Professor Dr. Ludwig Pick has discussed in an exhaustive manner the anatomical, histological, and chemical features of this rare form of pigmentation. He also gives a useful summary of nearly a score of similar cases, beginning with one recorded by Virchow, 1847, and three described in England by C. J. Williams, 1867, G. Newton Pitt, 1891, and H. D. Rolleston, 1892. Pick states that the coloration is not due to metallic deposit—it is essentially a pigmentation of the connective tissue of the mucous membrane, and haematogenous.

Although the pigmentation begins, as a rule, at the ileo-caecal valve and extends to the anus, cases have been observed in which it does not involve the whole length of the colon. When the colic membrane is involved throughout, the pigmentation is discoverable at the anus by means of a speculum. The condition is a clinical curiosity, but if detected in the course of a clinical examination it might have undue significance attached to it. It will be useful to have some further observations on this curious pigmentation.

I selected cancer of the small intestine as the subject of the address this evening because it is a matter neglected in the ordinary works on surgery. It is intended to hold a discussion at our next meeting on the surgery of tumours of the large intestine, and we hope to have an exhibition of illustrative specimens of tumours of the small intestine as well as tumours of the caecum and colon. The council is hopeful that a series of specimens illustrating hyperplastic tuberculous disease of the caecum will be shown. This disease, often mistaken for cancer of the caecum, has not received much attention in this country, although Henri Hartmann made it the subject of the oration in 1911. The society will also be grateful for the loan of specimens illustrating primary cancer of the vermiform appendix, a disease concerning which there is much uncertainty. It will also be useful to obtain some further observations on the curious pigmentation of the colon.

PRESENT-DAY LESSONS FROM THE LIFE WORK OF MITCHELL BANKS.

BEING THE THIRD SIR WILLIAM MITCHELL BANKS MEMORIAL LECTURE.

BY SIR VICTOR HORSLEY, F.R.S., M.B., F.R.C.S.

THOUGH the keeping bright the memory of our benefactors is a pleasant and, as in the present instance, a most honourable duty, the object and value of a memorial lecture rests equally on the fact that even a superficial consideration of the conditioning circumstances of our predecessors' work evolves a whole crowd of conclusions which are full of lessons for us.

The life work of Sir William Mitchell Banks in every aspect is not only interesting and educational but it is also a pleasant study of how much active public benefit can be secured to the community by the efforts of one of our profession, and how much can be effected by single-mindedness.

For Mitchell Banks tried to live up to Pasteur's scientific idea of what should be the inspiration of a medical practitioner, namely, that "A doctor must consider what people will say about him 100 years hence and not trouble himself about the blame or praise of to-day." Probably Banks, in his active advocacy and support of surgical and educational reforms, realized Pasteur's ideal more completely than many of his contemporaries.

Seventy-two years ago Mitchell Banks, as he was familiarly known to most of us, was born in Edinburgh (in 1842), his father being Mr. Peter S. Banks, Writer to the Signet, and his mother, Miss Ann Williamson. A student of the greatest ability, originality, and promise, he took his M.D. degree in 1864, with the highest distinction, and under the stimulating scientific influence of Goodsir, produced original work on the Wolffian bodies, which at once became the classical authority on that subject. Banks had, in fact, entered the profession at the moment when the greatest advances in medical science were being made by the anatomists owing to the development of microscopical technique. Goodsir had all John Hunter's

feeling that anatomy should be pursued not only as part of natural truth to be determined for its own sake, but also always with the interpretation of function as the purpose and object of progress, and Banks's work and thought throughout his whole life exhibited this practical combination.

We have to thank the Scottish School of Medicine and its various teachers for many things, but especially the academic recognition of anatomy. For just as Goodsir was the successor to the distinguished line of Munros, so Bell made his contribution to the progress of surgical science more notable through his enforcing dissection by all his students.

But Goodsir was far more than an anatomist limited to his special subject. There was another aspect of Goodsir's influence which, perhaps, has not been so well realized outside the inner circle of the Edinburgh School fifty years ago, and yet it must have exercised a considerable effect on Banks's active intellect. I refer to Goodsir's comprehensive views of medical work in general and its bearing on social life, which led him, though in many respects a recluse, to take a part in the organization of the Edinburgh Society which was called the "Universal Brotherhood of Friends of Truth," and the principles of which were declared to be the promotion of "Fellowship, Work done, Love for the good and beautiful," and, above all, "Co-operation." Pre-eminently among Goodsir's pupils Mitchell Banks tried to carry out these principles of his teacher in his social, communal public life, as well as in his professional work.

