only in a very limited sense be considered as pathological; it is, indeed, "well developed," and yet this very stage of great development may lead to its stillbirth or death. The large head cannot pass through the maternal passages without suffering such a degree of compression as leads to intracranial hæmorrhage, with all its results—asphyxia neonatorum in some cases, cerebral palsy in others. Again, the shoulders may be so broad as to cause delay in the labour after the birth of the head, and thus stillbirth may for this reason also be the consequence. Further examples of this variety of potential morbidity might be given, but enough has been said to show that, in many instances, conditions of the fœtus which were harmless *quâ* the fœtus become dangerous, if not fatal, *quâ* the newborn or young infant. There are other reasons for the great mortality which takes place at, and immediately after, birth; but the one above mentioned is of great importance, and must not be overlooked.

It must not, however, be forgotten that there is another side of the question. Some conditions which may be the cause of great danger to the fœtus *in utero* cease to be so the moment the child emerges from the maternal passages. Not long ago I saw an infant which had been born into the world puny and weak. The cause was looked for, and was found in the presence of a partially diseased placenta. After birth the infant with care lived and throve. The threatening and dangerous intrauterine condition was removed by the act of ligaturing the umbilical cord, and so separating the infant from the placenta. The defective supply of nourishment and the inefficient respiration were replaced by the normal extrauterine digestive and pulmonary functions, and the change was beneficial.

I have dissected a full-time infant which had evidently died a few days before birth from a tight knot on the umbilical cord, for mercury could be made to pass through the arteries but not through the vein. Now, had the tightening of this knot not occurred, the fœtus would in all probability have been born alive, for it was otherwise healthy. The real morbidity of the fœtus, then, may be checked or abolished by the change from an intrauterine to an extrauterine existence. This may not always, or indeed often, occur, but that it does happen sometimes is, I think, not doubtful.

This whole subject is capable of great expansion; a result which can only be accomplished by patient research and the systematic examination of stillborn and deadborn fœtuses; and the making of *post-mortem* examinations of cases of stillbirth would be greatly stimulated by their compulsory registration. What Morgagni said long ago is still to a large extent true. He remarked that "a wide and almost unbeaten track lies open for the investigation of the diseases of newborn infants;" and a very recent and striking instance of the truth of his statement is to be found in the appearance of such papers as that of Dr. Herbert R. Spencer on "Visceral Hæmorrhages in Stillborn Children."[1] In this contribution to infantile pathology, the prediction made more than fifty years ago by Dr. Stokes has been amply fulfilled. He said: "I believe that anyone who has the opportunity of dissecting a great many stillborn children, or of those who die immediately after birth, would, by examining the state of the different cavities, and publishing the results of his examinations, earn for himself very great reputation."

THE African Steamship Company have received an intimation that during the recent voyage of their steamer *Angola* from Liverpool to the West Coast of Africa, Dr. Jackson—the surgeon of the ship, who was 28 years of age and a graduate of Dublin University, and was a son of Sir Robert Jackson, of Dublin was drowned in the Bay of Biscay. It is stated that he met his death while endeavouring to alleviate the sufferings of an injured fireman. The weather was terrible, and everybody aboard the steamer had been cautioned against the danger of going aft. A fireman sustained an injury and was carried to the saloon, where the surgeon attended him. Requiring medicine for the sufferer, Dr. Jackson, in spite of warning of the risk he ran, attempted to reach his own quarters aft to procure it. He was never seen again, and was undoubtedly washed overboard and drowned.

[1] *Lond. Obst. Soc. Trans.*, xxxiii, p. 203, 1891.

REPORTS
TO THE
SCIENTIFIC GRANTS COMMITTEE

REPORT ON EXPERIMENTS UPON "ABSORPTION WITHOUT OSMOSIS."

