The collegian of 1666 and the collegians of 1885, or, What is recognised treatment? / [by Hugh Owen Thomas].

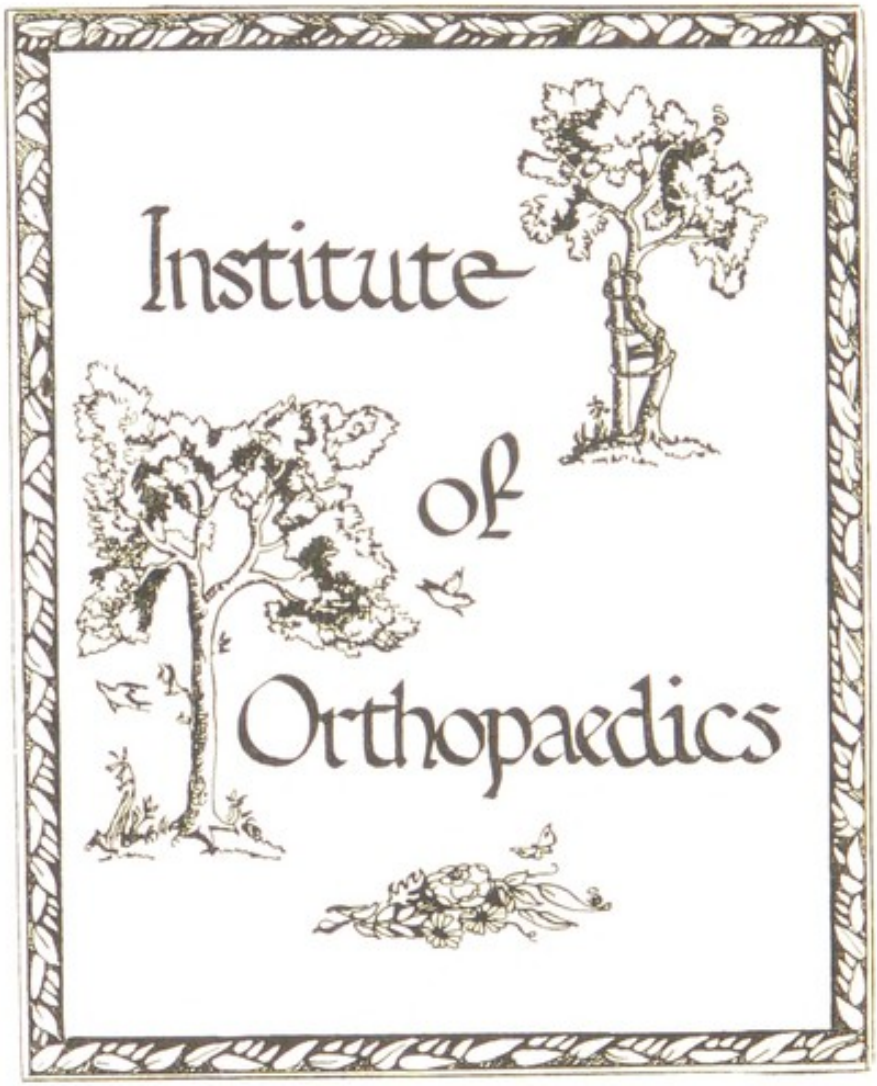
THE COLLEGIAN OF 1666

AND THE

COLLEGIANS OF 1885,

BY

HUGH OWEN THOMAS.



Bernard Roth.

CONTRIBUTIONS

TO

SURGERY AND MEDICINE.

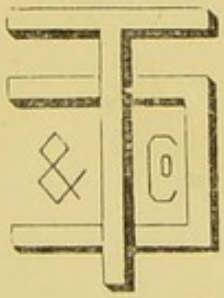
THE COLLEGIAN OF 1666

AND THE

COLLEGIANS OF 1885;

OR

What is Recognised Treatment?



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[1885]

THE COLLEGIAN OF 1666.

"Wherefore I perceive that *there is* nothing better than that a man should rejoice in his own works; for that *is* his portion; for who shall bring him to see what shall be after him?"—Eccles., c. iii., v. 22.

COLLEGIANS OF 1885.

"To sum up, I contend that, until some authority has shown that before 1874, he has treated cases of obstruction according to the true Sydenham method, *ab initio ad finem*, without modification or addition, the credit of rejuvenating the true and original rest, opium, and starvation treatment is due to Mr. Thomas alone, and until the treatment he advocates has been tried by the profession, it cannot be said to be the recognised treatment.—Dr. STEELE, *Lancet*, January 10th, 1885.

Two hundred years ago, Sydenham placed on record several condemnatory and strongly expressed opinions, as to the prevailing mode of treatment in intestinal disease. He appears to have felt very sorely the errors of "the learned men of the parts around." And when we weigh the man, and place in the scales the stringent rules* by which he confined himself, ere he dogmatized, his advice deserves a thorough trial before either qualifying or rejecting. From Sydenham's writings, it is evident that he gained his primary experience, and formed his conclusions, by observing the symptoms attendant upon the intestinal lesions of typhoid fever.

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*Preface to Third Edition.—"I by no means am satisfied with the record of a few successful cases of operations either of the doctor or drug. I require that they be shown to succeed universally or at least under such and such circumstances."—Sydenham.

From an early date, during my professional training, when articled to my uncle, the late Dr. Owen Roberts, St. Asaph, many opportunities offered to direct my attention to the common treatment of intestinal complaints, those specially included under the rather indefinite term "acute obstruction." My first literary contribution to the subject of intestinal disease was written 1875, but did not appear in print until 1878,* and the fourth and more detailed publication of my views was published in 1883. It will perhaps surprise the reader to find me writing a controversial pamphlet so soon, and having nothing to add or take from what I wrote in 1883. But since the last edition of my treatise on intestinal disease, I find, that while not a few of my contemporaries give willing ear, others are still strenuously opposed, to my views, and some deride the contention on my part that, it is any new departure in the treatment of these complaints. For instance, either from a misinterpretation of my teaching or not knowing how to use sedatives in certain cases of intestinal diseases, it has been stated that "opium only masks symptoms and brings about a fool's paradise where the surgery of hope may exercise

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*To my original manuscript there hangs a tail, as evidence to show the state of medical opinion regarding the treatment of intestinal diseases. When it was first handed to the editor of the *Liverpool Medico-Chirurgical Journal* in 1876, he returned it with the remark that I had better write upon a subject which I understood. After some nine months had elapsed, he requested to see the manuscript again, when he said he would accept it on the condition that there should be less Sydenham in it, as he could not perceive how references to that author were relevant to the subject discussed. I acquiesced, and it appeared in the journal, dated January, 1878. The publication of the Journal was unavoidably delayed one year, the manuscript of my paper having been handed to the editor in latter part of 1876.

itself at the expense of scientific knowledge and patients' lives."

Another has trumpeted forth that the Sydenham or physiological method "had now for many years been the recognised treatment," This was echoed by another "A measure the value of which I consider had been for years recognised." In fact, the assertions of these gentlemen amount to no less than this—that the treatment advocated by Sydenham and myself has been taught and practised by most physicians during at least this century. None have yet questioned the claims I have put forward on behalf of Sydenham, but, as regards myself, they do not admit the fact that I was the first to discover the essential merits of his practice. Sydenham is safe, but my position is not an envious one, for if the assertions be correct, I am guilty of strutting about with the borrowed plumes of my contemporaries, nay, of my friends and neighbours. I must admit that the gentlemen from whom I differ have my unalloyed respect, and that they certainly have expressed their dissent *as true artists*, and only in the interest of their art. To-day I maintain, that in the treatment of intestinal diseases, in their varied forms, the prevailing practice is a proof of the want of correct knowledge on this subject or we should not witness the random treatment which is at present followed. No two practitioners treat cases alike, even when exactly square in their conditions and signs,—those whom I have influenced being the only exceptions to this practice. I also maintain that the physiological method of treating intestinal lesions supplemented by a sedative, or the knife—if required—is rational and

satisfies the demands of my predecessor of 1666. That it is a rule complete for the practitioner and "from which, in this disease, he need not deviate a single straw's breadth," and, if followed, will, better than any method now known, tone down the urgent cry for a lessened mortality, and enable the physician to cure when possible, at the same time to be preparing the patient for any eventuality, and indicate the time when his assistant, the surgeon, is wanted. These cases belong to the domain of medicine and generally fall into the charge of the physician first, and as their medical treatment has hitherto been attended with a high rate of mortality, surgeons have supposed that the knife would much lessen this rate; hence the constant lamentation that this or that case was not operated upon earlier.

During the last thirty years, since the introduction of anæsthetics and the Listerian method of operation, incisions have been made oftener and earlier, and often needlessly, but we are told the mortality is not diminished, yet still the advice is "operate earlier." For this demand there are good excuses when we only take in consideration the "higgledy-piggledy" treatment the patient may have to undergo, every day an operation is delayed. Once an operation is performed whether it were wanted or not, it much neutralizes the evil of varied medication.

The physicians, conscious of their want of success in this field, are needlessly crying out for more and more surgery; and surgeons, perceiving that much evil is done by the

physician, before he is invited, cry out for the cases to be handed to them earlier, before medicine and interference, indefinitely used, has thrashed all vitality out of the sufferer.

The question of the correctness of my teaching is not mainly a matter of controversey, but a clinical one. Let it be therefore tried according to "the true Sydenham method *ab initio ad finem*, without addition or modification." I have elsewhere given such details that the mechanics of the method can be tested upon the healthy gut. There also have been indicated the signs of the smallest appreciable degree of intestinal malaise up to the intenser forms termed acute obstruction. There again have been made known the indications of complete recovery, very important additions to our knowledge, as hitherto many a sufferer has passed through the critical period of the ailment, only to die from relapse of the disease, the attendant seldom being acquainted with the signs indicative of its complete subsidence. I have also shown a safe method of employing sedatives, so that they become assistants to overcome, not aggravators, of the difficulty, as hitherto they have been observed to be.

In the following pages I shall give reasons and proof, that will set aside the objections of my present opponents, evidence which shall equal any confirmation required, outside of mathematics. As preparatory to my defence of the doctrines of Sydenham and myself, I wish to state a fact. Until I commenced to pen the first of my contributions to this subject, I was not aware that Sydenham had, in teaching or practice, preceded

me. Between him and me there is a theoretical difference ; he laid much stress on the use of sedatives, the dietary being supplementary only, which, we may reasonably conclude, led his successors astray.

My teaching is that the dietary is of primary importance; that the sedative, whether it be chloroform, chloral, opium, or brandy, is only supplementary—not essential—though at times very useful, but, when special discord of symptoms appear, these drugs are very hurtful.

It is a most remarkable fact, that since Sydenham's death, there has not been published by any one, myself excepted, any reference to his views, except in an encyclopedia of of medicine and surgery published during the latter part of the seventeenth century.* In this encyclopedia Sydenham's treatment of intestinal ailments is alluded to, and, like many of the present generation, the editor "got hold of the wrong end of the stick," and stuck to the opium to the neglect of a suitable dietary, and found that the sedative treatment was a long, pleasant, and certain way to kill. This is the method now termed "the opium treatment," and which had been always tried before and after the Sydenham period, alternating it with the main force treatment, irritating, stimulating, and indiscriminate interference, a short, most painful, and also a certain way to kill. The third way has yet to be tried—tried undiluted,

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* I have in my volume given evidence that even John Hunter knew not the value of Sydenham's treatment. (See page 8.)

a physiological or natural method, any interference with which arrests its progress; it only permits the use of supplements, even that of the knife, when it demands it, need not interfere for an instant with progress to resolution. This has been termed an expectant method, which is not a fair designation, as it by no means partakes of the do-nothing policy, because there is much to be done, but most of what is usually done has to be omitted.

The principle event which makes it imperative upon me to defend my opinions, occurred at a meeting of the Liverpool Medical Institute, October 23rd, 1884. The subject of Intestinal Obstruction was introduced by Dr. Hyla Greves, who prefaced the subject by the report of a case of mechanical obstruction, in the treatment of which, resolution of the difficulty was made possible by a supplemental interference—abdominal section. The debate which followed the reading of the report had been carefully organised, each contributor having been invited and requested to give notice of contribution, consequently, the utterances of those who responded to the invitation was of special interest. The introducer of the subject requested the members to debate according to the Socratic method, by placing before us the following questions for reply—

First.—"What are the principles which should guide us in the treatment of the earlier stage of obstruction, (*i.e.*, before operation is considered justifiable), and what are the means at

our disposal best calculated to give relief, and bring about resolution?"

Secondly.—"The diagnosis of acute obstruction having been established, is operative interference justifiable, either for the purpose of confirming the diagnosis, and if possible relieving the obstruction at the same time?"

Thirdly.—"If so, at what period should the operation be performed and what are the symptoms and circumstances of the case which justify such proceedings."

The entire proceedings have been since published in the organ of the Institute, the "*Liverpool Medico-Chirurgical Journal*," January, 1885,

Dr. Greves reported this case of obstruction as having been treated, before admission into a public hospital by purgatives by the mouth, and after admission, by enema, and externally, by counter-irritants. For these items of treatment Dr. Greves was in no way responsible, as he substituted an opium treatment. On the fifth day the symptoms convinced him that an operation was indicated, and abdominal section was successfully performed by Mr. R. N. Pughe, after which the case was, with intent, treated strictly by the physiological method, with such success, that on the ninth day after operation, the primary, secondary and testiary accumulations had passed, showing that an unappreciable degree of lesion only could be remaining at the part of the intestine involved, and consequently, thus early his dietary was less restricted.

The conclusions, drawn by Dr. Greves's from watching this case, were five. To the third of these I shall draw attention, "The utter failure—in this case—of dietetic and medicinal treatment to relieve symptoms." Now if we admit that no dietary or medicine could probably—though not impossible—have saved life in this case. Yet as the report informs us that the primary treatment consisted of a diet of milk and beef tea, with enemata, turpentine fomentations, belladonna embrocations, opium with belladonna internally to minimise the effect, of the latter, on the visceral nervous ganglions; this "recognised treatment," neither a Sydenham practice, nor an approach to the physiological method, which I have contended for, could not even have masked or mitigated the symptoms sufficient to prepare the case for operative interference.

The narration of this case being concluded, the speaker proceeded to answer the three questions which he had laid before the members of the Institute for solution. His answer is given, and classified, as the "dietetic," the "medicinal," and the "mechanical" means of controling intestinal obstruction. Under the head of "dietetics" he adopts all I have taught regarding the dietary, but under the title of "medicinal," he differs from me, for instance, "opium should be given in proportion to the pain; if this is not severe and if there is no shock the patient is better without, as it increases constipation and masks symptoms.' I maintain that the approach or the presence of shock prohibits the use of sedatives; and to the contention that sedatives must increase constipation I also object. Can the opiate or any

sedative increase the constipation? If the patient is dieted in the manner Dr. Greves fed his patient in this instance after operation, then I assert that no sedative, except it be given in sufficient dose to kill, can increase the constipation. This is not a question of opinion, *it is an easily demonstrable fact.* If an unsuitable diet were consumed then the sedative would rapidly and fatally constipate. In the case even of a healthy subject, if a suitable diet were used, no safe dose of a sedative could constipate beyond the tenth or twentieth day. It is an impossibility, a fact I have very many times clinically demonstrated to my medical friends. As regards the "masking symptoms" objection, the practice of medicine and surgery consists mainly in the masking of symptoms, and that the more we are able to mask the evils attendant upon the diseased state, the more we are valued by our patrons. That which is understood by skill, is that symptoms of over-action are moderated and those of depression are urged up to the normal standard. The practitioner who knowing the cause, dreads prescribing directly or indirectly, for the symptoms of the disease, acts like a sailor, who not having learnt the art of navigation, dreads to lose the sight of land. An intelligent practitioner must be able to know the real condition of the patient despite the intervening action of his remedies. This talk about masking the symptoms, so frequently advanced in discussions regarding intestinal obstruction, is an unwitting confession of our ignorance and proof of our want of defined knowledge. It is a remark seldom or never

made during discussions of other ailments; indeed it is a phrase which shows our undecided opinion regarding the present treatment of these complaints. Under the designation of "mechanical," Dr. Greves accepts means, the use of which makes the natural tendency to recovery improbable of progression or completion, granting that it were strongly inclined to commence. He completes his answers by recommending a recourse to earlier operative interference, and draws attention to the high rate of mortality after operative interference, which he ascribes to delay. Indeed when we analyse what the expression "a recognised treatment" usually means, then I say the earlier the knife is used the better, before the patient's courage and vitality have been frittered away by treatment partly intended to correct thedifficulty and partly in vain attempts to diagnose differentially, whilst developing symptoms that do not even pertain to the ailment. For instance, take the prevalent opinion that the discharge of blood and mucous is specifically diagnostic of intussusception, these latter signs are extremely rare in such cases, but they are common where the subject has undergone much meddling.

The next speaker was Mr. R. A. Pughe, who described the details of the operation, in this very necessary instance, which must have been performed with much deliberation and skill to have recovered so rapidly. In the last paragraph in page 131 of the Journal, Mr. Pughe gives as his surgical conclusions, and he relates what was observed during the operation as justifying such interference, but he did not

mention that he found before, during, or after the operation, symptoms or conditions from which he could lay down a rule that might guide a practitioner like myself, when confronted with a like difficulty. This omission can be filled up by the watching of symptoms during the strict practice of a method which I shall show to be as yet an unrecognised one.

At page 132-3 of the Journal report, Mr. Pughe candidly admits the difficulty attendant upon differential diagnosis, and proposes abdominal section "as a means of completing or correcting the diagnosis," and if no removable cause be found, then to convert abdominal section into that of a pesudo-gastro-enterotomy with exceptional risks, the long abdominal incision. My own sphere of practice, like that of Mr. Pughe, is mainly surgical, and so we are both likely to be biassed in the direction of the use of the knife, but withal, I believe that if this groping in the abdomen, for diagnostic purposes, were to become general, the mortality would rise higher than it has hitherto been. Listerism would not save us, for, as Mr. Pughe very pertinently remarked in these cases, we have to operate in the presence of a serious disease, a drawback by which ovariotomists are not hampered. At all times I have specially urged that, as a rule, the operation of gastro-enterotomy (ileo-laparrotomy) should have the preference, and was the first publically in this country to advise a return to this operation. Gastrotomy or abdominal section has its analogue in herniotomy, the opening of the sac and division of impediment, with reduction of gut

untouched, while gastro-enterotomy is more in accord with the opening of the hernial sac, relieving of the constriction, and incision of the protruding intestine. After abdominal section, the physiological method of treatment is very essential, but after gastro-enterotomy it is less an element towards success.

At page 132, Dr. Pughe refers to gastro-enterotomy thus—"As Mr. Banks writes, when he suggests ileo-laparotomy as the probable future operation in these cases, it gives nature time to act." At the end of the same paragraph an objection is advanced to the operation. For instance "A case of strangulated inguinal hernia would certainly end fatally if the hernia were not relieved and ileo-laparotomy were performed instead," Not certainly, but probably. With most of Mr. Pughe's remarks I cordially agree, but this comparison is not just to the pretensions of this operation, for instance, if the gut be strangulated by a band, as in the case reported upon, even the operation of gastro-enterotomy would have relieved almost all tension; first the accumulation above the impediment, this source of pressure being removed, the cause mainly producing tension of the constricting band would be gone, and as the chance accumulation below, the difficulty cannot effect the constrictor, there would remain the band less light, and liable, like an antisceptic ligature, to gradual relaxation or removal, it is otherwise with a strangulated hernia, gastro-enterotomy would only take off the weight from above, but would not relieve the contents of the loop griped by the sac entrance, a very powerful and resistant body; the intestinal loop contents

could not pass either up to the enteric opening nor down towards the anus. No one has yet proposed gastro-enterotomy as a substitute for herniotomy.

The history of the operation of gastro-enterotomy shows that which is "recognised treatment," and also what interpretation to place upon the opinion that my teaching is "a measure, the value of which I consider had been for years recognised." The history of this operation is also testimony showing that if Sydenham and myself do not monopolize all knowledge of this subject, yet we are better informed than those who think proper to maintain that we have taught only matter of common knowledge.

The merits of this operation over other modes of direct interference are very obvious, for it has been now for over a hundred years on trial, and has grown steadily in favour. Why? Because it has been the best antidote for the "recognised treatment," and is specially applicable for the relief of cases of obstruction, arising either as complications or sequelæ of other diseases.

