could be seen blood discs, epithelium, and numerous small single and double bright refracting bodies. With a more powerful eyepiece, movement could be made out in the field.

Next day the rabbit did not eat quite so well as usual, but did not refuse food; on the 23rd she was apparently all right. On the 27th she gave birth to a litter of five; on April 8th she weighed 3 lbs. 11 ozs.; May 30th, 3 lbs. 11½ ozs., when with the others she was removed from the hutch and allowed to burrow in the garden. June 25th, she weighed 4 lbs. 3 ozs., after which she had another litter of, I think, six. On 11th August she was quite well.

Experiment 2.—A doe, weighing 4 lbs 2½ ounces, was injected with fifty minims of the same fluid, and at the same time as the last rabbit. She did not appear to be disturbed in any way, ten days later weighing 4 lbs. 2 ozs. On April 7th she had slight diarrhoea, on the 8th she weighed 4 lbs. 2½ ozs.; on May 30th, 3 lbs. 7 ozs. For about a fortnight at this time we had very hot, sultry weather, and the rabbits were then in a small hutch only open in the front, and in a back verandah opening into a closed court admitting sun but no breeze. Two other rabbits looked out of condition at the same time, one of these being unaffected. I then removed the hutch to the garden, and allowed the rabbits to run at large and burrow, when the health of all improved. On June 25th she weighed 4 lbs. 10 ozs., and on July 9th gave birth to a litter of six. On August 11th she was quite well.

Four rabbits were injected with fluid collected from Mrs. T., a primipara, who had a tedious labour, was delivered on May 26th, 1882, with short forceps, and had a slight tear of the perineum into which two superficial carbolic silk stitches were introduced, though some might have considered these unnecessary. Healing took place by first intention. On the second evening, the mouth temperature was 99°, third morning, 99.2°; evening, 99.8°; fourth morning, 99.2°; evening, 98.6°. No odour was perceptible in the napkins at any time, and convalescence was good.

Experiments 3 and 4.—Sixty-five minims of fluid collected seventy-three hours after delivery from Mrs. T. were injected into these animals three-quarters of an hour after collection. The fluid was red, odourless, and exhibited under a quarter inch objective, a few epithelial cells, blood discs, granular matter looking like oil globules, and no moving particles were visible. These rabbits seemed in no way influenced by the operation. One weighed, on May 29th, 3 lbs. 8 ozs., and on June 21st gave birth to a litter of four, and on the 25th weighed 3 lbs. 15 oz.; while the other weighed 3 lbs. 14 ozs. on May 29th, on June 25th, 4 lbs. 10 ozs.; and gave birth to a litter of six on July 6th. On August 11th they were both well.

Experiments 5 and 6.—On May 30th, 1882, sixty-five minims of fluid, collected ninety-five hours after confinement from Mrs. T., were injected into these two animals an hour after collection. The fluid was reddish, odourless, and presented under a quarter inch objective, epithelial cells, broken down blood discs and granular matter—no movement was visible in the fluid. Next day both animals ate well, and were as active as ever; one gave birth to a litter of seven on June 9th, and to a litter of six on July 10th, whilst the other gained flesh greatly. Neither being in any way, apparently, influenced by the injection. Both were well on August 11th.

I injected eleven rabbits in all, but as five of these were with fluid collected on the second and third days after delivery, I shall omit them, though in doing so I weaken my case. I would call attention to the fact, that the conditions were somewhat more trying than those which accompanied Braidwood and Vacher's experiments. The fluid was not dialysed or mixed with any antiseptic in my case before being injected, in four cases it was collected during much hotter weather than occurs in England, the dose injected in every instance was larger, and both cases supplying the fluid were complicated with perineal rupture and the use of forceps.

It seems to me that the result of these six experiments, at least, throw a large amount of doubt upon Braidwood and Vacher's conclusions *a. b. c.* and *f.* It may be said, that in any experiments, some of the carbolic solution from the morning syringing remained in the vagina twelve hours. This is possible, but I reply that I habitually pressed the hypogastrium to squeeze out any blood or fluid that might have lodged in the vagina; that the solution used was a 2½ per cent. one; that there was not the faintest odour of carbolic acid in the lochial fluid collected, that if there had been any acid in the vagina, it must have been well diluted with the distilled water which was used to wash out the discharge. And, finally, Braidwood and Vacher cannot raise this objection, as they, in concluding, say, "antiseptic solutions mixed with the septic lochial fluid do not appear to modify its lethal influence."

