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Original Communications.

ART. I.—*Clinical Observations upon Diabetes Mellitus.* By Prof. OPPOLZER. Translated by ALFRED L. HASKINS, M. D., Boston, Mass.

THE true cause of diabetes mellitus is very obscure, and will remain so, till we understand the material changes which the economy undergoes in the disease.

The investigations in this direction have been, till recently, very imperfect, and it is only within the last ten years that special attention has been given to the anomalies of this disease. The older writers had no true knowledge of it. We find, indeed, a great number of cases of copious secretion of urine described by them, but no mention is made that sugar was found in the urine. In the year 1668, Willis had the great merit of discovering the sweet taste of diabetic urine, and, soon after, other English chemists succeeded in demonstrating that sugar was present in the urine.

Indeed, to English physicians belongs the undisputed honor of making the first and most important investigations in this disease. At the end of the last century an English physician, named Rollo, made the important discovery that the primary cause of diabetes was not a disease of the urinary organs, but a disturbance of the whole vital economy, and more especially of the functions of the stomach. Quite recently German and French pathologists, Traube, Schiff, Brücke, and Claude Bernard, have supplied very valuable contributions to the pathology of diabetes, and it is now conceded that no local disease of the kidneys, but a general disease of the system, is the cause of diabetes. We know that the blood of diabetic patients contains sugar, that it is constantly found in the urine and the other secretions, and may, indeed, be shown in the perspiration.

Notwithstanding all the careful investigations which have been made heretofore, diabetes still remains a very mysterious form of disease, and a clear insight into its nature, in the present state of science, is impossible. In the first place, the question arises, Upon what does the abnormal quantity of sugar in the *blood* depend? Upon this point there prevail the most diverse and contradictory views. If we take the established facts of physiology to assist us, we find that in the process of digestion the nitrogenous food is changed into albumen. A part of the albumen is absorbed and conveyed to the organs and tissues; the remainder, on the contrary, is decomposed in a manner not yet perfectly known. The non-nitrogenous food, of which the hydrocarbons are the greater portion, is changed into dextrine and sugar. A part of the sugar as such, or after a further change, is absorbed as lactic acid; the remainder is changed in the intestines into carbonic

acid and alcohol, and finally into water. The water is given off through the kidneys, and the carbonic acid through the lungs. The fats also undergo a like oxygenation, and finally the same changes as the other hydrocarbons. The formation of sugar in the economy from the food is not, therefore, an abnormal process, and hence it may be explained how sugar appears in the urine of otherwise healthy persons, as was first shown by Brücke. In a normal state of the economy sugar is found especially in the vessels which come from the liver, from whence it is conveyed to the lungs. Concerning the causes which produce the sugar in diabetic urine very diverse theories are adduced. Some assume that the sugar in the blood of diabetic patients arises from a defective digestion, which produces an excessive amount of sugar at the expense of the amylaceous food.

Rollo, therefore, in his distinguished work regards the stomach as the seat of the evil and the abnormal formation of sugar, and represents the affection of the urinary organs as a secondary affection. Following this theory, he proposed the exclusion of all food except meat as the primary treatment of the disease. Others assert that a defective decomposition of the sugar taken into the blood is the cause of its extraordinary accumulation in this fluid.

Claude Bernard considers diabetes a disease of the nervous system. It is, indeed, a very remarkable fact discovered by this investigator, that, when the floor of the fourth ventricle is pierced, sugar appears in the urine. He founded his views upon experiments made upon animals. As the vagus nerve takes its origin on the floor of the fourth ventricle, to the irritation of this nerve, as well as the sympathetic nerve, a remarkable

influence upon the formation of sugar in the urine is attributed. The pneumogastric nerve takes its origin from the corpus restiforme situated on the floor of the fourth ventricle, and by means of the jugular ganglion enters into connection with the superior cervical ganglion of the sympathetic nerve. Further experiments upon animals have shown that irritation of the origin of the pneumogastric nerve in the fourth ventricle is transferred to the sympathetic nerve, and thereby the quantity of sugar formed in the liver, which is under the influence of the sympathetic nerve, is increased. Schiff has, however, proved that irritation of other parts of the nervous system may also render the urine of animals diabetic. These facts cannot be denied; yet, experience teaches us that, in the examination of those who have died of diabetes, these changes in the brain, especially in the pons varolii and the fourth ventricle, are not always found.

This discovery of the artificial production of diabetes, or rather glycosuria, by Claude Bernard, is very interesting and remarkable, but the results of these experiments are manifestly exaggerated, and have led to very premature theories concerning the cause of diabetes. Schiff has recently made some very interesting researches concerning the formation of sugar in the liver and the influence of the nervous system upon the production of diabetes, and has thereby furnished some physiological facts for the theory of the disease. Nevertheless the study of diabetes is by no means completed, and these theories are all to be received with caution.

Other writers regard a disease of the spinal cord as the cause of diabetes, and especially tabes dorsalis. The spinal cord is regarded as the origin of the disease, on the ground that diabetic patients are generally im-

potent. It is very questionable whether these cerebro-spinal symptoms are to be regarded as the causes of the disease, since they may also be the results of it. Stotvis found the sugar in the liver increased 25 per centum in diabetes, and thought he had discovered the cause of the disease in a proliferation of the liver-cells and the consequent hyperæmia of this organ. But further experience teaches that hypertrophy of the liver is not a constant symptom in this disease, and that atrophy is often found.

Oppolzer believes that atrophy of the pancreas is simply a consecutive symptom of diabetes. The theory adduced by Miahle, that a ferment produces the decomposition of the sugar into its final products, alcohol, carbonic acid, and water, is not valid, especially as this ferment has not yet been discovered. The same may be said of the assertion, that a deficiency of oxygen and alkalies in the blood is the cause of the prevented change of the sugar.

It is also to be mentioned here that glycosuria is found in certain disturbances of the respiratory organs. Reynoso examined especially the influence of disturbed respiration in various troubles of the thorax, and he, together with Dechambre, found that the urine contained sugar in all the cases examined by them.

Following these investigations still further Reynoso found that in all diseases with disturbed respiration, as phthisis, bronchitis, asthma, pleuritis, etc., the urine contained sugar. He also found sugar in the urine of animals which had inhaled ether, chloroform, and other gases. Oppolzer's examinations, however, have not confirmed these views. But it is to be confessed that, although a large amount of sugar may be found in the urine in numerous other diseases, yet we do not by

this means obtain a sufficient explanation of diabetes, in which there is not only an increase of sugar in the urine, but also an entire group of other symptoms.

Claude Bernard also found glycosuria in different diseases of the liver, and especially in contusions and wounds of this organ. He performed experiments upon animals by exerting pressure upon the liver. He mentions the case of a man who was kicked by a horse in the region of the liver. In this case sugar was found in the urine till the contusion had completely healed. Rayer has observed similar cases. Monneret's testimony is to the contrary. He has scarcely ever observed sugar in the urine in numerous cases of disease of the liver. Frerichs also is not inclined to look upon hypertrophy of the liver as a constant attendant of diabetes. Oppolzer admits that glycosuria is often connected with diseases of the liver, but this fact gives no sufficient explanation of the diabetic disease itself.

As follows from the above discussion, it can rightly be asserted, that glycosuria may exist under very different conditions. Many other conditions are not yet accurately confirmed, and need a still further examination and observation. It is probable that these conditions may be acquired in a way which will contribute essentially to the advancement of our knowledge of diabetes. To this end still further clinical researches must be undertaken. Guilard has undertaken such researches in a number of cases, and has arrived at the following results: In one case of cerebral congestion with strabismus, in two cases of amaurosis, in one case of chronic hydrocephalus, in two cases of bronchorrhœa, in one case of gangrene of the lung, and in one case of progressive paralysis, sugar was plainly shown

in the urine. In one case of gangrene, in two cases of mania, and in one case of paralysis, the evidence of sugar was doubtful. Sugar was found in four out of five cases of tuberculosis. In three cases of epilepsy sugar was found in the urine immediately after the attacks. In small-pox, measles, carbuncle, hypertrophy of the heart and spleen, paraplegia, cerebral hæmorrhage, senile gangrene, gangrene of the bladder, cancer of the liver, pneumonia, cretinism, and idiocy, no sugar was found in the urine.

Diabetes is on the whole a rare disease. It affects both old and young. It appears most frequently between the ages of thirty and fifty years. There are instances, however, in which diabetes has appeared in children of one, three, nine, and twelve years of age. Sugar is often found in the urine of children who have partaken heartily of sweet food, or who are suffering from whooping-cough, convulsions, and asphyxia. Beyond the age of seventy the disease is very rare.

The disease occurs more frequently among men than women. Sugar is found in the urine of women during pregnancy, but it is not dependent upon diabetes. It is the glycosuria of the pregnant.

In respect to its geographical propagation, it is remarkable that this disease is especially frequent in Holland and England. The reason of this may be partly the climate, and partly the manner of living of these nations. With the exception of these two countries, statistics prove that the disease is more frequent in hot than in cold climates. In Italy diabetes is more frequent than in Germany. In France the disease is not rare, especially in Normandy. The cause of the frequent occurrence of this disease in Normandy has been attributed to the excessive use of cider.

This explanation does not seem to be true, as in many parts of Germany cider is used in great quantities, but diabetes does not appear more frequently there than in other regions. It appears in about the same ratio in wine and beer countries. In some families it seems to be hereditary. The inheritability of diabetes is, however, not established. The data for the solution of this question are still wanting. Concerning constitutional influences there are no data, but diabetes appears to develop itself in weak rather than strong persons. It is not probable that a dissolute mode of living, excess in venery, or want, has any influence upon the development of this disease, as it is found just as frequently among the wealthy and those leading a regular mode of life, as among the poor and those who are irregular in their habits. According to observations in German and French hospitals, traumatic lesions, especially concussions of the head and back, appear to have a real influence in the production of diabetes. If we consider the anatomical changes which are found in diabetes after death, we must also confess that investigations have not yet arrived at an absolute knowledge of them. In the majority of cases Oppolzer has found the kidneys enlarged and swollen. Their weight was increased, and they were hyperæmic or in a state of inflammation. The pyramids and cortical substance were hypertrophied. It is to be confessed that there are many exceptions to this rule. There are a considerable number of cases of this disease in which the kidneys suffered no characteristic pathological changes. There can be no positive rule given concerning the condition of the liver. Sometimes it is swollen and hyperæmic. In one case observed by Oppolzer, the liver was very hard and twice its usual size.

It extended almost to the umbilicus. The spleen in this case was enlarged. In this case, however, it was shown that the patient had for a long time suffered from intermittent fever before the existence of glycosuria. A further examination of the liver showed that it had undergone amyloid degeneration. When the diabetes continues for a long time, other secondary anatomical changes are found. In more than half of the patients who have died with diabetes, signs of tuberculosis were found. A further secondary anatomical change is the hydrocephalic ependymitis,* which has been repeatedly demonstrated by Lebert. Marchal de Calvi has directed attention to the spontaneous gangrene of the extremities, which occurs toward the end of diabetes, an observation which has often been confirmed by recent investigators. In a later work he shows that in several instances cerebro-spinal changes, especially apoplexy and congestions, arise in consequence of diabetes.† In many subjects lobar or lobular pneumonia are found as complications. Stockweis proved that the quantity of sugar in the liver was increased in diabetes. In the muscular substance of the heart, in the kidneys, spleen, and lungs, he also found a small quantity of sugar. In the pericardial fluid, in the arterial and venous blood, he found only an indeterminate amount, while in the brain no sugar was present.

Quite recently, accurate chemical examinations have been made of the urine of persons suffering from diabetes and also of the blood, peritoneal serum, the flu-

* Ependymitis is an inflammation of the membrane lining the ventricles of the brain.

† We must here mention that this well-known author, in his last work, draws a parallel between the gout and diabetes, and regards both, together with rheumatism, as different manifestations of the uric acid diathesis.

ids of the eye, the muscles and internal organs. The results of these examinations are by no means positive, and it would be hasty at the present time to draw conclusions from them. It deserves, however, to be mentioned that in most of the organs sugar was found in increased quantities. In the brain no sugar was found.

Especial mention deserves to be made of the creatinin which occurs in diabetic urine. It is a nitrogenous substance, which was first shown by Liebig, and later by Neubauer, to be a kind of albuminous substance, which is also found in healthy urine. The secretion of this albuminous substance in diabetic urine has been recently the source of numerous researches. At first a considerable quantity of this substance was thought to be found in the urine (8½ grains of creatinin in the whole amount of urine passed in twenty-four hours). There was thought to be some relation between this enormous secretion of creatinin and the secretion of sugar, and especially of the diabetic process. The later examinations of diabetic urine which were made at Oppolzer's clinic proved that the views in respect to the increased secretion of this material were exaggerated. In three cases of diabetes the quantity of creatinin was not sensibly diminished.

Among the symptoms of diabetes, the first and most important is the large amount of grape-sugar which appears in the urine and the other secretions. The quantity of sugar which is secreted in a day is very variable, sometimes amounting to a pound or more. The manner of living of the patient manifestly and unquestionably has much influence upon the quantity of sugar which is secreted, since an increase in the quantity of fluid which is taken and the indulgence

in amylaceous food augment the amount of sugar in the urine.

The Tests for Sugar in the Urine.—There are very many different methods, and a large number of chemical substances, by means of which sugar may be detected in the urine.

(*a.*) *Moore's Test.*—A small quantity of the solution of potassa is added to the urine to be tested, and then the urine is to be boiled. The upper part of the fluid becomes brown if sugar is present. Or, an excess of caustic potash is added to the urine, and then it is to be boiled. If sugar is present, a red color will appear. Heller advises that, after boiling, a little nitric acid should be added. If sugar is present, the odor of caramel or molasses is detected.

(*b.*) *Pettenkofer's Test* depends upon the action of gallic acid and sulphuric acid upon a solution of sugar. It gives to the urine a dark-violet color. On the other hand, the presence of gallic acid is proved by the addition of a solution of sugar and sulphuric acid. This test is, however, not very reliable, and is not much employed.

(*c.*) *Trommer's Test.*—A certain quantity of the urine to be examined is mixed with an excess of the solution of caustic potash, and a few drops of a solution of sulphate of copper are added. If sugar is present, the fluid assumes an azure-blue color. When warmed, boiled, or allowed to stand for some time, the oxide of copper is reduced to a suboxide, which is deposited as a red-brown powder. The reaction is made known by the yellow color which the fluid assumes, and which later becomes red, and finally dark brown. If the fluid is heated still more, a thin shining coat of copper is deposited on the walls of the test-tube.

(*d.*) *Boetger's Test.*—A solution of carbonate of potash or soda is added to the urine to be examined. A little nitrate of bismuth is then added. The fluid is then boiled. If sugar is present, the oxide of bismuth falls as a black powder.

(*e.*) *Lowenthal's Test.*—To the urine to be examined, a mixture of tartrate and carbonate of soda and chloride of iron is added. If sugar is present, the urine assumes a dark color. This test is not very certain, as oftentimes the fluid assumes a dark color when no sugar is present.

(*f.*) *Raspail's Test.*—Urine containing sugar assumes a violet color when sulphuric acid and albumen are added. This color may appear when no sugar is present. It is, therefore, an uncertain test.

(*g.*) *Runge's Test.*—A small amount of urine is poured upon a plate, and a drop of sulphuric acid added. The plate is then warmed over a spirit-lamp. If sugar is present, a dark spot appears where the sulphuric acid was dropped. This test depends upon the quality which a saccharine fluid mixed with sulphuric acid possesses of becoming black upon warming. Other organic bodies have the same property as sugar, and therefore this test is not sure. Reich recommends hydrochloric instead of sulphuric acid. This test has the same objection as that of Runge.

(*h.*) *Meauméné's Test.*—A woollen substance (merino) is moistened in a solution of chloride of tin and then dyed. If a drop of urine containing sugar is now placed upon the merino and then warmed, a dark spot appears. This test is very simple and convenient, as these pieces of merino can always be had. This test also is not infallible, as other hydrocarbons besides sugar produce the same reaction.

(*i.*) Another test for sugar is that of Cutton with chromic acid, which is reduced to an oxide of chromium when sugar is present. Instead of the original red color of the chromic acid, a green color now appears.

(*j.*) The test of Jones consists in allowing a drop of urine to evaporate, and then the deposit is examined for the crystals of sugar. Besides the sugar which is found in diabetic urine, there is also an increase of the urea, uric acid, and other salts, all of which contribute to its increased specific gravity. In some cases the specific gravity is 1042 and more. If the urine stands a considerable time, spores are found in it. The urine is clear, of a pale-yellow color, and oftentimes evacuated in very large quantities (five to nine quarts daily). The patients do not urinate in so great quantities *because* they drink so much, but they drink much because they have evacuated so much fluid from the body, and in this way seek to compensate the system for the great loss of water which it has suffered. The cause of this enormous secretion of urine is indeed not known to us, and we must for the present seek it in the diuretic influence of the sugar. Inasmuch as so much water passes through the kidneys in diabetes, the secretions of the other organs are necessarily limited. The skin of patients is generally rough and dry. Perspiration takes place very seldom and with difficulty. There are, however, exceptions, and in febrile complications and in advanced phthisis night-sweats occur. The intestinal evacuations are generally retained, and the secretion of semen appears to diminish considerably, and it is often observed that diabetic patients become impotent. In women the catamenia are often irregular and finally suppressed.

Besides, it is often observed that the temperature of the skin is decreased and the patients suffer from cold.

The further development of the disease extends its effects and consequences to the whole system, whose functions are greatly disturbed. The appetite is voracious, and can never be satisfied. Patients suffer from great thirst, and this is one of the first and most tormenting symptoms. It deserves the greatest attention, as it makes itself known without any symptom of fever or indisposition to explain its cause. This enormous thirst is due without doubt rather to the large loss of water, than to the secretion of sugar, as simple prolyuria also occasions an increase of thirst. The appetite, increased at first, is wont to disappear only toward the fatal termination of the disease.

There are two forms of diabetes to be distinguished. The distinction of these forms has for the practice an especial worth. Experience teaches that the anomalies of nutrition, which are manifestly the source of the disease, may be very different. In the *first* or light form of diabetes, the secretion of sugar is present only so long as amylaceous food is supplied to the body. If only carnivorous or non-amylaceous food be eaten, the secretion of sugar as well as most of the other symptoms of glycosuria ceases. In the *second* and more serious form of the disease, the glycosuria continues, although the food is of a non-amylaceous quality. In many cases the diabetes shows itself in the first form; but if the use of amylaceous food is dispensed with, not only does the secretion of sugar cease, but also the quantity of urine is diminished, the tormenting thirst and insatiable appetite abate, and the emaciation becomes less. In the second and more serious form it is otherwise. The symptoms either

continue the same, or there is only slight alleviation. These are to be regarded as the most serious cases of diabetes. As a matter of course, there are various intermediate grades. Sometimes the light form is only the beginning of the diabetes, which later passes over to the more serious form, so that both forms are only the different grades of one and the same disease.

In regard to tests for sugar, the test of Zwenger ought to be mentioned on account of its completeness. It depends upon the quality which nitrate of silver possesses, when heated to 212° F., in an ammoniacal solution of grape-sugar, of forming a bright metallic mirror. For this purpose some urine is evaporated. To the residue remaining after the addition of alcohol, water is added, and then an excess of nitrate of silver. The whole is then filtered, and the fluid resulting therefrom is saturated with ammonia. In order to render the test more certain, a small amount of nitrate of silver is now added, and the whole is then heated to 212° F. If a small amount of sugar only is present, a blue mirror of metallic silver is formed. If no sugar is present, the fluid is only clouded.

There are also various methods of determining the quantity of grape-sugar. The simplest method of ascertaining the quantity of sugar originated with Roberts, in Manchester. The urine containing sugar is submitted to fermentation. It suffers by this means considerable loss of weight, and from this loss of weight the quantity of sugar is determined. This method requires only an accurate pair of scales. The other methods require a great many instruments and test-fluids, among which are the polarimeter of Biot, the saccharometer of Soleil, the diabetmeter of Robiquet, etc.

The test-fluids of Fehling are especially adapted for determining the quantity of sugar. The instruments of Biot, Soleil, and Robiquet, by a deviation of the polarized light to the right, show very quickly and correctly the amount of the secretion of sugar. An idea can be formed of the amount of sugar in the urine from the fact that every deviation of the polarized light one degree on the circular scale corresponds to one grain of sugar in a thousand grammes of fluid. All of these methods require an especial apparatus. To determine the amount of sugar by fermentation according to Roberts's method, no particular apparatus is needed. The specific gravity of the urine to be tested is taken in the customary temperature of the sick-room. Several ounces of the urine are then poured into a vessel, and a piece of solid yeast of the size of a hazel-nut added. The vessel is then lightly stoppered, placed in a warm room, and subjected to fermentation. When this process is completed, which usually happens in eighteen hours, the vessel is again brought into the sick-room, and the fluid allowed to cool to the same temperature as when the specific gravity was taken. The urine is allowed to clear up, which takes place in five or six hours; it is then decanted, and the specific gravity again taken. Every degree of loss of weight corresponds to one grain of sugar in the ounce of the urine. It is to be remarked that the percentage of sugar in the urine is alone typical of the extent and intensity of diabetes. To arrive at a correct knowledge of the quantity of sugar secreted, the quantity secreted within a definite time, for example, twenty-four or forty-eight hours, should be determined. In order to arrive at a proper judgment concerning the signification of the sugar in the urine, the amount of

amylaceous food which the patient consumes must also be taken into consideration. For example, in one case where the secretion of sugar amounts to only three or four per cent., the disease may be much more serious than in another case where six to eight per cent. of sugar is secreted.

In most cases the quantity of urine is in exact proportion to the quantity of sugar secreted, i. e., the greater the quantity of sugar secreted, the greater also is the amount of urine, and *vice versa*. There are, however, exceptions to this rule, and Oppolzer has observed cases where, the quantity of urine remaining the same, the amount of sugar varied very much.

The specific gravity of the urine is also generally proportionate to the quantity of sugar secreted. There are, however, sometimes exceptions. The specific gravity may be quite high, and, nevertheless, the quantity of sugar is less than in another case where the specific gravity is small. The specific gravity of the urine may be increased by means of other ingredients than sugar. Respecting these other ingredients of the urine there was a long controversy whether they were present or not in the urine of diabetic patients. At present, however, it is conceded that the urea, uric acid, and pigment of the urine, suffer very little if any absolute diminution, but, on account of the enormously increased quantity of the urine, they appear relatively in much less quantity.

The disturbances of nutrition in diabetes manifest themselves by a remarkable emaciation, which makes its appearance, although patients have a good appetite and consume a great quantity of food. The muscles disappear, and a remarkable weakness shows itself. To this general wasting are also associated nervous dis-

turbances with the character of anæsthesia and paralysis. There are, however, numerous other symptoms of nervous trouble, for example, pain in the head, in the limbs, and lumbar region, a feeling of great uneasiness, a sensation alternately of heat and cold, numbness and weakness of the extremities, the mental condition is very deplorable, and a timid, unquiet feeling takes possession of the patient.

A very frequent symptom in the later stages of diabetes, as has already been mentioned, is impotence, the cause of which is, perhaps, to be sought in the frequently observed atrophy of the testicles. Finally, not unfrequently there are observed a looseness or a carious destruction of the teeth, inflammation of the skin, followed by furuncles, excoriations of the genitals, inflammation of the bladder and urethra, croupous inflammation of the lungs, abscesses and gangrenous destruction of the same, or inflammation of the serous membranes which make their appearance as pleuritis, pericarditis, and peritonitis. The heart generally shows no change except that it is sometimes found atrophied. Finally, tuberculosis and morbus Brightii are frequent complications, which exercise much influence upon the rapid termination of the disease.

That albuminuria may associate itself with diabetes is a fact confirmed by many authors, and has also been observed by Oppolzer. Indeed, the appearance of albumen in the urine is a very bad complication, and points with great probability to a diffuse inflammation of the kidneys. It is generally observed only a few weeks or months before the fatal termination. In one case of diabetes Oppolzer found, at the autopsy, the epithelium of the kidneys fatty, while the cortical substance showed the same characters as the kidneys of morbus Brightii.

In diabetes death may occur in various ways. It is generally occasioned by the complications. As we have stated, they are very numerous. Tuberculosis is the most frequent complication. A serous effusion into the ventricles of the brain, the pericardium, or thoracic cavity, may likewise be the cause of death. As terminal complications, are also observed hydrocephalic softening associated with delirium, and coma preceded by convulsions. Broncho-pneumonia and pleuro-pneumonia are not unfrequently observed as fatal complications of diabetes. In these complications, especially when preceded by considerable fever, the quantity of sugar may considerably decrease, but this decrease should not be regarded as a favorable symptom. Quite lately Marchal de Calvi has drawn attention to a complication of diabetes which has not previously been observed. He has shown that not unfrequently in advanced stages of diabetes gangrenous inflammation, as well as spontaneous gangrene of the skin, takes place. This observation has also been confirmed by Oppolzer. Intestinal tuberculosis is also one of the sequelæ of diabetes.

Among the numerous complications of diabetes the disturbances of the vision take a prominent place. They appear as frequently in diabetes as in morbus Brightii, so that they may, indeed, be regarded as symptoms and not as complications. For the diagnosis as well as the prognosis, they are of great value. The disturbances of vision which assume the first rank are amblyopia and cataract, second in order are amaurosis and diplopia. Authors disagree respecting the frequency of amblyopia. Lecorché and Bouchardat have observed it, at least, once in every four cases of diabetes. Others have observed it still more frequently.

Two different forms of amblyopia may be distinguished, a light and a serious form. The light form manifests itself in the initial stage of diabetes, and may very early direct the observing physician upon the track of the disease. This form of disturbance of vision generally appears in the following manner: Both eyes are attacked at the same time. The patient reads with difficulty, and only with the aid of convex glasses. All objects appear enveloped in a more or less thick mist. The disturbance of vision presents the peculiar symptom that during digestion it is much exaggerated. Sometimes the patient sees objects double. The cause of these complications is at present not known. Recourse has been had to various explanations of the cause of this amblyopia and amaurosis. It is more than probable that an affection of the retina, which at first is attacked with anæsthesia, and later with paralysis, lies at the bottom of these complications. The anatomical changes of the retina which are generally found at the autopsy are not always in direct proportion to the disturbances of vision which were manifested in life. When considerable changes are found, for example, apoplexy and inflammation of the retina, they appear to depend upon morbus Brightii and albuminuria, and not upon diabetes.

From all which has been observed up to this time it appears that amblyopia may appear in different degrees. The light form of amblyopia is the most frequent. Oppolzer has not unfrequently observed it among the first symptoms at the commencement of the disease. Very often he observed that it alternately became better and then worse, at times completely disappearing, but soon appearing again. He has not always found material lesions of the retina.

Cataract is a less frequent complication of diabetes than amblyopia. The cloudiness of the lens appears in the first stages of diabetes, but the complete maturity of the cataract is generally observed only in the later stages of the disease. Nevertheless they have been observed to form very quickly, and complete their development within a very few weeks. It cannot with certainty be asserted that the cataract is formed on account of the presence of sugar in the aqueous humor, as, in some cases, though sugar was present, no cataract was observed. The injection of sugar into the chambers of the eye produced no cataract. Oppolzer believes that the cataract is mainly caused by the great loss of water which the system suffers in diabetes.

It may be said of the etiology of the disease that there are very few cases in which diabetes can be attributed to any special cause. Concerning the influence of climate upon its development there are no accurate statistics. The statistics which we have respecting its geographical extension are manifestly of little worth. Most authors believe that glycosuria is more frequent in warm than cold lands. It is said to be much more prevalent in Egypt and Italy than in Germany, but this statement is opposed to the fact that diabetes occurs much more frequently in the cold climates of England and Holland, than in other countries lying farther south. There are also no positive facts concerning the influence of food and the manner of living upon the development of diabetes. From the statistics which have been thus far collected, it follows that the disease is much more frequent among men than women. Of one hundred cases of diabetes, twenty-five occurred among women, the remainder among men.

It is also certain that the middle age (between thirty and fifty) furnishes the greatest contingent for the disease. Oppolzer, however, has observed a few cases where the disease developed itself very early, and also in very advanced age (between sixty and seventy). Concerning the hereditary influences upon the development of the disease, nothing positive can be said. There are a few not very trustworthy data from which it appears that there is a certain family tendency to this disease. There are wanting the necessary facts concerning the influence of despondency, of misfortune, and venereal excesses. Diabetes is found among the wealthy and those leading a regular mode of life, quite as frequently as among the poor and those living irregularly. In a few cases Oppolzer has found concussion of the brain and spinal cord a direct cause of the development of glycosuria.

The prognosis of diabetes is always very unfavorable. In the majority of cases it ends in death. It is only in these cases where the secretion of sugar is not very considerable, and where the general health is not perceptibly disturbed, that the life of the patient can be much prolonged by a proper regimen. If the quantity of sugar in the urine is considerable the prognosis is hopeless. The patients succumb to marasmus. The prognosis is also very doubtful when the glycosuria is complicated with other diseases, as affections of the lungs and liver. If the patient is very much reduced and emaciated, or if tuberculosis of the lungs develops itself in diabetes, the prognosis is very unfavorable. If, together with albumen, sugar is also found in the urine, the prognosis is not less unfavorable. If sugar appears in the urine in consequence of cerebral lesions, it is only a temporary symptom. In such cases, how-

ever, the cerebral lesions are generally of a very critical nature, and may be the cause of death. There is here a double danger; danger from the wound itself, and also from the glycosuria. There are also instances known where, after concussions of the brain and blows upon the head, a persistent diabetes was developed, which occasioned death, although the wound in itself was very little dangerous, and attended by no bad symptoms.

None the less dangerous are the other complications of diabetes, broncho and pleuro-pneumonia, which generally terminate fatally. Gangrene of the lungs is of course very dangerous. The disappearance of the sugar in such complications should by no means be regarded as a favorable symptom. They cause for the time the disappearance of the sugar, and also the other symptoms of diabetes, but they produce no constant favorable influence upon the disease, for either the patient succumbs to the complicating disease, or, when this is overcome, the symptoms of diabetes return in undiminished force.

The facts are wanting to establish a true theory of the pathology of diabetes. In the normal condition of the system, sugar is found in the blood of the vessels coming from the liver, from whence it is conveyed to the lungs. The views of Claude Bernard, recently published, tend to confirm this physiological fact. According to his observations, the liver does not directly secrete the sugar, but another substance, which has much resemblance to starch. He calls this substance glycogene. This glycogene possesses the quality of being changed into sugar by the serum of the blood, and it may itself be procured from the liver. It constitutes the substratum of the liver-sugar which is

formed in the blood. According to this view, which has by no means been positively confirmed, but which is very probable, the glycogene is formed in the liver principally from amylaceous food, but sometimes also from albuminous substances. From the liver it is conveyed to the lungs, and is here employed and changed in the process of respiration. It is not yet established *how* this transformation takes place, but that the sugar is formed in the liver from the food, and later undergoes a transformation, is fully established. According to Claude Bernard, the sugar is not decomposed by a process of combustion in the lungs. In the fœtus, where no respiration takes place, sugar is formed in the liver, and disappears again by some process.

These physiological facts, confirmed by Bernard, throw but little light upon the pathology of glycosuria. If the liver of an animal is strongly compressed, the urine is found to contain sugar. The direct irritation of the liver also by means of contusions, wounds, and injections of ammonia into the vessels of the liver, produces an increased production of sugar. Indirect irritation of the liver, through the nervous system and the sympathetic nerve, has also produced a similar result. As has been mentioned, if the pneumogastric nerve be irritated at its seat of origin in the brain, the urine contains sugar. In the year 1855 Bernard discovered this fact. Later Schiff proved that the irritation of other nervous centres, besides the floor of the fourth ventricle, caused the appearance of sugar in the urine. Frerichs cites three cases of diabetes, in which a considerable hypertrophy of the liver was demonstrated. Oppolzer is, however, in no way inclined to regard the hypertrophy of the liver as a constant symptom of diabetes.

Sugar may also appear in the urine under various other circumstances. Reynoso found it in various disturbances of the respiration, and established that in many cases the influence of disease of the organs of respiration produced sugar in the urine. Oppolzer has also observed sugar in the urine for a short time after a severe fright, strong mental excitement, or labor.

From all these facts it is established that the secretion of sugar in the urine may be produced and increased by means of numerous influences, and it may with much probability be concluded that, in diabetes in the initial stage, the sugar is formed at the expense of the amylaceous food, and in its advanced stages at the expense of the albuminous food. The consumption and transformation of this material, which takes place in a normal condition of the system, is in diabetes in some way prevented, so that the blood and the most of the organs contain sugar. It is possible that some disturbance or modification of the respiration prevents the consumption of sugar. If the observations of Reynoso are accepted, who found sugar in the urine in various disturbances of the respiratory process, diabetes may be thus explained: that the saccharine material produced in the liver is prevented from further transformation, and thus an opportunity is given for its transfer into the blood.

Although the symptoms of diabetes are so remarkable, yet the cases are not unfrequent where diabetes is not recognized in practice; at least this occurs very often in the initial stage. The diagnosis of diabetes is not difficult, and as soon as we observe in a patient constant thirst, a continual feeling of dryness in the mouth, and later on polyuria, especially if there are no febrile symptoms present, it becomes our duty to

examine the urine for sugar. The physician should endeavor to observe the disease at its very outset. Amblyopia and retinitis, as well as various ulcerations, caries, and necrosis, are wont sometimes to introduce the disease. All of these symptoms will be the better estimated if an early examination of the urine is not neglected. The early diagnosis is also of great value for its treatment. The urine ought especially to be examined during digestion, when the quantity of sugar is generally increased. Especial attention should be devoted to the condition of the respiratory organs and the state of the liver, and this organ should be subjected to a careful examination. The simple proof of sugar in the urine does not always justify the diagnosis of diabetes, as sugar has been found in the urine when a large amount has been eaten, and Blot has shown that pregnant women and patients suffering from gout very often pass urine containing sugar. An increase in the quantity of urine evacuated is also not a sufficient proof of diabetes. This is simply polyuria, or diabetes insipidus. This last disease, however, is very unfrequent, and is to be regarded as an affection of the vagus nerve. In this disease there is also great thirst, the specific gravity of the urine is not high, and there is neither grape nor muscle sugar present. When the last-named sugar is present, the disease is called diabetes inorites. In diabetes insipidus the urine contains but a very small quantity of the urinary salts, and scarcely any urea; there is also great dryness of the tissues and increased thirst, and, as a large amount of water is drunk, the ingredients of the urine are much diluted. Oppolzer has observed polyuria or diabetes insipidus among children who had the mea-

sles, and administered in such cases the fused nitrate of potash.

℞. Potassæ nitratis fusæ ʒ i.
Aquæ destillatæ Oi.
M. et Sig. Drink in one day.

Tannin is also given. The French administer belladonna, carbonic acid water, and good food.

To combat diabetes mellitus, it is necessary above all things to regulate the diet and limit the patient to the least possible amount of amylaceous food. The treatment is purely dietetic and empirical. The remedies recommended as specifics, upon this or that hypothesis, have proved themselves wholly without effect. In many cases abstinence from amylaceous food causes the secretion of sugar in the urine, and most of the other symptoms, to cease. In such cases the sugar is probably prepared only at the expense of the amylaceous food, and the elements of the tissues have not yet been employed in its formation. Still more important are those forms of diabetes in which, in spite of exclusive indulgence in animal food, the secretion of sugar in the urine still continues. In these cases the formation of sugar takes place at the expense of the nitrogenous elements of the tissues. On account of the abnormal change of these most important organic elements, those symptoms appear which in diabetes are of such sad significance: extreme emaciation, such as scarcely any other disease exhibits, great weakness, and complete prostration of the muscular strength. For this excessive expenditure of material, the greatest possible reparation must be rendered, and this can take place only by means of a supply from the nitrogenous substances. The longer the supply is able to compensate for the waste of the system, the longer

will the patient survive. If the patient indulges in much amylaceous food, not only is the amount of sugar augmented, and many other troublesome symptoms increased, as secretion of urine and thirst, but also more power is demanded for the digestion of this food, and the supply of nitrogenous food, compensating for the organic elements which have been used, is limited.

The diet can exert no direct influence upon the disease itself; it is only able to prolong the life of the patient. Oppolzer sometimes employs benzoic acid in diabetes, although its good effects are questionable.

℞. Sodæ benzoatis ʒi.
Sacchari albi ʒi.
M. et Ft. pulver. div. in partes æquales No. 6.
Sig. One powder every three hours.

If in the course of diabetes violent febrile symptoms appear, as they are not unfrequently wont to attend the complications which have been mentioned, quinine should be administered. The best remedies which we know for this disease are empirical. They are carbonic acid, the alkaline carbonates, and those mineral waters which contain these alkalies. Among these waters Carlsbad and Vichy take unquestionably the highest rank. Carlsbad decreases the secretion of sugar, but by no means produces a complete cessation of its abnormal production.

As already stated, animal food is the best diet for diabetic patients. It was regarded by Rollo as a very appropriate diet, and ham was recommended by him. But, in recommending this diet, the amylaceous food should not be wholly forbidden. Bread and dessert may be allowed in small quantities, as an exclusive diet of meat becomes very repulsive. Care should be given that the diet is composed principally of meat,

but indeed when meat only is eaten, sugar is often found in the urine. Instead of common bread, a gluten bread has been tried in diabetes. This, together with meat, was given to diabetic patients, instead of the common flour bread. With some patients this substitute agreed very well. They were satisfied with it, and ate it freely. Oppolzer has, however, seen many patients to whom this bread was very repulsive, and they positively refused to take it, so that small quantities of common bread could not be denied them. Quite recently attempts have been made to bake a good and palatable bread for diabetic patients, which consists in part only of glue. This is given in small quantities, in cases where the common bread cannot be entirely dispensed with.

Care should be taken that there should be a variety in the use of animal food. If it is always given in the same form it becomes repulsive to the patient. The diet may consist of meat in different forms, roast meats, beef-steak, fish, ham, venison, and sausages of various kinds. Diabetic patients may also eat eggs prepared in different ways, and fatty substances, including fatty nuts and fruits. Of vegetables, cabbage, asparagus, lettuce, carrots, and all kinds of salads may be allowed. The food to be avoided is every kind of grain, potatoes, all amylaceous legumes, all sweet fruits, and the juices of fruit. Milk is also an improper food, on account of the milk-sugar which it contains; however, small quantities of milk, and especially good rich cream, may be allowed.

Moderation is to be recommended to patients, in satisfying the tormenting thirst from which they suffer in diabetes. As a drink, cold water, acidulated water, and lemonade, are good. Wine and brandy should be

allowed only in small quantities. Thus given, it is useful to patients. When the pecuniary circumstances of the patient permit, the daily use of a half or whole bottle of the red French wine, or any astringent wine, may be recommended. To poor patients, common red wine or a little brandy may be given. All sweet wines and beer are less useful. Tea and coffee, without sugar, but with some cream, good meat broths, with vegetables, gluten, and small quantities of rum, are allowed. When the thirst is great, carbonic acid water, soda and Seltzer water, are the best to be given. If these are not sufficient to satisfy the thirst, drinks made from hops or other bitter substances may be prepared. In the early stages of the disease a suitable diet generally succeeds in moderating the thirst. The patient is then better able to heed the advice to drink but little. Patients should also be advised to take their food slowly and in small quantities. The desire for food as well as drink must be controlled.

In order to complete the hygienic measures, diabetic patients are advised to take regular exercise. Oppolzer advises patients to guard against taking cold, and to wear warm clothing. This should be especially recommended to patients who suffer from cold and have a dry skin.

Of the medicines which have been recommended and extolled in diabetes, a few only have merited approval. Miahle has very highly recommended the alkalies in diabetes. He grounds his opinion upon the supposed existence of acids in diabetes, which are neutralized by the alkalies. Too much, however, must not be expected from the use of the alkalies. A moderate use of the alkalies in such quantities as are found in the alkaline mineral waters, in order to aid

digestion when this is much disturbed, may indeed be of use, and may aid the normal assimilation of the food. Oppolzer, however, has not observed the good effects of large doses of the alkalies. Miahle prescribes them through several weeks.

Attempts should not be made to control or suppress the secretion of sugar, which appears in the urine after meals. If an attempt is made to suppress this physiological function, more harm than good may be done. The same may be said of the glycosuria of pregnant and lying-in women; but if disturbances of the respiration, diseases of the liver, or lesions of the brain are the cause of the glycosuria, attempts should be made to remove them. Lesions of the cerebro-spiral system have been very frequently regarded as the cause of diabetes. This theory has for its foundation experiments made upon animals, whereby diabetes, or rather glycosuria, was produced by means of artificial lesions of the cerebro-spinal system. If, in a case of diabetes, any disease of the cerebro-spinal system was found, this lesion was immediately regarded as the cause of the diabetes. It is, however, difficult to maintain this view in all cases.

In numerous cases which we find recorded in medical literature, various nervous complications are mentioned. As complications of diabetes, Oppolzer has observed many disturbances of the various nervous branches, as, paralysis of the facial nerve, neuralgia of the trigemini, etc. These neuralgias appear to depend upon an excess of urea in the system. In the treatment of these complications, regard must be had not only to the symptoms of the patient, but also to the patient himself, who in this disease is much reduced and weakened. Bleeding, although local, in

such cases should be discountenanced, as the loss of blood generally reduces the patient still more. The benefit of the revulsives in some cases, however, is not to be denied. They should, therefore, be preferred to leeches and cups. In many cases of neuralgia, when it cannot otherwise be relieved, resort may be had to sinapisms and vesicants.

Oppolzer has observed in one case an obstinate sciatica, which came on in the course of diabetes. The patient was not able to sleep, and was in a condition of great excitement on account of the severe pain. The attacks attained such a violence that they were more dangerous than the diabetes itself. Laudanum and quinine, which at first produced a temporary relief, proved later of no use. Resort was had to the revulsives, which occasioned much alleviation to the paroxysms of pain. The patient, who was still more reduced by the pain, again recovered, but the sugar in the urine remained the same in amount.

Of the other remedies which are recommended in diabetes, carbonate of ammonia deserves mention. This was highly recommended by Bouchardat, and before him by Neumann and Barlow. He gave a solution of one to two drachms in four to six ounces of water, which was the quantity to be used in a day. Oppolzer, however, has never been able to obtain any good results with this medicine.

The use of opium in diabetes has been much praised by several physicians. Since the experiments of Bernard, who demonstrated the influence of the irritation of the nerves upon diabetes, this remedy appears in several respects to have done good service in this disease. But Peter Frank and other older physicians had previously contended that opium was best

adapted to lessen the secretion of urine. According to Oppolzer's experience, there are indeed some cases of diabetes in which the use of opium is beneficial; nevertheless, it is of use only in the symptomatic treatment of diabetes. It has by no means proved itself a remedy for the disease. From a theoretical stand-point, Bernard could indeed speak in favor of opium; in the practice, however, his views have been by no means verified.

Many other medicines, for example yeast, have been recommended in diabetes, but they have not been approved in practice. It has been shown that, in advanced stages of diabetes, yeast is able to accomplish little if any thing. Many astringent substances have been highly praised; experience, however, has not confirmed their beneficial effects, but has shown rather that they frequently do harm by disturbing digestion. Indeed, in diabetes, especially in the first stage, no drugs should be employed except in case of necessity. For the first stage of the disease, a suitable dietetic treatment is the best. In the second stage the treatment should be symptomatic. If the strength of the patient is much reduced, tonic medicines are indicated. Oppolzer employs iron and quinine. The doses and the preparation should be adapted to the individual, the digestion, and the special case, whatever it may be. Instead of quinine, a properly prepared wine of quinine may be given with advantage.

A frequent complication of diabetes is pneumonia. It arises quite often from a very insignificant, accidental cause, as a slight exposure to cold. Although similar exposures may produce a pneumonia in a healthy person, yet in diabetes such pernicious causes more frequently give rise to pneumonia than is the case in

healthy subjects. It may be said that in diabetes, generally, slight causes are sufficient to produce an inflammation of the lungs. Such a complication is, however, of the greatest significance; for in diabetes the appearance of pneumonia, however slight it may be, is especially dangerous. In the majority of cases it terminates fatally. The course of pneumonia in diabetes is quite different from that of pneumonia in a healthy subject. The reaction is insufficient, and, as the vitality is diminished, the patient soon dies. Indeed, while the course of diabetes is generally chronic, it is soon changed when pneumonia appears, which leads rapidly to a fatal termination. The treatment is generally powerless, if the pneumonia be at all extensive. Therefore in the treatment of diabetes, such dangerous exposures should be avoided as much as possible. As experience shows, diabetic patients are very sensitive to the action of cold. Patients must therefore be protected as much as possible from such pernicious influences. This can be best accomplished by means of reasonable prophylactic measures. The patient must always dress warm, and be warned of the dangerous influence of cold. A warm climate is more tolerable to such patients than a cold climate, and if practicable such patients should be sent into a warm region. As experience shows, diabetic patients are quite well in a mild region near the sea, and when sent here improve in health, and increase in flesh and strength. A damp, misty climate is very unfavorable to diabetic patients. If pneumonia attacks a diabetic patient, the treatment is generally of no use. It runs a very rapid course, and the patient generally dies within twenty-four hours. Neither local nor general bleeding should be resorted to, as the prostration of the system is very

great. From other medicines, as ipecac., calomel, and the revulsives, very little is to be expected. Dangerous symptoms are to be treated as they appear, and patients must be strengthened by tonics in so far as they are able to tolerate them. Oppolzer has seen a case of diabetes which overcame a light attack of pneumonia. Liquor ammoniæ was given. Small doses of opium were also administered to allay the cough. Transpiration was promoted by suitable remedies. The patient lived two years and a half after recovery from the pneumonia, and died finally of tuberculosis.

In diabetes gangrene of the lungs not unfrequently appears in consequence of the pneumonia. Such a termination may be diagnosticated from the fetidness of the sputa. They are blackish, rather adhesive, and very abundant. From medicines in this complication but little is to be expected. Tonics are mostly indicated.

Sometimes other peculiar complications appear in diabetes. Stomatitis and aphthæ, and oftentimes ulcers on the mucous membrane of the mouth, throat, tongue, gums, and lips, are observed. Extensive destruction of different parts also occurs. Oppolzer has observed an obstinate case of stomatitis in diabetes which defied all treatment. He employed as a wash for the mouth iodine in vain. Cauterization and scarification were also of no use, and there followed a very extensive gangrene of the mouth, gums, lips, and throat, in consequence of which the patient died. In other fresh and less obstinate cases, the above remedies and also washes of peppermint and camphor render good service. It is the presence of the primary disease which renders these complications so obstinate. Such

inflammations of the mucous membrane in diabetes easily assume considerable dimensions and lead to gangrene, as the power of reaction and the vitality of the system are very low. When the mouth, therefore, is attacked with these affections, it will often be impossible by any therapeutical treatment to afford relief. Local treatment is of no avail so long as the primary disease remains. A proper prophylactic treatment is, however, of great utility. In fresh cases, where the system is not much weakened by the general disease, much good results from cleansing the mouth with the above-mentioned washes. The first duty is to oppose the general disease, and, if this endeavor is successful, the local treatment will be much more satisfactory.

As frequent complications in diabetes, the affections of the skin are to be mentioned. Erysipelatous inflammations are the most frequent. These inflammatory complications were often observed by the older physicians, and sometimes they assume a very serious character, and give rise to extensive necrosis and destruction of the skin and cellular tissue. The termination of inflammation in diabetes is very frequently gangrene, and such an event is always to be feared. As complications in diabetes, other affections of the skin are described by different authors. Marchal de Calvi has directed attention to spontaneous gangrene of the skin extending over a large surface. He has also observed erythema of the skin. Fritz, Landouzy, and Wagner, have published similar observations. There are also numerous instances in which papular, squamous, and pustular eruptions were observed as complications of diabetes.

The gangrenous affections of the skin may be divided into three groups, the phlegmonous, the gan-

grenous, and the furunculous. There is much variety of opinion as to the influence which these complications have upon the primary disease. It has been asserted that these complications sometimes occasion an improvement in the general health, and a decrease in the secretion of sugar. Prout and others have observed the contrary. The observation of some physicians who have found sugar in the urine of patients suffering from carbuncles, or a number of boils, is of importance. In the treatment of these cutaneous complications in diabetes, the primary affection should not be neglected, and the principal treatment should be directed to this. The local treatment for the skin will vary according to the different lesions.

In the second stage of diabetes complications on the part of the digestive organs not unfrequently appear, while in the first stage perfect digestion is enjoyed. Not unfrequently in diabetes cardialgia shows itself. Oppolzer has observed such a case, in which, however, the digestion was not disturbed.

Gastric catarrh is also observed as a complication in the second stage of the disease. Oppolzer observed such a complication which made its appearance quite vehemently. The patient vomited very often, became much reduced in strength, and soon died. Oppolzer has observed also catarrh of the intestines in this stage. This hastens the fatal termination if astringents and opiates are not administered to quiet the diarrhœa. Especial attention should be given to the complications at their very commencement. In the first stage of the disease, dryness of the intestines is a prominent symptom, which is explained by the copious urinary discharges. Mild aperients should be given in such cases.

In the first stage of diabetes there are no disturbances of the liver. In the second stage, however, when the patient is much reduced and emaciated, an atrophied condition of this organ is not unfrequently observed. At autopsies, Oppolzer has often seen this organ atrophied, while in the beginning of the disease it was for some time enlarged.

Finally, the complications on the part of the urinary organs are to be mentioned. The kidneys are generally enlarged, or, at least, in a condition of hypertrophy, in consequence of the increased diuresis. The volume of the vessels and the Malpighian corpuscles are increased.

In many cases albuminuria is associated with diabetes, and the kidneys then undergo the metamorphosis which takes place in this disease. The cortical substance is then found in a condition of granular and fatty infiltration.

The bladder, in diabetes, is found considerably enlarged.

ART. II.—*A Case of Punctured Wound of the Thorax.* By JAS. A. JACKSON, M. D., Stoughton, Wisconsin.

May 8, 1868.—James Murphy, æt. sixteen years, was wounded this evening by stabbing with a jack-knife, the blade of which is two and three-eighths inches long and half an inch wide. He walked some little distance, but, falling down faint, was carried home. I saw him two hours after the receipt of injury; shock was not very severe; coldness of extremities and slight rigors; pulse not very frequent, full and regular; respiration somewhat painful, but not much, if any, accelerated. The knife entered between the eighth and

ninth ribs posteriorly, about one and a half inches to the left of spinous processes of vertebral column, wound of integument being three-eighths of an inch in length. External hæmorrhage, though previously slight, was now tolerably free; emphysema of cellular tissue around wound was manifest, and air was expelled forcibly from wound during expirations. Left lung clearly resonant to the base, affording everywhere vesicular respiratory murmur; no cough nor expectoration. He soon complained of pain in front over lower left costal cartilage, increased by respiration. I now discovered a murmur with both sounds of heart—churning in character—heard much of time, although not constant, and at times loud enough to be heard at a distance from patient; impulse and rhythm seemed normal; resonance on percussion over præcordia very clear. I enjoined absolute quietude, applied cold, wet compresses to chest freely, and gave liq. ferri persulph.; but the pain becoming so severe as to extort cries from patient, and to render respiration difficult and painful, I gave moderate doses of morphine, after which, he became easy and fell asleep; temperature of skin rose; pulse 80, full; respiration 20; external hæmorrhage ceased, and no signs of any occurring internally.

May 9.—Easy except a little pain in front; skin natural; pulse 75; respiration 18; tongue coated; anorexia; no external hæmorrhage, no cough; left lung gave clear resonance on percussion and vesicular respiratory murmur; heart-sounds regular and accompanied part of the time by murmur; resonance over præcordia appeared very clear; patient unable to urinate, bladder being distended.

℞ Magnesiæ sulphatis, ℥ ss.
Ft. Haustus.

℞ Pulv. opii. gr. viij.
Potass. nitratis ʒ i.
Antimonii et potass. tartratis gr. i.
M. et div. in chart No. 12.
Sig. Take one powder every three hours.

Barley-water allowed freely as drink, turpentine stupes applied over præcordia, with water dressing to wound. I drew off a large quantity of clear urine.

Evening.—Slight pain in front only, skin warm, pulse 84, respirations 18, bowels not moved; retention of urine; patient desires to eat. Repeated the saline mixture, and continued powders; hot fomentations to hypogastrium; a little food allowed.

May 10.—Feels comfortable, aside from some pain and tenderness in præcordial region and laterally; pulse 80; respiration 20; skin warm; tongue coated; thirst; no stool; complete retention of urine; ate some food, and desires more; wound is suppurating; præcordial space *seemed* duller, and cardiac murmur remains; percussion shows slight dulness with broncho-vesicular murmur over posterior and inferior portion left lung. Gave dose of jalap and calomel; injections per rectum.

Administered ipecac. gr. ss., potass. nitras gr. v., and antimon. et potass. tartras gr. $\frac{1}{12}$ every four hours.

Morphine *pro re nata;* light food; continue other measures.

May 11.—Easy except slight pain and tenderness; pulse 76; respiration 18; slight cough; bowels moved, and passed a little urine; has some appetite; physical signs about the same. Ordered a purge of jalap and bitartrate of potassa.

Blister over heart; continue injections.

May 12.—Has but little pain and soreness; feels comfortable and cheerful; pulse 72; respiration 18;

bowels loose, and urinates freely; marked dulness, with bronchial respiration and bronchophony over posterior inferior part left lung, below wound; dulness over præcordia more marked; heart's action and sounds regular and accompanied with murmur; wound is closed; continue treatment; moderate diet, with but little liquid.

May 13.—Feels well; pain only on deep inspiration; appetite good; pulse 60 to 64; respiration 18; physical signs remain about same; small doses nitre and ipecac.; moderate diet, restricting liquids; tincture of iodine applied to præcordia and back.

May 14.—Comfortable; some pain near præcordia on deep inspiration; slight tenderness here, laterally and posteriorly; appetite good; pulse 60; respiration 16 to 18; heart-sounds are good and nearly free from murmur; lung about the same as before; bowels not moved, nor has he urinated to-day; jalap and cream of tartar renewed; continue iodine externally.

May 15.—Feels very well; some soreness about chest; bowels loose; urine free; pulse 60; respiration 14 to 18; heart-sounds nearly free from murmur; lung shows improvement; allowed to sit up in bed.

May 16.—Pulse 60; respiration 18; heart-sounds normal and free from murmur; physical signs show progressive improvement; wound remains closed. Continue iodine. Allowed to sit up in chair.

May 17.—Walking about; physical signs show a nearly normal condition of lung and heart.

May 26.—With exception of occasional soreness about chest, and some debility, feels perfectly well; may be considered well.[1]

[1] This case opens up a nice point on the question whether the pericardium was wounded. The most probable source of error—pleuritis ex-

ART. III.—*Paracentesis Thoracis; Successful Result in Three Cases.* By JOSIAH S. WHITE, M. D., Memphis, Tenn.

A LADY twenty-one years of age, of nervo-sanguine temperament, had enjoyed uninterrupted good health up to the winter of 1859, when she was attacked with pneumonia, involving the left lung. The attack was violent, and produced miscarriage on the fifth day, she being at that time three and a half months advanced in pregnancy, and was followed by puerperal peritonitis of a very alarming character. In the third week or beginning of the fourth the active fever symptoms began to subside; and there were evidences of formations of pus in the left pleural cavity.

The case was apparently hopeless. She was threatened momentarily with suffocation, even should there be a spontaneous discharge through the bronchi; her feeble condition and the large collection of matter would, without doubt, produce death by suffocation. The collection of pus being well defined, I felt that the only chance of relief was by giving exit to the purulent collection. With the assistance of the late Dr. H. R. Robards, a skilful surgeon, paracentesis was performed in this manner: a semi-lunar incision was made over the lower posterior part of the mammary gland, the concavity looking forward and upward, the inner cornu receding high up, near to the centre of the gland and through the gland, and then separating its fatty base and fascia superfacialis from the pectoralis major

ternal to the pericardium—was eliminated pretty carefully, as the physical signs, recorded daily, seemed to show. It would be both interesting and instructive to have reports of analogous cases from other physicians, in the hope that some light may be thrown on the questions involved in the diagnosis.—[ED.

muscle to the intercostal space of the third and fourth ribs, one and a half inches from its sterno-cartilaginous connection. Thence raising up the valve formed from the gland, and inserting a trochar upward and backward through the muscles and intercostal space, on reaching the pleura costalis, the hand was elevated, and a direct entrance was made through that membrane, and fortunately into the abscess; for, on the withdrawal of the flattened stylet, the matter poured freely through the canula. Thirty ounces was the result of the first drawing, which relieved for the time the dyspnœa. A bandage had been placed around the patient previous to the operation. This was tightened on withdrawal of the canula, and the valvular flap held in position by a compress.

The instrument was used a second time. After this the pus would discharge freely on removal of compress, and on making a forced expiratory effort.

The discharges of pus at first were of a healthy character, but afterward took on a degenerated condition; and in a few days improvement would occur, only to be succeeded by a recurrence of the unhealthy character of the pus. Gradually the patient improved, and in the twelfth week was well enough to sit up, and in the fourth month to take moderate out-of-door exercise. The discharge, at times, was very profuse and exhausting.

The operation was performed in February, and in the ensuing fall I recommended her to take a trip to Europe, hoping the change might benefit her. I exacted from her a promise that she would, through her physician, keep me posted as to her condition and the result of the case. Communications were received from her occasionally, to the effect that her condition was slowly

improving, the discharge through the wound continuing, but varying much in quantity. In 1861 the civil war put a stop to those communications, and in 1865 I reopened the correspondence, and learned, to my great delight, that she had completely recovered. Accompanying this report, was a photograph of the chest and head, which I had requested of her.

The left mammary gland was very much atrophied; there was a sinking in or contracted appearance of the walls of the chest in the same region. Her physique otherwise, in consideration of the six or seven years' illness, was good. She also reports that the discharge continued up to 1862, gradually diminishing, and that she experiences but very little inconvenience. Has borne no children since.

The second case was of traumatic origin. Private Smith, of one of the Alabama regiments, Battle's brigade, received a wound from a Minié ball, on the 3d day of May, at the battle of Chancellorsville. The ball entered two inches above and slightly outside of the right nipple, passing through the lung and out at the point of the scapula, lodging under the common integuments, and was removed at the time by one of the field-surgeons.

There was considerable hæmorrhage for two days. A severe attack of pneumonia followed, from which he recovered sufficiently in two or three weeks to be sent to his home on furlough, where he was to report every two or three weeks to the nearest general hospital until recovery. November 3d, six months afterward, my attention was called to this case by one of the Board of Examiners for Discharge and Extension of Furlough, and, upon inquiry, found that he had latterly suffered with fever and much pain in the right lung. On ex-

amination, an abscess was detected in the lower portion of the upper division of the right lung, in the track made by the passage of the ball through the lung; suspected that the ball had carried into the wound a piece of clothing which remained in the cavity. The exit and entrance of the wound having closed, the patient could get rid of the accumulating matter only by expectoration, which was now very free. The favorable result, in the previous case, more hopeless than this, determined me to pursue the same plan of treatment.

The operation was done by making a semi-lunar valve from the common integuments and pectoral muscles, and cautiously introducing the flattened trochar. This was followed by a discharge of pus, and on the eighth day a piece of cotton shirt, about the size of an English shilling-piece, was found entangled in the opening, and was removed with the forceps, and from that time on there was a decrease in the discharge, and final recovery of the case. Sixteen months afterward the patient was seen by me, and was then in perfect health.

The third case resulting from pleuro-pneumonia was that of a lad fourteen years of age. The empyema followed an attack of pleuro-pneumonia. I did not see the patient until the day of operation. His father was a physician, and he, with others who had attended the lad, had discovered the abscess in the middle division of the right lung, in the fifth week from the inception of the disease. There was extensive pleural effusion, evidenced by the full and fluctuating intercostal spaces. Making the same valvular flap operation, and introducing the trochar through the fourth and fifth ribs and pleural investments, the abscess was

reached, and on withdrawal of the stylet eight or ten ounces of thick pus were passed, and then a slight withdrawal of the canula enabled the pleural effusion to discharge itself to the extent of some twenty or twenty-five ounces.

I saw this patient eight months afterward. His father informed me that he had been afflicted with a series of boils and superficial abscesses; otherwise he did quite well, and was fully recovered.

ART. IV.—*Description of an Apparatus for Teaching Hernia; its Anatomy, and its Mechanism.* By BENJAMIN HOWARD, M. D., Lecturer on Operative and Minor Surgery in the Medical Department of the University of New York.

EVERY teacher of surgical anatomy feels and acknowledges that, about the anatomy of inguinal hernia, more is told than taught. Students are more often qualified to answer questions upon it, than to treat it. Among the causes of this may be mentioned: first, undue reliance upon didactic description; second, obscurity of demonstration.

The surgical anatomy of hernia can be learned only from teaching which is objective. For this drawings are insufficient, it being impossible thus to show at the same time the parts which overlie each other as they do in this region. In order to communicate a competent comprehension of this subject, plain demonstration is indispensable, and in this nothing can be exclusively substituted for the cadaver. The cadaver, however, especially in private tuition, is troublesome to get, and inconvenient to keep. This, in the summer

months, is alike true even in our largest colleges. In either case, the expense is repeated at each demonstration.

Besides these obstacles *to* its use, there are two difficulties *in* its use. The first, in any case, is great and embarrassing. The second, in the lecture-room, is fatal to success. Both are inherent in the cadaver, and are in its use insurmountable.

The *first* of these difficulties consists in the natural order of superposition of the parts. This renders it well-nigh impossible so to exhibit the parts concerned, even to the smallest class, as to present a view of their actual relations *in situ;* it being necessary to remove one fascia in order to show another, the mind is occupied by each in succession; meanwhile the general relations are lost.

The *second* difficulty consists in the proportions of the cadaver. This prevents even such dissections as can be made, from being rendered visible to more than a few of those who in the lecture-room, vainly hope to learn, that which cannot thus be taught.

The *first* difficulty, I find to be obviated by the use of a model of the parts concerned in the surgical anatomy of inguinal hernia which I have so constructed as to enable the teacher to exhibit all the important relations *in situ* at one view.

The *second* is overcome by making the model so much larger than the natural size of the parts, that, when revolved upon its pedestal, the students at the remotest parts of the lecture-room can plainly see every part of the demonstration.

As this model was constructed to exhibit what drawings could not show, it is impossible, by drawings, fully to represent the model; nevertheless, a de-

scription of its construction and use may perhaps be somewhat aided by reference to the accompanying engravings:

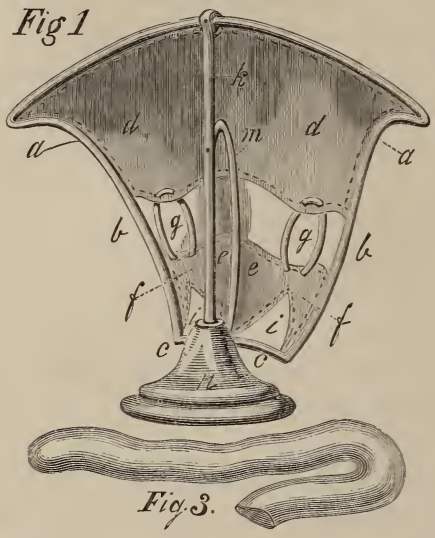

Figure 1 represents the posterior view of the hypogastric region of the abdominal walls: *a*, *a*, the anterior superior spinous process; *b*, *b*, the inguinal fold; *c*, crest of pubis. These constitute the outline or frame, and are made of stout brass or wrought-iron rod; *d*, *d*, sheet-tin to preserve the natural contour; *e*, *e*, another sheet of tin for same purpose, but, at *f*, having a guttered shape corresponding to the course of the inguinal canal; *g*, *g*, stout wire passed through holes in the tin plates to form the "deep abdominal rings;" *i*, *i*, fenestræ representing ruptures of conjoined tendon opposite the superficial rings in direct hernia. The perforations parallel with all the outlines of the tin plates are for the convenience of sewing thereto the sheets of linen after wardto be attached; *k*, stanchion; *m*, its arm; *n*, pedestal in which stanchion revolves.

Figure 2, *a*, *a*, *b*, *b*, *c*, *c*, are same as in figure 1; *d* is a sheet of linen, glued over the entire surface of both tin plates, stretched tightly over the interspace between them, and, when dry, sewed at all the perforated points; this represents the transversalis fascia, the opening in which, at *e*, is the deep abdominal ring.

Fig. 4.

A narrow strip of stout muslin is now sewed tightly all the way down from *f* to the crest of the pubis to form the linea alba. A sheet of linen, corresponding to the right half of the model is now sewed to the entire right half of the frame, and to the strip representing the linea alba. It is then cut transversely across at the lower margin of the deep abdominal ring; the upper section is detached, at its lower part, from the linea alba, and the lower section, at its upper part, from the frame or inguinal fold, near the deep abdominal ring, the object being to allow the lower end of the upper sheet to be raised, and the upper end of the lower sheet to slide a little downward. The linen is now sized, the lower part of the upper section painted with vermilion in water-color, representing the fibres of the transversalis muscle *g*, on the under side; the surface being painted in a similar manner to represent the internal oblique at *h*, and the rectus abdominis at *i*, *i*, while the body of the linen sheet, left blank between them, represents the conjoined tendon, to form which both muscles unite. In this the hook is seen to be fixed near the margin of the rectus abdominis.

A piece of red silk or cotton cord is sewed to the lower section of the conjoined tendon, representing the epigastric artery *n*, *n*, which, upon the conjoined tendon, at this point, is subject to motion downward and inward.

The superficial abdominal rings are seen at *o*, *o*; these

are formed of stout copper wire, bent as represented, and fastened to the frame, at the crest of the pubis only, the rest being free; they are stout enough to maintain their position, but pliable enough to yield a little before an advancing hernia. Another sheet of linen is now stretched over the right half of the model, and sewed to the frame along the inguinal fold and crest of the pubis. Its upper margin is left free, its inner margin being supplied with hooks answering to eyes, by which it can be neatly fastened to the linea alba, or be folded back at pleasure; while the sheet is tightly stretched and hooked, the section of canvas between the pillars of the superficial ring is cut, folded back, and stitched, so as to cover them.

The spermatic cord (*p*, *p*) is made of woollen cloth covered with blue silk. At its upper end it is stitched fast, just below the interior of the deep abdominal ring (*e*), and occupying the course of the inguinal canal, it is passed through the superficial abdominal ring (*o*, *o*), and is stitched to the frame at the crest of the pubis. The last mentioned linen sheet being hooked, stretched, and sized, the fibres of the lower part of the belly of the external oblique (*s*) are then painted in the same manner as the others, and with Indian ink or any neutral color, the fibres of the tendon of the external oblique are painted so as to show how, by their divergence, the superficial ring is formed. The left side of the model is reserved for the more minute demonstration of the descent of the testicle, and of the coverings of the herniæ. Behind the deep abdominal ring is a shifting piece of blue silk for the peritoneum. In front of the ring, a shifting loose fold of green silk for the fascia propria; over that, a fold of pink silk for the fascia transversalis. Some red elastic bands attached to the lower border of the internal oblique, and transversalis muscles, form the cremaster muscle.

At the superficial ring, the intercolumnar fibres, figure 2 (*o*), on the left side of the model, are made of elastic bands, which may be unhooked, or be made to yield before a descending testicle, or a protruding hernia, like the other fascia, which stretch across their pathway. If it be found difficult to make the testicle assume all these coverings, including the intercolumnar, without tearing, it will suffice just as well to exhibit these fascia *in situ*, in colors corresponding to those which, in the same order, may be reflected from a section of the model of a testicle. If it be desired to provide for a similar demonstration of femoral hernia upon the same model, it is easy to append (*x*) Poupart's ligament, the femoral ring, and all the parts concerned, in its surgical anatomy, by a sim-

ple continuation with the same materials. The same may be said of umbilical hernia, for either of which any particular directions would be superfluous.

In teaching the surgical anatomy of inguinal hernia with this model, the muscle and tendon of the external oblique is folded back as seen in Fig. 2, and at once is brought into view the superficial abdominal ring (*o*), the deep abdominal ring (*e*), the inguinal canal and spermatic cord (*p*, *p*) in their entire course, as also the epigastric artery (*n*, *n*) crossing beneath the cord; thus exhibiting clearly all the important relations at one and the same time.

I am aware that this exhibition requires the epigastric artery to lie upon, instead of beneath, the conjoined tendon and transversalis fascia; but, as it is the course, rather than the location, of the artery which is of paramount importance, the deviation is unobjectionable and entirely advantageous.

I find it quickly understood when with accompanying demonstration I say, the inguinal canal may be looked upon as a tube formed of the fascia propria, passing obliquely downward and inward through the abdominal walls; its upper end is the deep abdominal ring, its lower end the superficial abdominal ring; beneath the floor of this tube at about its middle crosses the epigastric artery, passing upward and inward to be distributed to the rectus abdominis muscle.

The Mechanism of Hernia.—This is demonstrated with this apparatus in the following way:

Fig. 3 is a silk or cotton bag of the same color as the covering of the spermatic cord, stuffed loosely with cotton wool. This is passed from behind forward through the deep abdominal ring, and made to simulate a large knuckle of intestine within the canal;

which, by hooking tightly the tendon of the external oblique, is converted into a bubonocele.

Continue to push it from behind forward, and it makes its appearance, protruding through the superficial ring, assuming its distinctive shape, which is unavoidably pyriform; seeing it is an oblique inguinal hernia.

Afterward, or at the same time, pass a similar portion of intestine of a pink color from behind forward through Fig. 1 (*i*), and through the superficial ring Fig. 2 (*o*), and the differential globular form of a direct hernia is as distinctly demonstrated.

The changes of relation to which the epigastric artery is subject in some of these conditions, and especially in old cases of oblique inguinal hernia, we have heretofore lacked the means to demonstrate. It will be remembered that the upper part of the lower section of the conjoined tendon, Fig. 2 (*k*), is partially detached from the frame at the inguinal fold; accordingly, as the hernia is pushed onward, the conjoined tendon, with the epigastric (*n*, *n*), which is stitched upon it, is crowded before it; the artery therefore increasingly embraces the inferior and internal portion of the neck of the sac, while the lower and internal margin of the deep ring slowly tends toward a parallelism with the superficial ring.

These demonstrated changes of relation in the artery suggest their own lessons in connection with the operation for division of stricture in strangulation; so, the difference in the course of the herniæ being clearly understood, we come next to

The Principles and Methods of Reduction.—To demonstrate these, we require an exceedingly thin India-rubber gut. In Fig. 4 it is seen partly filled with

water, its neck tightly tied around a fitting cork. For water, contents of various densities may be substituted, but the gut must not be made more than half or two-thirds full. The corked end of the gut is then made fast by its string to a point near the interior of the deep ring; through this the other end is protruded, the tendon of the external oblique is closely hooked to the linea alba, and as it emerges through the superficial ring a very natural-looking oblique hernia with typical shape is made to appear.

The stanchion may now be lifted out of its pedestal, and the frame turned partly or completely upside down; whereupon the contents of the gut immediately gravitate, and a well-appreciated demonstration is made of "Reduction by Position."

Taxis.—This may be taught to, and be practised by, the student. Use the left hand to constrict the gut in the canal, and with the fingers of the right press upon the centre of the fundus of the protruded tumor; and, notwithstanding the pressure be made in the right direction, it will be shown you are thereby rendering impossible that which you are attempting to perform; the pressure upon the centre of the tumor increases its circumference, flattens it over the pillars of the superficial ring, and prevents its return. The left hand may now be shifted to constrict the neck of the tumor, the method and advantage of traction upon the tumor for breaking loose slight adhesions being at this point shown if desired; then, the constriction being continued at the neck by the left hand, and the fundus of the tumor being completely grasped by the right, the contents of the gut recede with a gurgle; with the right index and middle fingers the India-rubber gut is passed in pursuit,

of its recent contents, and the demonstration has been completed of "Reduction by Taxis."

By adding the integument and scrotum, the various operations for the radical cure of hernia may be demonstrated, the parts beneath the skin may then be exhibited as fixed *in situ* after each operation, by the particular method adopted.

It may be observed that, in speaking of the "rings," I use the term "superficial" and "deep" exclusively. My reasons for this have been fully stated in a paper read before the County Medical Society of New York, January, 1868, entitled "A new Method of teaching the Surgical Anatomy of Inguinal Hernia" in which the terminology is fully discussed. I will here state, however, that I consider the total abandonment of the terms "external ring" and "internal ring" indispensable to prevent ambiguity and perplexity in description; by substituting therefor the terms "superficial ring" and "deep ring," the terms "external" and "internal" are left available for use in this, as in every other region of the body, with reference to the median line only.

A general advantage gained in the use of this apparatus is, that instead of a mere blind memorizing of arbitrary terms, the student is at once impressed with the facts, and sees how, and why, the coverings of the respective herniæ must be what they are, and could not be otherwise.

The apparatus is simple in its construction, and may be made by any person for himself.

From my own experience, combined with that of others in different parts of the country, who have had duplicates of it made, I would commend its exclusive

use as a preparation for, and its subsequent use as an auxiliary to, dissections upon the cadaver.

This apparatus presents the following advantages not possessed by the cadaver:

It is cheaper. It is convenient for any place and season. It exhibits all the important relations of hernia at one and the same time.

Its proportions may be adapted to the largest class.

It allows of an exhibition of the process of descent of the testicle; of the occurrence, progress, and strangulation of hernia, as well as of the methods of reduction, either by position, taxis, or operation.

It enables the student to learn, by actual manipulation, the practical treatment of these conditions, whether for their relief only, or for their permanent and radical cure.

Reviews.

ART. I.—*Therapeutics and Materia Medica.* A Systematic Treatise on the Action and Uses of Medicinal Agents, including their Description and History. By Alfred Stillé, M. D., Prof. of the Theory and Practice of Medicine and of Clinical Medicine in the University of Pennsylvania, etc., etc. Philadelphia: Henry C. Lea. 1868. 2 vols. 8vo, pp. 824–864.

Materia Medica for the Use of Students. By John B. Biddle, M. D., Prof. of Materia Medica and General Therapeutics in the Jefferson Medical College, etc. Third edition, enlarged, with Illustrations. Philadelphia: Lindsay & Blakiston. 1868. 8vo, pp. 384.

Dental Materia Medica. Compiled by James W. White. Philadelphia: S. S. White. 1868. 12mo, pp. 108.

So vast is the study of materia medica, so numerous the additions constantly made to our knowledge of the medicinal

properties of substances, both inorganic and organic, and so preposterous are the claims put forward by some enthusiastic experimenters of the efficacy of this or that agent, that he is truly a shrewd and clever compiler who can manage to keep pace with the real progress, and not be drawn into indiscriminate eulogy of what, after all, may be worthless. Dr. Stillé has attempted his work in this middle course, and has succeeded, all things considered, remarkably well.

He has seen fit, however, to omit some most valuable therapeutic agents, and to notice others in the most meagre way, far beneath the recognition which is almost universally conceded to them; and yet, with these shortcomings, the book stands confessedly a most valuable addition to our literature, and a reliable guide, so far as it goes, to the student. Therapeutics must always be to a certain extent empirical, and yet the known physiological effects of drugs, ascertained by experiment upon animals, are of immense service to us, as giving some rational ground on which we may base our practice. And at the present day these experimentations are so frequent and the results obtained so positive and so valuable, that we can only express our regret that Prof. Stillé has not devoted more space to them, although we observe that he has incorporated some even of the most recent, viz., we find the subject of bromide of potassium worked up to the latest day, and all the recent experiments and observations of any value brought together. A little of this painstaking would have added materially to the worth of other portions of the book. We are thus exacting in our demands, because Dr. Stillé's work is becoming the best known of any of our treatises on materia medica, and we as Americans naturally look to it, it being one of our acknowledged authorities, for every thing worth knowing, and sometimes we find ourselves sadly disappointed in the search. In the mere matter of the uses of medicinal agents, or therapeutics proper, it is not equal to Mr. Waring's book, which, though less than one-half the size of Stillé's, we consider the most remarkable book *of the kind* ever written, and yet the two books are not comparable, for they cover quite different ground. Dr. Stillé's should, from its scope, include the whole ground over which Waring with such untiring industry

has plodded, though we could not ask for or expect the minuteness there displayed, and at the same time he goes far beyond Waring, and gives us the history, descriptions, and physiological properties of the articles of the materia medica; so that he would err greatly, who, examining Stillé by the text of Waring, should pronounce the first the inferior of the second. Indeed, looked at in its totality, as our legal friends say, we consider Stillé's by all odds the most valuable work.

In the present issue of his book, Dr. Stillé gives about one hundred pages of reading matter more than are found in the last edition, and for the first time he treats of chromic acid, permanganate of potassa, the sulphites of soda, etc., carbolic acid, nitrous oxide, rhigolene, and Calabar bean.

We do not choose to enter into a minute examination of Dr. Stillé's work. His attention has been very forcibly directed by other reviewers to the special deficiencies apparent in the book. Our remarks are based upon a careful estimate of the whole scope and bearing of the work, and where we find so much that is good, and so little that need be added to make the book complete, we venture to express the hope that in forthcoming editions we shall see these deficiencies fully filled up and rounded off, so as to give an unexceptionable stamp of authority to what is even now one of the most valuable works in the language on the subjects of which it treats.

Dr. Biddle, within the compass of one handy little volume, attempts a description of the materia medica; and as we have just seen that Dr. Stillé, with his two portly tomes of over eight hundred pages each, has failed in many points to do full justice to his subject, it may readily be inferred that even the qualification "for the use of students" does not protect this book from a suspicion that it must necessarily be very imperfect. We object to books prepared expressly for use of students, and have previously and most emphatically put on record our opinion that such productions are only an abomination, and hinderance to knowledge. And yet Dr. Biddle's book is less open to this charge than many other students' books. We have used the previous edition of his book largely, and have carefully examined this new issue, and speak advisedly. The

truth is, that the title of the book is a misnomer, and it more nearly approaches the character of a syllabus of a course of materia medica, although lacking in the systematic classifications we naturally look for in a syllabus, and in the hands of a thorough student, or a judicious teacher, it can be made to do good service.

How far the author has touched any thing like completeness in his descriptions may readily be seen, when we state that, after devoting forty-two pages to a discussion of the mechanical and imponderable agents, the mode of action of medicines, their forms, the methods of application and administration, the circumstances modifying them, in other words to the principles of the whole study both of materia medica and therapeutics, the remaining three hundred and forty pages are given up to the special articles of the materia medica, and here we find enumerated no less than three hundred and four different substances. We say enumerated, for in many instances there is nothing more than a mere allusion to a drug, and in no small number of cases drugs of great value are dismissed with the most unceremonious brevity, that quite illy accords with their real value. And yet the author is up to time in the introduction of new substances, and we find introduced for the first time notices of carbolic acid, bichloride of methylene, the compounds of amyl, tetrachloride of carbon, iodoform, iodides of ammonium and sodium, nitrous oxide, guanara, coca, and woorara. At the same time, the more than usual curtness with which the author chronicles the virtues of these articles, would seem to indicate that he has but little faith in any thing new, or at all events that he desires time to make good the claims of their virtue.

The classification of the substances in accordance with their physiological effects is a good one, and special pains are taken to indicate those articles whose action, varying under different circumstances, would entitle them to a place in more than one class. Thus one source of perplexity to the student is provided for. To those who sit under the teachings of Prof. Biddle, we can readily perceive that this book may be very serviceable, but to others who desire any thing like a com-

petent knowledge of materia medica and therapeutics, we cannot conscientiously recommend it.

Our dental friends certainly are under obligations to Mr. White for the admirable little compilation which he has here brought together under the title of "Dental Materia Medica." He tells us that he prepared it as a ready-made answer to the frequent inquiries received, "asking information as to the properties, dental uses, and methods of applying the various medicinal agents and preparations advertised for sale or alluded to in the discussions of dental societies, and by correspondents in the journals."

Many who practise dentistry (and the same will hold good too, to some extent, of medicine) are not well grounded in that preliminary education which thoroughly prepares them for a full appreciation of all that pertains to their art, and they find themselves frequently at a loss in estimating the value of the infinite and many times useless claims that are put forward for this or that article. This little book gives succinctly an account of the really useful agents of the dental materia medica, and as such cannot fail to be of value, not alone to the dentist, but to that large class of physicians who, by reason of location, are called upon occasionally to play the part of dentist. Indeed, in our younger days we confess—

"Quamquam animus meminisse horret luctuque refugit"—

to having "pulled" many a useless molar, and administered to many an aching fang, and we should have been glad of so concise a medium of information as Dr. White's book.

No classification, not even alphabetical, is followed, and so we find mercury treading upon the heels of prepared chalk, and litmus sandwiched in between arsenic and chloroform; but a full index compensates for this defect, and we are bound to admit that the compiler has succeeded well in carrying out his plan of making the volume, "as far as it goes," convenient, useful, and reliable.

Although the little work is evidently intended in a large degree as a medium of advertising Mr. White's own stock in trade (for, as is well known, he is the proprietor of the

largest house in the country for furnishing dental materials), still we can overlook this little commercial dodge, in view of the fact that he has given such valuable information to his patrons.

Art. II.—*On the Pathology and Treatment of Albuminuria.* By William H. Dickinson, Cantab., F. R. C. P., etc. London: Longmans, Green & Co. 1868. 8vo, pp. 265.

Dr. Dickinson's book is one of the most valuable contributions to the study of diseases of the kidney that have been made of late years, and we base this positive assertion upon a most careful reading of the entire volume.

The title is, we think, objectionable, for the book is devoted to a consideration only of those forms of disease which are usually associated under the name of Bright's disease; this, however, is of small importance.

A description of the anatomical elements of the kidney and its appearances in a healthy state are given as a prelude to the subject proper of the book, and as essential to a right understanding of the classification of the different forms of disease, and attention is especially fixed upon the triple constitution of the organ, viz., the tubes, the fibrous tissue, and the blood-vessels. Now, disease may attack either one of these structures, and thus we have:

1. *Tubal disease*, characterized by an extravagance of cell-growth, for, like the bronchi and other secreting surfaces, these channels are prone to take on this condition, usually called inflammation, when under the influence of morbid stimuli. The tubes become loaded with epithelium, and distended; the whole gland is enlarged. This is the large, smooth kidney of Bright. The author terms it *tubal nephritis.*

2. *Disease of the fibrous tissue*, characterized at first by an increase of this structure, commencing at regular intervals on the external surface, and followed by a slow contraction, which produces little depressions, giving the kidney the appearance of being studded with small granulations. Ulti-

mately these processes invade the whole organ, and we have the small contracted granular kidney of Bright, or *granular degeneration.*

3. *Disease of the vessels.*—This is a change not peculiar to the kidney, but occurring in common with other organs, and consists in the pouring out from the minute arteries of a glassy material, which permeates the whole tissue, giving the organ at first a whitish or anæmic look, producing increase in size and density, and a peculiar translucent aspect. This new formation has a contractile power, so that shrinking of the organ ultimately follows. The special characteristic of this material is, however, its reaction by iodine, which gives to it a dark-brown color, quite unlike the tint produced in the healthy kidney by the same reagent. This is the so-called "waxy" or "amyloid" degeneration. Dr. D. claims that the infiltration of this peculiar material occurs usually after the system has been exhausted by a prolonged discharge of pus, and hence he terms the disease *depurative*—a most unfortunate (and of questionable etymology also) choice of terms, as, by common usage, this word has assigned to it an entirely different signification. And, besides this, the term is very apt to mislead a cursory reader or a careless student.

Now, these diseases differ widely in their etiology and symptomology; they affect different periods of life; have special methods of invasion and progress, and are unlike each other in their danger and in the treatment they require. The author, therefore, takes them in the order above given, and considers each one in all the relations we have just mentioned.

Such is a brief outline or skeleton of the book. We cannot follow the author step by step, but will take some points which presented themselves to us as worthy of note. In the section devoted to the consideration of tubal nephritis, acute and chronic, the chapter on the clinical history of this disease is remarkably concise and explicit, and, not being loaded down with technicalities, or obscured by theorizing, it is made plain and readable. Relying on his statistics, Dr. D. shows that the male sex is more liable to tubal nephritis than the female. In adult life this is unquestionably true; but, at the same time, it is questionable whether the sex is the predis-

posing cause. In childhood there is not so much difference between the sexes in the liability to the disease, Dr. D.'s figures being, boys 37, girls 24. The author infers from this "the fact that the difference exists in children, where the habits of the sexes are the same, is enough to prove that the masculine gender is a predisposing cause." We question the validity of this numerical reasoning, but, without entering into a discussion thereon, simply suggest that, as the disease is very rare until after the second year of age, there is, in our experience, enough of difference between the habits of boys and girls after this age to render the first as much more exposed to the influence of the immediately exciting causes of the disease as will account for this actual difference.

Considering the causes in detail, it is, of course, apparent to every observer, that, in adults, exposure to cold is the frequent source of the disease, while scarlatina in the young is the great producing source of the mischief. Even in childhood it is admitted that cold will occasionally, though very rarely, produce tubal nephritis. Thus, of 61 cases under 12 years of age occurring in the Children's Hospital to which Dr. Dickinson is attached, 50 were traceable to scarlatina, 2 to measles, 3 to cold, 6 uncertain (page 65). Again (page 54), Dr. D. refers to 43 fatal cases, but it is impossible to determine whether they are included in the above list, in which the disease was traced to wet or cold in two. We have recently had a very interesting case under our own care, in which the most careful and varied investigation failed to detect any thing more than exposure to a draft of air, while the body was heated, as the exciting cause. The patient was six years of age, in perfect health up to the beginning of the dropsical symptoms, had measles when three years old, but has never had scarlatina. There was no eruption, no sore throat preceding this attack, nor were there at any time any traces of desquamation. Against all our teachings, impulses, and experience, we were compelled to admit that this case was one in which the tubal nephritis was not preceded by any exanthematous disease. The child made a good though slow recovery, the treatment being hot baths of steam and water, the salts of potassa, diluents, mild but continued counter-irri-

tation over the kidneys, and, finally, the muriated tincture of iron in small doses.

The chapter on treatment is remarkably sound and instructive. We call attention only to one point, viz., the use of distilled water, given very freely. While we most cordially indorse this plan in practice, we are a little skeptical as to Dr. D.'s explanation of the action of the remedy (for remedy it is, as much as any drug, and vastly more reliable than many we are taught to use) by a mere washing of the accumulated cells thrown off from the lining epithelium of the tubes. Is it not equally plausible to suppose that in some way it modifies the changes of nutrition which are manifested by this increased cell-growth? The use of antimony, which Dr. D. disapproves of, and very justly, we call attention to merely to say that in an epidemic of scarlet fever through which we passed some years since, and when we had under our care six or eight cases of consecutive albuminuria, we failed to produce any impression on the dropsical symptoms until we fell back on the time-honored (?) remedy of antimony. It was used with a view of acting on the skin only, and in very small doses, the highest we gave being $\frac{1}{64}$ of a grain. One case proved fatal, from inflammatory complication of the respiratory organs. We might be induced to use this remedy again, but we think we should first work pretty faithfully with our hot-air baths, digitalis, and distilled water.

We pass that section of the book devoted to the consideration of the granular kidney, with reference to a single point only. There is much difference of opinion as to the starting-point of the degenerations found in this disease, Dr. George Johnson being, perhaps, the chief advocate of the view that the mischief begins in the epithelial cells of the tubes, and is primarily a crumbling or disintegration of these cells. Dr. Dickinson first advanced the view that the changes commence in the intertubular fibrous structures, and that the epithelial changes are no greater than is consistent with health. In the very advanced cases the epithelium may sometimes be found crowded together, as it were, and so altered somewhat in shape, presenting more angular outlines than in health. The presence of oil-globules in the cells is not more frequent than

in the healthy kidney. This intertubular contraction accounts neatly for the formation of the cysts so frequent in the granular kidney, and the stringing together, end to end, of the microscopic cysts points very plainly to the origin by constricting of the tubes. Altogether, we are of opinion that Dr. D.'s views on this point will ultimately be acknowledged as correct.

Dr. D. holds sharply to the opinion that this form of disease, the small granular kidney of Bright, is not an advanced stage of the large smooth kidney before described. The two are entirely distinct. This opinion is generally shared in at the present day by the English pathologists, though the view of Frerichs and Reinhardt, that the large white kidney will, if the patient live long enough, eventually become red, granular, and contracted, is accepted by most Continental observers. This view was adopted in Rosentein's recent work without discussion,[1] as if there could be no room for question on this point.

But the most novel and interesting part of Dr. D.'s book is that wherein he takes up the study of the so-called "waxy" or "amyloid," or, to use (under protest, however) the author's own term, the "depurative" degeneration. As this subject may still be considered *sub judice*, we shall content ourselves with giving a little more in detail the views of the author, making use of his own words as far as possible, in order to avoid any chance of misrepresenting those views.

The disease essentially consists of a general outpouring of a certain material which differs from the proper constituents of the body. This material can be distinguished by the action of iodine upon it, producing a persistent and deep reddish-brown color. As first observed, it is transparent and homogeneous, appearing in the walls of the small arteries and gradually working its way into the surrounding tissues. This deposit is not confined to the kidneys, but may affect the liver, spleen, suprarenal capsules, lymphatic glands, the mucous lining of the alimentary canal, etc.; in short, almost any part of the body to which blood-vessels are supplied, but the changes resulting in the several organs are somewhat different. It appears that the material is fibrine, which has been deposited in this form in consequence of the loss of the alkali

[1] See "Roberts on Urinary and Renal Diseases," p. 323.

with which it is ordinarily combined, and which seems necessary to hold it in solution. The results of ultimate analysis show the morbid deposit to agree in composition with fibrine and albumen. That it is fibrine and not albumen is shown by the facts that it has a strong tendency to undergo contraction after its deposition, that it becomes converted into fibroid tissue, a metamorphosis which is common with fibrine whenever it is deposited in small bulk, as a coagulum in the arachnoid, or vegetations upon the valves of the heart, and, lastly, that in certain cases it is identical in appearance and reaction, as well as continuous in position, with the hyaline casts, which are found in the renal tubes, the fibrinous nature of which it is not possible to doubt.

The most striking characteristics of the formation are, however, due to the absence of the free alkali. This deficiency is not only proved by analysis, but, if we add potash or soda to the diseased tissues, the characteristic reaction with iodine is no longer obtained, and, furthermore, by artificially depriving fibrine of its alkaline, we can produce a substance identical in all its reactions with the amyloid or waxy material. The material, then, which is poured out under certain conditions, is dealkalized fibrine.

By far the most frequent condition which leads to the separation of this material from the blood is protracted suppuration. The discharge must be copious and long continued, and it is not connected necessarily with tubercle or any other constitutional taint, or with disease of the bone. An analysis of the table, which comprises all the cases that came under Dr. D.'s observation, shows that five-sixths of his cases, taken indiscriminately, were associated with the purulent process, and this connection is so frequent as to compel the belief that they stand in the relation of cause and effect.

Thus, of 66 cases under observation, there were 51 in which there had been a positive history of protracted suppuration, while five others gave presumptive evidence that suppuration had previously gone on. In four there had been albuminuria, associated with some other form of kidney disease, and six cases in which the causes could not be traced. All the processes by which the blood is so altered as to contain excess of fibrine with deficiency of alkali we cannot as yet presume to be acquainted with, and it may be eventually shown that there are diseases which act upon the blood so as

to produce the same effect as a purulent discharge and induce the amyloid or depurative deposit, without the medium of a suppuration.

This statement of Dr. Dickinson's views covers, we believe, pretty much the whole ground he has gone over, and here we must leave it, for further observation is needed to place the points in question beyond controversy.

One other chapter of Dr. D.'s book we must call attention to, viz., that containing his views upon alcohol as a cause of renal disease. There his opinion is quite counter to that which usually obtains—at all events, in this country—on this subject, and he claims that the use of alcoholic drinks is comparatively inoperative as causing disease of the kidneys. He admits that the excessive use of such fluids may set up a renal catarrh, although, compared with the other causes of tubal disease, it is very infrequent. Again, certain liquors, by causing gout, may indirectly bring on granular degeneration, for this we know is a frequent sequence of the gouty diathesis. But alcohol, as a rule, attacks other structures in preference to the kidneys.

The woodcuts and exquisitely-colored plates add much to the beauty as well as the value of this volume, and we do not remember to have anywhere seen more accurate illustrations of the various diseased conditions of the kidneys. We again commend the book as of unusual merit, and worthy of a careful study by every physician.

Art. III.—*The Anatomy and Histology of the Human Eye.* By A. Metz, M. D., Professor of Ophthalmology in Charity Hospital Medical College, Cleveland, Ohio. 1868. Philadelphia, published at the office of the *Medical and Surgical Reporter.* 8vo, pp. 181.

The author of this treatise states that he was induced to write it because in teaching he "seriously felt the want of a text-book on the anatomy and histology of the human eye. There does not exist," he goes on to say, "to my knowledge, a treatise on this subject that includes the results of the labors

of the more recent histologists to be found in ophthalmological journals and in memoirs on special subjects. It has been my aim to collect this material into a connected form, and in such a manner as to adapt it alike to the requirements of the medical student and of the practising physician."

This statement bids us regard the book as a compilation, and to judge it as it may or may not fulfil the duty of properly presenting modern researches into the structure of the eye. Two tables, viz., the list of illustrations and the list of authors consulted, indicate that the author has not spared pains to procure material. The illustrations are borrowed, and with proper credit, from all the late text-books and memoirs, and the list of authors includes almost, if not quite, every thing which had appeared up to the date of publication. Krause has since that time issued a paper on the retina, and Henle's "*Handbuch der Anatomie des Menschen*" presents, in the part which appeared in 1866, a discussion of the anatomy of the eye, which deserved mention and careful consultation. Perhaps other treatises might be mentioned which are left out of the author's list, but we are not disposed to find fault. We are satisfied that the author made himself familiar with his subject, and we are delighted to find so much good anatomy in his pages.

We quote certain parts as samples—take the minute structure of the cornea. "Engelmann (*Ueber die Hornhaut des Auges*, Leipzig, 1867) says, that the cornea proper is composed of the finest fibrillæ, which lie close to each other, and lying between these are numerous cells and nerves. In the frog these fibrils have a thickness of 0.0001 mm., and each one is separated from its neighbor by an immeasurably small space, filled with a fluid. These fibrillæ are united into larger lamellæ, about 0.004 mm. in thickness, which are placed into 15 to 20 layers concentric with the corneal surface. The fibres of each layer run parallel with the corneal surface and with each other. The fibres cross each other at an angle of about 90° in two contiguous layers, resting on each other. In some places the fibres run from one lamella into another.

"Between two contiguous layers are found, distributed at equal distances apart, a large number of cells. These corneal

cells consist of masses of *protoplasma*, polygonal in form and without nuclei (*sic*). They are vertical to the corneal surface and flattened. In the centre of each mass is found a vesicular nucleus, with a nucleolus. These masses that surround the nuclei measure 0.02 mm., and have projecting from their corners from six to twenty processes, which run in various directions throughout the corneal substance. The majority do not project beyond two contiguous lamellæ; some, however, pass through the layers at sharp angles. Some of these processes terminate free in minute points; others are connected with neighboring cells. Each cell then is connected with other cells in the same layer, and also with the cells of the layers above and below, so that the whole corneal substance is connected by a penetrating net-work of this protein material. Neither the cells nor processes have membranes, but lie unenveloped within the inter-fibrillar spaces which they completely fill."

In one sentence it will be remarked that the corneal cells or corpuscles are said to be "without nuclei," and in the next sentence we read that "in the centre of each mass is found a vesicular nucleus with a nucleolus." This discrepancy must be a *lapsus*, and the last statement is what the author means to put forth. The cornea has not yet revealed to us all its histological secrets, and we must be thankful for the facts we receive. The fibrillæ which Engelmann finds have not been seen by all observers, but we incline to believe in his view. The remarkable "*wandering cells*," which Von Recklinghausen first noticed, we believe, are duly mentioned, and how they push their way among the fibrillæ. The existence of tubes or lymphatics is denied.

Of the crystalline lens, the minute anatomy is what Becker has given us, and is the best we have. The origin of the fibres from the marginal cells, and their termination on the planes of the star, are better described than by any other author. Mr. Hulke's labors on the structure of the capsule are also quoted.

In the retina, the views of Ritter and Schultze are most relied upon. We now know and were taught by H. Müller that connective tissue enters largely into the formation of this

membrane. The problem is to identify the really nervous elements and trace their connection with each other. Whether the retina contain six or nine layers is comparatively unimportant, but to unravel the meshes of fibres which go from the rods and cones, and trace them to the ganglion cells, and finally to connect these with the optic nerve fibres, is a most arduous task. It is interesting to compare the various drawings which have been put forth as showing the arrangement of the elements—to take the schemes in Nysten's dictionary and compare with the sketch as given by Ritter, page 67, or rather Metz's diagram, to show Ritter's explanation. The latter diagram is, of course, to some degree hypothetical, but it brings out some facts now undisputed—that the rods have an axial fibre, in which, at the base, is a granule, and the rod fibre runs into the mesh-work of the intergranular layer. The external granular layer differs in essential character from the inner granular, being composed of ellipsoidal cells, each having three striæ, and round cells with two striæ. These granules are contained *within* the fibres of Müller. This term being applied by Ritter, not to the connective tissue fibres, but to nerve fibres, which Müller confounded with connective tissue, the two kinds of fibres being inextricably mixed, and only lately distinguished from each other.

The intergranular layer consists of the two kinds of fibres, and then come the inner granules, which send off processes, on the external side to the intergranular layer, on the internal side to the fibrous layer. There they become connected obscurely with the multipolar ganglion cells, and with these the optic nerve fibres are distinctly seen to run.

Thus we gain some clearer notion of the complex relations of the retinal nerve elements. Besides these, the connective tissue remains, whose fibres are excessively minute and possess nuclei, and spring from the membrana limitans interna. But we cannot attempt a fuller description of the retina.

We have chosen those subjects which are confessedly the most difficult, to test our author's skill. The whole book deserves careful study, and will be found instructive and satisfactory.

Art. IV.—*Microscopical Examinations of Blood, and Vegetations found in Variola, Vaccina, and Typhoid Fever.* By J. H. Salisbury, M. D. New York: Moorhead, Bond & Co., Printers. 1868.

Any theory which proposes to trace to tangible causes diseases of hitherto inexplicable origin, and to substitute for indefinite "constitutional influences" physical changes and parasitic organisms, ocularly demonstrable, cannot fail to possess the greatest interest and fascination. For, beyond the gratification of scientific enthusiasm, and the pleasure of another assurance that medicine is rapidly advancing from empiricism to exactness, its influence upon treatment, and consequently upon the preservation of life, would be incalculable. We accordingly open Dr. Salisbury's book with considerable eagerness, which is enhanced by the fact that the author's name has been within a few years associated with a large number of pathological discoveries, any one of which, if established, would give it lasting fame.

In the first section, "On Blood Examinations," after stating the immense clinical experience from which his conclusions are drawn, he enumerates about seventy points which are a part of the things to be noticed in blood examinations. The chief stress, however, is laid on the appearances presented by the fibrine of freshly-drawn blood in the healthy and in pathological states. The conditions of things in rheumatism, pulmonary tuberculosis, and anæmia, are illustrated by diagrams. In the former of these diseases the microscopical appearances are not only peculiar and diagnostic, but antedate all symptoms, so that by timely examination of the blood "the causes of the dreaded disease may be discovered and removed before the patient is aware he is in danger, thereby saving perhaps severe suffering and grave pathological disturbance." We are unfortunately left in doubt as to whether phthisis and anæmia can be with equal certainty predicted and prevented. Now, if we did not know that medical writers are never betrayed into this fallacy, we should really have supposed it a *petitio principii*, to set down a perfectly sound man as a case of prevented rheumatism, simply

because his blood presented an appearance similar to that of a patient known to be rheumatic. We should, on the contrary, infer that the phenomena were in no way peculiar to the disease in question.

The second section, "The Blood Disk," contains experiments to prove that the disk normally contains cholesterine, which escapes along with the coloring matter, if the blood be allowed to stand. In addition, four new cryptogams are described, viz., *Entophyticus hæmactus; Zymotosis regularis; Crypta carbunculata*, which Dr. S. considers to be the cause of carbuncles; and *Zymotosis escularis*, the characteristic vegetable of an epidemic resembling remittent fever, which prevailed last year among the horses in the vicinity of Cleveland.

The remaining sections are devoted to the cryptogams which the author has found in variola and vaccina, and in typhoid fever. He styles them respectively *Ios variolosa vacciola*, and *Biolysis typhoides*. The former, he claims, presents two forms, a fungoid and an algoid. The fungoid development is the cause of variola. If, however, the plant finds a nidus in the tissues of the cow, it fails of its full development, by reason of these tissues not being well adapted to its growth. It there assumes an algoid condition, which produces vaccina, and cannot, when transferred to human tissues, redevelop into the fungoid state. He assumes that the spores of the same vegetable, if detected in fermenting substance, would produce in the man variola and in the cow vaccina. He illustrates his position by the similar conduct of the *Leptothrix buccalis* and *Sphærotheca persica* under analogous circumstances. Moreover, he deems it "highly probable that very many of the minute entophytal algæ are but special phases of development of parasitic mucedinous fungi." We suspect that this surmise of the author's contains much truth; enough, perhaps, in connection with some other points of premature inference, to quite undermine his beautiful, but insecure, pathological air-castle. The careful researches of some foreign observers lead to the same conclusion, that organisms hitherto esteemed distinct are but phases of the same thing.

Earnestly as we should welcome increased pathological

knowledge, when we are asked to accept as fact the theory that this or that cryptogam is the source of this or that disease, we have the right to require that the hypothesis should have sustained at least the following tests: That different experimenters should arrive at the same or similar conclusions regarding the particular parasite at fault. That the parasite should be found with a sufficient degree of constancy in connection with the disease. That it should not exist in health, or, if in health, under conditions sufficiently different to account for the change in its effects, and that a perfectly sound man should not be considered as diseased, simply for the benefit of the hypothesis. That the fungi, etc., of different diseases should be shown to be really distinct from one another; and, finally, That the disease should be actually produced by inoculation of the parasite, due precaution being taken that the disease should not be contracted in the ordinary way. The hypothesis should satisfy at least these sources of doubt before it can demand recognition. It does not appear, at least from the book before us, that any of these requirements have been complied with, unless we, perhaps, may except the second.

Like the other essays which have recently been published by the author, the book gives evidence of zeal, of hard work with the microscope, combined with an eagerness and enthusiasm that hurry the author on to a theory faster than logic will allow, or than the evidence, if he has given us all he possesses, will warrant.

The execution of the work, both in regard to press-work and illustrations, is admirable.

Bibliographical and Literary Notes.

[Under this heading we purpose to set apart a portion of the Journal for short notices and literary jottings of all sorts. We receive in the course of the year a very large number of pamphlets, monographs, and small books (many of them of great value), the notice of which, under the old system of displayed titles and the use of large type, was simply impossible, but, by adopting the plan of foot-note titles, with smaller type, we shall save space enough to enable us to say something of these fugitive portions of our literature.]

If we may place any faith in the truth of the old adage, *Ubi virus ibi virtus*, we think that Cincinnati, at the present writing, is entitled to the honor of holding two of the most immaculate of mortals, for we have rarely witnessed a more virulent controversy than that just transpiring between Profs. Blackman and Bartholow, of the aforesaid city, and of which controversy this little paper [1] is only a part. For the rest of the story, like our friends of the *Ledger*, we must refer to the columns of the *Medical Repertory*, even at the risk of enormously increasing the circulation thereof.

How the trouble originated we know not, but this special phase of the fight is a charge of literary larceny, by Prof. Blackman against Prof. Bartholow, in that his essay on Locomotor Ataxy, published originally in the *Cincinnati Journal of Medicine*, and subsequently reprinted as a monograph, is nothing but a literal translation of Topinard's Prize Essay presented to the Paris Academy of Medicine in 1864. Dr. Bartholow acknowledged to having made a "liberal use" of this essay, but Dr. Blackman thinks that it would require a most extraordinary stretch of courtesy to entitle a literal translation to the scanty meed of only a "liberal use" of another's work. He (Dr. Blackman) gives us side by side long extracts from Topinard and Bartholow, and, if these quotations be cor-

[1] On Bartholow and Pro's "Liberal Use" of Prize Essays, or Prize Essaying made easy, and taught in a single Lesson. By George C. Blackman, M. D., Prof., etc.

"When energizing objects men pursue,
The Lord knows what is writ by Lord knows who."

Cincinnati, Ohio, 1868. Pamphlet, pp. 16.

reçt, there is much of truth in the charge brought against Prof. Bartholow, and the case is really not far removed from the celebrated one of José Pro, whose essay on stricture of the urethra (which received the prize of the Société de Chirurgie of Paris in 1856) turned out to be only a literal translation of Mr. (now Sir Henry) Thompson's well-known monograph on that subject. This most "shameless and extensive" plagiarism was, we believe, first shown up in this country by Dr. Atlee, of Philadelphia.

We do not purpose to enter into the merits of the controversy between the two belligerent professors, but we do most earnestly enter our protest against the manner in which the warfare is carried on. It is simply disgraceful to both parties, and they alike deserve censure for forgetting the gentleman in putting on the disputant; and the severe terms in which their course has been reproved by other journals (for the controversy is not a mere private quarrel, but made public property) we trust will incline them to more moderation.

"Pistols and coffee for two" are hinted at by one of our contemporaries, but we fancy the valor of the combatants will be expended and their ardor appeased with less unpleasantly suggestive and sanguinary exploits. Between Hector and Achilles,

"Ira fuit capitalis, ut ultima divideret mors;"

but—the rest and the application we leave to our classical readers.

New Books.—We have received from the publishers, Messrs. Lindsay & Blakiston, too late for notice in this number of the Journal, the first volume of Aitken's Practice of Medicine. The exhaustion of the first edition within twelve months speaks well for the character of the work, and the estimate put upon it by the profession. It is, in our opinion, the best body of practical medicine extant.

We have also, too late for review, Dr. Emmet's book on Vesico-Vaginal Fistula, from the Messrs. Wm. Wood & Co., of this city; and Prof. H. R. Storer's work on Abortion, published by Little, Brown & Co., of Boston.

Among recent French publications we observe a work by Ch. Renault, "*On Syphilis in the Fifteenth Century*," and Dr. L. Ancel's treatise on the "*Nails in an Anatomical, Physiological, and Pathological Light.*"

Three books on Diseases of Children are announced—one by Prof. Geo. T. Elliot, of this city, to be published by the Messrs. Appleton. A second, by Prof. J. Lewis Smith, also of Bellevue College, to be published by Henry C. Lea, of Philadelphia. The third, by Thos. Hillier, M. D., of London, has already made its appearance in a beautiful reprint by Lindsay & Blakiston, of Philadelphia. We shall have a review of this book in our next issue.

Dr. Grainger Stewart, of Edinburgh, who is already well known by his valuable contributions to medical literature, is engaged on a work on "*Bright's Disease of the Kidney.*"

Wunderlich's work on "*Thermometry in Disease*" has appeared from the press of O. Wigand, at Leipzig.

Messrs. D. Appleton & Co. announce Dale's "*Compendium of Medicine*," and Fenwick on the "*Morbid States of the Stomach and Duodenum, and their Relations to other Diseases.*"

Reports on the Progress of Medicine.

OBSTETRICS AND DISEASES OF WOMEN.

1.—*A case of Double Uterus and Vagina* [L'Evénement Medicale, from the Monatschrift für Geburtskinde].

This case occurred under the observation of Dr. Rabe, of Dresden.

Madelaine B., a peasant, twenty years of age, entered the surgical department of the hospital of Dresden on account of a vaginal discharge and some slight excoriations near the vulvar orifice. Menstruation, which began at sixteen years of age, was always regular and sufficient. The external genitals were natural, but there was no hymen. In introducing a speculum of medium size, an obstacle was met with about a finger's breadth from the entrance of the vagina. On pressing the instrument lightly against the obstacle, two funnel-shaped fossæ were seen through the opening of the speculum, separated from each other by what appeared to be a vertical and prominent

fold of mucous membrane. By merely separating the labia, one might readily suppose that he was viewing a prolapse of the anterior wall of the vagina, but such, however, is not the case; this mucous fold is the anterior border of a wall or partition which separates the vagina into two lateral and equal parts, a fact of which it is easy to convince one's self by introducing two fingers simultaneously. The two vaginal canals are straight and provided at their entrance with a sort of a mucous fold resembling a hymen.

On the right side, as well as on the left, the exploring finger reaches, at about a finger and a half's depth, a vaginal neck (of the uterus), somewhat small, hard, and forming a prominent projection; on each side there is a transverse opening. The vaginal portion of the right side is a little smaller than on the left, and is directed rather against the wall, than in the line of the entrance of the vagina: the anterior lip of each of the two necks is longer than the posterior. Between the two exploring fingers, we readily feel the solid resisting wall, quite down to the bottom of the vagina, and it is impossible to ascertain the least communication between the two lateral halves of the vagina. A sound (Simpson's) can be introduced into the uterine orifice on the left side without difficulty, but cannot be introduced on the other side. The orifice of the right side is so constricted, that it is impossible to determine whether the uterus is double throughout. The patient shows no other anomaly in the conformation of the other organs. According to her own statement, coitus appeared never to have been perfectly and naturally performed.

2.—*Cases of Ovariotomy, with some Remarks upon the Operation.* By Wm. Warren Greene, M. D., Prof. of Surgery in the Medical School of Maine. [Boston Med. and Surg. Jour., March, 1868.]

Dr. Greene has done ovariotomy eight times, and here gives us a brief synopsis of each case:

Case I. Proved fatal on the third day, from hæmorrhage from the pedicle. No peritonitis; tumor developed from right ovary, weighed 40 lbs.; adhesions to abdominal parietes, stomach, and liver; ligatures carried through posterior cul de sac of vagina, passed out of the vagina and fastened to thigh by adhesive plaster.

Case II. Tumor multilocular, adherent to abdominal walls, intestines, stomach, and liver; weight 38 lbs.; pedicle right broad ligament; ligatures disposed of as in Case I.; abdominal cavity washed out with artificial serum after Dr. Peaslee's plan; recovery slow but complete.

Case III. Tumor multilocular, adherent firmly to abdominal walls, intestines, and bladder; pedicle left broad ligament, treated with Storer's clamp shield, and ligatures, which last were disposed of as in other cases; external sutures carried through the peritoneum; death on fifth day from strangulation in act of vomiting; tracheotomy; body examined and no traces of peritonitis.

Case IV. Tumor multilocular, 58 lbs.; recovery; nothing of special interest in the case.

Case V. Tumor unilocular; weight not given, size small, developed from right ovary; slight peritonitis, controlled, as in Case II., by injections into the abdominal cavity; recovery.

Case VI. Tumor multilocular; weight 34 lbs.; developed from left ovary; right also covered with little cysts, and was removed; recovery perfect.

This operation was done in October, 1867, and February 20, 1868, the patient thus writes of herself: "About the 15th of December, I felt as if I was going to be unwell, and had a slight colored discharge, lasting half a day. At the same time in January I felt the same, and had again a bloody discharge, but only a little, so it soiled my drawers. This month I have had slight symptoms, but no discharge, and the symptoms lasted only a day."

Case VII. Tumor multilocular; weight 25 lbs.; not stated on which side developed; mild peritonitis; recovery perfect.

Case VIII. Multilocular cyst right ovary; weight 20 lbs.; inner surface of wall of one cyst ulcerated; peritonitis, proving fatal on third day.

The operation was performed in the same manner in all these cases. Dr. Greene's comments on the operation are so judicious, that we quote them entire:

From a thorough study of my own cases, and of those which I have seen in the practice of others, with a somewhat careful examination of the literature of ovariotomy, I am led to the following conclusions:

1. That, where the tumor is large, it is impossible to form any accurate estimate of the extent or strength of adhesions that may exist, before reducing the bulk by tapping, and even then extensive visceral adhesions may be present which cannot positively be detected. It is not safe to argue the absence of such attachments because the patient has not suffered from marked symptoms of peritonitis, for the reason that such an inflammation often occurs sufficient to produce very firm adhesions, and yet so latent as to escape notice.

2. The existence of adhesions is no contra-indication for an operation, but on the other hand, such cases, even when the bands are numerous and strong, do the best as a rule. They suffer less shock and are less liable to peritonitis. The membranes seem, by the previous morbid action, to have

acquired a tolerance of such disturbing causes as would awaken inflammation in one that had never been diseased.

3. In preparing the patient, great care should be taken to secure a healthy state of the secretions, and the system should be sufficiently impressed with the muriated tincture of iron to insure plasticity of the blood.

4. The *utmost* gentleness and delicacy should be observed in all manipulations by surgeon and assistants. Strange as it may seem, this most important rule is too often violated, to my personal knowledge. There is no excuse for unnecessary handling of parts by rough, dry, or cold hands, or exposure to the air a moment after the operation is completed.

5. A most powerful prophylactic against shock and subsequent inflammation is the free use of artificial serum (common salt 3j., albumen 3j., pure water Oj.) *at blood heat.* Keep the parts thoroughly and constantly moist with it.

While I believe that this serum, acting as a mechanical protection to the parts, is in this way of great advantage, I still attach *much more importance to the heat.* A moment's reflection will convince any one, theoretically, that a delicate serous membrane suddenly exposed to the air, and its temperature reduced twenty or thirty degrees, and maintained at that point for any length of time, is much more liable to inflammation than one which has been carefully kept at or very near its ordinary heat, and that too by the application of a liquid almost precisely like its natural secretion; and my own cases of abdominal section afford to me conclusive evidence that this is true. In none of my cases has there been any thing like collapse.

Nor is this application of heat to be restricted to this class of operations. I have latterly discarded the use of cold water for sponging during any operation which exposes a large raw surface. In large amputations, in dissections for the removal of large tumors, especially about the neck and trunk, I am thoroughly convinced that the shock is very much less, as also the danger of inflammation, if hot water be used instead of cold. The cases where hæmorrhage requires the substitution of cold are so exceptional as not to invalidate the rule. I would much prefer multiplying ligatures to chilling the parts.

I prefer an elevated temperature of the room, but consider the moisture of the atmosphere of little importance, comparatively.

6. The treatment of the pedicle in the cases reported seems to me more reasonable than any other. The use of the clamp, for the fastening of the pedicle in the external wound by any means is only applicable to long pedicles, and even then, in case of peritonitis with much distension, is a serious complication, as also in subsequent pregnancies. To this is to be added the danger of intestinal strangulation. The same objections obtain against Dr. Storer's recent proposal to pocket the pedicle with additional ones in case primary union fails. The actual cautery is unreliable, and so is the ecraseur, notwithstanding the few cases in which the latter has been successfully used.

The cutting of the ligatures short and dropping the stump back into the abdomen would of course be *the* plan, if safe. But in the first place the immunity from sloughing is by no means established, and in very many if not all the cases there must be, aside from any such process, a collection of fluids, serous, sero-sanguinolent or purulent, more or less, which had much better be readily discharged than left to the care of the absorbents. (I know of one case that was reported cured by this operation, that died, after all, of septicæmia.) In all my cases there was a vaginal discharge from the first, usually slight and varying in character. By carrying the ligatures down through the posterior *cul de sac*, all danger from this

source is obviated. The opening is made at the most dependent part of the pelvic cavity where the fluids will naturally gravitate, and where they will thus find a ready exit. The pedicle is more effectually secured by the ligatures than by any other means, and if carried through the vagina they produce no noticeable irritation, and after their work is done no foreign body is left in the abdomen, and at the same time the external wound is allowed to heal by first intention.

But another great value of the opening into the vagina is the facility which it affords for washing out the abdominal cavity, to which procedure I attach so much importance.[1]

The after-treatment must be conducted upon general principles, and not according to any fixed rules. I think the cases very rare where large quantities of opium are required, or can be borne without harm.

Finally, the case must be a very peculiar and urgent one upon which I would operate and leave the patient for after-treatment in the hands of another person, except it be one who was experienced in the management of such cases.

3.—*Congenital Malformation of the Fallopian Tubes.* By T. Grainger Stewart. [Journal of Anatomy and Physiology, May, 1868.]

In a patient who died of typhoid fever about the middle of May, 1867, certain peculiar morbid conditions of the pelvic viscera were found. The following description of the case is extracted from the Pathological Register of the Royal Infirmary :

The *pelvis* was of natural size and form. The *bladder*, *vagina*, and *rectum*, were natural. The *uterus* was of natural size. The lower half of the cervix was inclined backward and to the left; but in the middle there was a sudden bend, so that the upper half of the cervix and the whole of the body of the organ was at an obtuse angle to the lower part, and was so far inclined toward the right, that the fundus was considerably out of the middle line. The *cavity* contained some reddish mucus. The *walls* were natural. The *right Fallopian tube* was connected with the uterus in the usual way, and it presented a natural appearance for one inch and a quarter from its commencement, but there it terminated abruptly in a cul de sac, and a fibrous band of the same length as the tube stretched across the space and connected it with the anterior wall of the rectum. The *left tube* was also natural at its commencement; it extended along for three inches; in the first two it gradually expanded until, on being laid open, it measured half an inch; from that point it gradually narrowed, and terminated in a cul de sac, without trace of fimbriæ, but with a number of small cysts, mostly on slender stalks, which appeared to correspond to fimbriæ. Its outer third was connected by a firm but slender fibrous membrane with the peritoneum covering the rectum. Both tubes contained a reddish watery fluid. The *left ovary* was of natural size; on its surface there were numerous cicatrices, both old and recent. It occupied its natural position, but was partially concealed by fibrous adhesions. The

[1] After trying several different methods for passing the ligatures through the canal, I prefer the following: Pass into the vagina a pair of common, uterine dressing forceps, with the blades closed, and push their point upward in the *cul de sac* until, looking into the pelvic cavity, the surgeon sees the membranes stretched over them behind the uterus. While in this position open the blades a little, pass a bistoury through the septum between them, close them, pass through the opening, seize the ligatures and drag them downward out through the vagina.

I obtained the idea of thus disposing of the ligatures, as also of using artificial serum, from Prof. E. R. Peaslee, of New York.

right ovary was also natural in structure, but it was more completely concealed by adhesions, and by a group of cysts. On section both were found to contain corpora lutea and Graafian vesicles. The *cysts* just mentioned were situated to the right of the ovary, and somewhat behind it, quite unconnected with it, but so closely connected with the *body of Rosenmüller* that it was evident that they had been formed by dilatation of some of its tubes. Together they formed a mass of the size of a walnut. The *body of Rosenmüller* of the left side was natural. On the right side there was a single group of small cysts on long pedicles (Hydatids of Morgagni). There were numerous adhesions between its different peritoneal surfaces, besides the two denser bands connecting the tubes with the rectum. The most prominent were some which connected the fundus uteri with the larger group of cysts, and some which deepened and narrowed Douglas's space.

In this case it appears to me we have a congenital malformation of the Fallopian tubes and a morbid development, in connection with the parovarium, as the original morbid conditions, while the numerous bands of adhesions were results of the irritation of the ova which escaped from the ovaries, and to them again the peculiar malposition of the uterus is referable. In this view my friend and colleague, Dr. Matthews Duncan, concurs. The patient was a married woman, but was childless.

The only points which call for remark are the Fallopian tubes and the position of the uterus.

I. The *Fallopian tubes.*

Klob mentions, in his excellent work on the morbid anatomy of the female generative organs,[1] that the congenital deficiencies of the tubes may be reduced to three classes:

(*a*) Those in which the tubes are purely rudimentary, represented either by traces of fibrous tissue or by bands of muscular substance.

(*b*) Those in which they are represented by a solid cord of fibrous tissue.

(*c*) Those in which the development of the fimbriæ about the extremity is defective.

It is evident that our case cannot be exactly referred to any of these classes; and, so far as I am aware, it would appear to constitute a new variety of malformation.

II. The peculiar flexion of the uterus.

Flexion more commonly occurs at the upper part of the cervix than at the middle, as in our case. Klob remarks[2] that in congenital lateral flexions the position of the cervical portion remains natural, while in the acquired it is inclined in a direction opposite to that of the body of the organ. Our case confirms this statement, and shows that the same is true in cases where the flexion is in the cervix, rather than between the cervix and the body of the organ.

4.—*Post-mortem Cæsarean Section—Child saved.* By Peter Brotherston, F. R. C. S. E. [Edin. Med. Jour., April, 1868.]

This case is worthy of notice from the rarity of the occurrence, and from the fact that such prompt interference succeeded in saving one life, that of the child. Dr. B. was

[1] *Pathologische Anatomie der weiblichen sexual Organe.* Wien, 1864.
[2] *Op. cit.*, p. 71.

engaged to attend the lady in her confinement, and, receiving a hurried message to the effect that she had fallen in a fit, from which she could not be aroused, he hastened to the house and found that she was already dead. The husband, being assured of the death of his wife, readily assented to the proposition of Dr. B. to open the abdomen, as offering a slight chance of saving the child. The operation was at once performed, another physician, Dr. Dickie, who had been summoned at the time the patient had fallen, being present and assisting. The child, a large healthy female, was extracted with ease, but gave no signs of life. Artificial respiration was resorted to, and in about fifteen minutes the heart began to beat, and the child gave a convulsive sob or two. The surface of the body being now quite cold, the child was plunged into a hot bath, and cloths dipped in quite hot water were applied to the head, artificial respiration being also persevered in. In about half an hour the child was struggling and crying, and in perfect condition. At the date of the report, three months after this occurrence, the child was remarkably healthy and robust.

An examination of the parts of the mother showed that there had been separation of the placenta for about two-thirds of its surface, from the fundus of the uterus. This separation had produced great hæmorrhage, rupture of the membranes in the vicinity of the placenta, and distension of the uterus. No escape of blood *per vaginam* and no sign of dilatation of the os. Dr. D. estimates very carefully the time Mrs. M. had been dead before the operation was commenced, and places it at twenty-three minutes.

5.—*Case of Cæsarean Section—Mother and Child both saved.* By W. W. Greene, M. D. [Boston Med. and Surg. Jour.]

Mrs. B., aged 28 years, primipara, the subject of deformed pelvis, resulting from rachitis during childhood; the antero-posterior diameter of the superior strait was less than two inches. We deem it hardly necessary to give our unqualified approval of the procedure adopted in this case, but more especially do we consider that the early adoption of the operation, without waiting for trials of forceps, or resort to the destruction of the child, to say nothing of the increasing danger to the mother, was eminently sound and conservative. In our estimation it was the only proper course to follow, and we have not hesitated, in face of the recorded opinion of many of the leading authorities in obstetrics, to advise this operation, even with an antero-posterior diameter of two and a quarter inches:

Her pains were strong and frequent, and she began to exhibit marked symptoms of exhaustion, to which her consciousness of peril contributed not a little. The child was very active *in utero*. Upon explaining to herself and friends the probable impossibility of delivery *per vaginam*, and that even were there a bare chance of success by evisceration, she would, in her exhausted condition, incur greater risk from the operation under such circumstances than from abdominal section, the latter operation was assented to.

The case was so urgent as to admit of no delay, and we were therefore obliged to proceed with less assistance than I could have desired.

The patient took a full dose of fluid extract of ergot with a little brandy, after which ether was administered. When under its influence, she was placed on a table, in the ordinary position for ovariotomy. I now, standing at her right, and while the abdomen was carefully supported on either side by assistants, with a common scalpel made an incision in the median line from a little above the umbilicus nearly to the pubes, which was soon carried through the abdominal walls and the uterus exposed. This organ was then incised from the fundus downward about six inches, the knife being used very cautiously until the cavity was opened and the *liquor amnii* evacuated. On carrying my right hand into the uterus, I readily seized the feet (which were on the left side, it being a vertex presentation), and with little delay extracted the body, but some difficulty was experienced in delivering the head, occasioned by the powerful and unremitting uterine contractions, intensified, as I suppose, by the ergot. This, however, was soon accomplished, and the little fellow—a boy of eight pounds—cried lustily. Without waiting to sever the cord, an assistant supporting the child, I again introduced the hand in search of the placenta. This was attached on the left side, about midway between the neck and fundus, and about one-third of it was detached. The remainder was readily separated, but its extraction, which was soon accomplished, with the membranes, was by no means an easy task. I had not anticipated so powerful muscular action in an organ thus mutilated.

There was considerable hæmorrhage during the delivery, but not sufficient to cause any serious apprehension, and it ceased at once upon the removal of the placenta, the edges of the uterine wound being nicely approximated by the contractions of that organ. Unquestionably the ergot had fulfilled the indication for which it was given, namely, to control hæmorrhage and secure apposition of the cut edges by its action upon the uterine muscular fibres.

After carefully cleansing the parts with sponges dipped in water at blood-heat, and then thoroughly moistening them with artificial serum at the same temperature, the external wound was closed by interrupted sutures placed half an inch apart, and including the entire thickness of the parietes except the peritoneum. These were of silk soaked in boiling wax, as we had no silver wire at hand, a fact that caused me not a little anxiety at the time, although I may say, not only from its use in this but in many other instances, that smooth, well-twisted silk sutures, thus prepared, approximate very closely in value to those of silver.

The abdomen, which had been unremittingly supported by the hands, was now enveloped in a firm bandage, and the woman put in bed, well covered, and dry heat applied to the extremities, which were rather cool. They soon became warm, however, and as soon as she could swallow she got twenty-five drops of fluid extract of ergot and half a grain of morphia. After the effect of the ether had passed away, the pulse was over 100 and rather feeble. Countenance pale, with that peculiar expression which indicates a marked shock. She was rather restless and *wakeful*. She now

got morphia and brandy, with beef-juice, and from 6 P. M. till 3 A. M. she took one grain of morphia and *one quart* of brandy. (This amount of morphia in addition to the half grain which she took at five o'clock, just after the operation.) Just after 3 A. M. she fell into a quiet sleep, which lasted five hours, from which she awoke in excellent condition.

The treatment now instituted was perfect quiet; anodynes *pro re natâ*, ten drops of fluid extract of ergot and twenty-five drops of tincture of muriate of iron every four hours, the two alternating—the former to be omitted in forty-eight hours and the latter to be continued, if borne by the stomach, until the external wound was healed.

The further history of the case contains nothing of special interest. The external wound healed throughout by first intention. A moderate peritonitis followed, but not sufficient at any time to require *heroic* doses of opium. The iron was well borne throughout, and the lochial discharge occurred and continued as after an ordinary case of labor.

In a letter dated August 30th (tenth day after the operation), Dr. Emery says: "Have just returned from Oak Hill, and am happy to report Mrs. B. in fine condition. I have removed the last stitch. There is very little fulness or tenderness of the bowels." The mother and child are now in excellent health.

6.—*A Successful Case of Cæsarean Section.* By DRS. D'AQUIN, BRICKELL, BORDE, AND BAYON. [New Orleans Journal of Medicine, July, 1868.]

This case is of peculiar interest on account of the condition for which the radical interference by abdominal section was advised and undertaken. We are of opinion that the course pursued was the proper one, though few at least of the older obstetricians will agree with us there.

The following is Professor Brickell's account of the patient when he first saw her in consultation with Dr. D'Aquin. She had then been in labor about ten days, Dr. D'Aquin having already been in attendance five days, and previously to this a midwife had been with the patient for five days, during all of which time the labor pains had been constant and severe:

Her condition, when I saw her at 2 P. M., was as follows:—Face expressive of great anxiety; skin of dusky hue; lips distinctly approaching lividity; tongue red, with a broken white fur over the middle; respiration twenty-six; pulse one hundred and forty-three to the minute, of pretty good volume, but losing force; all uterine action ceased for fifteen hours past. Examining *per vaginam*, I found the vaginal secretion almost arrested and the temperature of the parts much elevated. The lips of the uterus were enormously swollen (probably three-quarters of an inch in thickness), quite severely rent in three places, and were jutting prominently into the summit of the vagina. They and the whole vaginal canal were excessively sensitive, so much so as to cause loud complaints from the patient, who was certainly very courageous. The neck of the womb was very long and admitted of the tolerably easy passage of two fingers, but a third finger put the parts distinctly on the stretch, and, holding the three in a line, and advancing them steadily, I could distinctly feel the tissues of the neck breaking. Indeed, such was the friability of these parts, I am sure that I could, by firm pressure, have broken down the tissue of the lips

with my finger and thumb. The idea distinctly conveyed to my mind was, that the parts were on the verge of sloughing. The collapsed head of a putrid child presented high above the unyielding neck, and a portion of the scalp was lying in the vagina. The odor emitted was extremely disagreeable.

In addition to all this, there existed a band across the posterior wall of the vagina, alluded to by Dr. D'Aquin, and which appeared to me as an extensive and strong cicatricial mass of semi-lunar shape, and very seriously narrowing the canal.

My conviction was soon clear that the delivery of the woman *per vias naturales*, would be a most difficult and tedious operation, and I promptly recommended the Cæsarean section on the following grounds:

1. No considerable part of the child could be drawn through the constricted cervix uteri, now so friable, without laceration, the extent or direction of which could not be controlled.

2. In consequence of the first proposition, the child would have to be taken away piecemeal; and the vaginal canal being dry, and of very limited capacity, the opening into the uterus very limited, and the child high up, this operation would be very tedious.

3. The excessively sensitive condition of the parts, and the necessarily tedious nature of the operation would necessitate the use of chloroform, and the prolonged administration of any anæsthetic would seem to be contraindicated by the rapidity of the circulation and respiration, together with the existing degree of carbonization, the result of greatly prolonged labor.

4. The lips and neck of the uterus seemed already on the verge of sloughing, and prolonged manipulation would hasten such result, and would, in my opinion, endanger the woman very greatly.

These points were urged in consultation, but some great fears of Cæsarean section being expressed, I proposed to try and demonstrate whether any thing could at all readily be accomplished by instrumental means. I first tried faithfully to apply a pair of delicately-made Hodge's obstetric forceps to the head, but it was a failure, there being no adequate room for the introduction and adaptation of the second blade. I then tried faithfully, during half an hour, to reduce the head piecemeal with Meigs's craniotomy forceps, but there was so little room in the vagina and cervix for both the hand and instrument, that the progress was lamentably slow. To my mind the proposition was clear that many hours would be required for piecemeal delivery, that an anæsthetic would be absolutely necessary, and that the result of prolonged manipulation would be fearful.

The consent of the patient and her friends being obtained, the operation of Cæsarean section was performed in the usual way. The points of interest in the operation were—

1. An almost entire absence of hæmorrhage from the abdominal incision.

2. No hæmorrhage consequent on stripping the placenta from the surface of the uterus.

3. The failure of the uterus to contract after its contents (the child had been dead some days) were removed; of course there was a free hæmorrhage from the incision through the uterine walls, and to control this bleeding, silver sutures were passed through the uterine walls—the ends being left about a quarter of an inch in length, and folded smoothly down on the

uterine surface. Five months subsequently to the operation no inconvenience in any way had been manifested by the presence of the sutures in the abdomen. The mode of dressing the external or abdominal incision is worthy of note, for we believe that the accumulated weight of dressings often used is only a hinderance to the prompt healing of the wound:

> The surfaces of the abdominal wound were carefully sponged and brought together with fine interrupted sutures of strong silver wire—the same being plunged deeply through all the tissues, peritoneum included. Above and below and between the sutures, broad adhesive strips were placed, the same reaching across the abdomen; at right angles and across these four other strips were placed, one across the ends, and one on either side of the wound; and over this a light cotton bandage was placed to support the abdominal walls, and the result was adhesion by the first intention.

The original difficulty in this case is a matter of deep interest. Dr. Brickell thinks it was rigidity of the soft parts, which may have had its origin in some inflammatory action previous to or soon after marriage. The existence of the cicatricial band across the vagina lends credence to this view, for we can readily imagine a cervicitis to have existed simultaneously with the vaginitis, and that the result was an impairment or destruction of the dilating capacity of the lower segment of the uterus.

7.—*Absence of Urethra and Clitoris; Incomplete Development of the Nymphæ and Labia; Incontinence of Urine in a Girl æt.* 14. [Bull. Méd. de l'Aisne, 1866—Gaz. Méd. de Paris, No. 28, 1867.]

The only two parallel cases reported, it is believed, are by J. L. Petit. No mons veneris, no superior commissure, the labia terminating in a depression, one centimetre and a half wide upon the symphisis pubis, and continuous above with the abdominal wall, the skin covering it being hairless, very fine, resembling delicate cicatricial tissue, and lying directly on the bone below. There are a few hairs on the labia, and a few excoriations caused by contact of urine. Posterior commissure natural. The nymphæ are rudimentary, hardly one centimetre in length and breadth, about the middle of the labia, and, anteriorly, on their inner face. No trace of a clitoris. No vestibule. Immediately under the symphisis pubis there is a little bright-red tumor, about the size of the half a cherry, and very sensitive. Pressing upon it with the finger, it yields, and the finger passes into a cavity, easily explored, and the size of an egg, which is the bladder; urine immediately flows. The tumor is the mucous membrane of

the bladder, which closes the entrance to the bladder, and takes the place imperfectly of the sphincter, which is absent.

Petit thinks that the only means of avoiding the constant dribbling of urine in such cases is a compress bandage.

8.—*Hernia of the Ovary; Operation; Death.* [Lancet, June 20, 1868.]

Dr. Englisch mentioned this case at the meeting of the Medical Society of Vienna (April 3, 1868). The patient was thirty-two years old, had suffered from a reducible tumor in the left groin for thirteen years, and presented, on admission into the hospital, all the symptoms of strangulated hernia. As the taxis did not succeed, the usual operation was resorted to, and, on opening the sac, the ovary and Fallopian tube were discovered, but not a trace of intestine. The former were tied and removed. Twelve days after the herniotomy the patient died of severe erysipelas and partial peritonitis. At the next meeting, Dr. Weinlechner brought before the notice of the society a woman of thirty-two, presenting ovarian hernia on both sides. She had never actually menstruated, but at each period a molimen was apparent, with swelling of the inguinal tumors. The vagina ended above in a cæcal pouch, and absence or extremely small size of the uterus was suspected. The patient was not deficient in sexual sensations.

DISEASES OF CHILDREN.

1.—*Congenital Imperforate Urethra in a Male Child.* By Dr. David Murray. [Edin. Med. Jour., April, 1868.]

This class of malformations is very rare indeed. We have no cases recorded in which the urethra had been obstructed all through. The most general form is that in which a sort of membrane covers and entirely occludes the passage. This structure may be either membranous or fibrous in composition. It may vary both as regards extent and thickness. Sometimes it is so strong that it can only be overcome, if at all, by considerable force; at other times it may be ruptured with the greatest ease. In regard to its position, it may be situated at any part of the canal.

1. The most common situation is that immediately over the external orifice.

2. The second is that situated toward the vesical orifice. The catheter may be introduced a certain distance, but cannot be passed into the bladder.

3. The third locality is immediately behind the bulb, about an inch from the orifice. This form is very rare.

In regard to symptoms, the complete retention of urine is sufficient to indicate the necessity for prompt interference. This may vary. In those cases where the obstruction is merely orificial, an incision with a sharp-pointed bistoury, and the introduction of a small bougie or catheter occasionally, if there is any risk of adhesion, is all that is necessary. When the obstruction is situated farther down the canal, a probe-pointed bougie may be pushed through the obstruction, or, if this is not possible, the urethra must be opened by an incision in the perineum, or the bladder must be penetrated.

The case illustrating this class of obstructions occurred in my own practice, and is as follows: A. A. was born on the 2d day of November at 10 A. M. At birth nothing abnormal presented itself on a superficial view. On calling, however, some twenty-four hours afterward, I was informed by the nurse that the child had passed no water. On proceeding to examine, I found that the penis and scrotum were well formed, but I could not discover any external opening. Considering that it belonged to the first class of cases, those with a membranous obstruction over the orifice, with a sharp-pointed bistoury I made an incision, after which I endeavored to pass a catheter with considerable force. On that occasion, however, I failed to pass it. On calling a few hours afterward, I was informed that the child was still unrelieved, and was suffering much. Under these circumstances, it was thought advisable to call in the aid of Mr. Annandale. That gentleman succeeded in passing a probe-pointed bistoury about an inch, when he experienced a strong obstruction, which quite occluded the canal at that point. On calling next day, about fifty-eight hours after birth, and finding the child in much the same state, he reintroduced the bougie, and with considerable force broke through the obstruction, but still failed in passing the catheter. On calling in the evening, however, the nurse informed me that the child had passed its water freely in a full stream, and, what could hardly have been expected, without any pain. Since then every thing has gone on favorably, no further steps being taken. In this case there can be no doubt that the principal obstruction was caused by a band drawn across the urethra about an inch from the orifice, though the two edges of the glans seemed to be glued together by some glutinous substance.

On inquiry, the child's father told me that he had suffered from a similar obstruction when young. I failed to get any details on the matter; he states, however, that his mother, who resides in Aberdeen, told him that he was operated on when young; that a tube or probe, as he calls it, was introduced, and that he continued to wear it till he was about sixteen years of age. During all that period he was seldom free from pain or annoyance of some kind.

2.—*A Case of Diabetes in an Infant.*—By Jas. S. Brown, M. D. [American Journal of Obstetrics, May, 1868.]

Diabetes in infancy is so extremely rare, that we deem this case worthy of careful note:

The subject of the disease was a little girl, who was born of healthy parents, and was herself remarkably healthy until about the twentieth month of her age. She was weaned when about a year old, and from that time was fed almost entirely upon farinaceous food. Her appearance was that of a child in typical health, and she was always considered to be very strong and robust. She had never suffered from any of the usual diseases of childhood, such as measles, scarlatina, whooping-cough, etc., and had never sustained any injury from falling or in any other way. When about twenty months old her mother first noticed that she appeared to pass more water than usual, especially at night, and that she drank a great deal; but as her health seemed to be perfect in every other respect, this did not attract any particular attention.

It was not until five or six weeks after this time that her health seemed to be perceptibly deteriorating, and that my attention was first called to her. She was not even now *ill*, and would hardly be said to be ailing. Her appetite was less hearty than usual, but still good; she drank a great

deal, and passed considerable urine; there was some impairment of her strength, and perhaps a very slight loss of flesh. She had no febrile attacks, and in fact no other symptoms than those just mentioned. She would run about and play pretty much as usual, but would grow tired sooner, and want to lie down and rest. On hearing the mother's statement, and examining the child at this time, I did not consider the ailment to be one of any moment, and as the little patient was a relative of my own, I simply proposed to watch the affection until it should develop into something better pronounced, or, as I rather anticipated, disappear as gradually as it had come. I should here state that the symptom which was really the most significant in the case, was mentioned only casually, and with reference to what was supposed to be merely an incontinence of urine. The child used to wet the bed every night, and wet it *very much*, and it was only in this connection that any mention was made of the diuresis.

It was about a week or ten days after this that the mother's attention was attracted to some white spots on the carpet, where a few drops of the child's urine had chanced to fall, and impelled by some feeling of curiosity she wet her finger, touched it to one of the spots, and tasting it, found it to be sweet. This circumstance was at once communicated to me, and led to an immediate examination of the urine, which was found to be of high specific gravity and very saccharine. In the mean time the deterioration in the child's health had been progressive, and was now more distinctly marked.

The appetite still continued to be good, but there was a more perceptible loss of flesh, and a more obvious diminution in strength. The temper, too, began to be fretful and peevish, and the child was evidently out of health, though not appearing to be *sick*. About this time an erythematous eruption began to make its appearance on the buttocks and around the vulva, and subsequently became a source of much discomfort. There was also a peculiar fœtor of the breath not unlike the odor of chloroform, which became stronger as the disease progressed, and was one of the most marked features of the case. The child was still well enough and strong enough, however, to run about and to engage in its usual amusements, but with much less than its usual ardor. Her cheeks were still full of color, and her figure tolerably plump, and she yet retained the appearance of quite a healthy child. In three weeks more she was dead. There was a progressive loss of strength, a rapidly increasing emaciation, a gradual failure of the appetite, occasional febrile attacks of a few hours' duration, and death from simple exhaustion. There was no cough, no diarrhœa, no symptom of cerebral disorder, and in fine, no complication whatever. She was confined to her bed only four days, and just a week before her death went out with her mother and walked a couple of blocks. The amount of urine passed during the last three weeks of her life averaged about five pints per day. Its specific gravity ranged from 1030 to 1036, and it never contained albumen at any time.

It is difficult to estimate the duration of a disease, of which the invasion is so insidious; in this case the child lived about four weeks from the time that I was first consulted about her, and about ten weeks after her mother first noticed that she was passing more water than usual.

A post-mortem examination of the body was made by Dr. Jacobi, who saw the child about two weeks before her death. He found the brain large and well developed, having numerous and deep convolutions. That part of the arachnoid covering the anterior two-thirds of the hemispheres was not perfectly transparent, and there was considerable injection of the dura mater, and in fact of nearly the whole encephalon. There was some thickening of the arachnoid in the fissure of Sylvius, and a few granula-

tions having the appearance of very recent miliary tubercles. The ventricles contained very little fluid, and were entirely normal. The choroid plexus contained hundreds of miliary tubercles. On first inspection the lungs presented nothing abnormal, but a closer examination discovered a number of little dots, just visible, which were evidently newly-formed tubercle. The spleen was found studded with miliary tubercles, their number and development being much greater here than in any other part of the body.

Nothing abnormal was found in the liver or kidneys, except that the right kidney was somewhat larger than the left, and that the cortical substance of the left was more congested than that of the right. These were the only lesions found, and being all of recent origin and obviously secondary to the diabetes, the results of the autopsy, so far as the primary disease was concerned, were entirely negative. In a disease of which the pathology is so obscure, any circumstance that bears even remotely upon it may prove of importance. As already stated, the diet of this child from the time that she was weaned, consisted almost exclusively of food abounding in starch; she appeared to thrive so well upon it, and showed such a marked preference for it, that but little else was given her.

3.—*Polypus of the Bladder in a Child twenty-two months old.* [Edin. Med. Jour., June, 1868.]

The following is a brief abstract of a case of great rarity which lately occurred in the practice of M. Guersant:

The child was under the care of Dr. Penochaud, of Boulogne. It seemed otherwise in good health, suffering only from difficult urination with prolonged efforts. These symptoms were aggravated by an attack of diarrhœa, which first induced the mother to seek medical aid. On being sounded, a fleshy mass was found in the bladder, and a small tumor like a nut was seen to project into the urethra during the child's efforts to make water. The diagnosis of fleshy tumor of the bladder was made, and the child was sent to Paris to be under M. Guersant. The fleshy tumor at the urethra, the straining, and the absence of calculus, were then made clearly out; soothing external applications were recommended, and M. Demarquay was called into consultation. During four or five days, all the symptoms were aggravated. The child became feverish, the efforts at expulsion became more vigorous and frequent, so that the child's mother compared them to the pangs of labor, repeated every five minutes. The projection of the fleshy mass increased, and its surface appeared as if about to slough, being evidently strangulated. The vulva became inflamed; the urine, when drawn off, was muddy and fetid. It was obvious that something must be done to save the life and mitigate the sufferings of the poor little child; yet it was obvious that any interference would be dangerous.

The child was anæsthetized with great ease by a mixture of equal parts of chloroform and ether, and was placed on a table in lithotomy position. The tumor being seized and drawn forward by M. Demarquay, M. Guersant passed a metallic thread as far as possible round the neck of it, after gaining access by dividing the urethra by the scissors. The thread was gradually and slowly tightened by a *serre-nœud*, but broke before the tumor was separated, so the operation had to be finished by the ecraseur. The tumor was very vascular, and under the microscope was found to be composed of connective tissue, with numerous cells and nuclei. There was little bleeding at the time.

Next day the urine was bloody; there was no more effort at expulsion, as it came away easily, but gave pain in its passage. The child gradually

lost strength; and though the bladder symptoms improved, the soft parts became inflamed; there was sloughing. It had diarrhœa, refused its food, and died exhausted on the eighth day. No post-mortem examination was obtained.—*Gazette des Hôpitaux*, No. 23, 1868.

4.—*Strangulated Inguinal Hernia in an Infant seven months old.* [Edin. Med. Jour., July, 1868.]

The child was otherwise healthy and well developed. Two months before admission to the hospital Saint Antoine, the mother had noticed a small tumor in the right labium. This had been often reduced, and as often reproduced, but never was larger than a filbert. The child had never worn a bandage.

On the 28th January, 1868, the tumor returned, and the mother was unable to reduce it. No stools had been passed during the preceding night. Vomiting came on, and unsuccessful attempts at reduction were made by various medical men. Next day the child was brought to Saint Antoine, but was not seen till the afternoon, when another unsuccessful attempt at reduction was made. At 9 P. M. it was chloroformed, and a third attempt at reduction made, still without success. The child was now very weak, with small rapid pulse, and the characteristic *facies abdominalis*. No stool. Vomiting constant and fæcal. A rounded hard tumor existed in the right labium. M. Panas was then sent for, who operated just before midnight. The sac was exposed, of a reddish-brown color, and evidently contained a loop of intestine, and a hard body. On being opened, it was found to contain a fold of intestine about three inches in length, and the Fallopian tube, ovary, and broad ligament of the right side. The constriction was in the external inguinal ring, not in the neck of the sac. The child was dismissed cured in a fortnight, but eventually died of diarrhœa.

5.—*A Case of Hæmaturia in a New-born Infant.* By J. D. Smith, M. D. [Nashville Jour. of Med. and Surg., June, 1868.]

This case, from its rarity, is deserving notice. A male infant, large, well developed, and apparently healthy, forty-five hours after birth, while suffering much as does a child with colic, passed about half an ounce of blood from the urethra. The hæmorrhage recurred five times within twelve hours. At the end of this time Dr. S. saw the case, and gave one drop of tinct. ferri muriatis every hour, and continued the use of an infusion of water-melon seeds, which had been adopted as a domestic remedy. The hæmorrhage recurred again twice within the next twenty-four hours, when there was a copious flow of urine, the first since the hæmorrhage commenced.

The discharge of blood now gradually checked up, passing only in sufficient quantity to color the urine, which in two days more was entirely clear. The whole quantity of blood lost, Dr. S. thought, could not have been less than four or five ounces, the father thinks much more. The child took the breast freely during the entire attack, except when in pain just before the passage of blood from urethra. The pulse remained very weak and the child anæmic during the attack and for some days afterward, but there were no other abnormal signs to be discovered.

Now, whence the source and what was the cause of the hæmorrhage? It might be said that if it had been from the kidneys it would have been mixed with urine. But, little or no urine passed during the time, and when the kidneys acted freely the hæmorrhage measurably ceased, which arrest of function is an indication that the kidneys were in a state of congestion. Could it have been the result of urinary calculus formed in the kidney during intra-uterine existence? If so, why has there not been further evidence of such a formation? Could it have been the result of the hæmorrhagic diathesis? If so, would the blood have coagulated so rapidly? The mother, and the midwife who attended the case, both stated that it could not have been from mechanical injury, for the child had received no shock or concussion whatever.

6.—*The Pathology and Treatment of Croup.* By A. Jacobi, M. D. [Amer. Jour. of Obstetrics, May, 1868.]

No summary can do justice to this paper. It is a most instructive and carefully-prepared monograph, and especially brings forward in a forcible manner the arguments in favor of tracheotomy in croup, for which Dr. J. is a strenuous advocate, having operated sixty-seven times on children suffering with this disease. Of the sixty-seven cases thirteen recovered, a percentage of about nineteen and a half. Dr. Voss of this city has operated forty-three times, with ten recoveries; and Dr. Krackowitzer fifty times, with eighteen recoveries. The late Dr. Waldeman von Roth, one of the first advocates in this city for tracheotomy in croup, operated on forty-eight cases with eleven recoveries. These cases, almost all occurring in this city, show that even when there is a most positive certainty of a fatal result if the disease be left to run its course, tracheotomy affords hope for relief in a considerable degree, about twenty per cent. of the cases proving successful. And, perhaps, were the operation earlier resorted to, a still more favorable record might be made. We regret that the great length of Dr. J.'s paper will not allow us to transfer it to our columns.

THEORY AND PRACTICE.

1.—*Case of true Polypus of the left Auricle of the Heart.* By Dr. Douglas. [Edin. Med. Jour., April, 1868.]

Cases of morbid growths in the interior of the heart are very rare. Andral,[1] Cruvelhier,[2] and Dr. Bright,[3] record each one, and Dr. Douglas's case is of sufficient interest to war-

[1] Anat. Pathologique, edit. 1829, vol. ii. [2] Do., liv. xxix.
[3] Med.-Chir. Trans., vol. xxii.

rant quoting it. The patient was 35 years of age, of large frame, and extraordinary physical power, accustomed to a most active life; in easy circumstances, temperate, but a large eater. When first seen by Dr. D., on the 28th of December, 1867, he presented the following symptoms:

His breathing was laborious and panting, and he had an incessant, most harassing dry cough. He made no complaint of pain, only of the excessive breathlessness and increasing weakness. His surface presented a uniform faint straw-color, and his expression was anxious and dejected, though there appeared to linger the aspect of ruddy health which he had formerly enjoyed. The pulse varied in frequency—about 120 in the minute—and it was small and soft, but regular. Urine acid, diminished in quantity, and high colored.

The impulse of the heart wanted distinctness, had no heaving or force of action; but it had, especially to the ear, an apparent diffusedness and strength, or irritability, which contrasted with the weak and small pulse at the wrist. The exact situation of the apex cordis could not be well defined. The space, dull on percussion, was scarcely extended—no dulness existed on the upper part of the sternum, nor to its right side. The action of the heart was regular.

The sounds of the heart were not well pronounced—the *first* had lost its fulness; the *second* had acquired a degree of sharpness, so that the two were assimilated in their "clang." They had a greater loudness than the state of the impulse led me to expect; they were free of murmur.

There was nothing in the state of percussion or of the sounds of respiration to indicate any lesion of the lungs. There was progressive increase of distress during the first week of January, with increasing cough and dyspnœa, excessive sleeplessness, alarming and singularly vivid dreams, overpowering feebleness, nausea, inability to take food, and occasional paroxysms of excruciating, tearing, substernal pain. He suffered great distress on lying down, and dreaded the nights: the little sleep he had was obtained in an arm-chair, sitting and leaning forward. His pulse rose to 150, and was rarely under 120 in the minute—always small, but never irregular.

Up to the time of his death, on the 28th of January, 1868, he suffered from recurring attacks of dyspnœa, at times very severe and painful, and occasionally attended with semi-convulsive seizures, leaving him in a state bordering on syncope. Œdema of the legs commenced a few days before death, and extended with great rapidity to anasarca.

Sectio Cadaveris.—Fifty hours after death. Decomposition had commenced. The bulk of the body and the volume of flesh were great. The only organs examined were the lungs and heart, and the liver and kidneys.

The lungs were in a normal condition. The heart was flabby, bulky, and very slightly dilated. Its cavities contained well-coagulated blood. The right side presented no special abnormality.

On opening the left ventricle, the rounded nodulated extremity of a tumor, to be described presently, was seen projecting through the mitral orifice. On opening the left auricle, a tumor was found growing from its posterior wall of such bulk as seemed nearly to fill the cavity of the auricle, and hanging downward, its point projecting into the left ventricle. The

tumor was 4½ inches long, 2½ broad, and 1¼ deep at its deepest part. Its superficial and dependent part was coated with some layers of coagulated fibrine, and it presented nodules of a translucent appearance; but its base was organically connected with the auricular wall, and was dense in structure. On the outer side of the auricle, opposite the point where the tumor had its attachment, there were small outgrowths of a structure identical with that of the tumor itself. There was no coagulum in the auricular appendage, nor between the bands of the columnæ carneæ. The pulmonary veins were open; and the valves of the heart were healthy. The aorta was slightly dilated in its ascending portion, and just above it presented an insignificant narrowing, with a small cicatrix at the part.

