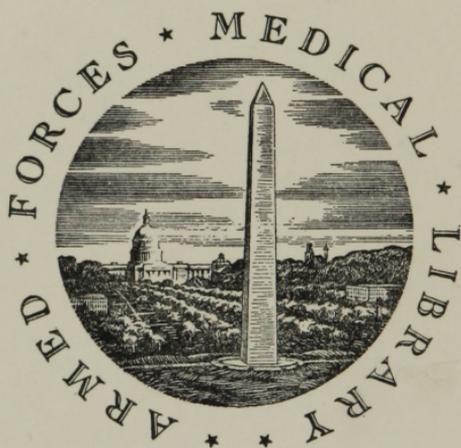




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*Gibson*

*Box*

**STRICTURES**

ON

**“MR. PATTISON'S REPLY**

TO

**CERTAIN ORAL AND WRITTEN CRITICISMS.”**

BY

**W. GIBSON.**

**PHILADELPHIA:**  
**PRINTED BY JAMES MAXWELL.**

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STRICTURES

OR

"MR. PATTON'S REPLY

TO

"CERTAIN ORAL AND WRITTEN CRITICISMS"

BY

W. GIBSON.

To,

Dr. Jackson

with the highest  
respect of the  
Author.

## STRICTURES, &c.

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“Novelty captivates the superficial and thoughtless; vehemence delights the discontented and turbulent. He that contradicts acknowledged truth, will always have an audience; he that vilifies established authority will always find abettors.”—*Johnson*.

Mr. Granville Sharp Pattison has assailed my private and professional reputation, in terms peculiarly rude and disrespectful. I am well aware that I have *ample scope for recrimination*; but, while I have too much regard for public opinion to indulge in remarks inconsistent with decorum, I hope to show by incontrovertible evidence, that every thing I have asserted in relation to this individual and his claims, is substantially true, and that my conduct, from beginning to end, has been upright and honourable.

Having had occasion to visit Philadelphia both before and after my election to the Surgical chair, in the University of Pennsylvania, I there heard from every quarter that Mr. Pattison had avowed himself the author of a most brilliant *discovery* in anatomy, that several distinguished physicians in this country, as well as some of the first anatomists and surgeons

in Europe, were not only satisfied with the equity of his claim, but convinced that highly beneficial consequences must result from the important change which would necessarily take place in the mode of performing one of the most difficult and dangerous operations in surgery. It was natural that I should feel a lively interest in the subject, believing, as I then did, that such reports were well founded; but, I had no opportunity of ascertaining the *precise nature of the alleged discovery* or the *peculiar* practical inferences deduced from it, until after the publication of an essay on Lithotomy, in the ninth number of the Medical Recorder. This essay I read with great attention, but I was grievously disappointed, for instead of finding any thing like a discovery, or any important directions for conducting the operation, I only met with an imperfect and awkward description of a structure, about which most of the best anatomists had said more or less, and views which no one but an inexperienced surgeon would for a moment have entertained.

About a fortnight before I delivered my lectures on Lithotomy, I obtained at the book store of Collins and Croft the "*Surgical Anatomy*" of Mr. Colles of Dublin, an anatomist and surgeon with whose reputation I had long been familiar, and who for the last fifteen years has ranked as the very first in his profession in Ireland. In looking over the anatomy of the perinæum I found that Mr. Colles had given an *unusually* full and *well written* account of those very parts which Mr. Pattison had *claimed* in his essay as *his own discovery*. In conversation with Drs. Physick, R. Patterson, and Horner, at the University, I mentioned these circumstances, and showed them the work. After comparing the descriptions of Mr. Colles' with dissections made by Dr. Horner, on the spot, they all declared, without hesitation, their thorough conviction that Mr. Pattison was not only not entitled in the slightest degree to the *discovery* of any one

part about the perinæum, but that Mr. Colles's account of the different structures was far more lucid and intelligible than that of Mr. Pattison, or of any other writer they had seen. Dr. Physick then remarked, that he had gone through the work of Mr. Colles slightly, some months before, and had stopped Dr. Dewees in the street, purposely to say that he believed Mr. Colles had described in a very clear manner, the "*prostate fascia*" which Mr. Pattison had claimed as his own discovery, but as the weather was then too warm to make an examination on the dead subject, he would not give a *decided* opinion until the ensuing winter. Dr. Physick added, that he made the same observations to Drs. Chapman and M'Clellan, and in confirmation of his opinion, had placed the work of Mr. Colles in the hands of the latter gentleman, in order that he might show it to Mr. Pattison.

I commenced my lecture on lithotomy, by observing that it was necessary I should give a general view of the parts directly and indirectly concerned in the operation. For this purpose, I exhibited to the class some magnified and uncommonly beautiful drawings, by Mr. Otis; some preparations in wax, and dissections of the real subject. Among other things, I took occasion to say that Mr. Colles, of Dublin, had described in a very circumstantial and perspicuous way, certain structures about the bladder and perinæum to which I called the attention of the class, by reading several extracts from his book. I then gave an account of the different operations of lithotomy, which I conceived to be deserving of attention from the earliest periods down to modern times. I inculcated in particular, a free incision of the prostate gland, and if necessary, of the neck of the bladder, and referred in a pointed manner repeatedly to the last and most approved operation of Cheselden in support of my opinion; I referred also to the authority of Mr. John Bell, and several other distinguished European surgeons, and to the ample experience of Dr. Physick; all of which tended to establish the position

which my own experience had taught me to be true. In exhibiting the operation on the subject, I remarked that it was necessary to employ in the living body, a gorget of *moderate size*, not from any apprehension or danger of making too large an incision in the *prostate*, but to avoid wounding the *pubic artery*, and pointed out in a particular manner, the necessity of afterwards enlarging the wound in the prostate and neck of the bladder, *by the bistoury*, in case the stone should be too large to come away without it. I mentioned at the same time, the difficulty of dividing *completely* the prostate gland, even by a gorget of considerable size, referred to an experiment made by Dr. Physick in the presence of his class a few days before, in support of the same sentiment, and concluded by passing a gorget of unusual magnitude, *merely to show* how large an instrument might be employed *without the entire division* of the gland being the consequence.

Mr. Pattison attended *my* lecture *without invitation*, and *immediately after*, pasted on the walls of our university an address to *my* pupils, requesting them at a stated period, to listen to a refutation of what I had advanced.

Soon after my observations on lithotomy were delivered in the university, a sort of quack bill or circumforaneous advertisement, emanating apparently from a Norfolk paper, was published successively in most of the Philadelphia newspapers, stating that Mr. Granville Sharp Pattison had written in the Medical Recorder, "*without doubt the most important paper, on the subject of lithotomy, that had appeared in any country, for a considerable time past.*" The object of this advertisement, was at once easily understood by most of the professional men, and its source readily guessed at by others.

There was no difficulty in perceiving, both from previous indications, and from the style of this particular panegyric, that the common European practice of wafting one's self along *by puff*, was about to be *introduced* into this country. It

was evident, moreover, that the article alluded to was intended to do away any impression which might have been created by my lecture respecting the validity of Mr. Pattison's claims as a *discoverer, or the importance of his practical deductions*.\* I therefore requested Mr. Poulson to insert a short notice, in which it was mentioned, that the praise lavished on Mr. Pattison, was not altogether merited. My name was left at the same time, to be delivered to any one who should choose to call for it. An answer appeared to my slight admonition, written at the earnest solicitation of *Mr. John Pattison*, tending to support the claims of his brother, and protesting against newspaper attacks. I was thus placed under the necessity to publish a rejoinder.

It soon became apparent, that neither Mr. Pattison nor his friends were satisfied with the course the affair had taken, for they let no opportunity slip to stigmatize me as an *anonymous writer*, although *perfectly acquainted*, to use their own words, with my "*individuality*," which indeed they could not help knowing, as I never myself, aimed at keeping it secret. It was insinuated moreover, that I had made *false and garbled* extracts from Mr. Pattison's essay, in the newspaper discussion. To get rid of this imputation, and to convince my brethren who had no opportunity of reading Mr. Colles' work and judging for themselves, I determined to bring the matter before them in a formal way, and accordingly published a review of the *anatomical part* of Mr. Pattison's essay, under the signature of W. in the tenth number of the Recorder.

This is before the profession, and requires no comment. But it will be necessary to reprint the articles which appeared in Mr. Poulson's paper, in order that the public may ascer-

\* I had always inculcated in my lectures, principles adverse to those advocated by *Scarpa* and by Mr. Pattison in imitation of him. If my observations then applied indirectly to Mr. Pattison, it was surely not my fault, but his misfortune.

tain how far Mr. Pattison's assertions *with respect to matters of fact* can be depended on.

MR. POULSON,

In your paper of this morning, I observe an article extracted from the Norfolk Beacon, in which it is affirmed, that the paper published in the last number of Mr. Webster's valuable Medical Journal, by Mr. G. S. Pattison, of this city, "is without doubt, the most important on the subject of lithotomy, that has appeared in any country for a considerable time past." *Without derogating in the slightest degree, from the merits of Mr. Pattison, which are respectable,* I have no hesitation in declaring, that the paper in question, contains neither an *Anatomical discovery*, nor *important practical precept*; as any one disposed to investigate the subject will find, by turning over the Surgical Anatomy of Mr. Colles, of Dublin, published so far back as the year 1811, the Surgical Journal of Mr. Charles Bell, and several other anatomical and surgical works. I have great pleasure in bearing testimony to the general excellence of Mr. Webster's *Medical Recorder*, which is circulated extensively throughout the United States, and is unquestionably the most valuable periodical work of the kind, in our country. ARISTIDES.

"*In nostros fabricata est machina muros.*"

MR. POULSON,

I am very sensible that nothing short of the interests of humanity can authorise the discussion of a medical subject, in a newspaper, and if the friends of Mr. Pattison had entertained similar sentiments, it is probable that no investigation of his claim to certain *discoveries* would have appeared before the public through such a medium.—As the affair now stands, however, it may be deemed necessary to substantiate the assertions which have been made, viz: "That the paper of Mr. P. contains neither a *discovery* nor *important practical precept.*"

It is notorious that Mr. Pattison came to this city during the last summer, and proclaimed that he had *discovered* a new fascia, and that some of the *first* anatomists of Europe had *acknowledged* his claim. But as this may be called "a mere dictum of an anonymous writer," Mr. P. will excuse the liberty I take of making him tell his own story. In page the 9th of the Medical Recorder, Mr. P. says, "I dissected the parts after a new method, one which enabled me to discover a new fascia." Again, he says, "This new fascia I have named the fascia of the prostate gland."

In page 22d, Mr. P. says, "I have demonstrated it to some of the first anatomists of Europe, and they have been perfectly convinced of its reality, and have believed it a *discovery*. I have since demonstrated it before Drs. Physick, Eberle and M'Clellan, who have all, without hesitation, acknowledged its *existence*. I have since demonstrated it before Drs. Parrish and Hartshorne, who have expressed equally strongly the conviction of *its presence*."

"One of my friends was rather *unfortunate* in the discovery, he *thought* he had made, that this fascia had been described by Mr. Collies (Colles) of Dublin, as the passage in that author, in which he conceived the prostate fascia, was mentioned, referred to another—a fascia *universally* known and demonstrated by anatomists. But although in the passage alluded to, Mr. Collies (Colles) says nothing of the fascia, still I believe from an *attentive* perusal of the work, that that anatomist had seen it. *He*, however, *only* saw it, and was neither aware of its *connections*, its *importance*, or (nor) its *uses*."

