hours without wanting to make water, by which time the wound will be covered with a firm clot, and the pain in micturition be considerably diminished. I do not leave any catheter in the bladder after the operation, and I allow the patient to pass his urine naturally. I believe I was the first surgeon in this country to dispense with the use of the catheter after the operation. If there be one practice more persistently insisted on than another in English surgical works, it is that of the necessity of the use of the catheter after urethrotomy, the instrument to be either left in the bladder or else employed to draw off the patient's urine; and the writers point out the disastrous consequences which will take place, in the shape of abscess, fistula, or infiltration of urine, if the practice be not observed. To Dr. Gouley of New York belongs the credit of having shown the utter groundlessness of the surgeon's fears.

After the operation, I am in no hurry to commence the passing of instruments, usually waiting till the fourth day, and not introducing them oftener than twice a week. At the end of ten days, I begin to teach the patient how to pass a catheter for himself, and order him to do so every Saturday night till further orders. By passing a large No. 25 bougie or catheter several times after the operation, the insertion of a good "cicatrical splice" is guaranteed.

Now as to results. I have operated in all on thirty-three cases, all of them of the worst description, and for that reason relegated to the operation, without a single death. In one instance, the urethra was so indurated in its entire length that I left in a catheter after the operation, to set up urethritis, and so lessen the thickening. The inflammation was, however, more than I desired; and abscess, followed by fistula, ensued. The case ultimately got quite well. In one case only had I troublesome bleeding. It proceeded from the meatus, which I had divided, and was a hint to me for the future not to cut the meatus at the same time as the stricture, but several days before. As a rule, not more than about a dessertspoonful of blood escapes either at or after the operation. Secondary hæmorrhage I have never seen. Rigors occurred in about two-thirds of my cases. I look upon them as entirely nervous and of no importance. It is stated in books that rigors are due to the passage of the urine over the wound. This, however, cannot be the correct explanation; for, unless pyæmic, they are never seen after lithotomy or external urethrotomy. I look upon rigors after internal urethrotomy as caused by the sudden stretching of the nerves in the wound through the distension of the canal by the urine. In three of my earlier cases, I had to repeat the operation, as my incisions had not been sufficiently free. I am sure that every one who performs the operation will be pleased with the soft supple cicatrix which follows it, so different from the rough, tough, irregular cicatrix which forms the so-called "immediate treatment", which is neither more nor less than absolute laceration, not always of the tough stricture, but sometimes of the unoffending healthy urethra, where, as elsewhere, "the weaker goes to the wall".

Now for statistics. I think they will be found to be eminently satisfactory, and will carry conviction. They are the largest, I believe, which have ever been placed before the profession, and show what internal urethrotomy can accomplish. No other operation for stricture with which I am acquainted can produce such favourable results. I consider that no operation can be performed on the urethra without a certain amount of risk; but how slight that risk is you will immediately see. I find by examination that the operation has been performed by six surgeons in London, Paris, Mobile, and New York, one thousand and ninety-five times, with but ten deaths; and there would probably have been two deaths less had it not been for the crowded state of the wards in the Neckar Hospital during the Commune.

In conclusion, gentlemen, let me say that the deductions drawn from the experience of a solitary surgeon may be misleading, but that the conclusions which must be arrived at from the overwhelming mass of figures which I have placed before you loudly proclaim that the operation of internal urethrotomy claims your consideration and demands your support.

NEW MODE OF TREATING VARICOCELE.

I find the following simple procedure an efficient method of treating varicocele. Pass a long and strong hare-lip pin between the veins and the scrotal walls, bringing the point of the pin close beneath, but not through, the scrotum; then make the point retrace its course, but passing now behind the veins, until it emerges near the puncture through which it entered. In a word, by employing that form of acupuncture known in the Aberdeen School as the method of retroclulsion, a varicocele may be effectually compressed and the veins obliterated.

S. MESSENGER BRADLEY, Manchester.

HISTORY OF OVARIOTOMY IN ITALY.

By T. SPENCER WELLS, F.R.C.S.,

Surgeon to the Queen's Household, etc.