Microscopic Examination of the Tumor.—The tissue, at its base, was rich in cells, many of them free, many of them resembling connective tissue bodies with long processes projecting from them. Many had prominent nuclei, but none presented the characters of typical cancer-cells. In some parts the cells contained fat granules, and the intercellular structure, which was mainly fibrous, was studded with similar elements. The muscular fibre of the auricular wall was altered, containing elements similar to those found in the mass of the tumor.

The liver was enlarged, reaching slightly below the margin of the ribs. It was intensely congested, and its cells contained fat granules and coloring matter of the bile. The gall-bladder and ducts were much distended with a dark-colored bile.

The kidneys were healthy.

Dr. D. appends to the case an elaborate discussion of the symptoms presented in this case, and, comparing them with those observed in the other cases referred to, suggests that the following summary of signs may afford us some indication of the character of a case, and yet the records are too few to give any positive certainty as to the clinical phenomena to be observed:

1. Rapid development of the symptoms.
2. A previously robust state of health.
3. Dyspnœa, in the absence of signs of pulmonary obstruction.
4. Persistent hurry of the circulation, with regularity.
5. Reflex nervous irritation, with a *quasi* hysteric breathing; paroxysmal cough without expectoration; retching, semi-convulsive attacks, and tearing substernal pain.
6. Delayed obstruction of the circulation of the *lungs*, the *kidneys*, and the *liver*.
7. Anasarca delayed, but rapidly developed.
8. Pulse small and regular.
9. Contrast of a more marked cardiac impulse than radial pulse.
10. Absence of cardiac murmur.
11. Assimilation in the "clang" of the heart's sounds.

2.—*Biliary Calculi discharged through an Abscess opening in Abdominal Walls externally.* By E. W. Boyles, M. D. [Chicago Med. Jour., Aug. 1, 1868.]

Called, June 6, 1867, to see Mrs. W.; aged 50 years; nervo-bilious temperament. Found her suffering intense pain in right hypochondriac region; nausea and vomiting; pulse quick and feeble; bowels constipated; urine light-

colored. Upon examination found considerable enlargement and tenderness in right hypochondrium, extending into the umbilical and right lumbar regions. In fact, most all of the symptoms characteristic of acute hepatitis. Owing to the suddenness of the attack and the paroxysmal character of the pain, I was led to suspect the presence of gallstones. Stated my opinion accordingly, and subsequently ordered the dejections examined therefor, but none were found. I gave opiates, and ordered fomentation. Called again next day and found my patient much relieved. She got up in a few days and attended her usual household duties, but tenderness and enlargement of the liver still remained to a considerable degree. I put her upon the use of nitro-muriatic acid—could not use mercurials, owing to the great susceptibility of the system thereto.

In September she was again confined to her bed for a few days with an attack similar to the first, though not so severe; after which she resumed her household duties. Treatment continued, together with various local remedies, such as iodine ointment, pustulation, blistering, etc.

January 4, 1868, I was again called to see her. Tumor in the side larger, and more circumscribed. I became convinced that an abscess was forming, and used means to hasten the process of suppuration, fomentations, and poultices, but the tumor remained hard—no fluctuation. I began to fear scirrhus; patient considerably emaciated and weak, but no appearance of jaundice. Digestion remarkably good, and plenty of bile in the stool.

The last of February tumor began to point at the upper border of right lumbar, near the line of the umbilical region. March 1st, discharging slightly through two small sinuses about one and a half inches apart, which openings I enlarged with the lancet, after which discharged freely a fluid about the consistency and appearance of glycerine, which continued, producing great prostration; gave supporting remedies freely, in which iron predominated.

May 2d I was sent for again. Patient said to be suffering a great deal of pain; discharges from the abscess ceased, bulging between the openings —thought it must be lanced again. I was not at home at the time, and did not call until next day, when I found four gallstones had been discharged through the inferior opening, the first one being as large as a bird's egg, irregular in shape, weighing grs. xvij; the others about one-half the size and pyramidal in shape, with smooth, bright surfaces. Upon manipulation, four others were discharged while I was there. Others were discharged from day to day, until one hundred and six had come away, weighing, in the aggregate, two hundred and fifty grains. Most all of them pyramidal in shape, with smooth, bright surfaces. What seemed strange to me, there was no appearance of bile in the discharges from the abscess until May 20th, and then for a few hours only, and twice since that time, and at each there was more pain and gastric disturbance.

The upper opening has entirely closed, and the discharge from the other gradually growing less. The patient is rapidly improving, with every prospect of complete recovery; was at my house to-day (June 25, 1868) visiting, having rode two miles in a spring-wagon.

Miscellaneous and Scientific Notes.

To our Readers.—After eighteen months' appearance in an antique dress, we come back to modern type, a change which we believe will be satisfactory to at least the majority of our readers. The use of smaller sizes of type for all matter of the Journal, except the "Original Communications," gives us a very large additional space, and affords us an opportunity for incorporating into the Journal material that, under the old management, we were compelled constantly to throw out. The amount of reading matter thus gained is equivalent to about thirty pages of the old Journal. In addition to this, we have enlarged the Journal, by adding another "form," an improvement which we think will be received with favor.

With this increase in the capacity of the Journal, we shall be enabled to give it a more varied and practical character, and for this purpose we respectfully solicit from our subscribers reports of cases, items of medical news, and short practical papers. Prompt acknowledgment of the same will be made.

On behalf of our publishers, we ask a little forbearance in the adjustment of the perplexing entanglement of accounts which has resulted from the former business management of the Journal. When once these accounts are made correct, we pledge ourselves that our subscribers will have no further cause of complaint, and, by way of enabling the publishers to adjust their subscriptions, we respectfully request attention to the following points:

Should there be any error in the bill for subscription, please return it, with a copy of your last receipt from the former publishers, and the proper corrections will be made. Inform us promptly of any change in address, or failure to receive the Journal in time.

Strength of Carbolic Acid Solutions.—In view of the fact that carbolic acid is now largely in use in medicine, with a probability that its range of application will be increased, it is well for prescribers to be very careful of the particular preparation they employ. Instances are reported where much damage has been done by the external application of this substance in solution, the prescriber not knowing the exact strength of the solution, and we ourselves have seen carbolic acid ordered from the apothecaries, in such a way as to evince plainly the fact of a most blissful ignorance of whether the medicine was a solid or a fluid, or in what proportions it was proper to use. Dr. W. T. Channing, of Providence, reports to the Boston

Journal of Chemistry several cases of serious results, from the use of the concentrated fluid acid, which is dispensed by some under the name of "solution carbolic acid," when the prescribers intended only a milder solution, which they had been in the habit of using, but had obtained it from other druggists. Until, therefore, some distinctive nomenclature shall be given to the various preparations of this substance, and some officinal "solution" shall be decided upon, physicians cannot be too careful in learning the strength of the solution employed, and it would be advisable to give explicit directions where to procure it.

The Early History of Syphilis in China.—Dr. Geo. Thin, of Shanghai, China, contributes to the *Edinburgh Medical Journal* some interesting historical notes on this subject. He was assured by many Chinese scholars that syphilis has been known to exist in China for many centuries, and he therefore undertook, with the assistance of a learned native antiquary, to hunt up the records. He finds that in the seventh century the venereal chancre was described under a specific name, which places its nature beyond a doubt, and that from this time onward there are various allusions to it, although in modern times the more ancient notices have been in a great measure overlooked, partly from change of nomenclature and partly from the fact that the works in which the notices occur are not likely to come before the general practitioner. Even anterior to the Christian era, there are many traditions and vague references, which are generally accepted as indicating syphilitic diseases. The earliest of these is to be found in a collection of odes made by Confucius five hundred years B. C.

Instructive but Tardy.—A correspondent of the *Medical Record* writes from Paris an account of an insane woman, who swallowed a silver fork, with the view of committing suicide. The fork was subsequently discharged through an abscess in the abdominal walls, and the patient recovered.

Eighteen months ago we published a translation from one of the French medical journals, giving a full account of this

case. This report was copied from our pages by some of the very journals that are now reprinting it from the *Record*, for the instruction of their readers.

A Physiological Curiosity.—*Lactation in a Lamb, one hundred and fifty days old, with absence of the Internal Organs of Generation.*

We are indebted to the kindness of Prof. Austin Flint, Jr., for the report of this curious case. The report is extracted from a communication to him by Mr. Wm. H. Seward, student of medicine, Walnut Grove, N. Y.:

In June last my attention was called to a young lamb upon my father's farm, apparently a perfect female, which presented a full flow of what appeared to be normal milk. This animal was about one hundred and fifty days old, and, in view of the extraordinary development of the lacteal function, I made, assisted by my preceptor, a careful examination after death.

The animal was bled to death in the usual way. Although the external parts were perfect, the internal organs of generation were entirely absent. A probe was readily passed through the meatus urinarius into the bladder, but the vagina, lined by a smooth mucous membrane, terminated in a cul-de-sac. Thinking that perhaps the opening might have become occluded from some injury, I looked carefully for a cicatrix, but none could be found, and on further and most careful dissection we failed to discover either uterus, ovaries, or Fallopian tubes.

Medical Colleges of New York and Philadelphia.—Under this caption the *Medical Repertory*, of Cincinnati, puts forth the following withering proclamation, to which we give, without charge and without alteration or addition, the full benefit of our circulation. We felt very bad after reading this valuation of our merits, and have not yet quite recovered from the overwhelming sense of shame and confusion that came upon us on learning how low down in the educational scale we really were. But even in the midst of the deep humiliation in which we are plunged, we confess to a dim degree of faith that we shall yet survive this crushing extinguisher, and we enter a feeble and penitent appeal that we may be allowed to

pursue our humble career unmolested by any further unpleasant reminders of our unworthiness.

We are not yet informed that the schools of this city have determined to close their doors, but, with a rashness that must now seem suicidal to our Western confrères (we mean those of the *Medical Repertory*), lectures will be resumed in a few days, even at the risk of empty benches and still emptier exchequer.

Furthermore, we beg leave mildly to intimate (for ourselves only), that we purpose to keep pulling away at that transatlantic udder (that is more euphonious and refined than "sucking at the European teat") which, though it may not be calculated to make us "fountains of instruction," occasionally furnishes us with a morsel of mental pabulum for which we are thoroughly grateful:

Although we have well-organized medical schools in the West yet many of our Western physicians have a penchant to patronize the schools of New York and Philadelphia, causing their students to incur double and sometimes even thrible the expenditures necessary for completing their medical educations. That men wanting in sense—for the world is still not without fools—who place a higher estimate upon presumed *eclat* in the qualifications of the physician than they do upon substantial knowledge, should be guilty of such folly we are not surprised, but it is a matter of astonishment when we see individuals from whom we would have expected better things, doing likewise. Every correct observer knows that the schools of the West, are in nearly every particular, superior to the schools of the localities mentioned. The West affords better teachers, men who are more disposed to observe and think for themselves and therefore impart their instruction in a manner as only one can who knows of what he speaks, besides, their knowledge is better suited to the wants of the Western physician. The physicians of our sister cities read and write much, but think little. Few of them ever carry on any original investigations or make any discoveries. They flood the country with books, but they are mere compilations from European works, oftentimes with very much that is valuable omitted. Their instruction is precisely of a kind we would expect of men whose knowledge is second handed—dressed up and showy in appearance, but not calculated to bear the wear of service; and just like its counterpart in merchandise, there is charged for it very much more than it is worth. We do not desire to

disparage our Eastern brethren, but, always sucking at the European teat, they are not calculated to be fountains of instruction.

Young men educated in any of the regular medical schools of Cincinnati, Chicago, or St. Louis are far better prepared to combat with disease as met with in the West than graduates of Eastern colleges. The people of the West are a hardier people than those of some portions of the East, both physically and intellectually, and in morals very far superior—*Romæ omnia venum ire* is more true of the latter than of the former. Such being the fact, we would expect, as we find it, that diseases would be of a different type requiring different treatment. Men whose whole experience has been in the diseases of a particular portion of country, and who are well acquainted with the character of the inhabitants both physically and mentally, are certainly, all other things being equal, the most competent to give instructions to those who propose to enter upon the practice of medicine in such localities.

Physicians trained up in schools where but very little positive knowledge is imparted, where very much of the information is of the Jack Bunsby kind (our knowledge of Dickens is obtained from Dr. Blackman)—"Mayhap, do I say so? which? whereby? why not? can any man say otherwise? The bearings of this observation lie in the application of it?"—are but illy prepared for the discharge of their duties. In many instances where there seems to be a germ of truth in it, it is not calculated to propagate in the soil of the West—"first principles" have to be gone over again and learned anew.

A little reflection in the matter, with a disposition to act for the best, we think, would stop the immense stream of medical students eastward bound that takes place every fall, and direct it to our Western schools, where it properly belongs. Under such circumstances the schools of the West, by their largely increased patronage, would be stimulated to renewed efforts in usefulness, and be enabled to confer greater benefits upon those in attendance upon their lectures. The fees for instruction could be lessened, and yet sufficient remuneration be paid to those engaged in teaching that the chairs might be filled by the best talent. Cincinnati, Chicago, and St. Louis afford all the advantages for medical instruction that any medical student could desire. We know from our own experience that, in the matter of clinics, Cincinnati furnishes a very much larger amount than can possibly be made use of by a student in attendance upon medical lectures; while the opportunities it supplies for the prosecution of the study of practical anatomy are unsurpassed anywhere. Now that the new Cincin-

nati Hospital will be completed in a month or two, which will be unexcelled in size, convenience and elegance, by any hospital in the world, it would certainly be the height of folly for any one to pass this city by to seek for advantages in the study of medicine in New York and Philadelphia, which can be had in the greatest superabundance here.

The Trustees of the Fiske Fund, at the annual meeting of the Rhode Island Medical Society, held in Providence, June 10, 1868, gave notice that no awards had been made on the questions proposed by them the past year.

They offer the following subjects for 1868:

1. Bromides, their physiological effects and therapeutical uses.
2. Cerebro-Spinal Meningitis, pathology and treatment.
3. "Grave's disease" (so called), pathology and treatment.
4. Carbolic Acid, its therapeutical effects and hygienic uses.

For the best dissertation on each of these subjects they offer a premium of one hundred dollars.

Every competitor for a premium is expected to conform to the following regulations, viz.:

To forward to the secretary of the Fiske Fund Trustees, on or before the first day of May, 1869, free of all expense, a copy of his dissertation, with a motto written thereupon, and also accompanying a sealed packet, having the same motto inscribed upon the outside, and his name and place of residence within.

Previously to receiving the premium awarded, the author of the successful dissertation must transfer to the trustees all his right, title, and interest in and to the same, for the use, benefit, and behoof of the Fiske Fund.

Letters accompanying the unsuccessful dissertations will be destroyed by the trustees, unopened, and the dissertations may be procured by their respective authors, if application be made therefor within three months.

Address,

S. Aug. Arnold, M. D., Providence,
Secretary of Fiske Fund Trustees

At the annual meeting of the Committee on the Boylston Medical Prize Questions, on the first Wednesday in June, 1868, it was announced that no dissertation had been presented on either of the questions proposed.

The following questions are proposed for 1869:

1. Food in Disease, acute and chronic; its variety, advantages, dangers, and relation to appetite.

2. The Surgical Treatment of Hæmorrhoids, and the Surgical Treatment of Fistula in Ano, with its result.

The author of the best dissertation on either of the subjects proposed for 1869 will be entitled to a premium of one hundred and fifty dollars.

Dissertations on these subjects must be transmitted, postpaid, to John Jeffries, M. D., on or before the first Wednesday in April, 1869.

The following are the questions proposed for 1870:

1. The Modern Pathology of Tumors.

2. Aphasia, or the Relation of the Brain to Speech.

Dissertations on these subjects must be transmitted as above, on or before the first Wednesday in April, 1870.

The author of the best dissertation considered worthy of a prize, on either of the subjects proposed for 1870, will be entitled to a premium of two hundred dollars.

Each dissertation must be accompanied by a sealed packet, on which shall be written some device or sentence, and within which shall be enclosed the author's name and residence. The same device or sentence is to be written on the dissertation to which the packet is attached.

The writer of each dissertation is expected to transmit his communication to the President, John Jeffries, M. D., in a legible handwriting, within the time specified.

Parasite of the Cheek.—Dr. G. H. Vance, of Oneida, Ill., reports to the *Philadelphia Med. and Surg. Reporter* the following curious case:

"In the month of February, 1867, a boy five years of age, and of a scrofulous diathesis, came into my office, accompanied by his mother, who wished me to examine the little fellow's left cheek. There was quite a protuberance manifested, in character approaching that of a good-sized boil, which was attended with redness, and at times a considerable pain, causing irritableness of temper, sleeplessness, and anorexia. Thinking it arose from an impoverished state of the system, I prescribed an alterative cathartic, with a local application of ung. hydg. nit. But at the expiration of a week, contrary to my diagnosis, and much to my astonishment, upon slight pressure, a curious object was developed, which, upon examination, proved to be alive, *three-fourths of an inch in length*, nearly transparent, and surrounded with several rings or joints. Upon removal

of the object, the tumefaction subsided, and the cheek soon returned to its natural appearance."

The doctor naively adds, that he had no microscope, and therefore his investigations were necessarily very limited and unsatisfactory, and he begs to be informed what it was, "a species of *guinea-worm*, *trichina spiralis*, *acarus scabei*, *or what*."

The italics are our own. We should say "what," most decidedly.

A Remarkable Monstrosity.—Profs. Joseph Jones and Paul T. Eve, of the University of Nashville, have made a report (in the *Richmond and Louisville Medical Journal*), on an infant whom they were called upon to examine, and who was the subject of a very extraordinary malformation. The infant, J. Myrtle Corban, has four legs and two distinct external female organs of generation, with two external openings of the urethra and two external openings of the double rectum. The external genito-urinary organs are as distinct as if they belonged to two separate human beings. The fæces and urine are passed (most generally simultaneously, particularly the urine) from both external urinary and intestinal openings, situated respectively between the left and right pairs of legs.

The head and trunk are those of a living, well-developed, healthy, active infant of about five weeks, whilst the lower portion of the body is divided into the members of two distinct individuals, near the junction of the spinal column with the *os sacrum*. As far as our examination could be prosecuted in the living child, we are led to the belief that the lower portion of the spinal column is divided or cleft and that there are *two pelvic arches supporting the four limbs*, which are situated upon the same plane.

The mother recollects no fright or disturbance during her last pregnancy. The presentation was fortunately the head, which accounts for the preservation of the life of the child. It would be curious to speculate on the trouble which might have been produced had the feet or breech presented, while the result, in all probability, would have proved fatal to the infant, and possibly to the mother. Mrs. Corban says that there was nothing peculiar in the labor or delivery. When three weeks old the child weighed ten pounds. It now nurses healthily, is thriving well, and we saw it urinate simultaneously, between the *two pairs of labia of the two vaginæ*, situated about six

inches apart. From the crown of the head to the *umbilicus* the child measures twelve inches, and from this point to the toes of the right and left external feet, eleven inches. From the *umbilicus* up, all is natural and well formed; all below this, extraordinary and unnatural. An inch below the navel is a mark of an apparent failure for a second one. There are four distinct, pretty-well developed lower extremities. They exist in pairs on both sides of the median line which resembles the cleft of an ordinary pair of legs; but here there are no marks whatever of *anus* or *genital* organs, and upon pressure we discover no *os coccygis* or *sacrum*. The outer legs of both sides are the most natural of the four (though the foot of the right one is clubbed), but are widely separated by the two supernumerary ones, which are less developed, except at their junction with the body, from which they taper to the feet and toes more diminutive and which are turned inward. One toe is bifid on the left extra inward extremity. At birth these extra legs were folded flat upon the abdomen. We are led to believe that there are *two uteri as well as two recti;* in fact that the pelvic organs are double. Of course, a minute dissection would alone expose the true condition of these parts.

Should this infant reach maturity and the internal generative organs be double, there is nothing to prevent conception on both sides. The first difficulty will, however, be in her walking. The outer, or external legs, may be used for progression; the inner or inturned ones, probably never. These might be successfully amputated at the knee, or higher up.

One of us recollects of being in London in January, 1830, at an exhibition of the Siamese Twins, when Sir Astley Cooper gave an opinion adverse to an operation with a view to separate them, but which has always appeared to us feasible and without much risk of *peritonitis;* an operation, too, which should undoubtedly be performed in case of the death of one them, for no medical man believes in the vulgar impression that they must die simultaneously. In the present case all surgical interference is, of course, out of the question, except that alluded to—removal of the extra legs.

Cases somewhat similar to the above have occurred and been described. Rokitansky refers to two completely distinct bodies conjoined at their *ossa sacra* or *coccyges*, as in the well-known Hungarian sisters, Helena and Judith, born in 1701, who survived their twenty-second year.

Geoffrey St. Hilaire alludes to cases of a trunk with two heads, some even Janus-like, having four upper and four lower extremities.

The case, however, recalled most vividly by Josephine

Myrtle, is that of Rita-Christina, well known in Europe, and accurately described in this country years ago by Prof. Meigs. In this wonderful instance, there were two heads, two necks, four arms, but only two legs; and was thus the reverse of our case. From the *umbilicus* down, there was one well-formed child, but above this all the organs were double; in reality there existed two beings. The *rectum* and bladder were common to both, but all else in the trunk was double and distinct. One would sleep while the other played, etc., for they had *two spinal marrows*, *two brains*, *two hearts*, but the last two occupied a common *pericardium*. Unfortunately, after surviving a little over a year, one sickened and died, when the other, then in health, instantly expired.

Rita and Christina were born in Sardinia, 1829, and described by Dr. De Michaelis, Professor of Surgery in the Royal University of Sassari, and lived eighteen months.

The late Prof. J. C. Warren, of Boston, first described the Siamese Twins, when purchased of their mother by Captain Coffin and Mr. Hunter (joint owners), and brought to that city, in 1829.

Monstrosity by Defect.—While at the County Hospital some weeks since, we were shown by the resident physician, Dr. Garwood, a singular monster, to which one of the patients gave birth. The lower extremities, the pelvis, and all that portion of the body below the plane of the umbilicus, were perfectly developed and normal; but these parts constituted the child, if child it could be called. There was nothing more —all above the umbilicus was wanting. Evidently the embryo from its earliest conception had consisted of nothing more than the parts mentioned. The integument was intact and perfect, and closed over what existed of the abdominal cavity, in such a manner as showed that there had never been any thing like amputation *in utero*. Not a trace of a scar was perceptible. At the same time with this monstrosity was born a child with imperforate anus and faulty development of one of its ears. Another most singular fact is, that of the twelve children which the mother has had, not one has been perfectly developed. We regret that circumstances have prevented our obtaining a more satisfactory history of this most interesting case.—*Pacific Med. and Surg. Journal.*

Death from Chloroform.—Dr. Billroth, of Vienna, relates, in the *Wiener Medizin. Wochenschr.* of June 6, 1868, a case of death from chloroform which occurred in his practice two days previously. The patient, a man aged 26, was

admitted into hospital in consequence of having, on the preceding Monday, cut the palm of his left hand with a piece of broken porcelain. There had been much hæmorrhage, and the man was very anæmic; and, on the removal of the coagula and charpie, arterial hæmorrhage occurred. In extending the fingers, in order to tie the bleeding vessels, so much pain was produced that chloroform was given. In about five minutes from the commencement of anæsthesia, there were convulsive twitchings of the whole body. The chloroform was discontinued for a moment, and then resumed; and, when perfect muscular quiet was obtained and the examination of the fingers was recommenced, it was found that the hæmorrhage had ceased, although the compression on the radial and ulnar arteries had been removed. The patient, who was in a semi-recumbent position, with his head bent back, was now seen to be pale, with livid lips and weak respiration. Dr. Billroth opened the mouth, and drew forward the tongue. The pulse was now all but imperceptible; but the respiratory efforts were distinct, though irregular and weak. Tracheotomy was performed, and artificial respiration set up. In the course of ten minutes, the patient made three hurried respiratory efforts; but there were no signs of restoration of the breathing or circulation. After the artificial respiration had been kept up half an hour, the case was abandoned as hopeless. Dr. Billroth attributes the death to very violent spastic contraction of the heart in a subject weakened by loss of blood.—*Brit. Med. Jour.*, June 13, 1868.

Unrecorded Deaths from Chloroform.—It is our duty to record—and we do so with regret—that two unpublished deaths have occurred quite recently in London from the administration of chloroform—one, while it was being administered, for the purpose of extracting teeth, to a lady of rank in apparently good health, and who had taken it with impunity on a previous occasion. In the other case, it was administered for a secondary operation on the eye. In neither instance has an inquest been held, as the coroners were in each case satisfied that chloroform had been administered properly and with due care and judgment, and that the deaths arose from pure misadventure. We hear also this week of an earlier unrecorded death from chloroform at Dublin, prior to the performance of an operation on the eye. It is, we think, due to science and to humanity, that these deaths should be recorded in some form. The blessings and advantages of anæsthesia under the knife are so great, that it is not likely that even the fullest knowledge of the risks attendant upon the

use of anæsthetics will influence persons who have to undergo serious operations to reject the opportunity of sleeping through the ordeal. But it is right that the full risks should be known both to the whole profession and to the public; and it is especially important that the particular mortality of each anæsthetic agent should be accurately ascertained.

We mentioned last week three recent cases in which the administration of chloroform had caused sudden death in this metropolis, under circumstances in which no blame could attach to the administrators, as it was given with all possible skill and precautions, and by able and experienced persons, and in which, therefore, the coroners did not think it necessary to hold inquests, nor had any public record been made. We regret to learn that subsequently to this another death under chloroform has occurred in a metropolitan hospital. The circumstances absolve the administrators from any shadow of blame; and no inquest has been held. We are of opinion, however, that it is the duty of all those to whom deaths from chloroform occur in their practice to take care that they are put upon record, with such details as shall render them instructive. The age, constitutional condition, and peculiarities of the patient; the nature of the operation; the specific gravity and chemical qualities of the chloroform; the apparatus used and mode of administration; the symptoms of the patient; and the means employed toward resuscitation—should all be described. We can well sympathize with the pain which those feel in whose practice these untoward, and at present unavoidable, accidents occur; and the publication of names may even cause unjust prejudice and injury to individuals and institutions. It is possible, however, to put the case satisfactorily upon record without such detail; and it seems to us to be a duty to science and to society, which is much neglected. In conversation this week, we hear of two other authentic but unrecorded cases, one in private practice in London, and another in Birmingham.—*Brit. Med. Jour.*, June 27, 1868.

Death from Chloroform administered for the Amputation of a Finger.—A death from the inhalation of chloroform has occurred at the Essex Lunatic Asylum, Warley. An inquest was held before C. C. Lewis, Esq., coroner. The patient was a man, aged 28, whose finger had to be amputated from recent injury. The quantity administered was, it was stated, only a drachm; and the verdict of the jury, after hearing the medical evidence, was—"That death resulted from convulsions; such convulsions resulting from fatty degeneracy of

the heart." The stethoscope had disclosed no signs of this disease. The chloroform was administered on a handkerchief in three doses of twenty minims, and maintained during the operation. The patient became violently convulsed, and the pulse ceased. There was tubercle at the base of the brain; the heart was soft and fatty, the liver heavy and soft.—*Brit. Med. Jour.*, July 25, 1868.

Death from Chloroform.—Mrs. Elizabeth Hammon, aged 35, mother of three children, apparently a healthy woman, went to a dentist (Dr. McDowell) April 16, 1868, for the purpose of having some teeth extracted. Thinking she could not endure the pain, she requested the doctor to administer chloroform; and, as he had given it to her once before (about six months since) without any bad effect, he consented to administer it again, and, pouring about two drachms of chloroform upon a sponge, held it a short distance from her face. After she had made three or four inspirations her respiration ceased; he felt for the pulse and found she had none. Artificial respiration was commenced at once, and stimulating applications applied over the heart and to the extremities, but to no effect. She made but two or three efforts at inspiration after he first noticed something was wrong. An autopsy could not be obtained.

A Needle six and a quarter Inches long remaining in the Heart and Thoracic Cavity thirteen Months.—M. Tillaux recently reported to the Imperial Society of Surgery the case of a man, aged fifty-five years, affected with general paralysis of the insane, who, having made a previous unsuccessful attempt at suicide, had forced a three-sided needle, six and a quarter inches long and about one-twelfth of an inch in diameter, into the wall of the thorax. When seen the next morning there was a very small puncture about one-fourth inch below the left nipple, and just outside and above this the skin was forcibly raised at each contraction of the heart, and the impulse of a rounded foreign body was plainly perceptible by the finger. There were emphysema and ecchymosis at the same point. The patient being then in his right mind said that he had forced in the needle the night before, pushing it with his tobacco-box, and had felt no pain after the resistance of the skin was overcome. Now his face was pale and anxious, his thighs were flexed on the pelvis, and his body bent forward so as to relax the thoracic and abdominal muscles; his respiration was difficult, and he complained of vague pain in the chest. Over the chest, and especially the emphysematous

portion, any pressure caused complaint, rather denoting anxiety than sharp pain. The respiration was feeble on the left side, on account of the pain of movement. The sounds of the heart were regular and normal, and with the first sound the foreign body was pushed directly forward and outward. No effusion was discovered in the pericardium; except the emphysema, there was no appearance of injury of any important organ. M. Tillaux, through fear of exciting hæmorrhage, and in the absence of immediate danger, waited till the next day.

Meanwhile the patient had had several attacks of severe pain with threatened syncope, and demanded immediate extraction of the foreign body. This had, however, worked its way farther into the tissues, and could scarcely be felt by the finger, and as at the first incision of the skin the patient had an attack of syncope, the attempt at extraction was abandoned.

Four days later all impulse of the foreign body had disappeared; there was some fever, pneumonic sputa, bronchial râles, but no dulness; the sphygmograph showed a regular, but feeble and depressed pulse; respiration was very irregular.

A month later, palpitation and great irregularity of the heart's impulse. This general state of things continued with occasional slight hæmoptysis, though he gained strength and was able to walk about, till eleven months after the injury, at which time there was a cardiac souffle with the first sound, loudest at the base. All localized pain had disappeared, though there were vague and erratic pains in the back and sides. During the next two months the pains increased, the cough and hæmoptysis became more frequent, and finally death ensued from syncope.

At the autopsy the needle was found traversing the whole length of the posterior wall of the left ventricle, but not entering its cavity, passing between the spinal column and the œsophagus and through the lower lobe of the right lung. The tissue of the heart showed no trace of inflammation, but in the lung the needle was surrounded by a membrane which separated it from the hepatized lung tissue.—*L' Union Médicale*, and *Boston Medical and Surgical Journal.*

There are no longer alchemists who believe in the transmutation of metals, the base into the precious; but those still exist who hold it possible to make diamonds from worthless materials. One of this class has proposed a solution of the famous problem to the French Academy, and the august body has smiled *upon*, not *at* the project. M. Caliste Saix is

the happy discoverer who has found the way to cheat Nature out of her riches, and with praiseworthy disinterestedness he has given his method freely to the world. The process consists in passing a stream of chlorine gas over fused cast iron, a perchloride of the metal is formed and volatilizes, leaving the carbon present in the mineral intact. According to the author, the carbon must, under the circumstances, crystallize and appear as diamonds. Sixty grammes' weight of jewels are to come from one kilogramme of iron; the raw material costing 20 francs and the precious product realizing 75,000 francs. The process is creating some stir in France, but we cannot hear that any diamonds have actually been made by it.—*Gentlemen's Magazine.*

How Quacks were treated in London in the Fourteenth Century.—The Corporation of London have published a very interesting volume, which is edited by Mr. Riley, entitled "Memorials of London and London Life in the Thirteenth, Fourteenth, and Fifteenth Centuries." These memorials consist mainly of a series of extracts from the archives of the City of London. The condition of our profession at the time is illustrated by a number of interesting facts. However much we may have improved in many respects, it is certain that our forefathers had a keen sense of their duty toward unqualified persons who assumed to be possessed of medical knowledge. They were punished with a rigor which would be incompatible with modern customs, but which affords a striking contrast to the lenient way in which medical impostors are now treated. The following instance, in particular, is worthy of mention: One Roger Clerk professed to be learned in the art of medicine, and prescribed, for a woman suffering from fever, the hanging of a certain document round her neck, containing certain words which he stated were an antidote to the disease under which she suffered. The charm did not work. He was summoned before the mayor and aldermen in the Guildhall of London, at the instance of the husband of the patient, to show upon what authority he practised the art of medicine. His own statement was sufficient to convict him of being a rogue and an impostor, and he was forthwith ordered to be placed in the pillory, and therein to be punished for the offence he had committed against society. His progress to the pillory is thus graphically described: "It was adjudged that the same Roger Clerk should be led through the middle of the city, with trumpets and pipes, he riding on a horse without a saddle; the said parchment and a whetstone, for his lies, being hung about his neck, an urinal also being hung before him and another urinal on his back."

The offence which Roger Clerk committed was venial compared with some of the flagrant crimes which quacks nowadays too frequently perpetrate. If he was righteously punished, how should we mete out punishment to the harpies and villains of our time who prey upon the weakness and credulity of the miserable victims who are attracted by their infamous advertisements to place themselves under their care?—*Lancet*, July 11, 1868.

The Velocity of Nerve-Force.—The nerve-current which transmits sensations to the brain, and the orders of the will to the extremities of the body, requires a certain time to travel in. Impressions coming from without are not perceived at the instant they are produced, they travel along the nerves at the rate of 20 to 30 mètres (25 to 40 yards) in a second, which is the same speed as that of the carrier-pigeon, of a hurricane, or of a locomotive engine at its quickest, but very much less than that of a cannon-ball. For instance, we can only be conscious of an injury to one of our feet about one twentieth of a second after it has actually occurred, and the commands of the will proceed equally slowly from the centre to the peripheries of the nervous system. In the human body the time thus occupied is unimportant, but let us take the case of a whale, where the telegraphic network of the nervous system is far more extensive. A boat attacks the whale, and a harpoon is driven into its tail. The impression thus produced has to travel over some forty yards before reaching the headquarters of the will; a second is thus lost. How long a time is then required for reflection? That must depend upon circumstances; but at any rate it is certain that the will has need of some definite amount of time for its decision. The order to capsize the boat is dispatched to the tail, but another second must elapse before the telegram reaches its destination, and in the time thus employed the whaling-boat has pulled off and escaped the danger.

Several methods have been devised by physiologists for measuring the velocity with which nerve-force travels. Thus, a physician of the middle ages,[1] mentioned by Haller, fancied that this might be calculated by comparing the supposed diameter of the nerve-tubes with that of the aorta, as he supposed the velocities of the blood and "animal spirits" to be in the inverse ratio of the vessels containing them, from which data

[1] The mediæval physicians and the schoolmen held as a consequence of Aristotle's and Galen's theory of "animal spirits," that time was required for their passage from one part of the nervous system to another; and it is even curious to remark how the later schoolmen opposed the Cartesians who thought the contrary—an example of the advantages derived by the schoolmen from including even bad physiology in their scheme of philosophy.—J. R. G.

he calculated that nervous influence travels 600 times more quickly than light.

Haller's own mode of procedure was scarcely more rational. He counted the greatest number of letters he could articulate in a given time, which he found to be 1,500 per minute. Now, the letter *r* requires, according to him, ten successive contractions of the muscle which makes the tongue vibrate, whence he concluded that this muscle can contract and relax 15,000 times, that is, can move 30,000 times in one minute. From the brain to the muscle the distance is one décimètre; if, therefore, the nerve-force passes over that space 30,000 times in a minute, it must travel at the rate of three kilomètres per minute, or fifty mètres per second. We need not point out that this process is a mere series of mistakes, but it is strange that the result should happen to be so near the truth.

No attempt was made until 1850 to study this question in a satisfactory manner, when one of the most distinguished of modern observers, M. Helmholtz, undertook its investigation. He at first employed Pouillet's "chronoscope," a machine in which a galvanic current of very short duration makes a magnetic needle deviate, the duration of the current being measured by the amount of deviation; by this means as short a time as some thousandths of a second can be measured. M. Helmholtz fixes one end of a muscle from the leg of a frog, and attaches the other to a small lever which forms part of a galvanic circuit, so that at the moment of contraction the circuit is broken and the time registered by the chronoscope. The current is first sent directly through the muscle, and then through a given length of nerve which has been left adherent; the difference in time between the two cases gives the velocity of the nerve-force, which by this process is found to be 26 mètres (85 feet 7 inches) in a second.

In a second method, also employed by M. Helmholtz, the lever raised by the contraction of the muscle has a point which traces a line upon a sheet of blackened paper, which is kept moving from the moment of excitation, and the curve produced by the movement of the lever registers all the phenomena of the muscular contraction. This apparatus, called the "myograph," gives the velocity of nerve-force as equal to 27 mètres (88 feet 10 inches) per second; several modifications of the instrument by different physiologists have given very closely agreeing results, and have also shown that the velocity is diminished by sending an electric current through the nerve, or by a low temperature.

Experiments with the same object have been made upon man in the following manner: An electric current is suddenly applied to the skin, the moment of application being regis

tered by the turning cylinder of a chronoscope, and as soon as the person experimented on perceives the slight prick produced by the current he touches an electric lever by which a second mark is made upon the cylinder. The interval between the two, which can be thus measured, is made up of the following elements, viz., transmission of the impression to the brain, the mental process there gone through, the transmission of the voluntary impulse to the fingers, and the consequent muscular contraction. But if this experiment be performed on two different parts of the body, as, for instance, at the groin and at the great toe, all the other elements of the delay will remain the same except the time occupied by the transmission of the impression upward, and the velocity of nerve-force in man can be thence calculated. M. Hirsch, the director of the Neufchâtel Observatory, was the first person to make these experiments, in 1861, and from them he concluded that nerve-force in man passes over 34 mètres (112 feet) in a second. Dr. Schelske has repeated the same experiments, and deduces from them a slightly less velocity, 29½ mètres (97 feet) per second. By similar means it has been shown that the rate of transmission through the spinal cord is the same as through the nerve-trunks, and that a reflex action requires from one-tenth to one-thirtieth of a second more than the mere direct conduction of excitement to the muscles.

The time required for certain cerebral operations has been measured by Dr. De Jaager in the following manner: It was preconcerted that the person on whom the experiment was made should touch the lever with his right hand when he received an electric shock on the right side, and with the left hand when he received a shock on that side. The interval between the shock and the signal was found to be 0.20 of a second when the subject of the experiment had been told beforehand on which side the shock would be given, and 0.27 of a second when he had not been told; 0.07 had therefore been employed in reflection.

M. Hirsch, again, has found that on an average two-tenths of a second must elapse before an observer can mark by a signal his perception of a sudden noise or flash of light, and MM. Donders and De Jaager have varied their experiments thus—one of them pronounced a syllable, the other repeated it as soon as heard; when the syllable had been agreed upon beforehand, there was an average delay of two-tenths of a second; when it had not been so agreed upon, of three-tenths of a second. These are, however, only average results, and subject to considerable individual variations, of which the "personal equation" of different observers of a transit is an example well known to astronomers.—*Journal af Mental Science.*

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ART. I.—*On Sea-sickness.* By FORDYCE BARKER, M. D., Professor of Midwifery and Diseases of Women, in the Bellevue Hospital Medical College, etc., etc.

IF we consider the number of persons who, either for pleasure or business purposes, cross the Atlantic, the many constantly going to and returning from California and other parts of the Pacific coast,—if we estimate the number on the steamers which run between our Northern and our Southern ports on the Atlantic coast, and on our Western lakes, I think the statement will readily be accepted, that there are few maladies which produce such an aggregate of human suffering as sea-sickness. And yet it may be said, that there is no malady which the medical profession has done so little to relieve, and none for which it is so seldom consulted. There are several popular errors in regard to this malady, which are current with the profession

as well as with the public, and which account, in some measure, for the statement just made. For example, I find the belief very general—

1. That sea-sickness is often beneficial, and that it is never permanently injurious. Now, I have never been able to convince myself that any one was really benefited by the sea-sickness, but the improvement in health from a sea-voyage is, as a general rule, proportioned to the freedom from sea-sickness. I have known many persons who have taken a sea-voyage for health, and, believing that the benefit they were to receive from the voyage would be in a ratio with the amount of bile vomited, they have been greatly disappointed because they were not sea-sick. In many instances I have known serious and permanent injury to result from sea-sickness. A gentleman in this city, whose business as an importer obliged him to visit Europe every spring and autumn, has been completely broken down from the effects of a ten days' sea-sickness four times a year. After the violent sickness and vomiting have ceased, because of the arrival on land, the stomach has remained greatly disordered, and a long time has been required for the restoration of its tone and digestive power. My advice has been frequently given in strong and decided terms, to those of depressed vital powers, with impaired and feeble digestion, whose past experience has demonstrated an innate liability to sea-sickness, to avoid the exposure to such a hazard.

Some twenty-five years since, I crossed the Atlantic in the packet-ship St. Nicholas. We were eighteen days from Sandy Hook to Havre, having two or three days of severe weather, but, on the whole, a very pleasant passage. One very corpulent gentleman, who was in perfect health when we left the Hook, was in-

cessantly sick during the whole passage. He was wholly unable to retain any thing except the smallest bit of sea-biscuit, soaked in wine or brandy; and I do not believe that, during the whole voyage, the aggregate of all he swallowed would amount to a half-dozen sea-biscuits. It may well be supposed that, on our arrival at Havre, he was extremely prostrated and excessively emaciated. I was called up to see him, the second night after landing, as he was delirious and had attempted to commit suicide. It was many days before he recovered the capacity for retaining and digesting food. While crossing the Atlantic in 1861, I was asked by the surgeon of the steamer to see a gentleman whom sea-sickness had reduced to a most dangerous state of prostration and exhaustion. This gentleman was obliged to remain in Liverpool two weeks before he sufficiently recovered from the effects of the sickness to be able to leave for London.

2. Another popular error is, that sea-sickness is never dangerous to life. It does not often result fatally, but I suspect that the number of deaths from this cause is greater than is generally supposed, for three have occurred within my personal knowledge. The first was a young physician, of decided talent and promise, who had at one time been a pupil of mine. Feeling himself somewhat run down by overwork, he thought to improve his health by taking a vacation of a few weeks in an excursion to the fishing-banks of Newfoundland. He was absent nearly four weeks, during which time he was absolutely unable to retain any thing on his stomach, and he died, delirious and exhausted from starvation, two days after landing. The second case was a young lady, who was to have been married immediately after her arrival here, but

who died on the passage between Havre and New York. Dr. A. H. Smith, the surgeon of the steamer, told me that he could find no other cause for death than the exhaustion from sea-sickness. The third case, which I saw in consultation with the late Dr. Pratt, of this city, was a young man twenty-two years of age, represented to have been perfectly healthy, who, in a rough passage of sixteen days from Liverpool, had been wholly unable to retain any thing on his stomach. This condition continued after landing, and he died on the fourth day after his arrival. Dr. Pratt, who had a very large hotel practice, told me at this time that he had known of three other deaths from this cause.

3. The belief is very general, both in and out of the profession, that the medical art is powerless for the mitigation, relief, or cure of this malady. It is true that there are no specific drugs which will cure or even prevent sea-sickness. It is often and truly said, that the land is the only cure; but I believe that every physician ought to be competent to give such good, sensible advice as will greatly contribute to diminish the tendency to this malady, and to mitigate and relieve the suffering and evil resulting from it. Having crossed the Atlantic many times, and, of course, having experienced all sorts of weather, from the most perfect calm to the most violent storm, not only on the Atlantic, but on the Mediterranean and the English and Irish Channels, I have had the opportunity of studying sea-sickness in all its different phases. Being myself exempt from the slightest tendency to this affliction, but, on the contrary, feeling at sea, particularly when it is somewhat rough, an exhilaration of mind and an elasticity and vigor of body which I do not feel on shore, I have been in the best possible condition for studying

the disease objectively. I am not sure that it can be called a disease, in the proper sense of the word, for the phenomena constituting sea-sickness are purely physical. It is not confined to the human race, as animals also are subject to the same malady. I have often noticed, with great interest, its effect on dogs, and that during rough weather they intuitively seek the centre of the ship. The same law of individual susceptibility and exemption from this affection exists in animals as in the human race. In returning from Newport, a few years ago, the sea was very rough in coming round Point Judith. My coachman came to me in great trouble, telling me that one of my horses was dying. I found the horse lying down, wet with a cold sweat, and groaning piteously. As he was perfectly well before leaving Newport, and lively enough after we landed here, it was evident that his whole suffering was due to sea-sickness. The other horse did not seem to be disturbed in the slightest degree.

I am unable to say to what extent the feathered race are susceptible to this malady, but I once had the opportunity of observing, on the Mediterranean, that fowls are not exempt "from the ills that flesh is heir to." One beautiful morning in June I was on a small steamer going from Leghorn to Genoa. There was a short, chopping sea, very trying for those of delicate susceptibilities, and I was the only passenger up and on deck. I observed on the forward-deck, near the bow, a coop, containing fifteen or twenty hens and two cocks, in watching which I became greatly interested and intensely amused. Most of the hens exhibited unmistakable evidence that they were neither comfortable nor happy. One of the cocks stood, balancing himself with difficulty, with head, tail, and wings drooping,

and winking with a decidedly sea-sick expression. While watching him with sympathy, his fellow gave an exultant crow, and immediately received from his mate a spiteful peck on his comb. A few moments after the crow was repeated, but this time his sick mate could only express his disgust and reproach by languid winks. I had the curiosity to watch the coop after it was landed, and found all the inmates restored to their normal liveliness.

Sea-sickness is manifested by a great diversity of symptoms in different individuals. Some suffer only from headache and a constant feeling of stricture across the forehead and over the temples, during the whole voyage, while they are free from nausea and vomiting. Others do not suffer much from nausea, but are suddenly seized with vomiting, and after the contents of the stomach are discharged they are free from all unpleasant sensations until the next recurrence of vomiting. With many the nausea and vomiting entirely disappear after being at sea for a few days. Others again are so unfortunate as to suffer from all these symptoms during the whole time they are at sea, whether the voyage be short or long. There are some who never can become habituated to the sea. I have been told by some naval officers, that they were always sick in rough weather, and I know that this is the case with two captains of Atlantic steamers. With some the suffering produced by sea-sickness can hardly be painted by words. I am sure that no personal inducement would be strong enough to tempt me to cross the Atlantic, if I were obliged to endure the terrible suffering that I have witnessed in most voyages that I have made. There is often a great change in the same individual, in the course of life, as to the susceptibility

to this malady. Some, who in early life have been martyrs to sea-sickness, have ceased to be so as they have grown older, while others, who have been so exempt from this liability that they have been accustomed to regard it as an affection which can be overcome by the exercise of a strong will, have themselves become most craven sufferers and pitiable victims. And yet it is curious that strong mental emotions, as apprehension, terror, fright, will suddenly and completely cure the most violent sea-sickness. It is often true, as is said in "Don Juan," that

> "fright cured the qualms
> Of all the luckless landsmen's sea-sick maws."

I do not purpose at this time to discuss the great variety of theories that have been suggested in explanation of the cause of sea-sickness. I will only observe that it seems to be due to the sudden and recurring changes of the relations of the fluids to the solids of the body, and the nervous disturbances which result from these changes. The liquids contained in their vessels, as well as the solids of the economy, obey equally the laws of gravitation, when the body is subjected to alternate movements of ascent and descent like those which are caused by the swing or by the waves of the sea. The blood, by its fluidity, yields more readily to the influence of descent, and less easily than the solids to the ascending impulse. Consequently, it does not return to the brain with the same regularity as in the case where the body remains stable, and leaves it more rapidly in the movement of descent. There result, as to the circulation, alternations of afflux and delay in the arrival of the blood to the different organs of the body, which disturb their functions, and those of the brain especially, analogous

to that which follows the loss of blood, in some persons who are nauseated and vomit after venesection. This disturbance of function is more or less pronounced according to the susceptibility of each individual, and in most persons it is overcome in a great measure by a habitude to a repetition of the causes. No sensible physician would therefore expect to cure sea-sickness by medication addressed to the stomach, or even by drugs which are supposed to act directly on the brain and its functions. The horizontal position, which, to a certain degree, modifies this disturbance of function, is the only approximation to a cure. It, however, does not follow that nothing can be done to prevent or at least diminish these functional disturbances, and to relieve or restore the system from their results.

To this end, it is of first importance that all the conditions which tend to increase the intensity and severity of the sickness should be thoroughly appreciated. I will mention some of the most common and prominent of these conditions :

1. An exhausted or depressed state of the nervous system, the consequence often of loss of sleep, want of food, excitement, or emotional causes, and bad air. I have observed that a large proportion of persons come on board the Atlantic steamers fatigued and exhausted by their preparations for the voyage. They have passed a large part of the previous night in superintending the packing of their trunks and in arranging their affairs for an absence from home, or they have been travelling night and day to reach the point of embarkation, or have badly slept in an ill-ventilated and strange room in a hotel. Then there is the excitement arising from separation from family and friends, and the vague, undefined apprehension as

to the perils of the sea. Of course, such persons have had no appetite, and have badly digested the little they have eaten.

2. Thus badly prepared to resist the effects of sea-sickness, they speedily bring it on by keeping up and staying on deck. They have an indistinct idea that it is a kind of moral weakness, which they may conquer by boldly doing the very things to bring it on. If they have had a previous experience of the malady, it is not strange that they should dread to be "cabined, cribbed, confined" in their badly-ventilated berths.

3. In many the digestive organs have been irritated by their previous habits of living, or by the action of cathartic medicines which have been taken as a supposed prophylactic against the sickness. I do not mean to say that those whose digestive functions are feeble and imperfect are more liable to sea-sickness than others; but, if the constitutional susceptibility to this affection exists, they are less capable of resisting its effects, they suffer more, and their recovery is much more tedious. Then, again, because bile-vomiting is the result, bile is supposed to be the cause of the sickness. "I expect to be sick, because I am bilious," or "I am never sick, except when I am bilious," are remarks which I often hear. But, in reality, the bile has as little to do with the sea-sickness as it has with the evil consequences to a child of a fall down-stairs. In both the bile-vomiting is not a cause, but a consequence, of the cerebral perturbation. And so I think it a great mistake to take cholagogue cathartics as a preparation for a voyage, as any thing that irritates or disturbs the functions of the system weakens the power of resistance to the constitutional susceptibility.

I will briefly allude to some other points in con-

nection with this subject. It is a very prevalent error that one never "takes cold" at sea. The fact is quite the reverse, and the exposure to the causes of cold is obviously greater at sea than on land. My own experience is, that it is very difficult to get rid of a cold when at sea.

I am often consulted in regard to the effects of a sea-voyage and sea-sickness on menstruation and pregnancy. In former times, when emigrants came over to this country in packet ships, having a voyage of from thirty to sixty days, amenorrhœa was a very frequent result, but I think that this was due rather to the bad hygienic conditions of the voyage, the bad air, and the poor and insufficient food of those crowded in the steerage, than to any special influence resulting from sea-air or sea-sickness. I am told that it is much less common in this class, now that they generally have much better accommodations and much shorter passages in the steamers. But I believe that the function of menstruation is generally more or less disturbed. From my investigations on this subject, I am led to regard the law to be, subject of course to numerous exceptions, as follows: When the voyage is commenced near an approaching period, it is brought on two or three days earlier, and the flow is more abundant, than ordinary. But when the voyage is commenced in the first half of the interval after a period, the next appearance is retarded and sometimes suppressed for one or two periods. I am often consulted, both directly and by letter, as to the safety of a voyage during pregnancy. Judging from what has been told me by patients, I should infer that French physicians generally, and many of the English, regard it as highly perilous. Now, the same causes at sea as on land will

produce miscarriage, but I have never known of a case of abortion from sea-sickness. I have, however, interrogated many surgeons of steamers in regard to this point, and find that occasionally abortion is induced in pregnant women from sea-sickness. I have known several who were always sea-sick when not pregnant, and who were entirely exempt from it in a rough passage across the Atlantic during pregnancy. My advice was sought for by a lady in Paris, who had four times suffered severely from the sickness of pregnancy up to the end of the fifth month, and who was excessively sea-sick during the whole voyage over to Europe. She was, at this time, in the third month of her fifth pregnancy, and very weak from constant nausea, vomiting, vertigo, and nervous irritability. Imperative family reasons required her return to this city, but her physicians had told her that it was out of the question for her to attempt it. But she returned in the same steamer with myself. On the second day out, all sickness disappeared, her appetite returned, and she arrived here in a very much better condition than when she left Paris.

I will now offer a few suggestions in regard to the prevention and management of sea-sickness, and the treatment of its results. In short passages, as on our lakes, and across the English or Irish Channel, all that can be done is by way of prevention. Those liable to be sick should make a good hearty meal not more than two or three hours before going on board. They should select a spot as near as possible to the centre of the vessel, and lie down before she gets under weigh. The horizontal position should be rigidly kept during the whole passage. Any attempt to raise the head or to stand erect will be sure, with the susceptible, to be

followed by an explosion, and then the case is hopeless for the remainder of the passage. The person should be well covered, not only to protect from cold, but to shield from disagreeable sounds, sights, and smells. On the packets on the English Channel I should advise one not to go down in the cabins below, where the sight of those lying round, with basins by their head, is of itself exceedingly provocative to a sensitive stomach, but rather to secure, by telegraphing beforehand, one of the little cabins on deck. Although the passage may not be more than an hour and a half or two hours, I have often seen the neglect of the above suggestions to be followed by very severe punishment.

For ocean passages one of the most essential points is the selection of the state-room as regards position, light, size, and ventilation. Of course, the nearer the room is to the centre of the ship, the less will be the motion. In going to Europe, it is better to be on the starboard side, and in returning, on the larboard, which will be the sunny side. Rooms near the furnaces are objectionable, not only on account of the heat, which is sometimes very disagreeable, but also from the noise, which, at certain hours, is made by the donkey engines in drawing up the ashes and cinders, and which is very trying to those of sensitive nerves. In screw-steamers, the inside rooms, as they are called, if of good size, are often to be preferred to the outside ones, on account of ventilation, as there is very little weather, except in remarkable summer passages, when the port-holes can be kept open, while the windows of the inside room open on deck, and can generally be kept open. As the air draws down the gangway, the nearer the gangway the better the ventilation.

The following suggestions for the prevention of sea-

sickness were first written out some years ago for a gentleman whose business required him to cross the Atlantic often, and who was always kept in his room by severe sea-sickness during the whole voyage. By implicitly following the directions given, he has suffered very little from sickness, and has been able to go on deck by the second or third day, and has been entirely exempt from sickness for the remainder of the voyage. They have since been copied many times, and their value thoroughly tested. The trouble, however, is, that most persons do not appreciate how much easier it is to prevent sea-sickness than to cure it; and so, none but those who have before suffered, will thoroughly carry out the directions, and, neglecting some of them, are disappointed in the results:

1. Have every preparation made at least twenty-four hours before starting, so that the system may not be exhausted by overwork and want of sleep. This direction is particularly important for ladies.

2. Eat as hearty a meal as possible before going on board.

3. Go on board sufficiently early to arrange such things as may be wanted for the first day or two, so that they may be easy of access; then undress and go to bed, before the vessel gets under weigh. The neglect of this rule, by those who are liable to sea-sickness, is sure to be regretted.

4. Eat regularly and heartily, but without raising the head for at least one or two days. In this way, the habit of digestion is kept up, the strength is preserved, while the system becomes accustomed to the constant change of equilibrium.

5. On the first night out, take some mild laxative pills, as, for example, two or three of the compound rhubarb pills.

Most persons have a tendency to become constipated at sea, although diarrhœa occurs in a certain percentage. Constipation not only results from sea-sickness, but in turn aggravates it. The reason has already been given why cathartics should not be taken before starting. The effervescing laxatives, like the Seidlitz, or the solution of the citrate of magnesia, taken in the morning on an empty stomach, are bad in sea-sickness.

6. After having become so far habituated to the sea as to be able to take your meals at the table and to go on deck, never think of rising in the morning until you have eaten something, as a plate of oatmeal porridge, or a cup of coffee or tea, with sea-biscuit or toast.

7. If subsequently, during the voyage, the sea should become unusually rough, go to bed before getting sick. It is foolish to dare any thing, when there is no glory to be won, and *something* may be lost.

ART. II.—*Aspermatism.* By W. H. VAN BUREN, M. D., Professor of Principles of Surgery, and Diseases of the Genito-urinary System, Bellevue Hospital Medical College, New York.

A GENTLEMAN, 30 years of age, whose wife had not conceived after four years of marriage, was brought to me by the late Dr. C. E. Isaacs, in May, 1858, and, being exceedingly anxious for a family, he desired to ascertain if any thing could be done to improve his sexual power.

He was a spare-built, undersized man, but healthy

and strong, and very straightforward and truthful in his manner.

He complained of the occurrence, at intervals of from two to six weeks, of erotic dreams, attended by profuse seminal emissions, and followed by a sense of weakness; also, of the escape of a "clotted glutinous fluid"—evidently seminal—from the urethra after passing water, about every other day, and most noticeably in the morning on rising. Now comes the curious feature of the case: He had never been able, in sexual intercourse, or in any other way, to bring about the venereal orgasm, or to provoke a discharge of seminal fluid. He had prolonged the effort, on many occasions, to the full extent that his strength would permit, but with no result. At present he experiences no pleasure in the sexual act, rather a feeling of disappointment, indulging (if this expression can be properly used in such a case) but once in a month, and then only as a matter of duty, and in the hope of a more successful issue.

In his dreams he is conscious of a full orgasm and free emission; but he has never had this happen to him when awake,—so that the only knowledge he possessed, as to the nature of the complete venereal act, had come to him in his dreams. He had never attempted to provoke an emission by unnatural means; had never experienced any temptation or desire to do so, and would not know how to go about it.

The case of this gentleman was a novel one to me, and I was able to suggest no remedy but circumcision, as his prepuce was very long, and I thought that it might interfere with the physiological performance of the sexual function. This was done by Dr. Isaacs, but the result was negative. The patient returned some

two months afterward, to report to me that his disability was still unrelieved.

I have never encountered a second well-marked case of this curious functional defect; nor have I seen any mention of it by any writer in the English language; but, in the *Traité de l'Impuissance et de la Stérilité* of Dr. Felix Roubaud (Paris, 1855), I find several cases, evidently authentic, detailed at length. Their features coincide very exactly with those of the case I have detailed. The inability to accomplish the sexual act at will, although with perfect erection, and the occurrence of full emissions during the unconsciousness of sleep, are mentioned in all.

Roubaud's treatise is thoroughly scientific in its character, and one of the best works on the subjects of impotence and sterility of which I have any knowledge. He gives the disease the rather unsatisfactory name of "Aspermatism," which is not literally correct, for it is characterized not by *absence* of the seminal fluid, but by the *impossibility of its ejaculation* under circumstances necessary to secure impregnation of the female. It is described as an affection entirely distinct from "priapism," and also from "erotomania."

A case is related by a Scotch physician, Dr. Cockburn, in his "*Essais et Observations de Médecine d'Edimbourg*," Paris, 1740. And another is to be found in the *Gazette de Santé*, No. 52.

The only pathological explanation which accounts for all of the phenomena in this unusual affection would seem to be an exaggerated spasmodic contraction of the muscular fibres in the walls of the ejaculatory ducts, leading to their occlusion under extreme excitement. If another case of this kind should occur to me, I would suggest the trial of a suppository of

opium and belladonna, and an attempt at intercourse under its influence.

ART. III.—*The Diarrhœas of Children during the Summer Months, as observed in Private Practice.* By A. B. ARNOLD, M. D., Baltimore, Md.

THE liability of children of tender age to suffer with diarrhœa at any season of the year—which is notably increased during the hot season—is the concurrent experience of observers. In what manner the high state of temperature during the summer months, in the latitudes embracing the Southern, Middle, and some of the Western States, acts as an exciting cause in producing the serious gastro-intestinal affections of children, may not be easily determined. Whatever may be the nature of the endemic influence which causes the sudden attacks of cholera infantum or its more insidious and chronic form, it is, nevertheless, indisputable that the occurrence of this type of disease is limited to the summer months. No fact is better established than the certainty of the beneficial effects which an interval of cool and pleasant weather will exert in even the worst cases of infantile diarrhœa. Much importance has been attached to the opinion that the hot season induces a predisposition which favors the development of grave disorders of the alimentary canal of children from slight causes. These have been sought for in errors of diet, the irritation of dentition, and confinement in a close and damp atmosphere. All these conditions, as well as all unfavorable sanitary influences, act undoubtedly as morbific causes, but it still leaves unexplained in what this predisposition consists which excessive heat induces. There is

some plausibility in the inference deduced from the physiological law of vicarious functional action, that the over-stimulation of great heat interferes with the secretive powers of the skin, and thus excites the mucous lining of the stomach and bowels to a morbid activity. It is highly probable that this relation is far more intimate in children than in adults, which, perhaps, accounts for the frequent intestinal catarrhs to which the former are subject. In this connection the significant fact should also be mentioned, that the altered state of the liver, which is so constantly found in persons who have succumbed to the pernicious dysentery of the tropical climate of the East Indies, has also been observed with great frequency in children who have died of cholera infantum. There is no difficulty in believing that a number of the common diarrhœas met with during the summer owe their origin to a cause of this kind, but still the well-known peculiarities of our indigenous infantile diarrhœa cannot thereby be satisfactorily explained.

A curious fact presents itself, in studying the etiology of cholera infantum, with regard to age. While this disease exclusively attacks children, it is strange that Asiatic cholera, to which it bears a striking resemblance in many of its symptoms and pathological changes, spares as a rule the period of childhood. Both diseases, in even the rapidly-formed cases, are frequently preceded by a slight diarrhœa or cholerine.

Among 63 carefully-noted cases during the months of July and August of 1867, in which diarrhœa of some degree of severity and obstinacy was the most prominent symptom, I found that there were 22 male and 41 female children. Their several ages ranged from 3½ to 24 months. This number is, however, too

small to infer from it that sex exercises an etiological influence in predisposing children to the diarrhœas prevalent in hot weather. Contrary to what might have reasonably been expected, 38 of these little patients suckled at the breast, and 23 were weaned.

Acute Cholera Infantum.—It is remarkable how frequently children will rally from the terrible stage of collapse which the sudden invasion of cholera infantum causes in a very short space of time, although a troublesome diarrhœa generally follows. The exciting cause of this disease seems to act in the manner that a corrosive poison primarily affects the mucous coat of the stomach and bowels; and it is singular that the indications of pain should be absent, for the jactitation and restlessness, which alternate with short naps of sleep, indicate the intense thirst which tortures the little sufferers. No amount of drink is able to quench this thirst, but the liberal allowance of cold water or ice seems to satisfy it for the moment. The repeated vomiting and purging seem to be accomplished by spasmodic jerks and to drain the system like a profuse hæmorrhage. One of the most hopeful signs of a favorable reaction, according to my experience in these cases, is the change of the great pallor of the face to a lively yellowish tint.

A most treacherous form of diarrhœa is sometimes met with, which greatly resembles this disease, but which proves itself to be the brief prodromic stage of a fatal meningitis. The victims of it are children above two years of age, a circumstance which may aid in the diagnosis. It commences either with a painless, bloody diarrhœa, or with copious serous discharges of an irritating quality. The vomiting comes on at considerable intervals, but nausea or effort to vomit,

in younger children, is constant. Delirium, convulsions, paralysis, and coma follow each other in rapid succession.

Chronic Cholera Infantum.—Under this head I intend to include all the diarrhœas of children occurring during the summer months, which pursue a protracted course, whether they be ushered in by acute symptoms or gradually assume a serious character. The mildest diarrhœa during the hot season should be speedily arrested, and if such prompt medical interference is insisted upon by Bouchut, who recommended it to European practitioners, it applies with yet greater force to the American physician, who has to dread the insidious approaches of a notoriously intractable bowel affection, that so often commences with a trivial diarrhœa. As long as the infant continues to emaciate and retains the dryness and sallowness of the skin, however much the evacuations may have lessened or improved in appearance, it is not prudent to announce the establishment of convalescence. Exacerbations, amendments, relapses, and complications, surprising recoveries, and unexpected death, make up the history of this disease. At any moment the fearful symptoms of invagination of the intestines may show themselves, or the ghastly features of the last stage of a fatal enteritis may arrest attention, or the first unmistakable signs of cerebral mischief may blast the hopes of the intelligent and watchful mother.

Very little information can be gathered from the state of the pulse, the febrile movements in this disease being best judged by the rise and fall of temperature which the back of the hand feels, when applied to the head or over the abdomen. Neither do I believe that much reliance can be placed on the nice diagnostic

distinctions, based on the frequency, color, and consistence of the evacuations, as there is little constancy in these appearances. It is, for instance, just as likely that the profuse serous discharges, or the colorless sago-like bodies that swim in a watery fluid, may be owing to some change of innervation of the sympathetic system of nerves as to the morbid anatomy of some of the abdominal viscera. Nothing positive is known concerning the manifold changes of color which the biliary secretion may undergo in the intestinal tube, in order to determine the diagnostic value of the variable tints of green and yellow which the excrements of the bowels so frequently exhibit in these chronic diarrhœas of children. No answer can be given, in explanation of the clinical fact, why the pale-yellow, flat, and somewhat round little cakes of fecal matter are nearly always accompanied with colicky pains. The great probability that the large mucoid discharges of a dark-brown or reddish-looking color, and of a very offensive odor, indicate a gangrenous condition of a portion of the intestinal mucous membrane, is inferred from the sudden collapse and speedy death which have been so often noticed to follow these untoward symptoms. Straining of the anus is often enough observed, especially if the discharges are small, mucoid, and tinged with blood; but that degree of tenesmus characteristic of the dysentery of older children and adults is seldom noticed in the bowel affections of infants.

Cerebral Complications.—A great source of danger in these diarrhœas is the frequency with which the brain becomes involved. This is probably owing, in many cases, to the liability of the nerve-centres of children to be sympathetically affected during the course of any grave disease. In a number of instances,

however, it will be found that, on close inquiry, no urine had been voided for some time, and direct examination will prove that the bladder is empty. The head symptoms, the convulsions, the coma, are here owing to uræmic poisoning, which reminds one of a similar state of things characteristic of the dangerous reactive stage of Asiatic cholera. It requires considerable care and attention to interpret correctly the first faint signs of brain complication in these cases. A continued heat of head and abdomen, while the extremities feel cool, ought to cause much anxiety. The sinking of the abdomen is very significant, and, when the head at the same time is found to bend slightly backward, convulsions are near at hand. The abrupt stoppage of the diarrhœa for 24 hours is much to be dreaded. The hydrocephalic cry, of course, cannot be misunderstood, but very frequently a somewhat similar symptom makes its appearance, which is of equally serious import. After the child has lain perfectly still and has apparently enjoyed a sound sleep, lasting perhaps not longer than from three to five minutes, it will suddenly arouse, look about as if afraid of something, become very restless and moan, and no effort of the nurse or mother can pacify it, until the paroxysm of pain in the head, which it evidently is, shall have subsided. This condition of things may last for days, but is sure to be followed by the more declared symptoms of cerebral complication. Sometimes the child will stuff its mouth with the fingers and make attempts to cough; or will move one of its hands or arms in an unusual manner, repeating the movement at regular periods. Occasionally it bites the nipple and cannot be easily made to relinquish it. These and other symptoms, which are so often witnessed in cases of tubercular meningitis of children, ought to direct attention to the head.

Prognosis.—There is a real phthisis of the alimentary canal in these summer diarrhœas of children. The emaciation that attends them is frequently extreme, and gives to these poor patients a hideous appearance. Some of the signs of inanition and several other points of prognostic value in these cases deserve to be noticed. The rough, wrinkled, dirty-looking skin, all over the body, but especially when the temples and forehead have the aspect of dried specimens of anatomical preparation which are covered by a pale glazing, indicates the last degree of emaciation. If the skin looks rather folded than wrinkled, and still feels soft and yielding, the case is more hopeful. A dry, brown, loaded tongue, or if it looks as withered as the lips, is of evil augury. The same may be said of the pale, pinky lips and gums, which appear as if they had been smeared over by a thin layer of light-colored varnish. When the partially-open eyelids during sleep remain shut for some time after they have been closed by the fingers, death is near. Ulceration of the cornea, aphthæ of the mouth, and excoriations of the anus, are, as usual, the forerunners of a fatal termination. A cheerful look and an occasional smile are hopeful signs, although the diarrhœa may not have abated, and the emaciation be considerably advanced.

Treatment.—I know of no remedy which is better retained and puts a stop to the incessant vomiting of cholera infantum sooner than often-repeated teaspoonfuls of mint julep. Cold water and ice ought to be allowed *ad libitum*, and no regard should be paid to their immediate rejection by the stomach, for the soothing effects obtained and the relief they give will soon become apparent. Sinapisms and irritating embrocations to the epigastrium I consider worse than useless.

But soft woollen cloths, wrung out of warm water and wrapped around the child from the axilla to the toes, I found to be grateful to the little patients, and to promote in a short time a healthful reaction. From two to five grains of the subnitrate of bismuth every two hours will be soon retained by a child under two years of age. The dark stools, of much less frequency, show that the medicine does good. The property of bismuth, of adhering tenaciously to mucous membranes, makes the drug very serviceable as a topical sedative, besides the constitutional medicinal effects which it is believed to exercise on irritated and congested mucous surfaces. In the protracted forms of diarrhœa, and especially in cases accompanied with much pain, this property of bismuth has the additional advantage of enabling the physician to combine with it Dover's powder in smaller doses than it is indicated; for the entanglement of the opiate with the bismuth allows of its longer retention, and consequently fuller absorption by the stomach, nor is it so apt to cause nausea, when thus combined, as when given alone. The timidity of administering opiates to very young children I do not share, and I can add my testimony to the highly beneficial effects often derived from them in the obstinate diarrhœas of children. When emaciation begins to show itself, the mild ferruginous preparations as first recommended by Dewees can safely be given. I give the preference to the following prescription:

℞. Ferri et Ammoniæ citratis, ʒ ss.
Tinct. Calumbæ, } aa. gtts. xxx.
Tinct. Nucis Vomicæ, }
Sacch. albæ, ʒ iij.
Aquæ, ℥ iij.

M. et Sig. Give a teaspoonful three times daily for a child between one and two years of age.

Frequent tepid baths and persistent sponging of the head and abdomen, when these parts show any increase of heat, do excellent service.

ART. IV.—*Notes on the Waters of the Gettysburg Mineral Spring.* By JOHN BELL, M. D., Philadelphia, Pa.

OF the various fashions that bear sway at the present time, the free drinking of the waters of mineral springs is decidedly in the ascendant. Even if carried to the extent of drenching, it is vastly preferable to the excessive drugging to which, it must be confessed, both physicians and patients are too prone. We need not enlarge on the collateral benefits derived from visiting mineral springs, in the change of air and scene, agreeable society, and freedom from the cares of business and professional toil. But while thousands annually turn these advantages to account, in the pursuit of health and pleasure, a still larger number, the majority in fact of our people, find it necessary to stay at home, and, as they cannot go to the springs, they do the next best thing, and have the waters brought to them. Until recently, the chief mineral waters obtainable in this way were the far-famed Saratoga, the Blue Lick, and the Bedford. To these may now be added the Gettysburg, the Welden, the Rockbridge Alum, the Missisquoi, etc. To enter into a comparative view of the therapeutic value of these different waters would lead us into disquisitions for which we have, just now, neither time nor room; and we shall content ourselves with noticing one, viz., the Gettysburg Mineral Spring, which of late makes large claims on public attention

and receives large professional countenance. Putting aside its fancied resemblance to the celebrated springs of Vichy and Baden-Baden, both of which are highly thermal, we see in the Gettysburg an alkaline water, as is shown in the following analysis made by Professor Mayer, for a while assistant to Professor Henry, of the Smithsonian Institute:

	Grains Troy.
Bi-carbonate of soda " lithia	46.05
" potash	trace.
" magnesia	76.05
" iron	trace.
" lime	81.00
Sulphate of lime	53.20
Chlorides	trace.
Silica	10.00
In an imperial gallon	266.30

This may be regarded as the maximum quantity of solid contents of the water, the strength of which varies, but the nature of the ingredients is unchanged. It requires a knowledge of its mineral impregnation to make us believe *a priori*, that the Gettysburg water, which has no more taste or smell than pure, common water, possesses decidedly medicinal properties. But after all, however much chemical analysis may seem to indicate this fact, the proofs must depend on experimental trials made with judgment and in good faith. A summary statement of the cases of cure effected by the use of the Gettysburg water, previously made by myself,[1] rests mainly on the averments of the patients themselves, who write with evident sincerity and an unmistakable conviction of their restored health being due to the course pursued. When medical testimony

[1] Medical and Surgical Reporter, Sept. and Oct., 1867.

is offered, it is to the same purport. Coming down to the plain matter-of-fact level, and closing our eyes to the would-be "marvellous" and "miraculous," as applied to the operation of this water, we cannot well deny its efficacy in the cure of certain not well-defined forms of dyspepsia and kindred disorders of the genito-urinary apparatus, and also, in a more especial manner, of chronic rheumatism and rheumatic gout, with often-associated cardiac affections. A case of complication of diseases of this nature, proceeding from uterine derangement, has just been recorded by Dr. R. B. Weaver, in which the drinking of the water at the spring was soon productive of the happiest effects. The patient, Miss M., when first taken to the spring, "had to be carried about like a child by her servants, owing to extreme debility and pain. She had a cough, which had been pronounced by some of her physicians consumption. Her first attack dated five years back, recurring at intervals with greater or less severity, until it reduced her to her present apparently hopeless condition. The joints of the fingers, toes, and limbs were enlarged by urate or chalky deposits." After the short period of thirteen days in which Miss M. drank the water, "she was enabled to walk with but little assistance from her servant, and improved so rapidly in all respects that she now appears to be permanently cured, being relieved from the cough, general debility, and the irregularities referred to, also the enlargements or chalky deposits around the joints. When she came to the spring, she only weighed 61 pounds; she now weighs 90 pounds. From a pale, sickly-looking, dispirited, decrepit invalid, she has emerged from the chrysalis condition into a young lady in the bloom of health, youth, and beauty."

The direct operation from the Gettysburg water is sometimes mildly aperient, but more generally diuretic. One of its not infrequent effects is a slight vertigo, which shows an action on the brain and nervous system, that should prompt to trials of its curative powers in the neuroses generally. While there are undoubted advantages on the side of invalids who visit springs and remain there to drink their waters, it must, at the same time, be admitted that the stay-at-home drinkers enjoy the benefits of a suitable and regulated regimen, and regular hours, which are too generally denied to the former or travelling class. The fact of the Gettysburg water retaining all its properties without change, for any length of time, after its transportation from the spring, is worthy of notice, and must serve to encourage those at a distance to its free use.

ART. V.—*On the Supposition of two Changes of Color in the Red Blood.*—By R. K. BROWNE, M. D., N. Y.

IN the winter of 1861, during a term of study as assistant of Prof. Austin Flint, Jr., I first had my attention peculiarly fixed upon the subject I proceed briefly to discuss. While I was intent upon a striking experiment which that indefatigable physiologist has made us familiar with—namely, the experiment of passing defibrinated blood by injection through the vessels of a pair of removed lungs—I was forcibly moved by the question, Is there any *other* change of color in blood than this being exhibited? Since that event, from time to time—occasionally for several consecutive months nearly exclusively—the nature of the change of color of blood has been a subject of

study of always increasing interest. I was not long in reaching conclusions in accord with those I present; nor was I much longer in regarding the question as being of commanding importance in the field of physiology. But, in the end, I have come to regard the proper knowledge of it as of superior importance to that pertaining to any other subject of biological research whatever, except, indeed, that of the nervous system; still I should never have ascribed the importance I do to the truth, if new experiments, and thence a correction of the misapprehensions pertaining to former ones, had not presented me with that various experimental illustration which, I distinctly foresaw, would be exacted to insure the conviction of other physiologists.

Without this, I should have been excessively slow to present what I conceive to be truths which not only break up the fixed tenor of our beliefs in this direction, but show them to be fallacious in every particular but one, and that unimportant.

Moreover, I was constrained to the conviction by an invariable experience in such cases, that nobody would be thanked, but rather the contrary, for bringing others, whose sole consequence is based upon their claims to knowledge, to a recognition of the errors they had so long held.

Further, I felt confident that, if the truth, when stated, came to be acknowledged, it would be in such an underhand way, either by an indifferent *nod* of recognition, or one that "amounted to the same thing," as would serve only to increase the sum of what we already lament of this kind of scandalous reception of important truths of science by reputed adepts. However, the experiments to which I referred

as recent are the first which ever showed, in any obvious way, the precise fact of the character of the case when oxygen leaves the globule, and the first which spread such a light on previous experiments as to make it perfectly easy to correct all the misapprehensions they had in part confirmed.

What I have said of the importance of the subject is of itself plain, when it is considered that our reputed knowledge of the blood is based upon our invariable belief in two changes of its color, and all date from the days of Priestley. He is said to have demonstrated that nitrogen, hydrogen, and carbonic acid, will *turn the blood black;* and we have repeated the lesson until the belief it embodies is so bound up in our mental structure, that we shall probably begin by regarding the student who asserts the contrary as out of his right mind.

Yet, Priestley's experiments *never* demonstrated the reputed fact, for there is no such fact as that alleged, and the blood is black without the slightest intervention of either of these gases. Moreover, neither of them will communicate from the air with the blood in circulation, nor of themselves mingle with any part whatever of it. It is not plain what fact Priestley's experiments *did* disclose. The distinguished physiologist we named declares that "there is no evidence that nitrogen enters into combination with the blood-corpuscle" (and might have as truly said the same of carbonic acid), while he quotes Priestley as showing that nitrogen and other gases darken the blood by "displacing the oxygen."

But, this much by way of preface. As it arose on the occasion I named, seeing that the blood passed dark red into the vessels of the insufflated lungs, and

came out bright red, the question was, Does any *other change of color* occur in the blood; or are there, as we were taught, and believed, *two* changes of color? Is there, indeed, *any* change of color, except that made, as here, by some normal constituent of the air? This was a question forced upon us, which traversed the length, breadth, and depth, of all we had yet learned upon the subject. We had all the learning pertaining to it, which several years' rather diligent study of this and other questions in physiology, and of authorities, could give us, and yet here we were mentally waylaid by a question as to whether this very knowledge was true; and from that moment we had no ease of mind, until we had become as conversant with the subject experimentally as we could. But our experiments did not, at first, radically enlighten us; for, as we afterward learned—a lesson the experiments did not teach us—they started with unwittingly assuming, *as a fact*, the very thing itself which the experiment should have determined, and which turned out to be the main error in the case. *This* was, that the color of venous blood is *itself a change of color* from another color. We had not the requisite knowledge, then, nor for a long time afterward, nor until we had experienced many heartfelt bafflings, which enabled us to perceive how it could be *otherwise* than we had learned. Priestley supposed he demonstrated this, and all succeeding experimenters do the same. What he did, he supposed, demonstrated the fact. To him, and to us ever since, there seemed to be neither error nor supposition in the case, but a plain matter-of-fact "demonstration." Yet there *was* such an error, as we shall see.

The question then was, Has the blood two changes

of color? We believed, as all believed, that it had. But it is plain that the first step toward an answer to this question resolved itself into a second demand to know what is the origin of these colors? We had not, to begin with, the slightest suspicions respecting them, except that the dark red was a change *in* the bright red, and the bright red *another* change in the first. Had the blood a color which was not a change?

To find ourselves putting either of these questions, was to give a *character* to the inquiry it had not before been endued with.

But, to abbreviate these private experiences in quest of the truth. The answer which came to these questions came to their eventual form, which was, Have not the blood-globules a color from their very birth and formation—a color, namely, which is inherent in their organic or anatomical unity? The globules which color the blood cannot have been originally *without* color, because, in that case, they would *now* be, like the white globules, uncolored. Muscular fibrillæ have a color. As the uncolored globules are in constant contact with whatever gas the liquor sanguinis contains, but have no color communicated to them thereby, how can carbonic acid—one of these gases—color the other globules? It cannot, unless they have color beforehand. Of course I looked for the substances *within* the body and within the blood, to account for whatever could be found respecting this coloration of the globules, and not to a substance *outside* of it, like oxygen.

By this time I began to suspect I had been looking away from the true source of the color, for the question recurred as to its origin. To this ques-

tion there could be but one answer, which was, that the red globules had a definite and distinct coloration from the beginning, i. e., by formation. In the nature of the case there could not be the slightest misgiving about this answer. I need not detail the further intellectual operation which ensued upon this, but at once proceed to state what I have long conceived to be the truth in its completest form, and what abundantly vindicates my assertions of the nature of the suppositions we habitually hold upon the changes of color in blood.

What is the fact, real or reputed, now, about the colors of the blood?

First of all, it differs from any other fact of color. It is *unlike* common facts of color. About them there is never any mistake nor uncertainty, nor any mystery, except what may be alleged of the origin or existence of any thing. Sense notes one color here, another there. It is a simple distinction which entirely exhausts the facts in the case. Here is one kind of coloration, there another, or other kinds; there is nothing unusual in the case; there is no "change of color." But the colors of the blood are different from this—they *are unusual*, and hence we speak of them as a change of color.

But does this difference between these two cases exist to the entire exclusion of any similarity or community, such, for instance, as exists between Prussian blue and vermilion? We have never supposed that there was any such similarity; for it is an invariable supposition that color of the blood has not, like other cases of colored bodies, an *origin by itself*, but that it only exists, each from the other, by constantly-repeated and frequent variations. And thus our invariable

(but erroneous) estimation is, that the bright red arises from some change in the dark red, while that arises from some change in the former.

But let us see if this coloration of the blood have not an origin and existence too, *similar* to other instances of color, outside of the circulation. Whatever *change* it may have, whatever *difference* therefrom, let us see if it have *not*, at least in *origin*, also a similarity with other instances of color.

Where does this color of the blood reside? In the blood-corpuscles! These are bodies which have been made, formed, or constructed.

In being made, they are organically or structurally endued with color. Otherwise, they are made *un*colored. But the globules made uncolored exist as made; and the striking difference between these and the red globules is, that the latter are endued with coloring matter or substance. And *they*, being made colored, will so continue to exist.

It thus turns out that the red globules, apart from any change, are colored; and that, in the origin and incorporation or embodiment of this characteristic, they have something in *common* with all other colored things of distinct origin.

Yet we have believed the contrary; we have invariably believed, not only that, besides something *intrinsic* to them, they have something *incidental* to them which is peculiar, but also that *all* which pertains to their existence is peculiar and *entirely* unique. But *besides* this, which they share with all other colored things, besides being like the latter, they *undergo* something which the latter do not. This *something* is, that they experience *a* change of color in the *circulation*.

And the precise deficiency in our physiology is,

that we were invariably totally unrecognizant of, and without the slightest suspicion of, this historic event—once for all, of this organically grown and developed color of the red globules; while the change of color, their current experience, what was incidental and *peculiar* to them, have been our sole idea.

What Priestley, then, and all succeeding experimenters needed to have done, was to have *identified* this color; and to have done this as a bare fact required very little experiment. Instead of *assuming* and supposing that, where this color was, it *was a change of color*, and then, mingling certain gases with it, mistakenly attributing this color to them, as induced by them, he should have corrected that very error, and ascertained the fact instead, which he could easily have done by showing that this color exists when not in *communication* with any one of these gases, nor any *other* gas. Or, if he had used a reducing agent to remove this color, precluding its immediate recovery of more, he would infallibly have found that, even when carbonic acid was not near, the dark color would have returned. He could not *experimentally* have shown what the color existed from, for he could not renew the process of its formation in the globule; but it was incumbent on him to have shown that these gases did *not* produce nor induce it, instead of confining himself mistakenly in the supposition that *they* did, and thus have given us at least the negative fact, in lieu of the false supposition.

It seems to us that the reader must, by this time, have his eyes so very wide open, that he could proceed to formulate the entire truth without further help from us.

He supposed that color existed as a change, and,

finding *that* it existed when these gases were added, he attributed it to them.

As *with this* color of the blood, by physiological genesis, no two colors by production of carbonic acid or oxygen can exist, which of the *two* colors of the blood is it thus made?

An infallibly satisfactory answer to this question need not occupy us long; but still, as both are *said* to arise by "change," or conversion, though one of them does not, it is incumbent to show which of them exists independent of any change. It must already, however, have been seen that, as there cannot be two colors by "production" or change, two changes cannot exist. It must also be distinctly recognized that we still suppose the dark color to be a change, and it will not be until we make plain *which* color exists by genesis (and hence not by change), that we may substitute the fact, in the case of one, for the error.

In the experiment of Prof. Flint, if the insufflation be suspended while the venous blood be passed through the lungs, change of color *ceases;* the blood is *without change;* the successive incomes of oxygen by insufflation are suspended. With the discontinuance of successive additions of oxygen, change of color ceases.

This change of color, therefore, is admitted. *It* exists. Of the two colors, one of which has been produced by genesis, this bright-red color cannot be that. But the experiment shows that this has to be *kept up* by successive additions of oxygen, a substance from without the body. This color, induced by change, cannot be that by genesis, and, as there is no other but the dark red, it must be the color by genesis.

Now, what stands in the way of our regarding this

as a perfectly satisfactory solution of the question? Nothing but our traditional and invariable belief that carbonic acid is in the globule of this dark blood. Perhaps no one of my readers has ever considered that, as Dr. Flint says, "there is no evidence that nitrogen enters the globules;" so there never has been any that carbonic acid ever did so. *Nor does it.*

No carbonic acid ever darkens the blood, nor ever enters its globules. And we need no experiment, nor any undertaking of any sort, to show the truth. We might say, as conclusive, that, as there *is* a color of the blood by genesis, which is not this change, the dark red must be that color. But we proceed to show that this dark-red color will exist when the blood receives no carbonic acid, when it contains none, and when carbonic acid *is not* in the case. Our invariable *supposition* to the contrary—our supposition, namely, that there is a nearly constant *in*volution of carbonic acid in the globule, from which it is only occasionally absent, from the force of expulsion by respiration—is *pre*sumed in every experiment, has never been shown by experiment, though experiments made under the *pre*sumption have seemed to sanction it. This supposition, itself unproved, will of course demand *dis*proval, or exact it to be shown that carbonic acid does not, as supposed, occupy the globule. And this in presence of these facts—first, that carbonic acid is a substance always *e*volved, and *never* like oxygen, which by its own act becomes *in*volved. And it is evolved, not because of any inherent property or quality to *combine* with other substances, but, on the exact contrary, to leave them, because it is extruded by the processes in the substances which produce it. All, indeed, which can be formulated to the exact

contrary of its being able to impose itself, after evolution, within any substance, as oxygen does, is true in the strictest sense of carbonic acid. The plant takes it up in the exercise of its physiological power to appropriate elements of nutrition, of its own behest, only to take away from it its carbon—showing that there is no power inherent in it to hold its own, or maintain its own identity, and showing that it is, in the nature of the case, devoid of the power to merge its identity into, or fuse, or form a union with, another substance, and, above all, with substances complete in their own composition, like the formed elements of the body. *They* might appropriate its carbon, as the plant does, not its carbon *them*. But this, which is its destination, can only be done by the tissue, *because* the carbonic acid itself will do nothing in the matter. It does not, of itself, even seek contact, or to communicate itself to other substances, and will not, of itself, even freely commingle with them; but, so far as *their* nature permits, even when brought into the apartment with them, will remain apart. It is dead in its passivity, being the mere "site" of sepulture of the *incautious* oxygen which has taken hold of carbon in the exercise of its invincible nature to combine, when uncombined with other substances. Not even other oxygen will *displace* that in the globule. And how can this imagined clash of atoms between the two occur to the displacement of oxygen by carbonic acid? It cannot.

And this brief exposition of the facts is not only very much to the point, but *solely* to the precise point; because it is this very inert compound substance we childishly suppose to combine with the red globule of the blood—a full, grand anatomical form—while *all*

our idea of the supposed change of blood to dark red turns upon (indeed, is nothing else than, under the name of *change*) the bare supposition that it displaces the oxygen from the red-blood globule, and thus *makes* the blood dark or venous. And the absurdity, philosophically speaking, is carried to an immeasurable extent, when, in supposing this, we suppose it to leave the tissues, where it is produced, at a very sensible remove or distance in any situation, and to travel *uncombined* from the outside of the blood-vessels into their interiors, and thence, meeting the plasma, transposes itself to the globules—since the globules seldom communicate directly with the internal surface of the walls of the vessels—there displacing the oxygen from the globule, and taking its place. This oxygen, which we invariably associate with the globule as forming a *union* with it—and almost invariably as forming a union so irreversible as to fuse with its carbon to form carbonic acid—we as invariably suppose is displaced by carbonic acid, which never yet formed a fusion or *union* with any thing. It would not be so preposterous as this—so plainly adverse to the truth of things—to suppose that the carbonic acid of the plasma appropriated, or affinitized with, the oxygen from the globule; for the only error in this supposition would be one of fact, the carbonic acid being no substance to do this, or to affinitize at all. But to suppose it, in face of its reticent characters, to enter into the dense substance of the globule replete with oxygen, and thence without delay to displace it, is certainly remarkable, *and stands to reason.* The supposition respecting the carbonic acid staying in the plasma, and affinitizing with the oxygen, we said above, was *not so* unscientific as the other; and we said so, because, *if* the car-

bonic acid were a substance like others in the body, unlike what it is, if it were a substance, the same as others in the plasma, of a nature or physiological character to affinitize with the oxygen of the globule, and *it* with them, there being considered no impediment to this in the relation of the oxygen and globule, *then* the oxygen would change contact from the globule to these materials in the plasma.

Now, this supposition, excluding the carbonic acid from the transaction, and substituting therefor the oxidizable materials of the plasma in solution—fluid materials—is an exact statement of the facts, put in the amended form of the supposition; and, being this, shows at once *all* the circumstances of the case—of what transpires, in capillary situations, in the experience of the globule with its oxygen, after leaving the lungs—not only the circumstances or particulars involved in the *passing* relation of the oxygen with the globule, but its eventual one in the plasma; and, by a definite disclosure of particulars, shows that one ordinary notion of "exchange"—of the oxygen doing precisely what we suppose the carbonic acid in an opposite direction to do, namely, to traverse the fluid from the globule through the capillary tissue, thence further through interstitial substance to tissue, is an unphysiological fancy, having no character, except that the one supposition is the invented contrast to the other—a "harmonious" fancy, and, as *fancy*, void of fact.

The theory of "exchange" is a compendious name for the *same* supposition respecting the displacement of oxygen. But the carbonic acid never enters the plasma in this imagined way, nor does it ever enter the globule. It enters the plasma—not at all by itself,

but as in, and transposed by, the fluids which move from the tissues into the plasma—almost entirely; first into the lymph-canals and vessels, and thence into the blood. This, and not the globules, is its physiological destination in the body. Neither, on the other hand, does the oxygen traverse into the tissues in this imagined way, nor first communicate with them, nor ever appear amidst them in that free way, but only as transposed to them in combination with the materials of the plasma; its eventual destination is the latter, though, no doubt, it also, with plasmatic materials, goes to the tissues. Nor are we to suppose the oxygen travels from the globule to new situations in the plasma; it does not travel away from the globule, or make any excursion. The plasma is on every side in contact with the globules, and with it the oxygen unites or combines. That it is not still is simply because the plasma moves, and the cardiac impulses and arterial reactions cause interior commotion of the materials of the fluid.

But what is the case as we now regard it? Supposing there is an impediment to the oxygen leaving the globule (in case we *do* acknowledge that it does leave the globule, which we not seldom contradict), and seeing no exact unity of event between this leaving and the dark color, but imagining an identity between it and the incoming of carbonic acid, we imagine the latter to arrive from the tissues, dispossess the globule of oxygen, and establish an intermolecular relation in it; and all this without the slightest encouragement of the globule itself. And this also when we imagine the same carbonic acid soon after to leave the globules, unimpelled, undislodged therefrom, for the air in the lungs. And this im-

agined circumstance, closing in this imagined act, we suppose, darkens the blood. How can we indulge our imaginations to this extent in the presence of facts? One of the most rousing facts which strikes the student, in conceiving our representations of this supposed origin by carbonic acid of the venous hue, is the confessed absence of any recognizable circumstance of a *chemical* character, or of any similar character in the globule; and this absence of, or want of, such character (which is, in truth, only another name for the absence of *any* event as imagined), we call "conversion;" for both the globule and carbonic acid confessedly maintain their complete integrity, and separate *unchanged.*

This imagined change from red blood to dark is what Priestley "showed." But by the removal of oxygen from the globule with any reducing agent, to the exclusion of any carbonic acid in the area of that operation, he would really have shown the fact, that the dark exists in the absence of the latter. The truth is, the blood undergoes no change at this time, being dark because of cessation of prior change to red, and recontinuing the same hue on cessation of *that* change. The change occurs from access of oxygen; and the recontinuance of dark color, on loss from the globules of oxygen.

After the reader has proceeded so far, let us ask him what he now thinks of this notable want of any change of a chemical character in this supposed change of color by carbonic acid? What does he make of this want of change—the absence of any difference in all the globules and in all its characteristics, after the supposed change by carbonic acid; such a difference as that which is signalized in converting starch

into sugar, making it thereby another substance? Nothing but the most entire inattention to this want in the case, while we were alleging to exist what did *not* exist in fact, could explain the absence of any endeavor on his part to reconcile this lack of converted substance with the rest of his theory, that carbonic acid enters and leaves the globule; for consistency made it incumbent on him to explain that, as the carbonic acid made no conversion of substance, and made no change longer than it stayed in the globule—which was thereafter, as it had been, of unconverted substance and color—the simple loss of the carbonic acid from the globule was all the antecedent needed for the globule to be of the same color as it was before—namely, the bright red. If he had given any attention to this point, he could not have failed to see that, if this supposition of carbonic acid darkening the blood was true, this also was true; and, if true of carbonic acid, was true of oxygen in making a change; for the globule remains precisely the same when the oxygen has left it, as it was before, which is only another name for saying that it remains dark red.

But to recur for an instant to our invariable habit of supposing that between the atoms of oxygen in the globules and the atoms of carbonic acid there is a process of what we call "exchange" coincident with the supposed darkening of the blood, meaning thereby—as the elements of this transaction—the carbonic acid arriving at the globule, and, conversely, the oxygen from the globule arriving at its destination in the tissues. From the moment we became intellectually persuaded that this was fabulous, that there was no such excursion of atoms of either one or the other, we had not the slightest peace of mind until we had

devised an experimental disproof of the alleged fact. At first sight, and for a long time, it seemed impossible to make a demonstration of the truth. The things which we habitually, and indeed invariably, believe and teach as knowledge, have rarely been demonstrated; and so in this case. What was demanded to appease the craving for truth was, to show whether these atoms of carbonic acid on the one hand, and oxygen from the globules on the other, did come and go.

The experiment we at length devised may be briefly recounted thus:

If into the pulmonary vessels of a pair of removed lungs, being insufflated, a stream of arterial blood from the vessels of a living animal be transfused, the stream traverses the lung-vessels, and quits the extremity of the vein, without the slightest change of color, and hence without loss of oxygen. This experiment shows that oxygen will not, *under these circumstances*, leave the blood-globules and *traverse the tissues*. It demonstrates additionally this, that it will not leave the globules, and go *from* the blood, and make an excursion even into tissues, which, from the air-vessels, it constantly traverses to communicate with the globules, or to enter the blood. But these are *not* the circumstances under which we generally suppose atoms of oxygen leave the blood to traverse the tissues. No. But they *are* circumstances under which these atoms *from* the blood would traverse the tissue, if they possibly could do so *under* any circumstances, or be made to do so. No objection can be imagined against the demonstration on this score; for what is alleged shows even more conclusively the demonstrated fact. The tissues are tissues which hold the same relation to the

blood as in other situations; and more than that, they are tissues which always are traversed by oxygen, *but not by oxygen from* the blood-globules.

But it may be said that the oxygen of the insufflated air *prevented* or opposed the exclusion. Ah, indeed! if oxygen be brought into communication with oxygen of globules, can it be supposed to *prevent* the latter from leaving the globule? How reasonable to habitually suppose that carbonic acid enacts the work of forcing it out of and away from the globule!

But the experiment was the same if the lungs were insufflated with air containing too small a percentage of oxygen to oxygenate the blood.

Of course we were satisfied that what the reason of the case had made plain, the experiment had demonstrated. This experiment, moreover, infallibly taught us that the respiratory process was not "diffusion." Here were all the supposed conditions of diffusion, and no respiratory process.

Nor, on the other hand, it was found at the same time, would carbonic acid travel through the tissue from its situation *into* the blood.

But we must draw to a close. The experiment we mentioned near the beginning may, without further consideration, for form, be thus stated:

Years ago, Brucke showed that blood saturated with oxygen, and situated in a vacuum, lost its oxygen, and the dark color appeared. He himself appears never to have apprehended the import of this phenomenon, but probably attributed the dark color to the carbonic acid in the plasma.

In the recent experiment, if a solution of red blood-corpuscle from arterial or oxygenated blood in the

serum be entirely excluded from the access of oxygen or air, it will be found to part with its oxygen, and the dark color supervenes. If the same experiment be now repeated with another quantity of blood in serum, freed from carbonic acid, the same result occurs. If shaken up with air, the bright red returns, but after a time will again "become" dark.

In this experiment no carbonic acid can communicate with the globules, and yet, in the entire absence of such communication, it will be dark.

But our common experimental experience furnishes as plain a proof of the truth that this dark color is *not* caused by carbonic acid. So wholly inconsiderate is the invariable belief that the blood darkens by carbonic acid, that we *suppose* it in our experiences, when the experience itself proves that it is not so.

Thus, if we expose a coagulum of venous blood to the common air, the oxygen of the air reddens it. Now, what is the supposition here? and what, after the change, still continues the presupposition? This, that it was dark by carbonic acid. And, if asked what induces it to be otherwise than dark, prompted by that supposition, we say, some of the oxygen of the air has *displaced* the carbonic acid. But this is *un*saying precisely all we have hitherto said about the matter; for we are habituated to say that carbonic acid "displaces" oxygen. In this case, there are the most favorable conditions possible to devise for the carbonic acid, *if it be* there, remaining in *statu quo*, and the blood staying dark red. The globule itself has ceased its movement, and the carbonic acid, as an inactive gas, must at least be able to continue its contact with the globular substance, either within

or without. This would suffice to limit the close contact of the oxygen. If it *is* able to displace oxygen, it must at least be able to occupy its place. Does it? Not a whit. Why not? *Because it has never entered into the globule.* Now change the case. The blood is reddened, i. e., the surface of the coagulum is bright red; place carbonic acid upon the surface. Will it enter the globule? By no means; not an atom of it will do so.

But, in deference to the limit of our due space, we must close.

To say all in one word:—

The organic coloration of the blood is dark red, and is precisely that we know as venous color. This color is *not* due to carbonic acid, or any gas. Its whole history is, that it is invariably of this color, with frequent interruptions, called a change, by the access of oxygen. This change is the one only change, frequently and times without number repeated; this change is the only extraordinary feature it has; the color which marks it is wholly *incidental;* it is a mere phenomenal change *of* and *in* the dark color, which itself is wholly intrinsic; it does not involve any "conversion" of the coloring substance of that color, it does not dispel it, but is a mere change or passing affection *of* it. The most unmistakable of all the facts in the case is, that the red globules which leave the lungs are not changed in either substance or form. The oxygen leaves the globule, and, in leaving it, leaves it of the color it *was*, i. e., dark red. The blood reddens; it never *darkens*, being dark in its very construction, and never ceasing to be so, except when subjected to oxygen.

To suppose it becomes dark, or is made so, is our

invariable misapprehension; but it is mere *superstition* in scientific matters.

Summary.—In order that there may not be the slightest obscurity upon my meaning, let me here say what I mean by "change" of color.

I mean simply that no new property in substance or form, at the expense of some old one thereby deceased or "converted" (or "changed" in the sole sense in which we always use the word), has been made. I mean, in terms from common parlance, that the coloring of the globule previously in *statu quo*, previously dark red, undergoes some *phenomenal* change, some agitation, molecular motion, among its particles. This phenomenon is no new *statu quo* instead of the old one, i. e., the dark-red; nor is it the revocation of the old *statu quo* and its decease or obliteration; but only a transient interruption of the *statu quo* itself, which will immediately be uninterrupted, or will again be a *statu quo*, until again interrupted by the disturbing oxygen. Now, the dark-red state of coloration will continue *un*interrupted, unless the oxygen be admitted. It is no disturbance of the light red, nor any second disturbance supervening on it; because the light red is simply this—a transient interruption of it. This dark red is the perpetual *sine qua non* of the frequent taking place of the change by which itself is or becomes light red. But by this the reader will not suppose me to say that the dark red is *hidden* under or masked by *another* color or coloration; what I say *pre*cludes such a notion. If it were so, how would this dark color recontinue—this *statu quo* resume? Or how, indeed, would this interruption of this *statu quo*, this interruption—namely, the light red—take place times without number? Nor will the

reader suppose me to mean this interruption of the *statu quo* to be a new and different *statu quo* or *effect*, but simply a phenomenal disturbance in it. This dark red or *statu quo* of color is the very *sine qua non* of the innumerable recurrences of "change" in it. The change by oxygen, i. e., the light red, is an innumerable repetition of one and the same change; it is not innumerable *changes* apart from repetition, but an innumerable repetition of *one* change; and what "other" there is, we mistakenly call "another change," meaning thereby a change in alternation with this repeated one change, having a like character originated in carbonic acid, as the latter arises from oxygen—is the *statu quo* of the dark red itself. *It* is not repeated by successive comings to it, and leavings of it, of carbonic acid; it is organically made, not by the incorporation of any *gas*, nor to last for an instant, but for the whole time of the organic unity of the globular substance; it *must* exist precisely as made, as the genetically-formed permanent *condition* of the light-red change. What can possibly be more plain than this? What can be more plain than that this "change" is a transient disturbance in it?

If the reader supposes it otherwise, if particularly he has no idea that this can be, let him try the experiment—the experiment, namely, of a change which is not transient, which is not a disturbance, as we represent this to be—and make a permanent change. In this case he will infallibly find he has made a *new statu quo* of color for the old dark-red one. He can easily do this by permitting the chemical union of carbonic oxide with the globule; and now he has substituted new organic condition of coloration for the old one; he has substituted for the latter organic condition,

in which a change can take place, another condition of coloring in which *no* change can take place. Of course, in the interests of the carbonic-acid theory, the old theory of change, he will be apt to say to the result of this experiment: "Nonsense! You have fixed its coloring precisely as it was in the act, with oxygen; you have destroyed the globule." But if you had, the fact is of itself the most emphatic testimony of the difference between the organic condition of the coloring of the globule, as it is made a constitutional condition, which is in its character adapted to "change," and the fixed and *static* condition of the same coloration, when, by a permanent addition of substance to it, you have made it a new coloration, and by this act of addition fixed its coloring matter beyond all its primary adaptability to change, you have made another globule out of the old one. But even this experimental experience of yours, which ought as much as any thing to educate us into a perception of the truth, you *cannot* accomplish without the old globule and all its coloration, and more than that, without *first* having in its primary and genetically-made coloration the innate character *for* change. This latter alone is what enables you to make, or is the very *condition* of your making, the permanent change by carbonic oxide. And so if a change is made not permanent, not by carbonic oxide, but by carbonic acid, as you suppose it incessantly to be, it also must take place in the same condition of coloration of the globule, and cannot precede the existence of that, and induce or produce it.

Look at the case now for a moment as if for the first time.

Here is blood bright red; there is blood dark red; they are both the blood of one person. What *makes*

these colors? As we have invariably been taught to regard it, we could only reply: The light color is made by oxygen, the dark by carbonic acid. But, says some one less merely literal than his fellows, How is this? Carbonic acid and oxygen cannot form of themselves colors; and more than that, here is soft solid substance—coloring substance. Ah, yes! but the oxygen and carbonic acid do not form this *substance*, but color it—form the color. Well; but there is no color which is not identified with substance—colored substance—unless it be the impalpable and bodiless rays of the spectrum; and besides this, if these gases are colors, or communicate color, these colors, you are supposing the substance to be otherwise *un*colored; and such substance neither carbonic acid nor oxygen can supply with coloration.

Then he might further inquire: How is it two colors arise in the same bodies? Oh, these are changes of color! But *changes* of color cannot occur, except in what is already colored. As yet there has been not the faintest stir of recognition of any intrinsic color in the case. But, you continue, *One* of these colors is a change in the other. Well; which one? Why, the light red. What has formed the dark red, then? Oh! that also is a change of color in the light red. This is to leave entirely out of the question the existence of any color *in itself*, i. e., which is not a change in another, and which other is not merely also a change. But the very embodiment of any change must be a substance or body in which it occurs.

And this coloring substance must have in one both these colors, as there is no third in existence, and when this coloring substance was made it was not of two distinct kinds.

If there is no *third* color, *not* a change in which these two arise as changes in it, and no distinct *two* coloring substances, one of those must be the coloring substance of the color in which the change arises, and the other must be a "change" in it. As there cannot be two changes, nor two distinct colors, unless one is a change, one of these two colors must be the change, and one the color in which the change arises.

It may be confidently averred that in this very experiment you suppose rather than otherwise that you have *re*colored the globule, imparted or incorporated in it additional coloration or coloring substance; for this is precisely the supposition we entertain of the fancied addition of carbonic acid to the globule: thus supposing that not the old coloring matter changed (permanently) alone exists, but has had that increased by so much as the quantity of carbonic oxide infused. Thus you suppose that a colorless gas, which cannot cease to be that and become colored matter by any incident of this experiment, makes coloring additional to that beforehand in the globule. You doubtless suppose this because you always assumed that carbonic acid adds coloration to the globule, i. e. darkens it.

But what can be more adverse to the truth of the case, and the evident truth, too? But, if you cannot make any thing additional in the way of coloration to the globules by carbonic oxide, you cannot by carbonic acid. The error of the supposition is, however, far the plainest in the latter instance; for, if the additional coloration by carbonic acid be the adding of itself to that of the globule, the abstraction of itself by its leaving the globule, it must be the abstraction of the color which makes the supposed difference be-

tween it and bright red. Hence our attributing the latter to oxygen is supererogatory.

But, for instruction's sake, let us suppose we remove the carbonic oxide. What, then, is the color of the globule? Not the same color; not, of course, bright red, for the oxygen is removed; neither the dark red, if carbonic acid does cause that, because it also has been removed, and is eliminated from the experiment. But does any one doubt, if we accomplished this removal of the carbonic oxide, that *any* color but the dark red would reappear?

But, indeed, the whole truth of the case must by this time be sufficiently obvious.

What simply there is to recognize here, is a coloring substance which, in a certain state of its particles, is seen, when impinged upon by light, to be dark red; but if the particles be disturbed by the active agency of some substance capable of communicating its energies to them, as oxygen, then it makes during its presence amid them such a condition, that the relation to light is changed, and the globules present the altered hue—the light red—we call another color, though it is only the same color *changed*.

We may close this long discussion by a single reference to the character of the fallacy of seeing this dark red as produced. We are so little accustomed to correct the misapprehension the imaginative power ascribes to sense, that a single example may be pointed out here.

We *suppose* we see this dark-red color made. We do *not* see what we suppose; what alone is sensibly discerned is the dark color; *see* this dark red we do, but not at all what makes it, nor how it is made. What we sensibly discern is the dark red precisely as

it is; but as to its making, we *suppose* that, and proceed to attribute *that* to sense. The error lies in the supposition, *not* in the organic fact of sight; the fallacy exists in the mental supposition, and the mental ascription of it to sense.

Accordingly, when carbonic acid is added to the drawn blood, and the oxygen ceases its exclusive connection with the globule, the light color thereby immediately ceasing (not as a *color by itself*, but as the cessation of the change), the dark red supervenes or *re*continues; then, we already assuming it, suppose this to be a change of color by carbonic acid. To see or sensibly discern this dark color to be made (exclusive of our false supposition) would be to see by the eye the carbonic acid or oxygen enter the globule; to assume that we *see* this is of course, as the reader knows, imaginary. We thus see that the error arises in the imaginative power, *which ascribes* to sense as eyesight what the sense does not and cannot report or *see*. The error is not in the organic fact of sight, but in the mind, and here (and not in the field of sense) must it be reversed.

Thus the dark red has no anterior color, nor any other and posterior color dark red, but only a change in its own hue; hence, if the blood never met the oxygen from outside its own channels, it would never be of changed color, but only colored without change. Surely no illustration is needed of this assertion, though we may make one sufficiently striking.

The mass of blood itself is a perfectly-colored *circuit* revolving in certain closed channels; it is by genesis of a distinct, i. e., dark-red color. A certain part of these channels is *skirted* by a deep natural basin.

This open basin is fitted with an atmosphere of gases; between the blood and this atmosphere there intervene the substance of the sides of the basin and that forming the sides of the closed vessels adjacent to it.

A certain portion of the gas leaks through this intervening tissue, and enters the blood; it has a certain effect on the blood passing there; its color, in passing this locality, becomes bright red, while it is always elsewhere dark red. The gas from the basin does not at first "diffuse" or spread itself through the fluid, but only joins the coloring matter of the blood. The moment the gas from the basin comes in contact with them, a change comes over their color; this change is an active phenomenon purely *incidental* to the flow; it lasts but a short time, or only so long as the gas *makes special contact with the red corpuscles.* Meanwhile these bodies which move *in* the blood are passing away from that portion of its vessels adjacent to this basin of gas, carrying along some gas in contact with them, to spread or diffuse it throughout the fluid. This absorbs the gas, breaking its special contact with the globules, which are now of the same color they were before the gas communicated with them, that is to say, they are dark red. The gas-basin is the long cavity.

Let us for a moment suppose the gas to make no excursion into the blood, passing in the channels in its near vicinity, but simply to permeate the intervening tissue, and establish itself within and along the interior surface of vessels against which the blood-bodies pass; and further, to suppose its influence on them strictly confined to that situation as they are passing. The influence is supposed to be done and

over, before the blood has passed from the portion of the vessels involved; thus supposed, it would be of a momentary character, and involve no actual going off with the globules of the gas. But, as in this case the blood would continue on its course without further change, and *be* dark for *want* of it, so in the actual event which differs only in lasting a little longer; it *is* precisely what it *was*—dark red—the only difference between the supposed and the real event being that the oxygen proceeds with the globules until it is lost in the fluid blood.

Now, in this phenomenon a portion of the circuit of blood is bright red, because some portion of the blood always in circulating is passing the location of the gas-basin; but, if we stop the leakage of the gas, or withdraw it from the basin, the phenomenon in the blood is wholly non-existent; and, in reality, shortly after it occurs it is non-existent for that portion of the blood in which it has taken place, so ceasing or becoming non-existent (which is identical) for that portion which, therefore, is a uniform dark red, with all the circuit of blood between it and that situation of the part next the gas-basin.

The bright-red color is a wholly *incidental* phenomenon of change—a change, truly speaking—and is not color which begins and ends, but the *change*.

Most unfortunately, we have never discriminated where it was most needed—namely, discriminated the thing changed from the change, and we do not merely *identify* them as we ought to identify color with coloring matter, but, invariably supposing this *thing*, having incorporated within it the gas, to be the "change" itself, we utterly fail to discriminate the change as a wholly *incidental* or accidental transaction or ex-

perience, and instead thereof are invariably prompted to name the thing itself, i. e., the globular coloring substance, a change.

Hence our invariable habit of naming the dark-red thing or color a change, and of naming the bright red thing or color another change, embodies the prevailing confusion of two changes.

We thus identify what is merely *incidental* to the thing, when it is named, as the thing itself—the dark color is a "change" of color, and the bright-red is a change of color. Then, again, we leave entirely unidentified the colors and coloring substance, and hence suppose the former (and so state it) to be put in and out of one and the same coloring substance at the pleasure of the gases. If we did discriminate, we would identify the one thing which is the one coloring substance identical with color, and one change in that thing as strictly incidental to it, and thus have come into possession of the truth.

Surely, the nature of this globule, its self or selfhood, *it*, as an embodiment of formed coloring substance, is not *a* change—any change, if we discriminate what *it* really is, being merely incidental to this thing.

Syrup of Lime in Rheumatism. [Boston Med. and Surg. Jour., July 2, 1868.]

In a former number of this Journal we noted Dr. Buckinham's treatment of acute rheumatism by the syrup of lime; and Dr. Carl N. Smith, of Kenton, Ohio, now reports fifteen cases treated upon this plan, "using no other remedy," with a cure in each case in from ten to sixteen days. It would be well for the profession to make public the results attained by the use of this agent, and especially to state, what Dr. Smith omits, whether there were any cardiac or other complications in the cases.

Reviews.

Art. I.—*Diseases of Children.* A Clinical Treatise based on Lectures delivered at the Hospital for Sick Children, London. By Thomas Hillier, M. D. Philadelphia: Lindsay & Blakiston. 1868. 12mo, pp. 402.

In his preface the author remarks that it had appeared to him "that, although several valuable manuals on the diseases of childhood were in existence in England, there was room for a book treating clinically of the same subject." The scope being thus defined, a systematic work could not be expected, since the author has confined himself to such cases as most frequently present themselves in a hospital for children past the age of suckling. No particular order of arrangement of the chapters has been followed. The topics treated of are—Pneumonia; Pleurisy; Tuberculosis, with particular reference to its cerebral manifestations; Hydrocephalus, Acute and Chronic; Diphtheria, Scarlatina, and Typhoid Fever; Chorea; Paralysis; Epilepsy, and Convulsions; Rickets; Pyæmia; Ascites, and Skin Diseases.

At the outset are given a few pages of general considerations on the peculiarities of the diseases of children and of their treatment. The whole book is characterized by a brevity of style, often amounting to abruptness, though not carried to the degree of obscuring the meaning. The cases are chosen with the view of illustrating as far as possible all the varieties of the disease under consideration; and the comments upon them often contain valuable hints. The articles upon individual affections generally begin with statistics gathered from the Registrar-General's Reports, as well as from foreign sources, regarding their frequency, mortality, age of greatest prevalence, etc.

In the chapters upon Broncho- and Lobar-Pneumonia, very little that is decidedly original appears, but a fair digest of the researches of others, up to the present time. Among the points noted is an unexplained increase in the mortality in England from bronchitis in children under five years of age.

Figures quoted from Ziemssen show another curious fact, viz., that in about ninety per cent. of cases of lobar pneumonia, the decline in temperature begins on uneven days.

The article upon Pleurisy contains quite a variety of cases; the chief interest of the section is, however, with regard to the indications for paracentesis thoracis. His opinion in regard to the time of the operation is, that it should be resorted to as soon as we feel certain that internal remedies have failed, the danger of empyema (in children) forbidding delay: "On the other hand, in a case of many months' duration, if the patient is not suffering from dyspnœa and hectic, it will be wiser to leave the side unopened, although it is much distended." These positions he defends by cases in point.

The section on Rickets is of less interest in this country, owing to the comparative infrequency of the disease.

The chapters upon Chorea, Pyæmia, and its connection with Otorrhœa, Epilepsy, and Convulsion, and Ascites, are quite brief, some of the articles being little more than comments upon the cases introduced. The remarks upon Skin Diseases are confined to treatment, the author having discussed the subject more completely in his well-known manual.

The articles upon Typhoid Fever and upon Paralysis are more extended and of greater value, the former as assisting toward the recognition of a disease too often mistaken, and the latter because relating to a subject little understood save by specialists.

Tuberculosis in general occupies a short chapter, while tubercle of the brain and its meninges, with acute and chronic hydrocephalus, fill quite a large portion of the whole book.

In the chapter on Diphtheria, Dr. Hillier advocates the theory of the identity of the disease with true croup. He says: "I can detect no distinction between membranous croup and laryngeal diphtheria." Again, "Epidemic croup is always diphtheria." He reviews Mr. Squires's article in Reynolds's "System of Medicine," as well as Dr. Jenner's remarks upon the two diseases:

"The points of distinction insisted on by Dr. Jenner are, that croup is a local disease, not contagious; that it does not

occur as an epidemic; that it does not affect any large proportion of adults; that there is no albumen in the urine, and that there are no symptoms of disordered innervation consequent upon it.

"Let us look at these points seriatim. It appears to me as impossible to maintain that croup is merely a local disease as that pneumonia is merely local, or catarrh, both of which are generally indications of a morbid constitutional state. As to contagion, diphtheria is only observed to be contagious where an epidemic prevails, or when a number of cases are brought together in ill-ventilated hospitals. Croup, as well as diphtheria, has been described as epidemic; sporadic diphtheria is not uncommon. Albumen has been found in the urine of patients with croup; it is only quite recently that it was found in diphtheria. The other two points (its frequent occurrence in adults and its symptoms of disordered innervation) have most weight in establishing a distinction; but, in reference to these, it must be remembered that where diseases become epidemic they are more liable to attack adults, who escape when the disease is only sporadic; and that a certain set of symptoms often prevails in one epidemic, which has been absent in other epidemics of the same disease. This is illustrated in the history of scarlatina, in different epidemics of which, renal complications may be very general, or may be almost unknown. Even when epidemics of diphtheria prevailed in former times, the nervous sequelæ were not noted; we have no record of these phenomena till a comparatively recent period. It is quite probable that even if symptoms of disordered innervation had followed sporadic croup in as large a proportion of cases as they follow epidemic diphtheria, they would not have been connected with the previous illness."

For practical value the chapter on Scarlatina is perhaps the best in the book.

The work, as a whole, will please the reader if he takes it just as it is offered, as a record of clinical experience. He will find it a book that will afford him much information in short compass, and accordingly valuable if he desires to refresh his mind upon certain topics without employing time in going over a number of authorities. One already expert in

children's diseases will fail to find much that is new to him. Those who desire to have one book to tell them the whole of the subject, will be disappointed.

The American reprint is rather careless in proof-reading, but presents a handsome appearance.

Art. II.—*A Hand-book of Vaccination.* By Edward C. Seaton, M. D., Medical Inspector to the Privy Council. Philadelphia: J. B. Lippincott & Co. 1868. 12mo, pp. 383.

This is a reprint of an excellent work. Dr. Seaton has extended his article on Vaccination, contributed to the first volume of Reynolds's "System of Medicine," and written a very thorough text-book on the science and art of vaccination. The subject is exhausted, and yet there is scarcely a line but has practical value. Although a general view of vaccination is given in most systematic works on medicine, no separate treatise has been published in the English language since Bryce's "Practical Observations on the Inoculation of the Cow-Pox," in 1809, now long out of print, and generally forgotten. The success of the operation of vaccination, seemingly so simple in itself, depends on many niceties, the value of which are but too generally unheeded by the practitioner. These are particularly insisted on by the author. The principles on which a Public System of Vaccination should be based, are of interest and importance to the whole profession, and are given with much precision and clearness. In Chapter X. the question of Alleged Degeneration of Lymph, and of Recurrence to the Cow, is discussed, and the opinion given, that lymph will not degenerate if due care be employed, and that the vesicles produced now by Jenner's lymph have the same character and course he described. The fact is mentioned that the lymph chiefly in use throughout England is mainly Jenner's lymph. Dr. Seaton adds: "Ceely, who has more knowledge of the cow-pox in the cow, and the effects of cow-lymph on the human subject, than any other man in England, was requested by the Medical Department of the Privy

Council, in 1862, to inspect all the stations from which lymph was contributed to the Establishment. He met with abundant evidence of the perfectly satisfactory character of the lymph in use." He indorses, from personal observation, the statement made by the National Vaccine Board, in 1854, "that the vaccine-lymph does not lose any of its prophylactic power by a continued transit through successive subjects, and that it is a fallacy to predicate the necessity of resorting to the original source of the cow for a renewed supply."

The Protection which Vaccination affords against Small-Pox is largely and thoroughly examined in Chapter XI. Dr. Seaton writes at the close of the chapter: "The evidence, then, is conclusive, that the vast majority of mankind may, by a single properly-performed vaccination, be rendered wholly unsusceptible of any subsequent action of the variolous poison; and that in the minority, whose susceptibility to that infection has not been entirely exhausted by the vaccine process, the small-pox will, with rare exceptions, be so modified, that if all the population were completely vaccinated, i. e., vaccinated in the best way, serious and spreading small-pox would be but little known among us, and entries of fatal small-pox would be all but banished from our death-registers." We are told that the present practice of revaccination aims not only (1) at repairing whatever was irregular in the course of a primary vaccination, but also (2) at supplying what was imperfect in the *amount* of infection in cases in which the *course* of the disease was regular, and further, (3) at extinguishing the susceptibility to small-pox which may remain, or may rearise, in an indeterminate number of persons whose primary vaccination may have been complete as well as regular. It should be done on all persons after puberty; not only where the primary vaccination has fallen short of the highest standard, but because it seems certain that, at or after this period of life, it may give additional security to many whose original vaccination has been complete, but in whom the susceptibility to variola has not been exhausted, and who may contract the disease after growing up. Fifteen years of age is the best time, for the age of most danger from post-vaccinal small-pox is from fifteen to twenty-five; and it should not be left to periods

when small-pox is epidemic. These views do not stand on any speculative reasoning, but upon broad grounds of observation and experience. The ability to stamp-out local outbreaks of the disease is incontrovertibly shown, and the opinion expressed, that "the spread of fatal small-pox, let it occur when it may, must be taken as in itself a sufficient indication of the apathy, indifference, or incompetence of the local authority within whose jurisdiction it takes place."

The final chapter, of the Objections to Vaccination, and the alleged Dangers of the Practice, is particularly clear, able, and convincing, and we especially recommend to such as may have had their notions befogged by reading the discussions which took place in the French Academy of Medicine in 1864. The admirable chapter on the Alleged Drawbacks from the Advantages of Vaccination, by Mr. Simon, in his Preface to "Papers relating to the History and Practice of Vaccination" (1856), has been largely utilized, and its facts and conclusions put within the reach of all. We have only space to quote Dr. Seaton's practical and common-sense view of alleged Syphilitic Invaccination:

"Suppose, however, that there existed risks of vaccino-syphilitic inoculation greater than I can allow to have been shown, and that the occurrence is one that, under some unknown condition, *might* take place in a properly-performed and careful vaccination; what, after all, looking at the matter in a plain, practical, common-sense way, do these risks amount to? Suppose we assume that in all the cases that have been advanced, except those in which, on the very face of the story, there was manifest malperformance, the vaccination had been properly done, what proportion do these bear to the millions upon millions of vaccinations that have been performed? If men who have been all their lives professional vaccinators, as Marson, or Leese, or Tomkins; if men whose professional lives have been spent in the midst of syphilis, as Lee, or Acton, or Langston Parker, have met with no cases in which syphilis originated in this way, the conditions under which the occurrence can take place must be indeed of rare exceptionality. During the eight years in which there has been systematic inspection of public vaccination in England, some millions of vaccinations have been performed; but the inspectors have no knowledge of any such accident having occurred in any one instance . . . . The danger, *if indeed there be any at all*, of communicating *through vaccine-lymph, as in an ordinary well-performed vaccination*, any other infection than its own, must be so infinitesimally small, that, for all practical purposes, we may regard it as non-existent" (p. 374).

ART. III.—*Conservative Surgery in its General and Successful Adaptation in Cases of Severe Traumatic Injuries of the Limbs, with a Report of Cases.* By ALBERT G. WALTER, M. D. Pittsburg: 1868. 8vo, pp. 213.

IN a paper communicated to the Académie des Sciences some eighteen months since, its author, the eminent French surgeon, Dr. Maisonneuve, starts with the proposition, that, out of 100 deaths after surgical operations, 95 of them are the results of poisoning by the septic properties acquired by the blood and other fluids of the body when they are exposed to the air. As we are more or less able to hinder the absorption of these putrid elements, in that degree will the surgeon be successful in the treatment of open wounds, whether the effects of accident or following operations. The first object, then, is to prevent the contact of atmospheric air—the putrefying factor—with the wounded surface; if this can be effectual, then the greatest surgical operations will be done without risk. The surgeon of the Hôtel Dieu is a strong advocate, therefore, for subcutaneous incisions, when practicable, and, in severe injuries of the extremities and after amputations, he believes that a simple and practical means has been discovered by "the method of continuous aspiration." His apparatus consists of a cap of caoutchouc, placed over the wound or stump, and connected with an air-pump, by which all the air in the bag is exhausted, any collection of fluids stopped, and their putrefaction arrested.

For thirty years Dr. Jules Guérin's hobby has been the dangers from the "oxidation of wounds." All the perils following wounds, he has zealously and perseveringly contended, are due to the evil influence of the oxygen of the atmosphere upon the effused fluids about them. If they can be protected from the poisonous action of the air, and the ills of suppuration avoided, all will go well. If the wound is an open one, involving the skin and subjacent parts, place it as far as possible in the condition akin to a subcutaneous one—hence his "Treatment of Exposed Wounds by Pneumatic Occlusion." He uses an apparatus like that of Dr. Maisonneuve: vulcanized india-rubber envelopes, of various forms

and sizes, and adapted to every part of the body, are applied over the wound, and introduced into a metallic receiver, which is kept constantly exhausted.

Mr. Lister and Mr. Syme, believers in Pasteur's theory, and attributing the decomposition of the fluids in and about wounds to the organic germs floating in the air, are as strenuous advocates for the exclusion of the atmosphere as either of the French surgeons, opposing their antiseptic, or carbolic, occlusion to the organic air-dust. Mr. Lister writes: "The essential cause of suppuration in wounds is decomposition, brought about by the influence of the atmosphere upon blood or serum retained within them, and, in the case of contused wounds, upon portions of tissue destroyed by the violence of the injury. . . . To prevent the occurrence of suppuration, with all its attendant risks, was an object manifestly desirable, but, till lately, apparently unattainable, since it seemed hopeless to attempt to exclude oxygen, which was universally regarded as the agent by which putrefaction was effected." (*The Lancet*, 1867, and this *Journal*, vol. vi., p. 265.) Dr. Bohm, of Vienna, lately stated that he believed the good effects of this method to be mainly, if not entirely, due to occlusion, for he had got the same results by using chalk-soil without carbolic acid.

Another Frenchman, M. Bouisson, of Montpellier, having a wholesome awe of the effect of the atmosphere on wounded surfaces, proposes to cover them with a protective film, produced by means of a current of air blown upon the exposed flesh by a common parlor bellows. (*N. Y. Med. Jour.*, vol. iii., p. 152.) These apprehensions of ill, from the disorganizing action of air upon the fluids about the damaged surfaces, are widely spread amongst the profession.

Our author, Dr. Walter, does not, however, share this dread of oxygen, or fear the septic property of the atmosphere, or of the organic air-dust, upon what he calls "the extravasata and secreta" of wounds. He believes, on the contrary, that pure air, "like the rain-drops descending from the great laboratory of heaven, the air—sweet, pure, and healthful," is as "refreshing, invigorating, and gladdening" to fresh wounds, as it is "to all the inhabitants of this beautiful globe. Its

free and unrestrained access to wounds and injuries, then, cannot be the cause of those direful complications—erysipelas, phlegmon, gangrene, tetanus, phlebitis or pyæmia—which are known to ensue in so many cases" (p. 200). He admits, however, that poison is mingled with the air in hospitals, and that "the only antidote is the prompt removal of the patient to a place free from all contaminating influence;" advice very sound in its way, we admit, but not always capable of being carried out.

We come now to the details of the "principle and practice," which, after "years of study and toil" and "humble, laborious, and protracted exertions," have enabled the author to "*add one more laurel to the brow of Conservatism*—bright, enduring, and priceless as any she wears," and "by which a whole *class of injuries* is rescued from mutilation, danger, and death" [*sic*]. These we shall give in as nearly the words of Dr. Walter as possible. In the treatment of injured limbs, with main arterial and nervous trunks intact, although not hopelessly mangled, such as are produced by the wheels of railroad-cars or heavy machinery, by which the bones are fractured and comminuted and the soft tissues lacerated or crushed, it is of the utmost importance, in these cases, if conservatism be attempted, that first of all *free vent* be given, by *long and deep incisions*, in the axis of the limb, for the escape of the effused blood, wherever it may be, and that all attempts to bring the soft parts together, when lacerated or cut, by stitches, be *strictly and absolutely discarded.* "*Free*, *deep*, and *early* incisions (the more timely made the better) are, I aver, the *only* measures, deserving of the name *conservative*, in injuries of this character" (p. 11). By these means, and "under the genial influence of *warm* aromatic *poultices* or *fomentations*, assisted by general and local [?] supporting measures, which diet and medication can afford to the system," our author submits that unequal success will follow the treatment of this class of injuries, and is "forced to the broad, yet truthful conclusion, that only a *very small* number of severely-injured limbs ought to be excluded from its blessings" (p. 14).

Dr. Walter then proceeds to give the evidence which is "to establish beyond the shadow of a doubt the rationality,

correctness, and success of a practice, novel, yet preëminently sound," by publishing a number of cases—how many, we cannot say, for we have not had the patience to count them, and there is neither contents nor index to the book; but they occupy one hundred and eighty-two pages out of two hundred and thirteen. These cases are fully and well given, and deserve careful consideration on the part of the practical surgeon.

Were the context a little less aromatic, and the author less enthusiastic and iterative, the work would stand a better chance of careful consideration by the profession; even by such of it as may not be inclined to admit, without further evidence, all that is now claimed by its parent for this "eminently conservative and highly successful" practice.

Art. IV.—*Criminal Abortion; its Nature, its Evidence, and its Law.* By Horatio R. Storer, M. D., LL. B., &c., &c., and Franklin Fiske Heard. Boston: Little, Brown & Co. 1868. 8vo, pp. 215.

For nine years Dr. Storer has devoted a good part of his energies to the subject of Criminal Abortion, "which has culminated, he has reason to believe, in an agitation which is now shaking society throughout our country to its very centre" (p. 2). Be this so or not, by the steady pursuit of his object, and his unflagging enthusiasm, he has fairly earned the brevet of an "especialist." (Good words, like evil deeds, will sometimes return to plague the inventor.) Had not some of the author's able and fearless writings on the matter come in rather questionable shape, we should have no mind to abate a jot of the praise he is disposed, from the pride of honest purpose and measurable success, to give himself. He writes:

"Eight years have passed. Not only has the medical profession been stirred to progressive action, but the outside community. Paper after paper on the subject has been issued by medical men; the religious press has become deeply interested; political economists have found, *as has been indicated in*

a previous edition of this book, an explication of otherwise inexplicable problems; an impetus of the most powerful character has been given to the movement by the publication of the *author's two books* . . . . The importance of the subject is rapidly becoming recognized by the legal as well as by the medical profession; and extracts from the *author's writings* upon the subject . . . have already affected the rulings of the courts" (p. 2). [The *italics* are our own.] Like Coriolanus, Dr. Storer may well exclaim, "Alone I did it." If he has writ his annals true, society owes him a deep debt, and may he long hear the still small voice of its gratitude.

> "To thee
> Our gratulations flow in streams unbounded."

Nor should we be too nice in measuring the real value of the services so modestly claimed, remembering that—

> "To observations which ourselves we make,
> We grow more partial for the observer's sake."

We now and then meet with a book of excellent matter, but "in the phrase which might indict the author of affectation," and which more or less mars its usefulness. Others remind us of the Great Frederick's aversion to Diderot's writings, which, "tout intrépide lecteur" that he was, he could never stomach. He wrote: "Diderot rabâche toujours la même chose; il y règne un ton suffisant et une arrogance qui révolte l'instinct de ma liberté."

The authors express the hope that this work "may secure the approval of the two great bodies of professional men, for whom, as a manual, it was written;" and which, we may add, it deservedly merits.

It is divided into two parts. Book I. considers the subject from the stand-point of medicine, and is by Dr. Storer. Aside from certain idiosyncrasies of manner and style, it is well written; all that can be said is fully and well said. Dr. Storer contends that public opinion, both in theory and practice, fails to recognize the crime, and that it essentially denies the true character of the offence. Of its great and increasing frequency in this country, convincing proof is given. "The crime is fast becoming, if it has not already become, an es-

tablished custom . . . . What are the causes of this general turpitude? (1) The low morale of the community as regards the guilt of the crime. (2) The doctrines of political economists. (3) The fear of child-bed. (4) The ease with which the character of the crime may, in individual cases, be concealed. (5) The unwillingness of its victims to give testimony that would also criminate themselves. (6) The possibility of their inducing abortion on themselves without aid. (7) The ease with which the laws, as at present standing, may be evaded. (8) The lack or inefficacy of judicial preventives; such as statutes for registration, and those against concealment of birth and secret burials. (9) The prevalent ignorance of the true principles of its jurisprudence, in both government officials and medical witnesses. (10) Social extravagance and dissipation.

Dr. Storer concludes his portion of the work by examining whether, and in what manner, the difficulties in the way of suppressing the crime of abortion can be overcome. To the question if it can be at all controlled by law, he does not hesitate to give an unqualified answer in the affirmative. "It is evident," he says, "that, in aiming to suppress this crime, the law should provide not merely for its punishment, but indirectly as well as directly, and, so far as possible, for its prevention . . . and this is to be accomplished by a twofold process: by rendering, on the one hand, its detection more probable, and, on the other, its punishment more certain" (p. 141). To effect these ends, the importance of strict registration laws, and laws against concealment of births and secret burials, is pointed out. The establishment of foundling-hospitals by the State governments, it is believed, will prove one preventive of the crime. A control of the public press, so far as the present system of openly advertising by abortionists is permitted, is urged. And, finally, less technicality and more directness in the statutes themselves.

Book II., by Mr. Franklin F. Heard, views the subject from the stand-point of law, which is thoroughly, clearly, yet concisely, treated of.

Bibliographical and Literary Notes.

Dr. Wilder, who is well known for his valuable studies in morphology and comparative anatomy, here[1] gives a most interesting paper, the object of which is to impress upon others the value of every fact relating to this class of cases (polydactylism), to suggest a general method of recording such cases, and to indicate, in a general way, the direction which we may expect will be taken by the final results of a much larger number of cases than are now recorded.

In this paper Dr. Wilder confines himself to what is called sexdigitism, or the presence of a single supernumerary finger or toe. Of such cases he has collected, from various sources, one hundred and fifty-two instances, and from an analysis of these he has deduced some most interesting conclusions. These studies at the present time, when the scientific world is flooded with theories as to the nature, causes, and significance of the variation of organized beings, have a peculiar interest; and now, of all times, is it necessary to bring in careful observation and large collections of facts, by which we may either test these existing theories, or upon which we may form more rational conclusions.

To aid in this important study, Dr. W. has prepared a blank form for the record of cases, and calls upon the profession to aid him in the collection of facts. Copies of this blank may be procured of Dr. Wilder, by addressing him at his residence, *Cornell University*, *Ithaca*, *New York*. We hope our readers will respond cheerfully to this request, and thereby assist in the prosecution of a most instructive line of study.

In the last number of the *Journal* we published a sharp notice of a pamphlet by Dr. Blackman, of Cincinnati, said pamphlet being mainly a charge of plagiarism against Dr. Bartholow, of the same place. The whole tenor and purport

[1] Extra Digits. By Burt G. Wilder, M. D. Extracted from the Publications of the Massachusetts Medical Society. Vol. ii., No. 3, 1868; pamphlet, pp. 22.

of that notice was directed against the style and manner of the controversy between these two gentlemen, and was equally severe on both. Dr. Bartholow, however, thinks we have done him an injustice in the matter of the charge of the plagiarism. We therefore cheerfully insert the following circular, which he has been kind enough to send us, and which, we are pleased to learn, has been accepted, by all parties concerned, as a final adjustment of this unpleasant and unprofessional controversy:

PROF. R. BARTHOLOW, M. D.

DEAR SIR: I understand that you are about to publish a pamphlet in reply to one issued by Prof. Blackman. The offensive personalities which have characterized this discussion are unprofessional in themselves, and injurious to the Medical College of Ohio, in which you are both Professors.

I have therefore to request that you withhold the publication of your pamphlet until a Committee of the Faculty, consisting of Profs. Graham, Comegys, and myself, shall have determined in what way this controversy shall terminate. Very truly,

M. B. WRIGHT,
Dean Fac. Med. Col. Ohio.

The Committee of the Faculty, consisting of the undersigned, adjudge that the controversy between Profs. Blackman and Bartholow shall terminate, by a reply from Dr. Bartholow to Dr. Blackman's pamphlet—said reply to be free from personalities.

This reply is subjoined, and is considered by the Committee to be final. The Committee have the pleasure to state, that both parties have agreed to this conclusion of a very unfortunate public controversy.

M. B. WRIGHT.
JAMES GRAHAM,
C. G. COMEGYS.

The Faculty of the Medical College of Ohio have interposed to terminate the public controversy now going on between Dr. Blackman and myself.

My colleagues entertain the conviction, as they inform me, that a controversy, which has been inaugurated and conducted throughout in a personal manner, is not only unprofessional but injurious to the Institution with which we are connected.

Feeling the force of these considerations thus urged upon me, I have consented to withhold the publication of a pamphlet now printed, which was written in reply to a pamphlet issued by Dr. Blackman. According to the rules of controversy, I am entitled to a reply. I am content, under the circumstances, to state the points of my argument as printed in my pamphlet, in respect to the only material accusation contained in Dr. Blackman's pamphlet—the charge of plagiarism.

My article on "Progressive Locomotor Ataxia" must be examined as a whole. It consists of three parts: the case of Kelch; a synopsis of the

history and symptomatology, as contained in Topinard's essay; and a discussion of the views of Trousseau, Meryon, Hughlings, Jackson, etc., and an examination of the original meaning of the term *tabes dorsalis*, as it occurs in the works of Hippocrates and Celsus. It appeared in the numbers of the *Cincinnati Journal of Medicine* for April, May, and June, 1866. In the beginning of the article, in a foot-note, I expressed my obligations to Topinard in the following language:

"I have made liberal use of the prize-essay by Dr. Paul Topinard, *De l'Ataxie Locomotrice, et en particulier, de la Maladie appellée Ataxie Locomotrice Progressive*, Paris, 1864."

The second part of the article appeared in the May number of the Journal, Drs. Blackman and Parvin being the editors. The manuscript of this part of my article had appended to it a foot-note in terms similar, and in meaning identical, with the following:

"This is a synopsis, chiefly, of the views of Topinard, *De l'Ataxie*, etc."

This acknowledgment does not appear attached to my article. The following statement from Mr. H. T. Ogden, of E. Morgan & Co., who printed the Journal, will furnish information as to the disposition made of the note attached to the second part of my article.

"The manuscript of Dr. Bartholow's second part of the article, *Locomotor Ataxia*, was in hand at the time of the appearance of the *Medical Journal*. There was a short note attached to it,* I think, to which I called Dr. Blackman's attention after he had read the article in the first number (April). On reading this he (Dr. B.) thought it not satisfactory, and appended some explanation to the same. This I showed to Dr. Bartholow, who declined to permit the addition. I am clearly of opinion that the article, with the note appended, was in hand before any thing had been said of plagiarism; Dr. Blackman had not yet read the article, when it (the manuscript) was placed in my hands by Dr. Bartholow.

"Respectfully,
(Signed) "H. T. OGDEN.

* The note, if I recollect correctly, Dr. Blackman tore up in my presence."

Mr. Robert Clarke, the publisher, says that Dr. Parvin carried off the note appended to the second part of my article. Dr. Parvin, in answer to my request to be furnished with this note, writes as follows:

"Mr. Clarke is mistaken in his statement that I carried off from the printing-office a note appended to any of your contributions to the *Cincinnati Journal*. I have never had such note in my possession, nor do I remember ever to have seen it. You will thus see that it is impossible for me 'to give you the note' or 'to state its contents.'

"Yours truly,
(Signed) "THEOPHILUS PARVIN."

I now come, in chronological order, to the letter of Mr. Robert Clarke, publisher. This letter explains itself.

"You ask me to state the circumstances under which you wrote the note attached to your second paper on ataxia in the May (1866) number of the *Cincinnati Journal of Medicine*.

"The charge of plagiarism made against you by Dr. Blackman on the appearance of your first article with the 'liberal use' note, created considerable talk in the circle of physicians, who took an active interest in the journal. I spoke to you, as did also, I think, Dr. Parvin and others, and you promised to have an explanatory note in the next number. You took the 'copy,' with your note attached, to the printing-office yourself. I did not see it. Dr. Blackman, however, saw it there, thought it unsatisfactory, and wrote an 'apology' over his own initials, to go among the 'miscellanies' of that number. The foreman had it set up, and sent a proof to me, asking if it should go in. It was about fifteen lines long; it was very personal, in very bad taste, and evidently written under excitement and on the spur of the moment. I sent word to Mr. Ogden, the foreman, not to insert it. I wrote a note to Dr. Blackman, informing him that I could not allow his note to go in, but would use my influence to obtain a satisfactory one from you. I then addressed a note to you, requesting an interview. You called at the store immediately, and I told you how the matter stood. We sent up to the office for your first note; it could not be found. You then sat down and wrote the note as it at present stands; I sent it up and had it inserted. Dr. Blackman did not see it till after the sheet was worked off. He wanted the sheet suppressed, or another note inserted from himself, to which I would not consent, as I considered your note quite sufficient to cover the ground.

"The above are the circumstances, as I recollect them. Mr. Ogden, to whom I have read the above, says they agree with his own recollections.

"Yours truly,

(Signed) "ROBERT CLARKE."

The second part of my article, then, has attached to it the note I wrote at Mr. Clarke's suggestion, and not the note appended to the original manuscript, which was destroyed at the printing-office.

It is thus apparent, I think, that I sought to give the author, from whom I quoted, full credit. But the question now comes—Did I, or did I not, make a synopsis of the author quoted? I affirm that I did. Now for the proof of this statement:

Topinard's essay contains 575 pages. The history in the essay is found from p. 135 to p. 142 inclusive. The leading facts of this, nearly in the author's language, is contained in my article in two pages. The symptomatology in Topinard's essay is comprised in 130 pages—from p. 143 to p. 273. I have stated the principal points in the symptomatology in 10 pages, chiefly in the author's own language.

It is perfectly evident, therefore, that the utmost that may be said is—I made a synopsis of a portion of Topinard's essay, and in the language of the author.

In addition to the direct acknowledgments of the two notes appended to my article, are there any internal evidences of the liberal use I have made of the author? There are many.

1st. I describe *one* case of this disease.

2d. The symptomatology of this case is contrasted—by a special reference, too—with the symptomatology as described by Topinard, after an analysis of 254 cases.

3d. The difference in style, manner, and matter, and the references to authorities not at all, or not readily procurable in this country.

4th. The use of the term "our author," as in the following paragraph:

"*Our author* strongly objects to the so-called *acute form* of this disease, in which the total duration of it is limited to some months. The age of the subject (referring to one of those cases reported by M. Burguignon), the rapid succession of the accidents, the total duration of four months and a half, the prompt and radical cure, are all in contradiction with the mass of our observations."—*Cincinnati Journal of Medicine*, May, 1866.

Expressing at the outset the liberal use I intended making of the author; informing the reader, in the second part, that what he was reading was a synopsis of the views of Topinard, and not concealing the internal evidences of the liberal use I had made of the author, I think any unprejudiced person must agree with me, that evidence is wanting of a design to mislead the reader. If I intended perpetrating a plagiarism, certainly all who knew me must do my understanding the poor credit to believe that I could have executed it more skilfully.

ROBERTS BARTHOLOW.

In his Manual on Extracting Teeth,[1] Dr. Robertson has given us an excellent and thoroughly practical little book—one that should be in the hands of every dentist. If its precepts were generally studied and applied, there can be no doubt that the aggregate of suffering, resulting from diseases of the teeth, would be greatly lessened. The rules for deciding when a tooth requires extraction, the chapter on the selection and proper shape of instruments, and that on the accidents liable to occur in extraction, are particularly worthy of attention.

Who discovered Anæsthesia is a question that has elicited no small amount of acrimonious controversy, both in this country and abroad, and the recent death of Dr. Morton, one of the claimants to the honor, has given a renewed impulse to the discussion of the question. In the little pamphlet before us,[2] by Mr. Shaw, a dentist of Manchester, England, but we

[1] A Manual on Extracting Teeth. By Abraham Robertson, M. D., D. D. S. Second edition, illustrated. Philadelphia, Lindsay & Blakiston, 1868; 12mo, pp. 200.

[2] Who discovered Anæsthesia? By S. Parsons Shaw, author of "Odontologia: its Causes, Prevention, and Cure," etc. Pamphlet, pp. 7. London: Trübner & Co., 1868.

believe an American by birth and education, the credit is given to the late Dr. Horace Wells, of Hartford, Conn. This is the view that we have always taken of the case, and we are glad to see so candid and unbiassed an opinion coming from a foreign source. The author was personally familiar with the history of the use of ether and nitrous oxide, and had, besides, an acquaintance with both Drs. Wells and Morton. Due credit is given to Dr. Jackson for the part he took in introducing ether to the notice of the profession.

Dr. Elliott Coues, Surgeon U. S. A., is engaged upon a work on the ornithology of Arizona, of which he printed what he called the *Prodrome* about two years ago, in *The Proceedings of the Philadelphia Academy of Natural Sciences.* Although a good deal of difficulty attended the author's observations—"my operations," he says, "were conducted at the most imminent personal hazard, from the continued presence of hostile Indians"—he still had very unusual advantages. For the foundation of his investigations he had the collections of the government expeditions along the 35th and 32d parallels of the Mexican Boundary Survey of the Colorado River, besides those of several private individuals. The region about Fort Whipple, his headquarters, is remarkably adapted to ornithological observations, so that the list which composed the bulk of the original paper—including 245 titles, all of birds which had been actually detected in the Territory, and many of which are followed by elaborate descriptions—was actually, what it purported to be, "an exposition of the present state of our knowledge of the Arizonian Ornis." In general, however, the natural history of the species was not enlarged upon—Dr. Coues reserving for his more extended work "the mass of omitted biographical notes" he had accumulated.—*Round Table.*

Since the publication of the first edition of this address[1] in

[1] *Consumption in New England and Elsewhere; or, Soil-Moisture one of its Chief Causes. Address delivered before the Massachusetts Medical Society.* By Henry I. Bowditch, M. D. Second edition. Boston, 1868. 12mo, pp. 154.

1862, in which Dr. Bowditch took the ground that consumption, at least in New England, was not equally diffused, and that certain spots owed their comparative immunity to the dryness of the soil, whilst the "consumption-breeding districts" were characterized by dampness of the soil, he has obtained, from various sources, facts and statistics confirmatory of his views, that soil-moisture is one chief cause of phthisis.

In 1865–'66, Dr. Buchanan having been directed by the Medical Officer of the Privy Council, Dr. John Simon, "to investigate the effect of drainage works, and other sanitary regulations, designed to promote the public health," made such a report of facts in connection with the etiology of consumption, as to indicate a partial dependence of pulmonary phthisis on certain unwholesome conditions, and to suggest the important conclusion that the drying of the soil, which has in most cases accompanied the laying of main sewers in the improved towns, has led to the diminution, more or less considerable, of phthisis. "The facts," adds Mr. Simon, "which are yet in evidence, seem most strongly to support this conclusion, which, should it be substantiated, will constitute a very valuable discovery"—which is, that by the artificial drying of the soil of towns the mortality-rates from phthisis are lessened. The Privy Council has directed that further inquiry on this subject should be made.

This is a reprint of a useful little book,[1] which has gone through three editions in England since 1863. The author writes: "An endeavor has been made to take a rational view of a very common and very troublesome ailment. No pretensions have been put forward to any great originality or novelty. The intention throughout has been to bring together every method of treatment, however apparently trivial; to neglect no source of therapeutic expediency, however subject to abuse, if impartial experience has proved it to be conditionally valuable." The great principle of successful treatment,

[1] *Constipated Bowels: the Various Causes and the Different Means of Cure.* By S. B. Birch, M. D., M. R. C. P., London, etc., etc. From the third London edition. Philadelphia: Lindsay and Blakiston, 1868. 12mo, pp. 181.

viz., the management of *each case on its own merits*, in connection with its *special cause* and any special complications, has been steadily maintained as far as possible. Routine in every shape has been condemned. The abuse of aperients, that most common and most injurious of all one-sided errors in treating constipation, has unsparingly received its deserts; while, at the same time, the *occasional* necessity of a *temporary* recourse to gentle evacuants, and still more rarely energetic cathartics, has been fully admitted.

"The lamentable habit of resorting to opening medicine on every slight emergency, has been dwelt upon as indisputably the most flagrant cause of obstinate bowels, while the variety of other agencies, which tend to implant a constipated habit, have received their share of attention, in their twofold capacity as single and as associate cause."

The suggestions regarding the management of this common and troublesome disorder, with its attending ills, are, in the main, judicious, based on common-sense, and a rational system of treatment.

New Books.—Among recent publications we notice, from the house of Longmans & Co., London, Dr. Charles Murchison's "*Clinical Lectures on Diseases of the Liver, Jaundice, and Abdominal Dropsy.*" These lectures have appeared in serial form in the *Lancet*, and are a very valuable addition to our medical literature. The same volume appears here, with the imprint of Wm. Wood & Co.

Messrs. Simpkin, Marshall & Co. have published a work by Mr. Dunlop Durham, entitled "*The Philosophy of the Bath, or Air and Water in Health and Disease; containing a History of Therapeutics and of the Hot-Water Bath, from the Earliest Ages. With an Introductory Chapter, Illustrative of the Present Condition of the Medical Profession.*" If the book is as heavy as its title, we fear it will not meet with a very lively reception.

From the press of John Churchill & Co. we have the "*Lettsomian Lecture*" for 1867, by Mr. John Guy, "*On Varicose Disease of the Lower Extremities and its Allied Disorders, Skin Discoloration, Induration, and Ulcer.*"

Holden's "*Manual of Dissection*" has passed to a third edition in England.

Dr. V. Audhoui has just issued, in French, a work on "*The General Pathology of Poisoning by Alcohol.*" Dr. H. Pernot a new work, entitled "*Etudes sur les Accidents produits par les Piqures Anatomiques.*"

"*Ponction Vesicale Hypogastrique*," par M. le Dr. J. Pouliot.

Lindsay & Blakiston, of Philadelphia, have in press, and will shortly issue, "*A History of the Medical Department of the University of Pennsylvania from its Foundation in* 1765 *to the Present Time, illustrated by Sketches of the Lives of Deceased Professors*," by Joseph Carson, M. D., Professor of Materia Medica and Pharmacy in that institution.—The second volume of Aitken's "*Practice*" will, it is understood, be ready early this month. They will also publish, during this month, Dr. Sœlberg Wells's complete work on "*Diseases of the Eye, with Illustrations*," and Dr. Beale's new volume on "*Kidney Diseases, with Illustrations.*"

They have also issued a copious catalogue of their own medical publications, to which they have appended a classified list of all the more important works on medicine and the collateral sciences published in the United States, and now in print.

They also announce Beasley's "*Druggist's Receipt-Book.*" Seventh revised edition.—Richardson's "*Mechanical Dentistry.*" A new enlarged edition.—Trousseau's "*Clinical Medicine*," vol. 2.—Pennsylvania "*Hospital Reports*," vol. 2, for 1869.—Holmes's "*Surgical Diseases of Children.*" 2d edition.—Beale on "*Diseases of the Liver.*" New edition.

Messrs. J. B. Lippincott & Co. will shortly issue "*Advice to a Mother on the Management of her Children.*"—Dr. E. C. Seaton's "*Hand-book of Vaccination*" has just appeared from their press.