BY E. WAYMOUTH REID, M.B.CAMB.,
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As a result of a previous series of experiments,[1] in which the course of the osmotic transfer of fluid across the skin of the frog was studied, it was concluded that a satisfactory explanation of the observed facts could only be given upon the assumption that there is present in the living skin of the frog a vital absorptive force dependent upon protoplasmic activity and comparable to the vital secretory force of glands, and that by virtue of this vital action the skin is actually able to cause a stream of fluid to pass from its outer towards its inner surface. This conclusion was based upon the following facts: first, the magnitude of an ordinary osmotic stream maintained through perfectly fresh frogs' skin by means of fluids whose injury to tissue life is minimal, is capable of variation in the direction of increase or decrease, by such experimental conditions as are known to exalt or depress the activity of living matter; secondly, given a physical diffusion current in the direction from without inwards through the living frogs' skin, the presence of a stimulant to protoplasm augments the stream, while a depressant diminishes it; while on the other hand, should the diffusion current have been set up in the reverse direction, that is from within outwards, the stimulant causes diminution and the depressant augmentation of the amount of fluid transferred from the inner to the outer side of the skin in a given period of time. These phenomena failed to manifest themselves when dead skin was used for experiment instead of that which had been but recently removed, and which was therefore still in a living condition.

In the series of experiments just referred to, the variations that could be induced in an artificially-produced stream of fluid through the frog's skin by alteration of the physiological conditions were observed and recorded. Obviously the evidence of the existence of vital absorptive force would be far stronger could one put *osmotic* transfer of fluid on one side, and actually observe the passage of fluid across the living skin *by virtue of its own unaided activity.*

It is the object of the present communication to detail briefly a method by which this may be effected.

Apparatus.—The form of apparatus finally employed was a modification of one originally devised by Matteucci and Cima[2] for some osmosis experiments. Its general appearance is reproduced in Fig. 1.

The apparatus consists of two exactly similar parts, which can be adapted to one another and securely fixed by means of small brass clamps. Each half consists of a small glass cylinder (A) of 30 c.c. capacity, provided with an ebonite flange at its open end for purpose of adaptation to its fellow. Near the closed end a fine glass observation tube (D) is fused in, having a length of some 8 centimetres, and of such calibre that 1 millimetre has a capacity of 1.64 cubic millimetres. In a plane at right angles to that in which the observation tube lies, a neck is set for the introduction of fluid; this neck is stoppered by the thermometer (C). The ebonite flanges are accurately planed, so that when the two halves of the apparatus are clamped up the joint is watertight. The whole apparatus is fixed to a solid stand by means of a brass clip gripping one of the cylinders. To test the apparatus, the open ends of the cylinders are closed by a layer of *baudruche* which has soaked for twenty-four hours in normal saline solution. The two parts are then clamped together, filled with normal saline, and the thermometer stoppers inserted after all air bells have been cleared out. The instrument is then set with the observation tubes horizontal, and the utmost care taken in the final levelling. Finally, ivory scales, graduated in half millimetres, are affixed to the observation tubes, and the position of the meniscus in each tube noted. If the apparatus is truly level, and the *baudruche* well soaked so as to avoid imbibition error, the level of the fluid in the observation tubes is found to remain unchanged for an indefinite period. A temperature error, of course, exists, but after repeated trial I have found that a rise of 4° C. only produces an alteration of 1 millimetre in the level of the meniscus. The area of the opening in the ebonite flange, cemented round the open end of each glass cylinder is about 452 sq. mm.

[1] *Journal of Physiology*, vol. xi, p. 312.
[2] *Ann. de Chimie et de Physique*, t. xiii, 1845.

This apparatus affords a means of holding an experimental living membrane vertical between the two ebonite flanges, and supported by an inert animal membrane bathed with normal saline solution; further, if any transfer of fluid should take place from one side of the experimental membrane to the other, it will make itself evident and capable of measurement by an increase of the volume of fluid in one cylinder, gauged by the rise of fluid in its attached observation tube, and a corresponding diminution in volume of the fluid in the other cylinder, estimated in the same manner. By means of a small syringe, with screw piston rod, and a suitable arrangement of tubes fixed into the thermometer-necks of the cylinders, and provided with stopcocks, it was found that, when actual transfer of fluid from one side of the layers of *bavdruche* to the other was effected, the fall in the column of fluid in the observation tube on one side was exactly equal to the rise upon the other side. The pressure upon the two sides of the membrane is obviously the same, and is, moreover, not subject to variation with transfer of fluid from one side to the other. In the experiments the procedure was as follows:

