cases it is formed from hæmoglobin, but it may also be formed from the colouring matters which I referred to before as myohæmatin and the histohæmatins. I have two good reasons for saying so: (1) I procured hæmatoporphyrin²³ from these pigments; (2) hæmatoporphyrin occurs as such in the integument of some invertebrates²⁴ in which no hæmoglobin but a histohæmatin is present, for example, slugs, star-fishes, and the mollusc *Solecuretus strigillatus*, as I have shown.

Urohæmatoporphyrin is not confined to the urine of acute rheumatism, as I at one time supposed;²⁵ for although it is found in the urine of that disease in greatest abundance, and in its most typical condition, I have found it also present in the following, and perhaps other diseases, namely, so-called idiopathic pericarditis, Addison's disease, cirrhosis of the liver, croupous pneumonia, Hodgkin's disease, measles, typhoid fever, meningitis, and peritonitis.

From a careful consideration of the conditions under which this pigment may appear in urine, I conclude that it may be present (1) when an excessive destruction of hæmoglobin or the histohæmatins is taking place, the glands which are concerned in the further downward metabolism of these effete pigments being unable to deal with the excess of effete pigments present in the blood; (2) where the pigment-metabolising glands are diseased, as in Addison's disease and cirrhosis of the liver. The products of destructive metabolisms of these colouring matters—hæmoglobin and the histohæmatin—are got rid of as bile pigments and urobilin under normal conditions, but under diseased conditions the urobilin is replaced altogether, or it may be supplemented, by urohæmatoporphyrin. Probably an increased destruction of hæmoglobin takes place in inflamed organs, and the altered pigment being in a condition of chemical instability comes under the influence of some energetic reducing ferment. It might be suggested that bacteria are the agents concerned in this fermentation, for we know some of these organisms exercise a reducing, others an oxidising, action, but no bacteria are present in some of those diseases in which urohæmatoporphyrin is found.

Le Nobel, of Leyden,²⁶ has repeated my experiments on urohæmatoporphyrin, and although he differs from me in a few minor details, he has fully confirmed these results. He thinks that the above pigment, whether in urine or prepared artificially, is a mixture of two, but he did not study the action of ammonia on the band at F. This is the only point of difference between us worth mention. He objects to the name urohæmatin, which I abandoned in 1885, and another criticism of his is based on a mis-translation of one of my papers. Le Nobel has also found that hæmatoporphyrin is converted into urohæmatoporphyrin by the action of acetone or aldehyde, and he suggests that it is by the action of these that derivatives of blood-colouring matter may arise in the body, but this is "not proven," and the explanation is rather far-fetched. He also suggests that a reduction product of hæmatin, what he names "urobilinoidin," may have been mistaken for urobilin by Kunkel,²⁷ who shows that the latter is increased in amount after the absorption of large blood extravasations,²⁸ and that the increase of urobilin in fever which Leube and Stokvis²⁹ attributed to increased destruction of hæmoglobin, may have been due to an increased excretion of urobilinoidin also. I myself am quite satisfied, as I said above, that what has been taken for pathological urobilin, and so named by myself, is in many cases a direct metabolite of hæmatin produced by an energetic reduction process. I would here protest against the prevalent idea that the occurrence of (pathological?) urobilin is diagnostic of the existence of pelvic hæmatocele. It may have been present in some cases, but it is just as likely to be absent in others.

Besides urohæmatoporphyrin, other metabolites of hæmoglobin may be present in urine. In the urine of a case of purpura hæmorrhagica, my friend Dr. Armitage and I found a substance

present which, on the addition of one drop of sulphide of ammonium, changed into reduced hæmatin; had hæmoglobin or met-hæmoglobin been present, reduced hæmoglobin would have been obtained. Here evidently the circulating hæmoglobin had undergone some profound change.

