month the symptoms recurred, and he died on December 12th.

The small intestines were quite healthy. The large intestine, in its whole extent, was much thickened from inflammatory deposit. The mucous membrane in many places was shreddy, and fell off on being touched, leaving the muscular fibre exposed beneath it. Just under the ileo-cæcal valve, and in several other places along the colon, there were small ecchymoses of blood. In the colon there were five perforations: two of them were round holes, the ulceration having proceeded in a direct line through all the coats: the three others were more oblique, the ulceration having first proceeded through the mucous and muscular coats, and then taken an oblique direction, perforating the serous coat at the distance of an inch or an inch and a half from the original ulcer.

The above cases may illustrate the following points.

1. Mental depression has a most fatal tendency in fever. Both the first cases were fine strong men, well able physically to fight with the disease; and the autopsy of the second showed how little the intestine had suffered.
2. Diarrhœa may be and often is absent, when the ulceration is proceeding even to perforation. In fever, probably the amount of diarrhœa affords little or no evidence of the amount of intestinal mischief.
3. Perforation is by no means necessarily a fatal occurrence. My third case was, without doubt, one of perforation, healed naturally. This mode of natural cure was seen at the autopsy of the second case, in which a band of lymph entirely occluded the orifice of the perforations. The condition, which was found in the first case, may also occasionally render such an occurrence comparatively harmless; the fecal matter having escaped into the cellular tissue behind the intestine. In this situation, a circumscribed abscess may be formed. I believe my colleague, Dr. William Budd, has seen a case of this kind, in which the abscess opened externally.
4. Typhoid fever and dysentery may coexist.
5. The coexistence of scurvy with typhoid fever may modify the peculiar character of the typhoid eruption.

Original Communications.

ON INCISIONS FOR STRICTURE, COMPARED WITH DILATATION.

By JOHN GAY, F.R.C.S., Surgeon to the Great Northern Hospital, etc.

PERHAPS there are few subjects of graver import connected with surgery than stricture and its treatment, seeing that the disease itself is not only most troublesome to the patient, and often most perplexing to the surgeon, but that it is a very constant cause of premature and rapidly supervening death in old persons.

The question of the value of incisions in some forms of obstinate stricture appears to be yet *sub judice*; some able surgeons, amongst them Mr. Henry Smith,* contending that their value is of small account; whilst others, and amongst them Mr. Henry Thompson, recommend them in certain cases, not only as palliative in most, but curative, in its widest sense, in some. I am led to agree very much with Mr. Smith in his estimate of the value of the incision method, compared with other modes of treatment, especially that by dilatation; believing that the advocates of the former found their

verdict upon its immediate and transient, rather than upon its ultimate and substantial, results.

Some years since, one of the most justly eminent surgeons of our age proposed external division in certain kinds of permeable stricture. Before that time, surgeons resorted to this course only in cases of impermeable stricture; or as an adjuvant in the treatment of permeable strictures, when complicated with perineal fistulæ. Mr. Syme's judgment and experience led him to propose this operation, or (as it has been called) "the perineal section" as "a simple remedy, and sufficient to accomplish a cure, quickly, safely, and surely" in cases of extremely irritable stricture, or those in which the stricture is so contractile as to resist repeated attempts at dilatation. Coming from such source, this proposal was received with, as it was entitled to, the greatest respect; and it had not been long enunciated ere it began to be carried into practice somewhat vigorously. But whether the cases chosen for this mode of treatment by all the disciples have been scrupulously such as the master would have approved, is, to say the least of it, questionable; and if, therefore, it has not been attended with the amount of success prognosticated for it, the failure cannot honestly be charged at his door.

The records of this operation, given in Mr. Thompson's excellent work, inform us that the results have been as follows: viz., a large proportion of cures, some failures, and a few deaths.

The cures, from the unqualified manner in which they are spoken of, may be supposed to be such, in the Johnsonian sense of the word; but the prevailing opinion (probably as near an approximation to the truth as the statistical evidence by which the contrary view is supported) is that they are not quite "cures", inasmuch as in the great majority of cases, the patients find the use of the bougie or catheter as much and perseveringly needed after external incision, as after any other mode of primary dilatation; for without it the stricture will as inevitably and invariably return. Perhaps it may be objected that this is but an opinion or impression. I do not see how we are to arrive at a much more satisfactory conclusion; for it would, obviously, be a matter of extreme difficulty to obtain the ultimate results in a number of cases, sufficiently large to form the basis of a practical inference that might be depended upon; whereas it is easy to obtain the immediate, which are, I am persuaded, the most favourable of the two. Cases constantly turn up in the course of practice, which have grievously disappointed the hopes of the patient, over which, at an earlier period after the operation, both patient and operator indulged the most sanguine expectations. I cannot but think, therefore, that the term "cures," in reference to this operation, has been used in somewhat too latitudinarian a spirit; and that, at least in some of the cases thus represented, the patients were only temporarily benefited.