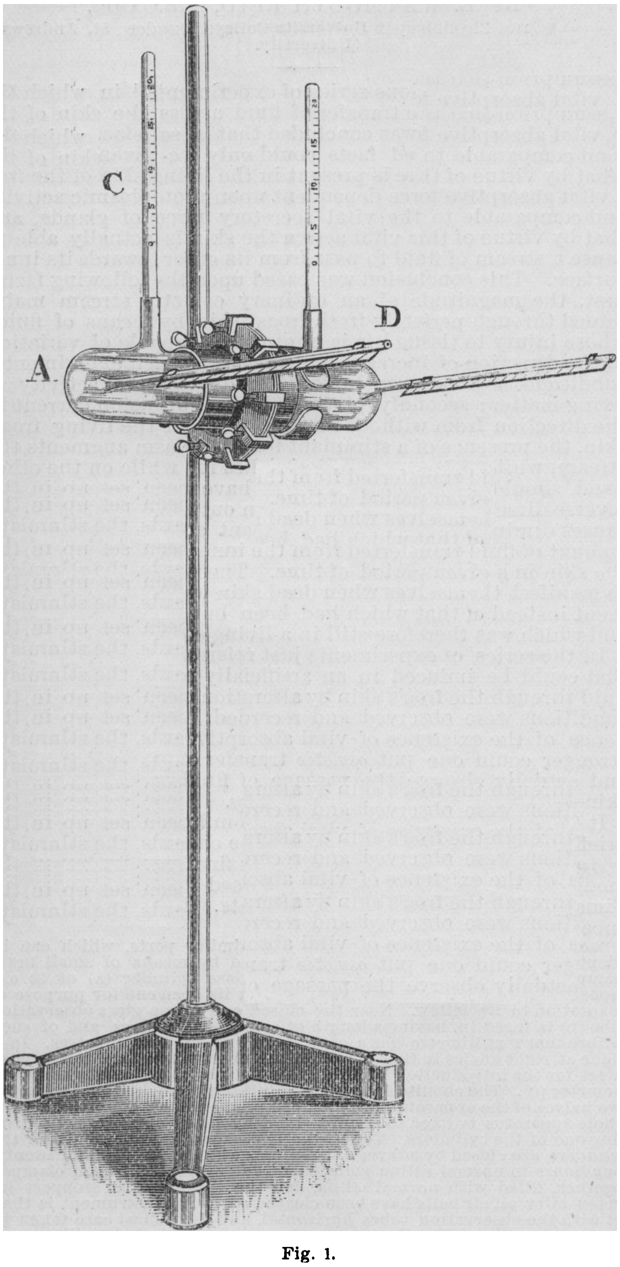


Fig. 1.

The ends of the cylinders having been closed with *bavdruche* that had soaked twenty-four hours in normal saline solution, a frog was killed—rapidly flayed—the skin washed in normal saline solution, and cut to such a size that it was just held by the edge of the flange on each side, when the two halves of the apparatus were joined together; this last precaution was taken to avoid the expression of fluids contained in the tissues of the skin by the pressure of the flanges, as much as possible, for such fluid would find its way into the cylinders and vitiate the readings of the observation tubes. The skin having been introduced between the two soaked membranes, the whole apparatus was clamped up securely, and the two cylinders filled up with normal saline; finally, the thermometers were introduced after all air bells had been excluded, the whole carefully levelled, and the fluid in each tube adjusted to a suitable zero by means of a fine pipette.