This operation is the making of an opening in the abdominal wall and stitching to this gap, a presenting portion of bowel and these incisins it. Historically, it has been known and performed in France for over a hundred years, but with indifferent success. The reader, by a perusal of Trousseau's lecture on intestinal occlusions, can, beyond doubt, be satisfied that the cause of the minimum success which resulted, was that the medical treatment then, is the recognised treat-

ment of to-day. This operation was tried in France by nine well-known surgeons previous to Nelaton, in conjunction with Trousseau, adopting it; one of the earlier surgeons going so far as to advise that gastro-enterotomy should supplement herniotomy if this operation failed to relieve the signs of strangulated hernia. In this country the history of this operation dates from 1817, when Mr. G. Freer, of Birmingham, performed it there. Then Mr. Daniel Pring, of London, operated in 1820; afterwards a writer in the *Liverpool Medico-Chirurgical Journal*, of 1877, advised its general adoption in obstruction, then followed Mr. Bryant in 1878, and, last of all, Mr. Banks, discerning the signs of the times, comes forward and takes the part of forerunner, proclaiming the operation of gastro or ileo-enterotomy as the "operation of the future." It is evidently going to be favoured with a "fair breeze" of popularity. Before parting, however, with this subject, I shall refer to some of the vexations the operation has had to contend with before it found its way into the confidence of its present admirers.

There are certain records of its successes and disappointments during several decades, and a study of these records informs us that no matter which decade we elect to represent "recognised treatment," the treatment before and after operation consisted of the same omissions and commissions in 1817 as in 1878. With like knowledge treatment must ever be repeated; if incorrect it can only be changed when better knowledge is gained. In the matter I am here debating, 1817 stands alongside

1878, but in 1884 there were and now again there are signs of change. In proof of the fore-going I shall give a summary of two pairs of cases, with an interval of seventy years intervening between each pair. In the *Medical and Physical Journal*, January, 1821, there is a report of the two cases of obstruction relieved by gastro-enterotomy by Messrs. Freer and King. In the *Lancet*, of May 25, 1878, Mr. Bryant reports two such cases.

If the reader will, without bias, compare these two cases of gastro-enterotomy treated in 1817 with those treated in 1878, he cannot come to any other conclusion than the following:— That Mr. Freer, at least, accelerated the death of his patient by the treatment before and after the operation; that Mr. Pring converted a case of chronic constipation into an acute one by his primary treatment, and that the same treatment repeated after operation was again near being the causing of the death of his patient.

That Mr. Bryant's first case did well because it was subsequent to operation, treated with less of the details attached to what is termed "recognised treatment." That Mr. Bryant's second case did not progress satisfactorily because the "recognised treatment" led to an "acute obstruction being grafted upon a chronic" one. That after the operation the "recognised treatment" was resumed, and after every application of this treatment signs of retrogression generally appeared. It is true that in this case a *post-mortem* showed occlusion of the gut, but, as after operation, interference with the lower segment of the bowel, which included the diseased part, was continued,

this, I believe, would not favour any tendency to repair. After the operation of gastro-enterotomy, interference by enemas is more injurious than that by purgatives ; for the operation isolates the diseased part from being mechanically irritated by the purgative, though permitting its being physiologically stimulated, while on the other hand, after operation enemas still reach, and may harm, the diseased part, now only a portion of the lower segment of gut.

Mr. FREER S CASE, 1817.

Mr. Lowe, farmer, aged 47 years, "temperate and regular in all his habits," residing near Birmingham, in charge of Dr. Johnstone, who had prescribed for him occasionally with temporary advantage. At the time Mr. Freer saw him "he complained of a fixed, dull pain at the lower part of belly," and "was a good deal troubled with flatulence and other symptoms." "He was exceedingly costive, and his fæces were compressed in a very unnatural manner." An examination per rectum informed the surgeon that "there existed a contraction of the rectum" almost beyond reach of the finger. Clysters were now used, and answered their purpose for two months, when they ceased to be effective, and for three days the constipation was complete, bowels tense, uneasy. "Now elaterium was given with clysters of aloes," but without success. About the sixth day of obstruction vomiting set in,

Mr. BRYANT, 1878.

W. B., stonemason, aged 57, February, 1877, six months before admitted into hospital he became ill, vomiting, violent colic, abdomen became swollen and hard, this recurring periodically once a week, the bowels constipated, latterly the attacks became more frequent, twice or more weekly. The vomit had a a fæcal odour. Took powerful purgatives ; "on an average he had a motion *naturally* about every four days." On admission the abdomen was found distended, never passed blood ; constipated *three days.*

February 12th—Had an enema, no effect followed ; opium at bedtime."

26th — Constipation with sickness ; belladonna given three times a day.

March 19th — Has had relief from the bowels, although during

Mr. Freer's Case—*Continued.*

pulse quick with anxious countenance; an attempt was made to divide the striction per rectum, but it was not successful. After this interference the patient was ordered an opiate and warm bath. On the seventh day "we found him tolerably free from pain, no sickness, no action downwards," although he had taken several doses of castor-oil and had had emollient clysters. Now the gentlemen conjointly in charge decided to "make an incision through the parietes of the abdomen in the left iliac region," and then incise the intestines. Mr. Freer says that the possibility of such an operation had occurred to him two years previously, and that he had, in conjunction with Dr. De Lys performed it on a child with imperforate anus.

On the eighth day the symptoms became worse, gastro-enterotomy was performed, and distension was fully relieved. On the ninth day pulse quick, tongue furred, and *an aperient mixture ordered* with an opiate at bed-time.

On the ninth day the patient "was bled," which relieved the extra pain; "the pulse, though quick, had become soft," and through the artificial anus was discharged thin feculant matter, and there was constant rumbling of wind, but no distension.

On the 10th day, as the bowels were rather tense a clyster was given

Mr. Bryant's Case—*Continued.*

the last ten days he has suffered much; was ordered nux vomica and opium.

April 5th — Has just passed through another of his attacks and had relief;" motion natural; medicine continued.

16th — Has had another attack; vomiting severe; bowels acted again.

26th—Another attack of pain; bowels constipated.

On the 29th it was decided to perform gastro-enterotomy.

After the operation the temperature was 97·6; pulse 84. Opium given every four hours.

30th—Comfortable, influenced by opium; no pain; fæces passed through the abdominal opening; takes nothing but milk. Temperature, 98·4; pulse, 66.

May 2nd—"Takes milk. It is said that 'he was very comfortable,' though in 'intense pain occasionally." Temperature, 97·6; pulse, 74. Tongue clear and moist.

3rd—Not so well; pain more frequent; fæces passed through the opening. Temperature, 99·3; pulse, 76.

Mr. Freer's Case—*Continued.*

through the wound, but it did not produce the desired result. This day the tongue was dry and furred, pulse quick, calomel and rhubarb were given, with an opiate at bed-time and an opiate mixture occasionally.

Eleventh day—The laxative had operated freely, but he had passed a restless night.

Twelfth day—"He continues very restless at night."

Thirteenth day — Better night, but his appearance was not satisfactory; no appetite; complained of a sense of sinking at his stomach, which was relieved only by taking food very frequently in small quantities. "His diet for a long time, and especially since the obstruction had become so inveterate, had been liquid of the lightest kind, and his beverage of the mildest, such as toast and water, or imperial," This day his pulse was very quick and weak, his tongue clean but dry, and intolerable thirst.

Fourteenth day — Better; had passed a quieter night, voided several stools, and a quantity of dark-coloured fluid *mixed with excrementitious matter* was passed per anum. Again he was ordered an aperient mixture.

Fifteenth day — Had passed a "very disturbed night, and he had lost all hope of recovering; pulse quick and feeble, skin cold and clammy, very thirsty, no appetite,

Mr. Bryant's Case—*Continued.*

4th—No sleep, great pain; took jelly, beef-tea, milk; fæcal matter passing; morphia given. Temperature, 97·6; pulse 70.

5th—Better. Temperature, 98·2; pulse, 76.

7th—Temperature, 99; pulse, 88.

8th—Temperature, 99·4; pulse, 88.

11th—Feels quite well; solid food allowed.

26th—Return of pain; enema given and hard motion passed per rectum.

29th—Gets up daily.

Left hospital August 20th.

On examination twelve months after, he was found in excellent health.

taking small quantities of brandy and wine and water; voided "several stools," "still as the belly was rather swollen and the stools unnatural, it was thought right to repeat the tincture of rhubarb" so as to promote the "free expulsion of wind and fæces."

Sixteenth day — Subsequent to the operation patient died.

Mr. D. PRING, 1820.

Mrs. White, widow, 64 years of age, of full habit, health generally good. About midsummer, 1819, she was first troubled with pains in the abdomen in the left side, below the ribs; stools often contained blood with mucus; her disease was supposed to be dysenteric and had been treated by "mercury, ipecacuanha, opium and saline aperients," which treatment appeared to cure until Christmas, when she suffered from "inflammation of the bowels, with obstinate constipation." "Her bowels had for some time previous been occasionally painful and the discharges irregular; she had fluid stools, but when not suffering from spontaneous diarrhœa was under the necessity of taking purgative medicines, to the neglect of which she imputed the attack of inflammation which occurred about Christmas. Re-

Mr. Bryant. Case No. 2.

Robert R., aged 50, admitted into hospital, after having suffered for two months from abdominal pain with constipation, which had steadily increased up to the time of admission.

On July 2nd, four days before admission, pain with vomiting appeared, the abdomen became distended, and he was treated by castor oil and turpentine enemata with purges.

On admission on the fifth day of the attack the symptoms were those of *acute obstruction grafted upon chronic*, distended abdomen, constipation complete, frequent fæcal vomit, pulse feeble. It was decided to perform gastro-enterotomy. After operation morphia was given by suppository. Temperature, 98; pulse, 82.

Mr. Pring's Case—*Continued*

covering from this attack she lapsed into her former state of chronic disease, of a dysenteric character. When the patient first consulted Mr. Pring she described her feelings as that of everything taken "appeared to stop at one place." A rectal examination gave no decisive information, and the case was treated by "castor oil, epsom salts, &c." When these failed, "as they sometimes did," then "powerful purgatives, aided by clysters with soap and aloes, &c., were used, but on the 25th of June, 1820, this method of treatment utterly failed. The obstruction which had long been increasing appeared to have become complete." *All the resources of art were afterwards exhausted to procure evacuations—salts, senna, aloes, colocynth, jalap, scammony, gamboge, elaterium, calomel, castor oil, and as vomiting was by no means frequent these medicines were retained, injection of every sort and by diffirent means.*" "It was attempted to pass a flexible catheter beyond the obstruction, through which clysters might be thrown into the bowels about the seat of difficulty." Pulse during this time 90, seldom over 100; tongue dry but clean; no vomiting, except after medicines or food; considerable distension and tenderness of the abdomen; when all measures had failed laudanum was given

Mr. Bryant s Case—*Continued.*

Next day, July 8, had passed quiet night, no pain, no vomit;

taken one pint of milk; discharge of fæces through the opening. Temperature, 98·2; pulse, 88.

9th—Going on well; Temperature, 98·2; pulse, 88; "enjoys milk;" morphia twice a day.

11th—Doing well; Temperature, 98; pulse, 80.

17th—Doing well; no change.

18th, 2.30 p.m.—A soap enema; 9.30, temperature, 98·4; pulse, 100; enema acted freely; nothing but motion passing; felt much better.

19th — Some pain; morphia given.

20th—Paroxysmal pains.

21st—No sleep on account of paroxysmal *pains in the abdomen, the painful part* (abdomen) *felt tighter*; an enema of oil and gruel of no effect, another of soap and water, when some hard masses passed *with shreddy tissue*

Mr. Pring's Case—*Continued.*

"in the supposed possibility of spasm existing." "It appeared quite clear after the persevering trial of the above means for many days, that the obstruction, by whatever caused, was one which could not be overcome." After this treatment had been tried twelve days, it was decided by consultation that gastro-enterotomy should be performed and it successfully relieved the urgent symptoms. The patient passed a good night, and took gruel.

Next day — second day after operation — pulse 100 and 110, tongue dry, partook of gruel, beef tea, and aperient medicine.

Third day — Wound inflamed; general appearance not favourable; pulse quick and feeble; tongue dry. For eight or ten days the wound was inflamed, which was of the erysipelatous kind, and continued to increase, and produced a most extensive sloughing of skin, cellular tissues, and fascia for several inches around the wound." During this sloughing the wound was poulticed, and the patient took bark, ammonia, and aromatic confection, port wine, beef tea, and purgatives.

The inflammation subsided in two weeks after the operation, and finally and very wonderfully this patient recovered. The full details, as given by Mr. Pring, almost makes the reader incredulous of the possi-

Mr. Bryant's Case—*Continued.*

25th, 11.45—Much pain in the *lower part of abdomen*; this also hard, slept well; vomiting returned accompanied by severe pain; another enema; vomiting continued; again an enema; vomiting ceased and he retained a dose of medicine in the stomach; temperature, 97·2; pulse, 130; pain paroxysmal.

26th—Pain, with vomit of fæcal odour.

27th—Nutritient enema.

28th—Some more enemata.

30th—Bad paroxysmal attack of pain lasting two hours; morphia given every twelve hours.

August 4—Soap enema, no pain.

6th—Ordered chicken.

23rd—Returned home.

June, 1878, this patient is reported to have died from bed sores at home. A *post-mortem* revealed the existence of an annular stricture of the ascending calon. At the end of the report of this case the following is given as the cause of the *post-mortem* appearance:—"He fell over a case of goods; and the injury induced chronic inflammation

Mr. Pring's Case—*Continued.*

bility of recovery. In the concluding notes of this case, we are informed that the patient after operation was "constantly under the influence of aperients" and clysters of four ounce measure which were never long retained.

Three months after operation the contents of the bowels began, and afterwards continued, to be voided per rectum.

Mr. Bryant's Case—*Continued.*

with gradual thickening and contraction."

Mr. R. A. Pughe concludes his interesting report, and replies concerning the surgical aspect of the question under discussion by informing us that "abdominal sections for intestinal obstruction have not up to this time been very encouraging."

In discussions regarding the question of mortality, there prevails all round a sad sameness. Statistics, no matter from whence culled, tell the same tale and give no information as to the cause. Here is my estimate of the weight to be attached to the statistics pertaining to past treatment. There are four classes of patients :—

First—There are those with inherent defect, they must have terminated fatally sooner or later.

Secondly—There are those who were subjected to needless operations ; some of whom might have lived.

Thirdly—There are those who were not operated upon, as defined signs demanding operation were not known, where a

sacrifice must have occurred. "The surgery of hope" practice might be credited with this class.*

Fourthly—There are those cases—and they have formed the majority—where at first the difficulty has been a chronic one, but the primary treatment has "grafted," or rather goaded the disease into an acute one.

The foregoing is my analysis of the high rate of mortality attendant upon these diseases.

On the termination of Mr. Pughe's instructive address and contribution, the matter of debate was taken up by Dr. Waters, and whose address may thus be summarized—

First, after admitting the difficulty of arriving at a differential diagnosis, he advised complete or almost complete abstinence from food, and the exhibition of opium and belladonna until acute symptoms subsided, and then to give large enemata, but no purgatives.

After this medical opinion had been delivered, Mr. W. M. Banks gave the meeting his views, which were principally surgical. The speech is here reproduced as reported, for certainly I think it well merits publicity, and further my publication of a summary of this speech would not be just in view of the very interesting and instructive correspondence which subsequently arose in relation to it.

Mr. Banks considered that two serious difficulties would always present themselves in connection with the question of operation, the first being the difficulty of diagnosis. After eliminating the more obvious causes of obstruction, such as impaction of fæces, intussusception in young children, and cancerous or other growths in the adult, capable of being felt, he

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* A phrase introduced by Mr. Bryant to designate the past method of using sedatives.

believed that in all the other cases anything like a certain diagnosis was impossible, and that in his own instance he now, after considerable experience, felt no more certainty about pronouncing upon the nature of the disease than when he first began. In the only case in which he had operated the symptoms pointed in a very clear manner to one affection, but when the abdomen was opened a different and totally irremediable condition was found. The second difficulty consisted in the fact of *spontaneous recovery under the opium, rest, and starvation treatment, which had now for many years been the recognised practice.* As everyone of any experience knew, recoveries took place under such treatment after obstruction had existed for weeks, so that he felt that an indiscriminate employment of the operation would result, for the most part, in the finding of conditions which either could not be relieved or which in time would have relieved themselves if left alone. On the other hand, while deprecating rash interference, he freely admitted that the *post-mortem* table every now and then showed us, to our great mortification, conditions which might have been relieved by early operation. At the present moment our pressing urgency was more knowledge in the way of clearly defining this limited class of cases. While he trusted that time would improve our means of diagnosis, at present he would only operate, to put the matter in the simplest possible way, in the case of a child or healthy young adult suffering from all the symptoms of an acutely strangulated hernia, but in whom a hernia could not be found. *

With Mr. Bank's surgical opinions, as expressed in the foregoing speech I concur, because I have not as yet changed the views which I held and taught in my various contributions to the literature of this subject. We do not vary on paper a "straw's breadth," and I trust that any person perusing my comments will not suppose that I am so vain as to imagine that from myself arose the influence which acted to form Mr. Banks' published surgical conclusions. I only maintain that

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* Reproduced here as published in the *Liverpool Medico-Chirurgical Journal*, January, 1885.

which is a fact,—that I preceded him only by a few years, not two hundred. During this speech, so correct in my opinion, in its surgical advice, the speaker digressed and ventured a passing remark regarding the medical aspect of the question. His senior, who had preceded him, had, I suspect, not come up to Mr. Banks' now advanced views, He said that "the second difficulty consisted in the fact of spontaneous recovery under the opium, rest, and starvation treatment, which had now for *many years been the recognised treatment*," an expression which appeared to some to be obscure, and led to a discussion relating thereto in the *Lancet*. This correspondence hinged almost entirely on the meaning of the two words, "many years." Now, as Mr. Banks gave utterance to these words, it is only reasonable to suppose that he also best knew what meaning was intended to be conveyed by them. The sense in which Mr. Banks intended the words "many years" to be understood, was as the equivalent of "now." My lexicon is Clinical Notes, written by Mr. Banks, published by Miller & Co., Glasgow, 1884, page 102, "Moreover, *now* that purging and other violent remedies have been given up, and treatment is directed towards ensuring repose of the bowels by opium, with the view of letting nature overcome the obstruction, such recoveries are decidedly on the increase." The last phrase of this quotation restricts *now* to a very late date.*

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*At page 274 of the last edition of my volume on Intestinal Disease there is proof that my personal knowledge confirms this.—The episode there refered to took place early in 1883.

This I know, if Mr. Banks acquired the information from the same fountain as myself, he did not mention it until now, but even so, I was there before him, despite the fact, I could find no light in front of me as a guide, and for a long while after my return, only one in the profession welcomed me.* Before Mr. Banks is qualified to question Sydenham's knowledge of this subject, there is a something wanting even in his surgery, viz. : an answer to Dr. Hyla Greves' question, "at what period should the operation be performed?" †

After Mr. Banks had finished, the meeting gave me their attention. My address, which is here reproduced as delivered, is merely a summary of the more detailed opinions published in previous years.

"Stated in a general way, the treatment, in my opinion, required for intestinal complaints, is this, that it should be directed towards controlling medically and the supplementing surgically of the automatic efforts at resolution, but not supplanting. The symptoms of obstruction, if they are carefully analysed, enable us to use proper means and give the patient the best possible chance of escape from his difficulty. During the early period of obstruction, the patient suffers principally from pain, thirst, vomiting, and loss of appetite. Then let the pain be neutralised, which must extend the patient's period of endurance of the disease ; thirst must be satisfied, and the liquid, if containing no ingesta, commences to prepare for removal the load situated above the spot obstructed ; vomiting will be moderated by the means known to ease pain, and thus the liquid

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* I hope that my reader will not interpret this paragraph as having been written in the spirit of one with a grievance, but I would rather it should be taken as further evidence that my teaching at least was not already a matter of common knowledge. It is my opinion that all innovations are much too readily accepted and practiced, especially if they are announced from some elevated rostrum.

† See *Lancet*, January 17th, 1885, page 131, correspondence,

imbibed is retained a longer time, and becomes better mixed with ingesta to be rejected by the mouth. The loss of appetite is accepted, and no food is offered; but should the appetite return, ere our purpose has been attained, then the quality of the nutriment allowed must be selected. The only meddling required is that by the knife.

What good can either medical or mechanical irritants do? The upper way is blocked and backed with a load. The lower segment of the intestine, in nearly every instance, has already been emptied by the peristaltic action excited by the irritation present from the initial period. Indeed stimulation even per rectum is seldom harmless and never useful. When the moment of primary relief comes such a torrent descends that no amount of chance accumulation in the lower gut can withstand it.

In answer to the question—"Is operative interference justified either for diagnostic purpose or to relieve the cause of obstruction. If so, at what period should the operation be performed, and what are the signs which justify such proceeding?"

It is my opinion that to operate for diagnostic purpose would be to endanger the many and perchance save the few; these cases of obstruction belong to the department of medicine; the surgeon is a mere assistant.

There is a time for medical treatment and a time for surgical intervention. It is the period when the physician observes that his remedies fail to beneficially influence the case all round. For instance, though pain may be lessened, vomiting moderated, temperature lessened, yet should the tongue appear brown and dry, and the pulse despite our sedatives decrease in volume, although reduced in rate, showing a general discord, the surgeon ought to be at hand, and a decision to operate should be come to and carried out.

