of pathology, intervening between inflammation and hypertrophy, to which the term inflammatory hypertrophy may be given, embraces a large number of morbid products which have hitherto remained unclassified, or which have been arranged in such a manner as to convey no idea of their real nature." He distinguishes three classes of cases, in the first of which inflammation lingers on a long time without causing any structural change; in the second, it lingers on in the same way, but is attended with hypertrophy; and in the third, it subsides, after having given a stimulus to the hypertrophy, which then goes on without it. It seems to me in the highest degree probable, that in many of those cases which Mr. Humphry would comprise under inflammatory hypertrophy, there is no true inflammation, but one only of its parts, viz. more or less of hyperæmic afflux. The instance of elephantiasis to which he alludes, seems, from all that we know of its pathology, to be certainly widely different in its real essence from any mere inflammation either of the lymphatics or of the skin. Whatever inflammation may have to do with it, there is, certainly, some other cause infinitely more potent. Were it not so, why should not a severe case of psoriasis, or repeated attacks of rheumatism or gout, or the irritation of diseased bone, or deep-seated cellular inflammation, cause similar hypertrophy? These hyperæmiating causes do not produce this effect, because with them there does not coexist that peculiar alteration of the normal qualities of the plasma and the tissue necessary to healthy nutrition, which is the chief factor in the process giving rise to elephantiasis. I would wish to name all these morbid changes in which the character of growth—of new formation—decidedly predominates over that of inflammation, according to their principal feature. I would call them simply fibroid thickenings, or fibro-cellular growths, and thus avoid giving them a name, either merely trivial, or of doubtful correctness. The matter is not one of nomenclature merely; it has important bearings on practice. Suppose a case intrusted to our care in which the liver was found considerably enlarged, projecting below the ribs, and that we have satisfied ourselves that the enlargement depends neither on engorgement with blood, nor on fatty change, nor on bacony deposit. The probability, then, is very strong that it is in a condition of cirrhosis, the fibrous tissue between the lobules being prodigiously increased in quantity. Can we hope by the administration of mercury or iodine to remove this deposit? If it were simply inflammatory, the attempt might be successful; but as it is a growth, one may confidently predict that it would be fruitless if not injurious. The same remark will apply more or less completely to any of the other instances before mentioned.

It seems to me that observers have not sufficiently held in view, although generally admitting as a matter of course, the vast predominance of that unknown force, call it what we please, which determined and still determines that here bone and there muscle, and in another spot nerve, and in another tendon, should be formed. There is some such force, and it is certainly liable to be deranged, and its derangements are just those which are most difficult to rectify. This perversion of the nutritive act (to borrow Andral's phrase) is that which determines the origin and growth of a cancerous, or a fibrous, or any other kind, even of a fatty tumour. It is the same, I believe, which causes the textural changes above enumerated, and some others which might be added, and not inflammation, which of itself and in its best marked form, produces no exudation which is capable of growing and enlarging. It is this property of vegetative increase which makes the removal of new formations and all allied productions so difficult: mere inflammatory effusions we can melt down and get absorbed, but these resist our "sorbefacient" remedies with the same persistence almost as the natural tissues.

The degenerative process which constitutes true morbus Brightii does not quite come under the above class of morbid changes. The best observers are agreed that production of new interstitial tissue constitutes no essential part of the morbid action. Most writers incline to view it as a nephritis

of more or less chronic character; but the great name of Prout can be cited in support of the view that the substratum of the process is something essentially different from inflammation. My own observation has always inclined me to adhere to this opinion. In the enlarged, often mottled, kidney we see simply a derangement of nutrition, producing hypertrophy of the epithelium concurrently with malperformance of its function. The albuminoid cell-growth does not undergo its normal changes; and instead of forming the secretion, it collects in the tubes, either choking them up and distending them, or getting washed out in the stream from the Malpighian tufts. In further stages, or in more rapid degrees, of change, the tubes and epithelium break up into mere detritus. In all this process we see really nothing to justify us in considering it as inflammation. So, also, in the atrophic form, the small dwindled kidney, we find no satisfactory evidence of anything else than a gradually supervening impairment of the nutritive process. The epithelium wastes, and the tissue decays, and collapses; thus far is certain; but how little is there to show that this is dependent on previous inflammatory disease? On the contrary, the frequency of recovery from acute congestion (as that of scarlatinal dropsy) shows that inflammatory and degenerative actions are widely different in their nature and results. The one exhibits the perturbation of healthy action by an intervening cause, the other is the decay of vital power from innate or induced weakness.