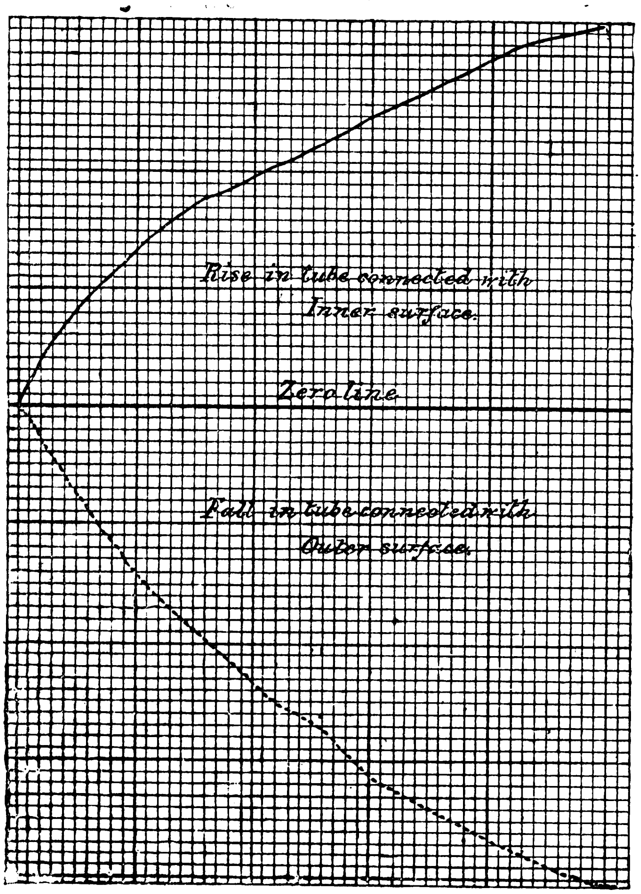
When freshly-removed still living frog's skin, bathed upon both sides with normal saline solution, is subjected to observation in this apparatus, it is found that, as a rule, the volume of the fluid in the cylinder into which the inner surface of the skin faces increases, while that of the fluid in the other cylinder decreases; the column of fluid in the observation tube of the former cylinder rises, that in the tube of the latter falls.

In the following table are given the figures in a typical case. The duration of this experiment was about eight hours and a-half, and readings of the two tubes and the thermometers were taken at intervals of ten minutes throughout this period. The total temperature variation during the whole period amounted to only one degree Centigrade, being, in fact, from 6° C. to 7° C.

1. Intervals of 10 minutes.	2. *Rise* of Fluid in mm. in observation tube of cylinder into which *Inner Surface* faced.	3. *Fall* of Fluid in mm. in observation tube of cylinder into which *Outer Surface* faced.	4. Temperature Variation in degrees Centigrade.	1. Intervals of 10 minutes.	2. *Rise* of Fluid in mm. in observation tube of cylinder into which *Inner Surface* faced.	3. *Fall* of Fluid in mm. in observation tube of cylinder into which *Outer Surface* faced.	4. Temperature Variation in degrees Centigrade.
1	2.0	1.5+	0.1	27	0.5—	1.0	—
2	2.0	1.5+	0.1—	28	0.5	1.0	—
3	1.5	1.0+	+	29	0.5	1.0	0
4	1.5	1.5	0	30	0.5	0.75	0
5	1.0+	1.5	0	31	0.5	0.75	0
6	1.0	1.0+	+	32	0.5—	0.5	0
7	1.0	1.5	0	33	0.5	0.5	0
8	1.0	1.0+	0	34	0.5—	0.5	0
9	1.0	·1.5	+	35	+	0.5+	0
10	1.0	1.0+	+	36	0.5—	0.5	—
11	1.0	1.0	+	37	+	0.5	—
12	0.5	1.0	0	38	0.5—	0.5+	0
13	1.0	1.0	+	39	0.5—	0.5—	0
14	0.75	1.0—	+	40	0 5—	0.5	0
15	0.5	1.0—	0.1—	41	0.5—	0.5	0
16	0 5—	1.0	0.1—	42	0.5—	0.5	0
17	0.5	1.0	0.1	43	0.5—	0.5	—
18	0 5—	0.75	0.1—	44	+	0.5	+
19	0.5	1.0	+	45	+	+	+
20	0.5	1.0—	0	46	+	+	+
21	0.5—	1.0	—	47	+	+	0
22	0.5	0.5	—	48	+	+	0
23	0.5	0 5	—	49	+	+	0
24	0.5—	0.5+	—	50	+	+	—
25	0.5—	0.5	0.1—	51	+	+	—
26	0 5—	0.5+	0.1—				

The sign + or — after a thermometer reading in column 4 lenotes that the temperature variation was slightly greater or slightly less as the case may be, than one-tenth of a degree C., the smallest graduation on the thermometer used. The temperature variations up to the end of the 19th period of 10 minutes are increments; from thence to the end of the 43rd reading they are either *nil* or decrements; the final variations are slight increments if marked +, slight decrements if marked —.

A graphic representation of these figures (excluding the thermometer variations, which, in this case, are so slight as to be negligible) is reproduced in Fig. 2, where the abscissæ are periods of ten minutes, and the ordinates represent 1 mm. rise or fall of fluid in the observation tubes on the two sides of the apparatus. The curve *above* the zero line represents the rise of fluid in the observation tube of the cylinder into which the *inner surface* of the skin faced, that *below* the zero line the contemporaneous fall of fluid in the tube attached to the cylinder into which the *outer surface* faced.