It is not a question of period, whether early or late, but a question of symptoms, that should influence us in deciding to interfere surgically; an operation may be required in a few hours, or it may not be wanted for some weeks. Nature always operates late, and her action is not preceded by the signs which I have enumerated as being an invitation to the surgeon. When those symptoms are present, death would supervene long before the tedious natural process of dividing the abdominal wall was completed, a process I have on several occasions observed.

Pure cases of obstruction, cases with signs of obstruction and not caused by invasion of the peritoneum, through perforation of the intestine

or by hæmorrhage from a vessel, if treated by the Sydenham method from the begining, never show urgent signs before the third day. Even in cases of invasion of the peritoneum the urgent signs may be delayed if the breakage has been very gradual, while sudden and great leakage is rapidly fatal, and accompanied by intolerable sufferings. Dr. Greves gives us statistical information which was not favourable to operative interference. Still we need not be discouraged by our past experience. What has hitherto been the treatment of cases of obstruction antecedent to operation?

What chance had the surgeon of success if the patient, previous to the operation, had been stimulated, kneaded, inverted, shaken, inflated, injected, effervesced, and galvanized. As in many instances a surgeon has been obliged to operate after all these feats had been performed upon the patient, no wonder that as yet supplemental aid has been attended by a high rate of mortality. Medical treatment of a character conservative of the patient's vitality makes delay an advantage not a hinderance, to surgery, inasmuch as the patient will be under less constitutional irritation, and the gut will contain less ingesta above the occlusion, these being items towards success after operative measures.

As regards a differential diagnosis, this I hold to be very uncertain of attainment. Every form of obstruction has been attended by the signs supposed to indicate a special form, while at other times a special form of obstruction, an intussuception for instance, has been present, yet none of the supposed characteristic signs were observed. I maintain that a differential diagnosis is not material to successful medical practice, but a correct diagnosis of the period for operating is very material, that we may save life when medicine fails, and operating only to find that interference was not required, which has been done on several occasions during the last three years, as shown by records which can be found in our periodicals."

Dr. Carter succeeded me, and after admitting the difficulty of diagnosing differentially, accepted the physiological method of treatment. This gentleman I know is in the possession of many clinical illustrations which would go to prove the correctness of my teaching, and he could have given one special illustration in proof that the signs which I have insisted

upon as indicating the advisability of direct interference by the knife, are true.

Dr. Caton followed, and remarked

"that so far as his experience extended, a majority of the cases of obstruction recovered if kept quietly in bed, on a limited diet, under the influence of belladonna and opium ; a minority die under this treatment."

That this speaker made no mention of the physiological method and the symptoms attending its application to these diseases, this quotation shows.

"the difficulty is to know when to rely on the expectant and when to have recourse to the operative method of treatment."

Dr. Davidson followed ; the tenor of whose address was that the value of surgery had been rather overrated ; he appeared to agree with most of that which Mr. Pughe had expressed, and he admitted the difficulty of diagnosing differentially. To the problems set by Dr. Greves he attempted no solution.

Now Dr. Glynn addressed the meeting and expressed this opinion, as regards the treatment of obstruction,

"Every student even ought to be familiar with them. He necessarily thoroughly concurred with the remarks made on the inappropriateness of the administration of purgatives and enemata, and on the importance of the withdrawal of almost all food, and on the value of opium."

In this address, the difficulty of diagnosing differentially was admittted, and operating for diagnostic purposes was objected to.

From Dr. Glynn's address I cannot venture to say whether he is now a follower of Sydenham, but we have it as a recorded

fact that in May or June of 1874,* he was not, yet Dr. Brinton had lived and taught before 1874.

Mr. Damer Harrison succeeded Dr. Glynn but utterly ignored the questions we had been invited to answer, and he also admitted the difficulty attending a differential diagnosis, † and questioned the correctness of that portion of my address which related to the time when direct interference might be required. But the premises he argued from were in no way related to the conditions I had in view when giving my opinion. Mr. D. Harrison's objection was grounded upon the "recognised treatment," consequently his objecton could not apply.

Medicine was next represented by Dr. Barr, whose address showed that he thought these cases belonged to the surgeon rather than to the physician. From Dr. Barr's speech it may be concluded that in mild or threatening cases of obstruction, he would treat the bowel as able, but unwilling to perform its function. In the so called acute condition of the bowel, he would in most cases, treat the intestine as helpless, and call in a surgeon, or treat by "rest, starvation, and opium."

From Dr. Barr's speech, it is very plain that he represents a school of practitioners who scorn the proposal to class as obstruction, those cases that are ushered in by, or attended with, only initial or mild symptoms. And should the total

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* See *Lancet*, 1874.

† I wish to specially draw the attention of my reader to these admissions, for I have been severely taken to task for insisting upon this difficulty.

signs appear, either from an increase of the disease, or by the "grafting" treatment, even then, if a *post-mortem* demonstrated that there existed no mechanical impediment, they would decline to class such a case as an obstruction. The holding of such theoretical views does not mend our treatment. Case 37 of my volume is an example of such; casually meeting the gentleman who performed the post-mortem in that case, I asked him whether he had found any obstruction present, he replied in the negative. From what I know of his views regarding this subject, he would not class functional incapacity as a cause of obstruction. This distinction has very banefully influenced the rate of mortality in these complaints, as functional obstruction is the acting cause in the majority of cases met with, and many of which die early. For instance, enteritis and peritonitis in contrast with the mechanical forms of obstruction, are very intolerant of the details of the recognised treament.

To Dr. Greves's query; at what time we are to call in surgeons to our aid, no definite response was given; and the answer, given to the medical question laid before the meeting, is so indefinite, that it is impossible to say what treatment Dr. Barr would advise us to adhere to. Regarding diagnosis, he advises that

> "the physician should arrive at as accurate a diagnosis as possible."

Surgery was now represented in the person of Mr. R. Parker. I here reproduce his remarks *in extenso*, as given in the Journal of the Institute. Sydenham's reputation would probably not

have been "resurrectioned" so early as 1875, if Mr. Parker had not, by his friendly coercion, influenced me to publish my views.

Mr. Rushton Parker considered the present discussion an advance on those of past years, and also thought that Dr. Greves and Mr. Pughe were to to be congratulated upon their case, which they had so skilfully managed throughout. Their treatment was justified by the result, though he agreed that good luck as well as good management was on their side. The discrepant opinions that resulted from the study of restricted varieties of cases, the differences of cause, and similarity in symptoms, alike pointed to the difficulties of diagnosis for differential purposes. The recognition of symptoms of obstruction was easy enough, but why was the separate indication of their course so difficult? No power had yet succeeded in making this generally possible, nor was it likely that any power ever would, owing to the very nature of the cases themselves. They might be classed under the following heads :—(1) The cases where symptoms of intestinal obstruction attended acute peritonitis, due to gradual escape of fæcal matter following perforation of intestine, frequently of the vermiform appendix. In these cases the patient died of septic poisoning, though the symptoms were those of obstructed intestine. Treatment was hopeless from the first, but we did not know that until afterwards, because the obstruction symptoms were similar to those arising from other causes, and might even be palliated by appropriate treatment ; but in vain, for all these symptoms were subsequently overshadowed by the fatal virulence of the poisonous absorption. (2) In cases of purely functional obstruction, a numerous class, the symptoms were found, as in other classes, presenting every degree of acuteness or chronicity, severity or mildness, but which, under the palliative treatment that we now all professs to approve, were capable of recovery in a majority of instances. (3) An intermediate class of internal strangulations, including intussusception, volvulus, band, and the like, not necessary fatal, yet largely so, though a small minority recover spontaneously, under varying and even conflicting treatment, or after abdominal section. Among these cases there were no certain methods of distinguishing the varieties at first, and under operative treatment the successes were not more numerous than without. Even to open the abdomen was to run the risk of meeting with one out of the class just referred to, and operating both unnecessarily and harmfully. The careful adoption of expectant and palliative treatment would here enable us to

save cases in which recovery was occasionally possible without sacrificing some in which that result would otherwise be attained with facility. He agreed with Dr. Davidson that there was no comparison between abdominal section undertaken for ovarian or other tumours and that practised for intestinal occlusion. While the advances in operative method, dressing, and after-treatment were shared by both sets of cases, the patients in obstruction cases did not seem equally capable of recovery, whether that was due to the previous state of the bowels or to some other cause. In the first class of cases to which he had referred, all treatment was futile, in the third a small minority might be assisted by palliative treatment to undergo spontaneous recovery, but the second consisted of cases of which a majority were capable of easy recovery if not improperly treated, and often if let alone. Upon these his hopes were based, and it was in the interest of these that he recommended the assiduous adoption of palliative measures uniformly in all cases, after the plan laid down by Sydenham and fully elaborated by Mr. Thomas.

To Mr. Paul, who followed Mr. Parker, is due the credit of organising this discussion, so that it became both instructive and useful. However, in his address, he ignored the actual problems before the meeting, by confining it to the question of statistics, but he expressed the opinion that a differential diagnosis was not so difficult.

Dr. Alexander addressed the meeting, and maintained the opinion that cases of obstruction occupied

"a debatable ground between the physician and surgeon,"

and that they should be relegated entirely to the surgeon. His views regarding the medical treatment were not expressed, but his opinion of the present "recognised treatment" was given.

"Death would occur in many healthy persons who had no obstruction of their abdominal contents when treated as the discriptions show they had been treated."

I have already published a case of healthy bowel fatally irritated, which is confirmatory of Dr. Alexander's opinions.

Dr. Rich succeeded, and commenced by admitting the difficulty of diagnosing differentially, but in the remainder of his discourse evaded the answers desired by Dr. Greves.

After this Dr. Rawdon resumed the debate. He also admitted the difficulty of diagnosing differentially, but ignored the points elected for consideration.

Now Dr. J. Wilson addressed us, and proved that he knew not Sydenham. He, too, ignored the several questions before the meeting.

From the remarks of the next speaker, Dr. Crawford, we learn that he was well acquainted with some of the details of the "main force treatment," however, he did not notice the several questions set forth for consideration. Dr. Archer, expressed his approval of the medical treatment applied by Dr. Greves in the clinical instance related to the members.

Mr. G. Hamilton terminated the debate, and, like many others, gave no heed to the points of both instruction and practice, which Dr. Greves specially requested the members to illustrate. Dr. Greves was allowed to comment upon the debate, which had lasted two evenings, but he did so under some disadvantage, having been refused an adjournment in order to prepare a review of the ground traversed.

Shortly after the conclusion of this debate, three gentlemen, who evidently had not a copy of "Clinical Notes" at hand,

wrote to the *Lancet* for information, to be found in that volume, and, as this correspondence, at first of little importance, became both interesting and instructive to all parties interested in the subject of obstruction, I have here reproduced it.

This correspondence, I am obliged to review, so as to define and defend the views which Sydenham and myself have held regarding the treatment of intestinal disease. From Sydenham's writings, I surmise that he would not object to co-operate in opposing the continuation of "the recognised treatment" of to-day, and further, I may show that some gentlemen have given judgment as to the value of the physiological method before they have understood it, or observed its progress at the bed-side.

To the Editor of THE LANCET.—*December 20th, 1884.*

SIR,—I was unable to take part in the above discussion at the Liverpool Medical Institution; but your faithful report of it has given me an opportunity of drawing attention to a point which I should not have allowed to pass unnoticed had I qualified myself to speak by sending notice.

Mr. Mitchell Banks, in adverting to the difficulties in connection with the question of operation, mentioned as one the fact of spontaneous recovery under opium and starvation treatment, "which" he said, "had for many years been the recognised practice." I need hardly say that the latter remark took me greatly by surprise. Although I am a young member of the profession, I can recall the fact that so far from this form of treatment being recognised in my student days the few who professed to appreciate the value of opium neutralised its benefits by giving milk, while others taught that calomel, enemata, inflation, &c., were the means and methods to which to look for success; and the words

of Dr. Waters, who immediately preceded Mr. Banks in the discussion, could not have reminded him more forcibly that the doctrine to which he alluded had yet to be received by his fellow professor. Nor was his the only opinion in the same direction. The opium treatment was even condemned by some, enemata, &c., advocated by others, and the number of those who adhered to the principles as laid down by the originator of this plan of treatment were very few. The explanation is evident. The opium and starvation treatment, properly so-called, is not even known or understood by those who have not made themselves acquainted with its originator through his work on the subject. The treatment of intestinal obstruction by rest, opium, and starvation was undoubtedly originated by Mr. H. O. Thomas, and we require no further proof of its non-recognition generally, even at the present day, than the fact that it is not even alluded to by Mr. Treves in his new and exhaustive work. Nor do I think Mr. Banks can refer us to any writings that will justify his statement, although I am quite prepared to apologise and retract if he will.

I enter this protest against Mr. Banks' attitude with all friendliness, feeling sure that upon reflection he will agree with me that his very able remarks were deprived of a finish which would have better adorned them had he but rendered honour to whom honour was due .

I am, Sir, yours faithfully

Liverpool, Dec. 8th, 1884, CHARLES E. STEELE.

To the Editor of THE LANCET.

SIR,—In your report of the recent discussion which took place at the Liverpool Medical Institution, on Intestinal Obstruction, I was very much astonished to read the following in Mr. Banks' speech: "The second difficulty consisted in the fact of spontaneous recovery under the opium, rest, and starvation treatment, which had now for many years been the recognised treatment." It would enlighten myself and others of your readers if Mr. Banks would favour us with his authority for this assertion. I have read the Jacksonian Prize Essay on this subject, and find no reference to the trinitarian treatment by opium, rest, and starvation, or, to speak more correctly, by opium and starvation, as the other two include rest to the bowel.

I am, Sir, yours faithfully,

Dec. 8th, 1824. A PROVINCIAL MAN.

To the Editor of THE LANCET.

SIR,—Last week's issue of your journal contains an account of a meeting held at the Liverpool Medical Institution, and the subject under discussion was, "The Treatment of Intestinal Obstruction." During the discussion Mr. Banks is reported to have stated that the recognised treatment of this ailment for many years was opium, rest, and starvation. Being interested in this subject, I am desirous of knowing upon what authority Mr. Banks claims antiquity for this mode of treatment?

I am, Sir, yours obediently,

ARTHUR HERBERT BUTCHER,

Hon. Surgeon to the Birkenhead Borough Hospital.

Birkenhead, Dec. 9th, 1884.

To the Editor of THE LANCET.

SIR,—Anyone who reads Mr. Steele's letter in your last number can only come to the conclusion that, when I said in the debate on obstruction of the bowels at the Liverpool Medical Institution that the treatment by "rest, starvation, and opium had for many years been the recognised treatment," I purposely overlooked the labours of Mr. H. O. Thomas in this direction, as set forth in his book published last year. I certainly had no intention. It has been my privilege, both in speech and in writing, to give my highest tribute of praise to Mr. Thomas for the admirable work he has done in another department of surgery, and therefore it is very unlikely that I would seek to deprive him of his just due in this. Mr. Thomas, however, in his speeeh at the Medical Institution arrogated no such claims for himself as are advanced by your correspondents, but modestly told how the real credit of the method of treatment under discussion was due to none other than Sydenham. Surely Mr. Thomas must pray to be saved from his friends. I must gladly acknowledge the valuable work done by him in pushing the doctrine of rest and opium, even although to some he may almost seem to have done so to an extreme point. He has drawn men's minds forcibly to the question, and has put it upon a better defined basis. He has shown how milk is a bad form of nutriment and has exposed the positive dangers of certain forms of mechanical treatment. For this he deserves, and has, our grateful thanks.

But if I am asked where I learnt the principles of rest and opium, I reply that I first knew of them from Laycock and Christison in 1863; although even before that I had heard Miller tell how Liston used to say that the bowels in cases of obstruction were to be opened, not with purgatives, but with opium and the lancet. In 1867 appeared Brinton's remarkable little book, and from that date I should think there is not a single teacher in Great Britain who has not taught on his lines. There has certainly not been one at the Liverpool School of Medicine, for I have asked every one of them what they have been in the habit of teaching. Looking over Dr. Cameron's notes for his lectures twenty years ago, when lecturing on obstruction, I find the following headings:—"Opium the remedy; constipation a necessary and curative symptom (principle of rest); purgatives to be avoided." Dr. Waters, in his remarks at the Institution, said that until acute symptoms had quite subsided, his treatment was "absolute rest, complete, or almost complete, abstinence from food, and the exhibition of opium and belladonna." In Aitken, Bartholow, Roberts, and Quain the same text will be found preached. In Ziemssen, Leichtenstern will be found following the footsteps of Brinton. Gentlemen who cannot see anything of the rest, starvation, and opium treatment in these works are determined not to see. Mr. Treves, in the very latest book on the subject, has been accused of knowing nothing about this treatment. I read in it: "It is worse than useless to attempt to feed these patients by the mouth. The patient may have ice to suck to relieve the sense of distressing thirst, but, apart from this, all food should if possible be administered by the rectum. There is certainly no one drug of more use and value in cases of intestinal obstruction than opium."

Personally I may say that during the eighteen years I have practised in Liverpool, I have seen as many cases of intestinal obstruction as falls to the lot of most men of my own age, and I never heard of any other treatment being employed, so soon as it was once discovered that the case was one of so-called obstruction, and not a mere ordinary attack of constipation. And here is where Mr. Steele quite fails to apprehend the matter. In cases of acute obstruction, with symptoms similar to those of a sharply strangulatied hernia, only one treatment can be and is employed, and none other—viz., rest, starvation, and opium, and, failing these, operation. But under the name chronic obstruction we are unfortunately obliged to class a variety of cases which differ infinitely in character from those which are produced by utterly irremediate causes to those which are termed merely "functional" cases. Now, every case of chronic obstruc-

tion begins just like any simple case of constipation, and, in the first instance, is to be treated as such. Things would come to a remarkable pass if the minute a man missed his daily motion for two or three times he was to be subjected to the treatment for serious obstruction when a dose of castor oil or a tumbler of warm water in the rectum was all he wanted. As regards the use of calomel, any physician of experience will tell Mr. Steele that minute doses of that drug are often most valuable in allaying distressing irritability of the stomach, while they cannot possibly add to the dangers of the obstruction. Concerning nutrient enemata in certain very protracted cases of obstruction, some sustenance, however slight, must be given to the patient, otherwise he will die of exhaustion before his cure can be effected. Experiment has incontestably proved that with the artificially digested aliments now in use life can be wonderfully supported by rectal feeding, while bowel irritation is reduced to a minimum. Concerning the use of inflation of the bowel in certain cases of intussusception one must remember that much that has been tried in that way has been in the form of experiment. And legitimate experiment too; for, after all, even rest and opium are mightily unsuccessful in really serious cases of obstruction, however much they may do for those cases which would have got well under any treatment, like the cases of diphtheria which some practitioners are always curing in vast numbers. And there is a limited number of cases where a low intussusception has been made out with certainty in children, where inflation undoubtedly has been most successful, a fact which cannot be controverted.

I rather regret that Mr. Steele, in his eagerness to champion Mr. Thomas, should have found it incumbent upon him to charge an old teacher and friend with lack of generosity to a fellow surgeon, but to that charge I have pleaded not guilty. I must, however, with much deference to the greater knowledge of Mr. Steele, adhere to my statement that for many years, rest, starvation, and opium have constituted the recognised treatment for intestinal obstruction by all intelligent and well-educated practitioners, and that this treatment was not for the first time heard of in 1883.

I am, Sir, your obedient servant,

Liverpool, Dec. 1884. W. MITCHELL BANKS.

To the Editor of THE LANCET.

SIR,—In the recent interesting debate at Liverpool upon the question of the treatment of intestinal obstruction, Mr. Mitchell Banks is reported to

have made the following statement:—"The second difficulty consists in the fact of spontaneous recovery under the opium, rest, and starvation treatment, which has now for many years been the recognised practice." (THE LANCET, Dec. 6th). I am somewhat surprised to find that no less than three of your correspondents take very decided exception to this statement. They dispute the assertion that the treatment named is a recognised mode of practice. Mr. Steele of Liverpool and "A Provincial Man" pay me the high compliment of observing that the measure is not generally recognised, because it is "not even alluded to" in my recent work upon intestinal obstruction. Mr. Steele, moreover, claims that this mode of treatment "was undoubtly originated by Mr. H. O. Thomas." In venturing to meet the objections that these gentlemen have raised, I might be allowed, in the first place, to endorse Mr. Banks' statement. With regard to the allusions to my work that have been so kindly made, I can only say that I have endeavoured in the book named to give especial prominence to this very measure. I have urged it—speaking especially of acute and subacute cases—not only as the primary and elementary basis of all treatment, but as a measure to be adopted by routine. In the chapter on treatment I deal first with "the feeding of the patient," or, as the paragraph would be as well headed, with "the starvation of the patient," and then proceed to the use of "opium" The question of rest falls under the consideration of these two factors in the treatment.

In detailing these measures I did not imagine that I was dealing with any "new thing," but rather that I was expressing as clearly as I was able the recognised measures adopted in the first treatment of intestinal occlusion. With regard to the claim made on behalf of Mr. Thomas, I regret that I did not see his able and original monograph until the M.S. of my book was entirely in type. The chief feature of Mr. Thomas's work appeared to me to consist in a very skilful and vigorous advocacy of the measure now in question—a measure the value of which I considered had been for years generally recognised. I cannot agree with Mr. Thomas in considering this particular treatment as final and all sufficient. It appears to me to be the best primary routine treatment, and the best introduction to such other and more active measures as the needs of particular cases may possibly demand.

