the contraction of the gastrocnemius; curve c. that immediately ter the cessation of the contraction

as nitro-glycerine, etc. The rise in general pressure which takes place is less easy to explain. It is probably in part due to reflex cardiac stimulation and in part to the diminution in calibre of the peripheral muscular arteries which occurs during the shortening of the muscles which are called into play. The last of these factors is most probably far the most potent. This initial rise in pressure can be considerably diminished by making the exercises very slow, but we have never succeeded in abolishing it entirely. The fact that even during the exercises, after they have been continued for some time, the pressure returns to its normal level, and may even sink below it, seems to point to the conclusion that by the contraction of a muscle some substance is produced which exerts a vaso-dilating influence, and as the amount of this substance increases its nett result is to overcome the diminution in arterial calibre which is mechanically produced by the shortening of the contracting muscles.

The conclusions which we are able to formulate from the above experiments fall under two heads—(I) Physiological, (II) Medical.

(I) Physiological. (a) Locally, gentle exercise is followed by a dilatation of the muscular arterioles with an increased flow of blood through them. This is shown by the fact that after the contraction is over the pulsations in the muscle have a greater amplitude, that is, there is a greater distance between the crest and hollow of each pulse wave than before the contraction. As these alterations in the circulation are purely local, the heart remaining the same, they can only be due to a local dilatation of the arterioles in the muscle, allowing them to empty themselves more rapidly during the cardiac diastole.

(b) Generally, the effect of exercise so gentle as to cause no hurry in the respiration and no increased frequency in the pulse on the general blood pressure is that during the exercise itself the pressure first rises above the normal, but begins to fall, even during the continuance of the exercise, continues to fall, so that at the end of the exercise it has usually reached the normal. After cessation of the exercise the pressure continues to fall. The pressure after the exercise may remain subnormal for half an hour or longer; after the expiration of this time it gradually rises again to its initial height.

These results may at first sight appear to differ from those of Oertel, who found that a rise in general blood pressure invariably followed muscular exercise. We believe that the difference between his results and ours depends upon the amount of exercise taken being different in the two cases. In his experiments the amount of exercise was sufficiently great to cause considerable strain. In one case, for example, the exercise consisted in making an ascent in forty minutes for which an hour was usually reckoned. Although he notes that no difficulty in breathing occurred, yet the respiration must certainly have been quickened.

If we compare the results of our experiments just mentioned with those which we found to follow massage,¹⁵ we notice that the primary rise of blood pressure upon exertion is greater than that caused by massage, but that the subsequent fall is both greater and of longer duration.

(II) General medical conclusions. (a) In cases where the heart is very feeble, so that the primary rise of blood pressure caused by even gentle exercise may interfere with its action, massage is the mode of treatment best adapted for restoring the circulation. (b) That when the heart is sufficiently strong

to bear the increased resistance presented to it by the primary rise of pressure occurring during exercise, gentle exercise is preferable to massage, inasmuch as the subsequent diminution of resistance is greater in amount and of longer duration. (c) The difference between our results and those of Oertel affords a scientific basis for the practical rule which has been found so advantageous at Nauheim, namely, that the exercises shall not be carried to such an extent as to cause any acceleration of breathing on the part of the patient.

We desire to express our thanks to Professor Mosso, of Turin, for allowing some of the above experiments to be done in his laboratory and also for his kind help in them; also to the Scientific Grants Committee of the British Medical Association for a grant towards the expenses of this research.

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THE ROENTGEN RAYS AND THE FLUOROSCOPE AS A MEANS OF DETECTING SMALL, DEEPLY- PLACED STONES IN THE EXPOSED KIDNEY.

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No renal operation is probably safer, easier, or more brilliant in its after-results than is the removal of an oxalate of lime stone from the surface of the kidney. The finger of the operator detects the stone in the cortex directly the organ is exposed; his finger-nail is sufficient to free it, and the patient is at once relieved, without danger, from a source of suffering which may have crippled him for years.

Not less easy, but not quite so smooth in its convalescence, is that case in which the stone is at once felt in the undistended pelvis, or that in which the stone is discovered on opening a dilated pelvis.

