cured, as she had informed me on several occasions. Naturally desirous of being certain how far the bromidism contributed to the result, I determined to test the question by excluding other conditions.

My second case was that of a pilot, aged 36, who had morphine injected hypodermically for urethral troubles in 1893, and later in that year for kidney mischief, after which he continued the habit without a day's interval until he came under my care. In the summer of 1896 he injected 40, 50, and 60 grains a day. Attempts had been made to withdraw the drug slowly, the last occasion being just before he came into my hands on February 26th, 1897, when he was injecting 5 or 6 syringefuls a day, each containing a grain. At that date he was greatly depressed, trembling in lips and limbs, irritable, unable to sleep without the injections, his upper and lower limbs literally covered with scars, nodules, and boils in every stage of development wherever he had been able to introduce the needle. He was admitted to hospital on March 1st, gave up syringes and supplies, and with some reluctance consented to the deprivation of the morphine on my own terms. A good deal of trouble at first arose from the boils on the arms, the movements of which were very painful and much restricted. The urine was normal. On March 6th he went out of hospital, unaccompanied for the first time, and on his return he at once gave up, when asked, a syringe and morphine solution which he had obtained at a Chinese drug store, informing me that the European drug store had refused to serve him. From March 2nd he had three injections of morphine daily, totalling on each day consecutively in grains $\frac{1}{2}$, $1\frac{1}{2}$, 1, 1, $1\frac{1}{2}$, $1\frac{1}{2}$, 1, $\frac{3}{4}$, $\frac{3}{4}$, $\frac{3}{4}$, $\frac{3}{4}$, and $\frac{3}{4}$ (15th.) He was ordered 30 grains sodium bromide every six hours on March 2nd, changed to every four hours on March 4th. On March 6th it was discovered that the *pharmacien* had made a mistake of 20 for 30 grains in his draught, and this was then rectified and given every six hours, and increased on March 8th to every four hours. No evident effect having been produced, except bromide rash on the forehead, on March 13th the dose was given every three hours, and on March 15th (the last day of the morphine) was doubled (60 grains). Next day he was stupid but able to walk about, very shaky, and on March 17th very drowsy in bed. The dose was reduced to 30 gr. next day, and on March 19th he got out of bed repeatedly, when the dose was increased again to 60 gr. every three hours. He had not asked for morphine since March 15th. On March 20th he was very stupid, answering incoherently, and from this day no drug of any kind was given. He practically slept till March 23rd, taking only milk from a spoon, about half a glass every two hours, muttering incoherently occasionally, unable to sit or stand, passing urine and stools in bed. He began to take solids on March 27th, but was still unable to stand on March 29th, and still drowsy. On April 1st he was talkative, only giving expression to nonsense, unable to walk even with assistance. On April 3rd, speech was still indistinct, but improving with the power of locomotion. He fed himself for the first time on April 10th and dressed, taking a long time over it, putting his shirt on over his vest. On April 12th he wished to go to Harrogate, not remembering where he was. On April 14th he drove out, recognised that he had delusions, and laughed over them. On April 17th he left hospital, and on April 27th left Shanghai for his work, feeling, he said, quite well and rejoicing over his deliverance.

An interesting feature in this case was that when, as he described it, his head was muddled with liquor so that he could not tackle work, an injection of morphine enabled him to resume work with a clear head at once. The desire for liquor was lost with that for morphine. Pulse, temperature, and respiration were never disturbed during treatment to any extent requiring notice.

No doubt in both these cases the mental and somatic symptoms were to some extent attributable to the morphine deprivation as pointed out by Erlenmeyer and others, but this could be to a slight extent only, from the rapidity of onset, and especially from the degree and nature of interference with the nervous function. It may be asked if it is justifiable to produce such a condition of the nervous system. It seems to me that the end justifies the means.

The following advantages over other methods of treatment can be claimed for the method pursued in these cases :

1. It did away with the suffering entailed by stopping the drug.
2. The patient could not bribe the attendants when the drug was withdrawn, he could not deceive his doctor, nor could he escape vigilance—he was powerless.
3. It acted equally well, whether the patient wished to be cured or not.
4. No special attendants or establishment was needed, only nurses who took ordinary care.
5. No violence or excitement is likely to result from, nor a taste to arise for, bromide given in this way.

In my next case I intend to, and I would suggest that anyone trying the method should, reduce the morphine by halving the dose each day, not giving it if not asked for, pushing the bromide more rapidly than I did in those cases, not, however, beyond the stage of marked torpor, stopping it altogether before that stage if the patient has ceased to ask for morphine. It may not be necessary to produce so marked a degree of bromidism as was present in these cases.

A CASE OF PERFORATING WOUND OF THE ABDOMEN

IN WHICH THE SUPERIOR MESENTERIC VEIN WAS WOUNDED :
LIGATURE OF VEIN : LAVAGE OF ABDOMEN :
TRANSFUSION : RECOVERY.

By A. W. MAYO ROBSON, F.R.C.S.,

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THE following case, which has been reported for me by my house surgeon, Mr. Levi, presents several points of interest which seem to make it worthy of separate notice.

