the rete mucosum. If this be the case, then, we ought to find it in ordinary jaundice of a temporary character. In the case I have related, the itchiness was an early symptom, and it became intense, and remained so up to the time the gall-bladder was aspirated on March 30th. The sudden removal of the fluid from the gall-bladder by aspiration took off the pressure, and the itching was relieved for two days; but, when the gall-bladder began to refill, the itching returned, and soon became a troublesome symptom. By the aspiration on March 30th, there were removed thirty-two ounces of fluid; by the operation on April 18th, there were removed twenty-four ounces of fluid; by this operation, all the fluid and all the free gall-stones (sixty) were removed, and a fistulous opening was established through which the dropsical fluid of the gall-bladder could and did escape in certain quantities. Now, after this operation, the itching never returned, and yet the skin was of the same deep brown mahogany colour that it was before the operation. If this colouration be due to bile-pigment, as we all believe, and if the itching be due to the direct action of the bile on the sensory nerves, why was the itching so modified by aspiration, and so perfectly relieved by the operation of establishing a fistulous opening for the constant discharge of the contents of the gall-bladder? This leads to the inference that the itching is not due to the irritating effects of the bile locally, but is a reflex symptom; that it depends more upon the pressure of distension; for we see, in this instance, that the temporary relief of the pressure and distension was followed by temporary relief of the itching; and that the permanent relief of the pressure and distension was followed by permanent relief of the itching. As death occurs often in this affection from passive hæmorrhages, so clearly the result of cholætoxæmia, it would be fortunate if we could diagnose it in its early stages, so as to resort to a radical treatment before the blood became so poisoned as to preclude the hope of cure.

The history of this case shows that the tumour was discovered on December 30th; that is, about one month after the jaundice set in. It then presented all the characteristics necessary for a complete diagnosis. But all our authors, without a single exception so far as I know, positively forbid any surgical interference until the tumour is found knocking at the abdominal parietes for the exit of its contents. Guided by the highest authority amongst us, we were not justified in performing any operation, except as a *dernier ressort*; and it was then done too late, as our patient died precisely as she would if no operation had been done. But, guided by the experience gained by this case, we may hereafter resort to operative procedures at an earlier day, and with greater prospects of success. No case like this ever gets well, unless nature or art, or both united, establishes a timely fistulous opening to relieve the gall-bladder of its contents, and thereby restore the channel for the bile. Now, what I propose is this. Whenever we have persistent jaundice, with clay-coloured stools, nausea, and intense itching of the skin, we may take it for granted that there is mechanical obstruction of the gall-ducts, and it is our duty to open the abdomen and search for the gall-bladder as soon as we can detect any swelling in the region of the liver. If we find a dropsy of the gall-bladder, we should deal with it as I have already indicated. If we have been mistaken in our diagnosis, then we have made a simple exploratory incision, which, under antiseptic precautions, is devoid of danger. I would extend the principle of exploratory incisions to other affections of the liver. We know that abscess of the liver is a very dangerous and often fatal disease. It sometimes bursts into the peritoneal cavity, causing rapid death from shock and peritonitis; again, it makes its way into some portion of the alimentary canal, or comes to the surface through the abdominal walls, or passes upwards through the lungs, simulating consumption; while, again, it destroys its victim by pyæmia before it can find an outlet by the slow process of nature. Only three years ago, one of the great statesmen of my own country died of abscess of the liver that made its way through the lung, and it was supposed that he had consumption till a *post mortem* examination revealed the true nature of the disease. And, about twenty-five years ago, one of the most eminent of American surgeons, hardly second to our illustrious Mott, died of pyæmic abscess of the liver, which was discovered only after death. With antisepticism and aspiration, it would now be possible to give relief to such cases when correctly diagnosed.

In cases of suspected abscess of the liver, I propose to make an exploratory incision, to introduce the fore and middle fingers, the hand, if necessary, to examine the liver minutely by the touch above, below, and in all directions; and, if we find fluctuation anywhere, to pass in a Dieulafoy trocar and evacuate the abscess. This I should expect to do with the same ease and the same impunity that belong to the exploratory incisions that we constantly make in doubtful ovarian tumours.