2.—*Abscissæ*: 1 division = 10 minutes. *Ordinates*: 1 division *above* zero line = *rise* of 1 mm. in column of fluid in tube of cylinder connected with *inner* surface of skin. One division *below* zero = *fall* of 1 mm. in column of fluid in tube of cylinder connected with *outer* surface of skin.

It will be noticed from the above figures and curves—(1) that the rise of fluid in the tube connected with the inner surface of the skin is fairly balanced by the fall in the tube connected with the outer surface; (2) that the rapidity of rise and fall respectively gradually decreases with time. It appears to me that the only satisfactory explanation of this result is that the normal saline solution is actually removed from the cylinder into which the outer surface of the skin faces, and transferred across to the opposite cylinder facing the inner surface. Only upon such a supposition can one explain the correspondence which occurs between the fall on the one side and the rise on the other. Error from capillary currents, evaporative removal of fluid, or actual leakage I am convinced cannot occur in the apparatus. Filtration is excluded because the skin is vertical, and the pressures on its two sides identical throughout the course of the experiment; indeed, I have at times put a pressure against the normal direction of fluid transfer, by tilting the apparatus on one side, without any marked effect. A curling-up of the piece of skin is hardly possible, seeing that its edges are clipped by the ebonite flanges, and even if such a curling did occur its effect upon the fluid in the tubes would be the opposite of that observed, for frog's skin in normal saline solution curls with the outer surface outwards, which would lead to rise in the tube connected with the cylinder into which the outer surface faces and fall on the opposite side, the reverse of what happens.

The only other source of error, as far as I can see, would be a combination of ordinary imbibition on the outer side, coupled with tissue rigor and an expression of the fluids in the tissue sponge, the latter making itself more evident upon the inner side. I have traced the imbibition curve of the outer surface in dead frog's skin, and I find it in no wise resembles the curve of the fall of fluid as shown in Fig. 2; it is nearly a straight line.

Fig. 3 is a representation to the same scale as Fig. 2 of the imbibition curves of the outer surface of dead skin at various times *post mortem*.[3] Again, tissue rigor could hardly set in at

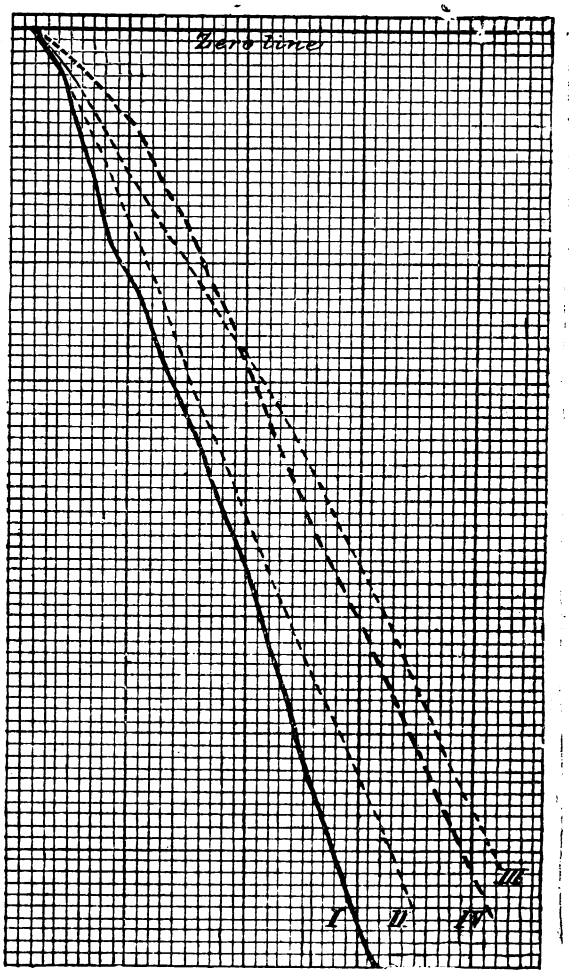


Fig. 3.—Imbibition curves of stale frog's skin (outer surface). I, starts 48 hours *post mortem*; II, 72 hours; III, 96 hours; IV, 120 hours. Abscissæ and ordinates as in Fig. 2. All four curves from same skin.