The subjoined scheme illustrates the transitions that seem to exist between two of the chief types of disordered nutritive action. In inflammation of acute sthenic character the hyperæmia is a main element, and the exudation is non-vegetative. In cancer (say encephaloid) there is no apparent or necessary hyperæmia, but the exudation is endowed with a vegetative power of most active growth. In simple deposit, as of tubercle or bacony matter, there is neither hyperæmia nor manifestation of vegetative power; the sole morbid change is the deteriorated state of the plasma.

Inflammation.

Acute sthenic.
Subacute.
Chronic.

Tubercle—bacony matter.
Simple deposit.

Encephaloid cancer.
Fibrous tumour.
Fibroid thickening.

Southwick Place, Hyde Park, November, 1853.

AN EXPERIMENTAL INQUIRY INTO THE EFFECTS OF INJECTING PUS INTO THE VEINS OF ANIMALS.

By JOSEPH SAMPSON GAMGEE, Esq.

EXPERIMENT I. Oct. 17th, 1853, 10 A.M. The subject was a perfectly healthy horse; pulse 33; the respiration 10 in a minute. The left jugular vein was opened, and a mixture of two and a half drachms of good pus, with the same quantity of tepid water, were injected into it slowly, and without the animal being in the least disturbed. It was ascertained that the vein continued quite pervious some moments afterwards, by its filling upon pressing on it just in front of the sternum.

12 meridian. No change had hitherto taken place, but the animal had now begun to breathe twenty-five times per minute. The pulse was 42, of good character.

4 P.M. Pulse 60; respiration 30, thoracic.

8½ P.M. Pulse 64; respiration 24. The horse fed well, and appeared in good health.

Oct. 18th, 7½ P.M. The animal fed, and was lively. The respirations were 20, and much less thoracic than last evening. Pulse 68, full, and oppressed. The vein was pervious.

2½ P.M. Pulse 54; respiration 20.

7½ P.M. Pulse 64; respiration 24.

EXPERIMENT II, instituted on the same animal on October 19th. 10 A.M. The pulse was 51, of good character; the respirations were 20, not manifestly thoracic.

10½ A.M. An opening was made into the right jugular

vein, and a mixture of two drachms and a scruple of pus, with an equal quantity of water, was injected into it. A good deal of difficulty was experienced in endeavouring to pass the nozzle of the instrument into the vein; and I infer from the following facts, that, after all, the fluid was only injected into the subcutaneous cellular tissue:—1stly. A swelling immediately appeared below the orifice; 2ndly. The vein distinctly filled above when pressed upon below, which it could not have done if the swelling had been due to its obliteration; 3rdly. The animal, ten minutes after the injection, was not in the least affected by it.

7½ P.M. Pulse 54; respirations 20.

Oct. 20th, 2¼ P.M. Pulse 96; respirations 20. Both jugular veins were pervious. The swelling on the right side of the neck was diffused and sore. A thrombus had formed on the left side, and pus escaped from the orifice.

EXPERIMENT III, performed on an aged grey horse, affected with extensive melanosis of the tail and anus, but otherwise perfectly healthy. Pulse 38.

Oct. 24th, 12 merid. A mixture of two drachms and a scruple of good pus, diluted with an equal quantity of tepid water, was injected into the right jugular vein. I had no sooner done this, and transfixed the lips of the orifice with a pin, than the horse began to heave at the flanks, after which he staggered a few moments, and fell; when down, he breathed laboriously thirty-eight times in the minute, and, with scarcely a struggle, and not more than two minutes' delay, expired.

