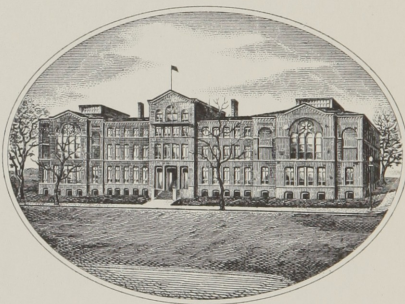




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*John Thomson Esq  
from his friend the Am*

## A REPLY

TO

CERTAIN ORAL AND WRITTEN

CRITICISMS,

DELIVERED AGAINST

AN

ESSAY ON LITHOTOMY,

PUBLISHED IN THE JANUARY NUMBER OF THE AMERICAN MEDICAL  
RECORDER.

By GRANVILLE SHARP PATTISON, Esq.

“Unnecessarily to mortify the vanity of any writer, is a cruelty which mere dulness can never deserve; but where a base and personal malignity usurps the place of literary emulation, the aggressor deserves neither quarters nor pity.”—SHERIDAN.

(Inserted in the American Medical Recorder for July, 1820.)

PHILADELPHIA, 1820.



## REPLY.

THE unshackled discussion of political topics, is not more important to the public weal, than the unrestrained exercise of just criticism is salutary to the investigation of scientific truth. In both, liberty may degenerate into licentiousness; in both, public good may be forced to succumb to private advantage; and truth, however important, assassinated by traduction, may be scouted by popular odium, or silenced by the superciliousness of favoured ignorance.

It happens fortunately, however, for the interests of liberty and science, that the enemies of both, insidious as they may be in their attacks, are easily detected, and that their impotent efforts tend, in most cases, only to strengthen the prosperity of the causes against which they have been levelled. This observation bears with particular force on literary criticisms. If unjust, the individual aggrieved, has it in his power to appeal; and if he has truth to support him, there can be no doubt, that by the mightiness of her pleading, and the justice of public feeling, his doctrines, which have been carped at and abused, will be presented under a clearer aspect, more fully understood, and finally established.

Anonymous writing is usually employed for conveying to the public, critical observations on the works of letters and science. Usage, from time immemorial, has sanctioned, as consistent with the character of a gentleman, this kind of writing:—but honour

demands, that these criticisms should be composed in the spirit of liberality, and published in regular journals. When written with a malicious feeling, for the purpose of injuring the character of a cotemporary, and, more especially, when published out of the regular channels which are employed for the dissemination of scientific communications, they lose all respectability, and merit the odious denomination of libels, equally inconsistent with the principles of honour, which should regulate the conduct of the gentleman, or the love of truth, which should direct the observations of the man of science.

In the justice of these remarks, I feel persuaded, that every man of principle and correct feeling, will agree. They have been suggested to my mind, from the consideration of certain oral and written criticisms, thrown out against the observations contained in the essay on the subject of lithotomy, I published in the January number of the *Medical Recorder*, and on the refutation of which I now propose to enter. But in order to render the subject more perspicuous, it may, perhaps, be judicious, to preface the following remarks, with a short history, of what occurred previous to, and immediately after, the publication of my essay.

The cause of truth, as well as the duty I owe myself, leave no alternative; my professional character must be defended or abandoned; but feeling shall be spared, and decorum observed, whether merited or not; truth being the only legitimate object in all scientific discussions.

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 connections, the causes of urinal infiltration. The Doctor, with that lively interest which he takes, in all that belongs to his profession, entered warmly into the subject, and stated to me, that, although not aware of any anatomical structure, which would modify the dangers of urinal effusion, he had been in the habit, for many years, of introducing through the wound, a gum catheter into the bladder, with the view of allowing a free passage for that fluid; and that, since using this instrument, the success of his operations had

been very great. It was agreed, that I should, the first opportunity which offered, dissect the fascia. Some weeks afterwards, being present at the examination of the body of a boy, whom the Doctor had attended, on account of false passages in the urethra, I took occasion, in his presence, and in that of Drs. Chapman, Dewees, M'Clellan, and several other gentlemen, to cut into the perinæum, and to give a partial view of the fascia. The parts were, however, in this subject, in such a gangrenous state, from urinal effusion, that the exhibition here given of the prostate connections, was far from being a satisfactory one. It was sufficient, however, to satisfy Dr. Physick as to the existence of a fascia, of which *he before knew nothing*, and drew from him a wish, that I would endeavour, at an early date, to dissect it in a subject, where the parts were in a state of health. The other gentlemen expressed with equal warmth, their conviction of the reality of my discovery.

