Mineral constituents :

Chlorine	...	...	...	...	.29
Sulphuric acid	...	...	...	...	3.19
Phosphoric acid	...	...	...	...	2.41
Soda	...	...	...	...	2.47
Potash	...	...	...	...	3.53
Lime	...	...	...	...	.27
Magnesia	...	...	...	...	.13
Loss	...	...	...	...	.20
					12.49

59.45

Total Quantity of Solids excreted during the Twenty-four Hours.

Urea	...	...	73.140 grammes (1128.55 grains).
Uric acid	...	...	1.272 " 19.59 "
Other organic matters	...	...	2.702 " 41.69 "
Chlorine	...	...	.477 " 7.36 "
Sulphuric acid	...	...	6.247 " 96.43 "
Phosphoric acid	...	...	3.975 " 61.25 "
Soda	...	...	4.070 " 62.80 "
Potash	...	...	5.883 " 90.72 "
Lime	...	...	.429 " 6.63 "
Magnesia	...	...	.207 " 3.18 "

Food Consumed.—9 oz. meat (lean of mutton chop and cold roast beef); 9½ oz. bread, made into dry toast; 14½ oz. oatmeal, made with milk and water into gruel; ½ pint jelly; ½ oz. mixed tea; 2¾ oz. coffee; 8½ oz. sugar; 2 oranges; 2 lemons; 4 yolks of eggs; 2 lbs. grapes; 1½ oz. prunes, eaten as such; 3 pints milk; 29 Osborne biscuits (Huntley and Palmer); 2 pints beef-tea, made from fresh meat.

Twenty-four Hours' Period, commencing Ten Hours subsequent to the completion of Weston's Seventy-five Hours' Walk (9 A.M., February 26th, to 9 A.M., February 27th.)

Amount of urine passed, 851.9 cubic centimetres (30 ounces); specific gravity, 1.031.36; free acidity, reckoned as oxalic acid, 1.19 grammes in 1,000 cubic centimetres (.52 grains in one ounce); dark in colour; copious deposit of lithates. Microscopic characters: Amorphous lithate of soda, with a few crystals of oxalate of lime.

Composition in 1,000 parts :

Water	...	...	...	922.00
Solid residue, after exposure to 240 deg. Fahr.	...	...	...	78.00

Organic constituents :

Organic constituents:				
Urea	...	...	...	43.88
Uric acid	...	...	...	1.31
Other organic matters	...	...	...	18.20
				63.39

Mineral constituents

Chlorine	...	...	...	4.31
Sulphuric acid	...	...	...	1.87
Phosphoric acid	...	...	...	1.76
Soda	...	...	...	4.57
Potash	...	...	...	1.74
Lime...	...	...	...	.65
Magnesia	...	...	...	.35
Loss ...	...	...	...	.36

78.00

Total Quantity of Solids excreted during the Twenty-four Hours.

Urea	...	...	31.950 grammes (492.99 grains).
Uric acid	...	...	.954 " 14.66 "
Other organic matters	...	...	15.99 " 246.75 "
Chlorine	...	...	3.152 " 48.60 "
Sulphuric acid	...	...	1.363 " 20.98 "
Phosphoric acid	...	...	1.278 " 17.75 "
Soda	...	...	3.323 " 51.23 "
Potash	...	...	1.269 " 19.44 "
Lime	...	...	.477 " 7.40 "
Magnesia	...	...	.256 " 3.86 "

Food Consumed.—February 26th, 10.30 A.M. (no food having been previously taken since completion of walk), 8 tablespoonfuls of baked haricot beans, 4 oz. salt pork, 1 rissole of salted codfish and potatoe, 3 biscuits, 3 buckwheat cakes, 2 breakfastcupfuls of coffee, with milk and sugar; 2.30 P.M., 1 plate of mock-turtle soup, boiled mutton and caper-sauce (moderate quantity), potatoes, cauliflower, 2 oz. bread, cup of cold coffee, with ice and sugar; 8 P.M., 7 tablespoonfuls of baked

haricot beans, 2 oz. salted pork, 2 rissoles of salted codfish and potatoe, 3 oz. steak, asparagus on toasted bread (about 6 oz.), 2 preserved peaches, 2 oz. bread spread with butter, 2 cups of coffee, with milk and sugar; 10.30 P.M., cup of coffee, with milk and sugar. February 27th, 1 A.M., 1 orange, ½ lb. grapes, ¼ pint jelly.