The great lesson that this case teaches is this. In dropsy of the

gall-bladder, in hydatid tumours of the liver, in suspected abscess of the liver, and in gall-stones, we should not wait till the patient's strength is exhausted, or till the blood becomes bile-poisoned, producing hæmorrhages, but we should make an early exploratory incision, ascertain the true nature of the disease, and then carry out the surgical treatment that the necessities of the case may demand. If this should be done under antiseptic precautions, I am sure that much suffering will be relieved, and many valuable lives saved that would otherwise be lost. Without Listerism, the operations I here propose would be hazardous; with it I believe them not only feasible, but perfectly justifiable.

THE MENSTRUAL SECRETION: INCLUDING CRITICAL REMARKS ON THE PATHOLOGY OF RETAINED MENSTRUAL FLUIDS IN THE ORGANS OF REPRODUCTION.*

By GEORGE HARLEY, M.D., F.R.S.

As there is almost more danger to medical literature from its becoming overburdened with false facts than with false theories, the interesting report of Mr. Lawson Tait's case, entitled *Menstrual Fluid Retained in the Left Falopian Tube, simulating Ovarian Tumour*, in the to-day's number (May 11th, page 677) of the *BRITISH MEDICAL JOURNAL*, induces me to call attention to certain "facts" concerning the physiological function of menstruation in the human female; a due consideration of which may, perhaps, induce Mr. Tait to somewhat modify the title of his very interesting case of retention of the menstrual fluid in the organs of generation.

The critical remarks I am about to make are entirely offered in a spirit of friendly criticism, and will, I trust, be received in an equally friendly spirit.

Not being a surgeon, I do not intend to review Mr. Tait's case from an operative point of view; but solely from the stand-point of a scientific physician; and, if I er in my judgment regarding the point of doubt, I shall be very pleased to have the error pointed out, and corrected by any one who may chance to be better versed than I am in the subject under consideration.

In order that the points of doubt shall be made perfectly clear, and the arguments stated in the fewest possible number of words, I shall put forward the case in a categorical form.

1. From the description of the pathological condition of the uterine organs, as well as from the tenor of the general remarks made by Mr. Tait throughout the paper, there can be no doubt that it was a veritable case of retained menstrual fluid in the organs of reproduction. But,

2. There exist many grave physiological reasons for doubting the justice of the author's assertion, that the fluid was retained in the left Falopian tube.

3. On the contrary, I imagine that the menstrual fluid evacuated by Mr. Tait did not come from the left Falopian tube at all; but from the uterus itself. And moreover, if he is correct in stating that the "organ" from which the liquid came was left-sided, I think it is not beyond the possibility of exactitude that the case was one of double uterus, and that the fluid was retained in and evacuated from its left cornu.

In order to show explicitly the grounds upon which I venture to propound this view, I shall ask, and answer, the three following pertinent questions:

A. What is the normal function of the human Falopian tubes?

B. What parts of the female organs of generation secrete the menstrual fluid?

C. What parts of the organs of generation are most likely to have their orifice or orifices occluded, and thereby become a receptacle for secreted, but non-excreted menstrual fluid, which might in due course admit of removal by surgical interference?

The replies to these three questions I shall now proceed to give in precisely the same categorical form.

A. No menstrual fluid (proper) is secreted by the Falopian tubes. The function of the Falopian tubes is simply passive, in so far as the function of menstruation is concerned; that is to say, the part played by the Falopian tubes, during the menstrual period, is precisely the same as the part played by the ureters during the process of urinary excretion. They are mere channels of elimination, and nothing more. The Falopian tubes are the ducts along which the ova are floated from the ovaries to the uterus. The ureters are the ducts along which the urine flows from the kidneys to the bladder; so that the passive func-

* This article is written according to the new method of spelling words, without encumbering them with needless duplicated consonants.

tions of both sets of ducts are precisely the same, viz., that of mere channels.