Messrs. Wm. Wood & Co. announce: "*Troltsch on the Diseases, &c., of the Ear.*" Translated and edited by D. B. St. John Roosa, M. D. New edition, illustrated.—"*A Practical Treatise on Fistulæ of the Rectum and Anus.*" By Dr. W. Bodenhamer. Illustrated.—"*Bright's Disease of the*

Kidneys." By T. G. Stewart, M. D. Illustrated.—On "*Epilepsy.*" By M. Gonzales Echeverria, M. D. Illustrated. —Hassall's "*Illustrations of Microscopic Anatomy.*" With colored plates.

Messrs. W. A. Townsend & Adams announce "*A Treatise on Ozone.*" By Wm. Elmer, M. D.—"*Woman's Complete Guide to Health.*" By M. E. Dirix, M. D.—"*A Practical Treatise on the Trunkal Muscles.*" By E. P. Banning, M. D.

In Veterinary Medicine Messrs. Claxton, Remsen & Haffelfinger, of Philadelphia, have in press, and will shortly publish, "*A Treatise on the Horse's Feet; their Diseases and how to Treat them.*" By Dr. L. A. Braley, Chief Veterinary Surgeon U. S. A. With sixty-five engravings, illustrating the horse's foot in all its points, as well as the diseases to which it is incident. 440 pp., 8vo.—"*The Diseases of Sheep Explained and Described, with the Proper Remedies to Prevent and Cure the same. With an Essay on Cattle Epidemics; especially dedicated to the use of Farmers, Sheep-owners, etc.*" By Henry Clok, V. S., graduate of the Royal College at Berlin, Prussia, and late Veterinary Surgeon-in-Chief of the U. S. A. 12mo, cloth. Illustrated.

Reports on the Progress of Medicine.

THEORY AND PRACTICE.

1.—*Cases of Unilateral Sweating of the Head.* [Cincinnati Lancet and Observer, Aug., 1868.]

In this Journal for August, 1866, we noted several cases of this curious affection, which were reported in the Med. Times and Gazette, and now call attention to the following, reported by Prof. Bartholow:

Case I.—The first case was that of a cachectic individual, who had a tumor, aneurismal or malignant, at the base of the neck, which had grown upward from within the thorax. Not having had an opportunity of examining this morbid growth, I cannot pronounce as to its nature; neither is the determination of this point material to the inquiry. The facts of importance are, 1st, the existence of a thoracic tumor on the right side of the thorax; 2d, the occurrence of unilateral sweating on the right side of the head. The sweating, which was profuse, terminated abruptly at the

median line. The pupil on the same side was contracted. No thermometric observations were made upon the temperature of the affected side; but there was considerable redness of the lobe of the right ear, and a subjective sensation of warmth in the affected parts.

Case II.—A gentleman of Cincinnati, aged about 50, in robust health. About a year ago he first observed that the right side of his head sweated very freely, while the left side was almost free from perspiration. This difference in the activity of the sudoriparous glands on the two sides became very marked indeed, so that he experienced great apprehension as to the result, although his general health continued good. When he consulted me, I explored the thoracic organs very carefully, especially the heart and great vessels, but I was unable to discover a tumor or lesion of any kind. The pupils were equal in diameter on the two sides, and there was no apparent alteration of the circulation in the right eye. Mental emotion or active exercise caused the sweat to break out on the affected side, where it stood in enormous drops, almost no moisture being perceptible on the opposite side. A subjective sensation of heat preceded the outbreak of sweat, but I could not perceive that there was a real elevation of temperature. Neither the direct nor induction currents changed the condition of the parts. Sensation—of touch, of pain, of temperature—remained unaffected over the whole sweating region. There was no apparent disturbance in the function of any organ.

Case III.[1]—S. M——, aged 39; nativity, Ireland; occupation, laborer. Two years ago was attacked with a severe cold, since which time cough has been always troublesome. Has suffered once or twice from œdema of inferior extremities.

Condition on Admission.—He is emaciated, feeble, and wears a cachectic aspect; tongue almost clean; pulse 90 and full; appetite poor; bowels rather constipated; slight depression in right infraclavicular region with marked dulness and much resistance on percussion; prolonged expiration in above-mentioned site, and abundant mucous and sibilant râles over both lungs; rhythm of heart's action normal, as also the area of precordial dulness. Careful examination of the great vessels detects no evidence of aneurism or tumor. Hepatic and splenic dulness natural; there appears to be no particular fault in the digestive organs, except the want of appetite, and the tendency to constipation.

The peculiarities for which the case is now reported are the state of the left pupil and sweating of the left side, especially of the head. The left pupil is more contracted than the right, and does not respond so readily to the stimulus of light. When quiet, there is a perceptible difference in the moisture of the right and left sides of the body. On taking active exercise the sweat stands in large drops on the left side of the face and head, the right being comparatively dry. The temperature of the right meatus auditorius is found to be 99¼ degrees F., whilst the left is 99 degrees F.; of the right axilla 99 degrees F., of the left 98⅝ degrees F. After active exercise the temperature of right meatus is 98½ degrees F. and of left 98½ degrees F.; of right axilla 98½ degrees F., and of left 99 degrees F.

Dr. B. comments at length on these cases, and collates, in addition to those we have above referred to, several others. All these cases are associated with certain conditions: 1. With aneurismal or other thoracic tumor; 2. With certain neuroses, as epilepsy, progressive locomotor ataxia, etc.; 3. With no

[1] Reported by Dr. A. Guthrie, Resident Physician, Cincinnati Hospital.

ascertainable lesion or alteration in the function of any part, except the skin affected. The explanation of the first class is easy, as the experiments of Cl. Bernard upon the sympathetic paved the way for a right understanding of them. Dr. Gairdner seems to have been the first to recognize the connection of unilateral sweating with thoracic tumors, and to attribute the symptom to the pressure of the tumor upon the cervical sympathetic, or its branches, thus inducing paralysis of the vaso-motor nerves. In the second class Dr. B. thinks that the influence upon the vaso-motor nerves, producing the unilateral sweating, must be reflex, and derived from the nervous system of animal life, and hence probably different in character from the paralysis induced by direct influence. In the third class, we can only fall back upon the supposition of some disturbance of the function of the trophic nerves (i. e., nerves exercising an influence over the nutrition of a part) distributed to this part. Dr. B. seems to take a middle ground between those who maintain that the sensory nerves are the true trophic nerves and those who hold that the ganglionic system alone monopolizes this function.[1]

In this connection we may note another case reported very briefly by Dr. H. C. Robbins, of Dement, Ill., in the *Boston Med. and Surg. Jour.*, July 9, 1868:

J. W., aged 22, while in the army, six years ago, contracted chronic diarrhœa, which confined him to hospital for several weeks. Upon his recovery, he first noticed the phenomenon of profuse perspiration of the left side of his face, which condition still continues, nearly six years after it was first observed.

His health is perfect in every other respect, and his mind is clear and strong, but every few minutes, winter or summer, he is obliged to wipe away the sweat from one side of his face. He is now a farmer, strong and energetic.

2.—*A Cure for Headache.* By George Kennion, M. D., F. R. C. P., etc. [British Med. Jour., June 13, 1868.]

I am desirous of bringing before the notice of the profession a very simple, and at the same time a very remarkable, cure for many kinds of headache. I have not the least claim to the discovery of this remedy, nor, indeed, am I at all aware who was its originator; but I believe that it is unknown to the profession generally; and having used it for nearly twelve months in a very large number of cases, and very rarely without affording immediate relief, I am desirous of making it more generally known. I heard of it first from a gentleman whom I was attending last year, and who told me that he thought it was used by a French physician. If this should come under his notice, I hope that it may be the means of inducing him to drop his incognito, so that he may receive the thanks of many to whom he has hitherto been an unknown benefactor.

[1] On this point see Echeverria's paper in this Journal, April and May, 1866.

The remedy, as I have already observed, is simple; it is the bisulphide of carbon in solution. Its mode of application is no less simple. A small quantity of the solution (about two drachms) is poured upon cotton wool, with which a small, wide-mouthed, glass-stoppered bottle is half filled. This, of course, absorbs the fluid, and when the remedy has to be used, the mouth of the bottle is to be applied closely (so that none of the volatile vapor may escape) to the temple, or behind the ear, or as near as possible to the seat of pain, and so held for from three to five or six minutes. After it has been applied for a minute or two a sensation is felt as if several leeches were biting the part; and after the lapse of two, three, or four minutes more, the smarting and pain become rather severe, but subside almost immediately after the removal of the bottle. It is very seldom that any redness of the skin is produced. The effect of this application, as I have said, is generally immediate. It may be reapplied, if necessary, three or four times in the day.

The class of headaches in which this remedy is chiefly useful is that which may be grouped under the wide term of "nervous." Thus neuralgic headache, periodic headache, hysterical headache, and even many kinds of dyspeptic headache, are almost invariably relieved by it; and although the relief of a symptom is a very different affair, of course, from the removal of its cause, yet no one who has witnessed (and who of us has not seen?) the agony and distress occasioned by severe and repeated headache but must rejoice in having the power of affording relief in so prompt and simple a manner.

As regards the *modus operandi* of this remedy, it is difficult, perhaps, to form a certain opinion; but I am disposed to attribute it to the sedative effect of the vapor of the bisulphide, absorbed through the skin, and acting upon the superficial nerves of the part to which it is applied. The remarks of M. Delpech (*Annales d'Hygiène*, Jan., 1863) point out very clearly the remarkable prostration of the whole nervous system produced in workmen who, in certain manufactures, are exposed to the vapor arising from a solution of the bisulphide of carbon; and we can readily understand that a somewhat similar effect, upon a small scale, may be produced by the application of this vapor to a limited portion of the surface.

I always procure the bisulphide of carbon from Mr. Morson, the eminent chemist in Southampton Row, Bloomsbury, who will also furnish the bottle with which the vapor should be applied, and a wooden case—a very necessary adjunct, on account of the offensive smell of the bisulphide.

3.—*A New Treatment for Chronic Dysentery.* By E. Malcolm Morse, M. D., San Francisco. [California Medical Gazette, Sept., 1868.]

In chronic simple uncomplicated dysentery, by which are meant those cases not kept up by organic disease of the heart, or phthisis pulmonalis, nor dependent on irremediable obstruction of the liver or spleen, Dr. Morse has met with marked success by throwing up into the rectum and colon from two to five pints of Labarraque's solution of chlorinated soda, largely diluted. The theory of the treatment is based on rational principles, and the remedy gives little or no pain, while experience has demonstrated that it is perfectly safe, no bad effects thus far having been observed. Chronic dysentery is a disease

so usually intractable and obnoxious to treatment that any plan which promises even moderate success is worthy of trial. Of course, in the complicated cases above referred to, the primary treatment must be directed to the disease on which the dysentery is dependent. Dr. Morse says:

"In order to get the patient into a proper condition to derive the most benefit from these injections, I am in the habit of pursuing the following method: I regulate his diet carefully, of course, and keep him in a recumbent position in order to assist the return of the blood from the engorged mesenteric veins, and those smaller tributaries which are distributed along the large intestine. This state of engorgement prevents the ulcers from healing, and renders each ulcer an outlet from which, in blood and serum, the stream of life ebbs out like water from the tubs of the daughters of Danaus. At daybreak on every alternate or fourth day I give a mild cathartic or aperient, in order to clear out the alimentary canal. The ordinary contents of the intestine produce great irritation when it is in this engorged and hyperæsthetic condition; and it is better to get rid of the fæces about the same time, instead of letting them run in driblets over the raw surface every hour or two. After the cathartic or aperient has acted sufficiently I inject very slowly from two to four pints of Labarraque's solution of chloride of soda, diluted, into the large intestine. In this way it becomes a topical application. The right strength for the first enema is twenty parts of water to one of Labarraque's solution. I inject as much of this mixture as he can be made to retain. Two or three pints will generally be enough. Sometimes as much as five pints may be given. Each enema should be made a little stronger until the patient says that he can feel it smart or burn. When this happens the solution is of the proper strength. The patient should be on his right side, or on his knees with his head low down, while these enemas are being administered. Occasionally it is necessary for him to change his position several times in order that the wash may reach every point where it is needed. Should there be much tenesmus after the injection has been passed, I give an enema of the tinct. opii, or an opium suppository. These applications of the chloride of soda should generally be made once a day. Occasionally it is necessary to give them twice a day, and sometimes, on account of the sensitiveness of the ulcers as they begin to heal, it is better to leave them off for several days, or give weaker solutions. The next indication in the treatment, after cleaning out the alimentary canal and washing the ulcers with the medicated solution, is to keep the bowels quiet, so that the ulcers may remain clean and heal up under the topical application. In suggesting the means of accomplishing this desideratum I am getting upon very debatable ground. The old proverb "*tot homines tot sententiones*" must certainly have been intended for physicians. Each one of us has his own way of using the arms with which we combat disease. I generally give large doses of subnitrate of bismuth three times a day; repeated opiate enemas and suppositories, in order not to disorder the stomach; Dover's powders, repeated if necessary; charcoal, or the mineral and vegetable astringents; the antacids, leeches, and fomentations; taking great care to *keep up* the effect of the medicines by giving them every hour or two. If one drug fails I try another, or give a combination of several of them, in order to have as few stools as possible passing over the ulcerated surfaces while they are healing."

4.—*Treatment of General Dropsy by the Hot Bath.* By Dr. Leo, Bonn. [Sitzungsber. d. niederrhein. Gesellschaft in Bonn. 1867. From St. Louis Med. and Surg. Journal, Sept., 1868.]

The case reported is that of a girl, æt. 13. She had, three years previously, suffered from articular rheumatism, and since then had been attacked every winter by difficulty of breathing, which received no treatment, however. In May, 1866, she received a fracture of the femur, which healed in six weeks. In Sept., 1866, she complained of chilliness, loss of appetite, and shortness of breath. She grew irritable, somnolent, and the legs swelled. These phenomena increased to such a degree that Dr. L. found her, at his first visit, 7th Nov., in the following state: The dyspnœa compelled her to sit up in bed; pulse 140; impulse of the heart hurried and indistinct; face and hands cyanotic; high degree of general dropsy: hydrothorax, hydropericardium, ascites, and general anasarca, especially in the labia pudendi and legs; urine contained much albumen; pain in the chest and abdomen, cough and dyspnœa dispelled sleep. A sure diagnosis of the cardiac affection was impossible under the circumstances.

The advice to transfer the child to the hospital was not followed till 8th Dec., 1866. The objective symptoms were unaltered; the debility considerably greater. Patient had now been in bed for three months; many remedies had been used in vain. Dr. Leo, therefore, concluded to make methodical use of the hot bath as recommended by Liebermeister and Ziemssen.

On account of the great debility of the patient baths were not given at once, but the child was "packed" in clothes wrung out of hot water. First packing, 9th Dec., followed by perspiration. At night, subcutaneous injection of 1-6 grain morphine to allay the severe dyspnœa.

10th Dec. Second packing: free perspiration. Both legs discharged fluid by drops from small excoriations.

11th Dec. Third packing. The perspiration in the blanket very uncomfortable, increased the dyspnœa. Injection of morphine.

12th Dec. Bath, 106° F., 15 minutes; followed by woollen blanket. Profuse perspiration. Ordered 1 tablespoonful of infus. digitalis (℈i—℥vi) with ℥i roob juniperi, 4 times a day.

One bath daily until 20th Dec. (eight in succession), gradually lowering the temperature to 99° F.; perspiration always very profuse. The dyspnœa diminished, the nights became more comfortable. On the 16th the legs, arms, and abdomen still much swelled, but the chest more relieved; secretion of urine increased. On the 18th the urine was free from albumen. On the 20th the anasarca had left the arms. Digitalis increased to ʒ ss in the mixture. Baths henceforward only three times a week, 99° F.; eight baths until 9th Jan., 1867.

On the 22d Dec. the abdomen was considerably smaller; the legs slightly so. Action of the heart quieter; appetite and sleep good. Improvement progressed rapidly, the dropsy disappearing from above downward. On the 25th the water had almost completely left the thighs and legs also; only the feet were swelled to above the ankles. Patient walks about after a treatment of 16 days, having been confined to bed for nearly four months. The last traces of œdema disappeared by the 2d January, and the patient left the hospital on the 9th. The cardiac trouble proved to be insufficiency of the mitral valve, with stenosis of the orifice and dilatation of the heart. The kidneys, which had suffered considerably, were relieved after the sixth bath.

This case shows that the hot bath, as recommended by Liebermeister and Ziemssen, is a highly valuable remedy in general dropsy following upon chronic disease of the heart with affection of the kidneys.

5.—*Iodine an Antidote to Strychnine.* By James I. Rooker, M. D. [Cincinnati Lancet and Observer, Sept., 1868.]

An article appeared in the *Lancet* for June (reprint), by Henry Wm. Fuller, M. D., Senior Physician to St. George's Hospital, entitled *Iodine an Antidote to Strychnine, and the Impropriety of Prescribing Quinine or Strychnine with Tincture of Iodine.*

The Doctor remarks: "In the course of my practice it has often occurred to me to prescribe a mixture containing quinine or strychnine, together with tincture of iodine," and have not been aware of any incongruity in the dispensing of it until last summer a chemist called my attention to the fact that it is impossible to dispense a mixture containing strychnine and tincture of iodine.

In whatever sequence the ingredients are mixed, I find that the whole of the strychnine is precipitated by the tincture of iodine. Indeed, so strong is the affinity between these two ingredients that the two fluid drachms of tincture of iodine are capable of decomposing six fluid drachms of the liquor strychniæ, producing an insoluble compound of iodine and strychnine.

"Now, it is obvious that, for medicinal purposes, a mixture in which such a precipitate occurs must be almost valueless. The patient not only loses the benefit of the quinine or strychnine, but of the iodine also. It is not unreasonable, therefore, to lay down as a rule that tincture of iodine ought not to be prescribed in a mixture containing either of the above-named alkaloids. Another question of practical importance arises out of this observation. May not a dilute solution of iodine be advantageously given in cases of poisoning by strychnine?" Coming from so high authority, the suggestion favorably impressed me, and, inasmuch as I had been called a few times to administer to those suffering from strychnine poison, and finding all my efforts proved futile, induced me to institute a series of experiments on the lower animals. In order to further test its antidotal properties, I therefore procured three healthy pups, of the same age, twelve young rats, and two kittens. The first two experiments were upon two pups. First took strychnine gr. ½, water ʒ ij, tincture iodine, ʒ i, at a dose for the first ten minutes. The only perceptible effect was frothing at the mouth, gnashing the teeth, whining, and shaking the head. In fifteen minutes falls down, laborious respiration, heart's action increased. Twenty minutes, tetanic spasms—violent; gave ʒ i tincture iodine, diluted with water, at a dose. Thirty minutes, no perceptible change for the better; in short, getting worse, a touch or noise producing violent spasms; death threatened from asphyxia. Forty minutes, much worse; commenced the administration of chloroform by inhalation; breathing became regular; heart's action more quiet; appears as in a quiet sleep. As soon as the effect of the chloroform passes off all the former symptoms return with the same violence. The only effect of the chloroform is palliative. I continued the administration of chloroform for three hours; it was then discontinued. Spasms returned at once with renewed force, and death came to its relief in a half an hour, from asphyxia.

Second pup, gave sulph. strychniæ, gr. ½, water, ʒ ij; attacked with the peculiar spasms in five minutes. Twenty-five minutes, gave tinct. iodine ʒ i, in water. Thirty-five minutes, still getting worse; chloroform administered for three hours; when discontinued, death followed in twenty minutes.

Post-mortem examination twelve hours after death in each case. First pup—Stomach full of indigested milk; mucous membrane healthy; could not detect with starch any trace of iodine, or of strychnine, by the taste; gall-bladder ruptured; right heart contained dark blood, with fibrinous clot; left empty; no further examination made. Second pup—Same condition, except gall-bladder entire, but empty.

Third Experiment.—Pup—Gave sulph. strychniæ, gr. ½, water, ʒ ij; attacked with tetanic spasms in two minutes; left to itself; died in two hours, apparently from exhaustion and asphyxia.

Fourth Experiment.—Twelve young rats—Gave Hall's Solution Strychniæ, ten drops to the first six; the other six same amount strychnia, containing equal amount tinct. iodine; all died in one hour.

Fifth Experiment.—Two kittens, same age. First took ʒ i, Hall's sol. Second, Hall's sol. ʒ i, tincture iodine, ʒ i, water, ʒ i; administered to both at once. Both died in two hours, in violent spasms.

Remarks.—These experiments were roughly made. Still they go so far as to show the inertness of iodine as an antidote to strychnine poison. But I do think that no intelligent physician, called to treat a case of this kind, would be so blind as not to see the indication for chloroform.

6.—*The Use of Ether and Etherized Cod-liver Oil in the Treatment of Phthisis.* [Lancet, Aug. 8, 1868.]

At the recent meeting of the British Medical Association at Oxford, Dr. B. W. Foster read a paper on this subject, of which the following is a summary:

He began by referring to the great difficulty of digesting fatty food which distinguishes the great majority of phthisical patients. This defective assimilating power had hitherto been treated by incorrect or insufficient means. The only true method of treatment to be adopted in such cases should be directed to the organs whose secretions are at fault. Physiology teaches that the digestion of fat is specially performed by the secretion of the pancreas, and the glands of the small intestine. Dr. Foster had long sought for a means of influencing these glands, and at last had found most ample evidence in the works of Claude Bernard that ether is capable of augmenting the pancreatic secretions to almost any degree. Bernard is accustomed in his experiments to give ether to animals in order to obtain a good flow of pancreatic juice. Applying this discovery to the treatment of phthisis, he had met with most satisfactory results. The ether was given in a mixture sometimes, but generally in the form of etherized cod-liver oil. Of the patients treated in this way, and all observed over some months, some over two years, 42 per cent. improved while under treatment, 30 per cent. remained stationary, and only 28 per cent. became worse. Twelve per cent. of the cases treated presented all the evidence of the arrest of the disease. In no case were the symptoms of physical signs alone accepted as evidence of improvement; every case was weighed from week to week, when under observation, and only a decided increase of weight in addition to other signs received as evidence.

7.—*Thermometrical Observations in Typhoid Fever.* By Dr. R. E. Thompson. [St. George's Hospital Reports, and Summary in Edinburgh Med. Jour., February, 1868.]

Dr. Thompson's observations extend over a period of three years, and are made from a careful study of forty-seven cases. They tend to confirm

Professor Wunderlich's researches on the same subject. In typhoid fever, a decrease of temperature is not always a favorable sign, nor is a rapid fall symptomatic only of a crisis. As a rule, the nocturnal increase of temperature in this form of fever is very considerable, and amounts to two or two and a half degrees. During the latter half of the first week, the heat increases day by day, and varies between 102° and 103° in the morning, and 104° or even 105° in the evening. These high temperatures are quite sufficient to distinguish typhoid fever from tubercular meningitis or from peritonitis, these diseases seldom showing much increase over 102° Fahr. During the second week, the temperature varies between 102° in the morning, and 103° and 104° in the evening, the oscillations being influenced by the amount of diarrhœa. The thermometer does not enable one to say, by a longer prediction than twenty-four or forty-eight hours, whether the case is likely to be fatal or not; but a steady rise in the temperature will often indicate the danger of ulceration of the bowels twenty-four hours before the intestinal lesion is manifested by diarrhœa and hæmorrhage. The mode of termination of the fever is characterized by extraordinary oscillations in the temperature between morning and evening, the difference being sometimes as great as nine degrees. This feature distinguishes the thermograph of typhoid fever from that of almost all other diseases. In one case of perforation of the bowel, which was preceded by hæmorrhage, the heat of the body was reduced from 102° to 99.5°, and was followed by a rise to 102.2°, where it remained for forty-eight hours, before death, during which period perforation took place.

8.—*Case of Bright's Disease consequent upon Ague.* By J. B. Bradbury, M. B. [Brit. Med. Jour., April 18, 1868.]

This case is interesting from the fact that Bright's Kidney is rarely found as an immediate sequela to ague. Dr. Roberts states that he has never met with such a case, and quotes Becquerel and Frerichs to the effect that, "in dropsies following intermittent fever, they had never found evidence of kidney-disease." The reporter omits all mention of the previous habits of the patient, which might have had an important bearing in the causation of this case. The heroic treatment to which this patient was subjected we can hardly advise our readers to follow:

W. B., aged 36, married, farm-laborer, living at Upnell, was admitted into Addenbrooke's Hospital, under the care of Dr. Paget, on September 8, 1866.

Previous History.—He was quite well till about twelve months ago, when he had quartan ague, since which time he had done very little work. The ague lasted for five or six months, and reduced his strength a great deal. Soon after the ague left him, his legs and feet began to swell, the dropsy gradually extending to the scrotum and abdomen. He was under Dr. Latham as an in-patient in the summer, and left the hospital at his own request before he was quite well. Shortly after returning home, he caught cold, and his legs and feet began to swell again. Sickness and severe headaches came on, and he lost his appetite. For seven weeks previous to his admission he had been confined to his bed.

State on Admission.—There were pallor and puffiness of the face and dropsy of the legs, scrotum, and abdomen. The tongue was clean, but

pale. Pulse 90, very feeble. The urine was about normal in quantity; of specific gravity, 1017; it almost became solid with heat and nitric acid. It contained hyaline, granular and fatty casts. He complained of pain across the loins. There was no cardiac or pulmonary disease. After his admission, he had four or five attacks of ague of the quartan variety, each fit lasting three or four hours.

He was at first treated with half-drachm doses of compound jalap-powder every other morning, and five minims of tincture of perchloride of iron three times a day. On the supervention of the ague, the iron was changed for quinine in three-grain doses. Under this treatment, there was very little diminution of the dropsy; so on September 27th his legs were punctured. He subsequently took quarter-grain doses of extract of elaterium every other morning for a week. Although the elaterium acted very freely on the bowels, the abdomen did not become much softer, so on October 8th he was ordered the following diuretic:

℞ Potassæ bitart. gr. xx; tinct. digitalis, ℥x; aquæ juniperi ℥iss.
M. Sumat bis quotidie.

After he had taken this mixture for a short time, there was considerably less œdema of the legs and scrotum, and he got smaller in girth. The urine became more abundant, but the quantity of albumen did not alter.

He became so much better toward the end of November that he was allowed to be made an out-patient. Dr. Paget informs me that the patient has presented himself from time to time at the hospital, and that the last time he saw him the dropsy had quite disappeared, and the man was able to work.

9.—*Gangrene of the Heart.* By D. S. Young, M. D., Professor of Surgery in the Cincinnati College of Medicine and Surgery. [Cincinnati Medical Repertory, May, 1868.]

Gangrene of the heart is a disease in regard to the existence of which nearly all modern pathologists are in doubt. In fact, but few writers refer to it; or, if they do, it is only to express their conviction of the incorrectness of the diagnosis of cases reported, and of the impossibility of life continuing until such a change could take place in so important an organ. On reading these opinions and the history of the cases, I also entertained the same impression, until I met with the following case:

Henry McNulty, a native of Ireland, aged 65, an inmate of the Cincinnati Infirmary for several months. When first admitted he was suffering from chronic rheumatism. He had been afflicted with this disease for a number of years, and was permanently disabled.

He had never felt any difficulty about the region of the heart, nor was I ever enabled to discover any unnatural sounds, having frequently examined it upon his first admission into the house. His general health, with the exception of the rheumatism, was as good as men of his age usually enjoy.

Some time about the middle of January, 1860, he injured the thumb upon the right hand, and, a few days subsequently, the bone (distal phalanx) was found to be necrosed, and was removed while the patient was under the influence of chloroform. He took the chloroform well, and rallied without any unpleasant symptoms, and the wound progressed without any unfavorable indications. Two days after the operation he complained to the nurse that there was soreness in his chest, and that his head felt badly; but, as these symptoms did not particularly distress him, no report was made to me. The next morning I found him with a hot skin, coated

tongue, rapid pulse, and complaining of a burning sensation on the chest, with considerable difficulty in breathing. The action of the heart was regular, but having a peculiar softness, which I had never before observed in any patient. It was nearly as full as in health; while the softness and compressibility of the artery were very singular and characteristic. He attributed his condition to the chloroform, and now, for the first time, said that he had not felt right since he inhaled it. I gave but little attention to the statement at the time, but during the following night and the next day, when he began to cough and complain considerably of his lungs, I gave it more consideration, although there were no indications that the anæsthetic had any thing to do in exciting the symptoms present. These continued to increase during the night and next day, and on the following night he grew very restless—his lips became blue at times, and the veins of his face and neck full and turgid with blood—he also suffered from urgent dyspnœa occasionally. His breathing was constantly hurried, and, as the disease advanced, the pulse grew weaker, and the brain gave decided symptoms of a congested condition. The sensibilities grew dull, and he ceased to complain. The pulse disappeared at the wrist, and in a short time he died. The action of the heart was always regular, and I never could detect any abnormal sounds.

At 10 o'clock A. M., of the same day, seven hours after death, I made a *post-mortem*. The corpse immediately after death was removed to the dead-room in the cellar, which was dry and well aired. The muscles were slightly stiffened, but the body still retained some warmth. After elevating the sternum, my attention was attracted to the dark appearance of the upper and front surface of the pericardium, and the tissues immediately around it. On dissecting carefully the cellular tissue from it, I found it of a dark color, but as firm and unyielding as when it was natural in appearance. Upon opening it a quantity of gas escaped, and the cavity collapsed —I had not observed that it was distended until this occurred. At the same time a most disagreeable fetid odor was experienced. This was so strong and characteristic of gangrene that it could not be mistaken for an instant. It filled the room, and the passage outside for some distance, and I had some difficulty in removing it from my hand. The cavity contained about two ounces of a dark-colored, fetid fluid, of the consistency of treacle.

The heart was dark colored, the right auricle being almost black. Upon attempting to elevate it to examine its posterior surface, my finger penetrated the right auricle, although but slight force was used. I then removed the lungs and heart from the thorax, and proceeded to examine them critically. Found the whole anterior surface dark colored. This hue was principally confined to the right side of the heart, and was most marked over the auricle. A greater part of its surface was black, or intermediate between that and a dark-chocolate color, with a tint of greenish blue. The color became lighter as the apex was approached, which was very livid. The anterior wall of the auricle could be easily separated by the fingers or forceps. At the point where it had been penetrated by the finger, it appeared to be almost ready to fall in pieces. When the fibres were torn and broken, they retained their shape, but were very friable. On making an incision from the rent to the apex, extending into the auricle and ventricle of the right side, and examining the cut edges, they were found to correspond in color to the external surface. The tissue was softer than in the normal condition, until within about a quarter of an inch of the natural surface—in some places rather more. This part appeared somewhat denser than the unaffected portion of the organ, and was of a light-maroon color; and corresponded as nearly as could be expected to a

similar condition around the margin of gangrenous tissues, which I have often since seen and divided in cases of traumatic hospital-gangrene. The remaining portion of the heart was quite natural—perhaps a little flaccid, but gave no evidence of fatty degeneration or inflammation. The whole organ appeared smaller than usual, with a shrunken appearance. The valves were perfect, except upon the right side—the outer portion of the tricuspid being changed in color, and less firm than in the healthy portion. The vena cava and pulmonary artery were unchanged. The right side of the heart was partly filled with blood—that in the ventricle was partly coagulated. The left side was empty.

The vessels of the brain were engorged, and ventricles contained more than their usual quantity of serum. The abdominal viscera were considerably congested with dark fluid blood. No other traces of gangrene were observed. The gall-bladder was full, and there was no transmission of bile to the adjoining tissues by exosmosis.

Dr. Young appends to this history an elaborate discussion of the nature of the case, and takes the ground, in face of the recorded opinion of most pathologists, that this was an undoubted case of gangrene. He refers to Dr. Kennedy's well-known case, which in several points quite resembled his own. Dr. Gross, in the last edition of his Pathological Anatomy, quotes Kennedy's case; but states his belief that this and the other so-called cases of gangrene are examples of softening rapidly followed by putrefaction. This is the only authority brought forward by Dr. Young in support of his view.

We incline very decidedly to the opinion that the case reported was an instance of acute myocarditis—a disease, it is true, of very rare occurrence, but of which we have a few recorded instances. The celebrated case of Dr. Radcliffe,[1] in which the disease was diagnosticated during life, is even more striking in its parallelism to Dr. Young's case than the one which he quotes from Gross. Dr. Stokes, who was the first to describe the disease, and who met with a single case only, is inclined to doubt the existence of gangrene, and to refer all these so-called cases to inflammatory softening or disintegration in various stages of progress of the muscular structure of the heart. No one of the recent works on practical medicine has more than a passing allusion to this variety of disease; but, on the question of the actual occurrence of gangrene of the heart, they are all silent. The point on which Dr. Young lays the weight of his argument is the presence of the gangrenous odor, which was plainly perceptible; but this odor is present in certain diseases—for instance, of the lungs, where no gangrene coexists—and, on the other hand, the odor is sometimes but very faintly marked in cases where no reasonable doubt of the existence of the gangrenous process can be entertained.

No microscopic examination of the tissues of the heart is

[1] Medical Press and Circular, February 7, 1866.

given—a fact which is to be regretted, as the exact reporting of these cases is quite essential to solve the existing doubts of their pathogeny.

SURGERY.

1.—*Three Cases of Perinephritic Abscess, complicated with Pulmonary and Pleuritic Disease; Incisions into the Renal Region; Recovery.* By Henry I. Bowditch, M. D. [Boston Med. and Surg. Jour., July 9, 1868.]

These cases are of rare occurrence, and the history of the three given by Dr. Bowditch is unusually instructive. They are too long, however, to transfer to our columns, and any summary would weaken their value. Dr. B.'s comments, however, contain all the essential points in diagnosis and treatment, and cover the whole of the teachings these cases convey:

These cases were three of the most interesting I have met with since commencing my profession. Singularly enough, they are the only ones I have ever seen of this very rare disease, viz., of perinephritic abscess, as Trousseau calls it. Still further—rare as the disease is in the practice of any one, these specimens all came under my notice during the brief period of nineteen months, and the two severest were within one month of each other. In all of them the abscess pushed upward into the thoracic cavity, and in one instance the perinephritic origin of the thoracic disease was marked by the severe thoracic symptoms that supervened after comparatively mild signs of disease below the diaphragm.

Analyzing them, I find as follows: Patients all males, of the ages 27, 29, and 38 years respectively, and following the professions of physician, printer, and clerk. The disease commenced always near the right renal and cæcal region, and in all was preceded by some debilitating influences. Superadded to these, there were in two an unusual physical labor and physical strain. In one there was a special strain (from digging) of the psoas muscles, and in the other exposure to cold and wet in a swamp while working.

The prominent symptom in all was *pain*, generally in the back and at the right renal region; at other times it was more in front and near the cæcum. It was, however, usually felt in both of these places, and sometimes down the legs. This pain caused lameness, which at times was severe and confined the patient to the bed with his knees flexed. At first, this pain was in one case superficial, but gradually it went deeper, and in two it was excruciating and attended with violent paroxysms of distress. There was at times slight obstruction of the bowels, easily removed, however, by enemata or by gentle cathartics. With the dejections came relief to the temporary urgent symptoms. The appetite was lessened or wholly lost, and in two there was nausea; but, as a rule, there was no disturbance of the alimentary canal, nor was there of the urinary functions, except that the urine was in one case temporarily redder than normal. In one case, where the urine was specially examined by chemistry and the microscope, no albumen, blood, or casts were noticed. No jaundice or other

disease of the liver in any of them; no general peritonitis; no cephalic symptoms.

In all three, a tumor was found which could be grasped between the two hands placed in front and in the renal region, and filled up that space. It was usually rounded, doughy, rather non-elastic, and generally non-sensitive.

In all there were signs that the abscess extended up into the right pleura, without apparently affecting the liver, after having probably forced its way behind that organ and along the psoas muscles, under the right crus of the diaphragm. This chest affection was in one case revealed only by auscultation and percussion, the respiratory murmur being less in the right back than the left, and some coarse mucous râles on friction were heard at the very lowest part, while no subjective symptoms occurred in that case. In another there were cough and a slight pleuritic effusion, coming on during recovery, and ten days after the opening had been made by the surgeon. Finally, in a third, the thoracic symptoms were so severe that the patient was held to be dying of them, yet the history of the symptoms and the physical signs at examination proved that pleurisy existed on the right side, while acute pneumonia had begun on the left.

In one, only, the breath was fetid, almost fœcal.

The pulse varied exceedingly, sometimes little accelerated, at others being very rapid. In all there were marked chills with fever, and copious night sweating. Emaciation and debility took place in all.

The treatment in all was tonic, with at times leeches, counter-irritants and opiates, with laxative enemata generally when needed during the earlier period. But the opening by the surgeon in the right renal region was in all the first and prominent step toward a cure, which, as we have seen, always took place. In all the bistoury and a free opening were used with great deference to the usual rules of surgery. I cannot but ask whether a trocar, either a small one with a suction-pump or one of larger size, might not be tried at least, and thus, perhaps, one great danger of operating with the bistoury be avoided.

It will be remembered that very severe hæmorrhage occurred in one of our cases. Trousseau names this accident as being at times hazardous. Certainly it was in our first case, the patient being very bloodless, with ringing in ears, etc. Nothing but the promptest surgical skill apparently saved the patient. In the second and third there was no hæmorrhage, and the happy influence of the operation was undoubted—immediate in one, and more slow but unequivocal in the other.

In two the kidney could be felt, floating, as it were, at the bottom of the abscess.

The recovery was prompt in two, in about six weeks. In the other, where the hæmorrhage occurred, the patient had a longer convalescence, and did not resume business till after six months.

A few words on the literature of this subject.

Great Britain seems almost wholly to have ignored the existence of this disease, so far, at least, as it may be considered one to which physicians would be summoned. Neither Graves, nor Watson, Aitken, Todd, Tanner, nor Chambers, alludes to it. Bennet, of Edinburgh, gives a few cases of abscess of the kidney—but not outside of it, except secondarily. Neither Dr. Wood nor Dr. Flint in this country notices it.

Trousseau[1] (preceded by Rayer,[2] Parmentier, and others, who have given cases) first made an elaborate article upon the subject. Monsieur Trousseau gives many causes: fatigue, strong muscular efforts, contusions, repeated and violent shakings of the kidney, renal calculi, typhoid and puerperal fevers, etc.

[1] Clin. Med., vol. iii., p. 713. Paris.

[2] Maladies des Reins, 1839

He speaks of the very great infrequency of the disease, and of its very insidious approach. He alludes to most of the symptoms recorded in my cases. Among them he dwells chiefly on the swelling in the lumbar region, the pain in the same part, the chills and fever, emaciation and debility. He does not particularly allude to two prominent facts noticed in all of our cases, viz.: the lameness in the movements of the right legs, owing to an interference with the free play of the psoas muscles, and the extreme frequency of pulmonary and pleuritic complications in consequence of the extension of the disease. And yet the anatomical structure of the parts concerned proves that nothing can be easier than the transmission by contiguity of the perinephritic abscess into the thorax. The psoas muscle extends up into the thorax behind the pillar of the diaphragm. Hence any abscess, pressing on or inside the muscle, will very readily gain access to parts above the diaphragm. It was the remembrance of the serious complications caused by this anatomical arrangement in the preceding cases that induced me, in the last case, to urge a speedy operation, when, according to the common rules of surgery and the absence of distinct fluctuation, a delay would naturally have suggested itself. In fact, one surgeon urged delay. Dr. Hodges, I think, was somewhat influenced by representations from Dr. Blake and myself, urging him to explore, at least.

It is a singular fact, however, that notwithstanding the pus in these cases goes directly back of the liver, jaundice was not seen in our cases, and is, I think, a rare complication of the disease.

In this connection we may add the following brief account of a case of the same nature, which was reported at a recent meeting of the Clinical Society of London, by Dr. Southey:

The patient had previously suffered from stricture with vesical catarrh, subsequently passing pus in albuminous urine. A deep-seated swelling formed in the left loin, which varied in size according to the greater or less quantity of pus present in the urine. Presently a large amount of pus was discharged after the fæces, and the patient began to suffer from extreme exhaustion. The tumor pointed and was punctured; almost five ounces of deeply-seated pus escaped. Later on, symptoms of dysentery caused much suffering, but after a time the discharge became less, and the general health improved. Eventually the opening in the flank closed, and the patient recovered, but the bladder was permanently drawn up toward the left kidney, and there were pain and spasm in micturition. In the absence of any evidence of caries of the spine, or of embolism, or of renal calculus, Dr. Southey concluded that suppuration began in or about the kidney, and the disease was throughout of a local character.

2.—*Successful Operation of Lithotomy for the third time on a Patient Eighty Years of Age.* By Dr. Scott, Surgeon to the Dumfries and Galloway Royal Infirmary. [Edinburgh Med. Journal, March, 1868.]

William R., laborer, Annan, was admitted into the Infirmary on the 3d of October, 1867, with undoubted symptoms of calculus of the bladder. Patient stated that sixteen years ago, and again four years ago, he had lithotomy performed by Professor Syme in the Edinburgh Royal Infirmary, where he remained four weeks and eleven weeks respectively.

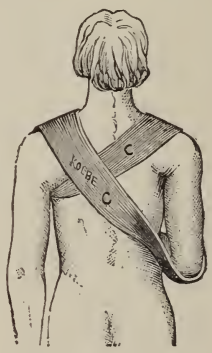
On the present occasion, as formerly, the lateral operation was performed, and, on introducing the finger into the bladder, a fibrous polypus,

of the size of a cherry, was discovered, which was easily twisted and removed by forceps. Three calculi were removed without any difficulty, two of which were of the size of marbles, and the third about the size and shape of a walnut. The patient made an excellent recovery, and left the house six weeks from the date of the operation, suffering, however, from incontinence of urine, which he had done since the second operation.

The object in publishing the foregoing case is to show that the operation may be undertaken at an extreme old age, with prospects of a favorable issue.

3.—*Apparatus for the Treatment of Fracture of the Clavicle.* By J. M. Boisnot, M. D. [Med. and Surg. Reporter, Oct. 3, 1868.]

Dr. Boisnot's apparatus is so simple and easily adjustable, that we have deemed it worthy of notice in the columns of the Journal.



The conical shape of the forearm, with the apex at its most dependent part, admirably adapts it for an attachment from which traction can be made in an upward and backward direction. A leather socket, fitting and laced around the forearm, is the simplest application which can be made for this purpose, while its corresponding shape is a support; the latter feature can be more fully developed by having a portion continued beneath the ulnar border of the hand. The long band of webbing attached to this leather socket or case, upon its front part and near the elbow, being carried upward to and over the injured shoulder at the point of fracture, elevates the shoulder, and prevents displacement of the fractured ends of the clavicle when once coaptated; having at its initial end a buckle with swivel attachment, the arm may be placed in any suitable position, and the forearm carried across the breast in any direction called for by the peculiarity of the fracture. No pad in the axilla is required. A stip of adhesive plaster should, and graduated compresses may, be applied over the seat of fracture; the latter near to or distant from the shoulder as may best tend toward keeping the band in its correct position; this, as well as the distance which the arm should be carried from, toward, or across the chest, every surgeon will determine for himself. The course of this band from the injured shoulder is across the back to the opposite axilla, then by a turn around this shoulder and again across the back to the opposite axilla, then by a turn around this shoulder and again across the back to the sec-

ond buckle at the elbow; the terminal end is then carried forward and beneath the forearm, to the turn around the shoulder (not shown in the cut); after forming a loop by passing through this turn, it can be pinned or sewed; a sling is thus formed, and the apparatus and parts contained kept comfortably firm.

As it is applicable to either side of the body, it is merely necessary, in sending for it, to state the age and sex of the patient.[1] The simplicity of this apparatus is evident, when compared with that of Dessault, or its modification by Fox; requiring for its impromptu preparation only a coat-sleeve and a strip of muslin shaped to the forearm and fastened together by needle and thread, I think it can be made anywhere and by any one undertaking the treatment of a case of fractured clavicle.

A further use of this apparatus consists in its application to cases after resection of the shoulder, and as a support after reduction of its dislocations. The small amount of dressing constituting the apparatus, and the manner in which it supports and retains the parts in the position natural to them when uninjured, contribute alike to the comfort of the patient and the attainment of favorable results.

4.—*A Case of Ilio-Psoas Abscess.* By A. Hammer, M. D. [Humboldt Med. Archives, July, 1868.]

We refer to this case with the special view of calling attention to the novel method of treatment adopted to relieve the pain and discomfort caused by the prolonged confinement in bed. The case also involved a nice question of diagnosis in its earlier stages, and is altogether quite instructive:

The patient was a young man of about 37 years of age, who, previous to the attack of illness under which he succumbed, had enjoyed excellent health, and was rather noted for his vigor and ability as an amateur gymnast. During the winter he had been travelling in Illinois on business, and had been much exposed to the inclemencies of the season, and on his return to the city was taken ill on the 6th of December, and was treated by Dr. Engleman for rheumatism, who afterward, from the symptoms and complications, thought the attack to be morbus coxarius. Upon Dr. E. leaving for Europe, the case was left in charge of Dr. Castlehuhn. Dr. Hammer had been called in consultation about the middle of April, and found the patient confined to bed. The left leg was flexed and rotated inward; the hip and thigh were both enlarged, and he had severe pain around the hip-joint, and extending to the knee; there was an abscess just below the greater trochanter, and fluctuation discoverable below Poupart's ligament, on the anterior and internal side of the thigh (Scarpa's triangle). Dr. H. was disposed to believe that the joint was diseased, but, for the purpose of a more accurate investigation of the case, the patient was put under the influence of chloroform, when no signs of disease of the joint were discoverable, and the abscess was pronounced to be periarthritic. Within the next few days fluctuation could be detected beneath Poupart's ligament, within the pelvis, and could be traced along the crista ilii. Dr. Hammer, under such circumstances, pronounced the abscess retro-peritoneal, within the ilio-psoas muscle, and this diagnosis was fully confirmed, when still a few days afterward fluctuation could be felt below the twelfth rib near the spinal column.

[1] Mr. D. W. Kolbe, 15 South Ninth Street, Philadelphia, and Messrs. Tiemann & Co., New York, are prepared to furnish the above apparatus.

Both the abscess below the trochanter and that in the groin were subcutaneously evacuated by the trocar, but, refilling rapidly, recourse was had to drainage-tubes. The pus in the abscess below the trochanter becoming putrid from access of air, the drainage-tubes were removed from both abscesses, and that below the trochanter was laid open its whole length—about eight inches—and filled with dry charpie. An incision was made below the ribs, and injections made, first of clear water, and followed by diluted carbolic acid. The discharge not diminishing, tinct. iodine was repeatedly injected, but without benefit.

When Dr. H. first saw the patient he had large bed-sores on both nates, and on the sacrum; he was very weak and prostrated; completely emaciated; his tongue was covered with aphthæ; had no appetite; repeated rigors followed by high fever, and unable to lie in any position without excruciating pain. To obviate this latter trouble it was finally decided to suspend him in a permanent water bath of about 90°, in which he remained with comparative comfort for twenty-seven days, when death relieved him from his sufferings. In the beginning, the bath was decidedly beneficial, as in combination with anti-septic remedies and stimulant treatment all unfavorable symptoms were markedly ameliorated. The aphthæ disappeared; the pulse fell from 130 to 85; his appetite increased enormously; he slept comfortably, and for the first time in many weeks could rest with comparatively little pain. So marked was the improvement that his friends felt confident of his final recovery; but the constant drain upon his system from the profuse suppuration proved so exhausting that he finally succumbed to inanition, and died in the bath, as though falling into a peaceful sleep.

The *post-mortem* revealed an abscess extending along the iliacus and psoas muscles, and diffusing itself between the soft parts surrounding the joint, and even on the anterior external portion of the thigh. The capsule of the joint was perfectly intact, thus giving positive evidence that the suppuration had been entirely peri-arthritic. The joint on being opened presented the interesting appearance shown in the specimen: The cartilage covering the head of the femur, and lining the cavity of the acetabulum, was in a state of softening, in some places showing the cancellated structure of the bone; and near the centre of the acetabulum even the osseous structure had been absorbed, leaving a spot about the size of a pea, which, on the internal or pelvic side of the bone, was only closed by the covering of periosteum. There was no pus in the joint, and only a very small amount of a dirty-looking fluid, the detritus of the process of softening.

Dr. H. expressed the opinion that the affection of the joint had only supervened during the last four or five weeks of the man's illness, inasmuch as the most careful examination, under the influence of chloroform, when he first saw the patient, did not reveal any kind of morbid affection of the articulating surfaces; and he further believed that it was the mere result of the pressure of the head of the bone in the acetabulum, caused by the continued contraction of the surrounding muscles.

With regard to the so-called pathognomonic pain in the joint and in the knee in the beginning of the disease, and which had led the first physician to diagnosticate rheumatism and coxitis, he stated that a retro-peritoneal abscess upon the ilio-psoas muscle would undoubtedly cause such an irritation of the crural nerve by pressure, etc., as to give rise to the same symptoms.

Miscellaneous and Scientific Notes.

Chloroform or Ether.—At a recent meeting of the Middlesex (Mass.) North District Medical Society, Dr. Burnham, of Lowell, read a paper on "*Chloroform: Its Advantages over Ether as an Anæsthetic.*" An abstract of the paper is given in the *Boston Medical and Surgical Journal*, and we find it there stated that Dr. Burnham "showed that chloroform is safer, and is attended with none of the baneful after-results which often follow the use of ether, such as insanity, epilepsy, convulsions, etc." We are quite at a loss to account for this most astounding assertion, and, in view of the fact that the medical journals are reporting almost daily cases of death from the use of chloroform, we think such a statement as the above can only be prompted by too close adherence to prejudice, or by an ignorance of the actual facts of the case. Deaths from chloroform are now on record by the hundred, and, what is worse, they are still occurring with alarming frequency; and, while we do not claim positive and entire exemption from danger in the use of ether, we challenge Dr. Burnham, or any other ardent admirer of chloroform, to produce the proof of the bold assertion that "chloroform, if properly administered, is devoid of danger," and safer than ether. We admit the advantages which, in some respects, chloroform possesses over ether, but at the same time we cannot for ourselves allow those advantages to be purchased at the cost of an increased danger to the patient's life, and we do not hesitate to deny, as emphatically as Dr. Burnham asserts, that chloroform is the safer agent of the two.

This subject is of the utmost practical importance, and there ought to be no halting of opinions upon it. Either Dr. B. is right or wrong, and an appeal to the records ought readily to determine the issue. We trust that he will feel it his duty either to make good his assertion or undo the injury which, in the minds of the inexperienced, he may have wrought.

Again we beg leave to request that he may enlighten us on the point of the frequent occurrence of insanity, epilepsy, and convulsions after the use of ether; for this sweeping and un-

qualified assertion comes upon us with a startling effect that threatens to quite sweep away our accustomed belief of the general innocuousness of this agent.

At the risk of converting our readers to the system of high dilutions, we quote from the *American Journal of Homœopathic Materia Medica* the following astounding cure. *Equo ne credite, Teucri.*

Some four years since, while practising medicine in New York, I was called to a case in Flushing, L. I., and, on driving me home the next morning, the gentleman, in speaking of his horse, remarked that it was a very favorite animal, but he regretted it was very much out of health—ate little, was languid, and seemed to have no spirit or life. I said, "Sir, your horse has dyspepsia; I should give it *Nux vom.*" At his request, I did up for him half a dozen powders of the 200th, containing about twice the quantity I should have given a man. "How are these to be administered?" Here was a poser. Never having given medicine to animals of a lower type than the *genus homo*, I knew not what to say, and, in my inexperience and embarrassment, told him to put the powder in a bottle of water, and, after shaking it well, pour it down the horse's throat. But it seems the animal was not to be persuaded. And here the good common-sense of the owner came to the relief of the physician's ignorance. The powder was put in the pail from which the horse drank, and, thus deceived like many a nobler beast, the medicine reached its destination.

After taking two or three powders in that manner, the horse passed "enormous quantities of worms." "Did you give the medicine for worms?" said the gentleman to me afterward. And here was another poser, in a question which I adroitly managed to set aside, without either affirming or denying, for I had not the least thought of such a result when the prescription was given.

Finding the good effects after giving the last powder, and suffering the horse to drink all that could be drunk from the pail, the gentleman filled up the pail with water, and gave the contents to his other horse, which was slightly ailing, though not apparently to the same degree; but the medicine was followed by the same results. (What potency shall we call the last?) I learned afterward that these horses had been taken to several veterinary surgeons of New York to be doctored for worms, but without any relief.

The horses remained well for one year. The owner then wrote me a note, asking for some more of "that same medi-

cine." Of course I sent it; and, seeing him the next winter, learned the animals had been well ever since.

This case proves two things: that the beasts might have been cured with much less medicine and in much higher potency than that given, as the last had only the refuse of what was left of the last powder given the first horse, and that at one draught only.

From the same source we take a "THERAPEUTIC HINT."

"*Cina.*—Children don't want to be touched. They can't bear even to have you come near them."

We know a good many children who are troubled in this way, and, as like cures like, we think our homœopathic friends have earned the lasting gratitude of thousands of anxious parents.

"*Angelica.*—Women become melancholic after confinement, and will not go out, *even to church.*"

We have met with a good many cases wherein the last-named characteristic was dependent on the fact of having nothing to wear. This is too horrible for contemplation, and we beg to be informed what potency of what remedy will be efficacious in such cases.

SPEAKING of women physicians, the *Spectator* wittily remarks: "If there be such a right as liberty, it includes a woman's right to learn and teach anatomy if she pleases; and to forbid her by law is tyranny, and tyranny without excuse, till we make both sexes modest by act of Parliament."

TRICHINÆ IN THE DOMESTIC FOWL.—Dr. Geo. S. Bryant reports, in the *Richmond and Louisville Medical Journal*, the finding, imbedded in the muscular coats of the stomach and intestines of the common domestic hen, of large numbers of entozoa, coiled in cysts in every possible attitude, and not unlike in appearance the trichinæ found in the human muscle. He suggests that the disease known as "chicken cholera" is dependent upon the presence of these entozoa, and promises to continue and report his investigations on the subject.

DR. A. B. SHIPMAN, of Syracuse, New York, died in Paris, France, on Tuesday, September 15, 1868.

Death from Mrs. Winslow's Soothing Syrup.—The *Medical and Surgical Reporter*, of August 29th, gives a report from an anonymous correspondent, in Washington, D. C., of the death of an infant, nine months old, from the use of Mrs. Winslow's Soothing Syrup. Between 10 o'clock in the evening and 6 o'clock in the morning, eight hours, the child had taken six teaspoonfuls. The symptoms, when the child was seen by the physician, were "semicomatose condition, with pallor of face, open eyelids, contracted pupils, cool skin, cold extremities, increased heat of head about vertex, and thumbs contracted into the palms. There was vacant expression of the eyes, constant low moaning, and perfect indifference to surrounding objects. The only movement was an occasional closing of the eyelids."

Treatment adopted was stimulation externally by means of mustard-heat and frictions. Internally, two-drop doses each of aromatic spirits of ammonia and tincture of belladonna with brandy were administered. No reaction was observable, save a slight temporary dilatation of the pupils, after ach dose of belladonna. Slight convulsions preceded death.

The new law in Ohio, compelling all physicians who have never received regular diplomas to suspend practice, took effect on the 1st of October.

Appointments.—Prof. E. R. Peaslee, M. D., of this city, has resigned the chair of Anatomy and Physiology, which he has occupied for the past twenty-eight years, to accept the position of Professor of Diseases of Women and Children, in the Medical Department of Dartmouth College, Hanover, N. H. Dr. Lyman B. Howe, of Manchester, N. H., for several years Demonstrator of Anatomy, succeeds Prof. Peaslee in the chair of Anatomy and Physiology.

The following appointments have been made in the Long Island College Hospital: Prof. of Obstetrics and Diseases of Women and Children, E. S. Dunster, M. D. Prof. of Chemistry and Toxicology, George W. Plympton. Prof. of Operative and Clinical Surgery, Benjamin Howard, M. D. Physiology and Microscopic Anatomy, William T. Lusk, M. D. Materia Medica and Therapeutics, Andrew N. Smith, M. D.

New York County Medical Society.—At the Anniversary Meeting of the Society held Monday, Oct. 5, 1868, the following Officers were elected for the ensuing year. President, George T. Elliot, M. D.; Vice-President, A. Jacobi, M. D.; Recording Secretary, A. E. M. Purdy, M. D.; Corresponding Secretary, Benjamin Howard, M. D.; Treasurer, W. B. Bibbins, M. D.; Censors, E. R. Peaslee, M. D., Ellsworth Eliot, M. D., E. S. Dunster, M. D., W. R. Whitehead, M. D., and W. T. White, M. D.

M. Gervais, Professor of Anatomy and Zoölogy, has been appointed Professor of Comparative Anatomy at the Museum of Natural History, Paris, France, to fill the vacancy caused by the death of the late M. Serres.

Académie de Médecine.—M. Alphonse Guerin has been elected into the Section of Operative Medicine, in the place of the late M. Lagneau, by the votes of 51 out of the 63 academicians who were present.

Army Personal.—The following changes have taken place in the Medical Corps, U. S. Army, since the date of the publication of the last official report, July 1, 1868:

Promoted.—Assistant Surgeon C. E. Goddard, to be Surgeon, to date January 6, 1868, vice Lewis Taylor, deceased.

Appointments.—Lieutenant-Colonel Jedediah H. Baxter, Assistant Medical Purveyor, U. S. Army, to be Colonel by brevet, to date July 20, 1867. Brevet Colonel E. Swift, Surgeon, U. S. Army, to be Brigadier-General by brevet, to date July 20, 1867. Brevet Lieutenant-Colonel B. J. D. Irwin, Surgeon, U. S. Army, to be Colonel by brevet, to date March 13, 1865. Brevet Major J. H. Janeway, Assistant Surgeon, U. S. Army, to be Lieutenant-Colonel by brevet, to date March 13, 1865.

Re-commissioned.—Assistant-Surgeon H. J. Phillips, to date March 30, 1864, to take rank on the Official Army Register next above Assistant-Surgeon J. H. Kinsman.

Resigned.—Assistant-Surgeon G. L. Porter, Brevet Major, to date July 16, 1868.

Died.—Assistant-Surgeon Cyrus Bacon, September 1, 1868, near Springfield, Illinois, while *en route* to his home, Niles, Michigan, of Bright's disease of the kidneys.

Dismissed.—Assistant-Surgeon P. J. A. Cleary, by sentence of General Court-Martial, approved July 2, 1868, General Court-Martial Orders, No. 45, Headquarters of the Army, Adjutant-General's Office, Washington, D. C., July 2, 1868.

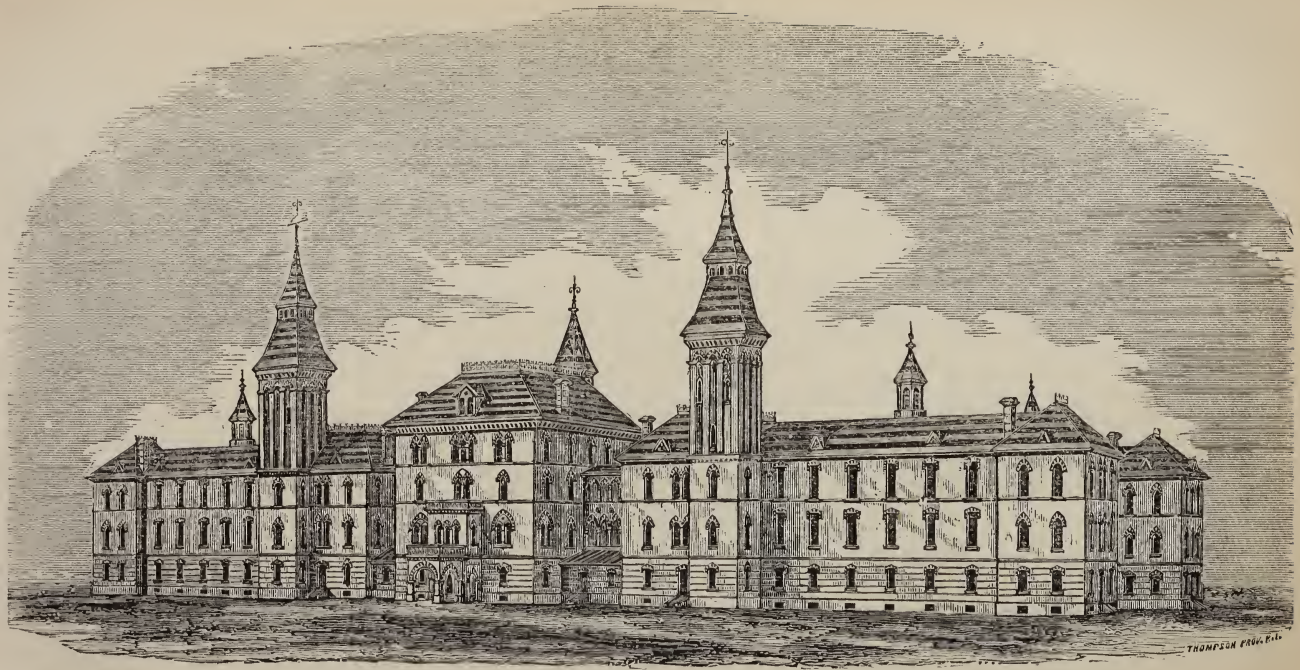
Description of the Rhode Island Hospital.—We are indebted to the kindness of Dr. Chas. O'Leary, an old and esteemed professional associate, for this interesting description of the new Hospital at Providence, R. I.:

This new and beautiful hospital was publicly opened on Thursday, the 1st of October. An address, appropriate to the occasion, was delivered by Prof. Wm. Gamell. Having paid a just tribute to the munificence of the benevolent citizens who founded this noble work of charity, to the skill of the architect who designed and completed the building in all its details, and to the zeal and earnestness of the medical profession, who for years have been advocating the claims of a charity of this kind on the people of Providence, the orator made a strong, fervent, and earnest appeal to the citizens of Rhode Island in behalf of the permanent endowment of an institution that is destined to shed blessings on the community for all ages. No one, contemplating the noble structure, can believe that the charity and humanity which have created it—and of which it is a grand expression and embodiment—will contract or decline under the lessons and teachings its presence in our midst will constantly inspire. Liberal and generous as the State of Rhode Island has been to its patriotic and brave soldiers, and to those invalided in the campaigns and battles of the country, it will not refuse to extend the hand of humanity and charity to the invalids of labor and industry, from which flow its wealth and prosperity. In this age, which knows no masters and slaves, no patrons and dependants, no landlords and serfs, no chiefs and retainers, when each citizen is taught self-reliance—and enters on the struggle of life inspired with the belief and confidence that success depends on himself alone—upon his own thrift and industry, a generous community should regard each worthy member as one of its family, and guard him from want and privation when disease or injury stops his labor, the source of his maintenance. Hospitals are no modern institutions; but the conditions of society that demand them, and the spirit that advocates their claims and support, are different from those of earlier times. Then it was that some princely bishop, or some lordly benefactor, or Christian monastery endowed and founded hospitals of refuge for the poor and friendless, of towns and cities, who could in

the days of sickness and distress claim no care from the lords of wealth to whom they had previously paid no service. Then it was a spirit of almsgiving or bounty to beggars—prompted by Christian charity—that guided one of the wealthy, more benevolent than his fellows, to erect a home of refuge for suffering and helpless fellow-beings. Now this Christian sympathy is not exercised by the rich alone—it is diffused through the whole community—and a feeling of common brotherhood, of generous philanthropy, of Christian charity pervades the whole people, and inspires them to provide that their sons and daughters of toil, whom disease or injury may compel to abandon the ranks of labor, shall suffer no want or privation. It is in the most enlightened and industrious communities that examples of this generous philanthropy most abound; and writing of a New-England institution we may be pardoned for referring to that tribute of praise which foreign writers—as Mill, Lyell, Dupin, and Chevalier—bestow on the New-England States for their many examples of charitable munificence, holding them up to other communities as models of the benevolent use of wealth. Rhode Island will not lag behind her sister States; her people, proud of the institution which private benevolence has founded, will provide that it does not fail for want of means of accomplishing all the good for which it was designed.

The hospital is beautifully located in a position which, from its elevation and the nature of the soil, is remarkably salubrious. An ample area of ground, with capacity for gardens, lawn, and park, encloses it, and isolates it from the noise and bustle of the busy streets, though the location has almost a central position within the cluster of towns which compose the city of Providence. The building displays great architectural beauty; it is of the Lombardo-Italian Gothic style, and in the hands of the architect, all that exquisite grace and beauty, of which this style from its inherent quality is susceptible, have been combined with the most thorough and perfect details of a hospital. The ventilation approaches nearer perfection, perhaps, than in any building of the kind yet constructed; and the arrangements for the comfort and hygienic wants of the patients, as well as for the convenient, quiet, and efficient administration of the hospital, have never been surpassed.

The hospital consists of two pavilion buildings, three stories high, extending from the opposite sides of a central building, and connected with it by corridors of corresponding height. The sides of the two upper stories of the corridors are of columns of exquisite beauty in their architectural effect.



FRONT VIEW OF THE NEW HOSPITAL AT PROVIDENCE, RHODE ISLAND.

These corridors serve as passages between the corresponding stories of the central and pavilion buildings. The central building, besides the superintendent's rooms and quarters for the resident medical officers, with executive offices, contains also a chapel and amphitheatre. The ground floor of the pavilion building is occupied with kitchen, store-rooms, reception-rooms for patients, etc. The two upper floors are devoted to wards, which have each 24 beds, with an allowance of not less than 1,500 cubic feet for each bed. Besides the four wards which the building thus gives, there are a number of private rooms intended for paying patients, and for such cases as it may be necessary temporarily to isolate from the public wards. At one extremity of the ward, and separated from it, are the bath-room, water-closets, and a drop for soiled clothing. The water-closets are connected with the chimney of the steam generator, which is built on the ground apart from the Hospital. By this arrangement all effluvia are swept off by the draught of the chimney, and there is never a back current into the water-closet chamber. The soiled clothing, let down to the lower floor, is conveyed by an underground passage to the laundry, which, with the boiler and engine rooms, forms a separate building. At the other end of the ward are the nurses' room, a pantry, and closet for dumb-waiter, which communicates with the kitchen on the ground floor. The nurses' room overlooks the whole ward. It is furnished with a bureau, with compartments corresponding to the number of beds in the ward, where clothing and articles for the patients' use are contained. Another set of compartments, with numbers corresponding to the numbers of the beds, is intended as a receptacle for the medicine, drink, etc., of the patients respectively. So there is little liability of giving, by mistake, to one patient what is prescribed for another. In the ward and close to or under the beds of the patients no utensils are allowed to remain for a moment after being used. Near the floor at the head of each bed there is an opening in the wall, with a close-fitting slide, intended as a receptacle for the vessels that are in use. These openings communicate with a common flue in which a current is created by means of heated steam-pipes, placed near the top of the building. Thus all noisome effluvia are removed, whilst any communication between this current and the atmosphere of the ward is cut off by means of the slide above mentioned. The natural ventilation of the hospital, favored as it is by its location, is made available to its fullest extent in the construction and design of the building. Besides this, the artificial ventilation approaches as near perfection as practical application has hitherto approached

theoretical principles in this difficult problem; there is a constant stream of air introduced, which is warmed to the requisite temperature by flowing over heated steam-pipes, and is filtered of all dust by passing through a fine spray of water that descends in a shower through wire gauze. The escape-ventilators, placed in the wall near the ceiling, communicate with heated steam-pipes that are placed in two towers that rise gracefully from the pavilion buildings. These towers are stately decorations to the entire structure, and would be worthy of being erected for their architectural effect alone; but the architect, who has so artistically combined beauty of design with every useful purpose, has made these graceful appendages subserve the most vital necessity of a hospital. In the construction of the wards, in their appointments, and in all the subsidiary departments of kitchen, storerooms, etc., the various requirements of a hospital, so far as sanitary science has demonstrated them, have never been more skilfully and thoroughly complied with than in this new Hospital of Rhode Island.[1]

This degree of success, next to the skill of the architect, Mr. Morse, is due to the unceasing care and watchfulness with which the President of the Board of Trustees, Mr. Ives, and Dr. Shepard, member of the Board, have followed the work from first to last. Dr. Shepard has even visited and examined the hospitals of Europe, in order to learn any improvements in administration and internal arrangements it may be still beneficial and desirable to adopt.

Besides contributing largely of their wealth toward it, these gentlemen have watched this institution, in its growth from the first stone laid, with as much fondness and care and cheerful anticipations as ever did rich man the building of the mansion destined to be the home of his family, and heirloom to his posterity. The honor paid in the community to such examples of benevolence, and unselfish devotion to objects intended for the welfare and protection of their fellow-beings, will not fail to inspire others to imitate their virtues, and extend the blessings of benevolence where suffering humanity needs them.

In an institution which seizes the mind of the observer only by its completeness and admirable adaptation to its special use, if there be any defect, it is in the arrangement designed

[1] The internal arrangements of this hospital will be more readily appreciated by a reference to the ground plans, which the reader will find stitched in at the close of the reading-matter of this number of the Journal. It was impracticable to procure them in season to insert them in the body of the Journal, nor was it deemed necessary for the purpose of description. —*Ed. N. Y. M. J.*

for the consultation of outside patients. There is but one chamber or hall for the reception and consultation of all classes of patients, medical, surgical, male and female. To one accustomed to the hospitals of Europe, and to the hospitals of other cities in America, this is a defect, and I apprehend will, on trial, be found so by the attending faculty.

The Nerves of the Heart.—Let us now take a peep into the mysterious laboratory of the College de France. Here we fall into the company of a dozen *sous-savans*, the aids of the learned professor and academician. They are called technically "the young *savans* of the College de France," simply because they are younger than their master in science, for on looking around we see gray beards among them. Those men are the hope of the experimental school of physiology; they have banded together under the eye of the master to perpetuate and glorify the name of the school of Paris. To-day they are unknown; to-morrow they will be illustrious.

One of them, Dr. Cyon, has just been awarded by the Imperial Academy of Sciences the annual premium for discoveries in Experimental Physiology, for the discovery of two nerves going from the spinal marrow to the heart, and belonging properly to that organ. His paper is entitled, "Researches on the Innervation of the Heart by the Spinal Marrow," and, besides a description of the newly-discovered nerves, relates demonstrations of new phenomena produced by these nerves in the functions of the important organ to which they belong.

The first nerve, which he calls the special cardiac accelerator nerve, emerges from the spinal column with the third branch of the cervical ganglion, and, when excited on the living animal, the heart's pulsations are increased. The same effect is produced whether the excitation is made on the root of the nerve, or on the spinal marrow near the nerve. The other nerve discovered by M. Cyon, uniting the heart to the spinal marrow, is not a motor but a sensitive nerve. By the medium of this nerve a reflex action is produced, starting from the heart, and influencing the capillary circulation in all the organs of the body. The most important facts in the physiology and pathology of the heart may flow from this discovery.

Here we have been going on these thousands of years, without knowing that the heart had nerves of its own. We have been wondering, this long time, how this mighty hydraulic machine got power enough, from the feeble branches of the sympathetic and pneumogastric nerves, to maintain with regu-

larity its hundred thousand beats a day. Was this mysterious power hydraulic, or vital, or mixed? The physiologists were always quarrelling on the subject, and they will continue to quarrel, perhaps, all the same, for one discovery brings another, and thus continues to open new fields of discussion.

But, honestly speaking, do we not owe our hearts an *amende honorable?* Where is there a friend so faithful? We profess wonder at the faithful watch that keeps up its tic-tac for a twelvemonth without stopping, but none for the heart that keeps up its regular tic-tac for a hundred years! The ingratitude of man has no bounds. He will eat too much, or drink too much, or run too much, without ever a thought for the overwork he is thereby giving his already overworked and faithful friend. He will push it by excesses into disease, and then complain when it grows tired, and threatens to stop. The physiologists, on their part, have a world of reproaches to charge themselves with, for abandoning this important organ in ignorance and doubt. "It will go on in its endless work anyhow, therefore let it go!" They were pretty sure that the feeble supply of nerves from the cardiac plexus did not supply the whole power of this wonderful organ. They knew that a great mystery hung over it, and that, whether its power was vital, or mechanical, or both, there was not such a distribution of nerves as accounted satisfactorily for all the phenomena of its movements. Now we know why. We have got a reënforcement of nerve-power, coming this time directly from the seat of locomotion and sensation in the spinal marrow. We have got two nerves, two telegraphic lines, going straight, without relay or branches, from the spinal marrow to the heart, one for motion and the other for sensation, and now we begin to understand better the heart's action, and its various manifestations in health and disease. M. Claude Bernard is already at work with his band of colaborers at the new nerves, and some important indications have been established, or nearly established, in relation to disease of the heart.—*Paris Correspondent of the Times.*

Deaths by Suicide.—Suicide is on the increase, steadily, alarmingly. The real number and the real increase, annually, of suicides, is probably greater than would appear from official records. Many deaths, recorded as accidental, should, doubtless, be set to the intentional account. Parents and friends willingly conceal what official recorders find no call to scrutinize too narrowly. From 1827 to 1866, in France, the increase of suicide has been annually, and by the million of inhabitants, from 54 to 134 cases. The whole num-

ber in the first five years, from 1827 inclusive, was annually 1,739; in 1866 it amounted to 5,119. The increase in the intervals is 148 per cent.; the increase of population in the same interval is a fraction under 20 per cent. The increase is much more and proportionately much more with men than with women. The test applied by countries and by the million of inhabitants, shows 123 for Prussia, 110 for France, 69 for England, 43 for Austria, 32 for the United States. Doubtless the greater accuracy in respect of the nature of deaths, which is observed in the mortuary records of Prussia and France, partly accounts for the comparatively small suicidal cipher set against Austria and the United States. But an auxiliary explanation will offer itself presently. The most suicidal people are the Danes and those of the North German States. The test of sex shows, in 14 states of which more or less perfect statistics are at hand, 30 females to 100 males; that of age shows an increase by years till the age of 60 or 70, the term being later with men than with women. The test by seasons shows for suicides, as for crimes committed on persons, and for attacks of insanity, an augmentation in summer over the winter months. The means of suicide are, in the order of their frequency of use: strangulation and drowning, fire-arms and cutting or thrusting instruments, poison. Poison and drowning are the favorite means with females. It is notable here again, in respect of seasons, that drowning is much rarer in winter than in summer. The avoidance of pain, at the first blush, is instinctive even with the seeker of death. Women yield oftenest to moral influences—love, jealousy, domestic troubles, excited sentiments of any kind; men to material trials—poverty, business losses, drunkenness and debauchery. Note that these last two causes figure very slightly as causes of female suicide. In Denmark one-third of the male and but one-fifteenth of the female suicides are attributable to drunkenness. Cities furnish an enormously disproportionate number of volunteers of death. The seventh part of all French suicides are committed in Paris. The proportion of Berlin to all Prussia and of Copenhagen to all Denmark is still greater. Fewer married than single people, but fewer maids and bachelors than widows and widowers; fewer agriculturists than mechanics and merchants; still fewer of these last than members of the liberal professions, and yet fewer of these than of the remaining non-professional population (of France especially, where the military, bureaucracy, domestic and other *employed* more or less senile classes are enormously greater, notably of the male sex, than in some other countries), the proportion of the agricultural to

the non-professional class being, in 100,000 individuals, as 9 to 59; fewer Jews than Christians; fewer Catholics than Protestants, die by their own hand.—*Paris Correspondent of the Tribune.*

Divorces in the United States.—Statistics, carefully compiled, show that divorces are on the increase in the United States. For instance, in Vermont, during the seven years ending with 1866, the ratio of divorces to marriages was one to twenty-one; in the year 1866, one to nineteen. In Massachusetts, during the four years ending with 1864, the ratio of divorces to marriages was one to forty-four; in the year 1864, one to forty. In Ohio, in the year 1866, the rate of divorces to marriages was one to twenty-six. In Connecticut, during the eight years ending with 1867, the ratio was a little less than one to eleven; and the last of those years it was a little less than one to ten. The Nutmeg State seems to have a very considerable number of couples anxious to untie the matrimonial noose. Illinois must look to her laurels.

The Origin of Bacteria.—A German lady, Frau Lüders, of Kiel, has been investigating this matter with the microscope, and has published her conclusions in Schultze's *Archiv*. Her paper is one of very great interest, and her researches have been ably and carefully conducted. She believes that she has proved—what many fungologists were prepared for—that vibriones (leaving aside the question of there being more than one species) are produced from the spores and germinal filaments of various moulds or fungi, amongst which are enumerated Mucor, Penicillium, Botrytis, Torula, Manilia, Aspergillium, Leptosporium, Arthobotrys, Acremonium, and Vorticillium. It is impossible here to give an account of the precautions adopted in growing these fungi, but they appear to have been satisfactory. Prof. Hensen, of Kiel, strongly supports all Frau Lüders says. She is also induced to believe that the blood of living animals contains vibriones, either in the catenated form, or in that of the constituent granules; but, during life and until putrescence commences, these are always quiescent and show no signs of active existence. In support of this, the following experiment by Prof. Hensen is quoted: The extremity of a glass tube bent in the form of a W, with the ends drawn out and quite closed, and which had been exposed for half an hour to 200° C., was thrust into the heart of a recently-killed Guinea pig and then broken off. After the blood had sucked into the tube from the other end, which was melted off in order to remove any

fluid that might adhere from the lips, the ends of the tube were sealed, and it was kept at a temperature of from 13° to 15° C. From one of the several tubes thus prepared the point was removed after two days, and a drop of blood expelled on the next day, which, when examined with the microscope, showed large quantities of fungus-granules; chains and rods; mobile rods were rare. Milk, eggs, the mouth, and many organic fluids, contain vibriones in this condition. Though Prof. Hallier, the greatest authority on microscopic fungi, does not accept Frau Lüders's results as to the connection of "moulds" and "vibriones," yet her researches on the blood have great importance in connection with his own. Prof. Hallier has recently announced that he has been able to isolate and identify, from the blood of typhus-fever patients, a distinct form of fungus; Prof. Salisbury, of Ohio, U. S., made known the observation of distinct fungi in the fluids of persons suffering from other contagious diseases. Are we not advancing to a great fact as to the nature of such diseases? Fermentation and vaccination may come to mean much the same thing. Frau Lüders has also shown that "yeast" may be grown from many "moulds," as first demonstrated by Hallier.—*Quarterly Journal of Science.*

Anecdote of Bouillaud.—Here is a good story told of M. Thiers: M. Guizot gave, in 1846, a ball, at which the most eminent people of Paris were present; among them were M. Thiers and Dr. Bouillaud. The yellow fever was then at Marseilles, which M. Thiers represented in the Chamber of Deputies. He asked Dr. Bouillaud to explain the disease. The doctor gratified M. Thiers's request, but failed to satisfy him. He exclaimed, "Doctor, I have studied that disease, and I have seen nothing you have described." Dr. Bouillaud replied: "M. Thiers, you are familiar with history, and write it better than most men; do you remember this historical incident? It is said that, when Hannibal was Prusias's prisoner, he heard of a man named Fabius, who delivered admirable lectures on military tactics and the art of war. Hannibal wished to make Fabius's acquaintance; he sent for him; he heard him, and exclaimed, '*Multos vidi delirare homines, sed numquam magis quam Fabium.*'"—*Am. Lit. Gazette.*

A Prize of £1,000.—The Marquis of Ourches has left the above sum, to be given next year by the Academy of Medicine of Paris to the discoverer of accurate means of distinguishing apparent from real death. The full prize will be awarded if these means may be understood and applied by any one; but it will be reduced to £200 if the method can only be used by medical men.

The Administration of Drugs.—I wish to offer a few suggestions, concerning the administration of medicines, which may tend to make this daily business of ours more available in increasing therapeutical science. (1.) Let us aim at giving only *one drug at a time.* I do not say this is always possible; but at all events let us keep the desire in our minds, and reckon a prescription good in an inverse ratio to the number of ingredients. This simplicity conduces not only to the good of science, but of our individual patients, for it soon makes us much more ready at suiting the special remedy to the special case. (2.) It is important, when we change our treatment, to allow a certain sufficient interval, different in different instances, between leaving off one medicine and beginning another. The experiments of Böcker and others have shown us, first an action of the drug lasting after its apparent disappearance from the body, and secondly, a reaction of the system opposite to, though weaker than, the original action. Advantage will accrue to the patient often from this rule too. For instance: hyoscyamus, given for hypochondriasis or mental depression, may be left off almost directly it has begun to produce its beneficial effects, and those beneficial effects will still go on toward restored health. Hydrochlorate of strychnia will continue to invigorate the peristaltic motions of the stomach and intestines, so as to produce steady digestion and evacuation, for days after such a soluble salt must have passed away. (3.) It is advisable for each observer to have as short a pharmacopœia as possible. The best workmen use the fewest tools—ay, and those who use the fewest tools become the best workmen. They become more adroit with them, know them better, and are able to instruct others in their employment. (4.) The union and coöperation together of those who are working at the same subject are of incalculable value. Incalculable—because you have not tried it. The skeleton of the machinery exists in the British Medical Association. Why should not each Branch or group of Branches take up a drug, and let us know after two or three years their experience of its action?—*From Address of Dr. T. King Chambers at the recent Meeting of the British Medical Association.*

M. Monneret, Prof. of Internal Pathology of the Medical Faculty of Paris, author of the "Compendium" and also of a well-known work on Practice, which is still unfinished, died in Paris, recently, of disease of the heart. It is somewhat singular that three members of the Faculty of the School of Medicine, of Paris, have died during the past year, leaving books unfinished.

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Original Communications.

ART. I.—*On the Treatment of Stricture of the Urethra.* By WM. H. VAN BUREN, M. D., Professor of Principles of Surgery and Diseases of the Genito-Urinary System, in the Bellevue Hospital Medical College. (Read at the Meeting of the Medical Society of the County of New York, Nov. 2, 1868.)

BY the request of our president, I have to ask your indulgence while I lay before you my experience *on certain points in the treatment of stricture of the urethra.* The frequency with which this affection leads to fatal disease of the bladder and kidneys, when not controlled, renders its treatment always a matter of interest to the practical surgeon. The familiar cases of bladder disease with long-standing stricture, which encumber the wards of every large hospital, prove that this treatment is not yet perfect; and the necessity of watching their progress, without the power to arrest it

by prompt and certain means, is a constant source of annoyance and regret to the hospital surgeon.

To ascertain the modes of treatment best calculated to diminish the number of these unfortunate cases, which have their analogues constantly recurring in private practice, will constitute the main object of my remarks.

The numerous operations which have been devised for the prompt and permanent cure of organic stricture bear witness to the strong desire of surgeons to accomplish this end. Each of these operations has had its period of popularity, some of them under the auspices of surgeons of great merit and large experience; but as yet the desideratum has not been attained. The prize offered by the old French marquis, who died after years of suffering, from the consequences of stricture, leaving his fortune in trust to the Academy of Medicine of Paris, for the discoverer of the best method of treating stricture, is still accumulating its interest. The Academy voted a prize to Reybard, the French surgeon, who so ably and enthusiastically advocated *internal incisions* as *the* method of cure. But experience has not confirmed his claims. His internal incisions were found to be too dangerous when made large enough and deep enough to accomplish their object of securing permanent enlargement of the canal, and they have been abandoned by judicious surgeons, except for strictures near its orifice.

More recently, the claims of Mr. Syme, of Edinburgh, were proffered and rejected by the French Academy, for the operation by *external incision* which bears his name—an excellent operation, but cramped in its application by mannerism, and damaged by too extravagant claims in its behalf.

Still more lately, Mr. Holt, of London, has proposed and largely employed *forcible dilatation*, or *laceration*, as it should be properly called, as the great remedy for stricture. But time, the inexorable critic, has demonstrated that Holt's operation requires periodical repetition; that it may secure a limited term of relief, but that it cannot effect permanent cure of old organic stricture. And this remark applies to all the other modes which have been devised for the cure of stricture by forcible stretching, or laceration.

For extreme cases of aggravated stricture, which have led to retention or extravasation, the operation of perineal section as described by Arnott—the *boutonnière* of the French—is held in reserve—an operation regarded by most surgical authorities as a difficult and desperate resource, and not coveted by operating surgeons.

My own experience has led me to think more favorably of this operation—with certain modifications, of which I will presently speak.

I should be glad here to introduce cases illustrating these several operations for the cure of stricture, and justifying the opinion I have expressed of their merits; but lack of time forbids. I have tried them all, and sought to profit by the experience of others in addition to my own, and I will endeavor to give you the results at which I have arrived.

It is a fundamental point in connection with the treatment of stricture, *that absolute cure of the disease, by any of the processes in use, is exceptional.* As a rule, a dilating instrument must be introduced at intervals, after active treatment has ceased, in order to prevent recontraction. This fact cannot, in my judgment, be too strongly asserted. I never consider that

I have done my duty by a patient with stricture until I have taught him to introduce an instrument for himself, with all due care and caution, and impressed him with the conviction that the *permanency of his cure lies* WITH *himself*. Many patients, before they fully comprehend the serious nature of an obstruction in the urethra, revolt at the drudgery of what they consider an unnecessary and an unnatural proceeding. But, when the surgeon is earnest and positive, the patient rarely resists; and I have never received an answer to the argument that the introduction of a smooth steel sound into the urethra every Sunday, for the purpose of preserving the integrity of a damaged water-passage, was no more unnatural a proceeding than the daily removal of the beard from the face.

I feel pretty confident that most permanently-cured strictures have been based upon faulty diagnoses. The diagnosis of stricture in its earlier stages is not very easy, especially in the deeper portions of the urethra. An irritable and contractile *compressor urethræ* muscle simulates stricture very closely; and I am cognizant of cases which have deceived very competent practitioners. This sort of muscular irritability, which is generally sexual in its origin, is usually benefited by the judicious use of instruments; so that the cure of such simulated strictures is apt to follow promptly, and, where marriage intervenes, it is permanent.

For the positive diagnosis of stricture, a series of bulbous bougies is indispensable. The preëxistence of gleet, or, in its absence, the presence in the recently-voided urine of floating filaments, which under the microscope prove to be rolls of pus-corpuscles enveloped in urethral mucus, is a strong point of constructive evidence.

I say nothing of the endoscope as an aid in the diagnosis of forming stricture, for the use of this instrument requires an amount of practical training which interferes with its general application.

For the great majority of cases of stricture of the urethra, especially of those occurring before middle life, the proper treatment is by slow and gradual dilatation.

This, in my judgment, is effected preferably by conical steel instruments, introduced with great gentleness and care, every third, fourth, or fifth day, and left in the canal not more than five minutes. The object to be attained is the stimulation of vital absorption in the newly-organized material which constitutes the increased thickness of the urethral walls, and not mere mechanical dilatation. This absorption often goes on for a week after the use of the dilating instrument, and therefore it should not be reintroduced after too short an interval. Too frequent use of instruments is liable to irritate and inflame the altered surfaces; and inflammation is incompatible with absorption. I have seen serious injury to the urethra follow the *daily* use of sounds.

Where a stricture will not readily admit a steel sound of the size of No. 6, I prefer to employ the conical French gum-elastic bougies, as I recognize danger of making false passages with smaller steel sounds.

In asserting that the great majority of strictures occurring before middle life are amenable to cure by dilatation, I must except strictures of *traumatic* origin —limiting the rule to those caused by gonorrhœa. Under the head of traumatic stricture I am in the habit of including lesions of the urethra by the nitrate of silver; and, in my experience, these are by no means rare.

Whether the result of mechanical or chemical injury to the urethral walls, these traumatic strictures have presented themselves to my observation much more frequently than I had been led to expect from the comparatively meagre details concerning them to be found in works on surgery. And of all the varieties of stricture, their treatment is the most difficult and unsatisfactory. They are apt to be "irritable;" and often so sensitive to the contact of instruments that a gentle exploration is liable to be followed by a chill, or an attack of retention of urine. They are generally very tight or narrow; dense and cicatricial in consistence; yielding very slowly to dilatation, and rapidly recontracting; possessing the characteristic of "resilience" in an eminent degree.

The gravest in its consequences, and at the same time the most common in occurrence, is that variety of traumatic stricture which results from falls upon the perineum, in which the urethra is crushed against the sharp edge of the pubic arch. This lesion occurs of necessity always at about the same point of the canal, nearer to the neck of the bladder than the deepest site of idiopathic stricture, and it is attended by more or less transverse crushing of the urethral walls.

In some experiments upon the dead body, made at my instance, by my friend Dr. Gouley, several years ago, we found that a pretty clean transverse cut of the urethral mucous membrane was the first and most constant lesion produced by a crushing blow upon the perineum. The lining membrane of the canal was in several instances completely divided, like that of an artery by the ligature, while its more fibrous outer coats escaped division.

Bad cases of stricture following this sort of injury

are not very rare in practice. I have seen it from the kick of a horse, from falls astride of a beam, a fence, the wheel of a carriage, and in several instances from falling with one leg through a coal-hole in the sidewalk. Boys are not unfrequently victims of this accident; and, for obvious reasons, they are the worst and most uncontrollable cases of stricture we are liable to meet.

Traumatic strictures thus produced tend to occlude the urethral canal very rapidly; and they give rise to retention of urine, over-distention and inflammation of the bladder, perineal abscess, and extravasation of urine, at an earlier period than strictures following gonorrhœa. Generally sensitive to the contact of instruments, dilatation, ever so skilfully managed, makes no headway against this kind of stricture, and is obviously not the remedy.

For strictures of this class, and for those idiopathic strictures which, from neglect and long duration, have involved the urethral walls in similar dense and cicatricial induration, it becomes the duty of the surgeon to employ measures of a more prompt and effective character.

Each of the operations for enlarging the narrowed canal—by cutting, stretching, or laceration—to which I have alluded, whilst falling short of universal applicability, has left us in possession of valuable practical knowledge. Thus, we learn, from Reybard's extensive trial of cutting instruments within the canal, that a certain degree of permanent enlargement of calibre can be secured by free longitudinal incisions; and that the new material thus added to the narrowed circle can be prevented from contraction by the use of the bougie or sound. And we learn incidentally, from his cases and experiments, that transverse wounds of the urethra are always followed by stricture.

Mr. Syme has taught us that *external* incision is the safe and proper remedy for bad strictures of the deeper portions of the urethra; and also that the cases are rare in which a delicate instrument cannot be introduced, even through the tightest stricture, into the bladder, to serve as a guide for the knife. We learn also, incidentally, from the experience of this eminent surgeon, that—in order to reap all the advantages from the operation of external incision—it is not necessary to keep a catheter in the bladder while the wound is healing; that the occasional introduction of a full-sized sound, at proper intervals, is alone required. From Mr. Holt's more recent experience in forcible dilatation, we have learned the valuable lesson that the urethra is singularly tolerant of this sort of violence; that the longitudinal lacerations of the strictured portion—which I believe always follow the application of his instrument—are also well borne; and that these lacerations are followed by as good results—in apparently permanent enlargement of the canal—in proportion to their extent and depth, as the more dangerous internal INCISIONS of Reybard.

From the occasional necessity of a resort to the old operation of *perineal section without a guide to the bladder*, there is no escape—unless by adopting Mr. Cock's practice of puncturing the urethra behind the stricture, which, although preferable in some respects to puncture from the rectum, like this latter operation, affords at best but temporary relief.

But we have succeeded in rendering this necessity still more rare since the French have supplied us with bougies of improved material and more slender proportions. It is not often, with the invaluable aid of ether or chloroform, that the practised surgeon fails in get-

ting one of these delicate instruments into the bladder to serve as a guide.

Availing himself of the broad facts gained by this experience in the surgery of the urethra, it is the privilege of the surgeon of the present day, in undertaking the treatment of a bad case of stricture, to select the mode of operation best suited to its peculiar features.

My own experience has led me to adopt the following modes of practice :

In all *organic strictures situated at, or near, the urethral orifice*, where hæmorrhage is controllable, I employ free incisions from within, and prefer for this purpose the instrument known as Civiale's urethrotome—using, subsequently, the largest steel sound that the urethra will admit, to prevent contraction while the wound is healing.

In *strictures situated in the deeper portions of the urethra*, which resist cure by gradual dilatation, and through which an instrument can be introduced as large as No. 4 of the ordinary scale, I employ forcible dilatation, carried on slowly and gradually to the point of rupture or laceration, and as much farther as to permit the subsequent introduction, with entire freedom, of the largest-sized conical steel sound which the healthy portions of the canal will admit. For this purpose I find the best instrument to be the "dilator" devised by Sir Henry Thompson. It works by means of a screw, and has an index on its handle pointing to the size of the sound which the dilating portion has reached. In the use of this instrument Sir Henry Thompson proposes to *dilate as much* and to *tear as little* as possible; he therefore performs the operation slowly.

I see no advantage in the avoidance of laceration,

and should not insist upon this point. I have always found that, at a certain stage of the dilatation, the free flow of blood indicates that laceration has taken place; and I recognize that more is gained, in the end, by carrying it to the fullest possible extent. There is this advantage in the slowness of the operation: that it causes less pain; and, in several instances, I have left the patient to turn the screw himself as the pain subsided. This diminishes markedly as soon as laceration occurs, and it is rarely so severe as to require ether or chloroform. The subsequent introduction of a full-sized steel sound speedily arrests bleeding by its pressure. In some cases, in order to get the full benefit from the forcible dilatation, I have also incised the orifice of the urethra, before terminating the operation.

I have had reason to be well satisfied with this operation; but, as in most other surgical procedures, its best results are only to be secured by judicious after-treatment. It possesses these positive advantages: by affording, at once, free passage to the urine, it diminishes the danger of progressive disease of the bladder; by its promptness it offers, in many instances, a better prospect of more permanent relief to the hospital patient who cannot or will not await the slower results of ordinary dilatation; and, finally, it causes less dread than the knife, and is therefore more generally available.

In more aggravated cases, where the stricture, although still permeable, is tighter or more unyielding, resilient, or irritable, and complicated perhaps with irritability of the bladder, false passages, threatened retention, or urinary fistulæ, or with perineal abscess or extravasation of urine, actual or impending, I prefer *external incision* of the stricture from the perineum.

Introducing a fine bougie into the bladder as a guide, a grooved staff, open at its extremity, is passed down to the stricture, and held in contact with it by an assistant. The bougie occupies the groove of the staff, and projects beyond it, through the stricture, into the bladder. Those of Benas, of Paris, containing whalebone, are preferable for this purpose. Commencing with a free incision in the central line of the perineum, the parts are successively divided until the extremity of the staff and the bougie are brought in view, and, guided by them, the stricture is divided longitudinally and freely, by means of a delicate probe-pointed knife —especial care being observed to carry the incision for some distance in either direction beyond the stricture, as this measure has great influence in preventing subsequent contraction. I make it a point, in this operation, to remove every obstruction to the easy introduction of a steel sound of the largest size—which is passed at once, and if any difficulty or obstacle is still encountered it is sought for and removed, if necessary, by more free division of the stricture. If, as often happens, other strictures exist in the interior portions of the urethra, they are removed at once by laceration, or, if near the meatus, are incised from within by Civiale's urethrotome. No instrument is left in the bladder; but, after the second or third day, a full-sized conical sound is introduced daily and immediately withdrawn. The urine is allowed to escape by the perineal wound, as after lithotomy. The scrotum is elevated by a sling, to prevent infiltration or abscess, which are liable to occur when this precaution is neglected.

In examining the records of the New York Hospital some years ago, I was struck by the frequency

with which chronic cystitis and perineal fistula followed the operation of perineal section for stricture; and, on further investigation, I was led to ascribe these consequences to the practice, which seems to have been uniformly pursued, until lately, of tying a catheter, generally of small dimensions, in the bladder, and leaving it there for an indefinite period—with the object, apparently, of facilitating the healing of the perineal wound.

I had seen the same results follow in my own operations, with others even more serious, and I finally arrived at the conclusion that the presence of a catheter in the bladder, under these circumstances, accomplished no useful object, but was, on the contrary, an unmitigated evil, keeping up inflammation in the urethra and bladder, and preventing the closure of the perineal opening.

Influenced by this conclusion, and also by Mr. Syme's experience, I adopted the plan of after-treatment I have just described, and am fully satisfied with its advantages. *The habitual passage of the urine through the perineal wound does* NOT *prevent its healing steadily and promptly, by the second intention, provided that there is no recontraction of the stricture.* This is proved by the results of lithotomy.

I have operated in this manner, I find, sometimes; and, although preëxisting cystitis has in some cases failed to get well after the operation, I have never had a permanent fistula, except in the case of a boy, where the difficulty of keeping up the regular introduction of the sound was insuperable. The cure has been complete generally in from one to two months, and permanent—with this condition, that the patient introduced his full-sized sound every week.

Sir Benjamin Brodie held that a perineal fistula will always close spontaneously when the urethra is restored to its normal dimensions; and, more recently, Sir Henry Thompson has asserted that the more thoroughly a stricture has been dilated, the less the prospect of its recontraction. I have learned to regard these as correct surgical opinions, and I attach great importance to the regular use of a sound of the largest size after these operations, as well as after an apparent cure by dilatation, and, whenever feasible, the instrument should be introduced by the patient himself.

In those desperate cases, which are occasionally thrust upon us, where extravasation of urine has occurred, or is impending, and where no instrument, however delicate, can be carried through the stricture into the bladder, the perineal section is to be undertaken without a guide. This is the best course to be adopted, because it promises not only immediate relief to the bladder, by affording the least objectionable artificial route for the urine; but also, if successful, the division of the stricture offers the best chance for its more permanent cure.

The question of delay, to secure a further chance of getting an instrument into the bladder, offers room for the exercise of delicate judgment and skill. The condition of the bladder and kidneys should be ascertained by careful palpation of the hypogastrium and loins, and the patient's general condition kept steadily in view. Injections of oil into the urethra, and the various devices for avoiding false passages, the influence of the warm-bath, opium, and finally of ether or chloroform, are all to be judiciously tried, with the aid of the finer and more delicate bougies lately brought into use, in the hope of securing a guide to the bladder.

When success in this respect is happily attained, the operation is at once rendered safe and certain in its result.

If compelled to operate without a guide, I have found it a good rule, where the anatomical landmarks have been obliterated by disease, to seek for the hole in the triangular ligament, through which the urethra escapes from the pelvis. And if the thickening and induration of the perineum, by abscesses and fistula, have added to the difficulty of the operation, there is another morbid change caused by the urethral obstruction, which materially aids the operator, and which I do not remember to have seen noticed in the books. I refer to the almost invariable dilatation of the urethra behind the stricture. I have seen it frequently large enough to admit the finger.

The first object in this operation should be to tap the urethra behind the stricture, so as to afford a free outlet to the urine; the second, to divide the stricture or strictures, so as to admit a full-sized instrument readily into the bladder, and, if possible, to avoid the necessity of tying in a catheter.

I have never had occasion to regret undertaking this operation, and in some cases have attained very satisfactory results.

I have thus briefly indicated the several modes of treatment which seem to me best adapted for the relief of stricture of the urethra, in the present state of our knowledge. They may be summed up as—*dilatation; incision from within the canal; laceration; external incision*, or what would be better called *perineal urethrotomy with a guide;* and, *finally, perineal urethrotomy without a guide.* I have endeavored also to suggest the class of cases to which each of these modes of cure is especially applicable.

ART. II.—*A Case of Aphasia, with Remarks.*—By CHARLES C. LEE, M. D., Attending Physician to the Charity Hospital, etc.

IN the January and April numbers of the *Psychological Journal* for the present year, appeared two highly interesting articles on the pathology of aphasia. In the first of these especially, which was an able and exhaustive *résumé* of the subject by Dr. E. C. Seguin, of this city, an appeal was made to the profession to place on record additional cases, that, by the comparison of a more extended series of observations, a nearer approach to solving the vexed question of aphasia might be reached. With this object I offer the following case, which occurred during my service last summer at the Charity Hospital:

CASE.—John W——, aged 52, was admitted to the Hospital for herpes zoster, August 16th. Upon examining him the following day, I was at once struck by his inability to articulate, which was the more striking in contrast with his intelligent expression and the readiness with which the tongue was extended. He understood perfectly all the questions put to him, and, on being asked how long he had been speechless, he counted on his fingers "one, two, three," etc., up to ten years, naming the numerals in succession; but no effort on his part could compass the expression "ten years" without counting on his fingers. He said "yes" and "no" without difficulty, and answered correctly by those monosyllables all simply affirmative or negative questions. The reading and writing power was quite lost, although he could formerly write with ease; but there was not the slightest hemiplegia either of motion or sensation. The case was, therefore, apparently one of

the second grade of aphasia, according to Trousseau's classification, viz., amnesia of speech and of written language, without loss of gesture. With some difficulty the following history was elicited, partly from the patient himself and partly from his wife.

Ten years ago, while in seemingly good health, he was seized with convulsions during sleep; these lasted for two hours, and were epileptiform (as described), being followed by stertorous breathing and partial coma for three days. There was also retention of urine during these three days, after which the patient gradually regained his normal health, with the exception of defective vision of the *right* eye and entire loss of speech. His memory was also for a long time impaired, but now seems restored. He has never been able to read or write since the date referred to, and can now only say "yes" and "no," and count up to ten. The right eye is sightless, and presents the sequelæ of general ophthalmitis; the globe is "boggy" upon pressure, which produces no scintillations, and no sympathetic irritation has existed in the left.

No cardiac disease or fatty degeneration could be detected, nor were the arteries atheromatous—no *arcus senilis* in either eye; the patient's appetite was good, and his habits moderately active. His gait was erect and steady, and, as above stated, not the faintest evidence of hemiplegia existed. These points were verified by repeated examinations, and the record is transcribed from notes taken at the time.

The patient soon recovered from his attack of herpes, which was treated in the usual manner, and was then placed on the use of bromide of potassium, more for the purpose of keeping him under observation than with the expectation of any therapeutic result.

In a short time, however, he became dissatisfied—alarmed, doubtless, by the amount of attention he received from his physicians—and insisted on leaving the hospital, whence he was discharged September 2, 1868. The case is still under my observation, and exhibits at the present date no change from the condition described.

The most noticeable feature in the above case is the length of time (ten years) the aphasic condition has lasted. Both the patient and his wife are positive in fixing the date of his seizure in 1858, which makes the duration ten years without such an advance of the cerebral disease as to cause hemiplegia. This, however, is not without precedent; for one of the two cases, upon which M. Broca ventured to propound his remarkable localization of the faculty of language, was a patient of Dr. Auburtin who had been speechless for twenty years. At the autopsy of this case the lesion was found in the left frontal lobe, but was not limited to the third frontal convolution.

It is no part of the object of this paper to discuss the literature of Aphasia. This has been thoroughly done by Dr. Seguin, whose record includes all that has been published on the subject from the time when Bouillaud located the faculty of speech in the two anterior lobes of the brain (1825), to the present date, with one exception. We allude to the essay of Dr. Albert Carrier,[1] which contains an excellent historical sketch of the subject, with an analysis of the cases of Fabret, Hughlings Jackson, Vulpian and Charcot, Lancereau, etc., and details some heretofore unpublished. Dr. Carrier is a strong partisan of Broca's doctrine, in

[1] Études sur la Localisation dans le Cerveau de la Faculté de la Language articulé. Par le Dr. Albert Carrier. 1867. Paris: Baillière.

spite of Trousseau's and Gratiolet's able arguments to the contrary, and seems to throw some grave doubts upon the priority of Dr. Marc Dax's essay. The author also makes an elaborate and ingenious effort to explain the frequent association of aphasia with disease of the left cerebral hemisphere, based upon Gratiolet's observations of the more rapid development of the left frontal convolutions than those of the right side; but he seems so warm an advocate of M. Broca's views as to be unable to judge the question impartially.

ART. III.—*Atropia: Its Chemical, Physiological, and Therapeutic Action; together with Experiments instituted to ascertain its Toxicological Properties.*[1] By SAMUEL R. PERCY, M. D., Professor of Materia Medica, Physician to Mount Sinai Hospital, etc.

ATROPIA. $C_{34}\ H_{23}\ N\ O_6\ \overset{+}{At.}$

SECTION FIRST.

DIVISION FIRST.—*History.*

ATROPIA is an alkaloid obtained from all parts of the belladonna-plant, but in largest quantity from the root.

It has also been inappropriately called atropina, atropine, atropin, atropinum. The termination in *ia* designates it an alkaloid, and, to avoid confusion and the liability to mistakes, the term atropia only should be used.

Vauquelin[2] made an analysis of the juice of the

[1] An essay, for which was awarded the prize for the years 1867 and 1868 of the Alumni Association of the Medical Department of Columbia College.

[2] Annales de Chimie, tome lxxii., p. 54.

belladonna, but did not succeed in isolating any alkaloid principle.

Brandes,[1] in 1819, repeated this analysis, but with no better results.

The alkaloid atropia was first exhibited in 1828, nearly simultaneously, by Geiger[2] and Hesse. In 1833, it was obtained by Mein.[3] Geiger and Mein obtained by their method about 20 grains of nearly pure atropia from 12 ounces of freshly-dried root of two and three years' growth.

Von Planta[4] made minute chemical analysis of atropia, and found that its chemical reactions were almost identical with daturia.

Schroff[5] repeated these analyses, and also tried both alkaloids therapeutically. He says that their chemical analyses are both alike, but that daturia is nearly twice as powerful as atropia.

Lübekind,[6] in 1839, stated that he had split up atropia, and had found in it a second alkaloid which he named belladonin, which had different chemical reactions from pure atropia.

Division Second.—*Processes for obtaining Atropia and its Salts.*

Mein adopted a process for obtaining atropia which may be found in full in *Journ. de Pharm.*, tome xx., p. 87.

M. Rabourdin, an apothecary of Orleans, published, in 1850, a formula for preparing atropia by

[1] Annalen der Chemie und Pharm., Bd. i., p. 68.
[2] Idem, Bd. vii., p. 269. [3] Idem, Bd. vi., p. 67.
[4] Cannstatt's Jahresbericht, Jahrg. x., p. 107.
[5] Idem, N. A., Jahrg. ii., 1. Abth., p. 133.
[6] Archiv für Pharm., 1839, Bd. xviii., p. 75

means of chloroform, which may be found in *Gazette Méd. de Paris*, 19 Octobre, 1850.

The process adopted by the U. S. and British Pharmacopœias may be found under their respective heads.

Professor William Proctor read a paper before the American Pharmaceutical Association upon the preparation of atropia from American belladonna-root; the process recommended by him has, with slight modifications, been adopted as the process of the U. S. Dispensatory. Professor Proctor's process may be found in the *Proceedings of the Am. Pharm. Association.*

When ophthalmic surgeons first used atropia, it was their custom to order the atropia to be dissolved in a sufficient quantity of water, by aid of sulphuric acid. In almost all instances, more acid was used than was required; in fact, it was impossible to dissolve it in its proper combining proportion, and, if the alkaloid was not pure, a very great excess of sulphuric acid was used. This acid-solution was very irritating to the eyes, and it soon became obvious that some formula must be adopted to obtain a perfectly neutral sulphate.

With this view, M. Maitre offered a formula for the preparation of neutral sulphate of atropia, which may be found in the *Am. Jour. of Pharmacy*, 1856, p. 360.[1]

In the last edition of the "U. S. Pharmacopœia" this formula of M. Maitre's is followed very closely.

Both of these formulæ give a sulphate of atropia in powder, which is much more liable to be adulter-

[1] In the manuscript all these various processes are given in full, with comments. To publish this essay in a journal, it was necessary to abridge it; the works in which the processes are found are therefore given.

ated than if in a crystallized form. M. Laneau, deeming that an agent so important in ophthalmic surgery should be of unquestioned purity, offered the following formula for its production in crystals:

"Take of crystallized atropia, . . 289 grains.
Absolute alcohol, . . . 800 "

"The solution is effected by agitation, in a glass capsule, aided by very gentle heat. Then weigh in a small phial,

"Sulphuric acid, sp. gr. 1.85, . . 40 grains.

"Dilute this acid with 300 grains of anhydrous alcohol, and add it, little by little, to the solution of atropia. To complete the saturation, the solution is stirred with a glass rod, moistened slightly with concentrated alcohol, until test-paper shows neutrality. The solution is then suffered to evaporate spontaneously. The crystallization is effected in three or four days in the summer, or five or six days in the winter. The thinner the stratum of liquid, the more quickly is the process effected. The crystals, which may be dried without destroying their form, are in colorless needles, more or less interlaced. When chloroform is used instead of alcohol, the salt is obtained in a gum-like mass. Crystallized atropia is soluble in water, weak alcohol, and absolute alcohol, but is insoluble in chloroform and ether."

Hydrochlorate and acetate of atropia may be obtained as crystalline salts; the valerianate as a thick syrupy mass.

Division Third.—*Physical Properties of Atropia and of its Salts.*

Atropia, as prepared by the U. S. Pharmacopœia, is impure; but, when purified and crystallized from

concentrated warm solutions, is in white, transparent, silky, prismatic crystals; when crystallized by very slow evaporation, it is in needle-like crystals. In the impure form, it is a fawn-colored powder. It is soluble in 300 parts of water at 60° F., in 3 parts of chloroform, in 25 parts of ether, and in less than 3 parts of absolute alcohol. In all these liquids, it is much more soluble hot than cold. It is without odor, but is of a very acrid bitter taste. It melts at 194° F., and the greater part of it is volatilized at 285° F. Heated for some time with potassa or soda, it is decomposed, and gives out an odor of ammonia. It has the property of left-handed circular polarization. It is of alkaline reaction, and forms crystallizable salts with sulphuric, hydrochloric, and acetic acids.

The sulphate of atropia of the U. S. Pharmacopœia is a white semi-crystalline powder. That prepared by M. Laneau's formula is in clear white needles. Both are neutral to test-paper, very soluble in alcohol and water, but insoluble in pure ether and chloroform. A solution of atropia can soon be known by its effect in dilating the pupil of the eye.

Valerianate of atropia does not crystallize. It is a thick syrupy liquid of a fetid valerianic odor, very soluble in water and alcohol. It is readily decomposed by the weakest mineral acid, with evolution of valerianic acid.

Division Fourth.—*Behavior of Atropia with Chemical Tests and Reagents.*

A solution of atropia in hydrochloric acid furnishes, with—

Potash,[1] a pulverulent precipitate, if the potash solu-

[1] A. von Planta, Liebig's Annalen, Bd. lxxiv., p. 245.

tion is concentrated and in slight excess; a large excess of potash dissolves the precipitate.

Carbonate of potash produces the same result.

Ammonia gives a precipitate much the same as potash, but is more readily soluble in an excess of ammonia than in potash.

Terchloride of gold gives an abundant yellow precipitate, which is soluble in an excess of hydrochloric, acetic, or sulphuric acid.

Bichloride of platinum gives a dirty-yellow precipitate.

Iodine, in iodide of potassium, an immediate, copious, brown, amorphous precipitate, which slowly dissolves in a few drops of a strong solution of potash.

Tannic acid, a dirty-white precipitate, soluble in a strong solution of potash, and in hydrochloric acid.

Nitro-picric acid[1] gives a sulphur-colored pulverulent precipitate.

Carbazotic acid,[1] an abundant light-yellow precipitate, readily soluble in acids.

Bromine, in bromohydric acid,[1] gives an immediate, copious, bright-yellow precipitate, which soon becomes a mass of twig-like crystals. If there is a deficiency of reagent used, the precipitate will dissolve, but is reproduced upon a further addition of the reagent.

Concentrated nitric acid[1] dissolves atropia without any change of color, upon heating the solution, and, after cooling, the addition of a drop of chloride of tin-solution gives a copious white deposit; without heating the tin, salt produces no change.

[1] A very full description of the various reagents in Micro-Chemistry of Poisons, by T. G. Wormley, M. D.

Peculiarities in the behavior of atropia, with various tests and reagents, may be found in *Annalen der Chemie und Pharm.*, Bd. cxxviii., pp. 273, 282; *London Chem. News*, May, 1864; Mayer, *Am. Jour. of Pharm.*, 1864, p. 234.

Von Planta,[1] in his analyses, determined that atropia and daturia were chemically identical, answering the same chemical tests and reagents. Wormley,[2] in his later and more elaborate chemical and micro-chemical researches, arrives at the same results. Many of the German ophthalmologists acknowledge the chemical identity, but assert that the two alkaloids differ somewhat in their physiological and therapeutic action.[3]

The chemical formula for atropia is:

$$C_{34}\ H_{23}\ N\ O_6 = 289 \quad \overset{+}{A}t.;$$

for sulphate of atropia:

$$C_{34}\ H_{23}\ N\ O_6\ S\ O_3 = 329.$$

Mr. Scorby read a paper lately before the Sheffield Literary and Philosophical Society, in which he set forth the difficulties which the toxicologist encounters in his efforts to prove a case of poisoning by belladonna. These difficulties, he thinks, are obviated by the use of the micro-spectroscope. "The spectrum of the juice of the belladonna is very distinct, especially when the coloring matter has been added to a solution of carbonate of soda. A small fraction of a single berry is sufficient to produce the spectrum-bands characteristic of belladonna."

Whether this be correct or not with regard to the fresh juice of the leaf or berry of belladonna, it would

[1] Liebig's Annalen, Bd. lxxiv. [2] Micro-Chemistry of Poisons.
[3] Veratrum Viride, Trans. Am. Med. Ass., 1863, and Chem. News, 1864.

still be of little value in examining the secretions of persons poisoned by that plant, and would only be serviceable providing some of the juice or berry remained in the stomach. It would be of no value in detecting poisoning by atropia.

SECTION SECOND.

Division First. — *Physiological Action of Atropia upon Animals.*

Since the discovery of atropia, it has been used as a substitute for belladonna chiefly on account of the certainty of its action, but also because it is easier to use, and much cleaner than extract of belladonna.

As to the relative strength of atropia and belladonna, Pfitzner[1] says that one part of atropia is equal to 240 parts of extract of belladonna. Geiger[2] says that one grain of atropia is equal to 200 of extract of belladonna, 600 grains of belladonna-plant, or 360 grains of powdered belladonna-root.

Whether used locally or generally, the effects of atropia upon the system are similar to those produced by belladonna; but it is more quickly absorbed, and produces its effects in shorter time than belladonna.

Reil[3] says that both herbivora and carnivora are readily brought under the action of atropia, but that the latter are more easily and more quickly affected than the former; that sharp-sighted birds and cats are exceedingly susceptible to its effects.

Cogswell[4] experimented upon frogs, injecting a solution of one grain of atropia under the skin of the right hind-leg, which produced intercurrent convul-

[1] Dissertatio de Atropino, 1846. [2] Hagen, p. 601.
[3] Ib, p. 602. [4] London Lancet, November, 1852.

sions on this leg, while the other was entirely relaxed. After a while, these convulsions ceased, but returned at the expiration of twenty-four hours.

Michea observed that snails did not suffer from the action of atropia, but that they were poisonous to animals that ate them.

The German physiologists have experimented with atropia upon a large number and upon different varieties of animals; they find its physiological action similar to that of belladonna, but more certain.

Case 1.—I administered to a large, coarse dog, which had been kept fasting for twenty hours, one quarter of a grain of the sulphate of atropia, at 9 o'clock A. M. It was wrapped in a thin slice of meat, and was swallowed greedily. The pulse at this time was 141 beats in the minute. In half an hour the pulse had increased 11 beats in the minute. The tongue was constantly thrust out, licking the lips, followed by masticatory movements and frequent deglutition; no saliva or mucus flowed from the mouth. A marked dilatation of the pupils was visible. Many efforts at vomiting were now made, but nothing was thrown up, and the efforts at vomiting did not cause a flow of saliva, as is most generally the case when a dog vomits. These efforts at vomiting continued about half an hour. A pail of water was then put by the dog, and he was allowed to drink as much as he pleased. At 11½ o'clock he stood with his legs spread wide apart, the eyes suffused, the pupils very widely dilated and staring; the respiration was accelerated, laborious, and abdominal. He made no effort to move; but lapped greedily, if water was placed to his mouth. A stick pointed at the eye did not cause the lids to close, and the lids did not close, unless the eye was actually touched. He made no movement when called, and seemed as though he was both deaf and blind. He would not eat, when meat was placed to his nose. A large quantity of urine was passed, but without moving from the position he was in.

At 12½ o'clock the dog was lying on its side, and did not move when struck a smart blow with a switch; the eyelids were open; the pupils enormously distended; the respiration rapid and abdominal; the pulse thin, wiry, and not to be counted.

At 6 o'clock P. M., the dog was still in the same position; the respiration was much easier, the pulse slower and fuller. At 9 o'clock, the next morning, the dog was walking about in a very dejected manner, the pupils widely dilated, with frequent strabismus. He ate sparingly, and drank freely. The next day he seemed quite well, but the pupils were still large.

Case 2.—To a dog somewhat smaller than the last mentioned, that had been kept without food or water for twenty-four hours, a quarter of a grain of the sulphate of atropia was given in a small bolus of meat. The symptoms that supervened were very similar to those last described; but the dog was allowed no water. Ten hours after taking the atropia, he was seized with convulsions which lasted about fifteen minutes; deep coma then supervened, and he died in about half an hour. The tongue was swollen, and indented with the teeth; the mouth was full of a ropy mucus, as were also the œsophagus and trachea. The mucous membrane of the stomach was slightly reddened, the intestines looked healthy, the heart was full of dark blood, as were also the arteries of the lungs; the kidneys were highly congested, sufficiently so to have caused death by uræmic poisoning. There was about half an ounce of urine in the bladder, which, applied to the eye of another dog, caused dilatation of the pupil.

Case 3.—To a much smaller dog, which had been kept without food or water for fourteen hours, a quarter of a grain of sulphate of atropia was given in meat. As soon as free dilatation of the pupils had taken place, a third of a grain of sulphate of morphia was given in a small bolus of meat. No water was given. No convulsions took place. The animal lay quiet; the respirations were full and deep, and not frequent; the pulse was slow and full; the pupils of the eyes as widely dilated as though no morphia had been given. In seven hours the dog died. The heart, lungs, and brain were full of dark blood; the kidneys were highly congested, and there was hardly a teaspoonful of urine in the bladder.

Case 4.—To a dog of about the same size as that last mentioned, which also had been kept for fourteen hours without food or water, a quarter of a grain of sulphate of atropia was thrown into the stomach, dissolved in half a pint of water. In half an hour this was followed by a third of a grain of sulphate of morphia, dissolved in two ounces of water, the pupils being widely dilated at this time. Mastication and deglutition were constant, but there was no vomiting. The dog was stupid, and unconscious to all noises, and it soon lay down. Four hours after giving the atropia, he was brought into a bright sunlight. By quickly removing a dark object, so as to allow the sun to shine quickly upon the pupil, contraction could be plainly seen. Eight ounces of thin Indian-meal gruel were thrown into the stomach. Nine hours after the first dose, the dog ate voraciously and drank a large quantity of water. The pupils were not largely dilated, and the animal seemed comfortable, though not playful.

I have repeated these experiments, and I find as a rule that, if atropia is given without water, the effects are much more irritative, and last much longer, than when a sufficiency of water is allowed. When death takes place where water is not allowed, there is always

congestion of the kidneys. Morphia, although an antidote to atropia in ordinary cases where water is freely allowed, is hardly an antidote where fluid is entirely withheld. A much larger dose of atropia may be borne without danger, if care is taken to keep the system well supplied with fluids; and the effects of poisoning pass off much more rapidly if warm diluents are prudently administered. Where diluents are freely given, the kidneys perform their function, and gradually remove the poison from the system; but where large doses of the medicine are given unaccompanied with liquids, the kidneys are unable to eliminate either the poison or the urea, and the animal consequently dies, frequently only from uræmic poisoning, at other times from the double effect of the poisoning from the alkaloid, and uræmic poisoning as well.

Case 5.—Two drops of a solution containing $\frac{1}{30}$ of a grain of the neutral sulphate of atropia were thrown, by means of the hypodermic syringe, beneath the skin over the supra-orbital nerve on the right side. The pupil of the right eye began immediately to dilate, and in one and a half minute but a mere border of the iris was to be seen. The dog's head was held so that a strong sunlight was shining in both eyes. In four and a half minutes the pupil of the left eye began to dilate slowly, and continued to dilate for five minutes; but it did not dilate to any thing like the size of the pupil of the other side. The dog came when called, but his steps were uncertain, and, when he attempted to go down-stairs, he fell all the way down. The eyes had a lack-lustre, staring, congested appearance. The pupils were still dilated thirty-six hours after the injection.

Case 6.—A solution containing $\frac{1}{16}$ of a grain of sulphate of morphia was thrown, by means of the hypodermic syringe, beneath the skin over the supra-orbital nerve of the left side, while, at the same time, $\frac{1}{30}$ of a grain of the neutral sulphate of atropia was thrown in a corresponding place on the right side. The dog's head was held in a strong sunlight. Immediately the left pupil began to contract, and the right pupil to dilate, and in two minutes and a half the left pupil was not much larger than an ordinary shot, while the right had already dilated to its utmost extent. No coaxing could induce the dog to take a step; he lay down, with his head between his paws. In the dusk of the evening, about eleven hours

after the operation, he ate, drank, and played quite freely. Upon applying a light to the eyes, they were still different in size.[1]

Case 7.—Six minims of a solution containing $\frac{1}{6}$ of a grain of sulphate of atropia were thrown by the hypodermic syringe into one of the large veins of the ear of a dog. In 80 seconds he fell on his side, and died of coma in $3\frac{1}{2}$ minutes. The pupils of both eyes were widely dilated. The dog had been sick, so no *post-mortem* examination was made.

Division Second.—*Physiological Action of Atropia on Man.*

Case 8.—To a gentleman of literary habits, who had used his eyes too much, a solution of the neutral sulphate of atropia was dropped into both eyes. In a few minutes the pupils were seen to be dilated, and in 45 minutes they were so widely dilated, that the iris was a mere border. He complained that he could not walk correctly, as he had altogether lost the

[1] On the Antagonistic Action of Opium and Belladonna.—Professor A. von Graëfe makes the following observations on the antagonistic action of opium and belladonna injected into the cellular tissue: When a solution of atropine has been injected hypodermatically, three or four minutes afterward the pupil becomes dilated, the pulse rises to 140–160, and other symptoms of narcosis by atropine are observed. If morphia is then injected, all these phenomena, which would otherwise last for hours, disappear in a very short time. After a hypodermatic injection of morphia, a considerable myosis is observed, and the pupil cannot be dilated. This is probably to be ascribed to an active irritation in the sphincter muscle, just as mydriasis caused by belladonna is to be explained by active irritation of the dilatator muscle. A new fact, which Von Graëfe has observed, is the antagonistic action of these medicines upon the faculty of accommodation; although it has not occurred in all the cases in which he has operated. *Atropine causes paralysis, and morphia a spasm of accommodation.* In consequence of this, the space allowed to accommodation becomes greatly limited, and myopia is the result. All distant objects are indistinctly seen; but, if concave glasses are used, this is obviated. It is true that the myopia is not so considerable as it appears to be when trials on both eyes are made, as, if only one eye is experimented upon, distant objects are more clearly distinguished; a circumstance which is, no doubt, due to the weakening action of morphia upon the internal muscles of the eye. But the phenomenon is only temporary, and is generally only observed three-quarters of an hour after the injection. It is probable that, if a stronger dose of morphia were used, it would last longer and also be more constant; but it would not be justifiable to do this in order to satisfy physiological curiosity. The symptoms described are to be explained in the following manner: Opium and belladonna have an antagonistic effect upon the muscular fibres of the tensor chorioideæ, as upon the muscles of the iris; and the analogy would be quite complete, if a double and antagonistic innervation of the tensor chorioideæ, by both the third pair and the sympathetic nerve, was just as certain as it is for the iris.—*Medical Times and Gazette.*

adaptability of distance. He could not walk up-stairs without holding to the banister, and he would not walk down without assistance. When brought into a bright sunlight, it caused continued sneezing — reflex action.

For two days we kept him quite quiet, and under treatment for his diseased state, by associating the use of atropia to the eyes for the purpose of compelling him to be quiet; but, on the third evening, he found he could read by using an old person's spectacles. On the next day, the atropia was dropped into the right eye only, and a piece of gelatinized calabar-bean solution was placed in the left eye. The disturbance of vision was greater than before, and the adaptability to arrange distances was entirely lost; he could not even feed himself from his plate correctly. He could look at the bright sunlight, if the right eye was covered; but, if he did so when the left eye was covered, it caused him to sneeze frequently, and gave him deep-seated pain in the eye-ball. This plan was continued for the purpose of keeping him from study for about two weeks; proper exercise was given, and his health improved very much. After discontinuing the use of the atropia, it was many days before the eyes recovered their natural power.

Case 9.—The author, while in perfect health, took $\frac{1}{10}$ of a grain of sulphate of atropia in an ounce of water. It had a distinct, persistently bitter taste, and produced a numb sensation upon the tongue, somewhat similar, though less in degree, to that produced by aconite. In about ten minutes it produced a sense of nausea, which continued to increase until an effort was made to vomit. Although the effort at vomiting was made several times, nothing was thrown up. To this, intense thirst succeeded, and a frontal headache, which was lessened by closing the eyes. Next, a dryness was felt in the throat, and the tongue and mouth felt dry and feverish. These symptoms were not relieved, and only mitigated for a short time, by drinking water. This dryness of the throat increased, and was persistent for several hours, causing at first almost constant deglutition, and, toward the last, a strong effort to avoid deglutition, which had become painful. During the first two hours the pulse was less frequent, but afterward became smaller and more rapid. The eyes soon lost all control of distance; a printed book, held at the usual distance, was a perfect blur; held at a long distance, letters could be distinguished, but the words, upon looking at them, soon ran into confusion. Objects at a distance could be plainly seen, even a little more plainly than natural (hyperopia), but near objects, though for a moment distinguished, soon lost their distinctness. A numb or crawling sensation, a formication, was felt down the back, upon the arms, and back of the hands. This formication was intense and very unpleasant upon the palate. Light became unpleasant to the eyes, causing pain deep in the eyeballs. Motion became unpleasant, and if the feet were lifted in walking, the floor seemed to recede from them before they again were put down. A sliding of the feet along the

floor seemed to be the only safe way of locomotion. The head began to be dizzy, and, for fear of falling, it was necessary to be seated in the easy-chair. Thirst, at this time, was very great, but only little water could be taken at a time, as the effort of swallowing was unpleasant. A languid feeling came on, as the semi-recumbent position was assumed, and whether sleep accompanied with wild dreams, or waking hallucinations, followed, it was impossible to tell. Whether the brain was troubled with hallucinations, or with wild, fantastic dreams, they were exactly the opposite to those produced by cannabis indica, for all the imaginations and conversations were of the long past—none of the future. Whether there was total blindness, or merely an imagination of blindness, could not be remembered; but, if there was blindness to the external eye, the mind saw all its images with great distinctness, and the impress of them was left with vividness. There was a consciousness of individuality, but the actions were performed by others, who were embodied spirits of those long since departed. Lengthy conversations of a most pleasant character were held with Plato, Alcibiades, Aspasia, and others. How long this state lasted, cannot be told; but sound, profound sleep must have followed, for consciousness to external objects did not return till sixteen hours after taking the atropia. Awaking took place suddenly; there was no pain, no headache, nothing abnormal but a languor and disturbed vision. The bladder had not been emptied during these sixteen hours, and, although a large quantity of water had been drunk, but a moderate quantity of urine was passed. The vision was not clear for several days.

The dose here mentioned is small, but I usually take about half the amount of medicine needed by other persons.

Mr. Warton Jones[1] has applied atropia to the smaller arteries, and finds it to constrict them, and states that the effects continue for several hours. This and other medical solutions he applied also to the web of the frog's foot. Other alkaloids produce opposite effects.[2]

SECTION THIRD.

Therapeutics of Atropia.

Case 10.—A large lymphatic woman sent for me, stating that she had been suffering very severely from facial neuralgia for some days; that, within a few hours, the pain had increased to such an extent, that she

[1] W. Jones, Sir Astley Cooper's Prize Essay.

[2] S. Percy, Am. Med. Assoc. Prize Essay, pp. 35 and 80, 1863.

feared she should go crazy. The face was suffused, the right eye bloodshot, pain very severe if the face or neck was slightly touched with a handkerchief, or brushed over with a feather; but the pain somewhat lessened if the warm hand was pressed firmly on the parts affected. By means of the hypodermic syringe, I injected $\frac{1}{30}$ of a grain of the sulphate of atropia beneath the skin under the angle of the jaw. Within two minutes there was perfect relief from pain, but the headache of which she complained was not in the least relieved. Dilatation of the pupil of the eye on that side soon commenced, and in half an hour the pupil was very large. The pupil of the other eye, at that time, was not at all dilated, though subsequently it became dilated, but not so largely as the other. The pulse was lessened over twenty beats in the minute. I gave a large dose of aperient medicine, and sent her to bed. She slept the whole night through, said she did not dream in the least, and did not awake until her bowels disturbed her in the morning. She was well, except that her vision was disturbed.

Mitchell, Morehouse, Keen, and others, deny that the hypodermic introduction of atropia relieves pain. Such has not been the result of my experience; and Tatum, Ogle, and other medical officers of St. George's Hospital, are constantly using it with great relief.

Case 11.—A young girl suffering from toothache applied to me several times. The tooth was decayed and hollow. I applied a very small ball of cotton, slightly moistened, and dipped into sulphate of atropia. It probably contained $\frac{1}{40}$ of a grain. It always gave instant relief. I think I used it a dozen times before I could persuade her to go to the dentist. It did not produce dilatation of the pupil.

A very large number of persons are troubled with a passive constipation of the bowels. This is especially the case with many females of delicate health and highly-nervous sensibility. With such persons, I have in many instances given permanent relief by the careful use of the sulphate of atropia. Sometimes this alone will give relief, at other times tonics and aperients are needed as well. But I can refer to a large number of cases that have been permanently cured by the following treatment:

I make a solution of sulphate of atropia in the following manner:

Sulphate of atropia, . . .	1 grain.	
Distilled water, ...	5 drachms.	
Alcohol, ...	5 "	Mix.

It will be seen that to each 10 minims of this fluid there is $\frac{1}{60}$ of a grain of sulphate of atropia.

Fleming[1] has recommended a solution of the same strength, but he uses instead the alkaloid atropia, and dissolves it in water by adding dilute muriatic acid. This is not so well, for a larger quantity of acid is always used than is necessary, and no two druggists prepare it of exactly the same acidity. There is no need of an acid-solution, as the sulphate is soluble in water, and, if made according to the formula found on page 248, is in a chrysaline state which the appearance alone will prove to be pure.

In beginning the treatment for this form of constipation, I give 20 minims in about a wineglassful of water early in the morning, either before getting up, or the first thing on arising. By watching the effects of this, it will be seen whether it is necessary to decrease or increase this dose. I have found, in some instances, 15 minims produce the same effect in one person that 60 minims would produce in another. The object is, to produce the first degree of the physiological action of the medicine—dilated pupils, slight confusion of sight, a dry throat, and a little thirst. It is never advisable to go beyond this first degree of physiological action, and, by carefully watching the effects, this degree can be reached. If 20 minims produce no marked effect, it may be increased 5 minims at a time, until the proper action of the medicine is

[1] Edinburgh Medical Journal, Jan. 7, 1863.

attained. This quantity should then be given once only in 24 hours, and then always upon an empty stomach. In commencing the treatment, it is necessary always to give it in the morning, because its effects could not be noticed during sleep; but, if, after it has been given safely for a week or more, and the proper dose has been correctly arrived at, the patient would prefer to take it on going to bed, there is no objection to giving it at this time, and patients frequently wish to avoid the unpleasant effects of it, and sleep while the worst effects last. It is, however, always necessary to give it upon an empty stomach, for it does not have its desired effect with any certainty if given on a full stomach. There are few medicines the effects of which are so permanent as atropia; it is not safe to repeat it in less than from 16 to 20 hours, and for this disease I give it only once in 24 hours; if given oftener, we are very apt to produce the cumulative action of the remedy, and produce more severe action than is required. The effects of one dose should nearly be spent before another is given, otherwise the accumulated action of both doses may alarm the patient. It should also always be given in solution, never in pills, for pills frequently lie in the stomach or intestines, entirely unacted upon, and may suddenly become soluble and produce alarming results.[1] I have continued this treatment for a month, taking care that a proper effort is made to empty the bowels every day after breakfast, and allowing no excuse for neglect of this habit of regularity. In nearly all cases, the first few days, the appetite is much lessened, but it afterward becomes much better than ordinary. As the bowels become regu-

[1] Am. Med. Times, vol. iv.

lar in their action, the stools become softer, and hardened masses are not passed. Occasionally I am obliged to assist with a little rhubarb at meal-times. I give the best of powdered Turkey rhubarb; made into ordinary-sized pills with a little honey—one or two of these pills at meal-time, discontinued by degrees.

Atropia has been highly recommended in the treatment of various other diseases, as in asthma, in rheumatism, in whooping-cough, in scarlet fever, in spasmodic stricture of the urethra, in strangulated hernia, in rigidity of the os uteri, etc.; but these disorders are more easily cured by other remedies that are less dangerous in their use.

Atropia, as well as belladonna, has been very highly vaunted *as a prophylactic against scarlet fever.* I have tried it many times for this purpose, but all my success, if it can be called success, has been of a purely negative character, and, in many instances where I have used it, it has had no effect in preventing the disease. Children bear a larger dose than adults, and the effects pass off more rapidly. (Med.-Chir. Trans., vol. xlii.)

To the ophthalmologist, atropia, or its congener, daturia, is indispensable.

A solution dropped on the conjunctivæ dilates the pupil, and this dilatation can be varied in quickness, in intensity or duration, according to the strength and the manner in which the solution is applied. If it is desired to dilate the pupil moderately, and have the effect pass over quickly, a solution of one grain of the neutral sulphate of atropia in ten drachms of water may be used. A few drops of this may be put into the eye, and allowed to remain for one minute, the eye-cup with cold water may then be used. Such dilatation takes place slowly, and generally passes over in

from fourteen to twenty hours. But if it is necessary to dilate the pupil more fully, three grains of the neutral sulphate may be dissolved in ten drachms of water, and a few drops of this solution put in the eye. In a few minutes this will dilate the pupil, and in an hour the pupil will be very largely dilated, and the effect will not pass over for twenty-four hours, and frequently it lasts from two to three days.

As the pupil dilates, the sight of the eye soon becomes impaired, there is a staring appearance, the lustre is dimmed, and after some hours the conjunctiva frequently assumes a dull, dirty-bluish tinge. It is not that the eye is congested with the red blood disks, but it looks as if congested with the serum of the blood.

If the atropia is put into one eye only, the sight of that eye only is impaired, and print that cannot be distinctly read with this eye, or with both, may be easily read if this eye is covered. The print seems smaller to the atropised eye than to the other, and although no difference is frequently observed with objects at a distance, near objects are not as clearly defined with the atropised as with the other. This effect is due to the wide dilatation of the pupil, and the inability of the eye to adapt itself to near vision. Many refer it to diminished sensibility of the retina; and, although others are not willing to acknowledge this to be any cause of the impairment of vision, I think there is but little doubt the altered state of the delicate arteries of the retina has a decided effect in confusing or impairing the vision. But the dilated state of the pupil is undoubtedly the principal cause; for, in an imperfect light, or in twilight, the impairment in vision is but little noted; the pupil does not then,

as in a bright light, receive too many and too diver gent rays of light. Again, if a piece of cardboard is placed against the eye, with a hole in it smaller than the dilated pupil, the vision is much improved. But that the vision is materially altered in the atropised eye is proved by looking through a telescope;[1] for the eye, that may be so thoroughly atropised as to be blind in all ordinary ways, can clearly distinguish through the telescope, if the focus is rightly adjusted by lengthening the instrument. In the use of the microscope also,[2] the same thing may be noted, for the atropised eye can clearly distinguish if the object is removed a little farther from the instrument.

The atropised eye bears the sun's rays better than the other. Upon dead animals atropia produces no action on the pupil. The *modus operandi* of atropia upon the eye has been discussed by nearly all the eminent ophthalmologists and anatomists, and, although volumes have been written upon the subject, it is yet "vexata quæstio." Upon this point I have not room to print within a reasonable space the various opinions expressed; I therefore act under advisement, and leave all that I have written upon the action of atropia upon the eye, and refer to Fleming's admirable paper "On the Action and Uses of Atropia;" to the researches of Budge, Brown-Séquard, Von Graefe, Bernard, Harley, Bell, and others; to Donder's work "On the Anomalies of Accommodation and Refraction of the Eye," which contains minute and useful information on mydriatics, and also on the antagonistic action of myotics.

Atropia has been used quite extensively by the German physicians in the treatment of epilepsy. Lus-

[1] Fleming uses an action of atropia. [2] Fleming, loc. cit.

sana[1] cured a patient suffering from true cerebral epilepsy, and also a case of chorea in a maniac. Namias[2] used atropia in doses of $\frac{1}{24}$ of a grain in epilepsy, and in other spasmodic diseases, and obtained by careful and prolonged use marked success. Volunterio[3] gave $\frac{1}{12}$ of a grain of atropia every two hours, with the best results in epilepsy. Lange[4] cured three men who had suffered several years from epilepsy, by small doses of atropia continued fourteen weeks; they had no relapse. He also treated in the same manner six females suffering from epilepsy; of these two remained uncured, one died, and three were cured after five to eleven months' treatment. Reil[5] says of the action of atropia in epilepsy and chorea: "My own experience authorized me to say that atropia in both the diseases under consideration is not only a safe palliative means, but often a true and curative means,[6] especially when the disease is not yet inveterate and the individuals are young, particularly in young girls at puberty."

[1] L'Union Médicale, No. 77. [2] Ed. Med. and Surg. Jour., 1851, p. 249.

[3] Hagen, p. 607. [4] Deutsche Klinik. 10. 1854. [5] Hagen. p. 608.

[6] Belladonna and atropia are, according to the *Medical Times and Gazette*, in favor at the Hospital for the Epileptic and Paralyzed, as remedies for epilepsy. Under these remedies the patients are benefited, and though many cases are not cured, the number of fits is often diminished and the patient's general condition much improved. Many who have suffered for years are rendered capable of resuming a comparatively active life. The prescription generally used is extract of belladonna a quarter of a grain, quinine one grain, in a pill three times a day. Of atropine, the $\frac{1}{120}$ of a grain is given three times a day. Both are gradually increased in dose, yet the only physiological effects observed are dryness of the throat and defective vision. The latter is in consequence of the ciliary muscle being partially or totally paralyzed, by which the power of accommodation is impaired or altogether lost. One method of Dr. Brown-Séquard is, to inject a solution of atropine and morphia into a part from which an aura starts. A solution containing $\frac{1}{60}$ of a grain of atropine and $\frac{1}{4}$ of a grain of morphia is injected with Wood's syringe, sometimes with excellent results. He believes that belladonna and ergot both act by producing contraction on the blood-vessels; the former on those of the brain, the latter on those of the spinal cord. In paraplegia from myelitis, he gives a pill containing three grains of fresh ergot and a quarter of a grain of the extract of belladonna, three times a day. The action of belladonna

Professor Oppolzer,[1] in Vienna, in a case of inflammation of the radial nerve, for which quinia, Fowler's solution, colchicum, iodine, and other remedies, had been tried in vain, employed repeated subcutaneous injections of atropia, and removed the pain and the thickening of the nerve.

Bouchardat, Croserio, and Lussana, used atropia in intermittent fever successfully, for which quinia had been given in vain. Lussana treated with it two cases of tertian intermitting fever, and says concerning it the following: "I beg the attention of physicians to the cure which I have obtained, by atropia, of two cases of intermitting fever existing for two months, in which the sulphate of quinia had been given to no purpose. Half a grain of atropia sufficed for the cure, and with $\frac{1}{8}$ or even $\frac{1}{14}$ of a grain, such a modification

in arresting the secretion of milk, and causing dryness of the throat, may be explained on the hypothesis of its diminishing the supply of blood to those parts.

Thus, then, up to this point, five facts appear proved:

1. That, in cases of chorea, extraordinarily large doses of belladonna and atropine are tolerated.

2. That the drug is absorbed into the blood, and therefore that the tolerance of it is not attributable to its non-absorption, nor to its being decomposed in the stomach.

3. That it does not accumulate in the blood, but passes out of the system with the urine and fæces, and probably with the other excretions.

4. That it does not exercise that amount of control over the choreic spasms which would have been expected from the readiness with which it is tolerated by the system.

5. That the tolerance of the remedy is not in proportion to the severity of the choreic symptoms.

The question, therefore, arises as to whether the existence of chorea had any part in producing tolerance of the drug, or whether that tolerance may not have been due to some other circumstances? With the view of determining this point, the author administered the extract of belladonna to two convalescent children, whom he kept in the hospital for the purpose. To the one, aged seven, he ultimately gave thirteen grains of the extract daily, and to the other, aged ten, twenty-eight grains daily, without producing dryness of the tongue or fauces, or any symptom indicative of the action of belladonna beyond some temporary dilatation of the pupils.

[1] Wiener Medic. Halle, 1861.

of the attacks occurred, that the peculiar curative action of the atropia was not to be mistaken. When it is considered that atropia is an indigenous remedy, and that with it the sick can be cured with little cost, whereas the treatment by quinia in Europe costs millions, it is proper that the experiments published by me should gain the attention of physicians."

It is necessary to dwell for a few moments upon the duration of the effects of atropia upon the system.

The effects of atropia remain longer in the system than any medicine of its class. If we compare it with the narcotics, we find that they are all eliminated from the system in a quicker time than atropia. If we compare it with the sedatives, we find the same result. Medicinal doses of atropia of $\frac{1}{20}$ of a grain will produce effects that will not subside in less than twenty-four hours, and frequently they last for double that time. Caution, therefore, need be used in administering this remedy, and doses must not be repeated too often, otherwise the system may be overwhelmed by the accumulated influence of one dose, given before the effects of previous doses have sufficiently passed over.

Unless patients can be very closely watched, it is better not to repeat doses of atropia oftener than once in twenty-four hours.

Of the various preparations of atropia, the best, and that most easily obtained, is the neutral sulphate. Atropia itself need never be used where one of its soluble salts can be obtained. The formula above given for the crystallized sulphate of atropia places it within the power of any good pharmaceutist to prepare a perfectly pure, and a perfectly neutral salt, and one that is readily soluble in all menstrua in which it would be necessary to use it. The advantage of pur-

chasing it in crystals is, that one is assured of its purity much more readily than if it was in powder.

For the use of ophthalmologists the *neutral* sulphate is the only preparation that should be used, and no apothecary has a right to dissolve the pure alkaloid by adding free acid for that purpose. The alkaloid is not dissolved without an excess of acid, and frequently a large excess, and this free acid always becomes an irritant to the eye, whereas the application of the *neutral* sulphate does not inflame. No ophthalmologist would order any thing but the *neutral* sulphate, and any apothecary who would prepare the acid sulphate is deserving of severe censure. Atropia, as well as all other alkaloids, should never be given in pills, but always in solution. Disks of paper and of gelatine properly atropised are to be purchased at druggists'.

In its effects upon the system, atropia may be classed midway between narcotics and sedatives. Of the class narcotics, together with stramonium and hyoscyamus, it most nearly approaches the sedatives. The primary stimulating action is very slight, and it soon passes over; the anodyne effect is soon felt, and a sedative effect upon the heart is soon noticed. It is not soporific, but deliriant. It does not constipate the bowels, and in medicinal doses it does not diminish the secretions. In poisonous doses, we see, by referring to cases quoted, that it checks the secretion of urine by causing congestion of the kidneys. It always produces dilatation of the pupil of the eye; generally a peculiar dryness of the throat accompanied with thirst. In full doses it is nearly always a deliriant, exciting and leading the mind astray, presenting unreal objects and imaginary fantasies; hallucinations supply the place of realities, and seem to be more permanent

than realities. Unlike soporifics, the mind is not extinguished, there is no perfect loss of consciousness, but the senses are deranged and deluded. Like the sedatives it deranges or destroys the action of the vagus nerve.

"Hahnemann and his followers have made the assertion that the administration of belladonna produces a rash similar in appearance to scarlatina, and, upon this assertion, they use belladonna as a cure for scarlet fever. Many eminent medical men accept this assertion as a fact, and, in one of our latest and best works on 'Skin Diseases,'[1] the author, in enumerating articles that produce an erythema, states the reiterated assertion that 'belladonna produces a rash of a rosy hue.' Dr. Fuller, in a paper on the 'Action of Belladonna,'[2] with a view to solve this question, gave belladonna to a large number of patients for some months, 'in doses varying from a quarter of a grain of the extract up to seventy grains daily.' The patients were examined four or five times daily, and the occurrence of any rash or eruption was carefully looked for. But in all these cases no rash or eruption was perceived."

I have watched a large number of patients, both in hospital and in private practice, where belladonna or atropia has been used with a like negative result. There are many unfounded assertions on the actions of medicines, but probably none so wide-spread and utterly groundless as this. It is well worthy to be classed with the other "facts" of Hahnemannic "provings."

[1] Dr. Tilbury Fox. [2] Medico-Chir. Trans., vol. xlii

SECTION FOURTH.

Toxicology of Atropia.

The effects of overdoses of atropia have been plainly shown in section second.

The toxicology of atropia is susceptible of two divisions:

1. The means necessary to be employed to counteract the effects of over-doses, in other words, the antidotal effects.

2. The means of detecting the alkaloid in cases of poisoning by it, as well as the means of recognizing the symptoms of poisoning.

It was long supposed that the vegetable alkaloids had no antidotes, and that antidotes were only found in that class of remedies that could be rendered inert by chemical decomposition; but the therapeutist has remedies that act as antidotes to symptoms produced by other remedies. The antagonistic actions of belladonna and opium are of this class. As the science of therapeutics makes advances, these antidotal powers will be better understood.

We see, by reference to cases 1, 2, 3, and 4, where atropia was administered in full doses and all fluids withheld, that the effects of the medicine were much more severe than when diluents were freely given. We see by these cases that the free use of water alone enabled the system to recover from a large dose, a dose that was large enough to destroy life where diluents were entirely withheld. We see also in these cases that the kidneys were highly congested and unable to eliminate the urea.

If a large dose of atropia has been taken, and time

sufficient has elapsed to have it absorbed into the sys tem, we should do more good to give diluents freely than to give emetics as usually recommended.

The mutual antidotal powers of opium and belladonna have been freely discussed in the medical journals, and many interesting cases are recorded. Of these the most numerous and perhaps the most interesting are reported by Dr. Norris, in the *American Journal of Medical Sciences*, October, 1862, p. 395. Twenty-seven cases are here given of belladonan-poisoning treated by opium, and of opium-poisoning treated by belladonna.

Another interesting case is reported in the same journal, for April, 1866, p. 434, wherein $\frac{1}{48}$ grain of atropia produced violent symptoms which were almost immediately relieved by morphia. In the *Philadelphia Medical and Surgical Reporter*, for September, 1866, p. 225, a case is reported where 3 oz. of laudanum were taken, and recovery took place by free use of belladonna. Another case of the same description is recorded, *American Journal of Medical Sciences*, April, 1856, p. 541. In the same journal, for January, 1861, p. 288, a case is reported where two grains of atropia were taken, and the patient recovered. I do not believe that in this instance the atropia was pure. There are several instances recorded where a grain of atropia was taken, producing alarming symptoms, from which the patient ultimately recovered.

A paper by Dr. Downs, on the antagonistic effects of opium and belladonna, is to be found in the *Transactions of the New York State Medical Society for* 1866.

Dr. Ogle, in the *St. George's Hospital Reports*, relates a case of trismus that was relieved by subcuta-

neous injection of meconate of morphia and sulphate of atropia combined. Dr. Tatum and others in this hospital are in the habit of using these two alkaloids in combination, and they state that they find more benefit from their combined action than from either substance alone.

There is, upon the whole, abundant evidence to prove that opium and belladonna, morphia and atropia, are mutually antidotal.

In the use of atropia for the purpose of dilating the pupil, the action of the atropia can be more quickly overcome if a solution of sulphate morphia, containing four grains to the ounce of water, is applied to the eye by means of the eye-cup.

A solution of calabar-bean, or the paper or gelatine of the same, produces an antagonistic effect to atropia when applied to the eye, and an eye that is atropised quickly recovers from its dilatation if the calabar-bean paper is applied within it, and, if a little stronger than the atropia, it will soon cause contraction of the pupil.

Calabar-bean has been used as an antidote to atropia, and, it is reported, with success.

On the second division of this subject—"The means of detecting an alkaloid in cases of poisoning by it"—there is no question in the whole science of medicine that needs more thorough research than this, and I feel that time may be well spent in experimenting, and in bringing together facts which will elucidate the difficult and yet unsettled points of the physiological action, the pathological conditions, the chemical reactions, and microscopic appearances, which are necessary to a full understanding of the medico-legal science in each case of poisoning.

Many medical men, and most of the public who compose a jury, suppose that a poison must necessarily be detected by a chemist before a criminal should be convicted of poisoning. This error has arisen from the fact that, until within a few years, the corrosive metallic poisons were the only ones used by poisoners, and these poisons were always readily found by chemical means. But the more educated part of the community now know that there are poisons that cause death more quickly, less painfully, and with more difficulty of identification, than was formerly the case. The chemist has given to the medical man the most powerful vegetable alkaloids, several of which endanger the life with fractions of a grain. To what extent has the toxicologist kept pace with the chemist, and how far is he enabled positively to assert the presence or action of an alkaloid in a case of poisoning? As yet, we have not many cases of poisoning with the alkaloids on record, but, as these powerful agents become more commonly known, they will be more frequently used. With what degree of certainty are we able to detect them?

Two interesting cases, with an opposite termination, have occurred within a few years in England, and the points raised in these cases are just such as need elucidation in all cases of poisoning by the alkaloids, and we will endeavor to clear up some of these doubtful points.

On the trial of William Palmer for the poisoning of Cook by strychnia, upon a careful chemical investigation after death, no strychnia was discovered; but the pathological conditions which strychnia induces, and the unmistakable physiological actions of the alkaloids during life, were present. The jury, satisfied

with the scientific acumen of the therapeutist, found the prisoner guilty, and he was punished for the offence. The toxicologist here could only prove one link in the chain of evidence—the action of the medicine—the presence was not found; it was only proved to *have* existed.

The other case to which I refer was the trial of a surgeon by the name of Sprague, living at Ashburton, in England, who was accused of placing atropia in a rabbit-pie for the purpose of poisoning his wife.[1] The physiological action of atropia was produced. A portion of the pie and some of the vomited matters were subjected to chemical analysis by Mr. Herapath, of Bristol. Mr. Herapath testified that atropia was discovered by him in the pie as well as in the vomited matters. The symptoms also, under which the poisoned person suffered, corresponded, in his opinion, with the symptoms produced by poisoning with atropia. The methods of analysis resorted to were described to the jury by Mr. Herapath, and a rigid cross-examination was resorted to by the prisoner's lawyers; but Mr. Herapath demonstrated the presence of atropia. The ground taken for the defence was, that rabbits and some other animals occasionally eat belladonna-leaves with impunity, and, therefore, there might exist in the flesh of this rabbit sufficient atropia to produce all the effects described. It was not proved that this rabbit had eaten belladonna, but that such a thing might have happened. The experiments of Runge, of Berlin, were quoted, and much stress laid upon them. Some forty years ago, Runge, *secteur* in the University of Berlin, stated in a paper published in the *Journal de Pharmacie*, x. 85, that he had fed a rabbit for

[1] London Chemical News, vol. xii., p. 72.

eight days on the leaves of belladonna, datura, and hyoscyamus, and that the animal suffered no inconvenience. He stated, moreover, that the poison had not escaped absorption, for it was found by the physiological test—viz., the dilatation of the pupil of a cat—to be present in the rabbit's urine. The latter fact would only show that some of the alkaloid—perhaps a minute portion—had been absorbed; but a further observation showed that in reality all had been taken up, for, when the rabbit was killed, the residue of the food in the rectum was found, by the same test, to be completely free from atropia.