A THEORY OF ASPHYXIA: A REPLY TO MY REVIEWER.

By P. BLACK, M.D., F.R.C.P.,
Physician to St. Bartholomew's Hospital.

I FEEL obliged by Dr. G. Johnson's notice of my lecture, and particularly by the fairness with which he has stated my theorem, having given it indeed in my own words.

The point on which I am at issue with the most eminent writers on the subject of asphyxia may be very briefly stated—they, on the one hand, connect it with the privation of oxygen (Reid, Carpenter, Huxley, Draper, and others); I, on the other, maintain that the true solution of the phenomena will not be found in the domain of chemistry, but that it is intimately connected with the heart's physiology; that it is indeed a mechanical question, and, moreover, that it very beautifully illustrates nature's method of ensuring the blood's aëration, by making this result to depend on the respiratory movements, without which the pulmonary heart cannot force the blood through the lungs.

The physiologists above referred to say, "Aërate the blood, otherwise it is impeded in its passage through the capillaries". I answer "No; give only respiratory movement, and venous or unchanged blood will pass with perfect freedom through these vessels, as freely indeed as arterial blood, even up to that time at which the medulla oblongata becomes narcotised, when other considerations intervene."

It is not necessary to experiment on dogs to show this, for we have crucial experiments of hourly operation, which are conclusive on the subject. Will anyone maintain for a moment, that nitrous oxide is able to satisfy the blood's requirement of oxygen? Does not every chemist tell us that this gas is a stable compound at a much higher temperature than that of animal life, and that it even resists decomposition when tested by ignited sulphur in a deflagrating spoon? Yet this gas, not being irritant, can be breathed with perfect safety, for a certain time, during which the blood is wholly unchanged—no obstacle is offered to its freed passage through either pulmonary or systemic vessels, and if death should perchance arise from its inhalation having been pushed too far, it will not occur in the manner which is more properly called "asphyxia", but rather in that which would be designated as "narcotism".

Some able physiologists have certainly fallen into error on this point, for Professor Draper recommends that nitrous oxide should be given to recover from drowning, whereas, from a chemist's point of view, this gas would be as objectionable as carbonic acid itself. Nothing, I submit, can be more satisfactory than the proof we obtain from the inhalation of this gas, that the phenomena of asphyxia cannot justly be ascribed to the non-aëration of the blood, which causes its stagnation in the vessels, whether pulmonary or systemic.

In all matters of physiological inquiry there are experiments and counter-experiments; and though truth must always be consistent, it is not easy in every case to harmonise facts, which, though not in reality, still seem to be in contradiction. Now if it be proposed to an unbiassed mind to make choice of two experiments, devised to try the point, whether venous blood is obstructed at the capillaries or not—which of the two following would appear best suited for the purpose? one in which a gas is freely breathed, but which does not oxygenate the blood, but leaves it venous as before; the other consisting of complicated machinery (bladders, barometers, stop-cocks, etc.) which allow other considerations to come in, and those too of the greatest importance in regard to the very point which is in question.

I find nothing in my able commentator's remarks to affect my position, which is this: "allow respiratory movement, and the unchanged blood will meet with no obstruction in the capillaries; withhold the movement, and the systemic ventricle will be starved of its supply, and asphyxia will thus result". The effect of arrested inspiratory movement is instantly observed in the swelling of the superficial veins of the neck. Let a person hold his breath for a few seconds only, as in passion or its imitation on the stage, and the very commencement of this state is seen: in the more advanced state it may be perceived in the exertions of the athlete: in the deferred inspirations of whooping cough; and during their absence in a fit of epilepsy. Another able writer besides Dr. Johnson has doubtless had Dr. Reid's experiments present to his mind in offering an adverse criticism on my theory. He

says, "had Dr. Black been acquainted with these, he would find that the 'facts' he adopts as his premises have been long since proved by direct experiment not to occur during asphyxia".