Some days after this dissection, Dr. M'Clellan called with Colles's Treatise on Surgical Anatomy, and stated to me, that an individual who had been present at the exhibition of the fascia, had brought that work to Dr. Physick, and had endeavoured to convince him, that what I had claimed as a discovery, was not due to me;—that the fascia was clearly described by Mr. Colles, in the passage at which he had folded down the leaf. The Doctor's ideas of the connections of the fascia, being from the imperfect dissection given, only general, he was unable himself to say, whether the description to which his attention had been called by this discoverer of my making unjust claims, referred to the fascia which I had shown, or to something else. I had no difficulty in convincing my friend Dr. M'Clellan, that it referred to a part altogether distinct,—one, which was regularly and constantly described by anatomists.

Reading Mr. Colles's work, which was left with me, I found as I have stated in my essay, that that author does, in a different chapter from the one marked, make some observations on the prostate fascia. So soon as I had satisfied myself, that I was not the first who had seen it, I called on Drs. M'Clellan and Eberle, and

stated to them my conviction, that although Mr. Colles did not, in the passage to which Dr. Physick's attention had been called, allude to the prostate fascia, that still, that anatomist had seen it, though he "neither was aware of its connections, its importance, nor its uses." These gentlemen would not even agree with me in this, and still insisted, that the passage in Mr. Colles's work, which I supposed referred to the prostate fascia, and which Dr. Gibson read to his class, and which W. has published in the last number of the Recorder, was intended to describe some distinct part.\*

Accompanied with Dr. M'Clellan, 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.*

I heard nothing more of the prostate fascia until after the publication of my essay. Before it went to press, I took occasion in two different subjects to exhibit its connections, and demonstrate its existence in the presence of Drs. Physick, Parish, Hartshorne, and M'Clellan, and so satisfied were they of its presence, that they allowed me in my paper to make use of their names, with the view of satisfying the sceptical as to its existence.

Immediately after the publication of the January number of the Recorder, the subject of the prostate fascia was again brought on the

\* Dr. M'Clellan in support of this opinion stated, that as Mr. Colles described his fascia as a layer of the triangular ligament, and as the levator ani muscle was interposed between that ligament and the one described by me, he must mean some other layer of fascia. I granted the correctness of my friend's objection, but insisted, that although in relation to this point, and others in the description, Mr. C's description was incorrect, that still I had no doubt but that the same fascia was meant.

tapis. And, although I had myself, been the first who had shewn that Mr. Colles had seen it, so little was that gentleman's book understood at this period, even by those gentlemen, who now declare, that his description is the clearest and most perspicuous possible, that they never attempted to take from me that discovery which they, incorrectly, conceived I had claimed, but endeavoured by childish jests and ill-tempered sneers, to make the world believe that the prostate fascia was a mere creation of my imagination, and that any man who could use a scalpel, could make a fascia just as easily, as a modeller in wax could make a nose. This pleasantry, though at my expense, as it could neither affect me nor change the structure of the perinæum, gave me no concern,—the thing denied, existed; and that truth, which they could not discern, was both capable of demonstration, and of vast importance to be known.

Unfortunately for the harmony of all concerned, Dr. Physick, when he came to the anatomy of the perinæum, regulated by a love of truth, demonstrated the "*prostate fascia*." This demonstration put an end to all mirth, and conveyed a most melancholy illumination to the minds of those who were before blinded. There was no longer a doubt that a fascia existed, which required for its formation, not a petty dissector, but the great "*horlogier de la nature*." But this was not the only enlightening effect produced by this demonstration. Mr. Colles's work was taken up anew, and, upon a re-perusal with minds illuminated, it was discovered, that his description was the clearest possible, and that I had been guilty of an unhandsome plagiarism, in claiming that, which belonged to another.

Having gone over this short historical introduction, I am now prepared to take notice of the different tangible attacks which have been brought forward against the essay. I shall confine my observations to Dr. Gibson's lecture, the anonymous letters published under the signature of Aristides, in Mr. Poulson's newspaper, and the criticism, which appeared in the last number of the Recorder. †

Every Professor is bound, in the fulfilment of his duty, to guard his students, against the reception of that, which he conceives to be either false or pernicious, in doctrine, or practice: and

+ Dr. Gibson

every man of science, is entitled through the media of scientific journals, to publish anonymous liberal criticisms, on the speculations of others in science. But neither the professor nor the man of science, who would wish to rank as a gentleman, is warranted to publish in a commercial newspaper, an anonymous and abusive criticism, against the doctrines of a cotemporary, that have been delivered in a manly and open manner, and published through a proper professional organ. It has been unjustly surmised, that I myself was the author of certain newspaper publications, which mentioned my name in flattering terms. But those who have assumed the liberty to make such insinuations, know them to be without foundation. As a man of honour, I declare, that I am exceedingly unwilling to have my name brought forward, either favourably or unfavourably, by anonymous newspaper writers; and that in the whole course of my life, I have never written or published a syllable to which I did not affix my signature.