Here then we have, in the same paper—first, the *discovery* of a fascia—secondly, the *existence* or *presence* of a fascia,—and thirdly, *no* discovery of a fascia. We might, in the next place, go on to show that Mr. Colles not only *saw* the "fascia of the prostate gland," but has given a more minute, clear

and satisfactory account of it than even the *discoverer* himself, so much so, that it would be quite impossible, owing to the extent of the information contained in his work, on the subject, to introduce any extracts in this place; we refer those, however, who are anxious for *proofs*, to the work itself.

With respect to the *precept* deduced from the *discovery*, it may be stated, that the practice advocated by Mr. P. is in direct *opposition* to that of most of the *best* authorities of Europe and of *this* country. It would appear too, from Mr. P's. own acknowledgment, that his *experience* in lithotomy has not been very considerable, since we hear of *two* operations *only* which he has performed, and from one of them, the patient died, although contrary to the expectations of his friends.—“As I left the apartment with my esteemed friend Dr. King, he shook me by the hand and said, *Pattison* it is impossible this patient can die.”

I conclude by re-asserting, *that I have no inclination to detract from the merits of Mr. Pattison*, whatever they may be, but his friend X. should be admonished to send no *puffs* to the Norfolk Beacon, in order that they may find their way progressively through our country. The *American* public will not be imposed on with impunity. ARISTIDES.

I shall next make some remarks on the “*Reply to certain oral and written criticisms*,” published in the eleventh number of the Recorder.

Notwithstanding the gentle admonitions which Mr. Pattison had already received respecting his *discoveries*, we find him still very unwilling to abandon his pretensions; for in the very outset he observes, “In a conversation which I had with Dr. Physick shortly after my arrival in America, I took occasion to mention the prostate fascia as a *discovery* and attempted to explain, from its connexions, the causes of urinal infiltration.” *Discovery*, is in fact, the perpetual burthen of his song, often repeated and always uppermost. *Omnia cum*

fecerit: Thaida Thais olet. In giving an account, however, of the discovery of Mr. Colles' book, he would appear to be not very accurate in *his assertion* that an individual, meaning, *it is said*, Dr. Horner, "brought that work to Dr. Physick;" at least I am authorised by Dr. Physick to say, that no one except the Hospital librarian ever put the work into his hands, and then not until he called for it of his own accord.

Again, it is asserted that a leaf was turned down in the work of Mr. Colles, in order to designate the parts to which Mr. Pattison had laid claim, but that the marked passages referred altogether to other parts. This assertion will perhaps turn out to be no better than the first, for it seems that neither Dr. Physick nor Dr. Horner remembers that a leaf was *turned down at all*. There is no difficulty in perceiving the conclusion which Mr. Pattison wished to be drawn *from his modest insinuation*, that Dr. Physick could not distinguish the difference between those parts "regularly and constantly described by anatomists," and *his discovery*, when he found *so little* difficulty in convincing his friend Dr. M'Clellan they were distinct.

"Accompanied by Dr. M'Clellan," says Mr. Pattison, "I visited Dr. Physick with the book, and with the *most perfect candour* assured him that I was satisfied Mr. Colles *had seen* the fascia, and then went over with him that gentleman's description. Dr. Physick's observation was, that it *was so confused, it was impossible* to understand exactly what the author meant, and added most unequivocally, that his having or not having seen it, would in no measure take from the honour *which was due to me*, for being the *first* who had brought it before the profession in a highly interesting and important practical point of view." Unfortunately Dr. Physick *does not recollect* that he ever made any such observations, and thinks it *not very probable* he could have called those passages in the work of Mr. Colles *confused* which he

had previously told Drs. Dewees, Chapman, and M'Clellan were very clear. But I am quite sure from what I know of Dr. Physick's views in relation to Mr. Pattison's *discoveries* and *practical conclusions*, that he never could have intended to convey the impression to him, or to any one else, that they were entitled to that sort of regard which Mr. Pattison has chosen to attach to them, and labours to make the public believe.

Mr. Pattison asserts that Dr. Physick, regulated by a love of truth, when he came to the anatomy of the perinaeum, *demonstrated* the "prostate fascia." This is very true, and perhaps more so than any other part of his whole essay, but it does not follow that he demonstrated Mr. Pattison to be the *discoverer* of the fascia, which it is surprising, considering his former good luck, he did not also assert. The fact is, Dr. Physick attached so little importance to the fascia, that he merely showed the part, and read a passage or two from Mr. Pattison's paper, without a single comment.

The next part of the "*reply*," relates to the "*anonymous letters of Aristides*." These are now before the public, who will judge whether they are *really so infamous and ungentlemanly* as Mr. Pattison has chosen to represent.

That Mr. Pattison was the author of the panegyric which appeared originally in the Norfolk Beacon, and subsequently in many of the papers of the middle and southern states, I cannot pretend to assert, for there are ways in which *he* could manage such an affair, so as to remove the odium of it from his own shoulders. I can only say, I verily believed him to be the author, and have yet had no good reason to change my opinion, on the subject, and that the whole tenor of his conduct, from his arrival in the country to the present time, has been *empirical in the extreme*, and such as would justify me, or any one else, in concluding he was at the bottom of the Norfolk advertisement, and of *all the numerous puffs* which have appeared at different times in his favour. The profession

in general and many of the citizens of Philadelphia, will bear testimony to the truth of what I have advanced.

Notwithstanding Mr. Pattison's extraordinary unwillingness to "have his name brought forward, favourably or unfavourably, by anonymous writers," so much so that he declares "that in the whole course of his life he never published a syllable to which he did not affix his signature;" yet it appears that he has no particular objection his *friends* should take this trouble off his hands, by standing forward in his defence while he remains behind the curtain. He will hardly venture to deny that it was *at the particular request of his brother*, that the answer to Aristides was written.

He may not find it very easy, to convince any one, not devoid of common sense, that it is an outrage upon decency and good fellowship, to publish a nameless paragraph in Mr. Poulson's, or any other newspaper, especially when it is known as an answer to one manufactured by himself or some of his friends, and for the express purpose of sounding his own praise.

In my review of Mr. Pattison's essay, I endeavoured to show, and trust I succeeded in proving, that he could not, with the slightest propriety, lay claim to the *discovery* of the "*prostate fascia*," which he had attempted to do in the most unequivocal manner. But whether I succeeded in convincing the profession or not, it is very evident from his subsequent *declarations* in "his reply," that my arguments and proofs were not altogether lost upon him, since we now find him shifting his ground with admirable dexterity, and asserting "that *he had not claimed the fascia as a discovery*." Notwithstanding his changeable nature, he cannot escape so easily as he imagines, if any credit is due to his own previous assertions, and to the unqualified testimony of some of the most respectable professional characters of this city.

But there is yet another view of the subject, the bearing of which He has not perhaps taken into full consideration.

I presume that he cannot deny that it is stated in his *reply* "I still continued to believe it was so (i. e. a *discovery*) until I read Mr. Colles' book in *Philadelphia*." "I would demand if there was any thing disingenuous in my conduct after I had read Mr. Colles' essay." Now it is certainly implied in these passages, that he *had never read* Mr. Colles' work until he came to this country. But this he had probably so far forgotten as to state in his reply to my lecture that he had been acquainted with the work in the *city of Glasgow*.

This acquaintance with Mr. Colles' book, which it *now* seems commenced in Scotland, was what I had long suspected, but which nothing except the unfortunate explosion which took place at my lecture during the winter, would probably ever have brought to light. As it is, we must give him credit for his "*most perfect candour*:" he would have been a little more candid, however, if he had only taken the trouble to *acknowledge* the same in his former essay and in his late reply. But such an acknowledgment, he well knew, would only lead him into other difficulties, from which he may now extricate himself as well as he can.

It is quite amusing, to observe how this individual, after having been driven from one post to another, and forced to confess that *he was not the discoverer* of the "*prostate fascia*," attempts to escape, by taking to himself the *merit* of the *discovery*, as he calls it, that Mr. Colles had "*really seen the fascia*." "His book," says he, "had been pryed into by those, who were most anxious to take from me the honour of the *discovery*, but they were unable to understand that he had really seen the fascia. It was left *for me* to make that *discovery*, and when I did so, I was *the first* to proclaim it to Drs. Physick, Eberle and M'Clellan." This is as much as to say, that although *he* obtained the work of Mr. Colles from Dr. Physick, through Dr. M'Clellan, that neither Dr. Physick nor Dr. Horner, had penetration or sagacity enough to

perceive, that it contained the *discovery* of Mr. Pattison. This is a fine specimen of his usual good luck and consistency, and corresponds very accurately with his declarations in his first essay, and with certain passages in his reply, to this effect, "That excellent and enlightened surgeon, Dr. Physick, a man who by his talents and professional enthusiasm, has deservedly raised himself to the very highest pinnacle of surgical eminence," &c. Again, "Dr. Physick, professor of anatomy in one of the first schools in the United States, a man who deservedly stands at the very head of his profession." But he might as well have spared himself the trouble of proclaiming Dr. Physick's culpable ignorance of the "*prostate fascia*," or his *uncommon eminence* in his profession, since no one would ever think of taking his opinion of either.

It certainly cannot be considered any thing less than sorry logic, to affirm that the *anatomists of Europe* did not know any thing about the *existence* of the prostate fascia, until his "demonstrations and publications," when he had already been obliged to confess, however reluctantly, that *Mr. Colles had at least seen it*. His assertion, that Dr. Physick was not acquainted with the structures about the perinæum, which he chose to call *his discoveries*, has no better foundation. But Dr. Physick could never dream that Mr. Pattison or any one else would venture to appropriate to himself the *anterior inferior ligaments of the bladder*, and the *membranous expansion covering the surface of the obturator internus*, and *levator ani muscles*, and took it for granted that inasmuch as a *discovery was claimed*, the merit of a discovery was *deserved*, until he had an opportunity of examining the parts, and satisfying himself to the contrary. His declaration that I was unacquainted with what all the best anatomists had described, is another sample of his precious good luck. But he judged rightly perhaps, not to attempt to *prove* my ignorance, since his bare assertion would no doubt be received, in the estimation of all who had ever

looked into his writings, or heard any thing of him, be equal to any *proof* he could possibly bring.

But how does it happen, since "*this important discovery*" was made *before his arrival* in this country, and known to the European anatomists *eighteen months before the publication of Mr. Charles Bell's Quarterly Reports*, that he should have so far indulged in languid supineness, as to forget the honour of his native home, and reserve the publication of the "*most important paper on lithotomy, which has ever appeared in any age,*" for a country like America, which he and some of his friends, if report say true, *were* disposed to consider as the Bœotia of the world? what documents moreover can he show to substantiate the *assertion* he has made, that some of the first anatomists of Europe have acknowledged his claim?

Having asserted in my review of the *anatomical part* of Mr. Pattison's essay that the "*prostate fascia*" which he supposed to be a *discovery*, was well known to most if not to all of the best anatomists long before the appearance of Mr. Colles' work, or the paper of Mr. Pattison: I shall proceed in the next place to offer unquestionable evidence of the accuracy of the observation.

