rapidly transformed into glycolic, ulmic, and formic acids, or rather into glyciates, ulmiate, formiate, which combine with the oxygen of the blood, and undergo a veritable combustion, with the production of water and carbonic acid." (P. 67.)

The above theory of the destruction of sugar in the blood will be found, on examination, to rest on four successive propositions, each essential to its establishment, and all of them more or less hypothetical:—

1. In the reduction of copper from an alkaline saccharine solution, the alkali acts by first converting the sugar into ulmic, glycolic, and formic acids.

2. When metallic salts, and particularly those of copper, are reduced in the animal economy to a lower state of oxidation, the agents of this reduction are the ulmic, glycolic, and formic acids.

3. These acids are produced *from the sugar of the blood* by the action of the alkaline carbonates.

4. And finally, the water and carbonic acid, expelled from the system, are (partly) formed by direct oxidation of the above acids.

None of the above propositions are free from doubt. With regard to the first, though the presence of an alkali be certainly necessary to the operation of Trommer's test for glucose, we are not aware that its precise mode of action, as above described, rests on anything more than inferential grounds.

In the second place, the reduction of metallic salts, when it occurs in the animal economy, may certainly be owing to the action of other bodies beside formic acid, &c. The albuminoid substances themselves absorb oxygen with great readiness under every ordinary condition, and may even produce, by their presence, a partial reduction in the solution of copper, used as Trommer's test. Thirdly, formic acid has undoubtedly been found in the blood and various other fluids of the body; but its source is unknown. Even in the red ants, from which it was formerly obtained in the greatest abundance, it is yet quite uncertain whether it be produced in the body of the insect, or introduced with the food from some vegetable source. And, lastly, the direct oxidation of these acids, when present in the body, is itself a matter of doubt. Indeed, formic acid has been found (by Lehmann) in comparatively large quantity in the normal perspiration. If it were, therefore, so readily oxidizable in the system as to reduce the salts of copper, when these are present, it is not easy to understand how it should itself escape destruction by the free oxygen of the blood, and arrive unaltered at the surface of the skin.

The theory of the mode of disappearance of the animal sugars, adopted by M. Mialhe, conducts him very readily to an explanation of the morbid phenomena of diabetes. In health, the normal sugars of the blood are destroyed by the influence of its alkaline carbonates. Anything which diminishes the alkaline character of the blood will therefore tend to produce an accumulation of sugar in the system, and a consequent diabetic condition of the blood and all the secreted fluids. M. Mialhe does not hesitate, therefore, to attribute the condition of diabetes to a preponderance of acids in the system by which the alkaline character of the blood is neutralized, and the oxidation of the sugar prevented. This preponderance may be occasioned, he says, by an abuse of acidulated drinks, by an exclusively azotized diet, or by a suppression of the cutaneous transpiration. Animal food, containing principally compounds of sulphur and phosphorus, gives rise in the body, by oxidation, to sulphuric and phosphoric acids; while vegetable food contains salts of the organic

acids (citrates, malates, &c.), which are destroyed by oxidation, leaving a residue of alkaline carbonates. A proper admixture of the two is thought to maintain the natural alkalescence of the blood, while a too great proportion of animal food diminishes or entirely neutralizes it. Finally, the cutaneous transpiration is one channel through which the free acids are eliminated from the system. There are, says M. Mialhe, five secretions with an alkaline reaction, viz., the tears, saliva, bile, and pancreatic and intestinal juices; and three which are acid, viz., the sweat, gastric juice, and urine, by whose constant activity the natural balance between acids and alkalies is preserved. If, however, the cutaneous transpiration be checked, its acid ingredients are retained in the system, the fluids lose their alkalescence, the sugar is not destroyed, and diabetes is the consequence. M. Mialhe therefore "maintains," in a formal manner, "that the cause of diabetes is the want of assimilation of glucose, through an insufficiency of the alkaline principles in the economy." (p. 77.)

