

Observations upon the new opinions of John Hunter, in his late Treatise on the venereal disease, ending with the subject of gonorrhoea, and second part of his work : to be continued / by Jessé Foot, surgeon.

Contributors

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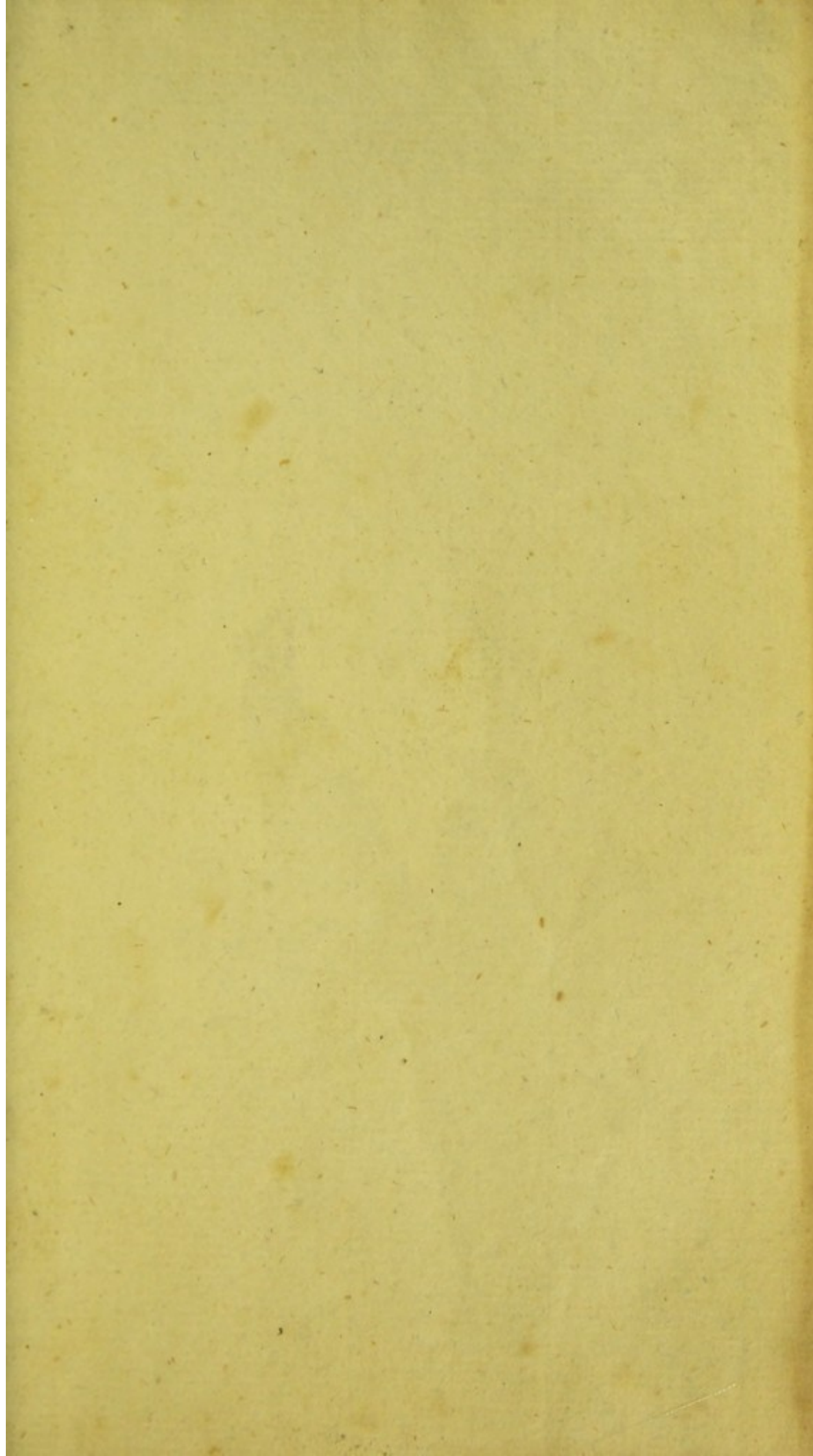
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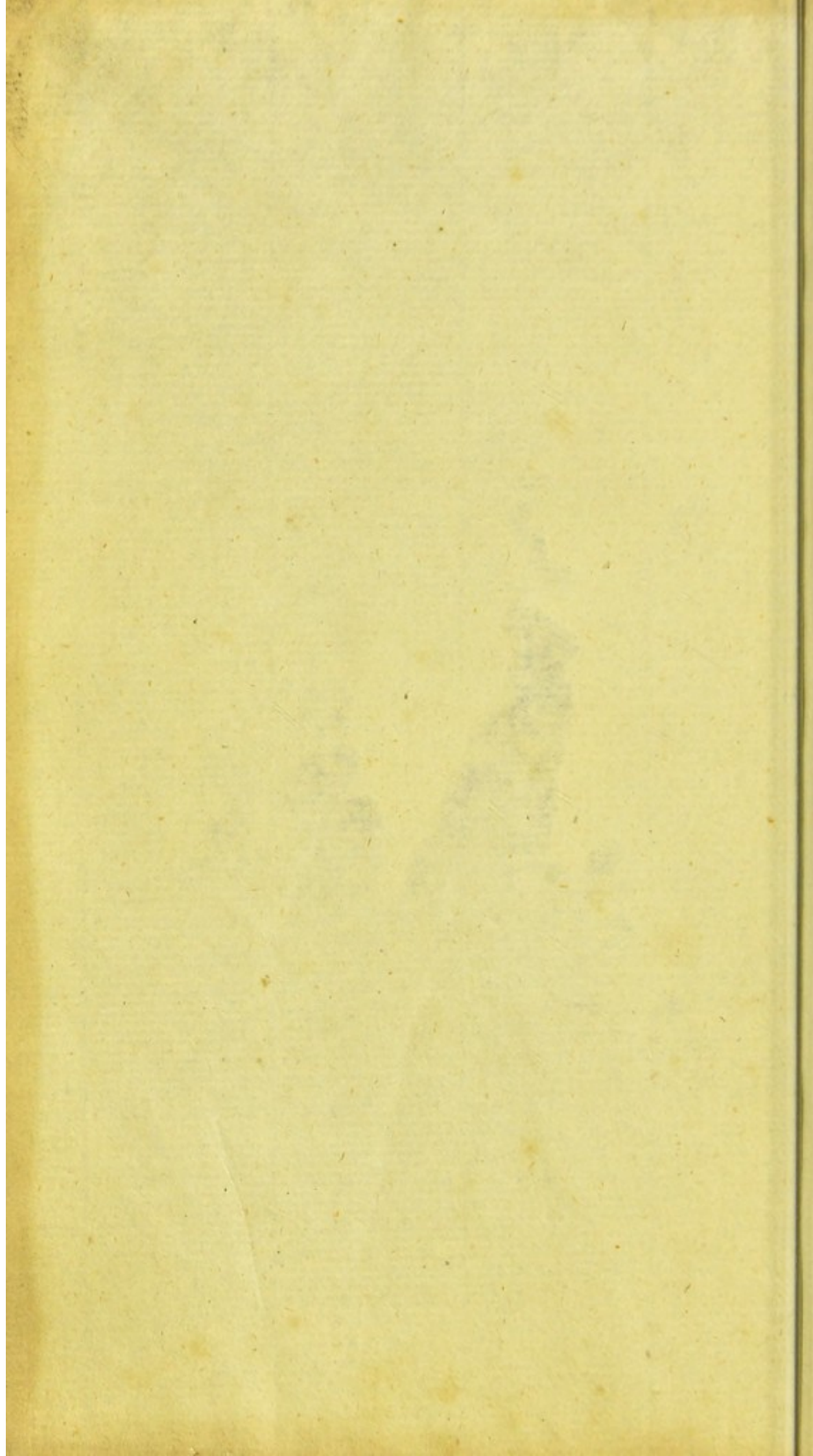
