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ORIGINAL COMMUNICATIONS.

ART. XLIV.—*Pathological Histology, by* DR. GOTTLIEB GLUGE. *Translated from the German by* JOSEPH LEIDY, ESQ., M. D., *Philadelphia.*

SIXTH SECTION.

On Gangrene.

Life exhausts all tissues, and their elements are unceasingly renewed from the blood. When such a renewal ceases, the tissues and organs dry up, or more frequently undergo decomposition, usually under, but also without the influence of the air, and lose the determinate form necessary to their function. They become gangrenous, or as we say of the bones, necrosed.

Gangrene, desiccation, or mummification (necrosis), and dying off, do not, however, have exactly the same meaning. Many tissue-elements of the body die off without becoming therefore gangrenous; that is to say, decomposed. In this manner the epithelial cells of membranes die, and are thrown off; the same is the case with osseous tissue, and this occurs without the structure of the tissues being particularly altered. Gangrene, on the contrary, is a true decomposition of the organs and tissues.

1. In gangrene, the latter break up into small molecules, which at first preserve the direction of the fibres entering into their composition. The muscles lose their transverse striæ, the cells lose their walls, and break up into nuclei, and nucleoli, which likewise finally dissolve, until a liquid black-red, putrid ichor, is formed, which contains only irregular molecules with numerous crystals never absent,

fat-globules, and pigment-granules. This constitutes moist grangrene, to which even the blood itself is liable.

2. Or the tissues mummify without breaking up, and shrink together in a black leather-like mass constituting dry gangrene.

The grangrena senilis frequently presents an example of the double form.* Beneath the mummified skin is often found the moist grangrene.

The mummification of the fœtus in utero, and the gangrenous dissolution of the spleen, and the centre of many tumors, are examples of both kinds of gangrene without the immediate influence of the exterior atmospheric air. Where stasis of the blood has occurred, the latter operates very rapidly in producing gangrene. Thus, in the strangulated hernia, the constricted portion of the red injected intestine becomes very rapidly black; as soon as the hernia comes in contact with the air after operation, so that each moment becomes of importance to the life of the patient.

The parts becoming gangrenous at first present a blackish red color, then green, then brown, and then black, he change depending upon the serum, with the dissolved coloring matter of the blood, which exudes from the capillaries, or pours out from such as frequently are lacerated. The blood-corpuscles, when the blood participates in the decomposition, are reduced to minute, scarcely measurable, gray molecules, and the fibrine diappears entirely, or is found only in small quantity converted into a gray dirty pulp. The chemical alterations are quite unknown.

Where no fluid blood is found in a dead organ, no alteration of color occurs, and thus necrosed bone appears almost unaltered, while carious bone is changed in this respect.

Gangrene exhibits an extraordinary variety in its extent and progress; thus I may refer only to the two extremes: the small gangrenous ichor, scarcely the size of a pea, of typhoid ulcers, and the gangrene of entire extremities in the aged.

The gangrenous parts are frequently separated from those which are unaffected by circumscribed inflammation and suppuration, when the gangrene is the result of local causes.

*The serum evaporates from the surface of the skin; and from the circulation being interrupted, it cannot be restored, and hence the mummification (desiccation), while in deeper parts the watery part of the blood remains, and hence favors decomposition.

The causes of Gangrene are :—

1. The cessation of the circulation in the arteries of an organ. If a small quantity of blood is permitted to pass through the fine interval, surrounding the concentric blood coagulum, or plug in the arteries, moist gangrenes occurs, but otherwise dry gangrene.

2. The cessation of the capillary circulation. Hence, mortification is a frequent result of inflammation.

3. Excessive failure of nutrition and poverty of the blood, as in gangrene noma (cancrum oris), on gangrene of the cheek and of the mouth. Long continued disturbance of of the functions of an organ, as instanced by the gangrene of the lungs from catarrh.

4. Long continued disturbance of the nervous system; as in the frequent gangrene of the lungs in cases of diseased mind; also in the gangrene after typhus.*

5. In consequence of decomposition of the blood from the introduction of putrid substances, or the agency of contagion and poisons. To this catagory belong the gangrenous abscesses of glanders, anthrax, gangrene from the use of secale cornutum, and the kind of mummification consequent upon the use of the peculiar poison developed in sausages. These operate in a manner analogous to putrid flesh in the decomposition of a solution of sugar, resulting in the production of alcohol and carbonic acid.

ART. XLV.—*Case of Hepatic Abscess—Perforating Abscess of the Lungs.* By A. O. KELLOGG, M. D., *Brock.*

Hiram Bigelow, Esq., of Lindsay, Ops, aged 50, merchant, of strong constitution and bilious temperament, previous to 1842 was a man of great activity, labouring hard, early and late, and exposing himself to every vicissitude of weather and temperature. Since that time his occupation has not been so laborious, but has exposed himself much in travelling, and at times has laboured hard; since March 1853 has travelled less, and led more of a sedentary life. Situation of his residence low, near the banks of the river Scugog; has been subject to attacks of bilious or intermittent fever, though on the whole has enjoyed good health up to this time. The loss of a large portion of a very valuable property this season had no doubt caused much mental anxiety and depression, though he appeared cheer-

*Typhoid.

ful and made no complaint until July, when he appeared to suffer unusually from heat and from the smell of newly painted rooms, though the family did not appear to suffer any inconvenience; seemed to droop and gradually become jaundiced; stomach and bowels somewhat deranged, and though his appetite was fair, he suffered much, particularly at night, with griping pains in the bowels and diarrhœa. He was able to move about and attend to his business, though manifesting many symptoms of biliary derangement, such as occasional costiveness followed by diarrhœa, pains in the region of the liver, jaundice, &c., up to Nov. 19, when I first visited him professionally. Complains of great uneasiness, pain and oppression over the whole surface of the abdomen, the integuments of which are particularly tense, but these symptoms are partially relieved by a motion of the bowels; stools clay-coloured, of the ordinary consistence, but extremely offensive, containing no traces whatever of bile; urine very dark, containing large quantities of bile, turning deep green on the addition of a few drops of nitric acid, and nearly black on the same being added in excess; appetite quite gone; the very smell of animal food when cooking disgusted him, and he is only able to partake of the lightest and most easily digested vegetable substances, such as stale bread, arrow-root, oat meal gruel, and sometimes a little milk thickened with rice or flour. The slightest departure from this severe dietetic regimen was followed by great pain, uneasiness and flatulence of the stomach and bowels, which continued until a motion took place; mind much depressed. Pulse 68, regular and full; respiration natural; tongue clean and moist; complains of some fever with thirst and head ache as night comes on, and is restless and uneasy; skin and conjunctiva of the eyes deeply jaundiced; suffers from constant itching; auscultation and percussion discovered nothing amiss in either heart or lungs, but below the right false ribs and over the epigastrium there appeared to be more dulness on percussion than is natural; has taken mercurials but without any relief, and was at this time taking a mercurial pill prescribed by Dr. Widmer when in Toronto in the early part of the month; ordered to continue the mercurial in increased doses, together with the nitro-chloric acid bath morning and evening; flannels wrung out of warm water and sprinkled with spirits Terebinthinæ were applied to the bowels when the uneasiness and pain became severe; these appeared to give him much relief and were followed by some hours of refreshing sleep.

Nov. 26. Has continued about the same. The cathartics operated well, but the stools were quite clay-coloured, and contained scarcely any traces of bile; extremely fœtid; urine highly coloured, turning green as before on the addition of nitric acid, and nearly black or dark purple when the same was added in excess; jaundice complete; some fulness of the abdomen; liver apparently enlarged; its edge could be traced on the right side some distance below the false ribs, but does not appear to be very tender or pressure; tongue moist, clean, and fissured; no desire whatever for food; nearly everything taken into the stomach produced in a short time a severe fit of gastralgia, with an indescribable sense of uneasiness and pain; pulse ranged from 78 to 80, regular and full; mind much depressed, foreboding and anxious about the result of his disease, his family and business matters, &c. &c.; to discontinue the mercurials as he thinks they weaken him, without having the slightest influence on his disease; to continue the bath, as he thinks it gives him some relief, with laxatives, wine and bark.

Nov. 30. No material change; has lost flesh and strength; has slept somewhat at intervals during the day, but this is frequently interrupted by pain and uneasiness in the stomach and bowels; at night is very restless, moving about from bed to sofa or chair, and walking the room; pain severe in the region of the liver, lower part of the thorax and epigastrium, extending to the back; short dry cough; some dyspnœa and oppression of breathing, together with a sense of throbbing and fluttering in the region of the liver; some fluid in the peritoneum, as evinced by fluctuation; jaundice as before, complete; pulse, 68, regular and full; urine, the same as before, containing large quantities of bile; to continue the bath, also to have ♏ X of nitric acid in a weak infusion of quassia and valerian every three or four hours, laxatives, wine, and such mild nourishment as the stomach will at all tolerate.

Dec. 3. Dyspeptic symptoms have increased; stools of the natural consistence, and contain traces of bile, the first observed in them for a long time; some of the stools are quite bilious, others clay-coloured as before; jaundice complete; countenance anxious and sunken; urine highly coloured, transparent but becomes turbid, opaque, and of a dark green colour on the addition of nitric acid; complains of pain in the head, in the region of the liver, and numbness of the right arm; cough severe, attended with a slight mucous expectoration, tinged occasionally with florid blood; tongue dry and fissured; prefers to lie on his back and right side, with the head and shoulders elevated, though, from his extreme restlessness and pain, he is continually shifting his position; great anxiety, frequent sighing and moaning; pulse 80, regular and full; respiration 20–22.

To continue the bath, internal administration of the acid to be suspended, as he thinksit distresses him by increasing the gastralgia, to have every three hours

℞ Quinin Sulph. gr. 3.
Inf. Quassia et Valerian ℥i.
Syr. Simp. ℥ss.

Wine and broths at short intervals.

Dec. 6. Has made no improvement but is evidently weaker; keeps his bed most of the time; lies mostly on the back and right side, sliding towards the foot; is very restless, continually throwing himself about; has had a great deal of retching, vomiting and hiccough, with a constant desire for cold drinks, which, when indulged, aggravate these symptoms, particularly the hiccough; tongue dry, dark, and fissured, and is but partially protruded apparently with much effort; sores collecting about the mouth; countenance sunken and haggard; takes less notice of what is passing, whereas up to this time he has conversed freely during the absence of acute pain, and has given directions to his sons as to business matters; dozes much, and when awake moans and coughs; cough is attended with expectoration of large quantities of dark blood and pus, mixed with particles of an olive-coloured friable matter, evidently broken down hepatic tissue, as it is entirely different from the other matter thrown up, and precisely similar to that expectorated by Rogers, whose case I have reported in the *U. C. Journal of Medicine*, p. 508. The right lung, which at the last visit appeared to be healthy, was now dull over the lower lobe, with complete absence of respiratory murmur; pulse 80, soft, regular and full; respiration 22, sighing; jaundice complete; urine as before; bowels confined. To have Ol. Ricine ℥1; warm embrocations to be applied to abdomen and surface of the chest; an emollient enema, and the following medicine at intervals when the cough and restlessness are severe:

℞ Morph. Sulph. gr. $\frac{1}{6}$.
Tr. Opii Camph. ʒi.
Inf. Valerian ℥i.
Syr. Simp. ℥ss.

Dec. 5. Has continued since last visit to expectorate large quantities of blood, pus, and the matter above alluded to, and is evidently sinking into a state of coma;

coughs, but now appears to swallow the matter raised, which is subsequently ejected by vomiting; at one time, shortly before my arrival, I learned from his sons, intelligent young men, who attended him most assiduously, that he vomited half a pint or more of the matter at one time, which led them to suppose that there might be a second opening of the abscess into the stomach or duodenum, but observing, as stated above, that he made no effort to eject the matter coughed up, I concluded it had accumulated there in the manner alluded to; he appeared to know those around him, and made an effort to protrude the tongue when desired to do so; calls for nothing, but takes his drink when put to his mouth; appears to swallow with difficulty, and the effort aggravates the paroxysms of coughing; pulse 85, regular and full. The anodyne mixture ordered on the 6th was discontinued after a few doses, as it seemed to disagree. The enema was not administered, owing to the great restlessness of the patient and the irritability and soreness of the anus; bowels confined. To repeat Ol. Ricino and to continue the warm applications.

Dec. 10, 6 P. M. He is evidently sinking rapidly; coma profound; breathing stertorous; has had a good deal of hemorrhage from the nose during the day; urine passed involuntarily; surface bathed with a calmy perspiration; pulse 80, regular; respiration difficult, evidently much obstructed by matter he is not able to expectorate; coughs occasionally and swallows the matter; he continued to sink, and expired at 5 o'clock on the morning of the 11th. No post mortem examination was obtained.

Remarks.—"Abscess of the liver," says Dr. Copland, "may follow any grade of inflammatory action, the acute, sub-acute and chronic, and it may occur with any rate of rapidity, but it is most frequent and the most to be dreaded in the sub-acute inflammation of the substance of the organ, attended with tumefaction, and with much disorder of the bowels."