There was thus from the first the development in him of that knowledge and sympathy for anatomy as a pure science which subsequently he so extended during his tenure of the chair of anatomy in the Medical School as to make that at first the most flourishing teaching department in University College at its foundation in 1882. And later on his work for the developing Medical School and University meant labour, not merely for medical science alone but his sympathy for social science as well, because of the inestimable benefit the University has conferred on the citizens of Liverpool.

What now of the main work of his life—his surgical achievements?

Two subjects will always be honourably associated with Mitchell Banks's name and the Liverpool School of Surgery—namely, cancer and hernia. To-day they are humanity problems just as important and just as much in need of constant notice, consideration and reconsideration as they were when he began to hold up the light of surgical progress.

Both, too, are subjects respecting which his teacher Goodsir had put the true value of surgical work in words which for their scientific precision and accuracy are worthy of resuscitation here, and for their just appreciation of the possibilities and limitations of clinical work ought not to be forgotten.

It is with methods of prevention and recovery that our profession has to do. If we make use of the terms *cure* and *remedy* we do so in their fundamental and not in their vulgar sense, for we do not profess to perform the one or to provide the other in their vulgar sense.

What we undertake to provide are, *firstly*, the conditions of relief or alleviation of pain as far as these have been discovered; *secondly*, the conditions of recovery, provided these conditions can be restored; and, *thirdly*, the conditions of longevity within the limits assigned to the life of man in the present phase of his existence.

Every one who followed Mitchell Banks's work, whether in the wards or as revealed in his writings, knows that these words exactly express what he did, and no one recognized more clearly the scientific limits of the surgery of his day on the fundamental questions of "cure," "relief," "longevity," etc., which were so clearly laid down by Goodsir. No one in speaking of "results" used the word "cure" with more meticulous care, and no one weighed up more carefully in the interests of his patient the risks, especially to life, of any suggested procedure. On this last of Goodsir's points—namely, "longevity"—a word must be interpolated here, for social changes advance so unnoticed by those participating in them that the handicapping conditions under which our predecessors have worked are seldom remembered and often unknown.

The numerous risks to life caused by even a simple operation and due solely to ignorance and want of civiliza-

tion which existed only a few years ago are already forgotten in the safety of modern operative procedures.

Thus, for instance, in the year 1864, when Mitchell Banks began his career, so backward were we in civilization that a troopship crowded with sick women, children, and time-expired men took more than five months to come from Bombay to this country. And again, to quote an instance from our immediate subject—namely, the survival of patients on whom an operation designed to prolong life had been performed—let me point out that Banks, as late as 1882, stated that the death of a patient by infection was due to such shocking neglect in hospital administration that “many of the beds in the ward in which this patient lay were found to be absolutely rotten”!

And yet under similar depressing circumstances Lister—misunderstood, contemned, and cold-shouldered by his ignorant and unscientific contemporaries—had completely solved long before 1882 the principal part of the “longevity” problem—namely, how to prolong and not curtail life by an operation.

Consequently, since we have besides protection from sepsis, modern means for successfully dealing with shock, Goodsir's condition of “longevity” is now fully satisfied in both these subjects of cancer and hernia.

CANCER.

With this preface and with these rather painful memories of a by no means distant past let us take the first of these cases—namely, cancer—and we must limit ourselves to the special case of cancer of the breast.

No subject is commoner and none has been dealt with more widely by surgeons, yet at the present time there cannot be said to be either a full consensus of opinion among surgeons as to the best operative procedure for dealing with it, nor anything like a fulfilment in practice of a general acceptance of the principles on which that procedure must be based.

Mitchell Banks, at a time when pathology was only just emancipating itself from the preconceptions of clinical hypotheses, began with the fundamental fact that cancer is primarily a local disease, and must be dealt with as such. Perhaps we owe to Edinburgh teaching the idea that cancer was not a general blood dyscrasia but a parasitic disease beginning locally and only gradually invading the body. It seems to have been first taught clearly by Bell, at any rate.