But what are these direct experiments which controvert my facts? He thus refers to them: "thus the blood may be seen in the *opened* thorax to pass freely through the lungs for some time after the respiratory movements have ceased, etc." I have italicised the word "*opened*", for the whole gist of the controversy lies here. My reviewer, like many others (I will not impute this to Dr. Johnson), seems to think and believe that what takes place in the *opened* thorax, must necessarily take place in the same chamber when *closed*. But the very point to be ascertained is what takes place in the chamber *on condition* of its being closed, and the vivisector with curious infelicity lays it open to ascertain the point, and my critic has perhaps fallen into this pit, and accepted his logic.

Dr. Johnson, preferring Dr. Reid's theory to my own (I am not answerable for the accuracy with which he has given it), says, "Dr. Reid clearly demonstrates that the first impediment to the circulation consequent on the exclusion of air from the lungs occurs, *not in the pulmonary, but in the systemic vessels*". Surely this is contrary to the generally received facts. Have not all observers testified to the *post mortem* proof of asphyxia, as consisting in distension of the right chambers of the heart, and absolute or comparative emptiness of the left? But how could this occur if the systemic capillaries refused passage to the blood? Should we not on such an hypothesis expect to find distension of the arterial system? The blood cannot pass onwards, retreat is obviously impossible, it must therefore remain incarcerated. If I admitted this supposed fact, I would give consideration to Dr. Johnson's ingenious explanation of it, but meanwhile I am excused from attempting this.

Passing from the subject of asphyxia, I go on to the very brief remarks that are offered on my theory of the *thin* pulmonary ventricle; and in the first place I notice what my reviewer is pleased to say about Dr. H. Davies' dismay on my believing an equality to exist between the openings of the pulmonary artery and the aorta. I feel quite certain that Dr. H. Davies will neither raise the eyebrow nor curl the lip on reading what I have said on this subject. I was not indeed acquainted with his excellent and instructive essay, until my attention was thus called to it, but having now read it, it is an agreeable duty to acknowledge his philosophic handling of the subject. But I must not be understood as consenting to all his inferences. Whilst there are some points which we hold in common, there are others on which there is a radical difference between us; and I should be quite willing to accept Dr. Davies' measurements in preference to my own, and still draw the same conclusions, which indeed are rendered rather stronger than weaker by this emendation.

THORACENTESIS: THE PLAN OF CONTINUOUS ASPIRATION.

By F. CRESSWELL HEWETT, A.M.D.

IN cases of empyema, where the cavity has been evacuated by the aspirator, etc., it frequently refills and has again to be emptied, the secretion of pus taking place from the thickened and vascular pleura, parietal and pulmonary, forming the walls of the cavity. In these cases, the removal of the fluid appears to determine an unfavourable result, for, the pressure being removed suddenly from the vessels in the walls of the cavity, transudation takes place, which is followed by the formation of pus, and consequently leaves the patient in the same condition as he was previously as regards the existence of the cavity, while he is greatly debilitated by the loss of the nutritive elements forming this new supply of pus. Beyond this, the occurrence of hæmorrhage into the cavity after its evacuation is by no means rare; and, in the general debilitated state in which these cases must necessarily occur, such loss of blood *per se* is sufficient to cause grave anxiety.

I think that, by the mode of procedure here described, such chances of the repeated filling of the cavity with pus or of hæmorrhage, may be materially lessened. The operation is briefly this: paracentesis being performed with an ordinary trocar and cannula, a caoutchouc tube is passed through the cannula into the empyemic cavity; the cannula is then extracted, and the outer end of the caoutchouc tube is attached to a glass tube piercing the cork and reaching to the bottom of a vessel containing a weak solution of Condy's fluid, or other antiseptic. By gently lowering and raising the bottle, the cavity is washed out, and by changing the first bottle for another and similar one, the cavity is evacuated of pus and filled with the antiseptic solution; thus, the pressure on the vessels in the walls of the sac is not much if at all

altered, and hæmorrhage and transudation are prevented. The amount of fluid allowed to remain in the cavity is lessened day by day by increasing the syphon-action of the apparatus by keeping the bottle at a lower level each day than the day previous. The thoracic and pulmonary pleura gradually approximate and coalesce.

