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XXXIX. *Some Experiments on Putrefaction;*
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Read Nov. 7, 1771. **T**HE celebrated lord Bacon [a] has, without doubt, shewn a very great sagacity, in pointing out to posterity, putrefaction, as a subject, worthy of making further inquiries into; and certainly, as there happen daily so many changes, not only in the inanimate, but also in the animate world, carried on by its means; the knowledge of every thing relating to it must clear up a great many points in natural philosophy, not thoroughly understood before. But these inquiries ought to be still of more consequence to mankind, as health depends greatly upon keeping in due bounds putrefaction, which the body naturally tends to. For these reasons, Sir John Pringle deserves, besides his other eminent merits, very great praises, on his having made many experiments on this subject; and medicine is indebted to him for considerable improvements resulting from them. He has besides opened

[a] Nat. Hist. Cent. IV.

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the way to many other gentlemen, among whom excell Dr. Gaber, and Dr. M^rBride, whose numerous experiments shew the ingenuity, and sagacity, they are possessed of: but the subject is not yet exhausted, nor will it be very easily. I have made some experiments relating to it; and should be very glad, if they threw a new light on some points of the greatest importance to medicine.

Dr. Gaber has proved, by his experiments, the presence of a volatile alcali produced by putrefaction; but as he did not discover by the same proceedings [b] any in its beginning or end, though there was a very putrid smell, he denies its existence in these states, and concludes, that this volatile alcali is not a necessary product of putrefaction [c]. This doctrine seemed to me not quite conformable to the phenomena: for, as all smell, as much as we know at least till now, depends on a saline matter, joined with a

[b] Acta Taurinens. Vol. I. p. 78. Cum attegerint summum effervescentiae gradum, continuato ejusdem loci calore effervescentiae vim amiserunt. P. 79. Citius plerumque prodiit foetor, quam alkali, idemque tardius desit. P. 82. Massam inde relinqui foetentissimam, sed emisso alkali ad effervescentiam ineptam.

[c] Id. p. 83, 15. Quum foeteret gravissime residuum distillationis, quamquam omni alkali orbatum, manifestum videtur, ab alkali foetorem exaltari quidem posse, & magis penetrantem effici, non autem ab eodem produci, quandoquidem superest eo sublato—16. Videtur is odor a volatilibus admodum particulis proficisci, sed quae ab alkali dissimiles sunt, plerumque citius gignantur, tardiusque dissipentur—alcalescentia adesse potest medico foetori conjuncta—vicissim maximus foetor absque alcali—Ex quibus differentia inter foetidas alcalinasque partes confirmari videtur.—P.84, 17. Videtur alcali non esse productum necessarium putrefactionis neque gradum alcaloescentiae gradui putrefactionis respondere.

phlogiston, and the saline matter producing the putrid stench, was not very likely an acid; I supposed it to be a volatile alkali, which, involved in phlogistic matter, might fly off, before the alkali was developed. I wanted to know by experiment, if I was right; for this purpose, I put, the 19th of June (the thermometer being 58° of Fahrenheit, and continuing between 58° and 62° all the time I observed), in a pretty large receiver, some beef cut in very small pieces; I covered the bottom with it thinly, and poured upon it water, about two inches high. The 22d, the putrid smell was very sensible: but I let it stand till the 24th, when I poured off the fluid [d], adding again about the same quantity of water to the flesh. I filtrated then the fluid through a piece of fine linen, and mixed with some of it the syrup of violets, which it did not alter; neither did it effervesce with the spirit of vitriol, diluted to a sharpness near that of the vegetable acid. I thought of keeping it in digestion for some days; but, for fear that some little solid particles might have passed through the linen, and by that means, in growing putrid, might give some alkali, and render the trial inaccurate, I distilled the fluid by a heat of about 160° , after which, I repeated the trial with the syrup of violets and the spirit of vitriol; but it produced no

[d] It requires some attention to find out the proper time when to pour off the liquor; if it is done too soon, it will give too little volatile alkali to be much sensible by experiments; for, though it smells strongly, it is known how little matter is required to produce a strong smell. If it is delayed too-long, it shews already signs of an alkali. For that reason, I made many experiments in vain.