Some blood, obtained from the orifice in the jugular just before death, presented no abnormal appearances when subjected to microscopic examination.

Section Cadaveris, 4½ P.M. The jugular vein and the right cavities of the heart were filled with dark coloured currant jelly-like clotted blood. The left cavities also contained a clot, but a much smaller one. Longitudinal section through both the lungs proved them to be intensely congested throughout, but most particularly at the bases; the apices and inferior borders were comparatively free. This cut surface was deep red; a large quantity of dark red blood exuded from it on pressure; the lung tissue thus congested floated on water.

It is important to notice that, from the manner the experiment was conducted, the introduction of air into the vein was impossible.

On examining the blood from the right side of the heart, I discovered on it a very large number of corpuscles, measuring, on an average, one two-thousandth of an inch in diameter, and having nuclei not distinguishable from those of pus-cells. So numerous were they, that it was impossible to count them.

EXPERIMENT IV, instituted on the horse which was the subject of Experiments I and II. Pressure over the lower part of the left jugular vein caused it to fill above, and blood flowed freely; on opening it between its bifurcation and the site of the first experiment on the 17th of October, a mixture of two drachms and a scruple of pus, diluted with an equal quantity of water, was injected into it on the 25th of October. A quarter of an hour afterwards, no change whatever had manifested itself; both in number and character, the pulse continued absolutely the same.

Oct. 27th. Pressure below the orifice, on the right side of the neck, caused the exit of half an ounce of good thick pus.

Oct. 29th. Since the last report, nothing whatever had occurred. The horse had fed, and appeared well, with the exception of occasionally coughing slightly. I ordered him to be destroyed by a blow on the skull, and proceeded to examine the body two hours after death.

Section Cadaveris. On removing the skin from the right side of the neck, the tissues immediately beneath appeared perfectly healthy; but, on exposing the jugular vein, the cellular tissue around it was preternaturally red, and matted together down to its entrance into the chest, within which all was normal. On slitting open the vein, its coats proved to be of normal colour, smoothness, and thickness, except or about an inch in the situation where the aperture was

made; here a quantity of buff coloured plastic matter occupied about two-thirds of the calibre of the vessel, but the remainder was pervious. The subcutaneous tissues on the left side of the neck were likewise healthy; but, on exposing the jugular vein, it appeared as a white opaque solid body. On cutting it open, its coats were found much thickened, and intimately adhering to a clot, which occupied the entire bore, and gradually tapered downwards to the extent of about four inches, but barely extended half an inch above the orifice: beyond these parts, the vein was perfectly healthy.

The heart contained well clotted blood, and was perfectly healthy.

Both lungs were in the greater part healthy, but both contained abscesses in the posterior lobes. These abscesses were in every respect similar to those met with in individuals who have died from pyæmia: the majority of these were the size of peas, but one was as large as a walnut; its contents appeared good pus to the naked eye, and proved to be such on microscopic examination.

On the diaphragmatic surface of the posterior lobe of the left lung were numerous little tenacious shreds of lymph; but there was no preternatural redness of the pleura, or effusion into its cavity.

EXPERIMENT V, instituted Oct. 20th, on an aged but healthy white pony. An opening was made into the right jugular vein, and, after repeated endeavours to pass into it the nozzle of the injecting instrument, thinking I had succeeded, I injected a two ounce mixture of equal parts of water and creamy pus, but without any apparent effect.

Seven hours later, the constitution was not at all affected; but the neck below the aperture in the vein was slightly swollen, hot, and tender.

EXPERIMENT VI. No new symptom having made its appearance, at 1½ P.M. on the 27th of October, this pony was again experimented upon. A longitudinal incision was made over the course of the left jugular, a little above the middle of the neck; the vein exposed, pinched up with forceps, and opened; after which the nozzle was fairly passed into it, and a mixture of two drachms and one scruple of pus, with an equal quantity of water, injected. This was followed by acceleration of the respiration and pulse, the former of which, while the animal was standing half an hour after the experiment, numbered 30, and the latter 60 in the minute.