Mr. Sprague was acquitted. This result seems to have been arrived at by mystifying the jury, who could not distinguish between the lawyer's special pleading and the facts presented by a scientific chemist. As a doubt existed in their minds, the prisoner had the advantage of it.

In nearly all our works on materia medica these experiments of Runge's are quoted, and have been generally accepted as accurate, or at least as probable, as some of the ruminants have been known to eat and suffer no inconvenience from medicinal plants that produce dangerous effects upon man.

"Some years after Runge's paper had been published, there appeared (in the *Archives Générales*, xviii., 302) an account of some experiments made by M. Reisinger, who stated that the dose of atropine and hyoscyamine—in his opinion identical substances—which produced in half an hour symptoms of poisoning in a puppy of three months' age, produced no effects at all on young rabbits, and he concludes his paper with saying that 'ces animaux paraissent insensibles à l'action interne ou externe de ces substances.'"

Dr. W. Ogle, lecturer on physiology at St. George's Hospital, with a view to elucidate this case, has performed a number of experiments upon rabbits both with belladonna and atropia. He says:

"Can rabbits eat belladonna with impunity?

"EXPERIMENT.—In the beginning of last August I was supplied, by the kindness of Mr. Squire, with a large quantity of fresh-cut belladonna. I first satisfied myself that the leaves contained atropine, by applying a crushed one to the eye of a kitten for a few seconds. The pupil was soon enormously dilated. I then fed a healthy young rabbit, about four months old, for six days exclusively on the plant. The animal ate the belladonna with the greatest readiness, consuming all the leaves, blossoms, and young green shoots, but leaving the harder stems and roots. It consumed several times its own weight during the course of the week. During the whole time it remained perfectly well. The pupils were very large at the outset and remained so throughout. The only symptom observed was, that, in running about, the animal occasionally gave a comical kick out with its hind-legs, which was taken at the time, by myself and others who watched the rabbit, for an accidental exaggeration of a movement often seen in rabbits, but which I have now no doubt was in reality due to the belladonna. After six days, my stock of belladonna being exhausted, the rabbit was killed. The viscera were quite healthy, the stomach and intestines full of *débris* of green leaves, the bladder empty.

"The rabbit, after skinning and cleaning, was given to a dog, in order to ascertain whether the flesh was in any degree poisonous. But, though the dog was kept for three whole days from all other food, it refused to eat a morsel of the rabbit, so that this part of the experiment came to nothing. I was not aware till then that some dogs will undergo starvation rather than touch the flesh of rabbits.

"This experiment corroborates that of Runge, and shows clearly that a rabbit can live, at any rate, for six days on the leaves of belladonna without any inconvenience.

"2. When a rabbit is fed on belladonna, is the atropine absorbed?

"3. Is atropine as distinctly poisonous to rabbits as it is to man?

"EXPERIMENT 1.—I administered to a healthy rabbit, three months old, a grain of sulphate of atropine dissolved in water. The rabbit's mouth was held open and upward, and the solution was passed down the œsopha-

gus in a tube, great care being taken that none escaped. The animal was watched carefully for many hours consecutively, and then visited and examined at frequent intervals. No other symptom was produced than dilatation of the pupils.

"Experiment 2.—To another rabbit of the same age, two grains of the sulphate of atropine were administered in the same way. Dilatation of the pupils was produced, and also the symptom that I noticed before in the animal fed on belladonna, viz., a slight exaggeration of the action of the hind-legs now and then, when the rabbit was running about. This symptom had not the least resemblance to muscular paralysis. The rabbit ran just as well as ever, and the movements of which I speak only occurred now and then at intervals as it ran. In all other respects the rabbit seemed perfectly well, and ate its food as usual."

It results from these experiments that a rabbit of three months' age can take by the stomach, without inconvenience, a dose of atropine which would kill a man. I now come to the more severe test of hypodermic injection:

"Experiment 3.—To a rabbit, about three months and a half old, I administered, by subcutaneous injection in the back, one grain of the sulphate of atropine. The rabbit, in a quarter of an hour, began to lick its forepaws, as though there was some uneasy sensation in them. After three-quarters of an hour it was seen again to lick its forepaws, as though there was some uneasy sensation in them, and then to drum with them rapidly for a few seconds, and this same spasmodic drumming occurred some half dozen times in the course of the next three hours. There was also noticed now and then, when the animal was running about, the same furious 'kick out' with the hind-legs which I have before mentioned as occurring in the rabbit fed on belladonna. There was no muscular paralysis whatever. The animal ran about as well as before the injection, playing with another rabbit, and from time to time nibbling at some lettuce-leaves. The uneasiness in the paws was not manifested in any way after four hours, and after that time no other symptom was observed than dilatation of the pupils, which began soon after the injection was made, and lasted till the next day."

I should say that I have produced the same symptoms—viz., dilatation of the pupils, drumming with the forepaws, "kick out" with the hind-legs—with half the dose used in this experiment. On the other hand, these symptoms have sometimes been so slightly

marked as almost to escape notice when the dose injected has been much larger.

"Experiment 4.—I injected three grains of the sulphate of atropine into each of two rabbits, one six months, the other three months old. In each the same symptoms ensued as were described in the last experiment, and, notwithstanding the tripled dose, there was no greater severity in the effects produced."

In the next experiment Dr. Ogle injected three grains and a half of the sulphate of atropia in the back of a rabbit, with the same effects as those last described. A few drops of urine, passed by the rabbit an hour and a half after the injection, were put into the eye of a kitten and produced marked dilatation. In the next experiment five grains of sulphate of atropia were used with a like result, the urine producing the same effect upon the eye of a cat.

Dr. Camus[1] has experimented with atropia upon rabbits, and he says that one gramme—15½ grains—constitutes the minimum dose fatal to a rabbit. Dr. Ogle has produced the same result with three grains.

It will be seen, by these experiments of Dr. Ogle's, that rabbits can tolerate enormous doses of atropia—doses large enough to kill several men.

Dr. Ogle draws the following conclusions:

"1. That a rabbit of middle age can live for, at any rate, six days exclusively on belladonna without inconvenience.

"2. That a rabbit can tolerate enormous doses of atropine administered either by the stomach or by subcutaneous injection, and that this tolerance is not due to non-absorption of the poison.

"3. That this tolerance increases with the age of the rabbit.

"4. That dilatation of the pupils is, however, produced just as readily, if not more so, in an old rabbit, as it is in a young one."

These experiments of Dr. Ogle's are exceedingly valuable; but it was a great misfortune that some chemist was not associated with him to ascertain how

[1] Gaz. Hebdom. de Méd. et de Chir., Aug. 11, 1865.

much of the alkaloid could be detected in the urine, the intestines, and the tissues of the rabbit, if killed at any given time during the experiment. As the matter now stands, we are but little better off than when Runge left it: the knowledge that we possess is but confirmed. The alkaloid is without doubt absorbed, and the question now to be answered is, Is the meat of such an animal fit for human food, and how much of an alkaloid can be retained in the flesh? For toxicological purposes we need not at present discuss how much of an alkaloid can be retained within the flesh of an animal when an alkaloid is given by hypodermic injection, or by the mouth; but the point of importance is, how much alkaloid may at any time exist in the flesh of an animal that has fed upon a poisonous vegetable? If, upon a case of poisoning, a plea is to be urged that the poisoned person partook of meat from an animal that was poisoned from eating food that was innocuous to it, but that was poisonous to man, we need to know with great exactness to what extent and in what form that poison may be found in the flesh of the animal. We need also to be able to distinguish the poison that may exist in the flesh of such animal eating such poisonous vegetable, from a poison that may exist by being given as an alkaloid to an animal while still living, either by way of the mouth, or by injection of the alkaloid subcutaneously. Such questions open an entirely new field in medico-legal science.

These experiments of Runge's, confirmed and largely amplified by Ogle, prove that rabbits can eat belladonna with impunity, and it is equally probable that hares share this immunity from danger; but it is not at all probable that these animals would in a

state of nature eat sufficient to make their flesh dangerous to human beings. But, in this case of Sprague's, the courts have established a precedent, and have acknowledged that such probability exists. Does this immunity exist with rabbits only? Is belladonna the only vegetable poison that may be eaten without injury? May not these animals have a like immunity with hyoscyamus, digitalis, aconite, lobelia, etc., etc.? I have tried the following:

Experiment 12.—To a large she-goat, having two kids by its side, I presented some freshly-gathered belladonna; the goat refused to eat it. As the goat was tied where the grass was very scarce, I went to a field and gathered a quantity of fresh grass. This I fed to the goat, mingling with it the belladonna; both were eaten together until she had eaten two ounces of the leaves of the belladonna. In two hours I again saw the goat; the kids had been suckling in the mean time. I could see no marked dilatation in the pupils of the eyes in either the old goat or the kids. I got about an ounce of her milk, which was fed to a kitten, without any ill effect or without dilatation of the pupils. The next day I gave four ounces of belladonna-leaves in the same way. In four hours, upon returning, I found the pupils of the eyes in both the old goat and in the kids dilated, but I observed no other effect.

About a week after, I gave four ounces of dried hyoscyamus alone to this same goat, without observing dilatation or any other symptom follow it.

To one of the kids I gave one ounce of dried digitalis, without producing any noticeable symptom; to the other I gave about ¾ oz. of hyoscyamus, with the same result.

Experiment 13.—I passed down the throat of a kid, about four months old, a bolus containing one drachm of extract of belladonna. In less than an hour the pupils were very widely dilated, and the kid did not play or eat. It remained lying down for some hours. When I saw it the next day the pupils were still dilated, but in every other respect it seemed quite well.

Experiment 14.—To another kid, of about the same age, I gave one drachm of extract of hyoscyamus. I did not see that it produced any effect whatever.

Experiment 15.—To the same kid mentioned in experiment 13 I gave 6 drachms of extract of belladonna. It produced about the same effects as those mentioned in experiment 13. The goat was not in any way injured, the only unpleasant effect being the enormous dilatation of the pupils. All the urine that this kid made for 15 hours after taking the bolus was saved.

It amounted to 19 drachms. This, with a syringe, was thrown into the stomach of a young dog, about four months old. Within 10 minutes the pupils of the dog's eyes were widely dilated. The dog was locked up and not again seen for 16 hours. It was found dead. The stomach was empty, the pupils of the eyes were enormously dilated, the heart was filled with black, thick, uncoagulated blood. The bladder contained a small quantity of urine. A few drops of this were placed into the right eye of an old dog; dilatation took place, which did not pass over for three days.

This urine, amounting to 180 grains in weight, was put into a watch-glass, and carefully evaporated. The residue was washed repeatedly with small quantities of strong alcohol, filtered, and again evaporated on a watch-glass. This residue was washed with a small quantity of chloroform, and drawn up into a small pipette; it amounted to ten minims. A drop of this chloroform solution placed upon a glass slide, and mixed with a drop of "an aqueous solution of bromohydric acid, saturated with free bromine,"[1] gave a peculiar agitated appearance under the microscope, with yellowish streaks, which again became colorless and again reappeared; as the chloroform evaporated, a yellow amorphous precipitate continued to appear, which after a while assumed a crystalline form—these crystals first appearing upon the edges, but being more distinct and more perfect in the centre.

Another drop of the solution, mixed with a drop of alcoholic solution of carbazotic acid,[1] gave the same peculiar agitated appearance during the evaporation of the chloroform; a yellow precipitate soon formed, which, stirred with a piece of fine silver wire, yielded distinct small crystals.

With a solution of terchloride of gold, a pale-yellow precipitate formed, after a while assuming a crystalline form.

With a solution of iodine in iodide of potassium, a reddish-brown precipitate formed. (Microscopic drawings of these crystals were shown to the alumni.)

The heart of the dog, together with the blood it contained, and that which could be collected from the large vessels, was cut in pieces and put into a beaker-glass, with four ounces of strong alcohol and ten minims of pure sulphuric acid. This glass was held in hot water for ten minutes, and constantly stirred with a glass rod. The fluid portions were strained through fine muslin, and evaporated to one ounce. This was filtered, evaporated to half an ounce, rendered distinctly alkaline by liquor potassæ, and then evaporated to dryness. This was three times washed with chloroform, and the chloroform allowed spontaneously to evaporate. The watch-glass was carefully washed with fresh chloroform, and this was drawn up into a pipette. (Wormley's process.)

It yielded no evidences of atropia with either of the reagents above mentioned.

Four ounces of the flesh of this dog, taken from the hind-leg, were

[1] Wormley's process.

treated in the same manner as the heart and the blood. No evidences of atropia were to be discovered.

Experiment 16.—An old male rat, caught alive, was placed under the influence of chloroform; a grain of the sulphate of atropia in solution was thrown, by means of a proper syringe, into the stomach, and a ligature was passed around the penis, so that no urine could be lost.

The rat showed the usual symptoms of poisoning by atropia, and died in a little over four hours. He did not vomit. The rat was skinned and opened. The stomach was found empty, and the mucous membrane reddened in patches. The intestines were not examined. The heart was full of black, thick blood. The kidneys were both highly congested. The bladder was full; it was tied and removed. The lungs were dark-colored, and the pulmonary arteries were filled with dark blood. The skin, the head, the stomach, and intestines were thrown away. The body was split in half, and one part given to a young, active dog. The dog was watched for 12 hours, and no ill effect was noticed; the pupils of the dog's eyes were not dilated.

The other half of the body, together with the heart and the blood it contained, was treated in the manner recommended by Prof. Wormley, and the residue subjected to the reagents used by him for the detection of atropia. I could discover no evidences of atropia.

The bladder was opened, and found to contain five drachms of urine.

Two dogs were selected, and a drop of this urine put into the right eye of each; marked dilatation took place in the eye of both; but it was not excessive, and had all passed over when next seen—about 14 hours.

The urine was subjected to analysis in the same manner as that mentioned in experiment 15, and with a like result, showing the presence of atropia.

Experiment 17.—A very large, strong male rat was caught in a trap. He was a coarse, dirty-looking animal, so, before handling him, he was put into warm water, and repeatedly thrust under it to wash him well. Chloroform was administered. Upon taking him out of the cage, a large, yellow circular crust was found upon the head, running down upon the neck, another crust of the same character was found upon the right hip, and another smaller one upon the outer portion of the right fore-leg. These were the cup-shaped crusts of porrigo favosa, similar to those described by Dr. Draper, of New York, to Bazin, as existing upon mice caught by him, and proved by him to be true porrigo by transmission to a cat, and from the cat to children. (A pen-and-ink sketch of the microscopic appearance of a part of this crust was shown to the alumni.)

A subcutaneous injection of half a grain of sulphate of atropia was used; in a few minutes the pupils of the eyes were found to dilate, and as the rat revived from the influence of the chloroform the pupils could be seen steadily to dilate. In 15 minutes the subcutaneous injection was repeated upon the other side of the animal. The rat was so thoroughly un-

der the influence of the atropia, and the pupils were so widely dilated, that he made no attempt to escape when placed upon a shallow box on the table. In 15 minutes more the subcutaneous injection of half a grain was repeated. In one hour and five minutes from this last injection the rat died. He was immediately skinned, excepting the head and feet, which were thrown away with the skin. The stomach was empty, but was deeply congested, the heart was full of dark, thick blood, the lungs were dark-colored, the kidneys were deeply congested. The whole carcass of this rat, divested of head, skin, stomach, intestines, heart, lungs, kidneys, and bladder, was given to a large, coarse dog. The dog ate it readily. He was immediately tied to the table and closely watched. The pupils were but very little, if any, dilated, and the dog suffered no inconvenience from his meal.

The carcass of this rat was in the same condition that the carcass of the rabbit, in the pie we have mentioned, would have been had the rabbit fed freely and fully upon belladonna; it was in the same state that the carcass of the rabbit was that Dr. Ogle fed upon belladonna-leaves for a week, and which his dog refused to eat. Within 45 minutes, one and a half grain of sulphate of atropia was given hypodermically, and the rat lived an hour after the last injection, evidently long enough, as the result proves, to have eliminated or to have destroyed the activity of the poison.

In warfare the ship-builder has constructed heavily-plated vessels, that they may approach without danger near to fortifications and destroy them. The artillerist, in self-defence, has not been idle or ignorant, and has made, to meet these iron-clad monsters, balls of enormous weight and of enduring hardness, propelled through rifled cannon, which are capable of holding sufficient powder to force the ball with such velocity that it will enter and rend the heaviest plate-armor.

The toxicologist has not been as successful as the artillerist. Fortunately, the knowledge of the alkaloids and the skill to use them are as yet confined to the better-educated class of the medical profession; but, as they become more common, and bad men learn their power, they will undoubtedly be more used for purposes of poisoning. When it is known that a subcutaneous injection of a few drops of poison will take

a life, leaving no trace whatever of the cause, and which no toxicologist can discover, it is greatly to be feared that such means will frequently be used. Every thing, then, that can be done to elucidate the physiological action of these alkaloids, the pathological conditions they leave of their action, the chemical reagents that will detect them in their minutest quantity, and the physiological action of the collected secretions, or collected poison, is of the utmost importance to the safety of society. It behooves us of the profession frequently to look and inquire how far our knowledge has advanced, and to give every aid and assistance in our power to those who make a special study of these matters.

Let us review what we know of the toxicology of atropia:

Atropia and strychnia are probably the most easily detected of any of the vegetable alkaloids, owing to well-known peculiarities of their physiological action. The first invariably causes dilatation of the pupil of the eye; the latter causes its own peculiar spasm. As we have seen, in the preceding pages, every man or animal to whom atropia has been administered has exhibited this characteristic dilatation. In the rabbit-pie analyzed by Herapath, he proved the presence of atropia by this well-known physiological action of the alkaloid upon the eyes of animals—touched with a little of the substance recovered from the pie, and from the matters vomited by the victim. Ogle collected urine from rabbits dosed with atropia, and with it produced the physiological dilatation on the eye of a cat.

In experiment 15, we see that the urine, collected from a goat dosed with belladonna, dilated the pupil

of the eye of one dog when placed in it, and, when passed into the stomach of another dog, destroyed its life, and atropia was found in sufficient quantity in the urine of the poisoned dog to be recognized by the chemical reagents pointed out by Wormley; and that a drop of this urine also produced dilatation in the eye of another dog.

In experiment 16, we find that the urine of a rat, poisoned with atropia, caused dilatation in the eyes of two dogs.

If we are not, then, always able to recognize atropia by chemical means, in cases of poisoning by it, we have strong probabilities of recognizing it by the "physiological test." Hyoscyamin and daturia produce the same effects, but daturia and atropia are identical in their chemical reactions and physiological effects.

Sprague was liberated on the plea that the rabbit in the pie might have fed upon belladonna, and thus have become poisonous. Herepath recognized atropia in the pie. Ogle did not carry his experiment far enough to ascertain if the rabbit, fed by him on belladonna, was rendered poisonous thereby. In experiments 15, 16, 17, where atropia was given to animals, and death caused by its action, the flesh of the poisoned animal was not poisonous, and failed to yield chemical evidence of being poisoned. Wormley,[1] however, was more successful, doubtless I should say more skilful, for he recognized atropia in the blood of a dog poisoned by fluid extract of belladonna, and also in that of a cat. But there is a difference in these cases of Prof. Wormley's and those I have presented, and I think an important difference. In the first instance,

[1] Wormley's "Micro-chemistry of Poisons," p. 636.

he killed the dog an hour and a half after giving the poison, and the cat died in three minutes after the poison was injected into the lungs.

In the cases I presented, all the animals lived for some hours, and died slowly from the natural effect of the poison.

It will be seen, in nearly all instances where I have experimented *with excessive doses*, that I have been enabled to prove the presence of the poison in the urine where I could not discover it in either the blood or flesh. The methods of analysis and the reagents used are those so ably pointed out for our use by Prof. Wormley.

It will not do to dismiss this subject of the flesh of animals becoming poisonous to man, or to other animals, by food the animal may eat; we have too many facts before us to prove this to be the case.

The "trembles," or "milk sickness," of our Western States is a strong evidence of this kind of poisoning. The flesh, the milk, the butter, or cheese, from one of these poisoned animals always produces sickness in any animal that eats it, and often death, and many men have died from cutting themselves in skinning an animal suffering with "trembles."

All who have yet written upon this subject assert that the cause of this sickness is a vegetable. Drake supposed it to be the *Rhus toxicodendron*, others have asserted that the *Esculus Ohioensis* is the cause, and latterly the *Eupatorium ageratoïdes* is said to produce the disease.

The "foot poison" is another instance of the same kind; several of the fungoid growths produce their own peculiar effects. The partridge-berry is said to render the flesh of birds poisonous, and the flesh of

sheep that have eaten the laurel is said to produce unpleasant symptoms.

We cannot, then, deny that the flesh of wild or domestic animals may be poisonous to man, from some poisonous vegetable eaten by the animal. We see that rabbits and goats can eat belladonna without injury to themselves, but there are very few instances on record where persons have been atropised by eating meats.

Since the introduction of the alkaloids, toxology has become a science, and investigators in various parts of the world have attained an accuracy in discriminating the different alkaloids that is truly surprising. Wormley, in his incomparable work on "The Microchemistry of Poisons," points out the characteristic distinguishments of the various alkaloids, and their behavior with different reagents. He shows infallible means of recognizing, not grains of poisons, but $\frac{1}{500,000}$ part of a grain, and, with a woman's delicacy of touch and artistic taste, his patient wife has drawn and engraved the camera-lucida portraits that each poison presents with the various reagents.

But even this excessive delicacy of recognition is not sufficient to detect the presence of several of the most potent alkaloids when given either by the mouth or by hypodermic injection. Aconitia is now prepared so pure, that "$\frac{1}{50}$ part of a grain"[1] is a fatal dose for an adult; $\frac{1}{100}$ part would be fatal if thrown either under the skin or into a small vein. This poison, if fatal, would of course be *absorbed*, and it would be utterly impossible to recognize it either by color, test, or by crystallization, with the most powerful microscope. "Assuming only the blood con-

[1] Proc. Am. Pharm. Ass., 1866, p. 239.

tains it, this equals the $\frac{1}{9,800,000}$ part of a grain. If diffused through the whole body, it would then be reduced to the $\frac{1}{44,800,000}$ part of a grain.

I have before referred to the recognition by Wormley of atropia in the blood of a dog, poisoned with five drachms of fluid extract of belladonna. Also in that of a cat. Duffield says: "Among numerous cases of poisoning by opium or its alkaloids, which have fallen to my lot to examine and depose on, I cannot conscientiously say that I ever detected *absorbed* morphia."

There are hundreds of cases scattered through the medical journals, giving like negative results, and proving the almost impossibility of detecting any of the alkaloids when given in ordinary fatal doses—doses that are absorbed into the circulation. No one can deny that these medicines are *absorbed*, because some few instances are given where they have been detected in the blood and in the urine; but the greater part of the poisons seem, so far as our present knowledge goes, to spend their influence upon the blood or tissues, and, in so doing, to be changed or decomposed. It is only in *excessive* doses that the kidneys eliminate a portion unchanged, or that the blood contains a recognizable quantity. My experiments are all with excessively large doses.

Dr. De Vry, who has made many experiments with the alkaloids, thinks that those portions of the alkaloids, which produce their peculiar action, are decomposed and changed.

With these facts before us, it behooves every medical man to become a better therapeutist; for the conviction of criminals who poison must be proved by the symptoms which are noticed in the dying person.

The various alkaloids produce their well-known and characteristic symptoms; these symptoms, if well marked, are as fully entitled to consideration as the facts presented by the chemist, which, after all, are but evidences of another character.

Upon the literature of the subject, in addition to the authors already quoted, I would refer to Cohn & Koerner (*Berl. Klin. Wchschr.* 16, 1865); F. Evans (*British Medical Journal*, September 21, 1861); T. Hayden (*Dublin Quarterly Journal*, August, 1863); E. Rollett (*Wien. Med. Wschschr.* 95, 1865); Otto (*Vrtjhrschr. F. ger. Med. N. F.* 157, 1866); Schmidt's *Jahrb.*, cxx, 181, 1865; Rud. Hohl (*De Atropini effectu*, Hal., 1863); Béhier (*L'Union*, 85, 1863); Chambers (*London Lancet*, 1864); Lubelski (*Gaz. Hebd.*, September 7, 1864); Commaille (*Jour. de Pharm. et de Chim.*, April, 1865); Woodman (*Medical Times*, 385, October, 1864). Donder's work, above quoted, is very full and instructive on all that relates to the action of atropia on the eye, and Wormley's "Microchemistry of Poisons" on all that relates to its toxicological properties.

ART. IV.—*A Case of Calcification of the Choroid, Ciliary Processes, Crystalline Lens, and Capsule.* By CHAS. A. HART, M. D., New York.

J. KRISSINGER, aged about 55 years, was presented to me June 25th by Dr. W. A. Garman, of Berlin, Somerset County, Pa., who requested me to examine the patient for a supposed cataract.

The history rendered was as follows: Twenty years ago, he began to experience a loss of vision in both

eyes, which, after a considerable period, rendered him totally blind in the right organ, vision being still preserved to a limited extent in the left; this condition being attended with considerable pain, relief was sought, and he presented himself to Dr. Gross, of Berlin, who pronounced his case one of hard cataract, and finally attempted the operation of depression, which afforded no relief. He has been the subject of frequent rheumatic attacks. Objectively the appearances presented, when I saw him, were as follows: The globe was atrophic, the cornea presented evidences of former inflammatory action, having a roughened and semi-opaque appearance. The pupillary space was occupied by a dirty yellowish-white body. The iris having lost its power of action, was uninfluenced by either light or solution of atropia.

Neither the ophthalmoscope nor concentrated light revealed any thing beyond the dense character of the lens.

The patient was very much distressed by the constant pain he was suffering, and there seemed to be no chance of relief other than enucleation, which being proposed, was accepted. Accordingly I performed the operation on the 30th of June. The morbid changes, found upon an examination of the eye, were a complete calcification of the choroidal tunic, very dense about the optic disk, gradually thinning off toward the ciliary processes, which were also filled with calcified deposits, though not in a perfect state of organization; none of the true choroidal tissue could be discovered. The retina was thickened, and in several places detached; the optic disk was atrophied, with no trace of the vessels remaining. The vitreous body was broken down and changed into a fluid mass, which contained

a quantity of crystalline matter resembling cholesterine, though I cannot assert that it was such, being without a microscope at the time. The crystalline lens and capsule were both found in the normal position; the capsule being thickened and opaque, and when opened grated under the knife like sand, revealing the lens shrunken in diameter and completely calcified, together with an unorganized white crystalline paste, which, having since been examined with the microscope, proves to be crystals of cholesterine with a few of the phosphate of lime. The lens was examined, by deflected light through the bull's-eye condenser, with the one-inch objective; the surface was a yellowish-white color, perforated by numerous minute foraminæ. Nothing resembling the minute structure of bone could be discovered. The patient recovered nicely from the operation, and has since been entirely free from suffering.

ART. V.—*A Case of Hernia of the Liver in a Newborn Child.* Reported by G. FRANK BRICKETT, M. D., Lawrence, Mass.

CASES of hernia of the liver are not often met with; yet the circumstance is well known among pathologists, and mention is made of many cases in the works on pathological anatomy.

Aug. 26, 1868, I was called to attend Mrs. H——, in her sixth confinement. On my arrival, I was told that the child was already born. I prepared to tie the cord, when I found what appeared to be a large tumor presenting itself at the umbilicus of the child; tied the cord about three inches from the tumor, and gave the child to the care of the nurse till I had further cared

for the mother. On examining the child, a female, it appeared to be perfectly formed, with the exception of this large tumor protruding through an aperture on the abdominal wall at the site of the umbilicus, extending from the lower edge of the sternum to near the pubes, smaller at the base, or, as I would call it, the neck or ring. The integuments were dark and excoriated. I satisfied myself the tumor was not intestinal, but a case of misplaced liver. At my visit Saturday, A. M., Aug. 29, I found hæmorrhage from the tumor, near where it seemed to unite with the healthy integuments. I invited my friend Dr. Sargent, of this city, to see the case Saturday, 4 P. M. He was at a loss to form a diagnosis, but the child died at 7 P. M. from hæmorrhage, and I held an autopsy a few hours after.

I add the result of the post-mortem in the words of Dr. Sargent, who assisted me in the same:

A few days since, Dr. Brickett invited me to see the infant child of Mr. H——, and a few hours after I saw it the child died from hæmorrhage from the tumor. I assisted at the post-mortem examination. On making a crucial incision over the abdomen, cutting through the integuments and abdominal muscles, which were very thin, giving the dark appearance to the tumor—on cutting into the tumor it proved to be an enormously large misplaced liver, the whole of which was in front and covering nearly all the contents of the abdomen, being above and anterior to the diaphragm. The right kidney was large, all the other organs were healthy and well formed.

In a practice of more than forty years I have never seen or read of a similar case.

Proceedings of Societies.

MEDICAL SOCIETY OF THE COUNTY OF NEW YORK.

Adjourned Anniversary and Stated Meeting, Nov. 2, 1868.

E. R. Peaslee, M. D., President, in the chair.

After the reading of the minutes of the Comitia Minora for the year, and the transaction of the usual business, Dr. Peaslee, the retiring President, addressed the Society, viz.:

Fellows of the New York County Medical Society:— Before leaving the chair to give place to my successor, I should thank you collectively and individually for that kindness which has rendered my official relations to you so agreeable to myself, and my duties so easy of performance.

In two respects this Society deserves the highest commendation, and its present prosperity is attributable mainly to these two agencies:

1. Its organization is as perfect for the fulfilment of its objects as I can conceive a similar organization to be. All its ordinary business being transacted by a committee of ten persons, elected specially for that purpose, the attention of your whole body is very seldom diverted, by the discussion of mere business matters, from the legitimate scientific and practical subjects which should occupy us here. I think I may say that not twenty minutes in all have been spent by this Society on business matters, aside from the regular programme for each evening, during the past year. We have all seen, in other similar organizations, how much time is lost in discussing resolutions and parliamentary questions. Some gentlemen seem ever on the alert to find a subject for a resolution, rather than to elucidate a scientific question. I would by no means deprive any one of the privilege all possess in this respect, but it is a privilege every member of a scientific body should be very careful not to abuse.

I consider this Society, therefore, a model for all county societies, so far as its organization is concerned.

2. But the present *spirit* of this Society also secures a

success like the present. I say *present* spirit, not because it has changed during the past year, but because I speak from positive knowledge, and an intimate acquaintance with its members during this period. This results in part from the advantage in organization which I have specified; but still more, and mainly, from the character and habits of its members. No one comes here merely to hear himself speak; but all for scientific and practical improvement. And I may say that no reasonable expectations have been disappointed at a single meeting the past year, in respect to the character and quality of the papers here presented. There has been no failure at a single meeting; and some of the papers read have already been highly eulogized in foreign medical journals. During the past year also about fifty members have been added to this Society; and it has for two or three years past been the largest medical organization in this country, except the American Medical Association.

Under the present influences, therefore, this Society must continue to prosper. But should it get under the control of a clique, or wander from its legitimate objects, from that moment its decadence begins. I trust that personalities may never find their way into discussions here. The provocation may sometimes be very great; but the gentleman and scholar must not yield to it. Science is not a personal matter; not a thing of to-day or to-morrow, or your property or mine, but belongs to all, and for all time to come. Let, therefore, the present spirit remain unchanged, and our exercises from month to month will continue to be worthy of men, the motto of each of whom should be that of this Society—

"Miseris succurrere disco."

The President then declared the Anniversary Meeting adjourned. On motion of Dr. Van Kleek, a vote of thanks was tendered to the retiring officers for the able, satisfactory, and impartial manner in which they had discharged the several duties of their offices during the year past.

The President-elect, Dr. George T. Elliot, then assumed the chair, and delivered the following address:

Gentlemen: In accordance with custom, a short address

is expected from the President-elect of this Society; and it is extremely gratifying to me to express my sincere thanks for the honor which has been conferred upon me. It has been spontaneous, unsought, unsolicited, nor had I any knowledge or surmise that such a step was contemplated until I received the official notification of my election.

Such kindness has inspired me with the warmest desire to prove myself worthy of the expectations of my friends, and with the earnest hope that during my year of office the Society may not derogate from the high position to which it has been advanced by my predecessors, and by gentlemen well known to you and within the reach of my voice.

For the accomplishment of these aims, I ask the cordial coöperation of all, and indulgence for faults which may spring from inexperience in the duties of a presiding officer.

As I understand these duties, they are chiefly:

To deal fairly and justly with all; to respect individual rights and susceptibilities while jealously guarding the rights and the time of the Society; to persuade the seniors to lay before you the gathered treasure of their experience; to stimulate the young to condense within the limits of a short paper the results of studies of the same subject published in different languages by observers from different stand-points; to encourage debate, since from its heated atmosphere a flood of light is often thrown upon obscure and vexed questions; to develop talent discouraged from lack of appreciation, or clouded by modest doubts; to bring together in this hospitable and metropolitan city physicians of education and earnest purpose from all nationalities, as well as those from all the States of our common country, who have cast their lot here with us; to weld together these constituents into one solid and homogeneous mass, for the support and pride of our profession; to stimulate by our example our sister county medical societies throughout the land, since on the full development of these and the State Medical Societies, culminating in the National Medical Association, depends much of the influence for good which we can wield for the community in which we live.

Nor can the greatest possible success in these endeavors conflict with that mutual interest and sympathy which we feel

for the other medical societies in the city working with us for a common purpose. We are linked together for good, and it is impossible for one to advance without aiding the others, and raising the standard of our profession. It has become essential for men to test the attainments of their fellows, and study their characters in these meetings. In the village and the town the evils of too great familiarity are often felt, in the great city the tendency is to estrangement. Men who do not join medical societies, or labor in public positions, or write, or teach, may live without the sympathy or the acquaintance of their brethren.

Such professional hermits hide their light under a bushel, though perhaps it may occasionally be as well that the amount and character of that light be kept from view. Nor is the value of these societies limited to the amount of professional knowledge to be derived from their meetings. Prejudices are often dissipated, misconceptions vanish, friendships are formed, kindly feelings developed, the hard repelling exterior is often shown to mark a strong intellect and generous sympathies, talent is borne to the surface, pretension and sciolism stand exposed.

With such feelings, then, we enter on the sixty-third year since the organization of this Society. All but one of its founders have passed away. Its archives teem with associations of the past. It is identified with the interests of our profession. God grant that from our labors we may reap a harvest fruitful of pleasant and profitable memories for the future, that we may do something to encourage and stimulate our successors.

The Chair then reported that certificates of membership had been granted by the Comitia Minora to Drs. J. Marion Sims, J. G. Perry, J. H. Griscom, Truman Nichols, Benjamin F. Dawson, Adoniram B. Judson, and Henry F. Walker. Announcement was also made of the death, since the last meeting of the Society, of Dr. John W. Shepard, a graduate of the Coombe Lying-in Asylum of Dublin in 1846, and of the Medical Department of the University of New York in 1852, and a member of this Society since 1861. Dr. Shepard died suddenly October 5, 1868.

Of Dr. Robert H. Maclay, a graduate of the College of

Physicians and Surgeons in 1824, and a member of the Society since 1831. Dr. Maclay had for some years been retired from the active duties of the profession. He died October 15, 1868.

The paper of the evening was then read by Professor William H. Van Buren, M. D., his subject being, "On Some Points in the Treatment of Stricture of the Urethra."

(This paper is published in full in another part of this number of the Journal.)

Dr. James R. Wood remarked that non-traumatic stricture, although beginning as a urethritis, was not commonly seated in the mucous membrane—indeed, structural lesion of this membrane was comparatively rare. The urethral constriction was commonly due to the deposit of organized fibrin in the submucous areolar tissue, simply corrugating the mucous membrane itself. This deposit might be so extensive as to obliterate the structure of the bulbous portion, as he had established by numerous dissections. In the treatment by simple progressive dilatation, therefore, exciting absorption of the inflammatory deposit, the mucous membrane was not stretched, except in the rare cases where it had itself become disorganized. He heartily agreed with Dr. Van Buren in considering this the best mode of treatment for the great majority of cases.

He had lately had the opportunity of meeting several of the distinguished surgeons named in the paper, and of observing their different modes of treatment. As a rule, each was the advocate of some one method, almost exclusively. Syme would hardly ever perform any operation but external urethrotomy. Holt would use his dilator in every case where he could manage to insert it. The Holt men contended that the infrequency of urinary infiltration, after the use of this instrument, showed that the mucous membrane was rarely torn; and this confirmed the view just expressed, that the stricture depends on submucous deposit. Syme performed his operation with a guide, having a very small grooved staff screwed to the sound. He cut in the median line, to avoid hæmorrhage, and was careful to divide the stricture throughout its whole extent, say from one-quarter to one-half an inch, claiming that after this complete division the trouble never returned. The patient

was taught to use the sound periodically, as in the practice of every good surgeon after any form of operation.

Dr. Wood had for several years practised a modification of Syme's operation, which he had never seen described. It consisted in cutting cautiously down, through the fibrinous deposit, to the mucous membrane, but not through it, thus freeing the constricting bands, as in the operation for strangulated hernia without opening the sac. On approaching the urethra, the sound, which had been pressed close to the distal extremity of the stricture, could be made gradually to penetrate it; or, if a slender instrument had already been passed through, this could be replaced by a larger one. The method was not applicable to all cases—not to those where the mucous membrane was diseased, as in traumatic stricture produced by caustic or otherwise. But where it could be employed, its advantage was manifest, in completely obviating the danger of urinary infiltration, and resulting pyæmia, the chief cause of a fatal event after perineal section.

Perineal section without a guide had been performed first in this city, and probably first in this country, by Dr. David A. Wells, whose operations the speaker, as his pupil, had often witnessed. Their uniform success was due, in great measure, first, to his careful selection of cases; and, secondly, to his careful preparatory treatment. The medical treatment was a matter of great importance, and apt to be too much neglected.

Internal urethrotomy Dr. Wood would practise, as he had often done, when the seat of stricture was within three inches of the meatus, or even when deeper, if Civiale's urethrotome could be introduced beyond it, so as to cut from behind forward. Internal incision of a deep stricture from before backward was very hazardous.

The occurrence of stricture of the prostatic urethra had sometimes been denied. Dr. W. had never seen it as the result of gonorrhœa; but, within two years, he had operated some three times, at Bellevue Hospital, for traumatic stricture of this portion, in each instance with success. He might never meet with another case.

Dr. Gouley said that constitutional treatment, in stricture of the urethra, should not be lost sight of, though too much

reliance had been placed upon it alone, by some practitioners.

He is in the habit of advising abstinence from spirituous liquors, rest, the use of diluents, quinine, the tincture of chloride of iron, and opium, to allay spasm and to relieve pain. This alone, he thought, was of little avail.

In his opinion, the main dependence of the surgeon should be on the mechanical treatment, which might be summed up as follows:

1. Gradual dilatation.

2. The immediate treatment—consisting of stretching, rupturing, and the internal incision.

3. The external division.

4. In certain cases of impassable stricture with retention, he thought favorably of the operation employed by Mr. Edward Cock, of London, and gave a detailed account of the mode of performing it.[1]

In support of the views expressed by the author of the paper under discussion, Dr. Gouley gave the statistics of forty-seven severe cases of stricture which he had treated within a few years past. Of the forty-seven cases, forty-four recovered and three died.

In twenty-six cases the "*immediate treatment*" was resorted to, and in twenty-one cases the "*external division.*" All of the twenty-six cases, in which the immediate treatment was employed, were followed by satisfactory results.

Of the twenty-six cases, thirteen were treated by stretching and rupturing. Thompson's dilator was used in six of these cases.

In the thirteen remaining cases, the *in*ternal division was performed with Civiale's, Maisonneuve's, and Gouley's urethrotomes.

No catheter was tied in, in any of the above cases, and they progressed with scarcely any untoward symptoms.

Of the twenty-one cases of "external division," eighteen recovered and three died.

In eleven of these cases a guide was used; in the other ten

[1] For a full description of this operation, see Guy's Hospital Reports, vol. xii., p. 267, *et seq.*

cases the operation was performed without a guide, as none could be passed. There were no deaths among these last ten.

In two cases a catheter was introduced into the bladder after free division of the stricture, secured in position, and allowed there to remain for forty-eight hours. Urethral fever followed in both cases.

In the nineteen remaining cases no catheter was tied in; they were treated as are patients after the operation of lithotomy. The urine flowed freely through the perineal wound, and firm union, in the majority of cases, took place within three or four weeks. In none of these did urethral fever supervene, not even in the fatal cases.

The causes of death were, in the first, erysipelas and pyæmia; in the second, advanced disease of the bladder, ureters, and kidneys; and in the third, thrombosis of the heart. Any other surgical operation might have proved fatal in this last case. The patient died within forty-eight hours after the operation.

In the twenty-one cases of external division, the following seemed sufficient indications for the performance of the operations:

1. Narrow traumatic stricture in sub-pubic curve.
2. Impassable mixed stricture (traumatic and gonorrhœal).
3. Impassable stricture with retention.
4. Stricture with retention and extravasation.
5. Narrow stricture with perineal abscess.
6. Stricture with a perineal fistula, which would not heal after persistent dilatation of the urethra.
7. Stricture which was undilatable, irritable, prone to bleed on the most delicate exploration, and attended with dribbling from overflow.
8. Stricture which, though dilatable to a considerable extent, recontracted very soon after cessation of the use of dilating instruments, notwithstanding that they had been used for a long time (the "resilient stricture of Syme").

Dr. Buck remarked that he found nothing to dissent from in the views presented by Prof. Van Buren. The most approved methods of treating stricture of the urethra had been brought before the Society, and their application to practice

in special cases accurately and judiciously discriminated; his own experience confirmed these views. He would briefly ask the attention of the Society to two or three points of practice, which might supplement Dr. Van Buren's very complete statement. Every experienced surgeon knows the formidable difficulties encountered in the operation of perineal section where no instrument, even of the finest size, can be passed through the stricture to serve as a guide for incising it. In his more recent experience, he had found great assistance in overcoming this difficulty in the use of an expedient, which is as follows: The largest-sized grooved sound, open at the end, is passed down to the stricture in the perineal portion of the urethra; an incision is made along the raphe, and the urethra opened a little anterior to the stricture. The sound being held against the stricture, a fine-sized probe or director is introduced into the wound and its point conducted along the groove, which will guide it to the orifice of the stricture, and often its passage through the stricture is accomplished with unexpected facility. The instrument in use at the New York Hospital for this latter step of the operation is found well adapted to the purpose. It is a grooved silver sound of the size of the smallest probe, and terminates in a fine probe point. When passed through the stricture, the groove furnishes a very convenient and sure guide for conducting the knife with which to incise the stricture.

He further stated that in one instance, in which an impassable stricture coexisted with extreme distention of the bladder from retention and accompanied with intense suffering, he had punctured the bladder through the trigone per perinæum. The case was that of a corpulent gentleman over sixty years of age, with stricture of long standing, that had given rise to perineal fistulæ at different times previously. An irreducible scrotal hernia on the right side distended the scrotum and appropriated to itself the integument of the penis, so as to completely conceal that organ. An existing phymosis still further complicated the case. All effort to pass the stricture, or even to enter it, having failed, and the extreme urgency of the patient's sufferings not admitting of delay even till daylight (it being midnight), it was decided to puncture the bladder, and to do it

per perinæum rather than per rectum. The operation was as follows: A transverse incision was made across the raphe one inch anterior to the anus, and, guided by the fore-finger in the rectum and the thumb in the wound, the incision was continued on between the urethra and rectum till the prostate was reached and accurately identified in its limits. The incision was carried still farther on between the prostate and gut, but the extreme depth of the perinæum did not permit the end of the finger to reach the posterior edge of the prostate. Beyond the end of the finger, however, a long-bladed, sharp-pointed bistoury was conducted flatwise and carried at least one inch farther, till it entered the bladder. Although no urine followed the withdrawal of the knife, owing to the falling together of the opposite sides of the track, a female catheter, introduced along the wound, entered the bladder without obstruction, and demonstrated the success of the operation by an abundant flow of urine. The catheter was secured in situ, and a bladder-skin attached to its outer end to serve as a receptacle. Six months were spent in the subsequent treatment, consisting of laying open the prepuce to facilitate access to the urethra, dilating two or three strictures anterior to the principal one in the perinæum. Various expedients were resorted to to overcome this last, till at length the patient consented to submit to the operation of perineal section. This was successfully accomplished, but not without encountering the most formidable difficulties. The catheter was now left out of the bladder, and the perineal track, that had been kept open for six months, healed without delay. Eventually the integrity of the urethra was restored, and the patient able to hold his water two or three hours and discharge it normally. He survived the operation more than two years, enjoying the benefit it had conferred till the last, though it continued to be necessary to introduce a full-sized sound once in four weeks during the whole of this time. If delayed beyond it, difficulty was experienced in passing the stricture.

Dr. Newman called the attention of the Society to the use of bougies of the *laminaria digitata* in the treatment of stricture of the urethra, and stated that in his practice he had met with marked success from them. He exhibited instru-

ments made of this material that he had frequently used. The advantages he claimed for this bougie were gradual yet somewhat rapid dilatation. The danger attending their use, from the end of the instrument toward the bladder becoming enlarged by the absorption of the fluids of the parts, so that the instrument might with difficulty be withdrawn, can be overcome by thoroughly coating the bladder end of the bougie with varnish, so that it cannot absorb moisture. The very gradual dilatation of the laminaria bougie is not productive of any unusual pain. The instrument will expand in a couple of hours to a size equivalent to from two to three sizes larger in the common scale of bougies. Of course, the patient must be immediately under observation during the whole of this time.

Dr. Howard desired to call the attention of the Society to one method of treatment, which had not been alluded to in the paper of the evening, or in the discussion, but to which he would advert in order to elicit the experience of the distinguished gentlemen who had preceded him as to its relative merits.

It is intended for the more deeply-seated strictures, and is called, by Sir Henry Thompson, the treatment by continuous dilatation. It differs from simple dilatation in this: that, instead of the momentary introduction of the largest bougie the stricture can be made to admit, every two or three days a bougie of an increasing but much smaller size than the stricture would admit is tied in; the stricture is kept thus loosely occupied for about twelve days, the patient being meanwhile in a recumbent posture. Dr. Howard regretted that the term continuous dilatation, used by Sir Henry Thompson, contradicted rather than described the process, which to him seemed to consist in no dilatation at all, but rather of continuous pressure without dilatation. He thought the pressure thus applied, while insufficient to inflict damage upon the mucous membrane of the urethra, might yet be great enough to induce slow ulceration, or absorption of the more lowly-organized constricting deposit beneath it. For these reasons he had anticipated, from this method, results more complete and permanent than from most of the other methods which had been mentioned.

Dr. Chadsey narrated a case, which came under his observation in 1844, in which, after free venesection and use of hot fomentations, etc., he failed in introducing an instrument into the bladder. Almost despairing of success, it occurred to him to apply the magneto-electric current from a small battery which he had with him. Passing an elastic catheter (with the end cut off) down to the seat of the stricture, he then inserted through the catheter a common knitting-needle, to which he attached the positive pole of the battery. The electrode of the negative pole was then brought into contact with the perinæum, and immediately the stricture appeared as it were to melt away before the current; in a few minutes he was enabled to pass the catheter into the bladder.

Dr. Gouley remarked that the doctor had unwittingly performed the operation of electrolysis, which had recently been so largely resorted to by Mallez and Tripier, of Paris.[1]

The Society then adjourned.

[1] Mallez and Tripier's cases are reported in this Journal, for February, 1868. It will be observed that in Dr. Chadsey's case the interrupted or so-called Faradaic current was employed, and the positive pole was applied to the seat of the stricture; while in electrolysis proper, as pointed out by Althaus, the continued current is essential, and the negative pole is brought into contact with the part to be operated on. The more probable explanation of Dr. C.'s case is, that it was a case of retention from inflammatory stricture, in which the treatment, previously adopted by the doctor, had brought the parts into a state so nearly approaching full relaxation, that it only needed the gentle stimulus afforded by the application of this magneto-electric current to give complete relief. E. S. D.

Boracic Acid and the borates have recently been discovered to exist in the hot-water springs near Massa Maratima, a town of Central Italy, 22 miles northwest of Grosseto, the capital of the Maremma. The principal spring has a temperature of 80 degrees Fahrenheit, and yields about 13,000 gallons per day. The minor springs yield about the same quantity. The borax lagoons of the Maremma, with which these new springs are supposed to be in connection, are the sources of immense profit to the owners.

Reviews.

ART. I.—*A Manual of the Pathology and Treatment of Ulcers and Cutaneous Diseases of the Lower Limbs.* By JOHN KENT SPENDER, M. B., Lond., etc. London: JOHN CHURCHILL & SONS, 1868. 8vo, pp. 89.

SOME of our older readers may remember a modest book, entitled "The Pathology and Treatment of Ulcers of the Leg," long since out of print and now mostly forgotten. That work, by the father of the author of the little manual before us, serves as the basis of the present work, and with true filial piety Dr. Spender acknowledges to it his obligations.

Opening up his book by a short chapter on the causes of ulcer of the leg, the author then proceeds briefly, but yet with sufficient clearness, to describe the forms of ulcer most frequently met with in practice, viz., the varicose, syphilitic, scrofulous, and the traumatic. Then comes what is for the reader the most interesting as well as in fact the most valuable portion of the book, viz., that devoted to the question of treatment. This he bases upon two principles: first, the imitation, as far as possible, of the natural processes of cicatrization; and second, restoring or introducing a healthy action of the parts. To accomplish the first object, we are advised to apply such a substance as shall form an incrustation over the edges of the sore resembling in its effects the natural scab, and to disturb the dressing as seldom as possible. In the chalk-ointment the author claims to have found the best substance for the production of this artificial crust, and, though we are inclined to look upon the persistency with which Dr. Spender advocates the use of this agent much as we regard the enthusiasm of one riding a hobby, or afflicted with a "mission," we must in candor admit that theoretically the proposition is well put, and appears tenable. Practically we have no knowledge of the efficacy of this special remedy, though we purpose acquainting ourselves with its action, and

advise our readers to do the same. The formula we give below.[1]

The indication in the second principle is best met by "powerful and well-adjusted compression of the whole limb," and this compression is better accomplished by the flannel bandage than by strapping, or elastic stockings. In the following chapter full details are given of the application of these principles, and though we might at first thought consider that too much space has been devoted to minutiæ, a moment's reflection tells us that the success in the treatment of obstinate ulcer of the leg can only be attained by the most careful attention to details. In one point Dr. Spender is at variance with most authorities: he does not deem it necessary to keep the patient at rest, but claims that exercise, to an extent short of absolute fatigue, so far from being injurious, only promotes the cure, provided the chalk-ointment, etc., are used. This does not accord with our own experience, and we can recall many a case, in hospital and dispensary practice, in which rest seemed the inducing agent in the cure, but we never used the "chalk-ointment." Again, the extreme pressure which Dr. Spender advocates and applies, in our estimation, unless most accurately adjusted, can only be productive of harm. But we must do our author the justice to say that precisely here he makes his strongest point, and insists upon this accurate adjustment. Were every one possessed of equal dexterity with himself, this would answer; but so long as beginners, to say nothing of veterans, will bungle, it will be wise to adopt a little moderation in the application of pressure.

But this chapter, with the following one, on the hinderances and difficulties met with in the treatment of this class of cases, is so full of good sound sense and practical ideas that we are not disposed to find fault. We feel sure that, if carried into practice, the author's teachings must avail in combating the absurd notions too generally prevalent, both among professional

[1] The best method of preparing the ointment is as follows: Take three pounds of prepared chalk, and two pounds of lard. Reduce the chalk to a very fine powder; melt the lard, and add gradually the chalk-powder. Stir and mix thoroughly until cold. This makes a much more homogeneous compound than can be procured by any process of trituration.

and lay people, of the management of these cases. Who, that has ever followed up the practice at one of our city dispensaries, does not recall the soap and sugar, the pork-rinds, the cabbage-leaves, villainous poultices, and all sorts of pet nastinesses there in vogue? It were worth a lifetime to have corrected this popular ignorance, and the dissemination of correct views among physicians is the first step in this laudable undertaking.

The reader must not understand that constitutional treatment is lost sight of by Dr. Spender. On the contrary, he insists upon it, and attaches to it its proper valuation. Finally, the author quietly proceeds to knock the underpinning from out the old-time belief that it is not always prudent or safe to heal a chronic ulcer of the leg; and this fanciful notion, which happily has not now the importance accorded to it thirty years since, is left without even a peg on which to hang a hope of a claim to recognition. In one other point, viz., that it is never necessary to resort to amputation—for this is the legitimate deduction from our author's statement, that "the proposal does not deserve a moment's discussion, but is recorded in these pages as a curiosity of surgery"—Dr. Spender will find many to differ from him; but, with improved methods of treatment, or, rather, with broader views of the true principles of treatment, the necessity for such a sad resort must rarely exist.

"*E libris nemo evasit artifex*," says our author most justly at the opening of one of his chapters; but we venture to say buy this book and adopt its precepts, and there be many we know of who would at all events become *better* workmen thereby.

Art. II.—*A Treatise on the Principles and Practice of Medicine; designed for the Use of Practitioners and Students.* By Austin Flint, M. D., Professor of the Principles and Practice of Medicine in the Bellevue Hospital Medical College, etc. Third edition. Thoroughly revised. Philadelphia: Henry C. Lea, 1868. 8vo, pp. 1002.

Three times, now, within the brief period of two years, we have called attention to the appearance of separate editions of

Dr. Flint's admirable book. The edition before us, by a change in typographical execution, has been only slightly enlarged in bulk, while it contains a very large amount of material not found in the last edition. It is a satisfaction to see a book so fully up to date, and also to see the attention paid by the author to the contributions found in our periodical medical literature. Many most valuable papers, appearing in journals of very limited circulation, naturally are brought to the notice of comparatively very few. Dr. Flint has rescued some such papers, perhaps, from oblivion—certainly has made them available for all.

This book, differing entirely from Aitken or Reynolds's in its scope and character, must be—we are inclined to believe—*the* text-book for students for some time to come; and if each successive edition is as carefully worked up as is the present, there is no reason why it should not long continue to hold the very front rank of books of its class. It is quite unnecessary for us to enter into any review of the book. On its first appearance, its merits were pretty well canvassed in our columns, and the author has availed himself, in each succeeding edition, of the criticisms of his reviewers, and has labored honestly, as well as successfully, to keep fully abreast with the advance of science, and has cheerfully corrected the deficiencies which are inseparable from the first getting up of such a volume. The sincere and hearty manner in which Dr. Flint has publicly thanked his reviewers for calling his attention to omissions in his earlier edition and the readiness with which he has availed himself of their suggestions go far to prove that there is not necessarily an antagonism between an author and his critics. Further, still, they tend to show that the author is a truly liberal-minded and catholic teacher of medicine. There are many others, whose books we are familiar with, who would be the gainers by the adoption of little more of this same spirit.

We have only to renew the commendation we have formerly made of Dr. Flint's book.

ART. III.—*The Science and Practice of Medicine.* By WM. AITKEN, M. D., Edinburgh, Professor of Pathology in the Army Medical School. Second American from the Fifth enlarged and carefully revised London edition; Adopting the new Nomenclature of the Royal College of Physicians of London. With Large Additions, by Meredith Clymer, M. D., ex-Professor of the Institutes and Practice of Medicine in the University of New York, etc. Philadelphia: Lindsay & Blakiston, 1868. 2 vols., 8vo, pp. 927, 1079.

ON the appearance of the first American edition of Dr. Aitken's work, we gave a somewhat detailed analysis of its contents. It is therefore unnecessary for us now to attempt a review of this edition, for pretty much the same ground must be gone over. But we desire especially to call attention to the additions made both by the author and the editor, and to endeavor to make some estimate of the comparative value of the English and the American imprints.

Dr. Aitken tells us that he spent fifteen months in revising his book, and the result is, that the present edition has increased in bulk upward of one hundred pages. The subjects of Malignant Cholera, Paralysis, Epidemic Cerebro-Spinal Meningitis, and Intestinal Obstruction, have been entirely rewritten. The new chapter on Cholera is admirably done. The experiences of the late epidemic, as is well known, gave rise to many new theories and accumulated a large amount of valuable observations and material. This Dr. Aitken has worked up in such a way as to give an excellent *résumé* of the existing knowledge on this knotty subject. He is bound to no theory, and hence he calmly canvasses the merits of theories and views of the various investigators of the many questions pertaining to cholera, its pathogeny, origin, and mode of transmission, contagiousness, meteorological conditions, localizing causes, etc. The fungus theory, which originated with Bœhme in 1838, but which has recently enlisted the labors of such distinguished observers as Klob, Hallier, Thomé, Parkes, De Bary, and Berkeley, is discussed at great length by Dr. Aitken; but our knowledge thus far is not sufficient to warrant

the opinion that the fungi are the cause of cholera or are the means of its conveyance. This chapter is unquestionably the most satisfactory of the changes made by the author in the present edition.

He also announces that "the subjects of Progressive Locomotor Ataxy, Progressive Muscular Atrophy, Glosso-laryngeal Paralysis, Aphasia, and Dilatation of the Bronchial Tubes, the Application of the Sphygmograph and its Tracings in Diseases where it has been of Use, are subjects considered for the first time in this text-book." This statement, however, is true only of the English edition, for all these subjects were fully treated of in the first American edition by Dr. Clymer, the editor. And furthermore, it appears that Dr. Aitken has mainly condensed from Dr. Clymer the articles which he (Dr. A.) has thus introduced—the only exception being the article devoted to "Dilatation of the Bronchi," which is an abridgment of Dr. T. Grainger Stewart's paper in the *Edinburgh Medical Journal* for December, 1867.

Another improvement by the author is the incorporation of the new nomenclature of diseases, adopted by the Committee of the Royal College of Physicians. The labors of this committee, comprising without exception the most distinguished representative men of the profession in England, extended over a period of ten years, and must be accepted as a decided improvement upon the former nosological tables of Dr. Farr, which are now abandoned. The proposed table, although wonderfully accurate and precise in all its details, strikes us as being quite too cumbersome—though this estimate may be partly due to a long familiarity with and use of Farr's tables. An equal acquaintance with the new table, which it is proposed shall be revised every ten years, in accordance with the advances made in pathology and nosology, and which is therefore termed the "provisional nomenclature," would doubtless impress us quite as strongly in its favor. Our space will not allow a comparative examination of the two nomenclatures; but it is evident that the provisional table must be for the present the accepted plan for the registration of diseases. We strongly advise all interested in this subject to familiarize themselves with it.

These are the more important changes and additions resulting from Dr. Aitken's fifteen months' labor. Let us now see what the American editor has done by way of still further increasing the value of the work. First he has stricken out his own elaborate articles on Aphasia, Locomotor Ataxy, Glosso-laryngeal Paralysis, and the Sphygmograph—being content with Dr. Aitken's abstract of the same—but he has reprinted side by side with Dr. Aitken's chapters on Cerebro-Spinal Meningitis, and on Bronchiectasis, his own papers on these subjects as they appeared in the first edition; and the contrast is so strikingly in favor of the American editor, that we can readily imagine the quiet—we had almost said malicious—sort of satisfaction he must have experienced in thus collating the two articles. Then, besides numerous and lengthy additions and interpolations to the author's text, he (the editor) has written *thirty-six* articles which have been incorporated in their proper places. Several of the articles are on subjects never before treated of in any text-book on the practice of medicine. These articles, as will be observed by an examination of the titles which we present below, are mostly upon practical topics, and essential to the completeness of the book.[1] We cannot enter into any critical examination of these additions by Dr. Clymer, but they

[1]
1. Camp Measles.
2. Spinal Symptoms in Typhoid Fever.
3. Prognosis and Diagnosis of Typhoid Fever.
4. Chronic Malarial Toxæmia.
5. Pernicious Remittent Fever.
6. Typho-Malarial Fever.
7. Chronic Camp Dysentery.
8. Cholera Morbus.
9. Cholera Infantum.
10. Hereditary Syphilis.
11. Corpulence.
12. Gonorrhœal Rheumatism.
13. Delirium of Inanition.
14. Chronic Alcoholism.
15. Epidemic Cerebro-Spinal Meningitis.
16. Progressive General Paralysis.
17. Acute Centripetal Paralysis.
18. Myo-Sclerosic Paralysis.
19. Physical Diagnosis of Diseases of the Cerebro-Spinal System.
20. Auscultation in Health and in Disease.
21. Irritable Heart.
22. Disease of the Heart, how far a Disqualification for Military Service?
23. Chronic Pyæmia.
24. Capillary Bronchitis.
25. Plastic Bronchitis.
26. Dilatation of the Bronchi.
27. Sclerosis of the Lung.
28. The Inoculation of Tubercle.
29. Curability of Consumption.
30. Acute and Rapid Phthisis.
31. The Neuroses of the Larynx.
32. Medication of the Throat and Lungs by Atomized Fluids.
33. Syphiloma of the Liver.
34. The Neuroses of the Stomach.
35. Addison's Keloid—Scleriasis.
36. Statistics of Tracheotomy.

are all marked by two prominent characteristics, viz.: an unusually thorough acquaintance with the bibliography and literature of the subject, and most accurate descriptions, so far as known, of the pathology of the special diseases treated of. Indeed, in these two respects, they are far in advance of Dr. Aitken's chapters. Taken together, these additions amount to over *five hundred pages* of the English edition. By the use, however, of different type and of a larger page, there are actually less pages in the present edition than are found in the first. Those additions are no insignificant item when mere quantity is taken into consideration; but, looking at the quality, we are bound honestly to state our conviction that the American edition is, by all odds, the more valuable and more desirable, of the two, both for student and practitioner. We have previously recorded the opinion that this work is in many respects the best body of practical medicine extant, and now, after a careful examination of the whole book, we have no reason to alter that opinion.

Bibliographical and Literary Notes.

Dr. Ellis's Medical Formulary[1] belongs to a class of books which are our pet horror. They undertake by a short cut to put a man on the road to practice, without grounding him in the principles of medicine. We do not mean to say that such is the intention of the authors of this class of works, but practically this is just about what it amounts to. That our opinion in the premises is worthless, and that the bulk of the

[1] The Medical Formulary: being a Collection of Prescriptions derived from the Writings and Practice of many of the most eminent Physicians in America and Europe, together with the usual dietetic Preparations and Antidotes for Poisons. To which is added an Appendix on the Endermic Use of Medicines, and on the Use of Ether and Chloroform; the whole accompanied with a few brief Pharmaceutical and Medical Observations, by Benjamin Ellis, M. D., late Professor of Materia Medica and Pharmacy in the Philadelphia College of Pharmacy. Twelfth edition. Carefully revised and much improved, by Albert H. Smith, M. D., Lecturer on Obstetrics to the Philadelphia Lying-in-Charity, etc. Philadelphia: Henry C. Lea, 1868. 8vo, pp. 374.

profession are not of our way of thinking, is evident enough from the announcement on the title-page, that the book has passed into the twelfth edition. And we have not the slightest doubt that these editions will continue to multiply, for our young men will continue to buy that which furnishes a ready-made treatment for all the ills that flesh is heir to. And so, whether it be a colic or a chordee, a tapeworm or a tonsillitis, a diarrhœa or a dropsy, it is all here, and you have only to turn by index to the appropriate page, to select an "elegant and judicious" prescription. Now, there are upward of one thousand such prescriptions contained in the book before us, and all can be purchased for the moderate sum of what you would receive as a fee for a single visit. What wonder, then, that the twelfth edition has been reached? There is, however, one redeeming feature in this book. The prescriptions, which are written in Latin, are, as a rule, written correctly; and if each owner of a copy of all these twelve editions will only study style, the result will be to elevate the standard of Latin scholarship, if it does not make us any better doctors. Step into one of our large drug-stores, and look over the prescription-book. The liberties there taken with the Latin tongue fairly make one of sensitive nerves shudder; provided, of course, he himself knows any thing of, and has any regard for, the purity of the language. If we must write our prescriptions in Latin, we ought to write them correctly; but, rather than display our ignorance, we should prefer to see the good old Anglo-Saxon universally adopted. Study, then, the orthography of these prescriptions, my young friend, for it is plain enough that you cannot resist the temptation of securing at one stroke a remedy for every disease. And even if you do not successfully combat every colic, etc., you will have the proud satisfaction of knowing that you have not outraged the proprieties of the classical tongue, which is imposed upon us as the only correct medium for conveying our therapeutic wants, and the serene consciousness will be yours that you have written an "elegant and judicious" prescription.

For sale, we presume, at all medical book-stores.

From the publishers we have received a copy of the

Visiting List for 1869.[1] These little vade-mecums have become so essential to the practising physician, that they need no commendations of ours to assert their utility. It is a matter of taste which variety of the Visiting List a physician may select; but, having chosen one, he will generally continue to use that particular kind. All other things being equal, we should prefer the simplest form, and, as a matter of fact, largely due to habit, we admit, we have always preferred and used the List published by Lindsay & Blakiston. It does not, however, contain that varied material which most of the other Lists present—material which is valuable, but which we have sometimes considered misplaced in a manual the primary object of which is merely to answer the purpose of an account and memorandum book. This material generally embraces lists, and more or less complete descriptions, of diseases and their remedies, the doses and uses of medicines, etc., and to one whose memory is at all treacherous it may frequently prove very serviceable. One's aim, however, we think all will admit, should be to avoid the necessity of carrying a Practice of Medicine or a Treatise on Therapeutics in one's pocket.

Dr. Samuel Gregory's mind is evidently quite severely exercised on the question of the appropriate title to be given to women physicians. And so, in the little pamphlet before us,[2] he discusses the question from various stand-points of view, viz.: philological, diplomatical (so far as relates to the dear creatures themselves), æsthetical, practicable, etc. And from all this he arrives at the triumphant conclusion that "Doctress" (abb. Drss.) is the only correct and proper designation, and this, too, without wishing to deprive his lady-friends of any titular honors, or to throw any obstacle in the way of their success.

Dr. Gregory, further admitting that the press is a potent

[1] The Physician's Hand-Book for 1869. By Wm. Elmer, M. D., and Albert D. Elmer, M. D. New York: W. A. Townsend & Adams.

The Physician's Visiting List for 1869. Eighteenth Year of its publication. Philadelphia: Lindsay & Blakiston.

[2] Doctor or Doctress? By Samuel Gregory, M. D., Secretary of the New-England Female Medical College. Boston, 1868. Pamphlet, pp. 8.

agency in moulding forms of speech, calls upon editors and other competent authorities for an expression of opinion for or against his views.

Our excessive modesty forbids our putting on record whether or no we are devoted to Doctresses, we beg pardon, we mean the title only, and not the seductive beings themselves; but, perhaps, some of our numerous readers, less diffident than ourselves, may boldly assume the responsibility, and thus relieve Dr. Gregory's suspense.

"Under which king, Bezonian? Speak, or"—

follow our example, and do not commit yourself.

By the way, while the doctor is at this sort of work, we would suggest that a little patching up of the title of the college, of which he is the secretary, would indicate quite as high regard for the purity of the Queen's English as is evidenced in the question he has thrown out for the worriment of peace-loving editors and other "competent authorities."

A little book, bearing the somewhat grim title of "Hurried to Death," has been published in London, wherein the author enters into the question of the geographical distribution of heart-disease in England and Wales, and argues that the proportion of deaths from this cause varies uniformly with latitude, longitude, and aspect of the maritime and physical character of localities.

Dr. Thomas Inman, of Liverpool, has written a very curious book, of which the first volume (8vo, pp. 800) has just been published by Trübner & Co., London, entitled "Ancient Faith Embodied in Ancient Names; or, an Attempt to trace the Religious Belief, Sacred Rites, and Holy Emblems of certain Nations, by an Interpretation of the Names given to Children by Priestly Authority, or assumed by Prophets, Kings, and Hierarchs." It is illustrated with many plates and woodcuts.

New Books.—There is a special activity just at present in the publication of medical books, as will be evident from the

lengthy list we present to our readers this month. It is gratifying, too, to observe that most of these works are of sterling merit; a fact which indicates pretty clearly the demands of the profession for an elevated and substantial literature.

The papers of Dr. C. J. B. Williams, that have for some months been passing through the *Lancet*, on the subject of Pulmonary Consumption as it appears in private practice, have been collected by Dr. Meredith Clymer, and will soon appear, with the addition of an Introduction on the present state of Pathology of Tubercle, etc. It will be remembered that Dr. Clymer was the American editor of the well-known works of Dr. Williams on Practice, etc.; and there seems to be a peculiar appropriateness in his now bringing forward this last contribution of Dr. Williams to medical science. We shall look with interest for the appearance of this little volume, as the conflicting views of recent observers on the Pathology of Tubercle have so shaken our accustomed beliefs, that, if called upon to give an opinion on this subject, we should feel much like resorting to the Bunsbyian tactics, and intrenching ourselves behind a fortification of high-sounding but non-committal generalizations.

Messrs. D. Appleton & Co. announce that the translation of Niemeyer, by Drs. Hackley and Humphreys, of this city, is now passing through the press, and will soon appear. Dr. Tilt's Work on "*Uterine Therapeutics*" will be ready in a few days. The numerous additions and emendations made by the author to this edition, which is not a reprint, but has been expressly prepared by Dr. Tilt for the Appletons' edition, virtually make it a new book.

Messrs. Wm. Wood & Co. announce a work on "*Post-mortem Examinations, for the use of Coroners and others*," translated from the French, author not named. A new and enlarged edition of "*Flint's Auscultation and Percussion.*" "*Beard & Rockwell's Treatise on Practical Medical Electricity.*" New edition of "*Tilt's Diseases of Menstruation and Ovarian Inflammation.*"

Messrs. John Churchill & Co., of London, announce the following medical works: "*Pyæmia, or Suppurative Fe-*

ver, by P. M. Braidwood. "*Injuries and Diseases of the Jaws*," by Christopher Heath, M. D. "*Dictionary of Materia Medica and Therapeutics*," by Adolphe Wahltuch. "*Clinical Lectures on Diseases of the Urinary Organs*," delivered at the University College Hospital, by Sir Henry Thompson, M. D. This work is announced for republication in this country by Messrs. D. Appleton & Co. "*Diseased Conditions of the Knee-Joint, which require Amputation of the Limb or Excision of the Joint*," by William Paul Swain. "*A Manual of Orthopædic Surgery*," by B. E. Brodhurst. "*The Mechanical Treatment of Deformities of the Mouth, Congenital and Accidental*," by Robert Ramsay & J. Oakley Coles. "*Ether and Etherized Cod-Liver Oil in the Treatment of Pulmonary Consumption*, by Balthazar Foster, M. D. "*The Practice of Surgery, Clinical, Medical, and Operative*," by Frederick J. Gant. "*Lectures upon Practical Pathology and Surgery*," by Henry Lee.

By Messrs. Longman & Co.: "*Clinical Lectures on Chronic and Gouty Bronchitis, and on Pulmonary Emphysema*," by E. Headlam Greenhow, M. D.

Among recent French publications, we observe a work by Dr. J. Carrière, "*De la Tumeur Hydatique Alvéolaire*," "*A Treatise on Apparent Death and Real Death*," by Dr. F. Gannal; and a monograph "*On the Therapeutic Properties of Picric Acid*," by Dr. F. Parisel. A new work by Dr. Bergeret, "*On the Urine: its Nosological, Pathological, and Therapeutical Indications.*"

Du Diagnostic des Maladies des Yeux par la Chromatopsie Rétinienne; précédé d'une Étude sur les Lois Physiques et Physiologiques des Couleurs. Par Dr. X. Galezowski, etc. Avec 31 figures, une échelle chromatique comprenant 44 teintes, et 5 échelles typographiques tirées en noir et en couleurs. Paris: J. B. Baillière et Fils, 1868.

The Diagnosis of Diseases of the Eye by Means of Retinal Chromatopsy (perception of color); preceded by a Study of the Laws Physical and Physiological of Color. By Dr. X. Galezowski. With 31 figures, a chromatic scale comprising 44 shades, and 5 typographical scales in black and in color. 267 pages.

In German medical literature we observe the following recent publications:

Kriegs-Museum in Washington: eine Schilderung des Washingtoner Pathologischen Museums. Von Dr. L. Horst, Kais. Russ. Marine-Arzt. 1865. 8vo.

The Army Medical Museum at Washington: An account of the Pathological Museum at Washington. By Dr. L. Horst, of the Imperial Russian Navy.

Die Frage über die Heilbarkeit der Lungen-Phthisis, historisch, pathologisch, und therapeutisch untersucht. Von Dr. J. B. Ullersperger. Würzburg: Stahel, 1867.

The Question of Curability of Phthisis Pulmonalis, in its History, Pathology, and Therapeutics. By Dr. J. B. Ullersperger.

Löschner (Dr. und Prof. Joseph), Schlussbericht über die vom 21. Mai 1849 bis Ende December 1851 in Prag beobachtete Cholera-Epidemie; nebst einer Abhandlung: Die Cholera der Kinder. Gr. 8. Mit zwei Tabellen und einer Epidemienverlaufskarte.

Löschner (Dr. and Prof. Joseph), Final Report on the Epidemic of Cholera, which occurred at Prague between May 21, 1849, and end of December, 1851; with a Treatise on Cholera Infantum, and a Chart of the Course of the Cholera Epidemic. Frederick Tempsky, Prague.

The Relief of Pain in Open Cancer.—The field for experience in cancer at this hospital (Middlesex) is, as is well known, an unusually large one, and opportunity has therefore been afforded for testing fairly the action of remedies in affording relief in this distressing disease. We learn that the exquisite pain which belongs to open cancer is found to be best relieved by the stramonium-ointment, which is employed at this institution. The following is the formula for this in the hospital pharmacopœia: Half a pound of fresh stramonium-leaves, and two pounds of lard. Mix the bruised leaves with the lard, and expose to a mild heat until the leaves become friable, then strain through lint. The ointment thus prepared is spread upon lint, and the dressing changed three times a day.—*Lancet.*

Reports on the Progress of Medicine.

SURGERY.

1.—*Five Cases of Strangulated Hernia operated upon without opening the Sac.* By Erskine Mason, M. D. [Medical Record, August 1, 1868.]

Dr. Mason gives a careful history of the five cases, three of which were femoral, one oblique inguinal, and oblique inguinal (congenital). One only of the cases proved fatal. Appended to the narrative are some very judicious remarks on the feasibility of this operation, from which we extract the following:

These five cases I have transcribed from my note-book, for the purpose of showing the readiness, even in small, as well as in large hernia, with which the operation of not opening the sac can be performed, as well as its giving promise of far better success than when the sac has been opened, the gut exposed to the air, and subjected to handling. So seldom do we find the stricture to be contained inside the sac, and so rarely does real necessity occur for the opening of its peritoneal covering, that I think we can rarely be justified in choosing any other operation than the one advocated in this paper. It has been contended by some, that this operation might lead to the reduction of the gut, when in a gangrenous condition. This objection, it appears to me, might be used with equal justice against the employment of any means to effect reduction short of a cutting operation. Should the contents of the sac be such as to forbid their reduction, this could be determined both by the eye and by the sense of smell, in the majority of cases, without wounding the sac. If not, then it would be time enough to resort to the old method of operation.

In one case we were tempted to resort to this practice, from the dark appearance of the intestine and portions of the sac, but warm applications, by means of sponges wrung out in hot water, proved sufficient, after the stricture was divided, to restore the parts to their normal appearance. The majority of deaths occurring after the operation for strangulated hernia are due to peritonitis, and this certainly seems far more likely to follow after the sac, which is so often inflamed, is wounded, and the intestines and omentum subjected to digital manipulation, than where these are carefully protected from such exciting causes. Again, should any vessels, as the epigastric or obturator, become wounded, the risk of hæmorrhage taking place into the peritoneal cavity is avoided; and the patient's chances for recovery thus increased. Looking at these facts, and as we shall show from statistics the very favorable results of this operation, it seems surprising that surgeons should ever think of practising any other, when the case would at all admit of it. This operation is the one I believe now advocated by English surgeons, but as yet not so much practised in this country. Prof. Gross, in his System of Surgery, when speaking of this operation, remarks that "in this country it has probably not attracted as much notice as it deserves." As far as I have been able to learn, but little has appeared in our literature upon this subject, and we are almost wholly indebted to English surgeons for what has been written upon it.

Some few years ago, Dr. Henry B. Sands published the histories of some cases in the *New York Medical Times*, wherein he had performed this operation, together with remarks on the same; with this exception I do not remember ever having seen this operation treated of in our medical journals, though case after case of various kinds of hernia has been published, operated upon after the old method. The operation of dividing the stricture without the sac is generally supposed to be due to S. L. Petit; but according to South in his notes to Chelius's System of Surgery (*American Reprint*, page 303), both Franco and Parè "had cut into the abdominal ring, and did not open the hernial sac, except when reduction could not be effected." To Jean Louis Petit, however, is due the honor of first generally recommending this operation. According to Lawrence, this operation was performed by Petit in 1718, who not only advised it in old and large hernias which were adherent to the sac, but also recommended its more general employment. In 1750 this procedure was brought forth as entirely new by Ravaton in a Treatise on Gunshot Wounds, and he speaks of having operated with success in three cases. Monro the second was also an advocate of the operation, his first operation being in 1770. Sir Astley Cooper recommends this method in his work upon hernia, in all old ruptures, and believes surgeons will employ it more generally when they have learned its advantages from experience. The revival of this operation must be ascribed to Mr. Aston Key, who in 1833 published a memoir on the "advantages and practicability of dividing the stricture in strangulated hernia on the outside of the sac." Prior to this date, however, he called the attention of the profession to this method in a clinical lecture, published in 1829. (*London Med. Gazette*, vol. iv., p. 193.) Mr. Luke also strongly urges the practice of Petit, and gives the results of this operation in his own hands. (*London Med. Gazette*, vol. i., 1839–'40, and *Medico-Chirurg. Trans.*, vol. xxxi, 1848.) Mr. Luke says: "I have attempted the performance of Petit's operation in eighty-four cases. Of this number the operation was completely successful without opening the sac, in fifty-nine. In twenty-five it was necessary to open the sac to effect a reduction of the hernial contents, the operation generally varying in extent from one-half to one-quarter of an inch. With respect to the mortality among these patients, of the fifty-nine in whom the sac remained unopened, seven died; of the twenty-five in whom the sac was opened, eight died. These cases included those of femoral, umbilical, and inguinal. In three of these cases he states that Petit's operation was successfully completed; but the sac was opened after the reduction of the strangulated parts into the abdomen, to remove some doubts as to their perfect liberation. The proceeding in each case, however, was ascertained to be wholly unnecessary. Mr. Erichsen, in his System of Surgery, p. 728, states that of 77 operations for hernia, reported by Sir A. Cooper, 36 proved fatal; and of 545 cases recorded in the journals, and collected by Dr. Turner, 260 are reported to have died. The result, therefore, of Mr. Luke's operation is most favorable, when contrasted with such as these." According to the experience of those who have written upon this subject, the operation appears to be more successful in femoral hernia, owing to the stricture being found frequently in the neck of the sac in the inguinal variety. In all cases it would appear to me that this operation should first be attempted, and *then*, if found unsuccessful, only that portion of the sac opened which involves the stricture. Certainly no operation has ever held out greater inducements than the one so strongly advocated by Petit, Key, and Luke.

2.—*The Torsion of Arteries as a Means of arresting Hæmorrhage.* By T. Bryant, F. R. C. S. [Lancet, Aug. 15, 1868.]

Mr. Bryant read an important paper on this subject at a recent meeting of the Royal Medical and Chirurgical Society. He commenced by an historical sketch of the operation, dating from M. Amussat's original investigations in 1829, and then detailed a series of experiments which he (the author) had made with a view of determining the physiological effects of torsion upon bleeding arteries. Two methods of employing torsion are described—the "free" and the "limited." In "free" torsion the end of the artery is grasped by a pair of forceps and twisted freely. In "limited" torsion the end of the artery is drawn out of its sheath and grasped transversely with a pair of clasp forceps, about three-quarters of an inch from the divided extremity, while, with another pair of forceps, the free end is seized and twisted freely, as in the former method. Three or four complete revolutions of the forceps are enough for small arteries, and six or eight for large. The object of fixing the artery by the first pair of forceps is to limit the twisting of the vessel, and prevent too great separation of the artery from its vascular attachments. The following summary closed the paper:

1. That hæmorrhage may, with certainty, be arrested by torsion, from even the largest vessels. 2. That it is a safe and judicious practice in all cases in which the vessels are small or of moderate calibre; and that, as far as experiments and practice yet prove, it is equally so in arteries of the first magnitude. 3. That torsion may be "free" or "limited," the free method being applicable to vessels of moderate size, and even to the largest of the extremities, limited torsion being more adapted for the large and loosely-connected vessels. 4. That in torsion, as in the ligature, the permanent hæmostatic processes are alike due to the sealing of the divided inner and middle tunics; but that in the ligature there is only an irregular division of these tunics, while in torsion there is a complete division, separation, retraction, and valvular incurvation. 5. That in torsion the twisted cellular coat forms, with the retracted and incurved middle coat, the direct mechanical obstacle to the flow of arterial blood, in the same way as the compressed cellular coat does in the ligature, but that in torsion the twisted cellular coat and incurved middle coat become subsequently a permanent means of occluding the end of the artery, while the ligature of necessity becomes subsequently a source of irritation, and, too often, a means of undoing what has been done by Nature's own hæmostatic processes. 6. That in torsion the twist in the cellular coat of an artery, the division and subsequent retraction, incurvation, and adhesion of the middle coat, and the coagulation of the blood in the vessel down to the first branch, are the three points upon which its temporary as well as permanent safety depends, while the permanent safety of acupressure rests upon the last point alone, and its temporary effects upon the pressure produced by the needle. 7. That there is every reason to believe that when torsion has been successful on its first application, the fear of subsequent hæmorrhage is altogether groundless, for there is nothing, as there is in the ligature, to interfere with the

physiological processes set up by Nature to occlude the divided vessel, and, unlike acupressure, the temporary obstacle to the flow of blood becomes a permanent one. 8. That upon physiological grounds torsion has decided advantages over the ligature and the acupressure-needle, and that, if subsequent experience confirms what has been hitherto observed in the experiments on animals and the application of the practice in the human subject, we shall have gained a point of no mean importance, and simplified surgery in no slight degree. The paper was concluded by the author stating his belief that the practice was a safe and valuable one in many cases, if not in all; that it was not a crude idea, based upon a theory spun out of a fertile imagination, for it had its origin in observation of Nature's own processes; that it was based on the well-recognized physiological principles of natural hæmostatics; and that it was artificial only so far as the surgeon's art was employed in rendering these processes most available.

Dr. Humphry, of Cambridge, read a paper on the same subject at the recent meeting of the British Medical Association, of which the following is a summary:

The professor gave the results of his experience of torsion of arteries after operations, as well as the results of experiments on animals, and on the arteries of man and animals after death. For many months he has practiced torsion after all operations, including three amputations in the thigh, amputations in the leg, of the breast, excision of the knee, etc. It has answered quite well. There has been no after-hæmorrhage in any of the cases; the wounds have healed more quickly, and there has been less pain, than after the ligature. The operation is rather more troublesome, and requires more care and time than the ligature. He simply seizes the end of the artery with strong forceps, and, holding the forceps in the axis of the vessels, twists till the portion included in the grasp is twisted off and the forceps are quite free. In the process of torsion, as observed upon an artery twisted after death, the thick, inner, musculo-elastic coat is first severed, often as though it had been cut by a knife or ligature. As the torsion goes on, it is so compressed or squeezed by the twisting of the outer coat that its divided edge is commonly turned up, reflected, into the tube of the artery, to a greater or less extent, as the resistance of the outer coat is more or less prolonged, forming a valvular or funnel-like projection into the vessel. Thus there are the two things—the inversion of the inner coat and the twisting of the outer. It is upon the latter that reliance is to be placed for resistance to the flow of fluid from the vessel, inasmuch as its pressure causes and maintains the valvular inversion of the inner coat, and, further, by its own strength, offers a direct obstacle to the escape of the blood. This the professor has proved by injecting water, and connecting a column of mercury with the vessel.

3.—*Aneurism of the Arch of the Aorta; Repeated Mistakes in Diagnosis; Puncture of the Sac; Death; Autopsy.* Reported by N. S. Richardson, M. D., and A. M. Williams, M. D., of Macon, Missouri. [St. Louis Med. and Surg. Jour., May, 1868.]

We quote this most extraordinary case of professional blundering, leaving to our readers the opportunity of forming such

opinions as they may think indicated in the premises. We must, however, remark that we are well aware of the difficulty at times in making a diagnosis between abscess and aneurism, and we recall several notable instances of mistakes by most eminent surgeons. Mr. Dease, of Dublin, opened an aneurism, which he supposed to be an abscess, and killed his patient. Velpeau tied the carotid of a patient on an account of supposed aneurism, which turned out to be only a harmless benign tumor, and his patient died from the operation; and other equally well-known cases could be given. But the onus in the case before us rests upon the fact that, so far as can be gathered from the report, no attempt at a differential diagnosis was ever made, and the management was based upon a series of false assumptions. Drs. Richardson and Williams deserve credit for reporting the case, and in so doing they seem not to have been influenced by any feelings of ill-will toward the physicians who had charge of the case, and whose names are not made public:

J. B., æt. 39; married; intemperate. One year previous to his death he fell from a horse, since which time until his death he had pain in the dorsal region of the spine. First attendance was given him by Dr. ——— for "nervous shock." Subsequently Dr. ——— treated him by mercury to ptyalism for "diseased liver." In turn, Dr. ——— pronounced the case "unimportant—not worthy of attention." Dr. A., next in charge, after examination, declared the existence of "an anomalous tumor." On the 11th of November last Dr. A. called Dr. S., one of the former attendants of the patient, in consultation. Under the impression that the tumor, for months previous apparent, was but a common abscess, a small incision was made by Dr. A. through the integument, and a female catheter introduced. Not finding pus, a bistoury was introduced beside the catheter, and an incision three inches in length made. At the bottom of this was found a very firm clot. Scarcely a moment transpired before this was driven out, followed by a pulsating stream of blood. At this moment the truth flashed on the minds of the operators. Promptly grasping the sides of the wound and closing them firmly, they retained them in apposition by stitches, then applied a firm wooden compress and a bandage. They were thus enabled to control the hæmorrhage; the amount of blood lost being probably three quarts. Supposing the aneurism to be situated on one of the smaller arteries, external to the cavity of the chest, and probably the *subscapular*, it was proposed by Dr. S. to ligate the left sub-clavian artery. Time passed without an operation. On the morning of November 14th the patient died. The reporters of this case were present at the *post-mortem* examination, which revealed the following condition:

On opening the thorax some injection of the pleura of the left side was observed; likewise adhesions of different degrees of firmness, and considerable serous fluid in the cavity. An aneurism of the aorta, situated on the posterior wall of the descending portion of the arch, was found, which had by pressure caused the absorption of the left side of the bodies of the third, fourth, fifth, and sixth dorsal vertebræ. About four inches of the fourth rib were also entirely destroyed, with partial destruction of the third and fifth ribs of the same side. There were adhesions of the sac and pleura at the point where the bony walls of the chest had been partially destroyed.

It can easily be seen by this that should the sac by any means open in its posterior portion, where it was already protruding through the opening in the ribs, there might still be no hæmorrhage into the pleural cavity. Such was really the case. The sac had opened posteriorly, and the blood had been poured out under the muscles of the back, and between them and the pleura costalis, forming in this manner a new tumor much larger than the original sac. It was this new tumor formed by the bursting of the aneurism, and not the true sac of the aneurism, which was incised. Both the original sac and the new cavity were, to a large extent, filled with white fibrinous clots, deposed in firm, strong layers, such as are always noted in long-standing aneurisms.

The locality, disposition, and effect of the aneurism described will undoubtedly enlist the attention of the profession; but, in addition, it is worthy of note, that notwithstanding the extent and duration of this remarkable lesion, its aneurismal character was not suspected by the medical men in charge!

4.—*Extravasation of Urine from Bursting of the Urethra behind a Stricture; Perineal Section; Recovery.* By Henry Gray Croly, F. R. C. S. I., etc. [Medical Press and Circular, May 27, 1868.]

Visited, in consultation with his medical attendant, Mr. ———, who had been suffering for years from stricture of the urethra, for the relief of which instruments had been introduced from time to time.

About a week before my visit he felt a swelling in the perinæum, for which, however, he did not seek advice. He passed water (as usual with difficulty) until the night before I saw him, when he felt an unusual and painful sensation in the perinæum, "as if something had given way," and soon afterward he found the scrotum becoming much enlarged. This swelling increased rapidly, and he passed a restless night. Being alarmed, he sent for his medical attendant in the morning, who recognized the serious nature of the case, and recommended additional advice.

On examination, I found him in the following condition: Scrotum enormously distended, and of a shining-red color, a fluctuating tumor, the size of a hen's egg, occupying the median line of the perinæum. He said that he passed some urine with great pain and difficulty during the night, after the sensation of "something having given way." His pulse was 136 in the minute, his tongue was furred, and other symptoms of constitutional disturbance were present. The room had a strong smell of ammoniacal urine. The urgent necessity for immediate operation was explained to the patient, to which he readily consented.

Having placed him on a table in the lithotomy position, and the pelvis being raised on a pillow, I endeavored to introduce a catheter into the bladder, but found a stricture (near the bulb) through which the instrument would not pass. I then introduced "Syme's staff" down to the stricture, and with a long straight bistoury made a deep and free incision in the median line of the perinæum, over the tumor. A large quantity of pus escaped. I next introduced the forefinger of my left hand deeply into the wound, and felt for the staff, and, having reached it, I got the knife into the groove, and opened the urethra freely from behind forward. Scarcely any blood flowed from the incision.

I then made three long and deep incisions into the scrotum, which was enormously infiltrated; a large quantity of fluid, smelling strongly of urine, escaped. The edges of the incisions bled freely; pledgets of lint steeped in oil of turpentine were applied, and a T bandage. The scrotum was

kept elevated. The patient was placed in bed with a hot jar to his feet; wine was given, and a full opiate administered. On the following day I ascertained that the patient had a rigor, and vomited soon after the operation; pulse 100 in the minute. He expressed great relief; slept well; urine passing freely through the perineal wound; scrotum much reduced in size. The pledgets of lint were removed, and the incisions were washed thoroughly, and subsequently dressed with carbolic-acid lotion. Half a grain of opium and a grain of quinine to be given every hour; also brandy and eggs, with strong beef-tea.

On the third day I observed a superficial redness in the left iliac region, along the track of the spermatic cord, tender on pressure, and at once made a free incision into the part. This relieved the tension, and prevented further mischief in that region.

The bowels not having been freed for several days, a turpentine enema was administered with "O'Beirne's tube." Urine continued to pass freely through the wound in the perinæum, and in small quantities through the urethra. Tincture of perchloride of iron in brandy and water was given every fourth hour. The scrotum was poulticed with linseed meal and solution of carbolic acid; a catheter was passed regularly to dilate the urethra. The carbolic-acid dressing proved very valuable, and tended materially to promote healthy granulation. The wounds healed rapidly, and the patient made a satisfactory recovery.

From the history of the foregoing instructive case there can be no doubt that the urethra, which was dilated behind the stricture, communicated with an abscess by ulceration, causing the infiltration of urine, and the dangerous symptoms which resulted. The treatment in such cases must be decided and prompt—the incisions must be free and deep, and the urethra must be opened behind the stricture.

5.—*Strangulation of the Large Intestine near the Junction of the Descending Colon and Sigmoid Flexure; Successful Treatment by an Operation.* By R. L. Wilder, M. D. [Boston Med. and Surg. Jour., July 23, 1866.]

March 30, 1868, was called to see Mr. G., aged 33, in consultation with Dr. J. B. D. Stillman. Found patient vomiting incessantly a thin, coffee-colored fluid. He had had no passage from the bowels for four days. There was some swelling of the abdomen, and considerable tympanites. Great pain on pressure; countenance pale and anxious; pulse 130, thready and irregular. There were no appearances of hernia. The patient referred his pain mostly to the *right* iliac fossa. This, with the suddenness of the attack, led us to believe that the trouble was intussusception at the ileo-cæcal valve.

Warm-water injections had been given every hour for the previous twenty-four hours, but had come away without apparently entering the colon. Insufflation was then proposed. Quite a large amount of air was pumped in, by means of a Davidson's syringe. We suffered this air to remain in for a time, in hopes that by distending the intestine gently but fully, we might be able to bring things to a normal position and condition.

The air gave the patient so much pain that in about an hour it was thought advisable to allow it to escape. On dilating the sphincter ani, no air escaped. The rectum and sigmoid flexure remained perfectly empty, while the whole extent of the colon remained distended. This turned our attention to the *real* cause of all these symptoms of obstruction, which we

decided must be a stricture of the large intestine, situated near the junction of the descending colon and the sigmoid flexure. It also seemed most probable that this obstruction was caused by a band of adhesion, suddenly inflamed; for the patient had had peritonitis about two years before, and had since, as he says, been troubled with "colic."

A long enema-tube, carefully passed up, met with a decided obstruction about eighteen inches from the anus. After several attempts to pass the tube into the colon, which were unsuccessful, the tube being stopped at the same place each time, we decided that his only hope of relief lay in an operation.

Later in the day, Dr. Calvin G. Page, of Boston, Mass., saw the patient with us, and, after a careful examination, agreed as to the diagnosis, and fully concurred with us in the opinion that an operation for the relief of the stricture afforded the patient his only chance of recovery. This statement being made to the patient and his friends, they consented to any thing we might propose to do. As it was late in the day, and as the symptoms were about the same as in the morning, we determined to postpone operative interference until the next day—in the mean time to give the patient the benefit of treatment by position.

March 31*st*.—The previous symptoms being more marked, and the abdomen having become more tympanitic and enlarged, the operation was immediately decided upon.

Operation, by Dr. Wilder, assisted by Drs. Stillman and Page. Patient etherized and placed upon his right side. Incision made about midway between the last rib and the crest of the ilium, commencing at the edge of the sacro-lumbar and long dorsal muscles, and extending horizontally toward the umbilicus about four inches. The muscles were then carefully divided on a director, in both directions, the entire length of the incision. A large amount of fat appeared and rolled up into the opening. This I carefully dissected through with my finger and the handle of my scalpel. The distended intestine now showed itself in the aperture. I carefully passed my hand into the cavity of the abdomen, through the incision, and at the same time introduced an olive-pointed œsophageal probang into the rectum *per anum*. Carefully following this, with my finger within the abdominal cavity, I passed the probang along till I arrived at the stricture, which was found to be at the junction of the colon and sigmoid flexure. The intestine below the stricture was loose and flaccid, while above there was great distention, feeling like an inflated bladder with a string tied around its neck. I felt what seemed to be a *fibro-membranous band* surrounding the intestine and constricting it. By pushing the point of the probang well up, and using my finger-nail, I succeeded in dividing the stricture. Immediately upon the division of the band, the probang slipped through into the colon; at the same time an immense amount of gas and fluid fæces escaped with great force from the anus. A long enema-tube was then introduced, which passed easily up the descending colon, through which more gas escaped. Drs. Stillman and Page also satisfied themselves, by an examination, that the stricture was divided, and that the tube passed freely up the descending colon. I now withdrew the tube. The flaps of the wound were brought together and the edges held by sutures. Strips of adhesive plaster and a roller were then applied.

Only one vessel was divided, and that was a small muscular branch, scarcely requiring a ligature. The hæmorrhage amounted to nothing. The depth of the dissection, and the care necessary, made the operation rather long and tedious. Ordered two ounces of brandy to be immediately given by the mouth.

The patient rallied well from the operation, and for a num

ber of days improved so that there was every expectation of a recovery. On the second day after the operation there was excessive tympanites, with delirium, and very rapid pulse and respiration. The abdominal cavity was punctured with a small trochar at two points, giving exit to large quantities of gas, and relieving at once the urgent symptoms. The wound made by the operation healed perfectly, the union being firm over the whole line of the incision, and the abdomen presenting a natural appearance. The consulting surgeon, Dr. Page, appends a note to Dr. Wilder's account of the case, stating that the patient died of pyæmia a few days after the conclusion of his (Dr. Wilder's) report. He remarks:

"The case above narrated by Dr. Wilder terminated fatally a few days after the conclusion of his report, death occurring from pyæmia.

"There are two points in the case that should go upon the record. The first is, that the patient was subject to occasional attacks of epilepsy, and had suffered some years previously from a fall through a scuttle, a distance of three stories, since which accident he had had frequent attacks of abdominal pain in the region of the liver. The *post-mortem* appearances in this region showed old peritoneal inflammation, with adhesions to the diaphragm, and a recent deposit of lymph and pus over a surface of several inches, but entirely confined to that region.

"The second point is, the great relief given by puncturing the peritoneum with the trocar and allowing the accumulated gases to escape. I examined the peritoneal points of puncture *post mortem*, and found no trace of inflammation. The intestines were not touched by the trocar.

"The intestine at the point of stricture showed an ecchymotic line an inch long by one-fourth inch wide, but was otherwise healthy. There was considerable pus found behind the peritoneum, between it and the line of incision, which had closed by first intention. It would, perhaps, have been better to have kept the most dependent part of the wound open, so that this pus could have escaped."

6.—*Ligature of the Common Carotid Artery.* By Dr. C. Pilz, of Breslau. [Archiv für Klinische Chirurgie, Bd. ix., 1868, and Brit. Med. Jour., June 27, 1868.]

The last number of Langenbeck's *Archiv* contains a long article from Dr. C. Pilz, of Breslau, on ligature of the common carotid. Included in this are statistical tables of 586 reported cases, which are arranged in the following manner: ligature for hæmorrhage, 220 cases; ligature for aneurism, 86 cases; ligature for tumors, 138 cases; ligature before and during the removal of tumors, 69 cases; ligature for nervous affections, 35 cases; ligature for Brasidor's operation, 38 cases. The total amount of cases is further increased to 600 by others, of which full details are not given. In 29 instances the common carotid was tied on both sides, in 257 on the right side, and in 194 on the left. The sex of the patient is not given in every case, but of 537 patients 403 were males and 134 females. The sympathetic nerve was in one instance included in the ligature. Affections of the nervous system followed the operation in 160 cases; hemiplegia occurred in 8 per cent.; and 76 per cent. of the patients so affected died. Of the 600 cases, 319 were cured after the operation, and 259 died;

the result in the remaining being unreported. The ligature in the majority of cases came away between the thirtieth and fortieth days after the operation. The nervous symptoms following deligation of the common carotid are attributed by Dr. Pilż to deficiency of arterial supply and to venous congestion, and also to nutritive changes brought about by the establishing of the collateral circulation. In cases of aneurism, Dr. Pilz advocates the application of digital and mechanical compression, and holds the opinion that deligation of the carotid should be performed only as a last resource when all other methods of treatment have failed.

7.—*Subcutaneous Injections in the Radical Cure of Varicose Veins.* [Medical Record, Sept. 15, 1868.]

Dr. Stephen Smith, in the *Medical Gazette*, recommends the subcutaneous injection of the persulphate of iron in this troublesome affection. The patient being in the erect position, from 5 to 15 drops of Squibb's preparation of the persulphate may be forced into the cavity of the vein by the use of the common subcutaneous syringe, the vein being pressed by the finger. In a few minutes the clot may be detected by the finger and the needle may be withdrawn. The patient should remain in bed several days and cold applications be made to the puncture. To prevent the possible escape of a clot into the general circulation, a compress and roller should be applied to the trunk of the vein on the cardiac side. The larger trunks are usually injected, and at several points of the same sitting. The clot at once perfectly occludes the vessel.

Miscellaneous and Scientific Notes.

The reception given by the physicians of Philadelphia to Professors Gross and Pancoast, on their return from Europe, was a truly notable event. It was not alone a spontaneous expression of the good-will and respect entertained for these distinguished gentlemen, but it was an indication of the high esteem in which our noble profession is held, not only by its own members, but by the educated and intelligent public. It was no mere hero-worshipping—no clannish ovation to an idolized chief—for all sects and professions were represented; distinctions of race and place were forgotten; and from all sides, in no spirit of intrusion, but with a truly catholic reverence for only the good and the true, came men to participate in the honors of the occasion. It was a generous tribute paid by education and refinement to honors well earned in the pursuit of a profession that is but too often imperfectly requited and unjustly estimated. By doing honor to these

her guests, Philadelphia has done honor to herself; and New York, we are glad to say, by being present through her worthy representatives and participating in the ceremonies of the day, shares in that honor.

On the Relative Claims of Bell and Magendie to the Merit of having discovered the Functions of the Roots of the Spinal Nerves. By Robert McDonnell, M. D., F. R. S. —Dr. McDonnell stated that he had been led to inquire carefully into this subject by the observations lately made by MM. Vulpian, Claude Bernard and others, who, contrary to the opinion generally received in this country and on the Continent, claimed the discovery for Magendie. The author felt assured that however gladly British physiologists would claim for a compatriot the honor of having made this discovery, they would prefer doing what truth and justice required. After carefully analyzing Bell's writings, he has come to the conclusion that, previously to 1822 (when Magendie made his experiments, and published the results), Bell's written works contained no evidence that he conceived the idea that the posterior nerve-roots were sensitive, and the anterior purely motor. He (Dr. McDonnell) assigned to Magendie and experimental physiology the merit of having discovered this fundamental fact in physiology. He had some difficulty in obtaining Bell's celebrated pamphlet of 1811, printed for circulation among the author's friends, and, having obtained it through the kindness of Professor Turner, of Edinburgh, he submitted it to the judgment of several competent persons, who were invited to give their opinions independently of each other. All agreed that in this pamphlet, upon which Bell and his supporters rest their claims to his priority, there was absolutely nothing which could, in fairness, be supposed to indicate a knowledge of the true functions of the nerve-roots.—*British Med. Journal.*

The October number of the *Psychological Journal* contains a very careful and well-elaborated article on this same question by Professor Austin Flint, Jr., M. D., of this city. Dr. Flint had access to the celebrated pamphlet of 1811, and, conducting his investigations entirely apart from Dr. McDonnell, has arrived at almost identical conclusions. He says, by way of summary:

Like many great discoveries, the idea, and the experiments by which it was carried out and elaborated, did not emanate from a single mind.

In 1809, Alexander Walker proposed for the first time the theory that the properties of motion and sensation in the mixed nerves were derived from the two roots by which they take their origin from the spinal cord. This idea was entirely theoretical; and sensation was assigned to the anterior root and motion to the posterior root.

In 1811, Charles Bell, who was the first to experiment on the spinal nerves in animals recently killed, ascertained by experiment that the posterior roots of the spinal nerves had little or no motor properties. He ascribed both motion and sensation to the anterior roots, and supposed that the posterior roots presided over what are now known as the vegetative or organic functions. He knew nothing about the sensibility of the posterior roots.

In 1822, F. Magendie, who was the first to experiment on the spinal nerves in living animals, ascertained by experiment that the anterior roots of the spinal nerves presided over movement and the posterior roots over sensation. He believed these to be the distinctive properties of these roots, but he thought at that time that the anterior roots might be slightly sensitive and the posterior roots might possess some motor properties.

From the experiments of Magendie dates all of our positive knowledge of the physiological properties of the two roots of the spinal nerves.

We may note here that Mr. Alexander Shaw, in a letter to the *British Medical Journal*, claims that Magendie was indebted to Mr. John Shaw (the brother-in-law of Bell) for his information on the subject of the functions of the roots of the spinal nerves. Mr. Shaw (J.) was in Paris in 1821, and in frequent communication with Magendie, to whom he furnished the various papers that had been prepared by Sir Charles Bell and himself. Mr. Alexander Shaw also asserts positively that Mr. John Shaw performed for Magendie experiments on the portio dura and fifth pair, to convince him that the nerves of motion were distinct from the nerves of sensation.

Prof. Joseph N. McDowell, of the Missouri Medical College, died at the age of 63 years, in St. Louis, September 25, 1868, of congestive chill. Dr. McDowell was the founder of the college, with which he was connected at the time of his death, and was prominently known as a practitioner and teacher.

Death from the Use of Arsenic by a Cancer-Curer.—The *London Lancet* reports the case of a lady suffering from a simple abscess of the breast, who was induced by the representations of her friends to employ a cancer-curer, one William Patterson, who was reported to have cured some one of cancer. Patterson pronounced the patient's disease to be cancer, and immediately proceeded to attack it heroically, applying first a blister, and then an ointment, which on subsequent analysis proved to consist of nearly one-half pure arsenic. The patient was immediately seized with headache, vomiting, excessive thirst, etc., and she died in ten days. Arsenic was found in the various tissues and organs of the body, and no trace of cancerous disease was detected on the closest examination. Patterson was held to trial; his only defence was that he had cured many cancers with this ointment, which had been laid on a little thicker than he had ordered.

Singularly enough, the Court, while admitting that the death of the patient had been caused by the arsenic, summed up in favor of Patterson, on the ground that the Court was not sitting for the protection of the rights and privileges of medical practitioners, and a man, though not licensed, was not to be punished for culpable homicide, unless the jury is satisfied that he acted culpably, and that a mere mistake did not imply culpability. The jury, however, took quite a different view of the case, and promptly found the prisoner guilty.

The characterizing of the conduct and ignorance of such a pretender, as the man Patterson evidently was, as a "mere mistake," is, in our estimation, the very sublimity of coolness, or shall we call it audacity?

Dr. E. W. Howard, of Akron, Ohio, reports, in the *Cincinnati Lancet and Observer*, the birth of a male child weighing nineteen and a half pounds. The mother was forty-four years of age, and had had ten children previously. On the same day the doctor had another case of confinement at full term, in which the child weighed only three pounds. The labor was as long and severe in the last case as in the first. A pretty good average for one day's work.

The Medical College recently organized at Detroit, Michigan, has commenced operations with the following corps of instructors:

Edward W. Jenks, M. D. (President of Faculty), Professor of Obstetrics and Diseases of Women and Children; Theodore A. McGraw, M. D. (Secretary of Faculty), Professor of Principles and Practice of Surgery and Clinical Surgery; George P. Andrews, M. D., Professor of Principles and Practice of Medicine and Microscopy; Samuel P. Duffield, Ph. D., M. D., Professor of Chemistry and Toxicology; C. B. Gilbert, M. D., Professor of Materia Medica and Therapeutics; William H. Lathrop, M. D., Professor of Physiology and General Pathology; James F. Noyes, M. D., Professor of Ophthalmology; N. W. Webber, M. D., Professor of General and Descriptive Anatomy; J. M. Bigelow, M. D., Professor of Medical Botany; P. P. Gilmartin, M. D., Adjunct Professor of Obstetrics and Lecturer on Medical Jurisprudence; H. O. Walker, M. D., Demonstrator of Anatomy.

The report that the Siamese Twins are going to Paris to be operated upon gives interest to the following case, which we take from the *Revue de Thérapeutique Médico-Chirurgicale* of August 1:

Dr. Boehm has successfully performed the operation of separation of twins that were adherent to each other by a fleshy band. The junction between the two children, females, who were very small, but in other respects well developed, commenced at the inferior extremity of the sternums, which were entirely distinct or separated, and, following over the xiphoid cartilage, terminated in a single or common umbilicus. This connecting band was soft to the touch, not unlike a thick cushion of cellular tissue; but there could be felt as it were some hard and knotty cords, which later on were found to be formed by the cartilaginous branchings which, starting from the two xiphoid appendages, joined together toward the middle of the band, in order to form a cord, by the side of which coursed the vessels of the umbilical cord. The operation commenced by dissecting up and isolating the vessels of the umbilical cord, which was single and had one envelope, a single sheath, closing in all the vessels. This sheath was divided by a bistoury, and the vessels were dissected up three or four inches from the umbilicus. In this way six arteries and one vein were dissected out on each side and were ligated separately. After this the operator made an incision upon the

band, parallel to the surfaces of the thorax, and, taking care always to keep in the median line, cut more deeply into the cellular tissue, divided the union of the cartilaginous appendages, and, passing between the two points of insertion of the umbilical vessels, arrived finally at the skin on the opposite side, where a single stroke of the knife finished the operation. Very little blood was lost. The two flaps had a length of five and a half centimetres, and were united at three points by suture. Union took place by the first intention; but one of the infants, which from birth had shown less strength and vitality than the other, died on the fifth day. The other is now living, at the age of five years, is in perfect health and well developed, except that there is a separation six centimetres in length in the linea alba below the xiphoid appendage.

According to the statistical researches of Fœrster, out of one hundred and fourteen similar and collected cases, this is only the second where a successful result has followed the operation of separation.

Death of Professor Schönbein.—The Atlantic cable reports that Christian Friedrich Schönbein, of Basle, died recently at Baden-Baden.

He was born at Würtemberg, October 18, 1799, and at an early age devoted himself to science; but, being far from rich, had to teach in order to get means to complete his studies. In London, which he visited in 1826, he became acquainted with Faraday; and in 1828 was appointed professor in the University of Basle, in Switzerland. He became famous in a few years for the boldness and originality of his generalizations; and, although always inferior to several contemporaries, as an experimenter, has, perhaps, never had a superior as a theorist.

In 1839 Schönbein made his great discovery of ozone, the form which oxygen assumes under severe electric discharges, and which gives to the air the peculiar odor which prevails after a stroke of lightning. This discovery gave the first impulse to those fruitful inquiries into the influence of different conditions of the atmosphere upon health, which have occupied the attention of M. Schönbein and other chemists for many years.

Twenty years later, in 1859, M. Schönbein discovered "antozone," another form of oxygen, which, however, is as yet known only in such compounds as the peroxides of sodium and potassium. These remarkable results are as prominent as almost any in modern chemistry.

In 1845 M. Schönbein invented gun-cotton; and for more

than a year there was a general belief that the whole military system of projectiles would be changed by it. But the explosive violence of the gun-cotton was found too great and too uncontrollable for this use; and it was employed chiefly for blasting.

But, among the singular properties of gun-cotton, it was found to be perfectly soluble in ether, and, after many experiments by chemists, this solution, to which the name of collodion had been given, was found to be the best material to be "sensitized" for photographic impressions. Mr. F. Scott Archer announced in the *Chemist* of March, 1851, his success in making iodized collodion for this purpose, and from that time the art of photography may be considered a success.

M. Schönbein was the author of several treatises on iron and its combinations with oxygen, on physical chemistry, on combustion, and on the results of his own discoveries. In private life he was universally esteemed.—*Medical and Surgical Reporter.*

The Medical Department of the University of Michigan numbers about sixty students less than last year. This loss, however, is not attributed to the homœopathic agitation—the chief result of which has been to take away from the Faculty two of their ablest teachers—Profs. Armor and Greene. Their chairs, though filled by able young men, are stripped of something of their ancient dignity and authority.

Prize.—The Massachusetts Medical Society offers a prize of Fifty Dollars for the Essay which shall best and in plain language describe an effective and ready method of ventilating sick-rooms—one that can be put into operation at once, at the moment needed, with the least difficulty and expense, in houses of ordinary construction. Essays to be sent before May 1, 1869, to the committee, care of D. Clapp and Son, the publishers of the *Boston Medical Journal.*

Nélaton a Senator.—Among the promotions of August 15th (a date habitually chosen by the Emperor of the French for distributing his favors) we notice the name of M. Nélaton as having been raised to the high dignity of senator. This is the only example of a medical man *in practice* being promoted to a seat in the Upper French House, whether Imperial Senate or House of Peers. Until now it had been foolishly considered that the dignity of a peer or a senator was scarcely compatible

with the nature and busy occupations of medical practice. Under Louis Philippe the peerage was offered to Dr. Double, the King's physician, on condition that he would renounce practising. Dr. Double's private circumstances were such that he might easily, if he would, have accepted the sacrifice in favor of the honor. But he refused through a feeling of pride, and a most laudable concern for the dignity of his profession. We are glad that the absurd prejudice has at length been destroyed, and that such an event has taken place in connection with the celebrated surgeon we have named.—*Lancet.*

A Woman's Estimate of Women.—The *Medical Times and Gazette* thinks that the naïveté displayed by the charming Miss Becker, in her address to the British Association, on the mental superiority of the female sex, must have been somewhat amusing to that dignified body. It says:

"This lady's propositions were so well-rounded and so categorically arranged that they must have overpowered many of our weaker brethren. Her utter disregard, however, of the necessity for urging something in support of these propositions was not a little characteristic of the lady debater, and the illustrations afford a happy example of the kind of science which is popular in the ranks of the *ci-devant* weaker branch of the human family. 'The superiority of sex was not always on the side of the male: witness bees,' said Miss Becker. This was most infelicitous. What is the domestic economy of the beehive? True, the males are not considerately treated, but then the really mentally superior and active members of the commonwealth are creatures we should be sorry to see Miss Becker selecting for her analogy—endowed with intelligence, but *devoid* of sex. The only female in the establishment leads a scandalous life of polyandry, is made a matron of as soon as she reaches maturity, is allowed to take no share in the affairs of the republic; and, finally, is kept hard at work perpetuating the species during the term of her natural life. Is this Miss Becker's notion of the female of the future?"

Aneurism on the Pacific Coast.—The last number of the *Pacific Medical and Surgical Journal* contains a report of an interesting discussion held at the San Francisco Medical Society, on the subject of aneurism. Dr. A. G. Soule presented a series of statistics, showing that in 34 months there had been in San Francisco 119 deaths from aneurism, a yearly average of 42:

"The average population of the city for that time being about 126,000, there would be one death from aneurism for each 3,000 inhabitants.

"For a period of nine years, from 1856 to 1864 inclusive, there were in New York City 243 deaths from aneurism, an annual rate of 27. Under the supposition that the average population of New York for the nine years was 720,000, there was one death from aneurism to every 27,000 inhabitants."

Thus the relative mortality from aneurism is nine times greater in San Francisco than in New York City.

As to the class of men affected with aneurism: "They have lived a rough, uncertain life, with no settled home or steady vocation; they have been tossed upon the sea of life and wrecked upon the shoals of disappointment. All were, necessarily in our young city, remote from their native land. Thirty-five were Europeans, and but ten natives of the United States. Nearly all had 'tried the mines,' worked hard, lived in a rough, exposed manner, imbibed freely of the ardent, returned disappointed to the city, taken lodgings in the rickety habitations of the city front, and worked by the hour or by the day at heavy, laborious, unsteady labor alongshore. Idle the most of the time, they indulged freely in drink, and, when at work, stimulants were used to brace them up in their heavy labor. Many complained of rheumatism; many did not; a large proportion had suffered from syphilis: autopsies were made in all the cases, and degeneration of the arterial coats, atheromatous, fatty, or calcareous, found."

Dr. Gibbons, Jr., presented some additional statistics, which are both interesting and valuable:

"In England for five years (1838–'42) there were 593 deaths from the disease, or an annual mortality of 1 for every 131,000 inhabitants. In the same country for four years beginning with 1860, there were 1,546 deaths, an average of 1 a year for every 52,000 people. In New York City for 21 months, ending September 30, 1867, 29 deaths are reported, or 1 yearly for about 44,000 people. In Brooklyn for 18 months in 1866–'7, there were 12 deaths—1 yearly for 37,000 inhabitants. Philadelphia reports 8 deaths in 1863, and but 4 in 1864—1 to 76,000 and 1 to 150,000 people, respectively. Boston reports 7 deaths for two years, 1864 and 1865, or one death to 65,000 people; while St. Louis, with a population of over 200,000, reports but one death from aneurism in 1865. No deaths from this disease occurred in Chicago in 1865, in Providence, R. I., in 1864–'65–'66, or in the State of Rhode Island in 1863. For

1858, San Francisco reported 6 deaths; for 1859, 10 deaths; for 1866, 35 deaths; for 1867, 35 deaths; for 6 months of 1868, 28 deaths. This would give a ratio to the population for the years respectively, of 1 in 12,000; 1 in 8,000; 1 in 3,500; 1 in 3,700; 1 in 2,400. The per cent. of death from aneurism to the total mortality is as follows: England, 1860–'4, 0.09; New York, 1866–'7, 0.06; Brooklyn, 1866–'7, 0.09; Philadelphia, 1863, 0.06—1864, 0.03; Boston, 1863, 0.06—1865, 0.09; St. Louis, 1865, 0.02; San Francisco, 1858, 0.52—1859, 0.70—1866, 1.39—1867, 1.40—1868 (6 months), 2.12."

Dr. Carl Vogt, of the University of Geneva, the pupil of Liebig and Agassiz, is to deliver a course of lectures in this country during this winter, on topics connected with Zoology and Anthropology. After his political connection with the Frankfort Parliament, of 1848, had forced him to resign his chair of Zoology, in the University of Giessen, he delivered at Neufchâtel, and subsequently published in book form, the Lectures on *Man, his Place in Creation, and in the History of the Earth*, which gave him a world-wide reputation, and a high rank among men of science in Europe.

From Glasgow is reported the death of Dr. William Mackenzie, the eminent oculist, at the age of 74. He was a fellow of the Royal College of Surgeons in England, and of many learned and scientific societies. Dr. Mackenzie was well known to the profession in this country as the author of two standard treatises, "On the Physiology of Vision," and "The Diseases of the Eye."

The death of Prof. Griesinger, of Berlin, the celebrated alienist, is announced. No particulars are given.

In consideration of the numerous victims of homœopathic treatment, a decree of the Emperor of Russia prohibits the practice of homœopathy in the entire territories of the Russian empire, under pain of a fine of 500 rubles, and two years' transportation to Siberia.—*Le Courrier Médicale.*

This report, we observe, is denied in the homœopathic medical journals, on the authority of the Russian minister at Washington.

Appointments.—Dr. Chas. E. Buckingham, Adjunct Professor of Theory and Practice, in the Medical Department of Harvard College, has been transferred to the chair of Obstetrics and Medical Jurisprudence, made vacant by the resignation of Prof. D. Humphreys Storer.

Dr. Chas. L. Ives, of New Haven, Conn., has been appointed to the chair of Theory and Practice, in the Medical Department of Yale College. This vacancy was caused, it will be remembered, by the death of Dr. Worthington L. Hooker.

Dr. Henry M. Field, of Newton, Mass., has been appointed Assistant Lecturer on Materia Medica, in the Medical Department of Dartmouth College. Dr. C. P. Frost, of Brattleboro, Vermont, Assistant Lecturer on Theory and Practice, in the same Institution.

Dr. J. J. Chisholm has been appointed Professor of Military Surgery and Pathological Anatomy in the Medical Department of the University of Maryland, located at Baltimore. The chair was created expressly for Prof. Chisholm.

Prof. Paul F. Eve, late of the University of Nashville, has accepted the chair of Surgery in the Missouri Medical College, St. Louis, Mo., made vacant by the death of Prof. Joseph N. McDowell.

Prof. Joseph Jones, also recently of the Nashville University, has accepted the chair of Chemistry in the Medical Department of the University of Louisiana, at New Orleans.

Pennsylvania Hospital.—J. A. Meigs, M. D., Professor of the Institutes of Medicine in the Jefferson Medical College, has been elected one of the attending physicians to the Pennsylvania Hospital, in the place of Dr. James J. Levick, resigned.

A Human Tripod.—This case will be read with interest in connection with the account of the remarkable monstrosity published in the October number of this journal. The case in question is reported by an anonymous correspondent of the *Lancet:*

Blanche Dumas was born at Ségry, of perfectly healthy parents, who had already had two healthy children, and nothing special was noticed during the pregnancy of the mother

which resulted in the birth of this monstrosity. The child is fairly grown for her age, and appears acute and sensible. When dressed, the most remarkable feature is the great width of the pelvis, and the fact that the left foot is clubbed, which, however, does not prevent the child walking with facility. On closer examination, it appears that there are two pelves fused in the median line, and that in connection with these there are *two pairs* of lower limbs. The right leg of the right pelvis is perfectly developed, but the left leg is quite rudimentary, being represented only by a nodule of fat. Both limbs of the left pelvis are fairly developed, and are both club-footed. It is the left of this pair that is used in progression, the right being twisted in front of the other, and not reaching the ground. The development of the genital organs is most remarkable. In the normal position on the left side there are female genitals, urethra, and anus complete. On the right side there are genitals and urethra, but no anus, there being merely a depression in the skin at the point. The child micturates through both urethras, and evacuates the bowels by the single anus. In addition, however, to these female genitals, there is at the junction of the pelves, and at the posterior part, a well-formed penis, and below this is a cicatrix, resulting, as the mother says, from some operation performed in Paris, by which a scrotum which existed at birth was removed. The penis was so sensitive that examination was not permitted; but, as at birth the child micturated by some orifice in connection with it which is now closed, I imagine that there must have been an hypospadiad opening which the operation has successfully closed. The child now wears a sort of bandage to support the part, and no urine passes.

The child has been exhibited in Belgium and France, and has been examined by various medical men of eminence. Professor Crocq, of the University of Brussels, regards the deformity as an instance of interposition of an abnormal pelvis between the bones of the natural pelvis. Professor Schwrann, of Liège, on the other hand, regards the deformity as an instance of double pelvis, classing it in the category of double monsters (*autosités non par parasites*) of M. Geoffroy Saint-Hilaire. This latter view appears to be borne out by the arrangement of the toes of the supernumerary leg, which shows it to be a right limb, and to belong, therefore, to the left pelvis.

I may remind the readers of *The Lancet* of the remarkable case of the Portuguese youth described at length in that journal of July 29, 1865, who exhibited the curious malformation of a third lower limb with double male genital organs well developed.

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Original Communications.

ART. I.—*A New Operation for Artificial Hip-Joint, in Bony Anchylosis.* By LEWIS A. SAYRE, M. D., Professor of Orthopedic Surgery in the Bellevue Hospital Medical College, Surgeon to Bellevue Hospital, etc.

PREFACE.

THE republication of my paper on "A New Operation for Artificial Hip-joint in Bony Anchylosis"—which was placed before the profession by the State Medical Society in 1863, and the addition to the same of the letters from the various gentlemen who saw the case referred to—has become necessary to vindicate scientific truth, and my own reputation, from the false statements made by Dr. Louis Bauer, in his recent work entitled "Orthopedic Surgery," published by Wm. Wood & Co., 61 Walker Street, New York, 1868.

In speaking of bony anchylosis, he says, on pages 324 and 325: "The true bony anchylosis of the hip-joint finds its relief in Rhea Barton's operation. I have never had occasion to perform it, and can therefore offer no suggestions drawn from

personal experience, but it would seem to me that the attempt at establishing an artificial joint at the line of division is unwarrantable for two reasons:

"1. An artificial joint could never give a sufficient support to the superstructure of the body.

"2. It inevitably protracts suppuration, with its impending danger of pyæmia.

"Sayre a few years ago performed this operation, as he alleged, with success; but his patient nevertheless died a few months after, from pyæmia.

"The specimen derived from the case did not sustain the assertion of that gentleman; no cartilaginous covering—synovial lining, or a new capsular ligament, having been formed."

As soon as I read this most extraordinary misstatement of an important scientific fact—which was calculated to mislead the professional mind, and prevent suffering humanity from receiving the benefit of an operation which I had proved was not only feasible, but perfectly successful, I immediately wrote Dr. Bauer the following note.

285 Fifth Avenue, *February* 17, 1868.

Dr. Louis Bauer,

Cor. Clinton and Warren Streets, Brooklyn—

Dear Sir: I received a few days since from Messrs. Wood (publishers), a copy of your "Orthopedic Surgery," for which please accept my sincere thanks.

I can hardly find language to express my surprise at your misstatement of the facts in relation to the cases of "artificial hip-joint," particularly the case of Miss Losee, as I thought you had seen the specimen taken from her, and therefore knew that there was a *perfect joint with a capsular ligament, cartilaginous* and *synovial covering*, and also a *bifurcated ligamentous junction between the upper end of the lower extremity and the new acetabulum.*

The specimen is before me now, has just been examined by two physiologists of acknowledged authority, and fully substantiates the above description.

It is due to me, it is due to science, that you should come and see it, in order to correct this error in another edition of your book, which I have no doubt will soon be called for.

You only mention one case of artificial hip-joint, and neglect to make any notice of Anderson's case, which is certainly doing the *subject* great injustice, to say nothing of the wrong attempted to be done to me.

Believing that your object is the development and statement of scientific truth, I send you another copy of these *two* cases in order to refresh your memory, and also a copy of my introductory lecture on Orthopedic Surgery, where you will see that I have done you, as I always try to do to every one, full justice, and have given due credit for your valuable contributions to surgical science.

Trusting that you will see the importance of returning the same justice to science, and to me.

I remain, etc.,

LEWIS A. SAYRE.

This letter, and the two pamphlets, were sent to Dr. Bauer's address, as found in the *New York Medical Register*.

The pamphlets appear to have gone safely, as they were never returned, but the letter was returned to me a few weeks after, from the Post-Office Department at Washington. This letter is now before me, and the above is an exact copy of it.

Why this letter, bearing the doctor's proper address, never reached its destination, or why it was returned to me, I have never been able to ascertain.

I then called upon the Messrs. Wood, his publishers, in Walker Street, and, stating the facts to them, requested them to communicate them to Dr. Bauer, which they promised to do.

After waiting several weeks, and having no response to my very considerate and just demands, I wrote to the different gentlemen who had been present at the *post-mortem* examination of Miss Losee, and asked them to write to me their impressions of the case as they recollected it. Many of these gentlemen, as will be seen by their replies, had moved from the city, were in different sections of the country, and it was with some difficulty that I was able to find their different addresses. Those at a distance responded promptly, as will be seen by the dates of their replies, and this vindication would have been placed before the profession at a much earlier date; but Dr. Parker was absent from the city most of the summer, and wished to reëxamine the specimen before giving his opinion, which he did very thoroughly the day before he wrote me his letter, a copy of which is annexed.

The operation of Dr. Rhea Barton, to which Dr. Bauer refers, was not intended to produce an *artificial joint*, but

simply to *anchylose* the limb in an improved position, and is referred to, as will be seen in my report. How Dr. Bauer could confound two such totally different operations, I am at a loss to understand, unless it was sought as an opportunity to bring my operation before the medical profession in the light in which he has attempted to describe it.

If it were merely to defend my own personal reputation against slander and detraction, I would feel that I owed an apology to the profession, for thus coming before them; but as it is in defence of a great surgical principle, and of a new scientific fact, I feel that not only is no apology necessary, but that I would fail in my duty to science, and my profession, were I to keep silent. It is probably fortunate for science that this false charge has been made, as it enables me to add the evidence of so many distinguished gentlemen to verify the accuracy of my report; and thus verified, I give my humble contribution to the profession for their consideration and judgment.

CASE I.—*Anchylosis of both Hip-Joints—Tenotomy and Brisement forcé in one, and in the other Exsection of Semicircular Segment of Bone above Trochanter Minor. Recovery with Artificial Joint.*[1]

ROBERT ANDERSON, native of Lexington, Ky., age 26, was admitted into Bellevue Hospital in May, 1862, and gave the following history of himself: During the summer of 1849, when 14 years of age, he was accustomed to go in the river every evening to swim, and on one occasion remained in the water some hours, having previously taken very severe exercise in running and jumping.

About the middle of September he was taken with a dull pain in the right hip, which continued about

[1] The portion of this paper from this point to the Appendix, page 366, is reprinted from the Transactions of the New York Medical Society for 1863.

one week, so gradually and imperceptibly developed, that the exact date of commencement is not known. During this time he continued in attendance at school, and enjoyed the usual sports and games of his schoolmates. One day, after having exercised more freely than usual, he was attacked with fever, and the following day stupor set in, which lasted nearly three weeks, with the exception of intervals; when aroused by the family, was totally indifferent to any thing that transpired around him, except when thus diverted by his friends. All this time he suffered intense pain in the right hip, which was sharp and lancinating. The hip was red, hot, and greatly swollen, the swelling extending half-way to the knee.

At the end of a month the swelling had much subsided, and the pain very greatly diminished, though when the limb was moved it was still very intense, of the same character, felt in the hip, and never at the knee.

About this time began to have pain in hip-joint of left side, and also in the knee, which was dull, and never of that sharp, lancinating nature which he suffered in the other joint. This continued two months.

Ten days after the commencement of the disease, pillows were placed under his knees to relieve the pain. These were increased in thickness and continued all the time he remained in bed, which was six months; also during the next six months, whenever he was in bed: but during this latter period he sat up occasionally in a chair. From the position assumed during this prolonged confinement, the legs were flexed upon the thighs, and the thighs upon the pelvis, and have been immovably fixed in that position ever since. Had occasional pains all this time in both hips, but most severe in the right.

At the end of two years from date of attack, an abscess formed in the left groin, which remained and discharged pus for two years. Abscesses also formed about the right hip; one beneath the gluteal muscle, and another near the anus. These discharged very freely, and continued open for nearly a year and a half.

At the end of the first year, began to use crutches—compelled to use them ever since.

For the last six or eight years, general health has been perfectly good.

On admission he had anchylosis of both hips in the position seen in the annexed photographs :

Fig. 1.

Fig. 2.

The left thigh was immovably fixed at nearly a right angle with the pelvis, by bony cementation of true anchylosis. The right was very firmly attached at an angle not quite so acute, and by a very careful examination I thought some slight motion could be

detected which indicated that the attachments were fibrous in character, or at most were osteophytes only, and external to the joint, and that there was no agglutination between the femoral head and the acetabulum, whereas the opposite side seemed perfectly cemented together. He could not walk, except by whirling himself in semicircles, first on one leg as a pivot, and then the other—or else by swinging himself on his crutches from the axilla. In order to get both feet upon the ground at the same time, his back was curved inward very much at the sacro-lumbar junction, the left knee flexed at an angle of about 135 degrees with the thigh, and the right side of the pelvis was some inches higher than the left. He could only sit, by assuming a most awkward posture, half reclining on his side upon a couch or sofa; and in lying down, was curled up either on one side or the other, or if upon his back, he had to be supported by pillows under his knees, and under the lumbar vertebræ. In fact, he was the most pitiable object I ever saw, and one that would excite the sympathy of any surgeon.

On the 4th of May I divided subcutaneously the adductor muscles, the rectus, tensor vagina femoris, and femoral facia of the right hip, and, breaking up the adhesions by some considerable force, obtained a very good motion of the joint. Extension was made to the limb by a weight and pully, and the hip enveloped in cloths wet with cold water; no serious trouble followed the operation, and in six weeks he could flex and extend, ab- and adduct his right limb with considerable freedom.

On the 11th of June, 1862, I removed a semicircular segment of bone above the trochanter minor of the left femur, for the purpose of establishing a new

joint. Drs. I. P. Batchelder, Woodhull, and Osborne, of this city, Drs. Hooker of New Haven, Ct., Hichborne, of Mass., and Dr. James S. Green, of Elizabeth, N. J., were present at the operation.

As this is the first section of this kind ever made in this bone at the place indicated that I am aware of, I will give a brief description of the operation, and my reasons for performing it in the manner I did.

It is well known that Dr. Rhea Barton, of Philadelphia, first operated for a deformity of this kind by making a ∨ section in the shaft of the bone, and thus bringing the leg from that point down parallel with the other and obtaining anchylosis in an improved position. And the late Dr. J. Kearney Rodgers, of this city, repeated the operation in another case, only higher up in the shaft of the bone, with equally good results. But my object was to go above the trochanter minor, so as to retain the insertion of the psoas magnus, and iliacus internus muscles attached to the lower fragment, for the purpose of flexion, and by cutting out a *semicircular* piece thus ⌒ with its concavity downward, and then rounding off the upper end of the lower section I would more nearly imitate the natural joint, and give him a fair chance for motion at that point, with less danger of the parts slipping by each other when he walked, than there would be if I cut out a parallelogram, or a V-shaped piece.

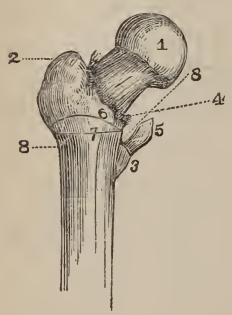

Fig. 3.

1, head of femur; 2, trochanter major; 3, trochanter minor; 4, line of insertion of capsular ligament (variable); 5, tendon of psoas mag. and iliacus internus muscle; 6, line of curved section; 7, line of transverse section; 8, 8, dotted lines indicating rounding off of lower fragment after removal of the segment.

The plan of the operation will be seen in figure, No. 3.

The description of the operation, and notes of the case, are taken from the hospital records, which were kept by Dr. Shaw, house surgeon at that time, and at present in the U. S. Navy:

An incision of about six inches was made over the trochanter major, in the axis of the limb. The cut was slightly lunate, with the concavity looking downward. The lips were then separated, and the deeper structures, including the periosteum, were detached from the bone.

A curved instrument, armed with the chain saw, was passed around the bone between the trochanters, and the femur first sawn transversely across. A roof-shaped piece was then sawn out of the upper fragment.[1] The limb was then put upon moderate traction, longitudinal and lateral; the margins of the wound approximated by adhesive straps, and cold dressings applied.

June 15*th*.—Wound begins to suppurate, and looks very well; no constitutional excitement.

16*th*.—He has considerable pain in the limb, and has been unable to sleep. Relieved by increase of extension.

20*th*.—Patient finds that pain is relieved sometimes by less extension.

July 4*th*.—He has less pain; purulent discharge, free.

Sept. 1*st*.—Since last report patient has experienced no untoward symptoms; discharge from the wound is now very slight. All extension is removed, and he begins to sit up. General condition very good, and has improved very much in flesh since admission.

Oct. 12*th*.—Since last report patient has been walking around the hospital on crutches, which had to be

[1] In my second operation, see page 352, I sawed the curved section first, and should advise the operation to be performed in that way, for reasons which are there given.

lengthened seven inches, as he is that much taller than he was before the operation, and is now quite straight, except the lateral curvature of the lower lumbar vertebræ, which raises one side of his pelvis more than the other, and makes the right leg apparently shorter than the one from which the segment of bone was removed; but this is easily rectified by a higher heel on that side. He can sit down in a chair, and get up without assistance, except such as he obtains from his crutches. To-day he walked into the amphitheatre by the aid of his crutches, and exhibited himself to the class, and left the institution well, and with very good motion at both hip-joints.

About three weeks after he left the hospital, he was attacked with acute pain in the region of the wound, which became inflamed, and soon suppurated. In a few days a small semicircular piece of bone came away, and, four days after, another similar piece; the two together making almost a ring, and seemed to be exfoliations from the lower fragment. All the pain immediately left him, and the wound healed in a very short time.

Mr. Anderson remained in the city until late in December, when he left, very unexpectedly, for Kentucky.

The night before he left he walked to my office, and could go up and down the steps without any difficulty; and could stand on either leg without either crutch or cane; could take a step with either foot twenty-seven inches, and, when he supported his body on his crutches, could abduct his legs so that his heels were thirty-six inches apart. He could cross either leg over the other below the knee, without assistance, but could not cross them upon the thigh.

The following extract is from a letter of his, dated the 20th January, 1863:

"My leg is getting on famously, since I came to Kentucky. The first day after leaving New York I grew very tired, but continued night and day until we arrived at Cincinnati. I believe that when I got to Cincinnati I was fresher than when I started. We were in the city about half a day, and then came on to Lexington, stayed all night, and again resumed our journey. So far from being exhausted at the end of the trip, I started next morning in a buggy, and drove some twenty miles. I think if I had been compelled to travel a thousand miles before stopping, I could *almost* have danced a jig at the termination of the trip. But to speak seriously, I think I am doing very well indeed, and my leg gains strength continually."

Fig. 4.

Fig. 4 is engraved from a *carte de visite*, which was received after the paper was sent to press, in a letter dated Spring Station, Woodford County, Kentucky, April 11, 1863, in which letter he states: "I can now 'rough it' a little, without apprehension of having to suffer for it afterward. I can bear my whole weight on my left leg without inconvenience, and can walk very well without other assistance than a walking-stick, and the improvement is as great in a month now, as at any previous time."

NOTE.—With the exception of Figures 1, 2, 5, 9, all the drawings were made by Gregory Doyle, medical student in my office, to whom I must express my obligations.

CASE II.—*Anchylosis of Left Hip, Section of Elliptical Segment of Femur above Trochanter Minor. Recovery, with False Joint and Good Motion.*

Miss Susan M. Losee, of Buffalo, New York, aged twenty-four, of healthy parents, and of a robust and vigorous constitution, was attacked with pneumonia, in March, 1856; attended by Dr. F. H. Hamilton. After three weeks went down-stairs, contrary to the advice of her physician, and the following day was attacked with intense pain in the left hip and thigh, which was constant, persistent, and most severe, for several months. She did not fall or receive any injury that she was aware of, but it was supposed that she must have wrenched her hip in some way going down-stairs, as she was very weak, and went down without any assistance. During the first few weeks her leg was straight, and could not be flexed, abducted, or adducted, without intense suffering. Bed-sores by this time had become so extensive as to make it imperative to change her position, and, in doing this, her limb was forcibly flexed at the knee and hip, but with the most intense pain; and, when flexed in this position, it could not be extended again without the greatest suffering, and was therefore permitted to remain in the flexed posture.

New sloughs appearing over the right trochanter, she was placed in a large chair, and was not removed for two months, when sloughing occurred over the tuber ischii, and at the extremity of the coccyx, and she was again compelled to assume the horizontal position; and, being forced to lie upon the right side, the left thigh was thrown over the right, in a flexed position, and thus became permanently and perfectly

anchylosed, at the expiration of about seven months from the commencement of the disease.

No local application was made to the hip, but the pain and constitutional difficulty was combated principally by morphine, and no extension was applied to prevent the muscular contraction and deformity.

Fig. 5.

Fig. 6.

When she recovered, her left thigh was permanently flexed, at about forty degrees with the pelvis, and strongly adducted across the lower third of the right thigh, as seen in the accompanying drawings, which were taken from life. Fig. 5 represents her standing. Fig. 6, in the act of walking.

In the erect posture, the heel of the left foot was ten and a half inches from the floor, and on the right side of the right leg. In attempting to walk, it was brought to the floor, still on the right side of the opposite limb, or cross-legged; and was made to reach the floor by a remarkable curvature forward of the lumbar portion of the spinal column; but walking was attended with great fatigue, and a peculiar dull pain in the lumbar region. Urination pro-

duced constant excoriation of the limbs, requiring great care and trouble in drawing a handkerchief or soft rag between the closely-compressed thighs, to keep them clean or comfortable. Several efforts were made to insert a catheter, in order that the urine might be led off without irritating her limbs; but it was impossible to insert the finger so as to reach the orifice of the urethra, either from the anterior or posterior position, although every effort was made, and with great perseverance.

She remained in this condition until the 6th of November, 1862, seven years. She came to New York and placed herself under the care of Dr. C. F. Taylor, in the fall of 1861, who thought the anchylosis was simply fibrous, and capable of being relieved by passive movements. Dr. Van Buren saw her at this time, and diagnosticated the case as one of true bony anchylosis. I saw her in April, 1862, in consultation with Drs. Taylor, Peaslee, and E. Lee Jones, and confirmed the diagnosis of Dr. Van Buren; but it was thought by all present that I might possibly break up the adhesions if I preceded the attempt by section of the tendons of the contracted muscles.

Accordingly, on the 10th of April, assisted by Drs. Peaslee, Taylor, and Jones, I divided, subcutaneously, the adductors longus and magnus, the gracilis and pectineus, the rectus, sartorius, and tensor vaginæ femoris, and immediately closed the wounds with adhesive plaster, and applied a firm roller. No hæmorrhage followed the operation. The pelvis was then firmly secured and every effort was made to give motion to the joint, that was consistent with safety or prudence, but without the slightest benefit whatever, and we were all satisfied that an entire section of the bone by

the saw was the only way that the limb could be moved from its flexed and fixed position. The patient was under the full influence of chloroform, administered by Dr. Jones, and was entirely insensible during the whole operation. The wounds healed kindly in a few days, without suppuration, and she was then in exactly the same condition as she was previous to the operation. As the weather was getting warm, I determined to leave her until fall, and then make a section of the bone above the trochanter minor, and give her a chance to form an artificial joint, similar to Anderson's case.

On the 6th Nov., 1862, assisted by Profs. Peaslee and Raphael, and in the presence of Dr. J. P. Batchelder and Mr. Done, medical student, I performed the following operation: The patient having been put under the full influence of chloroform, a longitudinal incision six inches in length was made over the trochanter major, commencing just above its crest, and as near as possible to its centre, and carried directly down to the bone. About the centre of the incision I made another at right angles to it, in the posterior flap, but only carried it through the tegumentary and adipose tissue and the femoral fascia. The blade of the knife was then laid aside and with its handle and an elevator something like an ordinary oyster-knife, I carefully peeled off the attachments from the bone, on its anterior surface, until my forefinger could reach the trochanter minor in front. The same thing was then done on the posterior surface of the bone, and the two fingers could then surround the bone, with the exception of a thin, firm fascia, between them on the front. This was readily pierced by a steel sound, curved to fit the femur, at this part, and a chain saw was then

drawn through above the trochanter minor, which could be distinctly felt and was my guide.

About half an inch above it I commenced to saw, and carrying it first *upward* and outward, then outward, and then *downward* and outward, I made a curved section with its concavity downward thus ⌒.[1] The saw was again passed around the bone, as at first, and inserted about an eighth of an inch below the first section, and the bone sawed square off, at right angles with the long diameter of the bone. The segment thus removed was one-eighth of an inch in front or internal margin, three-fourths at its middle, and nearly half an inch at its external margin, as seen in Fig. 7.

Fig. 7.

The bone was very dense in texture, almost eburnated, as seen in Fig. 8, which represents the lower section.

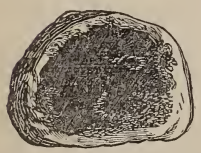

Fig. 8, View of lower surface.

There was not more than two ounces of blood lost in the operation, and no ligature was necessary.

The wound was brought together by two sutures and adhesive plaster, except the posterior incision, which was kept open by a tent of oakum. Adhesive

[1] It will be seen that in this case I reversed the order of the section of the bone from what I did in Anderson's case, and made the *curved* section *first*, and I should advise the operation to be performed in this way, as it is much easier, and you are more certain to make your saw enter at the part desired when the shaft is complete, besides having the limb to keep the parts steady while the section is made. And as it requires some little delicacy of manipulation to carry a chain saw in this position in the curve required, it is well not to add to the complication by having a movable bone.

It may be asked, why not make both sections curved? Because it is so difficult to do it with accuracy, when one end of the bone is movable, and, as the rounding off of the lower section is more simple and equally satisfactory, I prefer it.

plaster was applied below the knee, for the purpose of making extension, and a roller applied tolerably firmly, from the toes up, over the entire limb, and around the pelvis.

She was then put in bed, the foot of which was raised some twelve inches higher than the head, and a pulley applied, over which a weight was attached by a cord to the adhesive plaster, for extension, the same as in a case of fracture of the thigh. Lateral extension was also applied to the upper portion of the thigh, to keep the upper end of the femur from crowding against the femoral vessels, by means of a broad band passed around the thigh and a cord attached to its outer aspect, which played through a pulley fixed in an upright by the side of the bed, just below the pelvis, and a weight attached. By this means the limb was brought in its natural position, parallel with the other, and apparently of the same length. Ten drops of morphine were given, with instructions to repeat if necessary.

The following record of the case is an abstract from my note-book:

Nov. 17*th.*—Has had a very comfortable night; urinated without scalding her limbs, for the first time in seven years. No hæmorrhage, or much heat of limb; pulse 94; complains of pain in the back, otherwise perfectly well.

11 P. M.—Pain in the back very severe, just at the lower lumbar vertebræ, which is carried very much forward, and can only be relieved by being well bolstered up, and by raising the head and shoulders almost to the sitting posture.

18*th.*—Slept well all night, with only 10 drops of Magendie's solution; pulse 94, and only complains of her back, which requires to be pressed frequently and

quite firmly to make her comfortable; as it was difficult to use a bed-pan, and without it the urine soiled the bed and excoriated her person, I drew it by the catheter, which can now be inserted without the least difficulty.

19*th*.—Wound commencing to suppurate at the tent, the rest of the wound united by first intention; removed the sutures without disturbing the adhesive plaster; pulse 94; bowels moved naturally, and, with the exception of pain in the lower part of the back, feels well.

Dec. 1*st*.—No particular change since last report; suppuration healthy and not profuse. The only complaint she makes is from her back, and the difficulty she has in using the bed-pan. I put her to-day upon Dr. Nelson's fracture-bed, which is a triple inclined plane, with an opening for defecation, and it has made her very comfortable indeed—and the extension was accomplished by simply flexing the legs at the knee, over the inclined plane, as seen in Fig. 9.

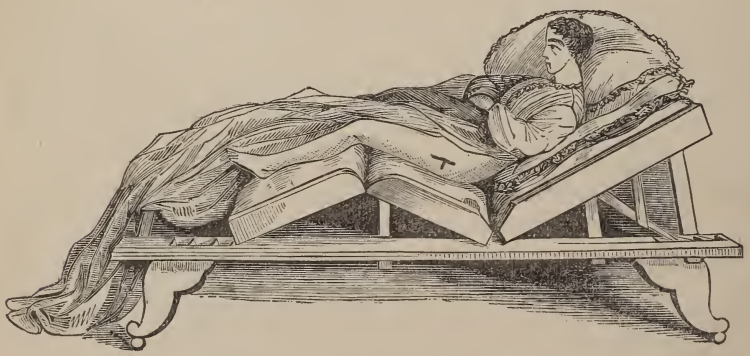

Fig. 9.

This fracture-bed was first constructed by Dr. Robert Nelson, of this city, formerly of Canada, and for convenience and comfort, as well as fulfilling all the

indications required, is the most perfect contrivance I have ever used, and I cannot speak too highly in its favor.[1]

From the time the patient was placed upon it until she entirely recovered, a period of nearly four months, she was perfectly comfortable—could be raised or depressed to any desired angle, as often as required, without inconvenience, which greatly added to her comfort, by the change of position. The wound healed entirely within four weeks, except a very small opening in the posterior cut, which was at the most dependent position, and from which a small discharge of pus escaped; this discharge gradually diminished and finally ceased about the 1st of March, four months after the operation. Two small pieces of bone escaped during this time, the size of a pin's head. For some weeks before its stoppage the discharge consisted of only a few drops in a day, of a very peculiarly whitish-yellow semi-fluid, of the consistency of thick starch water, and upon examination proved to be nearly pure albumen.

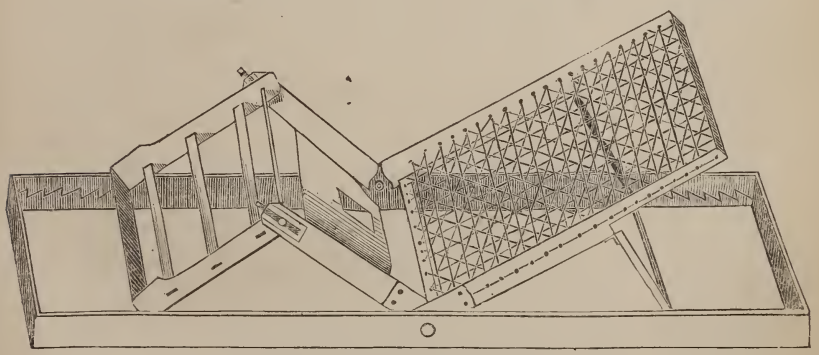

Fig 10.—Dr. Nelson's Fracture-bed.

[1] In Hesselbach's Handbuch der Chirurgischen, printed in Jena, 1845, will be found an almost exact duplicate of Nelson's bed on plate xxxix, with a description on page 1036, as having been constructed by Weckert; but as Dr. Nelson made his bed in 1820, we must give him the preference of priority.

After the first ten days from the operation I made slight movements of the limb very frequently, in order to prevent anchylosis, and this was also accomplished by the extension, which kept the severed bones from coming in contact with each other, and thus prevented osseous adhesion.

I gradually increased the extent of these motions, until, about the first of February, I could flex and extend, rotate, ad and abduct the limb with almost the freedom of a natural joint, and could also press the bones together with considerable force without pain.

On the 8th of February, 1863, she got out of bed for the first time—the limbs are perfectly symmetrical and parallel—the left nearly three-quarters of an inch shorter than the right, when her weight is put upon it; but when she stands erect upon the other limb, it falls down, and is nearly, if not quite, as long as its fellow. By pressing it up, you can shorten it a full half-inch, and by concussion it gives a smooth, cushioned feel to the hands, without any crepitus or pain to the patient.

Fig. 11.

July 20th.—She begins to have some control over the movements of her limb by voluntary muscular contraction, and can bear nearly her whole weight upon it as seen in Fig. 11.

The motions are nearly as perfect as those of the natural limb. From the perfect success attending the operation in these two cases of true anchylosis, and the freedom from all danger, as well as ease of its performance, I feel justified in recommending it to the profession as safe, and am satisfied that it will become established as one of the proper operations in surgery.

Sequel.

The patient progressed rapidly and favorably during several weeks, being able to bear her entire weight on the affected limb, with perfect freedom to passive motion, and gradual increase of control over the voluntary movements.

She was acquiring sufficient command over the limb to enable her, as the result of practice, to walk around her room, the exercise conducing to the improvement of her general health, as well as to the education and development of muscles which had long remained dormant; when, about the 1st of February, in opposition to my advice, she removed her flannels. She remained with them off for several days, and, on the 4th and 5th of February, being exposed for some hours to the intense cold then prevailing, she had a severe chill, followed by great difficulty in breathing, pain in the chest, cough, etc., arising from congestion of the lungs.

She neglected to send for me at once, and when she did, I was out of town, and she refused other medical attendance. She grew worse rapidly, and when I saw her upon my return, I at once recognized her condition as one of extreme danger, and requested the presence of Dr. Flint in consultation.

We found the left lung had become almost hepatized, and for some days no respiration could be detected on that side. Under treatment resolution gradually took place, with the exception of an abscess in the upper lobe of the left lung, which Dr. Flint thought was the result of an apoplectic effusion. Dr. Flint did not, at this time, diagnosticate tubercles, but did at a later period.

To the pneumonia was superadded, in a short time, pleurisy of the left side. The urgent symptoms of the pneumonia were subdued, but the cough, which was very distressing, continued. There was no expectoration at any time.

Under a sustaining plan of treatment, with spirits of turpentine locally over the hepatized lung, she improved, and I was encouraged in the hope that the abscess might become sacculated, and remain circumscribed.

The weather, up to about the middle of April, had been too inclement to allow her the advantages of passive out-door exercise, which, together with nourishment, was now considered the principal treatment required.

During all this time the cough had remained of the same racking, distressing character, and without expectoration.

On the 20th of April, she complained of some pain in the vicinity of the cicatrix of the wound left by the operation, and the lower part of the wound became inflamed and puffed out, although it had been closed several weeks.

On the 22d, an abscess having formed, the wound opened, and a small curved piece of bone escaped, about one-eighth of an inch long, and of the thickness of an ordinary probe, quite rough and jagged.

The wound discharged a little bloody pus for a few days, after which it gradually merged into the same kind of oily fluid as had exuded during some months subsequent to the operation.

This, in a few more days, began to diminish, and gradually the wound again closed, leaving no tenderness upon pressure, or from motion of the new joint.

She could again bear her whole weight upon the limb without inconvenience, and her command of its movements materially improved.

About the 1st of May she changed her residence, and for a number of days improved rapidly in strength and flesh, the principal annoyance being the cough.

On the 10th of May, having business out of town, I left the case in charge of Dr. Flint, who prescribed, for the cough, codeia, four grains, to simple syrup, four ounces, with directions to the nurse to give the patient a teaspoonful once in three hours while the patient remained awake, but to discontinue it while she slept.

During the night, as the result of larger and more frequently repeated doses of this mixture than had been ordered—which appeared from the admission of the nurse, and the small quantity left in the bottle—the patient had become thoroughly narcotized, and subsequently suffered for more than forty-eight hours, with most alarming symptoms of narcotic poisoning.