But many decades passed without this all-important axiom being generally accepted. Indeed, after Mitchell Banks had begun to make true surgical principles better known, an old practitioner wrote (anonymously, of course) to the *Medical Times and Gazette*, 1870, p. 762, suggesting that all surgical treatment of cancer of the breast was useless.

It was in this sort of intellectual fog which beset the profession that Mitchell Banks began the work which became so essentially his own, for which the human race must always be indebted to him, and which, like all achievements of real scientific value, not only enhanced his reputation but also added further distinction to the School of Surgery in Liverpool.

He first published in detail the results of his work in 1877, and formulated the true position of the surgical treatment of mammary cancer by putting forward two principles:

1. That the whole of the breast tissue must be removed, because even in the parts of the gland not obviously the seat of active cancer there were, nevertheless, changes which must be regarded as pre-cancerous.*

2. That all the lymph glands in relation to the mammae should be removed, on the ground that it is impossible to determine at the time of operation,† when they appear to be normal, that they are not really infected.

Though possibly acted upon in general, these two great principles I shall show directly are not even now fulfilled in practice with scientific loyalty, though thirty-five years have elapsed since Banks promulgated them.

It might be said that these principles of operation for

* Subsequently fully confirmed by Johanson, Beadles, and others.
† In 1877 histological methods were not sufficiently advanced to permit of rapid microscopic investigation of doubtful tissues at the time of operation. But such examination, though occasionally carried out, does not meet the necessities of the case in gland infection, since time does not permit of an exhaustive search. Consequently Banks's law is as true now as in 1877, and must be fulfilled to the letter.

cancer of the breast are, after all, but the practical recognition of the truth of Bell's dictum that cancer is at first a local disease, but that is only another way of saying that all genuine advance in operative surgery is based on pathology.

Moreover, the need for Banks's teaching was indeed great, since, owing to their lack of scientific appreciation of Lister's great discovery, surgeons in 1877 did not operate to remove the disease but to palliate it. Such palliative operations were so limited in design and execution that even ten years before (in 1867) Moore had protested against what he aptly described as wholly “inadequate operations.”

It was in reference to this inefficient fashion of operating in 1877 that Banks, in 1882, said in his characteristic way, “Very few people died as an immediate result of it (that is, such operation). Unfortunately, at a little later period they all died from want of a little more of it.” “For it never effected a cure.”

No doubt the fatal error of this incomplete operating was due in a large measure to post-operative difficulties and risks to life, which restricted the enterprise of the surgeons of that day, and which they could not or would not see were only due to their neglect of scientific facts and research.

Perhaps the best evidence of this is the fact that even in Mitchell Banks's own practice at that time (1882) the risk to life—that is, the mortality after operations for cancer of the breast—was no less than 13 per cent., a mortality which, in the hands of most modern surgeons, has now wholly disappeared from this branch of operative surgery.

Allow me to digress here for a moment on the matter of surgical terminology. I have always urged that the phrase “mortality of an operation,” so commonly used, is, in fact, a false statement, and should be replaced by “mortality after an operation.” For an operation *per se* is not a cause of death unless some accident occurs during its performance or unless it is unfortunately designed or executed. The fact is that cases are not adequately clinically analysed, and, as a rule, the old fallacy of *post hoc propter hoc* is allowed to prevail. Shock, for instance, is often attributed to the operative procedure in cases where it is wholly due to the condition into which the patient had been allowed to drift before operation was decided upon. Typical examples of this very common logical error in statistics are to be found in all elaborate tables and discussions of so-called operative mortality in the treatment of various diseases, and none more strikingly so than in the case of cerebral tumours, where the patient is rarely offered surgical treatment until in the last stages of the disease. Let me at the same time point out that this mistake in statistical method is not characteristic of the medical profession only, for mathematical statisticians when dealing with biological subjects also constantly omit to analyse their case material, and consequently arrive at most fantastic conclusions on all important questions of public health, such as tuberculosis, alcohol poisoning, infant mortality, housing, etc.