EXPERIMENT VII, 4¼ P.M. Respirations being 12, and pulse 60. The orifice made into the left jugular vein in the morning was reopened, and a mixture of one ounce and two scruples of pus, diluted with an equal quantity of water, injected. Ten minutes afterwards, the animal having just before got up, and being then standing, his respiration was 20, and heart-beats 64 in the minute.

28th Oct., 7¼ A.M. The animal continued standing. Pulse 76; respiration 36, laboured, and very decidedly abnormally thoracic. Immediately afterwards the animal was destroyed by a blow on the head.

Examination of the body three hours and half after death. The right side of the neck was scarcely at all swollen, but emphysematous crackling was detected on palpating it. On detaching the integument, the cellular tissue was found infiltrated with foetid bloody serosity from the angle of the jaw all over the side of the neck, in the bend of the elbow, in the deep cellular tissue which aided in connecting the shoulder to the trunk, and on the inner surface of the right foreleg down to the knee. The right jugular vein was filled with a smooth, dark red, non-adherent clot of moderate consistence. On detaching the integument from the left side of the neck, the cellular tissue was seen infiltrated with blood over a space of about two inches around the opening in the vein; beyond this part the subcutaneous tissues were perfectly healthy. The vein contained a common venous clot.

Heart. The right cavities contained a large amount of clotted blood, but there was barely a fourth of that quantity on the left side. On the left surface of the heart, opposite to the interventricular septum, were numerous little red

ecchymotic spots; some of these were bright red, others purplish. Microscopic examination of the blood from the right ventricle did not demonstrate any difference from the blood which was collected from the jugular vein prior to the injection of pus.

The *right lung*, on longitudinal section, appeared very much congested, particularly at the posterior and upper part; less so towards the apex and inferior border. The congestion was not quite so intense as it was in the horse suddenly destroyed by the injection of pus on 24th October, but it was very little less so. In the most congested part, the pulmonary tissue was not at all crepitant; yet it floated on water, even after firm pressure.

The *left lung* was in the same condition as the former, except that the congestion had posteriorly attained even a more intense degree. There were no nodules in either lung; the congestion was uniform.

From these experiments I infer:

1. That when pus is fairly injected into a vein, there is no obstacle to its moving on with the current of blood;
2. That the first organic effect of such injection is congestion of the lungs; which may be so intense as to prove instantly fatal, or be temporarily recovered from;
3. That the abscesses met with in the lungs of individuals who have fallen victims to pyæmia, may be produced by injecting pus into the veins of healthy animals.

Experiments for the purpose of ascertaining the effect of injecting pus into the bloodvessels have likewise been performed by Gaspard,* Trousseau and Dupuy,† Gulliver,‡ Renault and Bouley,§ Alan,|| d'Arcet,¶ Lebert,** Castelnau and Ducrest,†† Sédillot,‡‡ and Lee.§§ The results they have obtained are singularly contradictory; and it would appear as if the progress of time and the addition of experience have only served to augment the obstacles in the way of a definitive solution; for the most recent inquirer, Mr. Henry Lee, arrived at conclusions which tended to subvert those of Castelnau and Ducrest, and of Sédillot, who have particularly distinguished themselves for the number of, no less than for the manner of executing, their experiments. It is remarked by Mr. Lee that "the simple experiment of mixing some pus with healthy recently drawn blood, will at once show that such a combination cannot circulate in the living body. It will be found that the blood coagulates round the globules of pus, and forms a solid mass which will adhere to the first surface with which it comes in contact, and it will be evident that it is not till the coagulum thus formed is broken up or dissolved that its elements can circulate with the blood." Upon this passage I thus commented in a memoir on pyæmia, published in this JOURNAL for March 4th. "To argue, as Mr. Lee does, from the fact that out of the body blood coagulates round pus, therefore such a combination cannot circulate in the living body, is about as warrantable as it would be to predicate from the observation that pure blood coagulates in a basin, it therefore cannot remain fluid in the ventricles of the heart. The fact is, that the circumstances are so materially different, blood in an earthen vessel on the one hand, blood in the living body on the other, that no inference deduced in the one case is applicable to the other." The results of my experiments justify this criticism, and prove that the circulation of pus with the blood is perfectly possible.