I am, Sir, yours faithfully,

Gordon-square, W.C. FREDERICK TREVES,

Treatment of Intestinal Obstruction.

To the Editor of The Lancet.

Sir,—Mr. Banks has taken much unnecessary trouble in endeavouring to refute the charge implied in my letter, for, after all, his reply shows that he still looks upon Mr. Thomas as merely one of the many authors who have laid down the so-called "rest, opium, and starvation" treatment in intestinal obstruction. His letter, however, has entirely acquitted him of having intentionally overlooked Mr. Thomas's claim to the exposition of these principles properly so-called, for it is quite clear that he has not read that gentleman's work, and further, that he is quite ignorant of what the Sydenham method of treatment, as laid down by him, really involves. Mr. Banks writes of the rest, opium, and starvation treatment as taught by Laycock, Christison, Miller, Brinton, Cameron, Waters, Treves, and Banks, as if they were the same as that taught by Sydenham, utterly ignoring the fact, that is the wide difference between them upon which Mr. Thomas has been the first and the only one to lay such stress.

The grand distinction between the form of treatment by modern practitioners and that of Sydenham, as revived by Thomas, is that while in the former the principles of rest, opium, and starvation are applied in name only, in the latter they are a reality; in the former they are only introductory to, or supplementary to, other antagonistic agencies, while in the latter they are insisted upon from beginning to end, without qualification or modification. The authors quoted by Mr. Banks advocate plans of treatment entirely antagonistic to the Sydenham teaching. They do not believe in absolute rest. Mr. Banks himself, in support of Mr. Treves, advocates nutrient enemata. Sydenham, as quoted by Thomas, says: "A mere sugar-and-milk clyster may undo all that the paregoric has done." Mr. Banks, as a physiologist, surely knows that an enema, nutrient or otherwise, increases peristalsis not merely below, but above, the seat of obstruction, and unless he believes that increased peristalsis is consistent with rest, he is knowingly violating the very principle of rest—and why? Simply to avoid the fancied danger of starvation, for the fact that the Sydenham and Thomas method of feeding fairly maintains vitality is wonderfully borne out by Mr. Greves, who told us at the meeting of the Medical Society, that the child actually gained in weight. If Mr. Banks had ever appreciated Sydenham's views of starvation, he would not have written what he has about the necessity of giving sustenance to prevent

death from exhaustion. Sydenham clearly laid down that it was the pain and lesion itself which killed the patient, and not the want of food.

Enemata are not the only remedies which, though antagonistic to rest, are added to the Sydenham method in such a way as to defeat its objects. Belladonna is given with opium—its antidote. Calomel is given, and castor oil, and although I am much indebted to Mr. Banks for reminding me that the former is used as a remedy for irritability of the stomach, I fail to see what either that or the simple constipation to which he alludes has to do with the subject in hand. Even Mr. Treves, I repeat, nowhere alludes to the Sydenham method of treatment, nor to the quality of the diet as an all-important element, permitting the safe use of opium which he advocates. If, as Mr. Banks says, Mr. Thomas did not claim originalty, all I can say is that he has not claimed his due. I find on comparing their works that Sydenham taught that laudanum was essential, with control of diet; while Mr. Thomas teaches that the quality of the diet is all-important, and the sedative only supplementary.

Mr. Banks has stated his own views on the subject, and his faith in rest and opium is not too great to prevent him from saying that they "are mightily unsuccessful in really serious cases, however much they may do for those which would have got well under any treatment," which is tantamount to reducing the value of such treatment to a nonentity.

To sum the subject up, I contend that, until some authority has shown that before 1874 he has treated cases of obstruction according to the true Sydenham method *ab initio ad finem* without modification or addition, the credit of rejuvenating the true and original rest, opium, and starvation treatment is due to Mr. Thomas alone, and until the treatment he advocates has been faithfully tried by the profession it cannot be said to be the recognised treatment; and I challenge Mr. Banks, or anyone, to show the publication of any one case so treated from the time of the death of Sydenham to the first appearance of Mr. Thomas's work.

Let me say, in conclusion, that I am sorry Mr. Banks has taken such a personal view of the matter; I certainly never intended to hurt his feelings, although I fail to see with him that the fact of my being an old pupil of his should restrain me from entering a friendly protest on behalf of one whom, though not a fellow professor of Mr. Banks, I am proud to acknowledge as an old teacher.

Yours faithfully

Liverpool, Jan. 5th, 1885. CHARLES E. STEELE,

To the Editor of THE LANCET.

SIR,—Mr. Banks correctly informed you that at the discussion on Intestinal Obstruction I laid no claim to the discovery of the physiological method of treating such ailments. True as this may be, I had come to my present opinion, and carried it out in practice, many years before I was aware that Sydenham had in a very great measure anticipated me. It was only when the present Professor of Surgery induced me to publish my views that I found that Sydenham had anticipated me, and he alone. Between Sydenham and myself a void exists, and there are no records to fill up the gap.

I am, Sir, yours faithfully,

Nelson St., Liverpool, Jan. 3rd, 1885. H. O. THOMAS.

TREATMENT OF INTESTINAL OBSTRUCTION.

To the Editor of THE LANCET.

SIR,—If you will allow me a final word in reply to Mr. Steele's letter in your last number, I would merely observe that I consider it amply confirmatory of my original statement, that rest, opium, and starvation had for many years been the recognised treatment in cases of intestinal obstruction. He observes that I "still look upon Mr. Thomas as merely one of many authors who have laid down the so-called rest, opium, and starvation treatment." That is precisely my view of the matter, while at the same time giving Mr. Thomas every credit for pointing out certain improvements in diet, and for protesting against mechanical remedies when ignorantly and excessively employed. Mr. Thomas says that "between Sydenham and himself a void exists." This is not unlikely, but it fails to convince me that they are the only two persons who have ever known anything about intestinal obstruction. Mr. Steele describes himself as a young practitioner. With increase of years and experience I feel sure that he will discover that to rigidly apply one uniform and restricted method of treatment (to the exclusion of all others) to a disease under which are classed pathological conditions as widely asunder as the poles is neither logical nor in accordance with the principles which govern medical science in other instances.

I am, Sir, your obedient servant,

Liverpool, Jan. 13th, 1885. W. MITCHELL BANKS.

I shall show that the foregoing correspondence tends to prove that neither Mr. Banks nor Mr. Treves has ever witnessed the course of any form of obstruction while under treatment by the physiological method, and, I may add, that these letters make it very questionable if their authors even now understand the conditions essential for the uninterrupted progress of the physiological method.

During the past two thousand years, sedatives have been used for these complaints, and, from a very remote period, we have it on record that their use did not tend to save life. It has been the same with antiseptics, their use, without rule, rarely gained the result, which from their qualities we might have expected. As the Listerian method protects wounds from being impeded in their tendency to heal, so the details of the physiological method of treating intestinal diseases are merely items to minimise causes which may impede or thwart the inherent tendency of the parts affected to recover.

Mr. Banks' letters contain several assertions which demand attention. I shall try and give to each the consideration it merits, coming as it does from an eminent surgeon,

No. 1.—That Christison, Laycock, Miller, and Liston taught an opium method.

This is probably correct, and others before them have so taught, and that the majority of physicians do so yet. To directly prove that, Christison, Laycock, Miller and Liston

never taught the method which I have laid down as fititng for intestinal disease, is impossible, as none of these gentlemen has left any record of his teaching on this question. But the difficulty can be almost totally surmounted by taking the evidence of their colleagues and contemporaries, and also that of their pupils. Professor J. H. Bennett in his manual on the Principles and Practice of Medicine, 4th edition, page 534, gives the details of a case of obstruction of the bowels, in which castor oil with croton oil was used. At page 535 another case is given, which was treated with opium and enemata. At page 535 we are informed that to find whether a case is that of obstruction or not, we ought to use a "full purgative," "because without it, no one can determine whether or not there is an obstruction at all," and he tells us if an obstruction be found, to inject "oil, air, or other fluid." This is the recorded advice of this eminent teacher in 1865, two years after the publication of Dr. Brinton's volume, and six years after his lectures were delivered.

We will now consider the recorded practice of another University teacher, Sir J. Simpson. In vol. XVII of the Edinburgh Medical Journal, page 971, is reported a case of constipation with "chronic symptoms." Sir J. Simpson, in conjunction with others attended the case, their treatment being castor oil and opium, croton oil and morphia, enemata and the use of that instrument of torture known as O'Beirne's tube. This was Sir. J. Simpson's practice in the year 1872, thirteen years after Brinton had first formulated his opinions

regarding treatment. If the professor had any knowledge of the physiological method he certainly did not apply it in this very suitable instance, but there was one person interested, who sketched to those in attendance a physiological method —this person was the patient, but his remarks failed to influence his physicians. *

It would be rather remarkable that Christison, Laycock, Miller, and Liston should have been the possessors of principles, just the reverse of the teaching and practice of their colleagues, Professor Bennett and Sir J. Simpson, concerning a disease attended with a very high rate of mortality, and withal, have never recorded their dissent, although they were contributors to the literature of both medicine and surgery. I had the advantage of listening to their teaching, and recollection does not contradict my present assertions.

The periodical known as the *Edinburgh Medical Journal*, which best represents medical opinion beyond the Tweed, contains no evidence that any of the Scotch University teachers taught the physiological method of treating intestinal disease. In volume XXVIII (1883), page 53, a distinguished Edinburgh surgeon, and one who must have been a pupil of Christison, Laycock, and Miller, reports a case of intestinal obstruction, which was treated before operation by enemata and galvanism.

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* The case of the late M. Gambetta had several points in common with this, but while this patient merely gave his feelings and opinion of treatment, M. Gambetta was so self-willed that he did not submit to "recognised treatment," and thus escaped dying from obstruction, to die from erysipelas.

No.2—The foregoing correspondence asserts that Brinton taught the details which I have insisted upon. If this were true, then the question presents itself, "What was the cause of Brinton's failure to influence the treatment of his contemporaries and successors?" My answer to this question is; there is no record extant that Brinton possessed any definite rule of medical treatment applicable to all intestinal diseases, and to acute obstruction in particular. Brinton, in 1863, was still, as regards the principle of the treatment of these affections, behind Sydenham of 1666; much that the latter surmised regarding the etiology of the subject, Brinton demonstrated, but his teaching regarding treatment was not decided enough, and at times inconsistent. In page 90 of his volume he contends that a differential diagnosis is essential to successful treatment whether a case be medical or surgical, but gives no rule as to when surgical practice should supplement the medical. At pages 104-5, hydrostatic dilatation is referred to as an aid to resolution, whereas this cannot act until the part has become healthy. The primary mechanical action, that which relieves urgent obstruction, is the reduction of the consistency of the gut contents which enables them to pass downwards, and eases the part suffering from irritation caused by the weight of the accumulation. It is only after the injurious accumulation has been got rid of and the part is well, that suitable bowel contents can begin both to dilate and restore the contracted part to normal calibre. He was not aware of the important fact that primary relief should not be taken as an

indication that the patient could now certainly recover. Two or three or more "reliefs" must be observed before the practitioner can confidently prognose that the patient is freed from all danger—the primary relief being only the first step from danger.

Brinton also used enemata to diagnose the locality of the lesion; see pages 81-82. He had not grasped the dangers attendant upon the use of sedatives, so as to give his readers warning of the details of treatment necessary for their safe and useful administration. His remarks regarding the use of enemata are inconsistent with his views of both the etiology of the disease and of the principle of treatment; and the latter portion of page 115 is inconsistent with the advice to use enemata at all. At page 119, his discussion of the treatment by inflation shows much indecision of opinion, and again, in the foot-note, page 120, there is further evidence of the want of a definitely formed opinion as regards treatment.

I here reproduce Dr. Brinton's summary of treatment, which places him "out of court" as regards his being acquainted with the Sydenham or physiological method of conducting intestinal lesions.

"In intussusception of the large intestine, repeated injections of liquid into the rectum, so as to distend the bowel to its utmost dimensions. In stricture of the large intestine, the institution of an artifical anus above the obstacle. In obstruction from bands, diverticula, etc., mostly affecting the the small intestine, gastrotomy, and division of the cord-like cause of strangulation; a procedure which, if interrupted by unforeseen impediments, may further require the institution of an artificial anus in the most distended part.

In obstruction by stricture, however, a tobacco enema should be administered at least once ; a measure which should be repeated, if need be, in obstruction by bands, and especially by gall-stones.

In all cases, opium, and support, to be freely administered from the earliest stage of the malady, the bulkier liquid constituent of the food to be given as sparingly as possible by the mouth, but administered freely per anum.

Distensive enemata to precede all operations, if only as a means of aiding or assuring diagnosis.

When vomiting is excessive, nourishment to be also injected into the rectum in small and frequent doses.

After recovery, all food which can introduce indigestible substances into the intestine should be carefully avoided ; the bowel having sometimes undergone changes of calibre and arrangement, such as permit substances, easily transmissible through the healthy canal, to cause fatal obstruction."

To maintain that the foregoing method of treating intestinal diseases is identical with the teaching either of Sydenham or myself, would be as reasonable as to maintain that because a person conducted his business honestly five days only of the week he was entitled to the character of a moral man.

If Dr. Brinton had only published the first and second lectures and omitted that on treatment, his services would have been an undiluted advantage to the science of medicine, and our practice to-day would have been much reformed. The contents of the lecture on treatment is too much leavened with "recognised treatment for many years," and thus the great merit of his etiological teaching has gone to perpetuate the defects of his treatment, which the reader can judge of from the summary of principles appended, taken from page

122 of his volume.* Brinton failed to interpretate correctly many important facts which he was instrumental in making known to us.

Brinton's teachings are the lines, Mr. Banks says, that his contemporaries in medicine and surgery follow, when treating intestinal disease. With this I quite agree, but they are not the lines which Sydenham laid down, nor are they favourable to an unimpeded progress towards resolution of the disease. Mr. Banks has, as evidence to disqualify me for the distinction I am contesting for, referred to the writings of Aitken, Bartholow, Roberts, and Quain.† But in reading page 856, of Dr. Aitken's volume, published in 1868, I find he

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* I have purposely ignored Liechtenstern, as I fail to see in him anything more than a second edition of Brinton's labours, with no new thing that was useful.

† I have always thought that when a teacher of medicine has formulated his mode of treating typhoid fever, it becomes fair evidence of the principles that would guide him in the treatment of other lesions affecting the intestines and their surroundings. Bartholow, in his chapter on Typhoid, and his remarks last year in the *Boston Medical and Surgical Journal*, on the treatment of chronic enteritis, informs us that he runs close up to the Bath Lecture on Typhoid, by Sir W. Jenner. Bartholow, I find, attaches much value to the use of carbolic acid in typhoid. From what we know of the physiological effects of this acid, when taken internally, it may probably beneficially act. This fever is one accompanied by signs of hyper-action. As medicines, the carbolates are known to be inhibitors-sedatives, but not hyponotic. Carbolic acid and its compounds have been much employed of late years in contagious and infectious diseases, and their actions have very erroneously been attributed to their antiseptic properties, whereas it is by their physiological action only they are effective when given internally. No subject can live, and, at the same time, be as a whole or in great part pickled. The safe practice of Lister's antiseptic method involves the pickling of a fractional part of the body, and limitation of this action to the locality; if this area is overstepped, poisoning results, and we know death has followed. Listerism is chemical action only. No person would contend that a bread poultice, applied outwardly, would feed the patient. It has been supposed that inunction by fats can feed--even then they only economise heat; but if taken internally, they can develop heat, two very different effects.

advises the use of opium and belladonna for intestinal obstruction; also enemata and inflation—chloroform to be adminstrated during the use of the last means. On consulting Bartholow, in his volume on the Practice of Medicine, published 1880, page 111, I find he advises opium, and lays down the same rules as myself as to the frequency of the dose and the physiclogical effect to be gained, but as I know that my contribution to the subject of intestinal disease had been placed near him, eight years ago, his rules relating to the physiological action of opium, read very like a compilation from my writings, At page 112, as a remedy for intussusception, he advises a mechanical impossibility while the patient is alive; that is, to distend the intestine by hydrostatic pressure or disengaged gas, thus taking it for granted that half, at least, of the intestines can be distended with liquid or gas while confined within the limits of the abdominal cavity, and that despite the contra action of peristalsis. Bartholow gives us the "recognised treatment" nothing better; one of the internal remedies he recommends is carbolic acid. In his treatment of typhlitis, he attaches most value to Epsom salts; in enteritis, opium, camphor, castor oil and turpentine.* An examination of

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* I think I ought to confess that I am incapable of discerning the propriety of using enemata medicated with APERIENTS, or any irritants. If the large intestine does contain excretion or remains, warm water will reach and bring it away if the gut be healthy. If the aperient is expected to act upon the small intestine, why not give an aperient by the mouth? Why punish? nay, induce, perhaps, disease in the large intestine? A fatal example of this I have already recorded. For giving compound APERIENT enemata, there can be given no reason. The practice is a remnant of the days of our ignorance.

the various editions of Roberts's Manual of Practical Medicine is still only further proof of what the recognised treatment really is, and that it is not the physiological method. For chronic obstruction he advises a selected dietary and enemata and the avoidance of strong purgatives. For acute obstruction he instructs us not to give powerful purgatives.

"It is allowable to use enemata cautiously, so as to clear out the bowel below the seat of obstruction."

I suppose this is "allowable," because there is seldom anything to be brought away? He advises limitation of food, and that stimulants and food must be given per rectum—

"And frequently and in considerable quantities if the latter are needed."

The medicines he advises are opium, belladonna, with the external application of dry heat, poultices, fomentations, turpentine stupes or sinapisms, as "very serviceable," and intussusception is to be treated by hydrostatic enemata.

Dr. Roberts's teaching represents what I have maintained is the recognised treatment, and which Dr. Alexander denounced at the meeting, the proceedings of which led me into this controversy. How Mr. Banks ventured to refer to Quain as evidence that my teaching was not new, I cannot understand. The article on instestinal obstruction in Quain's Dictionary of Medicine is a condensed manual of the prominent details attached to recognised "main force" treatment.

No. 3.—Mr. Banks quoted Mr. Treves—

"It is worse than useless to attempt to feed these patients by mouth . . . . all food should be given by the rectum."

Sydenham would have taught thus—

"It is worse than useless to feed per rectum, as feeding that way will injure the patient, so suitable aliments must be given by the mouth. By my method of treatment, it may be retained sufficient to gain some alimentation."

This is also my contention. For when the patient is fed by the mouth food is welcomed, but if given by the rectum, it is an unwelcomed visitor, and excites peristalsis. By the physiological method, aliments are as long, or longer retained by the stomach than by the rectum; they may be retained from six to forty-eight hours. No doubt by long habituation, the rectum might be educated to take more kindly to feeding, but unfortunately, attempts are made to educate it during a period when it is in the worst of humours to be taught, and time is just then too valuable to be lost in such training.

No. 4.—Mr. Banks says he has never heard of any other treatment during the last eighteen years. No doubt. But he refers to the treatment as set forth by Brinton. This is very possible, for my writings have had a very limited circulation, the majority of my reviewers condemning the manner rather than the knowledge contained. This I must accept as an explanation, why, even in my own locality, the physiological method is not generally known, but it does not explain how for two hundred years no physician or surgeon divined Sydenham's principles of treatment. Mr. Banks graphically records the defect of "recognised treatment" (Brintonian), "For, after all, even rest and opium are mightily unsuccessful in really serious cases of obstruction."

No. 5.—Mr. Banks says that the method I have advocated "was not for the first time heard of in 1883." He is quite correct, it was first taught in 1666, and for the SECOND time in 1876.

No. 6.—Dr. Steele, in his correspondence, if he intended, failed to convince both Mr. Banks or myself that Sydenham and myself "are the only two persons who have ever known anything about intestinal obstruction." If he wished to do so, I think he attempted too much, as we have only contended for being possessors of very necessary knowledge, not a monopoly of all knowledge. I have purposely omitted to notice Mr. Banks's remarks relating to the treatment of ordinary constipation; that has never been discussed by me, but I have discussed a constipation which is not a rare one, but is often treated so as to become an uncommon one.

No. 7.—Mr. Banks concludes his able defence of the "recognised treatment" from being mutilated, with the following remark, that—

> "With increase of years and experience, I feel sure he (Dr. Steele) will discover that to rigidly apply one uniform and restricted method of treatment to the exclusion of all others, to a disease under which are classed pathological conditions as widely apart as the poles, is neither logical nor in accordance with the principle which guides medical science in other instances."