Leaving now the question of danger of treatment, as one now wholly without importance, we must proceed to the discussion of the chief object of Mitchell Banks's work on the treatment of cancer, namely, how to secure for the patient what Goodsir meant by “prevention” and “cure.”

In the case of cancer this means the immediate extirpation of the disease while it is yet only local, and it would naturally occur to an anatomist like Mitchell Banks that such extirpation must include all the structures likely to be invaded, in the order—breast, lymph glands, pectoralis muscle.

In speaking of this anatomical order of the mode of invasion of the body by cancer beginning in the breast it is worth while drawing attention to the close parallelism between it and another parasitic disease, namely, tuberculosis.

The local lesion, the subsequent infection of the glands and later of the viscera in tuberculous inoculation of guinea-pigs, presents in a few weeks exactly the same picture that carcinoma does in years when commencing in the breast.

Early and complete extirpation of the local disease and neighbouring structures effects a cure in both conditions

alike. That was Mitchell Banks's principle in respect of mammary cancer.

It might be thought that as his surgical teachers at Edinburgh were also primarily anatomists, he simply followed their inspiration, but I have not been able to find any evidence in their writings that these men had arrived at the point of intellectual evolution which marks the beginning of Mitchell Banks's original work.

One quotation will, I think, prove this. Thus Syme, his chief teacher in surgery, not only was in the habit of dismissing all cases of mammary cancer as hopelessly inoperable if he discovered any evidence of infection of the axillary glands, but also, in his last description of operating for the disease given in his *Principles of Surgery* (5th edition, 1863), he makes no mention of removing the glands as a measure of precaution.

There is certainly no evidence that the idea was present to his mind at all, and we may fairly assume that Syme represented the surgery of his day, which had not yet so freed itself from tradition as to be guided by scientific principle.

It was on this principle of making the local removal of the disease complete that Mitchell Banks advocated the clearing of the axilla, and the occasion of this lecture is the moment to consider whether we have thoroughly profited by his teaching, and to see whether the present-day operative treatment of cancer is always as complete as it should be, and as he strove to make it. In the first place, to judge by the textbooks at least, removal of the axillary glands is understood to be a *sine qua non*, and Küster, after practising Banks's method for a few years, saw out of 95 recurrences only one occur in the axilla.

But the same statement cannot be made of excision of the supraclavicular glands; indeed, in the recent monograph on *Cancer of the Breast*, by the late Mr. Cecil Leaf, this step in the operation was distinctly discouraged.

Yet from the standpoint of surgical principle it cannot justly be omitted, for what Banks said of the axilla, "Since you cannot tell whether the glands are infected or not, remove them and dissipate the doubt," applies with equal force to the supraclavicular region.

He might have added to his terse phrase the words, "thus giving the patients the further security due to them," for Halsted, who by carrying out Mitchell Banks's principles with completeness and originality has done so much to get their truth recognized, found by a laborious and accurate investigation that cancer was present in the supraclavicular glands and tissues in 34 per cent. of the 67 cases he examined. Plainly, therefore, we owe it to our patients to clear out both fossae.

It is not just, however, to leave the question of the design of an operation for mammary cancer at this point, for the subject has been further elucidated by subsequent investigators, notably Sampson Handley.

In his method of operating Mitchell Banks removed the skin freely, and in so far protected his patients from local recurrence, but it was from the empirical point of view of likelihood of original foci of disease being left, and this part of the scheme of operation was not put on a scientific basis until Handley's complete demonstration of the infection paths through the lymph vessels in the fascial planes.

Consideration of this point naturally brings us to the remaining one of the treatment of the pectoralis muscles. The paramount source of risk is, of course, the fascia on the pectoralis major, and the empirical practice of Volkmann in 1875, when he advocated the invariable removal of this fascia (more properly the muscle itself), unfortunately was regarded by the timorous (because septic) surgeons of the day as an extreme measure, and did not gain general acceptance until Heidenhain, many years after Banks and Volkmann, proved anatomically the widespread degree of involvement of these structures in an average case. Even then the mistaken idea of possible crippling prevailed in the minds of some against even the realization of its necessity.