Mr. Henry Lee has related seven experiments in which he injected pus into the veins, and they call for careful study. In the first of them (the vith of his series, *op. cit.*, p. 28),

three drachms of pure pus were injected into the left brachial vein of a healthy ass. When the operation was completed, the sides of the vein were brought together with a pin, and the animal was allowed to get up. The vein above the opening could now be felt as a hard unyielding cord, as high as it could be traced with the hand; but upon gentle pressure being made, so as to propel the blood in the course of the circulation, the hardness completely disappeared. Two hours and a half after the operation the pulse, which naturally was 36, had risen to 60; and the respiration, from 12 per minute, had increased to 26. The animal was destroyed two days after the injection.

Post Mortem Appearances. The wound in the left leg opened directly into the brachial vein, which was filled with lymph and a thin pus for a very short distance, both above and below the external opening. Immediately above this, the vein was healthy; nor was there any appearance of disease in any of the other veins of the limb, nor in the veins leading to the heart. The glands in the axilla were swollen. The lungs were found studded irregularly in different parts with circumscribed spots of livid congestion; these existed both upon the surface and in the substance of the lungs: they were generally about the size of a filbert, but in some places they occupied a single lobule, and were accurately circumscribed by its outline.

The result of this experiment, so far from supporting the doctrine propounded by Mr. Lee, is directly opposed to it. What was the cause of the vein above the opening feeling hard immediately after the injection, it is not easy to determine; that it was not due to the formation of a clot, is rendered more than probable by the fact that the hardness completely disappeared upon *gentle pressure*. Certain it is that the effects of pus circulating with the blood were speedily manifested by the acceleration of the respiration, not only absolutely, but also relatively to the increased frequency of the heart-beats. Thus: before the injection, pulse 36, respiration 12; two hours and a half after it pulse 60, respiration 36.

The *post mortem* appearances of the lungs, studded with spots of congestion, obviously the early stage of multiple abscesses, were characteristic of the entrance of pus into the circulation, perfectly concurred with my own experiments, and support the conclusions I have deduced from them.

In the second experiment (Exp. VII of series, *op. cit.*, p. 30), Mr. Lee, on Nov. 23rd, injected about an ounce of pus into the right jugular vein of an ass; the vein immediately became corded, and the blood appeared to have coagulated in the vessel. The operation did not much excite the breathing, but the pulse, which was naturally 35 in the minute, rose to 60.

Nov. 24th. The vein could be traced as a thickened cord as far as the sternum. Respiration 12 (the natural standard); pulse 90.

25th. The parts around the vein were much infiltrated with serum; pulse 50, respiration 12.

26th. The wound in the neck began to suppurate, and an abscess subsequently formed in the course of the vein about midway between the opening and the sternum. The general symptoms continued with very slight variation until the 4th of December, when the animal was destroyed.

Post Mortem Appearances. The jugular vein was found to have become inflamed only in the course of the circulation, and to be obliterated only a short distance below the external opening. The surrounding parts were greatly infiltrated with serum and lymph, and several abscesses had formed in the immediate neighbourhood. The lungs did not present any well defined patches of congestion, as in the last mentioned experiments.

Attentive consideration of the facts above recorded, makes it obvious that the pus was not injected into the vein, but into the surrounding cellular tissue. *a.* Though the pulse was accelerated, the respiration continued natural; this is the reverse of the former experiment, in which there can be no doubt that the pus did circulate with the blood. *b.* The serous infiltration of the surrounding parts noticed during life, and verified after death, with the additional discovery

* Journal de Physiologie, par Majendie, tom. i et ii, 1821-22.

† Arch. Gén. de Médic., tom. xi, 1826, p. 273.