I HAVE just received from Dr. Peruzzi of Lugo a pamphlet upon my practice of ovariectomy. This pamphlet, which is a reprint of articles in the ninth volume of the *Raccoglimento Medico*, consists in great part of a translation of one of my lectures, published in the BRITISH MEDICAL JOURNAL of December 15th, 1877. It also contains so very cordial a recognition of the effects of my work upon Italian surgery, that I should have received it with silent gratification, if it had not also contained a correction of an historical error into which I had fallen, but which I now hasten to acknowledge and correct.

In my work on *Diseases of the Ovaries*, there is a chapter on the Rise and Progress of Ovariectomy. At page 314, the following sentence may be found: "In Italy, the first successful ovariectomy was performed by Professor Landi of Pisa, in September 1863; the second by Professor Peruzzi of Lugo, in 1869; the third by Dr. Marzolo of Padua, in July 1871." On page 313, I had stated that Professor Vanzetti, the distinguished Italian surgeon, had performed the first ovariectomy in Russia in 1846. My work was published in 1872; and Dr. Peruzzi is not unnaturally surprised that I did not allude to a case by Emiliani of Faenza, in 1815, which had been published in 1843 in the *Bulletino delle Scienze Mediche di Bologna*, and had been again brought to light by Dr. Peruzzi himself, in the form of a letter to M. Boinet, entitled "*Aperçu Historique sur l'Origine et les Progrès de l'Ovariectomie en Italie*".

As this letter appeared in *L'Ippocratico di Forlì* in 1869, I should certainly have noticed it in 1872, if I had seen it. I now see it for the first time, in pamphlet form, reprinted from *L'Ippocratico*.

The author shows that, so long ago as 1752, Dr. Tarziani Tozzetti, in a published *Selection of Medical Observations*, had not (as supposed by Boinet) proposed to extirpate a diseased ovary, but only discussed the question of tapping and palliative treatment. So Morgagni, in 1761, admitted that ovariectomy might be considered in cases where the cyst was not adherent and the pedicle single and slender. Peruzzi goes on to quote from Monteggia in 1814, and Sacchi in 1832, sentences to prove that these surgeons were theoretically in favour of ovariectomy. These will not much interest your readers; but the account of the case of Emiliani, in 1815, is very remarkable.

Dr. Gaetano Emiliani of Faenza was a provincial surgeon. A married woman, twenty-six years of age, mother of three children, while nursing her third child, lifted a heavy pan of water, resting the edge of the pan against the lower part of the abdomen. There was no immediate suffering, but the next night she was awakened by acute pain in the left iliac region, which continued during the next day. Dr. Brunetti was called, and "discovered at the seat of pain a tumour almost isolated in the cavity, without any alteration externally, very painful to the touch". Four months passed without much relief, and the patient suspected pregnancy. In two months, with a great loss of blood, she passed a very large mole ("*una assai voluminosa mole*"); uterine discharge continuing for twenty-five days. Dr. Emiliani was called in, and found his patient feverish, with evening exacerbations; the tumour, a hard circumscribed spheroid, indolent to the touch, but the seat of occasional spasmodic pain. The diagnosis was "scirrhus condition of the ovary and tendency to carcinoma", and the only hope, extirpation. The proposal was approved by Brunetti, who, with a phlebotomist, assisted at the operation, which I describe in the words of the son of the operator, who first published the account of the case.

"My father divided the common integument along the linea alba for about two inches and a half, divided the subjacent muscles and the peritoneum very carefully, and exposed the left ovary, which was recognised by its position, form, connections, although enlarged beyond all ordinary degrees, scirrhus, superiorly covered by small vesicles, from which a foetid ichor ran, and adhering by its whole inferior part to the colon; around, some varicose vessels were observed. The ordinary connections and the unusual adhesions were promptly separated, and the arterial twigs were tied one after another as they were divided. The blood lost was calculated at less than half a pound. The lips of the wound were united by one suture."