It is true, that Mr. Syme, Mr. Thompson, and other gentlemen, whose statements or motives cannot for moment be doubted, testify to some instances in which external incision has been followed by a positive and permanent cure of the stricture. The cases in question have been subject to observation for years. But this success is not a feature of this mode of treatment exclusively; for it follows catheterisation as well in some few, and those even of the most severe, cases.

The deaths have been computed at six per cent. This, of course, is a correct computation as far as its elements go; but I am inclined to believe, from evidence which it has been my own lot to obtain in the course of my professional duties and intercourse, that the deaths have not been so scrupulously registered as the escapes.

The following case which occurred some few years ago, made a deep impression on my mind; and I narrate it in order to shew that the risk attendant upon

* See, in "Transactions of Medical Society of London," a paper by Mr. Smith.

external incision is one of the most serious objections to its adoption.

A captain in Her Majesty's service, in the prime of life and newly married, consulted me for an old stricture, firm, and exceedingly prone to spasm. I advised the ordinary remedies, with cautious dilatation; and whilst he followed my advice, his stricture ceased to be troublesome. Although, when affected by spasm, the stricture was annoying, I did not consider it a bad case; and I had little doubt but that a perseverance in mild treatment would have cured him as effectually as a more severe course. Mr. Syme's method was just then in the ascendant; and my friend was induced, by earnest solicitation, to try it. He was cut in the most able manner; and, in a very few weeks afterwards, called on me to express his great satisfaction at the treatment and the results, for he felt "assured that it had accomplished an effectual cure." Some months elapsed, and I heard that the captain was dead. He had suffered a relapse of his symptoms; had again submitted to the external division of the stricture; and had died within two or three weeks in consequence of the operation.

Since that occurrence, I have discouraged this mode of treatment, under any but the most exceptional circumstances; such, indeed, as are very rarely met with in conjunction with a urethral stricture.

I believe with Mr. Syme, that a stricture, not of traumatic origin, rarely if ever becomes impenetrable to a skillfully plied bougie or catheter (I invariably use the catheter); and, moreover, that when once passed, the catheter, if its use be steadily and cautiously persisted in, and combined with appropriate general and local treatment, such as leeching, opium, steel, etc., will ensure as complete a cure as can be effected by any other known means.

The advantage of this method over external division is its comparative freedom from risk; and I cannot but regard the argument in its favour as irresistible when we come to reflect that, out of every hundred, according to received tables, on whom division has been practised, six individuals have perished. This rate of mortality would be insignificant if the disease itself were mortal; which is not the case at that period of life in which such a procedure could for a moment be justified; or, perhaps, if there were an equivalent in the number of actual cures, which Mr. Thompson's tables do not, in my opinion, shew. It is true, that strictures do occasionally reach such a pass as to lead their sufferers to "protest that life is no longer desirable"; still, I cannot think that such a protestation, independently of some killing circumstances about the disorder itself, becomes, in any case, an admissible plea for an experiment in the shape of a possibly fatal operation.

It may have been observed that I do not exclude external division from the list of stricture remedies, believing with Mr. Thompson that the real aim of the surgeon should be to "utilise all"; but I do object to that estimate of the operation according to which it is regarded as a simple, safe, and effectual method of cure; and is, in consequence, adopted, as I have much reason to believe it still is by some, without either that regard to necessity, or that amount of caution which should ever be observed when life hangs on the issue.

Of the simple internal incision recommended by Mr. Thompson, I have had no experience; but, when confined to the constricting band or tissue, I can see no objection to it; believing that whilst, on the one hand, it can do no harm, it is very likely, on the other, to relieve congestion, inflammation, or irritability, and efficiently to prepare the way for what, in the end, will still be the "wedge" in the treatment, namely, persistent dilatation.

I come now to speak of dilatation, having long adopted Sir Chas. Bell's view that "the permanent cure of stric-

ture is to be accomplished by dilatation.....by such a stretching of the narrow part of the canal, that it grows large under the operation, and has no disposition to contract." (*Institutes of Surgery*.)