*Santorini* in speaking of what he calls "*ligamentum prostatæ novum*" says *Igitur ex interiore pubis facie, qua ea ossa committuntur, latum circiter pollicem supra imam eorundem partem; geminæ, utrinque altera, ligamentosæ productiones emergunt; quæ interdum diductæ, interdum latæ in priorem prostatæ faciem ac in proximum ani levatoris tendinem inseruntur: unde utrinque provectæ ad obtusum angulum, in summa ejusdem prostatæ facie ac vesicæ, vicinia concurrentes, firmissime cohærent. Si quis autem de istius ligamenti natura quod antenus dubitaret; tum illius albedinem, et firmitatem, tum fibrarum nexum, et positum exploret sedulo, ut quæ proposuimus pernoscat. Quantum vero commodi ex eo homini habendum sit, is facile intelliget, qui quantum hasce*

partes colligari, firmarique intersit, plane noverit. Duplici id potissimum commodo, præter tertium aliud, quod mox sumus allaturi constructum esse censemus. Ne recto scilicet intestino alligata prostata, in vehementiori excernendarum fæcum nisu, suis e sedibus divellatur: atque id utrique sexui commune est, quandoque et utrique prospicit ligamentum. In fœminis vero, in quibus et urethræ et vaginæ inseritur; ne in diuturnis interdum, validisque edendorum fætuum, contentionibus, laxatæ nimium dimotæque partes incommode aliquando prolaberentur."

"Porro quod circa prostatam observatu dignum reputo, atque ad rem nostram paulo infra explicandam faciens, est firmum expansumque involucrum; quod ad propriam intimioremque circumfusam prostatæ membranam is nihil spectare intelliget, qui eo circumquaque diligenter exsecto, integram prorsus, atque suis circumdatam tunicis nihilominus compariet: Quod si rem accuratius persequatur, eo etiam seminales vesiculas arcte concludi deprehendet. Ad ejus constructionem non nerveas dumtaxat, valentesque fibras cæterarum membranarum in modum compingi, observavimus; sed frequentes interjectos lacertulos vidimus, quo ejusdem involucris compages, quando nihil ad contractionem conferant, firmaretur amplius. In cujus quidem priore facie, qua pectinis ossibus jungitur, non exiguos muscularium fibrarum fasciculos in lacerationis præsertim cadaveribus intextos observasse meminimus: quarum duplex incessus erat. Aliæ etenim lata superne basi in prostatæ imum contrahebantur, ac velut inversum pyramidem exhibebant; aliæ vero quasi transversim ductæ subjectas fere ad decussim secabant."

"*Observationes Anatomicæ Jo. Dominici Santorini.*"  
"Lugduni Batavorum." 1739.

It still remains to be observed, that on each side of the bottom of the pelvis in both sexes, opposite to the lower part of the bladder, there is an aponeurotic or tendinous ligament which runs over the inner surface of the musculus obtur-

*ator internus* from before backwards. The anterior extremity of this ligament is fixed on one side of the middle portion of the symphysis of the ossa pubis, and the posterior extremity to the middle part of the ligamentum sacro sciaticum."

"A little above the elongation called the neck of the bladder *there is another ligamentary expansion*, on each side of the bladder; the fore part of which is narrow and fixed to the anterior extremity of the ligament already mentioned; and the broad posterior part to the side of the bladder. These two lateral expansions may be looked upon as proper ligaments of the bladder, by which it is connected to the inner side of both ossa pubis."

*Winslow's anatomical exposition of the human body. London, 1776.*

"Besides these, the bladder has two other ligaments which are fixed at their forepart to the upper and inner side of the ossa pubis, near the symphysis of these two bones; from whence they run back, becoming gradually broader to be fixed to the sides of the bladder."

*Monro's System of Anatomy, Edinburgh, 1807.*

"The firmest connexion is by means of a *ligamentous expansion which runs from each side of the neck of the bladder and prostate gland*, to be fixed to the inside of the arch of the ossa pubis. It is connected also at this place to the penis, by the urethra. It is composed of different coats joined together by allular substance, the first of which is only a partial one continued from the peritoneum."

*Fyfes' compendium of anatomy, Edinburgh, 1807.*

La vessie est retenue dans le lieu quelle occupe par plusieurs ligamens, dont deux sont inferieurs et anterieurs, un moyen et posterior, et un quatrieme superieur. Les premier sortent de la partie interne and superieure des pubis pres la jonction de ces os. Ils sont etroits and rapproches l'un de l'autre en devant; mais ils s'elargissent et s'ecartent

en arriere, pour se jeter sur les parties laterales d'un corps glanduleux qui embrasse le col de la vessie et que l'on nomme la prostate. La prostate sert d'attache a la plus grande partie des fibres musculuses de la vessie; elle est soutenue par les ligamens anterieurs de cette poche, lesquels de la partie posterieure et interne de chacun des os pubis, vout se porter sur ses parties laterales."

*Traité complet D'anatomie par M. Sabatier, Paris, 1791.*

"Par des ligaments anterieurs qui attachent la vessie a la partie interne et superieur de l'arcade du pubis, lesquels ligaments apres avoir donnè attache a quelques fibres musculaires de la vessie, se terminent sur les parties laterales de la prostate. Les parties laterales de la prostate donnent attache aux ligamens anterieur de la vessie et ne sont pas eloignées du muscle releveur de l'anus."

"Le prostate est soutenue par des trousseaux ligamenteux qui sont attaches a la partie posterieure et externe des os pubis."

*Cours d'anatomie medicale par Antoine Portal. Paris, 1803.*

"Du bas de cette region ou voit naitre un petit faisceau fibreux aplati de haut en bas, qui de la se porte horizontalement au dessous et derrierè la symphyse pubienne a laquelle il se fixe sous le nom de ligament anterieur de la vessie. Il est presque immediatement appliqué sur le gland prostate." Bichat p. 140.

Speaking of the prostate gland, the author remarks

"Sa surface supérieure qui regarde un peu en avant, est immediatement recouverte par l'expansion fibreuse appelle ligament inferieur de la vessie. La face opposeé est unie au rectum par un tissu cellulaire extremement dense et serré, surtout vers le sommet: ce dernier rapport de la prostate est d'autant plus etendue que le rectum est plus dilaté par les

excremens; de la l'indispensable nécessité d'évacuer cet intestin chez les sujets sur lesquels on sa pratiquer la taille."

Anatomie descriptive par X. Bichat,

Tom, 5. p. 205.

Paris, 1803.

"La Region anterieure, legerement incliné en bas est borné en haut par l'ouraue, et en bas par le col de la vessie. On remarque à sa partie la plus inferieure *deux petits faisceaux fibreuse, appelés ligamens anterieurs de la vessie*. Ils ont une direction horizontale et se fixent d'une part a la partie posterieure de la symphise du pubis et de l'autre a la partie superieure du col de la vessie ou il sont appliqués sur la glande prostate."

Traité complet d'Anatomie,

par M. Le Baron Boyer,

Paris, 1815.

Du bas de cette region on voit naitre un *petit faisceau fibreux*, déprimé, qui se porte horizontalement derriere la symphyse des pubis, a laquelle il s'implante, et que l'on appelle *ligament anterieur de la vessie*. Il est immédiatement appliqué sur la prostate."

Traité D' Anatomie Descriptive

par J. H. Cloquet.

Paris, 1816.

After these extracts from some of the best Scotch and Continental anatomists it may be deemed proper to insert the description of the "Prostate Fascia," given by Mr. Pattison in his "observations on lithotomy."

"The prostate fascia when superficially looked at," says he, "would appear to take its origin from the inner margins of the rami of the os ischium and pubis. If we, however, come to examine it more attentively, we may remark, that, although, it has here a connexion with the bone, that here it does not terminate, but that in fact *it is continuous with the*

*aponeurosis which covers the obturator internus muscle.* The best description, perhaps, which can be given of it, would be to state, that the fascia which covers the *internal obturator muscle*, having reached the rami of the ischium and pubis, forms there a connexion with the inner margin of these processes; and that from this it runs and is at last lost by becoming inserted into the basis of the prostate and into the rectum where lying below, and exterior to the gland. From this description the two following important facts are to be observed:

1st. That the prostate fascia separates the perinæum from the cavity of the pelvis.

2nd. That from the manner, in which the fascia passes from the rami of the ischium and pubis to its insertion, that a triangular space must be formed betwixt its pelvic aspect and the shoulder of the bladder; the apex of the triangle being formed by the union of the fascia with the basis of the gland and the base by the lateral boundary of the pelvis."

Mr. Pattison with his accustomed prevarication will doubtless deny the applicability of the passages quoted to the "*prostate fascia*." A very moderate acquaintance however with anatomy will enable any one to determine that the structure alluded to, differs from his "*discovery*" in nothing so much as in the name, and that even this is not very far removed from the appellation employed by Santorini and others to designate the same thing. But I have charity enough to suppose that Mr. Pattison as a private teacher of anatomy in Glasgow, was unaccustomed to peruse such works as Winslow, Monro, Fyfe, Sabatier, Portal, Bichat, Boyer, and never heard of their contents until his arrival in America, where he first ascertained through Dr. Physick that Mr. Colles "*had seen the prostate fascia*."

I shall go on the next place to show, although I think there can be very little necessity for so doing, so far at least as intelligent and experienced lithotomists are concerned, that

Mr. Pattison's *practical deductions* founded on the basis of his alleged *discovery*, if not incorrect or useless, are certainly gratuitous.

It is commonly believed that the celebrated Rau of Amsterdam, by observing the operations of Frere Jacques, and by dissecting his patients after death, was enabled to ascertain a mode of extracting the stone attended with unparalleled success, so much so that out of 1540 patients upon whom he operated, it is said every one recovered, leaving thereby no opportunity for investigating *the parts actually cut*. Governed by the most sordid motives Rau was induced to conceal the nature of his operation from the public, and even from his private pupils, who were unable to determine with certainty whether the prostate gland and neck of the bladder were alone divided or whether these parts were left untouched and an opening made into the body of the bladder itself. Albinus, the favourite pupil and assistant of Rau, entertained the latter opinion, and ventured to publish an account of the operation. This attracted the particular attention of the profession; and the distinguished Mr. Cheselden of London was the first to put the plan in execution, by distending the bladder with an injection and opening it between the tuber ischii and vesiculæ seminales without injuring its neck or the prostate gland. The result, however, was unfavourable, for he lost four out of ten patients in consequence of urinal infiltration, and the operation was therefore abandoned. Mr. Cheselden next turned his attention to the division of *the prostate gland and neck of the bladder*, which he accomplished by pushing a knife through the membranous part of the urethra, and carrying it backwards and outwards with its edge directed towards the tuberosity of the ischium. By following this practice he was eminently successful, but he found that the rectum was liable to be wounded from the edge of the knife being necessarily turned towards it; and in

consequence of this circumstance, was induced to change not the principle of his operation but the mode of executing it. Accordingly *in his third and most approved operation*, instead of cutting, in the first place, the membranous portion of the urethra, he made a free dissection of the parts adjoining the prostate gland and neck of the bladder, and then plunging his knife into the body of the viscus immediately above the gland, he drew it downwards towards him, and divided the prostate as far as the urethra. In pursuing this method he succeeded in saving fifty patients out of fifty-two, and out of two hundred and thirteen operations performed within a period of twenty years, it is said, only twenty were lost, making about two in every twenty-one.

I have given this short history of the lateral operation, to point out the principles by which Cheselden was governed, inasmuch as his second or third operations are practised by all modern surgeons of eminence at the present day. But since Mr. Pattison has ventured to affirm that it is "not consistent with truth" to declare that in his third operation he did ~~not~~ divide the prostate and neck of the bladder, I must proceed a little further, and show that such an assertion is altogether without foundation.