The after-treatment appears surprising in these days—rigid diet, bleeding repeated on the following day, tartar emetic, and lemonade. But recovery was complete; menstruation reappeared; and a year after the operation twins were born, which both died after a few hours. Five other children were born after these, two of whom, with their mother, the patient, were alive and well when the report of this case

was published by the son in 1843. Dr. Brunetti and the phlebotomist were also living.

"The ovary, after having been preserved twenty-eight years in alcohol, is pyriform, is nine *centimètres* long and five in its greatest breadth (three inches and a half by two inches). The peculiar structure is searched for in vain, being entirely degenerated by scirrhus. The specimen has been taken from my private museum, and is now deposited in that of the illustrious Medico-Chirurgical Society of Bologna."

There are several interesting points in this narrative. The ligatures appear to have been left in the abdominal cavity; and it is certainly desirable that the specimen should be carefully examined. If it be really an ovary, it will certainly appear that the first case of ovariectomy in Europe was that by Emiliani of Faenza in 1815. I have written to Dr. Peruzzi, suggesting that the specimen should be examined by some competent morbid anatomist.

THE CONNECTION BETWEEN STOMACHIC AND LABYRINTHINE VERTIGO.*

By EDWARD WOAKES, M.D.Lond.,
Surgeon to the Throat Hospital.

A GREAT deal of attention has recently been devoted to what is commonly called Menière's disease, but, which, from the anatomical organ to which the symptoms are mainly referred, is also known as labyrinthine vertigo. The elaborate treatment of Dr. Gowers, Dr. Hughlings Jackson, and Dr. Ferrier compasses the present knowledge of the subject, so far as it relates to that form of vertigo resulting from the progressive invasion of the labyrinth from lesions of the middle ear.

But there is another class of vertiginous patients in whom the auditory apparatus is, up to the time of the attack, perfectly healthy, and may remain so after it has passed off, whose symptoms are, nevertheless, due to a more or less temporary lesion of the labyrinth, to which organ, in fact, the general consensus of opinion refers every such disturbance of equilibration as expresses itself in giddiness. It is to this class of cases I wish to invite your attention this evening, and, as in their investigation we shall get some insight into stomachic vertigo, the area of interest of the subject will be proportionately widened.

The form of vertigo to which I allude is that in which the patient, without being aware that he is otherwise than quite well, is suddenly attacked with giddiness and falls prostrate. This may never in that patient's experience be repeated, or the attack may recur after a shorter or longer period. The cases in question differentiate themselves from those treated of by the authors referred to in the fact, already stated, that the auditory apparatus is previously quite healthy, though they possess this feature in common with them: that there is no loss of consciousness. After the attack, the patient finds himself in one of the following conditions: his hearing may be normal, as heretofore; or he may be quite deaf on the side to which he fell; or there may remain some impairment of hearing only, with probably confused noises in the head.

As regards concomitant symptoms, nausea and a splitting headache may remain. There is also a painful sense of fear, a horror lest the attack should be repeated, and a dread of becoming epileptic; to which apprehension the members of our profession who suffer from the attacks are specially prone.

The following example, the subject of which was a medical man personally known to me, illustrates some of their features. When about thirty years of age, and being fully engaged in a very arduous country practice, he was one evening summoned to a patient, being already fatigued with the usual day's work. On reaching the house, he became giddy, and, as the door was opened, fell into the hall, a proceeding which, though perfectly conscious, he was quite unable to avoid. It is needless to say he was very much frightened by the attack, but was not otherwise affected by it. In the course of the following year, a second seizure occurred exactly corresponding in character to the former; but, for more than twenty years subsequently, there was no repetition of the attack, although he was for a lengthened period the subject of intense headaches, with tinnitus, for which he submitted to the ordinary method of depletion then in vogue.

The time at which these events happened was over thirty years ago. Menière's disease was, of course, unheard of; and Flourens had not yet experimented on the semicircular canals. It was not, therefore, extraordinary that this patient should be haunted with the dread of becoming epileptic.