The table at the commencement of this paper records thirteen expe-

riments. Braidwood and Vacher performed many others* with the lochial fluid; but I have rejected—

1. All that were injected with fluid other than that of the fourth day. (See Conclusion *a.*)

2. All experiments with other animals than rabbits. (See Conclusion *a.*)

3. All that were treated otherwise than by injection into the pelvic peritoneal cavity. (See Conclusion *f.*, where it is stated that this is "the surest method".)

The numbers bracketed together in the table were injected with fluid collected from the same patient. It is of "normal" lochial fluid that the production of septicæmia is predicted. Nos. 4 and 5 were injected with fluid which had been kept in a dialyser for three days, and Nos. 6 and 7 for seven days, after collection. I do not suppose that the experimenters assume fluid thus treated to be "normal". Again, No. 3 had thirty minims of a saturated solution of cupralum; No. 9 thirty minims, and No. 10 twenty-two and a half minims, of a 5 per cent. solution of carbolic acid injected along with the lochial fluid—mixtures which are not "normal". Deducting, therefore, seven cases as abnormal, there were six rabbits injected with normal lochial fluid of the fourth day, with three deaths and three recoveries—a ratio which cannot surely warrant the use of so strong a term as "invariable", particularly when the proof of septicæmia is stated to be a *post mortem* one. Conclusion *b* runs thus: "It invariably induces in them septicæmia, as proved by *post mortem* examination." Braidwood and Vacher use the expression "normal lochial discharge" without defining the term normal.

From the six experiments which I have described, it is safe to infer that human lochial fluid, collected on the fourth day after confinement from patients treated after the antiseptic method described, is not septic to rabbits, when it is injected into the pelvic peritoneal cavity of these animals soon after its collection in the manner described, and in doses of sixty-five minims and less; and this affords at least a presumption that the septic influence (when present) of the discharge on patients so treated is lessened, and, it may be, destroyed.

From the experience of the last four years, I am in a position to claim the following advantages for the antiseptic (would that we could call it aseptic!) method of procedure described.

1. The shorter duration of the discharge; there being only bloody and serous, instead of red, green, and white stages.

2. Its lessened amount while it lasts.

3. Its freedom from odour.

4. Increased comfort to the patient from the above three, and its anæsthetic effects on the parts.

These points have been remarked by the patients as well as by myself; and, even if there were no other and supposed greater advantage, they alone would, in my opinion, much more than compensate for any disadvantages. Besides the trouble involved, the only drawbacks that I have observed are the sensation of burning when the patient is syringed, most pronounced when there is breach of surface; and twice I have remarked giddiness immediately after syringing. The former feeling passes away almost before the washing is over, and the recurrence of the latter can be avoided by diluting the solution. These objections only apply to the antiseptic used, and not to the principle involved. I have had no case of puerperal fever since I began the systematic practice described. But there is no branch of the healing art in which the fallacy of "a run of cases" is so common as in midwifery; and no statistics, unless those dealing with large numbers, can be of any use in a matter such as this.

SALICYLATE OF SODA (1) IN TONSILLITIS, AND (2) AS A LOCAL APPLICATION IN GOUT.

BY EDWARD MACKEY, M.D., Brighton.

(1) A PAPER by Mr. Hormazdj, recently published in a contemporary, treats of a clinical point deserving, I think, more attention than it has yet received—I refer to the value of salicylates in tonsillitis. The good effect of these salts, when prescribed for that complaint, has been illustrated by the clinical records of Fowler, L. Browne, Hunt, and others, in the JOURNAL of one or two years back; but is not yet so widely recognised as to be independent of further evidence. My personal experience is not large, but is based upon several very severe and acute cases in which ulceration had occurred before salicylate of soda had been administered. It was then given in ten-grain doses every two to four hours, and in each case I

* BRITISH MEDICAL JOURNAL, January 28th, 1882, pp. 108-11, Table of Septicæmic Experiments.

was struck, not only by the lessening of the pyrexia, but also by the relief credited by patients themselves to each dose of the remedy—much as one almost expects to hear of it in acute rheumatism. One surgeon, himself a sufferer, notes marked relief within twenty minutes (*Mid. Med. Miscel.*, No. 6).