Lately Drs. Lewin and Posner³⁰ have found such hæmatin-like pigments in urine; but they are wrong in supposing that in the majority of such cases the hæmatin has a renal origin; nothing but prolonged contact with acid urine could change hæmoglobin into hæmatin, and if freshly passed urine shows the reduced hæmatin bands when treated with a trace of ammonium sulphide, the blood has probably undergone some change before it has reached the kidney.

The interesting observations of Mr. Bond,³¹ of Leicester, show that the hæmoglobin of the blood is more prone to crystallise, and is therefore more chemically unstable in some diseases than in others.

In intermittent hæmoglobinuria I have found in every instance, where it was not due to a vasomotor disturbance in the kidney itself, that all the blood-colouring matter present in the urine had been changed into methæmoglobin. If the statement of Hoppe-Seyler³² be correct that when the urine contains methæmoglobin it also frequently contains bile acids, we have a clue to the causation of intermittent methæmoglobinuria. For if we suppose, that through some vasomotor disturbance of the hepatic circulation, some poison, such as an incompletely metabolised product of digestion or active ferment or the bile acids could pass over into the blood of the hepatic vein and thence into the general circulation, it could hardly fail to change a part of the circulating hæmoglobin into methæmoglobin, the latter would then become dissolved in the plasma—as is the case in methæmoglobinuria—and be excreted by the kidney.

There is one point in connection with urohæmatoporphyrin to which I should like to refer; it is this: The presence of this colouring matter in the urine of a case which is suspected to be of rheumatic origin should intensify our suspicion. It is almost always present in acute rheumatism. Hence the spectroscope may help one at times to settle a doubtful diagnosis.³³

Another fact which cannot fail to strike anyone is that in febrile cases we have a reduction product of hæmatin present in the urine; we have been taught that the febrile process is accompanied by increased oxidation; but it is just the opposite—it is characterised by increased reduction.

For maps of spectra mentioned here the various papers referred to in the foot-notes can be consulted, where the other characters of those pigments, so far as I have been able to study them, are given.

WHAT IS STONE IN THE BLADDER? A REJOINDER.

By SIR HENRY THOMPSON, F.R.C.S., M.B.Lond.,

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A FEW lines will, I hope, suffice to deal once more with the simple question I proposed above, and also with the long article raising numerous side issues—extremely tempting to reply to, if I had either time or taste for controversy—which Dr. Freyer has thought it necessary to write.

I must remark that the first statement he makes begs the question. He says: "Hitherto the universally accepted definition of stone in the bladder was any calculous formation, no matter of what size, which failed to pass out of the bladder spontaneously, and for the removal of which an operation was necessary."

But this is precisely the matter in dispute, and the contrary is in my opinion the fact. Surely it is notorious that small concretions and even large ones have been washed out, and removed from the bladder by forceps from time immemorial. But the procedure has never taken rank as "an operation for stone." I have operated for them thus and also by crushing them, many more than a hundred times, recording operations for "gravel" or "concretion," rigidly excluding them from my series of stone

²³ MacMunn: Further Observations on Myohæmatin and the Histohæmatins, *Jl. Physiol.*, vol. viii. No. 2.

²⁴ MacMunn: On the Presence of Hæmatoporphyrin in the Integuments of Certain Invertebrates, *Jl. Physiol.*, vol. vii. No. 3; and On the Hæmatoporphyrin of *Solecuretus strigillatus*, *Jl. Physiol.*, vol. viii. No. 6.

²⁵ MacMunn: *JOURNAL*, December 1st, 1883.

²⁶ Le Nobel: *loc. cit.*

²⁷ Kunkel: *Virch. Arch.*, vol. 79; quoted by Le Nobel, *loc. cit.* Cf. Ziegler's *Pathological Anatomy*, by MacAlister, vol. i. art. 68, and vol. ii. art. 268. This book contains many references to the subject.

²⁸ In two cases of this kind of which I have examined the urine, I have found pathological urobilin in large amount. In one I could prove that it had been derived from hæmatin, that is, the pigment closely resembled urobilinoidin.

²⁹ Leube: *Die Lehre vom Harn*; Stokvis: *Besprechungen*; *Deutsches Archiv für Klin. Med.*, 1893.