"The internal wound," says Dr. Douglass, "is through the bladder, prostate gland and urethra."

1st. "The vesica urinaria covered with the membrana cellularis, is cut in two places, viz: first a small portion of it a little above the prostate gland, on the left side, where he enters the knife, first into the groove of his staff, and then part of the bladder which lies round the orifice upon the upper part of that gland."

2dly. "The substance of one half of the prostate gland is likewise divided laterally from without, inwards in the direction of the urethra that lies within it, through the whole length of that part of the canal."

3dly. "The iter urinæ, or canal of the urethra is divided in two places, and both laterally. First, the beginning of it, which runs through the substance of the prostate lengthwise, at the same time the incision is made through it and the urethra into the groove of the staff."

"The next is the membranous part of the urethra with the circular muscle that surrounds it, beginning at the inferior apex of the prostate, and ending a little beyond the hole in the *septum tendineum*, under the pendulous part of the bulb."

"In England," says Mr. Allan Burns, "Cheselden improved the operation of lithotomy, but not until repeated failure had taught him the necessity of *making a free division of the prostate gland and neck of the bladder.*"

But it would be useless to quote authority on the subject, inasmuch as the testimony in favour of Mr. Cheselden's particular mode of operating is universal, and as well established as any thing else in surgery. Notwithstanding this, Mr. Pattison has brought forward some extracts from Mr. Sharp's *Treatise on the operations*, in proof of ~~his~~ *supposition*, that Cheselden did not divide the neck of the bladder. It is very true, that Sharp recommends "*the whole length of the gland to be cut, from within outwards;*" but it does not follow that Mr. Pattison's logical conclusion, that *the base of the gland* must therefore remain uncut, is altogether correct. I presume it will be admitted that the base of the prostate forms a part of its length, and ergo that if Mr. Sharp cut the *whole length*, he must also have cut the *base*. Mr. Sharp's declaration, that a dilatation is necessary both in the Marian and Lateral operations, would seem to establish not the conclusion which Mr. Pattison has drawn from it, viz: "if the base of the prostate and shoulder of the bladder were divided, there could be *no occasion* for laceration," but rather the reverse, that a free division of

the prostate and neck of the bladder was sometimes necessary, and if not made, *that laceration would be the consequence.* I am well convinced from reading Mr. Sharp's chapter on the stone, in the work referred to, that although he does not say in direct terms as Douglass and others do, that the *neck* of the bladder must be divided, he took it for granted the surgeon would conclude, inasmuch as the knife was to be entered at the base of the gland, that the neck of the bladder must *unavoidably* be cut, and in corroboration of this sentiment, I extract the following passage from his "*Critical Enquiry*," where speaking of the form of the knife that should be used in lithotomy, he says "besides that the back being blunt is a security against wounding the rectum, when we cut *the neck* of the bladder from below upwards."

Mr. Pattison conceives it is proved that Cheselden did not divide the neck of the bladder, "from the difficulties opposed to the operation." What these difficulties are, he does not venture to say. If he means that the neck of the bladder is too remote to be reached by the knife, he will recollect that Mr. Cheselden *in his first operation*, opened the body of the bladder as he says, "*very readily*;" and since the bladder itself is deeper seated than the prostate gland, it follows it cannot be so easily cut; but if cut, that its neck and the prostate may also and with greater facility.

It has been insinuated by Mr. Pattison, that although Cheselden may have intended possibly to cut the neck of the bladder, yet he never accomplished it except in a few cases, and "that the fatal cases were the ones where this occurred." This is an admirable specimen of his "*sound logic and pure philosophy*;" but he will not find it very easy perhaps to make other people *suppose* that Cheselden, the greatest anatomist and surgeon of the age in which he lived, was so ignorant of his own operation as not to cut the parts he

*intended* to cut, especially as he must have had some opportunities, notwithstanding his great success of examining them after death. Any one who has read Scarpa's Memoir, will readily perceive where Mr. Pattison obtained the idea of the difficulty of cutting the neck of the bladder behind the prostate. "But, in fact, it is not so easy, says Scarpa, as some not sufficiently acquainted with this operation might perhaps imagine, to conduct a knife through the neck of the urethra, beyond the orifice of the bladder," &c. It is plain, however, from what soon follows, that Scarpa never intended to insinuate that Mr. Cheselden found any difficulty, for, says he, "To facilitate to intelligent surgeons, but not so dexterous as Cheselden, the performance of the lateral incision was the laudable motive which led Hawkins to propose his cutting gorget." It remained then for Mr. Pattison to make this discovery, but as he has admitted that if Mr. Cheselden did in *reality* execute his own operation, "*the very root of his reasonings must be torn up,*" there can be no necessity I hope for following him any further, since every one must be fully prepared to acknowledge by this time, that the neck of the bladder and the whole prostate were completely divided in Mr. Cheselden's *second* and *third* operations.

Mr. Pattison, in speaking of Cheselden's operations as a strong and unanswerable objection to his maxims, remarks, "So satisfied do I feel of the justness of this observation, that I am unwilling to take leave of my critical friends without assisting them, with at least one observation, which certainly carries with it a much more powerful inference against my maxims, than any of those puerile objections with which their minds have furnished them." It certainly required no uncommon penetration to discover, that if Cheselden, by making an extensive incision of the prostate and neck of the bladder was enabled to save 193 patients out of 213, that his operation must stand as an objection to a contrary mode of proceeding, which had not been subjected to the

test of the same extensive experience. This Mr. Pattison might easily have inferred from what he heard in my lecture, or from a perusal of more than one passage in the dictionary of his favourite author, Mr. S. Cooper, to this effect: "Every practitioner who will take the trouble to look over the history of the lateral operation will find, that such lithotomists as have particularly distinguished themselves by their unparalleled success, as Frere Jacques, Cheselden, Cosme, &c. all made a free incision into the bladder. This fact alone, is enough to raise doubts of the goodness of the advice delivered upon this subject by Callisen and Scarpa, especially, as neither they nor any other modern surgeon, can boast of having cut patients for the stone with a degree of success, at all equal to that of the above mentioned operators."

I have already had occasion to state, that the operation of Rau was unknown to his contemporaries, and was so carefully concealed by him, that even his own assistant, the celebrated Albinus, an excellent anatomist, and most expert surgeon, could not discover his particular mode. Mr. Pattison is also aware it appears, "that Rau made a secret of the parts he divided," but he is so well pleased with Rau's success in saving 1500 patients, and with the declarations of those who saw him operate, and the principles of the operation correspond so exactly with *his own*, that he is induced to believe that Rau must have operated precisely after *his method*. If he means, as I presume he does, that *Sabatier* was one of those "who saw Rau operate," I may observe that, according to historians, Rau was born in the year 1668, and died in 1719; and that *Sabatier* was born just about the time of his death, and died in France within the last ten or twelve years. It is natural to conclude therefore, that he had no more opportunity of seeing Rau operate, than the fetus in utero had, and must have been fully as well qualified to describe his particular mode, which it seems *Albinus*

could not comprehend, although an eye witness to the whole scene.

It is stated, that “Foubert *invented* or rather *revived* the practice of Franco *and introduced a gum catheter into the bladder.*” It is not easy to perceive the impression intended to be conveyed by the *particular* and unnecessary mention of such an instrument. I may therefore observe, that Dr. Physick has for many years employed a *gum catheter* after the operation for lithotomy, and that Mr. Pattison wishes to insinuate he was not the first to put the plan in execution. Although it is well known that Franco, the Collots, Foubert, and several of the old lithotomists employed *metallic canulae*, yet the *gum elastic catheter* was invented by Theden in 1782, while the memoir of Foubert was published in 1743, in which he says, “cet accident sur lequel j’avois deja beaucoup reflechi quand je pratiquois le grand appareil, reveilla d’avantage mon attention, lorsque je reconnus qu’il avoit ete la cause de la mort de quelques malades tailles selon ma methode et je pensai que je pourrois le prevenir en placant dans la plaie *une canule* pour entretenir autant de temps qu’il seroit necessaire.” In a note Foubert adds, “Quoique les canules *d’argent* ou le *plomb* puissent servir en pareil cas, je prefere cependant celles *d’argent* qui sont flexibles que je couvre de linge fort doux et use.” Thus it appears from Foubert’s own words, that he used a *flexible silver catheter*, and not a *gum catheter* as Mr. Pattison has asserted. I have mentioned these circumstances, to show that Dr. Physick was, in all probability, the first who used a *gum catheter* to draw the urine from the bladder after the operation of lithotomy;—and to expose Mr. Pattison’s disingenuous attempt to take that merit from him, which is especially due, as the instruments used by the ancients, and by Foubert, must have increased the very difficulties they were intended to remove; as any one will be able to determine by examination, or

by referring to the plates in which they are represented in the old writers.\*

Mr. Pattison has remarked "one of the assertions, delivered by Dr. Gibson in his lecture was, *that no gorget was made sufficiently large to cut the whole body or base of the prostate gland.*" *I never made an assertion of the kind.* The extent of my observation, was, that the prostate gland was not so easily divided, as some imagined; that in fact a gorget from three quarters of an inch, to an inch in breadth, might be employed in most full grown subjects, without producing such an effect. I did not suffer the observation to rest upon my assertion, for I performed the operation in presence of the class, with a gorget, upwards of an inch in the blade, merely to put the matter to the test, and upon examination of the parts after lecture, while several of the gentlemen were looking on, it was distinctly seen that the gland *was not divided by the eighth of an inch.*

But the gratuitous assertion as it is called, did not rest upon my authority *alone.* Dr. Physick demonstrated to his class, by actual experiment, that a gorget much larger than the one usually employed in the operation on the living body, would not divide the prostate, and Dr. Horner repeated the experiment so frequently in the practical rooms, as to convince the most sceptical inquirer, and in fact to leave very little doubt on the subject, as the following statement will show.

\* Dr. Physick has never asserted that he first employed a *gum catheter*, after the operation of lithotomy, although not aware of its having been used by any one before him for such purpose. I believe that the profession is *entirely* indebted to him for its introduction, as I have not been able, after the most diligent research, to find it noticed, except in the tenth volume of the *Edinburgh Journal*, published in 1814; where its use is *theoretically* proposed by a Mr. Bythell who says, "*among the many eminent writers on lithotomy, I cannot find such a practice mentioned.*"

*Philadelphia, July, 28, 1820.*

DEAR SIR,

Being much interested last winter in the discussion which took place concerning the injury done to the prostate gland by passing a common gorget, and hearing Dr. Physick say that this body was seldom entirely divided, I performed five operations with a gorget, the blade of which was three quarters of an inch in breadth, with a view of ascertaining the fact. In none of these experiments was the side of the prostate gland divided; on the contrary, upwards of one eighth of an inch remained uncut.

I also found that the membranous expansion described by most of the old as well as the late anatomists, is connected to the bladder so high up, that an incision may be made through the whole side of the prostate, and into the lower fundus of the bladder, without dividing the membrane itself. If this arrangement holds in all subjects, it appears to me, that the fears entertained by Mr. Pattison, of cutting this membrane (even allowing it capable of resisting urinal infiltration) are groundless.