Adhering to the principle laid down in a former paper, we shall seek the clue to the symptoms in the source and vaso-motor relations

of the blood-vessels supplied to the region manifesting them: a proceeding which will show that this particular region, the labyrinth, possesses some very remote, and possibly unlooked for, alliances. At this point, it will be necessary to repeat what I have elsewhere had occasion to insist upon; viz., that the receptive part of the auditory apparatus receives its vascular supply from a totally distinct source—the vertebral artery—to that which is furnished to the conductive portion of the ear. This in itself is a suggestive fact, as the internal carotid, in its bony canal, is placed so close to the internal ear that one might naturally look to it as the source whence its vessels would be derived. As a matter of fact, however, neither the external nor internal carotid is in any way concerned with the circulation of the labyrinth. It is the vertebral artery and its relations we have to consider. The origin of this vessel, from the subclavian deep down in the neck, and its prolonged course upwards, guarded by the bony sheath formed by the vertebral foramina, acquire an increased importance when its relations to certain nerves are considered. In the first place, this position brings it into very close proximity with the inferior cervical ganglion, from which it derives a rich plexus of nerves, communicating, in their upward course, with the several cords which form the brachial plexus. It is important here to note that this lower cervical ganglion furnishes also the principal inhibitory nerve of the heart; viz., the inferior cardiac nerve. The experiments of Burdon Sanderson and others show that this nerve is capable of slowing the heart's action even to the extent of stopping it. We wish to point out its close relationship with the nerve which regulates the supply of blood to the labyrinth, both passing through the same sympathetic ganglion. Nor can it be without design that such an arrangement exists. Glancing for a moment at one aspect only of the functions of the semicircular canals, that, viz., by which, under circumstances of altered tension of their endolymph, they are capable of causing the individual to lose his equilibrium and fall to the ground, we get some insight into the object gained by associating the nerve which regulates this tension with that which tends to check the heart's action. The connection is such that an impression influencing the heart will affect the labyrinth. Thus a strong mental emotion arising centrally and propagated to the heart through its inhibitory nerve, which tends to stop its action, will also be deflected in the ganglion to the nerve regulating the blood-supply of the labyrinth, and, by suddenly changing the tension in this organ, will cause the subject to fall to the ground, so removing the mechanical impediment to the circulation which the upright posture implies. In this way, time is allowed for the excited influence of the heart to subside while the subject of it is placed perforce in the recumbent attitude.

Referring now to the communication between this ganglion and the brachial nerves besides those already mentioned, there are others proceeding directly from the ganglion to the brachial plexus. It is this arrangement doubtless which explains an occurrence noticed in gunshot-wounds of the plexus. It was first pointed out by Drs. Moorhouse, Mitchell, and Keen, in a treatise on gunshot-wounds of nerves published after the late American war. These observers record most succinctly that the subject of such a wound, whether received in the arm, axilla, or neck, immediately falls to the ground, without losing consciousness. This circumstance is quite unique; it has not been observed in connection with wounds of any other correspondingly non-vital part of the body. A certain amount of collapse attends these cases, from which it may be legitimately inferred that the shock is conveyed, not only in the direction of the labyrinth, but also through its inhibitory nerve to the heart.

Anyone who is conscious of the possession of what is popularly called the "funny bone" may, if he choose, verify these statements, at least to a certain extent, by giving it a blow considerably in excess of that which is sufficient to induce the well known tingling sensation in the fingers. It once happened to the writer to witness such an experiment in the instance of a lad who was struck on this spot by a hard tennis-ball thrown with considerable violence. Immediately he became giddy and confused in his head, and would have fallen but for the support of some railings, and altogether the extreme distress which he manifested appeared out of all proportion to the slowness of the cause. At the time of the occurrence, the symptoms were quite inexplicable by any known relations of the injured part. The observation of the American surgeons just quoted, as well as the anatomical continuity established through the inferior cervical ganglion between the brachial nerves and the labyrinthine circulation, afford, we venture to submit, a ready explanation of the phenomena to anyone who candidly examines them.

It will be apparent that we are taking for granted the physiological fact, that a shock communicated to the inhibitory nerve of a vessel temporarily withdraws its function, allowing it to become greatly distended with blood; it is this sudden distension which produces the pressure on the endolymph, and the consequent subversion of the

* Read before the Medical Society of London.