The above case presents an interesting example of a sub-acute or chronic grade of inflammatory action going on insidiously for a length of time, and resulting in abscess of the substance of the liver, and from its proximity to the gall ducts and gall bladder, implicating or pressing upon them and obstructing the flow of bile into the duodenum, giving rise to jaundice, and causing that distressing derangement of the digestive and assimilating organs which was so marked a symptom in this case. For a long time the stools contained no traces of bile whatever, and the derangement of the digestive process and consequent inanition, contributed much to the ravages of the disease, in hastening the fatal result. With Rogers, whose case I have reported (see *U. C. Journal of Medicine* p. 508), it was different, though an abscess undoubtedly formed and discharged itself through the bronchial tubes, and some months subsequent to the report, another through the walls of the abdomen, yet there was no implication of the gall ducts, no jaundice and no such derangement of the digestive organs; hence the system could receive due support until the force of the disease had expended itself, and recovery took place. The appearance of bile in the stools, simultaneously with the discharge of the contents of the abscess through the right lung, was an interesting feature in the case of Bigelow. Was not the pressure of the walls of the abscess upon the gall bladder and ducts during its formation, the cause of the prolonged absence of bile in the alimentary excretions? I regret that circumstances prevented a post mortem examination, as such most probably would have revealed a condition of things of no small interest to the student of morbid anatomy.

ART. XLVI. *On the Sedative Powers of Tartar Emetic, by* ELAM STIMSON, M. D.—*St. George, C. W.*

ST. GEORGE, C. W., January 23, 1854,

To the Editor of the Upper Canada Medical Journal.

SIR,—In the number of your *Journal* for the current month, I noticed and read with much interest a case of poisoning by the use of Tartar Emetic Ointment, reported by Dr. Wanless, Coroner of London, C. W.

Entertaining a very far different view, as respects the *modus operandi* of the tartar emetic, from that expressed by Dr. Wanless and the medical witnesses who testified at the inquest, I take the opportunity your *Journal* affords of reviewing the case, taking the symptoms there reported as premises on which to found my remarks and conclusions:—

That the boy, on whose body the inquest was held, died from the effects of the ointment applied to the head, and that tartar emetic was the deleterious ingredient in that ointment, I think admits not of a doubt. But from the opinion that the tartar emetic excited an *inflammation* which caused the death of the boy, I must wholly dissent.

The symptoms do, as I think, fully support the opinion that death was caused by the effects of tartar emetic as a *sedative.*

For this brief, unqualified, but, I trust, not discourteous expression of opinion, no apology is offered, but the importance of the subject:—That it is important that the profession form definite, uniform and correct ideas as to the nature and effects of an article of such efficiency and general use as the one in question, all must agree.

A state of inflammation, and that depressed state of the nervous and vascular systems caused by a pure *sedative*, are so opposite, and the remedies to counteract such states so unlike in their effects, as to make it most proper to set the point at issue unmistakeably and at once before the reader.

The limits of this article will not admit, neither is it necessary to enter upon, a definition of the term inflammation, or to relate all the circumstances necessary to constitute that state; nor need we attempt any disquisition as to the state of the system, or any locality, when under the effect of a sedative.

It will suffice for our purpose merely to call to mind the increase of nervous excitement or energy, and consequent increase of arterial action in the inflammation, contrast it with that diminished or depressed state of the nervous and vascular systems, when under the effect of a pure sedative.

No two medicinal agents can be more diametrically opposite in their effects than a stimulant and a sedative.

Tartar emetic we consider a pure sedative. And yet we often speak of it as stimulating the stomach to evacuate its contents; and books, with greater absurdity, treat of emetics, at the head of which tartar emetic stands, under the head of "Local Stimulants." Such loose and inconsistent expressions, together with the fact, that we are often unmindful that almost every article in use, as sedatives, are for most cases beneficially alloyed with anodyne or narcotic properties, has had the effect to leave the mind with crude undigested notions of the modus operandi of an article so uniformly purely and primarily sedative as tartar emetic. We say *primarily*, not only because its sedative is not preceded by any anodyne nor stimulatory effect, but because its emetic, diaphoretic and other effects are secondary, a consequence of the primary sedative impression.

Having premised thus much of inflammation, and the opposite sedative action of tartar emetic, we will now refer to the symptoms and appearances upon dissection, and see how far they go to support the opinion that death was the result of inflammation, excited by the tartar emetic, or from its sedative effects.

The father of the boy says, that after the ointment had been rubbed on his head twenty-four hours it was greatly swollen. And the medical witnesses, to support their assumed fact, say (and no doubt truly), that there was "unusual engorgement of the vessels of the brain." Had this and the swelling been inflammatory action, what symptoms would they have caused during life—the ointment smarted him, but was there deep-seated intense pain? any raving or violent delirium—any *want of sleep?* Can it be believed that inflammation could be excited upon the scalp continued to the brain, run through its different stages to a fatal termination in thirty-six or forty hours, without some of these strongly marked and distressing symptoms? Instead of these, after the ointment has been applied for four times, twice on Saturday and once on Sunday,—the patient was *sleeping*,—on Sunday "seemed as if he could not keep awake—was like to fall off the chair;" and on Monday morning his father went to the bed-side to see him, as he *thought he was sleeping too long, and found him dead!*"

Sleep is always preceded and attended by diminished energy of the brain. The ointment being applied to the nervous extremities, its effect was to diminish the energy of the brain and induce sleep.—It was diminished until it became extinct. Probably a more satisfactory experiment to prove the directly sedative nature of the article in question could not be made.

But how, it may be asked, is the swelling and "unusual engorgement" to be accounted for?

By reference to well known physical laws—the mutual, reciprocated influence existing between the nervous and vascular system. The blood-vessels, particularly the capillaries, are by the nerves imbued with an inherent power to transmit the blood from the arteries to the veins. The nervous energy being diminished or destroyed by the sedative, the capillaries lose the power to transmit the blood. It stagnates in them—in medical language, they are *conjested.* The unusual engorgement in the vessels of the brain and the swelling were just what might be expected, reasoning *a priori.*

The thickening of the pericranium was no doubt the effect of the disease, (tinea capitis), or it might as properly be said to be the disease itself.

It may now be properly asked, if the view here taken of the matter be correct, was the prescriber blameable—if so, who is to "cast the first stone." If the mischief is from a property in tartar emetic not sufficiently recognized and taught—and if that property remains undetected by the medical gentlemen at the inquest, with a full opportunity to learn all the symptoms it had developed; and if the profession generally have been in the daily practice of using it to counteract general and local inflammatory action, and, at the same time, were unaware or unmindful that its efficacy was owing to its powerful and purely sedative quality; I expect, if all this is so, how completely does it nullify any charge of culpable ignorance or rashness that can be brought against the prescriber? It further shows, too, that the Q. C., Col. Prince, in refusing to act upon the indictment found against him, exercised the combined virtues of charity, humanity and sound discretion.

I remain, Sir, yours,

ELAM STIMSON, M.D.

ART. XLVII. — *Rough Notes of a Clinical Lecture, delivered by* DR. BEAUMONT, F.R.C.S., *London, and one of the Surgeons to the Toronto General Hospital, on a case of False Aneurism. Reported from memory.*

Dr. Beaumont commenced by stating that he desired to make some observations on the case of traumatic aneurism which he was treating at the present time in the hospital. The difference between a false or traumatic aneurism and a true aneurism consisted in the one having been produced from a wound or injury in the artery, and the other having proceeded from disease of its coats. The case which he was about to bring under their notice was of the former description, and was a most interesting variety of that kind of disease; for the patient, who was a young man 20 years of age, by name Joseph Sterves, who it appeared had been stabbed in the left side of the neck with a bowie knife or some other sharp instrument. If they examined the patient they would see a tumour of about the size of a hen's egg, rather external to the line of the carotid artery and about an inch above the clavicle, the instrument having appeared to transfix the sterno-clido muscle. The summit of the tumour is marked by a cicatrix, and it has an evident pulsation. If they applied the stethescope, they could distinctly hear the bruit-de-soufflet; in fact they might hear the rush in the tumour produced by the constant whirl of arterial blood as it enters, takes its course through the tumour, and finds an exit again into the blood vessels. They could compress the tumour so that the swelling could be emptied completely, and they might feel the impulse of the arterial blood again forced into the tumour by the heart's action. It was conceived that the internal jugular vein had been also wounded, as it was fancied that it also received an impulse from the heart's action; but whether it was from a direct opening and communication with the artery as in varicose aneurism, in which the blood is permitted to flow directly from the artery into the vein, and so communicate its impulse; or whether it was the pulsation of the artery communicated to the vein from its lying in close proximity with the tumour, could not exactly be made out. Dr. Beaumont then detailed the nature of the accident, and said that about eight weeks since the young man was walking along on the side-walk, when a man with whom he had had some words called him over to the opposite side of the road, and without saying anything more to him than "do you think I am afraid of you?" plunged a knife into his neck above the clavicle. A most profuse bleeding followed, so that the wounded man became quite faint and perfectly insensible. He was taken into a house near by, and a medical man was immediately sent for. By the time that he arrived all bleeding had ceased, but the man lay extremely faint, and could scarcely be moved from the horizontal position without becoming insensible, and he declares that he did not know his father until the next day. It was about midnight when the medical man dressed the wound with some straps of sticking plaster, and applied a compress and bandage. By slow degrees the man appeared to revive, but the next morning, from an accidental effort at sneezing, the blood flowed again from the wound most copiously, so that he lay faint and almost lifeless, and it was expected that he would die every moment. Still, however, the hæmorrhage was again stopped by compression, and when the medical man arrived it had entirely ceased. He now put several sutures into the wound, brought the external edges of the skin together, and maintained it so by compress and bandage. Fortunately no further bleeding occurred, and the external wound healed by the first intention. The very next day a small beating tumour was observed below the seat of the injury, and this has increased to the present size. Dr. Beaumont made some remarks upon the nature of the accident and the proper treatment to be adopted in such a case; he pointed out that it was the duty of the surgeon to enlarge the external wound so as to allow him to place a ligature upon the wounded vessel both above and below the seat of injury; he said that such was the rule in surgery, but that very few of us at the present day would like to undertake such an operation on the spur of the moment without further consultation and assistance, especially when we remember the intricate anatomical relation of the parts in the situation of this wound in the carotid artery, that possibly the ovoid extremity of the external jugular vein, which is sometimes of very considerable dimensions near its termination; if this should be in the way he should place two ligatures upon it and cut the vein through; after that he should not use the point of the scalpel, but should take the extremity of the handle, which had some notches made in the ivory; with this he should tear and separate the tissue, and endeavour to get down to the seat of the injury in the artery and its sheath. If the brachio-cephalic vein had been in his way he should push it aside, and when he arrived at the wound in the bleeding vessel he should place two ligatures upon it one above and the other below the wound. He should also be particularly careful to avoid the pneumo-gastric nerve, or not to injure the internal jugular vein. He thought that the medical man who was called in this case acted judiciously, for when he found that there was no further bleeding, and that the parts stood a chance of uniting by the first intention, as they might observe that they did, it was far better than to undertake so dangerous and severe an operation, with all its chances of further hæmorrhage. At the time that the man came into the hospital, he made a plaster of Paris cast of the tumour in the neck, by which means he would compare its present with its future condition, and by it he hoped to ensure a more perfect knowledge of any increase or decrease in the size of the swelling. They would observe that on the cast the tumour appears to have been accumunated more when this was taken some 20 days since, than it seems to be to-day, while he also thought that it was considerably diminished in size.