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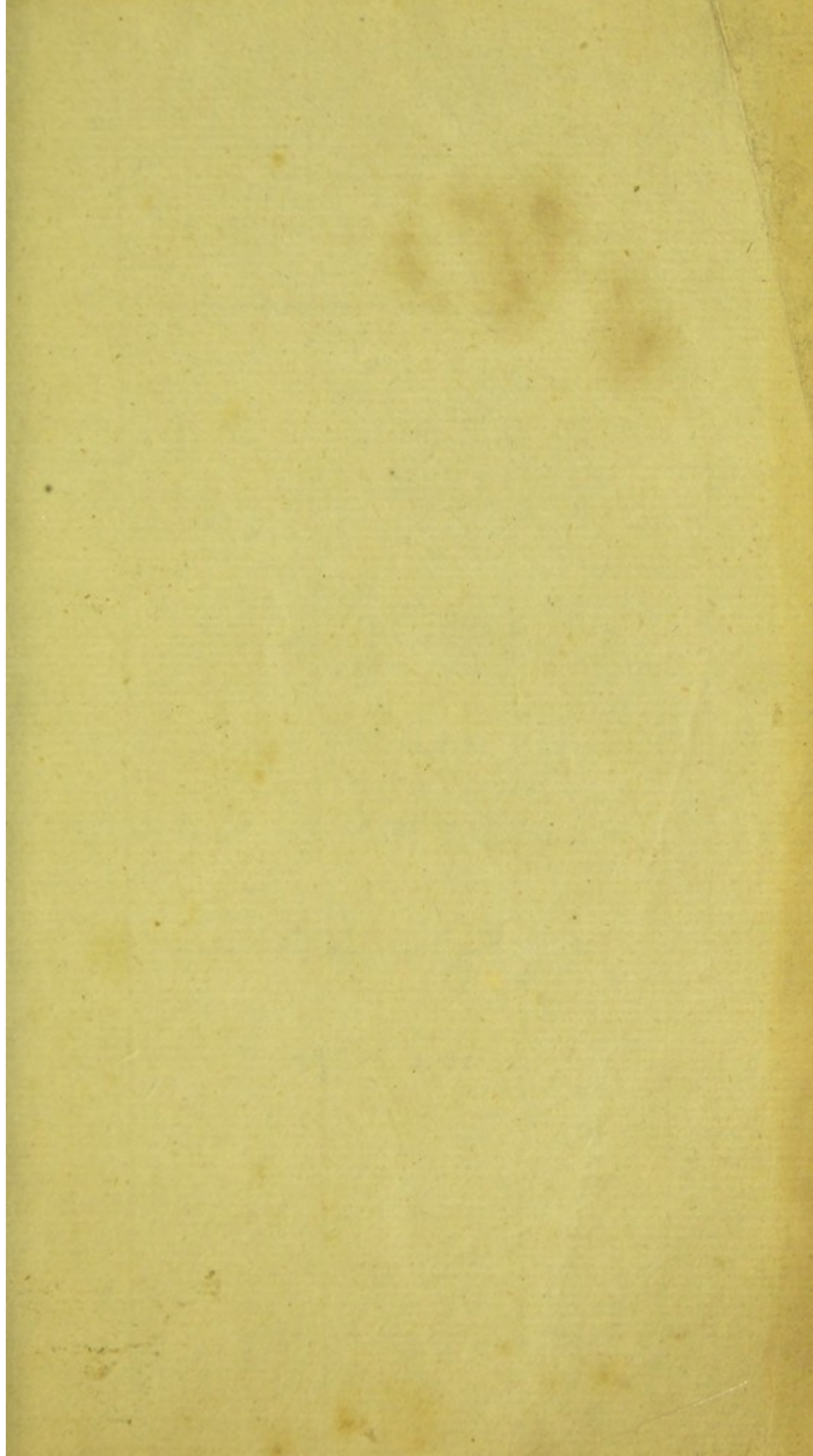
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O B S E R V A T I O N S