These three classes do not require skill—merely cleanliness, gentleness, and care. The real crux of renal surgery is encountered when we have to deal with a small prickly calculus imbedded in the end of a lengthy calyx of an undilated pelvis (Fig. 1 c). To detect such a stone and to remove it with but little damage to the delicate tissue of so vital an organ as the kidney is almost impossible with our present methods. The operator, moreover, is often haunted by the knowledge that certain forms of unilateral, non-somatic kidney disease (for instance, the granular kidney and the large white kidney), if locally inflamed or irritated by pyramid-chokage, simulate stone very closely; and if a diligent search is rewarded only by profuse hæmorrhage, he hastily concludes that his diagnosis of stone is faulty, and that he is dealing with a de-

generated kidney—yet all the while some deeply-placed stone is the real cause of the symptoms.

Nay, more, it is probably in the experience of most operators that after such a fruitless examination the patient has been so much relieved that he has returned to active life. For months, even years, he continues well, then he relapses, and as likely as not consults another surgeon, who removes a stone from the kidney.

It might be said that chronic inflammation of the kidney can be recognised by microscopy of the urine. Not so. Many examples of unilateral inflammation of the kidney ex-

Fig. 1.—Small calculus (c) imbedded in calyx.

hibit normal urine. Even *post-mortem* examinations may be quoted in which advanced disease of the kidney was discovered, and yet the examination of the urine undertaken a few days before death had shown that nothing abnormal could be found in the secretion. On the other hand, evidences of renal disease frequently appear in the urine of patients suffering from renal stone.

Have I overlooked a stone or have I explored an inflamed kidney? This is a question which I have sometimes found most difficult to answer after a negative exploration. I have removed stones from the kidney in thirty-four cases without mishap or death, and have, moreover, a tactile experience of over a hundred renal operations; and yet I feel convinced that I have overlooked small stones in probably six instances.

Fig. 2.—Use of Roentgen rays and fluoroscope on exposed kidney.

If with one finger in the pelvis and another on the cortex, I cannot detect a stone; if the bougie passes along the ureter smoothly into the bladder; if the cystoscope demonstrates the vesical orifice of the same ureter to be unimpeded, I conclude that my diagnosis of stone is wrong. Yet a more thorough cortical search might have revealed a stone. But a thorough search through the pelvis and cortex, even when conducted with gentleness, is fraught with present and future danger to the patient. I have seen one operator in London so thoroughly explore a kidney of a young man to remove a small stone that the patient died the same night from shock and hæmorrhage; and I am certain from my own work that inflammatory changes consequent upon extended examination take place in the gland, so that it often is subsequently crippled in its output, and is rendered very susceptible to suppression upon shock.

To try to obviate opening the kidney at all, when no stone is present, and to render an incision on to a located stone more precise, I have lately been working with the Roentgen rays from a 10-inch coil. These are thrown upon the exposed kidney (Fig. 2), and the viscus is then examined with a fluoroscope screen (Fig. 3). Although the method has

Fig. 3.—Fluoroscope for renal examination.

marked disadvantages for routine operations, I believe it may be of occasional value to those surgeons who can command the rays in private or hospital practice. Personally I should employ them when called upon to deal with a kidney which had been negatively explored before.

Fig. 4.—Radiograph of oxalate stone in substance of kidney.

With a little education of the eye, any stone—oxalate, urate, or phosphate—can be detected in the kidney or its pelvis, if sufficient strength of *x* ray is employed and the screen is held near the organ.

Figs. 4 and 5 represent the visual appearances of these

Fig. 5.—Radiograph of phosphatic and uratic stone in pelvis of kidney. three classes of calculus. The screen, which can be much improved, is of a kidney shape, so as to be easily adaptable to the edge of the loin. It was made for me by Mr. Montague, of New Bond Street, who has kindly helped me throughout the work.

The disadvantages of the method are as follows:

1. The kidney must be brought on to the loin, and it is not every kidney that can be dislocated. In some the vessels are too short, in others the kidney is fixed high up under the ribs, and the operator may at once feel that free separation ought not to be risked. Women, of course, are easy subjects for renal dislocation—in fact, renal surgery in women bears no comparison, as regards difficulty, with that of men.
2. The operating room must be absolutely dark, and the initial steps of exposing the organ of course need a fair light.
3. The rays ought to be from a 10-inch coil, and concentrated upon the kidney, the tube being placed near the organ.
4. The screen used must be a good one, for there are many screens on the market which do not deserve the name of a fluoroscope. It should be made so that it can be rendered aseptic, and must be brought near the kidney.

