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P.S.—I must say this, that I think the Gentlemen of the Society of Arts, &c. are very unhandsomely treated by the critic to whom I have alluded, as if they exercised no judgement on whom, or for what, they bestow their honourable and bountiful rewards.

XIX. *An Answer to the Observations of Dr. PEARSON (see our last Number) on certain Statements respecting the alkaline Matter contained in dropsical Fluids, and, in the Serum of the Blood.* By ALEX. MARCET, M.D. F.R.S. one of the Physicians to Guy's Hospital.

To Mr. Tilloch.

SIR, **ALTHOUGH** I feel disinclined to engage in any public philosophical controversy, especially when the object is to vindicate statements, the truth of which any common observer may easily ascertain by experiment; yet, as there are some points in the above communication which do not place the question in its proper light, and might mislead those who have no opportunity of referring to the original documents, I have thought it necessary to offer in return a few observations. The state of the question is simply this: all chemists have for a long time agreed that the blood, and probably all the animal fluids, contain, together with various neutral salts, a certain portion of alkali not combined with any acid. This alkali has generally been considered as being soda, although a few chemists had also noticed traces of potash in some of these fluids. Dr. Pearson, on the contrary, in examining various kinds of animal substances, and especially of expectorated matter, was led to conclude that the whole of the uncombined alkali contained in the animal fluids, was potash; and that they did not contain uncombined* soda in any proportion whatever.

In analysing the fluids of dropsy, I was naturally led to inquire into this question, and the results obtained induced me to conclude, that the only uncombined alkali present in the blood, or other animal fluids, was soda; and that the indications of potash, which by applying the test used by Dr. Pearson I was able to detect in these fluids, were owing to the presence of that alkali in a state of combination with the muriatic acid.

The experiments I adduced in evidence were of two

* By the expression *uncombined*, I mean not combined with acid.
kinds;

kinds; some of them showing that the uncombined alkali was soda, and others that it was not potash.

Portions of saline matter being procured from various animal fluids by evaporation and incineration, and brought by subsequent redissolution and evaporation to a crystalline state, crystals of determinate forms were obtained, some of which appeared to consist exclusively of subcarbonat of soda, some of muriat of soda, and others of muriat of potash; but none could be detected which appeared to contain any carbonat of potash.

Other similar portions of the saline matter being treated with acetic acid, in order to bring any uncombined alkali present to the state of acetat; and alcohol being added with a view to separate these acetats, the residue of this alcoholic solution appeared to consist almost solely* of acetat of soda; whilst, on the other hand, potash was found in the residue left undissolved by the alcohol.

In these various trials the presence of potash, in a state of combination, was proved by the tests of oxymuriat of platina and tartaric acid, both of which form precipitates with potash, and not with soda.

The uncombined alkali, on the contrary, was shown *not* to be potash by the last-mentioned tests failing to indicate the presence of that alkali; whilst, on the other hand, it was proved to be soda by the action of nitric acid, which, in combining with it, formed crystals of a rhomboidal instead of a prismatic figure.

I shall not enter into the particulars of these operations, because they are minutely related in the communication which has given rise to this discussion; but I shall now rapidly examine the principal objections which Dr. Pearson has made to the above conclusions.

Dr. Pearson's first ground of complaint is, that instead of showing his conclusions to have been erroneous, that is, I conceive, instead of following him step by step in his inquiry, I have contented myself with exhibiting my own experiments and conclusions. But I beg to observe, that the object of my inquiry was not to repeat Dr. Pearson's experiments, but to examine dropsical fluids; and that, if in the course of my analysis I met with results which militated against his conclusions, it could not be reasonably expected, that in stating these results I should think it incumbent upon me to wade through his laborious

* A trace of potash was detected in the alcoholic solution; but it must be remembered that alcohol, however rectified, will take up minute portions of muriat of potash, or indeed of almost any other soluble salt.

researches on the various forms of sputum or expectorated matter. I might indeed have abstained altogether from referring to his labours; but I thought it due to him, as a philosophical inquirer long known in the chemical world, to point out such similarities or discordances of results as occurred in our respective experiments; thus referring the matter to the decision of physiologists, and showing that there was no wish, on my side, to overlook the authority of former inquirers.

