THE APPENDICITIS DILEMMA, AND THE PRE-OPERATIVE DIAGNOSIS.

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APPENDICITIS is pre-eminently a surgical subject, but its enlarging public importance involves the entire profession in some responsibility. A question was once asked in jest, but we are drifting towards its consideration in logical earnest, whether a second Mosaic rite might not be in store for us. Our sphinx presents us with two propositions, which spell, unless we can solve them, "operate quick—and operate always." *Proposition one* reads: There may be no evidence of "gravity," yet the patient's life may be in the balance; and *proposition two*: There may be no "local evidence," yet the patient's dyspepsia or abdominalgia of any site may be the genuine expression of an appendicitis in progress, and if so, possibly of a serious appendicitis. If these premisses are right, there is no escape from a drastic conclusion. Are they right? As the whole question turns upon the uncertainty of diagnosis, there may be some excuse for a preliminary inquiry how we really stand with regard to the latter, reserving for a future occasion any practical suggestions in the direction of improvement.

The Present Situation.

This is not satisfactory for any of those concerned. The patient has to bear the ordeal, the surgeon the onus, and the physician that unearned discredit which may not always be to the best advantage for the interests involved. *Tempora mutantur*. Those days are not so far distant when, in duty to a now vanished etiquette, the physician was shy of the knife and hypodermic syringe; and the surgeon of the stethoscope—content to trust its report at second hand as a guide for an operation which sometimes his medical colleague might have suggested. At St. George's Hospital (in 1883) that honour was once done to a mere assistant-physician by Timothy Holmes, who agreed to cut down upon a small swelling which his junior had traced in the right iliac fossa. My patient long survived its extirpation, one of the earliest, if not the first, of pure and simple appendectomies, for it was none else than a convoluted but fairly healthy though "matted" appendix, which was not, however, recognized as such until the pathologist's report allayed the operator's alarm at having apparently excised a length of the ureter. Incidentally this brings to mind that some "practical use" has at last been found for the appendix, in the shape of its successful transplantation by Erich Lexer¹ to "stop a gap" in the urethra. More wisely our surgeons now trust their own percussion; less wisely, perhaps, they may often neglect that modicum of help which an adequately complete examination by the physician might possibly have contributed. Presumably they had found it wanting; but need that always be so?

Appendicitis "of Physical Signs," and Appendicitis "of Symptoms."

1. Physical signs are at a discount with the surgeon. Present when they are "superfluous" for his decision, they are generally "absent" in a majority of his cases where he would be the first to welcome them. That is the chief gravamen from the side of the patients (though, as will be pointed out, they also have another); they might fairly object if they knew that their operation was being undertaken *without any objective evidence*. Truth to tell, however well justified for ourselves, that position is hardly justifiable to public opinion.

2. The larger group have no physical signs so far: the symptoms or our dread of them decide the operation. The weakness of this broad classification, convenient so far as it goes for our discussion, is that it contains no reference to the gravity of the case. The most awkward of our facts is that some of the worst lesions must inevitably remain truly latent by reason of the occasional slightness of any inflammatory reaction. But, with that exception, taking the bulk of cases, the only "absolute latency" has been in reality that of any adequately efficient method of diagnosis. The desirable under this head would clearly be to reduce Group 2, now almost all-absorbing, to its

legitimate limits, which are much narrower than is generally suspected; and to assign to Group 1 its due proportion, which will probably be found to be increasingly considerable.

"Urgency" and "Expediency."