To sum up. Every operation for mammary cancer in order to fulfil all known requirements, and, by antagonizing all known avenues of the disease, really serve the interests of the patients, must include:

* The archaeology of surgery is always interesting, and on this point I would like to add that not only were the Bells and Fergusson anatomists, but that Liston taught anatomy till 1833, when Syme took his place. Though followers of John Hunter, they never seem to have been drawn towards biological physiology.

First, very wide removal of skin, breast, and margin of subcutaneous fat and fascia outside the gland edge, the pectoralis major, and the whole axillary contents—fat, fascia, and glands up to the edge of the first rib—in one mass; secondly, free removal of the supraclavicular fat, fascia, and glands.

So far as I know, however, this procedure is not followed in its entirety except by a few surgeons. Various explanations are often offered to justify omitting, in given cases of carcinoma, parts of the procedure, although every such omission is, for the reasons given above, a violation of surgical principle. To me such omission seems also to be a grievous wrong done to the memory of Mitchell Banks, and there we must leave it.

HERNIA.

Now let us turn to the other subject which Mitchell Banks with practical prescience seized upon as a very common infliction on suffering humanity, and yet one most imperfectly treated—namely, hernia.

Prominent among the innumerable benefits which have directly issued from Lister's work is the absolute safety he conferred on the surgical treatment of hernia, and it is interesting to see what a striking diminution of its so-called clinical importance has in consequence resulted.

But hernia still remains a very considerable national evil, and one which it is the business of our profession to remove.

In view of this responsibility upon us, and the fact that hernia as a cause of death is a wholly preventable one, it is very depressing to read in the last report of the Registrar General for England and Wales—namely, for 1911—that no less than 1,569 deaths were certified in one year as due to hernia alone.

Since, as I have elsewhere shown,[†] our national vital statistics are extremely untrustworthy (in respect of many causes of death—for example, alcohol, syphilis, etc.—utterly so), we must be careful in endeavouring to analyse this astonishing figure, but it may be assumed to mean that a very large number of persons do really die every year directly because of their suffering from a hernia. Whether strangulation occurs or not, however important clinically as being the immediately fatal factor, is a detail quite unimportant for the present purpose, and so also is the question how many of the 1,569 patients were operated upon.

The one grave fact which stands out clearly is that our present treatment of such a very common and simple condition as hernia is exceedingly faulty, and urgently needs reconsideration, because a very large number of our fellow citizens are clearly not protected from its fatal consequences.

But there is also an equally important socio-economic reason for condemning our present methods of dealing with hernia, and that is the continual loss of national working efficiency, owing to the personal incapacity caused by hernia to the hardest worked and most industrious classes of the community.

On this part of the discussion we must determine, if possible, what proportion of the population are thus handicapped or rendered practically useless to the nation.

In his recent statistical report to the Local Government Board Dr. Basil Cook gives tables constructed from the examination of recruits which at least constitute a rough basis for a minimal estimate. From these figures we are probably justified in assuming that at any given moment 500,000 individuals of all ages are suffering to a greater or less degree from hernia in the United Kingdom. As regards total incapacity leading to social failure and poverty, Dr. Basil Cook found that, out of 3,162 paupers, nearly 2 per cent. attributed their pauperism to their being rendered incapable by a hernia.

Now if we ask why the medical profession in this matter of hernia has failed adequately to protect the community from such loss, and done so little to preserve national efficiency, the answer is simple—namely, that until quite recently the largest proportion of the profession's mode of dealing with hernia has been by means of trusses.

Unfortunately for the nation, the application of a truss to a hernia is not treatment of the evil, but merely

[†] Evidence to First Report of the Royal Commission on Venereal Diseases, 1914. Also BRITISH MEDICAL JOURNAL, 1914, i, p. 923.

palliation. In my opinion we had drifted into this unfortunate habit of regarding a truss as treatment of hernia by following the usual path of least resistance, for the only procedure which merits the name of treatment is of course radical cure—that is, the obtaining by operation a permanent closure of the hernial defect in the abdominal wall. This being so, we have yet to explain why the profession had largely become satisfied with makeshift applications such as trusses. The explanation is not without interest, and shows that our maltreatment of hernia is due to the same causes as our inefficient operating for cancer of the breast, and therefore rightly became an object of Mitchell Banks's reforming zeal.