As the whole profession have not had the advantage of hearing the remarks delivered by Dr. Gibson in his lecture, or of reading the criticisms of Aristides, it will be necessary, for me, before I attempt a refutation, to state shortly the observation of the professor, and of the anonymous newspaper writer.

I must confess, that the lecture delivered by the learned professor, was a most remarkable one. He began by stating, that having "*accidentally*" taken up Colles's Surgical Anatomy, a few days before, he had discovered that, that author had given a most luminous description of a fascia connected with the prostate gland. I felt pleased with the lecturer's zeal, in satisfying his class on this head, for I naturally concluded from the repetition he employed, and the anxiety he displayed in convincing his students that Mr. Colles had demonstrated the prostate fascia, that although he would give me no credit as a discoverer, still he would go along with me, in my views, as to the great practical deductions, which were to be drawn from the connections of this fascia. Judge of my disappointment when I heard the professor begin with equal warmth, after he conceived, that he had persuaded his auditors of the justness of Mr. Colles's claims, to assure them that the existence or non-existence, of the prostate fascia, was a

matter of not the slightest practical importance. 1st, It was of no consequence, because no gorget was made sufficiently large to cut the whole of the prostate gland; 2dly, it was a matter of no importance, for if this impossibility could be accomplished, still the division of the base of the prostate, would have no effect in producing urinal infiltration; and lastly, he inculcated, that urinal infiltration was not attended with danger.

I am aware, that some ill-tempered people, have supposed that the whole scope and bearing of the professor's discourse, was to persuade the students, that I had been guilty of an unhandsome plagiarism, from Mr. Colles, and like a silly plagiarist that I had purloined that which was perfectly useless. But, I must do the individual referred to the justice to state, that he observed in relation to my name, and claims, the most delicate and profound silence.

The newspaper critic, if we can believe him, is a philanthropist. His motto is rather a remarkable one.

*"In nostros fabricata est machina muros."*

Some of my learned brethren have been at pains to unravel its bearing, but I am quite satisfied that all their explanations are very far indeed, from tallying with the one the author wished it to convey. "Nothing short of the interests of humanity," could have induced Aristides,—honest man! "to come forward in a newspaper." Yet this noble and generous sentiment, makes him sacrifice his delicate feelings. Every man who reads this introduction, must be ready to exclaim, wonderful benevolence! Surely, the individual who has induced this excellent and humane gentleman, to write for Mr. Poulson's newspaper, must be some city pest! some abominable quack, who attempts to poison our citizens. This I am aware is the natural inference, which will be drawn from Aristides's introduction, but most certainly it is not a correct one. I am very fallible, and may most assuredly have been mistaken in my views. But certainly I have delivered these in a regular, professional and gentleman-like manner. And even allowing, that the sentiments which I have taught are erroneous, they are assuredly not of a character to desolate our population, and to call for the interference of a philanthropic Aristides, for their suppression.

It is said that the first sentence of an essay is the most diffi-

cult to compose, and, as the tenor of the letter seems to have nothing to do with humanity, but is written merely to satisfy the commercial public of Philadelphia, that Mr. Pattison is no discoverer and a man of little experience in his profession, we are warranted to suppose, that the author, being at a loss for an initial sentence for his letter, took one, which, in a happy moment of inspiration, he had composed for another purpose, and which, although foreign from the intention of his letter, he conceived too good, to be lost to the public.

In conclusion, the anonymous author, with the view of damning the unfortunate Scotsman, endeavours to rouse the national feeling of Americans against him, and finishes his *humane epistle*, with this imposing assurance, that the American public will not be imposed on with impunity, in other words that they are not to be "*humbugged*" by an ignorant foreigner.

The last criticism is before the profession, and it is therefore unnecessary for me to make any introductory remarks upon the observations which it contains. When I received at Mr. Webster's store, the number in which it is published, I felt so anxious, I must confess, to see what this regular criticism contained, that I took a peep into it, as I walked home. I was aware, that its author was my anonymous friend Aristides, and as his previous publication demonstrated at least the disposition to be ill-tempered and abusive, I expected that he would have written a very severe critique. It is a fact, which the readers of reviews must have remarked, that if there is any bitter observation, this is generally kept for the last paragraph. It makes a good finish, and sends the reader to bed well pleased with the humour of the author. The final paragraph was, therefore, the one I selected for my walking examination. It is certainly an attempt at sarcasm, but most assuredly a feeble one. It takes leave of me as a "*child of science*," assuring me that had the author had time after the preceding laborious production, he would here enter on my practical deduction. This it would doubtless be too much to expect from him, for one three months, and the public and myself, are therefore doomed to wait for an extension of the critic's labours, until another opportunity.