The mode of treatment that he proposed to adopt in this case, was that which has been called Valsalva's method, which consists in the practice of frequent small bleedings and starvation, with the careful use of the digitalis. These means are intended to diminish the action of the heart and arteries, when he hoped the tumour would gradually diminish and the disease in the blood vessel would be gradually cured. But if, after he had given this method of cure a sufficient trial, and found that the aneurism is still increasing, he proposed to cut down upon the side of the tumour, as we would do in false aneurism, at the bend of the arm, and place a ligature both above and below the wound in the carotid artery. It had been proposed by some gentlemen to place a ligature upon the cardiac side of the carotid artery, between this aneurism and the heart; but when he examined a recent dissection of the parts, at the dissecting room of the medical department of Trinity College, he found that the position of the brachio-cephalic vein, the insertion of the external jugular vein and the deep position of the carotid artery in this situation, clearly forbade the operation. If they considered the character of this aneurism, that it is a false aneurism, the result of an injury, where the coats of the aneurismal tumour consisted only of condensed areolar tissue, for all the coats of the artery were wounded and the walls of the tumour are formed by adhesion and consolidation of the fibrous elements of the surrounding parts. In this case you have not the dilated coats of the artery as in true aneurism, hence the reason that he thought that the operation proposed by Brasdor, of tying the carotid artery on the distal side of the aneurism, would not be a proper operation. He thought that in this instance it would not take off the influence of the heart's action upon the aneurismal tumour, and that although the operation was performed the disease would go on, and that ulceration would eventually take place, and death by hæmorrhage be the result. Upon mature reflection he was convinced the most proper method of treating this disease was to leave it to nature, and as far as in our power to aid and assist her, which he proposed to do by diminishing the power of the heart's action and the velocity of the circulation. That is Valsalva's method. He had ordered the man to be bled, to be put upon low diet, and to take the digitalis. He had been opposed in this plan, hence his desire to explain to them the rationale of the treatment he had adopted, and in support of his opinion and practice, he would bring his authorities to show that he had adopted the correct line of practice. He should quote to them several cases from the work of Mr. Guthrie on the diseases of arteries. They must know that Mr. Guthrie is about the highest authority we have in such cases; Mr. Guthrie is an old army surgeon, who served through the campaigns under the late Duke of Wellington in Spain and France; consequently he has had the most extensive experience in the treatment of wounded arteries of any man living, and he looked upon him as our highest authority in these matters. Dr. Beaumont now read three cases from the work of Mr. Guthrie upon wounded arteries, and quoted several of his remarks. In the first instance he pointed to a case in which a man had been wounded above the shoulder; the pointed instrument struck both the subclavian artery and the vein; there was fearful hæmorrhage; the man fainted, and continued in a very low state for a considerable period; as there was no bleeding after the first impulse of the blood had stopped from the faintness, a simple compress and bandage was applied to the wound; the wound healed by the first intention, and although there was a small pulsating tumour in the situation of the injury, this never increased to any very great size, and after a series of years this tumour appeared to subside, and in fact never produced any other trouble than weakness of the arm. Soon after the injury the pulse at the wrist had ceased to beat, and such continued to be the case many years afterwards. The second case that Dr. Beaumont quoted was a wound in the femoral artery near the groin. In this case a similar alarming hæmorrhage, a like faintness and debility followed; but upon the flow of the blood stopping, the external wound was closed and adhesion by the first intention took place. Soon afterwards there occurred a pulsating tumour in the groin, attended with considerable swelling; this swelling inflamed and ulcerated, and when this ulcerated surface contracted it cured the aneurism, leaving a very large and hard cicatrix. Many years afterwards, upon the man making some extraordinary movement or exertion, this cicatrix in the groin gave way, and the man died instantly of hæmorrhage. The third case which the Doctor read was a wound of the common carotid artery near its bifurcation. The wound was made by some sharp-pointed instrument. Great bleeding ensued, and the man was prostrated to the utmost; but as the bleeding did not return, the same kind of treatment was adopted as in the former cases, and as in them adhesion cured the external wound, but left a pulsating tumour behind it. As the aneurism did not seem to increase, no operation was performed. By degrees the tumour subsided, and the man got quite well. Here then, gentlemen, would see that there are three cases very much to the point, and in direct support of the plan of treatment he had adopted; and he may tell them that Mr. Guthrie draws the same conclusions from these cases that he did, viz. that it is inexpedient to perform any operation in such cases of wounded arteries as these, for he says (and he thought he was supported and borne out by these facts), that nature is fully able to cure the diseases in the artery without the danger of having recourse to the performance of an operation. Now he also hoped, that they would see and appreciate the reason that he had adopted the method of cure proposed by Valsalva in this variety of aneurism. To show the danger of the operation of tying the carotid artery, Dr. Beaumont quoted some cases from the same work by Mr. Guthrie. In one of the cases detailed, a gentleman, in a fit of temporary derangement, had cut his throat. There was great bleeding, but it was not pure arterial blood, nor did it flow per saltum. When Mr. Guthrie came to examine the wound, he found that the hæmorrhage proceeded from a small cut in the internal jugular vein. Mr. Guthrie states that he seized the edges of the wound in the vein, and put a ligature upon it; by this means he brought the cut surfaces of the wound together and arrested the bleeding with obstructing the course of the blood through the internal jugular vein; to have tied this vein across so as to have obstructed its course, would have been to have prevented the return of veinous blood from the brain, and would have in all probability caused great derangement of the circulation, if it did not produce apoplexy and death. Hence they saw Mr. Guthrie took the wisest plan that could have been adopted, and the one he should advise them always to take in cases in which they had a wound of a very large vein to deal with. When Mr. Guthrie came to examine the carotid artery, he found that the external coats of that vessel were also cut through, but as there was no hæmorrhage from it, the internal coat being still impervious, he did not interfere with the artery; he simply brought the parts together and trusted for adhesion by the first intention. After some days, however, ulceration took place, the internal coat of the artery gave way, and fearful hæmorrhage occurred; the fainting stopped the bleeding for the moment. He was directly sent for, and when he arrived he immediately cut down upon the common carotid artery and put a ligature upon it; this seemed to arrest the bleeding, but the man died the next day from the great loss of blood which he had sustained. Another case which Dr. Beaumont quoted, was an injury of the carotid artery from the forcible protrusion of a tobacco-pipe into the mouth. Considerable hæmorrhage occurred, but was stopped by faintness. This bleeding returned several times, when it was resolved to tie the common carotid artery. This was done; still, however, the bleeding continued to return, and the man died from the injury. They could easily understand the facility with which the blood found its way to the wound in the artery, notwithstanding the common carotid artery had been tied. They knew the very free anastamosis of the blood-vessels at the base of the brain, and the great freedom of circulation in this part, for the wound in the artery must have been very near to that part of the vessel which traverses the bone to enter the skull, and assists to form the circle of Willis. These two cases, gentlemen, go to prove to you the impropriety and inutility of tying the common carotid artery at a distance from the part which has been injured, and would make him hesitate to do the operation in this case. Dr. Beaumont then pointed out that it was no trifling thing to tie the common carotid artery; that it was attended with many dangers; that it should certainly be avoided if it could with any degree of propriety. Setting aside all accidents that might attend the operation or follow its performance, there was always danger to the brain to be apprehended. The sudden arrest of so considerable a portion of the blood sent to the brain might do great harm; it might cause palsy and softening of the brain, which might terminate in the death of the patient. Depend upon it, the tying of the carotid artery is not so trifling a thing as some would wish us to believe, and ought never to be attempted except upon some urgent necessity.

Remarks on Dr. Beaumont's case of False Aneurism of the common carotid artery, by the Editor.

The case above detailed and treated by Dr. Beaumont we consider of the most interesting description, and we have looked upon it as our duty to lay an account of it before the medical profession. If the report is not verbatim in itself, we trust the detail is given in a proper and truthful spirit. The case is certainly an unusual one, and the treatment to a considerable degree a novelty. Some twenty-five years ago we had the felicity of attending some six or seven courses of Mr. Guthrie's lectures upon surgery, and cannot call to mind that he then advocated the principles which he has since maintained in his work on wounded arteries, from which Dr. Beaumont made his quotations, nor could we obtain an opportunity of inspecting the work, although we solicited the favour; nor was there another copy of the work to be had in Toronto. The principles laid down in this instance do not appear to have been endorsed by any other author that has come within our reach; neither South nor Miller make any mention of them. Still, however, if experience shall prove that the treatment is correct in this case, and it would appear most favourable for the trial to test its truth, the greater credit will be due to Dr. Beaumont for adopting it. The modus operandi of the cure of aneurism by the formation of a clot of fibrine and obliteration of the artery will not in this instance be expected. The only way in which we presume that this disease in the artery can be cured, will be by the contraction of the fibrous element which constitutes its walls. By Valsalva's method we depress the heart's action and diminish the amount of fibrine in the blood; by that means we certainly diminish the power that caused the dilitation of the fibrous cyst that constitutes the walls of the aneurismal sac. In all fibrous structures there is a natural power of contraction when they have been distended. We plainly see this in the healing of wounds, and the contraction of the basement membrane of the skin in all cicatrices. So may we expect in the aneurismal tumour that the fibrous element of which its walls are composed, when the power which caused its distention has been greatly diminished, that it will contract and after a time cure the aneurism. We should not expect that fibrine would form in the tumour; it certainly had not when we last saw it. We think the mode of treatment adverse to such an occurrence; nor do we anticipate that the calibre of the artery will be interfered with, so as to stop the course of blood through it; and reasoning from these premises, we should expect that it will be a long time before the cyst contract so as to form a perfect cure of the aneurism; and at the same time we should fear that there would be a great liability to return. We shall be delighted to find it otherwise, and shall not under those circumstances fail to accord our thanks to Dr. Beaumont for demonstrating this important fact, and for placing before the profession the right line of practice in these cases. We have been informed by the house surgeon at the hospital, that the tumour is rapidly diminishing; we wish we could have certified to the fact from ocular demonstration, but this was not allowed to us; for although we have been a daily attendant at the hospital at the regular hour, we have not been permitted to see the case for the last six weeks or two months; we have requested liberty, but it has been denied to us in consequence of an order from the Hospital Board (very correct no doubt), that no patient could be looked at unless the gentleman in attendance upon the person was present; and although we, daily awaited Dr. Beaumont's arrival, we have not had an opportunity of seeing the case in question. In saying this much we do not wish to make any unkind remarks upon the irregular way in which Dr. Beaumont attends at the hospital. Nevertheless we cannot fail to offer our modicum of praise for the zeal and industry with which we see that Drs. Hodder, Aikins and some other gentlemen attend to their hospital duties.

The interesting nature of the case and the novelty of the treatment will call upon us to watch it with attention as far as we may be permitted, and to report the progress and result to the profession.

REVIEW.

PRINCIPLES OF PHYSIOLOGY, GENERAL AND COMPARATIVE.—By WILLIAM B. CARPENTER, M. D., F. R. S., *Examiner in Physiology and Comparative Anatomy in the University of London: Professor of Medical Jurisprudence in University College, &c., &c.*

(*Continued from No. 5*).

Primary Tissues of Animals—continued.

In our previous considerations of Mr. Carpenter's treatise on General and Comparative Physiology, we have endeavoured to indicate the origin, the progress and development of the several cell formations of the animal composition; and particularly to point to the epithelial structures of the lymphatic glands, as the origin from whence these formations spring, be it in their normal or in their abnormal condition. At the same time that we ventured to detail the history of their production, we particularly pointed to one of their products in the circulating system—this was the fibrine; and we endeavoured to show that this material being the development of cell growth, it thereby obtained a peculiar condition of vitality which permitted it to exist as a part of the living frame, in the shape of fibrous tissue. It may now be proper, according to the course that Mr. Carpenter has adopted, to consider the individual character and appearance of the cell formations which exist in the blood.

The simplest form of cell to be found in the animal economy, and one of the most isolated and independent of its kind, is the chyle or lymph cell; this form of cell presents itself in all animals in which a proper circulation exists. If we examine the chyle immediately after it has entered the absorbent vessel, albumen and oil abound; but when we arrive at the mesenteric gland these materials are evidentiy diminished in amount; and in the chyle beyond the gland, we shall now find the lymph corpuscle to present itself; hence we may safely affirm that they have their origin in the glands, in all probability from the thick layer of epithelial cells that line that formation. These cells, upon examination under the microscope, present a diameter of from 1.7120th to 1.2600th of an inch. Minute granules may be observed within them, but they seldom at this stage exhibit distinct nuclei, even if we apply dilute acetic acid to them; but sometimes at this period we may observe three or four central particles which appear to be the rudiment of the future nucleus. These cells are without doubt the white corpuscles of the blood in their earlier stages of formation; they then enter the general sanguineous circulation, float in the blood of *invertebrata*, while in the higher animals they are accompanied by the red corpuscles of the blood. When arrived in the blood, they present the character of a cell perfectly round of about 1.3000th of an inch in diameter, apparently containing numerous minute molecules, which may sometimes be seen in active motion within the cell-wall. These white corpuscles of the blood, when viewed under the microscope, do not always present perfectly the same appearance, for it would seem that these differences were dependent upon the various stages of development which they gradually experience in the blood; so marked are they that this state can be readily traced from one condition to another. At an early period of their development, on the application of a little water, it will be absorbed by endosmotic action, and will swell out the cell-wall so that you may distinguish a large soft nucleus, a granular tuberculated mass within it, which seems disposed readily to break up into several parts; and if at this period the cell be treated with a weak solution of potass, the cell-wall will burst, the nucleus will be broken up, and the molecules will escape, but still the movement among the granules may be observed to continue for a considerable period. After a time the nucleus of the white corpuscles becomes smaller and more defined, and by the aid of dilute acetic acid, it may be plainly made manifest. A peculiar movement experienced by these white corpuscles has been described by Mr. Warton Jones, as happening in the blood not only of man, but also in that of vertebrated and invertebrated animals, a protrusion of the cell-wall takes place, first on one side and then on the other, and the corpuscle would seem to undergo many such changes before the process finally ceases. These changes, he says, may be observed while the cell is moving along in the column of the blood, or when it is adhering to the walls of the vessel. The refractive power of the white corpuscle is higher than that of the red; while it is distinguished by an appearance of greater firmness, and by a want of that tendency to aggregate together, so marked an attribute of the red corpuscles.

There cannot be a doubt but that the colourless corpuscle, as well as the red, has a definite term of existence, that some of these cells will be broken up and disintegrated, while others are advancing in development to supply their place; indeed, each and all of these cell formations are undergoing a continual change—new productions may take place with considerable rapidity, and increase of speed when this occurs will often be in proportion to the amount of blood that has been lost, in hæmorrhage, for example—and that these changes will be especially accelerated by a good and nourishing diet. To Mr. Warton Jones especially belongs the credit of having studied the development of this cell growth, in all its successive gradations, and for having pointed out the various phases which it exhibits in the different varieties of animals. He plainly shows that these cell formations experience the same changes in man, that may be observed to occur in the advancing series of animated nature; and that they only attain their complete form in the highest order of the series. In the blood of the *Invertebrata*, and occasionally in that of the *Vertebrata*, you will find the coarse granule cell; this appears to be typical of the earlier condition of the chyle cell, and is the first stage of development; while the fine granule cell may be regarded as the second, and is followed by the colourless nucleated cell, the highest development of the blood corpuscles in *Invertebrated* animals, and equivalent to the white corpuscle in the *Vertebrated* series. It will be seen, however, when we come to speak of the red corpuscle of the blood in man and the higher animals, that this white corpuscle is still but in a transitory state, that in the *oviparous vertebrata* we shall find a coloured nucleated cell, a step in advance of the white corpuscle, while in the highest order of the *vertebrata* the coloured non-nucleated cell will be found to abound, and that this is the red corpuscle fully and perfectly developed. It may also be observed, that varieties in the more or less perfectly developed red corpuscle may be found to occur in the several gradations of the vertebrated series, so that the blood corpuscles of one animal may be distinguished by the experienced microscopist from that of another, and to the medical jurist this might form an object of no inconsiderable interest.