³⁰ Lewin and Posner: Zur Kenntniss der Hæmaturie, *Centralb. f. d. Med. Wiss.*, No. 20, 1887.

³¹ Bond: A Contribution to the Pathology of the Blood, *Lancet*, September 10th and 17th, 1887.

³² Hoppe-Seyler: quoted by Charles in his book on *Physiology and Chemistry*.

³³ *JOURNAL*: December 1st, 1883, and February 4th, 1888.

cases. Not a few of these were removed many years ago with Clover's aspirator, which I was in the habit of using to discover and remove these small bodies. Thus, among many other cases in my collection is one, in 1872, of 500 small concretions, weighing together a drachm or more, all thus removed, but never recorded as an operation for stone. It was an operation for gravel, a proceeding which Dr. Freyer appears to ignore; for one of the cases in which he has merely washed out a particle weighing 2 grains, is actually recorded as an "operation for stone!" He says, and rightly, that there is no hard-and-fast line in the matter of weight between stone and concretion. Then, if a 2-grain stone be admissible, why not a 1-grain, or a $\frac{1}{2}$ -grain stone, or less, if the patient fails to pass it naturally? Is there henceforth to be no operation for gravel or concretion, in order that stone operations may be multiplied?

But Dr. Freyer quotes a definition of stone I formerly made, and says I have recently modified it. That is precisely the course I desired to take, and intimated it in my lecture in autumn 1887. When making the first, I confess I never supposed any English surgeon was likely so to construe it as to regard a tiny piece of gravel as "a stone in the bladder." The intention which prompted the later remarks—and I had a very distinct one—pointed quite in a different direction. Hence it was a matter of astonishment and severe disappointment to me, who respected and admired Dr. Freyer's skill and energy, to learn that he condescended to record these trifles in terms which have never been used before. For I have the means of knowing that such a precedent is opening the door to practice outside our profession, of a very questionable kind.

To close a paper already too long, I think there is little doubt, judging from the opinions which have reached me here, and from abroad, that the common sense of the profession will exclude a trifling concretion of 2 or 3 grains from taking rank under the well-known and time-hallowed term a "stone in the bladder." A hard-and-fast line being impossible, most surgeons will probably agree that a product at least four or five times that weight might be held to constitute the smallest size to which that title should be applied. In my own collection there is nothing less than 20 grains.

To that common sense I leave the verdict, feeling more strongly than ever, after reading Dr. Freyer's second communication, how necessary it was to raise this question. I did so with great reluctance, and with a feeling of infinite regret that a paper of Dr. Freyer's should have furnished the occasion.

PERFORATION OF THE PERINEUM DURING LABOUR.

By JAMES FERGUSON, M.B., C.M.,
Perth.

CASE I.—On March 13th, 1887, I attended Mrs. J., aged 25, in her first confinement. On arrival I found a well-advanced second stage, with an occipito-anterior position of the head. The patient's condition was good, the pains frequent and strong, and everything seemed to favour a normal termination. After a short time, with my finger over the fourchette, I was conscious of a sudden considerable movement of the child's head, followed by a profuse rush of fluid, both felt and heard, all this coinciding with the height of a pain. Immediately exposing the maternal parts, I was impressed by the enormous distension of the sacral portion of the pelvic floor, including the anterior wall of the rectum, and just in front of the anus noted a nearly triangular space of dark colour, which on closer examination proved to be nothing else than a portion of the child's scalp applied to a tear in the perineum.

Finding the margin of the vulva lying quite flaccid and free from pressure, my first thought was to hook it backwards, so as to bring it into the axis of expulsion, meanwhile detaining the head in the pelvis by my own efforts, the patient being ordered to desist from hers. But the uterus seemed only aroused to increased energy by the measures adopted, and, in brief, within a few seconds a male child of average proportions passed wholly through the abnormal outlet. The placenta was suddenly extruded by the same opening in a few minutes. Examination showed the rupture to extend through the external sphincter ani, while anteriorly almost an inch of skin separated the perforation from

the vulvar orifice; the posterior vaginal wall was torn for an inch and a half upwards.