‡ On the Frequent Presence and Effects of Pus in the Blood in Diseases attended by Inflammation and Suppuration. Veterinarian for 1839, p. 42-51.

§ Rec. de Méd. Vét., Mai 1840; et Arch. Gén. de Méd., 3e Série, v, viii, 1840.

|| Gazette Medicale, 1842.

¶ Thèse de Paris, 1842.

** Physiologie Pathol., t. i, p. 313-324.

†† Rechercher les cas dans lesquels on observe les abcès multiples, et comparer ces cas sous leurs différents rapports; in Mémoires de l'Académie Royale de Médecine, t. xii, 1846.

‡‡ De l'infection purulente ou pyœmie. Paris, 1849.

§§ On the Origin of the Inflammation of the Veins, and on the Causes, Consequences, and Treatment of Purulent Deposits. London, 1850.

of abscesses in the immediate neighbourhood, is precisely what occurred in my experiments II and V, in which dissection proved that the pus had been injected into the cellular tissue. The obliteration of the jugular vein, noted by Mr. Lee, is apt to occur from any cause which irritates the vein after an opening has been made into it. Thus, in experiments which I performed last year for the purpose of ascertaining the effects of injecting water and various saline solutions into the circulation, phlebitis occurred in several instances.

These observations are strictly applicable to Mr. Lee's VIIIth experiment; while, according to his narrative of experiments IX, X, XI, and XII, the injection of pus into the circulation was followed by such severe constitutional symptoms as to supply very weighty testimony in opposition to his teaching.

In restricting my analysis to the opinions of Mr. Lee, I have been chiefly actuated by a sense of their importance, based as they are on experimental researches, and opposed to conclusions, which ever since their announcement were regarded as marking an era in the progress of knowledge of the purulent infection. It was impossible to advance a step without admitting or refuting Mr. Lee's statements. To the latter duty, I have been led by careful experiments; the difficulty in conducting which has given me additional cause for respecting and being grateful to Mr. Lee for his endeavours to advance the knowledge of this highly important subject. In extenuation of not having as yet extended these researches, and analysed the opinions of all those who have hitherto engaged in them, I confidently appeal to the kind consideration of those who are acquainted with the nature of these investigations. I feel much pleasure in acknowledging the assistance I have received from my brother John, in the prosecution of the present inquiry.

Palazzo Corsi, Florence, Nov. 17th, 1853.

PERISCOPIC REVIEW.

SURGERY.

TREATMENT OF ANEURISM AND OTHER VASCULAR TUMOURS BY THE INJECTION OF A SOLUTION OF PERCHLORIDE OF IRON.

During the present year, the attention of French surgeons has been closely directed to a new method of treating aneurisms, viz., that of producing coagulation of the blood in the aneurism by injecting a few drops of a solution of perchloride of iron into the sac. The merit of first bringing this method prominently under notice is due to the late M. PRIVAZ of Lyons. We propose to give an abstract of what has been said and written for and against this method.

History. In a letter to M. Marjolin, published in the *Union Médicale* for May 12th, M. Pravaz gives the history of the treatment.

The author states that, in the year 1828, he commenced experiments on the means of preventing the absorption of poisons. Cauterisation of the poisoned wounds by galvanism appeared to succeed best; this he first tried in cases of bites by vipers and by Indian snakes, and of mad dogs. In his experiments, he observed the readiness with which blood coagulated under the action of galvanism; but it did not occur to him to apply this principle to the treatment of aneurism until M. Velpeau had stated that a coagulum was produced by introducing a needle halfway into a vessel, and there leaving it. This led M. Pravaz, by a series of experiments, to the conviction that galvano-puncture was likely to be a very successful treatment; and he communicated his idea in 1831 to several surgeons. For many years this method remained in abeyance, being merely referred to in works on medicine and surgery; and it was not until 1845 that M. Pétrequin successfully employed galvano-puncture in aneurismal tumour in the course of the temporal artery. M. Pravaz observed with interest the applications of galvano-puncture which were made since 1845 in most countries of Europe. He was led to believe that this method, from the care which it required, from the frequent imperfection of the apparatus, and perhaps from a certain idiosyncrasy in the blood, often failed.