U P O N T H E

N E W O P I N I O N S

O F

J O H N H U N T E R,

[PRICE TWO SHILLINGS AND SIXPENCE.]

OBSERVATIONS

ON THE

NEW OPINIONS

OF

JOHN HUNTER.

PRICE TWO SHILLINGS AND SIXPENCE.

OBSERVATIONS

UPON THE

NEW OPINIONS

OF

JOHN HUNTER,

IN HIS LATE TREATISE

ON THE

VENEREAL DISEASE,

Ending with the Subject of Gonorrhœa, and
Second Part of his Work.

TO BE CONTINUED.

BY JESSÉ FOOT, SURGEON.

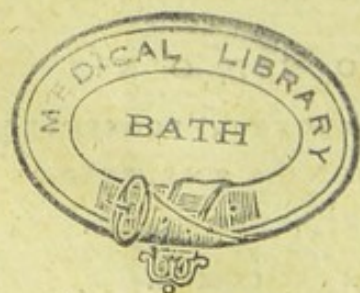
L'homme aime mieux être admiré qu' utile, merveilleux que vrai, difficile
qu' important. ABBE FONTANA, Tom. II. p. 63.

SECOND EDITION.

L O N D O N :

PRINTED FOR T. BECKET, BOOKSELLER TO HIS
ROYAL HIGHNESS THE PRINCE OF
WALES, PALL-MALL.

M DCC LXXXVII.



TO THE
READER.

A Giant is never seen to so great advantage, as when a Dwarf is placed by his side. The most eminent Painters have availed themselves of the high effect of contrast, and expressed it upon Canvas. The most beautiful Prospects in Nature attract our admiration from the same Result. Whilst the great Work of JOHN HUNTER is open before you upon your Writing Desk,

1 do

do me the honour to take this in your
Hand.

To those who have not already
purchased that great Work, I have to
observe, that I have faithfully given,
as far as I have already gone, the Ma-
terial Text of the Original upon which
my Comment is formed.

And to all my Readers I promise, to
persist in my Review of the whole of
his Treatise at my first leisure Hours.

*Dean Street, Soho, }
June 9th, 1786. }*

OBSER.

OBSERVATIONS, &c:

IT is the duty of every professional man to combat such opinions in science, as are either not founded in truth, or would be pernicious in practice. Speculative innovations which tend more to bewilder than to inform, and which are proposed merely to obtain a name are doubtless worthy of reprehension.

I therefore shall assert my right of offering to the public my objections to the new opinions of John Hunter in his late Treatise on the Venereal Disease. Because I cannot permit, as an individual in the profession of Surgery, that doctrines I find extravagant should pass at least without remark, or that they should be received without my protest.

Throughout my objections, I wish to be understood, that it is the theory of the Professor that stands in my way, and not the Professor himself. No intention can be further from me than personal attack. If I sting his theory, I mean to leave the man harmless.

I consider myself in the situation of a counsel at the bar, in a cause at issue, who, after he has heard, with attentive patience, the case of his opponent, rises with an humble hope, but with a diffidence that alarms him, to rescue established principles from unaccountable innovations, and to protect truth from the violence of perversion. The Professor has had my patient reading; and the attention I have paid was arrested by the most implicit submission; for although I have only the honour of his acquaintance, as we have met in the way of our profession, although, unfortunately, when I have had recourse to surgical aid, the Professor has not been always the person whose opinion I have invoked; yet who is there among us that delights in theory or practice, but must know the eminent situation he stands in? who can talk of Anatomical Improvements, where his name is not mentioned with honour? who has not
heard

heard of his experimental excellence? who does not applaud him to the very Echo for his knowledge in Comparative Anatomy? I wish my panegyrick could go further, but here it must stop.

Along time since we have heard of this elaborate work. A work, as it was formerly announced in the public prints*, that was to set criticism at defiance, and so exalted, in points of improvement, above others, as to throw them at the most humble distance. The prophecy that was foretold, is at length come to light, and the Professor is to be the instrument in Venereal Redemption; his production has scattered our fears to to the wind. The Venereal Disease will no longer be dreadful.

Considering myself now in full possession of the attention of my readers, I shall proceed with my objections; but not without begging their patience, which I do the more chearfully, as I know that they feel towards the Professor as I do, not the least wearied out by unintelligible abstruseness, or

* Vide paragraphs in the Spring 1785.

contrary assertions. But at the same time, it may be right to remark, that the length of the Professor's work, however we may be delighted with his talents, was not absolutely necessary ; since, notwithstanding the clearness of his head, and his extensive judgment, he has devoted many of its pages to mortifying confessions of ineffectual research.

Nullum numen abest si sit prudentia, said Juvenal. But how came the Professor so much off his guard, as not to retain that propitious guardian in his service ? Was there not one of his friends at his elbow, to put him in mind of such necessary precaution ? No one to lay before him the danger of embarking as an author ? No one to tell him that the highest point of honour is the pinnacle of danger, from which he may topple with precipitation ? The opportunities in his early life that Dr. Hunter's dissecting room afforded would have made a dunce useful ; how then could they fail to make the Professor a proficient ?—Why did he mistake education for genius, why the mechanism of Anatomy for Chirurgical superiority ?

I hope

I hope the Professor will take this in good part, and that he is disposed to put on as chearful a countenance as I am—As his theory goes only to sympathy in pain, I wish to be indulged to sympathise with him in pleasure.

The professor complains that his opinions have made their way to the world under other names. I wish to know who they are that entertain them. My reason may be better guessed, than told. But let us proceed to examine the Professor's budget of new opinions beginning with his *Introduction*.