I am prepared, at all times, to demonstrate by dissection, to persons whose opinions are unsettled on the subject, the correctness of the statements I have advanced, and in corroboration of my assertions, I appeal to the numerous class of medical gentlemen, who attended my winter and spring courses of practical anatomy, on whose minds I believe there can be little doubt, at this moment. I remain with much respect

Your obedient Servant,

W. E. HORNER,  
Demonstrator of Anatomy.

Dr. W. GIBSON.

In addition to the experiments made by Dr. Physick, Dr. Horner, and myself, during the winter at the University, I may state that I have performed the operation on the dead body under different circumstances repeatedly since the

publication of Mr. Pattison's reply, and that in one instance only, has the base of the gland been cut through as the following circumstances will show.

On Sunday, the 30th of July, 1820, in the presence of Drs. Colhoun, Edward Barton, and Lawrence; I divided the prostate gland in two subjects. In one I used a gorget of three quarters of an inch in the blade, and the gland was not divided by upwards of a quarter of an inch. In the other, I employed a gorget of a full inch without the division of the gland by nearly the eighth of an inch. In both instances the "*prostate fascia*" was untouched, and was connected to the bladder, by its posterior attachment so far back that it could not have been divided, except by a very extensive incision, and was not injured even when the gland was afterwards cut through by the knife. Upon measuring the divided part of the gland in both subjects at its base, from its external boundary to the edge of the urethra, it was upwards of eight in one, and fully nine lines in breadth in the other. The breadth of the urethra, also exceeded in both subjects, nine lines, making in the whole, including the urethra and gland, more than eighteen lines in one, and nearly the same in the other subject.

Thursday, August the 3d, 1820, Drs. Horner, Harlan, Wilstack, and Lawrence being present, I opened the membranous part of the urethra with a knife, and introduced a gorget, an inch in breadth into the staff, and divided the prostate gland, in two different subjects. In neither instance was the gland cut through by the eighth of an inch, nor was the fascia within the reach of a gorget considerably wider in the blade, than the one I used. In both cases the divided glands and urethra, measured each nine lines at their base, making together eighteen lines.

On the 26th of August, 1820, I performed the operation of Lithotomy, with a gorget exactly thirteen lines wide in the blade. Drs. Harlan, Lawrence, and three intelligent

medical students; Messrs. Wilson, Cuthbert, and Stoddart, were present. A small portion of the prostate, adjoining the urethra, where it opens into the bladder, was found undivided. The rest of the gland, together with a part of the neck of the bladder *was cut through*. "*The prostate fascia*" was not touched, and it appeared to every one that the incision might have been carried to the extent of three quarters of an inch further into the side of the bladder, without wounding it. The gland in this subject was uncommonly small, measuring only *six lines*. This was probably owing to the extreme emaciation of the body, the patient having laboured for several years under Pthisis Pulmonalis.\*

By all these results, I am strongly fortified in the position advanced in my lecture, that a gorget of three quarters of an inch to an inch in breadth, will not completely divide the prostate gland in most full grown subjects, and have therefore a right to conclude that the *anatomical fact*, insisted upon by Mr. Pattison, *is no fact at all*. But upon whose authority does this "*anatomical fact*" rest, that the prostate gland, does not exceed seven lines in breadth; not surely upon the authority of Scarpa, for he expressly declares that "the size or thickness of the prostate as its apex, is a little more than two lines, four at its body and six or *even eight* at its base, where it surrounds the orifice of the bladder."

It rests then upon the authority alone, of Mr. Pattison, who, as far as I can ascertain from his essays, has never made

\* Since some of these experiments were made, I have perused with great pleasure an essay, published in the last number of the Recorder, by Dr. Jameson, of Baltimore, replete with sound observation and correct anatomical views. It is very evident from the account which Dr. Jameson has given, of the situation and connections of the "*prostate fascia*" that he must have been aware, it could not be cut even by the "*entire division*" of the prostate gland, but I do not find that he has drawn such a conclusion. At any rate he is entitled to full credit, and his remarks generally confirm in a pointed manner, the experiments of Dr. Horner and myself.

*a single experiment* to substantiate the assertion which he brings forward with so imposing an air.\* But even admitting his declaration to be true, that “the gland very rarely measures above *seven lines*,” does it follow that a gorget of greater breadth than the gland itself, must of necessity divide the gland completely? It certainly does not, and for the very best reason in the world, that a considerable portion of the blade next to the beak of the instrument passes through the urethra, and does not touch the gland at all. What this proportion is cannot perhaps be *exactly* ascertained, since it will, in some measure, depend upon the size of the staff and its groove, and upon the size of the beak of the gorget. But it is reasonable to conclude, since I have been able in the experiments detailed, to carry a gorget of an inch in breadth into the bladder, without dividing the gland entirely which was found in numerous instances to equal nine lines, that the cavity of the urethra must have exceeded at least three lines, otherwise the gland would have been cut to mathematical certainty. It is here then that Mr. Pattison has erred, and it is proper the mistake should rest with *him* and not with Scarpa, who allows that the urethra measures generally five lines. The assertion that *many gorgets* “are made of *fourteen lines* in breadth,” I will not pretend to dispute. I can only say, that I have never met with such, either in this country or in Europe, or in any book on lithotomy, ancient or modern.

But I shall proceed in the next place to show upon other authority than that of Dr. Physick, Dr. Horner and myself, that the prostate gland is not only not cut so easily as Mr. Pattison supposes, but that in fact the difficulty of dividing

\* The idea of measuring the extent of the prostate gland in subjects of different ages did not originate with Scarpa, for we find Deschamps in his “*Traite Historique de l'operation de la Taille*,” the best work on lithotomy in the French language, entering minutely into the investigation, and confirming in a particular manner my assertion, that the gland measures generally, in a full grown subject, upwards of nine lines.

it even by a gorget of large size, is considered as the strongest possible objection to the use of that instrument. "The gorget ought to be," says Mr. Charles Bell, "of that breadth in the sharp edge, that it may cut through the left side of the prostate gland. If the prostate gland and the stronger fibres of the sphincter vesicæ be completely cut, the wound of the bladder will enlarge to transmit the largest stone. But if the base of the gland be not *entirely cut through*, there will be a bridling and stricture on the forceps. As the gorget is uniformly of one size (and indeed cannot be made broader without endangering *the cutting of the pudic artery*, it has *the fault* of not entirely cutting through the prostate gland and neck of the bladder. In operating with the knife in place of the gorget, the incision can be adapted to circumstances."

Mr. *John Bell* in his "principles of surgery" speaking of the gorget says, "but there is one paramount objection independent of the many dangers which attend this push of the gorget; the instrument, guide it is you will, makes an incision *inadequate* to the easy extraction of the stone! I have *often* compared the incisions I have made with the knife and with the gorget upon the dead body. I have observed also, *in the time of operating*, how difficultly the opening of the prostate admits *even the forceps*, how impossible that such an imperfect incision should *easily allow* the extraction of the stone. In all cases of particular difficulty, where using the privilege of an assistant, I have introduced my finger, I have felt *distinctly the stricture of the gland, the greater part of it being left entire*. The incision of the gland often admits the forceps so difficultly, that I am well assured the gland itself has sometimes by the mere pushing of the forceps against this firm, and narrow opening been *entirely separated* from the urethra! and after the forceps are pushed successfully through this narrow opening, and the stone caught betwixt their blades, all that remains of the gland is *inevitably lacerated*

with much danger and pain. But I would more willingly quote any authority than that of my own dissections or experience. Camper who has studied the subject, says “Incredibile est quam parva plaga ab omnibus etiam dexterrimis infligatur; nunquam forcipem robustam exciperet nisi dilataretur. Hawkensius solo conductore, cujus margo dexter in aciem assurgit idem præstat; *omnes plagam dilatant ut calculum extrahant* dilaceretur igitur semper vesicæ ostium et prostata.”

“But higher and better authority remains behind. Dease was, if I judged rightly of his talents, a stern and rude surgeon, but perfect in all the theory and practice of his art; he was not very explicit in his communications with me, but from the manner and the movement of his hand in demonstrating to me, rather than from what he said, I conclude that he cut after the manner of Rau, making the incision with the right hand, while he held the staff with his left. “In all the trials,” says Dease, “that I have made with the gorget on the *dead subject*, I have *never* found the opening into the bladder sufficiently *large* for the extraction of a stone of middling size, without a *considerable laceration* of the parts. I have frequently taken *the largest sized gorget*, and *could not find* that in the adult subject, *I ever entirely* divided the prostate gland, if it was any way large; and in the operations that were performed here on the *living subject*, the extraction was painfully tedious, and effected with great difficulty, and in some cases not at all.”

Other authority might be quoted to the same amount, but I have said enough to convince any one that the prostate gland cannot be cut in most full grown subjects, by passing a gorget of three quarters, to an inch in breadth along the urethra;—that a comparatively small opening is made, even by an instrument of the largest size;—that the “*prostate fascia*” of Mr. Pattison cannot be divided, by cutting through the gland, and even prolonging the incision some distance

into the body of the bladder;—and that, therefore, its influence upon the operation of lithotomy must be inconsiderable, if not nugatory.

After these observations, I conceive it quite unnecessary to argue with Mr. Pattison about D'Alembert's proposition, "that there is no analogy between living matter, which is active, and dead matter which is inert," for according to his own principles, the opening made by the gorget in the prostate gland of the living body, should be larger than the same instrument could produce in the dead, which, even admitting to be true, we find not sufficiently large, according to Mr. John Bell, Mr. Dease and others, to effect a complete division of the gland, or to permit a stone of small size to pass through without difficulty.

But suppose that the principles I have inculcated are incorrect, and those advanced by Scarpa and *reechoed* by Mr. Pattison are true, what becomes of their position that urinal infiltration is a necessary consequence of the division of the neck of the bladder, when it is ascertained that the greater number, if not all, of the most celebrated lithotomists, from the time of Cheselden down to the present day, have made a free incision of the prostate and bladder, and yet have been able to save at least seven patients out of eight?

The most successful lithotomists, in this country and in Europe, are in the habit of removing from the bladder stones of uncommon, and even of enormous magnitude, which cannot be extracted without a very extensive division of the prostate and bladder, and the patients *generally recover*. Can any man at all acquainted with the subject, believe then that there is really so much danger as Scarpa and Mr. Pattison contend from dividing these parts? "Mr. Orford, aged 56," says Dease, "calculous for many years, resolved in 1773 to undergo the operation, which Mr. Morris performed with Mr. Daunts' instruments, and happily extracted a stone which weighed fifteen ounces and a half. The operation was not at-

tended with any alarming hemorrhage. The patient recovered in the course of two months, and after some time could retain his urine as well as ever."

We are told by Gooch, that Mr. Harmer, of Norwich, extracted a stone from a patient aged 48, which *weighed fifteen ounces*, and the man afterwards recovered. "It was found impracticable," says Gooch, "to extract the stone through a wound of the common size, which the operator had made, or to break it by the force of the forceps; therefore, at his desire, I divided the parts occasionally, as he continued a gentle extraction."

Klein, a celebrated German lithotomist, lately extracted a stone, by the lateral operation performed by the knife, weighing, without the fragments, *twenty-six ounces and thirty grains*.