The utmost exertions on the part of Drs. Flint, Peaslee, and Wells, were required to sustain life, in consequence of the stomach rejecting stimulants, coffee, etc.

The cough had now entirely ceased, and never returned.

Great distress in the lungs was complained of, and partially relieved by counter-irritants. The stomach continued so weak as not to retain even a teaspoonful of iced water.

On the 12th she had recovered from the severe symptoms, when a relapse occurred from the administration of another dose of the codeia, in direct violation of orders that no more should be given, which it

seems were misunderstood by the nurse. During the night the patient was violently delirious, her screams arousing and disturbing the household until morning, when Dr. Wells administered, by inhalation, a small quantity of chloroform, which at once calmed the patient, and she slept for several hours.

I returned on the 13th, and found her still in a wild and distracted state of mind, and excessively prostrated, the stomach not having retained any thing for several days.

The process of nutrition was necessarily suspended, and the patient was dying in consequence.

The stomach had lost all tone as the result of protracted narcotizing induced carelessly, but with humane intent, and she was now sustained by enema.

On the 14th she had rallied, and become quite cheerful, but had no recollection of the terrible ordeal through which she had passed. Later in the day, while I was sitting by her bed, she suddenly had two severe convulsions, during which her lower limbs were flexed at a right angle, and strongly adducted, the left one requiring almost as much force to straighten as the right.

The nurse stated that the patient had had a similar fit during the preceding night, the limbs being fixed in the same manner for a long time, and that when the spasm passed off she voluntarily straightened her limbs.

On the 16th she sat up about an hour, and after getting back in bed, discovered that the wound had again opened and discharged a few drops of bloody serum.

She passed a remarkably good night, and on the following day felt so much better that she begged me to allow her to take a ride the next day.

I tried to persuade her that she was too weak, but she was quite importunate, and after I had left, in order to test her strength in view of the anticipated ride, she got out of bed, and sat up in a chair for two hours.

The exertion was too much, and she fainted.

I was hurriedly summoned, and found her cold and pulseless, except at the carotids. Pupils much dilated; jaws relaxed; respiration very feeble and slow; unable to swallow. Brandy was given in enema, but not retained.

She gradually recovered consciousness and ability to talk, which she did rationally, but grew weaker and weaker until about six P. M., on the 17th, when she died from exhaustion.

Post Mortem.—An examination of the body was made about thirty-six hours after death, in the presence of Profs. Bush, of Lexington, Ky.; Parker and Raphael, of New York; and Drs. Spencer, of Watertown; Batchelder, Dewees, Stone, Elsberg, Wells, Swift, Doyle, and Peck, of New York.

The body was extremely emaciated; the left leg being parallel with the right, the foot lying in the natural position, and was found to be half an inch shorter, and admitted of free, passive motion in all directions without crepitation. Upon opening the thorax, adhesions were noticed of various portions of the pleura and lungs, and a large abscess in the anterior portion of the upper lobe of the left lung. Two quite small abscesses were found in the lower lobe of the right lung, but neither of them communicated with the bronchi.

There was infiltration of deposit throughout the substance of the upper lobe of the left lung, which,

under the microscope, was determined by Dr. Dewees to be tuberculous.

Upon examination of the artificial joint, it was found to be provided with a complete capsular ligament, and the articulating surfaces were tipped with cartilage, and furnished with synovial membrane. (See Fig. 12.)

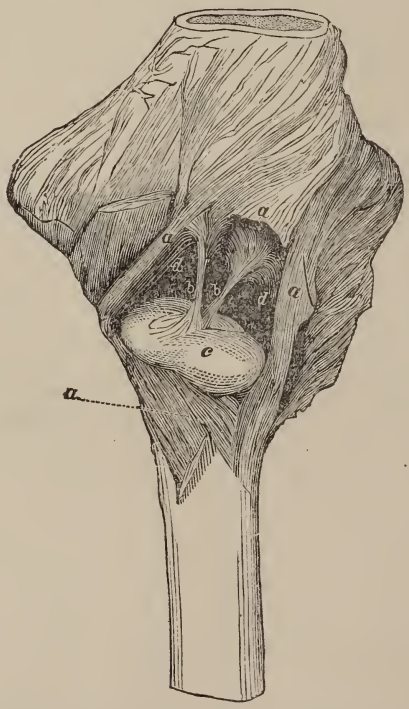

FIG. 12.—*a*, *a*, *a*, *a*, capsular ligament opened and reflected; *b*, *b*, round ligament in imitation of ligamentum teres; *c*, articulating head of lower section, covered with cartilage; *d*, *d*, new acetabulum, covered with cartilage; both lined with synovial membrane.

There was a very small spicula of bone, which had exfoliated from the lower section in the orifice of the external wound, and which would have escaped in a few days. Four other small fibrillæ of bone, about one-half inch in length, and the thickness of the lead

of an ordinary pencil, were found attached at one of their extremities, by periosteum, to the margin of the new head of the femur; their three extremities were thrust into the tissue around the joint. They were easily pulled off, having nearly exfoliated, and doubtless would have come away as the other pieces had done, had the patient lived.

All the other parts of the head and the new acetabulum were smooth, and covered with cartilage.

The conjunction of the articulating surfaces was perfected by the formation of two round ligaments spring-

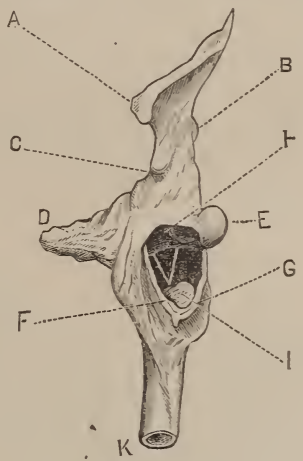

FIG. 13.[1]

A. Anterior superior spinous process of ilium.
B. Posterior superior spine.
C. Anterior inferior spine.
D. Pubis.
E. Trochanter major, completely fused by bony anchylosis with innominatum.
F and G. The capsular ligament—a piece having been removed to show the interior o the joint.
H. New acetabulum, formed in the trochanter major.
I. New caput femoris.
From H to I, a ligament (lig. teres), in shape of a V or Y, extends, as shown in the drawing.
K. Shaft of femur.

[1] This drawing, which represents the specimen in its present condition, with the ilium fixed in its proper position, has been prepared for me by Dr. L. M. Yale. The view is taken in the reverse position to the one represented on the opposite page.

ing from the surface of the new acetabulum, and by their convergence at the same point of attachment to the new caput femoris formed a new ligamentum teres.

These converging portions of the ligament were fan-shaped, and united at the sulcus of the new head of the femur.

A portion of the ilium, together with the cotyloid cavity, containing the anchylosed head of the femur, was removed, and, upon section through the original acetabulum and caput femoris, only a slight line of demarcation was discoverable, the whole joint being fused into one solid, bony mass. (See Fig. 14.)

Note.—As I have recently exsected a perfect artificial joint, in a case of ununited fracture, and which was examined by Prof. Austin Flint, Jr., and found to possess all the characteristics of the natural joint, and as I have not been able to find an examination of this kind recorded, I publish it in connection with this paper, as it illustrates the manner in which Nature effects a cure in these cases.

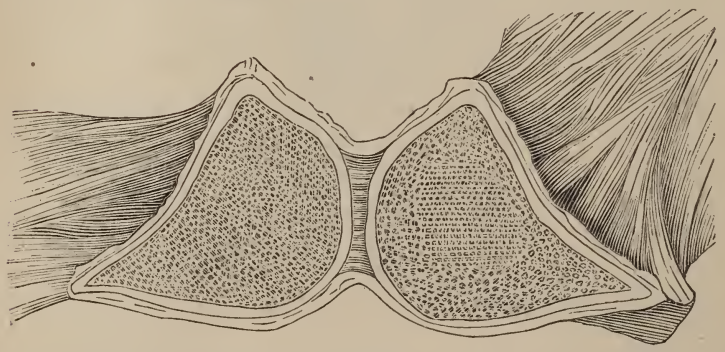

Fig. 14.

Exsection of Ununited Ulna—Perfect False Joint, with Capsule and Cartilage.

Mrs. G., aged 34, was admitted into the medical wards of Bellevue Hospital, in August, 1861, suffering from paralysis. In September she fell, whilst attempting to get out of bed, and fractured both bones of the forearm. She partially recovered from her paralysis, but the bones never united, and she was sent to the Island Hospital, and came under my charge in February, 1863, seventeen months after the accident.

She had so far recovered from the paralysis as to be able to walk tolerably well, and the upper extremities were as muscular and powerful as

ever, but the right arm was entirely useless on account of the ununited fracture about the junction of the middle and upper third of the forearm.

On the 19th of February, 1863, I cut down upon the bones at this point, intending to saw off the ends of the ununited portions and obtain reunion by a readjustment of the bones, but, when I came down to the point of fracture, I found it had been very oblique, and separated about a quarter of an inch, thus, and had become attached by a very firm ligamentous band, which admitted of quite a free gliding motion, without any crepitus whatever, and I decided to remove the entire portion, without severing the attachments of the artificial joints.

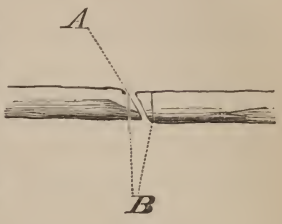

Fig. 15.

I therefore sawed out a portion half an inch in length, which embraced the artificial joint, and brought the bones fairly together and dressed with splints in the natural position, with the exception of the necessary shortening.

One of the sections passed through the new joint at *A*, thus giving an opportunity of examining its internal character and structure. *B* shows the

[1] Fig. 16.—Cartilage, Cavities and Cells.—A, cart. cavities without cells. B, cartilage, cavities and cells.

lines of the saw. The ligamentous attachment was more than one-eighth of an inch in length, very firm—tense, and of a silvery shining whiteness. On looking at the open section, it had the smooth and oily appearance of a natural joint, and the motions of the bones upon each other were perfectly gliding, without any friction or roughness.

Dr. Austin Flint, Jr., examined the specimen by the microscope, and reports that the lining is true cartilage, and it is therefore as perfect in all its physiological characters as any natural joint.

The annexed diagram (Fig. 16) shows the cartilage, cavities and cells, as taken by Dr. Flint under the microscope.

APPENDIX.

Letters received from gentlemen present at the autopsy:

Binghampton, New York, *April* 24, 1868.

Prof. Sayre—

Dear Sir: In perusing the work of Dr. Bauer, on Orthopedic Surgery, I was somewhat surprised to read there (page 325) as follows: "Sayre, a few years ago, performed this operation (artificial hip-joint), as he alleged, with success, although his patient died shortly after with pyæmia. The specimen derived from the case did not prove the assertion of that gentleman; no cartilaginous covering, synovial lining, or capsular ligament having been found."

This quotation refers to the case of Miss Losee.

As I frequently saw the patient, and took a particular interest in her case, I feel it my duty to disabuse the public of the false impressions which his statements are likely to produce. You can therefore, if you deem it proper, publish the following facts, to which I can clearly testify:

Miss Susan M. Losee, on whom you performed the operation for artificial hip-joint, was seen by me several times during the month previous to her decease. As far as the operation was concerned, it seemed in every way a complete success; but it was very evident to me that she was in the last stage of phthisis pulmonalis, in consequence of which her death took place on the 17th of May, 1863.

In company with several other medical men, I was present at the autopsy, which revealed important facts, which go strongly to sustain not only the feasibility, but also the justice, of the operation. On opening the thorax, the lungs were found to contain a large amount of tuberculous deposit, much of which had broken down, leaving several cavities. Our attention was next turned to the limb on which the operation had been performed. It was found to possess the property of being moved, with ease, in any direction, without crepitation; the artificial joint was then dissected down to, and was found to be provided with a capsule, very much resembling the capsular ligament of the normal hip-joint, being complete, and lined with a synovial surface. On opening the capsule, to get an interior view of the joint, we found the articular surfaces covered with cartilage, and provided with a double ligament, which seemed to answer all the purposes of a veritable ligamentum teres. In order to leave no doubt as to the substance on the articular surfaces being true cartilage, a portion of it was examined under the microscope by an eminent physiologist of New York, and found to contain cartilage cells. The ligament was bifurcated, having a single origin in the head of the bone, and then separating, and finding an insertion at two different points in the new acetabulum.

The specimen was taken from the body, and I prepared it for preser-

vation. I also made drawings of it while fresh, and took it to the photographers, and had a picture taken from it, in order, as you remarked at the time, that there might be no room for any one to think that the drawings were incorrect. Engravings made from the photographs were shortly after published in the *Transactions of the Medical Society of the State of New York.*

Now, the conclusion I draw from the case in question is this: if the operation succeeded so well in a tuberculous subject, how much better and more practicable would it have been in a perfectly healthy person!

Dr. Bauer makes large mistakes in his assertions as to there being no cartilage, synovial lining, etc. He knows, as every surgeon ought to know, that very often cases are met with where artificial joints are accidentally formed, as a consequence of non-union of fractures—the distal and proximal extremities being covered with true cartilage. Now, if Nature, under all the disadvantages of accidental contingencies, can form a new and nearly perfect joint, how much more effective would be her reproductive powers, if judiciously assisted by the skilful resources of art!

In conclusion, then, I feel justified in saying that the case of Miss Losee was a success, as far as the operation for artificial hip-joint was concerned; and it clearly illustrates the practicability of the operation, and affords a precedent for similar operations, which will yet be performed for the relief of suffering humanity.

I remain, as ever,

Yours truly,

GREGORY DOYLE.

Prof. Sayre, New York.

LEXINGTON, *April* 23, 1868.

MY DEAR DOCTOR: Yours of 14th April just received. I was present, with several professional gentlemen, Prof. Parker among the number, at the *post mortem* of your artificial hip case, which proved satisfactorily that the patient died of tubercular consumption.

The specimen derived from the case offered a beautiful illustration of artificial joint with cartilage, capsular, synovial, and ligamentous structures, produced by the operations of Nature, after surgical skill had prepared the parts. You may remember I pointed out the interarticular ligaments, one of which had been separated at one of its attachments, by the too free manipulations of the limb, by one of the gentlemen present. These interarticular ligaments were the most remarkable feature in the development of the joint; and you may not have forgotten my remark to you, upon the examination of the specimen, subsequently, at your office—"How wonderful and beautiful was Nature in this reproduction of even the ligamentum teres, in constructing the new hip-joint for your patient, imitating so well the anatomy of the normal articulation!"

Most truly your friend,

J. M. BUSH.

Elizabeth, N. J., *April* 29, 1868.

My dear Doctor: In a volume entitled "Orthopedic Surgery," by Dr. L. Bauer, at page 325, I am surprised to find the following sentences, which are so entirely at variance with my personal knowledge of the facts of the case referred to, and so unjust to you as an honest surgeon, that I desire to add my testimony to that of others, who saw the perfect success of the operation upon Miss Losee, while they in common with us regretted her untimely decease, not from *pyæmia* but from *phthisis pulmonalis.*

The sentences referred to are these: "But it would seem to me, that the attempt at establishing an artificial hip-joint, at the line of division, is unwarrantable for two reasons: 1. An artificial joint could never give a sufficient support to the superstructure of the body. 2. It inevitably protracts the suppuration, with its impending danger of pyæmia. Sayre a few years ago performed this operation, as he alleged, with success, but his patient, nevertheless, died in a few months after from pyæmia.

"The specimen derived from this case did not sustain the assertion of that gentleman; no cartilaginous covering, synovial lining, or a new capsular ligament having been found."

It was not my privilege to be present at the operation upon Miss Losee, but I saw her frequently afterward, and watched with great interest the progress and success of her treatment. It was in the spring following the operation. I was present when you placed her upon her feet; and she was able to bear the weight of her body upon the limb that had been operated upon. She was gaining gradual control over the muscles of the part, and the artificial joint yielded freely to passive motion without pain.

I was present at the *post mortem* of Miss Losee. Upon opening the thorax, the upper lobe of the left lung was discovered to be infiltrated with a tuberculous deposit, and a large abscess was found in the anterior part of the upper lobe of the lung of the same side.

Upon opening the artificial joint, the upper end of the femur was found to be rounded, and covered with a smooth white covering, which looked to me very much like cartilage covered by a synovial membrane; at any rate, it admirably answered that description; this was also true of the new acetabulum. The capsular ligament was there to all intents and purposes, formed out of the periosteum you had left, which had been thickened by Nature for its new office; and, more wonderful than all to me, she had furnished this new joint with a ligamentum teres, and a bifurcated one at that.

To the mind of an honest medical observer of Miss Losee's case, either during her life, or at her autopsy, there could be no question of the entire *warrantability* and success of the operation, of the fact that she did not die of *pyæmia*, as the result of the operation, and did die of phthisis pulmonalis.

But before I close this letter, I wish to say one word about the case of Robert Anderson (whom Dr. Bauer seems to be sadly ignorant of,

notwithstanding his case was reported in the same pamphlet as that of Miss Losee), upon whom you operated in June, 1862, for artificial hip-joint, and who still lives, moves, and walks, with practical agility. I was present at the operation. The left hip-joint was firmly anchylosed. Before the operation, his only mode of locomotion was by twisting himself from side to side, first on one foot and then upon the other. I saw him a number of times, at Bellevue Hospital after the operation, and on the night before he went home I happened to meet him at your office in Broadway, into which he walked unassisted. He could step some twenty inches with either leg, stand on either limb without artificial support, and abduct his limbs with ease while on his crutches.

Now, all this I saw, and therefore I say I am surprised to find such sentences as I have quoted from Dr. Bauer's work, since both these cases prove that the artificial joint *can* "give sufficient support," and that, as far as experience goes, pyæmia is not an imminent consequence of the operation for the artificial hip-joint, and also that Miss Losee's autopsy *did* "sustain the assertion of that gentleman," in regard to cartilage, synovial membrane, capsular ligament, etc.

Very truly yours,

JAMES S. GREEN.

WATERTOWN, *May* 21, 1868.

DR. SAYRE—

MY DEAR SIR: According to a statement made in Bauer's new work, you do not obtain much credit for the very valuable addition to the new and beautiful operation for anchylosis of the hip. You will remember my being present at the *post-mortem* examination, also at the meeting of the surgical section at Prof. Wood's; and, as I have paid some attention to cases of hip-joint disease, I took great interest in the case mentioned, and I think now as then, that the case was one of the most perfect cure. Both ends of the femur, from which the block was sawed, were perfectly covered with firm and solid cartilage, and Nature, in imitation of the ligamentum teres, had formed one from the lower portion upward about half an inch when it bifurcated, and each portion was attached to the upper portion of the femur. She had also formed a complete capsular ligament with a small fistulous opening, where she was attempting to throw out two or three small spicula of exfoliated bone, from the cavity of the newly-formed joint.

If Dr. Bauer could talk with Dr. Parker, who made a very thorough trial, by manipulation, before the joint was opened to obtain crepitus and failed, he would, I think, be convinced of the genuineness of the cure.

It was decided at the examination and at the evening meeting, that Miss Losee died of phthisis.

Yours truly, etc.,

H. GORDON P. SPENCER, M. D.

DAVENPORT, IOWA, *May*, 1868.

DEAR DOCTOR SAYRE: Your note of the present month in relation to the *post-mortem* examination of Miss Losee, at which I was present, and on whom you had operated for an artificial hip-joint, is received. I had the pleasure of seeing Miss Losee in company with you, when I was House Surgeon at Bellevue, several times, and as a consequence was much interested in the result of the case, knowing minutely the number of difficulties under which you labored, in operating upon and in the treatment of the unusually interesting patient. I remember that there were pleuritic adhesions in both sides of the chest, together with an abscess in the upper right lobe, and, what we know to be very uncommon, two small abscesses in the lower lobe of the left lung. There were also large quantities of tubercular deposit in both lungs. On making a careful examination of the seat of operation, we found an extremely interesting pathological condition, viz.: *a perfect artificial hip-joint, with perfect anatomical relations;* a capsular ligament, two ligamentum teres, both of which were attached to the articulating surfaces of bones. Each end of bone was, to my surprise, covered with synovial membrane and genuine cartilage, as was afterward corroborated by Dr. Flint, Jr. The *original hip-joint* was in a condition of perfect cementation, and as a joint obliterated. The entire specimen was removed, and if I mistake not you have it in your museum. Should I come to New York this summer I shall hope to enjoy the pleasure of giving it an examination. You remember how satisfactory the movements were during life, and painless.

Very truly yours,

W. F. PECK, M. D.

Lewis A. Sayre, M. D., 285 Fifth Avenue, New York.

153 WEST 15TH STREET, *New York*, *June* 3, 1868.

PROF. SAYRE,

DEAR SIR: In the latter part of the year 1862, you showed me the case of Susan Losee, upon whom you had performed your operation for artificial hip-joint; and in May, 1863, together with yourself and Drs. Batchelder, Bush, Dewees, Doyle, Parker, Raphael, Stone, Swift, and others, I was also present at the *post-mortem* examination. On this latter occasion, nothing abnormal could be detected in the position of the leg or foot of the affected side, on external inspection; though, on pushing the left foot toward the trunk, some shortening of the limb was perceived. On taking hold of it, it was found that all the various movements could be executed as well on the left as on the right side, and the new joint was perfect. Altogether, I can testify that the specimen is, in every respect, correctly figured and described on page 17 of your pamphlet (extracts from the *Transactions of the State Society*, 1863, p. 125), from the words "upon examination of the artificial joint" to "new head of the femur."

Very respectfully yours,

L. ELSBERG.

19 East Nineteenth Street, *July* 17, 1868.

Dear Doctor: I was present at the autopsy of Miss Susan M. Losee.

The description of the appearance of the artificial joint, which you gave in the published account of the case, is correct in every particular.

B. I. Raphael, M. D.

Prof. Lewis A. Sayre.

Bellevue Hospital Medical College,
April 29, 1868.

Prof. L. A. Sayre—

Dear Sir: In May, 1863, I received from you a specimen of a portion of the ilium, with the upper extremity of the femur, taken from a patient, upon whom you had operated, just below the great trochanter, for the purpose of making an artificial joint, the natural hip-joint being completely and irremediably anchylosed.

The patient's name was Susan M. Losee, and she died, as I heard, of tuberculosis, some time after the operation.

The specimen which I examined was the cut end of the femur, with a portion of the pelvic bones, forming a new joint. I found this end of the femur incrusted with true articular cartilage, and sent you, at the time, a report of the microscopical examination, with a drawing, showing the cartilage cavities and cells.

Yours very truly,

A. Flint, Jr.

Lewis A. Sayre, M. D.—

My dear Doctor: I was present at the *post-mortem* examination of Miss Losee in 1863. I can confirm your statement as regards the pathological findings in the parts, as well as the physiological separations—viz., the regeneration and replacement of synovial membrane, and of the necessary cartilaginous formations, as well as the formation of a capsular and an interarticular ligament—the latter of which had been reproduced in a double or bifurcated prolongation.

I remain,

Very truly,

H. P. Dewees.

New York, *October* 1, 1868.

40 East Thirtieth Street, *October* 16, 1868.

My dear Doctor: I recollect being present, at your request, at the autopsy of Miss Losee; that she died of phthisis; and that, in the limb on which you had previously operated for the establishment of a false joint, a new capsular ligament, interarticular cartilage, and ligamentum teres were found.

Very truly yours,

Foster Swift, M. D.

Dr. Sayre.

New York, *October* 15, 1868.

Dear Doctor: Agreeably to your wish, I, with pleasure, give from memory an account of what I saw at the *post-mortem* examination of Miss Losee.

The lungs were infiltrated with tubercles, which had caused her death.

The extremity of the femur, where it had been sawn, was covered with cartilage, but there was a very small portion, rough to the touch, which caused some discussion whether or not it was covered with cartilage. These rough points were not like ulcerated bone, and the little dimples, or depressions, between them, were satisfactorily covered with cartilage; and I believe that the whole sawn surface was, in the opinion of those present, also covered with the same.

One of the most remarkable and interesting facts was the growth of a ligament (ligamentum teres), which extended from the upper sawn surface to the lower.

Externally, to the bone, there were also appearances of a new capsular ligament.

The whole case, as to the operation and its results, was, to me, unique and extremely interesting, and will be of invaluable use in illustrating what may be done for such dreadful deformities, as well as showing, when an artificial joint is sought for by an operation, that cartilage will not be wanting to cover the bones, and that the necessary ligaments will be reformed to hold the joint together.

Very truly yours,

Dr. L. A. Sayre. John O. Stone.

New York, *September* 29, 1868.

Dear Doctor: In reply to your inquiry, I beg to state I was present at the examination of the body of Miss L——, in the spring of 1863.

I made a full examination of the limb operated upon, and the motion was *free* at the new joint. The parts were then laid open; the new joint consisted of a firm structure surrounding the point of operation, and made a capsular ligament. On opening this capsular ligament, the cavity was found to be lined by a synovial membrane, smooth and lubricated. Between the sawed surfaces of the bone an interarticular ligament was found. The case was of great interest, inasmuch as it verified views which we had under discussion.

Yours, etc.,

Willard Parker.

To Prof. L. A. Sayre.

The following letter is from the father of Miss Losee:

Jersey City, *May* 13, 1868.

Dr. Lewis A. Sayre, New York—

Dear Sir: I saw in a medical book published lately, the author's name I have forgotten, stating that my daughter, Susan M. Losee, died from the

effects of the surgical operation you performed on her, in November, 1862, for the purpose of forming an artificial joint near the hip-joint. The statement made by the author of that book is entirely unfounded and untrue; the operation was a perfect success.

The sore had healed up some time before her death. She was able to stand upon and raise that limb some weeks before her death, as you well know. She died of lung complaint, from the effects of a severe cold she took in February of 1863, by the carelessness of her nurse, which caused her death; not from the effects of the operation.

With great respect, I am,

Most truly yours,

S. Losee.

Art. II.—*Experiments undertaken for the Purpose of reconciling some of the Discordant Observations upon the Glycogenic Function of the Liver.* By Austin Flint, Jr., M. D., Professor of Physiology and Microscopy, in the Bellevue Hospital Medical College, New York, etc.

When it was announced by Bernard, in 1848, that he had discovered a new and important function of the liver, there being in this organ a constant production of the same variety of sugar that had long been recognized in the urine of diabetic patients, the great physiological and pathological importance of the discovery, attested, as it was, by experiments which seemed to be absolutely conclusive in their results, excited the most profound scientific interest. During the present century, indeed, there have been few physiological questions which have attracted so much attention; and the observations of Bernard were soon repeated, modified, and extended by experimentalists in different parts of the world. In 1857, Bernard discovered a sugar-forming material in the liver, analogous in its composition and properties to starch; and this seemed to complete the history of glycogenesis.

I do not propose at this time to give an extended review of the experiments which have been made in different parts of the world with the view either of confirming or overthrowing the theory advanced by Bernard; but will discuss the two opinions which are now most prevalent in English and French physiological literature. These two opinions are the following:

Those who accept the experiments of Bernard as conclusive assume that the substance of the liver and the blood in the hepatic veins always contain sugar. This sugar is believed to be formed in the so-called hepatic cells, from the glycogenic matter contained in them; and to be taken up by the blood as it passes through the liver, existing in the hepatic veins, the ascending vena cava, and the right side of the heart. It usually disappears from the blood in its passage through the lungs. Sugar is believed always to exist in the liver, the blood of the hepatic veins, and the right side of the heart, independently of the kind of food used. In the carnivora, the blood of the portal system never contains sugar when the animal is confined to a diet of nitrogenized and fatty matters; but sugar is found none the less invariably in the liver and the vascular system between this organ and the heart.

Others have accepted the view advanced by Dr. Pavy, of Guy's Hospital, who professes to have demonstrated that neither the liver nor the blood circulating between the liver and the heart ever contains sugar during life, but that the sugar which has been found in these situations is the result of a *post-mortem* change of the glycogenic matter, or, as it is called by Dr. Pavy, the amyloid matter of the liver.

These two opposite views are supported by experiments which seem to be conclusive; yet it is evident that, if the observations in both instances be entirely accurate, they must prove precisely the same fact. It was in the hope of harmonizing these discordant opinions, that I undertook some modifications of the experiments of Bernard and Pavy. I will not discuss the accuracy of the methods employed by these eminent observers, but intend merely to follow out a train of reasoning, which seems to me to be fully sustained by experiment, and which I believe will lead to a correct interpretation of the apparently opposite results heretofore attained.

Since the summer of 1858, I have been in the habit of repeating, several times each year, the experiments by which Bernard demonstrated the glycogenic function of the liver, performing the vivisections chiefly as class-demonstrations. I have followed most of the modifications of these experiments, which have been published by Bernard from time to time, and have almost always confirmed in every particular the results obtained by this eminent physiologist. I have never failed to demonstrate the absence of sugar in the blood of the portal system, when the specimens were taken with proper precautions from carnivorous animals that had taken neither starch nor sugar into the alimentary canal. I have found it important to apply a ligature rapidly to the portal vein as it penetrates the liver, and to make a very small opening into the abdominal cavity in this step of the experiment. When I have detected a trace of sugar in the clear extract from the portal blood of an animal in the condition just mentioned, it has been consequent upon delay in seizing the vein; and I have anticipated the probability of

finding sugar from blood, which, under these circumstances, regurgitates from the liver. The necessity of employing these precautions is fully insisted upon by Bernard. I have never failed to find sugar in the blood of the hepatic veins of healthy dogs that had taken neither starch nor sugar into the alimentary canal. In my earlier experiments, I never failed to find a great abundance of sugar in the substance of the liver, in dogs under the same conditions. In one instance, however, in the winter of 1859–'60, I failed to find sugar in the liver of a dog that was affected with what is known as "mange;" but considered this to be due to the peculiar condition of the animal.

On several occasions I have repeated Bernard's experiment of analyzing, for sugar, the portal blood, the substance of the liver, the hepatic blood, the blood from the right side of the heart, the substance of the lungs, the blood from the arterial system, and the substance of the muscles, the kidneys, and the spleen, all the specimens being taken from the same animal. I have always found that sugar existed only in the substance of the liver, the blood from the hepatic veins, and the right side of the heart, and in no other situations; showing, apparently, that sugar is constantly being produced by the liver, and is carried by the circulating blood to the lungs, there to be destroyed. Upon several occasions I have drawn the blood from the right side of the heart of a living animal, by catheterization through an opening into the right external jugular vein—a manipulation which presents no difficulty—and have never failed to find sugar. This experiment I have done without the administration of ether, following the operative procedure described by Bernard.

I have also frequently repeated the experiment of passing a stream of water through the liver from the portal vein, by which all the sugar can be removed in a short time, and testing the substance of the liver a few hours after, it having been kept in the mean time at a temperature of from 80° to 100° Fahrenheit. In this experiment I have always found an abundance of sugar. The glycogenic matter out of which this secondary formation of sugar is supposed to take place, I have extracted and studied after the method proposed by Bernard, and have confirmed his observations on this substance in every particular.

In these experiments I have used the various copper tests—viz., Trommer's, Barreswill's and Fehling's, and have made my clear extracts, generally, by boiling with an excess of sulphate of soda, but very often by mixing the blood or the watery extracts of the tissues with animal charcoal and filtering.

The theory advanced by Pavy, that sugar is not produced by the liver during life, and that, when this substance is found in the liver, it is the result of *post-mortem* change of the glycogenic matter (which he calls the amyloid substance), always seemed to me to be invalidated by the experiment of catheterization of the right side of the heart in a living animal, without the administration of ether; for, in the blood taken under these conditions, the presence of sugar is unmistakable. It being admitted that sugar is contained in the blood passing out of the liver, when ether has been administered, and the fact that sugar is sometimes produced in the body, in cases of diabetes mellitus (for there are undoubted cases in which sugar is discharged in the urine, when neither starch nor sugar has been taken as food), point to the probable nor-

mal production and destruction of this principle in the economy. Sugar can hardly be regarded as a heterologous substance, nor as a product of decomposition; and it constitutes an important article of food, from the fact that it is consumed in the body in connection with certain of the processes of nutrition. The hypothesis, that all the sugar which may be taken as food, and all that results of the digestion of amylaceous matters, is deposited in the liver in the form of amyloid matter, is inadmissible; and the pathology of diabetes cannot be satisfactorily explained by assuming that, under this condition, there is a deficiency in the formation of amyloid substance, by the liver, out of the starchy and saccharine alimentary principles.

Dr. Pavy, however, asserts that the liver never contains sugar during life, but that, after death, it is formed out of the amyloid substance, and its proportion goes on increasing for a number of hours, particularly when the organ is kept at about the temperature of the body. The experiments of Bernard with a liver washed out with a stream of water also show that sugar may be produced after death.

I was led to perform the following experiments, from the fact that, of late years, the experiments in which I have been in the habit of demonstrating the glycogenic function of the liver have inclined me to the opinion that the observations detailed by Dr. Pavy are entirely accurate, and that the error consists in his interpretation of the facts. The circumstances which lead to this view were as follows:

I formerly was in the habit of making my demonstrations of the formation of sugar in the liver upon animals that had been etherized; and then I always obtained a brilliant precipitate from a clear extract of

the substance of the liver, boiled with the test-liquid. I performed the experiment in this way before I had acquired sufficient dexterity to seize the portal vein readily, and to go through with the necessary manipulations with rapidity. I subsequently made the operation by first suddenly breaking up the medulla oblongata, then making a small incision into the abdominal cavity, and seizing the portal vein instantly, and following out the remaining steps of the experiment without delay. In this way, although I always found sugar in the blood of the hepatic veins, I frequently failed to obtain a distinct reaction in the extract of the liver; and the more accurately and rapidly the operation was performed, the more difficult was it to detect sugar in the hepatic substance.

It occurred to me, in reflecting upon these facts, that, inasmuch as no one has assumed that the actual quantity of sugar produced by the liver is very considerable, and as a large quantity of blood (in which the sugar is very soluble) is constantly passing through the organ, precisely as we pass water through its vessels to wash out the sugar, the sugar might be washed out by the blood as fast as it is formed; and really the liver might never contain sugar in its substance, as a physiological condition, although it is constantly engaged in its production. We know that the characteristic elements of the various secretions proper are produced in the substance of the glands, and are washed out at the proper time by liquid derived from the blood, which circulates in their substance during their functional activity in very much greater quantity than during the intervals of secretion. Now, the liver-sugar may be regarded as an element of secretion; and, possibly, it may be completely washed out

of the liver, as fast as it is formed, by the current of blood; the hepatic vein, in this regard, serving as an excretory duct.

To put this hypothesis to the test of experiment, it was necessary to obtain and analyze the liver in a condition as near as possible to that under which it exists in the living organism; and, in carrying out this idea, I made the following experiments:

Experiment I.—A medium-sized dog, full grown, in good condition, not in digestion, was held upon the operating-table by two assistants, and the abdomen was widely opened by a single sweep of the knife. A portion of the liver, weighing about two ounces, was then cut off and immediately cut into small pieces, which were allowed to fall into boiling water. The time from the first incision until the liver was in the boiling water was twenty-eight seconds. An excess of crystallized sulphate of soda was then added, and the mixture was boiled for about five minutes. It was then thrown upon a filter, and the clear fluid which passed through was tested for sugar by Trommer's test. The reaction was doubtful and presented no marked evidence of sugar.

Experiment II.—A medium-sized dog, in the same condition as the animal in the first experiment, was held upon the table and a portion of the liver excised as above described. The whole operation occupied twenty-two seconds. But ten seconds elapsed from the time the portion of the liver was cut off until it was in the boiling water. It was boiled for about fifteen minutes, made into a paste with animal charcoal, and thrown upon a filter. The clear fluid which passed through was tested for sugar by Trommer's test. There was no marked evidence of sugar.

Experiment III.—A large dog, full grown, and fed regularly every day, but not in digestion at the time of the experiment, was held firmly upon the table. This dog had been in the laboratory about a week, and was in a perfectly normal condition. The abdominal cavity was opened, and a piece of the liver cut off and thrown into boiling water, the time occupied in the process being ten seconds. Before the liver was cut up into the boiling water, the blood was rinsed off in cold water. The liver was boiled for about seventeen minutes, mixed with animal charcoal, and the whole thrown upon a filter.

Immediately after cutting off a portion of the liver and throwing it into boiling water, the medulla oblongata was broken up; a ligature was applied to the ascending vena cava just above the renal veins; the chest was opened, and a ligature applied to the vena cava just above the opening of the hepatic veins. A specimen of blood was then taken from the hepatic veins. This portion of the operation occupied not more than one minute. A little water was added to the blood, which was boiled briskly, mixed with animal charcoal, and thrown upon a filter. The liquid which passed through from both specimens was perfectly clear.

While the filtration was going on, Fehling's test liquid (a mixture of sulphate of copper, neutral tartrate of potash, and caustic soda) was made up, so as to be perfectly fresh.

The two liquids were then carefully tested for sugar with this preparation. The extract of the liver presented not the slightest trace of sugar. The extract from the blood of the hepatic veins presented a well-marked deposit of the oxide of copper, revealing unequivocally the presence of a small quantity of sugar.

In these experiments I did not attempt to show the absence of sugar in the blood of the portal system; for it would have been difficult, if not impossible, to have demonstrated this, and at the same time to have obtained the specimens of liver as rapidly as I desired. The fact, that the portal blood in a carnivorous animal, that has taken no saccharine or starchy matters into the alimentary canal, contains no sugar, I regarded as settled by the experiments of Bernard, which I have repeatedly confirmed. Neither did I attempt to show that sugar exists in the liver when a certain period has elapsed after death; for this fact has been demonstrated by all who have experimented on the subject. I only desired to ascertain whether the liver taken from a living animal, and the change of the glycogenic matter arrested before any sugar has had time to make its appearance—if its formation be *post mortem*—really contained sugar. A few seconds only elapsed before the liver was cut up into boiling water (which will effectually arrest the transformation of the glycogenic matter), and the presence of sugar in the decolorized extract could not be demonstrated. In Experiment III., particularly, very delicate tests were employed with the greatest care; and, although the extract of the liver contained no sugar, the presence of sugar in the blood coming from the liver was unmistakable. This experiment was peculiarly successful; and I could hardly expect to be able to collect the specimens with less delay. Anæsthetics were not employed in any of the experiments, and there seemed to be no circumstance which could interfere with the normal character of the specimens examined. The animals were perfectly quiet when the experiments were commenced, and were operated upon as soon as they were put upon the

table, the respiration and circulation being apparently normal.

CONCLUSIONS.

Although these experiments are not entirely new, my interpretation of them serves to harmonize, in my own mind at least, the results obtained by Bernard and by Pavy:

1. A substance exists in the healthy liver, which is capable of being converted into sugar; and inasmuch as this is formed into sugar during life, the sugar being washed away by the blood passing through the liver, it is perfectly proper to call it glycogenic, or sugar-forming, matter.

2. The liver has a glycogenic function, which consists in the constant formation of sugar out of the glycogenic matter, this sugar being carried away by the blood of the hepatic veins, which always contain a certain proportion of sugar, and subserving some purpose in the economy connected with nutrition, as yet imperfectly understood. This production of sugar takes place in the carnivora as well as in those animals that take sugar and starch as food; and is essentially independent of the kind of food taken.

3. During life, the liver contains only the glycogenic matter, and no sugar, because the great mass of blood which is constantly passing through this organ washes out the sugar as fast as it is formed; but after death, or when the circulation is interfered with, the transformation of glycogenic matter into sugar goes on; the sugar is not removed under these conditions, and can then be detected in the substance of the liver.

ART. III.—*Source of Free Hydrochloric Acid in the Gastric Juice.* By Prof. E. N. HORSFORD.

THE long-disputed position of Prout, that the gastric juice contains free hydrochloric acid, was at length established by C. Schmidt, who, in an absolute quantitative analysis of the juice, found about twice as much hydrochloric acid as was required to neutralize all the bases present.

The prolonged discussion of this subject, now since 1823, has brought to light, through the researches of Lassaigne, Claude Bernard, Schwann, and numerous others, the unmistakable evidence of the presence of lactic acid and of acid phosphates in the gastric juice, which latter might or might not be due to the presence of lactic or hydrochloric acid.

A point of special interest to the chemist and physiologist still remained, and was this:

How could free hydrochloric acid be secreted from the blood, which is an alkaline fluid?

The blood, freshly drawn, consists of a fluid (the plasma), in which there are swimming myriads of exceedingly minute, irregularly spheroidal bodies (the corpuscles). The plasma consists of two bodies—one of which, the fibrin, spontaneously separates from the other, the serum. The corpuscles are little sacs of delicate animal membrane, enclosing a fluid. This fluid has an acid reaction, and its ash contains a monobasic alkaline phosphate. The fibrin of the plasma contains, according to Virchow, a glycero-phosphate of lime, though the plasma, as a whole, has an alkaline reaction, and contains, in its ash, a great measure (eleven per cent.) of chloride of sodium.

The moist corpuscles constitute about one-half of the blood.

In healthy digestion, the blood-vessels of the stomach are engorged. Engorgement is the equivalent of obstruction. This must occur in the capillaries, where the diameter of the blood-vessels is least. The plasma, because of its fluidity, must move more freely than the corpuscles. The proportion of the corpuscles in the capillaries will be thereby relatively increased. Under the pressure that follows, the fluid contents of the corpuscles will pass through their membranous walls, and, mingling with the relatively lessened plasma, pass on through the walls of the capillaries. This mixture will therefore contain acid phosphates and chloride of sodium.

The mucous membrane of the stomach presents, on its inner surface, the mouths of numerous microscopic tubes, which, like stockings, are sometimes single blind sacs, or, like gloves, terminate in several blind sacs, like the glove-fingers. In the bottoms of these tubes, and along their sides, are several closed spherical sacs, containing other lesser sacs, and fluid within. The tubes, as a whole, dip down into the spongy tissue that underlies the mucous coat, where they are surrounded by the fluid, poured from the network of nutritive capillaries, which fluid, as remarked above, contains acid phosphates and chlorides.

Now, by pressure and osmosis, a portion of this fluid will pass through the walls of the gastric tubes, and the question is—

Whether the fluid that goes through will contain free hydrochloric acid?

The experiments I have made are conclusive on the principal point.

By employing acid phosphate of lime and common salt, I had this advantage, that as increased acidity on the one hand is a just inference from increased alkalinity on the other, and as increased alkalinity would be shown by the precipitation of phosphate of lime—a visible white powder—I could determine the qualitative fact without the difficulties and delay attending on accurate quantitative analysis of the solutions before and after the experiments, on both sides of the membrane.

I employed an acid phosphate of lime of specific gravity 1.117—of a constitution of 3 ($CaO\ PO_5$)+2 PO_5—with an amount of phosphate of peroxide of iron present, as one to twenty-eight of the acid phosphate of lime. The other solutions employed were the ordinary laboratory reagents.

On adding ammonia, in small quantities, to the solution of acid phosphate, with alternate agitation, it required, as might be inferred, several repetitions before the peroxide, with its phosphoric acid, became a permanent precipitate, and still several more before the precipitate of phosphate of lime became permanent.

In my earlier experiments, in which I employed parchment-paper, I was embarrassed with the presence of sulphate of lime in the precipitated powder, so that what was at first supposed to be phosphates of lime and iron, was found to be, in part, sulphate of lime. This sulphate was due to imperfectly-washed parchment-paper, which still contained sulphuric acid. This difficulty overcome, the experiments were made with parchment-paper, prepared from German and Swedish filter-paper, as well as with gold-beater's skin (animal membrane).

I employed the acid phosphate of the formula

above, with (each by itself) chloride of sodium, chloride of ammonium, chloride of potassium, chloride of magnesium, chloride of calcium and acetate of potassa.

With all of these, there was obtained the same kind of evidence of increased acidity on one side, and of increased alkalinity on the other—to wit, the powder thrown down from the mixture of acid phosphates and chloride. What successive additions of ammonia had been required to effect, had been accomplished by dialysis.

The same effect took place from a mixture of acid phosphate of soda and chloride of calcium.

It follows from the above, if these experiments fairly represent the case, and from the known composition of the blood, its condition in the walls of the stomach, and the structure of the gastric tubules, that free or uncombined hydrochloric acid must find its way into the bottoms of the gastric tubules, and thence into the cavity of the stomach.

It may be urged that I should show that the acid phosphate, pressed from the corpuscles, more than neutralizes the alkalinity of the plasma present. In reply, it may be said that I present a condition of things in which there is the *kind* of physical change required *going on*—namely, relative augmentation of the corpuscles, under pressure, the concomitant of engorgement.[1] Its *degree* must be inferred from the effects on the secretions, which I have endeavored to point out, by conducting an experiment under what I

[1] I employ the word "engorgement" simply as implying the condition of increased pressure in the capillaries. With constant elasticity of the walls of the capillaries, increased pressure would accompany increased flow of blood.

conceive to be essentially like conditions, and obtaining the result due to identical conditions.

The secretion of hydrochloric acid is, of course, mixed with acid phosphates and alkaline chlorides.

That such a result, as I have arrived at, would follow experiment, might have been predicted from Graham's researches in dialysis. Phosphates of lime and soda are colloidal, relatively, to more crystalloidal hydrochloric acid. Graham found that bisulphate of potassa, by dialysis, was resolved into two salts, or mixtures, of greater and lesser acidity than the original bisulphate. So he found that acetate of peroxide of iron was resolved by dialysis into hydrated peroxide of iron, and free acetic acid. It is possible, and probable, that the albuminoid bodies present take part in determining the contrast between colloid and crystalloid bodies. Graham found that, by dialysis, he could separate free hydrochloric acid from the gastric juice, thrown up by vomiting.

It may be further objected that anatomists are not agreed as to the structure of the corpuscles. But it will be seen that there is no more required than may be regarded as established. The corpuscles act in many particulars, if not in all, as if they were membranous sacs, more or less distended with fluid. They may be swollen by immersion in a thinner (less colloid) fluid, and reduced by immersion in a more colloid fluid; that is, they are susceptible of endosmosis and exosmosis, as membranous sacs would be. In their ordinary condition, as seen under the microscope, they present the appearance of collapsed spherical or oval sacs or cells. They appear as double concave disks. In swelling (by endosmosis) the lowest part of each concavity is the last to take on the spherical contour, just as they would

do if they were membranous sacs. The corpuscles sometimes so collapse (by exosmosis) that one-half of the hollow sphere is reversed while the other half retains its form unchanged, the former sitting like a cup in the latter—a conformation inconceivable on the theory of homogeneity of the corpuscles as a whole. Crystallizable substances may be extracted from the corpuscles by pressure and by endosmosis. They must have been in solution in order to crystallization, and solution involves a fluid.

The liquid expressed from the corpuscles has an acid reaction and contains an organic acid and acid phosphates. It contains among other bodies the hæmatoidin of Virchow. The ash of these crystals consists almost wholly of metaphosphates[1] which point directly to tribasic phosphoric acid in solution, combined with one atom of fixed base, which is inconceivable unless separated by membrane from the plasma, which is always alkaline.

In fine, whatever other peculiarities the blood-corpuscles may possess, they have the requisites for furnishing acid phosphates in solution, under pressure such as must attend engorgement of the capillaries in the walls of the stomach.

Let us glance at what takes place in all probability as the acid fluid enters the gastric tubules. Here are sacs containing fluid at the bottom and along the sides of the tubules. They are surrounded by a mixture of hydrochloric acid, acid salts, neutral salts, and albuminoid bodies. Dialysis must be repeated and a

[1] The ether extract of the blood-corpuscles yields, according to Schwann, an ash containing acid phosphate of soda. Owen Reese and Berzelius maintained the existence of oleo-phosphoric acid in the corpuscles. Andral (Berzelius's *Jahrs-Bericht*, 1847–'8, p. 894) places the contents of the corpuscles among the acid fluids of the body.

stronger acid solution pass into the sacs. The sacs, swelling by endosmosis and corroded by the acid, must at length burst, and the liquid contents, together with the disintegrated and partially digested membrane of the sacs, pass out to the stomach to constitute the gastric juice—the free hydrochloric acid, acid phosphates and chlorides, and the albuminoid bodies and disintegrated tissue (the pepsin?) to act in the liquefaction of food.

ART. IV.—*Sea-Sickness.* A Description of a New Method of Cure. By WM. H. DWINELLE, M. D., New York.

DURING a recent passage across the Atlantic in the St. Laurent, one of the French line of steamers, I had an opportunity of observing a new method of treating sea-sickness as practised by Dr. Le Coniat, surgeon of the Imperial French Navy, but temporarily surgeon of the St. Laurent. Something more than curiosity prompted my observations. I had a decided personal interest in the matter, not only for myself, but for an invalid sister who had hitherto been frightfully sea-sick in all her journeyings from port to port.

As we left Brest, on Saturday afternoon, Dr. Le Coniat requested me, in the event either I or any of my friends should be taken ill by sea-sickness, to send for him at once, as he felt confident that in a large majority of cases he could control the malady.

Strong head winds and a rolling sea soon developed sea-sickness in its worst form to many of our passengers, and none were more ill than my sister and myself. On Sunday evening her illness assumed an

alarming character; excessive vomiting and violent retching were succeeded by convulsions, followed by extreme prostration. In this extremity, Dr. Le Coniat was sent for, who, after a few minutes' manipulation, arrested every symptom of sea-sickness, and gave her entire relief. The disposition to vomit was completely arrested, nor did it assert itself again during the voyage, though the sea was as rough as before. She ate her meals without interruption, and with a relish, until our arrival in New York.

Although I was so ill that, with the exception of a single instance, I did not leave my berth from Saturday afternoon until Tuesday morning, during which time I had been unable to retain the least food in my stomach, I deferred resorting to the new remedy, thinking I would give time to Nature to come to my relief, should she be disposed to do so. Despairing of any such aid, I submitted to Dr. Le Coniat's treatment, the effect of which so fortified my stomach and removed all disposition to nausea, that I was enabled to eat my breakfast and retain it; nor did I vomit again during the voyage. The effect of the remedy in my case was not altogether complete and permanent, though I experienced great relief at the time. On submitting to the treatment a second time, I was entirely cured.

Dr. Le Coniat applied his remedy to many others during our passage, always producing great and immediate relief, and generally an entire cure. I recollect two instances where ladies had been confined to their berths for several days, unable either to eat or to raise their heads from the pillow. Immediately after the Doctor's treatment, they took their seats at the dining-table, and occupied them at every principal meal during the rest of the passage. Dr. Le Coniat's theory

is, that sea-sickness is induced by electric disturbance throughout the system, and that vomiting at these times is induced by an involuntary spasmodic contraction of the stomach from the pyloric to the cardiac orifice, thereby emptying that organ. In order to reverse this abnormal condition and restore the electric equilibrium, he places his patient in a horizontal position, uncovers the stomach, and applies to the skin, immediately over it, a solution of sulphate of atropine in the proportion of one grain to an ounce of water; he then places the negative pole of a galvanic battery, terminating in a flat disk, upon the stomach corresponding to the pyloric region. Then, with the positive pole terminating in a moist sponge, he manipulates across the surface of the skin from the cardiac to the pyloric orifice. These manipulations are kept up for three or four minutes, occasionally varying them by vertical passes downward. During the transit of the positive pole across the surface, the muscles can be seen to contract vigorously. The stimulus of galvanism rendered to the stomach by these means is much the same as that given to any other paralyzed or weakened muscle of the body—certainly the effect produced justifies the theory. It appears to be not only local in its influence, but pervading; the whole system seems to be brought under its control; its effects are soothing and refreshing, and generally accompanied with drowsiness, followed by refreshing sleep.

Dr. Le Coniat has been practising and improving his new remedy for about three years past; he has written one or two minor articles on the subject, which have been published in some of the French journals. On his return home, he proposes to publish a treatise on the subject for the benefit of science.

He claims to cure at least 90 per cent. of his patients suffering from vomiting and the pains of sea-sickness. He also claims that he is able, by the electrization of the stomach, with the *local* application of sulphate of atropine, to control the vomiting and sickness incident to the early period of pregnancy.

I am aware that electricity has heretofore been recommended for sea-sickness, but I think to Dr. Le Coniat alone is due the credit of perfecting a method by which practical and permanent results have been obtained.

The battery used by Dr. Le Coniat is one of the ordinary vibrating, carbon, and amalgamated zinc order, capable of double gradation. The solution for the battery is made as follows: Take ½ oz. of bichromate of potash, dissolve it in 9 ozs. of warm water; when cold, add ½ oz. of sulphuric acid.

All of the advice and remedies suggested in the excellent article of Dr. Barker on the subject of sea-sickness, in the November No. of the Journal, must ever remain in full force, founded as they are upon science and experience; no subsequent discovery can militate against them, though it may transcend the necessity of their entire observance.

ART. V.—*On the Microscope, as an Aid in the Diagnosis and Treatment of Sterility.* By J. MARION SIMS, M. D., New York.

(Read at the Meeting of the Medical Society of the County of New York, December 7, 1868.)

BY the kind invitation of your President, I have the honor of appearing before you, and of stating my views on the subject of sterility; a subject always in-

teresting, whether viewed in its bearings upon the happiness of individuals or the prosperity of states. It has engaged the attention of the profession for ages, but, till within the last twenty-five or thirty years, little or no progress was made in its treatment.

The first step in the right direction was taken by McIntosh, when he dilated the contracted cervical canal by bougies, and thus allowed the semen to pass to the cavity of the uterus. Sir James Y. Simpson followed out the same idea, when he subsequently incised the cervix to render its canal permanently larger. As the Edinburgh school has, then, established the fact that a dilatation of the cervix, whether by bougies or incision, is sometimes followed by conception, I claim to have established further facts in the same direction, which facts constitute the basis of the present paper "on the microscope in the diagnosis and treatment of the sterile condition." I have been accused of cutting open the cervix uteri recklessly and unnecessarily. True, I have laid down rules for the performance of this operation, under various circumstances; and I know that I have had some earnest and enthusiastic followers. If I have misled any of my brethren, it is my duty to hasten to rectify the error. So far as incision of the cervix uteri for dysmenorrhœa in the abstract is concerned, without reference to the sterile state, I wish it to be understood that I have nothing to recant, nothing to undo. But, so far as this operation may be indicated in cases of sterility, properly speaking, without regard to the relief of physical suffering, I candidly confess that I have a word of advice for my younger brethren; for I am now convinced that I have repeatedly cut open the cervix uteri, for the sterile state, when the operation was both useless and unnecessary;

and I am sure that almost every other surgeon, who has performed this operation often, has made the same mistake. How frequently have we all heard it said in consultation, "No operation is needed in this case, because the sound can be easily passed along the cervical canal!" And again, how often have we heard it said—how often have I said it myself—"An operation is necessary in this case, because the canal of the cervix is too small to permit the easy entrance of the semen!" Now, these important questions cannot be determined with any degree of accuracy in this hap-hazard manner. For it is not always necessary to incise the cervix uteri, simply because it does not easily admit the passage of an ordinary sound; nor, on the other hand, are we justified in condemning an operation, simply because the sound can be passed easily. In other words, a very small os does not always call for operation, nor does a larger one always forbid it. Do you not think, then, that a great service would be rendered, if we could reduce this question of operation, or no operation, from the broad domain of speculative opinion to the narrow path of absolute scientific certainty? There is nothing easier, for the microscope accomplishes this in the most perfect manner imaginable. It settles the question of operation, or no operation, in an instant, leaving nothing whatever to be guessed at, and nothing to be desired.

Is it surprising that positive knowledge of this sort should meet with opposition among honest, earnest cultivators of medicine? Not at all. For it is ever so with any great truth. It must first be opposed, then ridiculed, after a while accepted, and then comes the time to prove that it is not new, and that the credit of it belongs to some one else. The truth

here announced has had its day of opposition, and it must now soon take its stand as established and acknowledged.

On the subject of the microscopic examination of the utero-vaginal secretions, I have been misrepresented, maligned, and positively abused by a few both abroad and at home; and I have been misunderstood by many who have not taken the trouble to read, to investigate, to think, and to reason for themselves. And, Mr. President, under these circumstances, I cannot thank you too much for the high privilege of appearing here to explain and to defend my position by laying the facts in the case before this learned Society, this great gathering of my countrymen, whose decision, I am sure, will be in accordance with truth and justice.

We may all differ honestly about abstractions, and theories, and mere opinions; but, when it comes to facts and figures, there cannot long be a great difference among men of good common sense, with honesty of purpose in pursuit of truth. I have never yet been afraid of truth, however much it may conflict with prejudices, find it where I may; nor do I ever expect to see the day that I would fear to publish my convictions on any matter of professional importance, be the character of the opposition what it may; and, particularly, when I feel that these convictions are based upon facts that are immutable, and that lead to results of the gravest importance to the honor of medicine, and to the advancement of knowledge. Whatever gives to any department of medicine greater exactitude, helps to raise it to the dignity of a science. And this is what I claim to have done with the microscope in this direction.

The microscope has done, and is doing, a great work in medicine, as well as in the collateral sciences. But I know of no field in which it will be of more practical use than in the diagnosis and treatment of the sterile state. For, where every thing was a short time ago in doubt and confusion, all is now made clear by this wonderful instrument. Even in this day there are many very honest cultivators of medical science, who do not believe in the value of the teachings of the microscope.

The great Velpeau died, having no faith whatever in its practical utility. A few years ago,[1] I was one of those benighted scoffers who believed it to be merely a scientific toy, with which to while away leisure hours. Fortunately, my ignorance was dispelled, and I now look upon the microscope as essential to the daily duties of the physician.

With these prefatory remarks, I now beg leave to give you some illustrations of its use in the treatment of the sterile state.

In the investigation of any case of sterility, there are three questions that must be settled at the outset, if we expect to treat it understandingly:

[1] About eight years ago, my friend Dr. W. H. Dwinelle, of this city, a very accomplished amateur microscopist, was showing me some organic substances, under the microscope, in which I did not take any great interest. Indeed, I felt positively bored by the pertinacity with which he attempted to enlist me in his favorite study. When, at last, he said, "My dear doctor, I see you are very tired, but here is something that you must look at before you leave." He held the glass up, to show me there was nothing that my eye could detect, and then placed it under the microscope, adjusted the focus, and asked me to look. I am sure I was never more surprised in all my life, than I was then, to see, and read the Lord's Prayer. From that moment I was convinced that the instrument exhibited things just as they were. And I have often said, that I supposed this was not the first instance in which the Lord's Prayer was the means of opening a man's eyes to the truth.

1. We must be sure that we have semen with spermatozoa.

2. We must ascertain if the spermatozoa enter the utero-cervical canal.

3. We must determine whether the secretions of this canal are favorable or not to the vitality of the spermatozoa.

For, if the semen does not contain spermatozoa, of course the uterine condition does not call for any treatment whatever. But if it does contain spermatozoa, and if they do not enter the cervical canal, then there is the question of operation or not, to permit their entrance.

On the other hand, if we should find spermatozoa in the cervical canal, then, as a rule, no operation will be needed; and if we should find them there in abundance, and all alive, then the case needs no treatment whatever. But, if we should find them there, all, or nearly all, dead, then it is evident that the secretions of the utero-cervical canal poison them, and therefore the physical condition, giving rise to this abnormal secretion, must be searched out and treated.

The present advanced state of physiological knowledge warrants us in saying that conception is impossible without spermatozoa; and that it is impossible if the spermatozoa cannot pass into the cavity of the uterus; and, to these acknowledged truisms, I must add another, viz., that it is equally impossible if they die in the cervical canal, or are dead when they reach the uterine cavity. It is, then, self-evident that these three points must be determined—it matters not what other complications may exist. Fortunately, as I have said before, they are all easily and quickly settled by the microscope. Without the microscope, it is impossible

to determine either of them. Without the microscope, then, our treatment of the sterile state is simply blind empiricism. With it, our diagnosis becomes absolutely certain, and our treatment, at least, rational. What, then, are the first steps in this investigation? How can we begin? Where shall we begin? Now, as it is upon this very point that I have been so stigmatized, I will tell you exactly how I manage this delicate affair.

Given a case of sterility for investigation, the physician examines the state of the uterus and its appendages. His patient may have a frightful dysmenorrhœa, a flexed cervix, a contracted cervical canal, some malposition, a polypus, a fibroid, or something that would possibly prevent the passage of the semen to the cavity of the uterus. He may feel convinced, in his own mind, that conception cannot take place unless some surgical operation be performed—perhaps incision of the cervix uteri. This operation is usually done to permit the passage of the semen into the uterine cavity. But in this, or in any case, what right have we to say that the semen does not or cannot pass into the cavity of the uterus? We must not take it for granted that it does not, simply because the os seems to be small; for we know that cases are recorded where conception occurred when the os barely admitted a small-sized probe; and we know very well that spermatozoa now and then pass along the Fallopian tubes, which ordinarily admit a bristle. If the semen enter the cervical canal, we may lay it down as a rule, that a dilatation of the cervix by incision, or otherwise, is not necessary; but if it do not, it may be necessary. We may perform any rational operation for the relief of suffering, and for the restoration of

health; but I insist that we have no right to perform any operation, or to institute any treatment whatever, solely with the view of the cure of the sterile condition, till we have first settled the three propositions, above laid down, touching the presence and viability of the spermatozoa. To find out all at once, and with the least delay and trouble, I usually say to the husband or wife, as it may be, "It is very important, before instituting any treatment, to be sure that the seminal fluid enters the neck of the womb, for without this conception is impossible. We must also ascertain if the uterine secretions kill the semen; if so, a certain treatment will be necessary. If you will, then, send your wife here, or come with her any day, five or six hours after coition, it will be easy to settle these points at once." In nineteen cases out of twenty, the wife presents herself the next day. The speculum is introduced (and when I say the speculum, I always mean the one that bears my name), and some vaginal mucus is removed by the syringe, and placed on an object-glass. Then some cervical mucus is drawn out and placed on another object-glass. These two specimens are then examined under the microscope. If we find spermatozoa, well and good; but if we find none, neither in the vaginal, nor cervical mucus, our fears are at once aroused. What then is to be done? I simply say that I am not quite satisfied with the examination, and would like to see the wife again, at some future time, under the same circumstances. But, suppose we find no spermatozoa on this second examination? Then two questions immediately arise: either, that there are no spermatozoa, or that the semen has all passed off before the case came under observation. Sometimes the semen is all instantly

thrown off by the vagina, and then it would not do to pronounce the husband sterile till we are sure of a specimen of his semen, for investigation. If I fail to satisfy myself on this point, I then explain the possibility of the semen all passing off, in the act of rising and dressing, and show the absolute necessity of making the examination half an hour or so after coition, and before the erect posture is assumed. When the subject is presented in this plain, practical manner, and treated seriously, no man or woman of sense could oppose it; and with me, it has never, in a single instance, been objected to. When I am sent for to make the examination, if I find in the vagina a fluid with the characteristic seminal odor, I am satisfied with the microscopic examination. I have never, in but two instances, been compelled to resort to Mr. Curling's plan, of asking the man to squeeze a drop of mucus from the urethra, on to a bit of glass, immediately after sexual intercourse. But as this is sometimes necessary, it is well to remember it.

If we eventually find that the semen contains no spermatozoa, then all uterine treatment is at an end. But if we are at last satisfied that it contains spermatozoa, then we must determine if these enter the cervix uteri, and if so, do they there find a fluid favor able to their existence alive? And all this can be done only by the microscope.

The question of the entrance of the semen into the cervical canal, and of the effect of its secretion upon the spermatozoa, can be fully and satisfactorily ascertained only during a very brief period. We are sure to make a mistake if the microscopic examination be made just before the expected return of the menses; and why? Because, there is always a certain amount

of fulness of the uterus—of engorgement, so to speak —which precedes the menstrual flow; and the cervical canal may not admit the semen from mere turgescence of its walls. Besides, at this time, its secretions are almost sure to kill the spermatozoa even if they should happen to enter this canal.

Physiologists are generally agreed that conception takes place during the week following menstruation. Avrard says we have fourteen days of active uterine life and fourteen days of uterine hypnotism. He says that conception can occur at any time after menstrua tion up to the fourteenth day, counting from its commencement. For instance, if menstruation should last for three days, then we would have eleven days for the possibility of conception. But, if menstruation should last eight or nine or ten days, then we would have respectively but six or five or four days as the time possible for conception. After this time, the uterus, according to Avrard, lapses into the hypnotic state, when conception is impossible. While I am disposed to accept Avrard's dictum as the rule, I think I have seen exceptions to it, if we can always depend upon testimony seemingly reliable. Be this as it may, I am sure of this fact: if we wish to determine the effects of the cervical mucus upon the spermatozoa, we must make the experiment during the week that follows menstruation. About the fifth or sixth day after the flow is the best moment; for then the uterus is in the most favorable condition. The cervical mucus, which just before menstruation was perhaps thick and opaque, then becomes clear and translucent. If, by examination made at this particular period, we should find spermatozoa in the cervical mucus, we could safely say that it will not be necessary to incise the neck of the uterus. But

if the sperm do not enter the canal, then the probabilities are in favor of the necessity of some surgical interference. The semen may enter the cervix in great abundance, and we may find the spermatozoa all dead, even but a few minutes after coition. Then, as said before, we must find out the source of the poisonous secretion, and remedy it; for a vitiated secretion shows some organic condition requiring a special treatment.

When I wish to examine the action of the cervical mucus upon the spermatozoa, I order sexual intercourse in the morning—the dorsal decubitus to be retained for an hour afterward; and I expect a visit from my patient four or five or six hours after coition. Sometimes we find spermatozoa in great abundance in the cervical canal, and not one living. (I have occasionally examined the mucus, six, eight and ten minutes after coition, and found all the spermatozoa dead.) Sometimes, we find half of them dead; again, only about a third; again, two-thirds. Sometimes, in one portion of the mucus, every spermatozoon is dead; while, in another portion of the same sample with fewer epithelial scales, we may find them alive. Now and then, after treatment for a month or more, I have found the mucus drawn from the lower segment of the cervical canal full of living spermatozoa, and I have supposed that the case was cured; but when I came to examine that drawn from the upper segment of the canal, near the os internum, they were nearly all dead. This was evidently because the mucous membrane lining the lower segment of the cervix, being more easily reached and more thoroughly treated, had assumed a healthier character, and consequently its secretion was restored to a normal condition; while that higher up, and more difficult to reach, had not been so much improved, and

hence its secretion was still abnormal—a condition requiring further treatment.

The vaginal mucus, by its natural acidity, kills very quickly every spermatozoon. I do not now remember ever to have found one alive in the vagina, except when the examination was made very soon after coition; when, indeed, the vagina was full of semen but slightly mixed with other secretions. Examined three or four hours after intercourse, the spermatozoa found in that portion of the mucus of the vagina adhering to its walls are always all dead. Indeed, the normal vaginal secretion seems to be a perfect poison for the superabundant spermatozoa.

When, after a month's treatment, I wish to know whether the case is cured or not—in other words, whether all possible recognized barriers to conception are removed—I order sexual intercourse (just after menstruation) at night, and examine the cervical mucus twelve or fourteen hours afterward. If the majority of the spermatozoa be alive and active, I have great hope of conception. Before dismissing a case as cured, I think it necessary to examine the mucus thirty-six hours after coition; and, if it is then all right, of course I suspend the treatment, and patiently wait the hoped-for result.

So much care is necessary in the removal of the mucus for microscopic examination, that I may be pardoned for referring to it again. The patient is placed in the left lateral semi-prone position, as I have elsewhere so minutely described, and my speculum is introduced, and some of the vaginal mucus drawn up with a small glass syringe, previously washed out with warm water. This is deposited on the object-glass; the vagina is then cleared of all secretion, whether vaginal or cervical,

the whole of the vagina and the os uteri being thoroughly wiped over with a pledget of cotton. This is for the purpose of guarding against the possibility of mixing vaginal with cervical mucus, which would, of course, spoil the whole experiment. The cervix is then brought forward either by the depressor or a tenaculum, if necessary, which enables us to look directly into the cavity of the cervical canal. The syringe is then to be again thoroughly rinsed in warm water; its nozzle is passed into the gaping canal for half an inch, and the cervical mucus in its lower segment is drawn out. The instrument is emptied, washed out again with warm water, and reintroduced up to the os internum, and another portion of mucus is drawn out, provided there is any left after the first effort. Thus we have three specimens of mucus; i. e., one vaginal, and two cervical. The cervical secretion should be clear and translucent, and about the consistence of the white of egg. If it contain any little opaque specks of milky whiteness, it invariably poisons the spermatozoa to a greater or less extent. We sometimes find the cervical mucus perfectly clear, and yet poisonous to the spermatozoa. Here we would naturally expect to find excessive alkalinity of the secretion; but I have not been able to detect it. In these cases, it has seemed to me that the spermatozoa were killed—drowned, as it were—by the very abundance of the secretion. I do not here allude to cases of uterine catarrh, where the secretion is very thick and albumino-purulent; for, of course, this is a deadly poison to the living principle of the semen. But I allude wholly to such cases as have been changed by treatment to a condition giving rise to a secretion seemingly normal, so far as an ordinary ocular examination is concerned. Here the

microscope is our unerring guide. The mucus may be clear, and perfectly normal in appearance; but, if it kill the spermatozoa, then there is still some point in the canal of the cervix, or in the cavity of the uterus, that gives out a vitiated secretion; and this must be found out and corrected before the case is wholly cured. When we find living, active spermatozoa high up in the cervical canal thirty-six or forty hours after coition, we can pronounce the case cured, so far as it can be by surgical means, and not till then.

It is time for us to pause, and consider if there is not something more to be done for the sterile condition, than to split up the cervix uteri. I look upon this operation as one of great importance, as one of the most valuable in uterine surgery, but I think that we have followed too blindly the example and teachings of its illustrious author, Sir James Y. Simpson. For myself, I am now sure that I have cut open the cervix uteri, perhaps scores of times, when it was both useless and unnecessary; and I know that others have done the same thing. Do not misunderstand me. I speak here solely of the operation with reference to the sterile condition, when it would be wholly useless if the husband happened to be sterile, and certainly unjustifiable unless imperatively called for by considerations of health. Incision of the cervix for dysmenorrhœa is one thing; incision of the cervix for sterility, even if there be dysmenorrhœa, is another, and it behooves us to draw the line of distinction in every case, and not to take it for granted that every woman is sterile who may have dysmenorrhœa or feeble health, or that every man is prolific who may be vigorous and enjoy good health. I am sorry to say that I have had the misfortune to incise the cervix in half a dozen cases of sterility, where

I found afterward, to my great mortification, that the husbands were incapable of procreation, because their semen had no spermatozoa, and that, too, since I have known the value of the microscope. In each case the operation was called for to restore health, but was totally useless for the relief of its incidental accompaniment, sterility, and would not probably have been submitted to for considerations of health alone, had it not been for the hope of offspring afterward. I made the mistake of operating on these cases, because the social position, moral character, and appearance of health in the husband, conjoined with the excessive dysmenorrhœa and utter prostration of the wife, led me to operate without the preliminary step of ascertaining whether there were spermatozoa or not. I wish others to profit by my mistakes; and I am less ashamed to tell you of them, than I am to own them to myself. However, this can never happen to me again, and should not, after this warning, happen to any of my brethren. I know many men who have no spermatozoa, and cannot therefore become fathers. They are all strong, active men, in the prime of life, and all perform the sexual function with vigor. The very fact of their natural vigor and strong passions had been their ruin, for most of them had contracted urethritis during their early and unmarried life, and had suffered from its unlucky sequence, epididymitis. To further illustrate the necessity of the microscope in in this department of surgery, I shall append a few cases drawn up as succinctly as possible.

No. 1 had consulted two of the most eminent physicians in England, and remained under the care of one of them for many weeks. She said that during that time the neck of the uterus was repeatedly cauterized. She got impatient, and went to another physician, who told her that the caustic treatment she had submitted to was worse than useless; and

that a surgical operation was the only thing to be done. She consented to it, and he incised the cervix bilaterally. She did not conceive, and two years afterward went to Paris to see me. I found the uterus normal in all its relations, the os tincæ and cervical canal sufficiently patulous. I explained to both husband and wife the importance of examining the cervical mucus four or five hours after coition. They returned the next day; the cervical mucus contained spermatozoa; therefore there was no necessity for any further surgical operation. But the spermatozoa were all dead; therefore there was a necessity for a treatment to rectify the vitiated cervical secretion. She remained in Paris a few weeks under my care, was cured, and became a mother in a year after her dismissal. Now, if the first physician had used the microscope, as I direct, he would probably have found that the semen never entered the cervix at all; and, if the second one had done the same thing, he would certainly have found that the mucus of the cervix poisoned the spermatozoa.

No. 2, a lady, in the highest ranks of life, was sterile. The cervix uteri was incised bilaterally. She had pelvic cellulitis afterward. Two years after this I saw her, and she was still childless. The microscope showed that the cervical mucus, examined four hours after coition, killed all the spermatozoa. While this condition exists conception is impossible.

No. 3, sterile, was treated for sterility in America, for a long time (two or three years). She went to Europe; had the cervix cut open, and was sent away with the promise of offspring. I saw her some time afterward. The microscope proved that the husband was sterile. Therefore, the previous treatment at home and the operation abroad were useless. I could relate several other cases like the above. But, as I have often made the same mistake before I fully understood the value of the microscope, I forbear.

No. 4, married four years; sterile. She had dreadful dysmenorrhœa, followed by discharge of a bloody brownish mucus, of an offensive odor. The uterus was anteflexed; anterior wall hypertrophied; os uteri small. I was in doubt, at first, whether to recommend an incision of the cervix or not. I told the husband that an operation would be necessary if the semen did not enter the canal of the cervix; but, if it did enter, the case might be cured without cutting. The wife came to see me the next day, some five or six hours after sexual intercourse. A drop of mucus from the cervix contained spermatozoa in great abundance. Here, the whole question of diagnosis and treatment was settled at once, and in the only way possible, by the microscope. For this one examination proved all that was essential to know—viz., 1. That the semen was perfect; 2. That it entered the cervical canal, and therefore there was no surgical operation necessary; 3. That the cervical mucus poisoned the spermatozoa, and hence a treatment directed to the utero-cervical canal was indicated. After the next menstruation (a month's treatment), the cervical mucus was considerably improved, for it contained large numbers of active spermatozoa. At the end of two months, I found living spermatozoa in the cervical mucus,

thirty-six hours after coition. All treatment was now suspended, and after the next menstruation conception took place.

No. 5, married five or six years without offspring. The uterus was small, and retroverted by a fibroid, about the size of a walnut, on its anterior surface, just at the junction of the cervix and body. The os was very small, so small that a most distinguished accoucheur advised incision of the cervix, to admit the passage of the semen, although he was not in the habit of performing the operation, and, as a general rule, was opposed to it. In former years, I would have given the same advice without the slightest hesitation. But now I said, No. Let us first see if the cervix admits the semen. If so, the operation is hardly necessary. If not, it is. I saw the wife the next day. A drop of cervical mucus, under the microscope, determined the question against the operation at once; for the mucus was full of spermatozoa, but they were all dead. During the treatment of this case, I have seen the mucus in the lower segment of the cervix full of living spermatozoa, while that taken from the os internum was full of dead and dying ones. Nothing but the microscope could have revealed the truth in such a case as the above.

No. 6, married eight years, sterile, had been treated by several distinguished physicians for the sterile state; and had been to Ems and other watering-places, all for no result. At last she came to Paris, to see my friend Sir Joseph Olliffe, and he called me in consultation. I found a long, conical, indurated cervix, with a small os—just such a case as I would have pronounced sterile by necessity, and just such as I have over and over again operated upon without further thought. But now I wished to be sure, before recommending an operation. After explaining the necessity for it, I requested this lady to come and see me, four or five hours after coition. She returned the next day. I could find no spermatozoa in either vaginal or cervical mucus. I requested her to come again. I saw her two days afterward—no sign of spermatozoa. I then told her that perhaps the seminal fluid all passed away in the act of rising and dressing. She thought it did. After further explanations, she readily agreed to send for me some morning, to verify the state of affairs. She was a very sensible woman, and fully understood the reasons given. A day or two afterward, I saw her in bed, about thirty minutes after sexual intercourse. The vagina was full of semen; and I removed about a drachm of it, and went home immediately for the microscopic examination. But, unfortunately, there were no spermatozoa. Not very long ago (seven or eight years), I had the idea that sterility was essentially a female infirmity; that men were never sterile, except when impotent; and that any man, legally competent for the married state, was physically so for procreation. But the microscope unsettles and settles all such vague notions. It is natural to suppose that a strong, vigorous man is more fitted for procreation than a weak or puny-looking one. Some of the greatest lights of the profession have held such views as this. It was only two or three years before the death of the lamented Trousseau, that he said to me, in speaking of a case we had

under consultation, "If our patient only had a man for a husband, all would be right." I subsequently found out that the husband's passions were strong; that his semen was perfect; that it entered the cervix in great abundance; and that the spermatozoa were there poisoned by a vitiated secretion. I mention this to show that we must not judge from appearances, when it is so easy to settle the question by the microscope.

No. 7, married nine years, sterile, had consulted several distinguished physicians, one in Germany, who told her it was useless to try any further treatment, as she was now well enough, and that it was the fault of her husband that she did not conceive. I explained to her that there was nothing easier than to determine that question at a single visit. She came the next day. I removed some vaginal mucus; also a mass of cervical, as large as a pea, that was just hanging from the os; also some from within the canal. The vaginal mucus contained spermatozoa, but, of course, they were all dead. The mass of cervical mucus that hung out of the os contained spermatozoa in abundance, all dead. The mucus from the interior of the cervix was wholly devoid of spermatozoa.

Here the microscope settled the whole question. There was no longer any guess-work. 1. It was not the fault of the husband that there had been no conception. 2. The seminal fluid did not enter the canal of the cervix. 3. The spermatozoa were killed by the cervical mucus, where the two came in contact. As the shortest and best method of treatment, I incised the cervix. After the subsequent menstruation, semen was found to enter the canal of the cervix. After the next period, they were found there in abundance, and all living. In three months thereafter, she conceived. In another three months, she miscarried, in consequence of a fall. Six months after this, she conceived again; and a year ago she became a mother.

So far I have related only cases of natural sterility, and, were it necessary, I could give you scores more of the like character, but, as you perceive, there is so much sameness among them, that it would be superfluous. However, bear with me a moment longer, while I give you one or two illustrations of the value of the microscope in acquired sterility.

No. 8, aged 36, had given birth to one child, ten years ago. Her general health was perfect, but she did not conceive again. She was anxious for more offspring—had been to various watering-places, and had consulted several distinguished physicians. At last she fell into the hands of my friend Dr. Lheritier, who brought her to me. I found the uterus hypertrophied, and somewhat retroverted. The os was rather small and the cervix indurated, and I had some doubt whether the semen could enter the cervical canal. But a microscopic examination proved that it did, and that the cervical secretions killed all the spermatozoa. This case was under treatment in January and February, and again in May and June. When she left in June, living spermatozoa were found in the cervical mucus, in great abundance, thirty-six hours after coition. We, therefore, pronounced the case cured. She conceived a month afterward, and was safely delivered at term.

No. 9.—We often fail to cure curable cases because the treatment is sometimes so tedious that both patient and doctor get mutually tired, and both are glad to quit. Madame ——, aged 34, had one child eight years ago; subsequently had chronic cervical inflammation; was cauterized too much. The cervix became indurated, and the os contracted. She wanted more offspring. I was in doubt about cutting open the cervix. A microscopic examination proved that the semen could not enter the cervix. Accordingly, I incised the os. After this, the semen entered the canal of the cervix, but its mucus killed all the spermatozoa. The mucus was not as clear and limpid as it should be, and it had white milky specks in it, looking as if it had been mixed with a little of the vaginal secretion. The lining membrane of the cervix was too red and rather granular. This was cauterized even up to the cavity of the uterus; and various other local as well as general remedies were adopted and carried out from time to time for twelve months. The character of the cervical secretion gradually improved, and at times showed some living spermatozoa, and again all were dead. This patient did not despair, notwithstanding a fruitless treatment for so long a time.

A sponge-tent had revealed long ago a small flattened cystic tumor in the canal of the cervix, situated on its posterior face, just at the os internum. I had repeatedly suggested the propriety of extirpating it. After all other means had been exhausted for restoring the cervical secretion to a normal state, the operation was agreed to. In June, 1867, nearly two years after we began the treatment, a sponge-tent was introduced; the canal of the cervix was fully dilated, and a cystic tumor, about the size of the end of the little finger, was extirpated. Three months afterward, the cervical mucus was greatly improved; and in March last, after a treatment of more than two years and a half, I examined the secretions fifteen hours after sexual intercourse, and I had the satisfaction of saying, "At last, madam, I find the cervical mucus perfect; it is full of spermatozoa, and all very active. We can now hope for conception." Conception dated from that period, for she did not menstruate afterward.

But for the microscope, I would have dismissed the case as cured after the incision of the cervix uteri, and she would have remained, in all probability, sterile to the end.

Once I thought that the most common obstacle to conception was a contracted cervical canal, contracted at its outlet, at the os internum, or throughout its entire length. But, if I were now asked, "What is the most frequent obstacle to conception?" I should unhesitatingly say, "An abnormal utero-cervical secretion that poisons or kills the spermatozoa." I can call to mind numbers of cases where, in former years, I incised the cervix, when the operation was satisfactorily done, and yet the sterility persisted. In some of these I have now not the least doubt that the husbands were sterile, and in others I have as little doubt that the cervical mucus was poisonous to the spermatozoa. If I had then possessed the exact knowledge of to-day, how much more satisfactory would it have been for me—how much better for my poor patients!

I could go on for hours with cases to illustrate the principles already laid down. The foregoing are taken at random, and are sufficient for the purpose. I have not treated a single case of sterility as such in the last six years, without determining the three questions so essential to success that were stated at the outset of this paper, except the half-dozen cases already alluded to, and in these the microscope at last revealed the truth.

Before closing this paper, pray allow me to say a word personal to myself, which is, at the same time, in vindication of the honor and progress of medicine. When my book on "Uterine Surgery" appeared, in February, 1866, it was noticed, generally favorably by the medical press, and always honorably, with but one ex-

ception abroad, and two or three at home. The *Medical Times and Gazette*, one of the most excellent and influential journals of the day, conducted with great ability, and usually with liberality and decorum, condemned, in the strongest terms, my investigation of the seminal fluid, and said that "this dabbling in the vagina with speculum and syringe" was incompatible with decency and self-respect. Now, for myself, I see no indelicacy or impropriety in taking mucus from the vagina and uterus for microscopic examination. It is no more indelicate, no more impure, than to investigate the character and properties of saliva, or bile, or urine, or fæces, or pus. And where is the scientific physician, nowadays, who could or would dare to give an opinion on any obscure and complicated disease without some such investigation? To answer that question, I have only to call to the witness-stand such men as Beale, Hughes Bennett, Gull, George Harley, Sir William Jenner, Bence Jones, George Johnson, Stokes, and the immortal names of Addison and Bright; and in my own country, the great names of Alonzo Clark, Austin Flint, John T. Metcalfe, and a host of others. Opposition and ridicule are ever ready, but never yet crushed out a great truth. With the simplicity of my nature, and with the honesty of my purpose, there can be no indecency, and no sacrifice of self-respect in making any necessary physical examination whatever, if it be done with a proper sense of delicacy, and with a dignified, earnest, and conscientious determination to arrive at the truth—a truth without which every step is in the dark, but with which all is as clear as the noonday's sun.

Proceedings of Societies.

MEDICAL SOCIETY OF THE COUNTY OF NEW YORK.

Stated Meeting, December 7, 1868.

Dr. Geo. T. Elliot, President, in the Chair.

After transaction of the usual preliminary business, the President announced that, at the last meeting of the Comitia Minora, it was voted to grant certificates of membership to Drs. Horatio Paine, Eustis F. Langdon, David Magie, Jr., D. A. Goodwillie, Ernst Krackowizer, Emil Noeggerath, W. B. Neftel, Chas. A. Leale, F. A. Burrall, E. L. Keyes, Salvatore Caro, A. B. Crosby, Hermann Knapp, and O. A. White.

The death of Dr. John O'Reilly, of this city, a member of the Society since 1866, was announced by the Chair. Dr. O'Reilly was born in Ireland, in 1813, and had been a resident of this city, and engaged in the active practice of medicine, since 1849. Dr. O'Reilly was favorably known to the profession of this city. He was the founder of the O'Reilly Prize, offered by the New York Academy of Medicine.

After the presentation of the reports from the several committees, the paper of the evening was read by Dr. Marion Sims, on "The Microscope as an Aid in the Diagnosis and Treatment of Sterility."

[This paper is published in full in another part of this number of the Journal.]

The paper being accepted by the Society, Dr. Peaslee was called upon by the President for remarks. He regarded the paper just read as very complete in itself, leaving nothing more to be said upon certain points. He had hoped, therefore, that some side issues might have been raised by others, upon which he could base his own remarks. Though Dr. Sims's investigations were not in all respects original, yet he had pursued them further than any of his predecessors. And, despite the misrepresentations of his maligners, he had just reason to be proud of the manner and spirit in which he had pursued them, and of the important practical truths he had attained. Science was no respecter of persons; and her faithful votaries knew no such word as indecency.

The speaker was delighted to hear that Dr. Sims no longer advocated incision of the cervix, as a matter of course, in the treatment of sterility. He had himself never practised it in such cases, warned against it by its worse than useless effects in several cases that had come into his hands. In some of these, the incisions had been carried so far as to involve the os internum, so that, if conception were to occur, miscarriage would almost certainly follow. The paper had correctly indicated the circumstances under which the operation was proper But the criterion there given for considering the treatment complete—that of finding the spermatozoids in an active state, high up in the cervical canal, a considerable time after coition —was hardly sufficient. There were cases of flexion in which they might enter the canal, and find there the normal secretions, and yet be unable to pass the point where the flexion brought the walls into close contact. He had in these cases brought about conception by simply raising the fundus uteri, and thus partially straightening the bent canal.

The remarks in the paper upon the frequency of sterility in the male were very important. After double epididymitis, as long since stated by Lallemand, it was sure to occur, the spermatozoids not appearing in the semen for several months; and they were very likely to disappear again on any rekindling of the inflammation, from taking cold, for example. The physician should, therefore, satisfy himself concerning the husband's history in this respect, as the first step in his investigation of a case of sterility. Again, in some cases of urethral stricture, the semen, unable to pass the point of stricture during the turgescence of the organ, was regurgitated into the bladder. The doctor had known a similar result produced in a patient addicted to masturbation after a fashion certainly original. Deeming that the ill effects of the habit were due to the loss of semen, this man used to prevent its escape by tightly grasping the penis at the moment of highest excitement, thus causing regurgitation into the bladder. When he afterward married, he found that, although he had suffered no loss of sexual vigor, yet the seminal fluid persisted in following the course it had before been compelled to take. There was no emission attending the sexual act, but the spermatozoids

could be found in the urine. Of course, the urine speedily destroyed the vitality of the spermatozoids. This man would probably remain childless.

The speaker discussed at some length the physiology of reproduction, dwelling upon the many obstacles to impregnation, and upon the exhaustless resources of Nature, which could afford to waste millions of germs for one that came to fruit. He indorsed the view that the uterus takes an active part in the sexual act, contracting firmly during the period of excitement, and, in that of relaxation, expanding, and so exerting a suction power to draw the seminal fluid into the cervical canal.

The President had seen it stated, on excellent authority, that the mode of preventing emission of the seminal fluid, practised in the case related by Dr. Peaslee, was common among the French prostitutes, to avert the danger of conception; and that it had given rise, in some cases, to permanent impotence.

Cholera Fungus.—We understand that the Director-General of the Army Medical Department and the Senate of the Army Medical School have taken an important step with a view to the final settlement of the Cholera Fungus question. Acting on the advice of the professors of the School, the authorities have consented to send two of the most distinguished *élèves* to Germany to study the subject under Professors Hallier and De Bary. The young medical officers, after mastering the mode of investigation pursued in this difficult inquiry by the eminent men above named, are to proceed to India, and to be set apart to investigate it in that great field of observation—in the very home of cholera. It is to be hoped that this well-advised measure will meet with the success it deserves. It is much to the credit of the Secretaries of State for War and India that they have consented to carry out this measure in a wise and liberal spirit.—*Lancet.*

Salt of Lemons.—A serious accident recently occurred in England from the incautious use of the article bearing the above harmless name. Peroxalate of potash, as it is called by the chemist, removes stains effectually, but its resemblance to epsom salts renders it liable to be mistaken for that aperient medicine. The *London Medical Gazette* warns all housekeepers against this dangerous preparation of oxalic acid.

Bibliographical and Literary Notes.

We are compelled, by the great amount of material accumulated on our hands, to omit all reviews in the present number.

The author, who once resided in this country, now lives in Paris; he is writing, in French, a work on the diseases of the womb, and this[1] is a specimen chapter done into English by himself. He thinks that uterine catarrh is the most common of all the womb disorders which cause sterility, and, moreover, he asserts that it has always been materially bettered, and generally radically cured, by the rational treatment he recommends, which is, "first, general medication; second, a direct action on the organs affected, especially by means of intra-uterine recurrent injections." Of course, Dr. Gantillon has invented, and now "exclusively adopts," an instrument which does away with all the risks of this method, and insures the relief of "this rebellious and oftentimes reputed incurable affection" *cito*, *tuto*, *et jucunde*. This plan has other advocates in France, although the general feeling is unfavorable to it, many disasters having followed its use. We believe that there is always risk in its employment, if the cervical canal has not been previously dilated.

The ample title dispenses us from telling our readers the contents of this well-printed volume,[2] which is addressed more to the general public than to the profession. We were not before aware that muscular laxity of the intestinal canal, of urinary organs, hernia, and uterine displacements, were derangements to which the clerical, legal, and musical professions were particularly obnoxious, nor has the evidence in this book convinced us of the fact; but we are satisfied, from look-

[1] Uterine Catarrh frequently the Cause of Sterility. New Treatment by H. E. Gantillon, M. D. 1868. Pamphlet, pp. 88.

[2] A Rational Treatise on the Trunkal Muscles, elucidating the Mechanical causes of Chronic Spinal, Pelvic, Abdominal, and Thoracic Affections; and of Bronchial and other Derangements incident to the Clerical, Legal, and Musical Professions; with the Rationale of their Cure by Mechanical Support. By E. P. Banning, M. D. New York: Published by W. A. Townsend & Adams. 1868. 8vo, pp. 352.

ing over its tinted pages, that the author is given to indulge in what Mrs. Malaprop styles "a nice derangement of epitaphs."

The Sydenham Society publications for 1869 will comprise a second volume of Trousseau's Clinical Lectures; the second volume of Lanceraux's Treatise on Syphilis; the Biennial Retrospect of Medicine and Surgery, for 1867–'8; and a Sixth Fasciculus of Hebra's magnificent Atlas of Skin Diseases.

The Official Report of the International Medical Congress, held at Paris in 1867, has appeared from the press of V. Masson & P. Asselin, Paris. It embraces all the discussions and papers read at the Congress.

From Churchill's press, London, we have a new edition—the tenth—of Fowne's Chemistry, edited by Drs. Jones & Watts; also A Manual of Diseases of the Eye, by E. Macnamara, M. D.

Goodsir, the Great Anatomist.—Messrs. Black, of Edinburgh, have published two volumes of the scientific remains of the late lamented Professor Goodsir—one of the greatest ornaments of the Edinburgh University in our time. The volumes contain a Memoir by Dr. Henry Lonsdale, of Carlisle, with the valuable papers which were published by Mr. Goodsir in his lifetime; also, notes of two important series of lectures—the one on the place which man has in creation, and the other on comparative anatomy. The text is illustrated by plates and wood engravings, and a fac-simile portrait taken from a rare photograph which was found in the possession of a friend.

Lindsay & Blakiston, of Philadelphia, have in preparation a work on surgical diagnosis, by Addinel Hewson, M. D. They have also imported in sheets, and issued with their imprint, Dr. Lionel S. Beale's well-known work on "*Kidney Diseases, Urinary Deposits, and Calculous Disorders: their Nature and Treatment.*" They also announce to be ready in January, 1869: "*Cleaveland's Pronouncing Medical Lexi-*

con;" a new and improved edition (the eleventh), a small pocket-volume. "*Spence's Lectures on Surgery*," with colored and other illustrations; 1st fasciculus; to be completed in three parts. "*Mackenzie on the Laryngoscope and some Diseases of the Throat*," edited, with additions and an essay on Rhinoscopy, by J. Solis Cohen, M. D.; with lithographic and other illustrations. "*Ruppaner on Hypodermic Injections;*" a new and improved edition. "*Clinical Lectures on Chronic and Gouty Bronchitis, and on Pulmonary Emphysema*," by E. Headlam Greenhow, M. D., F. R. C. P., etc.

J. Campbell, of Boston, has issued a second edition of Dr. D. W. Cheever's monograph on "*Œsophagotomy for the Removal of Foreign Bodies.*"

Messrs. Hurd & Houghton have in press "*How not to be sick; a Sequel to the Philosophy of Eating*," by Albert J. Bellows, M. D.

From the press of Henry C. Lea, of Philadelphia, we are promised in a few days: "*On Syphilis and Local Cutaneous Disorders*," by Berkeley Hill, Surgeon to the Lock Hospital, London. "*A Complete Practical Treatise on Diseases of Children*," by J. Lewis Smith, Professor of Morbid Anatomy in Bellevue Hospital Medical College. "*Clinical Lectures on Diseases of the Urinary Organs*," delivered in University Hospital Medical College, by Sir Henry Thompson; with illustrations. "*A Conspectus of the Medical Sciences; embracing Anatomy, Physiology, Chemistry, Materia Medica, Practical Medicine, Surgery, and Obstetrics*," by Henry Hartshorne, M. D., Professor of Hygiene in the University of Pennsylvania; 1 vol., royal 12mo., of about 1,000 pages, with several hundred illustrations.

The magnificent edition of Cullerier, translated and edited by Bumstead, is now completed.

In the January number of the *Medical News and Library* will be commenced the republication of Eustace Smith's work "*On the Wasting Diseases of Children*," just issued in London.

Of German publications, we have observed notices of the following:

Atlas des Menschlichen Gehörorganes. Herausgegeben von Dr. Rüdinger, k. Adjunct und Prosector an der Anatomischen Anstalt in München. Nach der Natur photographirt von J. Albert. Erste Lieferung, enthaltend: 8 Tafeln Photographien mit 20 Figuren; 4 Tafeln Lithographien und den beschreibenden Text. Zweite Lieferung, enthaltend: 8 Tafeln Photographien mit 23 Figuren; 4 Tafeln Lithographien mit 9 Figuren und beschreibendem Text. Atlas of the Human Ear. By Dr. Rüdinger, Royal Adjunct and Prosector in the Institute of Anatomy at Munich. Photographed from nature by J. Albert. Munich. First Part, containing: 8 photographic plates with 20 figures, and 4 lithographic plates with descriptive text. Second Part, containing: 8 photographic plates with 23 figures, 4 lithographic plates with 9 figures and descriptive text. Munich, J. J. Leutner, 1867.

Gesammelte Abhandlungen über Physiologische Optik. Von Dr. August Classen, in Rostock. Berlin, August Hirschwald, 1868. Collected Treatises on Physiological Optics. By Dr. August Classen, of Rostock. 175 pages. Berlin, August Hirschwald, 1868.

Der Intra-oculare Druck und die Innervations-Verhältnisse der Iris, vom augenärztlichen Standpunkte aus betrachtet. Von Dr. Karl Stelwag von Carion, in Wien. Wien, Wilhelm Braumüller, 1868. Intra-ocular Pressure and its Relations to the Nerves of the Iris, considered from the ophthalmological stand-point. By Dr. Karl Stelwag von Carion, in Vienna. 100 pages. Vienna, Wilhelm Braumüller, 1868.

Der Mechanismus der Accommodation des Menschlichen Auges, nach Beobachtungen im Leben dargestellt von Dr. E. A. Coccius, Leipzig. Mit einer lithographirten Tafel. Leipzig, G. B. Teubner, 1868. The Mechanism of Accommodation in the Human Eye, from Observations on Living Subjects. By Dr. E. A. Coccius, oculist and professor in Leipsic. 153 pages. 1868.

Die Querextraction des grauen Staars der Erwachsenen. Von Dr. H. Küchler, zu Darmstadt. Erlangen, Ferdinand Enke, 1868. Extraction of hard Cataract in the Adult by a

transverse Section of the Cornea. By Dr. H. Küchler, of Darmstadt. 37 pages.

Die Theorie der Augenfehler und der Brille. Von Dr. Hermann Scheffler. Mit 68 Holzschnitten. Wien, Wilhelm Braumüller, 1868. Theory of Errors of Vision and of Spectacles. By Dr. Hermann Scheffler. With 68 woodcuts. 191 pages.

A translation of this book by Dr. Robert B. Carter is announced for publication by Longmans, Green & Co., of London.

Books Received.—"Prodrome of a Work on the Ornithology of Arizona Territory." By Elliot Coues, M. D., Asst. Surg. U. S. Army. Pamphlet; pp. 64.

"Recherches Expérimentales sur une Nouvelle Fonction du Foie, etc." Par Austin Flint, Fils, Docteur en Médecine, etc. D. Appleton & Co., New York, 1868. 8vo. pp. 22.

This is a translation into French of Dr. Flint's article which appeared in October, 1862, in the *American Journal of Medical Sciences*, and which attracted such marked attention from the scientific world. The compliment paid to the author by the translation, which was undertaken at the instance of Prof. Ch. Robin, is as handsome as it is well merited.

"Proceedings at the Opening of the Rhode Island Hospital, October 1, 1868." Pamphlet; pp. 55. (From Dr. Geo. L. Collins.)

Our extended description in the November Journal of this beautiful hospital renders unnecessary any notice of this pamphlet. The addresses made at the opening ceremonies were appropriate to the occasion, and the assurances of the substantial support of the hospital, furnished by the prompt endowment of so many free beds, must be especially gratifying to the originators and promoters of this admirable charity.

"Case of Excision of the Entire Scapula; to which is added a History of the Operations involving the Removal of all or considerable Part of this Bone, etc." By Stephen Rogers, M. D. Reprint from the *American Journal of Medical Sciences*. From the Author.

For a notice of this admirable monograph see the comments on a similar case, reprinted from the *Lancet* in the present number of the *Journal*.

"Report of the Proceedings of the Association of Medical Superintendents of American Institutions for the Insane at their Twenty-second Annual Meeting, held at Boston, Mass., June, 1868. Harrisburg, Pa., Thos. F. Schœffer, 1868. Pamphlet; pp. 207.

"Annual Report of the Officers of the Vermont Asylum for the Insane, August, 1868." Pamphlet; pp. 11.

"The Materia Medica in its Scientific Relations." (Anonymous.) New Haven, Conn., Judd & White. Pamphlet.

"The Physiological and Therapeutical Effects of Compressed Air Baths." By Charles A. Lee, M. D. Buffalo, N. Y., 1868. Pamphlet; pp. 34.

"Subjective and Objective Symptoms; a Preliminary Lecture delivered at the Hahnemann Medical College of Philadelphia, Pa." By C. G. Raue, M. D. Pamphlet; pp. 16.

"Thèse de Pharmacie présentée et soutenue à l'École Supérieure de Pharmacie." Par Armand Fumouze, M. D., de la Cantharide Officinale. Paris, Germer Baillière, 1867. 4to. pp. 58. Avec planches.

Reports on the Progress of Medicine.

ANATOMY AND PHYSIOLOGY.

1.—*On the Physiology and Physics of the Muscles.* [Journal de l'Anatomie et de la Physiologie. Janvier et Février, 1868, pp. 27.]

Dr. Jacob Chmoulevitch reports a series of experiments performed with a view of ascertaining the active force of muscles when separated from the body, and stimulated to contraction at different temperatures. He used for this purpose the gastrocnemii muscles of the frog, placed in a metallic vase, filled with an aqueous solution of chloride of sodium of the strength of 0.65 parts of salt for 100 parts of water. This solution had been found, by previous trials, to be the best for preserving the vital powers of the muscle uninjured for a considerable time. The tendon of the muscle was attached by a fine steel hook to the bottom of the vase, the upper extremity was left adherent in the natural manner to the bones; and a second steel hook introduced between the muscle and the bone, connected it with the short arm of a lever, working upon an upright standard. The long arm of the lever carried the weight to be raised by the muscular contraction, and was also provided with a pencil which registered the extent of its movements, and consequently the height to which the weight was raised. Before applying stimulus to the muscle, its tension was regulated and the equilibrium of the lever secured by a smaller weight attached to the middle of the short arm of the lever. The temperature of the muscle was varied as the experiments required, either by immersing the metallic vase in a jar of warm water, or by adding to the solution in which the muscle was immersed. The stimulation of the muscle was accomplished through the medium of the sciatic nerve, a portion of which was included between the electrodes of an electric apparatus, so arranged that an induced current could be produced at will.

The author distinguishes between the *partial* and *total* effect of the muscular action. The *partial* effect is that produced by a single muscular contraction; the *total* effect is the sum of all the separate actions produced by the muscle in successive experiments until the complete exhaustion of its contractile power. As the result of these examinations, the author concludes:

1. The partial effect of the contraction of the frog's muscle increases with the elevation of the temperature, up to 30 or 33 degrees Centigrade (86 or 91.4 degrees Fahrenheit), according to its length and tension.

2. For each muscle there is a certain tension at which it preserves the same length at different temperatures.

3. If the temperature of the muscle be raised above 30 to 33 degrees C., its mechanical effect begins rapidly to diminish. By continuing the elevation of the temperature a degree is then reached, at which the muscle, when supporting a certain weight, will no longer contract under the application of stimulus. Its mechanical effect is then reduced to zero.

4. Other things being equal, the muscle becomes more rapidly exhausted at a high than at a low temperature.

5. On account of this more rapid exhaustion of the muscle owing to the temperature, its total mechanical effect is always less when the temperature is high than when it is low.

These experiments appear to have been very carefully performed, and their results accurate and valuable. We cannot say as much for the conclusions, which the author desires to draw from them, in regard to "other phenomena." As too often happens, when the physiologist leaves the narrow path of direct experiment for the wide field of general speculation, his inferences lose their virtue, from the neglect of a variety of new conditions, which are very likely to influence the result.

"I hope," says Dr. Chmoulevitch, "that I shall also succeed in the explanation of other phenomena besides those observed in my experiments. At present, I only wish to call attention to certain facts which are explained by what I have demonstrated above. It is, in this way, that we can account for the greater energy and the rapid exhaustion of the inhabitants of warm climates in manual labor. It also explains why the inhabitants of the temperate zones cannot continue at work for any great length of time during summer weather; because, at a high degree of temperature the muscle is comparatively incapable of producing a mechanical effect. The temporary increase of muscular force by the use of alcohol, is, perhaps, to be explained, in part, by the greater production of heat in the muscle, consequent upon the increase of the molecular movements, which are, themselves, a result of the accelerated circulation. We can account in a similar way for the effect of a cold bath, which reëstablishes the vigor of the muscles, after they have been exhausted by an elevated temperature.

"It appears that the muscles of organic life are subjected to the same laws as those which are composed of striped fibres. We see, accordingly, why the inhabitants of warm countries are obliged to confine themselves to light and easily digestible kinds of food; because the muscular fibres of the alimentary canal are incapable of doing the necessary work to furnish, on the one hand, the requisite quantity of the gastric and intestinal juices, and, on the other, to effect the movement of the alimentary materials. The frequency of diarrhœa, during the summer season, may be due to the same debility of the muscular fibres; its treatment, accordingly, should consist in the administration of tonics (such as spirituous liquors, strong coffee, etc)., which invigorate the muscles, while opiates, on the contrary, by debilitating the muscles, would be injurious."

2.—*Researches on the Structure of the Fibrous Envelope of the Nerves.* [Journal de l'Anatomie et de la Physiologie. Janvier et Février, 1868, pp. 47.]

Prof. Sappey contributes an article on the above subject, in which he describes the structure of the neurilemma more carefully and fully than it has been done heretofore. This

envelope, which has thus far been considered as a membrane consisting only of laminated or condensed areolar tissue, with a few vascular twigs ramifying in its substances, Prof. Sappey describes as containing adipose tissue and elastic tissue, mingled with its other constituent parts, and as being supplied with blood-vessels, which are remarkable both for their size and their multiplicity. The most remarkable feature of the neurilemma, however, according to Prof. Sappey, consists of the nervous filaments distributed to its tissue, and described by him under the name of the *nervi nervorum*.

"This envelope," he says, "also receives nervous filaments, which are to the nerves what the *vasa vasorum* are to the blood-vessels; whence the name of *nervi nervorum*, by which I propose to designate them. The arrangement of these nervous filaments differs but little from that which they usually present in the fibrous tissues. They also follow the course of the principal arteries. Like the arteries, they give off and receive, during the course, numerous anastomotic branches; so much so, that at certain points they even form small nervous plexuses. They are to be seen, not only on the common sheath of the nerve, but also on those of the smaller nervous fasciculi. Nevertheless, they become at the same time more slender, and less numerous, as the calibre of the fibrous sheath diminishes; and very often they are no longer to be seen on the sheaths of the secondary fasciculi. They are always wanting in the sheath of the primary fasciculi; and accordingly they are not to be looked for in the sheath of any nervous filament of less than half a millimetre ($\frac{1}{50}$th of an inch) in diameter. The nervous tubules of which they are composed, are in general remarkable for their tenuity. Each one of them, notwithstanding, is composed of an external envelope, or perineurion, a medullary layer, and an axis-cylinder."

3.—*Experiments with the Poison of the American Copperhead* (Trigonocephalus contortrix). [New York Medical Record, September 1, 1868.]

Prof. Joseph Jones, M. D., Professor of Physiology and Pathology, in the Medical Department of the University of Nashville, describes the specific characters of the copperhead, and details the result of five experiments with the venom of the serpent upon the dog, the kitten, and the cock. From these experiments he draws the following conclusions:

1. The primary and chief action of the poison of the American copperhead (Trigonocephalus contortrix) is upon the blood.

2. The poison of the copperhead is directly destructive to the colored blood-corpuscle, altering its physical and chemical properties and relations, and rendering it unfit for the performance of its important offices in circulation, respiration, and nutrition.

3. The poison of the copperhead appears to have an affinity more especially for the coloring matter of the colored blood-corpuscles.

4. Under the action of the poison of the copperhead the animal temperature is but slightly increased, notwithstanding the profound changes

inaugurated in the blood; and after the establishment and propagation of these pathological changes the temperature descends.

5. The action of the heart is increased in frequency, and diminished in force, under the influence of the poison of the Trigonocephalus contortrix. This increase in the rapidity of the pulsations of the heart is not, as in the case of febrile diseases, attended by a marked rise of animal temperature. This difference may be due to the peculiar and direct action of the poison upon the colored blood-corpuscles.

6. In its action upon the cerebro-spinal nervous system, the poison of the Trigonocephalus contortrix resembles a mild narcotic—while rendering the animal sluggish and stupid, it may produce death without the establishment of profound coma.

7. The profound alterations induced in the constitution of the blood by the poison of the American copperhead give rise to passive hæmorrhages into the cellular structures, and from the intestinal mucous membrane. This phenomenon recalls strongly the passive hæmorrhages in certain febrile diseases, and especially of yellow-fever. Some have supposed that the black vomit of yellow-fever was the resultant of the effects of the preceding intense fever. Do not the present experiments indicate that it is rather the resultant of the action of a poison upon the blood and gastro-mucous membrane? We have here also an illustration of the mode in which dysentery might be produced by a poison introduced into the blood.

4.—*Localization of the Reflex Movements.* By Dr. J. Cayrade. [Journal de l'Anatomie et de la Physiologie. Juillet et Août, 1868.]

This article is for the most part a criticism of the conclusions sometimes derived from experiments on frogs, by which a certain degree of apparent intelligence or adaptiveness is attributed to the reflex movements in the decapitated animal. These experiments are more particularly those of Pfluger and Auerbach, quoted from Vulpian's "Leçons sur la Physiologie de la Système Nerveux," as follows:

"Pfluger places a drop of acetic acid upon the upper part of the thigh of a decapitated frog, and then sees the posterior limb of the animal bend itself, so as to rub the foot over the irritated point. He then amputates the foot, and afterward repeats the experiment; the animal again begins the movements as before, for the purpose of rubbing the irritated spot, but, of course, without success; and after some moments of agitation, as if, says Pfluger, he were trying to discover some other way of accomplishing his design, he bends the limb of the opposite side, and succeeds in that manner."

Auerbach has met with similar results. After amputating one of the thighs of a decapitated frog, he puts a drop of acid upon the corresponding side of the back. The animal at first makes efforts to reach it, but, as if he recognized their inutility, soon becomes quiet. A drop of acid is then applied to the back on the opposite side of the median line, and the frog immediately rubs the spot with the corresponding foot; then, as if he saw a means of reaching the spot previously irritated, he reaches over to that side with the remaining limb, and rubs it with the foot.

Dr. Cayrade gives reasons for believing that in all these cases the movements of an intelligent nature, directed to the relief of an irritated spot,

are dependent, not on the reflex action of the spinal cord itself, but on that of a portion of the cerebral protuberance (*le bulbe*) which is allowed to remain after decapitation. He regards the following conclusions as legitimate:

1. The phenomena of reflex action depend upon a preëxisting arrangement of nervous fibres in the spinal cord. They must, therefore, take place in an invariable manner, and we cannot attribute to them any character of spontaneity or of adaptation to a determined purpose.

2. In frogs, as well as in the superior animals, the destruction of the cerebral lobes abolishes permanently all spontaneous motion; that is, all motion due to the express will of the animal.

3. Frogs from which the optic tubercles have been removed, still retain the power of coördinating partial movements into general ones; but they lose altogether the harmony and character of equilibrium of associated movements, such as those of swimming, jumping, etc.

4. Frogs which retain a portion of the cerebral protuberance are still able to direct partial movements, according to the irritation which has been applied to them, but these have not the character of the true reflex movements. The cerebral protuberance, in the frog, is the analogue of the tuber annulare in the higher animals.

5. The name "reflex movements" should only be applied to those which are executed by the frog when the medulla has been divided just below the level of the cerebral protuberance, for these movements are performed blindly, after the application of a stimulus, and are subject to the influence of the spot excited, and the degree of the excitation.

6. If we make allowance for the effects produced by partial exhaustion, it becomes evident—

First, that the reflex action takes place on the same side as that to which the stimulus has been applied.

Second, that if the stimulus is directly communicated to the opposite side, it produces symmetrical movements in the two limbs, *provided always that these limbs are in a symmetrical position at the time of trying the experiment.*

Third, the reflex movements are most intense upon the side of the irritation.

Fourth, a reflex excitement has a manifest tendency to be localized on that side of the medulla which received the original impression.

Fifth, a reflex excitement spreads in every direction within the medulla; and longitudinally it is propagated as easily from below upward, as from above downward.

Sixth, a reflex excitement, spreading through the nerve-cells of the medulla, produces, by this secondary stimulus, various movements, corresponding with the direction of the current; but it usually puts the limbs in the opposite position to that in which they were at the time the stimulus was applied.

5.—*Lateral Hermaphroditism.* [The British Medical Journal, June 6, 1868.]

In the *Liverpool Medical and Surgical Reports*, October, 1867, Dr. Rawdon describes a case of true lateral hermaphroditism, in which a tolerably developed vagina and uterus were present. In the left broad ligament a Fallopian tube, a round ligament, and apparently a parovarium were found, but no trace of either testicle or ovary; in the right broad ligament a Fallopian tube and a distinct testicle, with an epididymis, and a vas deferens, which was traceable on the side of the uterus as far as the

cervix, the junction between the testicle and the epididymis being very feebly, if at all developed. The conformation of the pelvic cavity was between the male and female types. It is alleged by the person that a partial occurrence of the menstrual secretion took place regularly. The case is of interest in its bearings on the development of the genital apparatus, and, from the coexistence of a Fallopian tube and a vas deferens on the same side of the body, it supports the view that these ducts are developed from distinct embryonic structures, viz., from the Müllerian duct and the excretory duct of the Wolffian body.

Cases like the above are not, properly speaking, instances of *true* but of *false* hermaphroditism. True hermaphroditism, if it ever exist at all, is that condition in which the individual is at the same time both male and female; that is, in which both testicles and ovaries, or one of each, coexist in a state of complete development. An individual having either one or both testicles fully developed is a male, no matter how imperfect may be the development of the accessory parts; and on the other hand, the presence of ovaries is alone sufficient to mark the sex as female, though the uterus may be wanting, and though the external organs, as sometimes happens, may be so unnaturally enlarged as to present a deceptive resemblance to the penis and scrotum. The individual described in the case detailed above was therefore a male, with but one testicle (monorchia), in whom this single testicle had not descended below the inguinal ring, and in whom the accessory parts had taken on an unnatural development, so as to resemble in form those of the female.

6.—*Complete Transposition of the Thoracic and Abdominal Viscera.* [American Journal of the Medical Sciences, January, 1868.]

Dr. N. Hickman, Demonstrator of Anatomy in the University of Pennsylvania, has met with the following case of complete transposition in the dissecting-room of the university:

In the thorax, the *lungs* were reversed, the left and larger consisting of three lobes, while the right lung had but two. The *heart* extended obliquely from left to right, the apex pointing to the intercostal space between the fifth and sixth ribs of the *right side.* It was bound down to the pericardium and diaphragm by three fibrous bands, probably the remains of an old pericarditis.

The anatomical characters depending upon function were also reversed, the right side being thicker in its muscular walls, the ventricle giving off the aorta and the auricle receiving the pulmonary veins; while the walls of the left cavities were the thinner, the auricle receiving the venæ cavæ and the ventricle giving off the pulmonary artery. Judging from the universality of these transpositions, it is inferred that the valves were also transposed, the mitral separating the right auricle and ventricle; while the tricuspid separates the left ventricle and auricle. We are unable to state

absolutely that this is the case, because it is thought that to lay open the heart will impair the value of the specimen as a preparation.

The *aorta*, after its origin from the right ventricle, arches first to the left and then to the right, and descends on the right side of the vertebræ to its bifurcation at the top of the fourth lumbar vertebra. The coronary arteries arise as usual from the commencement of the aorta. The first branch given off from the arch is the *innominate*, which subsequently divides into the left common carotid and subclavian arteries. The next one, the right common carotid and subclavian, which it will be seen is the reverse of the usual distributions. The remaining branches of the aorta are as usual, except the *cæliac axis*, which is wanting, its place being supplied in part by the superior mesenteric which gives off the hepatic and gastric, while the splenic arises directly from the aorta.

The ascending cava is on the left of the aorta, necessitating the right common iliac vein to pass under the left common iliac artery, and the left common iliac vein passes under the artery to join the cava. The cava, as it ascends, receives its usual branches. The *right renal* vein receiving the *right spermatic vein*, crosses the aorta, and is therefore much longer than the left. The *left spermatic vein* empties into the vena cava. The descending cava is formed by the union of the right transverse vein (resulting from the union of the right internal jugular and subclavian veins) and the left innominate vein, after which it passes downward to terminate in the left auricle.

In the *abdomen*, the viscera are also reversed. The *liver* is situated in the left hypochondriac region, the larger lobe being under the ribs, while the smaller extends into the epigastrium. The organ is in other respects normal.

The *spleen* is situated deeply in the right hypochondriac region. The stomach occupies its usual space, but has its larger or cardiac end to the right and the pyloric in the left hypochondriac region, causing the small intestines to pursue an opposite course and to join the large intestine (cæcum) in the left iliac region. The large intestine, also, is reversed in its relations, and terminates in the rectum at the *right* sacro-iliac symphysis.

The *pancreas* extends from the spleen in the *right* hypochondrium to the duodenum, beginning in the *left*.

The *pneumogastric nerve* on the right side descends in front of the arch of the aorta into the posterior mediastinum in front of the œsophagus and stomach, thus taking the course usually followed by the left. The left nerve passes posterior to the œsophagus and stomach.

SURGERY.

1.—*Amputation at the Hip-Joint Twelve Days after Injury; Recovery.* By W. B. Beatson, M. D. [Med. Times and Gazette, August 1, 1868.]

It is probable that in this case the amount of injury done was so great that there was no chance of saving the limb, but had amputation been resorted to in the first instance it is questionable if the termination would have been so favorable. The seat of fracture being at the centre of the shaft, it would have been necessary to divide the bone in its upper third, not far below the trochanters, and even then the flaps would have been

formed of soft parts injured by contusion. The sloughing of the integument would have exposed the muscular substance of the flaps; there would have been profuse suppuration, and, in all probability, purulent infection of the system through the divided bone, necessitating a secondary amputation at the joint. As it was, the delay allowed the extent of sloughing to be defined, and a line of demarcation, as it were, to be formed, above which there was a certainty of obtaining sound flaps. The limit in front was so high that, had it been wise to divide the femur, the saw must have been applied close to the trochanters; but had abundant soft parts been available, amputation through the joint was the only proceeding which offered the patient a chance of life. The time the fractured portions had lain bathed in pus, the separation of periosteum, the protrusion of the medulla, and the daily increase of irritative fever, all made it certain that osteomyelitis had commenced, and had any portion of contaminated bone been left a successful result could not have been expected. Section of the bone after removal showed that the view taken was correct, as the medulla was found infiltrated with fetid pus. In performing the operation the anterior flap was, in consequence of loss of integument by sloughing, of necessity made smaller, and the posterior more bulky, than usual. This was at first a disadvantage, as the retention of pus was favored by it; but the ultimate effect was good, as the cicatrix lies high in front, and is not pressed upon in the sitting posture. The suppuration with which the healing process was attended in this case was somewhat profuse, and at one time it seemed probable that the patient would sink under it, notwithstanding the most active support. At the same period, also, the condition of the acetabulum gave cause for grave apprehension, as the bone was felt for some days lying bare and bathed in pus; no sequestrum, however, separated, but from its feel I believe the surface of the cavity was cast off by molecular disintegration. In the treatment of the case at this time carbolic acid was found most useful; in watery solution it was injected through the stump twice daily, and, mixed with oil, it was applied as a dressing to the surface. Although it did not suppress suppuration, I am confident that it greatly diminished the amount of pus, while it completely checked the evolution of fetid gases.

Jhubboo, cooly, Hindoo, male, aged 22 years, came under my care at the City Hospital, Nagpore, on April 18, 1868. On the 12th he was engaged in breaking down a brick wall, when the whole suddenly fell and buried him in its ruins. On being released and brought to the hospital he was found to have sustained several contused wounds on the head, a fracture of the left clavicle, and a compound fracture of the left femur, besides minor injuries. The seat of fracture was about the middle of the shaft, and the communicating wound on the outer side of the limb was small. It was therefore thought that the limb might be saved, and it was accordingly put up with a long straight splint. There had, however, been great bruising of the soft parts, and they soon began to take on unhealthy action, the limb became swollen so that the splint could not be borne, and sloughing commenced in the wound and integument of the front of the thigh.

When I first saw him, six days after admission, the limb was slightly shortened, the foot everted, and the thigh swollen. The skin of the front of the thigh was sloughing, and the wound admitted the finger to the seat of fracture; the bone was found to be broken transversely, and the fractured ends were partially denuded of periosteum. There was no doubt that amputation was demanded, but to this the patient refused to submit. There was very little constitutional disturbance; the skin was cool, the pulse 90, the tongue clean, the facial aspect good.

On the 20th the sloughing had extended, the communication with the

fracture was more patent, but the constitutional disturbance was still remarkably small.

On the 22d the sloughs were separating, and on clearing them away, another opening, communicating with the fracture, was found on the inner side of the limb. He had passed a sleepless night, the foot and leg were becoming œdematous, and there was fever during the day. Pulse 96.

On the 23d all symptoms were aggravated; the pulse was 100 in the morning, and 120 in the afternoon. The fractured ends of the bone were felt entirely denuded, and lying bathed in pus, and the medulla protruding from its canal in the upper fragment.

On the morning of the 24th he was much worse, had passed a sleepless night, and the pulse had risen to 120. He was now willing to submit to amputation. He was accordingly placed under the influence of chloroform, and, assisted by Dr. John Law, of the Madras Medical Service, I proceeded to remove the limb at the hip-joint. This was effected in the usual manner by antero-posterior flaps. The anterior flap was managed by Dr. Law, who followed the knife with his fingers, and grasped the flap so completely that no blood escaped from the femoral artery. This flap had to be made rather short, in consequence of the destruction of the integuments by sloughing, and the posterior flap correspondingly longer. The femoral artery and vein were then tied with silken ligatures, and every bleeding point was carefully secured. Very little blood was lost, but the shock was very severe. He was therefore not removed from the table, and stimulants were frequently given until reaction took place. At 2 P. M. there had been no bleeding; reaction was established. Pulse 126.

On the morning of the 25th his pulse was 126, and respirations 24. He had slept well, and took nourishment freely.

On the 26th the stump was dressed. On pressing it some dark sanious matter exuded from the outer extremity of the wound. The pulse was 120; the lips and tongue rather dry. Toward evening there was an increase of fever, and during the night some hiccup.

On the 27th, in the morning, the skin was cool, tongue moist, pulse 132, respirations 32. The stump in part united, in part suppurating healthily, while from the outer extremity of the wound there was still a discharge of dark sanious matter. Some sutures in this part were therefore cut out, several loose ligatures removed, and the cavity syringed with carbolic acid lotion. The surface was also dressed with carbolic acid and oil. Toward evening the pulse rose to 140 and the respirations to 40.

28th.—Had passed a bad night, and had four loose motions. A bed-sore was also forming over the sacrum. Pulse 120; respirations 36; skin cool and moist; stump discharging healthily. The evening febrile exacerbation was much less; pulse rising only to 120; respirations 30.

29th.—The ligatures came away this morning from the femoral artery and vein. The discharge is free and healthy; the pulse 116; respirations 28; appetite good. He takes abundant nourishment.

May 1.—He is doing well. The last ligature came away to-day. The pulse still ranges from 120 in the morning to 136 in the afternoon.

5th.—He is not so well; restless and more feverish; pulse rises to 138 in the afternoon. The discharge from the stump is decreased in quantity.

6th.—On examining the stump this morning a sense of fluctuation was found in the posterior flap, and, by exploring the wound with the finger, a considerable quantity of matter was evacuated. At the inner extremity of the wound the finger passed to the acetabulum, which was felt bare of cartilage.

On the 13th he had been daily improving. The cavities in the stump had been daily washed out with carbolic acid lotion; the discharge was

much decreased. The acetabulum can be felt covered with granulations, except in the centre, where it has a rough and sandy feel, from disintegration of its surface.

On the 20th he was much improved. Cavities no longer admitted the finger; discharge trifling; pulse 112; respirations 20.

27th.—The stump is now firm and entirely healed, with exception of a narrow sinus leading to the acetabulum. Pulse 96. He is getting fat.

June 11.—During the last fortnight the last sinus has almost entirely closed, and the stump is firm and sound throughout. He is fast gaining flesh and strength, and is anxious to be allowed to go to his home. Discharged cured.

2.—*Excision of the Scapula for Enchondroma.* [Lancet, Nov. 21, 1868.]

This rare operation was performed by Mr. Sydney Jones, at St. Thomas's Hospital, London, on the 2d of October:

Harry T——, brickmaker, aged forty-three, was admitted on the 22d September, 1868. For a rapidly-growing tumor of the shoulder he had consulted Mr. Henry Morris, of Gosberton, who thought the case one necessitating surgical interference, and sent it to London, to be under the care of Mr. Sydney Jones. The history which the man gave was as follows: That he had had a swelling of the left scapula from about the age of nine or ten; that for the next twelve or fifteen years it had slowly increased; that then it seemed to remain quiescent; that during the last three years growth had again occurred, and of late so rapidly that the swelling had, during the last twelve months, quite doubled itself. Until a year ago, he had felt but little inconvenience, except from a sense of weight; but latterly the movements of the shoulder-joint had become much impaired. He had no pain in the tumor, nor was it tender at any point; but its projection forward into the axilla had caused much numbness and aching of the hand, arm, and shoulder. The growth projected much above and beyond the tip of the left shoulder, so as to give increased breadth on this side when looked at in front; it projected considerably forward into the axilla, and it was this part which had lately so rapidly increased; it also extended forward beneath the clavicle, displacing downward and forward the brachial plexus and axillary artery, and so pressing forward the latter as to make its pulsations visible where lying beneath the clavicular origin of the pectoralis major. Transversely, from the axillary to the vertebral border, it measured 19 inches; vertically, from the superior costa to the inferior angle, 15½ inches; and obliquely, from the superior external angle (where overlapped by the deltoid) to the lower part of the vertebral border, 16 inches. The thickness, from the axillary border to the most projecting point posteriorly, was 9 inches. The surface was nodulated, and the integuments posteriorly were thin and much stretched over the tumor. The muscular development of the left arm was somewhat less than that of the opposite side. The scapula was perfectly movable upon the thorax; and the movements of the humerus upon the scapula were so free as to induce the hope that the part of the tumor above the shoulder might be simply overhanging, and that possibly the shoulder-joint might be left intact.

The patient was aware that his arm was becoming more and more useless; and it was evident that an operation, if delayed, would take a more serious form. The removal of the scapula was therefore proposed, and at the same time the serious nature of the operation was explained to him. Having been allowed to weigh the matter thoroughly, he expressed his desire for the operation to be performed, and with as little delay as possible.

He seemed a strong, wiry-looking man, was of a quiet and apparently good-tempered disposition, and was reported to be temperate. The operation was performed on the 2d of October, at 1.30 P. M., the patient having been chloroformed before being brought into the theatre. He was placed in an almost prone position, a block supporting the left side of his chest, and brought well toward the right side of the operating table. Mr. Le Gros Clark took charge of the subclavian artery. Mr. Sydney Jones, standing on the right side, made a long transverse incision (in a direction corresponding to the spine of the scapula, for this could not be continuously traced) from near the acromial end of the clavicle to just beyond the vertebral border of the scapula, and a second vertical incision from the centre of this transverse one to below the inferior angle of the scapula. The integuments were rapidly dissected off to the several costæ. The trapezius and deltoid were detached as far as the acromion, and the latter was at once sawn through just at its junction with the spine. All the muscles connected with the vertebral costa (the levator anguli scapulæ being especially large) were then divided close to the bone; the latissimus dorsi was raised from the inferior angle, and the teres major divided. The disease being found to encroach quite up to the glenoid cavity, the shoulder-joint was laid open, and the tendons connected with the greater tuberosity then divided. The mass was then readily enucleated; the omohyoid, coraco-clavicular ligaments, coraco-brachialis, biceps, subscapularis, and, lastly, the long head of the triceps, having been successively divided. Mr. Sydney Jones remarked that he had anticipated some difficulty with regard to the parts attached to the coracoid process, but the separation of these from behind had been a much more easy matter than he expected. The amount of bleeding was very small; perhaps three, certainly not four, ounces of blood were lost. The subscapular artery, the last divided, was at once ligatured, the posterior and suprascapular, as well as smaller, arteries having been secured, as the operation progressed, by small spring-forceps. Altogether six ligatures were used, the torsion-forceps sufficing to control other bleeding points. The wound was sponged with ether, and, all oozing having ceased, the edges were accurately adapted by wire-sutures. Pads of dry lint were placed in the axilla behind the clavicle, and in other situations where bagging might occur. These pads were kept *in situ*, and the arm was fixed securely to the side by a bandage. The patient was then removed to bed. The operation itself occupied less than ten minutes, but the patient was under the influence of chloroform until all was adjusted—altogether nearly an hour.

The patient died on the fourth day after the operation.

His pulse had, ever since the operation, been feeble, rapid, and fluttering —at times unaccountable and almost imperceptible, necessitating the pouring in of stimulant and nourishment to keep up his flagging powers. But the condition of the skin, the moist and for the most part clean tongue, his quiet, patient, and cheerful disposition, and his countenance, bright and free from anxiety, gave hope that he might pull through. The sickness and feeling of squeamishness may have been produced by the long inhalation of chloroform. The soreness of throat, dependent on inflammation of the muscles and cellular tissue of the neck, at last prevented deglutition.

The tumor weighed 10¾ lbs. It involved the whole of the scapula, with the exception of the glenoid cavity, the coracoid process, and the acromion; and these were closely enveloped at their bases by the growth. It was of the ordinary nodular character on its outer surface; but toward the chest it presented a smooth concavity, corresponding with the convexity of the thorax. The muscles were spread out into thin aponeuroses in those positions where they usually pass over bone; but from the ventral

surface the nodules projected between the fibres of the scapularis. On section, the tumor consisted of transparent cartilage, which was occupied in part by masses of cancellous bone (resembling an osteophyte), with, also, scattered points of greater density. The bony matter was more abundant toward the concavity of the tumor, where it approached to about two or three lines from the surface, and extended to a depth of about an inch and a half.

The *post-mortem* examination was made on October 7th by Dr. Lees. The left scapula had been removed by a T-shaped incision. The flaps were connected by wire-sutures. On the left side of the neck, behind and below the ear, was a swelling with a doughy feel. The subcutaneous areolar tissue and intermuscular tissue were infiltrated with sanious-looking pus and serum. This condition extended to the deep muscles of the neck and to the post-pharyngeal areolar tissue. Larynx, trachea, and bronchi healthy. Pleura healthy. Lungs congested, but crepitant. Pericardium healthy. All the cavities of the heart were filled with coagula, partly decolorized, and extending into large vessels; valves and orifices healthy. In the peritoneum numerous old adhesions connected intestines with abdominal walls. Liver fatty. Spleen soft and pulpy. Kidneys healthy. Pharynx, œsophagus, stomach, pancreas, and intestines healthy. Ureters and bladder healthy.

Exsection of the entire scapula, with preservation of the entire arm, is an operation of very recent date, first performed, it is believed, by Langenbeck, in 1855. The removal of the arm along with the scapula had, however, been many times performed, and, as a matter both of interest and instruction to our readers, we reproduce from Dr. Rogers's admirable paper, printed in the *American Journal of Medical Sciences*, a table of the operations thus far recorded. By Dr. Rogers's courtesy and assistance we have been enabled to add a column, showing where the cases are recorded. This adds greatly to the value of the table; for, without it, subsequent investigators would derive but comparatively little assistance from this otherwise very complete monograph. And besides, several additional cases are now included in the table, some that had escaped the previous search, and some have been communicated not before recorded, and two new cases have occurred since the date of Dr. Rogers's paper.

Dr. R. inclines to the opinion that in cases of malignant disease he would prefer the removal of the whole scapula rather than merely the diseased portions of it, and for the following reasons:

1. That the results are quite as good to the future usefulness of the arm.

2. That the dangers attending the operation are scarcely ever greater, generally less even, than when only a portion of the bone is removed.

3. The liability of the disease to return is probably less. In cases of necrosis and caries, however, these principles are not applicable.

Tabular Statement of the Operations involving the

NO.	OPERATOR.	DATE.	OPERATION.	DISEASE.
1	Mr. Cummings.	1808	Amputation of arm, including the scapula.	Gunshot comminution of these bones.
2	Van Walther...	1811	Attempt to remove the greater part of the scapula.	"Spongy swelling upon the bone."
3	Mr. Liston	1819	Removal of about three-quarters of the scapula, measuring from behind forward. The patient was a boy of 16 years.	Osteo-cancer of a remarkably vascular form.
4	Heymann, of Coblentz.	1823	Removal of all the bone except the neck, supra-spinous fossa, and the acromion process.	Osteo-cancer in a young man of 22 years.
5	Janson, of Lyons.	1824	Removal of all the bone except its processes and the neck and glenoid cavity.	Osteo-cancer of the bone in a female of 45 years.
6	Wutzer, of Münster.	1825	Removal of all of the bone except its processes and glenoid cavity. Patient a man of 44 years.	Osteo-cancer of the medullary variety.
7	Castara, of Luneville, France.	1826	Attempted removal of the greater part of the bone.	"Tumor of the scapula"...
8	Mr. Luke	1828	Removal of three-quarters of the bone, measuring from behind forward.	"Supposed malignant disease of the bone," the patient being a girl of 14 years.
9	Mr. Skey	1830	Removed all of the bone except the glenoid cavity.	"Fibrous tumor" of large size, patient being a man of 40 years.
10	Gaetani Bey....	1830	Removed the scapula and upper extremity.	Gunshot wound comminuting these bones.
11	Jäger	1833	Removed the entire bone except the glenoid cavity and acromion.	Caries; the patient being a female of only 8 years.
12	Dr. Crosby, of Hanover, U. States.	1835	Removal of the scapula, arm, and clavicle, except the sternal end.	Osteo-sarcoma, patient a male 30 years of age.
13	Syme, of Edinburgh	1836	Removed the neck and glenoid cavity, and processes and head of the humerus.	Enchondroma in an adult male.
14	Mussey	1837	Removed the scapula and clavicle six years after amputation at the shoulder, and 19 years after amputation at the metacarpus.	Osteo-sarcoma, the patient being, at the date of the last operation, 40 years old.
15	Larrey	1838	Removed the scapula and arm....	Gunshot comminution of these bones in a boy of 14 years.
16	Twitchell, of Keene, N. H.	1838	Removal of scapula, arm, and part of clavicle.	Not known, but probably malignant.

Removal of all or the greater part of the Scapula.

RESULT.	REMARKS.	AUTHORITIES.
Recovery	This accident occurred to an adult male.	Fergusson's Surg., Eng. ed., 1852, p. 308.
Died fourteen days after the operation, of suppuration and exhaustion.	This patient was a man 30 years of age. The operation was abandoned at an advanced stage, on account of the patient's having fainted, and it was regarded as too formidable to again undertake.	*Résumé* of Capital Surgical Operations, by Dr. G. B. Günther. Leipsic, 1861, p. 36.
Died soon after of a return of the disease.	The hæmorrhage during this operation is described as having been frightful, it chiefly taking place at the time the body of the scapula was divided to separate the diseased from the sound bone.	Edinb. Med. and Surg. Jour., vol. xvi., pp. 66, 215. Quoted by South in his edition of Chelius, vol. iii., p. 762.
Recovery, with good use of the arm, but he died a year after the operation, of return of the disease.	.....	Günther, ibid.
She was discharged 2 months after the operation, with a useful arm on this same side.	This growth is said to have been from the infra-spinous fossa, and to have projected forward into the axilla so much as to prevent the arm from falling nearer to the side than 45°.	Malgaine, Médecine Operatoire, p. 246; South's Chelius, vol. iii., p. 762.
Recovery, with good use of the arm, but the disease returned at length, and the patient died of it 4 years after the operation.	.....	Günther, op. cit.
Patient died of air in the veins during the operation.	This accident is reported to have occurred during the division of the bone between the sound and diseased tissue.	Günther, op. cit.
Recovered with a most useful arm.	This operation is described as having been a very bloody one, and that more than twenty arteries were tied during it.	London Med. Gaz., vol. v., 1830; and Lancet, Jan., 1859.
Recovery, with a useful arm, but died of return of the disease.	Hæmorrhage said to have been frightful.	London Lancet, Jan., 1859, p. 13.
Recovery, though the patient was a boy of only 14 years.	.....	Arch. Chirurg. Française et Étrang., and Boston Med. and Surg. Jour., 1842, p. 338.
Caries extended to the neighboring bones, and the patient died 9 months after the operation.	This case is reported to have been complicated with tuberculosis, and was probably a tubercular disease of the bones. So far as the arm was concerned, this case was successful, a very useful arm being preserved.	Günther, op. cit.
Recovery. Died 4 years subsequently from redevelopment of the disease in the lumbar vertebræ.	This case was never reported; I am indebted to Prof. A. B. Crosby, M. D., son of the operator, for this account of the case.	
No subsequent history	.....	Edinb. Med. and Surg. Jour., 1836, vol. xlvi., p. 269.
Recovery, and, at last accounts, 30 years after the last operation, the patient was healthy and active.	The malignant character of this disease is sufficiently demonstrated by the fact that it returned twice after operations for its removal. It is, therefore, a case well calculated to encourage the sur	Amer. Jour. of Med. Science, vol. xxi., old series, 1837, p. 390. geon to operate for
Recovery	the removal of cancerous bones. It is reported also that this subject received so severe an injury of the scrot	Günther, op. cit. um at the same time,
Recovery, but died some months subsequently from redevelopment of disease.	same time. This case was never reported; Dr. G. B. of the operator, has furnished these kept no record of this case.	performed at the Twitchell, nephew notes; his uncle

Tabular Statement of the Operations involving the Removal

NO.	OPERATOR.	DATE.	OPERATION.	DISEASE.
17	McClellan, of Philadelphia.	1838	Removed the scapula, arm, and most of the clavicle, at one operation.	Encephaloma in a boy of 17 years.
18	Mr. Travers....	1838	Removed all the scapula below the spine.	"Medullary sarcoma;" the patient was an adult male.
19	Rigaud, of Strasbourg.	1842	Removed the scapula and outer end of clavicle eight months after amputation at the shoulder.	Osteo-sarcoma; patient was a man 51 years old.
20	Dr. Cooper, of British Guiana.	1842	Removed one-third of the scapula and outer third of the clavicle.	Machine injury .....
21	Petrequin, of Lyons.	1844	Removed all the scapula except the glenoid cavity and processes.	Osteo-sarcoma; the patient being a boy of 20.
22	Mussey	1845	Removed the scapula, arm, and outer half of clavicle at one operation.	Osteo-cancer; age of patient not given.
23	Dr. Lewis, of Boston.	1845	Removal of the scapula and clavicle, and a fragment of the arm.	Machine injury, comminuting these bones.
24	Dr. Gilbert, of Philadelphia.	1846	Removal of neck and glenoid cavity, and processes and arm at one operation.	Osteo-cancer; the patient was a medical man.
25	Textor, Jr.....	1846	Removed all of the infra-spinous fossa except a narrow border on its posterior edge.	Enchondroma; the patient was a child of two years and two months old.
26	Mr. Fergusson .	1847	Removed the scapula and outer end of the clavicle three years after amputation at the shoulder-joint.	Caries; the patient was an adult male.
27	Langenbeck....	1848	Removed all the scapula except the glenoid cavity and coronoid process.	Fracture comminuted of the bone; patient was an adult male.
28	Textor, Jr., of Heidelberg.	1849	Removed the greater part of the infra-spinous fossa of the scapula.	Osteo-cancer of large size; the patient was a man of 56 years.
29	Langenbeck....	1850	Removed the scapula except the coronoid process.	Enchondroma softened; the patient was a man of 30 years.
30	Dr. S. D. Gross.	1850	Removed all of the scapula except the head and acromion process.	Osteo-cancer; the patient was an adult male.
31	Hertz, of Erlangen.	1852	Removed all the bone except the glenoid cavity and coronoid process.	Medullary cancer; the patient was a female of 20 years.
32	Barrier, of Lyons.	1853	Removed the infra-spinous portion of the scapula.	Enchondroma of large size; patient an adult.
33	Englehardt, of Riga.	1853	Removed all the scapula except the glenoid cavity and processes.	Necrosis; the patient was a man of 27 years.
34	Langenbeck ...	1855	Removed the entire scapula and three inches of the clavicle.	Osteo-cancer of the encephaloid variety; patient a boy of 12 years.

of all or the greater part of the Scapula—Continued.

RESULT.	REMARKS.	AUTHORITIES.
Recovered from the operation and did well for a time, but finally died of return of the disease 6 months after the operation.	McClellan claimed that, so far as he knew, this was the first case in which the arm, scapula, and clavicle had been removed by the surgeon for disease at a single operation. This was an error, as both Crosby's and Twitchell's cases antedated his.	McClellan's Surg., 1848, p. 412.
The patient died one year after the operation, of a return of the disease.	There is no record pertaining to the state of the arm after the operation in this case. The hæmorrhage attending the operation is recorded as very severe.	London Med. Gaz., 1838; South's Chelius, vol. iii., p. 762.
Recovered, and was enjoying good health 3 years after the last operation.	This case affords as much encouragement to the surgeon as Mussey's case.	London Lancet and Boston Med. and Surg. Jour., 1844, p. 208.
Recovered	This case is reported more in detail among the cases of tearing away from the body of the arm and scapula.	N. Y. Jour. of Med., vol. i., old series, 1842.
Died 25 days after the operation.	.....	Bull. de l'Acad. Imp. 1859–'60, p. 289.
Recovered, and was in good health 9 years after the operation.	It will be remarked that this operation was almost exactly similar to McClellan's, though its results were vastly better.	Gross's Surgery, vol. ii., p. 1041.
Died soon after the operation.	This patient is reported as having died in consequence more of other severe injuries than those involving the arm and scapula.	Statement by letter of Dr. Winslow Lewis, Surgeon, etc.
Died 5 months after the operation, of return of the disease.	It is reported that at a later period this surgeon performed a second almost exactly similar operation, but I have failed to find any record of it.	Amer. Jour. of Med. Sciences, vol. xiv., Oct., 1847, p. 360.
Was discharged well in 40 days with good use of the arm.	This is the youngest subject on whom this operation is recorded to have been performed.	Günther, op. cit.
Recovery perfect and permanent.	.....	Trans. Medico-Chir. Soc. for 1847; also Fergusson's Surg., 1833, p. 309.
Died of pyæmia on the 21st day after the operation.	.....	Günther, op. cit.
Recovered, with good use of the arm, but the disease returned and destroyed the patient.	It is reported that seven distinct operations were performed at various intervals upon this patient for the removal of reappearing cancerous growths.	Günther, op. cit.; Virchow has also published a full history of the case.
Died 17 hours after the operation.	It stands recorded that, in the opinion of the medical men present at this operation, the chloroform had the greater part in the production of the fatal results.	Günther, op. cit.
Recovered. No record of the condition of the arm.	Dr. Gross reports that this patient died of pleuro-pneumonia three months after the operation, and does not think that the fatal disease had any connection with the former disease of the scapula.	Amer. Jour. of Med. Sci., vol. xxv., 1853, p. 348; also Gross's Surgery, vol. ii., p, 1041.
Died of loss of blood an hour after the operation.	.....	Günther, op. cit.
Died within 24 hours after the operation.	.....	Bull. de l'Acad. Imp. 1859–'60, p. 295.
Recovered, and at 4½ months after was healthy.	.....	Günther, op. cit.
Died of a return of the disease 10 months after operation.	At the operation 2½ inches of the clavicle were removed, but, on account of necrosis at the point of division, half an inch more was subsequently removed. No account is given of the degree of usefulness of the arm after the operation.	Günther, op. cit.

Tabular Statement of the Operations involving the Removal

NO.	OPERATOR.	DATE.	OPERATION.	DISEASE.
35	South	1852	Removed all of the bone from the base of the acromion.	Osteo-sarcoma in a man of 30 years.
36	Syme	1856	Removed the entire scapula.....	"Sanguineous cyst of the bone," by others regarded as a vascular encephaloma; patient a female of 70 years.
37	Heyfelder	1857	Removed the scapula and head of the humerus.	Caries in an adult of 40 years, 3 months after resection of the spine of the scapula.
38	Dr. Crawford, of Ayr, England.	1857	Removed the scapula.....	Not stated .....
39	Dr. Carnochan, of New York.	1857	Removed nearly three-quarters of the scapula, the division being in an oblique direction from below upward across the spine.	Caries; the patient was an adult male.
40	Mr. Jones, of Jersey.	1858	Removed the entire scapula and about an inch of the outer extremity of the clavicle.	Caries; the patient was a girl of 15 years.
41	Niepce.....	1860	Removed the arm, scapula, and clavicle.	Machine injury.....
42	Dr. Hammer, of St. Louis.	1860	Removed all of the scapula and three-fourths of an inch of the outer end of the clavicle.	Osteo-cancer; the patient was a girl of 18 years.
43	Mr. Syme	1860	Removed the entire scapula and outer end of clavicle about two inches, the head of the humerus having been removed some time before.	Osteo-cancer; the patient was a man of 43 years.
44	Schuh, of Vienna.	1860	Removed the entire scapula from a child of eight years.	Osteo-cancer ....
45	Dr. Walter, of Pittsburg, Pa.	1860	Removed, by enucleation, all of the scapula except the neck, glenoid cavity, and acromion.	Necrosis; the patient was a boy of 17 years at the time of the operation.
46	Langenbeck....	1860	Removed the scapula and three-fourths of the clavicle, 5 months after amputation, at the shoulder-joint.	Malignant tumor developed from seat of fracture of the humerus, and returned in the scapula and clavicle.
47	Busch, of Bonn.	No date.	Removed scapula and part of clavicle after amputation of the arm.	Osteo-cancer; patient a female of 16 years.
48	Busch, of Bonn.	No date.	Removed the entire scapula except the glenoid cavity.	Cause not stated .....
49	Neudörfer.....	1862	Removed the greater part of the scapula.	Necrosis, following a gunshot comminution of the bone in an adult.
50	Mr. Paget.....	1863	Removed all the infra-spinous portion of the bone except the inferior angle, and also part of the spine.	Osteo-cancer in a boy of 15 years.

of all or the greater part of the Scapula—Continued.

RESULT.	REMARKS.	AUTHORITIES.
Good use of the arm, but at last account the disease was returning.	.....	Times and Gazette for Jan., 1856, p. 37.
Wound healed promptly, and the patient had the best prospect of retaining a useful arm, when she died of old age about 2 months after the operation.	During the progress of the cicatrization the head of the humerus was exposed for some time. This is the first recorded case of removal of the entire scapula in Great Britain.	Syme's Surgery, by McLean.
Died on the eighth day after the operation, of pyæmia.	.....	Günther, op. cit., and Deutsche Klinik, 1855.
Very little blood was lost.....	The report of this case is very meagre..	Times and Gazette, Aug. 8, 1857, p. 155.
Recovery, with good use of the arm.	.....	Oral statement of that surgeon.
Recovered promptly and permanently. Six years after the operation was in good health, with a most useful arm.	This was the first, and, so far as I know, the last case of removal of the entire scapula for caries, preserving the arm, that has been performed in Great Britain.	London Lancet, 1859.
Recovery.....	.....	Bull. de l'Acad. Imp. 1864–'65, p. 723.
Recovered, with a useful arm, but finally died of return of the disease 10 months after the operation.	Dr. Hammer performed a preliminary operation several days before this operation, at which he discovered that the tumor upon the scapula was an outgrowth from the bone. A piece of the mass was removed for microscopic examination, and, having been found cancerous, the whole bone was removed a few days after.	St. Louis Medical Reporter, March, 1866, p. 1.
Recovered, and had a useful arm, and was enjoying good health some years after the operation.	This is the case alluded to by Mr. Fergusson as the *ne plus ultra* of conservative surgery.	Syme's Surgery, by McLean.
At last accounts, which were soon after the healing of the wound, there was free movement of the fore-arm, but the arm could be moved only by aid of the opposite hand. No later history.	.....	Günther, op. cit., and Report of General Hospital, Vienna, 1860.
Recovered, and 6 months after the operation had a perfect use of the arm, and the bone was nearly perfectly reproduced.	Velpeau alludes to a case recorded by Rislau, and another by Chopart, in both of which a reproduction of bone took place after a sequestrum in the scapula had been removed. Dr. Walter states that his patient had a nearly perfect scapula, reproduced by the periosteum left behind.	Philadelphia Medical and Surgical Reporter, 1861, p. 557, Sept.
Died, 1½ years after, of disease of the lungs of malignant character.	.....	Deutsche Klinik, 1860, p. 217.
Recovered from the operation, and 2 years after she earned her living by work in a factory.	....	Allgem. Medic. Central Ztg., 1861, p. 560.
Died of exhausting suppuration.	.....	Langenbeck's Archives.
Recovered, with a useful arm.	.....	Langenbeck's Archives.
Died of return of the disease.	.....	Lond. Lancet, 1863, vol. i., p. 39, Amer. edition.

Tabular Statement of the Operations involving the Removal

NO.	OPERATOR.	DATE.	OPERATION.	DISEASE.
51	Weinker, of Giessen.	1863	Removed all of the scapula except the glenoid cavity and acromion.	Encephaloma; the patient was a young man of 17½ years.
52	Mr. Syme.....	1863	Removed the scapula and outer half of the clavicle with the arm.	Osteo-cancer; the patient was a man of 40 years.
53	Michaux, of Juvain, Belgium.	1864	Removed the entire scapula and no more.	Encephaloma.....
54	Buck, of New York.	1864	Removed the scapula and part of the clavicle.	Osteo-cancer; the patient was an adult male.
55	Fergusson	1865	Removed all of the scapula except the acromion process.	Encephaloma; the patient was a man of 25 years.
56	Fergusson	1865	Removed all of the scapula below the spine.	Fibro-recurrent tumor, involving all of the infra-spinous fossa; the patient was a girl of 19 years.
57	Mr. Pollock....	1865	Removed the entire scapula except the acromion.	Osteo-cancer of large size; the patient was a girl of 16 years.
58	Fergusson	1866	Removed all the scapula left by the operation No. 52, the clavicle, and the arm.	Fibro-recurrent tumor; the patient was 20 years of age at the time of this operation.
59	F. H. Hamilton, of New York.	1866 Feb.	Removed the entire scapula.....	Necrosis from gunshot wound.
60	Dr. Buchanan, of Glasgow.	1866 Dec. 26th.	Removed the entire scapula.....	Injury which carried away the arm at the shoulder-joint; exposed and comminuted the scapula; girl of 17 years.
61	Fergusson	1867	Removed the scapula, part of the clavicle, and the arm, at one operation.	Osteo-cancer; this patient was a man of 40 years.
62	Stephen Rogers, of New York.	1867	Removed the entire scapula and no more.	Osteo-cancer of the encephaloid variety in a girl of 7 years.
63	Sidney Jones, London.	1868	Removed all of the scapula except the acromion.	Enchondroma in a man of 43.
64	E. Krakowizer, of New York.	1868	Removed the scapula 5 years after amputation, at the shoulder-joint.	Enchondroma.....

of all or the greater part of the Scapula—Continued.

RESULT.	REMARKS.	AUTHORITIES.
Died of exhaustion within 24 hours after the operation.	This case was the theme of an inaugural dissertation at Felsing in 1863.	
Recovered, and was in good health some years after the operation.	.....	Syme's Surgery, by McLean.
Recovered, with a useful arm, but the disease returned, and the patient died 10 months after the operation.	.....	Gazette Médicale, 1866.
Recovered, but a few months after died of a return of the disease.	This patient had previously lost the arm by amputation for the same disease, and in respect to the number of operations for diseased growth it resembled case No. 14 of this table.	Oral statement of that surgeon.
Recovered from the operation, and at the end of the first month had considerable antero-posterior movement of the arm. There is no record of the case after that date.	Fergusson claimed this to have been the third case of removal of the entire scapula in Great Britain and London. He opened the joint first in front. This experience led him to say that, were he to perform the operation again, he would isolate the posterior portion of the scapula first, opening the joint from behind first, leaving the anterior portion of the incision and disarticulation to the last. He also advocated the leaving of the acromion when it is not diseased, as tending to reduce the deformity.	Lond. Lancet, 1865, p. 524, American edition.
Recovered from the operation promptly, with good use of the arm.	This patient finally became the subject of the operation described as the 56th one of this table.	Times and Gazette, 1866.
Recovered from the operation, and had a useful arm 11 weeks after the operation, date of last report of her.	This was precisely the operation of Fergusson, with his suggestion of isolating the tumor from behind first carried into practice by Mr. Pollock.	Lond. Lancet, 1865, p. 526, American edition.
Recovered from the operation, but there is no subsequent history.	.....	Times and Gazette, 1865.
Recovered with useful arm...	Two months after the operation, there was no observable reproduction of bone, though the periosteum remained in its place. The bone was detached from the periosteum and tissues, so that it was very easily removed.	Communicated by that surgeon.
Little loss of blood, but the patient died within 24 hours after the operation, of shock.	It is similar to Dr. Cooper's and Dr. Lewis's cases.	Times and Gazette, Jan., 1867.
Died of shock three days after the operation.	This operation was like McClellan's, as to extent, but its unfortunate termination was probably induced by the condition of low vitality of the patient, indicated by the fatty degeneration of the heart, kidneys, and liver.	Lond. Lancet, Nov. 2, 1867, p. 552.
Recovered from the operation, but died of return of the disease 6 months after the operation.	This patient had good use of the arm till its movements were impeded by new cancerous growths about the shoulder. Patient died of apnœa from the pressure of mediastinal cancer.	Amer. Jour. of Med. Sciences for Oct., 1868.
Died on the fourth day after the operation.	Not more than four ounces of blood were lost, and the death from exhaustion was suspected to be, in part at least, owing to the chloroform used.	Lond. Lancet, Nov. 21, 1868, p. 665.
Died 7 days after the operation, of exhaustion.	This case is interesting as one of recurrent enchondroma in a bone not affected by the first tumor.	Communicated by that surgeon, who will report in future in detail.