The "poles asunder argument" will not stand examination. Suppose, for the purpose of argument, we enumerate here several states which anatomically and pathologically are as widely asunder as the poles, enteritis, typhlitis, peritonitis,

lead colic, cholera colic, typhoid colic, intussusception with its colic. In these various modes of obstruction with their colic signs, the cause and pathological conditions of which are as widely asunder as the poles, Would Mr. Banks in any one of these classes of disease treat it upon separate *principles*, and direct that means with opposite qualities should be employed to influence the diseased locality? The diet is the local application, the medicine is the indirect means of favouring resolution, and the surgery is the direct method of influencing the difficulty. For which of these cases would it be proper to prescribe a diet of pork chops? For which a course of purgatives? Which of these never want the aid of surgery? In fact, they all want to be treated by the same principle. Where is the surgeon, who, when treating a compound fractured ankle, would advise the exercise of the joint? or if the ankle had been suffering from inflammation, gouty in its origin? Is exercise advisable in one of these instances, because the pathology is unlike? In fact, separate regions, even when suffering from diseases arising from wholly different causes, are treated so far as our knowledge goes at present, and ought to be treated by like means. The "poles asunder argument" is not yet favoured by either medicine or surgery, as we have not yet come to a remedy for every disease. Indeed, the longer the experience of and the older the practitioner, the fewer we find his remedies are fewer in number, and that he with confidence employs them to treat very many conditions in no way allied in their physiology or pathology.

No. 8.—The correspondence between Dr. Steele and Mr. Banks, and the remarks of Dr. Barr, at the discussion of this subject, show that there is a variety of opinions as to what really constitutes obstruction, such as to require the full rule of treatment. Mr. Banks very properly declines to include ordinary constipation. We have argued that ordinary constipation is not a sufficient deviation from health to require the advice of an expert to correct; but to Mr. Banks's teaching, that "chronic obstruction begins like any simple case of constipation, and in the first instance, is to be treated as such," I demur. It is this very treatment which leads to the "grafting" of the acute condition on to the chronic. No one, having a correct knowledge of the etiology and mode of resolution in these diseases, would agree with the opinion that chronic obstruction should be treated at first by the means commonly employed to relieve simple constipation. As such teaching is subversive of the fundamental principle applicable to the treatment of all intestinal difficulties, no wonder that Mr. Banks has found, during eighteen years practice, "that even rest and opium are mightily unsuccessful in really serious cases of obstruction, however they may do for those cases which would have got well under any treatment." Is it probable that a practitioner who at random treats a case of chronic obstruction, would, if called to treat an acute obstruction, be very successful? Is a surgeon, who unskilfully treats an incised wound, likely to successfully manage an amputation?

As showing what the random recognised treatment by diet

and opium really is, and further, the necessity of treating even the mildest forms of obstruction by the utmost art, I shall give here a summary of a case of intestinal obstruction reported in the St. Bartholomew's Hospital Reports so late as 1884-5, page 189. The case is reported with prefatory remarks showing ignorance of the etiology of the disease, the cause of symptoms and the course of resolution, and the treatment in use was the random "recognised" one.

The patient, a coach painter by trade, was admitted into the hospital on March 25th, then suffering from general ill-health; he commenced to suffer from rheumatism fourteen days after admission. "Occasionally he passed clotted blood in his motions." April 27th, the patient began to vomit matter with fæcal odour; up to this date he was not as yet constipated. May 1st, vomitted several ounces of dark brown fluid with fæcal smell, *pulse slow and regular*, *temperature below normal*, no tenderness of the abdomen, no action of bowels since April 30th. *Large quantities of water and olive oil were injected into the rectum, but they merely brought away two small coloured motions.* May 2nd, pulse fell to 40 and the temperature fell below normal, *the patient was fed by nutrinent injections*, but died May 3rd. The vomit ceased during the last eighteen hours of his life. A *post mortem* revealed just what the patient's retrogression indicated; small intestines distended by liquid matter, empty colon.

The reporter of this case makes the significant remark—significant as showing that even the acting cause of both the symptoms and unsuccessful treatment is not recognised, yet this case was treated in 1884, twenty-one years after Brinton had related all he knew relating to these diseases. The remark I allude to is the following:—

"The interest in this case lies, of course, in the fact that although the repeated attacks of fæcal vomiting, no mechanical obstruction of the intestine was found at the autopsy."

With the reporter of this case I agree, "that much of the interest of the case" lay in the fact that he could find no mechanical obstruction. But had he taken into consideration the following facts, the immediate cause of death, mechanical obstruction would have been obvious. First, that the gut examined at the *post mortem* was physiologically no longer living matter. Second, that while the patient was alive a portion of this gut had its muscular action held in abeyance by disease, while yet retaining vitality, and thus it was reduced to a condition of being a mere unvarying tube. Third, that this unvarying action of the circular fibres to propel and the longitudinal ones to make straight the way in front of matters propelled, was necessary to avoid obstruction to the passage of gut contents. Fourth, that the suspension by any cause of the action of these muscles, the longitudinal ones in particular, constituted a mechanical obstruction so long as the patient was alive, and so the intestine ought not be treated as inert material. The replications of the intestine, if not varied by muscular action to accommodate the matter descending the bowel, they become a form of mechanical obstruction.

The report of this case gives us an example of that to which I have so often objected, that a too mechanical view of the principle of treatment is generally taught; consequently, the patient is treated as though he was merely a composite of organic matter. The report of this case gave me the impression that it was one of curable mechanical obstruction with the characteristic signs; though a mild case, it ended fatally, and

it would be specially interesting to know, why it did so terminate? A positive answer certainly is impossible. But the following questions may lead to the probable indirect cause of death. Why were "large quantities of water and olive oil injected into the rectum of a patient who had passed clotted blood in his motions? Again, when the temperature fell after this treatment, showing that it might have caused the advancing collapse, why was the same experiment repeated in the form of nutrient injection? Again, why was nutrient injection given when vomiting was not incessant? These questions point to, at least, an accelerating cause of death. This case was treated in a metropolitan hospital, the medical staff of which must certainly have known what were the "lines of treatment" Brinton laid down. It certainly was not treated by the Sydenham method nor by any rational one.

It will assist my argument if I here admit the value of a long personal experience. Dr. Steele cannot yet have had an experience sufficient to include a period of eighteen years, and if he had such an "increase of years;" many years experience, in wrong practice is worse than no experience, as every day adds to our prejudices, and we become less able to be influenced by either reason or evidence. Dr. Steele, however, has in no part of this correspondence laid claim to be the introducer of this method of treatment, he has only pointed out that a certain person, with at least twenty-eight years of experience, has "elaborated" a method of treating intestinal

disease which has not been generally recognised. Dr. Steele, some years ago, adopted as a rule of practice this method, the labour of another, just as we all do in adopting medicine or surgery as a profession. The "young man" argument would have applied to Dr. Steele if he had been the original teacher of the knowledge in question.

Mr. Banks, in his last letter says :—

"In 1867 appeared Brinton's remarkable little book, and from that date I should think there is not a single teacher in Great Britain who has not taught on his lines. There has certainly not been one at the Liverpool School of Medicine, for I have asked every one of them what they have been in the habit of teaching. Looking over Dr. Cameron's notes for his lectures twenty years ago, when lecturing on obstruction, I find the following heading, 'opium the remedy: constipation a necessary and curative symptom (principle of rest), purgatives to be avoided.'"

With all asserted in the preceding quotation I agree. But the Brintonian treatment is not identical with either the Sydenham method of treating intestinal disease, nor the more complete physiological method. In experience, the last method is neither known nor practised beyond the range of my own influence. I hold that this quotation is a challenge to prove that the clinic of the Liverpool University School of Medicine did not possess the knowledge I have offered, nor practice according to that information. This is to me a very unpalatable task, and I fear I cannot perform it without being branded as a confirmed egotist; yet, if I declined this task, I should very properly be suspected to be a mere seeker after notoriety by a puff oblique. In relating the several practical examples of the mode of treating

intestinal disease by the clinic of the University, I shall attach to each such particulars that they can be identified. None of the instances of practice related shall refer to Dr. Waters, as he has so distinctly recorded his practice—Brintonism—but as Mr. Banks has specially alluded to my respected friend and fellow-citizen, Dr. Cameron, some of the instances I shall have to relate include examples of his practice.

By comparison of practice it will be shown what the recognised treatment cannot do and that which the physlological treatment can. I shall refer to examples of practice by members of the clinic of our local medical school, their pupils and others, and who have undergone sufficient training to enable them to be accepted as "intelligent and well educated practitioners."

During an interview some time last year, with my friend, Dr. Hodgson, of Bootle, he informed me that, while making a professional visit, he had been obliged to listen to a female who charged me with having maltreated her husband. The particulars given me revived my memory. The history of this lady's grievence is this :—

Trusting to my memory, I recollect that about fourteen years ago, this lady's husband was under my medical treatment for a disease which I had diagnosed as malignant disease of or connected with the intestines. At an early period of my charge of him, my prognosis was unfavourable to his recovery, and very naturally his friends were anxious that a second opinion should be had, and selected a personal friend of mine to co-operate with me in the treatment. He advised a compound remedy, the principal ingredient of which was turpentine, but as this prescription much aggravated his distressing symptoms, my colleague was set aside, and a member of the school of medicine was selected to succeed him. We had a consultation, and he strongly advised put. jalapæ co. in

dram doses ; to the employment of this remedy I demurred, but not firmly, partly because he was my senior in experience and partly because his reputation was deservedly great. However, by respectfully and cautiously giving my opinion, we agreed to a compromise, and jointly prescribed pulv. jalapæ co. half dram ; but as this dose much aggravated the patient's symptoms and caused hæmorrhage from the bowel, we were both discharged and a hœmeopathic practitioner was called in to take charge of the case.

Last year I learned that to me was given the credit of the evils that resulted from the two consultations. Now, if the teacher who assisted me then knew of a mode of temporarily relieving the patient, without aggravating the disease, why did he not advise it? At that time, I had not sufficient confidence in my own plan of treatment to firmly oppose him.

Some few years ago

A baker and flour merchant of this city, whilst in the act of descending from an omnibus, fell and injured his spine, this being the only lesion detected by me soon after the accident. But after the elapse of a few days very evident signs of traumatic enteritis appeared, and the friends of the patient became alarmed, and requested me to accept the co-operation of the second consultant of the preceding case. The omnibus company in the meanwhile had also selected this gentleman as their representative. We met, and during consultation he advised that the patient should have immediately administered to him one of the vile concoctions known as enemata, included in the British Pharmacopia. To this proposition I firmly demurred, and began to give my grounds for dissention, when to my surprise the consultant showed evident signs of impatience and displeasure, and while in this state of mind began to enumerate the sad results that would accrue if at once this enema was not given—"increase of years," "long experience," were more than I could hope to undo in ten minutes, so I ceased my discussion. The physiological method of treatment, however, was rigidly adhered to, and as the case was by no means a severe one, the patient soon recovered.

The next case came under my observation about three years ago.

The patient, aged 65, by trade a ship carpenter, had been the inmate of one of our public hospitals, and under the care of the physician referred to in the two preceding cases. He was discharged incurable. On my examination of him, I also came to the same prognosis; he suffered from the full signs of chronic obstruction, plus an easily diagnosed, small tumour, probably malignant, in the epigastric region. To relieve his symptoms and prolong life to the utmost, the patient was treated by the physiological method. The patient, however, instead of retrogressing was well in six months, and has remained so until this date and has long ago ceased to be treated medically.

The physician, here referred to, preceded me in the management of the case 25, page 228, of my volume on Intestinal Disease — the patient being a medical practitioner. My visit to the patient was made only after repeated invitations, as my seniors were already in attendance, yet, after the patient had experienced the effect of the physiological method, he would not release me from the duty of conducting the treatment.

These four cases are sufficient to show that one of the teachers, Mr. Banks refers to, does not practice the method which Sydenham and myself have not attempted to monopolise.

At page 248, of my volume on Intestinal Disease, case 30, there is recorded a case of intestinal obstruction. The patient was under the care of a "well educated" practitioner, who was assisted in the management of the case by another teacher in the Liverpool School of Medicine. That case is a crucial test of the truth of my contention. The recognised treatment failed (painfully), while the physiological treatment, carried out by the patient's friends, succeeded (pleasantly), all doctors

being dismissed. The obstruction in this case appeared to have been brought about by the injudicious use of a strong purgative by the patient, and the treatment by medicated enemata so aggravated it that it became a case of acute obstruction, endangering the patient's life.

I shall now give an example of practice by another teacher in the Liverpool School of Medicine, and show that the method of treatment which I practice was not known to him in 1874 :—

The obstruction in this case, like the preceding one, was caused by the administration by a friend to the patient, aged about 12 years, of a ounce dose of Epsom salts, to relieve constipation, the child being, up to the administration of the remedy, in perfect health. This patient's primary treatment was conducted by a locum tenens, whom the member of the School of Medicine had left in charge of his practice whilst from home. The locum tenens had only lately qualified. The signs of obstruction had existed only a few days, when the guardian of the patient invited me to assist the gentleman in charge of the case. I suggest certain modifications, mutilating the "recognised treatment" which the inexperienced practitioner had been taught ; these he readily adopted, and for a while the case improved ; but shortly after the principal returned home, took charge of the case, and insisted upon rectal feeding, which caused me to retire from the case, whilst declining to make known to the patient's friends my reasons for so doing. The case did not terminate satisfactorily.*

If the reader will consult page 226, of my volume on Intestinal Disease, he will find there reported another example of what the recognised method cannot do, and what the method I have taught can do.

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* In that instance the patient's friends were displeased because I would not give a separate opinion, and the gentleman I left in charge suspected me of doing so. This lead to a temporary unpleasantness; but I saw no better way of conduct than that which I adopted, as it was evident "recognised treatment" would make the case very critical. The professor alluded to in this case has since materially altered his views, but he was "well educated" before I knew him.

The patient in that instance had been an inmate of a large public hospital, and during the time he was there, was under treatment by a teacher in the Liverpool School of Medicine, and if the patient correctly informed me, he was examined by several of the staff before being dismissed, without any improvement of the abdominal ailment.

My first introduction to Dr. Steele was brought about by my being called to co-operate with him to relieve a case of intestinal obstruction arising from an injury :—

Some five years ago I was sent for to visit a sea captain (Spaniard) who had fallen into a steamship "hold." However, on my arrival, I found that Dr. Steele, who was the ordinary family medical attendant, had arrived before me, and I retired. Subsequently, I was again sent for and requested to co-operate with Dr. Steele in the treatment of the patient. On examination, I found that the patient was suffering from such an amount of injury to the spine that there was total paralysis of the lower extremities and of the bladder, with obstinate constipation, necessitating the use of purgatives and enemata. These had been employed until they ceased to be effective; the physical aspect of the abdomen indicated the existence of traumatic enteritis. I advised Dr. Steele to adopt a method, for the employment of which reasons and facts were given. He accepted my advice; and if success is any proof of truth—it is not always—then the result proved the propriety of my advice.

In this last instance, we have a late student of the Liverpool Medical School in charge of the case. If my teaching is not new, why was the physiological method not tried by him before I was introduced to the case to assist? Some of my readers, adopting the views of my opponents, might reply, Because it is not effective. My answer would be, How do you know it is not effective? As a general rule, outside of my own influence, the method has never been tried; there are plenty of registered records of recognised treatment—*not one* of the method my opponents so readily condemn, even to show its failure.

Surely if the torture by inflation is a "legitimate experiment," the treatment by a little more patience and much less pain may have some virtue in it, and deserves a trial. What recommendation is the physiological method short of? It is the result of thirty years matured thought, and of many years testing by myself and medical friends. I have now given practical examples of how several teachers in the Liverpool School of Medicine apply their principles to the treatment of intestinal obstruction. If other members of the staff attached to our Medical School have practised the method I have advocated, Why did they not give us examples of its failure if it has been "mightily unsuccessful in really serious cases"? Why was Dr. Hyla Greves congratulated by the members of our Institute? Where were the physicians and surgeons with their disastrous experiences to warn Dr. Greves from the risk he incurred by his adoption of Sydenham's hobby?

It may be said the proceeding examples and remarks are not proofs that some of the Medical School physicians and surgeons, with the practice of whom I am not personally acquainted, have not practiced the method in question. However, as I am surrounded by practitioners educated in the Liverpool School of Medicine (as excellent an institution for the purpose as any in the United Kingdom), who have been tutored by the *whole* staff, I ask, Where is there one who has practised the physiological method of treating intestinal disease on account of the principles or practice he had been taught during the period of his studies at the School of Medicine?

Case No. 31, page 251, of my volume on Intestinal Diseases, was under the care of a late pupil in our local medical school and shows the method he has been taught. Dr. Alexander's remarks at the Institution is sufficient comment on that case. But I shall give here an example of a practitioner, learned in our local school, whom I have influenced. He is frequently referred to in my previous volume in connection with cases of obstruction :—

> During last year, whilst he was attending a case of intestinal obstruction, the friends of the patient insisted upon his accepting the assistance of a physician attached to an hospital, in which the abdominal region (Gynechology) was a speciality. At a stage of the treatment the consultant urged the employment of abdominal section. My follower, however, insisted that the signs present did not indicate that surgery was required. In seven days from that time the patient was walking out of doors without surgical interference.

This is another link to the chain of evidence that my teaching is not "mightily unsuccessful in really serious cases."

The hospital surgeon who consulted me regarding case 32, page 253, of my treatise, was a pupil of the local school None of the staff had initiated him into the Sydenham method of getting over the difficulty that presented in the instance related. I could here increase the number of instances of practitioners, educated at the Liverpool School of Medicine, who passed through the whole curriculum of teaching, without being taught what we now are informed is common knowledge. They are too "well educated" and are too many to permit us to suppose that either obtuseness or accident may account for this want.

My professional neighbours are not limited to members of the staff attached to the local medical school, nor to its late pupils. There are those who come under the class "intelligent and well educated," that have been taught at other recognised medical schools.

As an instance of what can be done by rational treatment to prolong life of even hopeless cases, the following is an example. About two years ago I was requested to meet Dr. McDonnell, of Widnes, and we examined a case suffering from some abnormal condition of the abdominal contents :—

We found a large tumour situated over the position of the cæcum, and judged it malignant. There was partial obstruction to the emptying of the bowel ; the bowel contents could be at times distinctly felt passing as though over the front aspect of the tumour ; there was much abdominal distension, high temperature and accelerated pulse. During our consultation we agreed that the tumour was malignant, that the case was hopeless, and that it would soon end fatally. I advised that the physiological method of treatment should be strictly adhered to, as it would save the patient much suffering, the doctor would not have to visit so often, the medicine required could be entrusted to his nurse, and life would be prolonged. To this formula Dr. McDonnell agreed, and we now gave to the wife of the patient our prognosis—soon fatal, no hope—but encouraged her to try and prolong his life, etc. Two months after this interview, two seafaring men called upon me and asked if I would go to a small port in Wales, and inspect a fractured femur. I asked them if they had come from Wales that day? They replied, No ; we have come from Widnes. They asked me if I recollected meeting them at the house of Captain ——, a patient of Dr. McDonnell. I replied in the affirmative, and added, When did he die? The answer was, "He is now going about and has been out of bed some time. Directly you had placed your hand on him our friend begun to recover. As for Dr. McDonnell, he did nothing after you left." I protested that no manipulation beyond gentle examination by palpitation was used by myself, and that Dr. McDonnell had since my visit applied a treatment we had both agreed on. This explanation by no

means convinced them. I wrote to Dr. McDonnell for permission to come and inspect the case again with him, and he granted me this favour. We found the patient feeble, signs of partial obstruction still present, though not nearly so marked, abdomen slightly distended, tumour to be detected on manipulation, not apparent to the sight as before, patient able to go out of doors for short periods ; found also that latterly he gradually relaxed his attention to the advice we had given him. Dr. McDonnell and myself now warned him that he certainly would die if our advice was not rigidly attended to. But, I believe, our advice was not heeded—gradually the obstruction and the tumour reformed, suppurated, and the case ended as we had predicted, but not as we hoped it would, if he and his nurse had given us the co-operation such a case required.