B. The menstrual fluid of the human female consists of three distinct parts: *a.* The ova; *b.* The fluid of the ovarian vesicle in which the ova are developed; *c.* A sanguineous discharge from the uterine mucous membrane. These three, combined, go to form what is known under the title of the menstrual secretion. I permit myself to speak authoritatively on this point, as I once enjoyed an opportunity of studying the physiological function of menstruation on a woman, who committed suicide when on the very brink of menstruating, such as no man, as far as I am aware, ever similarly enjoyed. The condition of things then found was carefully delineated in water-colors, by a professional artist, and still exist in all their pristine exactitude to verify every word which I am now about to say.

A healthy married woman, of twenty-three years of age, in a fit of jealousy, drowned herself in the Serpentine in 1861. At the time, the scientific committee, appointed by the Royal Medical and Chirurgical Society, was actively engaged in the investigation of the best means of resuscitating the apparently drowned. As a member of that committee, I had occasion to make the *post mortem* examination of the young married woman just mentioned; and, although the woman's organs of generation had nothing whatever to do with the question of suspended animation—as I was at the time in question lecturing to the students of University College on the subject of reproduction—after finishing the necropsy, in as far as the pathological appearances of drowning were concerned, I turned my attention towards the woman's organs of generation, when, to my surprise and delight, my eye caught sight of the congested surface of an ovarian vesicle, distended with fluid, of the size of a large hazel-nut, projecting beyond the exterior of the left ovary. Although not then fully conscious of the invaluable specimen which accident had thrown in my way, I carefully extracted the whole group of the internal organs of reproduction, and conveyed them, for subsequent scientific investigation, to the Physiological Laboratory of University College, which was at that time under my immediate superintendence. On examination, the immense value of the specimen at once became apparent.

The uterus, which was a "virgin one", was found slightly congested, and towards its internal fundus was a vascular, engorged, puffy portion of the mucous membrane, covered over by a thin adherent layer of sanguineous mucus—more blood than mucus—and from it, extended downwards to within half an inch of the os uteri, a conical-shaped layer of blood; not a single drop of which had, however, as yet reached the internal orifice of the os. The true state of matters at once dawned upon my mind; and accordingly, I at once transferred my attention to the ovaries, both of which were slightly congested. On the surface of the left ovary was seen an ova-follicle, all ready to burst. Had I doubted that the follicle was on the very verge of bursting, the sequel would soon have undeceived me. For while I was handling it, and that tenderly too, it burst beneath my fingers, and from it flowed a whitish serous liquid, which was instantly carefully collected and microscopically examined. One ovum alone was detected in it. The condition of the Fallopian tubes was next inquired into. The fimbriated extremities of both were found to be highly congested—much more so, externally, than either the external coat of the ovaries or of the uterus. But the internal coat of the tube was perfectly free from any trace of congestion whatever; and, by its pale white, shiny tendinous appearance, formed a very marked contrast to the highly congested lining mucous membrane of the uterus, the congested radius of which abruptly ended at the very orifice of the uterine ends of the Fallopian tubes. Strange to say, it was actually at the margin of the Fallopian tube orifices that the congested appearance of the mucous coat of the uterus was most marked. It may not be here out of place, incidentally to mention, that the right ovary contained a well-defined, non-impregnation, corpus luteum (commonly called false corpus luteum), as well as a beautiful, congested, unripe, ovum follicle, of the size of a large pea, embedded in its interior.

From these facts, as facts they undoubtedly are, I venture to propound the following theory as regards the normal function of human menstruation.

1. The menstrual fluid, as already said, consists of three perfectly distinct parts: ova, white serous ovarian-follicle secretion, and sanguineous mucus uterine secretion.
2. The ovaries and the uterus are the only parts which supply the menstrual secretion.
3. The Fallopian tubes do not secrete any of the ingredients special to the reproductive function.
4. The Fallopian tubes are mere passive canals for the transport of the secretion of the ovaries into the uterus.
5. No sanguineous menstrual fluid whatever enters the Fallopian tubes, in a normal state. If it gets there at all, it can only be from the uterus by an abnormal condition of regurgitation.