Speaking of the functions of this white corpuscle, Mr. Carpenter says, (page 116) that "From these cells the red corpuscles of the blood are now generally supposed to take their origin; but as they are nearly peculiar to *vertebrata*, it is almost certain that the colourless corpuscles must have some other more general function; and there seems reason to think that this consists in the transformation of albumen into fibrine—that is to say, the elaboration of the spontaneously coagulating and fibrillating substance from the mere chemical compound which forms the raw material of the animal tissues. For we find them in every situation in which we know this transformation is going on; and we observe their number to bear a close relation with the amount of fibrine produced in the fluid.

The red corpuscle of the blood is also a distinct and independent cell, floating in the liquor sanguinis; they are easily recognized by their red colour, and present the form of flattened disks, circular in man and most of the mammalia, while in birds, reptiles and fishes they are oval. The corpuscle is a flattened doubly-concave cell, with a perfectly pellucid and colourless cell-wall, but its contents are coloured. In consequence of the concavity of the surface of the cell when placed under the microscope, a bright spot or a dark centre will be observed; this is dependent upon the difference of focus presented by the object, and is no proof of the existence of a nucleus, as was once supposed. It is very clear that the cell-wall is readily permeable to fluids, and that this will be affected in perfect accordance to the known laws in these cases. A very thin fluid water, for example, will be readily taken up, the cell will swell, become flat, doubly convex, and eventually burst, permitting the diffusion of the colouring matter and its other contents in the surrounding fluid; while, should the fluid in which the red corpuscles are floated, be of a denser consistency, such as thick syrup, or a strong solution of common salt, an opposite effect will take place; the cell will be emptied by exosmotic action, and will assume a shrunken appearance with a crenated edge; as this action progresses the central spot will be increased in size and distinctness until the cell is completely shrivelled up. There is no doubt but that the red corpuscles while floating in living blood are submitted to similar influences, dependent upon the constitution and density of that fluid; hence also the necessity in microscopic examinations of the blood, to place the red corpuscles in a fluid at about the consistency of the liquor sanguinis, if we expect to preserve the true shape and proper size of the cell while under observation. When we examine the red corpuscles, while circulating through the blood vessels, they may frequently be observed to change their shape, to become more elongated under pressure, but they soon regain their form when the influence is removed; in the capillary vessel they may suddenly be bent, twisted or elongated while passing through a narrow channel, but their elasticity will restore them to their normal size and shape when they again obtain sufficient space. It is said that the size of the blood disks may vary in the same individual a different periods of time. The positive condition of fluidity of the blood, in all probability is sufficient to explain this fact; and as this may vary frequently in the twenty-four hours, we may consequently find a corresponding change in the corpuscles. That this red corpuscle may degenerate and die, there is not the shadow of a doubt. Hewson says, that they undergo spontaneous decomposition, that the blood-disks become granulated and take on a mulberry shape, in all probability by being filled with oil globules, as we see in the inflammation corpuscles of Gluge; he even says that particles in which this change has commenced, may be observed in the blood at all times, and that this condition precedes disintegration and death of the cell; and we have imagined that we could see their debris also in the blood. As we have before said, the size of the red corpuscle differs exceedingly in the various tribes of animals; in man it is on an average diameter about 1.3200th of an inch, and in thickness does not exceed 1.12,400th.

In the chemical consideration of the red globules, it is said that a difference may be distinguished between the cell-wall and its contents. The essential character of the one is an organized albuminous compound termed *globuline*, while the material which gives the characteristic hue to the contents is named *hematine*.

With regard to the nature and functions of this peculiar ingredient, Mr. Carpenter makes the following observations (page 119):

"Its composition is notably different from that of the albuminous compounds; the proportion of carbon to the other ingredients being very much greater, and a definite quantity of iron being an essential part of it. Its formula is 44 carbon, 22 hydrogen, 3 nitrogen, 6 oxygen, and 1 iron. The iron may be separated from the hæmatine by strong reägents which combine with the former. Yet the latter still possesses its characteristic colour; its hue cannot be dependent therefore on the presence of iron in the state of peroxyd, as some have supposed. Regarding the nature of this compound, and the changes which it undergoes in respiration, there is still much to be learned; and until these points are fully elucidated, the precise use of the red corpuscles cannot be understood. There is evidence, however, that the production of hæmatine is like the production of the red colouring matter of the *protococcus nivalis*, a result of chemical action taking place in the cells themselves; for no substance resembling hæmatine can be found in the liquid in which these cells float, and scarcely a trace of iron can be detected in it; whilst, on the other hand, the fluid portion of the chyle holds a large quantity of iron in solution, which seems to be drawn out into the red corpuscles, and united with the other constituents of hæmatine as soon as it is delivered into the circulating current. The colouring matter appears in two states, the precise chemical difference between which has not yet been ascertained. In arterial blood it is florid scarlet; while in veinous blood it is of a purple hue. By circulating through the capillaries of the system, the arterial or bright hæmatine becomes converted into dark or veinous hæmatine; and the converse change takes place in the capillaries of the lungs, the original florid hue being recovered. Now, it is certain that the blood, in its change from the arterial to the veinous condition, loses oxygen and becomes charged with an increase of carbonic acid, although its precise mode of combination is not known; on the other hand, in its return from the veinous to the arterial state, the blood gives off this additional charge of carbonic acid and imbibes oxygen. These changes in the condition of the contents of the red corpuscles, taken in connection with the fact that these bodies are almost completely restricted to the blood of *vertebrata* (whose respiration is much more energetic than that of any invertebrated animals, save insects, which have a special provision of a different character), and their proportion to the whole mass of the blood corresponds with the activity of the respiratory function, leaves little doubt that they are actively (but not exclusively) concerned as *carriers* of oxygen from the lungs to the tissues, and of carbonic acid from the tissues to the lungs, and that they have little other direct concern on the functions of nutrition, than the fulfillment of this duty. Their complete absence in the lower invertebrated animals, in the earliest condition of the higher, and in the newly-forming parts, until these are penetrated by blood-vessels, seems to indicate that they have no immediate connection with even the most energetic growth and development; whilst, on the other hand, there is an abundant evidence that the normal activity of the animal functions, together with the power of generating heat, for both of which a copious supply of oxygen is requisite, are mainly dependent upon their presence in the blood in due proportion."

Another function of considerable importance has been lately assigned to the red corpuscles, and is derived from the chemical differences observed between the red corpuscles and the liquor sanguinis; in which it has been shown that the chloride of sodium abounds in the liquor sanguinis, and potash, phosphorus, and fat in the corpuscles; hence it has been concluded that the "phosphorized fats" are formed in the red corpuscles, and that from this source the pabulum of nervous matter is derived; while the muscular structures obtain their supply of materials for renovation and repair of potash salts from a similar source. The peculiar activity of the muscular apparatus in insects, whose blood, according to our observations, consists of little more than oil and albumen (the cell of Acherson), and is totally deficient of the red corpuscles, while in the muscular apparatus of insects the spiraculæ may be seen carrying the oxygen in atmospheric air, to each individual febrillæ, would seem to forbid this conclusion; for, under such circumstances, it would be plain that the muscular and nervous structures of insects would lack that renovation and supply which is certainly not deficient in their case, as evidenced by their activity.

(To be continued.)

BOOKS RECEIVED FOR REVIEW.

Lectures on Surgical Pathology delivered at the Royal College of Surgeons of England by James Paget, F. R. S., lately professor of anatomy and surgery to the College, assistant surgeon and lecturer on physiology at St. Bartholomew's Hospital. *Hypertrophy: Arophy: Repair: Inflammation: Mortification: Specific Diseases and Tumours*:—Philadelphia: Lindsay and Blackiston, 1854.

In the work above mentioned Mr. Paget has clearly exhibited splendid talent and untiring industry in employing the noble opportunity he possessed for the study of pathological science. At an early period we intend to review these lectures after a manner similar to that in which we have considered Mr. Carpenter's Principles of General and Comparative Physiology, and hope to cull for our readers a few of the new and interesting facts recorded by Mr. Paget, and so elaborately set forth in the work in question.

ERRATUM.—Page 214, 11th line, for "head" read *heart.*

EDITORIAL DEPARTMENT.

INCORPORATION OF THE MEDICAL PROFESSION.

The miserably divided and degraded condition of the medical profession in Canada West, has induced us to address a public letter to the Honorable John Rolph, calling upon him to adopt some means, to institute some measures that shall alter this lamentable state of things. Without a doubt, Dr. Rolph possesses the power; and we believe that he, after mature reflection, will be inclined to adopt the measures we propose. Although he has long been thought adverse to the incorporation of the medical profession, we are convinced that the love and interest he takes in it must by this time have fully demonstrated to him the impossibility of anything like unanimity among its members, or progress and advancement among its practitioners, without some such act of incorporation. Dr. Rolph is about the first medical man that has ever reached the elevated position in this Province which has been accorded to him, and we are fully convinced that he must generously sympathize with that state of deep degradation and miserable confusion which reigns triumphant in the medical profession of Canada West. Although the Doctor's answer does not give us any very flattering indication that he will adopt the action we propose, still we have hopes that common sense and reason will prevail, and that Dr. Rolph will note that he has the chance to make himself honoured and respected by the whole of the profession in Canada West.

To the Honourable John Rolph, President of Her Majesty's Executive Council in Canada:

Sir,—The near approach of that period when the representatives of the Canadian people will again meet in parliament to alter and amend the laws which are to regulate and guide the various sections of the inhabitants of this Province, has encouraged me to address you on the subject of the *Incorporation* of the *Medical Profession.*

The present abased and degraded condition of the medical profession in Canada, which has doubtless arisen in a very great degree from the unfortunate divisions, party cliques, and personal animosities among its members; a depression that all good men and all true lovers of their country must deplore; a condition that absolutely demands some remedial means that may conduce to raise the profession as a body in public confidence, and restore it to a proper condition of public usefulness. The elevated position which you hold in the councils of the country, naturally induces me to address you on this all important subject; besides which the love that I am bound to believe you bear to the medical profession, of which you have long been an acknowledged ornament, inclines me to hope that you cannot witness this degradation without feelings of sorrow, nor observe its deterioration in public estimation without desiring by some means or other, as far as in you lays, to change so unfortunate a state of things. I therefore trust that you will use your interest and exert your influence to find a remedy.

As an *Act* of *Incorporation* for the medical profession of Canada West on a most extended and liberal basis, would appear to be the only probable means whereby a remedy may be found for this discouraging condition of things, I would respectfully ask if such a measure will be likely to receive your sanction and encouragement in the next session of the Provincial Parliament to be assembled at Quebec? I think that there are many and forcible reasons to lead the medical profession to hope that you will feel inclined to give the subject of its incorporation your cordial support, and even lead them to expect that such an act should directly emanate from yourself. I must confess—to be plain and honest in my address—that if you do not adopt the present excellent opportunity of gaining the certain thanks and lasting gratitude of the medical profession, and of placing yourself in a position of honourable distinction by rendering to that noble profession an act of justice which has been too long delayed, I shall, I fear, have reason to doubt the acumen of your judgment, or the sincerity of your love for the honourable profession which has nursed you into *power*, and has helped to place you in the exalted position you now hold.

The clerical and legal professions of Canada have the benefit of acts of incorporation, by means of which they are permitted to regulate all matters appertaining to their particular class; and is it not unjust that the medical profession of Canada West should alone be excluded from a similar benefit? I think that experience must by this time have plainly demonstrated to all unprejudiced observers that the medical profession can never regain that condition of public usefulness, that position of honourable and honest bearing, that will give it the confidence of Canadian people, without some change in its condition. At the present moment the medical profession, legislatively speaking, is placed upon the same level with the grossest *quackery* and *humbug*; in all cases it is the slave of the public; and if its members possess the honour and honesty which is a natural attribute of our holy profession, they are bound by every tie, both human and divine, to abjure deception, although it be to their own injury and their own ruin. If under the present condition of things, deception stands in the place of truth and honesty; if quackery and deception receive the encouragement that should be accorded to medical knowledge; if a knowledge of the science of medicine or the acquaintance with anatomy be expected to be gained by inspiration and successfully practiced without hard and painful study, then must science retrograde, and the public before long will surely reap the dreadful effects of their misplaced confidence; for under such a regime science and learning must be banished from among the members of a once learned profession, while honour or honesty in such matters must become a bye-word, a deception and a snare.

I would, moreover, appeal to your acknowledged love of science, to that generous enthusiasm in the love of truth which every true votary of science must of necessity possess, to call upon you to come forward and use your utmost influence to produce a reformation in the present condition of the medical profession, by at least affording to its members a means by which its degradation may be stayed; or permit them a chance to place truth and science in the position of public estimation, which is now occupied by deceit and quackery. At all events, the parliament should be called upon to give a national verdict; a verdict that would surely be accorded in favour of the truth by every honest and enlightened representative, did he but properly understand this matter.

Having then appealed to your love of science, to your sense of justice, honour and honesty, will you permit me to offer some reasons that I think should lead you to take this matter of the incorporation of the medical profession in hand, and do all in your power to accord to it that position in moral and political condition which its public merits and private usefulness demands.