To say the least of this mode, abroad, in many cases where it is necessary to evacuate pus from the thorax, recourse must be had to the use of instruments which, in the great centres of advancement, are looked upon as almost primitive, and to their use the above method cannot add any complication; while, from the results of two cases, as far as the operation went, which were treated upon this plan, hope appears of doing something more for the patient than is frequently attempted or possible.

OBSTETRIC MEMORANDA.

VICISSITUDES OF LABOUR.

OCCASIONALLY patients give accounts of the practice of their previous medical attendants, which are remarkable only for the misconceptions they betray. The following history of successive labours, however, scarcely admits of that explanation, extraordinary as it is; and it occurs to me to be interesting enough to be placed on record. I was requested to deliver the patient on the last occasion mentioned below, and she subsequently gave me the following information. In her first labour, which occurred when she was 28 years old, she was attended by a student from a neighbouring hospital. At the instance of her friends, the resident accoucheur was twice summoned during the first twelve hours; but he saw no occasion to interfere. At the eighteenth hour, a neighbouring practitioner was called in, who expressed the opinion that the cause of delay consisted in a contraction of the pelvis, and he at once proceeded to deliver with the forceps, which, he thought, might have been advantageously employed much earlier. On the second labour, the same surgeon was engaged to attend. It was conducted by his brother, who, at the thirty-fourth hour, attempted delivery with the fillet; but, he failing, the former came to the rescue with the forceps, with which he extracted a moribund fœtus, which subsequently died. Appalled by her previous experience, and entertaining the idea that delivery might be effected in some other manner, in the seventh month of her third pregnancy she sought a new adviser. She was not disappointed; for this gentleman, without making any previous vaginal examination, promptly decided that the induction of premature labour was necessary, and within a few days performed the operation. Labour came on; the feet presented, and the body was born without assistance; but, in order to deliver the head, it was deemed necessary first of all to reduce its bulk by craniotomy. These vicissitudes convinced the patient that there must be something very serious in the condition of her pelvis, which appeared to offer greater obstacles to delivery with each succeeding labour; and on the fourth occasion she felt she would be scarcely safe but in a lying-in hospital. She obtained a bed, and at the full term labour commenced; but, instead of having a harder time of it than ever, she was delivered in the course of eight hours of a well grown female child without any assistance whatever. Such were the peculiarities of her pelvis. On the fifth occasion, encouraged by her last experience, she determined to lie-in at home, and engaged a midwife to attend her. All was again changed. The breech presented, and, after labour had continued for eighteen hours, the midwife made strenuous efforts to assist delivery. These failing, I was sent for, and, of course, I was informed that the child was "jammed" in the pelvis. The patient was suffering very severely, either from the protracted labour, or (as she said) from the interference to which she had been subjected. On making an examination, I first of all discovered an extraordinary gap in the foetal perineum; and, on making some remark upon it to the midwife, she volunteered that she was afraid the child's leg was broken. Such turned out to be the case. Although the pains were fair, and promising to expel the child in a reasonable time, the mother's condition made it necessary to deliver at once; and that, with the assistance of a ligature passed through the groin, was very easily effected, for the pelvis presented no abnormality whatever. On examining the child, I found that the midwife, mistaking the breech for the bag of waters, had thrust her finger through the perineum, producing a gaping wound an inch and a half deep. Much force must have been used to effect this; for the wound did not involve either the vagina or the rectum, so that the skin must have been penetrated. The right thigh-bone was fractured about its middle. The child perfectly recovered from both these injuries; but the mother suffered from a severe attack of fever of the milder kind, and was not well for two months.

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