In endeavouring to analyse the various objections brought forward by Dr. Pearson, I am so often at a loss to understand his meaning, and, I must add, so much embarrassed by the obscure and inaccurate manner in which he has stated some of my own proceedings, that it would be a task equally fruitless and laborious to follow his steps closely. I must, therefore, as much as possible, select those objections which are of a specific nature, and may be answered by an appeal to experimental evidence. Such is, for instance, the argument which he employs, no less than three times, (once in support of his own experiments, and twice with a view to invalidate my inferences,) on the effects of alcohol and acetic acid,—which argument is founded upon his belief that acetat of soda is *not* soluble in alcohol, and that it is *not* a deliquescent salt; two palpable errors, which half a grain of this salt and a few drops of alcohol, with no other apparatus than a watch-glass, would have enabled him to rectify.

But the objection which recurs most frequently, and that upon which the greatest stress is laid, is the minuteness of the quantities of saline matter subjected to experiment. It would appear that Dr. Pearson questions whether a few grains of saline matter may be expected to yield results similar to those which would be obtained from larger quantities; whether, for instance, the same inferences might be drawn from rhomboidal crystals of a minute size, as from similar crystals of larger dimensions;—or, whether experiments tried upon an ounce or two of my dropsical fluids, may be brought into competition with those which he performed upon two or three pints of his ropy sputum?

Such a scepticism, I must own, I have myself never entertained. I have always thought, on the contrary, that the chemical properties which belonged to a particle of matter were exactly similar to those which would be found to belong to a whole mountain of the same substance; that a rhomb of only one hundredth part of an inch might be characterized by its form as distinctly as one a hundred times

times larger *. But I carry the point still further; for I go the length of believing, that many experiments of research may be wonderfully facilitated by analysing upon a small scale—that a great deal of convenience, of œconomy, and sometimes even of accuracy, may thus be gained; and that, in some instances, we even acquire new powers of inquiry by operating upon small quantities †.

Thus, were it not for the assistance of minute microscopic observation, a great number of important facts which have enriched chemistry within the last twenty years, would in all probability have remained undiscovered; and this country might not have obtained that first rank in philosophical chemistry, to which it has but lately been raised, and which it had long held in other departments of science.

Is it necessary that I should specify particular instances? Can any philosopher, attentive to the progress of analytical chemistry, overlook so many discoveries in which neither furnace, nor forge, nor subterraneous laboratories, have been concerned—in which a watch glass, a blow-pipe, and a few drops of chemical re-agents, have been all the instruments required? Were not, for instance, the analyses of the Iceland springs by Dr. Black (the same eminent philosopher to whom Dr. Pearson appeals as an authority against microscopic observations) performed upon quantities of saline matter of astonishing minuteness?—Surely Dr. Pearson cannot have forgotten that it was by the accurate examination of only a few grains of matter, that the nature of no less than five kinds of urinary calculi has been ascertained, and their discrimination rendered easy and certain—that the nature of diamond has been established—that no less than four new metals have been discovered in the crude ore of platina—that the similarity between all the

* Thus. I have no hesitation in maintaining that, unless it be proved that nitrat of potash may crystallize in rhombs, my conclusions respecting the particular point in quest on, would stand upon that evidence alone; or that unless it be shown that carbonat of potash may crystallize in cubes, my inference respecting the presence of muriat of potash stands uncontroverted.

With regard to my attempt at expressing centesimal parts of grains, which is, with some apparent reason, noticed as an instance of singular pretension to accuracy, I beg to observe, that I have never actually attempted to weigh smaller quantities than decimal parts of grains; and whenever smaller fractions have been expressed, they have arisen from a conversion of those numbers to some general standard.