Dr. Physick, some years ago, in Virginia, took from a boy of four years of age, a stone measuring an inch and three quarters in its smallest diameter, and his patient speedily recovered. I operated six or seven years ago upon a child of the same age, in the Baltimore hospital, and extracted a stone nearly as large, and he recovered in two weeks, without a bad symptom. I operated three or four years ago upon a gentleman at Washington, who had two stones in his bladder, each as large as a hen's egg of common size, to extract which I was under the necessity of dividing the prostate and neck of the bladder, to a considerable extent by the bistoury, after I had made an opening with a gorget of three-quarters of an inch; and who, notwithstanding, got well without any difficulty whatever. It will be said, perhaps, that these were uncommon cases, that under the same circumstances the patient would generally die. I appeal to the whole profession, whether the most successful lithotomists of ancient and modern times, have not inculcated a free incision of the prostate gland and neck of the bladder, and whether any surgeons of eminence, except Scarpa and Callisen, have advised a contrary practice. It is stated, I know, by Mr. Pattison, that Le

Cat, Mr. Astley Cooper, and Dupuytren of Paris, are in favour of small incisions; but have we any other than Mr. Pattison's assertion on the subject? to which I may perhaps with propriety oppose my own by declaring, that during my residence in London in 1809 and 1810, I had an opportunity of seeing Mr. Cooper operate for the stone two or three times, with a *beaked knife*, and chiefly with a view, as he said at that period in his lectures, of making an ample incision of the prostate and neck of the bladder. Have we not also the authority of several late surgical writers that Mr. Cooper, within the last five or six years, has employed the bistoure cachee of Frere Cosme, an instrument better calculated than any other to make a large opening into the gland and neck of the bladder? I have moreover the authority of Roux, a celebrated French surgeon, who in his "Relation D' un voyage fait a Londres en 1814," says, "Le seul chirurgien de Londres a qui je l' aie vu pratiquer, est M. Cooper: je la lui ai vu faire deux fois. Dans les deux cas, *le col de la vessie et la prostate*, furent incisés avec un gorgeret tranchant."

Of the practice of Dupuytren I cannot speak from experience; but I am informed by Drs. J. G. Nancrede and Price, two most intelligent and able Surgeons of this city, who have witnessed his operations for the stone, in a great many instances within a short time, that he invariably made extensive incisions of the prostate and bladder. I may add that my friend Dr. W. Howard, of Baltimore, has confirmed these statements. As to the authority of Le Cat, I do not conceive that it can weigh in favour, or against any mode of practice. "Le Cat's talents," says his biographer, "were lost to himself and the public; *he never became a distinguished operator*; it was his fate to continue through life, writing volumes *full of ingenious theories*, the pages of which were never soiled by any finger, and *to invent instruments*, still more ingenious, which were never sharpened by any

hand but *his own*: even in France where every novelty has its admirers, Le Cat found no one to praise or follow him. After a *turbulent life*, embittered by *perpetual warfare*, all his *brilliant expectations* ended in utter disappointment. His innumerable improvements had so accumulated, that his method of Lithotomy was such as no one would be at the trouble of learning or the habit of performing."

Independently of the difficulty of extracting a *stone of large size*, through a small wound of the prostate, is there no other objection to the practice? I would ask those operators, who for fear of wounding the pudic artery, or of cutting through the prostate gland, have used a very narrow gorget, whether they have been able *readily* to find the opening they have made, and whether they have not, during the operation, forced the forceps between the bladder and rectum, and made unavailing efforts to extract the stone? I need hardly ask the question, for I am convinced that every Surgeon who has performed the operation half a dozen times, must have encountered more or less resistance from the gland, even where he has employed a gorget of considerable size. It cannot be said that the gorget has slipped between the bladder and the gut, and left the gland undivided, as sometimes happens, because the flow of urine follows so speedily the thrust of the instrument, as to give the Surgeon convincing proof, that he has opened the bladder, and leaves him to wonder why his forceps or finger will not follow the same route. "In one unfortunate case," says Mr. John Bell, "the Surgeon groping thus on the outside of the bladder, actually grasped with the point of his forceps and twisted away the greater part of the prostate gland, which when he had thrown down among his feet was picked up by a professor of anatomy, who assisted at the operation; and when the gentleman died the parts were dissected out, and are preserved." If such is the difficulty of finding an opening, made into the prostate, or if the

forceps will not easily enter after it is once found, how can the Surgeon expect to remove a stone even of small dimensions, through such an opening, without lacerating the parts to a considerable extent, and without exposing the patient to danger and agony, a thousand times worse than the disease itself. It would be said by Pouteau, in answer to this, and perhaps by Scarpa, who has taken many of his notions from him, that the opening in the prostate, would dilate gradually, and expand like the neck of the uterus in proportion to the pain which the patient suffered, or the efforts of the operator. But does not the history of the art furnish a sufficient number of examples of patients mutilated, and destroyed, by the tortures of the Marian operation, to dissuade any modern Surgeon from the revival of so horrible a practice. And yet this is the very doctrine inculcated by Scarpa. "Therefore," says he, "in the present day the *lateralizing of the great apparatus*, is justly considered as the greatest degree of perfection to which the operation of Lithotomy, in the perineum can be brought." Any one who has encountered a large stone in the bladder, knows by experience that the prostate does not dilate in the manner we are taught by some to believe; he finds that the prostate is tough and unyielding, and resists the utmost efforts he can use, that his forceps bend under his exertions to extract the stone, that the cries of the patient bear testimony to the violence committed, and finally, when the stone is extracted, that it is by main force and by the *laceration* of parts which nothing less than the strength of a Cyclops could rend asunder. We are told by Mr. Pattison himself, that the "prostate gland is composed of a very tough substance, one which is not easy of division." How can it be expected then, that this gland is to dilate to the extent of eight lines, as Scarpa says, and permit a large stone to pass without difficulty.

There is another objection to a small incision of the prostate, independently of the pain which the patient suffers, from the passing of the stone;—the stone itself is liable to be broken by the pressure of the forceps, during the forcible attempts, to get it away. If such an accident should happen, the operator has necessarily to encounter a vast deal of trouble, and the patient is exposed to danger, from inflammation of the bladder, or, if he survives, to the formation of another stone, from fragments left behind.

But the great apprehension of Scarpa, and Mr. Pattison, relates to urinal infiltration. Let us see, therefore, whether the urine, which escapes very readily, after a large wound of the prostate and neck of the bladder, is really productive of those injurious and fatal effects which these writers are inclined to believe. I do not know the extent of Scarpa's *experience in lithotomy*, however high his reputation in some other departments of his profession, but I have reason to think that the stone in Italy, is not a very common complaint, and that his opportunities of operating have consequently been a good deal limited. Mr. Pattison's *experience is confined to two cases* in his own practice, and those in young children, and to three dissections in the practice of others.

Let us examine then the testimony of those who teach a doctrine adverse to the views of Scarpa and his *followers*, and in the first place, that of Mr. Samuel Cooper, "who, according to Mr. Pattison, is perhaps better acquainted with the writings of ancient and modern Surgeons, than any other in Britain," and see how far his notions of the dangers of urinal infiltration, correspond with their own. "With respect to the degree of importance," says he, "which ought to be attached to the fear of effusion of urine, between the bladder and rectum, gangrene fistulæ, &c. I can only say that they are inconveniences which are *not commonly observed* after lithot-

omy in this country. *In two or three instances only* I have known the urine come through the wound longer than usual, and these cases *ended well*. As for the extravasation of urine, and sloughing, I shall merely remark, that although there cannot be a doubt of their occasional occurrence, they have not taken place after any of the *numerous operations*, with the results of which I have been acquainted."

"All these facts and considerations, therefore, incline me to doubt whether the apprehension of the effusion of urine, fistulæ, &c. be sufficiently serious and well founded to make it advisable for surgeons to relinquish the plan of making a *complete division of the side of the prostate gland and neck of the bladder* in the operation of lithotomy. Nor is it at all clear to my mind, that effusion of urine and sloughing are likely to be the effect of practising a *free opening*. Indeed, whenever they do happen, I believe they proceed from a totally different cause, viz: from the incision in the skin and muscles being too small, and too high up, and from the axis of the internal parts of the incision, not corresponding with that of the external wound. Hence the urine does not readily find its way outwards, and some of it passes into the neighbouring cellular membrane." It is stated by Mr. Pattison, in his former essay, while speaking of urinal infiltration, that "Mr. John Bell is most decided in asserting that the inflammation which produces death, arises from urinal infiltration." "It is indeed," continues Mr. Pattison, "the generally received opinion." In answer to this I may state, that I have not been able to find, after a patient investigation of Mr. Bell's work, a single expression which would induce me to draw such a conclusion. His observations respecting urinal infiltration, are exceedingly limited, and when he does mention it at all, it is not in reference to Cheselden's *last* operation, but to his *first*, which he and every one acknowledges to have proved fatal, *chiefly* on that account, for reasons which I shall presently endeavour to explain. If any

positive inference can be drawn, respecting Mr. Bell's opinion from his own writings, as to the most *common cause* of death, after the operation, I am disposed to think that he considers it owing to loss of blood, either from a wound of the pudic artery, or of the deeper seated parts, in consequence of which the cellular tissue is injected with blood; or to violence done the bladder and prostate, in getting out the stone through a small opening. Mr. Allan of Edinburgh, the friend and assistant of Mr. Bell, expressly says, in his Treatise on Lithotomy, "the two great dangers to which the patient is exposed, and which *chiefly* claim the attention of the surgeon, after the operation of lithotomy, are *hemorrhagy* and *abdominal inflammation*." The effusion of urine is not referred to by Mr. Allan as the cause of death, and is only spoken of as occasionally productive of *excoriation*.

That urinal infiltration *is not generally* considered, as Mr. Pattison asserts, as the cause of the inflammation which produces death, I may mention that very few, either of the English or French surgeons speak of it, as a consequence of the lateral operation as usually performed by the modern surgeon; on the contrary, when spoken of, it is in relation to the *High operation*, the operation of *Foubert*, of *Thomas*, and to *Cheselden's first operation*, as may be ascertained by any one who will take the trouble to look over the works of Cheselden, Douglass, Sharp, Daunt, Dease, Sir James Earle, John Bell, Dr. Thompson, Allan Burns, Charles Bell, Allan, Dionis, Le Dran, Dechamps, Sabatier, Boyer, Bertrandi, Desault, all of whom recommend a free incision of the prostate gland and neck of the bladder, with a gorget, knife, or bistoury. It is well known that almost all, if not the whole, of the best surgeons in England, Scotland and Ireland, follow the same practice, and generally with a happy result. In proof of this, I may mention the names of Hunter, Cline, Carlisle, Lynn, Abernethy, Home, S. Cooper, Lawrence, Blick, Blizzard, Pearson, Benjamin Gibson, Hey, Brown, Newbigging, Rus-

sel, Inglis, George Bell, Colles, Barlow, Foster, Chevalier, &c. In Italy, Nannoni and Flajani, who have as much reputation as Scarpa, always make large incisions. In France, Pelletan, Percy, Richerand, Patrise, Lallemand, Roux, Cloquet, Les Frank, Serrurier, Merat, Guillè, Le Roux, Becclard, Delpech, Nicod, Beauchene, &c., follow the same practice. In Germany, Klein and Langenbeck, two surgeons of the first eminence, both of whom have written copiously on lithotomy, say nothing of the dangers of urinal infiltration, although they have constantly made extensive incisions of the prostate and neck of the bladder. "In a work of more recent date," says Mr. S. Cooper, in the second volume of his first lines of the practice of surgery, "Klein lays down as the basis of his method, the necessity of always dividing not only the *prostate gland completely through*, but also a portion of the bladder itself. Upon this basis (says he) rests the success of my operations, and hence I invariably make it a rule rather to make the *incision too large than too small*, and never to dilate it with any blunt instrument when it happens to be too diminutive, but to enlarge it with a knife introduced, if necessary, several times." Again, "in proof of the success of his method, he tells us that, in 1816, he had cut into the bladder *seventy-nine times*, (for he almost always cut more or less of that viscus) and not one patient had died; unless when the prostate, bladder, kidneys or ureters were diseased. Notwithstanding the *free division of the bladder*, most of the patients *got well* in from eight to fourteen days; a few in a month; and *one alone* was three months in recovering. Though the *sphincter of the bladder was divided*, no *paralysis* of it was the result."