I. *On Sympathy.*

This he tells us is one of his *new opinions*, and explains it as applicable to those affections from Venereal Poison which have been hitherto understood to be inflammation in any particular part, arising from the translation of venereal poison to that part.

The Professor tells us, “ that the swelling
“ of the Testicle in consequence of a go-
“ norrhœa

“ norrhœa is produced from partial sympathy;
 “ thy; and that the symptomatic fever is an
 “ universal sympathy arising from the partial
 “ one. A hectic fever is an universal sympathy
 “ thy with a local disease which the constitution
 “ is not able to overcome. This takes
 “ place oftener and in greater degree in
 “ the *Lues Venerea*, than in any other form
 “ of the disease.” He goes on to subdivide
 his sympathies, through which I shall not follow
 him: I shall content myself with carrying
 along with me the spirit of his theory, and,
 by applying it to practice as he applies it, examine
 how far his arguments hold, and what the
 advantages are, that may be derived from
 this new arrangement; whether the particular
 distinctions which he makes be in terms
 only; whether they be evident improvements
 worthy of general adoption; or whether they
 be not in many instances calculated to mis-
 lead.

II. *Of Diseased Actions being incompatible with each other.*

III. *Of the Powers of different Parts of the Body from situation, from structure.*

IV. *Parts susceptible of Diseases.*

V. *Inflammation.*

VI. *Mortification.*

All this is compressed within eight pages, and I thank the professor for this brief. I shall not fail of availing myself of this generous advantage, whenever he brings either of these leading principles into play: I shall watch how they are applied, applaud them when I can, condemn them when I must; being nevertheless disposed where I find them perfectly innocent not to rouse them into action at all.

Part. I. Chap. I. Page 9.

Of the Venereal Poison.

The Venereal Disease, the Professor calls
 “ a morbid poison to distinguish it from other
 “ poisons, animal, vegetable, and mineral.”

I call it an animal poison peculiar to man.
 To call it by any other name would be admitting
 that all other animal poisons were possessed of
 the same properties. Now as they all differ
 in their action and effect, I see no reason for
 calling this a “ *morbid*” and not an animal
 poison.

I. Of the first Origin of the Poison.

The Professor shrinks from the enquiry, in-
 to the origin of the Venereal Disease, and
 refers us to Astruc, and to an anonymous au-
 thor. The fact is, and my readers shall be
 told it, that the authors who flourished at the
 first appearance of this disease in Europe, have
 described the first symptoms, not conformable
 to

to the theory of the Professor, as I shall show hereafter.

The first Author that can be traced I believe on this Disease, was Nicolaus Leonicensus of Vicentia, *Anno* 1497. And no less than thirty-three authors wrote on the same Disease after him, to the time of Johannes Baptista Montanus of Verona. Now all these thirty-three authors say not a word about a gonorrhœa, or any other disease of the Urethra. Johannes Baptista Montanus first observed and cured a gonorrhœa *Anno* 1550.

The above account must be true, for who can suppose that thirty-three authors, eager as they were at that time, to describe this new disease in all its conditions, would have been silent upon so important a symptom as gonorrhœa, if any such had then existed? The Venetian Collection furnishes us with this information.

Of the origin of the Venereal Disease also my readers may find much information and argument in No. 357 and 365 of the Philosophic Transactions.

Page 14, &c. *Of the Poison being the same in Gonorrhœa and in Chancre.*

The Professor in this section, enquires into the manner by which the Venereal Disease was communicated to the inhabitants of the Islands in the South seas.

This enquiry he introduces to prove that the poison is the same in gonorrhœa and chancre. I shall be glad to be informed by the Professor who ever doubted but that it was the same? I entirely agree with him that it is. But if he imagines that his appeal to the relation, given us by Wallis and Cook, confirms it, I totally differ from him. Nothing that either Wallis or Cook has said upon the subject, can give authority to the Professor to draw such a conclusion. Wallis proves that he did not introduce the disease there*. Cook found it there, but makes no mention of gonorrhœa, chancre, or bubo. “He presumed,

* Hawksworth's Voyages, Octavo Edition, Vol. I.
P. 323, 324.