The following abstract from the table will exhibit an outline of the ten, and only cases, known to the history of surgery, in which the entire scapula was removed, with preservation of the arm:

Langenbeck, in 1855, removed the entire scapula and three inches of the clavicle.

Syme, in 1856, removed the entire scapula, and no more.

Heyfelder, in 1857, removed the scapula; at the same operation the head of the humerus was also removed.

Jones, in 1858, removed the entire scapula and about an inch from the outer end of the clavicle.

Hammer, in 1860, removed the entire scapula and about three-fourths of an inch from the clavicle at its acromial end.

Syme, in 1860, removed the entire scapula and outer end of the clavicle, the head of the humerus having been removed at a previous operation.

Schuh, in 1860, removed the entire scapula, and no more.

Michaux, in 1864, removed the whole scapula, and no more.

Hamilton, in 1866, removed the entire scapula, and no more.

Rogers, in 1867, removed the whole scapula, and no more.

The five following cases are, however, practically equivalent to the above:

Langenbeck, in 1850, removed all of the scapula except the coronoid process.

South, in 1852, removed all of the bone except the acromion process.

Fergusson, in 1865, removed all except the acromion.

Pollock, same year, removed all except the acromion.

Sydney Jones, case quoted at length above, is similar to the three last mentioned.

Miscellaneous and Scientific Notes.

The Infants' Hospital.—This institution—temporarily located in one wing of the Inebriate Asylum, until the building now erecting on Randall's Island shall have been completed—has been, by the action of the Commissioners of Public Charities and Corrections, separated from the control of the Charity-Hospital staff, and placed in charge of a special medical board, consisting of Drs. Austin Flint, Sr., Geo. T. Elliot, Isaac E. Taylor, Abram Jacobi, Lewis Smith, and E. S. Dunster. Dr. F. A. Castle is the House-Physician.

Supposed Death from Chloroform.—Dr. Van Buskirk, of Gorham, Ohio, was found dead in his bed recently. Death was supposed to have been caused by the inhalation of chloroform taken to relieve a nervous headache.

At the anniversary meeting of the New-York Academy of Medicine, held at the College of Physicians and Surgeons, December 10, 1868, Prof. Austin Flint, Jr., delivered the annual address. His subject was, "The Past, Present, and Future of Medicine," and was presented in an unusually interesting manner. The address is to be published by the Academy.

Medical Journal Association.—The programme for January, 1869, is as follows:

January 8.—"Treatment of Aneurism of the Arteria Innominata." By Prof. H. B. Sands, M. D.
" 15.—"Anæsthetics and their Administration." By D. H. Goodwillie, M. D.
" 22.—"Embolism in the Eye." By Hermann Knapp, M. D.
" 27.—"Résumé on Insanity." By J. C. Smith, M. D.

Dr. Isaac Cummings, late House-Physician of the Demilt Dispensary, died suddenly at his residence, December 15, 1868.

At a meeting of the physicians attached to the Dispensary, held on the 16th December, 1868, with the object of uniting in an expression of their sentiments regarding the death of the late house-physician of that institution, Dr. Isaac Cummings, the following preamble and resolutions were adopted:

Whereas, it has pleased Almighty God to remove from us by death our late colleague, Isaac Cummings, M. D., while yet in the prime of life, and at a period of greatest usefulness: we, therefore,

Resolve, that we recognize in this dispensation of Providence the loss of a highly-esteemed friend and valued counsellor; that the Demilt Dispensary has in him lost one of its oldest and ablest officers; that the poor who seek its benefits have lost a good and skilful physician; and that the profession has lost an eminent member.

Resolved, that we sympathize with his family in their bereavement, as well as in the affliction of those who will lose his uniform kindness and honest advice.

Resolved, that these resolutions be published in the medical journals and in the daily papers of this city; that a copy of the same be sent to the family of the deceased, and to the Board of Managers of the Demilt Dispensary.

Editor New York Medical Journal—

Dear Sir: The following report of a very rare accident has been sent to me by Dr. Marsh, U. S. A., who is stationed at West Point, New York, he having, at my request, obtained the consent of the Surgeon-General to its publication. Dislocation of the metacarpal bone of the thumb is pretty common in either direction—dislocations of the other metacarpal bones backward are also not very rare, but a dislocation of the metacarpal bones of the fingers *forward* is exceedingly rare. I have seen but one other case reported. Yours truly,

F. H. Hamilton.

Forward Dislocation of the Proximal End of the Second Metacarpal Bone.—April 1, 1868.—Charles C——, corporal of ordnance department, aged 26 years, was holding a drill in his right hand for another man to strike, when suddenly the hammer-head flew off, and struck a glancing blow on the back of his hand. The hand was covered with a thick buckskin glove, and the hammer-head weighed about seven pounds. His hand was rendered helpless by the blow, and, on taking off his mitten, he noticed an irregularity in its appearance. I saw him about half an hour after the accident. There was already some swelling, but not sufficient to conceal a marked depression in the back of the hand over the carpal end of the second metacarpal bone. The bone was felt running down toward the palm, and on a lower (more anterior) level than the next metacarpal bone. It was fixed in this position, and no crepitus could be detected. As there was evidently a dislocation, I attempted to reduce it by making extension from the finger, and at the same time pressing with my thumb in the palm of the hand against the base of the metacarpal bone. It almost immediately, and with a snap, returned to its proper place, and the deformity disappeared. I then bandaged the hand with a ball of tow in the palm. The swelling gradually subsided, and on April 13th he was able to use the finger sufficiently to return to duty, although its full strength did not return for some time longer.

The case was seen also by Surgeon J. F. Head, U. S. A.

Sulphuret of Potash in Whooping Cough.—Dr. Mackelcan calls attention, in the *Dominion Medical Journal*, to this remedy. He was led to the use of it from a notice of it which he saw in a French medical journal forty years ago, and since

that time his success has been so uniform that he has used no other remedy. He says:

"The mode of administration I have adopted is to dissolve it in a mixture of syrup and distilled or rain water, in the proportion of one of the former to three parts of the latter (hard water, which sometimes contains sulphuric acid, partially decomposing it), the dose being one grain for each year up to four years of age, and after that half a grain additional for each year; the smaller doses being administered in a teaspoonful of fluid, and the larger more diluted, in proportion to the quantity of the salt in each dose.

"Its beneficial effects are not perceived for five days, when the intervals between the paroxysms of cough become longer, and after that their violence diminishes from day to day, until at the end of ten or fourteen days it is seldom necessary to pursue the treatment further.

"As the drug easily spoils by keeping, it is important to have it fresh. If it dissolves perfectly in the syrup and water, and the mixture is of a greenish color, it may be relied on; but if there is any sediment, it has been decomposed by exposure to air, and becomes a sulphate."

Another Death from Chloroform.—Dr. E. A. Clark, Resident Physician of the St. Louis City Hospital, reports, in the *Humboldt Medical Archives*, another death from the inhalation of chloroform, the *first* serious accident witnessed by him in several years of hospital practice. The patient, 38 years of age, had hæmorrhoids with prolapsus of the rectum which had become strangulated externally. The tumor being so painful that no manipulation could be performed, it was determined to bring the patient under the influence of chloroform. He was apparently in the full vigor of health, and an examination of the heart and lungs before the inhalation of the anæsthetic revealed no abnormal condition. The chloroform was administered "by pouring about a drachm upon a napkin, folded in such manner as to admit the admixture of a sufficient quantity of air." After the inhalation had been continued for a couple of minutes, manipulation of the tumor showed that the patient was still susceptible to pain. An additional drachm of chloroform was, therefore, poured upon the napkin. Soon the patient began to be affected with the spasmodic movements not infrequently observed in partial

anæsthesia. They were not, however, severe or protracted, and soon gave way to relaxation, and the chloroform was then discontinued. The rest of the report we give in the doctor's own words: "I observed that his face was more livid than usual, but the favorable condition of his pulse and respiration decided me to proceed with the operation. The patient was turned upon his right side in order to place him in the most favorable position for manipulating the tumor, but in about one minute from the time the chloroform was withdrawn, it was observed that he had ceased breathing, and that the pulse, both at the wrist and carotids, was imperceptible, while the superficial vessels were full and distended, and the face of a dark livid color. He was immediately placed upon his back, the head lowered, and cold water dashed upon the face and chest, with the effect of causing three or four long, full inspirations, without, however, affecting the circulation in the least. We then commenced a vigorous artificial respiration, at the same time withdrawing the tongue from the mouth and elevating the epiglottis with the finger.

This was continued without any relaxation for an hour and forty minutes, but without in the least reviving the action of the heart, which, I am confident, never beat again from the moment that natural respiration ceased; he was dead from that instant.

The *post-mortem*, made eighteen hours after death, revealed a considerable serous exudation beneath the arachnoid, which was doubtless the result of the venous congestion of the brain which was found to exist; the ventricles were empty, and in every way normal, except a congested condition of the choroid plexus; the heart was found to be entirely empty, containing scarcely a drop of blood, while all the valves, as well as the walls of the heart, were in a perfectly healthy condition. The lungs were likewise healthy, presenting, however, some hypostatic congestion on their posterior surface.

From the above detail of facts, the immediate cause of death in this case would seem indeed obscure, though probably not more so than in others not attributable to organic lesions. From, however, the empty condition of the heart, it would be most reasonable to suppose that death was the result of a sudden spasmodic contraction of the heart, which continued until life was extinct, this probably being the effect of the anæsthetic upon the ganglionic nervous system. At all events, we can hardly suppose it to have been the result of syncope, as is generally thought to be the rationale of most cases of death occurring from the administration of chloroform. This seems to be apparent in the present case, from the congested condi-

tion of the blood-vessels of the brain, as well as from the fact that we failed to resuscitate him by lowering the head beneath the level of the body, a method so generally successful, where a condition of syncope is supposed to exist, in cases threatening death.

Deputy-Inspector-General Longmore, of the British service, professor of military surgery in the army medical school at Netley, has been appointed honorary surgeon to her Majesty the Queen, vice Dr. Melvin, Inspector-General of Hospitals, deceased.

Here is an item, either hygienic or moral—as you choose to look at it—which we think should be set down to our credit. Will our suburban friends, who are constantly complimenting New York as the embodiment of every thing that is rascally and wicked, make a note of it?—

"At a recent meeting of the Metropolitan Board of Health, the sanitary superintendent of Brooklyn reported that he had made careful microscopical examination of twelve specimens of bread purchased at as many bakeries in New York city, and had found them all to be without any deleterious constituents. The bread was taken from among that on regular sale, and was obtained from shops in the poorer localities of the city, such as Cherry, Mulberry, Sixth, and other streets of that character. The bread was afterward delivered to Prof. Chandler, who will make a chemical analysis of it, and report to the Board."

Artificial Sulphate of Quinine.—A young chemist, attached to the Venereal Hospital of Paris, claims to have discovered a process for manufacturing this substance. He keeps the mode of preparation secret, but states that it is obtained from mineral substances like urea or alcohol, which have both been synthetically manufactured from inorganic materials.

This preparation appears in the form of a white crystalline powder, the taste of which offers the peculiar bitterness of sulphate of quinine. The crystals, when viewed under the microscope, are shorter and thicker than those of the ordinary sulphate, although otherwise similar in form. Its chemical composition is also said to be slightly different; it is a tribasic salt of quinine—that is to say, it contains three equivalents of quinine for one of sulphuric acid. It is fairly soluble

in water, but cannot be dissolved in dilute sulphuric acid, like the other sulphate; it is therefore generally prescribed as a powder. Several experiments have been tried with it, in cases of intermittent fever more especially, and it is said to act favorably. But the most important feature in its history is that it only costs half the price of our ordinary sulphate of quinine. If, therefore, all this is not a hoax (and pharmaceutical ingenuity has rendered most physicians skeptical on these subjects), the inventor will undoubtedly realize a handsome fortune, besides rendering an invaluble service to the poorer classes.—*Medical Times and Gazette.*

The *Medical Times and Gazette* thus comments on our remarks entitled "A Point of Ethics," in the July number of the *Journal:*

"By an editorial note in the *New-York Medical Journal* we learn with great surprise that a journal which occupies so deservedly a high position and enjoys so large a circulation as the *American Journal of Medical Science* steadily refuses the insertion of any advertisements relating to books written by authors, however eminent, if these are published by any other house, however respectable, than that of its own proprietors, Messrs. Lea, of Philadelphia. The writer well observes:

'All members of the profession are entitled to expect that each medical journal shall consent to advertise medical works written by proper men, and published by houses of respectability other than that responsible for the journal in question. The refusal of such advertisements, and their exclusion from a medical journal aiming to represent American medical literature, must show a spirit at variance with that exalted purpose which should give the tone to all medical journals, and even impair confidence in the fairness of reviews of publications of rival houses.'

"Well, we certainly in this matter exhibit a more liberal spirit than our American cousins. Nowhere can the competition of medical and literary journals be more keen than it is in London, but certain it is that no publisher who might be proprietor of any of them would ever dream of depriving a rival of his legitimate publicity. With regard to the American journal in question, the case is especially hard, as it is almost the only one that circulates everywhere, all over the Union and in Europe. For ourselves, we have often felt the want of complete lists of new American books similar to those with which we are so abundantly furnished of our own productions."

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ART. I.—*On the Mechanism of the Crepitant and the Subcrepitant Râle.* By AUSTIN FLINT, M. D., Professor of the Principles and Practice of Medicine in the Bellevue Hospital Medical College.

(Read at a meeting of the New York Medical Journal Association, December 13, 1868.)

MY chief object in this paper is, first, to give an account of the manner in which the crepitant and the subcrepitant râle may be artificially produced, using for this purpose a new commodity called the "patent india-rubber sponge;" and, second, to show that the crepitant râle may be proved, by means of this article, to be caused by the sudden separation of coherent surfaces, and not by bubbling. Before proceeding, however, to this twofold object, it will be proper to notice the opinions entertained respecting the mechanism of these râles, from the time of Laennec, and at the present moment.

In the immortal treatise by Laennec on "Mediate Auscultation," the crepitant râle is described under the name, "the moist crepitant râle or crepitation." Laennec likened the sound to that caused by the crackling of salt when thrown into a heated vessel; also to that which is heard when a dried bladder is inflated, or, as he says, "still better," to the crepitation of healthy inflated lung when it is pressed between the fingers. He adds that the crepitating sound is accompanied by a well-marked sensation of moisture. He attributes the sign to the bubbling of liquid in the air-cells, the bubbles seeming to be extremely small. Bubbling which in a greater degree conveys the sensation of moisture, and which appears to be caused by larger bubbles, he calls the subcrepitant râle. This is the account in the first part of the work, under the heading, "Auscultation of Râles." In the chapter on "Peripneumonia," of which affection he considers the crepitant râle a pathognomonic sign, he says, "it gives the impression of very small bubbles, of equal size, and it seems to have very little moisture." A discrepancy is here apparent. In his first account he says the "sensation of moisture is well marked," "*bien marquée.*" In the second account he says the sign has very little moisture, "*il paraît très-peu humide.*" The discrepancy, probably, goes to show that the accuracy of his observation in this instance was affected by his theory of the mechanism, viz., that it is caused by bubbling. Considered without any reference to mechanism, the sound is undoubtedly dry; and it is now distinguished from the subcrepitant râle, as a *dry*, crackling sound. Laennec did not distinguish with precision the crepitant from the subcrepitant râle. He did not limit the former to the act of inspiration,

which was first done by Dance. This limitation is now generally admitted to be one of the distinctive features of the crepitant râle. The equality of the crepitations, which was pointed out by Laennec, is another distinctive feature of this râle; and additional distinguishing characters of it are its rapid evolution, and its being heard at the end of inspiration whenever it accompanies a portion only of the inspiratory act.

The significance and value of the crepitant râle, as a diagnostic sign, were much discussed by Laennec's commentators, but the theory of the mechanism was, so far as I know, first called in question by M. Beau, who attributed it to the *friction of the pulmonary vessels*, rendered morbidly dry (*desséchées*) by incipient inflammation. As will be presently seen, M. Beau was not very far from the true explanation, but he failed in reaching it.

In 1842, five years after the publication of the fourth edition of Laennec's treatise, edited by Andral, the late Dr. Edson Carr, of Canandaigua, New York, communicated a paper for the *American Journal of Medical Sciences*, entitled "Suggestions as to the Cause of the Crepitant Rhonchus." This paper occupies only two and a half pages of the *Journal*. After referring to the hypothesis of Beau, Dr. Carr considers that in the first stage of pneumonia the air-vesicles contain a certain quantity of thick and tenacious matter which he calls mucus, by means of which the walls of the vesicles, at the end of the act of expiration, become coherent, or glued together. He then proceeds to explain the mechanism of the crepitant râle, as follows: "Now, during every *inspiration*, the air, rushing into and distending the vesicles, necessarily over-

comes these cohesions; and would not the separation of these membranes, thus glued together by tenacious mucus, naturally produce precisely such sounds as constitute the crepitant râle of pneumonia?" He adds: "A great variety of experiments might here be introduced to illustrate this view of the phenomena. One of the most simple is that of moistening the thumb and finger with very thick paste or mucilage, and alternately pinching them together and separating them, when held near the ear. By repeating and modifying this simple experiment, sounds may be produced so perfectly imitating the crepitant râle of pneumonia, that I am persuaded no one who will take the trouble to try it will doubt this explanation of its cause."[1]

Dr. Carr, whom I had the pleasure of knowing personally, was a man of a high order of mind; and he was distinguished among his associates for his professional attainments. Notwithstanding his comparatively limited opportunities for clinical observation, he cultivated zealously, and with much success, the study and practice of auscultation; and, in a paper read at a medical society meeting, he anticipated Skoda in the application of the principle of consonance to the explanation of certain auscultatory phenomena. The article on the crepitant râle, in 1842, is the only published paper by Dr. Carr, on this subject, that I know of, although his death did not take place for twenty years or more after the date of that publication.

Dr. Carr's short paper attracted very little attention. I do not recollect ever having seen a reference to it in any foreign work on auscultation. In a con-

[1] It is a noteworthy fact that all the examples suggested for the illustration of this râle afford no support to the bubbling theory. In addition to the illustrations cited are, the expansion of a dry sponge after it has been compressed, and rubbing a lock of hair close to the ear.

versation with Robin, in Paris, in 1854, he told me he had never heard of the crepitant râle having been explained in that way, and he at once declared the explanation to be, in his judgment, the correct one; but he did not remember to introduce the explanation in the subsequent editions of Nysten's Dictionary, of which he is the editor. From the date of Dr. Carr's article to the present time, as previously, and inclusive of the present time, most writers on auscultation have been satisfied to adopt the theory of bubbles originally offered by Laennec, notwithstanding the difficulties in the way of reconciling this theory with certain obvious facts, namely, the absolute dryness of the sound, and the limitation of the râle to the act of inspiration.

The writings of Walshe furnish an exception to the statement just made. Impressed with the difficulties pertaining to this theory, Walshe, in the second edition of his work, published in 1854, considers two other hypotheses. One of these attributes the râle to "the sudden expansion of the cells, unfolding with crackling noise minute portions of exudation seated between the vesicles in the actual parenchyma." The other attributes it to "the mere abrupt unfolding of the vesicles themselves." He concludes that the theory of Laennec is "most probably the true one." In a foot-note he admits that "the bubbling theory fails to explain the limitation of the rhonchus to inspiration," and decidedly expresses his dissatisfaction with this theory. In the third edition of his work on diseases of the lungs, which appeared in 1860, this author enters into a fuller consideration of the mechanism of the râle. He expresses still more decidedly his dissatisfaction with the theory of bubbling, and he is disposed

to believe that the production of the râle within the vesicles may "legitimately be questioned." He concludes, as the most probable supposition, that it occurs in the parenchyma of the lung itself, that is, in the intervesicular structure, and that "its physical cause is the sudden and forcible expansion of the parenchyma, glued together, as it were, by the viscid exudation with which it is infiltrated." He admits, in conclusion, that this explanation "is not wholly unopen to objection." In this consideration of the subject, Walshe comes so near the truth, that it seems to me certain he could not have read either Dr. Carr's article or the account of his explanation given in a work, an extract from which I beg permission to introduce in this connection.

In the first edition of a work on physical exploration, witten by the author of this paper, and published in 1856, Dr. Carr's explanation of the crepitant râle is adopted in the following terms: "The most rational theory, and the one which meets best the objections to that of Laennec, was offered several years ago by Dr. Carr, of Canandaigua, N. Y. Dr. Carr attributes the production of the sound to the abrupt separation of the walls of the cells which had become adherent by means of the viscid exudation incident to the early stage of inflammation. · · · · · A viscid exudation within the cells and bronchioles belongs among the local phenomena of the disease (pneumonia); and as it is not readily removed by expectoration, but accumulates until the cells are filled, and the lung solidified, the constancy of the râle for a certain time is intelligible. Its occurrence with inspiration only is fully explained by this theory. The conditions for the production of the sound are only present after the

lungs have collapsed with expiration, when the agglutinated walls of vesicles and bronchioles are separated with the expansion of the lung by the inspiratory act. Adopting Dr. Carr's explanation, it would be expected, as observation shows it to be true, that the sound would be present in the early stage of pneumonitis, the air in this stage still entering the vesicles, and subsequently cease, nearly or entirely, in proportion to the extent of the completeness of the subsequent solidification. The fact that, when solidification has taken place, a certain number of cells are not filled with the morbid exudation, and remain in the condition which characterizes all the cells in the early stage, explains the persistence of the râle in some cases during the second stage of pneumonitis, and its being developed, under some circumstances, by forced inspiration and especially at the end of the inspiratory act. The theory of Dr. Carr is also equally applicable to cases of œdema and hæmoptysis, in which the crepitant râle is observed. In these affections the vesicles contain a liquid which is glutinous, although in a less degree than in pneumonitis; and we can readily understand that the necessary physical conditions are present sometimes, but not constantly, on account of the greater facility with which the liquid escapes from the cells into the bronchial tubes, thus giving rise to the bubbling râles—the subcrepitant and mucous."[1]

It would be affectation not to admit that I derive a certain degree of personal gratification in the demonstration of the correctness of Dr. Carr's explanation, from

[1] This extract is from the second edition, in which a few verbal alterations were made, but the sense in no respect altered. In the second edition I added to the vesicles the bronchioles as the seat of the crepitan râle.

the fact that no other work on auscultation, within my knowledge, has adopted this explanation. Of prominent contemporaries I may name Barth and Roger, Fuller, Gerhard, Bowditch, Hughes, Blakiston, Latham, Gairdner, and Skoda (London edition, 1853), as adhering to the bubbling theory.[1] Gerhard, in his treatise on the chest, edition of 1846, attributes the râle in part to bubbles, and in part to "the dilatation of the thickened and stiffened vesicles." My friend Dr. Loomis, of this city, in his recent publication, entitled "Lessons on Physical Diagnosis," after stating that there are two views as to the mechanism, viz., one attributing it to bubbling, and the other to the separation of the walls of the air-cells glued together by a viscid secretion, expresses the opinion that "it may probably be produced in both these ways." He introduces, however, a diagram from the work on diagnosis, by Da Costa, in which bubbles are figured within the air-cells. In the late work on Internal Pathology and Therapeutics, by Niemeyer, I find the following sentence: "Perhaps it (the crepitant râle) originates in the following manner: the alveolar walls are glued together by a viscid exudation during expiration, while they are forcibly separated by the entrance of air during inspiration."[2]

Referring to other late works on the practice of medicine and to works on diagnosis, Aitken, in a table of the Râles, Rhonchi, or Rattles, gives the mechanism of crepitation as follows: "Probably due to the sud-

[1] This statement requires modification. Since writing this paper, the fact that certain distinguished German authors on auscultation have adopted the explanation by Carr, and attributed to him priority, has been brought to my notice by my friend Prof. A. Jacobi. Vide Appendix to this paper.

[2] Quoted from the French translation, Paris, 1865.

den and forcible expansion of delicate tissue, altered in its physical properties by the inflammatory state, and which probably undergoes minute ruptures." This language is not very clear. I presume that the author means to adopt the explanation by Walshe. Under the head of pneumonia, however, he uses this expression, "The bubbles composing the pneumonic crepitation." Tanner says nothing respecting the mechanism. I do not find any opinion as to the mechanism in Bennett's Practice. In Wood's Practice (third edition) it is stated that the râle may depend on "the successive bursting of very minute bubbles," or "as suggested by Dr. Carr, of Canandaigua, N. Y., upon the separation of the walls of the cells agglutinated by the plastic exudation." Dickson (second edition) does not give any explanation. Watson ascribes it to bubbling. Maxson gives the same explanation. Trousseau, in his "*Clinique Medicale*," waives discussion of the different theories relating to the mechanism. Da Costa, in his work on Diagnosis, as implied already, adopts the bubbling theory. In the "*Traité de Diagnostique Medicale*," par V. A. Racle, the mechanism is not considered. Finally, in the article on Auscultation, by Alfred Luton, in the "*Nouveau Dictionnaire de Médecine et de Chirurgie*," now in course of publication in Paris, I find Dr. Carr's explanation presented and adopted, but the explanation is credited to a French writer, J. Parrot, and to Wintrich, in Germany. The latter, who had precedence in point of time, is stated to have given the explanation in 1854, twelve years after the publication of Dr. Carr's paper. After referring to the idea of Beau, that the râle is due to the friction of the cell-walls which are morbidly dry, Luton says: "This idea was conjectured, but not proved. At a later date, J. Parrot

extended this idea, and rendered it more probable by supposing that during inspiration the air tends to separate the walls of vesicles, these walls having in expiration come into contact and become agglutinated by means of the bloody mucus which is characteristic of pneumonic expectoration; in fact, a sound like the crepitant râle can be obtained by the separation of two surfaces to which a viscid matter has been applied. This theory, which has also been proposed in Germany, by Wintrich (1854), seems to us very acceptable; it is the only one which accounts for all the peculiarities of the râle. The râle, however, should not be referred exclusively to the pulmonary vesicles, but, as stated by Wintrich, the minute bronchial tubes, into which the cells open, taking part in its production."

The opinions which have been noticed relate to the mechanism of the crepitant râle. As regards the subcrepitant râle there have not been differences of opinion. That this is a bubbling râle no one can doubt. The character of the sounds, their occurrence in both inspiration and expiration, the inequality of the sounds which enter into the râle, and the comparative slowness of their evolution, constitute adequate evidence of bubbles. I shall, however, presently show that this explanation of the mechanism may be demonstrated to be the correct one.

I will introduce here a clinical fact which has not been sufficiently considered; and the imperfect appreciation of this fact, as it seems to me, has occasioned not a small share of the confusion respecting the differential characters of the crepitant and the subcrepitant râle. The fact to which I refer is this: These two râles are often found in combination; in other words, associated with the very fine crackling sounds of the

crepitant, are more or less of the fine bubbling sounds of the subcrepitant râle. If both these râles be attributed to bubbling, and the differential characters to the difference in size of the spaces in which the bubbles burst, it would hardly be expected that there would be a sharp line of demarcation between the two. It is usual to say, not that the two râles are combined, but that the one merges into the other. The difference in the mechanism being demonstrated, an essential difference in the sounds is established; the one cannot be converted into the other, but the two may be united in varying proportions. Whenever a "sensation of moisture" (to use the language of Laennec) is apparent, notwithstanding the distinctive characters of the crepitant râle are present, there are bubbling sounds in addition to crepitation. It is owing to this combination that some writers have been led to state the occurrence of crepitation sometimes in the act of expiration. Not infrequently, in the resolving stage of pneumonia, both râles are present very obviously, the subcrepitant preceding the crepitant râle in the act of inspiration, and present alone in the act of expiration. I have for many years been accustomed to demonstrate the combination of the two râles at the bedside; and in the artificial exemplification of the subcrepitant râle, which I shall presently give, it will be seen that with it is associated crepitation, unless pains are taken to prevent the production of the latter.

After this introduction, which I fear may have seemed protracted, but which, as it seems to me, is appropriate, I proceed to the object of this paper as stated at the outset.

For my knowledge of the artificial production of the crepitant râle in the way I am about to describe,

I am indebted to my friend and associate, Dr. Henry F. Walker. Dr. Walker happened to purchase an article labelled "Patent India-rubber Sponge," which is designed to take the place of the ordinary sponge for the toilet. This article consists of a block of india-rubber which has been made to assume a cellular arrangement, evidently by the introduction of air or gas while the substance is in a liquid state and during its congelation. On examining the article, it will be seen to be made up of cells of unequal size, the appearance being very like that of a portion of emphysematous lung. The elasticity of the india-rubber causes the article to expand after it has been compressed, the well-known cohesiveness of this substance offering a certain amount of resistance to the expansion. Now, after having examined the structure, if each one present will compress with the fingers the article which I shall ask you presently to pass around, holding it close to the ear, and then allow it to expand, it will be at once perceived that a crepitant râle is beautifully represented. The fineness and the dryness of this râle are perfectly exemplified. It will be observed that the compression of the article causes no sound. This act of compression is to be considered as taking the place of expiration. The expansion is analogous to the movement of the lung in inspiration. The compression brings the walls of the cells into contact, and, from the adhesiveness of the substance, they cohere with a certain amount of force. There being no liquid present, the râle must be produced by the separation of the cell-walls by the elasticity of the substance. The intensity of the crepitation will be found to be proportionate to the force of the compression, the cell-walls being brought more

completely into contact and the cohesion being greater according to the amount of compressing force.

Purchasing the article for another purpose, Dr. Walker was led to notice this unexpected application, and being associated with me in giving practical lessons in auscultation, he called my attention to its usefulness, as affording an exact representation of the crepitant râle preparatory to the demonstration of this sign in cases of pneumonia. It is indeed highly useful for this purpose. But, in addition, it serves to demonstrate that the crepitant râle in cases of disease is produced, not by bubbling, but by the separation of the coherent walls of the cells and bronchioles. The representation of this sign is so complete by means of this article, that I do not see how any one can doubt that the mechanism is the same. I assume, therefore, that the explanation of the crepitant râle, published more than twenty-five years ago, by our countryman Carr, is proved to be the true explanation; and I claim in behalf of his memory the credit of the explanation which, by the author of the article on Auscultation, in the new French Dictionary of Medicine and Surgery, is accorded to others. As a friend of the late Dr. Carr, I cannot but have a feeling of regret that he did not live to see the correctness of his explanation established. As it is, justice to his memory, in respect of the originality and priority of the explanation, will be gratifying to numerous friends who held him in high esteem for his professional attainments and private worth.

The production of the crepitant râle, in the manner now illustrated, demonstrates the error of attributing the fineness of the râle to the small size of the cells. The fineness is not less marked when produced by the india-rubber sponge than when it emanates from the

pulmonary vesicles and bronchioles. Dr. Carr's mode of illustration, by pressing together and separating the finger and thumb moistened with thick paste or mucilage, also demonstrates this error.

In concluding my remarks on the crepitant râle, I will refer to an explanation of the peculiar quality of the inspiratory sound in the normal vesicular murmur, which, so far as I know, is original with me. Quoting from my work on physical exploration (2d ed., p. 133), I say with reference to this point as follows: "May not the peculiar quality (called the *vesicular quality*) be owing to the separation of the walls of the cells, or bronchioles, which, to a greater or less extent, are in contact, and, owing to the moisture of the tissues, become slightly adherent during the partial collapse of the lung at the end of an expiration? We shall see hereafter that this is the most rational explanation of an important and highly distinctive physical sign of disease, namely, the crepitant râle. The fact that the air does not circulate freely in the air-cells and bronchioles with each inspiratory act, renders probable the explanation suggested by the foregoing inquiry. Other facts supporting this explanation are, the increase of this peculiar quality of sound in the inspiratory act which succeeds a forced expiration in the act of coughing; the diminution of the quality in cases of permanent dilatation of the air-cells, or emphysema, and the limitation of this quality to the inspiratory sound." In view of the demonstration of the mechanism of the crepitant râle, the correctness of the explanation of the vesicular quality in normal respiration, which is offered in the foregoing quotation, seems to me extremely probable. We have the crepitant râle in pneumonia, because the air-vesicles and bronchioles are glued to-

gether at the end of expiration by a viscid morbid product. We have a slight approach to this râle in health, in the vesicular quality of the inspiratory sound, because the air-vesicles and bronchioles are very slightly coherent at the end of expiration. The peculiar quality of the inspiratory sound in the normal vesicular murmur, Laennec compared to a "slight crepitation," but in view of his theory of the crepitant râle he could not, of course, suppose that the mechanism of this slight crepitation and of the crepitant râle is the same.

A good imitation of the vesicular quality in the inspiration of health is obtained by wetting a fine ordinary sponge, squeezing it as dry as possible, then compressing it and allowing it to expand close to the ear. The liquid may be so effectually squeezed out of an ordinary sponge that there is not enough left for bubbling; but the moisture occasions a very slight cohesion of the cells when pressure is made, and the expansion gives such an approach to crepitation as constitutes the vesicular quality in the normal vesicular murmur. This mode of representing the vesicular quality goes to prove its mechanism.

Concerning the mechanism of the subcrepitant râle, as already stated, there is no difference of opinion. This râle is produced by the bubbling of liquid; it is therefore essentially different from the crepitant râle. The article which, as has just been seen, represents crepitation, may be used to exemplify the subcrepitant râle, and to illustrate certain points relating to the differentiation of the two râles. The application of the "india-rubber sponge" to show the mechanism of the subcrepitant râle was suggested by Dr. William J. Chandler, one of the house physicians at Bellevue Hospital. If a portion of the "sponge" be compressed

and allowed to expand under water, the cells are filled with liquid; and, now, holding it close to the ear and alternately pressing it and relaxing the pressure, fine bubbling sounds are produced. That bubbling is caused by the pressure, is shown when the portion of "sponge," of the cells filled with liquid; is compressed under water; small bubbles, of unequal size, in great abundance, rise to the surface. This artificial subcrepitant râle is produced alike by the pressure of the "sponge" and by the expansion after the pressure; thus, the fact of the occurrence of this râle, as a morbid sign, in both inspiration and expiration is illustrated.

The bubbling, as thus produced, is very fine, and the resemblance of the subcrepitant to the crepitant râle is admirably shown by producing alternately, with two portions of "sponge," one portion dry and the other filled with liquid, the representations of the two râles. This may be practised with advantage in order to exercise the ear in discriminating the differential characters of these two râles.

The bubbling is most apparent as contrasted with the dry crackling of the crepitant râle, by filling the "sponge" as completely as possible with water, and making light pressure, the "sponge" being brought very close to the ear. If strong pressure be made so as to expel much of the liquid, the expansion causes a mixture of the bubbling and crackling sounds, that is, the two râles are combined. This is an illustration of what often occurs in pneumonia, especially during the stage of resolution, and also in cases of œdema of the lung; the crepitant and the subcrepitant râle are mixed in variable proportions.

The bubbling is extremely fine if the "sponge" contain very little liquid. After strong compression,

so as to squeeze out the water as much as possible, these bubbles become extremely small and resemble closely crepitation, both during the pressure and the expansion. I suppose this is owing to the liquid being confined to the minute cells, the forcible compression having expelled it from the larger cells. A very small quantity of liquid in the "sponge" suffices to produce bubbling. For a day or more after wetting the "sponge" it retains liquid enough for bubbling, as shown by the character of the sound, and its being produced by pressure as well as by expansion, that is, with the movement representing expiration as well as the movement which represents inspiration. Under these circumstances, however, crepitation and subcrepitation are combined, the former predominating in the expansion movement. When the "sponge" is moist, but not containing liquid enough for distinct bubbling, the crepitation is intensified; that is, it is more marked than when the "sponge" is perfectly dry.

I suppose it to be a fair inference from the production of very fine bubbling, with a small quantity of liquid, as just stated, that bubbling is produced in the air-vesicles and bronchioles of the lungs as well as in the bronchial tubes. If this be true, the subcrepitant râle is not exclusively a bronchial, but also a vesicular râle. It is an error, therefore, to consider, as is generally done, the crepitant as the only vesicular râle. Clinically, a subcrepitant râle may occupy a portion of inspiration, and exist in expiration, while a crepitant râle is heard at the end of the inspiratory act after the subcrepitant râle has ceased. This fact, as already stated, I have repeatedly observed. Not only does this occur, but vesicular bubbling and vesicular crackling take place at the same instant.

This is probably the explanation, in some cases, in which, to the ear of one, the râle seems to be a crepitant, and to the ear of another a subcrepitant. It is generally considered, whenever the character of the sound renders it doubtful whether a râle be a crepitant or subcrepitant, if it be heard in expiration the râle must be a subcrepitant; but this is not proof of the absence of the crepitant râle, since the two râles may be combined, both being produced within the air-vesicles. Of course, I assume, that all fine bubbling sounds, whether produced in the air-vesicles or bronchial tubes, come under the head of the subcrepitant râle, the crepitant rale being exclusively due to the separation of coherent surfaces within the cells and bronchioles.

I have made some observations to determine whether the character of the bubbling is affected by the kind of liquid. A portion of the "sponge," filled either with thin mucilage or with glycerine, gives bubbling sounds not differing much from those produced when water is used. It may be inferred from this that the subcrepitant râle is the same whether the bubbling liquid in the air-vesicles or tubes be serum, mucus, pus, or blood. The pitch of the râles is affected by the condition of the lung as regards solidification or otherwise—but this is a matter foreign to the present topic.

From the very small quantity of liquid in the "sponge" required for bubbling, it follows that when the crepitant râle is produced alone, that is, not associated with the subcrepitant, the air-vesicles and bronchioles must be nearly free from liquid. In fact, the production of a subcrepitant râle, either with or without the crepitant, with an extremely small quantity of liquid, goes to show that the crepitant râle existing alone denotes only a sticky condition of the walls of the cells and bronchioles.

The important points presented, in connection with the artificial illustrations which have been given of the crepitant and the subcrepitant râle, are recapitulated in the following propositions:

1. The crepitant râle is caused by the separation of the walls of the air-vesicles and bronchioles, in the manner explained by the late Dr. Edson Carr, of Canandaigua, N. Y., in 1842.

2. It is highly probable that the peculiar quality pertaining to the inspiratory sound in the healthy murmur of respiration is due to the same cause, the cohesion of the walls of the air-vesicles and bronchioles not being sufficient to give rise to a crepitant râle.

3. The subcrepitant râle is caused by the bubbling of liquid in minute bronchial tubes, and also in the air-vesicles and bronchioles.

4. The essential distinctive character of the crepitant râle, as contrasted with the subcrepitant, is its dryness. The term "dry crackling" expresses this character, whereas, the phrase "fine bubbling" expresses the character of the sound in the subcrepitant râle. In addition, the crepitant râle is not produced in expiration, whereas, the subcrepitant may be produced in expiration as well as in inspiration.

5. Very fine bubbling due to liquid in the air-vesicles and bronchioles resembles the fine crackling sound which characterizes the crepitant râle; and the discrimination of the former from the latter requires a nice perception of differences in sound and some practice in comparing the two râles. The artificial production of the two râles may be made highly useful by affording this practice.

6. The crepitant and the subcrepitant râle are not infrequently found in combination. They are likely to

be combined whenever the air-vesicles and bronchioles contain liquid of any kind.

7. In view of the fact that for the artificial production of the crepitant râle no liquid is necessary, and in view of the fact that for the production of fine bubbling sounds an extremely small amount of liquid only is required, wherever in disease a crepitant râle exists, without the coexistence of the subcrepitant râle, it is probable that there is a morbid adhesiveness of the inner surface of the air-vesicles and bronchioles without the presence of an appreciable amount of liquid. Hence, in the cases in which the crepitant râle exists alone in either the first stage of pneumonia or in the resolving stage, the morbid product within the air-vesicles and bronchioles must be either a glutinous matter sufficient to give adhesiveness to the walls but not enough for bubbling, or the product is a semi-solid, in which bubbles are not readily produced; and, in cases of œdema of the lungs, or when blood is present in the air-vesicles and bronchioles, the crepitant râle can hardly be expected to occur without being associated with the subcrepitant.[1]

8. The characters of the subcrepitant râle are materially the same, although the bubbling is produced in liquids differing as regards consistence.

[1] The following well-known clinical fact exemplifies the occurrence of the crepitant râle without any accumulation of liquid in the air-vesicles: A feeble patient, with any disease, after keeping the recumbent posture on the back for some time, and breathing quietly, if raised to the sitting posture, frequently has for a few successive deep inspirations a well-marked crepitant râle over the inferior posterior aspect of the chest on both sides. The lining membrane of more or less of the cells and bronchioles, having been in contact for some time, adheres sufficiently to furnish the râle when these cells and bronchioles are forcibly separated, the râle, under these circumstances, remaining during a few acts of breathing and perhaps only heard with the first inspiration.

I may add, in conclusion, that bubbling, coarser than that which constitutes the subcrepitant râle, in other words, moist bronchial râles, produced in tubes of larger size than those in which the bubbling is fine, may be artificially represented by the compression and expansion close to the ear of an ordinary sponge containing liquid in abundance.

APPENDIX.

After the reading of the foregoing paper at a meeting of the "Journal Association," December 18, 1868, my friend, Prof. A. Jacobi, with whom I had previously conversed respecting the subject of the paper, presented some extracts from certain German authors, in which the explanation by Carr is not only adopted, but due reference is made to his publication in 1842. The extracts, which were translated by Prof. Jacobi, and kindly placed at my disposal, are herewith appended. That they were unknown to me when my paper was written, is owing to the fact that for knowledge of what is contained in German literature I am obliged to resort exclusively to English or French translations.

The following is from Wintrich's work on Diseases of the Respiratory Organs, published in 1854, p. 167: "While the lungs are within the closed thorax, they cannot contract to such an extent that the alveoli and smallest bronchi agglutinate, therefore there can be no crepitant râle proper in healthy lungs. But when the mucous membrane swells in the alveoli or smallest bronchi, and is covered with tough mucus, or with a tough pneumonic exudation, there is the possibility during expiration of such an approximation that cohesion may take place. Now, when a rapid forcible inspiratory current enters the smallest bronchi and the

alveoli during the expansion of the lungs, the agglutinated portions are torn apart, and thus give rise to the râle. During expiration the walls approach each other again, but this produces no sound. Previous to myself, Carr had an entirely similar idea.—(*American Journal of Medical Sciences, October*, 1842, p. 360.)"

The treatise on Percussion and Auscultation by Joseph Skoda, translated by Markham, published in London in 1853, and republished in this country in 1854, does not contain Carr's explanation. In the edition of 1864, however, Skoda adopts this explanation as set forth by Wintrich, and accords the priority to Carr in the following terms: "The vesicular râle (the crepitant râle of Laennec) can accompany inspiration only in consequence of its mechanism. I am entirely of the opinion of Wintrich, that the mechanism of the crepitant râle does not consist in the formation and bursting of bubbles in liquid contained in the alveoli—the former opinion—but that it is produced by the sudden separation of the walls of the smallest bronchi and alveoli, coherent from mucus, by the rushing-in current of air. The walls, adapting themselves to each other during expiration, cannot produce a sound. Experiments with lungs outside the cadaver leave no doubt as to the fact. In harmony with all this is the observation that the crepitant râle is frequently not heard except in forced inspirations, and only during a limited period, and then disappears for some time. The above opinion was first pronounced by Carr.—(*American Journal*, 1842.)"

The following extract is from page 163, in a Manual of Auscultation and Percussion, etc., by C. Gerhardt, Tubingen, 1866: "Crepitant râle, similar to the sound of salt in the fire, or that of the friction of hair near the

ear. It is mostly found very exclusively, and always in such a manner that it may be denominated as moist, although the above comparisons do not corroborate this latter character. In patients this râle is mostly heard under circumstances which allow of the supposition of the coexistence of air and liquid in the alveoli, principally in the first and second stages of pneumonia and in pulmonary œdema. However, it is difficult to imagine that a perceptible sound should be produced in the alveoli by the formation and bursting of very small bubbles; but it is just as difficult to prove its impossibility. Thus, we ought to consider the above supposition as not fully proved to be the correct one. But this much is certain, that the crepitant râle can originate in a different manner. There is a perceptible crepitant râle when the lungs removed from a recently-slaughtered animal are being inflated. And there are crepitant râles in patients which cannot be explained by liquid in the alveoli. When debilitated patients, who have lain on the back for a long time, begin to draw deep inhalations, there is over the lower lobes posteriorly, during the first inhalations, a crepitant râle of rather a dry character. In both cases it is to be assumed that, while the lungs were collapsed, the walls of the alveoli were attached to each other, and the râle is occasioned by the tearing asunder during inflation."

Again, at page 196, this author speaks of the peculiar sound, similar to the vesicular, crepitant râle, in peritonitis with fibrino-purulent exudation.

ART. II.—*A Clinical Lecture on Thoracic and Abdominal Aneurism.* By Prof. SKODA, of Vienna. Prepared for the *New York Medical Journal* by A. KESSLER, M. D., of Hartford, Conn.

A highly-interesting case of aneurism of the ascending aorta, which recently came under my observation, suggested to me the thought of writing out the substance of a clinical lecture given in the *Allgemeine Krankenhaus*, of Vienna, by Professor Skoda, last winter, in illustration of several cases treated at the time; and as it embodies the latest views of that distinguished authority on this important and by no means fully cleared up subject, it may prove acceptable to many readers of medical literature.

Acute and chronic inflammations of the arteries lead to aneurism. The former is of rare occurrence, and principally owing to traumatic causes, lesions, etc., while the latter happens more frequently, and is either dependent on a vitiated, anomalous nutrition, a general diathesis, or a morbid disposition of the vessels. A chronic inflammation of the arteries, giving rise to spontaneous aneurism, is either the expression of an anatomical degeneration of the arterial coats, or of a faulty state of the blood; perfectly sound vessels, with healthy blood coursing in them, are never the seat of aneurismal affections. The inflammation originates chiefly in the *adventitia*, but, with the enlargement of the tumor, the arterial coats become gradually involved. Young persons and females are but seldom affected. The disease is most frequent with men of advanced life, and especially among those that have to perform severe labor and undergo great fatigue.

Aneurism of the ascending aorta does not greatly interfere with the circulation, although the vessel—which enlarges during the ventricular systole and possesses sufficient contracting power during the diastole to carry forward the current of the blood—becomes, by the loss of its muscular tone, much reduced in its contractility; and despite the retention of blood within the aneurismal sac, the tumor, as such, causes no circulatory disturbances. But often, in consequence of its being compressed behind the aneurismal spot, the artery receives very little blood from the sac, and a diminution of the circulation and obstruction in the capillaries naturally ensue. Aneurism of the ascending portion of the aorta is generally combined with insufficiency; often, too, with stenosis of the semilunar valves. The dyspnœa usually signalizing and accompanying it is not produced by the pressure of the sac upon the lung parenchyma, and the consequent obliteration of the respiratory surface. The distressing symptom just mentioned owes its existence chiefly to the fact that the sac, in pushing its way toward the atrium and cavity of the heart, obstructs the circulation in the right ventricle, and produces congestion of the veins, the latter becoming enormously distended by the diminished contractile power of the ventricle. In approaching the walls of the chest, the aneurism can and does quite often impart to the neighboring region the same chronic inflammation which it suffers, not by pressure merely, but by a gradual extension of the inflammatory process. The lung-substance is frequently forced out of its place, or undergoes a partial absorption, which leads finally to its total destruction. The nerves in the vicinity, especially the vagus, respond to the presence of the aneurism, and its pressure upon the

posterior mediastinum, œsophagus, and spinal column, manifests often by excruciating pain and by various disturbances the effects of the aneurismal tumor.

The principal diagnostic signs of aneurism of the ascending aorta are, a prominence on the thoracic wall, an uplifting of the chest during the systole, distinct from that of the heart, dulness on percussion, and pain over the visibly elevated spot; whereas, tumors, not of an aneurismal character, move with the systole of the heart, only in one particular direction, the aneurism responds to the systolic impetus by a pulsation, which is *symmetrical and uniform in all directions.* Other important symptoms are—1. A rushing, systolic murmur, caused by the roughness of the inner arterial surface, although the same sound may be produced by neoplasms. 2. Insufficiency of the aortic valves, and hence the diminished force of the circulation. 3. Retardation of the radial pulse, though this symptom may also be owing to the lessened contractility of the artery, and deserves, on that account, no pathognomonic value.

Absence of any tone or murmur in the aneurismal sac is without significance. In larger aneurisms, to be sure, the heart-sounds, especially if already intensified, are invariably perceived with greater force and loudness over the tumor, and this is rather characteristic for an aneurism of large size, but not so in those of a smaller circumference. Aneurisms of the ascending portion of the aorta usually open to the right of the sternum, rarely to the left. But even if thus situated, we are not justified in ascribing them to the pulmonary artery, this vessel enjoying an almost perfect immunity from the invasion of the said affection. Unless the aneurism is large enough to present itself to the eye as a

prominent tumor, with all the characteristic signs described above, the recognition of its true nature is extremely difficult, and the differential diagnosis between an aneurism and a neoplasm, in or contiguous to the artery, cannot be established with any precision. Pain and a rushing sound may be chance phenomena in the artery; alone they are not characteristic of aneurism, and render its diagnosis only probable. The most essential, in fact, the pathognomonic symptom, is insufficiency of the valves of the aorta, especially the presence of a *loud first sound* over the tumor; whereas, in carcinoma and other neoplasms, the first tone is dull and hardly audible, and the diastolic sounds entirely normal. Smaller aneurisms, not reaching up to the parietes pectoris, cannot be diagnosticated. Mere rushing murmurs may be due and ascribable to unevenness and roughness of the inner surface of the artery. Small aneurisms burst oftener than large ones, and such a process may take place in cases that were not at all perceived in life. What has been described as a dissecting aneurism cannot be properly classed among the spontaneous affections of that kind; it is merely an enlargement and distention of the arterial walls.

The aneurisms of the arch of the aorta manifest almost the same symptoms as those of the ascending portion; but they produce always a pressure upon the left bronchus, and arrest, in consequence, the respiration in the corresponding lung. They press also upon the trachea, and cause there severe irritation, and a copious secretion from the mucous membrane of that organ. Destruction of the cartilaginous rings, maceration of the inner membranes of the respiratory tract, and hæmorrhages, are further sequels of a serious and often fatal character. The pulsation of the aneurism is distinctly heard in the fossa jugularis.

Aneurisms of the descending portion of the aorta are much rarer, and their diagnosis is made only with great difficulty. Pulsation is, even in larger ones, seldom perceptible, but they lead usually to a destruction of the vertebræ, which are gradually absorbed and worn away by the adhesion of the sac to the denuded portion of the bone, and give rise to excessive lancinating pain in the back. A systolic murmur and double tone, audible upon any circumscribed spot in the dorsal region, and which is quite painful, render the diagnosis of aneurism of the descending aorta pretty certain. Murmurs in this portion of the artery are quite uncommon, and can, therefore, almost with certainty be connected with aneurismal tumors. Physical exploration, pressure upon the back and ribs, and dulness on percussion of the painful region, aid in the true recognition of the disease. Another very important symptom is the retardation of the pulse in the lower arteries through the compression of the aorta below the aneurismal sac, or with the calibre of the vessel becoming excessively diminished, an entire cessation of the same in the femoral, crural, and the other arteries of the extremity; the smooth flow of the blood, without its usual undulating motion, accounts for the absence of the pulse. In obliterations of the aorta, a collateral circulation establishes itself, in the course of time, which is easily discoverable. If such collateral circulation be not perceptible, especially in the dorsal arteries, obliteration of the thoracic aorta is out of question, and the diagnosis of aneurism of the descending portion is rendered almost certain.

The abdominal aorta is even more rarely the seat of an aneurismal affection, but the diagnosis of it is comparatively easy, as the pulsating tumor can readily

be felt through the soft parietes, and its genuine character is placed beyond doubt, if the tumor enlarges uniformly in all directions, synchronous with the ventricular systole, a fact that can best be ascertained by grasping closely and on all sides the aneurismal sac, with the full hand, and following its motion.

We meet very often with a violent pulsation of the abdominal aorta, without being able to discover an aneurism. This pulsation is usually accompanied with great pain and oppression, but is by no means a characteristic sign of aneurism. It may be regarded as a peculiar disease, probably owing to a morbid alteration of the muscular coat, and elasticity of the artery, which, becoming irritated, together with its sheathing tissue, give rise to the accompanying pain and troublesome sensations. And it is very probable that this pulsation is not merely confined to the abdominal aorta, but also present in the thoracic portion of the same vessel, only we are unable to trace it there. Many distressing and even alarming symptoms of a gastric, nephritic, and nervous character, great oppression, debility, and prostration, appear in the course of this disease, but its termination is rarely fatal, and the most threatening disturbances abate often without any therapeutic interference. Rest, horizontal posture, and cold applications, exert but little influence upon the pulsations. Digitalis may afford some momentary relief, by retarding the contractions of the heart. Fulness and distention of the abdomen aggravate the mischief; a bland and scanty diet is indicated, and among remedial agents, quinia, with or without digitalis, appears to be the most useful and reliable. Formerly the pulsation of the abdominal artery was always identified with the existence of an aneurism; but re-

cent investigations and pathological anatomy have already established the fallacy of that view.

ART. III.—*A New Modification of Sims's Speculum.* By AUGUSTUS F. ERICH, M. D., Baltimore, Md.

THAT Sims's speculum is unequalled in the facility it affords for exploration and treatment, is a fact so well established, and so generally acknowledged by gynecologists, as to make it superfluous to dilate upon it at the present time. The principal obstacle to its introduction into general practice has been the necessity of the presence and aid of, not only an assistant, but a skilled assistant, in order to develop its real value. And even when in the hands of the most skilful assistant, especially during tedious operations, it becomes frequently necessary for the operator to put down his instruments, and correct the position of the speculum with his own hands, because he finds it next to impossible to convey to the assistant a description of the required position by any other means. This difficulty will be unavoidable while the speculum is supported by the hands of an assistant, and consequently liable to move independently of the patient's body, and as long as the assistant cannot be in such a situation as to enable him to see whether the speculum is in its proper position or not. Considering the large number of cases that can only be relieved by the aid of this speculum, and that quite a number of these patients cannot be induced to submit to an exposure before a third person, we need not be astonished at the numerous modifications of Sims's speculum presented to the profession during the comparatively short space of time

that has elapsed since he first published the invention. Although all these contrivances were intended to enable the practitioner to use it without an assistant, not one of them can be claimed to be equal to Sims's unmodified instrument when in the hands of a skilful assistant. Nevertheless, all who have been obliged to perform an operation upon the interior of the vagina, requiring a little unusual time, will agree with me when I say that the assistant holding the speculum is at best *a necessary evil*, and that any contrivance which will secure the speculum to the patient's body in such a manner as to produce the same exposure of the interior of the vagina, and permit equal access to the uterus, without the aid of an assistant, would be quite a boon to the profession as well as to the patients.

Being convinced that a want of such a modification really existed, and that none of those published had fully supplied it, I proceeded to analyze the motions produced by the assistant's hands when using the speculum, and found that they consisted, after the instrument had been introduced, of only two motions: he draws the perinæum backward and a little upward, both of which motions are easily imitated by levers. The principal point to be considered was, where to place the fulcrum, and from what point the necessary force was to be exerted. The internal surface of the inferior edge of the pubes, being the point against which most specula exert the force required to retract the perinæum, was rejected, because it will not bear as much pressure as will often be required in young and muscular subjects. After considering several other places that might be chosen, it was determined to place it on the posterior surface of the sacrum, and make extension from the left shoulder by means of a strap.

Without detaining the reader with a description of the successes as well as failures met with during a pretty long series of experiments, I shall confine myself to giving merely the result of the same, represented in the accompanying illustrations.

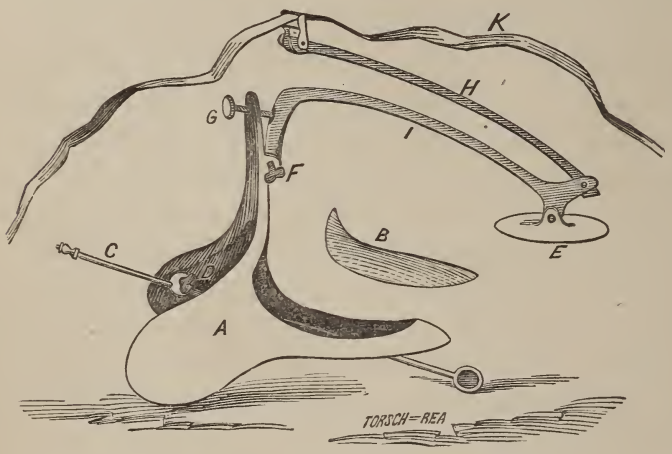

Fig. 1.

A. The speculum, the right wing of which is longer than the left, to support the right buttock when the patient is in the left lateral semi-prone position.

B. A plate fitting over and closing the fenestrum with which the speculum is provided, to facilitate operations for recto-vaginal fistula and other affections of the posterior wall of the vagina.

C. A depressor, having a sliding as well as circular motion; it may be securely fixed in any suitable position by simply tightening the screw D, by means of which it is attached to the speculum. A few turns of the same screw in the other direction will detach the depressor and washer in which it slides, leaving the opening of the speculum perfectly free. The depressor may then be used with the hand, as an ordinary one.

E. The fulcrum plate, intended to rest upon the sacrum.

F. A screw, by means of which the speculum can be detached, and others of different sizes secured to the levers.

G. A screw by which the angle of the speculum may be adjusted after it is introduced into the vagina.

H. The ascending lever, measuring six and one-half inches from the joint to the top of the buckle.

I. The descending lever.

K. A strap passing under the right axilla and over the left shoulder, the united ends of which are drawn through a spring-buckle on the top of the ascending lever, which seizes and holds the strap at any degree of tension.

They represent an instrument that will do all that an assistant can do with the unmodified speculum,

without getting fatigued as he will, and one that is sure to follow all the motions of the patient, without losing its relative position to her body. Nay, more than this, it will remain in its position whether she is standing, walking, kneeling, or in the left lateral semi-prone position, and at the same time securely keep the garments of the patient (crinoline and all) out of the operator's way.

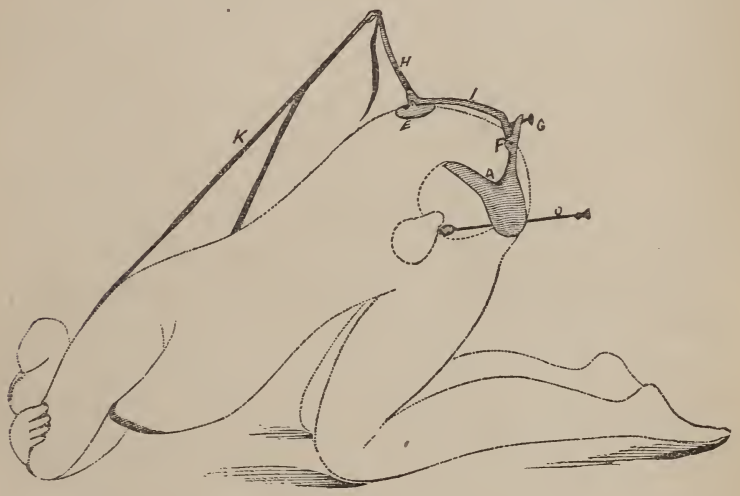

FIG. 2

Represents the speculum as used with a patient upon her knees. It is evident that, on tightening the strap, the fulcrum plate, levers, and speculum will slide upward as far as the yielding perinæum will permit, and then the levers will begin to act and draw the perinæum backward.

Diffident of my own judgment in a matter the success of which would afford me personal gratification, I referred the instrument to the members of the Pathological Society of Baltimore, and also to those of the Baltimore Medical Association, at their second meeting in the month of May last, and, upon illustrating its use upon a patient, present for that purpose, was gratified to find that it met with the approval of all present;

some of whom have already supplied themselves with the instrument.[1]

ART. IV.—*Phosphorus in Locomotor Ataxia.* By WALTER LAMBERT, M. B., Amherstburg, Ontario, Canada.

MISS F. B., aged 22, had been suffering slightly with anæmia and scanty menstruation for about one year. At different times, she took ferruginous preparations, with decidedly good effects; but, as soon as relieved, she would leave off taking the medicine, and her trouble would return. She also had ague once or twice during the summer, it being very prevalent at that time in the neighborhood. For it she was specifically treated, and from it she soon recovered.

For the chlorosis I sometimes gave mistura ferri comp. (Griffith's), sometimes tinct. ferri and quiniæ disulph.; lastly, I was giving her syr. ferri iodidi, with cod-liver oil. In September last, from exposure to wet and cold, her menses ceased, and all the symptoms of progressive locomotor ataxia set in. Her parents, who live in the country, came for more medicine, and casually told me that their daughter walked with great difficulty, and that her menses did not come on at their usual period; consequently I went to see her, and in her attempting to shake hands with me she grasped me by the wrist. This excited my fears immediately that she had Duchenne's disease. Upon further examination, my diagnosis was verified. The patient, in attempting to walk, staggered and swayed her body from side to side to keep her equilibrium. She would

[1] The modified speculum was made under my directions, by Mr. Rosendorn, surgical-instrument maker, No. 18 South Sharp Street, Baltimore, Md., from whom the instrument can be obtained.

suddenly halt to recover herself, and then would plunge forward, seemingly in a great hurry to reach the point to which she desired to go. She was unable to feed herself, from the want of coördinate action of the muscles, and, in fact, unless she was watching her hands continually, she was liable to drop whatever she had in them. Her speech was also affected; she was not able to articulate some words perfectly.

What is passing strange in this case is, that I was giving her syr. ferri iodidi at the very time that the disease manifested itself; the very medicine that Dr. Julius Althaus used with so much benefit in his case, the only one recorded, until lately, that had been much benefited by medicine.

As soon as I recognized the disease, I gave potass. bromid. grs. xv., ter in die, and submitted the patient to the action of magneto-electricity once every twenty-four hours. I also gave two pills of aloes and iron, which produced too much relaxation, the effect continuing two or three days. This, in fact, seemed to prostrate her to such an extent that she was obliged to take to her bed, and there remain for a time. Fortunately, just then I received the September number of the *New York Medical Journal*, and in it saw that Dr. Dujardin Baumetz had given phosphorus in this disease, with excellent effects. I immediately ordered acidi phosphorici dil. m. xv., ter in die, in simple syrup. The next day her menses came on, and in a short time she began to improve. In a few days I increased the dose to twenty, twenty-five, and then to thirty minims. After ten or twelve days, I omitted the acid, and gave her the pyro-phosphate of iron for a week, and then returned to the acid. I continued the electricity every alternate day. In two weeks she was able to sit up,

and had sufficient control over the muscles of her upper extremities to be able to knit. In one month she could walk about the house tolerably well. Now it is something over two months; she can take long walks, do housework almost as well as ever, and has become very fleshy. The electricity has been discontinued for about one month, and she is not at all regular with her medicine at the present time. However, I have the most sanguine hopes that she will perfectly recover. The improvement has been so great that it is impossible to discern any thing wrong with her, except a very slight irregularity in her walk.[1]

ART. V.—*Remarks on the Advantages of a Supporting and Confining Apparatus, and a Self-retaining Speculum in the Operation of Vesico-vaginal Fistule; Models of Certain Forms of Suture; their Results practically contrasted in the same Cases and upon the same Fistulous Openings.* By NATHAN BOZEMAN, M. D., New York.

(These remarks, in substance, were made before the New York Medical Journal Association, Nov. 20th.)

SUPPORTING AND CONFINING APPARATUS.

I WOULD remark that I have had this apparatus constructed specially to receive and support the patient in an old position for the operation of vesico-vaginal fistule, which I denominate the *right-angle position, upon the knees and chest.* This position was first proposed by Prof. Roux, nearly forty years ago, and was adopted soon afterward by Prof. Wutzer, in Bonn. But, as employed by these surgeons, two of the most

[1] We are informed by Dr. Lambert, in a note received since this article was in type, that the patient has perfectly recovered under this treatment.—E. S. D.

important advantages of it were not appreciated: first, freedom of the abdomen from pressure; and second, effectiveness with which the patient could be secured without the aid of assistants.

In our construction of the apparatus here presented we claim to have secured the above important advantages, and to have utilized the position for all operations upon the anterior wall of the vagina, and, we may add, operations generally about the anus and rectum of both sexes.

Having previously described and pointed out the many advantages of this contrivance before another society in the city, it is not necessary that I should do so again on this occasion. Suffice it to say, in its use three principal objects are sought to be attained:

1. Extension of the vertebral column and relaxation of the abdominal muscles essential to free gravitation forward of the pelvic and abdominal viscera.

2. Support and mechanical confinement of the patient by controlling muscular action at certain points without encumbering the abdomen, or interfering with the functions of respiration and circulation.

3. The safe administration of anæsthetics.

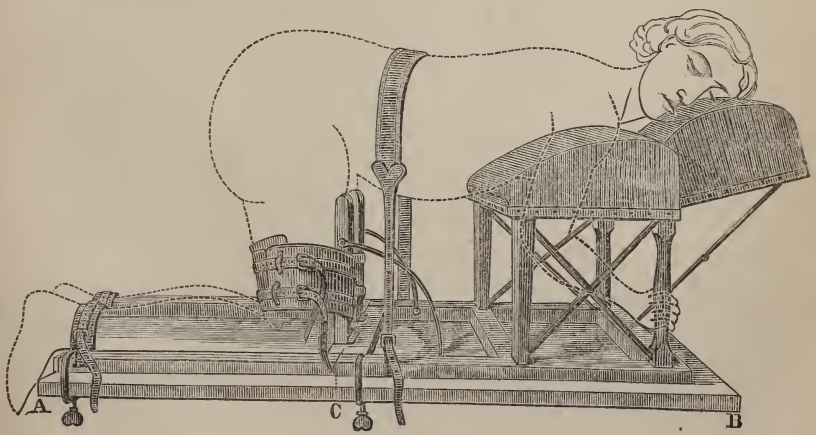

FIG 1

As may be seen, the apparatus is admirably adapted to all of these purposes. It is forty-four inches in length and eighteen in width. The construction, as seen, is simple and strong. It is light, weighing only seventeen pounds, and very portable when folded up. It may be placed for use upon any kind of table, to which it is made fast by the weight of the patient and a couple of small clamps.

Fig. 1 is an illustration of the apparatus in use. The whole figure is here exposed, in order that the relationship of every part of the apparatus to the body may be seen. In actual use, we would observe, there is little or no exposure of the person, the patient being placed in position, and secured as here shown, under a sheet. In fact, there is less exposure in this position than any of the others usually employed, for the reason that the patient is so steadily and securely held that her clothing does not become disarranged, as ordinarily results from her resistance and struggle under the pain of the operation.

The apparatus, as may be seen, is placed upon and secured to the table, A B. The patient first kneels upon a couple of cushions, the thighs being perpendicular to the table and received against the two upright splints, held in position by the corresponding braces. Around each thigh and splint is placed a pad, over which are buckled two strong straps to secure the whole. In a similar manner the ankles are confined. The lower limbs thus arranged and secured, muscular action is effectually controlled.

The patient is next required to bend the body forward until the chest and head are received upon their appropriate supports; she then voluntarily extends the vertebral column, which position is maintained

by the long girth seen passing across the loins. In this way she is deprived of all power to raise the body from its support, or otherwise to make any effective resistance. Relaxation of the abdominal muscles and gravitation forward of the pelvic and abdominal viscera are thus permanently secured.

In this position the chest and head suffer no restraint. Respiration and circulation go on smoothly. In short, the entire body is easy and comfortable, and the patient can remain for hours with as little fatigue as upon the back. Anæsthetics can be given with as much safety in this as in any other position; and I am induced to believe, from our experience so far, that it is even better on some accounts. In vomiting, for instance, no ingesta are liable to reach the larnyx, and consequently no delay is experienced in the operation from apprehended strangulation, as very often happens. The face being downward, every thing from the stomach is freely ejected, and the effort does not in the least interfere with the progress of the operation. I have now given ether and chloroform about thirty times in this position, without a single untoward occurrence, and from our convictions based upon this experience I have no hesitation in recommending it as entirely safe, with the observance of ordinary precautions.

When the patient is disengaged from the apparatus, it is doubled up, the hinge-joint C being placed near its middle for the purpose. The head-support, attached by two hinges and held in position by a brace, drops down as soon as the latter is removed, and is placed beneath the chest-support, to which it is fastened. The two supports are thereby made to stand back to back, occupying the least possible space. When thus folded up and set upon the floor, the apparatus resembles somewhat an ordinary chair.

As showing the high estimate that we place upon the many advantages of this simply-constructed apparatus, I would observe that I do not believe a patient can be found, I care not what her size, strength, or temperament may be, who cannot be effectually secured with it without the aid of assistants.

THE SPECULUM.

In this connection we would call attention to certain alterations and improvements which we have made in our *spring and self-retaining speculum* since the first description of it was published in the *New York Medical Record*, nearly a year ago. During the period of these improvements we have continued to use the instrument with the above supporting apparatus, and in every instance it has given entire satisfaction. In fact, we have had no occasion, since its adoption, to use any other form of speculum for examinations or operations about the cervix uteri. The main part of it has required but little change or alteration since its first application. The attachment, however, intended for the elevation and support of the posterior wall of the vagina, has been variously modified. The standing arch first used and described was soon discarded, for the reason that it could be employed only in a certain class of cases. Side springs were then substituted, which proved quite effective, though their management was somewhat difficult in inexperienced hands. In order to overcome this trouble, we attached to each of these springs a spatula-shaped depressor, about four inches in length, intended to elevate the springs and to support in a longitudinal direction the posterior wall of the vagina. This was effected by seizing the outer end of each depressor and pushing

up the spring to its proper place, which being done, this end of the depressor was next slid outward beneath the perinæum to its place of support upon the corresponding projecting arch, as is here shown in our medium-sized speculum. This arrangement we found admirably adapted to the purpose above named in the position upon the back, as well as that upon the knees and chest. With the patient in either of these positions we have been able to do with the instrument, thus completed, any thing in the way of examinations and operations without the aid of an assistant. Many have objected to these depressors, under the belief that they complicate the instrument. We have, therefore, made an effort to simplify the working of this part of the instrument still further, which we think we have done in the substitution of one broad depressor for the two heretofore employed in connection with the side springs, which are now dispensed with. This depressor is entirely separate from the main part of the instrument; it is about four inches in length, one in width toward its point, and one and a quarter at its outer end, where there is a short neck or handle, an inch and a half in length, turning upward at an obtuse angle. It is thin and slightly flexible, and is bent to sui the curves of the posterior wall of the vagina, to which it is firmly applied from the perinæum to the posterior *cul de sac*. To the under surface of it, near the outer end, is attached along the centre a small triangular plate, the base presenting backward and the apex forward, with a flange on the side edges. These grooves or flanges are intended to guide the depressor to its proper place when received upon the projecting arches of the speculum previously introduced into the vagina and expanded. The arrangement is such that

the depressor can be slid in upon the projecting arches at any stage of expansion, and in that relationship is securely held by the resistance of the perinæum and the recto-vaginal wall.

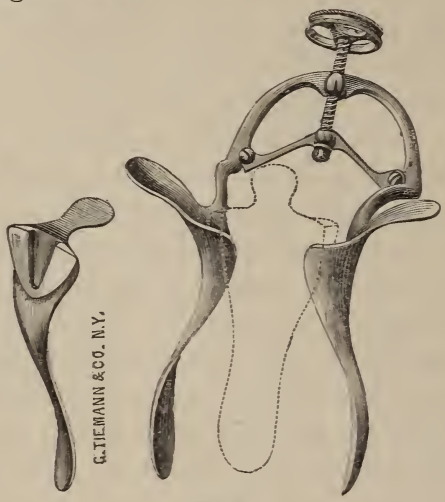

Fig. 2.

The illustrative cut Fig. 2 is a one-third size, and a three-quarter view of our medium-sized speculum. The instrument is represented partially expanded. The dotted lines in front and between the expanded or flaring blades are intended to show the shape and position of our last improvement of the attachment for supporting the posterior wall of the vagina. The relationship of it to the projecting arches of the main instrument is very clearly shown. The accompanying edge view of the same represents very well the curves described and the peculiar mechanism of the triangular plate on the under surface of it.

The main part of the instrument, having already been minutely described, may be passed over here. Before leaving the subject, however, we will briefly sum up the principal peculiarities of the instrument for which we claim originality, to wit:

1. The system of leverage employed, which gives us increased power over increased resistance.

2. Transverse dilatation with uniformly varying movement of the blades, which gives us a thin and favorable form of the points for introduction, and a reversal of the size of the two ends of the instrument when expanded within the vagina. By virtue of this *flaring expansion of the blades within the ascending rami of the ischia*, the instrument is made *self-retaining*, which distinguishes it from all others of this class previously constructed.

3. The *elasticity of flexure* belonging to the working-point of the instrument, which gives it an easy adaptation to the soft parts, both in the vagina and at its mouth. This is also a feature of the instrument that particularly distinguishes it from other valved specula, heretofore in use.

4. The applicability of it in all positions, and the advantages secured to the physician or surgeon, of making all examinations, or of doing all operations required upon the vaginal walls and cervix uteri without the aid of assistants.

The arrangement and working of this last improvement can be fully understood by an examination of the whole instrument, which I here present. As now completed, it is, I think, as simple and effective as it is possible to make it. Whether it will ever be adopted in general practice, or not, is a matter of no moment to the obstetric surgeon. As an instrument perfect in its adaptation to all operations upon the anterior wall of the vagina and the cervix uteri, without the aid of an assistant, we think it must sooner or later hold the first rank.

Since the adoption of our supporting apparatus

and this speculum, we have had six consecutive cases of vesico-vaginal fistule, presenting eight fistulous openings, one a *vesico-uterine*. All of these fistules have been closed at seven operations—one operation less than the actual number of openings. These operations, I may add, have been witnessed by nearly a hundred physicians and surgeons of this and neighboring cities, among whom I may mention Profs. Willard Parker, A. C. Post, Jas. R. Wood, Isaac E. Taylor, Drs. Thos. C. Finnell, Lothar Voss, John O. Stone, R. F. Chabert, of Hoboken, De Witt C. Enos, of Brooklyn, R. B. Bontecou, of Troy, and S. H. Tewksbury, of Portland.

Already, I may here observe, several modifications of this instrument have been made, but as yet we have seen no improvement upon the original. The most important of them is that of my friend, Dr. J. C. Nott,[1] of this city. Although this gentleman does not do me the justice to say his instrument is a modification of ours, it is nevertheless true, for the main feature of it I am entitled to the credit of priority, as any one can see at a glance by comparing the two instruments. The self-retaining principle of his instrument is due entirely to the *flaring expansion of two of his blades within the rami of the ischia*, the main principle upon which our instrument was first constructed. This principle of expansion within the pelvic bones, in order to secure the important desideratum of self-retention in the construction of our speculum, I may be permitted to say, I worked out by a series of experiments, extending through a period of nearly two years. I am therefore warranted in saying, from a careful examination of the records of the profession, that no one, previous to the first published account of our

[1] American Journal of Medical Sciences, October, 1868.

speculum, January, 1868, had ever attempted to construct an instrument upon the same principle; and that, from our experience with it thus far, we believe it to be the only principle upon which any simple and useful self-retaining speculum can ever be constructed.[1]

SUTURES.

As to the form of suture used, we would say a word.

While we regard the two improvements just described of very great value, and we think they mark a new era in the history of vesico-vaginal fistule; still the fact must not be lost sight of, that the kind of suture employed is no less important now than it has always been. Although the above stated results show an extraordinary amount of success considering the character of the cases, yet this is not to be attributed alone to the improvements above described. Much of it is due to the suture—the button suture, so called from its peculiar mode of action.

This form of suture we devised and first applied nearly fourteen years ago, and no other suture have we ever had occasion to use, in such cases, to the present moment. It is composed of silver wire, a leaden plate, and perforated shot.

We claim for it:

1. Separate and independent action of the sutures.

2. Perfect coaptation of the edges of the fistule, and power to hold them in a certain relationship during the reparative process.

3. Perfect steadiness and support of the edges of the fistule.

[1] Those who may have ordered our speculum before it was completed, can have the last improvement attached by Messrs. George Tiemann & Co., of this city, with but little additional cost.

4. Protection of the denuded edges of the fistule from the vaginal and uterine discharges, and from the urine, when there happens to be more than one opening, and it is not convenient or desirable to close both at the same sitting.

Here we have all the elements of success happily combined, which accord fully with principles recognized by all surgeons in the treatment of wounds in any of the external parts of the body.

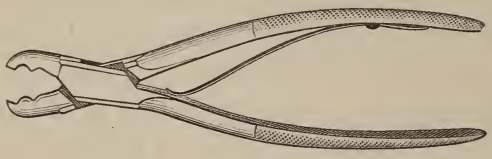

Fig. 3.

A very good idea of this form of suture, and the many advantages claimed for it, may be had from an examination of this model which I have made with a piece of buckskin. The leaden plate or button is two inches long, five-eighths of an inch wide, and grooved on the under surface with such an instrument as Fig. 3 represents, which I have invented, and term the *button-forming forceps.* There are seven perforations along the centre through which the two ends of each of the seven sutures are passed. On the top surface of the button are seen seven perforated shot, compressed upon the doubled ends of each wire. To the under or grooved surface of the button, the edges of the fistule, represented by the sides of the slip in the piece of buckskin, are smoothly and firmly applied, presenting, as may be seen, a line of approximation somewhat curved, though corresponding exactly with the line of holes described. This line, although elevated and depressed at certain points, is as perfect as it is possible

to make it from one angle of the opening to the other. At every point the coaptated edges are seen to be under the most complete control, and the closure on the opposite side is so even and perfect that it would be scarcely possible for a drop of water to enter. I will refer incidentally to the case in which this very button was used:

Case I.—Mrs. R., aged about forty-one, above medium stature, lost control over her bladder at the birth of her second child, July, 1853. She presented herself a few months ago, with a fistulous opening, involving the root of the urethra, the whole of the trigone, and a part of the *bas fond* of the bladder. The enormous chasm admitted three fingers into the bladder, and through it protruded constantly the superior fundus of the organ. A more miserable and wretched condition than this poor woman presented, the imagination can scarcely picture. During her fifteen years of affliction five or six operations were performed, some of them by eminent surgeons, though no permanent relief had been afforded up to the time she came under our care. The first application of this suture apparatus, as you see it here, resulted in a complete closure of the opening on the eighth day. This operation, I believe, was witnessed by one or two gentlemen present.

To show now the relationship of the urethra to the edges of the fistule as approximated in the above case, we have attached a tube to the model between the fifth and sixth sutures, through which is passed an elastic catheter No. 6, just as it was used in the operation.[1] This kind of a catheter I prefer to all others on account of the comfort to the patient and the ease with which it can be kept open without removal, simply by running a wire through it. This same catheter, we would remark, was lodged in the bladder five days, and by an examination it will be seen that not the slightest roughness ensued.

Fig. 5, intended to illustrate a fistule of smaller size than the above represents, nevertheless, a button almost precisely the same shape as the one described. Here only six sutures were required. From this cut

[1] See Fig. 4, on next page.

an excellent idea may be obtained of the adjustment of the sutures previously to passing the button down

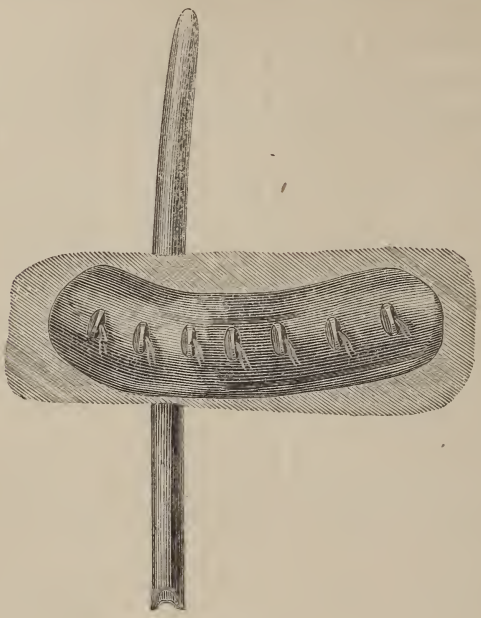

FIG. 4.

Fig. 4 is an illustration of the apparatus, only a portion of the catheter being shown.

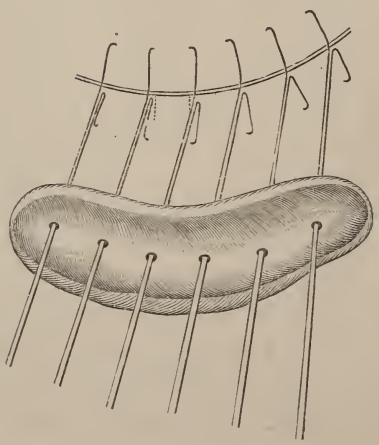

FIG. 5.

upon them, the latter being represented here on the double ends of the wires, sliding down to its proper place.

I will here cite another case, a lady of this city, upon whom we recently operated successfully with this form of suture, which is of unusual interest, from the circumstances attending its history and the result obtained:

Case II.—Mrs. G.; aged 34; medium stature; well formed; rather stout; nervous temperament; health to all appearances good. While on a visit to Long Branch, September, 1854, was taken in labor, and after five days was delivered, without instruments, of a still-born child, said to have weighed eighteen pounds. A vesico-vaginal fistule was the result—incontinence of urine being discovered almost immediately after the completion of labor. She states that she was not able to leave her bed for several months, owing to a loss of power over the lower extremities.

Returning to this city, she applied to a surgeon for relief, who (January 8, 1855) performed an operation for closure of the fistule, found to be situated just across the root of the urethra, and large enough to admit two fingers into the bladder. The clamp suture of Dr. J. M. Sims was employed, which was attended with only partial success, the failure being at the left angle. Little or no benefit, however, resulted, as regarded the incontinence.

September, 1858, a second operation was performed by the same surgeon, the clamp suture being employed again. A total failure resulted this time from the suture apparatus sloughing out.

December, 1861, the same surgeon performed his third operation, using the clamp suture again. Now union took place in the middle of the fistule, leaving a small opening at each angle. Still the incontinence of urine continued with but little if any abatement.

February, 1868, she was admitted into the New York State Woman's Hospital, and was operated upon there the fourth time by the distinguished surgeon at the head of that institution. Now both of the small fistules above described, as we are informed, were converted into one by a division of the intervening slip of tissue. The opening thus formed was then closed with the interrupted silver suture. The result was a total failure.

In April the same surgeon performed his second (the fifth) operation, using again the interrupted silver suture. Result, a partial closure, though little or no control over the urine was afforded, the incontinence proving to be about as it was after the third operation.

In September the patient was admitted into our private institution. A fistule large enough to admit a No. 6 bougie was found in a mass of cica-

tricial tissue, situated just above the left angle of the original fistule, far to the left side. October 17th, the parts being found in a suitable condition from our preliminary treatment, we proceeded to close the fistule with our button suture, the patient being placed in the position previously described, and our small-sized speculum introduced. Present, Dr. W. O. Baldwin, President-elect of the American Medical Association, and Drs. Finnell, Nott, Sabine, Carroll, and Morton, of this city.

After cutting out pretty freely the hardened tissues resulting from the repeated operations which had been performed, we found it neeessary to introduce only three sutures, which was quickly done, and the whole secured on our button principle. The duration of the operation did not exceed three-quarters of an hour, including the time taken up in explaining the several stages of the operation to the gentlemen present. The patient was kept all the while under the influence of chloroform, which had a most happy effect. As an interesting fact, we will state that this was the fifty-second time that this patient had been anæsthetized.

Nothing unusual occurred in the after-treatment. An elastic catheter No. 6 was kept constantly in the bladder, and the patient required to lie upon her right side. On the eighth day we removed the suture apparatus, and found the fistule entirely closed. After a few days the catheter was discontinued, and the patient then allowed to leave her bed. To her great delight, she now found that she had entire control over her bladder, this power having been lost upward of fourteen years. She was discharged a few days ago, feeling perfectly sound and well.

We would remark here that this lady, an intelligent person, during her long affliction, as might be supposed, watched very closely the results of treatment in her own case, and she indulged a peculiar fancy of collecting the various sutures which from time to time were used upon her, including *clamp suture*, *interrupted silver suture*, *and button suture*. Such a collection of sutures, I venture to say, the misfortunes of but few women have ever supplied. From the interest therefore attached to these relics of scientific progress, the long-suffering of the patient, and the final triumph of art in the case, it cannot be considered out of place here to examine critically these several surgical appliances which have been brought into such close juxtaposition by their application to one and the same fistulous opening.

The collection, as seen, we have placed upon a piece of buckskin, so that every part of the respective forms of suture can be examined in its proper relationship. The first surgeon alluded to in connection with the early history of the case performed and has seen performed five of the six operations described, and to him we are indebted for not only a correct and reliable history of the case from the beginning to the end of treatment, but the arrangement in the order of their use of the various forms of suture named, which is as follows:

1. The clamp suture, three applications—unsuccessful.

2. The interrupted silver suture, two applications —unsuccessful.

3. The button suture, one application—cure in eight days.

CLAMP SUTURE.

The *first pair of clamps* is one and five-eighths of an inch in length, and each clamp has four holes in it, corresponding to the four sutures used. In the piece of buckskin I have made a slit one and one-half inches in length, corresponding to the length of the original fistule. To this slit or opening I have applied and secured the same clamps just as they were used in the case. By examination of the model, a most excellent idea can be had of the peculiar mechanism of this form of suture. Although great care here was taken to introduce the sutures on a line equidistant from the edges of the slit, yet there is a want of parallelism in the two clamps. The result of which is, slight inversion near one extremity of the line of approximation, and eversion toward the other, a very common cause of failure in the use of this suture when it was in vogue.

A view of the edges of the slit on the opposite side, supposed to be in the bladder, will satisfy the most casual observer, I dare say, why this form of suture was formerly attended with so little success. The line formed by the coaptated edges, as may be seen, is imperfect, and the latter move freely upon each other. There is, so to speak, a rocking motion of the two edges, and fluid at almost every point can insinuate itself and reach the opposite side.

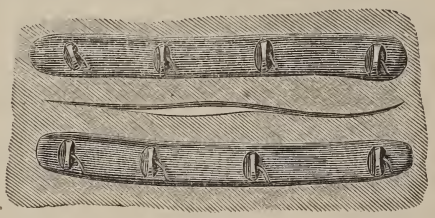

FIG. 6.

Fig. 6 affords a very good idea of this form of suture, and the original size of the fistule.

The *second pair of clamps* employed in the case, as here shown, is one and one-quarter inches long, with which three sutures were used. This operation was a total failure, as may be inferred from the peculiar condition of the clamps, which are seen to be incrusted from one end to the other with earthy matter, a circumstance which very often caused, in this operation, the included tissues, clamps, and sutures all to slough out, leaving an opening two, three, and four times the size of the original fistule.

The *third pair of clamps* employed has been lost, which we regret very much, though, from what we can learn, they were about the same length as the last pair described.

INTERRUPTED SILVER SUTURE.

We come now to speak of the interrupted silver suture employed in the fourth and fifth operations.

The patient was not able, unfortunately, to secure the sutures used in the first of these operations, but she was told that nine were introduced. The number of sutures employed in the second operation was thirteen, seven of which are here shown, the other six having been lost.

We have made in this same piece of buckskin a slit an inch in length, corresponding to the size of the fistule at the last operation, judging from the extent of the cicatrix left. The thirteen sutures, the number then used, we have introduced and secured in the usual way by twisting. The twisted ends of each, half an inch long, are turned down flat, alternately upon the right and left. The introduction and adjustment of these sutures will be recognized by every one as the plan usually followed by experienced surgeons, both as regards the distance of introduction from the edges of the slit and the intervals that usually separate them. It is a principle insisted upon by the advocates of this form of suture to introduce all the sutures as near the edges of the fistule as possible, rarely exceeding the eighth of an inch. In fact, the rule is absolute, and cannot be departed from without endangering the success of the operation by causing inversion. Especially is this the case when the fistule is of considerable size, and the traction is consequently great upon the sutures. The turning of the double ends of the wire down flat upon the tissues on the two sides of the line of approximation is a feeble attempt to counteract this tendency of the edges of the fistule to invert. The wire splint thus formed, it must be admitted, affords some support and steadiness to the coaptated edges; still it cannot prevent their inversion, excepting in small fistules where there is a redundancy of tissue.

We would direct attention now to the approximated edges of this slit on the other side of the piece of buckskin, supposed to be the vesical side of the septum. Here they are seen to gape from one angle to the other, and every suture can be counted at the bottom of the triangular furrow formed. Inversion of the edges exists throughout, notwithstanding the extreme nearness to them at which the sutures are introduced on the opposite side, where closure is as firm and complete as it is possible to make it. The effect of this inverted condition of the edges is that very little over half of the thickness of the buckskin or septum is in contact; the balance of the two surfaces thus separated is exposed, as in the bladder, to the poisonous action of the urine. Those who have had experience in such matters need not be told how uncertain any operation must be with the edges of the fistule brought together in such a relationship.

The crowding of the sutures in, as we have shown, to prevent inversion, and to secure some steadiness and support to the included tissues, is a practice also very often fraught with great mischief, as adopted in large fistules when the traction is necessarily great. The circulation under such circumstances is liable to be interrupted, and the vitality of the included tissues destroyed, resulting very often in a slough and loss of tissue, which may seriously jeopardize the success of a subsequent operation.

BUTTON SUTURE.

Let us next examine the button suture. Here is to be seen a leaden plate, about one-twentieth of an inch in thickness, one inch in length, something over half an inch in width, formed as before described, and along the centre three perforations or holes, a quarter of an

inch apart. The three sutures and the perforated shot used with it are placed by its side in the collection. The whole of the apparatus is here shown just as it was removed from the seat of the fistule in Case II., on the eighth day of the operation.

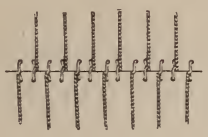

FIG. 7.

FIG. 8.

Fig. 7 shows the interrupted suture, the number employed in the case, and the peculiar mode of adjusting them, to secure the advantages of a wire splint.

Fig. 8 is intended to show the button suture as it was applied in the case.

Now we have made a third slit in this same piece of buckskin, which is precisely the same length as the one used to illustrate the application of the thirteen interrupted silver sutures. Here we have introduced three sutures only, at a distance of upward of a quarter of an inch from the cut surfaces or edges, about a quarter of an inch apart, and have secured them with just such a button and perforated shot as above described. The coaptation of the edges, as shown, is smooth and perfect throughout, notwithstanding there are but three sutures used, ten less than of the interrupted silver suture. On the opposite side, supposed to be in the bladder, the seat of the operation can scarcely be made out. Considerable force upon the parts is even required to indicate the line at a single point formed by the coaptated edges, and as to motion of the tissues included within the loops of the sutures there is none. Perfect coaptation, steadiness, support, and protection are afforded at all points. The edges here, instead of being inverted, as shown in the application of the interrupted silver suture, are turned away from the

bladder, as indicated on this side of the septum, by a depression at the seat of operation. In this position and relationship they are maintained throughout the reparative process.

The difference between the principle and mechanism of the button and interrupted silver sutures may be briefly summed up as follows:

BUTTON SUTURE.	INTERRUPTED SILVER SUTURE.
1. Large wire, No. 26.	1. Small wire, Nos. 28 and 29.
2. Three to five sutures to the inch.	2. Five to twelve sutures to the inch.
3. Sutures introduced from a quarter to half an inch from the edges of the fistule.	3. Sutures introduced from a sixteenth to an eighth of an inch from the edges of the fistule.
4. Sutures secured with a leaden plate and perforated shot.	4. Sutures secured by twisting and turning their ends down flat to form a wire splint.
5. Sutures not exposed in the bladder.	5. Sutures all exposed in the bladder by gaping of the edges of the fistule.
6. Edges of the fistule turned out, and controlled in that relationship.	6. Edges of the fistule turned in, and not controlled.
7. Perfect steadiness and support of the approximated edges of the fistule.	7. Little or no steadiness of the approximated edges of the fistule.
8. Protection of the denuded edges of the fistule from irritating discharges.	8. No protection of the denuded edges of the fistule.
9. The apparatus easily and quickly removed.	9. The sutures troublesome and tedious to remove.
10. Operation as certain in large fistules as in small ones.	10. Operation has but little certainty in large fistules, success depending on frequent repetitions.

These are the essential differences in the above two forms of suture which, in general practice, have been repeatedly demonstrated and proven, not only in the same class of cases, but upon the same fistulous openings—the most practical and conclusive test to which any operative procedure could possibly be subjected, as all must admit. And the result of no operation, under whatever circumstances performed, can be so well seen and noted as that of vesico-vaginal fistule.

In this manner were proven, some years ago, the many faults of the old clamp suture which we have here described. One of several cases, in which both this and the button suture were successively applied, we will mention. In this case, during a protracted course of treatment of three or four years, the former suture was applied by Dr. Sims himself twelve or fifteen times without success.[1] With the button suture, the two fistules that remained were closed each in eight days with the result of a perfect cure, and we have not the slightest doubt, from our somewhat extensive experience since, that in the second case here cited the same result could have been secured in the outset of the treatment by the latter form of suture.

The ultimate and complete success with this suture in the case may be attributed by some to advantages gained by previous operations in diminishing the size of the fistule, but such an explanation is without foundation, and can result only from a want of experience or familiarity with the principle of the operation. According to the facts stated, at the time of the fifth and sixth, the last two operations, the fistule was about the same size, but how different were the results!

The final operation, we are warranted in saying, would have been easier and just as certain had the fistule been of its original size and condition, admitting readily as it did two fingers into the bladder. As we found it, there was an obstacle to contend with not present at the beginning of the treatment, which now seriously militated against success, namely, cicatricial hardening of the tissues from repeated operations. This feature of the case is of importance, and should not be lost sight of in the estimate of the final result.

[1] See Louisville (Ky.) *Review*. May, 1856, Case IV.

As to the size of any fistule, provided the edges are movable, it is a matter of no consideration—the operation with this form of suture being just as certain when large as when small. Case I. affords the strongest proof of the fact. Here the fistulous opening was large enough to admit three fingers, instead of two, as in Case II., and yet in the former case the operation requiring seven sutures was just as successful and satisfactory as in the latter, where only three sutures were called for. This, we may add, is an important and distinguishing peculiarity of this button suture, and explains why the percentage of successful operations in a given number of cases does and will exceed that of all other known methods.

In speaking here of the results of these practical tests in the same cases, we wish it understood that no disparagement is meant to the skill of the several surgeons who preceded us. The entire competency of all of them to secure the greatest amount of attainable success with their favorite forms of suture is admitted, and we cheerfully accord to them as much judgment in the operation as we have, or any other surgeon or surgeons. The difference in the results of practice shown is to be found in the methods of operating, not in the superior skill of one surgeon over another.

The mode adopted of presenting the subject is intended to get at facts, and facts only, regarding the respective merits of the several forms of suture described. We think the importance of the subject warrants it, and, if there is a difference in the results and practice favorable to one or the other, the profession should know it.

What we wish is a fair and honest report of cases and results of operations of whatever form of suture

adopted. Facts will speak for themselves, and, when well authenticated, even one, to the practical mind, is worth a thousand opinions or assertions of any one, it matters not who he may be, or what are his opportunities and advantages.

Proceedings of Societies.

MEDICAL SOCIETY OF THE COUNTY OF NEW YORK.

Stated Meeting, January 4, 1869.

Dr. Geo. T. Elliot, President, in the chair.

After the reading of the Secretary's minutes, the President announced that, at the last meeting of the Comitia Minora, it was voted to grant certificates of membership to Drs. A. H. Smith, J. C. Nott, and Joseph Kammerer.

The reports of the various committees were then presented, after which the paper of the evening was read, by Prof. Wm. A. Hammond, M. D., on the "Physiology and Pathology of the Cerebellum."

(As this paper will be published in full in the forthcoming number of the *Psychological Journal*, we present here only the points which, in the author's opinion, may be accepted as established in the present state of our knowledge.)

1. The cerebellum, contrary to Gall's theory, exercises no special or exclusive influence over the generative organs. For, although it sometimes happens that injury or disease of this organ produces aberration of the sexual impulse, the relation exists with other parts of the brain and with the spinal cord.

2. It has no special or exclusive power to coördinate the muscular actions, and this proposition is supported by the following facts:

a. The consequences of removal of the cerebellum, if the animal survives the immediate effects of the operation, are not enduring.

b. The entire removal of the organ from some animals does not apparently interfere in the slightest degree, even for a moment, with the regularity and order of their movements.

c. The disorder of movement which results in birds and mammals, immediately after injury of the cerebellum, is not due to any loss of coördinating power, but is the result of vertigo.

d. The phenomena of cerebellar disease, or injury, as exhibited in man, are not such as show any derangement of the coördinating power.

e. In those diseases, such as locomotor ataxia and aphasia, of which the chief phenomena relate to derangement of the coördinating power, the lesion is not in the cerebellum, and the symptoms are altogether different from those which are due to cerebellar disease or injury.

These several propositions were supported at length by arguments drawn from experiments and pathology.

Prof. J. C. Dalton expressed his warm admiration of the paper just read; briefly alluded to the several theories it had reviewed, as showing the intrinsic difficulty of the subject; and suggested some general reasons for their successive rise and fall. He spoke of the light we should naturally expect to find thrown upon a problem of this kind by comparative anatomy; and of the false coloring it was liable to give, unless we adopted the most careful precautions. Among these were two especially important:

1. In comparing the sizes of a given ganglion in different animals, we must consider not only its absolute size, and its size with reference to that of the animal, but also the proportion it bears to the rest of the brain.

2. We must compare animals not too widely apart in the scale, if we would reach trustworthy conclusions. Suppose, for instance, that, to test the theory of the coördinative power of the cerebellum, we were to compare the cod and the porpoise. They are of equal size, live in the same waters, have similar habits, have swimming movements almost precisely alike, differing only in direction. We might expect, on our theory, to find the cerebellum of the same size in each. But the one is a fish, the other a mammalian, and we should find the former with a much smaller cerebellum, and also with the whole brain much smaller. Compare, on the other hand, the dog and the cat. They are as similar in their general habits

as were the animals just now considered. Their intelligence is about equal, and their brains, as a whole, are about equally developed. But the cat is far more varied and delicate, and fastidious in her movements; and her cerebellum shows a much greater proportionate size.

The speaker thought that, on the whole, the results of comparative study and of experiments made it probable that the theory, that the cerebellum is the muscular coördinator, is substantially correct, though perhaps requiring some degree of modification. The objection, that the disordered movements consequent upon removal of a large part of the cerebellum soon give place (where the animal lives) to regularly coöordinated ones, was to be met by supposing that the cerebellum, like other parts of the brain and other organs of the body, would bear the loss of a certain amount of its tissue; and that the paralysis of function at first observed was due to shock of the organ as a whole.

A very striking and important part of the paper was that discriminating between vertigo and the loss of the power of coördination. The term vertigo should include in its definition the loss of power to estimate distances and positions. The distinction Dr. Hammond had drawn would probably be of great service in clearing up the subject. Possibly the cerebellum might preside over the function of locating external objects, and owe to this its influence upon coördination.

Prof. Austin Flint, Jr., after characterizing the paper as the most complete exposition and the fairest discussion of the facts bearing on the question which had yet appeared, said that he would glance at two or three of the topics from a different point of view.

The value of the experiments, in which a definite part of the cerebellum was supposed to be removed, was much diminished (except in cases where the supposition was verified by *post-mortem*), by the great difficulties of the operation, when performed upon birds; and by the lower organization of the animals when performed upon reptiles, to which we had always to have recourse, if we would remove the whole ganglion. The chief difficulties were the excessive hæmorrhage encountered and the risk of touching the medulla oblongata,

rendering it impossible to estimate just how much of the organ one had excised. In experimenting upon reptiles, the discrimination between purely reflex motions and those due to voluntary action, was a matter of the greatest moment. Among the former might justly be classed many, which, to a casual observer, would seem to indicate special coördinative action. An essential point, in all experiments on the nervous system, was to use no anæsthetic.

Accepting the coördinative function of the cerebellum, Dr. Flint explained its return after lesion to the organ substantially as Dr. Dalton had done. He illustrated, by reference to the kidneys, the fact that Nature provides for all important functions an excess of apparatus over ordinary needs. A man may live in health for years with one kidney completely disorganized. Dogs show no sign of suffering from the removal of one kidney, unless something chances to derange the function of the remaining one, when they die of uræmia. So when cerebellar disease is slow in its progress, exciting no disturbance of the uninvaded portion of the ganglion, we may have no symptoms until the disorganization has progressed so far as to leave an insufficient portion of the organ to meet the functional demands. This explained, in accordance with the coördinative theory, some cases that might otherwise seem to militate strongly against it. The case that had been cited of the girl, who lived to be eleven years old, and yet was congenitally destitute of a cerebellum, presented more difficulty. But perhaps the coördinative power might have its place partially supplied by some other associated but not identical power; just as, in persons blind from birth, we find the touch and the special senses miraculously acute, and doing much to compensate for the lack of sight, though of course unable to give the faintest conception of vision.

Dr. Flint concluded that the view advanced in the paper, that the cerebellum was simply a supplementary ganglion, intended to reënforce the other parts of the brain, could not be sustained. It had a function of its own, and that function had some connection with muscular movement.

Reviews.

I.—*The Diagnosis, Pathology, and Treatment of Diseases of Women, including the Diagnosis of Pregnancy.* By Grailey Hewitt, M. D., Lond., F. R. C. P., Professor of Midwifery and Diseases of Women, University College, and Obstetric Physician to the Hospital, etc., etc. First American (from the second London) edition; revised and enlarged. With one hundred and sixteen Illustrations. Philadelphia: Lindsay & Blakiston. 1868. 8vo. Pp. 707.

II.—*A Practical Treatise on the Diseases of Women.* By T. Gaillard Thomas, M. D., Professor of Obstetrics and the Diseases of Women and Children in the College of Physicians and Surgeons, New York, Physician to Bellevue Hospital, etc., etc. Philadelphia: Henry C. Lea. 1868. 8vo. Pp. 625.

III.—*The Diseases peculiar to Women, including Displacements of the Uterus.* By Hugh L. Hodge, M. D., Emeritus Professor of Obstetrics, and the Diseases of Women and Children, in the University of Pennsylvania. With Illustrations. Second edition; revised and enlarged. Philadelphia: Henry C. Lea. 1868. 8vo. Pp. 531.

IV.—*A Handbook of Uterine Therapeutics, and of the Diseases of Women.* By Edward John Tilt, M. D., Member of the Royal College of Physicians, Consulting Physician to the Farringdon General Dispensary, etc., etc. Second American edition; thoroughly revised and amended. New York: D. Appleton & Co. 1869. 8vo. Pp. 345.

I. The diagnosis of the diseases of women was the main purpose of the first edition (1863) of Dr. Grailey Hewitt's work; pathogeny and treatment were secondary and digressive, and hence got but scanty notice. The present edition has, in some measure, been recast, and large useful additions have been made to the chapters on Pathology and Therapeutics, and they are now quite satisfactory. The disadvantages of a faulty arrangement are, however, as prominent as before, making repetition unavoidable, and obliging the reader to re-

fer back and forth to fill in the subject. Dr. Hewitt's style is neither elegant nor idiomatic, but it is plain, direct, and readable; and both as a writer and an observer he is conscientious, painstaking, and practical. The number and good quality of the original illustrations in this volume are noteworthy; there are over sixty, and the greater part are carefully-planned representations of cases which have been under the author's observation; they were first drawn in outline, life size, by himself, and afterward reduced by the camera; they embody the results obtained by examination of the case while under treatment. By these means an exact idea of the size and relations of certain pelvic tumors is attempted to be given by two sectional views of the same one, and often quite happily.

Part I. treats of diagnosis, the first section giving the data obtained without physical examination, and the second the data obtainable with physical examination. The former includes menstrual derangements, hæmorrhages, and substances expelled from the generative organs, non-sanguineous discharges, disorders of micturition, symptoms referable to the rectum, abnormal sensations referable to the sexual organs, etc. In the latter the several ways of physical exploration, the instruments to be used, and the parts and conditions to be noted, are described. In this section the chapters on the examination of the abdomen are particularly full, clear, and good. To our mind it is the most valuable part of the book, and well worth careful study.

Pathology and Therapeutics occupy Part II. In treating of the disorders of the womb and its appendages, Dr. Hewitt shows an acquaintance with, and an intelligent appreciation of, what has been done by others, and large and cultivated personal observation. He is a sensible practitioner, and understands the laws of life, and therefore owns the importance of the body as a whole, and the need of keeping up general vitality even when disease seems to be most local. If in his therapeutics there is nothing strikingly new, there is much that is discreet, and little countenance is given to methods and remedies of doubtful character. It is often as important to know what to avoid and when not to do, as it is what to use and

when to meddle. We could wish that some of the chapters of this part had been fuller, and that on some points more decided language had been held.

In a supplementary chapter Sterility is considered, though much on this subject had been anticipated in the body of the work, when speaking of the mechanical treatment of dysmenorrhœa, vaginismus, leucorrhœa, etc.

We rate Dr. Hewitt's book as one of worth. It will be particularly useful to those who value accurate and discriminating diagnosis, and clear and careful investigation of symptoms.

II.—Circumstances beyond our control have made us regretfully put off the publication of this notice of Dr. Thomas's Practical Treatise until now. The penalty is that we miss the pleasure and credit of having foretold its success. The voice of the profession has justly established it where the praise or blame of the reviewer can be of small avail. In this volume the whole subject of diseases of women is gone over with surprising thoroughness and ability, and we know of no work, of the same scope and character, of which a first edition is so free of shortcomings as this. A marked feature is the orderly arrangement and excellent proportion of the vast material the author had to handle: this has been done in a business-like way and with rare discretion; each subject has its due allotment, and the author has been but seldom tempted to indulge a preference, or slight an ungenial theme. The work is singularly compact as well as comprehensive, and while conspicuous for its completeness, it is free, both in its parts and as a whole, from diffuse abundance. That there should be deficiencies and blemishes is in the nature of things; but many of these are of a kind which, while they do not greatly affect the present value of the book, will most likely be lessened or abated in after editions; others belong to the very design of the author, which is, to set forth the "latest aspect" of gynecology. In doing this, Dr. Thomas has shown much book-study, and fair dealing with the views of others, and has well utilized a large and mature clinical experience. We would prefer at times more individualism, less listing of the views of others, and

greater generalization of the author's own experience. We venture this remark, because we think that, while his reserve shows a modesty which may raise our respect, it is to the cost of a class of readers for whom this text-book was written, who need the guiding hand of the master, and a certain tone of positiveness, when brought to face the conflict of opinions too often found among those who are looked upon as authorities.

We might wish, too, that the student and young practitioner had been warned against the extreme surgical tendency of the writings of some of the gynecologists of the hour; the doubtful usefulness and great risk of many of the proposed operations pointed out; and the conditions, which warrant a resort to them, clearly set forth.

The portions of the work treating of pathogeny are the least satisfying, and are hardly abreast with the doctrines of the day. French pathological husks have of late fallen into such just discredit, that we regret to find them still disfiguring the study of womb disorders.

Dr. Thomas's first chapter is a rather sketchy historical account of uterine pathology; indeed throughout the volume he constantly "makes former times shake hands with latter," by well-timed references to the authors of the Greek and Roman schools. Justice has hardly been done to the English writers of the beginning of the century, to whom we really owe so much; it has been truly said, "The key-note of the only true and scientific method of investigating uterine diseases has in reality been struck in the admirable works of Clarke and Gooch." The Germans are passed over in silence, and among our countrymen the honored names of Dewees, Hodge, and Meigs, *præfulgebant . . . eo magis quod illorum effigies non videbantur.* There are those who, by dash and "go," slide to the forefront, and win golden opinions of the many; but their hold is unsure and their glory brittle; such do little in the main for science or art: right direction in both is most often given by the patient search of quiet thinkers and earnest workers.

Chapter II., on the Etiology of Uterine Diseases in America, is highly practical and merits attention; it will be read

with interest and profit. Diagnosis of the Diseases of the Female Genital Organs is the subject of the third chapter, which is thoroughly good. Every thing needful is fully, concisely, and intelligibly told. Of one of the chief causes of faulty diagnosis Dr. Thomas says:

> The source of difficulty is almost always the same. The failure has not been due to having drawn incorrect conclusions from the means, but to not having brought these means fully into action, and properly applied them to the solution of the case in hand. In many instances uterine disease being suspected, the physician employs the vaginal touch, and follows it by the speculum. If the os and cervix be diseased, he is successful in diagnosis; but, if not, he becomes discouraged, forgetful of the fact that the rectal touch, uterine probe, dilatation by tents, conjoined with manipulation and other means, should be resorted to, and that, without appealing to these, even the most skilful diagnostician would be as helpless as himself. There are means at our command for exploring every tissue within the pelvis, the uterus, the ovaries, the areolar tissue, etc.; and, until they are brought into service carefully, systematically, and thoroughly, no one can feel that he has done justice to his powers of diagnosis, or allowed himself full opportunity for drawing correct conclusions (p. 62).

The importance, nay the necessity, of the principle of perineal retraction, by which the atmosphere becomes the dilating factor, in ocular examination, is insisted on, and the imperfect results got by the specula in common use are pointed out. "No one," he writes, "who has not tested the two methods of examination is really entitled to an opinion upon the point, and I cannot doubt the conclusion of him who has done so faithfully and intelligently."

In the fourth chapter Diseases of the Vulva are considered, and at the end Coccyodynia is treated of. We must demur to the recommendation to have speedy recourse to "one of the radical methods of cure" proposed by Sir James Simpson for this affection. It is often, we suspect, a reflected symptom of uterine disorder, and disappears when this is relieved. Luschka, however, has lately asserted, that the small aciniform body in the posterior part of the ano-perineal region, which he discovered in 1859, and described under the name of *Steissdruse*, or the coccygeal gland, and as an adenoid appendage to the nervous system, resembling the pituitary body in the cranial cavity, is the seat of cocygodinia (Virchow's *Archiv f. Path.*

Anat. und Phys., Bd. xviii., 1860; *Anat. des Menschen*, Bd. ii., p. 187).[1] If this prove true, the removal or destruction of the so-called gland would be the proper cure of this painful affection.

Rupture of the Perinæum is the subject of Chapter V. Vaginismus follows, and Dr. Thomas, speaking of Burns's operation, and the American modification of it, says "it is at present regarded as the only reliable method of cure;" and yet Scanzoni has recorded one hundred cases successfully relieved without resorting to it. We saw a year ago a horrible instance of mutilation in a young married woman, the result of the revival of this operation. We believe that it is rarely, if ever, justifiable.

Lack of space will hinder us from giving even an abstract of the contents of this volume, and we must limit ourselves to a brief notice of two or three subjects. Vesico-vaginal Fistula is well handled in a few pages, but we were surprised to find, in an article which generally shows such correct and thorough knowledge, no mention of Dr. G. Simon, of Rostock (now of Heidelberg), who has operated more frequently and with larger success than any other European surgeon, and has done so much to put the operation on a true scientific basis.

Six chapters are devoted to the important and vexed question of the inflammatory affections of the uterus. Dr. Thomas begins his general remarks with the following sentences:

> He who desires to become conversant with the diseases of women, to fully comprehend their pathology, and to be successful in their treatment, will do well to make the thorough understanding of inflammation of the non-pregnant uterus the basis of his education in this department of medicine. It is true that many diseased states of the pelvic viscera of the female are due to other causes, but it is not less true that the majority either

[1] After Luschka, Krause (*Zeitschrift f. Rat. Med.*, Bd. x., H. 2), Heschl (*Œst. Zeit. f. Prat. Heil.*, No. 14, 1860), Henle (*Jahresbericht*, 1860), and Kolliker (*Gewebelehre*, 4 Auf., H. 2, § 539), have described it; and, within the last three years, Julius Arnold, of Heidelberg, and G. Meyer, of Göttingen, have given full accounts of its structure and probable affinities. Dr. Alexander Macalister, Demonstrator of Anatomy, Royal College of Surgeons, Ireland, has published (*Brit. Med. Jour.*, No. 367, 1868) a very minute account of this body. He is disposed to think that it is not only a small rete mirabile, but likewise the centre of nerve-power, or a terminal sympathetic ganglion. It has nothing in common with secretory glands, as its closed follicles are certainly vascular and not truly adenoid, and Dr. Macalister proposes to call it glomerulus arterio-coccydeus.

take their rise in this, or in their progress become complicated by it, so that it forces itself constantly upon the notice of the gynecologist as the keystone of the arch upon which rest his knowledge and usefulness. These facts were, to a certain extent, recognized by the physicians of the Grecian and Roman schools, but, judging from those of their works which have reached us, their appreciation did not compare in thoroughness with that of our century. After the revival of letters, the importance of the pathological view, which gave to metritis such prominence and moment, was almost entirely lost sight of until the beginning of the nineteenth century, when Recamier and Lisfranc reinstated it. It has been already stated that to Dr. J. H. Bennet [Henry Bennet?] we owe its dissemination not only in his country but throughout America (p. 189).

We have here the key-note to all which follows in the ample chapters on Acute Endometritis, Acute Metritis, Chronic Cervical Metritis, Chronic Cervical Endometritis, Chronic Corporeal Endometritis, and Chronic Corporeal Metritis.

Dr. Thomas is of the strictest sect of the French school, and the exposition of his views on uterine inflammation, temperate but decided, has a strong Gallic flavor. We cannot quite go along with him. Names, however faulty, become a necessity for want of better ones, and from inconvenience of change, and may or may not convey a true idea of the nature of the disorder to which they are attached, and hence are comparatively unimportant; but a pathogenic theory of precise purpose will, if faulty, work much mischief to medicine as a practical art. We have no room and little mind to discuss at this time the part of inflammation in womb disorders; the few words we will say shall be limited to the subject of chronic metritis. If by this term it is intended to express the equivalent of what is commonly implied by the inflammatory processus, there are good grounds to doubt its accuracy; if used in an arbitrary sense, to express those tissue-changes which follow derangements of nutrition, the result of an irritament, it will serve as well as another, so long as it is not misleading. Lisfranc has been saddled with a large share of responsibility for the doctrine of the overwhelming influence of inflammation in uterine disease. If we rightly understood his teachings, he never went so far; by the word *engorgement*, he meant something more than *congestion*, and less than *inflammation*, as expressions of condition. Read by the light of later

pathological dogmas, he would not be found much astray. But his pupils bettered their instructions, urging them beyond reasonableness and the intention of the master. Their extravagances and onesidedness are not a natural outcome from his precepts. To them, metritis and its whole gamut seem to be as necessary and comforting as the blessed word Mesopotamia was to the pious old woman, and with as much understanding of its meaning. The entity inflammation, fallen from its high and palmy state, is hanging by its eyelids as a pathogenic factor in most of the organs of the body; its last resting-place seems to be the womb, and here still it has a good foothold. Why should uterine pathology alone be cumbered by an outworn theory?

We cannot but believe that the time is not far off when this vexed, but important, question will be reopened, and examined in a fair-judging, and not peremptory and dogmatic, spirit, uninfluenced by prejudice, prescription, or tradition; and that, measured by a new standard, and settled by the requirements of a more enlightened knowledge of the laws of life, present differences will be reconciled, hostile opinions conciliated, and the angry voice of adverse factions be heard "not any more forever." The reproach is often made that timid thinkers find safety in what Bacon called "middle principles;" but we suspect they come across a goodly amount of truth in their refuge.

"Virtus est medium vitiorum, et utrimque reductum."

Scanzoni, who has written an excellent monograph on Chronic Metritis (1863), embodying much observation and experience, and which has been overlooked by Dr. Thomas, says: "There are a great number of cases where not only there is no inflammation at the beginning, but where even its existence cannot be suspected. On the contrary, we may affirm with certainty what are the causes which have provoked the chronic hyperæmia and the consequent anatomical changes; and yet, in such, induration of the uterine tissue, of more or less extent, is noticed. These cases often present great difficulties in their etiological explanation, for we cannot admit an inflammatory beginning. It is possible that, in these

instances of really passive hyperæmia, an exudative processus may intercur occasionally, but we cannot prove it. Supposing even that it were demonstrable, it would not on that account be an essential condition for the explanation of the various lesions of nutrition heretofore described."—(*Die Chronische Metritis*, p. 43.) And he concludes his chapter on the pathology of the disorder in this wise: "The term chronic metritis is not strictly proper in all cases to which the name has been given. Many even of the engorgements of the womb (gebärmutter anschwellungen) have nothing of inflammation in the exact sense of the word; they are nutrition troubles (nutritions störungen), just as we see them happen in other organs after long-continued hyperæmia" (l. c. p. 43).

Of the treatment of chronic metritis Dr. Thomas is not very consoling. He says:

> No one in the present state of uterine pathology and therapeutics can write very positively on this subject, for it really constitutes one of the opprobria of gynecology. The rules laid down for the treatment of parenchymatous disease below the os internum will disappoint us here. We cannot, from the same means, expect the same flattering results. . . Unfortunately, in corporeal inflammation they often all fail to accomplish a good result. Nevertheless, since some cases are relieved by them, and a smaller number cured, it is our duty to essay them cautiously—so cautiously as to feel assured that, if we accomplish no good, at least we shall do no evil (p. 256).

This confession of judgment reminds us of Gil Blas's conversation with his master during the fever at Valladolid: "Sir," said I one evening to Dr. Sangrado, "I take heaven to witness that I followed your method with the utmost exactness, yet, nevertheless, every one of my patients leave me in the lurch. It looks as if they took a pleasure to bring our practice to grief." "I have not often," replied the doctor, "the satisfaction of curing those who fall into my hands; and if I were not as sure of my principles as I am, I should begin to believe them contrary to the disorders I treat." "If you take my advice," I said, "we will change our method; the worst that can happen will be that we shall be no worse off." "I would willingly," answered Doctor Sangrado, "make the experiment, but I have published a book, and would you have me discredit my own work?"

When we reflect that every ailment which can afflict woman has been referred to some form of uterine inflammation, our author's language is not encouraging to the fair sufferers, nor flattering to our art; but we would fain believe that it is less hopeful than it might be. When tissue-changes happen in any organ to the degree so frequently met with in those long-lasting disorders which collectively go under the name of chronic metritis, a return to health is tedious and often incomplete. Yet we believe, as we shall come more generally to look upon this class of womb troubles as we do upon like textural alterations in other organs, our difficulties will lessen, and failure will be the exception and success the rule. With a truer pathology and more rational therapeutics, there will be less trust in local means, and larger faith in constitutional treatment. Strong testimony in behalf of these views has been lately given by Dr. Henry Bennet, the chief British apostle of the inflammatory theory. At a meeting of the Obstetrical Society of London, October 7, 1868, he said: "He was profoundly convinced, that the real benefit to be derived from the treatment of uterine diseases generally was oftener prospective than present. For his own part, *he looked for the real result six months, a year, or more, after the entire cessation of treatment. . . . Once treatment ended, he usually placed his patients under a good home hygiene system*, including cold-water uterine douches, and *then*, if the patient be really free from inflammation, *Nature unaided generally does the rest, fines down hypertrophied tissues, and restores the uterus to its natural size.*"—(The *Lancet*, October 24, 1868.) We have strong hopes for the phlogistic school; Dr. Bennet has made a long stride toward truth.

If space allowed it, we should be glad to follow the author through the volume, but we can only briefly enumerate some of the remaining chapters. In treating of uterine displacements, no mention is made of a purely mechanical variety of prolapsus which happens in women past child-bearing age, and is due to absorption of the fat, and the atrophy of the other tissues, which pad the pelvis and help to keep the womb in place; a form to which Dr. Barnes particularly has directed attention, and names prolapsus from senile atrophy.

Dr. Thomas, speaking of the radical cure of procidentia uteri by surgical means, says "the operation has been known and practised for a long time on the continent of Europe, especially in Germany," but he does not distinguish between Elytroraphy or Kolporaphy, in which an attempt is made to narrow the vagina, and Episioraphy, or closure of the external labia. The first notice that we find of Kolporaphy (*kolpos*, vagina, and *raphe*, suture) is in the *feuilleton* of the *Gazette Médicale*, January 8, 1831, in a letter from Berlin. Speaking of Dieffenbach, the writer says: "Il a le premier guéri les prolapsus de l'utérus au moyen de l'excision d'une partie de la circonférence du vagin. La cicatrice qui se forme rétrécissant le diamètre du vagin ne permet plus la sortie de l'utérus. La même operation a été exécutée avec succès pour le prolapsus du rectum." Here we have the so-called American operations for cystocele and rectocele. In 1836, Dieffenbach, in a paper published in the *Medicin. Zeitung*, No. 31, 1836 (*American Journal of Medical Sciences*, August, 1837, p. 509), states, that besides several cases of prolapsus associated with sloughing of the vagina, in six of which there were radical cures following cicatrization, he had frequently performed the operation of dissecting away strips of the mucous membrane of the vagina, and bringing the parts together by suture. Some of Dieffenbach's operations must have been done, then, as early as 1830. In November 26, 1831, the late Dr. Marshall Hall published a case of procidentia uteri which had been operated successfully on by Mr. Heming, of Kentish-town, by a new method suggested by him to the operator. After the womb had been pressed down by the patient, a strip of the vaginal mucous membrane, two and a half inches wide, was removed from the neck of the womb to the outlet, on the anterior face; a stitch was put in through both edges of the wound, near the neck of the womb, the womb itself returned, and the threads tied firmly (*Medical Gazette*, vol. ix., 1831). On the 24th of November, 1833, Mr. Ireland, of Dublin, operated on a case by a modification of Hall and Heming's method. He made parallel incisions on the sides of the vagina, to avoid the bladder and rectum, and which converged at their extremities. He applied all his sutures before return-

ing the womb (*Dublin Journal of Med. and Chem. Science*, vol. vi., p. 484, 1834). In August, 1835, Velpeau and A. Bérard presented to the Académie Royale de Médecine a case each of procidentia uteri on which they had operated by Ireland's method somewhat modified. Velpeau describes his operation as follows: "Trois lambeaux étaient enlevés de la muqueuse du vagin—un antérieur, et deux latéraux—chacun large de dix lignes; chaque lambeau était pris de partir de la vulve et eu deux et demie pouces de long." The ligatures were inserted by both operators before the womb was put back. Velpeau's patient was fifty years of age; nothing untoward followed in either case, and the hæmorrhage at the time was slight.—(*Gaz. Méd.* 1835; *Archives Gén. de Médecine*, 2ième serie, t. viii.) On the 25th August of the same year, Dr. Gerardin, of Metz, sent a communication to the Académie, in which he claims that, in a paper read before the medical society of his town, in 1822, he had proposed, in cases of procidentia uteri, to contract the vagina, or in women who had ceased to menstruate to obliterate it, by the removal of a zone of the mucous membrane of the vagina around the prolapsed tumor. He does not assert that he had even operated; indeed, he owns frankly that by his reclamation: "Il a voulu assurer à l'operation son origine, et maintenir à la chirurgie française la priorité de l'invention si non celle de l'exécution." (*Gaz. Méd.*, 1835, p. 558.) This charming *naïveté* reminds us of Lamartine's patriotic egotism, in his assertion that, when God has a new idea to give to the world he always puts it in the head of a Frenchman. Tanchou claims to have done the operation, but kept it a secret until after the publication of Heming's case, when he put in his claim to priority. The late Dr. Laugier, in 1833, cauterized the vagina in prolapsus with the acid nitrate of mercury, and the white iron, with a view of causing its narrowing; and in his communication to the Académie de Médecine, August 25, 1835, states that he had then in the Hôpital Necker a patient recently cured by that method. Dr. Evory Kennedy, of Dublin, in a letter to Sir Benjamin Brodie (*The Lancet*, June 8, 1839), mentions having treated many cases of falling of the womb successfully by the actual cautery at a white heat. He applied it high up, taking in more or less of

the circumference of the canal, and producing an eschar varying in extent, according to the degree of relaxation. He had used acids and caustics, but found them inferior to the iron at a white heat. Mr. A. Colles at the same time was using the cautery, but he made a ring-eschar high up in the vagina. Dr. Kennedy gives no dates. Mr. Benjamin Phillips, in the winter of 1837, used fuming nitric acid in a case of procidentia, brushing it over a large surface of the vagina with a camel's-hair brush. The pain was not severe nor long continued. The whole thickness of the mucous tissue sloughed, and there was a granulating surface. At the end of a month there was no sensible improvement, but, from that time, the disposition to displacement gradually lessened and then ceased. On examination, two years afterward, the capacity of the vagina was found materially smaller—reduced to the size of that of a woman who had not borne children.—(*The Medical Gazette*, June, 1839.)

Such were the several means that had been employed to radically cure procidentia uteri, by narrowing the vagina by means of cicatricial contraction, when Dr. Marion Sims first operated, in 1856. His method and Dr. Emmett's modifications of it are minutely described by Dr. Thomas. It may be very naturally asked what are the merits of the principle, and what has been the real success of the operation. When revived by Dr. Sims, it had fallen into disuse in Europe. The cases of Velpeau and Bérard, although at first promising, turned out failures. Of the three cases of the latter, in one the prolapsus returned in twenty days; in another, two operations were done, and both were unsuccessful; the third, exhibited to the Académie de Medécine, in June, 1835, as a 'plein succés,' was reported in August of the same year to be unrelieved. Scanzoni performed kolporaphy, after Ireland's and Velpeau's method, fifteen times, without a single success. We have now under our charge a patient operated upon nine years ago by Sims's method; in a year the cicatrices had given way, and the procidentia returned. Three years ago she was operated on twice by Emmett's method; in little more than a year the bands gave way, and her condition was worse than before, for the vagina was so deformed by the cica-

trices that it became impossible to adjust a pessary. In this case there was a practical loss of perinæum, and it is difficult to imagine how elytroraphy could have succeeded, without having been preceded by perineoraphy. We have heard of another similar case, while this article is passing through the press, where two operations were done a year ago by Emmett's method, and with supposed success, in which the procidentia has been reproduced. Heming's case was examined two years after the operation, at St. Bartholomew's Hospital, and the womb and bladder were found in place.

Dr. Thomas does not speak of episioraphy—*epision*, labium, and *raphe*, suture—or occlusion of the vaginal outlet by adhesion of the labia, thus converting a procidentia into a prolapsus. This operation was proposed and first done by Fricke, of Hamburg, March 3, 1832.—(*Annal der Chirurg.-Abtheilungs der Allgemeines Krankenhauses in Hamburg*, Bd. 2, 1833.) In 1835 he sent a communication to the French Academy of Medicine, reporting four cases and three successes. In 1839, Dr. Geddings, of Charleston (then of Baltimore), performed episioraphy four times; two of the cases were known to have been successful, and of the other two no doubt was entertained of the results, from the condition of the parts when the patients went to the country.—(*American Journal of the Medical Sciences*, August, 1840.) We cannot find that subsequently Fricke reported any more successes; Loscher had a happy issue, and Knorre one, but several failures. Scanzoni tried it four times, and failed; Roux and Velpeau, each once; and Stoltz several times. Malgaigne performed an operation similar to Dr. Geddings, in 1837; and Dr. Simon, now of Hiedelberg, several subsequently; and they were all failures.

With regard to the several methods that have been proposed for the radical cure of this infirmity by surgical means, we confess, after much examination of the subject, and from our own observations, to have come to the conclusion that its advocates have been rather over-sanguine as to lasting relief, and to agree with Dr. Courty, who writes:

"The disadvantage of all these operations is, that they expose the patient to the risk of serious dangers for the cure

of an infirmity which, as a rule, gives rise to none; and, moreover, to substitute for this infirmity a deformity, which is only permissible in old women, but which in younger ones may be followed by other troubles, and expose them to new dangers. . . . These operations seem to us only indicated where there is extreme relaxation of the vulva and vagina, and where cystocele and rectocele are present, not only as consecutive elements and secondary complications, but as chief elements and serious complications of the displacement of the womb."—(*Traité Pratique des Maladies de l' Utérus*, p. 749.)

Versions and Flexions of the Uterus are largely treated of, and the multitude of mechanical means for their rectification described, Dr. Thomas giving them the benefit of impartial suffrage.

In speaking of Inversion of the Uterus, and of the several ways of replacing the organ in cases of long standing, Dr. Thomas gives credit to Viardel for the method lately proposed and successfully practised by Dr. T. A. Emmett, namely, "dilating with the fingers the constricting neck and forcing up first the tissue which came forth last" (p. 345). We have both editions of Viardel's work before us, and we can find nothing in Chapter III., *De la Précipitation ou Chûte de la Matrice après l'Accouchement, et de la Manière de la réduire*, which would in the least justify Viardel's right to priority; nor does the accompanying engraving, representing the mode of reduction, show any similarity of method. (2ième ed., p. 345.)

Excellent summaries are given of Peri-Uterine Cellulitis, Pelvic Hematocele, Fibrous Tumors of the Uterus, Uterine Polypi, Cancer of the Uterus, Diseases resulting from Pregnancy, and Functional Disorders of the Uterus. The chapter on Ovariatomy has been prepared with much care and judgment, and is a complete digest, showing the practical acquaintance of the author with the subject. Diseases of the Fallopian Tubes close the volume.

III. M. de Fontenelle is described by Voltaire, under the thin guise of the Secretary of the Saturnian Academy of Sciences, as "homme de beaucoup d'esprit, qui n'avait, à la ve-

rité, rien inventé, mais qui rendait un fort bon compte des inventions des autres." The systematic digester of original material, who cleverly works it up for easy appropriation and daily use, is, in this fast age, unquestionably a blessing, particularly for those whose assimilative faculty is weak. But, however much may be the well-earned popularity of those who fix and epitomize the shifting doctrines of the day, it is most always ephemeral; while they who live laborious days in diligent observation, and register the teachings of experience, who love truth beyond immediate gain, and write, not to put money in their purses, but to better their kind, have, oftentimes after patient abiding, the just meed of enduring reputation. The present eye praises the present object, but time is wonderfully even-handed.

"Scribendi recte, sapere est et principium et fons."

The second edition of Dr. Hodge's work "On Diseases peculiar to Females" embodies the results of the personal experience and reflections of an active and honorable professional life of near half a century. A chief object of the author is "not merely to present what he deems a more correct theory and practice in inflammatory diseases of the uterus, but also to insist that a very large proportion of the so-called cases of metritis are in reality but examples of irritation, where inflammation has subsided, or where it has actually never existed." To carry this out, the chapter on uterine inflammation has been altered and enlarged, and is preceded by an Introduction, in which the views of many high authorities on the several subjects of irritation, congestion, and inflammation, in contrast with those advocated by the author, are given, so that "the difference of opinion and practice may be more distinctly observed." Here Dr. Hodge shows his familiarity with recent gynecological literature, and his criticisms are sober and well-founded. His own views agree with what we know happens under similar circumstances in other organs of the body, and, we believe, that until they are applied to those conditions now grouped under the indistinctive name of metritis, it will continue to be a subject of wrangle to the pathologist, and its treatment a therapeutic failure and reproach. We might

wish that our author had chosen another name for these disorders of the womb than that of Irritable Uterus, which hardly indicates their pathogeny. We have long been satisfied, both from clinical and post-mortem observations, that the destructive process inflammation has, as a rule, no part in these affections, and that we must look for their intrinsic pathological nature in those differential nutritional changes, which, under the influence of the "reiz," or irritation, stimulated by the *momentum excitans*, cause multiplication of the connective-tissue elements, or periplasts, ending in stromal hyperplasia. The time-honored word "plastic exudation" should then be consigned to the tomb of the Capulets, and we must look to overgrowth of the intercellular substance for a true and simple explanation of the processus. To pursue further at this time a question, whose importance cannot be overrated, would be to trespass on our space, and we for the present dismiss the subject, with an earnest invitation to our readers to examine it by the lights of modern pathology. On no subject has there been so much repetition of error from book to book, sometimes wilful, but more often in the spirit of Richter's sheep, every one leaping at the same spot, though the bar had been let down, which made the bell-wether jump. Currency has thus been given to mischievous doctrines that had no other foundation than the prejudice or ignorance of their originators; exceptions have been confounded with rules; and accidents mistaken for essential properties.

A large part of this volume is given to the consideration of the Misplacements of the Uterus. We particularly commend the author's observations on this subject to students and practitioners. It has never before been so thoroughly and scientifically handled. Ample justice is being done to Dr. Hodge's pessaries, and to the principles of treating womb misplacements which he has so long taught. At a recent meeting of the Obstetrical Society of London, strong and willing testimony was given to the excellence of these instruments by Drs. Priestly, Savage, Barnes, and others, and Dr. Tilt, as we shall presently see, thoroughly indorses them. Dr. Savage remarked that "Hodge's was the best pessary, because in shape it corresponded with the contracted vagina,

and with it the lateral vaginal action was unhindered, and the series of curative processes permitted;" and further, he deprecated bringing out modifications of this instrument under new names, "when the one thing to remember was the principle." In these views Dr. Barnes expressed his concurrence.

In uttering the hope that this work may be marked, learned, and digested by all practitioners who may be especially devoted to the treatment of diseases of women, we feel that we only urge the recognition of doctrines which have stood the test of observation and time, and which, when generally accepted, will save much woman misery, and go far toward redeeming one branch of our art from just reproach. But there is another feature of this volume which should not be overlooked, and that is its healthy tone. We have a sensational literature, a sensational drama, and we are, we fear, drifting toward a sensational gynecology. Iatro-mechanical ideas have an irrational and unmerited preëminence; each day brings forth a new machine for stretching, or cutting, or rectifying the womb; and some wonderful feat of the knife is blazoned to catch the credulous. It has lately been well said, that to use remedies with patience and skill, and with an intelligent belief in their powers to do good, requires far higher faculties than are needed to slit the os uteri. There is, moreover, and we regretfully allude to it, a growing leaning toward methods of investigation, which are simply nasty, and outrage decency. The plea that scientific inquiry justifies the means, scarcely deserves serious thought; once admitted, it cannot be limited, and every sort of vileness and crime would get a precious ally and advocate. Science is soiled when put to base purposes; it truly then becomes the apostle's "science falsely so called." The teachings and spirit of such works as Dr. Hodge's are alike contrary to the misaiming cleverness of mere handicraft, to the evils of unnecessary digital or other examinations, and to practices which bring peril to the body and poison to the mind. That we stand not alone in our estimate of the unfortunate tendencies we speak of, the following sentences by a valued contemporary, received after our article was written, will show:

Practitioners who are much consulted about the diseases of these organs, know full well the peculiar mental and moral effects that are produced by the constant direction of the attention to them. There is much reason to believe that, in many instances, morbid uterine sensations are simply intensified and maintained by local treatment, of whatever kind; and there is a strong and growing feeling, particularly amongst country practitioners, that all uterine specialists are not unmixed benefactors to the human race. There is a growing belief that, in a majority of ailing women, there may be found sufficient flexion, or displacement, or congestion, or tenderness, or ulceration of the uterus, to furnish grounds for the employment of local means, and that these means often do more mischief than good. On the other hand, it is perfectly beyond question, that many women, with real and serious uterine ailments, suffer much unnecessary pain and distress so long as local treatment is neglected, and are speedily restored to health and comfort when it is used. On the whole, we lean to the opinion that cases of the latter kind are outnumbered by the former. We incline to the belief, that for every woman who is greatly relieved, or altogether cured, there will be others who leave the hands of the uterine specialist in much the same state as when they went to him, except—and the exception is important—that they have become accustomed to think perpetually about their genital organs, and to have them examined and manipulated. . . . . We would strongly urge, with regard to the sound and the pessary, the famous question that Lord Melbourne was wont to ask with reference to political activity: Cannot you leave it alone? If the doubt were in every case, "Is not this an instance in which local treatment may be dispensed with?" rather than, "Is it not one in which local treatment may be used?" —we are sure that there need be no neglect of any patient requiring it; and at the same time there would be less of the dissatisfaction which is now somewhat widely felt, and not altogether uncalled for, at the prominence given by some specialists to trifling variations in the condition of the uterine organs.—(*The Lancet*, November 28, 1868.)

IV. The profession is largely indebted to Dr. Tilt, first, for his advocacy of the doctrine that the distempers of the womb are not all bounded by the cervix, and that fundal disorder, often connected with affections of the ovary and of the adjacent peritonæum, merits a share of the attention of practitioners; and, second, for having resisted the mechanical bias of the gynecology of the day. When he first made known his views, cervical metritis and lunar caustic were pretty much every thing in uterine pathology and therapeutics. His works are favorites; they are pleasantly written, show industry, information, and straightforwardness, and contain much that is valuable. The "Hand-book of Uterine The-

rapeutics and of the Diseases of Women" has reached a third edition in England, has been translated into German, and a French translation is about appearing. The present edition, the second American, has been revised by the author for the New York publishers.

The main points insisted upon in this work are—(1.) The paramount importance of hygiene for the relief and cure of diseases of women. (2.) The constitutional nature of many of these disorders, and the impossibility of curing them without constitutional remedies. (3.) The manifest reaction of uterine affections on the female system, and the impossibility of curing many womb complaints without surgical measures. (4.) The great value of therapeutics to assuage and cure diseases of women, and a belief in the value of those remedial measures that are as old as medicine itself. We have room only to refer to a few of the innumerable subjects treated of.

In an excellent Introduction, which we commend to all young practitioners, amongst many sensible remarks, Dr. Tilt writes:

> I wish this enlarged edition to be taken as my protest against the disparagement of a large amount of therapeutical wisdom that has been handed down to us for our guidance in the treatment of disease, from one generation of medical observers to another, notwithstanding the advance of civilization, the increase of medical knowledge, and the various systems that have, each in its turn, ruled over medicine. It is not surprising that many of those who devote themselves to the study of the diseases of women should have participated in this loss of therapeutical faith, and should have favored an exaggerated development of obstetric surgery. This work will sufficiently show that I do not wish to disparage the use of surgery in the treatment of diseases of women, ... but it is worse than useless for a host of complaints for which there are approved remedies.—(*Preface*, p. vii.)
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> With rare exceptions, of which cancer is a prominent example, those who treat diseases of women may deal largely in hope, and I feel my duty is not well done toward a patient, if she leaves me without the belief that her cure is certain, though it may be delayed. One great advantage of experience is that it teaches us hope. . . . If their courage can be kept up, so as to make them persevere with more or less active treatment, during one, two, or even three years, one is justified in instilling hope as the best way of working cures. The hope that is held out will, of course, be proportionate to the nature of the disease and its duration, or the practitioner's reputation will be seriously damaged (p. 9).

In Chapter I., "On Surgical Appliances and Modes of Examination," the author, speaking of the univalve speculum—which was the common speculum in use two hundred years ago, and is described by Dionis in his *Cours d'Opérations de Chirurgie*, and figured at page 246 of the 3d edition, 1777—and of the suitable position of the patient, says: "Most women would rather continue to bear the annoyance of chronic uterine affections, than in presence of an assistant submit to be rolled over on the front, in a left lateral semi-prone position." This is not our experience, nor that of any one who makes this, the only reliable and satisfactory method of ocular examination, the rule. On the contrary, patients become themselves satisfied that it is more thorough than other modes, whilst it has greatly the advantage over them on the score of delicacy; and Dr. Tilt himself admits that "a frame of mind eminently calculated to give efficacy to treatment," is one in which the patient "leaves the consulting-room convinced that her complaint has been better understood than heretofore, and by one who is anxious to cure her."

Dr. Tilt is a decided advocate for gradual dilatation of the cervix, and rightly uses it—

when its narrowness prevents the ascent of the semen into the womb, or leads to the retention of the menstrual fluid, and to the formation of clots, which cannot be expelled without great and protracted pain. The same operation is required, whenever the cervical canal does not afford free egress to the mucus and matter secreted by the lining membrane of the body of the womb, or to the membranes which it exfoliates (p. 31).

He adds:

Dilatation must not be attempted until inflammation of the cervix has been subdued, and until that of the body of the womb has become passive, and it is only by supposing that Dr. M. Sims overlooked these conditions, that I can understand his having thrice seen metro-peritonitis follow the gradual dilatation of the cervix for the relief of uterine stricture. While thus asking us to believe that the gradual dilatation of the cervix is a dangerous process, this distinguished surgeon wishes us to admit that no serious results are to be feared when the cervix is so rapidly dilated as to permit, in a few hours, the passage of the index-finger. If the first mode of dilatation be dangerous, the second is more so, although quite justifiable for the diagnosis and cure of internal polypi and of intractable disease of the uterine cavity. The only bad results that I have observed to be

caused by the gradual dilatation of the cervix, are uneasiness, uterine pains, an increase of mucous discharge, a little loss of blood, and the advance of the menstrual period. I have seen no other evil results follow dilatation, so I do not side with those who represent this process as dangerous, and the slitting up of the womb free from danger (p. 34).

With respect to hysterotomy, Dr. Tilt writes very sensibly:

So great has been of late the tendency to represent the slitting up of the cervix as the sovereign remedy for all uterine affections difficult of cure, that the proposal requires to be carefully considered before it be adopted by other British surgeons (p. 282).

The tediousness of the process of dilatation, the difficulty of doing it well, and its inutility in a certain number of cases, have caused the slitting up of the cervix to be preferred by many. I own that in my previous editions I have been too much opposed to the operation, and that it must be considered another of the many improvements that we owe to the inventive genius of Sir J. Simpson. . . . If I have exaggerated the dangers of the operation, others have exaggerated its advantages. The tenor of Dr. M. Sims's valuable work is to represent pregnancy as insured by slitting up the cervix, and the assertion that he had done so 500 times in the Women's Hospital of New York [pretty well in an institution of twenty-five or thirty beds, a portion of which were always occupied by vesico-vaginal fistula cases—Rev.] shows to what extent he carried the practice. . . . I have no hesitation in sanctioning hysterotomy where it is difficult to introduce a No. 1 or 2 bougie, and when the vaginal extremity of the cervix has a conical shape, for in that case it is obviously difficult, if not impossible, for the semen to enter the womb, even if there be no stricture (pp. 350–2).

Dr. Tilt incises his stricture tissues, layer by layer, with scissors and bistoury, in broad daylight, thinking it better to operate "with the full knowledge of what one is doing, rather than to work in the dark, with more or less complicated instruments."

Of arsenic he speaks well, having given it, "with good results, in cases of chronic uterine subacute inflammation, with marked tendency to relapses." This accords with our experience of the drug in this class of affections, and we are glad to record our author's favorable testimony. His views with regard to the Turkish bath, in the treatment of uterine disorders, we can indorse cordially. They are applicable in a limited number of cases, and then, when cautiously used, are decidedly beneficial:

In chronic inflammation of the body of the womb, there is often a state of languor and debility, which predisposes to relapse, and I have found that Turkish baths, taken once or twice a week, brought up the vital powers to the utmost limit of their capability in the shortest possible time. My worse cases of uterine inflammation—those accompanied by obstinately distressing pain or singular nervous symptoms—have occurred in women who had a deficient action of the skin, who were always cold and never perspired. This indicates the Turkish bath (p. 74).

This state of the skin happens in a very large proportion of such cases, and the Turkish bath, as a fillip to cutaneous action, is of much service; this done, it should be laid aside. The mistake that has been made, is making it a method of treatment, and not a part of one.

With respect to Uterine Flexions, Dr. Tilt says: "Notwithstanding all that has been written to the contrary, I still believe that if practitioners would carefully eliminate the congestion or subacute inflammation that attends uterine flexions, they would very seldom require the application of pessaries" (p. 279).

We well remember when Velpeau taught, and demonstrated too, that all the varied forms of uterine suffering were produced by misplacements and flexions of the womb, whilst, on the same day, Lisfranc, at La Pitié, would affirm that they were all due to engorgement, and that le petit maréchal de la Charité did not know what he was talking about. The recent debates in the Obstetrical Society of London show that these opposite views are as stiffly maintained to-day as they were a quarter of a century ago, and that time has done but little to modify them. It is strange to see gynecologists divided into two hostile camps on a question which, as has been remarked, "if special experience be worth any thing, and if the statements of those particular specialists be accepted as trustworthy, ought to be set at rest in a week. . . . We fail altogether to see any real incompatibility between these opinions. Abundant evidence has been adduced in favor of both; and impartial observers can only remain in doubt with regard to the relative frequency of the one or of the other sequence of events."—(*The Lancet*, December, 1868.)

Dr. Tilt admits, however, that pessaries are in many cases

necessary, and says: "One of the best instruments for maintaining the flexed womb in a right position is due to American ingenuity. . . . In my last edition I drew attention to these instruments [Hodge's], and urged their more extensive trial, and I am glad to find that my own experience of their utility is confirmed by Drs. F. Churchill and Graily Hewitt" (p. 280).

The following sentences have much sound doctrine:

> Too great importance has been attributed to ulceration of the cervix, in the production of diseases of women. . . . If this be sound pathology, it stands to reason, that to cure uterine ulceration, the first indication is to strengthen the conservative force by suitable food and stimulants and by tonics. At a rough guess, I dare say that, by these means, some fifty per cent. of ulcerative lesions of the uterine mucous membrane are cured. If, in addition to tonics, vaginal injections, and the other injections set forth in the chapter on Home Treatment, are carefully carried out, perhaps thirty per cent. of somewhat more sever cases of uterine ulceration would get well. With regard to the remaining twenty per cent. . . in such patients ulceration will not heal without caustic treatment (p. 152).

Chapter IV., on Sedatives, may be profitably read. We are satisfied that, were this class of remedies more frequently used, both generally and topically, in uterine affections, patients would be greatly the gainers. The chapter on Caustics is an elaborate one, and the author here is disposed to ride a hobby; indeed, on this subject, he is more the earnest and clever pleader than the impartial judge. We cannot agree with Dr. Tilt as to the harmlessness of the issue treatment by caustic potash applied to the neck of the womb. We have frequently seen a bad condition set up, even when there has been no immediate alarming effects. In Dr. Tilt's hands it may have been followed by all the benefits he asserts, but we still think ourselves justifiable in giving a warning against the practice.

We should be glad to notice other portions of the work, but we must bring this article to a close. Though not always at one with Dr. Tilt, we bear willing testimony to the excellence and usefulness of his book. It will be largely read and duly prized.

Reports on the Progress of Medicine.

OPHTHALMOLOGY.

By Henry D. Noyes, M. D., Professor of Ophthalmology in Bellevue Hospital Medical College; Surgeon to New York Eye and Ear Infirmary.

MUSCLES OF THE EYE.

1.—*Nystagmus.* By Dr. L. Kügel, of Bucharest. [Annales d'Oculistique, tome lix., Mai–Juni, 1868, p. 209. Archiv für Ophth., bd. xiii., ii., s. 413.]

In this affection the incessant oscillation of the eyes has been commonly attributed hitherto to some primary lesion of the muscles or nerves, but Dr. Kügel, in the above article, as well as in others which he has written in the *Archives* for ophthalmology, shows that it has in most cases a very different origin. He divides the cases into three classes: 1. When the trouble appears in early infancy, and is produced by a serious anomaly of refraction, or of the membranes at the bottom of the eye. The anomalies of refraction are opacities in the media, hypermetropic astigmatism regular and irregular, and extreme degrees of myopia. In all these cases the retina receives an imperfect image, and is so feebly impressed as not to excite the due amount of reflex action to enable the muscles to act in harmony. When the muscles have learned to direct the yellow spot of the retinæ upon an object, nystagmus is not liable to be caused by opacities of the cornea or internal disease taking place at a later period. But while some cause of amblyopia lies at the bottom of nystagmus, there is not infrequently an auxiliary muscular defect. Such secondary forms are as follows: the patient holds an object to one side and looks at it, with the head turned to the opposite side. At this angle the oscillation is least. Again, the nystagmus is least when the object is held in the median plane, while it increases if the object be at either side. Class second contains cases of choroido-retinitis, and these do not appear in infancy. Both eyes are always diseased, and to such a serious degree that the macula lutea fails to have its proper superiority over the rest of the retina, and there is no motive for the muscles to fix this point upon an object in preference to other parts.

In class third are included cases of true muscular debility, insufficiency of the recti interni, frequently attended by weakness of the ciliary muscle. The trembling of the eye does not appear until by protracted effort the muscles become exhausted.

Treatment is most satisfactory in the third class, and for them prisms or spectacles are the more usual means, and also tenotomy. In the first class operations may be performed to mitigate the ill effect of opacities in the cornea or lens, viz., iridectomy, keratonyxis, etc., and glasses may also be chosen to correct in some degree the hypermetropia, myopia, or astigmatism. For the second class there is naturally no remedy.

ORBIT.

2.—*Wound of the Brain by Penetration through the Orbit.* By Dr. Riedle. [Schmidt's Jahrbuch, 1868, p. 315.]

A man received a thrust of the point of an umbrella into the inner angle of the left eye. The stick was two and a half inches long, and pushed out the globe, which returned to its place when the stick was pulled out. The

patient was kept under strict antiphlogistic treatment for fifteen days, and no trouble occurred. On the sixteenth day came a chill and violent headache, the pupil became enlarged, the patient could not sit up, soon could neither speak nor swallow.

The left half of the face, the right arm and foot became paralyzed, the left arm became convulsed, and the patient died in delirium and sopor.

Autopsy showed fracture of the left anterior clinoid process and the adjacent portion of the temporal bone. The surface of the sella turcica had a hole as big as a pea, with ragged edges, made by the point of the stick. The ethmoid plate on the inner side of the orbit had been perforated, as well as a part of the frontal and lachrymal bones.

Neither the globe nor its muscles had been injured. The track of the wound was one inch and five lines long, and passed from without inward, and from below upward. The superior turbinated bone and a part of the vomer were also fractured.

In the brain there was no suppuration, but great hyperæmia of the left hemisphere, congestion of the vessels at the base, as well as of the corpus striatum and optic thalamus of *each* side; bloody serum in the right ventricle; infiltration of the pituitary body, and moderate extravasation on the sella turcica.

3.—*Tearing out of the Eyeball by the Ring of a Door-Key.* By Dr. Herrgott. [Schmidt's Jahrbuch, 1868, p. 219.]

The patient was an epileptic of eighteen years, and was seized with a fit while opening a door. She fell with her right eye upon the key, which was in the lock. When seen by the doctor she was in bed, and the eyelids greatly swollen, and at first nothing but severe contusion was thought to have occurred But it was stated that the eyeball had been found hanging in the ring of the key. On examination the globe was discovered to be held by a few shreds of conjunctiva, the muscles were all torn from the sclera, at a few millimeters' distance, while the optic nerve was torn out from its insertion. The patient made a good recovery.

4.—*Fibro-plastic Tumor in a Rectus Muscle.* By Dr. J. Vose Solomon. [British Medical Journal, Jan., 1868—in Schmidt's Jahrbuch, 1868, No. 4, p. 56.]