I am fully convinced that by this time your enlightened knowledge in all the departments that appertain to the principles and science of the medical profession, must have enabled you to make a lively distinction between true knowledge and positive humbug; and I am convinced that if you really appreciate the truth of science, you will do all in your power to sustain and honour it. You know full well, as a simple fact in medicine as in religion, that if our rulers exalt error and discard the truth—or, what is tantamount to it, make no distinction between them—error will surely work its dreadful effects upon society; for error and deception will be sure to bring their curse. May I then hope that a more enlightened policy than has hitherto had effect in Her Majesty's council, with respect to the medical profession of Canada West, may now, under your guiding influence, be induced to grant that encouragement to true learning which has long been withheld from it; and which I believe to be the sincere endeavour, nay the honour of all truly enlightened statesmen always to accord to it? You must also, I apprehend, be fully alive to the fact that to encourage true science is to promote the public good; to foster that profession which was intended as a blessing to mankind, and so to encourage its free development as to permit us to anticipate that at some future day the light of science shall so expand that its truths and principles shall become matter of positive deduction; until such shall happen, while any degree of doubt or mystery shall remain spread over the operations of nature, it should clearly be the duty of the statesman to yield every encouragement in his power to advance and promote truth in the science of medicine, and by every means in his power to oppose the progress of error and set his face against quackery and deception.

I think also that the numerous medical students that now flock to the schools in Toronto require to be considered in this matter; these are individuals who are bound by every honourable incentive to study the medical profession with untiring zeal and industry; to apply their whole minds to a science that shall take them years efficiently to acquire its principles, and to obtain a practical aptitude so necessary to its useful prosecution. Nay, even the law of the land demands that the medical student shall occupy a certain time in the apprehension of the facts of his profession, and requires him to undergo an examination before he can obtain a certificate to practice that profession; and then it places him on a level with the quack, who is permitted to practice without being required either to study the medical profession, or to give a public exhibition of the amount of knowedge that he possesses of it. This is a manifest injustice, a legislation against a class who, to say the least of it, profess to honour the truths of science, and who surely have followed the only steps to acquire a competent knowledge of the profession by hard study and diligent application. It is then both a deception and an injustice to call for any exhibition of professional knowledge from the medical student if he is to gain no benefit by it. Why not give him the same privilege as the quack? The answer is, because you hope by such means to encourage a proficient class of professional men, who shall serve the public with zeal, fidelity and truth. Does the public think that the learning which the medical man obtains at the class room is to suffice for his progress through life? I am sure you do not think so. No, for he there only learns the true method of induction that must be more or less followed in every case, in every patient that is presented for his consideration. The processes adopted in the schools are held out to the medical student as promise and encouragement for his future success. Why should he be afterwards deceived and disheartened? Why should not similar encouragement be held out to the medical practitioner in after life, so that he may still go on improving for his own and his country's good; and why should he be discouraged by finding ignorance, presumption and quackery, without any of the necessary guards which are required from true science, exalted to the first position in society, and placed upon a level in public estimation and legislative condition with truth, honour and honesty? It is really a monstrous condition of things, that surely demands a speedy revision. It is a discouragement to the due study of medicine, and will ultimately end in the total abandonment of the correct apprehension of science, and the employment of persons as medical men who have but the vagaries of science to recommend them.

It is clear from the facts herein advanced that common justice and the public good equally require that the medical profession in Canada West should obtain an act of incorporation. Let me now consider what should be the nature of that act. It would appear just that all the medical practitioners who enjoy a license from the Medical Board of this Province should be constituted into the College of Physicians and Surgeons of Canada West; that twenty of the seniors upon the list of the Board should constitute the Council of the College, five or seven of whom should form a quorum for the transaction of business; the senior for the time, being the President of the College for life. This Council to have full power for the direction of the affairs of the College, to draft a constitution and to make bye laws for the guidance and regulation of the medical profession in Canada West. It should also be enacted that no individual be permitted to practice medicine or surgery without having passed a proper examination before the Council of the said College and having become a member of the said College of Physicians and Surgeons of Canada West, and that this should be supported by a sufficient penal clause. It should be permitted to the Council of the College to hold property sufficient to maintain a proper establishment for all the purposes of the corporation and for the benefit of medical science, such as the formation of a museum and medical library for the use of the members of the said College. And here it would appear wise and just that the Government should make a grant of land to the said College to erect a building upon, and that a spot opposite the Upper Canada College in the City of Toronto now vacant and in the hands of Government would be a suitable location; also that a grant of £10,000 be made from the funds of the University College to build a proper establishment, in lieu of the advantages the medical profession formerly enjoyed from the funds of the Toronto University. That the parliament should fix the tariff of medical charges by proper ascertained data, obtained by evidence from the examination of competent persons before a Committee of the House of Assembly; this public safeguard would compensate for the penal enactment against practising without being a member of the College. That a similar process be resorted to—to fix by law—to ascertain the amount of study and qualification that should be considered necessary to be enjoyed, before an individual should be eligible to offer himself for examination to become a member of the said College, and that upon obtaining a diploma the parties should pay a certain fee that should be applied to the increase of the library and museum, as well as to compensate the examiners for their loss of time and trouble in the matter.

Moreover, that all apothecaries, druggists, and venders of medicine should be required to have a competent knowledge of the business they follow, and be obliged to obtain a certificate of the same from the College.

And lastly, that all midwives should be obliged to gain a certain amount of practical and theoretical knowledge of their business, before they are permitted to practice it, and should be obliged to undergo a certain examination before Council of the College and obtain a certificate of the same.

The need of a medical library and museum which shall be open and accessable to all the members of the profession in Canada West, is an absolute necessity. It is perfectly impossible for the funds of private individuals to accumulate a respectable medical library in this country, and it is impossible for them to obtain a sight or knowledge of all the new publications that are continually issuing from the medical press; in most cases, the works that fall to their share are few indeed; but with the advantages of a public library, from which they could obtain an ample supply upon the payment of an annual subscription, would be a vast public benefit, and readily bring home to the poor members of the medical profession a great amount of knowledge that would be employed for the public good. It is absurd to suppose that a mechanic needs a greater amount of knowledge than a physician; but he often gets aid from the State in the purchase of his library; agriculture also gets encouragement under similar circumstances, and why should the medical profession go unheeded and unassisted in this matter? Knowledge in the one case would give an excellent polish to the workman, but without sufficient knowledge the medical and surgical practitioner is like a mad-man armed with a two edged sword, that cuts without consideration and judgment, and is more likely to do harm than to do good.

Having then laid this matter of the incorporation of the medical profession before you, may I trust that you will give it an early consideration and inform me, for the benefit of the profession, what action you are likely to take in this matter.

I have the honour to be, Sir,

your very humble and obedient servant,

S. J. STRATFORD,

Editor Upper Canada Medical Journal.

Toronto, C. W., Dec. 28, 1853.

QUEBEC, January 12, 1854.

DEAR SIR:—I hasten to acknowledge your important and able letter bearing upon our noble profession. Our peculiar constitutional position may not admit of the early legislation you desire and the question deserves, and therefore hope to have the pleasure of seeing you and discussing the whole matter before parliamentary action can be taken. Should it be otherwise, I will let you know and communicate with you more fully. * * * *

I am, dear Sir, yours faithfully,

S. J. STRATFORD, Esq. &c. JOHN ROLPH.

DOCTOR BEAUMONT'S CASE OF ANEURISM.

Since we transmitted to Dr. Beaumont a proof of the lecture and remarks upon it, we have had the pleasure of seeing the patient affected with aneurism, whose case is reported in this journal; and we have the satisfaction of being able to confirm the statement made by Dr. Clarke, the House Surgeon, (as far as we could judge by the eye,) that the aneurismal sac had very nearly disappeared; we observed that the skin and areolar tissue under it were puckered and appeared loose over the tumour, the cicatrix standing out like a nipple. There appeared to be considerable pulsation in the carotid artery, but the disappearance of the tumor was almost complete; convincing us that the contraction of the aneurismal sac was the means of curing the disease. To our mind, there must have been a peculiarity in the wound of the artery; it must have been very small, and the instrument must have entered the artery and its sheath at an angle; hence the comparatively permanent arrest of the hæmorrhage by a clot formed between the artery and its sheath; this served to plug the wound in the artery: for if the ordinary hæmostatic principles only had had effect without the sheath, there would certainly have been much more extravasation than was present in this case.

The result of this case is certainly a signal illustration of the principles laid down by Mr. Guthrie, that in little over seven weeks from the time Dr. Beaumont commenced the treatment of this aneurism, after Valsalva's method, the tumour should have disappeared. The case does Dr. Beaumont's management infinite credit, and will be a source of gratification to the great master in surgery who first advocated this kind of treatment in such cases. There are few wounds, however, of so large an artery as the common carotid, in which hæmorrhage at the time of the accident can be arrested by such simple means as were employed in this case; commonly, nothing but deligation will suffice. This case is but an exception to the general rule: hence we must not be misled in our practice and leave such cases to nature; but when we find that such has been the result—that bleeding has not occurred for several days, and adhesion by the first intention has really taken place—then we may safely follow out the principles laid down by Mr. Guthrie. We understand that Dr. Beaumont means to publish a more full and complete history of the case than we are able to give; it will, no doubt, be found of the highest interest to the profession, both in Europe and on this Continent.

MEDICAL TARIFF.

Having published in the *Medical Journal* the tariff of medical fees issued by a portion of the medical practitioners of the City of Toronto, we cannot refuse insertion to a similar document emanating from some of the profession in the County of Halton, C.W. The diversity of medical charges, and the confusion that arises from it in the minds of both patient and practitioner, is equally a source of great inconvenience to the public and the profession, often giving rise to most unpleasant disagreements between them. We, however, hope that a change in this matter is at hand, and sincerely trust that so fruitful a source of litigation will be set at rest by the legislature during the next session of the provincial parliament.

	Minimum. £	s.	d.		Maximum. £	s.	d.
Day visits in villages	0	2	6	...	0	5	0
Night do. do.	0	5	0	...	0	10	0
Day visits into the country—for first mile, 5s.; and for each subsequent mile, 1s. 3d. to 2s. 6d.							
Night visits into the country—for first mile, 5s.; and for each subsequent mile, 2s. 6d.							
Consultation visits	0	10	0	...	0	10	0
Mileage to be charged as above.							
Detention in any case, per hour	0	2	6	...	0	2	6
Midwifery cases (12 hours)	1	5	0	...	1	5	0
Each subsequent hour	0	2	6	...	0	2	6
Instrumental and complicated cases	2	0	0	...	5	0	0
Fracture or dislocation of upper extremity	1	5	0	...	2	10	0
Do. do. lower do.	2	10	0	...	5	0	0
Subsequent attendance to be charged as usual.							
Capital operations	5	0	0	...	10	0	0
Minor do.	1	5	0	...	2	10	0
Subsequent attendance to be charged as usual.							
Advice at office	0	2	6	...	0	5	0
Bleeding, Vaccinations, Tooth Drawing, Opening Abscess, &c. &c.	0	2	6	...	0	5	0

At a public meeting of the Medical Practitioners of the County of Halton, held at Potsville, on the 24th of December, 1853, it was unanimously agreed to adhere to the above scale of Fees.

NATL. BELL, J. COBBAN,
G. S. HEROD, CHAS. GARDNER,
W. C. WRIGHT, C. W. FLOCK,
D. D. WRIGHT, J. B. CUNNINGHAM.
CLARKSON FREEMAN.

SELECTED MATTER.

A COURSE OF LECTURES ON ORGANIC CHEMISTRY.

Delivered in the Laboratory of the Royal Institution of Great Britain, by Dr A. W. Hofmann, F.R.S., Professor at the Royal College of Chemistry.

Lecture V.

I mentioned to you in the last lecture that a mode of verification frequently employed in ascertaining the formulæ of organic compounds consists in the determination of their specific gravities in the state of vapour. To-day it is my intention to explain to you the manner in which chemists avail themselves of this verification; but, in order to do so, you must allow me to recal to your minds a few facts, elicited in the study of the relations which exist between the equivalents of the elements and their specific gravities in the state of gas or vapour.

In the following table the specific gravities of several elements are compared with their equivalents:

	I.	II.	III.			IV.
Hydrogen	1·	0·0692	1·	or	1×1	1
Nitrogen	14·	0·9713	14·03	or	1×14	1
Chlorine	35·5	2·44	35·26	or	1×35·5	1
Bromine	80·	5·39	77·89	or	1×80	1
Iodine	127·	8·716	125·95	or	1×127	1
Mercury	100·	6·976	100·80	or	1×100	1
Oxygen	8·	1·10563	15·97	or	(2× 8)	½
Phosphorus	31·	4·326	62·51	or	(2×31)	½
Arsenic................	75·	10·37	149·88	or	(2×75)	½
Sulphur	16·	2·218(a)	32·05	or	(2×16)	½

(a) At 2120° F. (1000° C.), according to recent researches of M. Bineau.

Column I. gives the equivalents of the elements; column II., the specific gravities referred, as usual, to the weight of air, as unit; in column III. you find the specific gravities, the weight of hydrogen being taken as unit.

By comparing the numbers in column III. with those in column I., it is evident that they coincide in the first six substances, and that, in the following four—oxygen, phosphorus, arsenic and sulphur—the numbers in column III. are double those in column I.

Now, as the numbers in column III. represent the weights of equal volumes—being, as I have stated, the specific gravities—it is evident, that the volume occupied by one equivalent of hydrogen being assumed as unit, the equivalents of nitrogen, of chlorine, bromine, iodine and mercury, form likewise one volume of gas or vapour; while the equivalents of oxygen, phosphorus, arsenic and sulphur, respectively correspond to half a volume of gas or vapour.

For the sake of perspicuity, the volumes occupied by the equivalents of the several elements are likewise given in our table—namely, in column IV. This column exhibits in a striking manner the simplicity of the proportions in which the elements combine with each other. These numbers, which represent the *volumes* in which the elements combine, with reference to hydrogen as unit, are called combining measures, combining volumes, or equivalent volumes, in contradistinction to the terms, combining numbers, combining proportions and equivalents, which apply exclusively to *weights*.