Passing from this confused and comparatively useless grouping to practical clinics, we come to the common-sense surgical classification. "Operate at once for obvious necessity." "Diagnose if you can, but operate always whenever in doubt, and rather sooner than later, for safety." (1) For most of us *urgency* of symptoms is a supreme indication. This has always been true of *pneumonia*, as it is now of *appendicitis*. The last thing the pneumonic patient required was the exhaustion of an exhaustive examination: there was one thing needful and only one, "immediate treatment" to give him the best chance, his "first" which too often is in reality his last chance, by strictly harmless but efficient help. Yet surgeons there are who are not immediate operators. That is a purely surgical question; but the plain physician's view is that they take upon themselves the serious responsibility for a most careful watch. The excuse of "urgency" being dropped, a complete examination day by day, or at least some adequate examination as to the progress of a probable abscess, would seem to be an unavoidable duty. (2) *Expediency*! Where does it begin and where does it end? In the vast remainder of cases which require a *preoperative diagnosis* there are three views. The patient claims evidence, and all the evidence, for his consent; the physician in these technical matters is his appointed trustee; whilst the surgeon has to bear the brunt of the responsibility, with single eye to the patient's welfare, which is synonymous with the success of his operation. But surgical views and methods vary. For instance, antiseptics had strong opponents in London, even for its mere trial, long after Holmes had first brought it from Edinburgh to St. George's Hospital; a physician's view might sometimes have been of use in those days. Here, however, there is no suggestion as to any revolution; it is simply additional light that is desired, and eventually it would be welcomed by all surgeons. Meanwhile the physician, acting in the patient's best interests, might still sometimes, perhaps, be able to suggest surgical advantages for the surgeon's own consideration. This specially refers to the patients' other gravamen under our present routine of practice. They would, if consulted, insist upon every small guide that might conceivably favour the operative procedure. I admit that, had I to operate upon the usual urgent case, I should make little of the necessity for any searching abdominal examination, seeing that the operation was about to furnish ocular demonstration; and yet there is still a reservation for those cases which do not immediately yield up their secret to the abdominal incision. But as regards all other less urgent cases, we should secure for the patient the full advantage which he might gain from the operator having previously considered all the evidence obtainable from a complete preliminary examination. For the surgeon this is an unobjectionable and eminently satisfactory course, for the patient and his physician it would seem to be the only satisfactory fulfilment.

"Anterior" Appendicitis: "Central" Appendicitis: and "Posterior or Retrocaecal" Appendicitis.

This localizing classification, each member of which has its subvarieties, "urgent," "acute," "subacute," and "chronic," and its varied complications, is not an exhaustive analysis, but is probably as much as any surface exploration can achieve. Yet, speaking under correction, I believe that surgical diagnosis does not quite run to it yet, and that its chief lapses concern the largest group, that of the "retrocaecal" cases, whether these be severe (including abscess), or, as in the majority of instances, subacute and relapsing with comparatively slight lesions. All these almost invariably elude the most searching palpation; only "anterior" appendicitis, and sometimes also, though much less surely, the deep-seated "central" varieties, being amenable to the usual examination.

It is for this large retrocaecal contingent that further help is most needed, of a kind universally available and

both easier and more reliable than skiascopy which has been recently proposed. The method which I have elaborated, and practised for upwards of two years, aims at materially reducing that roll of latency. Some cases must continue to remain latent in the true sense; but it succeeds in demonstrating in a considerable percentage the presence of the abnormality, though not necessarily its nature. Moreover, it also aims at testing by means of a "normal standard" the healthy subjects who may have been passed as apparently quite free from physical signs as well as symptoms, as to the genuine soundness of their retrocaecal district. There for the present the matter must rest, as I am submitting to the Surgical Section at Liverpool the results of my clinical observations, in the hope that in spite of the onus of novelty, of any taint of false doctrine, and of the inevitable imperfections of the thing "in worden" which sometimes may hold more of the future than it quite knows, they may obtain a surgical hearing and a demonstration.

REFERENCE.
1 *Wien med. Klin.*, No. 39, 1911.

NEW METHODS FOR THE CULTURE OF BACTERIA.

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STARTING with the idea that the different tissues of the body possess their own specific salt content apart from having a specific protein content, knowing further that in each individual tissue the essential difference would be one depending on the nuclear metabolism of that tissue, recourse was had to the following procedure, which has been in use by me for nearly one year.

In every case the different organs, such as the brain or liver or skin, are freed as much as possible from blood with the view of eliminating bactericidal principles. The organs are then taken through a sterilized mincing machine and the minced material is placed in an Erlenmeyer flask, the weight of which is known. To every gram of the organ, taken originally, 2 c.cm. of sterile distilled water are added, and finally about 10 c.cm. of chloroform for each 100 grams of the mixture of the organ and water. No definite amount of chloroform can be stated, for certain organs, such as the brain, will bind more chloroform because they contain a higher percentage of fatty substances than will other organs such as the spleen in which the fat content is practically nil.

After shaking up the minced organ with the water and the chloroform for ten minutes, and then waiting for ten minutes to allow the excess of chloroform to separate out, at least 5 c.cm. of pure chloroform should be at the bottom of the flask. The Erlenmeyer flask is closed with a well-fitting stopper, perforated by a glass tube having 1 mm. diameter to allow of pressure adjustments, and is then incubated at 3 ° C. It is shaken up twice the first day and at least once every succeeding day to make sure of the whole content of the flask being saturated with chloroform. After a period varying from twenty-four hours to sixty days the supernatant fluid containing the products of digestion is poured off, and is filtered through a sterile barium sulphate filter paper (using preferably a suction pump) to get as clear a solution as possible.