In 1851, he first attempted to produce coagulation by employing voltaic electricity as a means of conveying through the tissues one of the elements of a saline solution placed on the part. This plan had been partly proposed by Strambio in 1847. M. Pravaz thus describes his discovery of the treatment by injection of perchloride of iron.

"In the course of the experiments which I have related, the object of which was either to abolish acupuncture while galvanism was retained, or to reduce the operation to the introduction of a single needle, I observed that perchloride of iron instantaneously caused a solution of albumen to coagulate in mass. Considering, at the same time, that the preparations of iron are generally harmless when administered in a moderate dose, I was led to reflect whether, in place of endeavouring to suppress acupuncture in the treatment of aneurism, it would not be better to renounce electricity, and retain puncture as a means of introducing the coagulating material into the sac. I thought to realise that idea by means of the trocar which is used in the exploration of tumours. Those, however, which are in common use are not sufficiently delicate; and I waited the opportunity of a visit to Paris to have some made fit for the end which I proposed. M. Charrière perfectly fulfilled my wishes in this respect. I obtained also a small syringe, the piston of which was moved by means of a screw, so that the injection was performed steadily and continuously, and could be regulated at the will of the operator.

"On my return to Lyons, I attempted, with the assistance of my son, to produce coagulation of the blood in the carotid artery of a rabbit; but the artery was too delicate to allow the trocar to be easily introduced without transfixing it. . . . I had proposed to recommence my experiments on larger animals, when a severe illness interrupted my researches. They would probably have been altogether suspended, if the stay of M. Lallemand in my house had not both restored me to health and reanimated my scientific ardour."

M. Pravaz and Lallemand then performed experiments in conjunction, in which M. Pétrequin participated for a time.

"The limits as to quantity which M. Lallemand and myself have determined, cannot be passed without producing symptoms of intoxication, and dissolution of the clot; but the injection of an excessive quantity may give rise to severe inflammation of the sac, consecutive ulceration, and expulsion of the coagulum."

" . . . M. Lallemand and I have estimated the quantity required to coagulate each centilitre (about one-fourth of an ounce) of blood as being three drops. I would even go below this limit; but, in order to furnish a sure guide to the operator, the degree of concentration of the solution of perchloride of iron must be observed."

In an article published in the *Gazette Médicale* for October 1, M. PÉTREQUIN of Lyons claims to share, equally with M. Pravaz, the merit of originating this mode of treatment. He states that, in 1845, he proposed to himself to search for an agent possessing the properties of fluidity, smallness of volume, capability of producing coagulation without carbonising the blood, non-liability to produce excessive irritation, and capability of being absorbed without danger. He also says that, in 1852, M. Pravaz proposed to him to perform a series of experiments with perchloride of iron. They performed some experiments in conjunction; but subsequently M. Pétrequin continued his researches independently; experimenting, however, with perchloride of iron and manganese. He does not, however, appear to have actually employed this agent in the treatment of aneurism.

The mode of operation is thus described by M. Lallemand:—"The method proposed by M. Pravaz consists in coagulating the blood in the vessels by the injection of a few drops of a solution of perchloride of iron at its maximum of concentration. The injection is effected by means of a very delicate trocar, which must be introduced very obliquely through the walls of the artery, by a kind of rotatory movement. To this trocar is fitted a syringe, of which the piston moves by means of a screw, so that the liquid may be injected steadily, and the quantity accurately measured. At the time of operating, the flow of blood in the vessel must be arrested." In the treatment of aneurism, the solution must be introduced into the aneurismal sac, and the artery must be compressed for four or five minutes.

Cases. At the meeting of the Academy of Medicine on the 8th Nov., M. MALGAIGNE read a paper on the treatment of aneurism according to the method of M. Pravaz. The article is published at length in the *Union Médicale* for November 10th. The author passed in review the history of the operation from the reading of a letter from M. Lallemand before the Academy of Sciences up to the present time. He referred to the experi-