The combining proportion, or the equivalent of a compound is, as you recollect, obtained by adding the combining proportions or equivalents of the elementary constituents.

The combining measure, or equivalent volume of compounds, is frequently obtained in the same manner.

Thus, the combining volumes of the well-known hydrogen-acids of chlorine, bromine and iodine, are simply ascertained by summing up the constituent volumes.

The atomic composition of these acids is represented by the formulæ—

$$HCl \qquad HBr \qquad HI$$

If we translate these formulæ into volumes, we have, in the case of hydrochloric acid,

H	1	=	1 volume of	hydrogen.
Cl	35·5	=	1 "	chlorine.
HCl	36·5	=	2 volumes of	hydrochloric acid.

The combining volume of hydrochloric acid, then, equals 2 volumes of gas: *i. e.*, the equivalent of hydrochloric acid (36·5) is represented by two volumes; the equivalents of hydrobromic acid (81) and hydriodic acid (128) likewise correspond to 2 volumes of vapour.

Again, the equivalent of cinnabar or protosulphide of mercury is represented by HgS.

Hg	=	100	=	1 volume of	mercury-vapour.
S	=	16	=	$\frac{1}{2}$ "	sulphur "
HgS	=	116	=	$1\frac{1}{2}$ volume of	cinnabar vapour.

The combining volume of cinnabar is $1\frac{1}{2}$; *i. e.*, the sum of the combining volume of mercury and the combining volume of sulphur.

It appears, then, that in the case of the four substances just mentioned, and in that of a great many others, we obtain the equivalent volumes simply by adding the constituent volumes.

But it frequently happens, that the sum of the constituent volumes does not represent exactly the equivalent volume. A contraction frequently occurs during the combination of the elements, and the volume of the newly-formed compound is less than the sum of the volumes before chemical combination had taken place.

In such cases, however, we find that the actual equivalent volume invariably bears a very simple relation to the ideal volume, if by that term I may be allowed to express the sum of the equivalent volumes.

Thus we find the equivalent volumes of ammonia (NH_3), of water (HO), and of sulphuretted hydrogen (HS), to be as follows:

N	=	14	=	1	volume of	nitrogen.
H_3	=	3	=	3	"	hydrogen.
NH_3	=	17	=	4	"	the mixed gases, found by experiment to become
				2	"	ammonia.
H	=	1	=	1	"	hydrogen.
O	=	8	=	$\frac{1}{2}$	"	oxygen.
HO	=	9	=	$1\frac{1}{2}$	"	the mixed gases, found by experiment to become
				1	"	aqueous vapour.
H	=	1	=	1	"	hydrogen.
S	=	16	=	$\frac{1}{2}$	"	sulphur vapour.
HS	=	17	=	$1\frac{1}{2}$	"	the mixed gases, found by experiment to become
				1	"	sulphuretted hydrogen.

Experience has proved, that, in all cases which have hitherto been carefully examined, whether the elements combine without change of volume, or whether a contraction takes place during combination, that equivalent weights of compounds, when converted into vapour, occupy a bulk which corresponds to one of the three cases which I have just mentioned; namely, that they fill either 1 volume, like the equivalent of water; or 1½ volume like the equivalent of cinnabar; or, lastly, 2 volumes, like the equivalents of hydrochloric, hydrobromic, and hydriodic acids, and like ammonia. It has been found, moreover, that the rarest cases are those in which the equivalent corresponds to 1½ volume, that those in which it correspond to 1 volume are more frequent, whilst those in which the equivalent represents 2 volumes occur by far the most frequently.

This result having been established by a very considerable number of observations, it is evident that the examination of the volume, which the equivalents or formulæ of compounds occupy when converted into vapour, must furnish a very valuable mode of testing the correctness of the equivalents or formulæ in question.

Suppose that we are studying a new compound, that we have ascertained the relative proportions of the elementary atoms of which it consists by the processes which have been already explained to you, and that we have established the simplest atomic expression of the compound; we now want to determine whether this formula represents the equivalent of the compound. All our attempts to combine the substance with bases or acids have failed, the origin of the compound is unknown, it yields no characteristic products of decomposition. But, suppose that we are able to ascertain the volume corresponding to a weight of the compound expressed by the formula, and that we find this weight corresponding to two volumes of vapour, we admit at once the formula as representing the equivalent; if it corresponds to 1 volume, it may still pass. On the other hand, it becomes doubtful if it corresponds to 1½ volume. But we reject the formula as unlikely to represent the true equivalent of the compound if we arrive at any other number of volumes, such as 2½, 3, or 4, etc.

The question then resolves itself into this, How can we experimentally find the volume which the formula of a chemical compound represents? It is evident that chemists possess a simple and perfectly trustworthy method, in the determination of the specific gravities of the vapours of such compounds. Suppose we refer, as I have done in the above table, all specific gravities of gases and vapours to hydrogen as unit, instead of air or oxygen, which are frequently assumed as the standards of comparison, we find in such cases that the specific gravities of the substances which have already been named are represented by the following numbers:

Hydrogen	1
Hydrochloric acid	18·02
Hydrobromic acid	39·46
Hydriodic acid	64·11
Ammonia	8·61
Water	9·
Sulphuretted hydrogen	17·18
Sulphide of mercury	77·3

What do these numbers represent? Evidently the specific gravities of the gases or vapours in question, but in addition they represent the absolute weights of 1 volume of these gases, inasmuch as we have agreed that 1 (by weight) of hydrogen should also be 1 of hydrogen by volume.

There is now no longer any difficulty in determining the volume corresponding to the equivalent: it is obtained by a simple rule of proportion. Assuming the case of hydrochloric acid, the equivalent of which is 35·5 + 1,

$$18{\cdot}25 \; : \; 36.5 \; = \; 1 \; : \; x$$

$$x = \frac{36{\cdot}5}{18{\cdot}25} = 2$$

The specific gravity of the vapour of sulphide of mercury is 77·3, its equivalent being 100 + 16 = 116. The combining volume is obtained by the proportion:

$$77{\cdot}3 \quad . \quad 116 = 1 : x$$

$$x = \frac{116}{77{\cdot}3} = 1{\cdot}5 \text{ or } 1\tfrac{1}{2}$$

Lastly, in the case of water, both the specific gravity of the vapour and the equivalent being = 9, it is obvious, without any calculation, that this latter corresponds to one volume of vapour.

The rule then would be:—The quotient obtained by dividing the equivalent by the specific gravity of the vapour, (both referred to hydrogen as unit,) represents the combining volume of the compound. This quotient should be 1, 1½, or 2; its utmost excess over one of these numbers ought in no case to be greater than may be fairly attributed to unavoidable errors in the determination of the specific gravity of the vapour.

Let us apply this method of controlling the equivalent—or, what amounts to the same thing, the chemical formula—to the three compounds, the analysis of which has engaged our attention in the preceding lectures. The specific gravities of the vapours of benzoic acid, aniline and benzol, have been found to be those given in the following table, which likewise shows the equivalent at which we arrive:

	Specific Gravity.	Formula.	Equivalent.	Combining Vol.
Benzoic Acid	61·	$C_{14}\,H_6\,O_4$	122	$\frac{122}{61}=2$
Aniline................	46·5	$C_{12}\,H_7\,N$	93	$\frac{93}{46{\cdot}5}=2$
Benzol	39·	$C_{12}\,H_6$	78	$\frac{78}{39}=2$

The volumes corresponding to these formulæ are represented by the quotient given in the last column of this table. It is evident that the equivalents of benzoic acid, of aniline, and of benzol, correspond to two volumes of vapour; and hence the determination of the specific gravities of these substances affords additional evidence in favour of the formulæ, which we established by other means.

But suppose that, in the case of benzol, the true formula of this substance could not have been deduced in the manner in which it has been done: suppose that, unable to trace the origin of benzol, or to form products of decomposition, we had been compelled to verify the original formula ($C_2\,H$) by the determination of the specific gravity of the vapour, what would have been the result of our experiments?

We should have found the specific gravity of benzol exactly as before—39; but, since the weight represented by the formula $C_2\,H$ would then have been only 13, the quotient $\left(\frac{13}{39} = \frac{1}{3}\right)$ would have at once pointed out the improbability of the formula $C_2\,H$. A combining volume ⅓ not having yet been observed, we should have rejected the formula $C_2\,H$, and should have multiplied by 6, so as to obtain $C_{12}\,H_6$, whereby the equivalent would be raised from 13 to 78, and the combining volume from $\frac{1}{3}$ to $\frac{1}{3} \times 6 = 2$, the combining volume generally observed.

Now that I have explained to you the manner in which the knowledge of the density of a vapour assists in establishing, or at all events, in testing, the formula of a compound, I will exhibit to you experimentally the several operations which are involved in taking the specific gravity of a vapour.

Let me remind you of the object of the experiment. We seek to find the weight of a certain volume of vapour; by comparing this weight with the weight of an equal volume of hydrogen, oxygen, or air, taken at the same temperature and at the same pressure, we obtain the density or the specific gravity of the vapour referred to hydrogen, to oxygen, or air, as units. In practice, the densities of vapours are generally compared with that of air; but the relation between the specific gravities of air and hydrogen having been ascertained with the greatest accuracy, a simple calculation is sufficient to translate densities, determined with reference to air, into densities referred to hydrogen as units.

There are two methods of ascertaining the specific gravities of vapours, which involve essentially different modes of manipulation. The one consists in accurately measuring the volume of vapour which a given weight of substance produces at a given temperature. It was originally suggested by Gay-Lussac, who employed it successfully in a great number of experiments. The second method proceeds in the opposite direction; it determines the weight of the vapour, which fills a vessel of given capacity at a given temperature. The latter process, for which we are indebted to M. Dumas, whose ingenuity first directed the attention of chemists to the importance of the density-determination of vapours, as a general method of research, is much simpler; it has, in fact, superseded the former one, at least for chemical purposes, being the only method at present practised in the laboratory.

The vessel used in this operation is a globe of glass, of from twenty to thirty cubic inches capacity, provided with a slender glass-tube. As these globes are always blown, they are apt to contain a certain quantity of moisture, and this has to be carefully removed. For this purpose, the vessel surrounded with hot sand, is connected with an ordinary hand-syringe, and repeatedly exhausted, air being admitted again from time to time through a chloride of calcium tube. We thus succeed in replacing the moist air in the globe by perfectly dry air. The tube is now heated before the blowpipe, and drawn out into a fine point, which is bent so as to form an angle of 90° or 100 with the remainder of the tube.

If this vessel be weighed, after having been successively filled with air and with the vapour, the density of which is to be determined,—if we succeed, moreover, in actually determining the weight and the capacity of the globe, it is evident that we have all the data which are necessary for calculating the specific gravity of the vapour.

We begin with weighing the globe, filled as it is with dry air upon an accurate balance. Let us suppose that the globe is found to weigh in this state 750 grains. This number represents the weight of the glass as well as that of the air contained in it. We have to recollect, moreover, that the weight of a given volume of air materially depends upon the temperature and pressure at which it is taken, and on this account we accurately observe the thermometer and barometer. I find that the temperature of the room is about 15·5° C. (60 Fahr.) The barometrical column I will, for the sake of simplicity, assume to be the normal one, that is, 30 inches.

The next operation we have to perform consists in introducing the substance under examination into the globe. This appears at the first glance, rather difficult, on account of the extremely narrow orifice of the point, which is nearly capillary, but it may be readily effected, as you observe, by calling in the aid of atmospheric pressure. For this purpose, I heat the globe over a spirit-lamp, and in this manner expel a certain quantity of air. By now immersing the open point into the liquid which is to be introduced, (I will take benzol, the compound which has repeatedly engaged our attention,) the pressure of the atmosphere will force a certain quantity of the liquid into the globe as it cools, a process which may conveniently be accelerated by sprinkling ether over it. We thus readily succeed in introducing from 100 to 200 grs. of substance, the quantity necessarily depending to a certain extent upon the capacity of the globe. The liquid is now heated to ebullition, when its vapours will gradually sweep out every trace of atmospheric air. The operation of heating is generally performed in a water bath, or in an oil bath, according to the temperature at which the substance under examination boils. In the case of benzol, which forms the subject of our experiment, the temperature of boiling-water is not sufficient. I have, therefore, slightly raised the boiling point of the water by dissolving some chloride of calcium in it. The water bath which I use is peculiarly arranged for this kind of operation. You observe it is a cylindrical vessel of copper, provided with two metallic stems, one on each side; one of these holds an arm with two rings, between which the globe may be fastened; to the other a thermometer is attached, which indicates the temperature of the bath. The vessel having been well secured between the two rings, I lower the arm and immerse the globe entirely in the boiling liquid, so that only the open point projects. After a few seconds, the liquid enters into ebullition, and torrents of vapour escape from the point. This you at once recognise by the hissing sound produced, and it may be made even more perceptible on bringing a spirit lamp to the orifice of the point, when the vapour will be set on fire. At first a mixture of air and benzol-vapour issues from the point, burning with a pale blue flame, but gradually the benzol-vapour begins to predominate, and the brilliancy of the flame shows you that the air is rapidly diminishing. After a few minutes, the quantity of gas escaping from the globe perceptibly diminishes, the flame becomes smaller and smaller, and is at last extinguished. If a taper held before the orifice burns quietly we consider that the excess of liquid is entirely expelled, and that the whole globe is filled with benzol-vapour of the temperature which is indicated by the thermometer of the bath. I observe at this moment 110° C. (230° Fahr.) The barometer, we will suppose, has not changed since last we observed it, which is generally the case, but should be carefully observed in accurate experiments. The point of the globe is now exposed to a powerful blow-pipe flame, and accurately sealed. A certain attention is necessary in this operation, on the dexterous performance of which depends the success of the experiment. After the whole of the liquid in the globe is converted into vapour, an accidental depression of the temperature of the bath has to be carefully avoided, as it would be followed by the introduction of a small quantity of air, which could no longer be entirely removed.