If this theory of normal menstruation be correct, it follows, as a natural sequence, that Mr. Tait erred in supposing the menstrual fluid that he evacuated had been pent up in a distended Fallopian tube. In case he should, however, still entertain any doubt on the subject, I shall now proceed to quote passages from his own paper, which are in direct confirmation of my views.

1. He states that the tumour "presented the red muscular appearance of the uterus".
2. A fragment of the tissue of the tumour, when subjected to microscopical examination, "proved to be composed of an abundance of unstripped muscular fibre".
3. "After the cyst was emptied", it contracted round and grasped the finger.
4. He "could find no canal leading into the uterus".
5. "About six quarts of thick dark-brown fluid, having the peculiar smell of menstrual fluid", were evacuated by means of a trocar.

These facts, taken in conjunction with the physiological description I have just given of the female organs of generation, as they appear at the menstrual period, seem to my mind to warrant the acceptance of one of the following theories regarding the source from which the grumous fluid was evacuated: namely, that the fluid was actually evacuated from a normal or from an abnormally formed uterine cavity. A. Because a grumous dark-brown fluid, posing a distinct menstrual odour, could not have readily existed at any other point of the reproductive organs above the os uteri. B. Because an occluded os uteri is not an unknown pathological condition.

Lastly, as the patient was an unmarried lady, thirty-eight years of age, and presumably a virgin; and as Mr. Tait distinctly states that it was from an organ on the left side of the body that the fluid was evacuated; it is by no means impossible that the patient was the subject of a double uterus, and that it was in the left occluded cornu of the uterus that the pent-up secretion was retained. This latter view is the one which accords most intimately with the description which Mr. Tait has given of the anatomical relations of the parts; and, had he only given as well a general history of the patient's catamenia, from the time of their first appearance up to the date of the operation, this theory could almost have been reduced to an axiomatic certainty, or the reverse.

SOME INDICATIONS FOR THE DIAGNOSIS AND TREATMENT OF AORTIC ANEURISM.*

By F. A. MAHOMED, M.D.,

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It has been frequently remarked, that no disease presents itself to the physician under more varied aspects, nor offers greater difficulties for diagnosis, than aneurism; it is desirable, therefore, while studying it, that we should employ every method of investigation we possess likely to throw light upon the subject. It is in this and similar obscure conditions that we naturally look to those modern instruments which are supposed to give precision to clinical observations, and ask, What can they do for us, and how can they advance our knowledge of the condition under discussion? As an instrument especially devoted to the investigation of conditions of the circulation, the sphygmograph has been called upon, in the diagnosis of aneurism, to remove all difficulties from our path, and to demonstrate with scientific accuracy the presence and position of the aneurismal sac. When the ever-varying conditions of the circulation in health and the innumerable modifications introduced by disease, are considered, surprise cannot be felt that the mere resultant of unknown and numerous forces—for as such the pulse may be regarded—does not afford sufficient data from which to calculate the character of each force concerned in its production.

The sphygmograph gives increased facilities for observations on the blood-stream; but, until the causes of its varying indications are well ascertained, the deductions drawn from them must necessarily be fallacious. Those, then, who attempted to make use of it without previously ascertaining these, blamed the instrument for their mistakes, rather than their own want of experience.

Some years ago, I attempted to give some account of the results obtainable by the sphygmograph in the diagnosis of aneurism.† The question, however, is one of so much importance, that I feel justified in returning to it and stating the results of further experience. In thus confining myself to the consideration of a single symptom, let

* Read in the Section of Medicine at the Annual Meeting of the British Medical Association in Manchester, August 1877.

† "The Physiology and Clinical Use of the Sphygmograph". *Medical Times and Gazette*, Nos. 9 and 10, 1873.