We should anticipate at least, then, from so confident a statement, that this want of alkalinity in the blood, in cases of diabetes, had actually been observed; and that this fact served as a basis for the author's opinion. Far from it. M. Mialhe even acknowledges, with an admirable candor, the difficulty of the objection that, in point of fact, the blood of diabetic patients "is never acid or neutral; but always maintains an alkaline reaction;" and the singular manner in which so apparently stubborn a fact is disposed of, is not the least remarkable part of his theory. In the first place, he says it is "exceedingly difficult" to determine by experiment the precise degree of alkalinity of the blood; intimating, therefore, that it may be diminished in diabetes, without our having observed it. But the remaining part of his answer to the above objection is the more important, and its ingenuity deserves a quotation in the author's own words:—

"In the condition of health," he says (p. 76), "the alkalinity of the blood is determined mainly by the alkaline carbonates, but to a slight degree, also, by the alkaline phosphates; these last, notwithstanding their power of restoring the blue colour of reddened litmus, are not included by chemists in the list of alkaline substances proper; and, furthermore, they do not, as we have proved, give rise to the decomposition of glucose. Now it is our belief, that in diabetic patients the blood remains alkaline *because it is rich in phosphates and poor in carbonates*, so that the degree of alkalinity, produced by the presence of the phosphates, is entirely inefficient for the decomposition of the glucose; which can only be effected by the influence of the carbonates."

It is upon such grounds that M. Mialhe rests his theory of the pathology of diabetes.

The remainder of the first portion of the book is occupied with the consideration of the digestion of albuminoid and fatty matters, the endosmotic properties of albumen and albuminose, and the occasional passage of these substances into the excretions and exudations. It contains little that has not been already before the profession, or that is presented in such a way as to excite any great degree of interest.

The remaining chapters constitute by far the most interesting and important portions of the book. They are devoted to the *absorption and chemical transformation in the system of drugs and poisons*. But very little has yet been done in this field. Almost all we know with regard to the subject is, that such and such matters are, or are not, absorbed into the blood; or that they do, or do not, make their appearance in the secretions. The manner of their absorption, and the changes which they undergo before elimination, constitute

an extremely important subject, the study of which may be said to be now first commenced in earnest by M. Mialhe. He commences by establishing the principles that no substance can have any action on the living body without being absorbed; and no substance can be absorbed without being liquefied and soluble; and then passes in review the actions of iodine, sulphur, phosphorus, metals and metallic salts, resins, volatile and fixed oils, and the vegetable alkaloids with their salts.

Notwithstanding the very slight solubility of *iodine* in water, when administered internally, or applied to a denuded surface, it rapidly undergoes changes which result in its absorption. A minute portion of it, according to M. Mialhe, is dissolved by the water of the fluids, and is then converted into iodides and iodates by the action of the alkaline carbonates; and, as the iodides of potassium and sodium themselves exert a powerful solvent action on free iodine, a large portion of this at once enters into solution, and, as the author believes, coagulates the animal matters of the fluids; the coagulum afterwards disappearing by the gradual conversion of the iodine into iodides and iodates by the alkaline chlorides and carbonates of the blood and their subsequent absorption. He lays it down, therefore, as a precept, that iodine should never be administered in substance or in alcoholic tincture, in any case where it is our object to produce merely a constitutional effect; since its coagulating tendency is productive of a troublesome local irritation, and interferes, at the same time, with the absorption of the drug. A much better form, under almost all circumstances, is the iodide of potassium, which is already soluble, and does not coagulate or irritate the tissues. Iodide of potassium, as it is usually administered, does, it is true, sometimes produce a considerable degree of gastric irritation; but this is owing, according to M. Mialhe, to the presence of iodate of potass, which not unfrequently, as he says, occurs as an accidental impurity in the iodide of potassium of the shops. This iodate of potass is decomposed by the acid of the gastric juice and its iodine, liberated from union with the potass, acts on the mucous membrane as a coagulating and irritating substance.