The globe is now taken out of the bath, carefully dried, and weighed again with great accuracy. Suppose it now weighs 758·36 grains. This number represents the weight of the globe itself, and that of the vapour at the temperature 110° C. (230° Fahr.), and 30 inches pressure.

It now only remains to ascertain the weight of the glass, and the exact capacity of the globe. These data are obtained by the following operation: The globe being perfectly cooled, its point is immersed in mercury, gently touched with a sharp file and broken off, the whole operation being performed under mercury. The vapour being perfectly condensed into a liquid, the pressure of the atmosphere forces the mercury into the globe, and gradually fills it, if the operation has been successful. Sometimes a small space of the globe is not filled up. This shows that the air had not been altogether expelled, or that a small quantity had entered again by a slight depression of temperature during the operation of sealing. The experiment is not useless on this account, but an additional observation becomes necessary, and the calculation becomes more complicated. The globe being filled with mercury, the volume of this metal is now carefully determined by pouring it out into a graduated measure. Suppose we find that the volume of mercury amounts to 23 cubic inches; it is evident then that the capacity of our globe is 23 cubic inches, which must have been likewise the bulk of the air and of the vapour successively contained in this globe.

We have now all the data which are requisite for the determination of the density of benzol-vapour. In the first place, we calculate the weight of the glass globe alone. You recollect that our first weighing furnished us with the weight of this globe *plus* the weight of a certain volume of air of 15·5°C. (60° Fahr.) and 30 inches pressure. We have since determined the volume of this air; there is no longer any difficulty in calculating its weight. According to the latest researches of Regnault 100 cubic inches of 15·5°C. (60° Fahr.) and 30 inches pressure(*a*) weigh 30·83(*b*) grains. The weight of 25 cubic inches is, therefore $\frac{30{\cdot}83}{4} = 7{\cdot}7$ grains. By deducting this weight

(*a*) More accurately, 29·92 inches.
(*b*) More accurately, 30·82926.

from the joint weight of the globe and of the air we arrive at the weight of the empty glass globe:

$$750 - 7 \cdot 7 = 742 \cdot 3$$

And, lastly, by deducting the weight of the globe from the joint weight of the globe and of the benzol-vapour, we find the weight of 25 cubic inches of benzol-vapour of 110° C. (230° Fahr.) and 30 inches pressure—

$$758 \cdot 36 - 742 \cdot 3 = 16 \cdot 06$$

The experiment then has taught us, that 25 cubic inches of air weigh 7·7 grains, and that the same volume of benzol-vapour weights 16·06 grains. Both volumes have been observed under the same pressure, but not at the same temperature. The air having been weighed at 15° C. (60° Fahr.), the benzol-vapour at 110° C. (230° Fahr.), it therefore remains now to calculate either the weight of 25 cubic inches of air at 100° C. (230° Fahr.), or that of 25 cubic inches of benzol-vapour at 15·5° C. (60° Fahr.) These calculations may be easily effected, the expansion and contraction of gases, as depending on variation of temperature, having been carefully established. Without entering into the detail of these calculations, which I could not clearly explain without diverging too much from the main subject of our inquiry, let me tell you, that 25 cubic inches of air at 110° C. (230° Fahr.), weigh 5·8 grains.

The specific gravity of the benzol-vapour is therefore found by the proportion—

$$5 \cdot 8 : 16 \cdot 06 = 1 : x$$

$$x = \frac{16 \cdot 06}{5 \cdot 8} = 2 \cdot 77$$

And if it was our object to know the specific gravity of benzol with reference to hydrogen, we recollect that the number has to be increased in the same ratio in which the specific gravity of air is higher than that of hydrogen. We have the proportion:

$$0 \cdot 0692 : 2 \cdot 77 = 1 : x$$

$$x - \frac{2 \cdot 77}{0 \cdot 0692} = 39$$

which is the number quoted as representing the density or the specific gravity of benzol-vapour.

In the preceding sketch I have omitted to describe to you many of the minor precautions which have to be observed in order to insure the success of an experiment. I have, moreover, slightly modified the ordinary calculation. But my task being to bring the main features of these questions before you in as intelligible a form as I possibly can, I have considered myself justified in sacrificing exhaustion of the subject to simplicity of design, and absolute accuracy to transparency of delineation.—*Medical Times & Gazette.*

ON A NEW METHOD OF DETERMINING THE QUANTITY OF UREA IN THE URINE.

By John W. Draper, M.D., Professor of Chemistry and Physiology in the University of New York.

Much attention has of late been paid to the methods of determining the composition of the urine, it being very generally acknowledged, that if we possessed the means of a quick and accurate analysis of it, we should be able to settle many contested questions both in physiology and pathology.

Among the constituents of the urine, the nitrogenized bodies, urea and uric acid, are perhaps of the greatest interest, for they represent the waste which has taken place in the soft tissues generally. Accordingly from time to time new processes have been published for the estimation of these bodies, and more particularly of the first—urea. The methods recommended in the works on animal chemistry and organic analysis are, however, very far from satisfactory.

Thus Simon, in his *Chemistry of Man*, effects the determination of the quantity of urea by forming the sparingly soluble nitrate, and Bowman, in his medical chemistry, resorts to the acetate, but both of these are very tedious and very disagreeable operations, and what is worse, uncertain in their results. Liebig has recently recommended the ternitrate of mercury, but the preparation of the test liquors is troublesome, and since the estimate eventually depends on the production of a particular tint or shade of a yellow color, it cannot be exact.

There are however some simple methods which will give absolutely accurate results. These all depend on the principle, that urea and uric acid, when brought in contact with nitrous acid, undergo immediate decomposition with a brisk effervescence, owing to the escape of carbonic acid and nitrogen gas.

The quantity of these nitrogenized principles in the urine may be ascertained by determining the quantity of carbonic acid or of nitrogen thus set free, during the destructive decomposition of urea and uric acid by nitrous acid. Forty-four parts of carbonic acid, or twenty-eight of nitrogen, answer to sixty of urea.

One of these methods which is extremely exact, I have recently described in the London and Edinburgh Philosophical Magazine. It is to conduct the disengaged carbonic acid into water of barytes, and weigh the resulting carbonate of barytes.

I have also, in examinations which I am constantly making of the urine, frequently resorted to the other plan of estimating the urea, from thequantity of nitrogen set free; and this I have done in two different ways:—1st, by determining the quantity of nitrogen by weight; or 2nd, by volume. The following is a more particular description of each of these:

A liquid suitable for the decomposition of urea is easily and economically prepared by taking a single cell of Groves' voltaic battery, and placing strong nitric acid in the porous cup, and otherwise charging the cell in the usual way. After a few minutes the nitric acid turns green, becoming charged with nitrous acid. It is then to be decanted for use. If this liquid be poured into urine, filtered from its mucus, or into a solution of urea, a brisk effervescence sets in, and if a sufficient quantity of acid is used, so that red fumes are disengaged, the urea is totally decomposed, carbonic acid and nitrogen gases escaping.

In the first of the preceding methods, viz.—That of determining the urea from the weight of the nitrogen, a known weight of urine (2 grammes), filtered from mucus, is placed in a bottle containing a tube filled with the nitroso-nitric acid above described: from the bottle a bent tube conducts the escaping gases through potash-water, and then through a chloride of calcium tube. The operation is conducted in the manner well known to laboratories for the analysis of the carbonate, the loss of weight of the whole apparatus gives the quantity of nitrogen which has been set free. This operation requires about half an hour, and is quite exact.

In the second method, viz.—that of determining the urea from the volume of the resulting nitrogen—the operation is essentially the same, only instead of letting the nitrogen escape into the air, it is received into a gasometer, and its quantity ascertained. As conducted in my laboratory, the amount of urea in a sample of urine may thus be determined in from ten to twelve minutes, and with certainty, to one thousandth part of the weight of the urine; a degree of exactness far beyond that of the old processes, and an expedition which at once recommends this method to the physiologist and pathologist.—*From the Virginia Medical and Surgical Journal.*

ILLUSTRATIONS OF TUBERCLE.

By Dr. Edward Henry Sieveking, Assistant Physician to St. Mary's Hospital, &c.

[The term tubercle, though synonymous with "nodule," has generally come to imply a certain form of deposit occurring in a peculiarly fatal diathesis. The importance of this need not be dwelt upon here.]

That consitutional tendency which leads to the deposit of tubercle, and is to be found wherever the necessary stimuli of life, and, above all, pure atmospheric air, are deficient, predisposes to other diseases as well; it assists the approach of the enemy, and actually places the fortress in his power before a formal assault is made. Though we no longer believe in the elements of Thales, we may, without a great stretch of the laws of the natural sciences, admit that air is the chief element of health or disease, according as it is supplied to the lungs in its unadulterated condition of four-fifth nitrogen and one fifth oxygen, or as it carries diffused through it carbonic acid gas, carbonetted hydrogen, sulphuretted hydrogen, the effluvia of cesspools and drains, the poison of influenza or cholera, the emanations of the variolous or typhous patient.

Allow me briefly to advert to the origin of tubercle We possess sufficient evidence to shew that it is derived from the blood: that it transudes from the capillary vessels of the part in which we find it: and that, after having been deposited it is liable to undergo certain further changes. On a close examination of incipient tubercular deposit, we may always note that there is congestion in the tissues immediately surrounding it. In the pia mater of the Sylvian fissure, we see an increased redness, in which a few vessels are more prominent than usual; in the pulmonary parenchyma we may, especially by the use of the microscope, discover the engorgement of the interlobular capillaries investing the air vesicle into which the tubercle is being secreted; in the mucous membranes of the intestines we see the exquisite arborescent arrangement of the congested vessels, tending from the mesentric attachment to the point where we observe the deposit shining through the mucous surface from the submucous tissue, in which it has collected.

The first elimination of the morbid products acts like a magnetic point of attraction, and generally serves as a centre round which the deposit progressively enlarges by eccentric deposition. The amount of vascular action accompanying the elimination, varies in different individuals; in some, there is scarcely a perceptible increase in the sanguineous current; in others, we cannot deny the presence of acute inflammation, shown both by the congested state of the blood-vessels, and by the presence of plastic exudation and exudation corpuscles. In ordinary inflammatory conditions, we may actually observe the part taken by the capillary vessels in the process of transudation.

We see the inflammatory product immediately after its passage through the vascular membrane, coating the vessels; and, if my limited observations justify the statement, we may see the same matter within the vessels adhering to the coats previous to its discharge. Whether it be so or not, whether we may be enabled to observe the transition of the contents of the vessels into the surrounding parts or not, it is evident that we ought not to be satisfied with ascertaining the fact of the exudation as the primary change. We are driven to take one step more, before we gain the fountain head of the malady; we therefore look to the constitution of the blood itself in tubercular disease, in order to ascertain whether any deficiency in the normal components, any variation in their relative amount, any new products, are to be met with, which may explain the source of the extravascular deposit. All observers, who have brought either the microscope or chemical analysis to bear on this subject, are agreed that there is an alteration in the blood indicating want of vigour and tone. There is a general increase in the fluid parts, the water and albuminous constituents; while the solids are diminished, the fibrine and the red corpuscles are reduced in quantity, and both exhibit what has been termed diminished vitality; the fibrine possesses less plasticity; the blood corpuscles are feebly formed, their outline less defined, their color faint, and the colouring matter easily yielded up to the surrounding fluid.—The actual relation of the white cell to the red corpuscle in various diseases has not as yet been satisfactorily demonstrated; but we are inclined to view an excessive development of the former as indicative of debility, and an aplastic, if not cacoplastic, condition of the blood; we certainly have noted an increase in their number in persons affected with tubercle. Many peculiarities in the blood of tuberculous individuals are also met with in the blood of individuals laboring under other diseases. We are not, therefore, justified in laying down absolute indications which are conclusive evidence of the tubercular infection; and if we are unable to define the specific constitution of the blood that accompanies tuberculosis, it follows, *à fortiori*, that we are not possessed of the means of predicating a mere tendency to tubercular deposit from the constitution of the blood. This however is the point to which we must hope to arrive, if, as we believe, one of the primary elements of the malady is traceable to the blood, and the seeds of the disease are sown, and therefore must be destroyed here, unless they be anticipated before their introduction into the body. On this point Mr. Ancell, in his laborious work on "Tuberculosis," judiciously remarks:—"The predisposition differs from the general disease only in degree, and the condition of the blood in the predisposition is the same, differing only in degree;" and so undoubtedly it is, but we yet want that positive and conclusive sign by which the predisposition may be recognised by analysis of the blood. It is manifest that, with regard to tuberculosis, as well as other diseases, such an indication would be of extreme value; for, as the diagnosis of morbid processes in the thorax has improved, our treatment of the disease has commensurately acquired greater simplicity and greater certainty. In the same way, it is tolerably certain that, if we discover the means of recognising the seeds of a malady before they had taken firm hold upon the system, we should be enabled to eradicate them, or to counteract their influence more effectively than we now can.