Deschamps, in speaking of suppuration of the bladder, says "L'inflammation dont la vessie est frappée, est quelquefois si vive qu'elle fait périr le malade tres promptement; *Il est rare* alors qu'on observe des *abcès à l'ouverture des cadavres*." Again, in speaking of gangrene the same author

remarks, "J'en ai cherché dans les vessies de plusieurs calculeux morts dans les trois premiers jours, à la suite de l'inflammation, et je n'en ai point trouvé: une seule vessie m'a présenté trois points véritablement gangreneux; mais ayant été témoin de l'opération, je les ai attribués au choc des instrumens qui avoient violemment contu ce viscère."

If urinal infiltration was so common a cause of death after the operation of lithotomy, by the lateral operation, as now performed by the gorget and knife, ought we not to have some mention of the circumstance?—instead of which almost every writer I have met with, has passed it by as if no such consequence had ever ensued. That it cannot be of so much importance as has been attached to it *even in the High operation*, we have the authority of Sabatier, who in his essay, published in Fourcroy's "*Medecine Eclairée*," says "La collection de pièces sur la taille au haut appareil, publié en 1730 par M. Morand, à l'occasion d'une opération de cette espèce qu'il venoit de pratiquer, presente plusieurs exemples de guérison malgré la blessure du péritoine, et celui-ci ajoute a la certitude qui en resulte, que les épanchemens d'une quantité mediocre d'urine dans le ventre, ne sont pas mortels." Morand, in speaking of the high operation as performed by Bernier, states that thirty-five out of forty patients recovered.

Bromfield, in operating for the stone upon a boy between six and seven years of age, thrust his gorget through the fundus of the bladder among the intestines, which, descending through the opening to the external wound, were with difficulty kept from the grasp of the forceps. "Contrary to expectation," says he, "the child had a very good night, and was perfectly well in a little more than a fortnight, without one alarming symptom during the process of cure; neither did the intestines ever once descend through the ruptured peritoneum after they had been returned when the operation was finished."

Dr. Willison, in the Edinburgh essays, says in the beginning of March 1735, "a smith pushed a red hot iron with such force into the buttock, an inch and a half from the anus, of a young man of twenty years of age, that the point of it came out through the *linea alba*, about an inch above the *ossa pubis*, having pierced through the pelvis. He had violent bilious vomitings, from time to time, great pains in his belly, thirst, watchings, cold sweats and faintings. He passed no urine until thirty hours after the wound. In six weeks he was cured."

Larrey, also, in his Surgical Memoirs, relates several cases in which the bladder and rectum were penetrated by musket balls, where the patient recovered, notwithstanding urinal infiltration to a great extent, was the necessary result. If urinal infiltration is really so dangerous as represented, how does it happen that so few inconveniences followed the extraction of stones from females through the vagina and fundus of the bladder, as practised by Hildanus, Mery, and others, the success of whose operation has given rise to the observation of Dupuytren, that wounds of the *bas fond* of the bladder are not so dangerous as has been supposed. Qu'on rapproche des faits, observes par Fabrice, par Rousset et Tolet, l' idee emise par Mery et renouvelée depuis par Louis, de pratiquer chez les femmes la lithotomie par le vagin; et si l'on n'est pas persuade qu'on doit preferer cette methode a celles que nous avons, exposees plus haut, on sera du moins convaincu les plaices qui etablissent une communication entre le vagin et le corps de la vessie, ne sont pas incurables comme on l'a pense, et comme peutetre on le pense generalement encore." Lithotomie. p. 55.

If effusion of urine is necessarily attended with such dreadful consequences as Mr. Pattison conceives, what is to become of those patients operated on by Sanson, who removes the stone by cutting the prostate gland and bladder, through the rectum? But volumes of examples might be cited, if

necessary, all of which tend to subvert the positions advanced by Scarpa, and subscribed to by Mr. Pattison.

When Cheselden opened the bladder above the prostate in *his first operation*, he was so unsuccessful as to be obliged to abandon his mode on account of effusion of urine. "My patients," says he, "for some days after the operation seemed out of danger, but the urine which came out of the bladder, continually lodging upon the cellular membrane on the *outside of the rectum*, made fœtid ulcers, attended with a vast discharge of stinking matter; and from this cause, I lost four patients out of ten." Mr. Pattison, in quoting Camper, *through the medium of Scarpa*, makes it appear that Cheselden lost *eight* out of ten patients by following this method. But the result was not so unfavourable it will be seen, as Camper has chosen to represent, as *four* instead of *eight* out of ten were lost. Even this is a convincing proof that urinal infiltration is not so hazardous as we have been led to believe, since the *greater* number recovered. The ill success of Cheselden, however, is satisfactorily explained by Sharp, in the following manner. In order to make the opening above the prostate with greater facility, he distended the bladder to the utmost by an injection; so soon as the instrument entered, the fluid was discharged, and the bladder suddenly retired in such a way as to place the opening so far back in the pelvis, as not to correspond with the axis of the external wound, and the urine of necessity accumulated in the cellular membrane for want of a free egress. If the prostate gland and the urethra had been divided along with the bladder, would such consequences have taken place? I think not; and for the following reasons; when an opening is made through all these parts, the urine will continue to pass off in a small stream or guttatim, as fast as it is secreted, without ever accumulating in such quantity as to fill the cellular membrane at the upper part of the wound. On the contrary, if the prostate and urethra remain entire, it is very

evident that it cannot escape in this gradual manner, but must collect in greater or less quantity. When the bladder contracts for its expulsion, it is forced through a wound which does not correspond with the external one, and effusion is the necessary result. If these principles are correct, it must follow that a large and free incision of the perinæum, of the urethra, prostate gland, and neck of the bladder will permit the urine to pass out with comparative freedom, but if the incision in any of those parts be confined, and especially if there be not a complete *debridement* of the gland, then the urine will be detained, and by creating more or less irritation may, in some instances, give rise to sloughing of the parts. That a more extensive sloughing would be the consequence of the lodgment of urine above the neck of the bladder, than would happen from its infiltration below the neck I can easily conceive, but that the division of the neck of the bladder, and of the "*prostate fascia*" will contribute to such an effect so long as the urethra and prostate are at the same time freely divided, I cannot believe. I would ask if it was not upon *the principles* inculcated, that Frere Cosme was enabled to succeed, in saving an immense number of patients after the High operation, by making an opening in the perinæum, introducing a catheter, and suffering the urine constantly to dribble away by a depending outlet? Is not the success of Dr. Souberbielle of Paris, (represented by Carpue to exceed that of any modern operator,) owing to the same circumstance; and would not Cheselden have met with similar success in his *first operation* if he had opened the urethra and gland, and permitted the urine to flow continually through the wound or through a catheter? The result of his *second* and *third* operations amply demonstrate the truth of the proposition.

But, I would ask what circumstance can possibly give rise to effusion of urine behind the fascia, before or after its division, and that of the whole gland?

Is it a fair experiment to prove the danger of urinal infiltration, and to show that the cellular membrane about the bladder is capable of restraining the course of the urine, to pour water into the cavity of the pelvis, and ask if it will run out without the division of the fascia? Does not the experiment prove, in fact, directly the reverse; that as soon as the fascia is cut, the water flows in a full stream out of the opening in the perinæum? If Mr. Pattison had filled the bladder with water, and then cut the prostate and neck of the bladder and fascia, his class would have had a full and very satisfactory demonstration of the fallacy of his principles, as the urine would have escaped *pleno rivo*, and could not possibly have been detained by the fascia, or any similar structure.

After repeated perusal of the memoir of Scarpa, I do not find that he has brought forward a single case to substantiate his assertion, that "*gangrenous suppuration*" takes place after the operation by the gorget or knife, or after any other operation, and have a right to conclude that he is not overstocked with materials for establishing a general practice. As I have not the work of *Callisen* before me, I cannot speak of its particular contents. Only one case is adduced by *Mr. Pattison* as having occurred *in his own practice* where the patient died, not from peritoneal inflammation, but from the collection, *he supposes*, of *two drachms of pus* "betwixt the bas fond of the bladder, and vesiculæ seminales." In *three* cases where he had an opportunity of making dissections of patients, operated upon by some of his "*brethren*," the symptoms, says he, which preceded death in all of these, bore a considerable resemblance to those which had occurred in my own case, and *in all, pus was found betwixt the bladder and the rectum.*

I would ask those surgeons who have witnessed the effusion of urine into the cellular membrane of the scrotum, or into the cavity of the abdomen, from wounds or from sloughing of the fundus of the bladder, from over distension

of its coats, if they have ever discovered any *purulent matter* upon dissection? They will answer, that whenever urine is extravasated in such quantity as to produce the patient's death, *gangrene* has speedily ensued, and the parts have been discharged like wet tow, and that true *suppuration* has not taken place.\* But, admitting that true pus was actually formed in the cases related by Mr. Pattison, to the extent of *two drachms* between the bladder and rectum, would this be sufficient to cause the patient's death, when it is expressly declared, there was nothing like peritoneal inflammation present? If a simple lodgment of matter about the rectum were capable of destroying the patient, should we not expect the same thing from a similar accumulation, in cases of abscess preceding the formation of fistula in ano, or fistula in perinæo? That a thin offensive discharge accompanies gangrene or sphacelus, cannot be denied; but as this is very different from pus, and as no marks of gangrene but simply "*a considerable matting and thickening of the parts,*" together with two drachms of pus were found, I conclude that the patients of Mr. Pattison, and those "of his brethren" did not die from this cause, but some other. In all probability, Mr. Pattison became aware after the publication of his first essay, that pus could not arise from the cause he pointed out; and therefore, in imitation of Scarpa, he observes in his reply that he found "*gangrenous suppuration*" between the bladder and rectum of his patient, although previously, in giving an account of his dissections, he had not said *one word about gangrene*.

At any rate, I think we are justified in concluding, that if no worse effects than the accumulation "of *two drachms* of pus" between the bladder and rectum succeed the operation of lithotomy, that ninety-nine out of one hundred patients ought to recover.

\* This circumstance was first noticed by Dr. Physick.