The filtrate, smelling strongly of chloroform, has next passed through a stream of sterile air. Originally the air was rendered sterile by allowing it to pass through four successive Erlenmeyer flasks, of which the first contained strong sulphuric acid, the next 5 per cent. potassium permanganate, the next 5 per cent. caustic potash, and the last flask distilled water. Now I use simply a glass tube, a foot long, having an internal diameter of 1 c.cm., and filled with absorbent wool, till sucking air through the tube, by placing it in one's mouth, becomes rather difficult. This tube is slightly drawn out at the ends, and is sterilized in an autoclave along with the Berkefeld filtering plant. The candles generally used are No. N, and should never be relied on till they have been tested with bacterial cultures.

A great difference will be found in the behaviour of extracts made from different organs, even if they have

been digested for the same length of time, on sucking air through them, because some will foam a great deal more than will others—for example, the lung (because of the mucus) and embryonic skins are especially difficult to free from chloroform. It will be found best to place the filtered organ extracts into two litre flasks, and to keep the flasks constantly on the move. Apart from organs which contain mucus, which latter is not digested, or which at least does not cease to remain slimy, in all organs the foam formation will be lessened by the length of the digestion, as digestion liberates electrically active amino-acids, which, of course, lower the surface tension. Generally speaking, passing air for one to one and a half hours through 200 c.cm. of organ extract will remove the chloroform completely; but in the case of extracts of the central nervous system it may be necessary to keep the extract at a temperature of 38° C., but not higher. One of the advantages of the method given above is that each tissue has its own specific salt content—by which I mean that some tissues are rich in potash salts, while others are abundant in sodium salts, etc.

What seems at first a disadvantage, namely, the dilution of the salt content from the normal to one-third, by taking two parts of water to one of the organ, is in reality an advantage, because the hypo-isotonicity produced by the addition of water is counterbalanced by the increase in the number of molecules set free by digestion. Only in the case of nucleo-protein extracted by water is it necessary to add salt to bring the solution to about 1 per cent. isotonicity in the preliminary experiments, and then subsequently to dilute or increase the tonicity according to the effect we desire to produce in the morphology of our bacteria or other parasites. In the case of nucleo-proteins another precaution will have to be taken—namely, that of neutralization. I use a modified Locke's solution:

Dissolve NaCl=9; KCl=0.5; NaHCO₃=0.25; CaCl₂=0.25 in water 100 c.cm. Sterilize in autoclave, and add one part of this mixture to two parts of the organ extract before filtering the latter.

In the case of auto-digested organs, apart from the nucleo-protein problem, the best results I have obtained so far by using one part of the organ extract—for example, brain or liver—to from one to three parts of Deetjen's solution:

Deetjen's solution for blood platelets, leaving out the 1 per cent. agar, contains NaCl=0.6 g., sodium meta-phosphate (NaPO₃)=0.6 g., di-potassium phosphate (K₂HPO₄)=0.3 g. Water up to 100 c.cm. Boil till the meta-phosphate is dissolved. I make a stock solution ten times the strength just given and autoclave it.

The time the organs are allowed to auto-digest depends on what special radicals in the tissues we want to study in relation to their effect on bacterial growth. Thus the nucleo-proteins are readily extracted from the cells whenever the fluid medium which was used for this purpose is about one-third of the original isotonicity of the cell. The life of the nucleo-proteins in watery media in the presence of chloroform is, however, not very long, judging by the oxidase reaction of the nucleo-proteins, and therefore it is necessary to use extracts of organs which have not digested for more than twenty-four to forty-eight hours if one wants to study the effects of the nucleo-proteins on bacteria or other parasites.

Of course, if it is a question of simply studying the effect of nucleo-proteins themselves apart from auto-digestion, we would keep the material which we subsequently wish to auto-digest in an ice-chest to allow of the diffusion outward of the nucleo-proteins, then we would decant the supernatant fluid. Now we may add chloroform in the percentage stated above, and allow auto-digestion to go on in the incubator.

If we do not desire to study simply the effect of the nucleo-proteins, but also that of the cytoplasmic radicals, such as mono-amino and di-amino acids both in open-chain and ring compounds, it is not difficult to see that the longer we allow cells to auto-digest the less will the final products of digestion resemble the original combination in which these radicals existed, although the numerical proportion between the individual radicals out of which the cytoplasm was built up will not be altered.

It follows, therefore, that if it is our desire to study how different organs, after a preliminary auto-digestion, will behave towards any known bacterium or other parasite, it