Having read the author's apology for his not having proceeded with the criticisms, I was certainly excusable, in going home in the belief, that the Recorder of which I had just become possessed must contain a very long review of my essay, and one in which there was a great deal of original matter. So soon, therefore, as I had composed my spirits with a cup of tea, I retired to my study, to spend the evening in its perusal. My astonishment was not a little, when I discovered that the paper contained only seven pages, and of these forty-three lines only were made up of original matter. I recollect once of hearing an old story of a professor who found it no easy matter to deliver his lectures, apologizing to his class, for not giving them a valedictory, nearly in the following words: "Gentlemen, I intended to have written you a very fine lecture, but to tell the truth, I am so morally and physically exhausted by my exertions, that I have been unable to accomplish it." I have no doubt, that W. was influenced by similar motives. He intended, I sincerely believe, to have written a very severe and spirited attack upon my opinions, but he became so morally exhausted in composing the forty-three lines which are original, and so physically fatigued by copying the six pages from Mr. Colles's work, and my essay, that his amiable intentions were frustrated.

The tenor of the criticisms, which have been brought against my essay, may be divided into two classes.

1st. Those which go to state that I have claimed as a discovery, that which belongs to Mr. Colles.

2d. Those, which attempt to disprove my practical deductions.

I shall divide my reply into two parts; in the first, I shall endeavour to refute those observations which belong to the former criticisms, and in the second place, attempt, in opposition to what has been brought against them, to establish and confirm those practical doctrines, which, I conceive, may be drawn from the anatomical structure of the perinæum.

I might give a very short answer, to those criticisms which accuse me of being guilty of claiming as a discovery, that which belonged to Mr. Colles. I have in the most candid manner possible, allowed in my own essay, all that is due to that gentleman.

I there, distinctly and clearly state that, "*that anatomist has seen it.*" In the garbled extracts which Aristides gives in the newspaper and Recorder from my essay, he wishes to convince the public, that, I do lay claim to the discovery, and in proof, makes quotations from the first part of the paper, to establish his assertions. I no doubt there employ the expressions quoted, "*a new fascia,*" "*this new fascia I named the fascia of the prostate gland.*" But Aristides must have been aware, that in using such terms, I am giving a history, a diary of my thoughts. The essay contains, in fact, a historical account of my thoughts on the subject of lithotomy. It is, distinctly, stated in the following words; "*in continuing the account of the diary of my thoughts in relation to lithotomy,*" a passage which immediately precedes the expressions quoted. I would ask the question, was it possible for me in writing a diary of my thoughts, to have used any other expressions? It was really, and truly to me, a discovery. It was considered as such, by all the professional friends, with whom I conversed on the subject in Edinburgh, London and Paris; and I still continued to believe it was so, until I read Mr. Colles's work in Philadelphia. I would demand, if there was any thing disingenuous, in my conduct after I read Mr. Colles's essay. His book 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. Every just mind, must therefore allow, that my conduct in relation to Mr. Colles, has been most candid and honourable.

I trust, I have by these facts proven, that I have not claimed the fascia as a discovery, and shall now show, that had I been desirous of appearing before the profession, in the character of an anatomical discoverer, I might, with equal propriety have done so with those, who are universally acknowledged as such.

I suppose, when I state, that William Harvey is the discoverer of the circulation of the blood, and Gaspar Asselius of the lacteal vessels, that it will be allowed, that I am making a correct statement. But is not the following assertion equally consistent with

truth, that Andrew Cesalpinus, described the circulation through the lungs, and that Realdus Columbus, La Faye and Garengot, all wrote, more or less, distinctly of the movement of the circulation prior to Harvey; they understood it, without drawing clear physiological inferences from it, in the language of the biographer of Harvey,—“ Il étoit réservé à Harvée de développer cette vérité et l'on ne peut, sans injustice, lui refuser la gloire d'en avoir tabli la preuve jusqu'à la démonstration.”

It may, with equal truth, be remarked in relation to the discovery of the lacteal vessels, that Hippocrates, Plato, Aristotle, Erasistrates, Herophilus, and Galen, had seen them; but Asseilius was the first who saw them physiologically; and it is he, therefore, who is honoured as their discoverer. If physiological inferences are necessary to constitute an anatomico-physiological discovery, certainly surgical deductions are equally required to establish an anatomico-chirurgical one; and I trust, that even W. will allow, that I have been the first to draw these inferences from the connections of the prostrate fascia.

Again, it would appear, that previous to the publication of my sentiments regarding the connection of the prostate fascia, none of the anatomists in Europe, to whom I had demonstrated it, were aware that such a fascia existed; and in America, no surgeon had ever thought of this connection. Dr. Physick, professor of anatomy in one of the first medical schools in the United States, a man who deservedly stands at the very head of his profession, in this country, allowed that he knew nothing of it before; and the professor of surgery in the same institution, does not, I believe, pretend that he was aware of it, until after Dr. Physick's demonstration, when he “*accidentally*” took up Colles's Surgical Anatomy.