The next case supporting my argument appeared to me to be identical with case 29, referred to in my volume on Intestinal Diseases. A deformity, in connection with intestinal disease had arisen, namely, contraction of the flexors of both lower extremities :—

During October, 1884, a German resident in this city, in the business of baker, visited my abode and requested me to make an appointment with him, and examine his daughter suffering from deformed limbs that they might be corrected. He informed me also that he was originally intended for, and had entered upon the study of, the profession of medicine. On my arrival at the patient's house, I made an examination of the defective limbs, and found them much emaciated, irritable, so that it was with much difficulty that an examination could be made such as would give me the knowledge required. The opinion I gave was: That the limbs were so irritable and emaciated, that no appliance could be tolerated, as contact anywhere would abrade the skin; and further, the patient, to all appearance, was unfit, constitutionally, to undergo any surgical interference. Now, the question was asked, What had been the lady's original complaint which led to her confinement in bed up to the present time? The answer was, that six months previously she had an attack of acute obstruction ; but of this she was perfectly cured, and her medical attendant ceased visiting for the last two months. I now examined the abdomen, and found no physical signs of obstruction ; but from the patient's friends I learned that no action of the bowels ever

occurred without enemata, and even then scybala passed with occasional mucous and with casts of the bowels — doubtlessly a case of imperfectly cured obstruction, which had settled down into the chronic state. After this information the father was requested to come to my house and I would give him my opinion and advice if he wished it. On his arrival I informed him that it was impossible to correct the deformity so long as his daughter remained emaciated, feeble, and irritable, and I advised that she should he dieted thus: One meal of light farinaceous food, morning and evening; one meal of liquid aliment, such as beef tea and arrowroot and water, one to be taken mid-day and two hours before midnight; and a dose of muriate of ammonia an hour before each meal; no enemata to be given. I also informed him that this treatment might be accompanied by a constipation lasting from four to seven days, not longer. Further, he was told that if the regimen and advice offered were strictly followed we should not be disappointed of an expected effect; and that any deviation from the rule laid down would not only be an attempt to deceive me, but would be actual self-deception. He assured me that his short training in medicine, as well as his anxiety to benefit his daughter, would make him very watchful of the management. Our conference on this occasion terminated with my informing him that I should not visit the patient again for one month, and he was to remind me when the time expired and I would again examine her. At the expiration of a month from my first visit I visited the patient again, and, on entering the bedroom, I was surprised to see my lately crippled and emaciated patient sitting at a table near the window playing at cards with a friend. On being requested to present the limbs for examination, she did so, quickly, by the act of the will only, so much improved that the deformity was reduced by one-half. Five days after my first visit the bowels had acted spontaneously, sometimes once or twice a day, the excretion being rather less in consistence than natural. I saw that my chance of being employed to reduce the deformity was waning away. When writing this case for the press, I thought it would be very proper that I should see and get such information that my statement would be beyond all doubt, as more than six months had elapsed since I last examined the patient. Consequently, on the fourth day of last week I wrote a post card to the father, requesting the favour of a visit from him; and, as no answer was returned for several days, I thought the case had not progressed well; but it was otherwise. The father visited me and informed me that the defects of the lower limbs had passed away; bowels

acting regularly, and the patient in excellent health. I now thanked him for the information, and was dismissing him, when he begged to occupy my attention for a few minutes longer. He stated that two months ago he had met a friend whose wife appeared to be suffering from the same complaint as his daughter. She had been using aperients as remedies, until their use had become painfully intolerable; her general state also resembled that in which I had found his daughter. These symptoms induced him to urge his friend to treat his wife as he himself had treated his daughter. His advice was adopted, and the lady has now so far recovered that she has left him to enjoy a trip in the country parts.*

To some physicians and surgeons the foregoing case would be no evidence in favour of the Sydenham method of treating intestinal disease. Neither of them were truly acutely obstructed; but we know very well that patients who are not seriously ill are very desirous to become perfectly well. Would it much console a sufferer to tell him that he never will be cured? and that he might linger for two years, then die? and

* An extension of time with observation, by enabling us, if possessed of the ordinary amount of perceptive faculty, to note and interpret new facts, gives to experience a worth. Up to this date I had not divined to what extent the lower limbs were secondarily influenced in intestinal disease, fixed flexion of both thighs, bu no lumbar curve. Authorities who have written on this subject have, like myself up to the present time, been silent regarding this symptom. Others, as well as myself, have noticed flexion of the right thigh in connection with perityphilitis and typhlitis. This is explained by the relation of the locality diseased to the flexor muscles of the hip joint. Does the flexion of both right and left thigh, observed in some intestinal diseases, point to typhlitis and inflammation of the sigmoid flexure or its surroundings? I have noticed several double flexions. During the early part of this year I had a typical example of this double flexion. The patient, a lady, consulted me about eighteen months ago, when suffering from what she judged to be dyspepsia. My diagnosis was that her symptoms pointed to chronic enteritis, with chronic constipation. I advised and prescribed—my advice was imperfectly followed—but the remedies were taken as directed; she sometimes appeared better, other times worse. After my attendance had extended over several months, the patient became imbued with the belief that she was suffering from either an abdominal tumour or uterine disease. My own diagnosis still excluded either of these difficulties, and I tried to prevail upon her to consult some of my neighbours with a reputation for skill in the diagnosis, etc., of ailments of the character which she believed to be the cause of her

that he ought to be delighted because life will not be extinct in a week? The "recognised treatment of to-day" would have treated the foregoing instance by the principle of the bowels being able but unwilling to act—so-called torpidity—whereas the principles that should guide us is to bear in mind that the bowel is willing—alive—but unable. And the patient should be fed so that the contents of the gut can traverse with a minimum of peristalsis, and a maximum of mere mechanical action; that is, by reduction of the consistency of gut contents and its gravitation.

I shall not further draw upon my recollections of practice to strengthen my argument, although my stock is by no means exhausted. Examples far more telling could here be recorded, but many of them are not my own property.

Truth, though elbowed aside for a period, will ultimately

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symptoms. This counsel she would not follow. Soon after I had thus advised, my patient having occasion to go to London, desired me to name a specialist whom she might consult. The patient was examined by a professor of midwifery. This gentleman informed me that he could not detect either an abdominal tumour or any uterine disease, and advised tonics with plenteous rich feeding. This advice was accepted and practised for a short time only, as exacerbation of symptoms followed so that my patient was obliged to resort to a modification of my primary advice. This, however, gave only imperfect relief. Suddenly, about three months ago, she was taken very ill, attended with all the signs of physiological obstruction, and now willingly accepted and followed my advice fully. In ten days the signs were mitigated and primary relief obtained. Though warned, she relaxed strict adherence to my advice, and in three days the full signs of obstruction reappeared in an intenser form. Again she returned to the method I proposed to her, and on the nineteenth day primary relief occurred, the secondary on the fifth day following after which the bowels acted on an average every third day. During the relapse it was noticed that the patient, though always lying on her back in bed, could not perfectly extend the lower limbs, and she informed me that she felt that should an attendant attempt to forcibly extend them, that something would "tear away in my inside." The difference between flexion, a sign of hip joint

get its place and retain it. The correspondence between Mr. Banks and others, which I have in the preceding pages discussed, happened in December, 1884. On April 25th, 1885, there was published a number of the *British Medical Journal*, containing the report of a meeting of the Medical Society of London. At page 28 of this pamphlet, a reference is made in a footnote to an occurrence early in 1883. The journal contains a *resumé* of a paper read before that society by Mr. Banks, entitled "Treatment of Grangrenous Intestine." The case was treated medico-surgically; the gangrenous portion of the intestine was skilfully removed; the surgical portion of the management of the case was a success, despite some difficulties, all of which are reported. The medical treatment is also given. It was as fair a model of the Sydenham method as could reasonably be expected from a recent convert. One of the details of the Sydenham method is intelligent patience.

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disease, and that which is typical of intestinal disease, is that in the former we can observe flexion plus lumbar curve; in the latter the curvature is absent. The difference is so obvious that the flexion attendant upon intestinal disease will rarely be mistaken for hip joint affection. However, as evidence to prove beyond doubt that such an error can occur, and more, what will be useful, to show what the physiological method of treating intestinal disease can do, I will relate a case. In April this year, an elderly female, consulted me at my house, suffering from what appeared to be hip joint disease. As she appeared very feeble and in much pain, I did not request the removal of sufficient clothing to enable me to properly apply my flexion test to the joint. The obvious and sensible signs could be observed; she also gave a history of falling and injuring her side, and without crucially demonstrating hip inflammation, I concluded its existence and arranged to visit her and fix the joint. This was done next day. On my remarking to the patient's friends that she was in very indifferent health, I was informed that for some time she had been under the care of a physician for liver disease, and had no appetite for food. I prescribed for her a mixture containing a mild aperient with a bitter infusion. On making my third visit I noticed that the pulse was accelerated, tongue dry, no appetite, and on

I well remember that the breakdown of my patience at one time cost me sad experience; but I know myself to-day that my patience was not then of the quality to stand a beneficial strain.

To those who take an interest in the subject of this discussion, it would be interesting to known why, in this "really serious" case, the Sydenham method of medical treatment was adopted, knowing as we do how "mightily unsuccessful it is in really serious cases"? An intestine, after excision, is surely, not then, a simple case? The date attached to this case rather bewildered me, for I was personally cognizant of the fact that, in the early part of 1883, Mr. Banks's patience was severely strained on noticing a constipation of six or seven days; but from a perusal of the report of of this paper, read before the Medical Society of London, his

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examination, I found the abdomen slightly distended, and no improvement of the hip as regards sensitive symptoms. When next visited I found the abdomen more distended, tongue brown and dry, quick feeble pulse, and nausea, no action of the bowels for several days. Her total signs indicated early dissolution, and her husband made certain arrangements to avoid inconvenience in regard to money matters, in apprehension of her death. Now, I suspected that the hip signs were indicative of some disease of the colon, and I removed the hip splint. I carefully explained tne purport, and advised the details, of the physiological method of relieving intestinal diseases, and gave five minimum doses of liq. morphia every three hours. I visited my patient two days after, and found her much better. She continued to improve, and on the thirteenth day the bowels acted freely—primary relief—acted afterwards every two or three days, and ultimately, three times daily for several days. About the fifteenth day following primary relief, the patient addressed me thus: "Doctor, I will not take any more of your medicine, it is purging me to death. I feel as well as ever, pain gone from leg, and can now walk and am very hungry." To appease her I ordered a mixture of chloroform water only, and permitted—limited in quantity and selected in quality—two solid and two liquid meals a day. This arrested the too frequent action of the bowel. This treatment during three weeks restored the elderly lady of sixty-seven years to a better condition of health than she had enjoyed for years.

patience only broke down after noticing a constipation of twenty three days. Evidently the meeting of Mr. Banks and myself, in the early part of 1883, was not a barren one either to the patient or ourselves. This patient recovered, and I made, it is to be hoped, a distinguished convert.

This is the conclusion I came to on comparing the dates—early in 1883 and November, 1883; but the correspondence containing the sentence "mightily unsuccessful in really serious cases" occurred in December, 1884. This fact looks as if more experience had altered an opinion; but I find that though the case of incision of a portion of the intestine was performed in 1883, the report of it was read to the Medical Society of London on April 20th, 1885. No change of opinion is alluded to in the report. The foregoing comments indicate to the reader what I think are inconsistences, but possibly they may not appear so to another.

Among the letters which compelled me to defend the physiological method of treating intestinal diseases is one signed "A Provincial Man." This correspondent, having alluded to that which he thought was an omission in Mr. Treves's volume of Intestinal Diseases,* Mr. Treves wrote in reply endorsing Mr. Banks's statement, and added—

"The measure now in question—a measure the value of which I considered had been for years generally recognised—I cannot agree with Mr. Thomas in considering this particular treatment as final and all sufficient."

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* There being no reference of the Sydenham method.

And subsequently, in a discussion at a meeting of the Bristol Medico-Chirurgical Society, Mr. Treves expressed his opinion in this spirit—

"Such advice is not in the direction of progress. It is a retrograde step—a lapse into the happy days when diagnosis was a dream and pathology was not."

If the foregoing comments by Mr. Treves are true, then Sydenham and myself never have had any excuse for writing on the subject, there being no information wanting, and no practice to reform. Dr. Alexander's remarks at the meeting in the Liverpool Medical Institute were most unjustifiable and open to censure. Mr. Treves has been the first to write (up to this date, May, 1885) a complete manual on Intestinal Obstruction, so exhaustive of the subject, that those who may follow, will find it a very difficult task to combine in a volume on this subject more meritorious information. I had examined Mr. Treves's volume before he had expressed an opinion in correspondence regarding the Sydenham or the completer physiological method of treating intestinal diseases, and failed to find any reference to Sydenham's method in it.

In defence of my idol and self, I will review the chapters devoted to treatment in Mr. Treves's volume. That the unconnected details of the physiological method of treating intestinal ailments are contained in Mr. Treves's volume, and have long been known, I must admit. But like the two ancient cities, which, in late years, have been exhumed, the details of the Sydenham method has been buried under a

number of "special methods," so that from the bedside no signs of the natural mode of recovery have ever been recorded in our periodical medical reports. I maintain that the physiological method applies to all forms of intestinal ailments, and that any or every form of intestinal ailment may require surgical aid. This has always been my contention.

Mr. Treves's poetic allusion to our predecessors in the distant past is not just. For certainly their writing shows that their practice was much in advance of their pathology. Can we be justly credited with this merit? The Halls, Wisemans, and Sydenhams were not non-progressive. The past practitioners of medicine, like those of other arts, have always been a discontented lot. The acquisition of knowledge never satisfied their craving for more. However, it will be interesting to know what modern pathology has done to improve the treatment of intestinal disease. It is my belief that modern pathology, by improving our knowledge of physiology, has done much to point out that the Sydenham method is "all sufficient" as a rule of medical treatment.

Mr. Treves, however, does not think that pathology has aided us much as yet. At page 371 of his volume, his opinion is given thus—

"Our knowledge of the pathology of the affection is by no means imperfect, and yet an increase in that knowledge has not been attended by a corresponding increase in our familiarity with the clinical history of the condition."

If we search our clinical records and sum up from them what treatment "has been for years generally recognised,"

it will be seen that the preceding extract is strictly correct. But it is not the pathology of the subject that is scanty, it is the erroneous interpretation of our pathological facts which is at fault. The pathology of the subject is interpreted direct, without percolating it through the science of physiology before deciding to advise or prescribe treatment — a too mechanical theory of treatment prevailing. I will admit, for the sake of argument, that the details of my teaching are not new; nay, more, that long before Sydenham's time, the rule of treatment I have contended for was a recognised one; but of what use is a rule utterly ignored by the invariable practice of deviations from it? It has been to me a matter of astonishment that my contemporaries cannot see that the exceptions are constantly proving the truth of the rule. Yet they cling to the exceptions as a standard of practice.

Before discussing the question of the treatment of intestinal obstruction, as laid down in Mr. Treve's volume on this subject, I shall give the reader and the investigators of the future my opinion as to what are the gaps in the teaching of my myself and others. With the majority of the gentlemen who co-operated in the discussion of this subject in the Liverpool Medical Institute, and who expressed their belief that a differential diagnosis is difficult to arrive at, I agree, and at all times have insisted upon this fact; but further, I have also contended that while such an exactness of diagnosis would be useful, an inability on our part to diagnose at the time ought not hinder us from treating intestinal diseases so successfully that the mortality may be

vastly diminished. No writer up to this date, May, 1885, has yet given us either one or a collection of symptoms that can be trusted to indicate any special form of obstruction. That certain collective signs may excite us to suspect the existence of special forms, I admit; but it is only rarely that this suspicion has afterwards been demonstrated to be a fact. Unfortunately, the records of cases show that a differential diagnosis has been frequently incorrect. That our differential diagnosis may inprove, is probable; but if this generation will continue to ignore the information which pathology and physiology have given us relating to the subject, then progress in this direction is improbable. Much harm has been done in late years by attempts at differential diagnosis to classify the case, whether it be from the beginning one of such a character that medical treatment only or surgical and medical treatment combined will be required. I have contended that any of the many forms of intestinal ailments may require surgery so soon as signs appear showing that an impediment, functional or mechanical, exists which surgery only can remove, so that nature may resume her work.*

Under the term intestinal obstruction, Mr. Treves would not class "lead colic," "cancer of the omentum," "tubercular peritonitis," and "peritonitis." It would be an error of diagnosis to do so. Yet among them is one "peritonitis"

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* Since I promulgated this doctrine, I find time confirms my advice. Abdominal section has been performed for suppurative peritonitis, lead colic, and gastro-enterotomy for typhoid lesion.

which is the most critical form of obstruction of the intestinal functions we have to treat. The "cancer of the omentum" may lead to obstruction, and the physician ought to have been taught the method of putting off the evil day with the least cost to the patient and best preparation for surgical interference. The same principles of treatment apply to the lesions, excluded by Mr. Treves from the category of intestinal obstruction, which apply to those more generally accepted as cases of obstruction. Mr. Treves's classification, like the recognised treatment, is too mechanical. The severer forms of obstruction almost take care of themselves; the signs are so striking and distressing that "at first sight" they negative interference. It is otherwise with the so-called milder forms. They generally are treated by the mainforce method, as though the intestine was more contumacious than sick, and finally go to swell the bills of mortality, and, in the treatment, symptoms appear to describe which new terms have been invested, such as "grafting" and "masking;" so that, as Sydenham has remarked, remedies are prescribed for the remedy, the original complaint is aggravated by the treatment, and the patient succumbs to the drug and the disease. That I am justified in making the foregoing remarks I shall try and prove by reference to our latest authority on this subject.

To maintain my position as a dissenter to the comments made by Mr. Treves on the teaching of Sydenham and myself, I have to prove that, as a method, my teaching was not a measure generally recognised, and that neither modern

pathology nor physiology condemns me. In chapters xxiv. and xxvi., Mr. Treves gives us the principles of treatment commencing with "Non-operative measures." Mr. Treves, on this matter, boldly demands a "little more attention" than teachers have hitherto been giving to the subject of feeding.

FROM TREVES'S.*

Rules for Feeding the Patient during Treatment by the Recognised Method.

"Certainly in not a few instances one of the factors in the exhaustion that leads to death depends upon the patient's inability to take or to retain food. When the case has lasted four or five or six days the patient's prospect of recovery is comprised by the debility induced by want of nourishment, and this debility may seriously modify the result of any operation. In not a few instances, more especially in cases of intussusception, a process of spontaneous relief is found to be nearly complete at the time of death and to have been arrested by a fatal exhaustion, to the production of which an inability to take food has no doubt contributed. The position of acute and subacute cases of obstruction, with reference to the question of feeding, is as follows: The patient is very sick, he not only vomits everything that he takes, but will vomit at other times than

THE SYDENHAM RULES.

Rules for Feeding the Patient without adding to the Symptoms of the Disease.

Certainly, in a few instances only, does exhaustion lead to death from the patient's alimentation not being sufficient to enable him to tide over the average period of the disease. Sir W. Jenner's remarks, in the Bath lecture on Typhoid, applies to all intestinal diseases. "The sufferers generally die from the disease, not starvation." In the severer forms of obstruction during the first four or five days there may be a loathing for all food – too short a period to kill by starvation. After these first few days, when our treatment has began to have its effect, we are confronted by a another difficulty—the intense desire of the patient for food, whilst thirst has become moderated. Now the patient and his friends pester the practitioner to relax his restrictions; and to satisfy these unreasonable suggestions is often a more difficult task than even to cure the patient.

* Intestinal Obstruction, pages 415 and 493.

From Treves's—*Continued.*

after the ingestion of food. In many subacute cases, where the sickness is not so marked, the taking of nourishment excites the act of vomiting after the symptom has abated, and the patient may for awhile only be sick after he has taken food.

"It is obvious that it is worse than useless to attempt to feed these patients by the mouth. There is usually an entire lack of appetite, and a disgust of food, quite apart from the circumstance that every mouthful swallowed is apt to aggravate one of the most distressing of the symptoms. Moreover, even if it be supposed that the food can be retained, it is scarcely possible to imagine that it can be digested and absorbed.

"In the treatment of the case the patient should, in the first place, of course, be kept absolutely at rest. The lower bowel should be emptied by an enema. The patient should be allowed ice to suck, but no food should be given by the mouth. The strength should be supported, if thought fit, by nutrient enemata, although it must be remembered that such enemata may cause distress and may have to be discontinued."

The Sydenham Rules—*Continued.*

The question of feeding by the rectum has often been entertained; but "it must be remembered that such enemata may cause distress," and may make the case intenser by adding a second cause of irritation. Can the patient be fed at all? Certainly, the vomiting being now moderated, suitable food may be given, enough to pall the pain of hunger, and can be retained long enough to be of some benefit. If suitable food be selected, he will not vomit any oftener than he would by mere drinking, for he must, as long as the obstruction lasts, vomit occasionally the liquid taken. This act, if not very frequent, is by no means an evil. Moreover, as the patient has an appetite, then it may reasonably be supposed that during the period ailiment is retained it is assimilated. If there were no appetite, assimilation might be doubtful. After the fiercer onset of the disease, the patient should not—as though they were remedies—be persuaded to take ice and cold drinks, as they stimulate peristalsis. The patient's natural heat should be economised by covering, the abdomen only being exposed if pleasing to the patient, so that he may gain by saving; thus less food would be required.*

* To be strictly treated and cured expeditiously, chronic cases require the same rules of treatment as the acute ones.

The question of feeding is too generally considered as a question of what quantity rather than quality can do, and the evil effect from the diminished alimentation, inseparable during all intestinal disease and acute obstruction in particular, is much magnified. A sufferer from obstruction is generally lying in a comfortable room on a warm bed, and after a few days have passed, has suitable forms of food, soothing remedies, with kind sympathy and encouragement, the shock of the disease being the only real evil to try his endurance. Is he worse off than two or three persons cast away at sea in an open boat, with rough food, limited drink, and wet clothes, exposed to keen winds at night, a scorching sun by day time, while working by turns at the oars? Castaway people often stand this exposure for a week, and are "picked up" still living. In 1877 a number of men were immured for nine days, the result of an explosion at the Tynwydd Colliery, Pontyprydd; they were totally unprepared for such an eventuality, yet all were rescued alive.