A tumor was found in the external rectus muscle of the left eye of a child four years old. It was as large as a hazel-nut, hard and smooth, concealed behind the outer wall of the orbit. There had been no injury; it had been noticed several months. In the operation an incision was made in the direction of the fibres, the insertion of the muscle loosened and turned back. The tumor was enclosed in the sheath. It was dissected out of the orbit, and more than one-third of the muscle was sacrificed. The conjunctiva was then properly replaced, and the tendon of the internus divided so as to restore the equilibrium of the muscles so far as possible. Six months after there had been no relapse, and moderate converging strabismus remained. The tumor was firm, white, and of homogeneous structure. Two small cavities, filled with pus, were found within it. Microscopic structure not detailed. Such a case is extremely rare.

NERVES OF ORBIT.

5.—*Comminuted Fracture of Nasal Bones and Right Superior Maxilla; Sinking of Eyeball into Maxillary Sinus.* With Illustration. By Prof. Langenbeck. [Archiv für Ophth., bd. xiii., ii., s. 447.]

Dr. Langenbeck gives an account of a railroad official whose head

was caught between a locomotive and its tender. The eyelids were torn away from the orbit, and a deep wound ran down from the inner canthus to the upper lip. A probe could be passed into the antrum, not a trace of the eyeball could be found; while in the orbit was a bluish-black pulsating mass. The nasal bones comminuted. Patient conscious, but sleepy, pulse slow, violent pain on right side of head. A week afterward, as head-symptoms disappeared, and the extravasated blood had been somewhat absorbed, a closer scrutiny could be made. The eyeball was discovered to have escaped from the orbit into the antrum by a hole, in the orbital margin of the upper jaw, big enough to admit the finger easily—the axis of the eye standing vertical, the cornea downward.

The fragments of bone were adjusted as well as possible, and the eyeball replaced in the orbit. It was uninjured, and vision was perfect.

About ten weeks after, by two blepharoplastic operations the eyelids were brought into a tolerably good condition. They could be closed, and usually so remained, but could be opened enough to expose the cornea and permit sight. The globe was however perfectly immovable. About five months after the injury, ulceration and suppuration of the cornea occurred, and the globe atrophied.

6.—*Paralysis of both Sixth Nerves.—Autopsy.* By Dr. F. Magris.—[Schmidt's Jahrbuch, 1868. 6, p. 352.]

The history of the case is not stated; only the morbid appearances are given, and these are sufficiently curious. The calibre of the cavernous sinus was unusually large. The carotid was atheromatous, and made a remarkably strong S-shaped curve. From the perpendicular turn of the S a branch was given off to the base of the skull, toward the ganglion of Gasser. Between this twig and the side of the carotid the abducens nerve was entangled and compressed. That part of it between the brain and the carotid was of normal size, while in front of the vessel the nerve was atrophied to half its usual thickness. This condition existed on both sides.

Another anomaly was found in this subject: in that a supernumerary bundle of muscular fibres took origin from the most posterior part of the external rectus of the right eye, and was inserted into the sclera at the outer border of the inferior rectus—its course was obliquely forward and inward.

LACHRYMAL APPARATUS.

7.—*The Cure of Stricture of the Ductus ad Nasum by Internal Division.* By Dr. J. Stilling. [Cassel, 1868, with a Plate; pp. 24.]

In this account of a new theory of curing strictures of the nasal duct, Dr. Stilling professes to follow the analogy of treatment of strictures of the urethra. He first gives a careful detail of the anatomy of the lachrymal passages; and the fact which bears most strongly upon the practice proposed is the existence, beneath the mucous membrane of the nasal duct, of a noticeable amount of erectile tissue—a layer which Henle particularly describes. This is mingled with elastic fibres, and unstriped muscular fibres. The novelty in treatment consists in dividing the stricture from within by incisions in three or four directions, and leaving the parts afterward untouched. No sounds or probes are to be employed, except in the preliminary exploration. The superior punctum is opened, and then Dr. Stilling uses a short and narrow-bladed knife, which tapers to a moderately sharp point, to cut the stricture. The instrument is straight, and, being first plunged to its full length into the duct, the blade is withdrawn, turned around a little to be again pushed in, and yet again, so as to cut the

stricture with the greatest freedom. If blood flow freely from the nose, the operation may be considered to be well done, and a full-size conical probe (Weber's) may be introduced. No other ill effects follow than a little ecchymosis of the lower lid. In two weeks, as the average time, the parts are healed. Dr. Stilling narrates nine cases which presented the ordinary features of lachrymal catarrh and obstruction, in which this simple treatment effected a complete cure. He was able to verify the result after several months. The cases which Dr. Stilling regards as curable in this way are those for which the treatment by Bowman's probe would ordinarily be undertaken.

8.—*Treatment of Stricture of the Lachrymo-nasal Passages by Internal Division.* By Dr. Warlomont. [Annales d'Oculistique, tome lx. Sept.–Oct., 1868, p. 117.]

The mode of treatment suggested by Dr. Stilling has been adopted by Dr. Warlomont, and in the above paper he narrates the history of eight cases, and says that he has treated a score besides, whose history is absolutely the same. Whether the cure will be permanent, he does not affirm positively, but in some an experience of five or six months has exhibited complete relief from lachrymal trouble.

One case (obs. iv) is that of a young and pretty girl, 20 years old, who for more than three years had been treated by Bowman's probes, and could pass No. 4. This was done twice a week, but the epiphora continued. After having Stilling's operation done, she found herself in a week more comfortable than for three years previous. The cure became complete, and was known to remain for five months.

In some cases the canal was found beset by rugosities, and the obstruction to be so hard as to creak under the knife. It was necessary sometimes to use considerable force. In all cases the small conical sound of Weber was first used, both to explore the canal and prepare the way for the knife, by dilating the passage as fully as it was capable of doing. After the stricture had been freely divided, an injection of warm water was sometimes made on the following day. Usually the catarrhal secretion from the sac began to diminish, the swelling of the sac to subside, and fistulous openings to close without any further treatment. Slight conjunctivitis would also disappear, while troublesome ophthalmia tarsi and ectropium would require a few weeks' treatment.

If, in extreme cases of lachrymal catarrh, the disease did not disappear spontaneously, Dr. Warlomont recommends a proceeding, devised by Dr. Libbricht of Gand, for facilitating the introduction of medicated fluids into the sac. A stylet made of platina, in size equal to probe No. 1 of Bowman, and grooved on three sides, is worn in the nasal duct. It is about one and a half inches long, and bent at its upper end into the form of a hook, so as not to sink out of place. It is designed to favor the penetration of fluid, which may be simply dropped into the inner angle of the lids. The substance preferred by Dr. Libbricht is chloride of zinc in solution of about gr. ½ to ʒj, dropped into the eye three times daily, and continued seldom longer than two or three weeks. This mode of proceeding is a good substitute for injections by the syringe, which require to be done by the physician, and are consequently not done as often as the disease requires.

9.—*Treatment of Disease of the Lachrymal Sac.* By Prof. Alfred Graefe, of Halle. [Zehender's Monatsblätter für Augenheilkunde, August, 1868, s. 223.]

The author suggests one or two new points. To aid in voiding the sac of secretion, he causes the patient to perform the manœuvre of Valsalva,

namely, blowing forcibly while mouth and nose are closed. If this be done immediately after a probe has been passed, the air will find its way into the lower end of the nasal duct and escape by the canaliculi, bringing with it more or less of the catarrhal secretion. For two or three days only will this manœuvre be practicable, when another introduction of the sound will permit it to be repeated for another period. To avoid the accident of emphysema, which might ensue if the probe have torn the mucous membrane, Prof. Graefe cautions us to put the finger lightly on the sac, as the patient is blowing for the first time. After the passage has been dilated a little, there is slight danger of this occurrence.

Prof. Graefe also uses a douche apparatus, recommended by Dr. Herzenstein, in place of a syringe, for effective cleansing of the passages—a small tube is inserted into the canaliculus, and this is by a flexible tube connected with a pump.

But cases are sometimes met in which the stricture is wholly overcome, and the catarrhal secretion dried up, while epiphora continues. In one such case the cause was found to be hypertrophy of the caruncle, which, by pressing on the canaliculus, impeded the entrance of tears. The excision of the caruncle relieved the epiphora. The mass need not be very large to cause such trouble; and, in cases where the duct is brought to proper size, the catarrh of the sac is cured, the eyelids are in correct position, and the muscular apparatus sound, and yet epiphora continues, Prof. Graefe recommends the trial of this operation of excision of the caruncle.

CONJUNCTIVA.

10.—*Pathology of Trachoma, from Examination of the Eyelids of certain Domestic Animals. An Inaugural Thesis.* By Dr. Paul Blumberg. [Dorpat, 1867. Quoted in Zehender, April–May, 1868, p. 132.]

The author examined the conjunctiva of the pig, the dog, the rabbit, the horse, and the ox, in reference to their proneness to occurrence of trachoma, and to ascertain in what the so-called trachoma follicles consist. He found that these become developed in the conjunctiva when it consists of adenoid tissue—that is, of fibres of connective tissue in which are embedded lymphoid cells. In the pig, the adenoid character is strongly marked in the *culs de sac* of the conjunctiva, becomes less decided on its tarsal portion, and least in the conjunctiva bulbi. Of mucous follicles there are none. In the pig, trachoma follicles are very abundant. The conjunctiva of the dog, rabbit, horse, and ox, is adenoid in the adult, but in the young subject is not. In the cat, the conjunctiva always consists of simple fibrillated connective tissue. If the membrane normally possess the adenoid structure, it will exhibit trachoma; if its structure be not such, there will not be trachoma.

In human beings the same fact is true; for in infancy, where the conjunctiva is composed of merely connective tissue, trachoma is very rare, but the disposition to its occurrence increases as the membrane acquires more of the adenoid character. If a trachomatous eyelid be immersed for one or two days in vinegar, the trachoma follicles will be exposed by merely brushing away the epithelium. Pricking them with the point of a needle, a quantity of lymphoid cells will escape, and leave behind a cavity traversed by very delicate fibres of areolar tissue—the blood-vessels are only on the surface of the follicle, not within.

The author, therefore, regards trachoma follicles as neither glands nor congenital structures, but simple hyperplastic growths of lymphoid cells, which have undergone proliferation. He compares the trachoma follicle, in its relations to the adenoid tissue, to the relations which obtain between

a lipoma and normal adipose tissue. Furthermore, the hypertrophy of the cells causes atrophy of the connective tissue, especially of that which is contained within the trachoma follicle, and, as it grows by pressure, it becomes enveloped in a membrane of connective tissue.

11.—*The Track of Foreign Bodies which enter the Vitreous Humor.* By Dr. R. Berlin. [Archiv für Ophth., b. xiii., ii., s. 275.]

Penetrating Wounds of the Eye. Illustrated by Experiments on Rabbits. By Dr. Lubrisky, of Kronstadt. [Archiv für Ophth., bd. xiii., ii., s. 377.]

Dr. Berlin has made careful examination of eleven eyes which he enucleated on account of penetration of foreign bodies into the vitreous chamber. In two cases the foreign body was stuck in the sclerotic wound; in three cases it remained in contact with the opposite inner wall of the sclera; in six cases it was found at the bottom of the eye, often in the vicinity of the lens or ciliary body.

On making a minute inspection of these six cases, it became evident that the foreign body came into this position by rebound from the posterior wall of the eye. This was proven by finding a spot on this wall where the retina and choroid had been wounded, and this lesion, in a straight line with the point of entrance of the offending body, whether this was in the cornea or anterior part of the sclera.

The fact of impact on the back wall of the eye was indicated in many cases by adhesion of the choroid and retina at this point to the sclera. If detachment of the retina had occurred, besides attachment at the papilla, there would be oftentimes a second attachment at the spot of impact. Another sign, indicating this fact, is, that sometimes from the sac in which the foreign body may be encapsuled a fibrous cord proceeds backward, to be attached to some point of the fundus.

This fact that foreign bodies so frequently, namely, in six out of eleven cases, taken at random, behave in this way, has hitherto been almost entirely overlooked. But, as to their final resting-place, it is not always at the spot where they fall, after the rebound from the back surface of the interior of the globe. Their position may subsequently be altered by two kinds of morbid action: First, a subretinal effusion, as it slowly increases, may push it in front of itself; second, if enveloped in vitreous opacities, these, as they contract, will carry it with them. The direction in which it is displaced by the first cause is accidental; by the second, it is always drawn toward the axis of the eye.

The important practical deductions from this paper are: first, the tendency of penetrating foreign bodies not to pass entirely through the eye, but to remain lodged within it; secondly, to understand the signs by which we may become convinced that a foreign body is actually lodged within the vitreous chamber.

If a foreign body simply penetrate the iris, or lie just behind it, it necessarily causes iritis and oftentimes hypopyon; and is frequently enveloped in a mass of yellowish plastic exudation, that forms a little tumor, and points out its exact situation.

If the body be in the lens, after the first reaction passes away, and the irritation which depends on the more or less rapid swelling of the lens, and often assumes the glaucomatous type, no other symptoms remain save those of cataract. But it must be observed, that quite often such eyes are very intolerant of operative interference—the reaction which ensues may be excessive. In case the foreign body should subsequently drop into the anterior or posterior chamber, inflammation may spontaneously arise; but if it remain in the lens, the eye commonly continues quiescent.

If the foreign body get into the vitreous, the inflammation generally takes on the character of irido-choroiditis or irido-cyclitis. The globe may finally atrophy, or, if the foreign body be simply suspended in the vitreous, the eye may for years be free from any signs of irritation. But internal irritation will in the end almost certainly begin.

The signs by which to become convinced that a foreign body has passed into the deep parts of the eye are to be gathered from the history: the discovery of an external scar or wound; the marks of its having passed through the iris or lens; the nature of the inflammation; and, finally, the kind of injury to vision.

To omit certain signs, generally well understood, which indicate the penetration of a foreign body, we will consider the following: An inflammation, whose severity is out of proportion to the apparent gravity of the wound, implies the lodgment of a foreign body. If in the vitreous, the immediate damage to sight may be in the form of a cloud floating before the eye—which is due to obscuration by the foreign body or by a blood-clot. The loss of sight at a later period will be central or peripheral, or both, and more or less complete. But a very important sign is limitation or encroachments on the field of vision. A general impairment of sight (leaving cataract out of view) depends on blood in the vitreous, or vitreous opacities, the result of inflammation. Contraction of the visual field is caused by subretinal effusion, or by wound of the retina, which cuts off the conductivity of more or less optic nerve-fibres.

The author describes minutely the microscopic changes in several eyes which he examined, but we may not quote any further. One point may be selected, that, in one eye, extirpated two days after the injury, the optic nerve-fibres were found to have undergone precisely the same changes which have been noted in nephritic retinitis. This layer was thickened by a collection of glistening pedunculated bodies, described by Zenker as modified nerve-cells; by Virchow, as sclerosed nerve-cells; by H. Müller, as diverticulated nerve-fibres. The last explanation is most generally accepted.

Dr. Lubrisky inflicted 38 wounds upon 23 rabbits' eyes, with a knife, and afterward examined them anatomically at various periods. The following facts may be gleaned from his statements: The sclerotic wound never closes by direct union of its edges—an intermediate tissue is formed to which the conjunctiva and choroid cohere, and the latter often projects appreciably into the eye.

The choroid about the wound becomes bereft of pigment, and the epithelial cells are irregular in size and distribution.

The edges of the retinal wound do not unite together, but rather with the cicatricial tissue. As soon as the scar begins to project inward, the retina becomes detached. One of the changes in it is, that, on its outer surface, fibres begin to dovelop, and as they grow they throw the retina into folds, the nerve-elements disappear, and, as this fibrous tissue contracts, detachment takes place.

12.—*Observations upon Foreign Bodies in the Vitreous Body.* Dr. R. Berlin. [Archiv für Ophthal., xiv., ii., 274–332.]

In this second paper on this topic, Dr. B. adds 11 new cases, making in all 35.

He found in nine of the new ones that the posterior wall of the globe had been wounded by the foreign body. He details the history, treatment, and autopsy of ten eyes, giving minute details of microscopic appearances. The most interesting point is the examination of the visual field, and the value to be attached to the symptom of its being encroached upon—as a sign of the presence of the intruding body in the eye. The mode of mapping the field is as follows: The patient commonly has traumatic cataract.

He must have bright light, within three or four feet, for central fixation, The peripheral field is examined by a small bright light, carried around in remote positions—it may, from time to time, be shaded by the hand. The mirror of an ophthalmoscope may be used when a light is behind a patient. The movable light may have a tin chimney, with a small hole in it.

This examination will take time, but, well conducted, gives valuable information. Great diminution of central vision is commonly due to blood in the vitreous.

Another point to be studied is, whether the situation at which the visual field is impaired may not help us to discover the site of the foreign body. In a certain case, the visual field was damaged outward and upward—the foreign body was, therefore, supposed to lie at the inner and lower quadrant of the globe. A cut was made at the inner border of the rectus inferior, $4\frac{1}{2}'''$ from the cornea, running $3'''$ inward. A hard body was encountered by the edge of the knife, but the forceps failed to find it. The eye was enucleated, and the incision found to have actually grazed the posterior edge of the piece of steel. On examining his specimens, Dr. B. found that compact bodies, which did not adhere to the inner wall, sank to the bottom of the eye, while those of a scaly form floated in the vitreous; the heavier sort were always in the anterior and lower quadrants, and could generally be hit by an incision $3'''$ distant from the cornea, parallel to the equator, and $3'''$ to $4'''$ long.

Whether mere removal of the foreign body would suffice, in lieu of extirpation of the eye, is a question to be decided by the peculiarities of each particular case.

CORNEA.

13. —*Memoir on the Cause of Opacities in the Cornea.* [Zehender, April–May, 1868, p. 144.]

This treatise cites a series of experiments intended to show that opacities are induced by the penetration into the cornea of morbid secretions from the conjunctiva.

14.—*Keratoconus.* By A. v. Graefe. [Berliner Klin. Wochenschrift, No. 23, 1868.]

The distortion in the form of the cornea known as staphyloma pellucidum, or conical cornea, presents the symptoms of extreme myopia, but is very imperfectly corrected by concave glasses. The curve has been measured by Dr. Leber, at Prof. Graefe's instigation, and found to be that of a hyperboloid, and while, in a certain case, at the apex the radius is as short as 2 mm., at the lateral parts of the staphyloma it was 10 mm. The radius of the normal cornea is about 8 mm. The apex is often more or less opaque, but only in the most prominent cases: during the early stages the apex remains transparent. Sometimes the ectasia is the result of an ulceration, but the majority of cases have never had any such cause. The only explanation is, that the middle of the cornea becomes affected by a real atrophy; upon what this may depend as a remote cause is unknown. The means of relief hitherto in use have been suitable glasses, both spherical and cylindrical, the latter affording considerable diminution of the irregular astigmatism which always exists; these combined with a stenopaic hole or slit. For surgical treatment, frequent paracentesis, and a pressure bandage, have been employed with slight advantage, and also two iris operations, iridectomy and iriddesis, single or double. It is often true that by none of these means does the patient obtain as much improvement in sight as is desirable. Prof. Graefe has put in practise a method to which he has already called attention. He endeavors to flatten the cornea by

producing a cicatrix and opacity upon its apex, and afterward to make an iridectomy. He shaves off a bit of the summit of the cornea, about three-quarters or one line in diameter, not so as to open the anterior chamber, and touches the raw surface once in three to six days, very lightly, with a pointed stick of *lapis mitigatus* (2 to 1), neutralizing the caustic carefully with salt and water. The treatment may continue for six or eight weeks, and the vision for a time is made worse than before. There is also some risk of the production of a greater amount of infiltration of the cornea than is desirable, or, perhaps, of destructive inflammation. Dr. Meyer, of Paris, treated a case under Prof. Graefe's direction in this way with satisfactory result, and Graefe relates a case in which, at the beginning, vision was $\frac{1}{36}$, and was improved to $\frac{1}{6}$. Before treatment he could read Jager, No. 16, at 3″, and afterward, with concave glasses, could read the same print at 3 feet. The precise period at which the iridectomy is done is not indicated.

15.—*Ulceration of the Cornea in Young Children the Effect of Encephalitis.* By Dr. J. Hirschberg. [Berliner Klin. Wochenschrift, No. 31, 1868.]

This affection is rare, and attacks infants of two to six months old: the trouble of the cornea is only an incident in the graver malady. While for the first or second week the infants seem healthy, soon marasmus sets in, the digestive organs are disturbed, there is no fever, none of the usual symptoms of brain-disease. The corneæ quickly become infiltrated with whitish-yellow substance, they rapidly fall into slough, suppuration takes place, and may involve the entire globe. The disease assumes the aspect of a profound impairment of nutrition of the membrane. The result is not only loss of sight, but of life. The autopsy shows fatty degeneration of the cells of the neuroglia and of the white substance of the brain, especially of the hemispheres.

In the summer of 1867 six cases were seen in Graefe's clinique, and then only a few single cases occurred, until the summer of 1868, when within two months seven cases presented themselves. All presented similar symptoms; in one the disease continued two months before the fatal termination.

IRIS.

16.—*Anatomy of the Iris.* [Fr. Merkel's Zeitschrift für ration. med. Bd. 31, page 136, 1868. Zehender Monatsblätter, 1868, page 254.]

This is an investigation into the question of the existence of the dilatator iridis. Valentin Brücke, Budge, and Kölliker, have asserted its presence—others, and among the latest Grünhagen, have denied it. Dr. Merkel used a saturated solution of oxalic acid in water to remove the layer of pigment on the back surface of the iris, and says that in a rabbit's eye the connective tissue after fourteen days will be so transparent as to make it easy to see all the other elements of the iris. He recognizes the radiating fibres as muscular, but admits that they form a very thin layer—their cells are long, and sometimes almost equal to the radius of the iris—they are grouped in bundles from their origin at the ciliary margin—the bundles are connected with each other by single fibres or smaller bundles, which cross over from one to another. These transverse bridges become stronger and thicker as we approach the sphincter, until finally the radiating direction is lost in a reticulum of fibres running in every way. Out of this network the fibres may either bend into a circular course or unite with the sphincter at nearly right angles.

In man the radiating muscular fibres do not unite in bundles as in the

rabbit, but constitute an unbroken layer, which, without forming a network, passes into the sphincter by a few processes.

It must be added to the above—that Grünhagen, having tried Merkel's mode of investigation, is still unable to convince himself of the existence in mammalia of a dilatator pupillæ.

CATARACT.

17.—*An Improved Method of Extraction of Cataract with Results of* 107 *Operations.* By J. R. Wolfe, M. D., with Illustrations, pp. 71, London, 1868.

Extraction of Hard Cataract by a Cross-section. By Dr. H. Küchler, pp. 37. Erlangen, 1868.

Report of a Second Hundred Extractions of Cataract by the Linear Section at the Sclerotic Margin. By Dr. H. Knapp. [Archiv für Ophthalmologie. Bd. xiv., 1. s. 285–317.]

The above citations indicate the interest which is felt among ophthalmologists in the treatment of cataract. No less than three new methods of extraction are brought forward. One certainly claims the merit of originality, viz., that in which the section is made transversely across the middle of the cornea, including the limbus at each end. The capsule is opened by a small sharp hook; and the lens expelled by pressure on the globe, which may or may not be combined with the use of the hook in separating the lips of the wound—of course the lens must make a quarter revolution in coming out. The author asserts that there is no difficulty in bringing out even large and soft lenses quite clean.

Another peculiarity of Dr. Küchler's method is the bandage which he applies, which is double-headed, and makes many turns over both eyes, and is finally made immovable by being enveloped with a few turns of coarse lace (perhaps mosquito-netting is the thing), and smearing the whole with a thin layer of plaster of Paris. In this solid helmet the head remains for six or seven days.

Of course a scar remains on the cornea, directly across the pupil. The author details various experiments to show how little interference with sight such a line will cause—much will depend on the width of the scar compared with the size of the pupil.

The results in twenty-eight operations are as follows: In eighteen perfect sight, five able to read coarse print, four obtained a little sight (these were all complicated cataracts), one suppuration.

In ten cases, there was a secondary operation for artificial pupil; six times there was prolapse of vitreous at the operation; in seven, there was a considerable residuum of cortical matter; in six, peripheral synechiæ took place; in six cases, the cornea collapsed after the section, but this is asserted not to prejudice the healing by first intention.

The author's reason for this operation is the great facility with which it can be done. His results are scarcely as good as those of modified linear extraction, and the number of his cases too small to cover all the possibilities of accident.

An interesting fact is, that in 1861, when he did this operation for the first time, and on an eye which had extensive posterior synechiæ, he introduced a suture into the apex of the cornea, which he allowed to remain four hours, and then removed—the wound seemed at that time partially united, and subsequently did well. Another remark may be made, viz, that this operation is linear extraction in the purest sense—the section being on a great circle of the globe.

Dr. J. A. Wolfe, of Edinburgh, reports one hundred and seven operations by a method which he describes as peculiar to himself; but we fail

to find in it features with which we have not been familiar. He makes an iridectomy upward six weeks before the extraction; he makes a section at the margin of the cornea, which is one line longer than one-third of the circumference—the chord of the arc is 5‴ or eleven millimeters. He does this with a narrow Beer's knife: at the apex of the section he turns the blade, so as to leave a conjunctival bridge, which he divides with scissors. The subsequent steps are as usual. The operation is like that of Desmarres, save the precedent iridectomy. His results are good, as shown by the following table:

	Senile.	Traumatic.	Glaucomatous.	Total.
Healed well: perfect vision	81	12	1	94
Healed well: no vision	2	3	2	7
Healed well: secondary operation	2	0	0	2
Loss by internal hemorrhage	1	0	0	1
Loss by suppuration	2	0	0	2
Loss by iritis	1	0	0	1
	89	15	3	107

Results of operation, as respects healing: successes 103, failure 4=107. Results as regards sight: successes 96, failure 11=107.

18.—Prof. Graefe, *in Archiv für Ophthalmologie*, Bd. xiii. ii. s. 549–566, gives an account of his present mode of effecting the expulsion of the lens and also makes incidental observations on certain points in the operation.

At the time of writing this article, which was in the latter part of 1867, he had added to his previously published cases two hundred and thirty operations—these having been done in the preceding eight months. To all of these he applied the method of expulsion which he calls the pushing manœuvre.

In only two cases was he obliged to use a traction instrument within the eye—in one of these the wound was too small, in the other the vitreous escaped during the section. The *modus operandi* is as follows: The lids are still kept apart by the spring speculum, and now that the last step of the operation is to be done, the fixation forceps are removed from beneath the middle of the cornea, and applied at a point 1‴ to 1½‴ lines to the nasal side, so as to be out of the way of the spoon. The spoon is of hard rubber, highly polished, with a narrow bowl, bent on the shaft at a pretty sharp angle, and somewhat elastic. Its convex surface is applied to the lower edge of the cornea, and the upper part of the rim pressed against the limbus, the bowl being turned half upward. While making steady pressure, he moves the spoon a little upward, for the distance of about ½‴, and during this movement, the edge of the lens appears in the wound. The convex rim of the spoon is now made to press toward the centre of the eye, but as the lens emerges more and more, the line of pressure is correspondingly changed so as to follow after it. The force acts more and more in an upward direction, and finally comes to lie at a tangent to the surface of the cornea. In fact, the spoon pushes the lens out of the eye, and does not cease its function until all the lens, including cortex, is expelled. The fixation forceps must not make the least pressure, but is kept in use until the act is completed—the same is true of the speculum.

The amount of force needful is various in different cases—there is great difference in the tension of eyes after the section has been made. In some a mere touch of the spoon suffices. In others the cornea collapses, and the spoon must be buried deep in the eye, to procure the needful expulsive

force. It seems as if serious damage were being inflicted on the cornea, but experience proves the contrary. That the cornea does not suffer, either by loss of epithelium or by contusion, has been satisfactorily demonstrated. As to loss of epithelium, no abrasion has ever been discovered. As to injury by contusion, Prof. Graefe felt some anxiety. He noted, in twenty-eight operations, that "the spoon, to be effective, must be firmly pressed on the cornea," and in seven of these cases adds the qualification that the pressure is "ad maximum." In the seven specified cases, the healing was perfectly normal, and out of the twenty-eight cases, there were but three which had any disturbance—of these three, one had a perfect result, one a half result, and one doubtful. In this way the spoon may be considered innocuous to the cornea.

Prof. Graefe keeps the fixation forceps in his own hand, and does not deem it needful to press with a spoon on the posterior lip of the wound, to aid its opening. He thinks the lens itself, acting like a wedge, the best dilator of the wound.

He assigns as the proper length of the section a measurement of 5‴, from one angle of the wound to the other, this being the length of the chord of the arc. He does not now make the wound quite so perpendicular to the sclera as formerly, but permits it to be a little more oblique. By this change the height of the flap, which is the distance between the middle of the arc and its chord, instead of being $\frac{1}{6}$‴ or $\frac{1}{4}$‴, becomes $\frac{1}{3}$‴, almost 1 mm. Prof. Graefe acknowledges the truth and merit of the investigations of Adolph Weber (which were reported in the February number of this Journal in 1867), to the effect that a linear wound closes more accurately than a flap wound. The wound on its inner surface will measure $4\frac{3}{4}$‴, about ten mm.

The excision of the iris should be made deep in the wound, and its whole length. This must be done even when the iris does not spontaneously prolapse. The deformity of the pupil, produced by entanglement of the iris at the angles of the wound, is more injurious to sight than the fullest degree of coloboma which iridectomy can cause.

The capsule must be torn to the fullest possible degree.

In these two hundred and thirty cases, prolapse of vitreous has been less frequent than before—it happened only nine times—less than four per cent. In three out of the nine, the escape occurred at the section, and was caused by synchisis of the humor.

The removal of cortical matter has been very easy and complete by the pressure of the spoon, and if it be noticed that the surface is disposed to come off of the lower part of the lens, the spoon must be reapplied at the lower edge of the cornea, and the pressure repeated in the same way as before.

The total result is declared to be better than in the cases previously reported, both in regard to healing and sight. Prof. Graefe's first operation was done May 19, 1865. In the Archives, Bd. xii. ii. s. 151 (1866), he gives the number of his operations at three hundred—and the results in sight to be ninety per cent. (v.=$\frac{1}{6}$ to $\frac{5}{6}$), that four per cent. would by secondary operations come within the category of perfect results, that is, have $\frac{1}{6}$ and better:—the remaining six per cent. consist of total losses and imperfect results.

Now that the experience of this operation in Prof. Graefe's hands has risen to between five hundred and six hundred cases, and perhaps more, we may hope for statistical statements of great reliability; and, without doubt, no other method of extracting cataract can exhibit similarly brilliant triumphs.

An interesting statement is given, in a foot-note, respecting the comparative advantages of flap and linear extraction. In forty patients in whom

on one eye flap extraction had been done with bad success, Prof. Graefe has done modified linear extraction on the other eye with entire success, save in two instances—one a total loss, the other a half result. In nine cases, where flap extraction had been done sucessfully on one eye, modified linear extraction was done on the other with perfect result. Eight of these patients declared their preference for the last operation, because the after-treatment was less burdensome—sight in the two eyes about alike. The one, who preferred flap operation, did so because his eye, operated on by Graefe's method, was more dazzled than the other, yet its vision was sharper.

19.—*Von Graefe's Method of Extracting Cataract, and the Advocates of the Corneal Section.* By Dr. J. Jacobson. [Archiv. für Ophthal., Bd. xiv., 11, 247.]

The title of this article suggests a polemic character, and such, in part, it is. Dr. Hasner has published several bitter attacks on Graefe's method, which have been replied to by Graefe. In Dr. Jacobson's espousal of the modified linear extraction the operation has gained its most brilliant convert. He first pointed out the importance of a scleral section, and of a large iridectomy, but made a flap. His results were the best ever published up to that time. Now he modestly gives up his own mode for Graefe's, insisting strenuously on the scleral section of sufficient size, on a broad iridectomy up to the ciliary attachment, and prefers to give chloroform.

He shows by cases how little disadvantage to sight results from the coloboma, and admits that certain dangers are almost impossible by this operation which belong to his own method, and much more to corneal extraction. The chief advantage of Graefe's mode lies in the indisposition of the wound to gape, like a flap-wound, whether in the sclera or cornea.

Dr. Knapp gives an account of a second series of 100 extractions by Graefe's method, the results as respects sight being:

Total loss	. . . . .	2
Imperfect result	(v. = $\frac{1}{12}$ to $\frac{1}{100}$)	12
Perfect result	(v. = $\frac{1}{2}$ to $\frac{1}{10}$)	86

Of the second category, Dr. Knapp says at least 7 would, by absorption of the remaining lens matter, so far improve as to pass into the third class, and give the result: total loss, 2 per cent.; imperfect result, 5 per cent.; perfect result, 93 per cent.

The section which the author makes is such that puncture and counter-puncture are in the sclera, as close to the iris as possible, the middle of the cut approaching the transparent cornea within $\frac{1}{2}$ a millimeter, but never closer, the greatest length being 9 to $9\frac{1}{2}$ millimeters, measured in a straight line from one angle of the wound to the other, and at its inner surface.

This opening is sufficient for the expulsion of the largest cataract, whose dimensions never exceed 9 mm. in equatorial diameter, and 4 mm. thickness. Dr. K. goes into a mathematical discussion to prove that a wound in the sclera, whose opening is 9.5 mm. long, will permit a gaping of 4 mm., and in so doing will be made no shorter than 9 mm. The length of the arc, of which 9.5 is the chord, will be 10.14 mm.

This measurement pertains to the inner lip of the wound. To this must be added the thickness of the sclera, which is stated at 1.3 mm., and, being doubled, we have 9.5 mm. + 2.6 mm. = 12.1 mm. for the length of the outer wound taken as the chord of the arc.

The usual diameter of the cornea is 12 mm., and it follows that the wound made by Dr. K. is situated in the sclera, so that a line dropped perpendicularly from its extremities would touch the cornea as a tangent.

The limited number of suppurations is ascribed to the fact that the

wound is exclusively sclerotic. This, however, has the effect of causing prolapsus of vitreous humor to a greater or less amount, 18 times in the 100 cases. In explanation of this frequency of this accident, he says that oftentimes the middle of the section was at 1 mm. or more rather than $\frac{1}{2}$ mm. from the edge of the cornea. In 6 of these cases portions of lens-matter were retained in the eye.

In 10 other cases, making in all 16 times, portions of lens remained behind. This makes a total of 28 times that the operation was complicated by untoward occurrences. It must also be observed that Dr. K. was very rigorous in his search for remaining lens-matter, looking for it by oblique illumination.

In 72 cases the operation was perfectly smooth. In 84 cases, the healing was entirely satisfactory.

In 9 cases iritis occurred. Of these, 2 were after perfect operations; 5 after loss of vitreous. In these vision varied between $\frac{1}{4}$ and $\frac{1}{100}$, and with a chance in the worst for improvement by secondary operation. In 3 cases secondary hæmorrhage occurred. Vision attained was from $\frac{1}{3}$ to $\frac{1}{6}$. In 2 cases the vitreous was found by the ophthalmoscope to be hazy, while the operation had been good; vision, $\frac{1}{5}$.

The 2 suppurations occurred, one in an old man with Morgagnian cataract, the other in a healthy woman, 52 years old. The operation was perfectly satisfactory.

The average time of treatment for the 100 cases was 17.8 days. The number of secondary operations is not given. Iridectomy or extraction of capsule is said to have been indicated not more than 5 times. On the other hand, discission of delicate opacities was done in 10 cases, and, imitating Mr. Critchett and Mr. Bowman, done within three weeks of the operation, and safely.

Dr. Knapp carefully analyzes his cases to ascertain what causes are most active in producing bad results, and the most injurious he finds to be the retention of lens-matter in cases of prolapse of vitreous. The simple retention of lens-matter damaged the healing in only 2 out of 10 times, by causing iritis, and sight in 6 was less than $\frac{1}{7}$; in the remaining 4 it was better than $\frac{1}{7}$.

Prolapse of vitreous caused no disturbance in healing or sight in 6 out of 12 times. In the other 6 cases sight ranged from $\frac{1}{3}$ to $\frac{1}{100}$. In the 6 cases where both accidents occurred, viz., retention of lens-matter and loss of vitreous, in 4 the healing was normal; in 2 iritis followed. Sight in the 4 was from $\frac{1}{6}$ to $\frac{1}{20}$; in the last 2 was each $\frac{1}{40}$.

Dr. Knapp remarks that while the danger of suppuration is very greatly reduced by making the section in the sclera, it is not entirely avoided. For this reason he concludes that it is better to bring the middle of the section up to within $\frac{1}{2}$ a millimeter of the cornea, and perhaps nearer, because the lens will then escape more easily, the wound is more linear, and prolapse of vitreous much less liable to take place.

He speaks favorably of discission of slight capsular opacities within a few weeks after the operation. In this commendation many will be disposed to wait further experience before venturing to adopt a suggestion which is so much opposed to what good operators have hitherto considered prudent. He is not sanguine about the good result of operations for removing both lens and capsule together, which Pagenstecher and Wecker have practised, and which Dr. K. has done a number of times. He finds that iritis, secondary hæmorrhage, and chronic changes in the vitreous are among its consequences.

He has found the expulsion of the lens greatly facilitated by the hard rubber spoon used to make pressure on the lower edge of the cornea. He gives the fixation forceps to an assistant, with one hand makes pressure on

the lower edge of the cornea, and by a curette also presses on the posterior lip of the wound. The assistant is cautioned not to drag on the eye. He makes the iridectomy before opening the capsule, and does the latter very thoroughly, but so as not to luxate the lens.

Many of his patients were discharged at a time when their sight was still improving, and among his formerly reported cases he finds 10 with v.=1, 1 with v. $\frac{1}{10}$, becoming v.=1: and so with other cases. On the whole, Dr. K.'s testimony in favor of Graefe's operation is extremely satisfactory, because his results are good and his analysis exhaustive.

20.—*Extraction of Hard Cataract with the Capsule.* By Dr. Ferd. Bergmann. [Archiv für Ophthalmologie, Bd. xiii., 11, s. 383–397.]

This paper gives an account of 13 operations done by Dr. Knapp, 10 of which were lenses of full size, requiring the use of the spoon, and 3 were shrivelled cataracts which could be removed with forceps. The section made upward, and chloroform not given. Loss of vitreous at the operation occurred only 4 times, and once only in large quantity. Another difficulty unexpectedly occurred, that 5 times the capsule ruptured in bringing out the lens, and gave rise to difficulty in evacuating the cortical matter.

Dr. B. says that where the lens and capsule were extracted entire no iritis followed, but this comparatively favorable statement is modified by a remark in a late paper of Dr. Knapp, who has seen iritis ensue in such a case.

Not less than 9 times was secondary hæmorrhage observed—happening from the second to the fourth day. In 2 cases it was several times repeated. The ciliary body was the probable source of the bleeding.

Opacities of the vitreous, both diffused and membraniformi, were very common and abundant.

The average time of treatment was 18 days. The vision in 6 was better than $\frac{1}{10}$, from $\frac{1}{3}$ to $\frac{1}{10}$; in 3 was $\frac{1}{20}$ to $\frac{1}{40}$; in 3 consisted in mere perception of light. Such results do not encourage further trials of this method. Yet it must be added that it is adapted to over-ripe cataracts in which the central part of the capsule has become greatly thickened. And, secondly, this operation becomes easier the older the patient, because the zonula seems to become atrophied. But age of the cataract is not the equivalent of age of the patient.

CHOROID.

21.—*Tubercules in the Choroid.* By A. v. Graefe and T. Leber. [Archiv für Ophthalm., B. xiv., 66th, i. s. 183–206. Soelberg-Wells. Med. Times and Gazette, No. 914, January 4, 1868. Zehender Monatsblätter, 1868, July, 218.]

Tubercular choroiditis was a term formerly applied to a peculiar sort of inflammation, but, since Cohnheim drew attention to the occurrence of miliary tubercles in the choroid, the term must be given up. The first to notice and describe their occurrence was Ed. Jaeger, in 1858 (*Zeitschrift für Ration. Med. Jahrg.*, 1, No. 2.) Galizowski, Mans, and Wells have related cases. In the Berlin Pathological Institute, within fourteen months, eighteen cases have been seen by Cohnheim. Both eyes are affected, and it would appear that the choroid becomes affected in no other form of tuberculosis than the miliary. Herein, the ophthalmoscope may again offer valuable diagnostic help to the general practitioner, in view of the difficulties which often invest the recognition of miliary tuberculosis. Prof. Graefe gives the following points to be borne in mind as belonging to choroidal tubercles: 1. They cluster about the region of the posterior pole, the yellow spot, and the nerve. 2. They have a rounded form, and

are from $\frac{1}{3}$ mm. to 2.5 mm. in diameter. 3. The larger they become the more the color becomes discharged from their centre—there is seldom any increase of pigment around them, although this has a few times been noticed. 4. Their prominence makes them conspicuous, especially when inspected by the upright image.

Prof. Graefe details the history of a case in Prof. Griesinger's ward as follows: A man 32 years old, who was dumb, had suffered four weeks before entering the hospital with severe headache and phosphenes. At the time of entrance he had fever and pain in the forehead; on the left chest in front and above percussion tympanitic, behind and above dull, all over the left chest breathing rough and occasionally tubular; in the right vesicular, breathing rather sharp. Slight facial paralysis on the right side. The fever continued, the respiration a little accelerated, consciousness became gradually dull; he would groan, gnash his teeth, etc. Prof. Griesinger, from these symptoms, inferred the diagnosis of miliary tuberculosis of the pia mater, and desired an ophthalmoscopic examination. In both eyes there was extreme hyperæmia of the retinal vessels both small and large, but no infiltration of the tissue. A few rounded spots, varying from $\frac{1}{4}$ to 1 mm. in diameter, as estimated with the inverted image, were found not far from the optic papilla. They were of lighter color than the adjacent fundus, from a pale pink to a yellowish or whitish tint. They could not be discerned to have any prominence above the surface—but the examination was made under great difficulties. Patient died the following day, and Dr. Cohnheim found old tubercules of the lungs and acute deposit of miliary tubercles in almost every organ of the body; and also meningitis tuberculosa with hæmorrhagic encephalitis. In the eyes, whose posterior portions alone could be removed, tubercular nodules were found in the choroid; in one eye 12, in the other 9. Many of these were invisible to the ophthalmoscope, chiefly because they had not induced any pigmentary changes and were very small.

Another case is related: A child 15 months old, who was suddenly taken with coma and vomiting, after a slight diarrhœa: in the lungs, nothing but slight catarrh; respiration rapid, irregular, pulse 100. The first examination of the right eye disclosed nothing; but a few days after, when the left was inspected, 8 tubercular spots were found in it and 2 in the right. They were $\frac{1}{3}$ to $\frac{1}{2}$ the nerve diameter in size, and of a pale-red color with indistinct border; in no case was there abnormal pigmentation. The child was comatose. An increase in the number and size of the tubercles could be detected from day to day. Death ensued in a few days, and, at the section, miliary tubercles were found all over the body—in lungs, liver, spleen, kidneys, heart, lymphatic glands, choroids and in the meninges of the brain. In the eye, no more than two nodules were found in addition to what were observed by the ophthalmoscope—one very eccentric, the other very small.

As to the microscopic relations of these deposits, nothing is said in this article.

As an appendix, it is stated that Dr. Cohnheim, in his experiments upon the inoculability of tubercle, has found in Guinea pigs that the deposit occurs in the choroid as well as in other tissues. The matter for inoculation was taken from a cheesy tuberculous gland, and the animal died after five weeks. Miliary tubercles were found in all the viscera.

Mr. Wells brought to the notice of the London Pathological Society a similar case, which is perhaps the same which was by him submitted to Mr. Vernon for microscopic examination (see Ophthalmic Hospital Reports, vol. vi., 2, 162).

The following points may be selected from Mr. Vernon's report:

When the choroid was separated from the sclerotic, the spots could not be seen through the thickness of the choroid. By low powers their connection with the larger vessels could not be traced. Under high powers it was seen that the pigment epithelium was almost entirely absent over the spots, but the elastic lamina perfect. Beneath were large numbers of colorless cells of uniform size, closely packed together, each containing one or more nuclei, and except that they were smaller, not to be distinguished from cells of pus or lymph. The outer layer of these cells blended gradually with the stroma of the choroid, and cells of a like character and appearance were scattered thickly amongst the dark pigment-cells. At this spot, however, the cells were not so uniform in size; here and there were larger cells full of nuclei, and the cell-walls could not so easily be distinguished.

22.—*Treatment of Irido-choroiditis. Two Cases from the Clinic of* Prof. Ed. Junge, St. Petersburg. [Zehenders Monatsblätter, August, 1868, 239.]

The usual antiphlogistics failing to check the disease, and large masses of exudation appearing in the anterior chamber, recourse was had to paracentesis. This was done daily, if possible, and with marked acceleration of absorption, and improvement in the inflammation. It was done from eight to ten times. No accidents occurred, and to one patient it was not painful, to the other it was. When this proceeding was adopted, all other remedies, namely, purgatives and mercurials, were laid aside, except atropia.

RETINA AND OPTIC NERVE.

23.—*Temporary Blindness in Typhus and Scarlatina.* By Dr. Ebert. [Berlin Klin. Wochenschrift, v. 2, 1868, in Zehender, March, 1868, 91.]

Dr. Ebert communicated to the Berlin Medical Society four cases of transitory blindness in typhus and scarlet fever. In the one case of typhus the subject was a girl ten years old, who, on the fourteenth day, had a copious bleeding from the nose; the next day she was so blind as to be unable to perceive light. This condition lasted about forty hours, and by the third day vision was completely restored.

In the three cases of scarlet fever the blindness continued from one to three days, and developed under symptoms of acute intercurrent nephritis. In one, on the seventeenth day, slight œdema of the face, feet, and back occurred; urine scanty, dark, and highly albuminous: on the twenty-fourth day vomiting, violent headache, pain over the eyes, and delirium—the urine red, muddy, and no albumen—sopor set in, and tonic convulsions for a few minutes. The somnolence increased, urine and fæces voided involuntarily, and the next morning, with extreme headache, complete blindness took place. There was no perception of light until noon of the second day, and on the third day both sight and intelligence were restored. During the blindness the pupils responded to light, and when the sight returned albumen reappeared in the urine. On the second day of blindness the eyes were examined by the ophthalmoscope, but, besides a little sinuosity of the vessels of the retina in the left eye, nothing at all could be discovered.

One of the three cases was fatal; in all, the symptoms were similar. Dr. Ebert was disposed to consider the cause of the amaurosis to be œdema at the point of origin of the optic nerves from the brain, analogous to œdema observed in the lungs and skin under the same circumstances. Prof. Graefe called attention to the fact that, during the period of blindness, the pupil continued its activity. This implies that the interruption of

communication lies between the corpora quadrigemina and that part of the sensorium which presides over the perception of light. The movements of the pupils are to be explained as a reflex action, the perception of light reaching as far as the corpora quadrigemina, but not going beyond into the deeper parts of the brain. In many severe and acute diseases, sudden blindness may happen, and be permanent. The ophthalmoscope may not find any lesion for weeks, until at length atrophy of the optic nerves declares itself. In these cases the pupil does not respond to the light, and this always gives the prognosis a less favorable character. Not, however, so as to preclude the possibility of returning sight, because there are lesions of the optic nerves and of the tubercula quadrigemina which may ultimately disappear. Prof. Graefe would, therefore, attribute the blindness in Dr. Ebert's cases, not to a lesion of the optic nerves, but of the brain, at some point between the tubercula quadrigemina and the locality which presides over perception of light.

24.—*Double Amaurosis after Hæmetemesis and Intermittent Spinal Neuralgia.* By Dr. Jacobs. [Berl. Klin. Wochenschrift. Zehender, March, 1868, page 90.]

A woman, 42 years old, at the age of 36 was attacked with nausea, pain in the stomach, and vomiting of blood, the quantity filling an ordinary chamber-pot, and she fell into prolonged syncope. After two days, bloody vomiting recurred, and the blood was voided per rectum. At this time a pain of "unspeakable" severity occurred in the back of the neck, and lasted several days. This was considered to be malarial, and relieved by quinine and opium. On the twelfth day after the hæmetemesis she began to complain of loss of sight, the pupils were enlarged and reacted feebly to light. The vision daily diminished, and after five days there was no perception of light, the pupils large and immovable. Three weeks after the sight-trouble began, the interior of the eye was ophthalmoscopically examined by Dr. Warren, who found the retinal veins swollen, and in the retinæ were black spots and apoplectic clots. After six years the patient was again examined, and the optic nerve and retinæ were found atrophied. She remains perfectly blind.

25.—*Atrophic Changes in the Optic Nerve, and Remarks upon its Normal Structure.* By Dr. Th. Leber. [Archiv. für Ophthal., xiv., 11, 164–220.]

Without attempting a minute analysis of this elaborate article, the following points may be cited as having a general interest: It is well known that atrophy of the optic nerve ensues after the most diverse affections of the nerve-centres. But a special interest attaches to its occurrence in gray degeneration of the posterior columns of the spinal cord, because the optic nerve undergoes precisely the same alterations. Of the same nature is the atrophy of the nerve in the progressive paralysis of the insane; in them the motor troubles have been shown by Westphal to depend on gray degeneration of the posterior columns of the cord. The same change takes place in a part of the lateral columns in another class of cases, usually simply called cases of general paralysis; the lesion is styled chronic myelitis, and consists in the development of granule-cells.

When both the optic nerve and spinal cord become thus affected, it cannot be by simple continuation of tissue, as when the brain and optic nerve are diseased. They are not so related to each other as to permit such continuity; on the other hand, each tissue must be regarded as being primarily and independently the seat of the disease. It is hoped that a careful study of the process in the optic-nerves may throw some light on its nature in the spinal cord, whose structure is so much more intricate.

The cases examined are described in their captions as follows, and this serves to give a general notion of their character: Case 1. Paralysis and imbecility; chronic meningitis, with adhesion of the pia mater to the brain. Myelitis (granule-cells) in a part of the lateral columns of the cord. Gray degeneration of both optic nerves. Case 2. Paralysis and imbecility; complete amaurosis. Atrophy of the brain, hydrocephalus, myelitis of the anterior, lateral, and posterior columns of the cord, gray degeneration of the optic and olfactory nerves. Case 3. Softening of the cerebrum in several spots; contraction of the left half of the body; insanity. Gray degeneration of the posterior columns of the cord. Commencing gray degeneration in the optic nerves, especially in the left.

In all these cases there were atrophy of the optic nerves, disease of the spinal cord, and impairment of the mind. The psychical disturbance, in the third case, due to the brain softening; the other two cases belong to the category of spinal-cord disease in the paralyzed insane.

The lesions of the nerves may be summed up as atrophy of the nerve-fibres, increase in the intermediate connective tissue or neuroglia, with or without increase of its cells, and development of granule-cells and amyloid bodies. In the spinal cord the changes were the same, and, in situation, presented further analogy; in both cases the surface and outer layers were farthest advanced in degeneration. In both the lesion takes place in distinct spots, and the bundles of atrophied nerve-fibres present similar arrangements. In neither structure is there any evidence that the atrophy of nerve-substance is due to extreme development of connective tissue, such as would result from ordinary inflammation. It is true that, when granule-cells are found in great abundance in the cord or brain, the condition is called myelitis and encephalitis—by analogy, the same condition in the nerve must be styled neuritis; but as to the fitness of this nomenclature Dr. L. does not commit himself—he merely points out the resemblance.

The only alteration in the blood-vessels is that their outer or fibrous coat is increased in thickness, and often sclerosed. This is not always to be seen, but sometimes strongly marked.

The ophthalmoscopic appearances in the first two cases were, a shallow excavation, opaque white color of the optic nerve—this the usual condition, and depending on the comparative predominance in the papilla of connective tissue. In the third case the papillæ began to be whitened, but their level scarcely changed. The gray degeneration had advanced to the lamina cribrosa, but as the papillæ were nearly normal, the faded color must be explained by the shrinking of the minute blood-vessels. It has been long observed that signs of atrophy first make their appearance on the outer half of the optic disk. This is explained by the anatomy of the papilla, whose outer half is always flatter than the inner half; because the fibres which radiate on this side go obliquely upward and downward to pass around the macula lutea. On the inner side their course is more direct, they are heaped in a thicker layer, and this half will not so quickly exhibit a whitish decolorization as will the thinner inner half.

26.—*Troubles of the Eye depending on Diseases of the Spinal Cord.* By Dr. G. Dujardin-Beaumetz. [Paris, 1868, pages 46.]

In this little volume are brought together eight cases of chronic disease of the spinal marrow, which were complicated by visual disorders. Locomotor ataxy is the usual spinal affection, while the eye-troubles are of three classes, viz.: lesions of the optic nerves and retina; paralysis of the external muscles; disturbances of the pupil. The first two are essentially the lesions of the cord propagated to the optic and motor nerves; sometimes the fifth becomes implicated. The pupil is at first very small, may

be distorted, while the iritic contractions are not impeded. Sometimes both, sometimes only one pupil is affected. The phenomena may for a time disappear, and afterward return. In explanation of this lesion, it is stated by M. Donezan that, while the cervical ganglion may, to the naked eye, be changed, its microscopic structure is not altered, but the filaments of the nerve were utterly degenerated. In other cases no such lesion could be seen, and the cause must be found in the cord at the place where this filament is given off. It is unimportant to the production of the oculo-pupillary symptoms whether the anterior or posterior columns be diseased, it is only needful that what is called the cilio-spinal portion should be affected, and this lies between the last cervical and sixth dorsal vertebræ, inclusive.

The attempt to trace a continuity or direct connection between the disease in the cord and that in the optic and other motor nerves of the eye utterly fails. The same is stated by Dr. Leber (*vide supra*).

27.—*Anatomische Untersuchungen über Glioma Retinæ.* Von Dr. J. Hirschberg. [Archiv. für Ophthalmologie, Band xiv., 2, seite 30–102.]

Zusätze über Intraocularen Tumoren. Von A. v. Graefe. [Ibid., xiv., 2, s. 103–144.]

Fall von Cavernösem Sarcom der Aderhant. Von Dr. Th. Leber. [Ibid., xiv., 2, s. 221–227.]

Die Intraocularen Geschwülste. Von Dr. H. Knapp. [Seiten 223, 1868.]

Anatomical Investigations into Glioma Retinæ. By Dr. J. Hirschberg.

Supplementary Observations upon Intraocular Tumors. By Dr. A. von Graefe.

A Case of Cavernous Sarcoma of the Choroid. By Dr. Th. Leber. [Archiv. für Ophthalmologie.]

Intraocular Tumors, from Clinical Operations and Anatomical Investigations. By Dr. H. Knapp. With seventy Illustrations and one colored Plate. 8vo, pages 223, 1868.

In the articles and treatise above designated we have an elaborate account of the most common forms of tumors which occur within the eye. Dr. Hirschberg gives a description of the anatomical appearances of eight eyes, which were enucleated by Prof. Graefe, and in which there was glioma of the retina. Prof. Graefe offers observations founded upon an experience, as he says, of about 150 cases of tumors growing within the eye. Prof. Knapp gives a minute account of fourteen eyes which he extirpated, and general remarks upon the whole subject; his drawings are very clear, and the whole brochure is characterized by thorough treatment of the subject.

We may attempt to convey only the salient features of the papers, and rather as a digest of them all than by quoting particularly from each or all of them.

The tumors which form in the eye are chiefly glioma, sarcoma, carcinoma, and gummata. Sometimes these types are combined, and there are other forms which occasionally occur. The most common are glioma, which begins in the retina, and sarcoma, which belongs to the choroid. The first of these is the disease which, in its extreme degrees, corresponds to the old name of encephaloid of the eye, or fungus hæmatodes; a name which would cover any kind of protuberance which was greatly inclined to bleed and to grow with rapidity. Of course, the advance of morbid anatomy has rendered this term valueless in an anatomical sense. Glioma

is a name first given by Virchow to a growth whose type is in the layer of granular cells of the retina. Its distinctive elements are cells about as large as the white corpuscles of the blood, with a large nucleus, and with or without one or more nucleoli; they are embedded in a homogeneous matrix, which, after a time, appears fibrillated. Sarcoma presents varieties of type, but is a tumor of firmer consistence, and contains round and fusiform cells, which may be small or large, mingled with a fibrous intercellular substance, and this in varying proportions. Dr. Knapp divides sarcoma into the melano-sarcoma, the simple white sarcoma, the vascular, and the inflammatory sarcoma. There is also described by Dr. Leber a case of sarcoma with cavernous structure.

In the anatomical sense, neither glioma nor sarcoma is a cancerous disease, although both may be mixed in the same growth; for example, Virchow describes a case which he calls sarcoma carcinomatosum, in which, besides the round and fusiform cells belonging properly to sarcoma, were alveoli filled with large round or polygonal cells packed closely together, some colorless, others colored. The latter elements belong to the carcinomatous type. But both sarcoma and glioma are to be considered malignant diseases, inasmuch as they may recur at the same spot, are liable to occur in other organs, and are often fatal.

These growths in the eye have an interest for general pathology, because they can be studied from their very beginning, and they may help to solve the general question of curability, and, when removed early, there is entire certainty that the whole of the disease has been gotten rid of.

Glioma is especially the disease of children; it may affect both eyes simultaneously or in succession. It is the most common cause of the visible yellow reflex seen through the pupil called first by Beer cat's eye amaurosis. It is first made known by partial or total blindness, and by the ophthalmoscope is recognized in the form of small yellowish nodules or plaques in the retina. No inflammatory symptoms precede, and by this fact the disease will be distinguished from similar appearances which result from plastic choroiditis, in consequence of cerebro-spinal meningitis. When the deposit has begun it steadily increases—it soon excites sub-retinal effusion—it invades the choroid and fills up the vitreous—*it extends outside of the eye along the optic nerve.* When it has filled the globe to a certain point, it causes symptoms of irritation and tension, in a word, the phenomena of glaucoma. It makes its appearance outside of the eyeball in from one to three years. If this occur on the back part of the globe it is difficult to recognize, until the movements are visibly impeded or the globe becomes prominent. Before escaping from the globe the glioma may cause internal suppuration and atrophy of the bulb. To diagnosticate a tumor within a phthisical bulb is not easy. On this point Graefe relates an instructive case. A patient had had sub-retinal effusion, afterward the eye became glaucomatous, and during this period the eye was, of course, very painful, and as atrophy ensued the pain did not relent. Then the woman was sent to Professor Graefe for enucleation of the eye. He, from the history, suspected the existence of tumor as the cause of the primary sub-retinal effusion, and had his suspicions confirmed by the ensuing glaucoma, the atrophy, the painfulness of the eye, and its peculiar physiognomy. When phthisis bulbi and tumor coincide, the globe has a peculiar flattening of its front portion, while the equatorial diameters remain as great as usual. Very soon retro-bulbar growth takes place, and this gives the atrophied ball a degree of exophthalmus.

It may be remarked, as Graefe states, that increase of tension, united with sub-retinal effusion, gives strong reason for suspecting the growth of an intraocular tumor, because sub-retinal effusion, when caused by the

ordinary lesions, is attended with normal or diminished tension. Sarcoma may give rise to phthisis bulbi, and Graefe thinks this tends to retard the increase of the tumor, but such is not the effect on glioma. It extends chiefly along the nerve, it is never regressive. It may undergo fatty degeneration, partial calcification, and pigmentation from hæmorrhage. It contains a large number of blood-vessels. It soon involves the orbital tissues, and, if the patient survive, may present an enormous mass of protuberant and disgusting substance. It invades the brain, and may extend to the spinal cord, as is related by Dr. Knapp in his sixth case. Metastasis upon other viscera may take place, but is not so common in glioma as in sarcoma. Dr. Knapp has seen congenital glioma, has known four children out of seven in one family to have it, and of his seven cases six were boys. The prognosis is bad, perhaps not quite hopeless, if the eye be extirpated early. Graefe insists strongly, and with great wisdom, on the necessity of excising as much as possible of the optic nerve when the globe is enucleated. He does this with a long slender curved knife, which is passed deep into the orbit before the connections of the eye are severed. It is much easier to cut the nerve thus, than to pick it up after the eye is out; its end is concealed by the blood and muscles so as to be almost indistinguishable. Dr. Knapp advises that, if the operation is not done until glaucomatous symptoms have begun, we should not be content with simple enucleation, but remove all the contents of the orbit; because, at this stage the germs of glioma are almost certain to have been disseminated in the orbital tissues. The disease is certain to recur in the orbit if it have not been thoroughly extirpated. We are not certain that rigorous and entire ablation will protect the patient from a fatal issue from subsequent appearance of the disease at some other point—in the brain, in the diploe of the cranial bones, etc. But we do make it very unlikely to reappear in the orbit, and Graefe, from the experience of about fifty cases of glioma, urges the earliest possible removal of the eye.

Sarcoma has certain features in which it differs broadly from glioma. It occurs in youth and adults, not in young children, does not affect both eyes, except in very rare cases; grows slowly, one case remained stationary ten years; is subject to regressive metamorphosis by fatty degeneration; is very liable to development of pigment, constituting the melanotic tumor of former times; it imperils life, not so often from direct extension to the brain, but by metastasis to remote organs, especially the liver, but also the kidneys, spleen, etc.; it extends outside of the eye along the sheath of the nerve; it forms extraocular tumors, but may be completely extirpated with more certainty than can glioma, and is not as liable to repetitions in the orbit. Graefe has seen three cases, in which the second eye became blind by simple atrophy of the nerve, and in two of these cases melanotic tumors were found at the base of the brain pressing in the chiasm. In all forms of intraocular tumors the patient is liable to severe attacks of pain from hæmorrhage, which suddenly increases the tension, as well as from true glaucomatous symptoms—also sub-retinal effusion usually occurs at an early period in both glioma and sarcoma. The refractive media remain clear until inflammatory symptoms are set up.

The diagnosis between glioma and sarcoma is founded upon the age of the subject, the former belonging to the very young, and the latter never yet known to affect both eyes. Glioma begins as several nodules or a patch; has a glistening, yellow color. Sarcoma begins as a single node, is white or black, less vascular, and overlaid by the retina. If there be post-retinal effusion and the fluid turbid, a sarcoma may be entirely concealed for a time; but as it grows it pushes the retina forward in a bosselated form, and shows more or less of its dark color. Dr. Knapp recommends the use of

direct sunlight in a dark room, to make the case clearer, and this can do a blind eye no harm except by its heat. In glioma, if the retina be detached, a bright, ochrey, yellow color shines close behind the lens, while a sarcoma, if large and white, will have a more dim and gray color. The retina in glioma is smooth, but in sarcoma floats about unless it have acquired adhesion.

The diagnosis between gliomatous and sarcomatous tumors has a value upon prognosis, rather than as regards treatment—removal being necessary at an early period in both—but the prospect of saving life is much better in sarcoma. In one case Graefe knows the patient to be surviving ten years after the operation.

Miscellaneous and Scientific Notes.

We notice, among the patents recently issued at Washington, one in the name of Dr. Alfred C. Garratt, of Boston, Mass., for a new battery, for medical purposes.

Academy of Medicine.—At the annual meeting, held January 7, 1869, the following-named officers were elected: President, Dr. H. D. Bulkley; Vice-President, Joseph C. Hutchison; Trustee, Dr. James L. Banks; Committee on Admissions, Dr. Wm. T. White, long term—Dr. John H. Hinton, short term; Committee on Medical Ethics, Dr. T. C. Finnell; Committee on Medical Education, Dr. John C. Draper.

Pathological Society.—The following-named officers were elected at the annual meeting, held January 13, 1869: President, Lewis A. Sayre, M. D.; Vice-Presidents, J. C. Hutchinson, M. D.; E. Lee Jones, M. D.; Secretary, George Shrady, M. D.; Treasurer, W. B. Bibbins, M. D.

National Institute.—Academy of Medical Sciences.—This Academy was organized December 31, 1868, by the selection of the following officers to serve during the year 1869: President, Austin Flint, Sr., M. D.; Vice-President, Wm. A. Hammond, M. D.; Secretary, D. B. St. John Roosa, M. D.; Treasurer, Otis N. Fessenden, M. D.; Council, Thomas M. Markoe, M. D.; Frank H. Hamilton, M. D.

This Academy is the seventh organized under the Constitution of the National Institute. It is hoped that the Institute will be in full and perfect working order during the present year, and that it will become, from the nature of the organization, and the combination of so many elements, prominent among our scientific associations, and an honor to this country. Every effort will be made to give the broadest possible scope to its workings, and make it in fact as well as in name a national affair.

Dr. Usher Parsons died in Providence, R. I., December 19, 1868, at the age of eighty years. Dr. Parsons was a surgeon in the Navy in the War of 1812, and was present at the battle of Lake Erie, under Commodore Perry. He was a native of Maine, and entered the Navy at the age of twenty-three. He resigned in 1823, and was subsequently appointed Professor of Anatomy in Dartmouth College, but soon after settled in practice at Providence, R. I., where for many years he was regarded as the leading member of the profession. He was well known as an author, and contributor to the medical journals.

The death of M. Sichel, the celebrated ophthalmologist, of Paris, is announced.

Dr. A. E. Peticolas, Superintendent of the Eastern Lunatic Asylum at Williamsburg, Virginia, and formerly Professor of Anatomy in the Richmond Medical School, committed suicide, November 28, 1868, by leaping from a window in one of the upper stories of the asylum. He had for some time past been insane. Dr. Moore, late Surgeon-General of the Confederate Army, has been appointed to fill the vacancy caused by Dr. Peticolas's death.

Dr. Cohnheim, well known to the profession in this country by his pathological researches, has been appointed Professor of Pathological Anatomy at Kiel.

Removal of the Entire Tongue.—Dr. Fenwick, of Montreal, Canada, assisted by Professor G. W. Campbell, of the

McGill University, removed the entire tongue on account of epithelioma, on the 20th of November last. The operation was speedy and bloodless, and quite similar to that of Mr. Nunneley, of Leeds, England. The mouth was entered from the floor, the incision being in the median line, between the chin and hyoid bone—the dissection being then carried between the genio-hyoid muscles. Through this opening the chain of a Chassaignac's écraseur was carried on a long curved needle back to the base of the tongue, close to the epiglottis. The process of cutting through the tongue with the chain occupied nine and a half minutes. The case progressed most favorably, the patient being able to return to his home, a distance of one hundred and ninety miles, on the twelfth day after the operation. This is said to be the first occasion of the performance of this operation in Canada.

Mr. Sampson Gamgee has also recently performed this operation at the Queen's Hospital, Birmingham, England, but not with an equally favorable result. His plan of operation differed somewhat from Dr. Fenwick's procedure. Following the method proposed by Professor Regnoli, of Pisa, he made a semilunar incision along the base of the lower jaw from the symphysis outward on either side, to a point just anterior to the facial artery. A second incision was carried vertically downward from the symphysis to the hyoid bone. The triangular flaps thus formed were dissected back. Entrance was then made, as in the previous case, through the floor of the mouth, and the tongue, being drawn down through the wound, was transfixed at its base and removed by a double, or rather by two écraseurs cutting right and left. The extent of disease was much greater than in Dr. Fenwick's case, and the patient died on the ninth day after the operation.

Dr. Lespiaud, one of the staff of the Hospital of Val-de-Grâce, has recently inoculated himself with tubercular matter from a phthisical subject. The result of this heroic experimentation will be looked for with interest; but we cannot help thinking that, whichever way it may turn, it will prove of little practical value. Villemin's experiments have now often been repeated on the lower animals, and his conclusions

shown to be erroneous, or at least delusive; but this is probably the first instance of putting the human body to the direct test of questioning by experimentation with tubercular matter. We may readily applaud M. Lespiaud's heroism, but, at the same time, the thought will obtrude itself—might not this daring have been put to better uses?

Doctors' and Lawyers' Patrons.—I have known many people who would listen to any quack in medicine, and swallow almost any prescription, but never one who, when he found himself involved in a legal difficulty, did not desire the advice of a legal practitioner, and the best, too, whose services he could command. A man, who is positive and dogmatical with his physician or his clergyman, is apt to be submissive to his lawyer, for the reason that when he meddles with the law he knows that he is trifling with edged tools, which may cut deep when he least expects it. "What are you going to do next?" said a client to an astute old lawyer in a neighboring city. "I am going," said the lawyer, "to file a demurrer." "A demurrer, and what is that?" "A demurrer is what your Maker never intended that you should understand!"—*Geo. Wm. Brown.*

Prof. Claude Bernard and Dr. Brown-Séquard.—The chair of General Physiology at the Sorbonne, Paris, created for Magendie, and so worthily filled by Claude Bernard, has been transferred to the Museum of Natural History. By this means a large laboratory will be secured—the government expending 100,000 francs for the purchase and completion of instruments—and the professor be enabled to pursue his investigations and experiments publicly.

The chair of Comparative Physiology of the Museum passes to the Sorbonne, under the title of the Chair of Physiology, and this it was understood would be given to Dr. Brown-Séquard; but the *Lancet* (January 2, 1869) announces that the chair of Comparative Medicine, which had been created for Rayer, and which, since his death, has remained unoccupied, will be again put up, and given to Dr. Brown-Séquard, under the new name of the chair of Comparative Pathology.

Advice to Students on taking Notes of Lectures.—Surveying the careers of some of my old companions, I see that some who took but few notes have become distinguished men of robust intellect; and, on the other hand, I see others, whose note-books would form a library, who have settled down into the most humdrum routine—very types of mediocrity. I therefore conclude that a man of many notes will not necessarily make a man of note. What is the great end of education? It is not simply to heap up the greatest amount of knowledge in the mind. The aim should rather be to discipline the intellect, to give precision and quickness to the faculties of perception and observation, to strengthen the power of reasoning and comparing, so as to form just and rapid conclusions upon the cases and problems that arise in daily practice. "J'aime mieux," said Montaigne, "forger mon âme, que la meubler." Now, this end is not attained, I think, by trusting to note-books. The art of writing and the invention of printing have even been thought by some to have done doubtful service in strengthening the human intellect, however vast may be their influence in extending and diffusing knowledge. Plato said that, without this delusive aid of alphabetical writing, "men would have been compelled to exercise the understanding and the memory, and by deep meditation to make truth thoroughly their own. Now, on the contrary, much knowledge is traced on paper, but little is engraved on the soul." Quintilian too said, "Memoriæ plerumque inhæret fidelius, quod nulla scribendi securitate laxatur." And I may enforce this argument by quoting the opinion of one honored alike amongst women and amongst men, and deserving of special honor in this hospital. Florence Nightingale says, "If you find it help you to take notes, by all means do so; I think it more often tames than strengthens the memory and observation." What I have said of note-taking does not apply to recording facts and observations. There is no better means of training the mind to the habit of orderly observation and precision in judgment than the methodical practice of taking notes of cases of sickness. The power of taking a case correctly, seizing the important points, and avoiding that prolixity and repetition which render subsequent study of the history tedious and repulsive, is invaluable to the student and to the practical physician. Each well-recorded case has its individual worth as an exercise and as a record of facts; and cases acquire an interest and impart instruction which cannot be limited when their number affords the materials for comparison, and for drawing general conclusions.—*From Barnes's Introductory Lecture at St. Thomas's Hospital.*

Aphorisms for Bathers.—The committee of the Royal Humane Society have issued the following rules for the guidance of bathers. They were framed by Dr. Christian and Dr. Sieveking. These rules, which are sensible and practical, cannot be too extensively known:

"Avoid bathing within *two* hours after a meal.

"Avoid bathing when exhausted by fatigue or from any other cause.

"Avoid bathing when the body is cooling after perspiration; but—

"Bathe when the body is warm, provided no time is lost in getting into the water.

"Avoid chilling the body by sitting or standing *naked* on the banks or in boats after having been in the water.

"Avoid remaining too long in the water. Leave the water immediately there is the slightest feeling of chilliness.

"Avoid bathing altogether in the open air if, after having been a short time in the water, there is a sense of chilliness with numbness of the hands and feet.

"The vigorous and strong may bathe early in the morning on an empty stomach.

"The young, and those that are weak, had better bathe three hours after a meal. The best time for such is from two to three hours after breakfast.

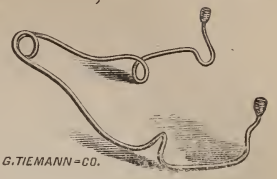
"Those who are subject to attacks of giddiness and faintness, and those who suffer from palpitation and other sense of discomfort at the heart, should not bathe without first consulting their medical adviser."—*Lancet, Aug.* 8, 1868.

A new Speculum Oculi especially adapted for Graefe's operation (modified linear extraction) for Cataract. By Charles A. Hart, M. D., of New York City.

In the summer of 1867, while performing for the first time Graefe's operation for cataract, I became very much embarrassed, and nearly foiled, during the formation of the flap, from the heel of the knife rubbing, and catching upon the spring or heel of the ordinary speculum I was using. This I had noticed before with other operators, but up to this period had failed fully to appreciate the annoyance and danger it caused; but, having once experienced it, I determined, if possible, to obtain some safer and better appliance for separating the lids.

Not being able to find, among the numerous instruments for this purpose, any thing to fulfil the required indications, I devised the following instrument, composed of wire, with the

heel or spring bent at an angle of about 45°, which allows it to rest alongside of the nose. The extreme points of the retractors are left long, and furnished with a small roughened bulb, by which the instrument is seized for adjustment.



The advantages of this arrangement over other ocular speculums are—

First. The external commissure is left unoccupied by the spring, so that a free space is afforded for the manipulations of the instrument to be used.

Second. It is easier of adjustment; the smooth springs of the others frequently allowing them to slip from the fingers, there being no roughened point to seize them by.

Third. It is equally applicable to operations upon the nasal side of the globe, by simply reversing the adjustment.

Since devising the instrument, it has been used a number of times by my friend Dr. Ceccarini and myself, in Graefe's and other operations, with entire satisfaction, and I take pleasure in offering it to the profession, feeling assured that its merits, for linear extraction at least, will recommend it for adoption. The instrument is made by Messrs. Tiemann & Co., of this city.

Influence of Marriage on the Duration of Human Life.—Dr. Stark has recently directed attention to the "Influence of Marriage on the Death-rates of Men and Women in Scotland." The following extract, quoted in the *London and Edinburgh Monthly Medical Journal* of 1841 from the *Bulletin Méd. Belge* of 1839, will show how many of the most important of Dr. Stark's conclusions have been anticipated: "Bellefroid, a Belgian physician, has recently published a statistical memoir, in which he states that marriage greatly increases the probability of life in both sexes. Women, he says, who marry at twenty have a chance of life of eleven years greater than that of those who remain single. The same doctrine holds true, apparently, at all periods of life. The probabilities of life for married men exceed those of bachelors by nineteen years, thus exceeding that of the married female by eight years—a difference probably caused by the mortality resulting from childbirth. Thus it appears that, from the age of twenty to thirty, the mortality of husbands to bachelors is

as one to twelve, while that of wives to spinsters is only as one to six for the same period of life."

How to test the Purity of Water.—It is of importance to be able to test the quality of water, not only when for special purposes absolutely pure water is required, but even in cases where such purity is not requisite, it may be of great interest to ascertain of what the impurities consist. The following short notice of the tests for the most commonly occurring impurities, will be welcome and useful to many of our readers.

Pure Water must satisfy the following Conditions.

1. It must have no residue whatever when evaporated in a clear porcelain or platina dish.

2. It must form no precipitate with a solution of nitrate of silver, which would indicate common salt, some other chloride, or hydrochloric acid.

3. It must not precipitate with a solution of chloride of barium, which would indicate a sulphate or sulphuric acid.

4. It must form no precipitate with oxalate of ammonia, as this would indicate some soluble salt of lime.

5. It must not assume any dark or other shade of color when passing sulphuretted hydrogen gas through it, or mixing it with the solution of a sulphide salt, as this would indicate the presence of lead, iron, or some other metal.

6. It must not become milky by the addition of lime-water, or a clear solution of sugar of lead, as this would indicate carbonic acid.

7. It must not discolor by adding solutions of corrosive sublimate, or chloride of gold, or sulphate of zinc, which discoloring would indicate the presence of organic substances. When boiling water with chloride of gold, the least trace of organic matter will reduce the gold, and color the water brown.

Results of these Tests.

1. Almost all spring-waters are found to leave a residue upon evaporation.

2. Common salt is not only found in most springs and rivers, but even in rain-water, many miles inland, when the wind blows from the ocean.

3. Sulphuric acid and sulphates are found in many springs. The Oak Orchard Spring, N. Y., for instance, is very rich in the free acid.

4. Waters from lime regions all contain lime in large quantities, and, in fact, this is the most common impurity of spring-waters.

5. Iron is contained in large quantity in the so-called chalybeate springs; also copper and other metals are encountered; lead incidentally, by the lead tubes through which it often is made to pass.

6. Carbonic acid is the most common impurity, even distilled water is not always free from it. Water will naturally absorb carbonic acid gas from the atmosphere, which latter always contains it; its principal source of supply being derived from the exhalations of man and animals.

7. Organic substances are often found in the water of running brooks, streams, and rivers, and are of course obtained from the vegetation and animal life in the water itself, and from the shores along which it floats.

Remarks.

1. The healthfulness of water depends on the nature of the residue left after evaporation; for many chemical and other operations, where absolutely pure water is required, the leaving of residue at once proves the water unfit for use.

2. The existence of small quantities of common salt in the water is not objectionable, it being not injurious to health.

3. Sulphuric acid and sulphates may be objectionable for daily use; however, such waters are used medically to stop diarrhœa and excessive tendency to perspiration.

4. Lime-waters do not agree with some constitutions, producing diarrhœa and divers disturbances; very small quantities of lime, however, are not injurious.

5. Iron is healthy, and is a tonic; in fact, this metal and manganese are the only ones which may be used in large doses, not only with impunity, but even with benefit; however, there is also a limit. Over-doses of iron may produce diarrhœa, and slight eruptions of the skin, or pimples.

6. Carbonic acid is not objectionable when drinking the water; on the contrary, it makes it more palatable, and most mineral waters owe their reputation to this substance.

7. Organic substances are perhaps the most objectionable, principally when decaying; such waters may even propagate diseases, and require careful filtering, or boiling, or both, to make them fit for internal consumption.—*Scientific American.*

The Medical Aspects of a Fast Life.—We do not mean a life of excitement and dissipation, or a disregard of moral considerations, but that species of go-aheadism which we see in its freest development, perhaps, among the Americans. In no country in the world is the struggle for gain more actively carried on than in the United States, and those who have watched the habits of the commercial classes in London and

New York, declare that the industry of the former is as child's play compared with the unremitting energy of the latter. The number of New York lawyers and merchants who take what Englishmen would call a vacation is said to be exceedingly small; and, according to the correspondent of the *Daily News*, they send their wives and families to the seaside or the mountains, where they join them at night, or run down by rail from Saturday to Sunday. The consequence of all this high-pressure business energy, combined, as it frequently is, with a considerable amount of excitement and anxiety, and a devotion to iced drinks and whiskey, is an increased occurrence of cases of nervous exhaustion, often ending in paralysis or lunacy. It will be remembered that, during the period of extreme heat at New York, there was a very unusual prevalence of heat-apoplexy, and if, as we are informed, ardent spirits are consumed there very much as beer is in this country, we need not be surprised at it. As the late Sir Charles Napier remarked, in reference to the maintenance of his own health, the heat of India found no ally in the alcohol in *his* brain. The moderate consumption of bitter beer and the enjoyment of a thorough holiday in the country, or at the seaside for some weeks, by our city merchants, are undoubtedly much more favorable to the maintenance of health and life than the habits of their hard-working, energetic cousins of New York.—*Lancet.*

How to Disguise the Taste of Quinine.—Dr. R. W. Parke, of Mobile, Ala., says that chocolate will completely disguise the taste of this medicine.

Let the patient obtain a few "chocolate drops" from the confectioner, and he can take quinine in solution without tasting it. Immediately after each dose is swallowed, put two or three chocolate drops in the mouth and chew them up, and the bitter taste of quinine will no longer be perceived. Chocolate, perhaps, would answer the same purpose, but I have not tried it. Any one can satisfy himself of the truth of the above statement by filling the mouth with a solution of quinine, and using the chocolate drops immediately after ejecting it. By this simple means, the solution of quinine can be used, when otherwise the pillular form would have to be resorted to. Oftentimes it is desirable to get the patient quickly under the influence of the remedy, which could not be done where pills are used.—*Med. and Surg. Reporter.*

M. Auzias Turenne on Rabies.—This ingenious investigator, in a paper lately read before the Academy of Medicine

of Paris, endeavors to establish a parallel between the phenomena of rabies and those of syphilis. It will be remembered that Marochetti considered that the virus, after having been absorbed by the wound, enters the circulation, and then gathers under the tongue. From the third to the ninth day little vesicles or pustules appear on either side of the frenum linguæ; they contain the virus, and are called lyssæ. Marochetti believed that by cauterizing these vesicles the further progress of the disease might be stayed. Experience has not confirmed his views; but M. Turenne, starting from these phenomena, has attempted to liken rabies to syphilis, and the lyssæ to the infecting chancre.—*Lancet.*

Distinguished Madmen.—Another and very different illustration of Insanity in Fact is to be found in the long list of the world's most distinguished statesmen, generals, philosophers, men of science, poets, and musical composers, who were at some period of their lives, and in some form, insane in a medical sense.

> "What thin partitions do our souls divide!
> Great wits to madness nearly are allied"—

is true of all times and peoples; and of no time or people more true than of our own. Were we to confine ourselves even to Scotland at the present day, or to our own memory or experience, it would not be difficult to draw up a long sad catalogue of distinguished men—of great ones of the past or present—whose reason became beclouded. But to do this would be invading the privacies and outraging the proprieties that belong to domestic life. The details of the existence of celebrities who lived in remoter ages have been given to history, and are public property; and, in *their* case, there is not the same delicacy required in quoting them for our example or purpose. We cannot here, however, do more than give a few of the principal instances that occur—instances of men whose names are, and are likely to remain, household words:

Socrates (according to Plato and Xenophon), Luther, Shelley, all had hallucinations. Joan of Arc gloried in her celestial visions. Mohammed, Van Helmont, Loyola, St. Francis Xavier, St. Dominic, Swedenborg, Cordan, Lavater, Zimmerman—all had visions. Julius Cæsar and Napoleon were epileptic; Cromwell, Dr. Francia, Dr. Johnson, Beethoven, were hypochondriacal. Chateaubriand and George Sand had suicidal impulses. Paganini was cataleptic; Molière was subject to convulsions. Metastasio early suffered from nervous affections; Donizetti died in an asylum; and Linnæus died in *senile dementia.* Peter the Great, Sir Isaac Newton, Tasso,

Swift, Cowper, Chatterton, Lucretius, Charles V., Richelieu, Rousseau, Fourrier, Pascal, Auguste Comte, Albertus Magnus, were all, at some period of their lives, insane; while Bernardin St. Pierre and St. Simon were also probably so.—*Excelsior, or Murray Royal Institution Literary Gazette.*

Detection of Arsenic in Cases of Poisoning.—M. Buchner has several times recognized the presence of sulphide of arsenic in the bodies of persons poisoned by arsenious acid. Certainly this fact has never been observed except where the corpse has been in a more or less advanced state of putrefaction; the sulphurization would appear to be due to sulphuretted hydrogen, a constant product of putrefactive decomposition. The last observation upon this point M. Buchner has made was upon the remains of a woman who had been poisoned eleven months previously. The large intestine was in full decomposition, and there were yellow marks upon the mucous membrane, caused by a fine powder which could not be removed by washing. This powder resembled the yellow deposit which is produced in arsenical solutions by sulphuretted hydrogen; further, it gave the characteristic reactions of sulphide of arsenic. Examining now whether the arsenic had been administered as sulphide, he concluded in the negative, for the following reasons: The contents of the stomach and small intestine being boiled with hydrochloric acid, and the vapors from the distillation of the acid collected in water, in a few minutes a quantity of chloride of arsenic was obtained; such would not have been the case with sulphide of arsenic, notwithstanding that this sulphide is not absolutely unacted upon by boiling concentrated hydrochloric acid. The sulphide of arsenic being insoluble in pure water and in acidulated water, it would not be carried into the circulation, also it would not be found in the liver and spleen, both of which in this particular case were saturated with arsenic. A part of the stomach and small intestine, cut up and placed in the dyalyser with water acidulated with hydrochloric acid, gave at the end of twenty-four hours a solution containing arsenious acid in sensible proportion, a fact proving that all the arsenic had not passed into the state of sulphide.—*Chemical News.*

Advice to Students—The Importance of Keeping Records of Cases.—Every student should keep a record of his cases. But note-taking, to be of any service, is by no means so easy as it appears. Like other things, it requires an apprenticeship. Johnson, referring to the numerous corrections made by Milton in his earlier works, observes that "what we hope ever to do with ease, we must first learn to do with

diligence"—a remark that is eminently applicable to note-taking. To make a satisfactory report of a case demands orderly arrangement and a good method, keen, careful, and patient observation, and accurate description of symptoms and phenomena; and to these may be added intelligent appreciation of what is essential. The unpractised hand makes a sad jumble of a really valuable case by leaving out the important and inserting unessential particulars; and nothing but constant practice will enable him to take such notes as will be useful for reference at a subsequent period. The student will, perhaps, best consult his own advantage by limiting his efforts to taking three or four well-selected cases at a time, and following them out thoroughly, whether to recovery or to a fatal issue. The time absolutely required to enter the particulars of an important case is very considerable. Take, for example, a patient affected with renal disease. The report of such a case cannot be considered complete, unless, in addition to the details of the variations in the quantity, specific gravity, and chemical characters of the urine, thermometrical observations, and a careful ophthalmoscopic examination have been superadded. But all these require assiduous labor. From one such case, nevertheless, the student will learn far more than from sauntering round with the physician for a month. Evening work should consist in reading diligently some sound and practical work on the cases actually under observation. If this plan be diligently carried out, the student will find at the end of a year that there are few diseases or surgical affections which have not fallen under his notice. Such a plan will enable him to avail himself of the opportunities he now possesses of learning the mode of application of the different ingenious instruments by which the interior of the body is explored. The use of the ophthalmoscope, the laryngoscope, and the endoscope he will find very difficult to learn in after-years by himself, and he will be puzzled to describe or avail himself of what is actually under his eyes; while a few lessons from a good teacher will in a short time give him such a grasp of the subject as will enable him readily to pursue and extend it as opportunity offers.—*Lancet.*

Solvent Power of Glycerine.—The solvent power of glycerine, upon several substances commonly used in medicine and the arts, is as follows: One part of sulphur requires 2,000 parts of glycerine; iodine, 100 parts; red iodide of mercury, 340 parts; corrosive sublimate, 14 parts; sulphate of quinine, 48 parts; tannin, 6 parts; veratria, 96 parts; atropia, 50 parts; hydrochlorate of morphia, 19 parts; tartar emetic, 50 parts; iodide of sulphur, 60 parts; iodide of potassium, 3 parts; sulphide of potassium, 10 parts.

The late Prof. Serres, of Paris, has left £2,400 to the Académie de Médecine, the interest of which, every three years, is to be given as a prize to the best work on general Embryology, applied, as far as possible, to Physiology and Medicine.

Fungi and Disease.—The question which is now on the *tapis* in professional circles, and which it is of the highest interest to obtain a satisfactory reply to, is: In how far do fungi and disease stand to each other in the relation of cause to effect? Professor Hallier, Mr. Simon, Dr. Salisbury, and all that school, believe implicitly in the influence of fungi as causes of disease. On the other side, we have two very able authorities in this country in Dr. Thudichum and the Rev. J. M. Berkeley, who utterly deny the fungus hypothesis. Now, it must be admitted that up to this time the advocates of the fungus theory, on whom the *onus probandi* fairly lies, have failed in all cases to do more than show the coincidence of fungi and disease. But this is but small ground for the inference they draw from the observation. As has been often suggested, both the disease and the fungus may be coincident terms of the same unknown condition. The crucial test is a tolerably easy one. Let Dr. Salisbury and his party propagate their fungi, and then, by inoculation, reproduce the disease with which the parent fungi were originally associated. This would convince every one. But really, till it is accomplished, it is unwise to push too far a fascinating hypothesis, which too sanguine practitioners may make the basis of an unsound and therefore dangerous practice.—*Med. Times and Gaz.*

A New Galvanic Battery.—The *Telegrapher* says that a new voltaic combination of great power has just been exhibited to some of the learned societies of Great Britain. It is the invention of Messrs. Dr. La Rue and Hugo Miller, and has been designed for Mr. Gassiott. The elements consist of small cylinders of pure zinc and chloride of silver.

In the battery shown, the cylinders were only three inches long and about the size of a goose-quill, arranged in two ounce phials cut down to two-thirds of their length; but a series of ten such couples decomposed water with great rapidity. By the chemical action taking place in the cell, the chloride of silver is reduced and chloride of zinc formed. The action proceeds so long as any chloride remains, for the reduced silver adheres to the wire as a spongy mass, which allows the liquid to permeate to any unreduced chloride.

The first cost of such a battery will be considerable; but as the only loss will be a little zinc, it will be very economical in working. M. Gassiott, it is said, is having a battery of one thousand pairs constructed, of which, no doubt, the scientific world will, in good time, hear and learn much.—*Scientific American.*

Monstrous Birth.—The Berlin journals give an account of a remarkable monster of which the wife of a joiner at Schkeudnitz was delivered on the 26th of May. It was a still-born child, apparently of normal length, but with a somewhat large thorax. Between the shoulders two well-formed and shaped heads are to be seen, covered with hair; the features are well wrought, each head being on a rather long but strong neck, and between the two necks a third arm projects, which is somewhat bigger than the two normal arms; the hand of that arm has two thumbs in an opposite direction, producing the impression of two arms having been united in one. One day before delivery, all the signs of a living fœtus had been observed. The mother had borne several healthy children, and in this instance, too, the time of pregnancy was perfectly normal.—*Medical Press and Circular.*

We learn, upon reliable authority, that there is every prospect that the attempt which is being made, to add an important staple to the resources of the island of Jamaica, in the cultivation of cinchonas, will be attended with success. Under the direction of Mr. Robert Thomson, operations were at first confined to propagation, which was undertaken in a systematic way in the early part of 1866. At the present time, as the result of the growth of cuttings and of seed furnished by Dr. Hooker from Ceylon, there are about 25,000 plants in vigorous growth. In May, 1867, a score of *Cinchona simarubra* were transferred to a site of an altitude of 3,700 feet, when they were about six inches in height. At the beginning of the present year, they had grown to that of three feet. The larger number, however, of the 25,000 were in pots; 500 only had been planted out at a height of 5,200 feet. Mr. Thomson, we understand, is fully impressed with the opinion that the cultivation of the cinchona will be highly remunerative.—*Standard.*

Social Statistics of England.—In the mean time the publication of the annual statistics of the police department give a very faint and imperfect idea of the condition of society under the existing arrangements. In England and Wales there are nearly one million of paupers. The wretched-

ly poor, who are helped by private charity, but are not upon the rates, are a much larger number. The persons registered as belonging to the dangerous or criminal classes are 112,403. Of these the known thieves and depredators are 22,889; prostitutes, 25,619; suspected persons, 28,378; vagrants, 32,558. Of these various classes, 15,109 are under 16 years old. But these figures really give but a partial idea of the extent of these social evils, which no ten days' revolution can drive across the frontier. But even this imperfect record gives some curious results in its analysis. One might suppose that "the social evil" would be rife in the large manufacturing towns. It is the contrary. The rule there is early marriage or concubinage. Women of known bad characters, and registered as such by the police, exist, in proportion to the population, in commercial ports 1 in 202; in the pleasure-towns, resorts of the rich and idle, 1 in 243; agricultural towns, 1 in 296; London, 1 in 573; in manufacturing towns, from 1 in 651 to 1 in 873. Perhaps the character of the towns engaged in the woollen manufacture is, upon the whole, the worst of any, as the returns show 1 bad character in each 99 of the population. Yet this amount of vice and crime does not affect one like the vast amount of disease, poverty, and misery, found in all the great centres of population, and which make life itself, to the reflecting and philanthropic, a continual suffering.

The poor law, as a remedy for pauperism, is an admitted failure. It has created a pauper class. It does nothing to prevent and much to continue the pauper condition. It has encouraged improvidence, early marriage, and increase of population. It has diminished parental responsibility. No country in Europe has the same burdens of pauperism and crime. In several Continental states, which make no boast of power and prosperity, or even of superior enlightenment, the entire population are educated, industrious, and comfortably provided for. Why need England, which has so much to boast of and boasts so much, in these all-important social matters, drag in the rear of civilization? But there are some signs of amendment. This is a season of self-examination and confession; let us hope that it will be followed by repentance and reformation.

President Haven, of the University of Michigan, in his last report to the board of trustees, comments on the question of introducing into the faculty homœopathic professors, viz.:

I believe that good reasons do exist why a "Professor of Homœopathy" should not be appointed, and I believe that all unprejudiced persons will be able to see them. I beg here explicitly to state that I do not argue in behalf of the medical

profession, or "Allopathy," or any particular class. I am not conscious of any particular interest in any class or party, on this subject. So far as I am personally concerned, it might be more politic to say nothing on the subject, but, as a custodian of the interests of the university, I must express what the interests of sound education seem to me to require, irrespective of party or sect.

Observe, then, first, that we have no Professor of "Allopathy" in the University of Michigan. This is no subterfuge, but a solemn fact. If a grant of money was offered to the university on condition that a Professor of Allopathy should be appointed, I should be compelled to show the unreasonableness of the condition. We do not want in a university professors of special ideas or theories, who believe that their special ideas or theories embrace all truth in their respective schools, and that all outside of their special ideas or theories is false and to be rooted up and condemned. You make the university, by such a course, a place of strife and discord, and not a place for the harmonious inculcation of all truth. What we want in the department of medicine and surgery is a number of professors who shall present all the *subjects* and all the *information* properly belonging to the science and art of medicine and surgery. They should be, as they are, Professors of *Anatomy*, *Physiology*, *Pathology*, *Surgery*, *Diseases in General*, *Diseases of particular classes*, *Chemistry*, *Materia Medica*, etc., etc.—embracing the whole orb of the science and art of medicine and surgery—but not Professors of "Allopathy," "Homœopathy," "Hydropathy," or any other special theory; and the graduates should receive, not a title—"Homœopathic Doctor," or "Allopathic Doctor," or "Hydropathic Doctor," or doctor of any particular kind, but simply the old, time-honored M. D.—Doctor of Medicine.

This is no sublimated, unapproachable theory, but the only proper basis of a university. The university does not establish a department of medicine and surgery in the interests of any particular class of physicians, or in the interest of conflicting classes of physicians, or with the special purpose of making doctors of any particular kind, or of all kinds, but to teach the science fully and broadly—not in conflicting schools and debates, but, as far as possible, thoroughly—without reference to local interests and partisan distinctions. Once establish the precedent that every party in the world shall be recognized by name, and have a professor bearing its *partisan name*, and irreparable injury is done to the university.

A FACT of some interest, if it be confirmed by further experiments, has recently been announced by the French medi-

cal journals. M. Telephe Desmartis, a medical experimenter of Bordeaux, has succeeded, it is said, in inoculating upon plants tubercular matter taken from the human lung. The result has been the production of a particular kind of mycelium. This leads M. Desmartis to establish a comparison between tubercle and sphacelia, or ergot of rye.—*Lancet.*

From a recent Vienna journal we translate an item that may interest our readers:

"Among the students attending the clinical lectures of Dr. Politzer on aural surgery is a young American lady, who has crossed the ocean for the purpose of devoting herself at the Vienna University to the study of diseases of the ear, with the intention of practising aural surgery as a specialty in her native country. This young lady, 19 years old, is the daughter of a New-York lawyer, and by diligent private studies eminently qualified to pursue the career she has entered upon. She is the first lady that has ever—though as an extraordinary student (*ausserordentliche Flörerin*)—been matriculated here, and the present dean of the medical faculty, Professor Dr. Brücke, deserves all praise for having permitted her at once, disregarding all academic prejudices, to attend lectures. All the blanks, matriculation and examination papers, had to be altered to suit this particular case. Miss Laura M., a prepossessing personage, with modest demeanor, who, in her plain but elegant toilet, shuns scrupulously all conspicuousness, frequents diligently the otiatric lectures of Dr. Politzer, attends besides several private courses on anatomy and pathology, and spends several hours daily in the anatomical theatre dissecting. Her associates have already become used to the presence of a woman in their midst, and the more so as she combines in the different manipulations and operations skill and assurance with womanly neatness and tenderness."

Treatment of Stricture by the Magneto-Electric Current.—Dr. Chadsey, of this city, has kindly furnished us the notes of the case of stricture referred to by him at the November meeting of the County Medical Society, and reported in the December number of this Journal:

When practising in the country, in the winter of 1844, I was called several miles from my residence—Ballston Spa, Saratoga County, New York—late in the afternoon, to see a strong, healthy, hard-working, temperate man about 40 years old. I found him in great distress, his bladder very much distended, and that difficulty in micturition had existed for

over a week and entire suppression for two days. The cause was hard work in getting out timber upon the mountains, and exposure to cold while riding home at night. This had been his daily employment for some time previous. He had had no gonorrhœa, nor had he received any injury other than above described that I could learn. I immediately introduced a common-sized silver catheter, and found a stricture of the urethra about two-thirds of the distance from the end of the penis to the bladder. The stricture was so firm that it resisted all the efforts I dared to make to pass the catheter. I tried a common-sized gum-elastic catheter, with like success. I had no smaller catheters or bougies with me. The stricture was very firm. It extended about an inch and a half. Above it I could feel the urethra very much distended with urine. It was night, the snow deep, and no assistance or proper instruments for operating could be procured before the next day, and my patient would probably die before morning without relief. He had taken diuretics for several days with no benefit. I placed his feet in warm water—applied flannels dipped in hot water to the scrotum, sacrum, and abdomen—bled him from the arm two pints. Injected warm olive-oil and goose-oil into the urethra and pressed it hard against the stricture, and tried to overcome the stricture with the catheter repeatedly, but did not succeed in relieving him. I had in my sleigh an electro-magnetic battery, made by Pike, of this city; one of the first he made, I think, as I had used it several years. I had it brought in. I cut a small orifice in the end of the gum-elastic catheter, introduced into the catheter a common-sized steel knitting-needle properly bent, and introduced the catheter into the urethra up to the stricture—pressed the knitting-needle through the orifice in the end of the catheter hard against the stricture, applied the positive pole of the battery to the knitting-needle, and the negative pole on the outside of the scrotum over and above the stricture, and let the battery play upon it, gradually increasing its force for about twenty minutes, when I found a little urine escaping through the catheter around the knitting-needle. I stopped the battery and removed the catheter, when a small stream followed, which gradually increased in size until the bladder was evacuated of a very large quantity of urine. The opening from the bladder into the urethra had been so long distended that the sphincter muscle did not perfectly contract but let the urine pass off as it was secreted during the night. The next morning before I left I introduced the silver catheter into the bladder, and found the urethra where the stricture had been nearly as large as at any other place between that point and the end of the penis. While the catheter was in the urethra, and the end

just within the bladder, I let the electro-magnetic battery play upon the urethra and the sphincter muscle lightly as before, by connecting the positive pole with the catheter, and passing the negative over the external parts for about ten minutes, when, after withdrawing the instrument, the sphincter contracted perfectly, and my patient said he was well. I introduced the silver catheter into the bladder once a day for three days thereafter, and found no indications of a return of the stricture. I saw my patient ten years thereafter, and he had had no return of the stricture. I have applied the electro-magnetic current several times since, in nearly the same manner, to partial strictures, with success.

A Blow to the Fungus Theory of Disease.—In a short communication to the *Centralblatt*, Drs. Bergmann and Schmiedeberg describe a crystalline substance, to which they have applied the name "sulphate of sepsin," obtained from putrefying materials, and which, they believe, represents the proper poison of organic substance undergoing this kind of fermentation. It is obtained by diffusion through parchment-paper, precipitation with corrosive sublimate from an alkaline solution, removal of the mercury by silver, of silver by sulphuretted hydrogen, evaporation, and purification of the residue. Large, well-defined, acicular needles are thus obtained, which are deliquescent in the air, and, exposed to heat, melt and carbonize. They possess a powerfully-poisonous action. A solution containing scarcely more than one-hundredth of a gramme was injected into the veins of two dogs. Vomiting was immediately induced, and after a short time diarrhœa, which in the course of an hour became bloody. After nine hours the animals were killed, and on examination their stomachs and large intestines were found ecchymosed and the small intestine congested. Frogs could be killed in the same manner.—*Lancet.*

Prof. Fischer, of Berlin, has been engaged in a series of elaborate experiments, to test the value of this supposed discovery. He was unable to obtain the crystals described by Bergmann and Schmiedeberg. He arrives at the conclusion that there are several putrid poisons in putrefying pus, some of which are diffusible through animal tissue, others non-diffusible; and, although there can be little doubt that the active principles of these poisons are bodies with distinctive characteristics, yet they cannot be isolated by any means at present known to science.

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ART. I.—*Remarks on Dr. Sayre's Paper entitled "A New Operation for Artificial Hip-joint, in Bony Anchylosis."* By LOUIS BAUER, M.D., of Brooklyn.

DR. SAYRE'S paper was placed before the profession in 1863, at the meeting of the Medical Society of the State of New York; it subsequently made its appearance in the public transactions of that body, and also in pamphlet form.

The title was so striking, that it naturally attracted my attention, as no doubt it did that of many who, like myself, take an interest in the advancement of surgical art.

After a careful perusal of the paper, I could not help noticing grave discrepancies, nor withhold a passing remark on its pretensions, in the later edition of my work on "Orthopedic Surgery."

That I had no specific desire to disclose the weak points and the literary and professional errors of Dr. Sayre's paper, is plainly evident from the fact that I withheld my criticism four years, and inserted it in so unpretentious a manner as not to invite particular attention. No one, who is at all familiar with the intimate relations which he and myself entertained for at least a decade of years without interruption, can suspect me of unfriendly feelings toward the author.

That the author has taken offence at my strictures is painfully evident, from his late article, published in the January number of the *New York Medical Journal.*

To meet the respective charges of "false statements," "detraction," and "slander," I find myself under the necessity of placing my scientific estimate of Dr. Sayre's paper on record, leaving the adjudication of my offence to the profession at large.

Before entering upon my task, I wish to state here, that I never received the letter which Dr. Sayre has spread before the profession. I certainly agree with him that the miscarriage of the said letter is entirely inexplicable. Neither has any message from Dr. Sayre been transmitted to me by the publisher of my book; nor do I think that an approach in either way could have changed my views in reference to the "new operation," or modified the criticism pronounced. It is altogether preferable that the author should have submitted his grievances to the profession, and thus elicit a more appropriate review on the merits or demerits of his literary and surgical enterprise.

It will not be expected that I should pay any attention whatever to the array of letters which constitute so formidable an appendix to the before-men-

tioned publication. These may be needful indorsements for the author, but are of no material import to the analysis upon which the complaint is based. I have only to deal with the pamphlets which Dr. Sayre had the goodness to send me.

It appears that on the 11th of June, and the 6th of November, 1862, respectively, Dr. Sayre performed two operations: one upon Robert Anderson, of Kentucky; the other upon Miss Susan M. Losee, of Buffalo, N. Y.

These operations were undertaken for the ostensible purpose of both removing the deformity caused by bony anchylosis of their respective hip-joints, and improving the locomotion of both patients, by the establishment of artificial joints. The doctor claims that these operations were novel and unprecedented in character and original in design.

The *naïveté* with which Dr. Sayre speaks of his operation as evolving a "great surgical principle," and a "new scientific fact," is painfully noticeable by all his well-wishers; and the audacity with which he charges me with "slander and detraction," because of my "false statements" and my "confounding" Dr. Rhea Barton's operation with his, is certainly startling.

According to Dr. Sayre's conception, Rhea Barton's operation "was not intended to produce an artificial joint, but simply to anchylose the limb in an improved position."

I could refer him to the third volume of the *North American Medical and Surgical Journal*, in which the article of Dr. Barton originally appeared. This alone would be an unexceptionable defence against the imputations which the author has hurled at me. Inas-

much, however, as that journal may not be convenient to himself and to readers in general, I will refer him to the excellent standard work of American Surgery, by Professor Gross, which certainly graces or ought to grace his library, as it does that of every practitioner who lays claims to surgical proficiency.

In the first volume, and on page 1038, the author will readily find the information, that—

1. Dr. Rhea Barton's operation was performed in the year 1826.

2. That he "divided the bone through the great trochanter and part of its neck."

3. That "twenty days after the operation the limb was gently and cautiously moved in different directions," and

4. That the patient eventually "could not only rotate the foot, but abduct it twenty inches, and carry it backward and forward to a still greater extent."

In addition to this, I can further inform the author that this very patient of Dr. Rhea Barton retained the use of his artificial joint for six consecutive years, when it became gradually obliterated, though it had been in constant use.[1]

Barton performed another operation; Rogers[2] followed in 1830, in a patient forty-seven years old, in which the success was so perfect that all the movements of the extremity could be executed.

Textor the elder[3] performed this operation in 1841. Pseudarthrosis ensued, but the patient died of *pulmonary tuberculosis* six months afterward—a case almost as successful as the second of Dr. Sayre.

[1] Heyfelder's Resections, Vienna, 1861, p. 93.
[2] Ried on Resections, p. 395.
[3] Heyfelder on Resections, p. 94.

In 1847, Maisonneuve[1] operated with the same intent upon a young man eighteen years of age; and G. Ross[2] in 1857, upon a woman twenty-three years old. In both cases, the divided bones united firmly by callus.

From the preceding quotations it appears that—

1. I am not guilty of any misstatement, confounding, detraction or slander; and—

2. The very same operations have been performed for the very same purpose, at the same place, and with almost the same technical execution, long before Dr. Sayre entered the professional arena.

I can well imagine that the author feels mortified, not only at being deprived of the originality of the operation in so unceremonious a manner, but also at being instructed in the literature of his own language, and acquainted with the merits of his own countryman by a foreigner. But, Dr. Sayre having taken the initiative in this controversy, I could not spare him the correction.

Most of the operators before mentioned exsected a triangular piece of bone with regular surfaces. Dr. Sayre claims to have so manipulated the chain saw, that there were left, on one extremity of the divided bone, a convex surface, and on the other a concave one. But even in this "original suggestion" Professor Meyer, of Wurtzberg,[3] has preceded the author by six years. The practical value of the semicircular method is at best very dubious, and offers no apparent advantage over the triangular. The ensuing suppuration has surely no respect for the author's mathematical finesse;

[1] Gazette des Hôpitaux, 1847, p. 94.

[2] Contributions to Orthopedic Surgery, Altona, 1858, p. 25.

[3] Deutsche Klinik, Berlin, 1856.

it will destroy and carry off the two horns of the one—as has been experienced in both cases published—and spontaneously round off the surface of the other.

Again, the author claims to have chosen a new location for the division of the bone. As far as I can ascertain, all the surgeons who have preceded him in the operation have selected a place above the insertion of the iliaco-psoas muscle. A few lines more, above or below, cannot make any material difference, so long as the lesser trochanter remains below the division.

All reliable authors insist, moreover, that the bone should be allowed to form its new connection, and the wound to close, before the establishment of passive motion. For, as long as suppuration exists, the danger of pyæmia prevails, and no prudent surgeon feels inclined to prolong this period by premature interference.

The undue haste with which the author disturbed the process of repair on the tenth day after the second operation (Losee), by passive motions commenced and continued to the 1st of February, 1863, i. e., eighty-four days, besides preventing the bones from coming in contact, was so daring a practice, and so entirely beyond the province of all therapeutical maxims, that disaster seemed inevitable.

In fine, the author claims, for both his operations, "success and recovery." To estimate that "success" and that "recovery," which is purported to have been attained, it will be necessary to analyze minutely the clinical facts which the author enumerates in his paper.

The case of Robert Anderson was taken in hand and operated upon, as already stated, on the 11th of June, 1862. The deformity was successfully overcome by the operation. The wound healed kindly, and, after the discharge of an unmentioned amount of sequestrated

bone, the patient reported himself well April 11, 1863. This proves the recovery of the patient, with a straight and useful extremity. Thus far the merits of Dr. Sayre are conceded. But, when he insists that he "succeeded in establishing a pseudarthrosis," he either deludes himself and others, or has furnished such fragmentary evidence that serious doubts must necessarily arise. The hospital records of Dr. Shaw simply prove that the patient, with the aid of crutches, could walk on the even floor of the corridor into the amphitheatre, sit down, and get up again.

Next, Anderson was examined by Dr. Sayre in the latter part of December, 1862. This examination did not furnish the very least acceptable proof of pseudarthrosis. The letter received from the patient shows that he can use the affected limb with a certain degree of facility, and that he was well satisfied with the result of the operation, as, indeed, he ought to have been.

How easily even distinguished surgeons may be misled, is exemplified in the case of Maisonneuve. When he exhibited a patient, on the 19th of January, 1849, to the Société Chirurgicale of Paris, he did it under the fullest conviction that he had secured mobility at the place of operation; this conviction was confirmed by the locomotive feats of the patient, which were more perfect than Dr. Sayre claims in the case of Anderson. Maisonneuve was brought to acknowledge his error by Michon and Gosselin.[1] It was clearly demonstrated, beyond the shadow of a doubt, that the movements of the extremity concerned were executed by the opposite hip-joint, the ilio-sacral synchondrosis, and the spine; and that the femur was completely consolidated.

Barwell likewise bears testimony to the difficul-

[1] Gazette des Hôpitaux, p. 54, 1849.

ties of discriminating between the movements performed between the affected and substituted joints. Under the caption of Anchylosis of the Hip-joint,[1] he makes the following statement: "A patient was in Charing-Cross Hospital, at the end of the last, and the beginning of the present year, in whom the mobility was remarkably developed; it was impossible to come to any conclusive judgment until chloroform was administered."

Being myself familiar with the adroitness with which patients substitute other joints in place of the anchylosed hip-joint, and aware that mistakes happen with the best of diagnosticians, Dr. Sayre must pardon me when I consider his statement, and the evidence adduced, insufficient to establish the fact that pseudarthrosis was achieved in the case of Anderson.

For this very reason, I did not refer to Anderson's case in my criticism. But my reticence shows only the doubts I entertain in reference to the reality of pseudarthrosis, and should not be construed into a denial or detraction.

In putting a "new scientific fact" upon record, or giving to the profession the details of a "new, feasible, and perfectly successful operation," the profession has the right to insist on the most positive and accurate proofs.

In the case of Miss Susan M. Losee, certain vital points are obscure or ignored, and to others, of trifling importance, superlative prominence is given. It is also impossible to understand, completely, from the paper, the chronological succession of events. If we gather the truth from the vague statement, the operation was performed on the 6th of November, 1862, and the fatal

[1] Barwell on Joints, London, 1861, p. 320.

catastrophe occurred on the 17th of May, 1864. In singular contrast with the dates given, is the publication of the reprint of the Transactions of the State Medical Society in 1863.[1]

The author informs us that within "a period of nearly four months," the patient had "entirely recovered."

On the 20th of July, 1863, we have the additional assurance that the new operation was "perfectly successful," and the patient free from all danger. But for the singular "sequel," the operation would have been one of unparalleled surgical brilliancy. In spite of this statement, however, it appears that the wound was like a volcano, sometimes quiescent, sometimes eruptive. Small fragments of bone were discharged a few months after the operation. On the 20th of April, "the wound became inflamed and puffed out." Two days later an abscess formed and the "wound opened, and a small, curved piece of bone escaped," which was "quite rough and jagged." The day before the singular death, the wound again parted; and, on *post-*

[1] By the courtesy of the Editor, I am informed that Dr. Sayre has corrected the date given in the history of the case (page 356), from *July* 20 to *February* 20, and that this correction will appear in the present number of the *Journal*. It is somewhat singular that this discrepancy should have escaped Dr. Sayre's observation until now, and that the article should have three times been published with this glaring error. In accepting this correction, I withdraw the strictures based on this point. The date of the death is thus brought a year nearer the date of the operation, and, therefore, the chances for the occurrence of pyæmia are proportionately increased.

I am sorry to add that this correction, however, only produces a new chronological and pathological discrepancy, for, on the supposition of the correctness of the dates as now given, we find that she was suffering from acute disease, and hepatization of the left lung, at the very time when we may infer that she was beginning to use the limb, and to "bear nearly her whole weight upon it."

mortem examination, a "small spicula and four fibrillæ (!) of bone half an inch in length, and as thick as the lead of an ordinary pencil," were found.

The operation, in one sense, was indeed a perfect success. A union of the bones, under the bold treatment employed, was effectually prevented. It is possible that some exfoliation of the bones might have occurred, even if perfect rest for a suitable time had been observed. It is, however, very evident that "increasing passive motion," and constant disturbance of repair, must have materially enhanced the liability of osseous decay, and furthered the unpleasant "sequel."

The effort made to disconnect the death of the patient from the operation and after-treatment, is obviously unsuccessful.

The disastrous effects ascribed to the codeia[1] and the alleged transgressions of the nurse are not very apparent.

That the ends of the bones were at no time sound, is proven by the author's own record; and that there is exceeding danger from pyæmic infection, as long as the osseous structure is decaying and granulating, notwithstanding the temporary closure of the wound, is admitted by every well-informed surgeon.

[1] According to the United States Dispensatory, by Wood and Bache, ninth ed., p. 534, Dr. Gregory experimented with the nitrate of codeia, in four and six grain doses, upon himself and upon his pupils, without producing more than an "agreeable excitement."

"Barbier, of Amiens, administered codeia *uncombined* in numerous cases, in doses of from one to two grains." "In sufficient quantity, it induced sleep without those marks of cerebral congestion occasioned by opium."

From these statements, it is obvious that the convulsions of Miss Losee have no connection whatever with the administration of codeia at one-eighth and one-quarter grain doses—more especially the second attack, which was preceded by only one dose of codeia, the one-eighth part of a grain.

The unfortunately superficial and cursory *post-mortem* examination gives but little ground upon which to establish an opinion. The alleged disuse of flannels, and the delinquencies of the nurse in the administration of codeia, cannot account for an acute tuberculosis.

It must be taken for granted that the doctor had fully satisfied himself in regard to the thoracic organs of his patient, before the operation. The pulmonary troubles of Miss Losee commenced suddenly, without any premonition, on the 5th of February, 1864, and terminated her life in two months and a half. The disease is said to have taken a very acute course, and in a short time obliterated the entire left lung, so that "no respiration could be detected on that side" (!), cough very distressing, and "no expectoration at any time."

Notwithstanding the aggravations of a constitution already enfeebled by a severe operation, the irritation caused by sequestrated bone, the purulent discharge, and the inevitable confinement, the patient rallied and the pulmonary infiltration receded, while the apex of the lung implicated became the seat of an abscess, "which Dr. Flint thought was the result of an apoplectic effusion."

Whatever might have been the nature of the attack as described, it was obviously unconnected with tubercular disease. The clinical character of the case is incompatible with the views of the author. Irrespective of the obvious causes, the lung-disease was too sudden and rapid to admit of a tubercular interpretation. It is neither at all likely, nor demonstrated by experience, that such an extensive tubercular infiltration could have occurred and afterward limited itself in

such a circumscribed manner. Multilocular abscesses were found in *both lungs*. These abscesses were entirely separated from each other; they had no communication with the bronchial tubes. During the development of the disease, not the least expectoration occurred. Pathological facts of such a direct character cannot be covered over by a microscopic examination, which at best affords a negative result in tubercular disease, as the testimony of the most renowned microscopists shows.

It seems exceedingly strange that, in this case, published for the express purpose of demonstrating the feasibility and practicability of a claimed *new operation*, the autopsy allowed should not have been a thorough one. If the examination had not been undertaken with a preconceived idea of the pathological points, it is inexplicable why the abdominal organs, and especially the liver and kidneys, were not carefully and minutely examined, and why the brain did not receive proper attention.

Granting, for the mere sake of argument, that tuberculosis of an acute character did really destroy Miss Losee, such a theory is not in the least degree incompatible with the processes of pyæmia, as the experiments of Villemin and others clearly demonstrate.

Whether the convulsions preceding the death of the patient were caused by uræmia, or by thrombosis and emboli of the cerebral vessels, cannot unfortunately be determined at this stage of the inquiry. Most of the symptoms presented in the case of Miss Losee are in every respect compatible with the views I have previously expressed—and which I still hold—in regard to the cause of death.

In reference to the specimen, but little remains to

be said. Inasmuch as I have myself, with due care, examined its pathological character, I am fully able to form and express an opinion without the spectacles of others. I most emphatically admit that, with the exception of some morbid appearances of the femur, Dr. Sayre has fully succeeded in establishing an artificial joint with its ordinary attributes, of which the specimen is a very appropriate representative. But he must pardon me when I utterly fail to recognize, with him, all the physiological attributes of a perfect hip-joint, and moreover an extra ligamentum teres.

Poetic coloration is obviously out of place in pathological anatomy.

ART. II.—*Embolic Diseases of the Eye.* By H. KNAPP, M. D., of New York, late Professor of Ophthalmology at the University of Heidelberg.

(These remarks, in substance, were made before the New York Medical Journal Association, January 22, 1869.)

OUR knowledge of *embolic diseases* in general is yet of a recent date, but already it has thrown full light on so many morbid processes, formerly quite inexplicable, that it may with justice be called one of the greatest acquisitions pathology has made in modern times, and it alone would secure to the name of *Virchow* a prominent place in the history of medical discoveries. Soon after the invention of the ophthalmoscope, "which crowns the forehead of *Helmholtz* (now Professor in Heidelberg) with the laurel of immortality," Professor Virchow prophesied that it must be possible to *observe directly*, with this instrument, *emboli in the living eye.* Four years later the prophecy was realized by

a third no less illustrious representative of the German school and of medical progress, that of Professor *Von Graefe*. It is not without intention, gentlemen, that I pronounce these glorious names, for only some five or six weeks ago I had the honor to listen, in the New York Academy of Medicine, to a lecture on Medical Progress in the Present Century, and therein not one German investigator was mentioned. Despite these omissions, the lecture was very elaborate and excellent. Had it not been so, and had it been delivered by a lesser man than *Austin Flint, Sen.*, I should not have minded it. But since even the most learned cannot aim at completeness, and we all here form an association for mutual assistance, I thought it might not be an unfit subject for a minor mind to fill up some one or other of the omissions of the great.

Out of the large series of embolic diseases, I can exhibit before you only one branch, *embolism in the eye*, and even this would take the time of several evenings, if I were to describe it fully. Nevertheless, I shall try to give you an uncurtailed picture of it, but executed in such a way, that the known parts are only roughly sketched, while I shall more minutely draw those which are but little elucidated, and point out others that are not yet known at all.

Embolic diseases of the eye may be thus divided:

A. *Thrombosis* (*a*) of the retinal, (*b*) of the choroidal, and (*c*) of the ophthalmic veins.

B. *Capillary embolia* (*a*) of the choroid, and (*b*) of the retina.

C. *Embolia* (*a*) of the choroidal, and (*b*) of the retinal arteries.

In thrombosis we must always distinguish between primary and secondary.

A (*a*). *Primary thrombosis of retinal veins has never been described.* Yet there are conditions for its development in several eye-diseases, where the venous current is checked in a high degree, so that coagulation of blood appears possible, especially under favorable conditions of the general system. In *glaucoma*, the retinal veins are compressed at the point of their exit from the eye; they are swollen and tortuous. A notable retardation of the current must be the consequence. At the same time, the increase of intraocular pressure allows only of smaller supply of arterial blood into the eye. Often even the continuous arterial current is interrupted, and the increase of intravascular pressure, caused by the systole of the heart, throws a certain quantity of blood into the retina, while during the diastole of the heart the retinal artery may be entirely compressed by the tension from the globe. In this way we find, in glaucoma, different causes for the retardation of the venous current: 1. Impediment at the outset by compression of the venous trunks. 2. The greater filling of veins and capillaries consequent to the pressure on the trunks, increasing the column of blood to be moved by the vis a tergo. 3. Weakening of the latter through compression of the arteries by the increased tension of the eye.

Although we know that, in blood-vessels with healthy walls, normal blood, even when stagnating, will for days remain fluid, the conditions for coagulation under the circumstances just described appear so favorable, that I think it fit for ophthalmoscopists to direct their attention to this point. Quite similar are the conditions in certain forms of *neuro-retinitis*, where an exudation into the ocular end of the ophthalmic nerve compresses the central retinal arteries as well as the

veins, causing swelling of the latter, and not unfrequently hæmorrhages. As we see these hæmorrhages most abundant in that form of retinitis which is dependent on Bright's disease, where the coagulability of the blood is increased, I do not think it quite devoid of sense to take into consideration the question whether the retinal hæmorrhages in this disease may not be in some connection with thrombosis in the retinal veins or capillaries.

Another pathological state which may possibly occasion primary retinal thrombosis is *detachment of the retina*, especially one form of it, which I may be allowed to point out here for the first time; I mean *detachment caused by contraction of tendinous cords* such as are seen in rare cases of plastic (interstitial) retinitis. Although such cords are not so very uncommon after certain forms of retinitis, especially syphilitic, they very rarely cause detachment. One striking example of it, however, I had the opportunity of examining by the favor of my esteemed colleague, Dr. *C. R. Agnew*. The retina was detached, or, rather, folded up like a frill or ruffle, and through its tissue ran a number of grayish-white cords, lying, in part, beneath the retinal vessels, in part enveloping them. The vessels were evidently compressed by these cords, for they disappeared or became very narrow where they passed through them, but were exquisitely tortuous and engorged in their neighborhood. Some looked so very dark red, that an extreme slackening, or perhaps an arrest of the blood-current within them, might be fairly assumed.

I feel the lack of positiveness in the above statements, but analogy led me to point them out. I do not wish to pass them for more than mere possibilities, apt

perhaps to be the forerunners of some direct and useful observations.

Secondary thrombosis in the retinal veins has not yet been described either. Of this, however, I am able to give an exquisite example occasioned by embolic occlusion of one branch of the retinal artery, and shall describe it hereafter, when I speak of this case in full.

A (*b*). *Primary thrombosis of the choroidal veins is wholly unknown. Secondary thrombosis* may arise either from embolism in the capillaries of the choroid, or be, in a retrograde direction, an extension of thrombosis in the orbital veins. One dreadful case of it I shall never forget. It occurred in the course of erysipelas and phlegmone of the orbit, running under quite the same symptoms as metastatic choroiditis. I enucleated the eyeball on account of extreme painfulness and imminent danger in the other eye. The anatomic examination stated the origin of the suppuration lying in the choroid. The transition of the suppuration from the orbit into the globe can only be accounted for by conveyance of some mischievous material through or along the blood-vessels; and this admitted, the simplest way and that most conforming to analogy is to assume a stagnation and coagulation of the blood in the vasa vorticosa, extending into the globe and causing thrombosis of the choroidal veins and capillaries, and, in consequence thereof, pyæmic abscess. The other eye of the same patient (a healthy man of twenty-four years) became similarly affected; erysipelatous swelling and formation of several abscesses in the orbit, protrusion of the eye, diffuse opacity of the refracting media, but never so intense as to prevent the ophthalmoscopic aspect of the fundus; retinal vessels hyperæmic, diffuse turbidity of the retina, entire loss of sight.

All the symptoms disappeared in the course of four weeks; the fundus only showed tortuous retinal veins; blindness remained. In this eye, too, I think thrombosis was conveyed from the orbit through the vortex veins into the eye, but did not extend in such a fatal degree to the capillaries, and became retrogressive without causing destructive suppuration.

A (*c*). *Primary thrombosis of the orbital veins* may happen as a result of injuries, orbital phlegmone, erysipelas, etc. Great swelling by hyperæmia and œdema, pulsation in the orbit by collateral fluxion, inflammation and formation of abscess, protrusion of the eyeball, pain and fever, will be the principal symptoms. It mostly heals, but in some cases the thrombosis may extend through the cerebral ophthalmic vein into the cavernous and other sinuses, and cause death, which occurrence is confirmed by two good examples of *post-mortem* examination. The inverse, centrifugal progression of the thrombosis from the orbital veins to those of the globe has just been spoken of.

Secondary thrombosis in the ophthalmic veins may proceed either from thrombosis of the cerebral sinuses toward the eye, or from the latter to the orbital veins and the cerebral sinuses, thus accounting for the occurrence of death after severe inflammation of and operation on the eye-ball. Besides the cerebral symptoms, those of the eye are: protrusion of the globe, hyperæmia and œdema of the orbital, conjunctival and palpebral tissue, photophobia, wide, immovable pupil, amblyopia, and paralysis of the ocular muscles.

I must pass rapidly over this highly interesting subject of the connection between ophthalmic and cerebral thrombosis, having already given a full description of it in vol. xiv., pp. 220–236, of the *Archiv für*

Ophthalmologie, in a paper on the Obstruction of the Blood-vessels of the Eye.

B (*a*). *Capillary embolia* (*or perhaps thrombosis*) *in the choroid is observed in nephritic retinitis.* It was first noticed and described by *H. Müller* as sclerosis of the choriocapillaris. I have seen and examined one exquisitely marked specimen of it. Part of the capillaries of the choroid were filled with a uniform, finely-granulated substance, which penetrated to a certain extent into the smaller venous and arterial twigs. As a collecting trunk on both sides of the capillaries was seen choked, the one must have been an artery, the other a vein. This finely-granular mass looked very similar to molecular fat, but did not dissolve in ether, a fact which pleads for its fibrinous character. Neither red nor white blood-corpuscles, nor any other structural elements, were contained in it. The choroid was quite normal, aside from these small islands of plugged capillaries. Here is another promising subject for further studies. I may be allowed to make one suggestion on its symptomatic signification. All physicians know that every variety of diminution of sight is met with in Bright's disease. In the majority of cases this is accounted for by very marked changes in the retina, but in some nothing very conspicuous is found on ophthalmoscopic examination. In such instances, the weakness of sight is ascribed to the presence of a detrimental substance in the blood impairing the functions of the brain, and was called by Professor Frerichs *uræmic amblyopia.* It is possible that a number of such cases fall under the head of capillary embolia of the choroid.

Another group of cases of capillary embolia of the choroid most probably consists of those ocular affec-

tions we see in *cerebro-spinal meningitis* and certain other severe constitutional diseases, typhus, etc. No such eyes have been subjected to a thorough anatomo pathological investigation, but the symptoms during life are so similar to the cases of metastatic choroiditis we witness in puerperal fever, that I have no doubt both processes are of similar origin. The destructive ophthalmia occurring in severe cases of puerperal fever is sufficiently known in its symptoms. I have myself examined three such eyes during life and microscopically after death. There are regular pyæmic abscesses formed in the choroid, which commonly destroy all the structures of the eye, but, in rare cases, are arrested at a certain stage of their course. Then the pus, investing the inner side of the choroid, becomes more or less inspissated; the eye shrinks, but retains its shape, being incurably blind. Under these conditions, I have seen about thirty eyes perish in the epidemics of cerebro-spinal meningitis which reigned, four years ago, in the upper valley of the Rhine, between Basel and Mannheim. The results of my experience and anatomical research of these diseases I have published already in an article on Metastatic Choroiditis (*Archiv f. Ophthalmol.*, xiii., pp. 72–181). Therefore I shall not dwell on the subject any longer.

B (*b*). *Capillary embolism of the retina* has never been observed, although the retina is the most fitted location to study embolism during life with the ophthalmoscope, and after death under the microscope. Since I do not know any reason why it should not occur in the retina as well as in other structures, I think that due attention paid to it will be rewarded.

C (*a*). *Embolism of ciliary arteries* is very little studied as yet. There are only a few cases of it on record,

and they all belong to me. (*Arch. f. Ophthalmol.*, xiv., pp. 237–251.) I have seen more instances of it than I have described, but all did not allow of a definite diagnosis. The novelty and general importance of this disease will justify a brief summary of its symptoms. A patient suffering from cardiac disease, especially when combined with acute articular rheumatism, perceives quite unexpectedly, on awaking in the morning, or in the course of one or several hours during the day, a general haziness before one eye. This haziness increases in one part of the visual field very considerably, even to absolute blindness: scotoma, or defect in the visual field. The defect has in most instances a triangular shape, the apex at or near the centre of the field of vision, the basis at the periphery. Both sides of the triangle are mostly straight lines. The extent of the angle varies from 35° to 90°. In cases where the apex of the triangular scotoma does not reach the point of fixation, the patient enjoys good central vision, being able sometimes to read small print. Besides the general cloudiness and the scotoma or defect in the field, the patient complains of photopsy and chromopsy, but no pain or other symptom of distress is felt. The eye looks healthy in its external appearance, sometimes there is some discoloration of the iris and slight circumcorneal injection. Motion and tension of the globe are normal.

In ophthalmoscopic examination we find, in the first days, the background of the eye lightly veiled. Nevertheless, its details can be recognized. There invariably exists a less or higher degree of retinal hyperæmia: the optic disk appears redder, the veins are dilated and tortuous. Besides that, serous infiltration at the optic disk and in its vicinity is manifested by diffuse

grayish turbidity and swelling of the retina. Both the hyperæmia and œdema of the retina are always more marked and extended further toward the periphery, in that direction which corresponds to the scotoma in the visual field. Sometimes there is an increase of swelling and cloudiness in the retina, and a much greater dilatation and tortuosity of its veins, at one certain circumscribed spot in the affected quadrant of the fundus. This spot then is distinctly raised, and quite resembles retinal detachment. On examining the intensity of the scotoma in the visual field, we find it darkest in a place corresponding to this more infiltrated and hyperæmic portion of the retina.

Beside those symptoms, slight floating opacities in the vitreous body may occur, but I never observed ecchymoses, which is rather surprising. Probably they will be observed when more cases come to our notice. After a few days' aggravation, the mild cases begin to improve, and get well in one or some weeks. Others have a slower course, and are apt to relapse in conjunction with exacerbations of the cardiac disease. I observed one such case for eight months, until all symptoms had completely disappeared. First the general cloudiness in the field of vision fades away, then the scotoma gets gradually less dark, until at last it disappears too. The retinal hyperæmia and infiltration diminish proportionately with the improvement of sight; but, whenever there is a circumscribed raised opacity in the retina observed, it leaves very long a slight grayish-white veil on the fundus of the eye. Till now, I have not seen any marked changes in the choroid after the affection had terminated.

Combined with these symptoms are the general symptoms of the cardiac disease and its consequences.

Nearly always there are cerebral symptoms present leading to the assumption of embolisms in the brain, and once I found the other eye had lost its sight by total embolism of the central retinal artery.

The diagnosis of embolism of a ciliary artery, after such a definite complexity of symptoms during life, has not yet been verified by autopsy. It was derived from the alterations of structure, the functional disturbance of the living eye, and the history of the disease. Cardiac disease, especially endocarditis, sudden appearance of a circumscribed scotoma or triangular defect in the visual field, hyperæmia and œdema of the corresponding part of the background of the eye, which in the retina are due to collateral fluxion, further the simultaneous occurrence of embolic symptoms in the brain, and, in one case, evident embolism of the central retinal artery of the other eye: these symptoms made the diagnosis so probable, that it might almost be called certain. But there are cases in which this series of symptoms is less complete, and the diagnosis must be made probable by exclusion of other possible affections. I think here too is opportunity for further studies, and I would particularly recommend physicians to watch those sudden obscurations in the visual field of patients affected with endocarditis.

C (*b*). *Embolism of the central retinal artery was first described, in* 1858, *by Prof. v. Graefe.* Since that time, about twenty-four cases are on record, five from myself. Of a sixth and most remarkable one I shall speak hereafter. The symptoms briefly enumerated are: sudden, almost instantaneous loss of vision without pain or inflammation. Retinal arteries extremely thin, like slender threads. Veins likewise thin, but somewhat thickening toward the periphery.

By pressure upon the globe, no change in the conformation of the retinal vessels, especially no arterial pulsation, can be produced.

After some days the region of the yellow spot becomes grayish opaque; in rare cases, some ecchymoses between yellow spot and optic disk set in. After a fortnight the retinal vessels begin to be refilled to a certain degree, pressure again produces arterial pulsation, the opacity around the yellow spot disappears. The ultimate result is atrophy of the optic nerve and total blindness. The causes are mostly endocarditis; in one case I found an aneurism of the common carotid artery. In some cases, where a cause has not been detected, atheroma of the arteries may be supposed. In one instance I found this disease occasioned by stabbing with a knife into the orbit.

Three cases are on record, in which sight was not lost; in one, from *Steffan*, it reacquired about one-tenth of the normal, in another, from *Schneller*, one half, and in the third, from myself, it was completely restored. In these cases, at least in the latter, the embolic obstruction was incomplete, and a sufficient arterial supply soon reëstablished, partly by maceration or contraction of the embolic mass, partly by collateral circulation.

Embolism of branches of the retinal artery has only twice been observed, by Prof. *Saemisch* and Dr. *Hirschmann*. There was a defect in the field of vision, extending throughout its lower half. In the first case, the principal upper artery was thin from its exit of the papilla, until at some short distance it was abruptly metamorphosed into a white cord, the result of secondary changes in the walls of the artery, common in obstructed vessels. In the second case, no such change

had taken place, but the artery appeared ever afterward as an extremely thin red thread. To these two cases I can add *a third, perhaps the most interesting of all on record.*

A lady, laboring under an acute exacerbation of endocarditis, felt on a sudden, while reading, a mist spreading over the book. On shutting the eyes alternately she found out at once that the left eye only was obscured. Two weeks afterward she came to consult me, her sight not having improved, nor got any worse either. Nothing abnormal in external appearance; motion and tension of the globe normal too. She read ordinary print with that eye, but finest with the other. The field of vision was deficient: the inner lower quadrant totally failing, the apex not exactly reaching the point of fixation. With the ophthalmoscope, I found the lower half of the optic disk and five-sixths of the background of the eye in a healthy condition, but the most remarkable changes in a triangular space, lying with its acute angle in the optic disk, while one side went horizontally outward, the other one diagonally outward and upward, defining an angle of about 50°. The retinal artery emerged in the centre of the optic disk and divided, as usual, in one upper and one lower principal branch. The latter ran its normal course, while the former was covered midway between the centre and the margin of the optic disk by a gray, slightly-elevated speck.

In the direct prolongation of the vessel, a very slender red line emerged out of the speck and ran upward as far as the length of one diameter of the optic disk. There it swelled abruptly to nearly the calibre of the corresponding lower branch, had a double out line, and continued its course toward the periphery in

a normal way. The beginning of the inner branch was marked by a short, oval, dark-red swelling, out of which three small arteries came forth.

The explanation of these conditions is the following: An embolus, from the endocardium, was carried through the arteria centralis retinæ as far as the first division of the upper of its primary branches. There it plugged one secondary branch (the upper) completely, the other (inner one) incompletely. At some distance from the optic disk, an anastomatic vessel from the choroid communicated with the upper branch, and conveyed a considerable amount of blood into the channel beyond the point of its occlusion, thus establishing a collateral circulation.

But, what was most remarkable, was a *hæmorrhagic infarctus of the retina.* I think this is *the first and only instance that a hæmorrhagic infarctus has been directly seen in the living body.* It was triangular in shape, corresponding exactly to the defect in the visual field. All the veins were engorged, dark red, and tortuous, some not to be traced up toward the papilla. Numerous smaller and larger hæmorrhagic patches encompassed and covered the small venous twigs, while the branches of the artery ran through the ecchymoses without giving rise to any extravasation. In the neighborhood, the twigs of the lower retinal veins and arteries were somewhat more swollen and tortuous than usual. The retinal tissue in the whole triangular region of the infarctus was diffusely gray. These changes extended from the optic papilla until the periphery of the ophthalmoscopic field of vision. The *explanation is as follows:*

An embolus plugged the artery entirely. The blood was driven by the elasticity of the arterial tunics

into the capillaries and veins, where it stagnated, the vis a tergo being absent. The stagnating blood coagulated. Then a small amount of blood flowed into these same capillaries and veins from the neighboring twigs —collateral fluxion—causing a greater filling of the former, since the coagulated blood offered resistance to the current. This stagnating blood, and the want of supply with arterial blood, brought the walls of the veins and capillaries into unhealthy nutritive conditions; relaxation, dilatation, and rupture of the vessels, extravasation of blood, and transudation of serum, were the consequences. All this was directly ascertained with the ophthalmoscope.

The collateral circulation caused gradual absorp tion of the ecchymoses. The retina got more transparent, the veins smaller and straighter, and, after about six weeks, a sufficient regular current was re-established. When I saw the patient last, six months after the attack, no ecchymosis was left, the artery was in the same condition as in the commencement of the affection, the veins still somewhat tortuous, and the retina a little misty. The acuteness of vision in the centre was nearly normal; the defect in the visual field had not changed.

In conclusion, I may add yet, *that this highly interesting subject of embolism in the eye may be studied experimentally.* The late O. Weber, our much regretted professor of surgery in Heidelberg, injected serous pus into the crural vein of a cat. Two days later, death. Among embolisms in different parts of the body, there were found some in the visual tract of both eyes, and the retina of the right eye showed numerous ecchymoses and exquisitely distinct emboli of the smaller arteries, their peripheral ends ischæmic,

the corresponding veins engorged and some of them burst, similar conditions to those observed in the case just above described.

ART. III.—*Fracture of the Fibula, two and a half inches from the Summit of its Malleolus, with Displacement of the Astragalus, Rupture of the Internal Lateral Ligaments, and Outward Dislocation of the Left Foot; Apparatus removed thirty-ninth day, and Cure perfect.* By J. THEUS. TAYLOR, M. D., New York City.

MR. J——, of this city, aged sixty, enjoying good health, and of sound constitution, on the 7th of March last made a false step on a stairway, the foot being abducted; hence this injury. The jar was not very great, and merely occasioned by the last step of a flight happening to be two or three inches higher than the rest.

I saw him two hours afterward, at No. 5 Bond Street, and found the ankle much swollen and discolored, and the foot *everted to an extreme degree*, and *very much drawn* upward and outward by muscular action. There was not, however, any great difficulty in reduction nor was it productive of much pain. The limb was then bandaged *secundum artem*, in order to control muscular irritability, and thereby retain the parts in more perfect accord, and over this a cushion, splint and two rollers (*à la* Dupuytren) were applied, which completed the dressing. The limb was then placed at an easy elevation on its outer aspect, reposing upon pillows, and directed to be moistened from time to time with a lotion of arnica, laudanum, and iced-water, of suitable strength; and such doses of Battley's liq.

opii sed. as might be necessary to allay pain and promote sleep. For three days inflammatory symptoms ran high, and it was necessary to control the febrile reaction by appropriate treatment; and the bandages had to be loosened quite frequently, and the limb douched with cold water; moreover, the heel became so much irritated as to threaten an abscess. This, however, was fortunately prevented by dressing with almond-oil and cotton bats, removing all pressure, and maintaining as *great an elevation* as could be comfortably borne. The lotion was discontinued on the third day, as all febrile symptoms had disappeared, and the engorgement of the parts greatly diminished. On the seventh day the first dressings were cut away, the limb being placed with its outer aspect upon a board covered with a Scultetus bandage and cotton batting, so as to prevent the least disturbance of the bones, and effectually guard against *redislocation* from *muscular spasm*, as *had occurred* during the *first dressing*, owing to the unskilfulness of the assistant. The *many-tailed* bandage was then accurately applied in the usual manner, over the cotton batting from the toes to the knee; a well-fitting pad, and a single splint twenty inches long, two and one-half wide above, and one and three-quarters below, light, thin, springy, hollowed on the side next the pad, and made of well-seasoned Northern pine, were then fitted to the *inside* of the leg, extending from the knee to below the sole of the foot, and properly secured by the *usual turns of roller below the knee*, and a figure of eight around the foot and ankle, so as to *constantly maintain* as *perfect an adduction and inversion of the foot* as possible.

It was, however, soon discovered that the splint had too much mobility, being constantly disposed to slip for-

ward; and to correct this it became necessary to secure it more effectually by two and a half turns of broad adhesive strap passed *around it*, opposite the ankle, from behind forward and within outward, then below and around the foot back to the point of departure; and thence over the splint, and behind and around the leg, and back to the same.

This dressing was continued until the sixteenth day; but great care was taken to unbandage the limb frequently (about every other day), and have it well sponged with cold water, rubbed, and anointed with almond-oil, and sufficient movement *given* to the joint to guard against anchylosis, and keep up the tone of the muscular system. Then, as it had become necessary for the gentleman to leave his room, and go about in the discharge of important duties, the immovable apparatus *now to be* described was resorted to, viz.:

1. The limb was sponged, rubbed, and anointed as above, and enveloped *thickly* in cotton batting to *a nicety;* the elasticity of which would prevent the bandages from exercising such undue pressure as would interfere with its normal circulation and nutrition, or render the patient uncomfortable. 2. This was secured by a bandage of Scultetus, each tail being in turn fastened by a brushing of the prepared chalk and powdered gum arabic and water mixture,[1] carefully made, and triturated smoothly to the consistency of cream. 3. The outside of the bandage was then thoroughly brushed and rubbed with the same, and made to fit perfectly, and without a wrinkle. 4. The sole and sides of the foot and ankle were next well protected from danger of pressure by carefully-ar-

[1] *Vide* Braithwaite's Retrospect, Part li., p. 123.

ranged compresses of cotton batting; over which two *supporting bands* of the usual stout roller material, two and a half inches wide, and reaching from the head of the fibula down the leg, under the foot, and up to the tubercle of the tibia, were *firmly* applied, being first saturated with the mixture. The object of these bands was to maintain the foot in a fixed position of *adduction and inversion* during the treatment, so as to keep the fractured bones in apposition, and *effectually prevent* (as very often occurs) the lower fragment from gliding *within the upper* toward the tibia, whereby the foot would be permanently distorted outward, to the great injury of locomotion, and deformity of the limb. 5. These *supporting straps* were then carefully secured by *two layers* of bandage *rolled in* the mixture, and accurately applied from the toes to the knee, which completed the dressing. Great care was taken to *hold the foot in position* (*as above*) until the apparatus was again thickly coated with the mixture and smoothed with the hand, when the limb was laid on its *outer aspect* upon a pillow covered with lint-cotton. The next morning I found the dressing almost entirely dry, and quite firm when it had been exposed to the atmosphere, but still moist on the side next to the pillow. The position of the limb was therefore reversed to the inner aspect, and by night the whole apparatus had dried perfectly, and assumed an almost stony hardness; and the gentleman could move the stiffened leg at will in any direction, and made no complaint whatever of discomfort from pressure. At my next visit I found him sitting up quite comfortably, after a good night's rest, and most anxious to try a pair of Crandall's admirable crutches. Support was first attempted to be given by a girth passed

under a gaiter slipper, to the sole of which it was secured by a strap, and then slung around the neck. There was, however, some difficulty in locomotion; so the band was changed to below the knee, and thence behind the thigh, and back to the neck; but I soon abandoned this likewise, and left the limb supported alone by its muscular system. In two days the gentleman had acquired good use of the crutch, and after this time went about attending to his business with perfect facility; and, strange to say, *never suffered* from the fixed, rigid confinement of the apparatus during the twenty-three days it was worn. It was then removed by my friend Dr. Meredith Clymer, who kindly attended the case for me during a visit South; and he reports that "*notwithstanding the twenty-three days of immobility*, the limb *exhibited no marbling*, as after ordinary bandaging; and that he found its *nutrition very little if at all below the normal standard, and the cure perfect.*" He ordered douches of cold water, frictions, gentle motion of the joint (*to be gradually increased*), and a roller from the toes to the knee during the day. I returned to the city four days afterward (the forty-third of the injury), and found the gentleman doing excellently well, in high spirits, and making admirable use of a rocking-chair, the movement of which, the foot being on the floor, was precisely what was required to impart tone to the muscles, and sufficient mobility to the joint. I directed the treatment ordered by Dr. Clymer to be continued, merely substituting a gum-elastic stocking for the roller; and this was worn for a month or two during the day; and the patient directed to support the limb on a cushion whenever practicable; so as to relieve the engorgement of the debilitated member after exer-

cise. Both crutches were used for a month more, and then one, assisted by a walking-stick in the other hand, for about the same time; and the gentleman especially cautioned not to throw too much stress on the injured limb, for fear that the callus should give way under the premature pressure. It took some time for the joint to regain its full mobility, and the muscles their lost power; but he now adducts, abducts, flexes, and extends the foot normally, and walks so well that no one could perceive that the limb had been injured except on very close scrutiny. The cure therefore is *absolutely perfect;* and there is no doubt that the natural elasticity of step will be recovered in due time.

Immovablé dressings may be traced back to Hippocrates, who used waxed bandages in the general treatment of fractures;[1] and starch and flour for securing a fractured nose.[2] The Greeks have long used a mixture of prepared chalk and white of eggs, rubbed up with oil and butter.[3] Ambroise Paré, following Hippocrates, recommends the wax dressings;[4] he gives another

℞. "Thuris, Mastichis, Roti Armeniceuve,
Sanguinis Draconis . . āā ʒ ss.
Aluminis Rochoe, Resinæ Pini Sicci āā ʒ ij.
Pulverisentur Subtilissimé
Item Farinœ volatilis . . . ʒ ss.
Albuminorum ovorum . . quantum sufficit.
Incorporentur omnia simul, et fiat medicamentum."

and likewise a third preparation,[5] with which he was dressed for compound fracture of both bones of the leg: "On me pensa avec un medicament tel que nous peusmes pratiquer an dit lieu; le quel nous composames de blanc d'œuf, de farine de froment, de suye de

[1] Hippocrates, chap. viii., De Fracturis.
[2] Ib., Sent. 46, section 2d, De Articulis, 467.
[3] Velpeau, Clin. Chirurg., p. 62.
[4] A. Paré, Œuvres, t. v., p. 303. [5] Ib., p. 306.

four, avec du beurre frais fondu."[6] Guy de Chauliac mentions a preparation of wax, or white of egg beaten up.[7] Belloste, a French surgeon, in 1696, eggs beaten up with rose-oil and vinegar.[8] Moscate, in 1739, white of eggs alone.[9] Cheselden, of London, in 1750, eggs and flour.[10] Baron Larrey, in his Moscow campaign, white of eggs, subacetate of lead, and camphorated alcohol; supporting the bandages with compresses, straw splints, etc.; and in 1830 revived and brought into general notice this almost-forgotten practice, by the publication of his observations.[11] Velpeau, as early as 1823, used vinegar and flour; in 1830, white of eggs, Goulard's lotion, and camphorated brandy, but subsequently introduced dextrine, which he preferred to all others. His formula is ℞ dextrine, 100; camphorated brandy, 60, and hot-water, parts 50.[12] Seutin, of Belgium, introduced the starch-bandage in 1834, supported by pasteboards.[13] Dieffenbach, the plaster of Paris.[14] Lafargue, the plaster of Paris and starch combined.[15] Sir Charles Bell recommended, in 1827, white of eggs and flour, or resin sprinkled on the bandage and dipped in alcohol or glue.[16] Mr. Alfred Smee, in 1840, gum-arabic and prepared chalk;[17] gum-arabic alone is used in four English hospitals at this time, and glue in two.[18] All these dressings require the

[6] A. Paré, Œuvres, t. v., p. 303.
[7] Guy de Chauliac, Chirurgerie, p. 356.
[8] Le Chirurgien de l'Hôpital, 1696.
[9] Mém. de l'Acad. de Chirurgerie, t. iv.
[10] Cheselden, Anatomy, p. 452.
[11] Larrey's Clinique des Hôpitaux Militaires.
[12] Velpeau, op. cit., p. 469, *et seq.*
[13] Seutin, Œuvres. [14] Dieffenbach. [15] Lafargue.
[16] Bell's Surgery, vol. ii., p. 196.
London Lancet, January, 1840.
[18] Dublin Quarterly, February, 1865, p. 149.

support of splints until they harden, except the plaster of Paris, which sets so rapidly, that the limb, when it is applied, may be sustained (after the method of Malgaigne) by the fingers of two assistants."[19]

The important principle of *deambulation* was first developed by Mr. Amesbury, of London, in his work on fractures (1827); but he had previously practised it for fifteen years. He says: "With my apparatus the patient can move the limb, get out of bed, and walk with crutches," etc.;[20] and M. Théodore Léger, about the same time, made a machine which enabled the patient to walk even without the support of crutches.[21] The above is in part taken from Velpeau's admirable and candid work, who thus concludes his notice of these very interesting points: "General deductions—1. The idea of immovable dressings for fractures has existed since antiquity. 2. It was first methodized and systematized by Larrey. 3. Seutin yet more fully generalized and varied its applications. 4. Seutin substituted starch for the egg mixtures; and the roller and pasteboards for tow and wooden splints. 5. To me (Velpeau) belongs the last simplification of this dressing, the *generalization of the roller and compression.* 6. Deambulation had been vaguely indicated before Bérard; but he was the first to reason it out explicitly; and to Seutin belongs the honor of having rendered its generalization possible and easy."[22]

Principles.—"*The indications* to be fulfilled in the treatment of all fractures and luxations," says Paré, "are three: 1. To replace the parts in position. 2. To maintain them there; and 3. To guard against the su-

[19] Malgaigne, Traité des Fractures, vol. i.
[20] Amesbury's work on Fractures, p. 21.
[21] Velpeau, Clinique Chirurgicale, t. ii. [22] Same.

pervention of any bad accidents."[23] And he shows a perfect knowledge of the fracture in question at p. 328, although at p. 297 he says that "one may walk upon the foot when the fibula is broken, because it only serves to support the muscles, and this *is* the case when the fracture is four or more inches from the malleolus. Percival Pott was the first surgeon, either of ancient or modern times, who philosophically explained the mechanism of fracture of the fibula, with outward dislocation of the foot, so accurately portrayed in his plates.[24] Hence we of England and America have long called it, after him, Pott's fracture. He, like Ambroise Paré and Sir A. Cooper, had his leg broken; therefore it was, perhaps, that he studied these injuries so carefully. Boyer divides the honors between Pott and Fabre, but lays no claim to the discovery himself.[25] Desault is by no means so elaborate in his description of it as Pott or Boyer.[26] Dupuytren alludes to Pott on the same footing with David, Fabre, Broomfield, Pouteau, Boyer, and Charles Bell, but gives priority to Duverney and J. L. Petit.[27] Malgaigne does full justice to all parties, and blames Dupuytren for so coldly alluding to Boyer, from whom he extensively drew the ideas which were elaborated into his method.[28] M. Nélaton mentions Bloomfield, Pouteau, Boyer, and Desault, but entirely ignores the claims of the celebrated Englishman.[29]

Methods.—Hippocrates used the waxed dressings

[23] A. Paré, op. cit., t. ii.

[24] Pott's Works, vol. i., pp. 433–438, and Commentaries of Galen and Van Swieten.

[25] Boyer, Surgery (Stephens), vol. ii., p. 123.

[26] Desault (Caldwell), p. 382, *et seq.*

[27] Dupuytren, Clinique Chirurgicale, t. i., p. 194, *et seq.*

[28] Malgaigne, op. cit.

[29] M. Nélaton, Pathologie Chirurgicale, t. i., p. 811, *et seq.*

immediately after reduction, and over these suitable compresses were attached to the limb, which was then reposed on cushions, supported by a board, at an easy elevation, and in the straight position. Before applying the bandages, he saturated them with the cerate, which was likewise freely rubbed upon the limb.[30] This dressing was removed every other day (if required), and the parts carefully bathed, until the seventh or eleventh day: when the patient could bear more permanent dressing, as follows: 1. The skin was anointed as before. 2. Three bandages were applied, secured at each turn with the cerate, and well rubbed with it. 3. Suitable compresses were fitted to the limb; and over these. 4. Splints of requisite length (see next page), which were secured by some turns of bandage. Celsus followed this method, somewhat modified, using six instead of three bandages, large instead of small compresses; and wine and oil in lieu of cerate.[31] There is no doubt that the many-tailed bandage, so long credited to Scultetus, was habitually used by Hippocrates.[32] Time has set his thousand years seal upon the excellence of the above principles, which we find successfully advocated by so many of the *illustrati* of our profession, extending through age after age, down to our own day. Ambroise Paré was of this number, and devoted his life to the study of the great master whom he so constantly followed, and often even surpassed. Thus, he anointed the member after reduction, impregnated the bandages, and applied suitable compresses to maintain the parts in position; he, moreover, directs them to be

[30] Hippocrates, De Fracturis.