W. states that in the 5th Number of Mr. Charles Bell's Reports, there is a plate given by him, to illustrate an essay by Dr. Gairdner, on the anatomy of the parts concerned in the lateral operation, and that the letter L designates “ the fascia which surrounds the prostrate gland, and which afterwards covers the inside of the levator ani, and obturator internus.” I have not been able to obtain a sight of this number of Mr. Bell's Reports, and can say

nothing of the fascia alluded to by Mr. Gairdner. It is impossible for me to say from the quotation, whether it is intended for the one I have described or not. But this will in no measure militate against my claims, as the work quoted was not published until eighteen months after I had made public, as a discovery, the prostate fascia.

In answer to the next original sentence in W's paper,—“in fact, these parts appear to be spoken of, both by Mr. Colles and Mr. Bell, as matters of course, as things which have been long known, and to claim which, as discoveries, would undoubtedly seem in their eyes ridiculous in the extreme.” This is saying in very distinct terms, that not to be acquainted with the prostate fascia, is to be a mere tyro in anatomy and surgery.

I think this sentence has been written by W. without his being aware of all its bearings. I am satisfied, that no man, American or European, who is acquainted with Dr. Physick's professional character, will say, that he is ill-informed on the subject he professes, or the one which he practices; and from what I know of W. I can with perfect confidence assure the profession, that he is the last man living, who would wish the professor of surgery in the University of Pennsylvania, to be considered an ignoramus. If W., therefore, had only remembered that neither the professor of anatomy nor even Dr. Gibson were aware of the existence of the prostate fascia until I came to this country, I hardly think he would have insinuated, that every surgeon short of a fool, was familiar with it.

Every man who reads my essay with an unprejudiced mind, must be satisfied, that it is written, not for the purpose of laying claim to an anatomical discovery, but simply with the view of enforcing what I conceive to be highly important practical maxims and observations. In concluding it, I observed, “*the only claim I will make, and in this I am confident I will be supported, is, that until the present, no rational explanation has been given of the manner in which the urine is effused, and consequently, no operation has been philosophically proposed to prevent it.*” I shall not insult the understanding of the reader, by reasoning longer on this part of the subject. I feel perfectly satisfied, that every indivi-

dual whose good opinion is worth coveting, will be ready to award to me, much more than I have claimed.

The reader, in recalling to mind the history of my own experience, in lithotomy, as delivered in my former essay, will observe, that in the only cases where I ever had an opportunity of making dissections after death from that operation, I discovered gangrenous supuration betwixt the bladder and rectum, which, unquestionably, arose from the infiltration of urine into the cellular structure, which connects the bas-fond of that viscus with the gut; and that these operated as the cause of death, there can be no doubt. These facts are, I conceive, of themselves, quite sufficient to establish the correctness of my axiom, that one of the greatest dangers of the operation, is the effusion of urine. But in refuting the criticisms, which have been circulated against my opinions, I am unwilling to place any force, either in my own experience or assertions. I am desirous to confirm the correctness of my doctrines, by bringing forward to their support, passages from the works of those, who are, with justice, considered the great fathers of our art, and who, writing without a bias to this or the other maxim, must be considered as unprejudiced.

It will not be denied, I believe, that one of the greatest dangers of the *apparatut altus*, is the infiltration of urine, into the cellular substance which surrounds the bladder. If it be allowed, that the effusion of that fluid from an opening made into the bladder above the symphysis pubis, will, from its irritation, produce gangrenous inflammations, and suppurations in the cellular substance, with which it comes in contact, it can hardly be believed, that it will not produce precisely the same effects when allowed to infiltrate from a wound made into the shoulder of the viscus. Richerand, in his *Nosographie Chirurgicale*, Tome III., speaking of urine being effused into the cellular substance, mentions it in the following words: "*accident mortel toujours redoutable après la taille hypogastrique.*" And again, "*alors elles se feroient jour par la plaie supérieure s'infiltreroient dans le tissu cellulaire, et causeroient une gangrène mortelle.*" These observations of Richerand's, go to prove, that the effusion of urine above the pubis into the cellular substance, is an "*accident mortel*,"

one which gives rise to a "*gangrène mortelle*." From the next quotations it will appear, first, that urine produces the same effects when it infiltrates into the cellular substance, which connects the bladder to the rectum; and secondly, that this fluid will be apt to become effused, if the wound be continued either through the base of the prostate gland, or made through the prostate fascia, directly into the shoulder of the bladder.