On January 27th of this year, there was admitted into the Leeds General Infirmary, a young mechanic, aged 20, in a collapsed condition, with all the signs of internal hæmorrhage, pulse feeble, 90, temperature subnormal, skin pale and covered with a cold, clammy, perspiration. He was said to have vomited blood directly after the accident. He was found to be suffering from a penetrating wound of the abdomen, caused by the sharp end of a file without a handle, which having caught in a revolving wheel of a lathe was pushed through his clothes and abdomen. There was a small punctured wound a little above the umbilicus and almost in the middle line. The abdomen was nearly flat, with dullness in both flanks. Liver dullness was present and there was resonance over the front of the abdomen.

OPERATION.

The patient was immediately taken to the theatre and ether administered, the surface of the abdomen having been rapidly cleansed and carbolised. An incision about $4\frac{1}{2}$ inches in length was made above the umbilicus including in its line the site of the puncture. When the abdomen was opened, there was no escape of gas from stomach or intestines, but the abdomen and pelvis were found to be filled with dark liquid blood, which after a careful search, and after turning the intestines over to the left, was found to be escaping from the trunk of the superior mesenteric vein which had been wounded by the file. The superior mesenteric artery could be felt pulsating close by. The divided ends of the vein were ligatured with catgut. The small intestines were black, owing to effusion of blood beneath the peritoneal coat. The spleen, liver, and other solid organs were then examined, but no injury was found in any. The blood was washed out of the abdomen by free lavage of the peritoneum with warm boracic lotion. Cyanide gauze was packed gently over the ligatured ends of the vessel, the end of the gauze being drawn lightly through a rubber drainage tube out of the abdominal incision. The peritoneum was then closed with a continuous catgut suture, as was also the fascia over it. The skin was united with silk-worm gut sutures. The wound was dressed and the patient sent back to the ward.

TRANSFUSION.

On reaching his bed he was in such a state of collapse that transfusion was performed by Mr. Seaton, the resident surgical officer. Three pints of normal saline solution were in-

jected, and the pulse, which was about 180 at the end of the operation, came down to 120, and was much stronger. He appeared to be considerably relieved.

NOTES ON AFTER-TREATMENT.

January 23th, 1897. Patient looked much better. Temperature 98.6°, pulse 126, respirations 28. Wound dressed. January 29th, 1897. Wound was dressed and part of the gauze gently withdrawn. Flatus passed *per anum*. February 2nd, 1897. The gauze packing which had been shortened daily was wholly removed. Temperature fluctuating between 99° and 100°, pulse 100. Patient cheerful and looked well. There was no fullness of epigastrium and no tenderness. There was no marked anaemia. February 4th, 1897. Bowels moved.

February 5th, 1897. The temperature rose to 104° last night owing to a stitch abscess, although the remainder of the wound looked quite well. This was manifestly dependent on the operation having to be undertaken without time to prepare the skin properly. There was neither fullness nor tenderness of the epigastrium and no sign of intraperitoneal suppuration. The daily dressings were discontinued, and boracic fomentations ordered to be applied every four hours. February 7th, 1897. There was free suppuration around several of the sutures, which were all removed. February 11th, 1897. The wound was beginning to look better and there was less pus on the fomentations. February 14, 1897. Very little suppuration. February 16th, 1897. Wound doing well. Fomentations were discontinued and the wound dressed simply. February 25th, 1897. Wound almost completely healed.

March 4th, 1897. Wound healed. An abdominal belt was applied, and he was allowed to get up. March 6th, 1897. Patient was discharged to-day quite well.

REMARKS.

The nature of the injury is remarkable in that the perforating instrument, being somewhat blunt at the end, must have pushed aside the stomach and colon instead of perforating, though the fact of blood having been vomited shows that the stomach must have received the impact of the file, and have suffered injury to its internal coats. The mesenteric vein lying at the back of the abdomen should be one of the vessels least liable to injury, and probably could only be lacerated by a stab or gunshot wound. The extensive retroperitoneal hæmorrhage gave rise to the suspicion that the pancreas might have been injured, but on enlarging the aperture at the root of the mesentery made by the file, the freely-bleeding vessel was seen and easily ligatured.

Having arrested the hæmorrhage and cleansed the abdomen of blood and clot, the question arose in my mind as to the behaviour of the intestines under their new circumstances, for, in addition to the blood extravasated beneath the peritoneal coat, all the vessels must have been seriously congested owing to the return of blood along the superior mesenteric vein. Paralysis of bowel and peritonitis at least was to be feared—possibly gangrene—but, fortunately, neither complication interrupted recovery.

SALOL CALCULI.

By C. R. MARSHALL, M.B. VICT.,

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In connection with the recent communication of M. Brossard to the Académie de Médecine (March 30th),¹ the following account may be of interest.