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change. I then put it, the 25th, into a retort, fitted to it a receiver, applied to the jointure a ring of paste made of flower and water, covered it with a piece of wet bladder, and exposed it in a *balneum arenæ* to a heat of 108° to 116° , till the 29th of June, when the whole fluid was distilled over. I perceived during this operation, that the liquor, from being quite transparent, grew turbid; the first distilled transparent fluid grew also turbid in the receiver, and at the bottom of the retort there was a small settlement of a whitish earth. The liquor had a particular smell, but quite different from a putrid one, inclining to the volatile alcali; and shewed a slight but sensible degree of effervescence with the spirit of vitriol; and the syrup of violets was turned evidently green by it.

In the mean time, the flesh with the water continued to emit a putrid stench; and the 28th of June I found the fluid colouring the syrup of violets greenish, and shewing a kind of effervescence with the acid. Both these qualities were increasing every day, till the 8th of July, when, on account of a journey, I could not observe it any longer. I had left the mouth of the receiver open; and on my return the 1st of August, I found an exceeding putrid smell; I covered the vessel; and the 2d, examined the fluid, but it did not effervesce any more. I then filtrated the liquor; but the flesh was so rotten, that a great many particles passed through the linen, and rendered it turbid. I put it into a retort, adapted a receiver, and luted it, as before-mentioned; the heat was also the same, between 108° and 116° . In this warmth it continued for about four days, when the fluid was
distilled

distilled over. On opening the vessels, the smell was again entirely changed, not near so disagreeable as before. In the receiver I obtained a fluid, which turned the syrup of violets green, effervesced very smartly with the very same spirit of vitriol I had used before; gave the smell of a volatile alcali, on adding to this the fixed alcali; praecipitated the calces of metals dissolved in acids, and shewed itself by all proofs a true volatile alcali. In the retort remained a yellowish matter, almost without any smell. I put to it some water; and after 24 hours, it gave the herbaceous smell, but shewed no signs of any alcali. I let it stand four days longer: the herbaceous smell continued; but there was no alcali to be discovered. I distilled it with a gentle fire: but neither then did there appear an alcali [e]; and by applying a stronger fire, I got nothing but a kind of empyreumatic oil.

I had poured, the 3d of August, some fresh water on the putrid matter; its putrid smell continued; the 7th I decanted the fluid, filtrated it, and made it undergo the same operation, with exactly the same effect as before; which I did again the 11th, with the very same effect. I did not repeat it oftener, as I had occasion for this putrid flesh to some other purpose.

These experiments shew, I think, that the volatile alcali is present as long, at least, as the putrid smell

[e] What this herbaceous smell did depend on, I did not enquire any farther, as not relating to medicine, since a living body never was found in such a state: but very likely it depends on some volatile alcali, which is perhaps in so very small a quantity as not to be perceptible by experiments.

continues; and that this volatile alcali is the basis of it, because, as this was distilled over, the residuum, being still in intestine motion, got only the herbaceous smell. The reason, why the volatile alcali has been distinctly observed at a certain period of putrefaction, and not in the others, is, I believe, this; the volatile alcali has, it seems, a tendency, to disintangle itself, by intestine motion, of all such matter as it is involved with; but if it is not combined with such fixed matter as retains it till it has gone through all its evolutions, it is, being itself volatile, carried off by the still more volatile phlogistic matter with which it is commonly joined. For this reason, I suppose, the putrefying matter shews in its beginning no sign of a volatile alcali; because its smell depends only on those particles, which have been on the surface, without any strong cohesion with the substance. In the farther progress of putrefaction, the matter involving the alcali, or forming it, is intermixed, and in cohesion with the solid particles of the substance, and is by these means retained till the alcali is come to its purer state. Towards the end of putrefaction, the cohesion of the particles being almost entirely taken off, the volatile alcali is carried off before it can go through all its states.