*Sulphur* is thought to be capable of absorption when administered internally, notwithstanding its insolubility in water. The fact seems to be sufficiently proved by its frequently producing a general constitutional excitement in addition to its laxative effect, by the sulphurous odour it communicates to the breath and excretions, and finally by the property which it communicates to the skin, after prolonged administration, of blackening certain metallic substances. This absorption is accomplished by the alkaline carbonates of the intestinal fluids, which transform the sulphur into alkaline sulphurets and hyposulphites, substances which are soluble, and, consequently, directly absorbed into the circulation. Arrived at the skin, these combinations are decomposed by the acid of the perspiration, and hydrosulphuric acid is disengaged if the excreted substance be a sulphuret, and sulphurous acid with free sulphur if it be a hyposulphite. Whether sulphur be administered by the stomach or by the endermic method, therefore, the author recommends that it be always associated with an alkali (potass, carbonate of magnesia, or soda), in sufficient quantity to neutralize the acid reaction of the gastric or cutaneous fluids, and in addition to form soluble sulphurets and hyposulphites with the drug.

*Phosphorus*, equally insoluble in water with sulphur, is thought to be absorbed by a very similar process, being converted by the alkaline intestinal juices into phosphoretted hydrogen and hypophosphites of soda and potass. It should therefore be given dissolved in oil or ether, in order that it may be

thoroughly exposed to the contact of the intestinal fluids, and its transformation into soluble substances effectually secured.

*Arsenic*, in a metallic state, is regarded as entirely insoluble, incapable of absorption, and without action on the living tissues; but nevertheless as practically a very poisonous substance, because it readily becomes oxidized and converted into arsenious acid. M. Mialhe has convinced himself not only that metallic arsenic readily gives rise to arsenious acid when exposed to the contact of moist air, or water containing air in solution, but also that this oxidation is much facilitated by the presence of the chlorides of potassium or sodium; and as the greater part of the animal fluids contain both oxygen and the alkaline chlorides, powdered metallic arsenic must, when introduced into the body, always give rise to more or less arsenious acid, and become absorbed under that form.

In the treatment of arsenical poisoning, the antidote which has heretofore been regarded with most favour is the hydrated peroxide of iron. M. Mialhe proposes, as much more efficacious, the *hydrated sulphuret of iron*, which produces by decomposition an insoluble sulphuret of arsenic and a peroxide of iron. He says that a comparative experiment with these two substances and a solution of arsenious acid shows that the hydrated sulphuret of iron decomposes the poisonous substance much more rapidly than the hydrated oxide. The insoluble sulphuret of arsenic is, however, slowly reconvertible into arsenious acid by the chlorides of the intestinal juices; and the antidote should therefore be always administered in excess, in order to guard against such an accident. The great advantage, however, of the hydrated sulphuret of iron, and for which M. Mialhe particularly recommends it, is, that it is an antidote, not only for arsenic, but also for all the metallic salts liable to be used as poisons, except cyanide of mercury—reducing, in the same manner with arsenic, the salts of copper, lead, tin, antimony, bismuth, mercury, and silver; so that in any case of doubt as to the precise nature of the metallic poison which had been administered, it would be infinitely safer as an antidote than the hydrated peroxide.

In treating of the mode of action of the different preparations of *iron*, the author considers it as certain that nearly all of them are efficacious, to a certain extent, as remedies, and more particularly in the regeneration of the blood-globules in chlorotic patients. Those preparations which are insoluble in water, are mostly soluble in the acids of the gastric juice, and act afterward in the same manner as the soluble compounds. The mode in which he imagines the metallic preparation, however, to produce its final result, is a little complicated. He finds, in the first place, that all the soluble salts of iron, without exception, precipitate with the animal matters of the gastric juice. The insoluble preparations, including metallic iron, are first dissolved by the acid of the gastric fluids, and then precipitate at once, like the former, so soon as these fluids are in excess. A small portion of the iron, however, escapes precipitation, is taken up by the blood, and meets there with the alkaline carbonates, by which it is decomposed; the insoluble oxide of iron which is set free being appropriated by the blood-globules, and prevented by its insolubility from escaping by the secretions. It is in this way, according to the author, that it contributes to the regeneration of the blood-globules and the cure of chlorosis.