[Though the stomach is no doubt a great agent in the production of a tubercular disease, yet Dr. Sieveking inclines to the common opinion, that the organs of oxygenation have a greater share in its development than those of sanguinification. He says:]

Baudelocque, who has written some of the wisest remarks on the relation between the respiratory function and tuberculosis that I have met with, affords some very striking illustrations of this position. He states as the result of his examinations and experience, that a truly scrofulous disease is invariably caused by vitiated air, and that it is not always necessary that there should have been a prolonged stay in such an atmosphere. Often a few hours each day may suffice, and it is thus that patients may live in the most healthy country, pass the greater part of the day in the open air, and yet become scrofulous, because they sleep in a confined place where the atmosphere has not been renewed.

M. Baudelocque illustrates these observations by numerous well chosen instances; he refers, among others, to the shepherds of his country, who may become scrofulous although they lead an open-air life; but although, as he says, the disease with them is attributed to exposure to storms, to atmospheric vicissitudes, and to humidity, attention has not been paid to the circumstance that they pass the night in a confined hut, which they transport from place to place, and which protects them from rain; this hut has only a small door which is closed when they enter, and also remains closed during the day; six or eight hours passed daily in vitiated air, which is never renewed, is the true cause of their malady. I have spoken of the bad habit of sleeping with the head under the clothes, and the insalubrity of school-rooms in which a number of children are assembled together. The repetition of these circumstances is often a sufficient cause of scrofula, though they may last but a few hours a day. Human beings and animals are equally affected by vitiated air; close rooms, as Dr. Arnott has pithily remarked, act like extinguishers to the vital flame; and the extinction literally takes place at the point at which the fuel accumulates for want of being burnt off.

If space and time allowed, I would multiply the evidence that has been adduced in support of the particular view which I have dilated upon; but this is beyond the limits of this paper, and moreover, there is now scarcely a necessity for enforcing what, I believe, is the prevailing opinion among medical men.

To sum up, while we would not deny that defective supply of food and raiment influences the production of tubercular disease, no cause so certainly predisposes to and generates it as defective aeration. The exact part taken by the light in this matter cannot be appreciated. Its direct influence upon the health of everything living is proved irrefragably; but whether its absence can alone duce morbid states of a definite character, remains yet to be proved.

As the tubercular deposit is derived from the blood, it is not surprising that all the organs of the body are more or less liable to become the seat of the morbid product. Some tissues present a greater proclivity to the elimination than others: and some, as the fibrous and tegumentary tissues, appear to enjoy almost an immunity from tubercle. At the two ends of the scale, we may place the mucous membranes and the fibrous tissues; the former are the true soil for this tree of death; the latter are rarely, if ever affected.

There can be little doubt that this depends in a measure upon certain physical laws, influencing the current in the vascular system, and determining the greater or less facility of transudation, in the first instance. I should venture to suggest that we may lay it down as a law regulating the deposit of tubercle, that is effected at the point of an organ or of a tissue where the smallest amount of pressure is exerted upon the capillary system. This does not exclude the operation of other laws, which determine the attraction to any one organ. It does not offer any reason why in one case we find tubercle in the spleen, in another in the mesenteric or bronchial glands, in a third exclusively in the pulmonary tissues; but it seems to embrace the various circumstances modifying the exact site of the deposit in these different parts of the system. The *vis a tergo* varies but little in the different parts of the capillary system; but the relation to surrounding tissues differs very much. Thus, while the force with which the blood is driven into the interlobular plexuses of the lungs is identical, the pressure which the respective capillary systems meet with in a case of congestion, which implies a tendency to exudation, is necessarily greater in the bone than in the soft parenchymatous structure.

No organ is more frequently the seat of tubercular deposit than the lungs, and in none do we find the capillary ramifications of the vessels with so little covering. They almost lie naked on the surface. Beyond the basement membrane forming the air vesicles, and possibly a delicate epithelial layer, there is nothing between the capillary network and the atmosphere. We need not therefore wonder that the ultimate vesicle in which the bronchule terminates is, above all other points, that of tubercular election. The receptacle is ready: the product being in the blood, a slight increase of pressure will over-balance the natural and healthy equillibrium between the external and internal fluids, and the discharge takes place. If our view is correct, nothing but a previous change in the ultimate vesicles or bronchules could give rise to a deposit of tubercular matter in the intervesicular tissue in the parenchyma of the lung itself, as contradistinguished from the respiratory cavities. We can suppose the obliteration of a portion of the breathing apparatus might leave the intervesicular texture less resistent than the air vesicles; and in that case we should expect to find an interstitial deposit. Whether this does actually occur I am not prepared to say. I have not seen any appearance that would justify the assumption of a primary interstitial deposit, but I have seen a distinct deposit of tubercular matter within the air vesicles, and I have traced its primary deposit, in the semi-liquid form, in the solitary vesicles, to the deposit in numerous adjoining vessels, causing destruction of their breathing power and obliteration of the bronchule terminating in them.

[Dr. Sieveking observes that the ultimate bronchule is perfectly free and patulous, and that the tubercular matter fills the vesicles as a bullet fills its mould. He then proceeds:]

The law to which we have adverted as, in our opinion, regulating the deposit of tubercle. viz., that the tendency to the deposit in any organ is inversely as the pressure the vessels sustain, or that it is in the ratio of the laxity of the tissues, is supported by the views which are commonly held with regard to the chemical constitution of tubercle, by the form and mode of deposit in the various organs of the body, and it also assists us in explaining why certain parts of different organs possess so marked a liability to become the seat of tubercular exudation. This feature constitutes an essential difference between tubercle as a mere effusion of a certain constituent of the blood, and those other new formations in which we cannot but see a tendency to independent development or organization. The most familiar instance of pathological processes with which I would compare it, are the serous effusions that take place into the peritoneal cavity, from obstruction to the vena cava or portal system, inducing congestion and consequent liquid discharge at the most yielding points. If we adopt the view suggested, it appears to me to offer an explanation of the circumstances that the apices of both lungs are the chief seats of tubercle, while it tends to shew the importance of encouraging the use of all the physicial means at our command to promote a free and active circulation of the entire vascular current, and to obviate and anticipate anything approaching to local congestion in the organs and parts of organs which we know to be most liable, at different periods of life, and under different circumstances, to become affected with the disease in question.

The manner in which I would apply the law to the explanation of the predominant proclivity of the pulmonary apices, is simply this: the upper portions of both lungs are surrounded by more unyielding parietes than the inferior; they have less room for expansion; consequently, if there is any increase in the vascular current supplying these parts, the difference between the pressure of the parietes and of the atmosphere within the vesicles will increase unduly, and effusion will take place into the latter. In acute tuberculosis, we do not observe this peculiar election, because the process is of a more active character; the strain upon the capillaries of the entire organ is greater than they can bear, and we consequently find the deposit takes place with much uniformity throughout the lung. In the chronic forms in which tubercular deposit generally occurs, the balance of the forces in the different parts of the vascular system is in a measure preserved, and only the very weak points are assailed.

We do not at all deny that other forces come into play, and that there are elective affinities between different tissues and the morbid products with which we are not even acquainted as yet; but it appears that the circumcumstance alluded to is one of considerable importance in its bearings upon tubercle, both in the lungs and in the brain and abdominal tissues. To take a single instance from the latter: in scrofulous deposit in the kidneys, where does the tubercular matter invariably present itself? In the loose texture of the cortical substance. The dense tubular tissue, with its stronger basement membrane and firmer epithelial coat, wards off the encroachment; but the feebler texture of the convoluted tubes is unable to repel the enemy.

[Lastly, the author examines the ultimate constituents of the tubercular deposit, with a view to its general recognition and the cure of the condition on which it depends]

The simplest definition of tubercle appears to me to be, according to the present state of knowledge, the following: *Tubercle is an exudation from the blood of a protein compound, incapable of organisation, but undergoing certain physical changes independent of vital influences.* Tubercle is, in fact, effete matter which the powers of the system are unable to use as building material to repair the normal waste; and it is deposited in this or that organ of the body according as it is invited by the greater or less debility of the part. Tubercle is not a plastic material; it is not a growth; it is not the manifestation of a depraved germinating power superadded, as it were, upon the normal energies of the system, or taking their place, such as we find to be the character of malignant disease; nor, on the other hand, is it identical with the effusion of blood-constituents which result from an exaltation of the normal energies, and continue in possession of their vitality, by which they are susceptible of organisation. This we do not see in tubercular deposits, which must be viewed as bearing to the diathesis giving rise to it very nearly the same relation as, to use the strongest comparison that suggests itself to us, calculus in the bladder bears to the calculous diathesis to which it is due.

There is, unfortunately, no such emunctory for the effete protein compounds as there is for the excess of saline constituents of the blood; or tubercle might accumulate, as the latter do, at a given point, and the product be removed by operative proceedings, or by chemcial solution. The lungs and the skin have this duty to perform; but I need not stop to point out to you why we have not yet succeeded in destroying the tubercular product in the former, and removing it from them, by direct applications. I do not myself despair of a remedy being discovered which, in a gaseous form, may be conveyed to the deposit in the lungs, and, by dissolving it, enable the patient to expectorate it; but this would only affect a single organ. The cachexia leading to the local product will ever remain the real malady to deal with, so that we may anticipate its local effects.

The changes which take place in the deposit itself and which have been the source of much discussion, and of some very wild speculations, are, as I have already observed, closely allied to what we see taking place out of the human body in organic substances. They seem to follow the laws regulating crystallisation and chemical decomposition, rather than those of vital action.

The earliest form in which tubercle presents itself to the eye is that of a faintly granular blastema, in which we are only just able to trace a tendency to aggregation into circular forms.

The next form in which we find tubercle presenting is that of more definite corpuscles; they offer an oval form, with a more or less sharp outline, and a granular surface. These corpuscles are surrounded by the granular blastema before mentioned, which now becomes more definitely marked, and by and by appears to eliminate oil-globules in a greater or less quantity.

The tubercular corpuscle does not present a nucleus as its normal constituent; it is, in fact, regarded by some, among whom I may be allowed to mention Dr. H. Jones, as itself a nucleus. We occasionally find cells with nuclei in tubercular matter, but I am inclined to think that they are generally, if not always, derived from the normal tissues of the organs in which the deposit has taken place.

A term has of late been brought into vogue by the authority of great names, to which, before concluding this brief sketch, it is necessary that I should allude, as the subject to which it refers is closely associated with tubercular disease. I refer to *fibrinous deposits.* Many of the cases which are thus denominated present no differences perceptible, either to the naked eye or under the microscope, by which we can distinguish the product from tubercle; and in such it is scarcely in accordance with sound induction to assume a different disease, until we are able to demonstrate a distinct primary lesion in the blood.—*Association Medical Journal, May* 27, 1853, p. 451.

ON CHRONIC EXCORIATIONS OF THE TONGUE OF CHILDREN.

By Frederick Betz.

Professor Moller, of Konigsberg describes a chronic desquamative process of the tongue, which he had observed in six cases.* The patients were middle-aged females. The excoriations appeared in the form of deep irregular spots, for the most part sharply circumscribed, either altogether stripped of epithelium or very thin over them, while the hyperemic and swollen papillæ projected somewhat beyond the level of the surrounding parts. No morbid secretion could be observed on these spots, nor did any deep ulceration take place. They existed chiefly on the borders and tip of the tongue, more frequently on the under surface, and on the inside of the lips, never on the posterior parts of the mouth. They occasioned a troublesome sensation of burning, gave the patients a disgust for food, deprived them of the sense of taste, and interfered with the free motions of the tongue.

Dr. Betz observed a very similar disease in five cases in children, which he does not wholly identify with that described by Professor Moller, but the points in difference do not appear to be great. Perhaps Professor Moller may bring forward further communications on the subject, when he shall have had opportunities of observing his form of disease in children. A red spot of roundish or oval form appears on the edge of the point of the tongue, but never on its middle line, nor on the base, and is surrounded by a well-defined, often slightly elevated redder margin. This red, sharply defined spot increases from the edge inwards, extending in a curve, and when it arises behind advancing towards the tip. Dr. Betz has only seen it on the back of the tongue; but he has observed the spots in three situations at the same time—on the left half of the tip and on both margins of the body of the organ, the remainder of the tongue being covered with a whitish fur. The tongue is not swollen or harder in these spots, nor does any secretion or ulceration arise. The little patients made no complaint, nor did the disease appear to be an object for treatment. The morbid process consists in an exfoliation of the horny epithelium of the papillæ filiformes,† splitting at its points into many thread-like processes—a desquamation of the filamentary papillæ of the tongue. They consequentiy appear much lower on the red fleshy spot; even lower than the papillæ fringiformes. The spot so denuded is not painful to the touch. Each half of the tongue desquamates by itself: that is, the desquamation does not attack both halves at the same time, nor does it advance simultaneously on both. When the tongue has exfoliated, the process recommences after three, or six, or eight days. I have watched these chronic excoriations during three years in a boy in whom they existed since an attack of the jaundice, which occured about four weeks after birth. The other patients were girls, the eldest of whom was eight years of age.—In these children eczematous and impetiginous eruptions appeared from time to time on the face and on the hand. As the disease occurs before dentition, a bad tooth cannot be regarded as its cause. Dr. Betz could not perceive any influence on the motion of the tongue, or on the sense of taste. Since the desquamation of the epithelium is connected with hyperœmia of the filiform papillæ of the tongue, but without the occurrence of ulceration, the author would be inclined to substitute the designation "Pityriasis linguē" for that of chronic excoriation.—*Journal für Kinderkrankheiten. N. Y. Journal of Medicine.*

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* Deutsche Klinik, No. 26

† See Kolliker's Gewebelehre, 1852, p. 351.