On perusing the two parallel columns a reader might say, Surely the rules in the right hand column are not intended for the same complaint as the rules in the left one, as the symptoms do not appear parallel? But the reader ought to remember that the rules in the left column refer to obstruction plus "the recognised method," while the rules in the right one refer to obstruction minus any interference. For instance, a practitioner who adopts what I hold not to be the

recognised treatment, would not give an enema for diagnosis* or feeding the patient, as either would be an interference not conservative in their tendency, as its effect is not confined to the mere excitement of peristalsis—it causes also more or less shock, and excites earlier the act of vomiting; which again increases thirst and diminishes the period of retention of aliments and assimilation—as in physics so in medicine—mutiple effects here result from a single cause.

No wonder that the contents of the parallel column do not read as though they were prescriptions for the same complaint. Mr. Treves's volume may contain—isolated—the details of the physiological method of treating intestinal diseases, just in the manner in which the numerals 1885 have been employed before now to indicate the year 1588. At page 422, Mr. Treves lays down the rule for the use of a sedative. He, like myself and others before us, prefers opium. To further complete my proof that my teaching is not "a measure the value of which I consider has been generally recognised,"† it is my duty to show that I have pointed out the rule by which we can avoid the danger which all my predecessors have insisted upon as attendant on the use of opium. To Sydenham certainly is due the credit of regulating the quality of the patients' food, so that sedatives have no evil effect—that is, to harmonize the use of food and sedatives.

* Mr. Treves's Intestinal Obstruction, page 399 - I am well aware of the fact that at page 396 of the same volume, the use of enemas for diagnosis is condemned, but why advise their use at page 399?

† See correspondence.

After laying down rules as regards the "feeding," Mr. Treves, at page 420, commences to discuss the value of opium as an aid to successfully treat cases of obstruction. Like other writers on this subject who have preceded him, he gives this drug in preference to other sedatives. His theoretical opinion is not of a definite tone. He says that the action of opium may stimulate or paralyse special nerves—two opposite effects. How two opposite results can follow a sedative cause he explains upon the supposition that nerves contain inhibitory fibres. This explanation is contrary to my own conviction, as it will not explain the effects of all remedies or other causes which induce variations from the standard of health.

"The theory of the action of remedies," which I have laid before the profession, and which excludes the existence of inhibitory nerves, will explain from every point of view the known action of medicines and septic poisons, physiological phenomena excepted, inasmuch as our interpretation of physiological experiments upon nerve structure is as yet by no means definite or consistent. The following, taken from page 421 of Mr. Treves's volume, is an example of the theoretical teaching from which I dissent:—

"Morphia appears thus to exert on the intestinal apparatus an action comparable to that which digitalis exercises on the innervation of the heart, which consists in a stimulation or paralysis of the inhibitory fibres of the vagus, according to the dose employed."

There is the same difference between "stimulation and paralysis" as there exists between a positive and negative

effect. If we use morphia in the treatment of obstruction, guided by so indefinite a conviction of its effect that we are not certain whether its action be that of stimulation or inhibition, are we not likely, like our predecessors, to injure the patient by what, in latter days, has been termed "masking?"

The paragraph on the value of opium ends with a warning lest the use of opium should cause "masking." No wonder! nowhere is there given any rule by attention to which the sedative can be used, so that the practitioner may be certain that, if the patient dies, his death would not be due to the opium. Indeed, at page 422, in a case taken from Leichtenstern, Mr. Treves approves of a dangerous use of opium. The sufferer had all the signs of pure collapse, a form of collapse in which reaction is always probable by careful management; a collapse with harmony of pulse and temperature. The dose of morphia aided the reaction, by what I hold to be the indirect method of stimulation—localised direct sedative action—a risky method. Had the dose of morphia been exceeded, then instead of the collapse being followed by reaction, the patient would have died—"masked." Why not employ, to avert or neutralize the collapse, a pure stimulant? Why not use the direct method? This method has advantages. If the dose is small it can, if required, be repeated, and can do no harm if its full physiological effect is produced, whereas, if the indirect method—stimulation by sedatives—be employed, the risk of an over dose is great, and a dose, which may not have the effect desired, if repeated, may

produce too much of an effect—"masking" to death. The full physiological effect, if produced by the indirect method and employed to bring about reaction from collapse, is a very serious error, and its continuance must end in permanently adding the word "masking" to our medical vocabulary.

What is the source of the danger now termed "masking?" The so-called "masking" is caused by an error in judgment as to the conditions the sedative is applicable to, and a misinterpretation of the action of sedatives in general.

I maintain that if the details of the physiological method of treating intestinal disease be followed, then a sedative cannot mislead an "intelligent and well educated" practitioner, for, should it be observed that a discord exists between the pulse and the temperature, surgical interference should be tried; and, even if the temperature and pulse be in accord, should the former be below normal, the use of sedatives is a wrong practice. The last paragraphs of page 426 and the first of page 427 are proof that Mr. Treves has no definite conception of any rule by which sedatives can be safely employed in these cases; of a rule, by attention to which the most inexperienced of us is placed on a par with the man of many years. The rules I have laid down for the safe use of sedatives are so definite that certainly the patient cannot die from the sedative, and until these rules are shown to be unreliable, then my teaching cannot be said to be "a measure the value of which had been for years recognised."

At page 427, the question of employing "aperients" is discussed. Mr. Treves commences by emphatically condemning their use. For example—

"Aperient medicines in these maladies have rendered subacute cases acute, and have caused chronic forms of obstruction to take on an acute development."

Further on, however, he is not so firm of opinion :—

"In obstruction, however, due to fæcal accumulation, aperients are of especial value, particularly when combined with enemata and administered with proper caution. In cases also of obstruction depending upon partial mechanical occlusion of the bowel, laxatives cautiously administered are often of the greatest service. They render the intestinal contents fluid and prevent accumulations above the obstruction. Violent aperients are, however, often almost as obnoxious in these cases as they are in examples of acute strangulation. They hurry along the contents and rapidly plug the stenosed segment, at the time that they roughly disturb the disordered bowel above the obstruction."

These two quotations prove that, like Dr. Brinton, Mr. Treves had not divined the method of relieving constipation in obstruction without the use of aperients, that is by adhering, as close as may be consistent with the maintenance of life, to the natural method of relieving constipation. This method I was the first to make known, and if neither Brinton nor Treves make any reference to it, then I am entitled to the admission that my teaching has not been "for years generally recognised" by the profession.

Mr. Treves, at page 430, next entertains the question of using metalic mercury for the relief of obstruction. At page 432, he concludes his judgment on the worth of this mechanical interference, thus—

"The use of metallic mercury in larger doses would appear to be worth trying, especially as the mode of treatment appears to be attended by no special risk."

To use metallic mercury for relieving obstruction is only to go back even before the time Sydenham lived, a period very bare of pathology. Sydenham says—

"Whatever may be said about bullets and quicksilver, things which, whilst they can effect little good, can do much harm."

Brinton has pointed out to us that the bowel contents, their consistency being reduced by liquids imbibed, act as an hydrostatic dilator and expand a constricted part as much as is practicable during the short period which may elapse before primary relief is obtained. Of what use is the mercury at all to the already dangerously over-weighted locality of obstruction.

I suspect that many of the instances of reported successful removal of obstructed contents by mercury—for the impediment it cannot remove—were only cases in which that metal was used, after many remedies had been tried and time had elapsed; thus relief was near, and the last remedy was credited with the result. The more I examine Mr. Treves's method of medical treatment of obstruction, the more I perceive that it is the recognised one, and that which I have for "many years" refused to recognise as a proper one.

At page 432, the use of "ice" is considered. Mr. Treves has not noticed that after the fierce primary period of the malady has subsided, ice sometimes excites colic, even if given to assuage thirst; but I find he in some instances

advises the use of enemata of ice for paresis of the bowel, but not for acute obstruction :—

"Enemata of iced water are apt to excite considerable peristaltic movement, and may, therefore, in many cases, do more harm than good. The treatment is probably more adapted for the relief of obstruction due to paresis of the bowel than to that due to mechanical causes."

If the first portion of this paragraph is theoretically correct then the later is incorrect.

In my volume on Intestinal Diseases and in this pamphlet, there are references which show that the "elaborated" treatment I have taught did relieve cases of paresis of the bowels which enemata were not able to do.

At pages 433-7 the use of "electricity and massage" is considered, and isolated cases of success are referred to. I must candidly admit that I am mentally incapable of understanding how electricity or massage can aid us in the treatment of acute or functional obstruction, neither can I find any pathological facts which justify their use.

At page 438, Mr. Treves commences to discuss treatment by enemata. This item of treatment is a very important one, as, from noticing the pleasant and thorough effect which follows the moderate use of an enema in healthy persons, most of us have concluded that it is equally harmless and efficient in many abnormal conditions of the intestines, the majority of practitioners forgetting that during the existence of true and functional obstruction, a very efficient method of producing a watery evacuation of the bowel can be prepared

through the mouth. This Brinton pointed out, but few bear the fact in their memory when confronted with the "really serious cases."

When giving his opinion regarding the use of enemata during the existence of obstruction, Mr. Treves says, at page 438:—

"Copious enemata of water are of considerable service in cases of intestinal obstruction. Beyond the fact that an enema may excite peristaltic action in a large segment of the small intestines, they are of use only when the obstruction involves in whole or in part the larger intestine."

He refers to several methods of giving enemata, and says—

"Enemata, as administered by one or other of these methods, are of infinite service in many cases of obstruction. In cases of obstruction, due to fæcal accumulation, enema constitutes the principal active treatment."

That enemata are useful for cases of simple fæcal accumulation, whether rectal or intestinal, no person has ever questioned; but I know of no diseased condition of the intestines, whether accompanied or not by obstruction, in which enemata ought to be given. A perusal of Mr. Treves's article on "enemata" must convince any reader that, if the physiological method of treating intestinal affections has been "a measure the value of which had been for years generally recognised," Mr. Treves has utterly ignored it in his very comprehensive treatise. In the paragraph devoted to "enemata" every quality and every method of giving enema are alluded to; in many instances he advises their use, and in others he inhibits their employment.

At pages 442-4, "insuflation and enemata of carbonic acid

gas" is mentioned, Mr. Lund and Ziemssen being mentioned as counselling their use. It is with satisfaction that I here record the fact that Mr. Treves gives no opinion regarding the practice of insuflation in the treatment of obstruction. Mr. Banks informed us that Ziemssen practised the Sydenham method. As Ziemssen figures as an advocate of this procedure, all I can say is that his treatment is inconsistent with an acceptance by him of the Sydenham method. How any "intelligent and well-educated" practitioner can resort to the practice of insuflation for treating obstruction is beyond my understanding. But others, even to-day, perceive advantages in resorting to insuflation and gas enemata, as we have been told by an "intelligent and well educated" surgeon, that it is "a legitimate experiment," even upon the living subject.

The bone of contention between myself and eminent contemporaries has chiefly related to the question of what is and ought to be the medical treatment of intestinal diseases. As regards the surgical treatment of intestinal diseases, I have recommended a reform by a wider extension of surgical interference, and not limiting the use of the knife to cases of absolute obstruction; that is, that even in typhoid lesion, peritonitis, enteritis, and other phases of functional defect, surgery should be tried when the resolution of the disease by the aid of medicine is a failure. Since my advocacy of this extension of surgery, I find that Mr. Treves, among others, have thus succeeded in saving life.

The surgical portion of Mr. Treves's volume is a splendid

contribution to the surgery of the intestines. The only defect I can find is the fact that the question of surgical interference is made to depend upon an improbable diagnosis—a differential one. I think that, taking the physiological signs as a guide and operating, no matter what the cause may be, will give us the least mortality; and if we notice the tendency of practice during the last two years, as recorded in our medical periodicals, we are rapidly coming to the opinion that the field of intestinal surgery ought to be widened and not limited to hernias, volvulus, intussusception, etc. The application of surgery for the relief of these difficulties will probably be more successful than its employment in other forms of functional intestinal ailments, as the former partakes more of the traumatic character, the latter generally being of constitutional origin, and consequently, not accompanied by the same tendency to restoration.

The analysis I have made of the teaching and practice of two gentlemen who have recorded their dissent from my views regarding my own teaching in regard to the treatment of intestinal diseases, does not include all who have done so. In the *Lancet*, issued November 22nd, 1884, and in subsequent numbers, Mr. Bryant published several lectures (Harveian), and, as might be expected from the author's reputation, their perusal repaid me. They contain much information. I especially noticed that the author rose above the dry and commonplace facts of surgery, and became momentarily eloquent while discanting on the unreasonable and purposeless

practice of using opium or sedatives in certain cases of obstruction of the intestines. In fact, he appeared to denounce the "recognised method" as a method of mere "hope." My feelings on completing the study of Mr. Bryant's lectures were those of pleasant satisfaction to find a leader on surgery seconding my views.

This comfortable feeling, however, did not last long, because, no matter with whom of my professional brethren who had read the lectures and with whom I discussed them they insisted that the "eloquent" portion referred to myself! The persistence with which this opinion has been maintained has inclined me to think that those who have thus interpreted Mr. Bryant's lectures may be correct. Consequently, I have decided to finish this discussion with a review of them. I will try to show that Mr. Bryant's criticisms do not apply to my teaching, but to the recognised treatment of intestinal ailments. No matter what theoretical opinions Mr. Bryant holds regarding the treatment of intestinal diseases, his practice has been the disastrous recognised one. He may be a better operator than most of those he lectured to; but his contributions to the literature on this subject informs us that his principles of treatment are no better than others; many of the evils he would correct by surgery are preventable, so that they may not come within the reach of surgery.

Mr. Bryant has titled these lectures "The mode of death from acute intestinal strangulation and chronic intestinal

obstruction." In the first paragraph of Lecture I. he proposes to consider—

"First, how death is occasioned in intestinal strangulation; and secondly, how life is destroyed by intestinal obstruction."

In the second paragraph, we are informed that the placing of cases of stangulation among those of obstruction is—

"'A grevious error,' since in strangulation of the intestine obstruction is only one of its symptoms, but not the cause of danger or death."

I fail to perceive the utility of this information; for, if the strangulation did not exist, the intestine would not be obstructed, but supposing the strangulation as existing, then surely the load coming down on to the strangulated part cannot be referred to as a mere sign; for, if the bowels were strangulated and always empty, then the strangulation would have nothing to obstruct, and it would be an instance of pure strangulation, with pure signs of this difficulty. But the keeping empty of the intestine above the occlusion is impossible in the living subject, consequently, it is an impossibility to separate the signs of strangulation from those of obstruction; for the signs attending strangulation are at times identical with those arising from functional defect.* The history of the difficulty, not the actual symptoms, being the only clue that may enable us to venture upon a differential diagnosis. This makes the information tendered by the lecturer to be of no practical interest to us.

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* This I constantly insisted upon, and Mr. Bryant supplies me with evidence in confirmation of my teaching. See case lviii., page 1228, *Lancet*, December 20, 1885.

In the fourth paragraph these questions are asked :—

"Is it true that patients die from the obstruction, or is it true that the obstruction is merely a symptom of some condition which, if not relieved, must bring about a fatal result ; fatal, however, not from obstruction to the passage of fæces through the lumen of the bowel, but from changes in the bowel itself and the parts above? I believe this latter observation to be correct."

That the primary and urgent cause of danger and death in cases of strangulated intestines must be constriction is obvious; but it is an error, when treating such cases, to ignore the load obstructed by the strangulation.

If a strangulation or any mechanical impediment is suspected to exist, it must have the first attention ; but if it has been removed, there still exists functional defect which continues the obstruction—no trivial difficulty. When strangulation of the bowel exists, it must be the foremost cause of death ; but the contents obstructed must accelerate death and diminish the chance of the bowel escaping from the grip.

The lecturer's deductions are the following :—

"It was from want of appreciation of this fact that the older surgeons gave purgatives in cases of internal intestinal strangulation, as well as in cases of strangulated hernia after its reduction ; and it is, I believe, from the want of a full appreciation of the bearing of the same fact that, in examples of intestinal strangulation not hernial, practitioners seem, even at the present day, to trust too much to physic, and various manipulative and other acts, when there is nothing less than the removal of the strangulating cause from which the slightest good is to be anticipated."

I maintain that some of the errors of treatment made by the "older surgeons" arose from their adoption of the very classification Mr. Bryant invites his audience to consider.

They underestimated the serious importance of the physiological defect that remains after relief of the griped intestine. The "older surgeons" missed discerning the compound character of the difficulty, and after operation, for relieving any strangulation of the intestine, treated the bowel as "able but unwilling."

Mr. Bryant's teaching so far is in the direction of the "happy dream" days when pathology was rather scanty. In these lectures, like the "older surgeons," the lecturer takes a too mechanical view of the ailment and treatment. He gives his opinion of the recognised medical treatment in the following paragraph :—

"What bearing, then, should these facts have upon surgical practice? Are they such as to lead the practitioner to depend upon a Surgery of Hope, based upon the administration of drugs which mask symptoms, but do nothing towards the relief of the mechanical conditions upon which the symptoms depend? Or should they lead him to look boldly at each case as it presents itself, and to act decidedly and with precision? In a case of strangulated hernia, the rule is now well recognised that, on the appearance of vomiting—from the first occurrence of which symptom the date of strangulation is calculated—no time should be lost in the reduction of the hernia, either by taxis or herniotomy; for surgeons and pathologists well know that nothing less than the mechanical condition which is called strangulation in a hernia can be of essential service; and that, until this end be secured, opium only masks symptoms and brings about a fool's paradise, where the Surgery of Hope may exercise itself at the expense of scientific knowledge and patient's lives."

I can assure the reader that I perused this paragraph with intense satisfaction. The lecturer appeared to me to be sincerely desirous of hastening the suppression of the unmethodical recognised practice of using sedatives in obstruction, which

practice, justified by the precedent language of three able surgeons, I will myself venture to designate as an example of "killing no murder." The lecturer refers to practitioners of medicine thus :—

"Practitioners, even in the present day, trust too much to physic and various manipulative acts."

This is too true; but my teaching proves that this reference is not applicable to myself. But the practitioners may ask, Are there in these lectures any definite rules by attention to which we may amend the medical treatment? The lecturer only invites the physicians to hand over the cases earlier to the surgeon, and that upon insufficient evidence of urgency. He cites, in support of this advice, *post-mortem* evidence of urgency —the ante-mortem signs are in some instances recorded in the report of cases, but not pointed out to his audience. This omission is frequently to be noticed in the teaching of surgeons when criticising the acts of the physicians; the latter again, noticing the frequency with which some cases diagnosed by surgeons as requiring their interference, get well, are tempted to "hold out" and trust to chance. Indeed, they are not without excuse, so long as we cannot agree as to the principles of treatment, or what the signs of urgency are.

This lecture is illustrated by a series of cases, brought forth mainly to bolster up the paragraph in which, as the "Surgery of Hope," Mr. Bryant eloquently condemns the recognised medical treatment—"physic and manipulative acts."

"Case I. died after the hernia had been explored."

"Case II. died with severe symptoms of intestinal obstruction."

"Case III. died on the eleventh day. The *post-mortem* showed the existence of a functional cause of obstruction and also the existence of a strangulation; there existed ulcerative peritonitis, leakage, volvulus, and band."

The patient was an inmate of a well-known public hospital; no operation was performed. The signs, which pointed to the advisability of having resource to surgery, must have been observed in case iii. Is it possible that the patient was simply treated by opiates up to his death? How can we explain why the physician in attendance did not observe that his remedies were not beneficially influencing the case? and why was the co-operation of a surgeon not secured? If the reader will bear in mind the details of the recognised method of treating obstruction by sedatives, and compare the Surgery of Hope paragraph, which is part of a comment on case iii., with my own teaching, he will be able to satisfactorily answer my questions. In the commentary on this case, the "masking of symptoms" argument is again reproduced. It has been my contention that if the case is one of obstruction which is not remediable by advice or medicine, then no sedative, short of a toxic dose, can "mask" the character of the symptoms. An opiate may tone down the evil symptoms, but never alter their character. It is against this very erroneous use of drugs and the baseless "Surgery of Hope" I have always protested.

As so well-informed an author, as we know the lecturer to be, while warning his audience against the use of sedatives,

yet gives no rules for their safe use, I may reasonably conclude that no rule is "generally known." To operate merely because the vomiting was persistent, a sign which Mr. Bryant estimates as all important, would not lessen our present rate of mortality.

Cases iv. to xi. are only records of instances in which surgery ought to have been applied. Case xi. is so especially, as there were some of the signs pointing to the advisability of surgical interference; pulse 120, thready; vomiting persistent, etc.

The lecturer thus comments on the first nine cases :—

"I have thus given you nine cases of internal strangulation of the bowel, all of which could have been relieved, and possibly cured, had the rule of practice I am now advocating been carried out. In several of the cases in which the operation was performed, failure followed, from the measure having been applied too late. May the future record be more satisfactory!"