If it is therefore true, that the volatile alcali is essential to, or at least always present in putrefaction, it seems to follow, that the alcalies never can be used in living bodies, as antiseptics [*f*], for laying

[*f*] It is very difficult, methinks, to account for the antiseptic power of the volatile alcali, and other salts, on dead animal sub-

aside their stimulating quality, which must prevent their use in most of the putrid diseases, they would increase the morbid matter, by being intimately mixed by circulation with phlogistic matter, which they find in abundance in such bodies. It has been objected to this, that the exhalation of stale urine, though shewing a great quantity of volatile alcali, is inoffensive to health [g]: and that some persons have taken the volatile alcali in very great quantity, without its bringing on a putrid disease [b]: but there

stances: I once thought, that as the ammoniac salt, nitre, &c. bring down the thermometer several degrees, perhaps all these salts acted by instantly absorbing the heat produced by the beginning intestine motion; and that, as a certain degree of warmth is necessary to putrefaction, in preventing this degree from coming on, it might hinder the whole operation. To see by experiment how far this might be true, I put into phials a certain quantity of water, with that proportionate quantity of alcalies, fixed and volatile, sal ammoniac, &c. which Sir John Pringle had found (Append. p. xvi. xvii.) to be antiseptic; and in one as much pure water as a standard. I stopped every one of them with a cork, in which I had made a hole for a thermometer of Fahrenheit. I exposed all these phials to the same heat; Sir John had used about 112°; but I found, that both those with the salts and that without it marked the same degree of heat; and that therefore the absorption of heat can by no means be the reason of the putrefaction being stopped. May this phenomenon not depend upon the salts penetrating the body, and giving to the particles more *puncta contactus* (according to their greater or less affinity)? and may not these salts, in augmenting cohesion, hinder the fluids from separating themselves from one another, and, in consequence, prevent intestine motion? Is this not somewhat confirmed by the action of adstringents? and by the most powerful actions of metallic salts, as being of the greatest specific gravity?

[g] Sir J. Pringle, Append. p. vi.

[b] Id. *ibid.* p. xcii.

are however some examples [i], where it has been hurtful. It is urged further, that a person, being only for a short time exposed to really putrid exhalations, may be infected with putrid diseases; and therefore that this effect of putrid exhalations does not depend on the volatile alkali, as it may be taken pure in very large doses, without producing such effects. To this I reply, by an analogous instance; a small quantity of ferment will bring on fermentation in a large mass of fermentable matter, and yet as much acid as could be obtained from the ferment, far from exciting an intestine motion in the fermentable matter, would rather check it; but can it, for all that, be denied, that the involved acid in the ferment is the chief cause of setting the whole mass in fermentation? In the same way, the alkali combined with phlogistic matter may produce such intestine motion as the pure alkali cannot; and very likely the first would not produce it, if the volatile alkali in it could be changed.

To bring this about, the most powerful means seem to be the use of acids; and the most celebrated physicians agree in the good effect they have observed from acids in putrid diseases, and recommend them strongly. Dr. M'Bride thinks otherwise, and his reasons are these: *first* that if the acids came unchanged to the absorbent vessels, they would not admit of them [k]; *secondly*,

[i] Huxham on the sore throat, p. 67, 68. Ejuisd. Essay on fevers, p. 118, edit. 5.

[k] Experiment. Essays, edit. sec. p. 20. The austere acid (generated in the first passages of weakly persons) is exactly in the same state with a foreign acid, for the lacteals will admit none of it.

if they did, they would be dangerous [1]; and *thirdly* that they are quite changed, before they leave the *primæ viæ* [m]. As for the *first*, I do not know what reasons Dr. M'Bride founds his assertions upon, as acids never are given in so concentrick a state, as by their astringency to make these vessels shut up their orifice; and as metallic salts themselves are absorbed in their very compound state (which seems clear with regard to the corrosive sublimate, and other such saline preparations), I do not see, why the simple acids could not be absorbed. The *second* reason seems to be founded upon some of Dr. M'Bride's experiments (p. 132, 133), *viz.* that putrid flesh, sweetened by distilled vinegar and spirit of vitriol, was firm; but on being boiled went quite to pieces, whereas that sweetened by volatile alcali did not. But, I conceive, these experiments are not applicable to a living body: for the acid being there mixed with the fluids, cannot act in this way on the solids, till the fluids are (if I may use that

[1] Ibid. p. 134. the acids dissolve the elementary earth, and thus destroy the texture of that substance, whose soundness they are supposed to restore.—P. 148. we are not to expect, that they are to pervade the minute branches of the vascular system; when indeed it is evident, that they ought not to be allowed to pass into the blood in their acid form; since it is plain, that, from their dissolvent nature, the body must be destroyed, and its most solid parts melted down to a jelly, if naked acids were to be received into the general mass of fluids.