According to this theory, the only preparations of iron useful in the treatment of chlorosis, are those whose compounds, dissolved in the gastric juice, and absorbed into the blood, are decomposable by the alkalies, leaving an insoluble oxide to be retained in the system—such as metallic iron and its

oxides, chlorides, bromides, iodides, and, without exception, all its oxyalts. On the other hand, those preparations, such as ferrocyanide of potassium, not decomposable by the alkalis of the blood, remain soluble, pass into the secretions, and merely traverse the body without producing any therapeutical effect. The preparation which the author regards as the most efficacious is the *double tartrate of iron and potass*, which he prefers for the following reason: All the other oxyalts, as has just been mentioned, after allowing a minute quantity to be absorbed from the stomach, precipitate with an excess of gastric juice, and pass into the intestine. Here they are decomposed by the alkaline fluids of the intestine, and the whole remaining iron set free in the form of an oxide, which passes through the intestine unaltered, and is discharged with the feces. The only one of these preparations not decomposable by the alkalis is the tartrate of iron and potass. It therefore continues to be absorbed through the entire length of the intestine, and introduces into the blood a much larger quantity of iron, for the same dose, than can be the case with any of the other preparations. Once introduced into the blood, however, its tartaric acid is destroyed by oxidation, replaced by carbonic acid, and the carbonate of iron is then decomposed by the alkalis, and its oxide retained in the system, as already described.

How much of the above is the result of direct experiment, and how much inferential, it is not always easy to determine from the author's account. He starts, however, with some very grave errors, which detract considerably from the value of his conclusions. He regards, for instance, the two following facts as "incontestablement acquis à la science."

1. "The globules of the blood contain a combination of iron; *no other part of the living body contains iron.*" (P. 285.)

It should hardly have escaped the recollection of the author that, at the present day, iron has been demonstrated to exist, in the healthy condition, in the urine, the sweat, saliva, gastric juice, and bile, as well as in the blood-globules.

2. "The combination of iron contained in the globules presents the reactions of an *oxygenated compound* of the metal;" from which the conclusion is drawn that the iron is combined with the globules under the form of an oxide.

The grounds, however, upon which this opinion was first adopted, have been found to be quite insufficient; and it is now acknowledged by the first authorities in organic chemistry, that we are entirely ignorant as to the particular combination under which iron exists in the blood-globules. (Lehmann, Robin and Verdeil.)

Although these errors, however, may invalidate the author's theory with regard to the ultimate mode of action of the compounds of iron in the system, they do not necessarily impair his conclusions as to their mode of absorption, or the superiority which he claims for the tartrate of iron and potass over the other ferruginous preparations.

The researches of the author on the *preparations of lead* bring him to the conclusion that they are all, as well as metallic lead, when introduced into the digestive canal, acted on by the alkaline chlorides of the intestinal juices, and converted into chloride of lead; this again unites with the excess of chlorides remaining, so as to produce a double chloride of lead and potassium or sodium. This "alkaline chloro-plombate" is distinguished from the simple chloride of lead by being more soluble than it, and not forming any precipitate with an albuminous fluid. It is therefore much more readily absorbed, and enters freely into the circulation. When, therefore, any soluble and



coagulating saturnine preparation—as, for example, the subacetate of lead—is taken into the stomach, its first effect is to precipitate with the albuminous matters of the digestive fluids. But it is afterward decomposed by the chlorides, and absorbed under the form of a double chloride into the circulation.

A very interesting series of experiments are reported by M. Mialhe, which he undertook with the view of ascertaining whether any of the compounds of lead could be given off as gaseous exhalations when mixed with spirits of turpentine. It has been thought that paints, having compounds of lead for their basis, when used with volatile oils, might give rise while drying to emanations containing lead, and become poisonous by inhalation. M. Mialhe has demonstrated, however, by a very ingenious and satisfactory contrivance, that this is not the case; and that any injurious effects produced by such emanations must be attributed to the spirits of turpentine, since they contain no lead that can be chemically demonstrated, even when examined in a very concentrated form.