In Fig. 5, which is introduced to emphasise the fact that the kidney is exposed, the fluoroscope is, for the sake of clearness, placed too far off the kidney.

THE EXAMINATION OF ONE HUNDRED CASES OF TYPHOID FEVER BY WIDAL'S SERUM TEST.*

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SINCE the end of February last we have examined, by Widal's method, the serum of 132 cases in the pathological laboratory of Queen's College, Belfast.

* Read at the Annual Meeting of the North of Ireland Branch of the British Medical Association.

Belfast is an excellent centre for any investigation concerning typhoid fever, as the disease is pretty well endemic here. During the past spring and early summer there has been quite an epidemic, 1,293 cases with 127 deaths having been notified to the medical superintendent officer of health from March 1st to June 23rd, 1897.

The technique employed has been as follows: We have modified the collecting tubes recommended by Professor Delépine by drawing out in a blowpipe flame pieces of glass tubing from $\frac{1}{8}$ to $\frac{1}{4}$ inch calibre, so as to form small elongated bulbs with long tapering ends. A finger of the patient, after being carefully cleansed with soap and absolute alcohol, is pricked with a sterile needle: the two ends of the tube having been broken off, one end is inserted into the drop of blood, which then rises in the tube by capillary attraction. Sufficient blood having been obtained (about two drops), the ends are again sealed in the flame, care being taken to first seal the end which is next to the blood. The tube should be kept upright, with the blood end downwards till required for use. After the serum has separated the lower sealed end is again broken off, and the serum expelled on to a sterile glass slide by heating the other end of the tube. One loopful of this is thoroughly mixed by means of a platinum loop with ten loops of a twenty-four-hour old broth culture of bacillus typhosus. A loopful or two of the mixture is then placed on the centre of a clean coverglass, inverted over a hollow slide, and examined under the microscope, $\frac{1}{4}$ inch objective being the most suitable power to use. A fresh broth tube is inoculated every day from the preceding one, it being well to do this as a preliminary measure to avoid the risk of contamination.

At intervals of three weeks or so we make an agar plate culture, and start fresh growth from a separate colony, as otherwise the bacilli are apt to become sluggish. It is also well to isolate fresh strains from any typhoid *post-mortem* examinations as opportunity offers.

On now examining the preparation, and supposing it to be a typical case, a very marked change is observed. The bacilli, which were formerly extremely active and uniformly distributed over the field, are now motionless and clumped in masses, the intervening portions of the field being clear. The foregoing may be considered the complete reaction, and usually takes place either immediately or within a few minutes; this is what is observed in the majority of cases.

Occasionally (that is, in cases which are not typical) no change is noticed for from ten to twenty minutes; then an imperfect reaction occurs in which either (1) there are quite distinct clumps, but some limited movement still persists, both in isolated bacilli and in the members of the clumps, whilst the field is not entirely cleared; or (2) the bacilli are motionless but the clumping is poorly developed; this latter is not of frequent occurrence as far as our observations go. With blood serum, other than typhoid, no reaction takes place; the bacilli continue to move freely, imparting their motion to any blood corpuscles which have remained in the serum, and there is no attempt at clumping.

As will be observed, we have used a dilution of 1 to 10, following Delépine. In some of our later cases we have also diluted the serum with normal salt solution to make a 1 to 20 dilution, which seems to be preferable, as the clumps are more sharply defined, owing to the reduction in the number of bacilli. We have adopted Grünbaum's half-hour limit for observing the reaction.

A glance at the appended tables will show that we have divided the cases broadly into two divisions, namely, those clinically typhoid and those clinically non-typhoid; subdividing the first into four classes—(a) reaction immediate and complete; (b) reaction incomplete, but complete, or nearly so, when retested at a later date; (c) reaction incomplete but not retested; (d) no reaction. The total number of cases in this division is 105. Of these, 84 gave a complete reaction, 12 an incomplete reaction, more complete subsequently; 7 an incomplete reaction not retested, whilst in 2 cases we failed to get any reaction.

The second division comprises:

(1) Cases clinically doubtful at the time, which subsequently ran a non-typhoid course. Total 13, all giving no reaction.

(2) Cases certainly not typhoid. Total 7.