[m] Ibid. p. 148. acids are neutralized during the alimentary fermentation; and therefore they cannot act as acids, by saturating any thing of the alkaline kind that they meet with in their course of circulation.

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expression) supra-saturated with the acid [n], which in putrid diseases cannot be the case. And farther, a heat of 212° of Fahrenheit never can increase the action of the acids in living bodies, as it did in the experiments; for, though Dr. M'Bride denies this consequence, and will prove the contrary, as the flesh with the alcali did not dissolve; yet this circumstance proves nothing more, than that the volatile alcali has not such power of dissolving the gluten of animal fibres, as acids have; for, if the effect depended only on the action of the acids by themselves, the flesh would rather have been dissolved when immersed in them, than when boiled in water.—The Doctor besides seems not quite consistent on this head; for, p. 151, he says, “Astringents can only “be of importance in those cases, where, from “extreme relaxation and resolution of the solids, the “dissolved fluids are suffered to transude, and either “form spots of different hues, or run off by actual “hæmorrhage; here, indeed, the acid of vitriol, as “an astringent, not as an acid, is found of great use “in gaining time.” As the acid could not exert its astringent power on the vessels, without coming to the *secundæ viæ* (p. 153.) he seems not afraid, in this case, of its melting down the most solid parts to a jelly.

In proof of his *third reason*, he alledges some experiments; *viz.* the third, p. 40, where a mixture of *flesh*, bread, lemon juice, and saliva, did not effervesce, after fermentation with an alcali; and the 5th,

[n] This has, it seems, happened in some rare cases quoted by Dr. M'Bride, and Dr. Haller, p. 148.

p. 42, where a mixture of bread, water, saliva, and spirit of vitriol effervesced smartly, before the intestine motion; but not at all after it. I could object against these experiments, and especially the 5th, that perhaps the proportion of the saliva to the acid was too great, and that a person in a putrid disease ought to take more acids than could be neutralized by the inquiline liquors. However, I will not insist on this; and suppose these experiments to be quite applicable to the case: but if these mixtures do not effervesce any more, does it follow, “that they are neutralized, and therefore act as acids, by saturating any thing of the alcalinous kind, that they meet with, in their course of circulation?” There are some saline bodies, which do not effervesce when mixed together; which will, however, change one another’s nature. Thus *f. e.* brimstone, mixed with a strong fixed alcali, does not effervesce [o], but changes, on being dissolved, the nature of the alcali. A solution of soap does not effervesce on the addition of an acid, but joins with the acid, and neutralizes it. These instances made me suspect the conclusion drawn by Dr. M’Bride from his experiments; and to clear up these doubts, in this particular case, I referred to experiments. For this purpose, I mixed, the 4th of August, the thermometer being at 64°, three ounces of saliva, a dram of the liquor of

[o] This applies also to the solution of brimstone in limewater, out of which the lime particles have been precipitated, by the introduction of fixed air.

putrid flesh, and a very small quantity of bread : and added as much of the diluted spirit of vitriol, as to make it four, and effervesce definitely with the alkali. There was not any sign of intestine motion till the 7th of August, when from time to time some air bubbles, and also some solid particles, rose to the top ; and this continued till the 8th. Not perceiving any farther motion, I poured off the clear liquor, which did not effervesce any more with the alkali. I mixed, the 9th, six drams of the putrid liquamen, with about the double of this liquor, and put in besides four solid pieces of flesh, which had lain three days in the liquamen : these pieces were of a prodigious stench, and so rotten, that with the least force they were torn to pieces. There appeared no signs of intestine motion : the 10th, the putrid smell was very much abated : the 11th, it was changed, and there remained only a smell much like that of sound flesh : the pieces were without any smell, and had acquired again some degree of firmness. In this condition they remained for a week, and I did not observe them any longer.

This experiment proves, I believe, that acids, though changed in the alimentary canal so far, as not to effervesce with alcalies, may notwithstanding check putrefaction ; and that, therefore, their use is of great consequence, and ought not to be omitted in putrid diseases. Though Dr. M'Bride believes that these diseases may be cured with fermentable substances only ; I must own that I do not agree with him, and am not quite convinced of his opi-

nion, that putrefaction depends only on the loss of fixed air. I rather believe this an effect than the cause of putrefaction ; but I shall refer this subject to another occasion.

END OF PART I.