Hormazdji recommends twenty-grain doses; and, having noted relief in fifty-seven cases of tonsillitis caused by cold and damp, concludes that such cases give the best results, and that, when the malady arises from bad air or drainage, it is less amenable. This seems reasonable, but is not exactly my experience. For instance, of a family of nine children living in a house too small for them, and defective as to drainage, five in succession got severe acute tonsillitis, several having a temperature over 103° Fahr., and nocturnal delirium, and all going on to more or less ulceration. The earlier cases got a calomel purge, small frequent doses of aconite and potash-chlorate, sulphurous spray, a benzoïn inhalation, and ice; later, iron; they were relieved within thirty-six hours, but all relapsed on the third day. It was only when the sixth child—a delicate one—was attacked, that I had become aware of this use of the salicylates, when I found him suffering with severe throat-symptoms like the others, I ordered, after his purge, five grains of the soda-salt every two hours in liquor ammoniæ citratæ. The boy, who expected the same illness he had seen his brothers and sisters go through, expressed, with surprise, relief from the first few doses, was free from pain in twenty-four hours, had a very slight patch of ulceration, and no relapse. (The above prescription, which I gave this patient, should form a clear colourless solution, which agrees well, but, when dispensed by some chemists, I have known it turn brown, and cause irritation and vomiting; for this I have no satisfactory explanation.) Since this note was written, some half-dozen other cases of ulcerative tonsillitis have shown similar good results with the same remedy; of course, when actual suppuration (quinsy) has occurred, equal relief will not be expected.

(2) N., a man nearly 70, with what he called "compressed gout", showed an inflamed and swollen great toe, and complained of characteristic burning pain and tension in it and in the whole foot. He had several attacks before, never so bad as this; and had no sleep for three nights; overworried lately, he had probably exceeded in sherry; he was thin, weakly, and subject to diarrhoea. I ordered a lotion containing salicylate, two drachms to eight ounces of water with one or two drachms of laudanum, to be applied warm on lint with oiled silk; also ten grains of the salt internally every four hours. The lotion arrived first, 9 P.M., and was applied at once; in ten minutes the pain was relieved, the patient went to sleep, and had a good night; the medicine was not commenced till next day. He kept in bed, as ordered, three days, and had no severe return; he went out too soon, and ten days after got a partial relapse, which yielded to the same remedy.

Another gentleman, also nearly 70, and long subject to irregular gouty attacks, got a sudden acute development of the malady in one finger and an elbow-joint, a slighter one in the big toe; for twenty-four hours, he objected to any local application, as too troublesome, and took iodide of potassium with magnesia, etc.; on the second day, however, he was glad to apply the salicylate lotion, and reported quick and marked relief from it. Since the date of these cases, similar ones have been recorded by other observers.

In a third case, of a young married lady with very marked gouty family history, the immediate cause of desiring advice was an eruption of lichen planus on one arm. A salicylate lotion, containing also a little prussic acid, suited this patient extremely well; though, in a more chronic case of the same kind, but with extensive systemic disturbance (as usually associated with this eruption), it proved of no more service than other remedies. Its specialty lies in relieving gouty and rheumatic joints, which it seems to do even better than the ordinary alkaline compounds.

THE CAROTID PULSE IN AORTIC INCOMPETENCE.

By JAMES T. R. DAVISON, M.D. Edin.,

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ON feeling the pulse in the carotid arteries in a case of aortic incompetence, I accidentally found, one day, that there was a difference in the strength of the pulsations on the two sides. Since then, I have examined the carotid pulses in every case of aortic incompetence that has come into hospital, with the following result. Out of ten cases, in one group A, consisting of six cases, the right carotid pulse was distinctly stronger than the left; in another, group B, consisting of two cases, both were of the same strength; while in the third group C, consisting of the remaining two cases, the left carotid pulse was distinctly the stronger. In every instance the radial pulses were of equal strength.