Some time ago Professor Bradbury sent me an almond-shaped crystalline mass, possessing a yellowish colour and weighing about 1 gramme, for examination. It was said to have been vomited by a young lady who had been taking salol for some months. The drug had been ordered for intestinal flatulence, and had given marked relief. For nearly six months a cachet containing 10 grains was taken once or twice daily. Then attacks of severe colic, accompanied by vomiting, occurred, which nothing would relieve except the hypodermic administration of morphine. In one of these attacks the body in question was brought up, and at the time the statement was volunteered that similar masses had been frequently passed by the bowel. The substance had a salol-like odour, and gave the chemical tests of this compound. The melting point was almost correct, so that the substance was practically pure. The salol was stopped, and the patient improved. Dr. Bradbury tells me that no further attacks have occurred.

Brossard's case² was a neurasthenic lady, aged 45, suffering from gastric dilatation, with paroxysmal gastralgia, gastrosuccorrhœa, and hyperchlorhydria. Vomiting was frequent, and was sometimes accompanied by hæmatemesis. The patient refused lavage, and the treatment consisted in the administration of large doses of alkalies and a milk diet. The attempt at a milk diet produced febrile symptoms, and salol and calomel were

therefore given. The salol was administered in 0.5-gramme ($\frac{7\frac{1}{2}}$ grain) doses, and 4 to 5 grammes (62 to 77 grains) were given daily. After ten days (that is, after the administration of 40 grammes, rather more than $\frac{1}{4}$ ounce), the patient, who was habitually constipated, presented severe symptoms of intestinal obstruction. Purgatives, large enemata, the continuous current, were repeatedly tried, but without effect, and the symptoms continued for thirty-six hours. Finally, a motion was passed, and thinking that a biliary calculus might have produced the pain it was carefully washed. About ten crystals, weighing altogether 4 grammes (62 grains), were found, the largest of which weighed 1.8 gramme. Similar small calculi occurred in the two following motions. They all consisted of pure salol.

An interesting feature in the case I have mentioned is the occurrence of a calculus in the stomach. As the vomit was not bilious it is probable that this was the seat of formation. Girode,³ in a case of cholera which had been given salol two days before death, found two masses of salol, weighing 3 grammes, in the stomach *post mortem*, and as far as I am aware this is the only similar case on record.

The question of the formation of these calculi is an important one. Brossard consulted Pappel, a chemist, and they concluded, probably rightly, that it was inadmissible that salol could be decomposed into its constituents, and that these should unite again. Experiments were then made to determine if, in an acid or alkaline medium, in the presence of pepsin or pancreatin, salol could be dissolved without decomposition, and, on changing the medium, if it could be deposited anew in the crystalline form. But only negative results were obtained. It was then thought that on account of the constipation the drug would only be eliminated slowly, and that a new dose would come into contact with the decomposition products of the old ones, that is, with salicylic acid and phenol. The effect of these upon the physical condition of salol was then determined. Salicylic acid was without effect. A solution of phenol, however, caused the powdered salol to agglomerate, and after some time to become liquid. It did not mix with the carbolic solution, but assumed the form of a transparent ball. If now the phenol solution was replaced by water, and a crystal of salol added, the whole of the salol was converted into a crystalline mass. This displacement of the phenol may be regarded as analogous to its absorption in the body.

The theory seems to me weak in many points. The phenomenon described by Brossard cannot be obtained at ordinary temperatures in this country. It is quite true that salol will melt at a lower temperature in an aqueous phenol solution than in ordinary water, and this may account for the results obtained by Brossard and Pappel, whose experiments were carried out at Cairo. (In connection with the phenomenon it should be mentioned that phenol is capable of dissolving salol.) But even if we grant the results of those observers to hold at all temperatures it is difficult to accept the view put forward by M. Brossard. The most essential factor—the presence of sufficient carbolic acid to produce any influence—is improbable. Salol is only gradually decomposed in the intestine, and it is very probable that most of the carbolic acid formed is quickly absorbed. The explanation seems to me a simpler one. It is due to the fact that the melting point of salol (42° C.) is a little above the normal temperature of the body. Very often, especially after meals, the temperature of the stomach is above this point, and if salol is present melting of the crystals must occur. The fluid thus produced does not readily mix with the food, and when the gastric temperature sinks to, or better, as after partaking of cold water, below the normal, recrystallisation occurs. The mass thus produced is not composed of minute individual crystals as in the first instance, but is in the form of single solid lumps made up of the crystals in close union. That this may occur can easily be demonstrated by a few test-tube experiments. The solidification does not necessarily occur in the stomach. It may happen in the intestine, even in the presence of a 1 per cent. alkaline (sodium carbonate) solution. When pure salol is administered in cachet form the best possible condition for the formation of a calculus is present. The crystals, when softened or melted, readily cohere, and may subsequently recrystallise. Even in a well-made emulsion, when heated to 40° to 42° C., the salol tends to aggregate, though I have not seen perfect conglomeration. Probably the reasons that salol calculi have not been described oftener are (1) they have not been recognised; and (2) that some gastro-intestinal disease is necessary as a predisposing cause. In Professor Bradbury's case