so early a period after removal, for I found that the gastrocnemii of the same frogs whose skin was used, when immersed in normal saline solution, did not begin to trace their rigor curves till from twenty-four to fifty hours after death. Moreover, could it be granted that tissue rigor and expression of fluid accounted for the rise on the inner side, and imbibition for the fall on the outer it is extremely unlikely, I would almost say impossible, for the two processes to advance *pari passu*. That the process is a vital one is shown by the fact that no such curves can be got with skin that has been kept for days in normal saline, and which is dead beyond doubt; also by the fact that the addition of some poisonous drug, such as muscarine, to the saline, speedily brings the move-

[3] These imbibition curves, gained by my apparatus, show also the fact noted by H. Quincke, namely, that the process of combination of an animal substance with water is accompanied with contraction, so that the sum of the volumes of both, after soakage, is less than it was before. *Pflüger's Archiv*, III, p. 332, 1870.

ment of fluid in the tubes to a standstill. I have looked in vain for evidence of a transfer of fluid in the opposite direction, that is from the inner towards the outer surface, a transfer that might indicate action of the glands with which the skin of the frog is studded. Inactive skins, especially from the weakly frogs, obtained during the breeding season, are not uncommon, but though I have occasionally observed a slight rise of the fluid in the tube towards the outer surface at the commencement of an experiment, I have never observed any corresponding fall or even diminution in the rate of rise of fluid in the tube towards the inner surface. Even with pilocarpine no such effect could be obtained. It is possible that in such cases as the one shown in Fig. 2, one is really only reading the difference between two transfers, the absorptive overbalancing the secretory, and one would expect the former to have the upper hand in the exsected skin; for while the secretory cells have double work, namely, first to manufacture a special product out of what pabulum they can find in the lymph spaces of the bloodless skin, and secondly to pour it forth, the absorptive cells on the other hand have merely to pick up a supply of bland fluid and pass it on; so that as the life processes of the starved cells run down, we might expect the stream from the absorptive cells, with their easier task, to swamp that from the secretory cells throughout the course of the experiment.

The idea that this fluid transfer may be of electro-osmotic origin has naturally occurred to me. The frog skin current which Engelmann[4] attempted to make serve the purpose of a motor of secretion, might be called in, upon the return journey of its circuit, to do the work of absorption. Indeed, with electrical currents circulating as they do in the skin,[5] the question of outward or inward transfer of fluid would have become one of relative "porosity" in the two directions. With equal resistance to fluid transfer in the two directions, theoretically, there need be no passage of fluid across the skin; with unequal "porosity" there might be movement of fluid along current lines coinciding with lines of lesser filtration resistance. Unfortunately the skin offers a greater filtration resistance from without inwards than in the reverse direction. Again, if what I have observed be of electro-osmotic origin, the coupling up by a good conductor of the fluids on the two sides of the skin should increase the rapidity of fluid transfer from without inwards, by diminishing the amount of current flowing *in the skin against* the direction of the absorption stream. When I have done this I have failed to find any effect, though an included galvanometer gave evidence of the normal skin current. I do not, therefore, see how the electrical hypothesis can be made to suit the case.

The facts I have observed, then, seem to point to the conclusion that it is possible to have a true absorptive process taking place without the aid of ordinary osmotic action. That intestinal absorption is governed by other laws than those of ordinary osmosis has long been known. I am at present engaged upon work with the intestine upon the same lines as that detailed above, and the results so far tend to show that a non-osmotic vital absorption is there also capable of demonstration.

[4] *Pflüger's Archiv*, Bd. VI, 1872.
[5] V. Rosenthal, *Reichert u. du Bois-Reymond's Archiv*, 1865, p. 311.

NOTE ON THE KNEE-JERK IN THE CONDITION OF SUPER-VENOSITY.