† I would also observe, whilst upon this subject, that there is a degree of neatness gained by reducing the scale of operations, which is often incompatible with processes conducted in the large way: thus, I have never found it necessary, in analysing, to introduce amongst the enumeration of contents “a little dirt,” as some old-school chemists have been in the habit of doing.

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meteoric stones has been proved—that the identity of the chemical agencies of electricity, whether excited by the common machine, or by the Voltaic battery, has been demonstrated—that in a neighbouring country the formation of crystals has been explained upon systematic principles—that amongst us a new and wonderfully accurate instrument of crystallography has been invented—and above all, that the metallic bases of alkalies, those extraordinary bodies which nature had hitherto concealed under an impenetrable disguise, have at last been brought to light ! Let it be remembered as one of the most glorious circumstances of that discovery, that it was by examining mere atoms of these substances that their properties were first ascertained ; and that when, in consequence of subsequent improvements in the mode of obtaining these bodies, they were procured in larger quantities, and their general properties were re-examined, no error was discovered, and no important information was added to that which had originally been gained from microscopic quantities.

It is far from my intention, however, to contend, that, on some occasions, new and important facts may not be brought to light by means of processes conducted upon an extensive scale, which would not admit of being reduced to a small compass. I only mean to assert, that such instances are comparatively but rare ; and that no philippic against the examination of small objects—no appeal to old masters—no slight upon modern improvements, ought to deter chemical inquirers from adopting methods which some of our contemporaries have employed with so much utility and success.

Amongst other inaccuracies in the critique which has given rise to these remarks, my paper on dropsical fluids has been represented as being the joint work of Dr. Wollaston and myself ; for which supposition there was no other authority than a note in the paper in question, in which I acknowledged my obligations to Dr. Wollaston for the information and assistance which I have on this and other occasions, derived from his kindness. I need not say how highly I should have been flattered by such an association ; but I think it due to him to state, not only that he had no share in the general inquiry, but that he did not even see the paper in question previous to its publication.

I cannot refrain from noticing, amongst Dr. Pearson's remarks, another kind of licence which appears to me still less warrantable. I allude to the practice of quoting in italics, or placing between inverted commas, words or phrases

phrases which have not been used, and to seize upon them as a subject of ridicule. This is the case with some proposed *elegant* changes, and with my supposed recommendation to transfer chemistry to the "fire-side of the drawing-room:" expressions which I have not used, and yet upon which Dr. Pearson has thought proper to be extremely jocular.

I have only further to add, that should Dr. Pearson again write upon the subject, I shall not easily be induced to resume the controversy. I am sorry, therefore, to see it intimated at the conclusion of his paper that he proposes to continue his observations in your next Number; and as it appears that those intended remarks are meant as a return for the notice which I have taken of his papers, I regret the more that he should take so much trouble. For praise, when used as the vehicle of irony, is the worst kind of censure. The discovery of truth ought to be the only object of philosophical discussion. There are, doubtless, many errors in my humble attempts at chemical analysis; but unless Dr. Pearson points out those errors, or brings forward new facts connected with my inquiries, I confess that I had much rather he would not again honour them with his notice.

"Quicquid id est, timeo Daedalus et dona ferentes."

I remain, sir, &c. &c.

Russel Square, Feb. 21, 1812.

ALEX. MARCET.

XX. *Description of the Diacatoptron. Communicated by Dr. GIBBES, of Bath.*

VARIOUS optical instruments have been applied to the purposes of drawing and copying, and have been carried to a great degree of perfection. The Camera obscura, the Delineator, and Camera lucida, have severally been justly celebrated for the facility of tracing outlines, and the rules of perspective have been correctly illustrated by these representations of natural objects on a plain surface.

The great simplicity of the instrument now about to be described, may entitle it to rank among the most useful of the kind, as, to the advantage of its simple structure and applicability without straining the eyes, it adds a degree of truth not to be exceeded. Without the aid of additional reflecting surfaces, this instrument represents its objects as an ordinary mirror, at the same time that it allows sufficient transmitted light to pass for the guidance of the hand behind it. Thus, by a very little management, both the
hand