I speak from some considerable experience, and that of the treatment of the worst forms of stricture, when I say that I have very rarely met with a case that has not been amenable to the use of the catheter, with such adjuvants as opium, leeches, steel, etc., when especial circumstances demand them.

Space forbids my treating of different kinds of strictures in detail, or their pathology; but it must be remembered that that idea of an urethral stricture is miserably imperfect, which reduces it to the simple narrowing of the passage. A stricture is a structural lesion; and is not only the consequence of some disorder of the urethral tissues, but becomes itself the centre, and, perhaps, the seat of disorders specifically its own, such as inflammation, congestion, hæmorrhage, spasm, etc. And it is most constantly the case that the patients suffering from stricture do not become conscious of its existence, or at least do not awake to its importance until their attention is drawn to it by its having called into painful activity one or the other of these accessory forms of disease.

It is not, then, the stricture *per se*, except in urgent cases, that should first claim the surgeon's notice, but its incidental affections; and all mechanical treatment should be withheld until these are brought under control by appropriate management, or, if the delay can be borne, altogether overcome.

The most general causes of failure in the use of the catheter are, as far as I have seen: 1. Its inopportune employment; and 2. Impatience. To the first I have just alluded. With regard to the second, I should say that in no case should the catheter be used with any but the slightest force; and this should not be persisted in but for a very short time. It is better to make several efforts at intervals of one, two, or three days, than overcome the resistance of a stricture by one effort, if that should necessarily be a violent one, and such as would lacerate or do other injury to the urethra. Gentle and regularly repeated efforts will eventually ensure success in, I believe, the most aggravated cases. I am in the habit of observing two rules when attempting to pass a catheter through a stricture, of the greatest service: 1. Invariably to keep the convexity of that part of the catheter between the curve and the point well back against the posterior wall of the urethra, the point being tilted slightly forwards and upward at the same time by a finger commanding the handle side of its curve; bearing in mind the anatomical relation of the urethra to the pubic arch, as it passes through the triangular ligament; and 2. To desire the patient at the same time to make an effort to void his urine, whereby all resistance to the introduction of an instrument, on the part of the extrusor or detrusor muscles of the urethra, is overcome, and a channel, though ever so small, is, of necessity, formed.

In passing, I might remark that this is an invaluable test of the *existence* of stricture, especially in young subjects.

After the catheter has once passed through the stricture, it does not follow that it will be admitted on subsequent attempts. These may fail from many causes, and may fail even again and again; but, after a time, the failures will be less signal, until, at length, they cease altogether.

There should never be a hurry to substitute a larger instrument for one that passes. Sometimes a small instrument will dilate, and that with permanency, more quickly than a large one; although, on many accounts, the latter is preferable to the former in the generality of cases. I am assured that, by the gentle but patient and persistent use of catheters, well adjusted, as far as

their size and shape are concerned, to the capacity of the strictured passage, and to the normal or abnormal course of the urethra in its entire length, most strictures can be overcome, and the urethra restored to its original condition, with as much security against relapse as by any other plan of treatment that has hitherto been devised, and with less risk of injury to the patient; and that the call for the knife is exceedingly exceptional.

Finsbury Place South, May 1861.

DOES THE PERICARDIUM INVARIABLY BECOME ADHERENT TO THE HEART AFTER ACUTE PERICARDITIS AND RECOVERY?

By F. HUGHES HEWITT, M.D., Physician to the
Worcester Dispensary.

THE BRITISH MEDICAL JOURNAL, for March 16th, contains some remarks by Dr. Hinds, of Birmingham, on this very interesting question. The author thinks it should be answered in the negative. He admits the difficulty of "dealing with it very positively and conclusively," owing chiefly to the rarity of opportunities of examining the bodies of persons who have made recoveries from acute pericarditis. He assumes, correctly I believe, that most pathologists hold with Dr. Watson, that the cessation of the to-and-fro sound is owing, not to absorption of the effused lymph, but to adhesion of the opposed surfaces of inflamed pericardium, and that "in this catalogue of apparent but unreal recoveries many of Bouillaud's cases of 'pericarditis terminating in health' ought to be included." He then relates two cases, occurring in his own practice, with the results of which he thinks "the principle above laid down is not consistent."