Seventy years ago the misdeeds of itinerant quacks and the disasters of septic infection had brought all operative treatment of hernia into such disrepute that as late as 1840 the then leader of operative surgery, Liston,¹ said, "Operations for unincarcerated (that is, without any symptoms of strangulation) hernia are not justifiable," and he added, "Those who have operated in such circumstances give a very unfavourable account of the experiment."

In fact, throughout the whole preantiseptic era no progress could be made until Lister had not only freed operative procedures from the risk of septic infection, but in this case of hernia also from the further risk of producing more functional and material injury by suppurative inflammation occurring just where most cicatricial strength was needed. On this latter point I may remind you that Denk² showed from his examination or the statistics in von Eiselsberg's clinic that of the cases of hernia operations in which suppuration had occurred, recurrence followed in 43 per cent. The same point had been demonstrated by Damer Harrison³ seventeen years before.

Although Lister's work is now forty-five years old, such is the unfortunate conservative influence of tradition and custom in our profession that there still exists a certain prejudice against the radical operation, and some surgeons in consequence still speak of "treating" hernia (even in the adult) with a truss, as though this mechanical placebo was something more than a makeshift.

In fact, a truss, like "treatment" by taxis of a strangulated hernia, is one of those *reliquiae diluvianae* which will still survive in medical practice, to the detriment of the community and suffering public, until the only rational treatment by radical cure has become so habituated that operation will immediately follow diagnosis.

This custom being even now prevalent in our profession, it can easily be understood that it was not one of the least difficulties which Mitchell Banks had to contend against when he began, and even as recently as 1882 such was the prevailing darkness on the subject when he read his paper at a (Worcester) Branch meeting of the British Medical Association that Mr. Spanton, in the discussion, pointed out that in the "textbooks of surgery all operations for the cure of hernia were alluded to, only to be condemned as unnecessary and unjustifiable."

No doubt the idea that a truss deserved credit as a form of treatment arose from the number of so-called "congenital" cases in infants which are apparently cured by a rubber truss or even a figure of 8 woollen band.

All that actually happens in these circumstances is that the normal developmental process of closure of the funicular infundibular cavity occurs, and possibly more rapidly than if no support were used, but that a permanent cure is really produced in all the cases in which the hernia does not recur in adolescence there is no evidence to show. Of course these cases of so-called cure of congenital hernia are never followed up, and often the previous existence of the hernia in infancy is wholly forgotten.

Thus as lately as 1895 the principal textbook of British surgery contained the following passage:

In young children the operation (that is, radical cure) is comparatively rarely required, for during the first few years of life herniae show a great tendency to undergo spontaneous cure.

I know of no statistics which justify the use of the word "cure" as here given. One can imagine Goodsir asking the pertinent questions—What is a cure? What was the subsequent adolescent and adult life-histories of these

cases? How many relapsed soon, and how many developed herniae with advancing age?

In truth the actual clinical facts are entirely the other way, as the following analysis of the conditions which underlie the causation of hernia prove.

The old clinicians taught that the causes of hernia were manifold, but that the most important should be all grouped together under one head—namely, muscular effort and strain. That is to say, the assumption was made, and is still taught, that by muscular pressure on the abdominal contents the bowel is driven against the muscles and tissues, gradually stretching these until a sac is completely developed.

I never could bring myself to believe that a smooth soft elastic coil of intestine could be forced through the abdominal wall by violent contraction of muscles, the action of which is not in fact convergent on any spot but over the superficies of the abdomen generally. *A priori*, then, the old theory of the causation being possibly violence alone is not reasonable and moreover cannot be said to have been proved to be really operative in the way supposed.

Further, every surgeon knows that clinically in the large majority of cases a hernia is stated to have been discovered accidentally by the patient; therefore it was not suddenly produced by any such supposititious violence. Thus, Coley found that of 4,780 cases of hernia in adult males it appeared in no less than 3,102 without any known cause or violence.

Frankly, the only reasonable pathogeny is that so clearly worked out and formulated by Hamilton Russell—namely, that a preformed sac is the causal factor of hernia, the clinical converse of this pathological position being the rule first urged by Mitchell Banks, and to the discussion of which I will revert directly—namely, that no cure of a hernia is possible unless the sac is removed.