The injured parts were smeared with eucalypto-vaseline (1 to 9) as the only antiseptic at hand, and then appropriately brought together with carbolised catgut. Two sutures in the vaginal wound and half a dozen in the perineum were used. Opium was prescribed, to favour constipation rather than to soothe the pain, which was not complained of after the suturing, and the subsequent treatment fell to my friend, Dr. Sinclair Kennedy, in whose detention I had been temporarily acting in the case. Antiseptic injections and applications were employed, and the bowels not moved till the ninth day. By this time the wound had healed, for the most part as if by first intention, and the patient described herself as only uncomfortable by being kept in bed. The temperature never passed 99° F. By the twenty-first day the patient could walk, and alleged she felt no pain.

Meanwhile, Dr. Kennedy had ascertained an abnormality of the pelvis which quite accounted for the abnormal labour, and by his kind request I was allowed to confirm his observation. The chief facts were an unusual depth of the symphysis pubis and undue narrowness of the pubic arch. The vertical measurement of the former, allowing freely for its soft coverings, was fully three inches and a half, an excess of two inches over that commonly assigned to the female symphysis. Exact measurements of the arch were not possible, but the male type was very perceptibly presented, an acute angle taking the place of the rounded arch, and the pillars approximating in a corresponding manner.

CASE II.—Called to Mrs. P. in her third confinement on April 2nd, 1888, I found the head approaching closely to the perineum. The finger also detected a hard wedge of tissue extending upwards and backwards from the vulva, which suggested inquiry about former labours. My patient told me that at her previous delivery forceps were used by a doctor well-known in obstetric work in the town she then lived in, and that, though nothing was said about a tear at the time, for weeks after she began to move about "something like a piece of flesh hung down," and caused much pain, especially when sitting. As the child's head distended the perineum, the above-mentioned wedge persisted in passing doggedly forwards, refusing to share in the dilatation, and carrying the labia before it. Instructing the patient to use no voluntary efforts, and to cry out during the pains, I pressed the head backwards and upwards towards the sacrum, and attempted to dilate the vulva by stretching with two fingers over the fourchette (an old method recently revived, *vide* JOURNAL, 1886, vol. i, p. 12). The head was then allowed to come down with a minimum of pressure, the hand trying to guide it forward past the obstruction. Despite these methods, however, the hard body still advanced before the child, and I was alarmed to detect a sudden yielding of the perineum just in front of the anus during a pain. Recognising only danger in further delay, I pushed the head clear away from the pelvic floor, and with some difficulty—continuous uterine effort having been provoked—adjusted the forceps and delivered by the vulva, its margins yielding freely. The perineal wound was afterwards found to be almost three-quarters of an inch in length, communicating with the vagina, the sphincter escaping by a thin cord of skin. The wedge behind the vulva consisted of cicatricial tissue, passing about an inch upwards and obliquely backwards, and, strange to remark, bore trace of only the slightest laceration. A deep and superficial stitch having been inserted, and weak lotions of Condy used, the patient made an unretarded recovery, the wound healing apparently by first intention.

The treatment of perineal lacerations being a subject on which conflicting opinions prevail, the last-mentioned case may be regarded as evidence of some value on one side of the question. It is clear that a not inconsiderable tear at the time of the second confinement was ignored, and was the cause of much suffering for weeks afterwards. Not only so, but the closer apposition of the torn surfaces secured by timely stitching would have saved the patient suffering at her third confinement, and averted the risk of an "accident" of a serious nature, and one which would have certainly occurred had not professional assistance been at hand. May I not also ask, supposing more serious injury had occurred, despite attempts to avert it, on whom would any possible discredit have fallen, the first or the second practitioner?

As to the treatment appropriate to those cases of central rupture which are recognised before the birth of the child, Charpentier has advised that, to save the sphincter ani from danger, the tear should be carried forward by a cut into the vagina.