It may be asked, since I do not allow that urinal infiltration, is attended with so much danger as Scarpa and Mr. Pattison conceive, what I suppose to be the cause of death in most cases, after the operation of lithotomy? I answer, without hesitation, that I believe it to proceed, in nine out of ten casès, from violence done to the bladder, prostate gland, and adjoining parts by protracted and forcible efforts to remove the stone;—that this difficulty of getting out the stone, arises either from an opening not being made into the bladder or from its being made so small as not to be found, or if found, insufficient to admit the finger or the forceps,—that after the forceps are introduced, so many fruitless attempts are made to grasp the stone, and so much force employed in pulling it away after it is seized, that the parts are lacerated and mangled, and the bladder, probably, in many instances in a state of inflammation, before the patient is removed from the table,—that in consequence of all this rudeness on the part of the operator, peritoneal inflammation is speedily induced, and the patient dies in a very few days. This is not only my opinion, but the opinion of nine tenths of the best informed and most experienced operators in the world. It is the particular opinion of Mr. Pattison's best authority, Mr. Samuel Cooper who says, "In the course of the *nineteen* years, that I have been in the profession of Surgery, I have seen the lateral *operation* performed more than fifty times either with various kinds of gorgets, beaked knives, the lithotome caché, or common scalpels. In all these examples, the avowed intention of the surgeon, was to make a free opening into the bladder. I do not mean, however, to say that this was actually accomplished, since the bad construction of the instruments employed, and other causes, sometimes frustrated the wise design of the operator. *But what was*

*the consequence?* Generally speaking, those surgeons who made *only a small incision* into the bladder, and kept their patients a long while upon the operating table, ere they succeeded in getting out the stone, by the repeated and forcible use of the forceps, had the mortification to see very few of their patients recover, a large proportion of them being carried off by peritonitis, on the third or fourth day after the operation. On the contrary, when the incision *was ample and direct*, so that the calculus could be easily and gently removed, the patients were almost always saved."

That patients sometimes die from violence alone, in a few hours after the operation, and before peritoneal inflammation could have taken place, or before effusion of urine could have produced suppuration, or gangrene, there can be no doubt. They die in such cases, from *absolute irritation*, or from the shock communicated to the nervous system, by the operation, in the same way that patients are lost in many instances, from amputation and other severe operations.

On the other hand, I have known persons die in the course of eight or ten hours after the operation, where no sort of delay or difficulty was experienced in getting the stone away, where the patients suffered comparatively little pain, and where no vestige of suppuration or of gangrene, or of abdominal inflammation could be discovered after death. A case of this sort occurred in the Edinburgh Infirmary, under the care of Professor Thompson, while I was a student.

Again, there are cases, where patients are kept on the table for hours, during which the utmost violence is committed, where the bladder is torn and shockingly mangled, either by the instruments, or by the passage of rough stones, and yet recover almost without a bad symptom. This I have also seen in Europe, and Dr. Physick has witnessed similar examples in this country. Surely then we have a right to conclude, that there is something inexplica-

ble in many instances, as to the cause of the patient's death or recovery after the operation; and that what will quickly destroy one, will have no effect on another.

That death is a very frequent consequence of hemorrhage, after lithotomy, I do not believe, but that it sometimes occurs is very certain; for upon dissection, the bladder has been found filled with blood, and all the neighbouring parts extensively injected and separated from their connexions. In such cases the blood may flow either from the internal pudic, or from the artery of the bulb, or from the transversalis perenei artery. I believe that it may proceed occasionally from an extensive incision through the prostate gland, into the body of the bladder;—in consequence of the large veins, *the venæ vesicales*, which are distributed in great profusion about these parts, being cut across. This I would consider then a stronger objection to a prolonged incision into the neck of the bladder, than the fear of urinal infiltration from the division of the "*prostate fascia*," or as it is better called by Carpue and others, the "*obturator fascia*." It was probably owing to the division of some of these vessels, that Boyer experienced so much trouble in stopping the hemorrhage in the interesting case detailed in Fourcroy's journal; as the blood proceeded evidently from the cavity of the pelvis, and not from the pudic artery. I do not believe that the pudic is cut so often as is imagined, but I am very sure that it may be divided, and bleed for hours, in some cases, without the operator being aware of it. If the surgeon were to follow the advice given by Mr. Pattison, in his first essay, to "continue the incision down boldly from three to four inches, in a line which runs betwixt the tuber ischii and the anus, inclining it *considerably towards the former*," I am confident that in six out of ten cases the *pudic* would be cut across, and the patient's life endangered. Mr. Charles Bell, in speaking of hemorrhage after lithotomy remarks: "The internal pudic

is often cut, and the patient dies. Three of our first operators, have each within the last year lost a patient, by hemorrhagy." It is surprising under such circumstances that the English and French surgeons, should continue to employ the canula and sponge, instead of Dr. Physick's forceps and needle, an instrument calculated with certainty, to suppress the bleeding.

I have said enough to convince any one, that the assertion delivered in my review of Mr. Pattison's essay, that the practice recommended by him, is adverse to the best European authority, has been proved beyond the possibility of contradiction. I shall therefore endeavour to substantiate the remaining assertion, that it is equally repugnant to the principles and practice of the best authority of this country. For this purpose I submit the sentiments of one whose knowledge of the subject, and extensive experience can never for a moment be called in question.

Dr. Physick has performed the operation of lithotomy, generally every year, and sometimes three or four times a year, ever since 1797. He has always made a free division of the muscles, and other soft parts of the perinæum. To avoid wounding the pudic artery, and not from fear of making too large an incision in the prostate gland, he has employed a gorget from a half to three quarters of an inch in breadth. Whenever the stone has been found so large, as not to be extracted without difficulty he has divided the neck of the bladder with a bistoury, and never found any inconvenience to result from the practice. He is convinced that many patients have been lost in consequence of the opening in the prostate and bladder being made so small as to require forcible efforts to remove the stone, which has not been extracted without considerable laceration.

As regards the cause of death after the operation, Dr. Physick is inclined to believe, that it is owing frequently to

the shock communicated to the system, sometimes to hemorrhage, sometimes, though rarely, to violent inflammation of the internal coat of the bladder, sometimes to mortification of this viscus and of all the parts in its neighbourhood, and *never* to urinal infiltration, so far as he has been able to ascertain by dissection, or by other means. In one instance, in his practice, the urine did not come away through the wound or urethra after the operation, and he concluded that it must have been effused into the cavity of the pelvis. Under this impression a gum elastic catheter was introduced into the bladder, through the wound, but not a drop of urine flowed. He then conceived that the action of the kidneys was suspended, and that the urine did not escape, because none was secreted. His suspicion was confirmed by dissection, for the bladder, rectum, and all the adjoining parts, were found dry, without a vestige of suppuration, of gangrene or of effused urine.

Mr. Pattison in his "observations on lithotomy," says, "Dr. Physick has observed to me, that for a considerable number of years back, he has been in the habit of introducing, from the wound into the bladder, a piece of a gum catheter, which he allows to remain, and that the *success of his operations had been much increased* by the introduction of this instrument. *The superiority of success* attending those operations, where the catheter was introduced over those where it was not employed *is a strong argument in proof of the justice of my observations.*"

Upon inquiry I find *Dr. Physick* has been in the habit of saying that since the use of the gum catheter his patients had been rendered *more comfortable than before*, in consequence of the urine being deposited in a vessel, kept in bed for the purpose of receiving it; thereby obviating excoriation of the edges of the wound, and preventing the bed clothes from being soiled and wet.

It was this circumstance, that induced Dr. Physick to continue the use of the gum elastic catheter. But he has found it useful in another point of view;—viz. in the suppression of hemorrhage, from the deeper seated parts. In this case, the catheter is introduced through the wound into the bladder, and insures an outlet for the urine, while compression is made by filling the wound with lint. It is obvious, without such a contrivance, that the urine could not flow, but would be detained by the lint, and give rise to inflammation, and other ill consequences. Dr. Physick does not introduce “*a piece of catheter,*” but uses the entire instrument.

In addition to the authority of Dr. Physick, I may state, that other distinguished surgeons in the United States, are in the habit of making free incisions of the prostate gland and neck of the bladder, and with the happiest result. I am informed by my friend, Dr. Stevens of New York, that Drs. Post, Kissam, and Mott invariably aim at the complete division of the prostate, and that their success has been very considerable. In two instances where Dr. Stevens had occasion to operate, extensive incisions were made, and in one case a stone was extracted, the smallest diameter of which measured two inches and a quarter. Both patients recovered without difficulty. None of these gentlemen (as I am authorised by Dr. Stevens to say) believe urinal infiltration to be a common occurrence, or the cause of death after the operation of lithotomy, but are persuaded that patients die from hemorrhage, more frequently than from any other circumstance.

Professor Caldwell, of the Transylvania University, during his late visit to Philadelphia, informed me that Dr. Dudley, well known throughout the western country, as an able anatomist, and accomplished surgeon, had operated for the stone fourteen times without having lost a patient, and was induced to attribute his success in a great measure to ample incisions of the prostate gland, and neck of the blad-

der. The late professor Dorsey, who was particularly distinguished as a lithotomist, preferred an extensive to a small incision. In speaking of two cases where the *neck of the bladder healed by the first intension*, he remarks, I have no hesitation in ascribing the successful termination of these cases to the use of a gorget which is so perfectly keen, as to pass without any resistance *through the prostate gland and neck of the bladder.*" Dr. Dorsey, in speaking of the cause of death, after the operation, barely mentions, "sometimes gangrene results from the escape of urine into the surrounding cellular texture, and sometimes the *bladder inflames violently* and death results from *this cause.*"

A surgeon well acquainted with the principles upon which lithotomy should be performed, may operate with almost any instrument. But he will prefer such as will enable him to execute his purpose with the least trouble to himself and danger to the patient. Even an experienced lithotomist, or expert artist will find it no easy matter to understand the design or shape of the weapon figured upon a plate in Mr. Pattison's essay. If the awkward thing exhibited in one of the shops of this city under the name of "Pattison's Gorget," be a faithful copy of the original, it certainly cannot answer conveniently the purpose for which it was designed by its inexperienced projector. It is professedly an improvement on the instrument of Peile of Dublin. Peile's instrument, I may remark, was copied, with a slight alteration, from that of Dease, and Dease simply modified the lithotome of Daunt; Daunt probably copied Pouteau, and Pouteau the older French surgeons. However varied, the straight handle was always left as more convenient for introducing the blade into the bladder. Mr. Pattison has given the finishing touch to the instrument, and rendered the handle *crooked*, for the purpose, no doubt, of adapting it to his own practice. An additional beak or protruding peg, is fixed near the shoulder of the blade, to prevent it from leaving the groove of the

director, which, in imitation of Mr. Allan Burns' instrument and that of some of the older lithotomists, is straight instead of curved.

Any one who has leisure to examine these lithotomic curiosities, will find that the ungainly handle can answer no other purpose than to encounter the ischium, which by reacting will dislodge it from the groove of the conductor in spite of its double beak. These beaks, moreover, must of necessity hitch, and prevent the blade from moving with facility in the groove.

It is advised by Mr. Pattison, to introduce a bistoury and cut a portion of the *right side* of the prostate, in case the opening made by his gorget, be not large enough for the stone to pass through. Dr. Physick, many years ago, employed a gorget with a beak in the centre, and an edge on each side, for the purpose of cutting to a certain extent both sides of the gland. He used it once only, and the patient died. Mr. Astley Cooper has employed a similar instrument, in several cases, but with no better success. Scarpa has contrived a gorget of the same kind.

Now I take leave of Mr. Pattison forever. If his essays are full of sound doctrine and useful practical information, my censure will prove harmless. But there is a criticism from which no work is secure. If I have been unjust I should regret it. Mr. Pattison's merit I have no desire to undervalue. That he is a good anatomist it gives me pleasure to allow, that he may become, in time, an intelligent surgeon I am willing to hope. But I never should have placed him where he has ventured always to place himself. His petulant invectives I have disregarded. His vain and abortive efforts at sarcasm, I have not deigned to notice. "If any coarse expressions have escaped me, I am ready to agree that they are unfit for me to make use of, but I see no reason to admit that they have been improperly applied."





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