Cheselden, in attempting to imitate the operation performed in Holland with the most extraordinary success by Rau, cut directly into the shoulder of the bladder, necessarily dividing the prostate fascia. The melancholy detail of the unhappy result, speaks volumes in proof of the dangers of dividing that fascia. Camper, in his *Anatomical Demonstrations*, thus records it. "*Cheseldenus, ut omnia tentaret, vesicam aqua hordei implebat, quantum ægri ferre proterant; dein vesicam incidebat, sed infausto successu, propter urinam inter vesicam et portas vicinas remorantum, unde gangrena, qua ex decem octo moriebantur.*" The operation of M. Foubert, which entered the bladder at the same point with the one described above, was equally unsuccessful. Sharp, in his *Critical Enquiry*, &c. details its fatal consequence; but he, like every author, who has considered the subject, gives an erroneous explanation of the cause. "Another great evil," he observes, "attendant upon a wound of the bladder in that part, is the want of a free egress for the urine, which insinuates itself into the cellular membrane, producing *abscesses* and *gangrenes*, which often prove fatal. Or, if they do not destroy, yet by lying on the rectum, produce a slough there, and thus form a communication between the bladder and rectum." To obviate this danger, M. Foubert invented, or rather revived the practice of Franco, and introduced a gum catheter into the bladder.

I should hope, that I have been enabled by the quotation of these passages to prove the justness of the opinion I have delivered, as to the great dangers which arise from urinal infiltration, and shall now bring forward some other quotations, which will further establish this doctrine, and at the same time prove, that although aware, of the great dangers which arise from infiltration, surgeons were not aware of the true reason, why a large wound,

which divides the basis of the prostate gland, and enters the shoulder of the bladder, is usually followed by them.

M. Sabatier, in his work "*De la Médecine Opératoire*," in attempting to explain the cause of urinal effusion, after those operations in which the shoulder of the bladder is cut, thus expresses himself. "Parce que l'écoulement des urines permet à la vessie de se contracter, et parce que la plaie de ce viscère cesse d'être parallèle à celle des graisses, et des tégumens. Ce défaut de parallélisme, augment la disposition aux infiltrations intérieures."

Desault, in his "*Ouvres Chirurgicales*," gives nearly a similar explanation of the cause of urinal effusion, "d'un autre côté, celui de ne pas établir de parallélisme entre l'incision extérieure des tégumens qui est oblique et celle du col de la vessie et de la prostate, que se trouvè alors horizontale. De-là la possibilité des infiltrations par les obstacles qui les urines trouveront à s'écouler." The last quotation, which I will give, in proof of my assertion, that there has been no correct statement given, until the present, of the cause of urinal infiltration, is taken from one of the very last essays which have been published, on the subject of lithotomy. Mr. Samuel Cooper, criticising the memoir of Scarpa, which recommends small wounds, says: "indeed, wherever urinal infiltrations do happen, I believe, that they proceed (not from the shoulder of the bladder being cut), but from a totally different cause, viz. from the incision of the skin being too small, and too high up, and from the axis of the internal part of the incision, not corresponding with that of the external wound."

Having thus established, in opposition to the assertion made against them, the two facts, 1st, That urinal infiltration is attended with great danger, and, 2dly, that the reason why it is more apt to follow an operation, where the wound made into the bladder is large, rather than one where it is small, has not until the present been explained. I come next to overturn the assertion of Aristides, "that my precept," not to cut the shoulder of the bladder, is in direct opposition, to the advice of the best authorities in Europe and this country.

From a rational anatomical explanation of the causes why urine is more apt to infiltrate in those cases, where the basis of the pros-

tate is cut, rather than in those, where it remains undivided, having never until the present been offered; I am prepared to grant, that, some of the most eminent surgeons, both in this country, and in Europe, have advocated a different practice from the one I have recommended. But, I feel assured, that I am supported by truth, when I state, that the practice of making large wounds into the bladder, has been followed with such unhappy consequences, that a majority of the very first surgeons, of this, and of the three preceding centuries, have, without any philosophical knowledge of the cause, been taught by the lessons of experience, to advocate the plan, of entering the bladder, by a small, in preference to a large wound.

Pierre Franco, one of the most eminent surgeons of the 16th century, and a man, who had, perhaps, more experience in lithotomy, than any surgeon of his time, thus expresses his opinion, in his work, entitled *Petit traite contenant Vue de parties Principales de Chirurgie, &c.* "Iaçoit que la moindre incision soit la meilleure;" and again, "Bref il est requis de tenir mediocrite." Brownfield, a man whose surgical knowledge is not disputed, argues, strenuously, against cutting the base of the prostate. "Nan tametsi aliter visum sit multis scriptoribus, fateor tamen, me non posse non putare valde perniciosam esse, partem, membranosam vesicæ sauciari, et si nihil aliud affert mali, fistulas orituræ maxime est verisimile." Scarpa, the great surgeon of Italy, assures us, that the shoulder of the bladder cannot be cut, "without the danger of occasioning urinary fistulæ, and gangrenous suppuration, in the cellular substance situated between the bladder and rectum;" and in another passage of his work, he thus writes; "if an incision was made through the base of the prostate, and into the orifice of the bladder, it would infallibly occasion infiltrations of urine, into the cellular substance, between the rectum and bladder, and subsequent gangrenous abscesses, fistulæ, and other severe accidents." Were I anxious, for further written proof, in support of my statement, I might quote from the works of Le Cat, Callison, &c. but this is, I conceive, unnecessary, recalling to my reader's mind, the fact, which I brought forward in my former essay, that two of the most eminent surgeons of Europe, Mr. Astley Cooper, and M.