[31] Paulus Ægineta (Sydenham), vol. ii., pp. 459, 507.

[32] Commentator, do.

wet with oxycrate in simple fractures, and strong wine in the compound variety;[33] and used three rollers, one extending from the fracture upward to the knee, another from the same point to the toes, and the third from the toes to the knee, applied contrariwise to the first.[34] In his own case of compound fracture of both bones, he used the eggs, flour, etc., as before mentioned, which he calls, in the edition of 1564, "an easy remedy for a recent fracture for want of another;" and in that of 1575, "a good village medicament promptly applied."[35] He thus describes the completion of *his dressing:* "And when the bandages were completed, splints were applied; some three fingers broad, others two; and all half a foot long, hollowed to fit the limb, and tapering toward the ends. These were placed at one finger's interval from each other all around the limb, and then tied with small ribbons, such as ladies use for tying their hair, and were made to fit more tightly at the seat of fracture than elsewhere. The limb was then suitably supported by compresses of tow enveloped in linen, and the dressing was finished by two long straw splints, strengthened each by a little stick running along its centre, and encased in a half sheet. These splints extended from the heel nearly to the groin, and were secured at four points; and thus the leg could not be perverted, or turned to one side or the other. It was then placed in a straight position, and not bent (which shows that he often used, as at p. 288, bent splints, and practised the semiflexed position), and elevated at a moderate height, gently and uniformly, in order to avoid pain, congestion, inflammation, and other accidents."[36] Paré used splints and gutters of

[33] Ambroise Paré, op. cit., p. 303.
[34] Ib., op. cit., p. 331.
[35] Ib., note to p. 330.
[36] Ib., pp., 288, 330, 331.

wood, lead, zinc, leather, pasteboards, and bark of trees; and modified his apparatus according to the injury and its seat.[37] Guy de Chauliac says: "We must replace the bones in their *niveau*, and take care that there be no inequality, and that the muscles be not constrained, for bones have their particular reductions. They being reduced, before the application of the proper bandage, an embrocation is made with oil-of-roses; and we make use of the cerate, or the white of eggs beaten up. We moisten the compresses and bands with oxycrate or strong wine, and must not use splints at the first dressing, especially if there has been a great contusion; but when the accidents are appeased, to maintain the reduced bones in good position. The fracture being bandaged and accommodated, it only remains for us to place the limb in an easy posture. If the fracture be complicated, we should place the bones in position, and use the bandage of eighteen tails; in order to examine the parts more frequently without disturbing the reduced bones. And if the pain should be great, we must at once undo the bandage for fear of gangrene.[38] The difference that there is between the dressing for luxation and fracture is, that the luxation should be reduced before the application of the dressing; but, on the contrary, the dressing should be made before the reduction of the fracture."[39] Pott used the eighteen-tailed bandage, compresses of suitable size and shape, and two lateral bent splints, extending from above the knee to below the ankle, and maintained the limb semiflexed, and reposing on its outer aspect upon pillows. He practised the "semi flexed position in all fractures, except those of the

[37] Ambroise Paré, note to p. 330.

[38] Guy de Chauliac, Chirurgerie, p. 356. [39] Ib., ib., p. 360.

olecranon process, and patella."[40] Boyer used two lateral splints; the outer extending a little beyond the sole of the foot, and the inner reaching no farther than the malleolus. He likewise employed the Scultetus bandage.[41] Desault used two lateral compresses, sustained by two strong splints, and one compress in front of the tibia, and another behind the os calcis, secured by bandages.[42] Dupuytren a wedge-shaped cushion, one splint and two bandages to secure them, as previously described. He was the first to *carry out* the right principle, and maintain an adducted and inverted position of the foot *by this apparatus.* He thus treated 207 cases, and cured perfectly 202; three died of the injury itself or its accidents, and two of complications independent of it. All these cases preserved the normal shape except two, who had the heel elongated and thrown backward, and the inferior extremity of the tibia a little salient anteriorly. There was always a certain immobility of the joint, combined with obliquity of the foot, on the removal of the apparatus; but they disappeared in a few days with frictions, exercise, etc. Sometimes, however, the dressing was changed to the outer aspect, when the limb would be restored to its proper shape in a few hours.[43] Sir Charles Bell applied on the *inside* of the leg and foot a single splint, fenestrated to receive the inner malleolus, with suitable compresses; and reposed the member, semiflexed, on its outer aspect.[44] Sir A. Cooper employed the bandage of Scultetus and angular splints, one on either side of the limb, etc., and

[40] Pott's Works, vol. i., pp. 417–440.
[41] Boyer, Surgery (Stephens), vol. ii., p. 123.
[42] Desault (Caldwell), p. 382, *et seq.*
[43] Dupuytren, Clinique Chirurgicale, t. i., p. 194, *et seq.*
[44] Sir Charles Bell, Surgery, vol. ii., p. 184.

directed the great toe to be kept in line with the patella;[45] and thus cured himself. Malgaigne used two pillows supported by lateral splints, until the inflammatory period had passed, and then applied an immovable dressing of starch or dextrine, but in certain cases preferred plaster of Paris. He always supported the dextrine and starch dressings with splints until desiccation.[46] M. Maisonneuve does so likewise, and practises the primary dressing after the method of Velpeau.[47] M. Nélaton sustains them with his high authority, and, though he treats the subject briefly, yet throws a flood of light upon it. He says that one of the great objects accomplished by Dupuytren's philosophical method was to give such flaccidity to the tissues on the inner side of the foot and ankle as to bring the *ruptured lateral ligaments into juxtaposition*; and he maintains that *in all cases* of outward dislocation, with fracture of fibula, these ligaments *must of necessity be ruptured.*[48] Sir William Fergusson prefers McIntyre's apparatus, as modified by Mr. Amesbury, and above all Mr. Liston's splint, for *all cases* of fracture of tibia or fibula, with or without dislocation; and advocates the straight position of the limb in accordance with the Hippocratic teachings. He approves of starch bandages after inflammation has ceased and union of the bones commenced.[49] Mr. Syme,[50] Mr. Erichsen,[51] and Liston,[52] all indorse Dupuytren's method; and Chelius concurs with them,

[45] Sir A. Cooper, Dislocations and Fractures, p. 278.
[46] Malgaigne, op. cit. [47] M. Maisonneuve.
M. Nélaton, op. cit. t. i.
[49] Fergusson, Practical Surgery, p. 309.
[50] Syme, Surgery, p. 252.
[51] Erichsen, Science and Art of Surgery, pp. 233, 234.
Liston, Practical Surgery, p. 69.

but sometimes uses two lateral splints, *à la* Boyer, the inner reaching to the malleolus, and the outer beyond the sole of the foot.[53] Our American surgeons mostly concur with these high authorities.— *Vide* Profs. Gibson,[54] Gross [55] and Hamilton,[56] *et al.* Drs. Harris and Randolph and Dr. Geo. Norris introduced the method of Seutin into the old Pennsylvania. Hospital more than thirty years ago.[57] I make this reference, as I have not the works of Seutin, and they cannot be found at the libraries. Velpeau advocated the early application of the dextrine dressing, and applied the bandages *so forcibly* as to exercise considerable *compression* upon the limb. He says: "It must not, however, be believed that this is beyond reach of accidents; like all others, it has its reverses; but they should not in any manner be imputed to it." [58] His method is thus described: "The foot being strongly supported and carried inward by the assistant who is making extension, the surgeon, after enveloping the limb in fine dry linen, applies over it upon the anterior interosseal fossa, on both sides of the tendo achillis, and behind the malleolus, graduated compresses, which he secures by two or three layers of dextrined bandages extending from the root of the toes to the knee; and, to maintain the foot forward and inward, a wooden splint may be applied until the complete drying of the bandages." [59] The late distinguished Dr. Valentine Mott directs that "pieces of pasteboard be added to the above dressing, that we should readjust the bandages from time to

[53] Chelius, System of Surgery, vol. i. pp., 581, 582.
[54] Prof. Gibson, Surgery.
[55] Gross, System of Surgery, vol. ii., p. 202.
[56] Hamilton, Fractures and Dislocations, p. 459.
[57] Vol. i., Medical Examiner.
[58] Velpeau, op. cit. [59] Ibid.

time, and maintain the foot inward and forward until complete desiccation. We thus procure, without any effort or fatigue to the patient, all the advantages of Dupuytren's dressing united to those of the bandage of Scultetus, and the ordinary splint and compressing bandages.[60]

It were easy to increase this long list of authorities; but already, perhaps, too many have been given for the patience of the reader on *all* the points. I will therefore hurry to the conclusion.

CONCLUSION.

These various apparatus are, as we have seen, often applied at the first dressing, immediately after the reception of the injury; and this method is recommended by very high authorities; yet I think that in cases at all grave, and surely in such as above described, it would be, to say the least of it, very bad surgery; not only endangering the life of the patient, but inevitably productive of great suffering from the pressure of a *prematurely* applied, fixed, and unyielding apparatus to an engorged and *yet swelling* limb. If, however, we defer such procedure for a week or eleven days (after Hippocrates), or much better, I think, as in this instance, to the sixteenth day, the results, things being equal, *must* be more favorable. It is a great satisfaction to the patient to be assured that the period of inflammation has passed by without material injury to so important a joint; and of this no surgeon can be certain unless the parts be frequently inspected, in the *nude state*, throughout the earlier days of treatment. In this consisted the peculiar excellence of the method of Dupuytren, which admitted the application of such

[60] Mott and Velpeau, vol. i., p. 188.

lotions or cataplasms as might be required without disturbing his apparatus; and Malgaigne, by securing the limb in easy position between pillows until the fixed dressings could be safely applied, has beyond doubt improved upon his illustrious predecessor. For when *we know* that no danger can result from their application, that the joint is safe, the bones in apposition, all engorgement and inflammation subsided, and that every thing that remains to done is to secure the limb in such a manner that the patient may take exercise, breathe fresh air, and nourish (so as to assist the all-important process of bone-formation); *then only, and not till then, has the time arrived.*

I purposely delayed doing so, as narrated, for the above reasons; and would not even then perhaps have been willing to dispense with Dupuytren's apparatus (assisted by the Scultetus bandage to control muscular spasm, and give more absolute repose to the limb; and likewise by the two and a half turns of adhesive strap, which fulfilled the indications much better than M. Nélaton's dextrined bandage could have done *in securing the splint*, as the dressings *had to be removed* so frequently), but that I felt sure of being able to maintain the requisite position of *adduction and inversion of the foot by the two supporting bands, which are my own;* and constitute, I think, the essence of the treatment, and the apparatus. I was thus enabled to sustain the parts in position without being *constrained to use the splint*, as is the case with *all the movable dressings of our day*, except the plaster of Paris.

I therefore claim to be the first who has practised a method by which two simple straps, so applied as to carry out the right principle, enable us to dispense

with the splint after the fixed dressings; and I moreover maintain that *splints will never again* be required *at all* in the treatment of these cases. For as these two supporting bits of bandage, impregnated with the gum-arabic mixture, answered so admirably in this instance, on the sixteenth day (assisted only by the outer parts of the dressing), so I insist that the *same supporting power* would have been equally applicable at the first dressing, immediately after the reception of the injury, if rightly applied. Thus for instance: we first reduce the dislocation and fracture by extending the foot obliquely inward, the leg being bent upon the thigh to quiet muscular contractions. 2. The foot is held by an assistant, in such a position of adduction and inversion combined, as to move the upper end of the inferior fragment of the fibula, from its inclination toward the tibia, and bring it in apposition with the superior fragment. 3. Compresses of cotton batting must then be placed upon the external malleolus, sole of the foot, and malleolus internus; and over these two or more strips of adhesive plaster, two and a half to three inches wide, *firmly applied from without to within*, beginning at the head of the fibula, extending down the outer aspect of the limb, beneath and across the sole of the foot, and thence obliquely upward to the tubercle of the tibia. Last, the leg should be enveloped in cotton batting, and the eighteen-tailed bandage carefully adjusted, so as to give support to the limb without exercising undue pressure: which may then be reposed semiflexed upon pillows, lying on either aspect, until the deambulating period have arrived, when the immovable apparatus will be resorted to as before described.

ART. IV.—*Some Unusual Phenomena attending Anæsthesia.* By FREDERIC D. LENTE, M. D., of Cold Spring, New York.

IN a recent number of the *Richmond and Louisville Medical Journal*, and copied into the *Boston Medical and Surgical Journal*, is an article by Dr. W. H. Shepherd, entitled "Apparent Exercise of Volition during Anæsthesia complete in all other Respects." The writer thinks it very extraordinary that, although the anæsthesia was apparently perfect, the patient's jaws were firmly closed. "The resistance," he says, "was not such as we find in tonic spasm, but appeared to be the result of voluntary effort, and never yielded, although the anæsthetic (chloroform) was used until the condition of the pupils forbade its further employment." There was, he adds, the usual relaxation of the other muscles of the body.

A rigid condition of certain muscles, under perfect anæsthesia, is not, I think, so very uncommon, and even the perfect exercise of volition is also quite possible, as I have seen exemplified in two cases so remarkable that I am tempted, by reading the report of Dr. Shepherd's case, to give them publicity, although the long period which has elapsed since their occurrence, and the possession of no notes, must render the report very meagre.

During the first year of the use of anæsthesia in the New York Hospital, to which I was, at the time, temporarily attached, as surgical assistant, a case of perineal section occurred in the practice, I think, of Dr. Gurdon Buck. The operation was without a guide through the stricture, and the most tedious and difficult of the kind that I have ever witnessed. The pa-

tient was on the table over an hour, and the exemption from pain was, all the while, complete; and yet he was laughing and talking, and making droll remarks, in conversation with the bystanders, most of the time. I remember one of the house-staff making notes of some of his queer sayings. The other case was a tedious operation for necrosis of the tibia. The subject was an unusually stupid boy, some fifteen years of age; and yet, under perfect exemption from pain (etherization), he sang numerous comic songs, and made rather witty remarks on the peculiarities of the surgeons around him. The pain, on examining the disease with the probe, previous to the operation, was unusually severe, judging from the outcries of the patient. I cannot remember how often a reapplication of the ether was required, but the sponge was away from the face a good part of the time consumed in the operation. These cases were not deemed extraordinary at the time, and no note was taken of them, as it was then the infancy of anæsthesia, and it was considered quite likely that these events would become not unusual occurrences. But I have never since, to my recollection, met with a record of any thing precisely similar, although it is sufficiently common for patients to recover their mental faculties to such a degree as to enable them to answer questions intelligently, and to coöperate, to some extent, in any necessary movement of the body after the operation has been completed some minutes, and yet feel no pain during the handling and dressing of the wound.

Two cases of troublesome rigidity of muscles, which I call to mind as having happened in my private practice, are briefly as follows: Mrs. N., a rather nervous married lady, about thirty years of age, had visited a

neighboring village for the purpose of having a number of teeth extracted; and, according to her statement, had exhausted a pint of ether in the vain attempt of a physician to anæsthetize her. She was assured that such a thing was impossible in her case. However, I undertook the job, and with three ounces of ether, and within the space of four or five minutes (ether enough and time enough to etherize any patient) I had her breathing stertorously, and thoroughly relaxed except the muscles which it was most important to have in that condition, namely, the masseters. It required a strong leverage with a pair of stout forceps to force the jaws open, so that Mr. Davis, the dentist in attendance, could extract the teeth. On recovery, she exhibited no unusual phenomena, and insisted, for some time, that it was her sister, standing by, and not herself, who had undergone the operation.

A young man, and healthy, who had suffered amputation of the leg below the knee some months previously, wished a peg-leg, but the knee-joint was anchylosed in the straight position, and required to be flexed for this purpose. I accordingly administered ether to relax the muscles, as well as to annul pain. The anæsthesia was quite complete, the respiration stertorous, and yet the muscles of the thigh were as rigid as iron. The etherization was pushed to the verge of danger, in the hope of producing relaxation, and thus maintained for several minutes, but unsuccessfully. The patient was therefore allowed to recover somewhat from its effects; and before the return of consciousness or sensibility, the muscles then becoming slightly relaxed, the stump was suddenly flexed by a powerful effort on my part, and the adhesions thus ruptured. In this case, I cannot call to mind whether the

other muscles of the body were likewise rigid. In fact, the untoward and perplexing effect of the anæsthetic was so annoying that I paid no particular attention to the state of the general muscular system. I cannot pretend to say whether voluntary effort had any influence in determining the muscular contraction in these two cases. A *moderately* firm contraction of the maxillary muscles is not an uncommon occurrence in dental operations under an anæsthetic.

Clinical Records from Private Practice.

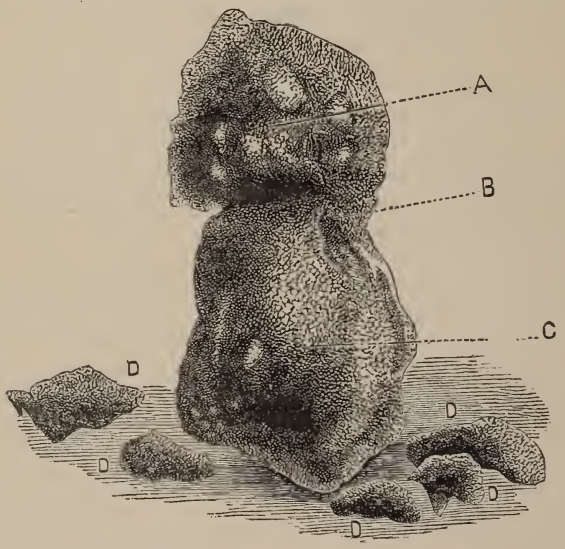
I.—*Partially-encysted Stone in the Bladder, complicated with Perineal and Femoral Fistula.* By Frank A. Stanford, M. D., Columbus, Ga.

Joseph Aguera, aged seventeen years, was the subject of this very unique and interesting case. He had had symptoms of stone since his earliest recollection, and at the time first seen by me was in a most miserable condition. Besides the painful symptoms which usually accompany this difficulty, I found him with two fistulous openings, through both of which there was a constant flow of urine: one of these was situated in the perinæum, while one occupied the upper and inner aspect of the thigh. Through both a constant escape of urine was taking place, and the parts adjacent were kept excoriated in consequence.

July 17, 1866.—Two days after his first sounding, when I detected the stone, placing him under the influence of chloroform, and with the assistance of Drs. Bozeman, Foard, Moses, and others, I made a bilateral section of the perinæum into the bladder. The stone was readily come in contact with, when, after several ineffectual efforts at its extraction, it was ascertained to be partially encysted. It was occupying the infundibuliform portion of the bladder, the lower and anterior portion being free, while the upper part was firmly embraced in a cyst. Passing a bistoury guided by the finger down upon this,

I made a free division of its surface down upon the stone proper, when it was easily removed from its bed, and extracted through the perineal opening.

The patient made a regular and good recovery. The fistulæ ceased to discharge urine, and after a few days the cyst, very much like those we often find containing sebaceous matter, was passed with the discharges through the opening. The fistulæ were clearly caused by the fixed position of the stone near the neck of the bladder, held so by the cyst.



The accompanying plate gives the exact size and view of the stone, after its removal, and as now seen in my collection: A. Upper portion, round, covered by the cyst; B. The line of extent of cyst, constricting the stone in the shape of a neck; C. The lower portion of stone free and exposed in the bladder; D. Débris, broken in my attempts at removal. The weight of the stone and fragments was ten drachms.

July 13, 1868.—Joseph is now in full health, and has been attending school since shortly after the operation.

II.—*Operation of Artificial Joint for Fracture of Tibia and Fibula.* By A. D. Hull, M. D., Lansingburg, N. Y.

Noticing Dr. Sayre's article upon artificial hip-joints, I

thought the following case of artificial joint might not be uninteresting:

I was called late one cold evening in December, 1860, to see, within one block of my office, a Mrs. Mary C——, a servant, aged thirty-five years, weight one hundred and sixty pounds. She was sitting upon a chair, in a house next where she lived, and was considerably intoxicated. She was found in the alley in rear of the house where she lived, falling down and getting up and trying to walk, and was brought where I met her. On examination, there was a compound fracture of the fibula, just above the external malleolus, and dislocation of the tibia inward, at the ankle-joint; the foot was turned outward, the bones protruded through the flesh, and she had been walking on the ends of the bones. The malleolus remained *in situ*. She was removed to where she lived, laid upon a couch, and I removed one-half inch of the articulated portion of the tibia and fractured end of the fibula, replaced the bones in their proper places in connection with the joint, and placed the whole in a felt splint adapted to the foot and leg.

The second day after the operation, she was taken to the Marshall Infirmary, and I saw no more of her until, about four months after, I met her one day walking in the street with a cane. She could then bear her weight on this foot, had free motion, and handled it about as well as the other. She soon laid her cane aside, and at this time, at an ordinary gait, no one would think she had ever been lame.

Let me remark, lest some of the profession should doubt the necessity or utility of this operation, why I resolved to do it. Some three months previous, I had been called to a case, a man sixty years of age, who had been thrown from a wagon, producing precisely the same injury, viz., fracture of the fibula close to the malleolus, and dislocation of the tibia inward at the ankle-joint through the flesh. This happened three months previous to my seeing it, and had been attended, during this time, by a physician who died about the time the patient came into my hands. The limb had been dressed with a splint and bandage, and an attempt made to keep the foot in line with the leg, but the swelling about the joint was so great and

the contraction of the outer muscles so marked, that the pain was unendurable with the limb so confined, and all dressing had to be removed. With the limb thus unconfined, the foot immediately turned out and the bones protruded. Upon consultation with Dr. Bontecue, of Troy, we decided to cut off a portion of the tibia, and we accordingly did so, with the happiest results so far as the success of the operation was concerned; the parts were adapted and placed in a felt splint. Previous to the operation the discharge of pus had been very great, but it now soon ceased, and the wound rapidly healed, so that at the end of two weeks the joint seemed quite firm. Unfortunately, at this time the patient was attacked with acute pleuropneumonia, and died in five days. He had been a free liver.

This joint would undoubtedly have been anchylosed, as the cartilaginous surfaces had been destroyed by suppuration.

III.—*Double Floating Kidney.* By Henry Shiff, M. D., New York.

Miss J. C——, aged twenty-six, of a rather weak constitution, has never had any serious illness. She suffered slightly from chlorosis at fifteen, before the establishment of menstruation, but has always been regular since that time.

At the beginning of last December, she had an attack of pleurisy, for which she applied to me, after having been fifteen days without medical advice, and of which she is now well.

Six years ago, she felt pain in the region of the right kidney, which she describes as having been pulling and dragging. It caused nausea, but she never vomited. The pain was very slight when at rest, but became distressing with any exertion, and increased mostly when she raised her right arm. She was for several days unable, on account of the pain, to comb her hair with her right hand.

A few weeks after the appearance of the pain, she noticed a tumor on the right side of her abdomen. This tumor, very small at first, has gradually been growing larger.

She has all along been able, by lying on her back and making pressure upon the tumor, to cause it to disappear entirely.

Two years afterward, she felt the same kind of pain in the region of the left kidney, but it did not give her the feeling of nausea, as the pain on the right side had done. Shortly after the appearance of this pain, she noticed a tumor on the left side of her abdomen. This tumor, very small at first, has also increased in size, but is much smaller than that on the right side. It has always been movable, and she has at all times been able by pressure upon the tumor to cause it to disappear. This she can do standing as well as lying on her back.

Having become alarmed by the presence of these tumors, she asked my opinion about them; and a recent examination revealed the following state:

When she stands, there is to be felt just below the last rib on the left side, and touching it, a well-marked tumor, the outlines of which are, however, somewhat indistinct. It is about two inches in both diameters, and dull on percussion. By pressing upon the tumor, it disappears entirely; and the place formerly occupied by it is sonorous on percussion.

On the right side, there is an oblong tumor, about three inches by two, the longest diameter being parallel with the longitudinal axis of the body and touching the umbilicus. The outlines of this tumor are very distinct, and it has the shape of the kidney. An arterial pulsation can be felt at its side. There is dulness by percussion over it. When pressure, even moderate, is applied upon it, the patient lying on her back, it disappears entirely; the place formerly occupied by it becomes sonorous when percussed. The arterial pulsation can be felt still, where the tumor was, and it can be followed up by the finger some two or three inches in a slanting direction toward the right ribs. These pulsations are due very probably to the renal artery.

There is pain in the region of the stomach when the tumor of the left side is pushed backward. There is no pain when the tumor on the right side is pushed back; but the patient complains of a constant uneasiness on the right side. She describes it as a dragging from the back, with a desire to lean forward. But any exertion, such as lifting a weight, or leaning forward for some time, as when washing linen, causes very distressing pain; at the same time a cold sweat covers her

body, and she feels as if she were going to faint if she continued the exertion.

Floating kidney is by no means rare, though it seldom comes under the notice of the physician, because, in the great majority of the cases where it exists, it gives rise to no symptoms whatever. The researches of Dr. Walther, of Dresden, show that floating kidney is to be found in a great many persons who are not even aware of any thing abnormal. Out of thirty-five observations collected by Dr. Fritz, of Paris, thirty were in females. It is also much more common on the right side. Cruveilhier seems to think that the use of the corset in the female and wearing too tight clothes in the male are the main causes of the displacement; and explains the greater frequency on the right side, by the fact that the spleen and stomach on the left hypochondrium offer more resistance to pressure than the organs on the right side.

Trousseau relates the case of a man, who had been several times treated for peritonitis, where the tumor became occasionally painful after exertion; and that of a woman, who had been told the tumor was of malignant character. He advises, when there is any pain, to apply poultices, with rest in bed; and, after the pain has been subdued by these means, to cause the patient to wear a belt with pads placed so as to give support to the prolapsed kidney. Such a belt I prescribed to my patient with apparently some benefit. I should have stated the urine was normal in every respect.

IV.—*Abscess in the Abdominal Walls communicating with the Intestinal Canal, originating from Inflammation and Ulceration of the Vermiform Appendix; Pyæmia: Death.* By E. C. Hun, M. D., Albany, N. Y.

A. B. S——, aged sixty-two years, gas-fitter. Has for years been subject to occasional attacks of dyspepsia. September 28, 1868, complained of pain and tenderness in the right iliac fossa. There seemed to be some inflammatory action in the neighborhood of the caput coli. No general peritonitis. This inflammation gradually subsided under the influence of opium and rest, with the external application of hop poultices, leaving a hard cake or induration in its seat. October 19th. He sat

up and moved about the room, and seemed to be doing well. October 25th. After several movements of the bowels, he again complained of pain in the right iliac fossa, and had a severe chill, which greatly exhausted him. October 27th. Had several chills, followed by extreme weakness and depression. November 3d. Has had more chills, complains of no pain, but is rapidly losing strength. Muscular tremor, and picking at the bedclothes. Is perfectly rational. November 7th. Frequent chills, failing in strength. Bed-sores. Had a movement of the bowels, which seemed to contain purulent matter. November 9th. Had a natural movement of the bowels, followed by a proper discharge of fluid blood. November 11th. One chill during the night. November 12th. No chill, feels brighter, and thinks he is going to get well. November 13th. Began to sink early this morning and died at 9.30 P. M. Was perfectly conscious to the last.

Autopsy eighteen hours after death.

Body emaciated, skin slightly jaundiced.

Head not examined.

Thorax.—Lungs normal, except some adhesions about the apices. Cardiac valves were normal, but the muscular structure was friable.

Abdomen.—Injection of the peritonæum with adhesion of the viscera and parietes in the right iliac fossa. The appendix vermiformis was adherent to the abdominal walls, and communicated by a perforation with an abscess in the muscular tissue of the abdominal wall. A portion of the ileum was adherent at the same point, and communicated by an ulceration with the cavity of the abscess, which was large enough to hold a horse-chestnut, and contained feculent and purulent matter. When the intestine was straightened out by breaking up the adhesions and dividing the mesentery, the point of ulceration in the ileum was found to be about eighteen inches distant from the ileo-cæcal valve, and between these two points the ileum descended into the cavity of the pelvis and was firmly adherent to the rectum, where it formed an acute angle in the shape of the letter V. At this point it was full of dark, clotted blood. The portal veins in the liver were filled with pus, but no proper metastatic abscesses were found in that organ.

The spleen was enlarged and infiltrated; there was a sanguino-purulent fluid. A large abscess had formed under the peritonæum in the left hypochondriac region, and was bounded by the adjacent spleen and convex surface of the kidney. Its contents had fused down along the sub-peritoneal connective tissue to the point where the psoas-magnus passes under Poupart's ligament.

V.—*Coxo-femoral Dislocation at Four Years of Age.* By Stephen Rogers, M. D., New York City.

On account of the acknowledged rarity of the accident, I am induced to send you the following history of a case of dislocation of the hip-joint in a child of four years of age:

A well-grown, healthy girl of four years, while playing, slipped upon the carpet, and fell, her lower extremities being, as is supposed, extremely abducted. Her cries from pain attracted the attention of her mother, who found the child unable to move the right leg and thigh. The family physician soon saw the little patient, and recognized the character of the accident suffered, but properly regarded it as a very unusual and delicate piece of surgery, and sought assistance. I saw the patient within four hours after the accident, and under chloroform verified, beyond a doubt, the first diagnosis of the physician of the family, viz., dislocation of the head of the femur upon the dorsum ilii. This decided, the leg was flexed upon the thigh, the knee carried from its position against the opposite thigh near the patella, upward over the opposite side of the pelvis from that of the injured joint, then outward over the umbilicus to the perpendicular line of the dislocated joint; thence the thigh was extended without the slightest difficulty, the head of the bone having resumed its natural position. The precise stage of this manipulation, at which the head of the bone slipped into the acetabulum, was marked by a sensation which would be described by *jar in the movement*, and took place at the moment of the commencement of the extension movement of the thigh. Surgeons will recognize the manipulation practised in this case, as the one which, though not new in fact, was so clearly demonstrated and forcibly advocated by Dr. W. W. Reid, of Rochester, N. Y.,

as to entitle him to a claim of originality.[1] All deformity and immobility at once disappeared.

The patient had no untoward symptom, and, after a forced quietude of ten days, walked and ran as if nothing had ever occurred to her joint. Coxo-femoral dislocations, in a subject so young as this, are very rarely recorded. Dr. Gross remarks that "it is a very uncommon thing to meet with a luxation of the hip-joint in children"[2] Dr. Hamilton[3] refers to *one* on the dorsum ilii at one year and six months; *one* at two years and one month; *one* at three years; *one* at three years and six months; and *two* at four years. He also refers to *one* into the ischiatic notch at three years, a total of seven cases of luxation of this joint at four years and under, which, added to the one here recorded, completes the list so far as I know. I should be glad to see this history call forth unrecorded experience with this class of cases, and reference to cases which have been published, but escaped my observation.[4]

Proceedings of Societies.

NEW YORK ACADEMY OF MEDICINE.

Stated Meeting, January 21, 1869.

Abstract of the Inaugural Address of Dr. H. D. Bulkley, President-elect.

Dr. Bulkley, on taking the chair, spoke happily of the responsibilities attaching, not only to the office he had been called to fill, but to fellowship in the Academy as well—responsibilities commensurate with the dignity of an institution whose history was the brilliant record of some of the best efforts of so many distinguished men; with the unlimited

[1] "Transactions of the State Society" for 1852.

[2] "System of Surgery," vol. ii., p. 175.

[3] "Fractures and Dislocations," p. 620.

[4] A case occurring in the practice of Dr. Fanning, of Catskill, N. Y., in a child two years and six months of age, will be found recorded in this Journal for September, 1867.—E. S. D.

opportunities for original investigation presented by this metropolitan city; and with the demands of a profession which calls upon all its members to devote the finest fruits of their toil to the common weal.

The speaker dwelt upon the urgent necessity for a building which should be recognized as the suitable home for such an Academy, which should attract the medical men of New York, not alone as the meeting-place of this Society and of others that might cluster around it, but by its library and museum, its rooms for reading and for conversation, where, besides enjoying social intercourse and gleaning the freshest intelligence, one could be sure of finding facilities for the pursuit of his most elaborate researches. The building committee were commendably active, and, if their labors met the proper response, we might soon see the foundation of an edifice that should do honor to the profession and to the country.

In order rightly to make use of the privileges and to meet the responsibilities above referred to, every member must bear in mind the necessity of one thing—work. And the organization of the Academy was such as to utilize the labors of all to the best advantage. The several sections brought the best results of their sessions to the meetings of the common body for further discussion, and in this way had been initiated some of the best debates in the history of the Academy. The Obstetrical Section was now full of vigor, and that of Theory and Practice gave signs of a renewal of its old vitality; with proper effort, the others would soon follow. With the sections thus to elaborate for discussion the material constantly accumulating, and with the original papers which the Academy had a right to expect from those fellows who had already done so much to advance medical science, and from those who, though younger in their labors, were equally earnest, the only difficulty would soon be lack of time at the meetings properly to dispose of the mass of valuable matter presented.

A Comprehensive Title.—The celebrated Surgeon Langenbeck, of Berlin, rejoices in the following official title: "Geheim Ober-Medicinal Rath General-Ärzt Professor Dr. von Langenbeck."

Reviews.

ART. I.—*The use of the Laryngoscope in Diseases of the Throat, with an Essay on Hoarseness, Loss of Voice, and Stridulous Breathing in relation to Nervo-muscular Affections of the Larynx.* By MORELL MACKENZIE, M. D. Second edition. With Additions, and a Chapter on the Examination of the Nasal Passages. By T. SOLIS COHEN, M. D. Philadelphia: Lindsay & Blakiston, 1869.

AFTER a careful perusal of the above work, we desire to call attention, first, to the original matter by Dr. Mackenzie, and next to the additions by the editor, Dr. Cohen.

The first eight chapters, pages 9–147, are a verbatim reprint of the first London edition, issued in 1865, of the author's work, with which many of the profession are sufficiently familiar. Not one new fact having been added, nor a single sentence changed in these chapters, comment is unnecessary. In Chapter IX., pages 147–158, we find, what purport to be, the principal additions on the use of the laryngoscope, from the second London edition. This chapter is presented under a misnomer, since, in place of additions to the use of the laryngoscope, it really contains the history of some valuable cases of intra-laryngeal surgery, by no means a novelty, with a description of the instruments used.

Chapter X., pages 159–182, by the editor, Dr. Cohen, containing a description of instruments familiar to laryngoscopists, might better have been omitted. These instruments, with scarcely an exception, were first fully described in Dr. Ruppaner's book "On the Principles and Practice of Laryngoscopy," published about a year ago. If additions are made by an editor to an author's work, we have a right to expect something that will enhance the value of the work in question, or that something new be presented. Neither of these requisites is fulfilled here. *À propos*, regarding the importation of instruments, why can English instruments not be as easily obtained as the German ones? We find it equally convenient to obtain the one as the other.

The value of the present edition is much enhanced by the

addition, Chapter XI., pages 183–260, of Mackenzie's valuable monograph on "Hoarseness, Loss of Voice, and Stridulous Breathing, in relation to Nervo-muscular Affections of the Larynx." This essay was first published in 1863, but in the present edition its scope is much enlarged. The favorable results obtained in the treatment of nervous affections of the larynx since the introduction of laryngoscopy, and the more rational employment of galvanism as a remedial agent, may well challenge comparison with any branch of therapeutics. But why should the editor omit the appendix on atrophy of the vocal cords and give us the essay in an incomplete form? The subject-matter is of too great importance that we should be willing to be deprived of part of it, nay, of even a single paragraph or sentence. Another omission in this monograph we cannot pass unnoticed. It also applies to the editor's addition to this chapter. We refer to the total silence on the treatment of aphonia, that is, cases of recent standing, by what Professor Van Brüns calls "the gymnastics of the larynx." That cases of aphonia, dependent upon a temporary abnormal condition of the nervous system, yield readily to this mode of treatment, that is, the well-regulated exercise of the vocal cords in the presence of the laryngoscopist, admits of no doubt. Besides, Van Brüns, Tobold, of Berlin, and Dr. Ruppaner, of New York, have reported successful cases. It is a satisfaction to know that cases of aphonia can be cured without either medicine or galvanism, and we should have been glad to have ascertained the author's opinion on this subject. The appendix on "Rhinoscopy," covering twelve pages in the original edition, ably and comprehensively written, will be missed with surprise by all familiar with Mackenzie's work. For what reason the editor has seen fit to ignore a portion of the author's own work, and substitute therefor what professes to be an original chapter on the same subject by himself, we are the more at loss to discover, inasmuch as Chapter XIII., by Dr. Cohen, presents nothing that is not really found in the original appendix, except, perhaps, the mention of a hard-rubber tongue-depressor, of which there are many kinds, one as good as the other.

Barring these exceptions, we are delighted to see this new

edition of Mackenzie, hoping that it may find its way extensively into the hands of the profession.

Art. II.—*Pronouncing Medical Lexicon, containing the correct Pronunciation and Definition of Terms used in Medicine and the Collatèral Sciences. With Addenda, containing Abbreviations used in Prescriptions, and List of Poisons and their Antidotes.* By C. H. Cleaveland, M. D. Eleventh edition. Philadelphia: Lindsay & Blakiston, 1869. 16mo, pp. 302.

This chunky little volume, which is presented to us with a most attractive exterior, was prepared—the author virtually tells us in his preface—for the especial enlightenment of that class of practitioners whose early education has been neglected, and who therefore are wont to take unauthorized liberties both in the pronunciation and application of the learned terms with which our chosen science is fettered and hedged in. This loading down of scientific language with Greek and Latin terms may be unfortunate for the class above alluded to, but the legacy has been imposed upon us, and we cannot throw it off. We must therefore do the next best thing when using these terms—endeavor to use them correctly. Now, when one sets himself up as a teacher in such matters, it is proper, to say the least, that he should be especially careful in his teachings—indeed, he ought to be sure of what he advises—for the unfortunates to whom his book is directed are not able to judge of the accuracy or inaccuracy of what is set before them, but accept every thing at first sight as correct; and so, if the teachings have been bad, they are confirmed in their blundering mispronunciation or bewildering misapplications of technical terms. This is precisely the effect that the study of the little book before us would have on one not well up in the use, meaning, and pronunciation of medical terms. We do not intend to say that the book is thoroughly bad—but there is so much in it that is bad, that its value as a guide is entirely lost upon those for whom it is prepared, for they can never be sure when it is right; and on the other hand, those who can make this distinction have no need of the book at any time. In the matter of pro-

nunciation, the errors are comparatively so few that it is to be regretted they were not amended, and thus fixed some value upon the book. But economy, rather than orthoepy, appears to have had the larger claim on the author's attention, and so, in successive editions, he has used over and over the old stereotyped plates, repeating errors which we doubt not have already been pointed out. Take the Latin words ending in *icus*, the rule is that these words shorten the penult; but there are about a dozen exceptions to this rule, and it is singularly unfortunate that three of these exceptions are words in common use in medical language: they are *anticus*, *posticus*, and *umbilicus*. They lengthen and therefore accentuate the penult. In the book before us the accent is thrown back on the antepenult, where it does not belong. We cannot stop to make good our assertion with regard to the words we have observed that are wrongly accentuated, but merely on a most cursory examination mention a few of those in most common use. We give the accentuation after the author, that our readers may judge for themselves: *cícatrix*, *cóccygis*, *eczéma*, *exanthemáta*, *prúrigo*, *impétigo*, *glandúla*, *prúritus*, *fremítus*, *venéris*, *vésica*. There are also many other words, in less common use, which are given with incorrect accentuation.

It is in the matter of definition, however, that this book is most worthless, and of positive injury to a beginner. We need only open the book at random, to pick out some most absurdly choice specimens; but, to avoid all appearance of partiality, we will commence on the first page, and we find "Abalienation; *corporeal* or mental decay;" "Abaran-Temo, a Brazilian tree"—wonderfully explicit, that, to say nothing of the practical uselessness of the term, which is out of place in a pocket lexicon. "Abdominal Pregnancy, pregnancy when the fœtus is above the uterus in the oviduct." How is it, should the fœtus be out of the oviduct or to one or other side of the uterus? On the next page we find "Abelmoschus; an evergreen shrub." "Abies; fir, an evergreen tree;" and on almost every page we find similarly unsatisfactory explanations of botanical names. "An African plant," "an evergreen shrub," "a Brazilian tree," etc., may convey a definite idea to our author's mind, but the most of us poor mortals, we fear, are too stupid to be much enlightened or edified thereby. We

should be inclined to exclaim with the oracular Solon Shingle, "Jes' so," and then inquire what it all means.

But we cannot undertake to follow the author's pages *seriatim*. Life is too short since the antediluvian era to indulge in the thankless task of pointing out all of other people's short-comings and wrong-doings, however instructive or beneficial such a task might be; and besides, in this instance, *le jeu n'en vaut pas la chandelle*. We may, however, note an occasional glaring absurdity or inconsistency merely to substantiate the position we have taken. Apoplexy is defined as "a disease of the brain." "Comatose, the state of profound stupor in congestive fevers." "Diabetes, a disease characterized by dextrine or sugar in the urine." "Diosma, a medicinal plant." "Entozoa, parasitical animals," "Inferior, name of certain muscles." "Shingles, an erysipelatous eruption around the middle of the body." "Catalepsy, a species of apoplexy." "Volvulus, ileac passion." "Throbbing, pulsating pain."

But these instances must suffice. Any one, who chooses to amuse himself, can find scores of others equally unsatisfactory, incorrect, or absurd. As to the matter of omission of terms now in common and daily use, we have nothing to say except that the book is in this respect most strikingly and lamentably deficient. We have given thus much space to this book, vastly more than it is worth, for the reason that we have noticed that some of our exchanges, evidently without having looked inside the covers of the book, have praised it most extravagantly. If any of our numerous readers should be tempted to buy the book, we would repeat to them Mr. Punch's advice to the young man about to be married, and simply say, "*Don't.*"

Art. III.—*A History of the Medical Department of the University of Pennsylvania, from its Foundation in* 1765. *With Sketches of the Lives of Deceased Professors.* By Joseph Carson, M. D., Professor of Materia Medica in the University of Pennsylvania, etc. Philadelphia: Lindsay & Blakiston, 1869. 8vo, pp. 227.

The early history of medical teaching in this country is the early history of the Medical Department of the University

of Pennsylvania, and of "its founders and first professors, who were prominent in the enterprise of transferring medical education from the Old World to the New, and who by their learning, talents, and energy contributed to its success." The materials of this history have been carefully collected and arranged by Professor Carson, of the University, who has done his pious task discreetly and acceptably.

It is probable that the first anatomical demonstrations in the United States were given in Philadelphia, about 1751, by Dr. Cadwalader, of that city, who had been a pupil of the celebrated Cheselden. Dr. William Hunter, a native of Scotland, and a relative of the celebrated Hunters, soon after settling in this country, gave lectures on anatomy at Newport, Rhode Island, in 1754, '55 and '56. In 1762, Dr. William Shippen, Jr., returned from Europe, and in the *Pennsylvania Gazette*, November 25, 1762, may be read the following announcement: "Dr. Shippen's Anatomical Lectures will begin to-morrow evening, at six o'clock, at his father's house in Fourth Street: Tickets for the course to be had of the Doctor, at five pistoles each, and any gentlemen who incline to see the subjects prepared for the lectures, and learn the art of Dissecting, Injections, etc., are to pay five pistoles more." The Introductory was delivered in the State-House; the number of students who attended this course was twelve; three courses were delivered, and the Medical School of America had its origin here. Up to this time there was no systematic instruction in medicine anywhere in these colonies; all the medical education to be got in the country was limited to the offices or shops of the preceptors, where a training in the handicraft of the profession was got. This apprenticeship, which lasted seven years, was no sinecure, and had its vexations. "The pupil (or apprentice as he was called) lived for the most part with his master—was constantly subject to his orders, whether in the task for preparing medicines to be used in his daily rounds, in carrying them to the patients, or in making fires, keeping the office clean, and other household duties now devolving upon domestics" (p. 30). We find that quite a number of these students, on the completion of their term of service, went to Europe, attracted by the reputation of the

schools at Edinburgh, Leyden, and Paris, and the hospitals of London.

We said that the Medical School had its origin in Dr. Shippen's lectures; they were in full operation when Dr. John Morgan arrived from Europe, in 1765. These two gentlemen were the fathers of systematic medical teaching in this country. It would appear from a letter of the celebrated Dr. Fothergill, written in April, 1762, that the matter of a medical school in the province had been already agitated. Sending some crayon anatomical drawings to the Pennsylvania Hospital, he writes: "In the want of real subjects, these will have their use, and I have recommended it to Dr. Shippen to give a course of Anatomical Lectures to such as may attend. He is very well qualified for the subject, and will soon be *followed by an able assistant, Dr. Morgan, both of whom, I apprehend, will not only be useful to the Province in their employments, but, if suitably countenanced by the Legislature, will be able to erect a School of Physic amongst you, that may draw students from various parts of America and the West Indies*, and at least furnish them with a better idea of the rudiments of their profession, than they have at present the means of acquiring on your side of the water" (p. 42). Morgan having served four years in the provincial army during the French War, spent five in Europe, under the most celebrated masters in every branch of medicine, having during that time, as he tells us, expended in this pursuit a sum of money of which the very interest would prove no contemptible income. He graduated as M. D. at Edinburgh, in 1763, his thesis being the mode of formation of pus, which he maintained was a secretion, thus anticipating Mr. John Hunter, there being no proof, according to Dr. James Curry, that the latter taught or even adopted such an opinion until a considerably later period. Dr. Morgan, while in England, became a proficient in injecting the glandular organs with wax, and preparing them by subsequent corrosion, and his skill in this sort of anatomical preparation was rewarded, when he went to Paris, by his being made a member of the Academy of Surgery. He was also elected a Fellow of the Royal Society of London, was admitted a Licenciate of the College of Physicians, London, and a member of the

College of Physicians, Edinburgh. When in Italy, he visited the venerable Morgagni at Padua, who was so pleased with him that he claimed kindred with him from the resemblance of their names, writing on a copy of his works which he gave him: "Affino suo, medico præclarissimo Johanni Morgan, donat auctor." Dr. Morgan seems to have been the first physician in Philadelphia who followed the European habit of graduates of medicine, and restricted himself to simply prescribing for the sick, separating himself from the handicraft, which requires distinct skill and long special training. He insisted too on the distinction being made between medicine proper and pharmacy.

Shippen had been sent to Europe in 1757, graduating at Edinburgh in 1761. While in London, he resided in the family of Mr. John Hunter, but was also associated with Dr. William Hunter and Mr. Hewson. He devoted quite a share of attention to the rising department of obstetrics, and his thesis was entitled "De Placentæ cum Utero nexu." These two zealous young men seem to have concerted while abroad the plan of establishing a medical school in their native city, and Shippen's anatomical lectures paved the way, before Morgan's return. The latter had, moreover, secured the favor of the Proprietor, the Honorable Thomas Penn, in behalf of his "proposal for introducing new professorships into the Academy for the instruction of all such as shall incline to go into the study and practice of Physic and Surgery," who wrote a letter to the Board of Trustees recommending that what Morgan had to offer concerning his scheme of lectures might "be taken with all becoming respect and expedition" into their most serious consideration. Approving letters from Fothergill, W. Hunter, Watson, and Cullen, were at the same time presented. The College of Philadelphia had been founded in 1749, sixteen years before. The trustees approved the project, and "entertaining a high sense of Dr. Morgan's qualifications, and the high honors paid to him by different learned bodies and societies in Europe, they unanimously elected him professor of the theory and Practice of Physic" (*Minutes*); and thus on May 3, 1765, was the first medical professorship in America created. The population of the city of

Philadelphia at this time was about twenty-five thousand, and of the colonies in the aggregate less than three millions. In September following, Dr. Shippen was, on application to the Board, unanimously elected Professor of Anatomy and Surgery. It is noteworthy that, at this time (1765), five of the most prominent physicians were members of the Board of Trustees of the College, yet there was no attempt to secure a place for any one of their own members, although one of them, Dr. Thomas Cadwalader, had, as we have seen, been the earliest teacher of anatomy in the country. For two years lectures were delivered by the two professors under the sanction of the College. In connection with their labors, Dr. Thomas Bond, one of the physicians to the Pennsylvania Hospital, began a course of Clinical Lectures in that institution. It may be told here, for it is worthy of record, that, at the time of an application for its charter to the Provincial Assembly—

"One of the objections offered to the measure was, that the cost of medical attendance would alone be sufficient to consume all the money that could be raised, it was met by the offer on the part of Dr. Zachary and the Bonds to attend the patients gratuitously for three years. This became the settled understanding with the Board of Physicians and Surgeons; nor have we learned that the compact has ever been annulled or abrogated during the period of one hundred and seventeen years (from 1751 to the present date), an instance of disinterested philanthropy which has been generally followed in the charitable institutions depending on medical attendance throughout the length and breadth of the land" (p. 37).

In such striking contrast was this with the custom then and now of Europe, especially in Great Britain, where the students' fees are quite an item in the income of hospital physicians, that the notorious Brissot particularly mentions it in his travels in the United States in 1788.

In 1767 the medical gentlemen of the Board of Trustees, with the two professors and the provost, framed a set of rules for the Physic School. These were submitted to the Board of Trustees, and adopted May 12, 1767; they regulated the conferring of the usual degrees in physic on deserving students—the bachelor's degree, and the doctor's degree. In the announcement given to the public press, it is stated that—

"This scheme of a medical education is proposed to be on as extensive and liberal a plan as in the most respectable *European* seminaries, and the utmost provision is made for rendering a Degree a real mark of honor, the reward only of distinguished learning and abilities."

In January, 1768, Dr. Adam Kuhn returned from Europe, and was at once appointed Professor of Materia Medica and Botany. He held the chair of Materia Medica for twenty-one years, when he was transferred to the chair of Practice.

The next event is an important one in the history of the school, and about which there has been some controversy—the bestowal of the first medical honors in America. It should be mentioned here that in 1768 a medical school had been organized in New York City, under the direction and government of King's College,[1] now Columbia College. In an inaugural address, delivered at the opening of Rutger's Medical College, November 6, 1828, the late Dr. David Hosack claims to correct an alleged error of the late Dr. Thomas Sewell, of Washington city, "relative to the first medical degrees conferred in the colonies, now the United States. . . . He dates the first medical degrees as conferred at the commencement held in Philadelphia, in June, 1771, whereas the doctorate had been previously conferred in the month of May of the preceding year [1770] in the city of New York." The late Dr. John B. Beck, in "An Historical Sketch of the State of Medicine in the American Colonies" (*Transactions New York State Medi-*

[1] The instructors in this school were: Samuel Clossy, M. D., Professor of Anatomy; John Jones, M. D., Professor of Surgery; Peter Middleton, M. D., Professor of Physiology and Pathology; James Smith, M. D., Professor of Chemistry and Materia Medica; John V. B. Tennant, M. D., Professor of Midwifery; and Samuel Bard, M. D., Professor of Theory and Practice. During the occupation of the city by the British, the lectures ceased; and after the peace an attempt was made to reëstablish the school, but it was unsuccessful. In 1792 Columbia College organized a Medical Faculty, and placed Dr. Samuel Bard, son of Dr. John Bard, one of the early Philadelphia physicians, who removed to New York, at its head; but it appears from the records of the college that, from that date to 1811, only thirty-four students completed their courses of study, and received the honors of that institution; so that when Dr. Morgan wrote to Hewson, respecting the New York rivalship, "for my part, I do not seem to be under great apprehensions," he judged rightly.

cal Society, 1850), says: "The first medical degrees were given by the College of New York. In 1769, the degree of Bachelor in Medicine was conferred upon Samual Kissam and Robert Tucker. In 1770 the degree of Doctor of Medicine was conferred on the last of these gentlemen, and in May of the following year [1771] upon the former. In June, 1771, the degree of Doctor in Medicine was conferred on four students of the Philadelphia College, being the first given in the institution." Now, the facts are, that, on *the twenty-first of June*, 1768, at a commencement of the College of Philadelphia, the degree of Bachelor of Medicine was conferred on ten gentlemen, namely: Messrs. John Archer, of New Castle County; Benjamin Cowell, of Bucks; Samuel Duffield and Jonathan Potts, of Philadelphia; Jonathan Elmer, of New Jersey; Humphrey Fullerton, of Lancaster County; David Jackson, of Chester County; John Lawrence, of East Jersey; James Tilton, of Kent County; and Nicholas Way, of Wilmington. The ceremonies are noted with exactness in the minutes of the Board of Trustees of that date, which sets forth with the declaration that "this day [June 21, 1768] may be considered as *the birthday of medica l honors in America*." A Latin oration, De Honoribus qui in omni œvo in veros Medicinæ cultores collati fuerint, was delivered by Mr. John Lawrence. A dispute, whether the retina or tunica choroides be the immediate seat of vision, was ingeniously maintained by Mr. Cowell for the retina, and Mr. Fullerton for the choroid. Then came, Questio, num detur fluidum nervosum? Mr. Duffield holding the affirmative, and Mr. Way the negative, both with great learning. Next, Mr. Tilton delivered an essay on respiration, and we are told that the manner in which it was performed did credit to his abilities. The provost afterward conferred the degrees; and an elegant valedictory oration was spoken by Mr. Potts. All this is circumstantial enough. Besides, Dr. Morgan writes prospectively to Mr. William Hewson, of London, November 20, 1767: "I have twenty pupils this year at about five guineas each. *Next year* [1768] *we shall confer the degree of Bachelor in Physic on several of them, and that of Doctor in three years after.* New York has copied us, and has six professors, three of whom you know. . . . Time will show in what light we are to consider the rivalship."

In June, 1769, the degree of Bachelor of Medicine was conferred on eight candidates. At the commencement in June, 1771, four of the graduates, who had received the primary degree in 1768, now received that of Doctor of Medicine; they were, Jonathan Potts, James Tilton, Nicholas Way, and Jonathan Elmer; their theses were written in Latin, and were published, according to the enacted rules of the Board, and are now in existence. Dr. Beck, in the interesting paper referred to, is in error when he states that the *only* inaugural dissertation published until after the War of our Independence was from the New York College, and by Dr. Samuel Kissam, "On the Anthelmintic Virtue of the Phasceolus Zuratensis," a copy of which is in the library of the New York Historical Society.

With regard, then, to the claim of precedence for the first medical honors in America between New York and Philadelphia, "it appears that the claim of priority of conferring degrees in medicine must be awarded to the Philadelphia School [1768], while the precedence of conferring the doctorate must be given to New York [1770]" (p. 68).

In 1769 Dr. Benjamin Rush, on his return from Europe, was elected Professor of Chemistry; so that, for the session of 1769–'70, there were five professors—Morgan, of Theory and Practice; Shippen, Jr., of Anatomy, Surgery, and Midwifery; Kuhn, of Materia Medica and Botany; Rush, of Chemistry; and Bond, of Clinical Medicine. "Rush was but twenty-four years old; Kuhn, but twenty-eight; Shippen, thirty-three; and Morgan, thirty-four. Bond only had arrived at that age when experience is supposed to bring the greatest wisdom, he was over fifty years" (p. 75).

We must refer our readers to Dr. Carson's interesting volume for the history of the growth and maturity of this ancient school of medicine; this nursery of medical teachers of the American continent. The old Alma Mater is as vigorous as ever; age has not withered her; her way of life is still full of the spirit of youth; and she yet keeps true to the gathered memories of those who made, and those who have kept, her fame.

> "There may these gentle guests delight to dwell,
> And bless the scene they loved in life so well."

Bibliographical and Literary Notes.

Many years since, the reading of De Quincey's "Confessions" aroused within us the intensest desire to taste the pleasures of opium-eating, and though then a mere fledgling, the opinion was formed, which subsequent and more mature experience has substantiated, that his book would only be productive of evil, especially to the young and susceptible reader; for there are few, we apprehend, but would yield something to the seductive influence of the wondrous, word-compelling power displayed by that marvellous-minded man in his dangerous narrative.

It was, therefore, with no little misgiving that we took up the volume[1] now before us, fearing that, from the very nature of the subject treated of, perhaps more of bad than of good influences might be evoked. But a careful perusal of its entire contents enables us to record our opinion that, rightly studied, the tendency of the teachings here conveyed can only be salutary; and we earnestly commend the book to all physicians, although the class of readers for whom it is prepared will derive precious little consolation from a perusal of its pages.

It consists of a series of individual narratives, collected by an anonymous compiler, and intended especially for opium-eaters; as the instances narrated of success in breaking off the habit may serve them for encouragement and guidance. The first narrative, however, is the only really satisfactory one recorded of success in this laudable undertaking. But the other instances show the terrible sufferings entailed upon the victims of the opium-eating habit, and still more vividly do they point out the agonizing distresses—worse than death itself—which invariably are aroused by the withdrawal of the accustomed stimulus. And herein lies the greatest obstacle to cure, for few there be of nerve enough to pass through so dreadful an ordeal. Death itself—to say nothing of the relentless bondage in which this habit holds its unhappy victim—would be preferable to most men.

[1] The Opium Habit, with Suggestions as to the Remedy. New York: Harper & Brothers. 1868. 12mo, pp. 335.

But we cannot undertake an analysis of the book. It is of absorbing interest, too much so, we fear, for a calm survey of its teachings. While the narrative portions of the volume will most interest the general reader, there are, incidentally scattered through the book, many facts that the thoughtful physician may turn to good account. But the part that most naturally comes within his province is the closing chapter, contributed by Fitz-Hugh Ludlow, entitled "Outlines of the Opium-Cure." With a masterly pen he here maps out for us a systematic and rational line of treatment to be pursued, premising the one essential condition that special and well-appointed institutions are absolutely necessary for the proper management of these fearful cases. In this we think the profession will entirely accord with him, but experiences on a larger scale than are thus far recorded are wanting, to bear out the author's sanguine expectations of a cure in so large a proportion of cases.

The first edition of Dr. Hartshorne's "Essentials of the Principles and Practice of Medicine"[1] was noticed in the sixth volume of The Journal. The present one has been revised and enlarged by some thirty-odd pages. The title is a palpable misnomer—a good catch-title for a publisher, but unworthy an author; and Dr. Hartshorne would be the last to claim that all that is *necessary* in the principles and practice of medicine is to be found in this libellus. If this were so, how foolish it would be for physicians to purchase and waste time in reading Aitkin, Flint, Wood, Bennett, Williams! It is simply a medical primer, a first-class book, and contains the outlines of the science and art of physic; and, as such, is of the very best quality. It is much superior to any other work of its sort; and, if rightly used, will be found highly useful. Dr. Hartshorne not only knows what others have done or are doing, but he uses wisely his faculties of observation and reason, and has very decided opinions of his own, which he does not

[1] Essentials of the Principles and Practice of Medicine. A Handbook for Students and Practitioners. By Henry Hartshorne, M. D., etc. Second edition, revised and improved. Philadelphia: Henry C. Lea. 1869. Small 8vo, pp. 452.

hesitate to express, even when in opposition to the current doctrines of the day. In a work so generally accurate, we are surprised to find the statement that amyloid degeneration "consists in the conversion of tissue into a substance having physical and chemical properties resembling those of starch or cellulose" (p. 44), which has for some time been abundantly proved to be a chemical error of Virchow.

This book of Mr. Marshall[1] will be found useful by those who do not care to go very deeply into the science of physiology, and who are satisfied with getting their knowledge of the subject through a medium which makes it undergo a very extensive process of dilution. In the main, the author is clear and concise; but the absence of references to his statements unfits the book for the purposes of any but superficial students, or the general reader.

A prominent and useful feature of the treatise is the comparative physiology, a part of the science not ordinarily sufficiently considered by the medical student, and not deeply touched upon in the present volume. However, what there is of it may serve a good turn with some, who otherwise would know nothing whatever of the matter; but the fact that this part of the book is printed in small type will doubtless make many pass it over as non-essential.

The English edition was issued in two volumes. The American publisher, however, has seen fit to compress them into one unwieldy and ugly volume, printed on bad paper, and no better, in appearance, than hundreds of others which have come from the same house.

The additions of Professor Smith, though not extensive, supply several omissions of the author, and add to the value of his treatise.

Thirty years ago it was heresy in Dublin to speak of two kinds of continued fever, although at that time so much had been done toward both the symptomatology and therapeutics of fevers by eminent Irish physicians, particularly the late Dr.

[1] Outlines of Physiology, Human and Comparative. By John Marshall, F. R. S., etc. With Additions by Francis G. Smith, M. D., etc. Philadelphia: Henry C. Lea. 1868. 8vo, pp. 1026.

Graves, and Dr. Stokes. Several years previously, Dr. A. P. Stewart, then of Glasgow, and afterward of the Middlesex Hospital, London, had attempted to show the non-identity of typhous and typhoid fevers, but his observations, founded on over three thousand cases, during the years 1836, '37, '38, as well as the facts and arguments of both French and American physicians, failed to bring conviction to the medical mind of Britain; nor were the essential differences of the two forms of fever, both in origin, morbid phenomena, and anatomical characters, generally recognized there until the publication of Dr. (now Sir William) Jenner's papers, in 1846. There are yet those who maintain the identity of the two disorders, and among the ablest and most persevering is another eminent Dublin physician, Dr. Henry Kennedy, who believes that typhus and typhoid fever are the result of a single poison, and that no other hypothesis can explain so well all the difficulties of the case.

The object, says Dr. Hudson, "I have had in view in delivering the ensuing lectures,[1] and in now publishing them, is to furnish the student with a guide to his bedside analysis of each case, by treating of febrile phenomena in succession: first, generally or abstractedly; and secondly, in their relation to each form of the disease—"thus forming in his mind an *ideal* of fever, such as he may readily apply to the case before him, and which he may certainly find to conform to that case, be it of what species, or how complicated soever it may" (Preface, v.).

We can cordially recommend this work to our readers, as one of practical merit, and, though not adding any thing to the stock of knowledge, yet what is known and approved is generally well put.

We welcome with pleasure the appearance of this new periodical,[2] devoted to cutaneous and venereal diseases. The

[1] Lectures on the Study of Fever. By Alfred Hudson, M. D., M. R. I. A., Physician to the Meath Hospital. Philadelphia: Henry C. Lea. 1869. 8vo, pp. 316.

[2] Annales de Dermatologie et de Syphiligraphie, publiées par le Dr. A. Doyon. Première année, No. I. Paris: Victor Masson et Fils. 1869.

editor, Dr. A. Doyon, of Lyons, is already known to venereal specialists by his valuable contributions to syphiligraphy. The present number contains a "Contribution to the Study of Gonorrhœal Rheumatism," by Alfred Fournier, the one of the numerous venereal specialists in Paris who promises to be a second Ricord in deserved reputation; an article on the use of ice in certain affections of the testicle (especially gonorrhœal epididymitis), by our valued and always entertaining, though visionary friend, Dr. Diday; one on venereal diseases of the uterus by the able, learned, and sound Rollet; another on "Diatheses in diseases of the skin with reference to treatment;" together with a bibliography, and a review of articles in medical journals pertaining to the skin and syphilis.

The list of "collaborateurs" announced embraces the names of those best known in these specialties in France, and several others in Germany and Italy. This journal will be indispensable to specialists, and of importance to all who take an interest in the subject of which it treats.

The present volume,[1] besides the Minutes of the Nineteenth Meeting of the Association, and of the several Sections, with the address of the President, contains but few papers which claim even a passing notice. It is unusually barren. The reports on the Climatology and Epidemic Diseases of West Virginia, the District of Columbia, Texas, and Pennsylvania, are interesting and instructive as far as they go, but are open to the charge of want of thoroughness and completeness. The several articles which will particularly claim and deserve notice are: "On the Conveyance of Cholera from Hindostan through Asia to Europe and America," by Dr. John C. Peters; the "Report of the Committee on Ophthalmology," by Dr. Joseph S. Hildreth; "Report on the Treatment of Club Foot," by Dr. Lewis A. Sayre; "A New Method of reconstructing the Lower Lip after its Removal by Disease," by Dr. Gurdon Buck; and "The Treatment of Syphilis by Hypodermic Injection," by Dr. Elsberg.

[1] The Transactions of the American Medical Association. Instituted 1847. Vol. xix. Philadelphia: Printed for the Association. 1868. 8vo, pp. 497.

In one respect this volume is an improvement on some of its predecessors; there are no prize essays. The Treasurer, in his report, writes: "A great abuse has grown up in the Association, and has largely assisted in the impoverishment of the treasury. The Association is permitted to give an annual prize of one hundred dollars for an essay of great worth, if such a one should be presented, and even to vote one hundred dollars to a second, if several should be presented of great value. Under this permission it has become a custom to vote away two hundred dollars annually to the two best essays presented, whatever be their intrinsic merit; thus, not only voting away two hundred dollars, but entailing a heavy expense for printing and illustrating essays that may deserve no such distinction. Will the Association give special heed to this fact?" The Association seems to have heard and heeded, and hence we have the gratifying intelligence that "the Treasurer has the honor to report that the American Medical Association is again solvent."

In using oxygen for the treatment of disease, Dr. Birch[1] prefers the gas itself administered by inhalation, though he mentions with approval oxygenated water, nitrous-oxide water, ozonified oil, perchloric acid and its compounds, and oxygenated bread. The solution of peroxide of hydrogen, proposed and recommended by Dr. Richardson, he does not find generally useful.

As to the affections in which oxygen is deemed advantageous, they are generally those which are due to or accompanied by a depressed condition of the system. In such diseases it would seem to be *a priori* indicated, and Dr. Birch gives several interesting cases of its success. We incline to the opinion, however, that he is over-enthusiastic in its praise, and that fuller experience will not confirm all he alleges in favor of its efficacy.

Messrs. Wm. Wood & Co. will issue in May next the first number of the "Archives of Ophthalmology and Otology,"

[1] On the Action and Use of Oxygen in the Treatment of various Diseases otherwise incurable or very intractable. By S. B. Birch, M. D., etc. Second edition. London, 1868. 12mo, pp. 149.

edited by Profs. H. Knapp, M. D., of New York, and S. Moos, M. D., of Heidelberg, Germany. The "Archives" will contain only original papers, and will be printed simultaneously in English here, and in German at Heidelberg. The same plates and illustrations will be used for both editions. The well-known reputation of the editors is a sufficient guarantee of the scientific ability of this new publication, which commends itself to all interested in these special studies. The edition will be limited, and subscribers are therefore requested to send in their names early.

The Lippincotts announce "The Structural Lesions of the Skin," by Dr. H. F. Damon.

Messrs. W. A. Townsend & Adams announce a reprint of Dr. Letheby's "Lectures on Food," which are now appearing in the *Journal of Chemistry*.

We have received from Messrs. Wm. Wood & Co. a copy of Klob's "Pathological Anatomy of the Female Sexual Organs," translated by Drs. Kammerer and Dawson. On the first appearance of this book, we gave it quite an extended notice, and now we need only say that the externals of the volume are much more creditable than under the former publishers. There is no change made in the body of the book, and consequently we have no change to make in our comments thereon.

The *Humboldt Medical Archives*, one of the most enterprising and successful of our numerous exchanges, changes its title to the *Medical Archives*, with the commencement of the new year and volume. Dr. E. A. Clark, Resident-Physician of the St. Louis City Hospital, will hereafter be associated with Dr. Whitehill in the editorial management of this journal.

We have received from Dr. S. W. Butler, the publisher and editor of the *Medical and Surgical Reporter*, a copy of his visiting list. We have previously called attention to the advantages which this list presents, and have now only to re-

new them. The patent clasp, a most admirable contrivance, adds greatly to the convenience of using this little book.

We have received from the Messrs. Churchill, of London, a copy of the third edition of Tilt's Hand-Book of Uterine Therapeutics. The lengthy notice which in the last number we gave of the American edition dispenses us from any further notice of the work.

Mr. Berkeley Hill's new work on Syphilis and local Contagious Disorders has appeared from the press of James Walton, London, and is also reprinted in this country by H. C. Lea.

Dr. Wilson Fox has published, through the M'Millans, of London, his observations on the Artificial Production of Tubercle. The volume is issued in superb style—quarto form—with chromo-lithographs and engravings.

From Trübner's press we observe a second edition of Dr. Chapman's "Sea-Sickness and how to Prevent it." Our readers will recall this method as the pet plan of Dr. Chapman, by applying his ice-bags to the spine. We are informed by a physician who has experimented with this method, that unquestionably, in many cases, it proves efficacious.

Messrs. John Churchill & Co. have nearly ready a second and enlarged edition of Dr. F. W. Pavy's treatise on Diabetes, its Nature and Treatment.

The "Vorlesungen," or lectures of Dr. Lewis Büchner, upon the theories of Darwin, and their relations to science and philosophy in general, are to be translated into English, and will be published, it is understood, by Leypoldt & Holt, of this city.

The *St. Louis Medical Reporter* has changed hands. Dr. O. F. Potter retires from the editorship, and is succeeded by Drs. W. M. McPheeters and G. M. B. Maughs. It will now appear in monthly issues instead of, as previously, semi-monthly.

In the *Atlantic Monthly* for March, Dr. Henry I. Bowditch completes his admirable series of papers on Consumption. These papers are prepared especially for the instruction of the public, but every medical man in the country should read them. They are not only interesting, but valuable and instructive in the highest degree. We hope soon to give our readers a full abstract of them.

French literature has recently contributed a number of new works on medicine, of which we notice the following: "Physiology and Instruction of the Deaf and Dumb according to the Physiology of different Languages," by Dr. E. Fournie. "Studies on the different Forms of Encephalitis," by G. Hayems. A translation of Niemeyer's "Internal Pathology and Therapeutics." "Gestation in Relation to its Influence on the Physiological and Pathological Constitution of Woman," by Dr. Th. David. "Elementary Treatise on Surgery," by Dr. Fano.

"Théorie Physiologique de la Musique fondée sur l'Étude des Sensations Auditives." Par H. Helmholtz, Professeur de Physiologie à l'Université de Heidelberg, etc. Traduit de l'Allemand par M. G. Guéroult, ancien élève de l'École Polytechnique; avec le concours pour la partie musicale de M. Wolff. Avec figures dans le texte. Paris: Victor Masson et Fils. 1868. "A Physiological Theory of Music based upon a Study of Auditory Sensations." By H. Helmholtz, Professor of Physiology in Heidelberg University, etc. 544 pages. Translated from original German by M. G. Guéroult, formerly Pupil of the Polytechnic School; and with the assistance, in the musical portion, of M. Wolff. With figures in the text. Paris: Victor Masson & Son. 1868.

"Lorain, Études de Médecine Clinique et de Physiologie Pathologique." Le Cholera observé à l'Hôpital Saint Antoine, Paris, 1868. Avec 8 planches graphiques intercalées dans le texte, et en partie coloriées. Paris: J. B. Baillière. "Lorain, Studies in Clinical Medicine and in Pathological Physiology." The Cholera of 1868, as observed in Hospital St. Antoine, Paris. With 8 partially colored plates in the text.

Dr. Christol's "Leçons de Clinique Chirurgicale," delivered at the Hôtel Dieu, of Lyons; published in numbers.

The following medical additions to literature are announced from Paris: "Aphorisms on Venereal Diseases," with a special formulary, by Edward Langlebert. Syphilis—Jerome Fracastor's Latin poem, translated by the same author.

"Mexico, from a Medico-Chirurgical Point of View," by Leon Coindet, chief surgeon of the 1st and 2d divisions of the Mexican Army.

"Photographic Studies of the Nervous System of Man and some of the higher Animals, from Dissections of congelated Nerve Tissues," by Dr. Pierre Rondanovsky; 203 Photographs in 20 Plates.

"A Memoir on Surgical Intoxication," by M. Maissoneuve.

"The Method of Continuous Aspiration as a means of Cure after Capital Amputations," by M. Maissoneuve.

Books Received.—Annual Report of the Surgeon-General, United States Army, for 1868.

This report shows that, during the year, very satisfactory and decided progress has been made in the preparation of the materials for the forthcoming Medical and Surgical History of the War. Eight chromo-lithographs, eight lithographs, three diagrams, and one hundred and twenty-two woodcuts have been completed during the year. Five hundred pages of manuscript are ready for the printer, and a large amount of statistical material is in such a stage of advance that it can be made ready for the press at a short notice. This History, if it ever be completed, will undoubtedly be one of the most valuable and important publications ever issued; and we hope to see a wise liberality manifested by Congress in furthering so useful a work.

An Inquiry into the Physiological and Medicinal Properties of the Veratrum Viride, together with some Physiological and Chemical Observations upon the Alkaloid Veratria. Prize Essay, to which the American Medical Association awarded the Gold Medal for 1863. By Samuel R. Percy, M. D. Reprint from the Transactions of the American Medical Association. From the Author.

Digitalis; its Chemical, Physiological and Therapeutical Action. An Essay to which was awarded a Prize by the American Medical Association, May, 1866. By Samuel R. Percy, M. D. Reprint from the Transactions of the American Medical Association. From the Author.

Vaccination Impartially Reviewed. By Ferdinand E. Jencken, M. D. London: John Churchill & Sons. 1868. Pamphlet, pp. 28. From the Publisher.

Dr. Watter's Doctrines of Life. Reprint from the St. Louis Medical and Surgical Journal. Pamphlet, pp. 28. From the Author.

Physician's Medical Compend and Pharmaceutical Formula. Compiled by Edward H. Hance. Philadelphia: Hance, Griffith & Co. 1868. 12mo, pp. 214.

Thirteenth Annual Report of the Trustees of the State Lunatic Hospital at Northampton, Mass. Pamphlet, pp. 43.

Reports on the Progress of Medicine.

OPHTHALMOLOGY.

By Henry D. Noyes, M. D., Professor of Ophthalmology in Bellevue Hospital Medical College; Surgeon to New York Eye and Ear Infirmary.

(Concluded from the February Journal, page 557.)

28.—*The use of Calabar Bean in Fistula of the Cornea.* By Dr. William Zehender. [Klinische Monatsblätter für Augenheilkunde, February, 1868, pp. 35.]

By fistula of the cornea is not meant a permanent opening through which the aqueous humor must constantly drain, but that in consequence of ulceration an exceedingly thin spot is left, which from time to time gives way and empties the anterior chamber; the globe remains at all times rather soft, the anterior chamber imperfectly filled. The condition is often very difficult to cure, and may lead to atrophy of the globe by irido-choroiditis. Atropia and a pressure bandage, the cauterization of the ulcer with nitrate of silver, are the chief remedies employed. Dr. Zehender had under treatment a girl ten years old, whose left eye was attacked by blenorrhœa, and a large ulceration of the lower portion of the cornea—perforation and adhesion of the iris ensued, and a fistula was formed. The usual remedies were employed for six weeks or more, without preventing the reopening of the fistula once in several days. The place where perforation occurred was at the margin of the pupil, which was dilated to a medium degree. The extract of calabar bean was then tried—one drop of a fluid preparation daily.

On the seventeenth day the aqueous again escaped, but not again. The treatment was kept up five weeks longer, and after the lapse of several months the cornea remained healed. The opacity of the cornea was so extensive as to make it impossible to see the exact relations of the fistula to the edge of the pupil, but Dr. Zehender thinks they must have been in contact with each other. It is easy to see how the active contraction of the pupil would present a barrier to the rupture of the thin spot of the cornea, and give it an opportunity to attain sufficient strength to withstand unaided the pressure of the fluids of the eye.

29.—*Disease of the Corneæ in a Case of Extensive Cutaneous Anæsthesia* (Elephantiasis Græcorum Anæstheticum?). By Dr. Chisholm, of Charleston, U. S. [Ophthalmic Hospital Reports, vol. vi., 2. pp. 126–131.]

The subject of this disease was a man 44 years old, in whom the general malady had existed about eighteen years. He had lost many of the pha-

langeal bones of the hands and feet. Anæsthesia has become general, and he can feel pain in being pricked only over the upper portions of the spine, the back of the head, and the chest from under the arm-pits to the waist. The fingers and toes are contracted, and the extremities of his feet ulcerated, and give rise to an offensive ichor. Though he cannot feel the prick of a pin on his feet, and once scalded them with boiling water without knowing it until the cuticle came off, if he treads on a pebble he has a sharp pain shooting up the limb. His mucous surface is healthy, except a moderate amount of ozena. Four years ago a red spot appeared upon the lower edge of the left cornea, and gradually developed itself into a fleshy, vascular thick mass, coextensive with the whole cornea. No pain accompanied the process. After three years a similar change began in the right cornea, and has involved its lower half, changing it into thick, opaque, pinkish, fibro-cellular tissue. The margin at the sclerotic juncture is abruptly elevated one-fifth of an inch. It slopes off to the normal level at the neighborhood of the pupil, and fades into a cloudy opacity. The thick, fleshy, red disk, which replaces the left cornea, is about three-eighths of an inch above the level of the sclera, and its edges steep. It is described as covered by a comparatively healthy and not much thickened conjunctiva. In it a few large vessels run to the slightly depressed centre of the disk and then disappear in its substance. The ocular conjunctiva only slightly injected, and otherwise healthy. Lachrymal secretion is in excess. Irides healthy. Patient never had syphilis and has been treated by all possible medicines without avail.

30.—*The so-called Canal of Fontana or Schlemm—the Circular Venous Sinus*—(the space between the cornea, sclera, and ciliary muscle.) By Dr. P. Pelechin, of St. Petersburg. [Archiv. für Ophthal., b. xiii., 11. s. 422–446.]

The canal above alluded to has been commonly considered to be a vein. Schlemm declared that he found it filled with blood in a man who had been hanged; others have professed to be able to fill it by injection. Dr. Pelechin made an extensive series of investigations on men and animals, both macroscopic and microscopic—performed injections of veins and arteries by various methods, examined eyes of men and animals which had been hanged, dissected more than a hundred human eyes, and the following are his conclusions: *Anatomy.*—1. The canal of Fontana is a space which, in the eyes of men, rabbits, dogs, cats, rats, and swine, is formed by the attachment of the ciliary muscle to the place of junction of the cornea and sclera, and in men and rabbits is formed chiefly of elastic fibres from the sclera. 2. In oxen and horses the canal is made by a separation of the ciliary muscle itself, whose inner bundles unite with the sclera. 3. In birds the canal is proportionately larger than in oxen, but similarly formed. 4. That it is also found in fishes, but is smaller.

Physiology.—Multiplied experiments, in which most complete injections of all the capillaries were obtained, proved that the canal is not a venous sinus, nor any kind of blood-vessel. It was also proven not to be a lymphatic duct. Whether it communicated with the aqueous chamber was not satisfactorily determined.

The function of this space is declared, in accordance with the hypothesis of Helmholtz, to be to afford space for the peripheral parts of the iris to draw back in near vision. When relaxed, the iris takes hold on the anterior border of the canal of Schlemm (Fontana), when tense it takes hold of the posterior border. The distance between the situations is 0.45 millimetres.

The size of the canal in man (its section is oval) is for its long diameter 0.6‴, for its short diameter 0.2‴.

31.—*Pathological Specimens recently added to the Museum of the Royal Ophthalmic Hospital.* [Oph. Hospital Reports, vol. vi., 2, 155.] Mr. B. J. Vernon.

An account is given of four eyes, in three of which were tumors, and the fourth contained a hæmatocele, which simulated a melanoma. The first was a glioma, the second a glio-sarcoma, the third a melanoma. The first two were children, the third a woman, æt. 66. The fourth is interesting, in view of the error of diagnosis. Five months before operation, patient received a blow on the eye while chopping wood; by this he lost sight. After two months he received another blow, and the eye then became painful, and began to enlarge. Three months after, when he came to the hospital, the lids were swelled and dusky, eyeball very prominent, much enlarged, and in the upper ciliary region was a staphyloma of deep-black color, and very tense. He was in considerable pain, and the case was thought to be one of rapid melanosis. The globe was wounded in the excision, and this was followed by a gush of bloody fluid, and collapse of the bulb.

The eyeball, on examination, was found to have been reduced to a mere bag, in which hardly any traces of the normal structure of the globe could be recognized, its sole contents seeming to consist of a dark-colored fluid, with some partially decolorized blood-clots adhering to the sclerotic. The sclerotic appeared to have been distended, and then to have split in many directions; the continuity of the wall of the bag had been maintained by the orbital fascia and the expanded tendons of the muscles. The sclerotic was much thickened, very brittle, and its inner surface of yellow color, an appearance much resembling that of an atheromatous artery.

32.—*Tumors of the Orbit and the Globe.* By Dr. J. Hirschberg. [Zehender Klin. Monatsblätter für Augen. 1868, June, 153.]

Three cases, operated on by Prof. Graefe, are reported, two being tumors behind the eye, and one a tumor of the globe itself, both external and internal.

One case is remarkable. The tumor had been growing in the orbit six years, and for one year had caused blindness of that eye. There was subretinal effusion, and also subchoroidal effusion at the upper half of the fundus, the sac projecting far into the vitreous, and having a bluish-gray look: vision consisted of recognition of the hand in a good light. The tumor extirpated without interfering with the eye, and three weeks after patient could count fingers at eight feet, and the visual field was perfect. The subretinal effusion was completely absorbed; *both the retina and choroid perfectly restored to their proper place.* This result is surprising, and well worthy of remembrance. It happened to Prof. Graefe once before, after the evacuation of an orbital abscess. This shows that subretinal effusions, from external pressure and irritation, have a much more hopeful prognosis than when caused by prolongation of the globe, as in posterior staphyloma.

33.—*Cases of Neuritis Optica, Neuro-retinitis, and Retinitis.* By J. W. Hulke. [Ophthalmic Hospital Reports, vol. vi., 2, page 89, 1868.]

There are 39 cases, more or less completely recorded, and classed under four heads: 1. Those from injury; 2. Those resulting from an intracranial disorder; 3. Those dependent on dyscrasia; 4. Miscellaneous. Of the first class there are 2 cases; of the second class, 12 cases, in one of which an autopsy was made, and showed a node at the sella turcica and the hollow for the cavernous sinus, and the upper surface of the petrosal bone, meningitis, and softening of the anterior cerebral lobe. The corresponding optic nerve was healthy in general appearance for three-

fourths of its length within the orbit, but at a point four lines from the globe it began to enlarge, and continued to increase up to the eye. The tumefaction depended on infiltration by products of inflammation.

In the third class are 17 cases, viz.: syphilis, 5; anæmia, from rapid child-bearing, prolonged suckling, and leucorrhœa, 6; phthisis pulmonalis, 1; rheumatism, 3, although the connection as cause and effect is not considered certain; diphtheria, 2, one a girl 18, and the other a girl 14. This lesion is not often seen, and deserves to be remembered.

In the fourth class, one is ascribed to masturbation and sexual excesses, another to disease of the aortic valves, the others to unknown causes.

The cases are very briefly noted, and could not be further condensed; the ocular changes were such as are usually seen.

34.—*The Formation of Pigment in the Optic Disk and Retina.* By Dr. H. Knapp. [Archiv für Ophthalmologie, B. xiv., Abth. 11, s. 252–261.]

Effusions of Blood in the Optic Nerve and Morbid Deposits of Pigment in the Optic Disk. By Dr. Wecker. [Zehender's Monatsblätter für Augenheilkunde, 1868, 204.]

These papers refer to a lesion of the papilla, which is extremely rare. Ed. Jaeger and Liebreich alone have published cases. The pigment appears in the peripheral parts of the nerve, but is by no means to be confounded with the pigment which so frequently marks the choroidal ring. Liebreich's case was the result of an injury; of Jaeger's no history is given. Dr. Knapp's case was a young woman who, for three days, suffered severely from headache and fever. On awaking in the morning she found herself totally blind, and remained so until the examination, which was six years after the occurrence, while the head-symptoms soon vanished. He found both papillæ atrophied and covered with black pigment at their periphery. The condition is shown by a chromo-lithograph.

Liebreich supposed the pigment generated in new-formed cells of connective tissue, which, in atrophy of the optic nerve, replaces the nerve-fibres. Dr. K. believes it to be the result of a hæmorrhage into the intervaginal space of the optic-nerve sheath, the blood to have penetrated this space from a cerebral apoplexy, and shows itself by oozing through the lamina cribrosa. Dr. K. has examined two eyeballs, in which extravasations were found in the intervaginal space, near the sclera. The clot makes so much pressure on the retinal vessels and the nerve-fibres as to produce immediate blindness. That this theory of the pigmentation of the papilla is correct, Dr. K. thinks is shown by a case in which the outer wall of the orbit was blown away by a pistol loaded with shot; it was an attempt at suicide, and the muzzle was pressed against the temple. The muscles of the globe, and the optic nerve, were exposed to view up to the apex of the orbit. When he saw the patient, the cavity was filled with blood and inflammatory secretions—the eye was totally blind. By the ophthalmoscope a mass of blood was found on the lower and outer side of the papilla, running out a little distance into the retina; no other lesion. After five months the case again examined, the wound closed, the upper lid paralyzed, the eye turned a little downward, movable in all directions, though not quite to the full extent, its form and tension normal, still blind. In the fundus was a black and white mass, covering the outer half of the nerve, and running out to the yellow spot. This was a pseudo-membrane, and the pigment on its borders, and at another place below it, was due to transformation of the clot previously observed.

Dr. Wecker publishes three cases of hæmorrhage into the papilla, all having a traumatic origin, but in no case was pigmentation afterward observed. In one the bleeding followed iridectomy for glaucoma, and was

wholly absorbed in twelve weeks. In the second case a tumor was removed from the orbit, and a third of the surface of the nerve was covered by a clot, the visual field perfect v=1: in six weeks the blood was gone. The third case was seen only once; he was operated on for pterygium. Dr. W. thinks the blood in these cases came from the vessels of the papilla. Dr. Knapp traces the blood in his case to an intracranial source, and believes it to have travelled down along the intervaginal space, and subsequently left its mark in the pigment which he saw.

A case, presumed to be hæmorrhage into the sheath of the nerve, was published by Dr. Sands in the *New York Medical Journal*, for November, 1866, page 106.

35.—*Embolus of the Arteria Centralis Retinæ.* By Dr. R. Schirmer. [Zehender's Monatsblät. für Augenheilkunde, 1868, Feb., page 38.]

Occlusion of the Blood-vessels of the Eye. By Dr. H. Knapp. [Archiv. für Ophthalmologie, B. xiv., II., s. 205–252.]

Embolism of the Arteria Centralis Retinæ. By Dr. L. Wecker. [Schmidt's Jahrbuch, 1868, No. 7, p. 76.]

Dr. Schirmer reports one, and Dr. Knapp five cases of occlusion of central artery of the retina by embolus, except one, which was caused by a wound in the orbit. In one case the cause was atheroma of the aorta and its valves; in another, aneurism of the common carotid; in another, atheroma of the arteries; in two cases no cause could be found. The symptoms are sudden loss of sight, though often retaining perception of light, and visual field concentrically shrunken. If the remote cause do not lie in some concomitant brain-trouble, there will be no headache, dizziness, etc., but under certain etiological conditions there may be cerebral symptoms. The lesions in the eye are what Graefe first pointed out, the arteries excessively small, containing blood for a little distance beyond the papilla, then often entirely empty, the veins fullest at the periphery, sometimes tortuous, and tapering to a point at the papilla; the papilla a yellowish white, not the dead white or gray of atrophy; no pulsation producible in the arteries by pressure with the finger on the globe; the retina soon becomes infiltrated, and the macula has a more or less positive red color.

Dr. Schirmer saw his patient four hours after the attack. The borders of the nerve were sharp, and the retina perfectly transparent, the macula red, but he says not the redness of ecchymosis, merely the color seen in many children. On the second day the papilla became more red, pervaded by infiltration, which also extended into the contiguous retina; the macula remained free.

In one of Dr. Knapp's cases the obstruction was not complete, and sight returned; but the issue of these cases is generally complete blindness, from atrophy of the nerve.

Dr. Knapp also relates four cases of thrombosis of the sinuses of the brain, in which grave ophthalmic symptoms occurred. He examined the eye of one case; the other three cases are quoted from *Ducheck's* "Handbuch der Spec. Path. und Therap." The distinctive symptoms were protrusion of the globe, redness and œdema of the connective tissue of the orbit and conjunctiva, intolerance of light, fixed and dilated pupil, loss of sight, and paralysis of ocular muscles. These local signs are added to other and grave symptoms of cerebral origin. In no case is it intimated that any search was made for pulsation over the region of the eye and temple. It would be natural to expect its occurrence in these acute cases, as well as in chronic cases, which are alluded to above.

Dr. Knapp justly remarks that the fatal meningitis which sometimes

follows slight phlegmonous inflammation of the face may, perhaps, often be explained by thrombosis of the orbital veins, involvement of the sinuses, thus the cerebral mischief. Such was probably the process in the case of the lamented Dr. Conant, of this city.

Dr. Knapp adds two cases of embolism of the ciliary arteries. The diagnosis in these cases cannot be regarded as absolute, yet is, at least, probable; in both there was heart-disease—one chronic, the other acute. The former had had embolus of the retinal artery of one eye, and the other eye was attacked with what was considered a similar lesion of the posterior ciliary arteries. This occurred at a time of exacerbation of the heart-trouble; his sight suddenly became darkened, and the cause was infiltration of the retina between the yellow spot and the nerve, as well as of the papilla; the peripheral retina healthy. The patient finally recovered vision one-half.

Three cases are cited: A woman of 22 years, who fell from her chair in syncope, and found herself blind in one eye. No special cause to be assigned for the embolus of the vessels.

Another woman, 72 years old, who became suddenly blind in one eye after taking a warm bath. No other cause than sclerosis of the arteries.

A man, 52, in whom the loss of sight in the eye was not complete, and the retinal vessels were found to be only partially occluded. No opacity of the retina at the macula. Both veins and arteries reduced to half size, and some arteries were mere threads. The nerve hazy, and its outlines indistinct. The sight improved under treatment—v at first=$\frac{1}{5}$, it became $\frac{2}{3}$. At one foot the visual field was only about five inches in diameter, and this did not increase as vision improved.

36.—*A Case of Anisometropia: also the General Theory of this Error of Vision.* By Dr. H. Kaiser, of Dieburg. [Archiv für Ophth., Bd. xiii., 11, s. 352.]

The author, who is 49 years old, and in early life had a slight injury of the left eye, found himself obliged to use glasses to correct presbyopia. In one eye the defect of accommodation called for +15; in the other for +20. The term anisometropia he uses to express the fact of difference of optical value of the two eyes. He ascertained, by careful measurement, the cardinal points of each eye, both in the vertical and horizontal meridians. He gives an extended mathematical discussion of these matters, including the moderate degree of astigmatism which his eyes have. He found that the astigmatism of the lens in some degree corrected the astigmatism of the cornea. He also remarks, that if the astigmatism of the lens and of the cornea should happen to be in the same meridian, or, in other words, their asymmetry should be alike, then, in the act of near vision, the astigmatism would become greater than it is in distant vision (page 361).

He gives this rule for choosing glasses when there is anisometropia: Determine for each eye separately the glass needed for its near point of comfortable vision, then give each eye the same glass, and one which shall be a mean between the two. If the difference be an odd number, take that glass which shall be nearest to the weakest eye.

The author was first impelled to take glasses by presbyopia accompanied by a disposition to converging strabismus. By using, for one eye, the left, +15, and for the other +20, the presbyopia was neutralized, but soon the strabismic tendency recurred. After working a time with these glasses, on taking them off for seeing at a greater yet moderate distance, e. g., for playing billiards, he found himself extremely sensitive to the dissimilarity of the accommodation of his eyes, and the effort of seeing became very painful. He then chose numbers not quite so different, viz., left +16, right +18. By these he was for some time made comfortable; but having

studied more closely the optical theory of the matter, he came to the conclusion that it would be best to use for each eye +17. Immediately on trying this he was perfectly convinced of the correctness of his theory. For near vision, +17 fully answers the purpose; on being taken off, for distant sight, there is no disagreeable sense of strain. His eyes are, under all circumstances, perfectly comfortable. The difference between $\frac{1}{15}$ and $\frac{1}{20}=\frac{1}{60}$; by adding half of this, viz., $\frac{1}{120}$ to $\frac{1}{20}$, we get almost exactly $\frac{1}{17}$.

The principle here enunciated deserves careful consideration, and, if fully borne out by experience, will be a great advance in practical physiological optics.

BLOOD-VESSELS OF EYE AND ORBIT.

37.—*A Summary of the Results of Ligation of the Carotid for Pulsating Tumors of the Orbit.* By Dr. Zehender. [Zehender's Monatsblätter für Aug., 1868, pages 99–119.]

In 1867, two cases of successful treatment of pulsating tumors of the orbit were published in the British journals—one by Mr. Joseph Bell, in the *Edinburgh Medical Journal;* another by Dr. J. Z. Lawrence, in the *Ophthalmic Review.*

Dr. Zehender takes occasion to survey the whole subject, and attempts to report all the cases that have been made known; he gives 31 cases, putting them into a table, with various particulars. He remarks that he was unable to get a paper by Dr. Morton on the same subject, published in the *American Journal of Medical Science,* April, 1865. By comparing the two papers, I find that Dr. Morton gives five cases, of which four are American, and, perhaps, the fifth also, which Dr. Zehender does not possess. I have added a number never yet published, as will be seen below.

In these cases the eyeball always protrudes. Two questions arise in diagnosis—first, as to the nature of the tumor; second, where it is situated. The tumors may be traumatic aneurisms, spontaneous aneurisms of the ophthalmic artery (Guthrie found one in each orbit), arterio-venous aneurisms, cavernous, or erectile or malignant tumors. Some of the cases operated on have been for nævi of the skin, which penetrated into the orbit. The diagnosis of the exact nature of the swelling it is sometimes very difficult to make, at other times it is easy. One remarkable case must not be forgotten, where Mr. Bowman tied the carotid for pulsating exophthalmus, in the firm conviction that the woman had orbital aneurism. She died, and autopsy revealed the arteries to be perfectly healthy; the ophthalmic vein was plugged up as it entered the cavernous sinus. This obstruction was all that could be found to account for the pulsation and protrusion of the globe.

An intracranial lesion may cause the symptoms: cases of this kind, are aneurisms near the sella turcica (*Nunnelly*); a hæmorrhage around the carotid, causing obliteration of the ophthalmic artery and dilation of the vein (*Gendrin*); aneurism of the internal carotid communicating with the cavernous sinus, diagnosticated during life and proven by autopsy (*Nélaton*); carcinomatous tumor at the cavernous sinus (*Nunnelly, Lenoir*).

The diagnosis between intracranial and intraorbital lesion is aided by these considerations—a comparatively good degree of vision, and, perhaps, accompanying paralysis of some muscles, indicate intracranial disease; great injury to sight, absence of paralysis, and detection of a tumor by the finger, bespeak *orbital* trouble.

The cause of pulsating exophthalmus is commonly some injury; sometimes it has occurred during pregnancy, once during delivery. In four cases the disease has affected both sides. One surgeon has done six operations (*Nunnelly*).

Other remedies, besides ligation of the carotid, have been used, but none deserve mention save compression of the artery and injection of stypties into the tumor. Success has followed compression of the carotid by Gioppi, Scaramuzza, and Freeman.

In making up the subjoined tables I have collated those of Zehender and Morton, and have, by the kindness of Dr. J. R. Wood, been able to add some which are related in a paper which he published, giving the "Early History of the Operation of Ligature of the Primitive Carotid Artery, with a report of 48 unpublished cases; and also a Summary of 44 cases, with Remarks by Valentine Mott, M. D. Reprinted from the New York Journal of Medicine for July, 1857."

Among Zehender's cases is one of orbital aneurism, seen, but not operated on, by Mr. Poland, and referred to by him in "Royal London Ophth. Hosp. Reports," vol. ii., page 221. The patient had previously had the *right* carotid tied for aneurism in the *right* orbit. Mr. Poland thinks because this patient was, in many respects, similar to one operated on by Dr. Van Buren, of New York, that in Dr. Van Buren's account of his case the word *left* had been by mistake written for *right*. But I am assured by Dr. V. B. that his patient's *left* carotid was tied for aneurism in the *left* orbit. By pressing on the *left* carotid of Mr. Poland's patient, the pulsation and pain of the tumor ceased; in a few hours he was to ligate this vessel in the same manner that the other had been previously treated, when the patient left the hospital.

By applying personally to Dr. Van Buren and to Dr. Buck, I am able to give the correct account of these cases. Mr. Poland did not see Dr. Van Buren's patient, but he did, no doubt, see Dr. Buck's patient, the history of whose case has never yet been published. I give an abstract of it below, and of one other case hitherto unpublished, operated on by Dr. Halsted.

There is still another American case, published in the *New York Medical Record*, April 15, 1868, vol. iii., No. 52, page 75, in which Dr. Foote, of Cincinnati, tied *both* carotids for an orbital aneurism, making two cases in this country in which this has been done.

Through the kindness of Dr. Poore, I can give the following extracts from the case-books of the New York Hospital:

The first case is Dr. Van Buren's, already published, but which I reproduce in condensed form:

Robert Duggan, æt. 23, May, 1854, by a fall sustained injury of the head, with symptoms of fracture at base of skull; was unconscious, etc., and serum issued from left ear; slight converging strabismus, chiefly of left eye; paralysis of left 7th nerve appeared on the 16th day; on the 22d day exophthalmus took place, and a bruit was heard about the temple—sight almost perfect. On the 26th day the left carotid tied. The ligature dropped 15 days after. On the 18th day *bruit* reappeared, but could be checked by pressure on right carotid. Sometimes bruit would cease spontaneously. In September, 1854, discharged much relieved. A note is appended that, a year and a half afterward, patient was seen and his condition was good.

The following case, in which Dr. Buck tied both carotids, is doubtless the one seen by Mr. Poland:

John Hays, aged 22, sailor, entered New York Hospital in December, 1857. Ten weeks previous had a fall from aloft to the deck, and struck on his feet; was insensible until next day, then found his sight gone. Four weeks after pain commenced at inner angle of *right* eye, with throbbing and whizzing in the ear. At present time has marked exophthalmus, eye displaced outward and downward, veins of upper lid enlarged and tortuous, especially those occupying outer half of lid, pulsation distinct, and ar-

rested by pressure on right carotid, conjunctival and scleral vessels distended, pupil widely dilated and immovable.

December 22, 1857, right common carotid ligated by Dr. Gurdon Buck; tumor did not subside; pulsation less marked. January 1, 1858, oxophthalmus decreasing. February 4th, pulsation increasing, exophthalmus increasing. June 11th, discharged in about the same condition as when admitted.

February, 1859, patient returned to the hospital, having been to sea and made a visit to London, where he had been examined in one of the hospitals. Exophthalmus greater, eye has scarcely any perception of light, lancinating pain in eye, and extending to temple. *Left* common carotid tied by Dr. Buck February 23, 1859. A few minutes after, a thrill still perceptible in tumor, but less distinct. June 15th, protrusion nearly disappeared, has perception of light but not of objects; at times a bruit heard in the tumor. November, 1859, no longer any bruit, tumor all gone, vision nil, pupil enlarged.

In the two cases following the carotid was tied by Dr. Halsted:

Ernst Krause, 37, entered New York Hospital December 10, 1857, having fallen through a hatchway. After eight days complained of noise in left ear, left pupil sluggish, more contracted than right; on the 10th day diplopia, injection of both eyeballs. February 2d, almost two months after injury, ptosis of *right* upper lid. February 8th, *left* eye begins to protrude, chemosis in both eyes, distinct bruit heard on left temple, and all over the head, most decided over left frontal sinus, pulsation detected by pressure on the eyeball. February 14th, left carotid tied; immediately pulsation and bruit ceased, tumor diminished. February 15th, ptosis of right upper lid diminishing, noise in ear gone. On the 20th bruit and noise in ear returned. April 3d, discharged cured, no bruit, sight occasionally dimmed.

Tumor of orbit, ligature of carotid, and afterward extirpation:

Sarah L. Cook, 13, had tumor protruding from outer canthus of left eye, which had been growing for 3½ years, and pushed the eye forward. There was offensive discharge from the nose. The tumor pulsated, and left carotid tied May 11, 1858. The exophthalmus immediately diminished, but soon began to increase, and more rapidly than before. Child entered hospital February 7, 1859. Sight unimpaired until two months before; has now only perception of light, movements of eye perfect, pupil active. February 8th, both eye and tumor removed. On April 2d discharged cured.

The following case of ligature of both carotids, by Dr. Foote, and reported by Dr. Williams, of Cincinnati, seems to have been overlooked by Dr. Zehender, and I condense the account:

Dennis C., 20, seven months before entering hospital, June 15, 1867, had a blow and depression of skull 2½ inches long, from vertex to left frontal boss. Immediately afterward the eyeball protruded. When examined, the external vessels of left eye were very much increased in number and size; pulsation, thrill, and bruit very strong. By ophthalmoscope (and this is the first recorded case thus examined), the retinal vessels seem much enlarged and tortuous, the optic nerve swollen, borders ill defined, gray in color, and speckled by minute ecchymosis. Along the veins some extravasations, the whole appearance that of neuro-retinitis. June 22d, carotid tied, thrill and murmur ceased, but returned in two hours. Vision, which consisted in ability to count fingers at two feet, unaffected. After 30 days, symptoms not being relieved, the other carotid tied; bruit and thrill silenced, but returned in five minutes. After 14 days left eye examined again by ophthalmoscope. Swelling and opacity of papilla nearly gone, retinal exudation and ecchymosis almost gone, vessels of nerve, arteries, and veins

both, instantly and completely emptied by the least possible pressure of the finger. "I could see them grow pale even before I was conscious of making any pressure. Still more interesting was the slowness with which both sets of vessels filled after the pressure was relaxed, and the entire absence of pulsation. This was true of retinal vessels in both eyes." Three weeks after second operation vision improved, bruit very faint. August 21st, discharged cured.

Dr. A. B. Mott gave me the date of his father's case, No. 11, and stated to me the facts respecting his own cases of orbital tumor, where he tied the carotid and extirpated the tumor at one sitting.

The following case was given me by Dr. Frank H. Hamilton, in a brief note:

A child of Mr. Gardener, Clifton, Canada West, aged 2 years. A tumor began to present itself near the outer angle of the right eye about six weeks before I operated, February 12, 1860. The tumor was half the size of a Sicily orange, elastic, pulsating, and to the ear presented a rasping sound at each pulsation. The eye was pushed inward and protruding. Sight of this eye totally lost. Assisted by Dr. Lothrop, of Buffalo, Dr. Newburn, of Canada, and my pupil, Dr. Damainville, I proceeded at once, having placed the child under the influence of chloroform, to tie the carotid.

After the application of the ligature, the pulsation in the tumor and rasping sound ceased, and its size was sensibly diminished.

I learned, subsequently, that, after a short time, the ligature came away, but that the progress of the tumor was only temporarily delayed, and that the child finally died of what proved to be a vascular malignant growth.

NO.	COUNTRY.	DATE.	OPERATOR.	RESULT.	DISEASE.
1	England.....	1809	Travers	Success	Aneurism by anastomosis.
2	England.....	1813	Dalrymple..	Success	Do. do.
3	France.....	1829	Roux	Partial success.	Aneurism by anastomosis; pain and exophthalmus.
4	United States.	1829	Warren	Success	Spontaneous aneurism.
5	United States.	1829	Warren	Failure.	Traumatic aneurism.
6	England.....	1834	Scott.....	Success	Do. do.
7	England.....	1836	Busk.....	Success	Traumatic aneurism; years after confirmed by autopsy.
8	United States.	1839	Dudley.....	Success	Spontaneous aneurism.
9	France.....	1839	Jobert	Success	Traumatic aneurism.
10	France.....	1839	Velpeau....	Partial success.	Do. do.
11	United States.	1842	Wood.....	Success	Aneurism by anastomosis—infant.
12	France.....	1844	Herpin.....	Success	Spontaneous aneurism.
13	France.....	1845	Petrequin..	Death..	Pulsating tumor, not cured by ligature nor by electro punctures.
14	England.....	1851	Walton	Success	Aneurism by anastomosis—infant 5 months.
15	England.....	1851	Brainard...	Failure.	Traumatic aneurism; tumor cured afterward by injection and two introductions of a hot needle.
16	England.....	1852	Nunnelly...	Success	Traumatic aneurism.
17	United States.	1852	V. Mott....	Success	Aneurism by anastomosis—infant.
18	United States.	1854	Van Buren.	Success	Traumatic aneurism.
19	England....	1854	Curling....	Success	Do. do.
20	United States.	1854	A. B. Mott.	Success	Malignant disease in orbit; several extirpations of mass been followed by relapses; the last removal of tumor, combined with ligature of carotid, prevented recurrence for 14 years (verbal statement).
21	United States.	1855	Coe	Success	Traumatic aneurism.
22	England.....	1856	Nunnelly...	Success	Spontaneous aneurism; slow during pregnancy.
23	United States.	1857	Van Buren.	Death 13th day	Encephaloid cancer; death from pyæmia.
24	United States.	1857	Woodward.	Death 8th w'k.	Cancerous tumor of orbit and brain.
25	England.....	1858	Nunnelly...	Death..	Spontaneous aneurism; man 65 years old; cerebral arteries atheromatous.

NO.	COUNTRY.	DATE.	OPERATOR.	RESULT.	DISEASE.
26	England.....	1858	Bowman ...	Death 8th day.	Injury, no aneurism, but ophthalmic vein occluded as it entered the cavernous sinus.
27	England.....	1859	Nunnelly...	Success	Spontaneous aneurism; occurred during pregnancy.
28	England.....	1860	Bowman ...	Success	Spontaneous aneurism.
29	England.....	1860	Syme	Success	Do. do.
30	England.....	1861	Hart	Success	Traumatic aneurism.
31	England.....	1862	Greig	Success	Do. do.
32	England.....	1863	Nunnelly...	Success	Do. do.
33	England.....	1863	Nunnelly...	Partial success.	Death 1½ years after; multiple cancerous tumors.
34	United States.	1864	Morton.....	Success	Spontaneous aneurism; sudden during pregnancy.
35	Poland.....	1864	Szokalski ..	Success	Traumatic aneurism; pressure tried in vain.
36	France.....	1864	Legouest...	Success	Traumatic aneurism; 6th nerve paralyzed.
37	United States.	1864	A. B. Mott.	Success	Cancerous tumor extirpated and carotid tied; no relapse after 1½ years.
38	France.....	1867?	Lenoir.....	Failure.	Encephaloid tumors; death after 9 months.
39	England.....	1867	Lawrence ..	Success	Traumatic aneurism.
40	England.....	1867	Bell.....	Success	Do. do.
41	United States.	1857 1859	Buck.....	Success	Traumatic aneurism; both carotids tied at interval of 2 years; case seen by Mr. Poland.
42	United States.	1857	Halsted.....	Success	Traumatic aneurism.
43	United States.	1858	Halsted.....	Failure.	Tumor in orbit; after 9 months eye and tumor removed; no immediate return.
44	United States.	1867	Foote	Success	Both carotids tied at interval of 30 days.
45	United States.	1860	Hamilton ..	Failure	Traumatic aneurism; death some time after.

Table of Results of Ligature of Carotid for Orbital Disease.

DISEASE.	SUCCESS.	PARTIAL SUCCESS.	FAILURE.	DEATH.	TOTAL.
Aneurism spontaneous	8	0	0	1	9
Aneurism traumatic	17	1	2	0	20
Aneurism by anastomosis	5	1	0	0	6
Solid tumors, "malignant" and others	2	0	2	5	9
Occlusion of ophthalmic vein and of cavernous sinus	..	..	..	1	1
Total	32	2	4	7	45

The two successful cases of malignant orbital tumors were treated both by extirpation and ligature of the carotid.

Orbital Aneurisms treated by Injection.

NO.	COUNTRY.	DATE.	OPERATOR.	SUBSTANCE.	RESULT.
1	England.....	.....	Brainard	Ferri lactatis	Success.
2	France.....	.....	Bourguet.....	Ferri perchloridi.....	Do.
3	France.....	.....	Desouveaux.....	Ferri sesqui chloridi.....	Do.
4	France.....	.....	Wecker.....	Ditto.....	Do.
5	England.....	1858	Walton	Tannin.....	Do.
6	England.....	1858	Taylor	Tannin.....	Do.

Orbital Aneurism treated by Compression.

NO.	COUNTRY.	DATE.	OPERATOR.	DURATION OF PRESSURE.	RESULT.
1	Italy	1858	Gioppi.....	Four days intermittingly	Success.
2	Italy	1858	Scaramuzza	Eighteen days intermittingly (7 hrs. 20 min.)..	Do.
3	Canada....	1861	Freeman ...	Pressure on tumor, cold lotions and digitalis, several weeks	Do.
4	England....	.....	Baum.....	Twice daily for 5 minutes during 10 days.....	Failure.
5	England....	.....	Hart.....	Several hours daily for 3 weeks.....	Do.
6	England....	.....	Nunnelly...	"For a very long period ".....	Do.
7	France.....	.....	Legouest...	During 4 days	Do.
8	Poland.....	.....	Szokalski ..	Fifty-six hours	Do.

In all cases but one the pressure was on the artery in the neck. It is probable this sort of treatment has been tried without success in many other instances.

The following treatises and articles are not included in the above summary:

Leçons sur la Cataracte par Ém. Foucher, pages 280. Paris, 1868.

Des Méthodes d'Extraction de la Cataracte et de l'Extraction semi-elliptique, nouveau procedé par L. de Lucé (de Vire), pages 54. Paris, 1868.

Phakologische Studien—eine Streitschrift über die Staar Operationen—von Prof. von Hamer. Prag, 1868.

Der Mechanismus der Accommodation des Menschlichen Auges nach Beobachtungen im Leben—dargestellt von Dr. E. A. Coccius, pp. 153. Leipzig, 1868.

Experimental Untersuchungen über der Mechanismus der Accommodation von V. Hensen und C. Voelckers, pp. 60. Kiel, 1868.

Die Theorie der Augenfehler und der Brille von Dr. Hermann Scheffler, pp. 191. Wien, 1868.

Du Strabisme dans ses Applications à la Physiologie de la Vision, par Émile Javal, pp. 77. Paris, 1868.

Uber den Mechanismus der Accommodation des Menschlichen Auges von Dr. Albert Schumann, pp. 24. Dresden, 1868.

Ophthalmologisches aus dem Jahre, 1867, von Dr. F. Heymann, pp. 52. Leipzig, 1868.

Retinitis Nyctalopica, by Dr. Arlt, of Vienna—a translation of an article in the "Bericht über die Augenklinik," by Dr. J. F. Weightman, of Philadelphia.

Atropia; its Chemical, Physiological, and Therapeutic Action, together with Experiments instituted to ascertain its Toxicological Properties. By Samuel R. Percy, M. D., pp. 47. New York, 1868.

Enucleation of the Eyeball—Section of the Ciliary Nerves and Optic Nerve. Some Unnecessary Causes of Impaired Vision. By B. Joy Jeffries, M. D. Boston, 1868.

Du Diagnostic des Maladies des yeux par la Chromatoscopie retinienne, précedé d'une étude sur les lois physiques et physiologiques des Couleurs, par X. Galezowski, pp. 266. Paris, 1868.

Handbuch der Augenheilkunde für praktische Ärzte von Dr. J. Rheindorf, pp. 232. Leipzig und Heidelberg, 1868.

Gesammelte Abhandlungen über Physiologische optik, von Dr. A. Classen, pp. 175. Berlin, 1868.

Der intraoculare Druck und die Innervations verhältnisse der Iris, von augenärztliche standpunkte aus betrachtet, von Prof. Stellwag, pp. 100. Wien, 1868.

Archiv für Ophthalmologie—General Register zu Band 1–10, bearbeitet von Dr. L. Wurm, pp. 67. Breslau, 1868.

Lehrbuch der Ophthalmoscopie, von Dr. Ludwig Mauthner, pp. 468. Wien, 1868.

A Treatise on the Diseases of the Eye, by Soelberg Wells. London and Philadelphia, 1869, pp. 741.

Miscellaneous and Scientific Notes.

The connection of the undersigned with the New York Medical Journal has been but nominal for the last two years. With this number it ceases altogether.

The Journal will continue under the editorial supervision of Dr. E. S. Dunster.

WILLIAM A. HAMMOND.

We ask the especial attention of our readers to the publishers' announcement of a reduction in the subscription price of the *Journal*. A large proportion of our subscriptions expire with the present number, and our patrons, in remitting for the coming year, will please bear in mind this reduction.

The success of the *Journal* since it passed into the hands of the present publishers has been, in the highest degree, substantial and encouraging. We congratulate our subscribers on this success, which inures entirely to their advantage, as we are authorized to state that still further improvements will be made just in proportion to the endorsement and support received from the profession; with this assurance from the publishers, we the more willingly call upon our subscribers to aid in extending the circulation, and thus assist in improving the character of the *Journal*.

We desire to direct the attention of our readers to the report on Ophthalmology, by Professor Noyes, completed in this number of the Journal. The report is the most complete one ever issued in the English language, covering the same length of time—one year. The general practitioner will find in it much that he may avail himself of in his every-day work, while its value to those engaged in this special study can hardly be overestimated. We would especially call the attention of surgeons to that part of the report on ligature of the carotid. More cases are here brought together by Dr. Noyes than have previously been collected in any single paper. The labor involved in the preparation of a report of this magnitude and character is simply immense, as any one can testify who has ever undertaken such work, and we have reason to congratulate our readers that the author has been willing so freely to give the results of that labor to the profession.

Our readers will observe that we have opened up a new department of the Journal, in which, under the head "Clinical Records from Private Practice," we have grouped together a number of interesting cases. It seems to us that the coöperation of our readers is only needed to make this a most valuble feature in the Journal. We shall be pleased to receive contributions for this department, and venture to suggest, to those who may so favor us, three important points :

1. Make the narrative as brief as possible consistent with an intelligent appreciation of the case.

2. Do not fail to give an outline of the treatment pursued.

3. Always state the result, waiting a sufficient time, in case of recovery, to justify that statement.

The State Medical Society.—We had the satisfaction of being present, merely as a looker-on, at the recent meeting of this Society in Albany, and are pleased to record the fact that the session was one of especial interest and instruction. If any one thing struck us more prominently than another, it was the entire harmony of the proceedings, and the evident interest manifested by all. It speaks well for our profession that a three days' session should have left, so far as we were able to learn, from careful inquiry and observation, such kindly and pleasing impressions upon all who were present, either as spectators or delegates. The papers presented were of a high order, and were received with satisfaction. We should be pleased to give our readers a report of the proceedings, but the scope of this Journal hardly warrants it. Those who are interested, however, will find a full report in the columns of our enterprising contemporary, the *Medical Record*, of this city. In the election of Professor White, of Buffalo, as President, and Dr. Burr, of Binghamton, as Vice-President, for the ensuing year, we have every guarantee that the interests of the Society will not suffer, and that we shall have a continuance of that hearty good-will and fellowship so manifested this year. We need scarcely add that the efficient Treasurer and Secretary were reëlected. A pleasing incident of the meeting was the announcement, by the venerable Dr. Corliss, of his intention to offer a prize for an Essay

on Tubercular Consumption. The cash prize this year was awarded to Dr. J. C. Hutchinson, of Brooklyn, for his Essay on Acupuncture. We are glad to note that New York City was so well represented at this meeting, but it seems to us hardly just that with her large number of delegates, and with the many who are now and have been for years eligible for permanent membership, she should have no larger representation than a country district which some years will not furnish enough delegates to fill the two vacancies to which each district is annually entitled. We hope to see some revision made of this disproportionate representation. Drs. James L. Banks and J. R. Van Kleek were elected permanent members from this district.

The American Medical Association.—We are informed by Dr. James P. Hibberd, of Richmond, Indiana, that a first-class steamer will be chartered to carry delegates from Cairo to New Orleans and return, fare not to exceed $35.00, including meals and state-room. The time from Cairo will be about four days. Should the delegates from the North select this route, the trip will undoubtedly be both pleasant and profitable. Further particulars will be published as soon as the arrangements are completed.

The Long Island Medical College Hospital.—A new and beautiful little hospital, capable of accommodating about forty patients, has just been added to this institution, while the entire college buildings have been put in perfect repair, adding vastly to the comfort both of the students and the teachers. The hospital, with the Dispensary attached to the institution, affords a large amount of clinical material for class-demonstrations. The lectures open on the 1st of this month.

Hindoo Remedy for Elephantiasis. By Charles A. Hart, M. D., New York City.

In a book upon Asiatic history and sciences, published in London in 1792, and principally compiled by Sir William Jones, occurs a translation of an article furnished by a Delhi native physician, and prefixed by a short notice of the disease by the translator.

The elephantiasis nigrum, the *judhám* of the Arabs, is so

well known, at least by pathological description, to all physicians, that I pass over these remarks of the translator, more interesting to antiquarians than to physicians, and condense the account of the Hindoo physician, At'har Ali Khan, of Delhi, himself. Given to him by a friend, who apparently learned it on a journey to Lucknow in 1783, it is represented as a secret of the Hindoo physicians, who applied it to the cure of "*palsy*, *distortions of the face*, *relaxations of the nerves*," and especially to the *judhám* (elephantiasis nigrum), and the Persian fire (lues venerea). The receipt is as follows:

"Take of white arsenic, fine and fresh, one *tólá*; of picked black pepper, six times as much; let both be well beaten at intervals for four days successively in an iron mortar, and then reduce to an impalpable powder in one of stone, with a stone pestle, and completely levigated, a little water being mixed with them; make pills of them as large as *tares*, or small pulse, and keep them dry in a shady place.

"One of these pills must be swallowed morning and evening with some *betel-leaf*, or, in countries where betel is not at hand, with cold water; if the body be cleansed from foulness and obstructions by gentle cathartics and bleeding before the medicine is administered, the remedy will be speedier." By a foot-note of the translator, we find the tólá to consist of 105 grs. Considering the size of the tares or pulse as about that of a two gr. pill, the dose would consist of *two-sevenths of a grain of arsenic*, *and one and five-sevenths of black pepper*.

At'har Ali goes on to say that, "conformable to the directions of his learned friend, he prepared the medicine; and in the same year gave it to numbers who were reduced by the disease above mentioned to the point of death; God is his witness, that they grew better day by day, were at last completely cured, and are now living (except one or two who died of their disorders), to attest the truth of this assertion. One of the first patients was a *Parsee*, named *Meméchehr*, who had come from *Surat* to this city, and had fixed his abode near the writer's house; he was so cruelly afflicted with the confirmed lues, here called the *Persian fire*, that his hands and feet were entirely ulcerated, and almost corroded, so that he became an object of disgust and abhorrence. This man consulted the writer on his case, the state of which he disclosed without reserve; some blood was taken from him on the same day, and a cathartic administered on the next. On the third day he began to take the arsenic pills, and, by the blessing of God, the virulence of his disorder abated by degrees, until signs of returning health appeared; in a fortnight his recovery was complete, and he was bathed according to the prac-

tice of our physicians. He seemed to have no virus left in his blood, and none has since been perceived in him."

"But the power of this medicine has chiefly been tried in the cure of the *judhám*, as the word is pronounced in India, a disorder infecting the whole mass of blood, and thence called by some *fisádi khún*. The former name is derived from an *Arabic* root, signifying in *general* amputations, *maiming*, *excision*, and particularly the *truncation* or *erosion of the fingers*, which happens in the last stage of the disease. It is extremely contagious, and for that reason the Prophet said, '*Ferrú miná lungdhúmi camá teferrú miná l áfad*,' or 'Flee from a person afflicted with the *judhám*, as you would flee from a lion.' It is *hereditary*, and in that respect is classed by medical writers with the *gout*, the *consumption*, and the *white leprosy*.

"In February, in the year just mentioned, one *Shaikh Ramazá ní*, who then was an upper servant to the board of revenue, had so corrupt a mass of blood that a black leprosy of his joints was approaching, and most of his limbs began to be ulcerated; and in this condition he applied to the writer, and requested immediate assistance. Though the disordered state of his blood was evident on inspection, and required no particular declaration of it, yet many questions were put to him, and it was clear from his answers that he was a confirmed *judhám:* he then lost a good deal of blood, and, after due preparation, took the arsenic pills. After the first week his malady seemed alleviated; in the second, it was considerably diminished; and, in the third, so entirely removed that the patient went into the Bath of Health, as a token that he no longer needed a physician."

It is noticeable that the good effects of this medicine were as marked in the cases of syphilis as in those of elephantiasis, insomuch that this and the apparent resemblance of the last stages of the former to the symptoms of the latter led this Hindoo physician to imagine that syphilis often ended in elephantiasis. This, of course, we know is a mistake, and that mercury, while curing syphilis, only inflames elephantiasis, but arsenic has been used for the former, and with beneficial results; so that there can be no reason to doubt its effects when applied to the latter, for which it was more particularly meant. The rapidity of the cures is in a great degree owing to the spare habit and healthy constitution common to almost all the natives of the East, whose food is to so great an extent vegetable; as was noticed in Algiers by the French physicians in suppurating wounds on native subjects. This drug has never had a fair trial by either European or American sur-

geons, and in my estimation it is not *less* likely to succeed than the present fashionable method of ligating arteries for the cure of elephantiasis, a method of treatment which recent experiences have shown to be largely unreliable.

Carbolate of Lime and Scarlatina.—The best authorities do not consider scarlatina to be contagious. It undoubtedly spreads through some epidemic influence independent of contagion. It is possible that, under some extreme conditions of filth and want of ventilation, it may be infectious, and particularly when accompanied by putrid sore throat. There is no certain preventive known. Belladonna has had this reputation to some extent; but extensive experiments show that it cannot be relied upon. He who shall discover the real cause of scarlatina, and a certain preventive for it, will merit honor and reputation certainly equal to that of the discoverer of vaccination.

There is a tendency among physicians, at the present time, to attribute many diseases to microscopic spores, or fungi, or animalculæ. There may be some reason for this, though, when these are found in the blood and secretions, it is by no means certain whether they are the cause or the effect of the disease. However this may be, it is certain that *carbolic acid* will surely destroy all microscopic life, whether animal or vegetable. If, then, the cause of scarlatina is any thing of this description, and there is some reason to think it may be, carbolic acid may be useful as a preventive. I have recommended it, and it has been used to some extent in this city with *apparently* good results in the disinfection of rooms where scarlatina exists, and in preventing the spread of the disease. But I am well aware of the universal tendency, in relation to these subjects, to jump at conclusions, and accept them as true upon entirely insufficient evidence. I would not, therefore, offer the experiments already tried as of any value as proof; but would recommend a trial of carbolic acid as a preventive of scarlatina on account of its well-known properties of destroying all microscopical animal and vegetable life. It is not simply a deodorizer, but is an actual disinfectant, or destroyer of infection, and is the best disinfectant known for general use.

The best and only preparation of it, for general practical use in the sick-room, is the *carbolate of lime*, which is a dry powder of a bright-pink color. Let this be kept exposed in the rooms where children are sick, and in other rooms if desired, in small quantities, just sufficient to make the coal-tar odor perceptible at all times. A more agreeable odor of the acid may

be made by pouring the solution of the pure acid on dry slaked lime, but this would be much more expensive and probably not as efficient.

The carbolate of lime has been used, in the same manner, quite extensively, to mitigate the severity of the spasms in whooping-cough, and I think the evidence is sufficient to show that it has been found useful for this purpose.—*From January Report of Dr. E. M. Snow, Supt. of Health, etc., Providence, R. I.*

Dr. Dunlap, of Ohio, the well-known ovariotomist, has recently performed his fortieth operation. Nine of his cases have been unsuccessful, but in two out of the nine death was accidental, being due respectively to chloroform and to an overdose of morphine. Excluding these two cases, the mortality is about 17 per cent.

Of the last twenty-three cases of ovariotomy by Dr. Thomas Keith, of Edinburgh, all except one are now alive and well. The last operation was done in December, 1868.

Mr. Spencer Wells, in a note to the *Boston Medical and Surgical Journal*, gives the following statistics of his operations for ovariotomy:

1st	100	cases,	66	recovered, and	34	died.
2d	100	"	72	"	28	"
3d	100	"	77	"	23	"
	300		215		85	

A general mortality of 28 per cent.

M. Kœberlé furnishes (*Gazette Hebdomadaire*, August 7th) an elaborate account, accompanied by several tables, of the ovariotomy operations he has performed. Commencing in 1862, he has, to the present time, performed the operation sixty-nine times. All these cases he has published, whatever their results, as they have arisen, and this is the general recapitulation of the whole: As the general result, the recoveries amount to two-thirds, but in the last twenty-two cases there have been only five deaths. During the first year there were 6 cases, with 1 death; in the second, 4 cases and 2 deaths; in the third, 8 cases and 2 deaths; in the fourth, 9 cases and 4 deaths; in the fifth, 19 cases and 9 deaths; and in the sixth, 23 cases and 6 deaths—total, 69 cases and 24 deaths. The results as regards the most serious cases, owing to the improve-

ments which have been effected in the operation, have greatly improved during the later years. Thus, while during 1867–'68, of 11 *cas graves* 6 recovered, in 12 *cas graves* occurring during the preceding two years only 2 recoveries took place.—*Med. Times and Gazette, Aug.* 22, 1868.

The Effect on Man of a Residence at Great Heights above the Level of the Sea.—At the meeting of October 26, 1868, of the Boston Society for Medical Improvement, Prof. Robert von Schlagintweit, of Giessen, in Hessen, was present by invitation, and made an interesting address on this subject—the abstract of which we copy from the *Boston Medical and Surgical Journal:* "There is a height above which human life is impossible; in a balloon Mr. Glaisher fainted when 32,000 feet above the level of the sea; probably no man could live at an elevation greater than 34,000 to 36,000 feet; this will, however, depend much on the state of the atmosphere, the idiosyncrasies of individuals, and the habit of living in high places. The professor himself, on first reaching an elevavation of 17,000 to 18,000 feet, felt great inconvenience and distress, but at another visit was not much affected. People living at a moderate elevation, on going higher suffer full as much as the unaccustomed traveller. In 'High Asia' the effects of elevation are shown by headache, hæmoptysis, dyspnœa, anorexia, muscular debility, and low spirits, all increased at night, and at times every one gasps for air, apparently in vain; moments occur when every one believes that he must inevitably be suffocated. In the day-time epistaxis may occur, but if the nose is not too much irritated it seldom occurs. He had never seen bleeding from the eyes, lips, or ears. All these symptoms disappear as soon as one begins to descend. In the Andes it is said besides these symptoms are also intense headache, swoons, bleeding from the nose, lips, gums, and eyelids, especially the tunica conjunctiva. The height at which these symptoms come on among the Andes is not nearly so great as in High Asia; in the latter country being not below 16,500 feet, while in the Andes the effect of height has been repeatedly felt as low as 10,700 feet, lower than anywhere else. No satisfactory explanation of this fact has yet been given; Prof. S. thought it might be owing to the different geological construction, but the existence of volcanoes in the Andes would not wholly account for the difference. In balloons, symptoms do not come on till a much greater height is reached, bodily exertion rendering one much more likely to suffer; in a balloon the passengers keep perfectly still, any

exertion at a great height causing intense depression and greatly heightening the pulse. Cold does not increase the intensity of the suffering, but wind decidedly. One could stay for days at heights of 16,500 feet and not suffer during the first portion, but at evening a breeze usually sprung up, rendering every one sick; in the morning the appetite came back and the bad symptoms were gone. The effect of great heights is influenced by the state of the atmosphere (which is always better in the morning than in the evening), the existence of wind, or clouds, or electricity. There is a great decrease in the atmospheric pressure, the barometer at the height of 22,259 feet showing only $13\frac{3}{10}$ inches. In High Asia, at a height of 18,600 or 18,800 feet, the atmospheric pressure is one-half of that at the level of the sea. These symptoms, which all are liable to in great heights, prevent human beings from living there, even if all conditions are at hand for their thriving well.

In none of the pastures in Thibet is the height greater than 16,320 feet, and they are only used in certain portions of the year. A French author, Paul de Carmoy, has described a village in the Peruvian Andes, named Pueblo de Ocoruro, at a height of 18,454 feet, whose inhabitants spend all the year there, but from his own experience Prof. von Schlagintweit thinks this impossible; Carmoy's statement rests either on an erroneous observation or on a wrong measurement; he has probably mistaken a transitory settlement, only inhabited for a few days, for a permanent abode.

Dr. Parks said some years ago he ascended Monte Rosa, and when near the summit, in the midst of a flurry of wind and snow, had an attack of dyspnœa and other disagreeable feelings, which all passed away on reaching the summit.

Prof. von Schlagintweit said these symptoms were not usually felt on the Alps, which were only on the confines of the elevation at which these symptoms were likely to occur. They might be felt in an exceptional case, in a storm, as in Dr. Parks's experience, or by people of highly nervous temperaments.

Why should this influence show itself at so much lower an elevation among the Andes than in the Alps or elsewhere? Whole villages live in Asia at the height of 10,500 feet above the level of the sea. The inhabitants are robust, with well-developed chests; their stature is somewhat less than that of Europeans or Americans, but their strength is enormous, that of the women as well as the men. The diet varies with the race, some living on vegetable, some on animal food alone. The Hindoos live principally on rice; they also make use of an intoxicating liquor made of millet.

Animal traces are found at very great heights; the yak (*Bos grunniens*) at 19,400 feet, wild horse (*Kyang*) and several species of wild sheep and ibex at 18,600, but very few birds.

As to the diseases: in Thibet we find goitre but seldom, while it is common in some Himalaya valleys; rheumatism is very common, as is also constipation; small-pox causes fearful ravages in Thibet; no apoplexy; no phthisis, but, on the contrary, consumptives find great relief in these high altitudes. Prof. von Schlagintweit anticipates happy results from the study of the hygiene of high regions.

Infanticide.—The distinction between the evidence required, where secret disposition of the dead body of an illegitimate child and murder are alleged, has been illustrated in a late trial in one of the English courts.

A girl had been delivered of a child, of which she had certainly disposed. But no body could be found which could be identified as that of her child. Although the proof of delivery was complete and indisputable, the accused was acquitted. The judge remarked, that, had the girl been charged with murder, she could have been convicted on the evidence adduced.

From the Annual Report of the Surgeon-General, U. S. Army, for 1868, it appears that, for the *white troops*, the total number of cases of all kinds reported under treatment was one hundred and thirty-one thousand five hundred and eighty-one (131,581), or two thousand nine hundred and eight (2,908) per thousand (1,000) of strength—nearly three entries on the sick report during the year for each man. Of this number of cases, one hundred and eighteen thousand nine hundred and twenty-five (118,925) were for disease alone, and twelve thousand six hundred and fifty-six (12,656) for wounds, accidents, and injuries; being two thousand six hundred and twenty-eight (2,628) per thousand (1,000) of strength for disease, and two hundred and eighty (280) per thousand (1,000) of strength for wounds, accidents, and injuries. The average number constantly on sick report was two thousand eight hundred and fifty-two (2,852), of whom two thousand five hundred and ten (2,510) were sick and three hundred and forty-two (342) wounded, or fifty-five (55) per thousand (1,000) constantly under treatment for disease, and eight (8) per thousand (1,000) for wounds and injuries. The total number of deaths, from all causes reported, was one thousand three hundred and fifty-three (1,353); of which, one thousand one hundred and seventy-five (1,175) were from disease, and one hundred and sev-

enty-eight (178) for wounds, accidents, and injuries; being at the rate of twenty-six (26) deaths from disease and four (4) from wounds to each thousand (1,000) of strength. Of the deaths from disease, four hundred and twenty-seven (427) were from yellow fever, one hundred and thirty-nine (139) from cholera, and six hundred and nine (609), or thirteen (13) deaths per thousand (1,000) of strength, from all other diseases. The proportion of deaths from all causes to cases treated was one (1) death to ninety-seven (97) cases.

Nine hundred and eighty-four (984) white soldiers, or twenty-two (22) per thousand (1,000) of strength, were discharged upon surgeons' certificate of disability.

A New Test for distinguishing Blood-stains.—This test, devised by Dr. Day, of Geelong, Australia, consists in treating a watery solution of the suspected substance with a little tincture of guiacum, and, afterward, with a drop of the ethereal tincture of peroxide of hydrogen—the result is a strong blue color, which indicates the presence of blood. The test is regarded as valuable in cases where the blood-globules have become so altered by long drying, or by soaking in water, that they can no longer be recognized by the microscope. As, however, it cannot decide, even negatively, the question, whether the blood be human or not, it cannot have much weight in medico-legal examinations. The test and its mode of operation are described as follows:

If a drop of blood be mixed with half an ounce of distilled water, and a drop or two of guiacum be added, a cloudy precipitate of the resin is thrown down; and the solution has a faint tint, due to the quantity of the tincture used. If now a drop of an ethereal solution of peroxide of hydrogen be added, a blue tint will appear, which will gradually deepen and spread after a few minutes' exposure to the air. This test acts better when very small quantities of blood are used; as otherwise, if the blood is in excess, the solution is red, and gives, with antozone, a purplish or dirty-green color. So minute and delicate is the reaction, that, in a case where the microscope failed to identify any blood from a stain in a man's trousers, Dr. Day succeeded in obtaining sixty impressions.

Water has the effect of destroying the shape of the blood-corpuscle, and so it cannot sometimes be recognized by the microscope; but, it in no way interferes with this new chemical test. Its accuracy may be thus shown. A piece of linen was stained with blood in the year 1840 (Guy's *Forensic Medicine*, 3d ed., p. 316); from this a fibre was taken, containing at its extremity a most minute stain of blood; this was placed on a white slab, and treated first with a drop of tincture of guiacum, and then with a drop of "ozonized ether;" and, although the quantity was so small, and no less than twenty-eight years old, the characteristic blue appeared at once. We have found the same result in blood obtained from the urine in a case of hæmaturia, and also in blood drawn from different animals. Dr. Taylor, in the *Guy's Hospital Reports*, has shown that red coloring matters, cochineal, kino, catechu, carmine, etc., exert no such in-

fluence; and, as far as it is at present known, no other red stain will produce this result.

Black currants will cause a stain resembling that of blood more than any other; but antozone has no effect upon it.

Ink-stains will cause a blue with guiacum; so will rust-stains produced by citric or acetic acid on iron; but then, *no* "ozonized ether" need be used, and this at once distinguishes such stains from blood. "Ozonized ether" is a wrong term to use; for it contains antozone, and not ozone; and to this is due its reaction. Ether which contained an ozonide would blue guiacum resin, whether blood were present or not. The test-solution is the ethereal solution of peroxide of hydrogen, which is an antozonide.

The so-called "ozonized essential oils," as oil of turpentine, lavender, etc., really contain antozone; and to this may be ascribed their use in detecting blood; for at first oil of turpentine was used, instead of the peroxide of hydrogen; but the results were unsatisfactory.

If the blood-stain be on dark cloth, the test, as above described, may be used; but then an impression must be taken off on white blotting-paper, otherwise, the blue color will not be visible.

The exact nature of the chemical change that takes place is doubtful; but the test is so simple and easy of application, and, above all, so very delicate, that it is likely to become very generally used. This test fails, as other tests have failed before, to show whether the blood-stain is human or not. The microscope will point out whether a corpuscle comes from a fish, a reptile, or a mammal; but we do not think any microscopist would rely on the mere size of the corpuscle to say whether a cell came from one class of mammals or another, seeing that slight differences in the density of the fluid considerably alter the shape of the corpuscle.—*British Med. Jour.*

In the *Cincinnati Lancet and Observer* for December, 1868, Dr. J. I. Rooker, of Castleton, Indiana, publishes a supplementary account of the condition of a patient on whom he performed the operation of castration, in 1861, for the cure of epilepsy. It will be remembered that Dr. Rooker was most severely criticised for the performance of this operation. The patient was a confirmed victim to the vice of masturbation, and every repetition of the act produced an epileptic fit. The case proving intractable to all ordinary treatment, and being apparently hopeless, both testicles were removed by Dr. Rooker. Since that time (spring of 1861), there has been but one epileptic fit, which occurred on the day following the operation.

Recently, Dr. Rooker saw this patient, and had him examined by a number of other physicians.

"He stated that previous to the operation 'he was not able to do a day's work, owing to general debility and loss of mind;' that he had had attacks of epilepsy almost every day

for the past eight years. In short, he was an object of charity. At present, with the exception of the 'chills,' was in the enjoyment of excellent health; that his weight had increased from one hundred and twenty to one hundred and sixty pounds, and that the 'nervousness' had all left him; and that he could do as much labor as any man. Is able to read and write; stated that he had but little 'passion left for the women.' I could not detect any change in his voice. He is glad the operation was performed. His intellect appears as good as any of his class."

Dr. Rooker adds that, while he is no advocate for an indiscriminate resort to this operation, he is fully satisfied with the results in this case.

Deaths from Chloroform.—A case of death by chloroform is reported (*Brit. Med. Jour.*, Dec. 19, 1868), in London. A druggist, who had been in the habit of taking chloroform to relieve pain in the face, was found dead in the evening with a handkerchief in his right hand and an empty phial, which had contained chloroform.

Another case of similar kind is recorded by Dr. W. B. Slayter, in the *Provincial Medical Journal* (Nov., 1868). A delicate man was found dead in his bed, with a bottle of chloroform lying beside him. From the evidence at the inquest, it appeared that he had been in the habit of inhaling chloroform from time to time to relieve the paroxysms of asthma. On the night of his death he took a little over an ounce.

The same physician records, in the same journal, still another case, which occurred in the Provincial and City Hospital. A man, aged 40, was placed under the influence of chloroform for amputation of thigh owing to inflammation of knee-joint. Chloroform was administered in the usual way on a towel. In a few minutes the patient was fully under its influence, breathing good, pulse strong. The limb was then amputated about the middle third of the thigh, the arteries were tied without delay, and about the usual quantity of blood was lost. Immediately the leg was off, the chloroform was discontinued; at that time the patient was breathing naturally, and the pulse was very good. About three or four minutes after this, the teeth became firmly clinched, respirations stertorous and gasping, pulse very small, and skin covered with a clammy perspiration. The jaws were immediately forced open and the tongue drawn forward; artificial respiration, stimulants, and other remedies were applied, but in vain. The patient died about ten minutes after the first alarming symp

toms set in. On examining the diseased joint the synovial membrane was found to be converted into a gelatinous mass; the cartilage covering the inner condyle was perfectly sound, that covering the outer condyle, the heads of the tibia and fibula, was completely destroyed and the bones roughened. *Post-mortem* examination about thirty hours after death. The heart-substance, valves, and aorta, were perfectly healthy, cavities quite empty. The lungs, stomach, spleen, intestines, and kidneys were all healthy, but quite pale from want of blood. The brain was quite pale, and its blood-vessels empty. —*Medical News and Library.*

At Wrexham a coroner's inquest has shown that death occurred from this agent, properly administered by a qualified man for an operation for fistula.

At Leicester, Mrs. Adams, thirty-three years of age, died from the effects of chloroform given for the operation of extracting the stumps of several teeth. The evidence at the inquest showed that every precaution was used, her own attendant and another medical man being present. The jury found that deceased died from chloroform, "in reference to which more than usual precaution had been taken." These cases show the dangers that exist even in the hands of skilled persons. A forcible example of the folly of those who are not qualified being intrusted with such an agent is seen in the sad case of the Hon. and Rev. Arthur Sugden, who died from taking chloroform by the stomach in mistake. The deceased gentleman kept the drug by him, and was in the habit of inhaling it sometimes to relieve neuralgia, from which he suffered. On one occasion he seems to have swallowed a large dose in mistake for something more innocent. It appeared the late gentleman had also "a *diseased* heart"—a fact that would render his *inhaling* chloroform more than usually hazardous, and be an additional reason why he ought not to have been intrusted with the drug.—*Medical Press and Circular.*

Army Personal.—The following changes have occurred in the medical corps since the date of publication of our last report:

Promoted.—Assistant-Surgeon P. C. Davis, to be Surgeon, to date October 17, 1868, *vice* B. Randall, retired; Assistant-Surgeon James F. Weeds, to be Surgeon, to date October 27, 1868, *vice* L. N. Holden, retired.

Appointed.—Frederick W. Elbreg, Augustus W. Wiggin,

Washington Matthews, William R. Steinmetz, John D. Hall, Curtis E. Munn, Ezra Woodruff, Philip F. Harvey, William H. King, Stevens G. Cowdry, John M. Dickson, Charles B. Byrne, Frank Reynolds, and Clarence Ewen, to be Assistant-Surgeons, to date November 16, 1868.

Retired.—Surgeon Burton Randall, October 17, 1868; Surgeon L. N. Holden, October 27, 1868.

Resigned.—Assistant-Surgeon J. N. Randall, to date October 17, 1868.

The Academy of Medicine has issued a powerful appeal to the profession to aid, by the contribution of funds, the erection of a new building. The Academy has now on hand a fund of some $12,000 devoted to this purpose. The needs of such a building are evident, and, with a wise liberality, the Academy proposes to make it subservient to the interests of the profession at large, and to place it at the disposal of the other medical societies of this city for their meetings, etc. It will also afford, what is now much needed in this city, the opportunity for founding a medical library which shall be available to the profession. Promises of several very valuable libraries have been made to the Academy so soon as a suitable building shall be furnished in which to deposit them.

A Case of Procidentia Uteri of Seventeen Years' Standing, cured by Kolporaphy and Modified Perineoraphy.—Falling of the womb followed the birth of the patient's last child. She was unable to walk, or in any way to gain a livelihood, and was only free from pain when lying down. The womb was altogether external to the vagina. It was much enlarged, thickened, indurated, and around the os and upon different parts of the vagina were several small ulcerations. Pessaries of different forms had been used to support the womb, but without effect.

Mr. Norton, of St. Mary's Hospital, removed an elliptical piece of the mucous membrane of the vesical wall of the vagina, three inches and a half in length by two inches in breadth. The cut margins were then brought together by means of ten silver-wire sutures, and the uterus replaced. The bowels were confined by opiates until the sixth day, and then relieved by castor-oil. The vagina was syringed daily with warm water, and the sutures were removed in eight days.

Fourteen days after the first operation, Mr. Norton removed by a horse-shoe incision rather more than an inch of the mucous membrane of the posterior and lateral walls of the vagina, including the cutaneous margins of the fourchette. The denuded surfaces were now approximated as in the operation for ruptured perinæum, and were firmly fixed by means of silver-wire quilled sutures; the projecting lips were more evenly adapted by a second row of wire sutures somewhat deeply placed. The bowels were kept quiet for a week by opiates. The quilled sutures were removed after forty-eight hours, and the others in seven days.

Within a fortnight of the second operation, the patient left the hospital apparently cured. A year and a half after the operation, she remained well, and, being a milk-carrier, was bearing excessive weights, suffering from no inconvenience. —*Lancet*, January 23, 1869.

The *Anthropological Review* for January contains as an original article a paper entitled "The Formation of the Mixed Human Races." This paper is a translation from M. Quatrefage's "Report on the Progress of Anthropology in France," etc., and was made by Dr. Dunster for the *Quarterly Journal of Psychological Medicine and Medical Jurisprudence*, and it appeared in that Journal in July last. The *Anthropological Review* copies it entire—eighteen closely-printed pages—but it gives no credit whatever for the article, leaving its readers to infer that it was prepared for that journal.

The Toland Medical College of San Francisco, Cal., graduated six young gentlemen at its recent commencement, November 5, 1868.

Henry G. Wright, M. D.—We regret to announce the death of this physician, of chronic pulmonary disease, at the early age of forty-one. Dr. Wright was Physician to the London Samaritan Free Hospital for Woman, and the author of several successful books. His work on Headaches had passed through four editions, and has been reprinted in this country; that on Uterine Disorders was favorably reviewed in this Journal, May, 1868. He wrote at one time much for *The Lancet*, and was a frequent contributor to the *Saturday Review* and *Pall Mall Gazette*. Dr. Wright visited the United States in

1856, on his return from Australia, whither he had gone on account of failing health.

Something like a Stomach.—M. Decroix, Veterinary Surgeon to the *Garde de Paris*, writing to the Académie de Médecine, states that for the last eight years he has been in the habit of eating the flesh of all the horses that have died in his service, no matter what their disease, whether farcy, glanders, typhoid, *charbon*, or what not! In his opinion, the flesh of phthisical cows and oxen may be sold without the least detriment to the public health.

Some time since[1] we called the attention of our readers to a physiological puzzle occurring in the practice of Professor Richet, at La Pitié, in Paris. It was the preservation of sensibility in the parts supplied by the musculo-spiral nerve, after complete division of that nerve. Dr. J. C. Nott, of this city, in connection with this case,[2] recalled attention to a case occurring under his own observation, and which had previously been commented on at length in our pages.[3] We have now another instance of this peculiar condition of things observed in a patient at St. Bartholomew's Hospital, in the service of Mr. Savory. A large tumor—which proved to be a neuroma—was removed from the lower third of the right arm. The musculo-spiral nerve was seen to enter into and emerge from the substance of the tumor, and the nerve was divided about an inch from either extremity of the tumor. Thus no less than five or six inches of the nerve were entirely removed. The patient made a good and speedy recovery.

From the time of the operation, the muscles on the back of the forearm were paralyzed; but on testing the sensibility of the skin which is supplied by branches of the radial nerve, it (the sensibility) was but little if any impaired. The rest of the report we give in Mr. Savory's own words:

When the skin upon the outer part of the back of the hand, or over the back of the thumb and forefinger, was lightly pricked, the man cried out sharply. He could distinguish in the same region two points of contact when they were not more than an inch apart, both in the long and transverse axes

[1] New York Medical Journal, June, 1868.

[2] See Journal, August, 1868. [3] May, 1866.

of the hand; when they were closer than this on any part of the back of either hand or forearm, he confused them; and, indeed, in comparing the sensibility of this region with that of the inner portion of the back of the hand, and two inner fingers, or with the corresponding part of the opposite hand, no very striking difference could be detected. All portions, too, of the back and sides of the middle finger appeared equally sensitive. He could distinguish also between the contact of hot and cold bodies in this region as well as in other parts. He always remarked the distinction between the touch of a cold steel sound and one that had been previously dipped in hot water. When desired to experiment upon himself by touching, scratching, or pricking the different parts, he repeatedly declared either that he could perceive no difference of sensibility, or that the radial portion of the dorsum of the hand was rather more sensitive than the other half. These observations were carefully repeated in various ways, day after day, with a uniform result; indeed, the man at last got tired of them, concluding, no doubt, that the existence of acute sensibility had been abundantly proved. It need hardly be said that in many of these experiments the man was blind folded, and otherwise prevented from forming any idea of what we did except through the sense of touch.

Now, what is the interpretation of this remarkable fact? All anatomists will agree that, so far as ordinary dissection goes, the skin on the outer half or thereabout of the back of the hand, and of the back of the thumb, forefinger, and outer portion of the back of the middle finger, is supplied only by branches of the radial nerve; and when any variation is noticed in the distribution of this nerve to the back of the hand, it is almost always that it supplies more than the usual proportion of integument, going on sometimes to the ring-finger. I cannot doubt therefore that in this instance the parts in question were supplied in the usual way—only through the branches of the radial nerve. But then how could these parts retain sensibility after the removal of some inches of the nerve-trunk? I can only account for it thus: We know that in the forearm, just above the wrist, some small branches of the external cutaneous nerve communicate with the radial, and it may be that at this junction the radial receives filaments from the external cutaneous, which so pass down to be distributed, with the filaments of the radial, even to the skin of the hand. This seems to me to be the most probable view of the matter—indeed, the only reasonable one. There should assuredly be more difficulty in conceiving such a transference of the filaments of the ulnar and radial on the back of the hand. The arrangement

of the small branches so near their distribution would hardly admit of this. But if this explanation be the true one, it suggests a much larger question. Are nerves so isolated in their distribution as they are now believed to be? Is not the purpose of the communication between different nerves in their course more extensive and complete than has been hitherto recognized? The question is one which appears to be beyond the reach of any ordinary dissection, and perhaps it will be best answered hereafter by observation and experiment in cases like the present one.[1]

The Army Medical Museum continues to increase in value and usefulness. During the year, six hundred and seventy-three (673) specimens have been added to the surgical section, one hundred and twenty-one (121) to the medical section, two hundred and two (202) to the section of comparative anatomy, six hundred and eighty-seven (687) specimens and one hundred and fourteen (114) photographic negatives of microscopical specimens to the microscopical section. An anatomical section of one hundred and sixty-three (163) specimens has been formed, and is rendered of especial interest by the large proportion of typical crania of the North American aborigines which it contains. A collection of one hundred and eighty-seven (187) specimens of Indian weapons and utensils has also been added. Two hundred and sixty-six (266) discarded specimens, the histories of which could not be found at the period of publication of the Catalogue of the Surgical Section, have been identified and restored to the collection. For purposes of exchange with other museums, or with learned societies, either for specimens or publications, four thousand four hundred and seventy-two (4,472) photographs, illustrative of injuries and operations, have been printed. There were during the year fourteen thousand four hundred and forty-eight (14,448) visitors to the Museum, including many military surgeons of eminence.—*From Report of the Surgeon-General, U. S. Army.*

Dr. J. Blackburn sends to the *Lancet* a description, accompanied by an engraving, of an enormous biliary calculus, passed without any aggravation of the usual signs of passing a

[1] In the Buffalo *Medical and Surgical Journal* for June, 1868, Dr. J. F. Miner records three cases which were suggested by our report of Professor Richet's case. Dr. Miner's case, however, are not parallel, although, in one where three inches of the median nerve were excised on account of intense pain supervening on the healing of a gunshot wound, there was, if we understand the case correctly, but a slight diminution of sensibility.

gall-stone. The stone measures three and three-eighths inches in length, one and a half inches in its widest part, and weighs one ounce and six drachms.

Transverse Hermaphroditism.—We are indebted to the kindness of Dr. W. W. Ely, of Rochester, N. Y., for the following contribution to the study of comparative anatomy:

The term hermaphrodite, although not strictly applicable to any instances except those in which the two sexes are fully represented in the same individual, as in certain of the lower organisms, is nevertheless a convenient designation for abnormal developments of the generative organs where the sexual characteristics are more or less blended. Transverse hermaphroditism, therefore, according to Dr. Simpson, comprises those cases in which the internal and the external organs of generation belong to different sexes. Examples of this development in domestic cattle have been called "Free Martins." Similar malformations have been found in other animals, and even in the human subject. The following instance in the deer occurred to the writer in August last. The animal had been killed in hunting a few hours before my examination. It was partially dressed, for the convenience of transportation, leaving the pelvic organs *in situ*, and in this condition it weighed 139 lbs. It was supposed to be a doe, with an unusual development of horns. The antlers were like those of a buck, large and branching, each having four prongs. The external sexual organs were of the female type. There was a well-formed udder of a size proportionate to that of the animal. The vulva were perfectly normal. The vagina was about half the usual length, narrow, and contracted toward the bladder, where it terminated. The urethra was short, and opened into the upper part of the vagina. There was neither uterus nor penis. The vesiculæ seminales were also wanting. Two small testicles were found attached to the peritoneum, and connected by this membrane with the bladder. The vasa deferentia were represented by short, impervious cords, which could only be traced a few inches, and might have been severed when the viscera were removed. The horns were in the velvet, showing that they were subject to the periodical growth of these organs, as in the perfect male.

Errata.—In Dr. Sayre's article on Artificial Hip-joint, January number, page 356, for *July* 20 read *February* 20; page 357, lines 11 and 14, for *February* read *April*. The correction of these dates lessens very materially the significance of some of the strictures made by Dr. Bauer in his reply to the paper alluded to.