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I HAVE observed that the knee-jerks are absent in some cases of emphysema with bronchitis where the blood has become venous to an extreme degree. Dr. Penberthy and Dr. Collins assisted me in the investigation of the cases alluded to. As the patients we observed were near death when the knee-jerks were absent, I hesitate to come to the conclusion that they were absent as a mere consequence of extreme super-venosity. Dr. James Harry Sequeira has given me notes of the case of a girl, aged 9, suffering from diphtheria, who was tracheotomised (successfully) at midnight on account of urgent respiratory difficulty producing cyanosis. Before the operation, her knee-jerks were absent; next day, when she was breathing easily, cyanosis having disappeared, the jerks were obtained, and they were elicitable until her discharge about a month after the operation. (This patient was under the care of my colleague, Dr. Stephen Mackenzie).

Dr. Risien Russell, at my suggestion, examined the knee-jerks of a dog artificially asphyxiated by clamping its trachea; the animal's knee-jerks became exaggerated until knee-clonus was produced; but in the third stage of asphyxia no reaction could be obtained. As (Hitzig, Franck, Pitres, Russell) asphyxia diminishes, and in an extreme degree annuls the excitability of the motor cortex, it may be that the preliminary exaggeration of the knee-jerk observed by Dr. Russell was owing to loss of cerebral control upon lumbar centres, and that these strongly organised spinal centres succumbed later to the poisonous influence of super-venous blood than did the controlling cerebral motor centres.

This communication is merely preliminary, and thus qualifications to foregoing statements are not given, and certain objections, some very obvious, are not stated. One reason for writing this note is to suggest that when oxygen is administered to cyanosed patients their knee-jerks should be tested before the gas is given, and also afterwards. If successfully used, that is, if the patient's blood becomes well oxygenated, it is possible that knee-jerks, unobtainable before administration of the gas, may be elicitable afterwards.

If super-venosity is a cause of loss of the knee-jerks, the fact may be important with regard to the apoplectic state, and possibly somewhat with regard also to post-epileptic coma. In some cases of apoplexy from cerebral hæmorrhage,[1] the knee-jerks are lost, in others not. It is worth while in all cases of apoplexy or coma to note the degree of super-venosity, and to investigate, in regard to it, the state of the patients as to tendon-reactions and superficial reflexes.

[1] Buzzard, *Medical Press and Circular*, March 12th, 1890.

A CASE OF INTERMITTENT FEVER WITH UNEXPLAINED HIGH TEMPERATURE.

By STEPHEN MACKENZIE, M.D., F.R.C.P.,
Physician to, and Lecturer on Medicine at, the London Hospital, etc.

IT has seemed to me proper to place on record the following case, which is reported by my late house-physician, Dr. J. H. Sequeira, on account of the extraordinary high thermometric readings. It will occur, no doubt, to many to doubt the genuineness of the temperatures registered, but there can, I think, be no doubt as to the accuracy of the observations. I am always very sceptical of extraordinary temperatures, and in a paper read at the Clinical Society in 1881 exposed a case in which a temperature of 120.8° F. was registered. At the same meeting the late Dr. Mahomed mentioned a case in which a fictitious temperature of 128° was recorded. Cases, however, of temperatures, believed to be genuine, of 122° (possibly 125°) by Mr. J. W. Teale, and of 130° by Dr. Little, of Dublin, have been published.

Expressing my doubts as to the genuineness of the temperature recorded in the present case, extraordinary care was taken by Dr. Sequeira and the nurses to prevent any manipulation or fraud, and the high readings of the thermometer were all taken when the thermometer was held by the observer in one or other axilla. On one or more occasions two thermometers were simultaneously placed in the same axilla, and were found to correspond, a point of considerable importance where fraud is suspected. It was, as a rule, impossible to take the temperature in the mouth, as the patient was in the condition of a rigor, but when so taken it was raised proportionately.

E. G., aged 27, was admitted to the London Hospital on October 2nd, 1891, under my care. He was born in Malta, but had been to nearly all parts of the world as a sailor during the previous eleven years. Eighteen months before admission, while in Colombo, he had his first attack of shivering fits. He was in hospital there two months; he has also been under treatment in several hospitals—at Dublin, Sierra Leone, and Rotterdam. The present attack began on September 28th, 1891, with slight shivering, loss of power in the legs, headache, and lassitude. On admission his general health was good. The liver was not enlarged, but there was some tenderness over the right hypochondrium. The splenic dulness extended to the costal margin. The pulse was 70 per minute; there were no abnormal signs in the heart or lungs. Intelligence was good. Nothing abnormal was to be observed in the retinæ. On the evening of