The first is the case of a gentleman, aged 27, who exhibited all the symptoms of pericarditis. He was treated by local bleeding, calomel and opium, and diuretics, and in eleven days the friction sound, together with "a little bruit with the first sound of the heart" had completely disappeared. This was three years ago, and the patient "shews not the slightest departure from robust and perfect health."

The second case is that of a child, aged about two years, who died after a few days illness, no heart symptoms having been detected during life. At the autopsy "the heart was found glued to the pericardium throughout its whole surface by a thick toughish layer of recent lymph."

Now, cases such as these are, comparatively speaking, common enough. Every practitioner who has seen much of acute disease must have met with instances of pericarditis in which all the symptoms, general and auscultatory, have disappeared, and apparent recovery has taken place; and others, which have terminated fatally, and in which obliteration of the pericardial cavity from recently effused lymph has been found after death. But what bearing they have upon the question with which Dr. Hinds heads his paper I am at a loss to understand. If his patient who had pericarditis three years ago were to die, and his pericardium were found to be free from adhesions, we should then have something like evidence before us. The grounds, however, upon which Dr. Hinds claims the cure as "complete and real", and arrives at the general conclusion that "pericarditis, with exudation of lymph on the surfaces, is a curable disease, and that in a strict and just sense of the term", appear to be:—

1. That he cannot reconcile the fact that his patient, a young man, appears in perfect health three years after an attack of pericarditis, with the idea that adhesion took place at the time of the attack. "If the heart," he

asks, "had been glued to the pericardium, could such complete and lasting recovery have occurred? Surely not."

2. That he has never seen, and doubts the existence of, cases of adherent pericardium, "if we except those from recent disease." "Do they ever occur?" he asks. "Has any practitioner seen many, or any, such cases in which the patients, well of the cardiac, have died of some other disease, or even 'apparently' well of the cardiac disease, living in apparent health for any considerable period afterwards?" "I have never myself met with any case of adherent pericardium, except from recent disease."

I must confess that I read these remarks, especially the last sentence, with amazement. How long it has been an established fact that old pericardial adhesion is occasionally met with at *post mortem* examinations, usually quite unexpectedly, I do not know. But many readers of this JOURNAL must remember the discussion which took place some twenty years ago between Drs. Barlow and Chevers as to whether such a pathological condition led to hypertrophy, or to atrophy, of the muscular tissue of the heart. It has been thought by many physicians, by Dr. Munk among them, that it may be detected with tolerable accuracy during life. Specimens of it are to be found in almost every museum. Dr. Wilks observes (*Pathological Anatomy*, p. 65), "more frequently than this" (partial adhesion) "the heart is found universally adherent to its serous covering; thus, at the beginning of this year we found it three times in one week, and quite unconnected with the disease of which the patient died, one indeed being an accident, and another a surgical case." Besides numerous instances in the *Pathological Theatre* at Guy's Hospital, I have met with two or three in private practice; in one of these, a child aged 9 years, the pericardial adhesion, which was universal and evidently of long standing, was quite a minor feature, death having been caused by the suppurative mass of enlarged bronchial glands, which had opened into the œsophagus, and penetrated the pulmonary veins, giving rise to immense and rapidly fatal hæmorrhage.

Chronic adhesion of the pericardium, then, is a condition by no means unfrequently met with, and is compatible with the continuance for a very considerable period, not merely of life, but of tolerable health, with an absence of cardiac symptoms, provided it be not complicated with endocardial disease; in short, "simple" pericardial adhesion produces no appreciable untoward consequences." (Wilks, *op. cit.*)

As regards the second of the two propositions, already alluded to, with which Dr. Hinds sums up his paper, viz., that pericarditis is a "curable" disease, I will only say that, without denying the *possibility* of the absorption of the effused lymph, I think the evidence he brings forward wholly inadequate. The circumstance that slight and partial adhesions only are sometimes found seems to render it probable that a portion of the exudation, at all events, may be absorbed (supposing that the inflammation in these cases originally involved the whole of the membrane—which is not certain). But we ought, it seems to me, to suspend our judgment on this point till we are in possession of well authenticated cases of patients who have had unequivocal symptoms of pericarditis, who have recovered, have subsequently died, and have been found on inspection, free from pericardial adhesion.

CASTOR-OIL BREAD. The difficulty of taking castor oil, when but a mild effect of it is wanted on the bowels may be removed by kneading, etc., bread with it, instead of butter or lard. Bread made with it will be found quite as good to the taste, if made with fine castor-oil or finer than with butter, etc. (*Med. Times.*)