But Mr. Bryant does not point out nor record the urgent signs, beyond persistent vomiting, which would have justified surgical treatment.

The remaining portion of Lecture I. is devoted to the consideration of intussusception, and the lecturer delivers comments based upon the signs which accompany the recognised treatment of these ailments, but which are seldom observed in the cases which have not been treated at all. Those of my readers, who would wish to know what the recognised treatment of these difficulties really is, cannot do better than study cases xiv. to xxii. in this lecture; they have been taken

from the note-book of one of the larger Metropolitan hospitals, to which is attached a school of medicine, consequently, they must represent "recognised treatment." The recognised treatment I wish to reform, and against which the lecturer makes out a strong case, in the direction of supplanting it by surgery, an innovation that would not lessen the present mortality, but only save a life here and lose another elsewhere. I advocate a more extended resource to surgery, but I also want the physicians, by a trial of my teaching, to better winnow their cases and keep the surgeon to his own sphere so that needless operations by invading the abdomen, in cases of obstruction, will become more rare. Of this we have two examples in two of our weekly medical periodicals issued in May, 1885.

After a preliminary discussion of the causes of death in cases of intussusception, among which the lecturer does not include the result of recognised or so-called "legitimate" treatment, seven cases are given in illustration of his views. Most of these cases are rather scantily reported; but some of them are reported with sufficient details, that we are enabled to estimate what was the influence the treatment had upon the progress of the patient towards recovery or death.

"Case XIV. was inflated, the peritoneal covering was cracked and turned out, and in the sacculi the longtitudinal muscular fibres were torn and the transverse separated."

"Case XVII. Inflated, injected, trocared. Result—rupture of bowel, extravasation peritonitis."

"Case XVIII. Inflation, injection; rupture of peritoneal coat of bowel."

"Case XIX. Inflation, purgatives, collapse; peritonitis."

The lecturer, after reading these cases, questioned thus :—

"Might I ask, could the operation of laparotomy have been more fatal in these cases than that of inflation proved? Might it not, with a great probability, if employed early, have been more successful?"

My answer is, certainly, laparotomy is vastly safer than inflation, and if the signs indicative of the arrest of spontaneous cure be present, then the surgeon ought to invade the abdomen early. The existence of a tumour, even with vomiting, would not indicate arrest of the tendency of the disease to resolve.

The lecturer, I find, notwithstanding the cogent evidence he has brought forward against the practice of inflation, still retains it among others of his methods of interfering with obstruction. For instance :—

"In the more chronic cases, such as in severity seemed to be parallel with cases of obstructed or incarcerated hernia, running on to strangulation, inflation may be justifiable, and even successful; but then it must be employed in the early days of symptoms, that is, within the first three days; later on, changes in the bowel are almost certain to have taken place, which would render the treatment by inflation or injection fruitless, and probably dangerous."

The first lecture terminates with five conclusions; these show what are the defects of our diagnosis, interpretation of symptoms, and the "generally recognised treatment."

"By way of conclusion, I would lay down the following rules of practice :—

1. Laparotomy should be undertaken as soon as the diagnosis of acute intestinal strangulation is made. There should be no delay allowed for the formation of a specific diagnosis of its cause. It should likewise be proposed in all cases of acute intussusception, and of chronic, which have failed within three, or, at the most, four days, to be relieved by other treatment.

2. In all operations of laparotomy, it is to the cæcum that the surgeon should first advance, since it is from it he will obtain his best guide. If this be distended, he will at once know that the cause of obstruction is below; if it be found collapsed, or not tense, the obstruction must be above. Adhesions or bands, are, moreover, more frequently near to, or associated with, the cæcum, than with any other part of the intestinal tract. It is also in the right iliac fossa that the collapsed small intestine, in cases of acute strangulation, is usually to be found; and, with this as a starting point, the surgeon will have less difficulty in tracing up the intestine to the seat of strangulation than if he begins at a distant coil, when it will be a matter of chance whether he travels away from or towards the special object of his search—the seat of obstruction.

3. In a laparotomy, when the strangulated coil of bowel is gangrenous, it should be brought out of the wound, and the gangrenous knuckle resected. The proximal and distal ends of the resected bowel should then be stitched to the ends of the wound, and an artificial anus established.

4. Nélaton's operation of enterotomy should be undertaken in all cases of intestinal strangulation, when laparotomy is rejected or seems inapplicable, as well as in cases of intussusception in which the invaginated bowel cannot be readily released. It should be performed in the right groin, or, rather, right iliac fossa.

5. If the laparotomy succeed, the cause which called for it is removed, and the normal action of the bowel is restored. If resorted to early, and as a rule of practice, it is probable that it would be more successful than the treatment by opium, inflation, or purgatives, which has hitherto been in vogue."

The first rule is in character with the whole of the lectures, and applies to cases treated by the "recognised treatment," but is not applicable to that which I contend to be the scientific treatment; "the measure now in question," as one eminent surgeon

has termed it, and has said "had been for years generally recognised." No one can object to the advice in this rule, that there should be no time wasted in the useless attempt to divine the mode of strangulation, and to operate because an intussusception has failed to be relieved by treatment within three or four days, is, I think, sound advice; but of what use is this advice if the full signs of the failure of other treatments is not even alluded to anywhere in the lecture? Mr. Bryant gives us many instances of failure of treatment. How did it happen that those in attendance did not notice that the treatment was abortive? The first rule is not interesting to me without being informed of all the signs of urgency. The second rule is an excellent one, and well worthy of being acted upon during a search in the abdomen, and to Mr. Bryant is due the merit of first giving us this useful hint. The third rule is also an excellent one. In the fourth rule enterotomy, as suggested by me some years ago, is accepted. The fifth rule is also an excellent one, so long as we cling to the "treatment by opium, inflation, or purgatives, which has hitherto been in vogue." But, unfortunately, the performance of laparotomy does not end all the difficulties, in connection with the lesion, for the same treatment, and to which Mr. Bryants alludes to with curt courtesy, is generally resumed after operation, but not under such disadvantages as before.

In this lecture the post operation medical treatment is not brought up to date, if we accept Mr. Banks's teaching "now."

"Moreover, now that purging and other violent remedies have been given up, and treatment is directed toward ensuring repose of the bowel by opium, with the view of letting nature overcome the obstruction, such recoveries are decidedly on the increase."

Lecture II. relates to the "Mode of death in intestinal obstruction." As a primary cause, the lecturer, before the production of evidence, mentions the changes of intestinal structure effected by obstruction.

"The changes in the bowel above the seat of obstruction are the main cause of death when these cases are left to take their natural course."

As there has never yet been recorded a case of obstruction, which was permitted to take a natural course, it would have been very interesting if the lecturer had given us an example of such an instance. The several cases alluded to are of great interest.

Cases xxiii., xxiv., xxv., xxvi. are instances of "imperforate anus;" an unnatural instance, not permitting of a natural resolution. I do not question Mr. Bryant's pathological facts, nor his opinion regarding their cause; but, like Mr. Treves, I am dissatisfied with the limited use we have been making of these and other pathological observations.

"Case XXVI. The details of this case informs us that this case had not been left to its natural course, the medical treatment was one that supplanted it."

"Case XXVIII. is evidently an instance of obstruction from congenital deformity of the gut."

Indeed, most of the cases referred to in this and the other two lectures are of much interest, and I think we are much

indebted to Mr. Bryant for his trouble in collating and publishing them for general use. This lecture is mainly devoted to the discussion of the chronic phases of obstruction. After case xxxvi., there is the following comment :—

"These brief notes of cases, not to mention many others that might be quoted, are enough to demonstrate the fact that, with rectal obstruction, from whatever cause, the distending pressure in the bowel above, caused by the accumulating motion, together with the ineffectual efforts of the intestine to urge on its contents, is prone to be followed by ulceration, sloughing, and perforation of the distended bowel. In some cases, it is the cæcum that suffers ; in others, the colon ; but in all cases the risk is run that, from overdistension or ulceration, some perforation of the walls of the bowel will take place, and with it death. I would, therefore, ask all my medical friends, in cases of chronic obstruction, to have the probability of this change before them, and not to allow time to pass by unnecessarily, when relief to the overloaded bowel can be afforded. The trouble demonstrated will come sooner or later, and it is well to make provision against it by surgical means as soon as medical measures have been proved insufficient ; the practice of prevention being as valuable in the treatment of cases of intestinal obstruction as it is known to be in those of intestinal strangulation."

I have invited my medical friends, in cases of chronic obstruction, not to allow any time to pass by, without unloading the intestine from all avoidable excrementitious matter, and maintaining it so for a period until all the signs of recovery are well established, and thus avoid the necessity of surgical interference, and further should malignant disease eventually be found to be the cause of the obstruction, the patient will not have to be operated upon with an overloaded intestine, which, as Mr. Bryant informs us, "is the general exciter of peritonitis in all cases of obstruction, and is too often the cause of death after colotomy." Handing the cases even early to the surgeon

may not be advisable, if the intestine has not been relieved of its avoidable contents. If my teaching had been a "measure, the value of which I considered had been for years generally recognised," Mr. Bryant would have been able to advise his "medical friends" how in many cases to prevent "sooner or later" the necessity of "surgical means" having to be employed to gain a chance of saving life. The lecturer's idea of treatment is that of preventing the cause from having its expected effect. My proposition is, that we should first try the effect of early taking away the cause.

No hint is given how to remove the cause, which may remain even after surgical interference, an annoyance to the sufferer, and hamper the after treatment of the surgeon. This omission I have pointed out in the cases of Messrs. Freer, Pring, and Bryant at page 19 of this pamphlet.

The third lecture is devoted to "The differential diagnosis of acute intestinal strangulation and typhlitis." Like most writers, Mr. Bryant teaches with great confidence how to diagnose differentially; but we seldom meet with a series of successful illustrations from any one. This lecture, like the two preceding ones, gives evidence in support of my contention that to arrive at a differential diagnosis is extremely difficult. Indeed, although the lecture is titled as one on differential diagnosis, I fail to see anything in support of his theme. They are also, in tone, very undeferential to the recognised methods of treating obstruction. These are the lecturer's rules for differential diagnosis of typhlitis from internal strangulation :—

"Under these circumstances, I think I am justified in adding that, as a rule, the diagnosis of peritonitis, the result of typhlitis or peri-typhlitis, ought not to be difficult, and that these cases should not be confused with those of intestinal strangulation; that the only symptoms in common between the two classes of cases are sudden acute abdominal pain and vomiting; and that, whereas in intestinal strangulation these symptoms come on usually in a patient who has been hitherto perfectly well, in cases of typhlitis, on the other hand, there will either be a history of local trouble, or other symptoms to point to it. In typhlitis, whether acute or chronic, the pain will almost always be on the right side of the umbilicus, and, in some cases, will pass down the right thigh in the course of the anterior crural nurve; whilst, in some, the flexor muscles of the thigh will be involved, and extension rendered painful, if not impossible."

This I know, from actual observation,* is an utterly unreliable rule. A case of typhlitis may have no simulation of hip disease, and, if there be ulceration, may go on to perforation, without a single symptom of any disease existing, leakage only inducing signs of interrupted health and simulating strangulation.†

With Mr. Bryant's conclusion, given in the first lecture, I agree—

"There should be no delay allowed for the formation of a specific diagnosis of its cause."

In intestinal difficulty as the principles appertaining to the medical treatment of the disease is not affected by any differentiation we might make.

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* See Part I. Intestinal Obstruction, page 254, case 33.

† The symptoms of single or double hip flexion observed in diseases of the colon may, perhaps, be caused by sympathetic nerve stimulation; the cramp of the lower extremities, noticed in cholera, may also be so caused.

In none of these three lectures is there any information by which we can be guided in coming to a differential diagnosis. But they do contain evidence of failure to specially diagnose the exact cause. Up to this date, no information upon which reliance can be placed, has been forthcoming to supply this want. I have always maintained that a differential diagnosis is not essential to correct treatment. In case lviii., the lecturer made an error in his special diagnosis, but it led to no error in practice; because an operation was as applicable to the condition he discovered as to the one he believed he would find. The error we really have to guard against is that of surgical interference with cases that are in progress to resolution, aided by medicine; hence the importance of taking all the signs of the ailment into consideration, and not trusting to one symptom.

Mr. Bryant, in his lectures, lays much stress upon vomiting, as forecasting much evil; but this act is conservative in its intent. Mr. Treves attaches much importance to collapse as portending extreme danger. Of all the symptoms observed in obstruction collapse certainly is the most serious. A general practitioner, like myself, would like to know what are the signs observed in conjunction with vomiting which indicate that the vomiting must fail in its object.

In intestinal obstruction, until relief occurs, the tendency of the constitutional state is to glide from malaise, to pain, shock, and finally collapse—a sign of extreme urgency. Mr. Bryant would, perhaps, write over this "rather late, perhaps

too late" now to operate. From what we know of operations for hernia there is only a small chance of success once collapse has set in. Is it then advisable to operate in cases of obstruction if collapse has set in? Mere collapse is no justification for employing surgical interference. There are two characters of collapse, as I have before alluded to; that with harmony of pulse and temperature; pulse slow and feeble, temperature below normal, the other signs being present but moderated.

This is a phase of collapse to which medicine is applicable, but not for surgical interference. But when a discord is noticed between the pulse and temperature, low temperature, quick thready pulse, surgery ought to be tried. The "Surgeons of Hope" are very probable of being disappointed in their "hope" in the presence of the last form of collapse, if they do not apply their art. The "Physicians of Hope" are equally certain of disappointment if they think that any remedy, stimulant or sedative, will ultimately save life. The only effect the first can have is to prolong life for a short time, and the last remedy can only shorten it.*

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* In my address at the Medical Institute, I recapitulated to the members the rules I had previously laid down as indicating that surgical interference ought to be tried. I am sorry to have to confess that, on two occasions, these rules were not adhered to by myself, and I operated in a case of what appeared to be a hernia, with acute strangulation. The patient said that the hernia had always been reducible. Taxis failing, I operated, and found only adherent omentum, firmly filling up the abdominal opening. It was not interferred with and no harm followed. On another occasion, when vomiting was persistent, the hernia, previously reducible, could not be returned by taxis. This case was seen with me by Mr. R. Parker. Both of us advised surgical interference, believing that the signs of urgency would soon appear. The patient's friends, however, objected, and in six days after the patient was out of bed, no action of the bowels occurring for some days after. These two mistakes arose

During the use of opiates, or any sedative, in the treatment of intestinal obstruction, serious evils may follow if we do not carefully keep avoidable excrementitious matter from being introduced into the gut, or if their use is persisted in when their all-round beneficial effect is not apparent.

I ought not to terminate this contribution to the question—Of what is the fundamental principle which should guide us in the treatment of Intestinal Diseases?—without expressing my opinion that the profession, and myself in particular, ought to feel well satisfied with the well meant act of Messrs. Banks,

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from my not attending to the lesson of my experience. Here, however, is an instance in which I did advise, basing my counsel on my own rules:—

Some years ago, at the request of a medical friend, I visited a public hospital to examine a case of obstruction; collapse was just commencing. He, a physician, agreed with me that an operation was required, the characteristic symptoms of urgency being all present. The surgeons of the institution, however, decided otherwise. The patient died, and the *post-mortem* revealed strangulation by a band.

On another occasion, being invited to assist with a case where there was much constitutional disturbances from what appeared like an hæmatocele, the tumour was aspirated, and it totally collapsed after removing six ounces of blood. The odour of the liquid removed convinced me then that a hernia existed. A third consultant was called in and we agreed that a hernia was improbable, as there was no physical evidence. However, as all the signs showing that medicine was not beneficially controlling the disease, I operated and found the hernia. The division of the construction not improving the symptoms, gastro-enterotomy was performed. This also availed not, and the patient died, when a volvulus was found.

The last case in which I performed gastro-enterotomy, was that of a patient, residing in Roscoe-lane, who commenced to suffer so suddenly and acutely, and that while in bed, that I suspected either a volvulus, strangulation, or intussusception. Medical treatment never had any hoped for effect beyond moderating the signs; no change in their character, as the abdomen was resonant all over. I tried repeated trocaring, but instead of gas escaping, a small measure of liquid only flowed. This showed me that the procedure was not advisable. On the the fifth day I operated, and from the gastro-enteric opening there discharged fully a quart of purulent stercoraceous pus. The patient died next day, and a *post-mortem* revealed diffused phlegmanous enteritis, with separate portions of the intestine gangrenous, some portions being many inches in length. No pus in the peritoneum.

Treves, and Bryant, in giving us without reserve their own theoretical opinions and thus enabling us to get a closer estimate of what the recognised treatment really is. Several of our leading medical periodicals have noticed my contributions to this subject, and I think have very fairly estimated my service in the direction of the reform of the treatment of Intestinal ailments. Objection has been made to the tone of many of my contributions. While admitting that my voice has been often pitched above the key of ordinary conversation, I would like to remind my critics that, as "one of the crowd," "speaking out" is necessary to me, not having a vantage ground to address from. Labouring under disadvantages, my desire to be heard and hope of being understood account for my intemperance of language, and ought to be allowed an excuse for it. Now, I may ask, when have I applied such a sweeping condemnation to the recognised treatment as Dr. Bryant and Dr. Alexander have done? Perhaps some of my readers will say, that the last gentleman's language was not supported by evidence—

"Death would occur in many healthful persons who had no obstruction of their abdominal contents when treated as the descriptions show they had been treated."

But if the reader will take the trouble to consult the *Lancet*, issued October 2, 1876, or the Clinical Society Transactions, he will find there reported a case of functional obstruction in a female which for forty-nine days was subject to recognised treatment, and that the treatment was not thought improper is proved

by the discussion which followed. This patient, with all the signs of a strong tendency to recovery, died on the forty-ninth day, after having had WE KNOW nine medicated enemata, some of them being medicated with castor oil and turpentine. From the report it may be gathered that, inclusive of nutrient enemata, the number of enemata given were nearer twenty-seven than nine; further, she was injected, inverted, and shaken twice, kneaded once, powerful purgatives thrice are recorded as having been given, with opium and a neurotic stimulant, ice, calomel, aloes, podophylin, and twice she was galvanised. If those who contend that my teaching is not a late one will take the trouble to study this case in the original report, noting who occupied the chair of the society, and joined in the discussion, they can have no difficulty in coming to a conclusion as to what the "recognised treatment" really is. At this meeting of the Clinical Society, leaders in medicine and surgery, both in the metropolis and provinces, joined in the debate which followed the report of the case—a case evidently destined to terminate fatally by repeated recurrence of the difficulty, if she had tided over the first attack. But there is not one healthy subject out of a thousand that could have lived forty-nine days if his intestines were subjected to such a persistent application of our recognised treatment as was done in this instance. The total recorded symptoms observed show incontestibly that the patient had exceptional vitality and tendency to recover.

It is reported she died "unexpectedly," this is indeed the correct word to describe the fatal occurrence—"unexpectedly."

Why? Because up to the forty-eighth day there had not appeared any sign or combination of signs showing extreme urgency. Then what was the cause of death? certainly not the obstruction.

The pathological condition observed at the *post-mortem* showed only functional defects leading up to partial obstruction, which the recognised treatment had "grafted" into an absolute one, and supplemented pathological conditions not connected with the original cause of the initial symptoms.

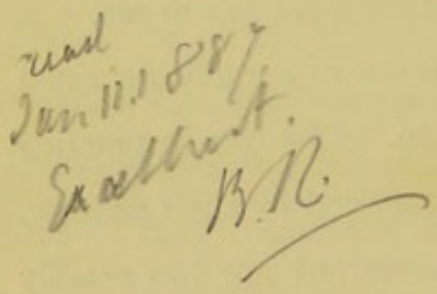
A few months after this case was placed before the president and members of the Clinical Society, I made my public protest against the recognised treatment which I am now informed is only the Brintonian. After I had been in the field some years Mr. Bryant joined me, but only to hamper my efforts, by drawing attention in a wrong direction, relegating these cases more to the surgeons, whereas there is more reformation required, and obvious need of it, in the medical treatment than there is in the other department of our art.

To every new contribution to medicine or surgery we must expect and there ought to be opposition. Changes in practice should not be accepted merely recommended by their novelty. It is my opinion that immatured innovations are too readily added, admitted with a "short record," and consequently have soon to be substracted. Upon this opinion I have acted, and never ventured to urge my views upon others during the first fifteen years of my professional life. The physiological method of treating intestinal ailments, as I

have "elaborated," is presented to the profession with a good private record of twenty-eight years, and collegians of to-day are invited to test my teaching, and if it be not such an addition to our art that it places the man of no experience nearer to the level of the man of many years, then my teaching makes known no new principles, nor can it improve the treatment of the diseases discussed in the foregoing pages.

"To know if any theory or position be true or rational in practice, the method is to carry it to its greatest extent; if it be not true upon the whole or be absurd, it is so in all its parts, however small."

That my suggestions in the direction of the reformation of the treatment of these diseases is about to be fairly tested, the signs of the times indicate. For "now" around me I notice a desire of several of my contemporaries to assist in bringing about a change they see to be inevitable, though they do not so plainly perceive what its exact nature should be.



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