One of the most important chapters in the book relates to the action of the *mercurial compounds*. According to the author's investigations, all the preparations of mercury in use as medicinal agents, are converted, to greater or less extent, by the chlorides of the animal juices, into corrosive sublimate; and that, whatever be the compound employed, the bichloride is the only active agent in the production of its constitutional effects. He finds that calomel, exposed to the action of a solution of chloride of sodium and sal ammoniac, produces, at the end of twenty-four hours, a quantity of corrosive sublimate, which varies according to the amount of alkaline chlorides present, and the elevation of the temperature at which the mixture is kept. Similar results were obtained with the other preparations of mercury. The addition of various organic matters—sugar, gelatine, albumen, &c. to the mixture, sometimes retarded, but never entirely prevented the reaction. It even took place when calomel was mixed with the serum of the blood; and finally, the author has reason to believe that he has detected bichloride of mercury in the urine, after calomel had been administered by the stomach. The bichloride itself, then, in minute doses, if managed with prudence, might replace all the other preparations of mercury. Still, calomel has some practical advantages, which will probably enable it always to retain its place as a therapeutical agent. Taken into the stomach in an insoluble form, it is very slowly and gradually transformed into the bichloride, and constantly absorbed in successive minute doses during its entire passage through the intestinal canal. Its local action is, therefore, very mild, and an overdose is not liable to be attended with any serious results, since the amount of bichloride produced is not in proportion to the calomel ingested, but to the quantity of the chlorides which it meets with in the intestine; and these never occur in so concentrated a form as to produce dangerous results. The different preparations of mercury are, therefore, more or less active in proportion to the readiness with which they become converted into corrosive sublimate under the influence of the alkaline chlorides. M. Mialhe arranges them in the order of their activity, as follows: First, the bichloride itself; then the biniodide, the red oxide of mercury, calomel by precipitation, calomel by sublimation, the protiodide, and lastly metallic mercury. The protiodide is usually regarded as more active than it really is, because, according to the author, when prepared according to the method of the French Codex, it always contains, as an impurity, more or less of the biniodide, sometimes amounting to eight, or even ten per cent. of its weight.

The author directs the attention of the profession to two facts in this con-

nection, which are very important in a medico-legal point of view: first, that corrosive sublimate may exist in the dead body, in cases of poisoning, and not be detected by the usual tests; and secondly, it may be detected in the body when it has not been administered during life.

"The method usually adopted by chemists," he says (p. 463), "to detect corrosive sublimate in an aqueous solution, is to extract it with ether. Now, we have ascertained that corrosive sublimate, when accompanied with a notable quantity of binoxide of mercury and an alkaline chloride, *will not dissolve in ether*; such is the mixture which is produced by the addition of a few drops of a fixed alkali to an alkaline chloro-hydrargyrate—that is to say, to a solution containing, at the same time, corrosive sublimate and an alkaline chloride."

The possible detection of corrosive sublimate in the body, when none has been taken by the stomach, results of course from what has already been said of the transformation of all the mercurial preparations by the alkaline chlorides of the intestinal fluids.

In the remaining chapters there are many interesting observations on the preparations of silver, the fixed oils, and vegetable alkaloids, the localization or stagnation of poisons, compatible and incompatible substances, pharmaceutical preparations, and the special mode of action of caustics, astringents, vesicants, purgatives, &c. The whole of this portion of the treatise is extremely suggestive; and though some of the author's conclusions may appear to be founded on too slight grounds—as, for example, what has been already quoted in regard to the preparations of iron—all of them are deserving of attention, and many will probably be more thoroughly established at some future time. The only danger, in this kind of study, lies in anticipating too confidently what reactions will take place in the animal fluids from what we know of their separate ingredients. It cannot be too constantly remembered, that it is impossible to foretell what will be the reaction of any chemical substance in an animal fluid. So many unexpected results have already been obtained from direct observation in this particular as to show the extremely uncertain character of inferential conclusions; and we cannot better express the present and future requisitions of the science, in this respect, than by quoting M. Mialhe's own words:—

"Chemical reactions do not take place with such simplicity and precision in the interior of organized bodies as in the experiments of the laboratory. The human body is not an inactive capsule, a simple test-tube, without influence upon the phenomena which take place in its interior. It is, on the contrary, an exceedingly complicated and movable organization, all of whose conditions and elements must be investigated, ascertained, and reunited, before we can properly appreciate their influence upon any new combinations which may arise; otherwise it is impossible to establish a single reliable conclusion. It is by too often forgetting these principles, and by misconceiving the composition of the animal solids and fluids, that we have sometimes retarded science rather than advanced it, while making a hasty application to physiology of incomplete chemical ideas."

J. C. D.