Dupuytren, advocate, although on erroneous principles, small wounds. I trust, it will be allowed, I have made good my position, viz. that some of the very first surgeons of this, and the three preceeding centuries, although not aware of the anatomical causes, why urine should be more apt to infiltrate after an operation where the wound is large, rather than after one, where it is small, have been taught by experience, that such is the fact.

One of the assertions delivered by Dr. Gibson in his lecture, was, that no gorget was made sufficiently large, to cut the whole of the body and base of the prostate gland. The professor will, I trust, excuse me, when I state that this assertion of his, is perfectly gratuitous; that it is incorrect may, I think, be readily demonstrated. The anatomical fact, that the breadth of the prostate gland, very rarely, in a state of health, measures above seven lines, cannot be disputed. The statement, that many gorgets are made fourteen lines in breadth, is equally consistent with truth. Now, such being the case, I am at a loss to understand, how the ingenious professor will satisfy any man of common sense, that he can carry a sharp cutting instrument, fourteen lines in breadth, through the prostate gland, which, even in extreme cases, measures only seven, and still leave a part of it uncut. The assertion appears to us, to carry with it, such a palpable contradiction, that we are astonished it could have been entertained for a moment by Dr. Gibson. The prostate gland is composed of a very tough substance, one, which is not easy of division. It lies unsupported in the dead body, *in perinæo*, and consequently, when we carry a gorget into the bladder, in the subject, the two causes above mentioned, will operate, in allowing the instrument to enter, without making a division of the gland, proportionate to its breadth. Of this fact, any man may satisfy himself, and, I should hope, that Dr. Gibson, in delivering the assertion above quoted, was deceived himself, and was not desirous to mislead his students. I will take the liberty of recalling to his mind, the aphorism of De l'Ambert, that "there is no analogy between living matter, which is active, and dead matter, which is inert." When the instrument enters

the bladder, in the living body, there is no yielding of the prostate; it is not there unsupported, but is in fact fixed and pressed down, upon the sharp edge of the gorget, by the levatores ani muscles. It is self-evident, that if there is any disproportion now betwixt the extent of the wound and breadth of the gorget, that this will be in favour of the size of the wound.

The correctness of the second assertion of the professor, that even allowing, that it were possible to cut the basis of the prostate gland, that still it would be a matter of no moment, and would have no effect on urinal effusion, is equally erroneous, and can be proved as such, in a very few words.

I need not recapitulate here, the description of the connection of the prostate fascia given in my former essay. I have, since I came to this country, demonstrated it to above four hundred persons, and all of those, with whom I have had an opportunity of conversing, have given their hearty and cordial assent to my two-fold position, "that the prostate fascia separates the perinæum from the cavity of the pelvis, and that the basis of the gland remaining uncut, *it is physically impossible, for one drop of urine to infiltrate into the cellular substance, which connects the bladder to the rectum.*" It may be said, that it is merely my assertion; it is true, but it is an assertion, which, if false, I would not be likely to make, as it would be refuted by the four hundred individuals, who have been present at different times, when I made the dissection of the perinæum, and one, the correctness of which, I shall at all times be happy to demonstrate on the subject, to any of my professional brethren, who may desire it.

It may appear strange, if the verity of what I have advanced be substantiated, that the professor of surgery, could have made an assertion so directly opposed to it. I trust, however, I shall be able to explain this, without suspecting him of conduct so unworthy of his situation, as that of intentionally deceiving his pupils. There was something upon the table, which he told us, had the fascia shown upon it. If it was really dissected, it did not come under the sphere of my vision, and I am, therefore, charitable enough to suppose, that, as the gentleman who first brought Mr. Colles's work to Dr. Physick, mistook his description

of another fascia for the one described by me, Dr. Gibson was equally misled, by dissecting another layer of fascia, instead of the one connected with the prostate gland.