Hamilton Russell's view that all herniae are due to a primary congenital defect is further proved by the fact that the topical frequency of hernia occurs in precisely the proportion to be expected—namely, according to the regions in which congenital deficiencies and arrests and failures of development most frequently occur. For this reason inguinal hernia heads the list of herniae by such an enormous majority.

Thus all facts—anatomical, pathological, and clinical—confirm the conclusion I have put forward that so-called congenital hernia is not "cured" by trusses; that when the funnel-shaped fossa in the peritoneal surface and often part of the infundibular tube remain, and when in later life the tone of the muscles is diminished the hernia naturally becomes re-established.

The logical conclusion follows that for the good of the community—that is the State as well as the individual—every hernia must be cured by radical operation as soon as it is discovered, and such I believe is the opinion of every modern surgeon.

Mitchell Banks's share in bringing us up to this level of surgical progress was a very interesting one, because although as late as 1893 he was an advocate for the use of trusses, his scheme of real curative treatment was practically what is now universally adopted, I believe, as a radical cure, namely, the fulfilment of two principles:

- (a) Removal of the sac with restoration of the continuous peritoneal surface (that is, abolition of the fossa).
- (b) Protection of the hernial area by closure of the inguinal or femoral canal.

Of these two Mitchell Banks undoubtedly thought the first most important, but he always fulfilled the second by closing the canal with permanent—that is, buried—silver sutures.

(a) *Closure and Removal of the Hernial Sac as the Chief Step in Radical Cure.*

I have already stated the causal importance of the hernial sac as a developmental relic. If the old theory of causation by sudden violence were correct, then simple ligature of the neck of the hernial sac could not be effective as a radical cure.

Yet that it was actually so has been proved for years by the work of many surgeons, especially Professor Rushton Parker,⁴ and this fundamental principle for which Mitchell

Banks contended at a time when little or no support was forthcoming is thoroughly accepted for the cure of inguinal hernia.

The anatomical conditions of inguinal hernia secure that with the closure of the neck sac there is also an abolition of any infundibular fossa, but this is not the case with femoral hernia, and I believe that some unfavourable criticism of radical cure of hernia in general has resulted in consequence of the operative treatment of femoral hernia failing to completely satisfy Mitchell Banks's first principle—namely, complete abolition of the sac. All present-day operations for the radical cure of femoral hernia which approach the sac from below and include various steps, such as closure flaps of aponeurotic tissue like the pectineal fascia, etc., are liable to failure, though a certain number succeed.

They should, in my opinion, be wholly abandoned in favour of the method of operating from above which I employed from about 1890 at University College Hospital.

It consists, first, in a 3-in. horizontal incision through the abdominal wall just above the inguinal canal, ample room being obtained by firm retraction especially of the rectus inwards. The peritoneum being exposed, traction is made upon it and the femoral hernial protrusion lifted out of its bed intact. The sac is then completely removed and the continuity of the peritoneum made normal. The upper orifice of the crural canal is then closed, preferably by a flap of periosteum and a thin scale of bone from the posterior surface of the pubic bone.

I would suggest that recurrence is impossible by this method of operating, and that it involves no more special procedures than those of approach from below, but that its chief value lies in its complete fulfilment of Mitchell Banks's principles.

Schwartschild⁵ has since employed a similar procedure for the radical cure of a remarkable case of recurrent obturator hernia.

(b) *Protection of the Hernial area by Closure of the Inguinal Canal.*

I have already shown that Mitchell Banks fully recognized the importance of this question, but, curiously enough, he does not seem to have fully grasped the most important step of supporting an inguinal hernial defect by transplanting downwards the lower border of the internal oblique muscle and fixing it firmly to Poupart's ligament.

Perhaps the elastic value, and therefore immensely higher efficiency, of muscle tissue as compared to fascial fibrous tissue was not then realized, nor the fact that the rigidity of aponeurotic fibres not only does not prevent their slowly stretching, but also, of course, precludes any physiological elastic recoil which is essential for the support of the viscera.

Owing, unfortunately, to so many cases coming for radical cure when they have been imperfectly supported by trusses for many years until muscular atrophy has resulted, surgeons have been obliged to employ filigree or other materials to furnish a shield to the abdominal defect, and Trendelenburg indeed transplanted bone for this purpose, but no case of hernia should be allowed to sink into such a difficult and hopeless condition.