The best method to examine a patient is to get him on to a chair, or if this be not possible, to make him set up in bed, his head being slightly reclined backwards; then to place oneself behind him, and feel the carotids about the lower level of the thyroid cartilage. It is necessary not only to use the same pressure, but also to keep the same relation between the fingers and the artery on each side; for if the artery be felt in front of the finger a different impression will be produced from that which is perceived when it slips to its side.

In trying to ascertain what was common to each group of cases, I found that neither the degree of ruptured compensation, the conditions of the pulse, the presence of mitral regurgitation, nor yet the degree of aortic obstruction could be identified with any of them. One condition appeared to distinguish the groups, and that was the relative size of the heart. The two cases in which the left carotid pulse was the stronger had the largest hearts. The two cases in which the pulses were equal had very large hearts. Of the six cases, in which the right carotid pulse was the stronger, four had hearts smaller than any of the preceding ones; one, whose heart I did not percuss before death, had a heart weighing only twenty ounces; and the last had a heart slightly larger than either in group B, but less than either in group C.

In health, the carotid arteries pulsate equally strongly, so that we cannot regard these differences which occur in disease of the aortic valves, as mere accidents, depending upon differences in the lumen of the arteries. And this is borne out by the fact that these differences of strength in the carotid pulses are subject to occasional variations in the same subject. I only had opportunities of examining a few of the cases on more than one occasion, and the variations observed were, that where the right carotid had been the stronger, it became of the same strength as the left; and where the left carotid had been stronger, it became of equal strength with the right. I never found that where either carotid had been the stronger, it ever became the weaker. Normally, when the left ventricle contracts, it sends forth its wave of blood into an aorta whose coats are distended by the blood-pressure within it. Therefore this wave, by simply increasing the blood-pressure, will act equally on each carotid. When, however, aortic regurgitation is present, immediately before the ventricular systole, the coats of the aorta and carotids are to a certain extent collapsed, and those of the right carotid more so than those of the left, because its axis lies more in a line with that of the aortic orifice, and thus the right vessel will be more fully emptied than the left during the diastole. When the ventricle now contracts, its wave will no longer act simply by increasing the blood-pressure, but has first to distend the walls of the partially-collapsed arteries. In effecting this, the wave goes straight up to the right carotid, but before it reaches the left carotid it loses a portion in distending part of the transverse arch. Thus the right carotid with each ventricular systole will receive more blood than the left. Hence the greater strength of the right carotid pulse, and this greater strength will be the more apparent to the touch; that the right carotid is more fully emptied than the left during the diastole. We have seen that this condition obtained in six out of the ten cases. When the heart is very much enlarged, it may drag down the ascending aorta with it, and, by so doing, will bring the axis of the left carotid more in a line with that of the aortic orifice, so that the axial relations of the carotids to the aortic orifice will be about equal on either side. Under these conditions, we would expect that the carotid pulses would be equal, and this is what we find in group B, where the hearts are very much enlarged. Again, in group C, where the hearts are still more enlarged, a more extended change in the axial relations of the carotids and aortic orifice may explain the greater strength of the left carotid pulse, while the variations occasionally met with may be due to temporary slight displacements of the heart, brought about by flatulence, etc. Whether this be the true explanation or not, the fact remains that differences in the strength of the carotid pulses are very common in aortic incompetence. This fact is important as an aid in the diagnosis of this disease, but chiefly as a possible source of fallacy in the diagnosis of aortic aneurysm.

A CORRESPONDENT of a contemporary draws attention to the unsatisfactory method in which dust is collected in the City. He writes, "Coming down Broad Street every morning about nine o'clock, after inhaling the invigorating air of the suburbs, one finds the salutary effects of such inhalation sadly deteriorated by clouds of dust scattered in all directions by the employés of the City dust contractor in clearing the deposits from the several houses—which, I may add, are in boxes placed at the edge of the kerb, to the imminent danger of foot-passengers." These practices, which are as unsanitary as they are unpleasant, certainly merit the attention of the National Health Society, or some similar body.