I have thus gone over, in order, the different charges which have been advanced against my essay. I fondly, and confidently hope, that I have clearly refuted them to the satisfaction of my professional brethren. The task which I have had to perform, has not been a difficult one; some of the criticisms may have been marked by ill-temper, but none of them, assuredly, by professional erudition, or mental acuteness. I have really felt sorry, that the critics have not brought forward some ingenious arguments against my doctrines, for had they done so, a more elaborate train of reasoning would have been required from me, for their refutation; and as the brightness of truth is like the brilliancy of the diamond, the more it is examined, the more refulgent; so their criticisms, if of a superior character, would only have tended to establish and confirm my opinions. 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 has been stated, that Cheselden in his third, and most successful operation, carried a knife into the bladder, behind the prostate, and cut the body of the gland outwards; now if this statement was a correct one, it would, of itself, be sufficient to tear up the very root of my reasonings. If Mr. Cheselden, in his most successful operation, cut the basis of the prostate gland, urinal infiltration could not be one of the great causes of danger, and consequently, cutting, or not cutting that aponeurotic expansion, which, entire, renders the infiltration of urine impossible, would not be a matter of such importance as I have insisted on. I am aware, that many believe, that Mr. Cheselden did perform his last operation in the manner which I have described; but I feel persuaded, that although this is a generally received opinion, it is not consistent with the truth. 1st, I think it can be disproved from the difficulties opposed to the perform-

ance of such an operation, and I have no hesitation in asserting, that was a dextrous surgeon to operate on twenty living subjects, and proceed with the view of executing his operation on the principles which were supposed to regulate Mr. Cheselden, that not in more than one out of the twenty, would the wound extend through the base of the gland, although he might himself believe, that this was accomplished in every instance. On the dead body, the operation may be much more easily accomplished; but even here, experiment will demonstrate, that supposing you have divided the gland, and really done so, are two very different things.

2dly, I conceive, that the fact, that Mr. Cheselden only divided the whole body of the gland, leaving the basis entire, is settled by the account of his operation, which is published by his pupil and assistant, Mr. Sharp. This gentleman, in his *Treatise on the Operations of Surgery*, thus speaks of it:—"This wound (external) must be carried deeper between the muscles, till the prostate can be felt; when searching for the staff, and fixing it properly, if it has slipped, you must turn the edge of the knife upwards, and cut the whole length of the gland, from within outwards." From this, it is evident, that the whole length only of the gland was divided; and that its base was left uncut, is satisfactorily proven by another remark of Sharp's; "there must be laceration, as in the old way (*apparatus major*); but in the one case, the laceration is small, and made after a preparation for it, by an incision; and in the other, all the parts I have mentioned are torn, without any previous opening." Thus we are in the plainest language informed, that the only difference betwixt this method and the Marian one is, that a small wound prepares the parts for dilatation; that this is required in both. If the base of the prostate and shoulder of the bladder were divided, there could be no occasion for laceration.

From these facts, I do conceive, that we are perfectly warranted in supposing that in the great majority of the instances, where Mr. Cheselden performed his operation, he left the base of the gland uncut, that in a few of them, it was divided, and that the fatal cases were the ones where this occurred.

I shall conclude this Essay by calling my readers' attention to Rau's operation, as I conceive it affords strong and unanswerable evidence, in support of the justness of my doctrines. It is well known, that the celebrated Dutch lithotomist, made a secret of the parts he divided, and as death never occurred after any one of his operations, although he cured fifteen hundred patients, no opportunity was afforded the profession, of ascertaining, by dissection, the nature and extent of his incisions. I think, however, from the accounts which are handed down to us of his method, by those who saw him operate, that he was guided by exactly similar principles with those, which we have endeavoured to inculcate. Sabatier, in his "*Médecine Opératoire*," thus speaks of his cutting into the bladder: "Il incisait sur la sonde et pénétrait jusque dans la vessie. Alors il donnait la sonde à tenir, prenait le lithotome de la main gauche, et glissait à sa faveur un conducteur mâle. Le lithotome ôté, ce conducteur servait à en introduire un femelle, et il achevoit l'opération comme il a été dit en parlant du grand appareil." Here, we have an operation recommended, the principles of which, coincide exactly with the one I have advised; an operation which, although executed on fifteen hundred patients, was never followed by a single death. How very different is the success of the most celebrated lithotomist of the present day, to that which attended Rau's operation? From the records of the Norfolk and Norwich Hospitals, we learn, that the number of deaths, for the last seventy years, have averaged four in twenty-nine. Rau had not one in fifteen hundred!! It is not fair to get over the difficulty by explaining the cause of the difference of success, upon the principle, that the cold phlegmatic constitutions of the Dutch are more favourable for the operation than the warm sanguineous English. But it is consistent with the principles of sound logic, and pure philosophy, to elucidate it, by showing, that in Rau's operation, a division of the base of the prostate gland could never happen, and consequently, *that urinal infiltration was, in all his cases, rendered physically impossible.*