To sum up this question of hernia, the removal of which from the community is a matter of such vital importance, I would say that our plain duty is to press to their logical conclusion—namely, early and invariably operation—the principles sketched out for us by our predecessors like Mitchell Banks. Our reward will be that we have then achieved a great benefit for our people and for the State by abolishing a source of danger and suffering.

This leads me to conclude with what can only be a very brief reference to a very large share of Mitchell Banks's useful life—namely, his public work for his city, his infirmary, his university, and his profession. The help he gave to each was so great that it is impossible to do more than express our gratitude by meeting on an occasion like this.

Deeply interested in all phases of humanity and of human life, and especially in the interests and success of his fellow citizens in the city of his adoption, he emphasized in the following pungent sentences the duty of every one of us to take some active part in public affairs and assist in the development of social progress:

We must be something more than mere prescribers of physic and healers of wounds. In my youth I had it strongly recommended to me to stick to my profession and leave everything else severely alone. The life of a doctor was to see patients, do operations, order drugs, and collect fees. I thank God that I entirely repudiated this idea of my profession.

Let us show our respect for Mitchell Banks by trying to follow his example.

REFERENCES.

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PINEWOOD SAWDUST AS A SURGICAL DRESSING.

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SAWDUST has been used from time to time by many surgeons as a surgical dressing, but its merits do not yet seem to be appreciated as they deserve. At the present time, when an efficient, cheap, and easily prepared material is desirable, I wish to draw attention again to sawdust.

Among those who have recognized its value are the late Owen Thomas of Liverpool, the late Surgeon-Major Porter, R.A.M.C., the late Mr. Callender,¹ and the present active surgical workers in Cashmir, the brothers Nevé. In a recent paper by Professor Saundby² reference is made to the value of pinewood sawdust as a deodorizer, which was demonstrated to him by the late Professor Vivian Poore.³

I have used it extensively in time past, and am using it now in Edinburgh at the Royal Infirmary and at the 2nd Scottish General Hospital at Craigleith, not only for operation wounds but also for septic cases of all sorts. It has also been much appreciated as a dressing for the shell wounds of the wounded German sailors in Edinburgh Castle, and it is very valuable for cases of incontinence of urine.

As to the wood, the sawdust from various kinds of pinewood has given good results. The softer kinds are the best, being more absorbent, while the harder kinds, although less absorbent, have the advantage of containing more resinous material. Commercial hard woods like mahogany or oak should be excluded if pinewood can be obtained. A simple method of testing the absorbent property of a sample piece of planed wood is to draw a stroke across it with a pen full of ink. If the ink spreads laterally, and dries in quickly as it would on blotting paper, the fibre of the wood is good for surgical purposes. If the ink remains on the surface, as on writing paper, the absorbent power is deficient. Apart from the wood itself, however, a good deal of absorption will take place between the individual particles of the wood.

The method of preparation I have found useful is the following:

Two large standard wire sieves are required—No. 8, that is, eight threads per inch, and No. 40, that is, forty threads per inch. In order to reject the coarse fragments the sawdust, as obtained from the sawmill, is passed through No. 8 sieve and allowed to fall on to No. 40.

It is then well shaken and rubbed on No. 40, and the very fine particles which pass through are discarded, because they would escape too readily from the prepared pads and cause an inconvenient dust. The sawdust which remains on No. 40 sieve is put into a box or sack and sent to the theatre sister.

The bags are made of butter muslin, and the sizes may be regulated according to requirement. Two sizes have, however, been found generally useful with us, and these may be shaped from a square yard of muslin as follows: Fold the square once on itself and divide the elongated double piece into three. Each of these thirds will make one large or two small bags. One end is left open, the others are closed by sewing if they do not happen to be folded. The nurse fills the bag about two-thirds full with the sifted sawdust, and then closes it with a coloured thread. The filled bags or pads are sterilized by steam in the same manner as other dressings. After use the coloured thread is withdrawn, the sawdust thrown away, and the bag washed, boiled, and dried for future use.

Besides the advantage of cheapness, and the relative