“ that they had found a specific for it, as he
 “ saw some on whom the disease had made a
 “ great progress. And one who went away
 “ infected, returned after a short time in per-
 “ fect health.” Again he says, “ But some
 “ had fallen victims to its rage, abandoned by
 “ their nearest relations, their hair and their
 “ nails fell off, and their flesh rotted from
 “ their bones *.” He says again in his last
 voyage, particularly describing their condi-
 tions. “ Broad ulcers with thick white edges
 “ that were shocking to look at. And yet he
 “ met with some that seemed to be cured of
 “ it, and others in a fair way of being
 “ cured; but this was not effected with-
 “ out the loss of the nose, or the best part of
 “ it †.”

Afterwards he tells us, “ that the disease is
 “ now almost universal; for this they seem
 “ to have no effectual remedy. They allow
 “ that in a few cases, nature, without the
 “ assistance of a physician, exterminates the
 “ poison, and perfect recovery is produced;

* Hawksworth's Voyages, Octavo Edition, Vol. III.
 p. 82, 83.

† Cook's Northern Voyage, Vol. I. p. 382. Dublin
 Edition.

“ they also say if a man is infected with it,
 “ he will often communicate it to others in
 “ the same house by feeding out of the same
 “ utensils or handling them *”.

The Professor asserts that Cook has described the disease to be in “ every form †,” as he has given us no reference, I shall be glad to know in what part of his voyage he has made such a declaration.

Now how do these reports apply at all to the purpose in point, and for which the Professor designs them? It appears that the Venereal symptoms mentioned by Cook, are symptoms of the confirmed Lues Venerea; and as Cook says nothing of gonorrhœa, the conclusions of the Professor can be only presumptive, which may as well be formed out of a dream, as from this appeal.

To prove that gonorrhœa and chancre arise from the same poison, he has made a voyage to the South Seas. As I said before, no one ever doubted the fact. But no such inference

* Cook's Voyage, Vol. II. p. 148. Dublin Edition.

† Vide Page 14.

is to be drawn from any of the accounts of the state of the disease, as it was found in the islands of the South Seas. Not a word is said of gonorrhœa, and it is at least worthy of remark, and I hope my readers will particularly attend to it, that this silence concerning a gonorrhœa in thirty-three of the authors, when it was originally imported into Europe, proves it more than probable, that gonorrhœa, and chancre may arise out of lues venerea, as well as lues venerea out of gonorrhœa and chancre: or how came gonorrhœa to be noticed fifty-three years after all other symptoms?

What had, I should be glad to know, the imperfect description of a disease in general given by men unacquainted with nice venereal distinctions to do with the Professor's case in point, or what was there in the disease on the other side of the equator, that could not be better compared at home?

Page 17. *Of the Causes of the Poisonous Quality. Fermentation. Action.*

The Professor examines very abstrusely into the cause, and the action of Venereal Poison. As it is too obvious that a specific irritation belongs to venereal virus, I shall say no more upon the subject here, but wait to see if he applies his theoretic ideas to more practical success.

Page 23. *Of the different Forms of the Disease.*

The Professor says, the Venereal Disease affects the body two ways, local and constitutional. The local are gonorrhœa and chancre, the constitutional, blotches, ulcers, thickened periosteum and bones. There is also an intermediate way, which he says is the bubo. All these are more violent or favourable, rapid or slow in some than in others.

Page 26. *Of the Lues Venerea being
the Cause of other Diseases.*

The Profeffor obferves, that the lues venerea rouses into action, fcrofula, cancer, and other difeafes, where there is a predifpofing caufe in the conftitution; and with much modefty concludes, “ that it is impoffible to
“ take notice of every variety of fymptoms;
“ but that if his general principles be juft,
“ they will help to explain moft of the fingularities of the difeafe.”

If the Profeffor had throughout his work meafured the truth of his new opinions by the uniformity they fhould have produced, if his above obfervation had been his rule of right, we fhould have had no occafion to remark the contrary effects, and all have received profit, where we have met with difappointment.

Page 29. *Of Gonorrhœa.*

At length we are arrived at the immediate subject of gonorrhœa, and I congratulate my readers upon it ; for upon this theme, ingenious as the Professor is allowed to be, I think he will not have the opportunity of flourishing beyond all comparison.

He tells us, and very properly, “ that
 “ when an irritating matter of any kind is
 “ applied to a secreting surface, it encreases
 “ that secretion.” But he tells us also, but not with the same propriety, “ that this ir-
 “ ritating matter changes the secretion, which
 “ in the present disease is pus.”

The Professor says, that “ until about the
 “ year 1753, it was generally supposed that
 “ the matter from the Urethra in a gonorrhœa
 “ arose from an ulcer or ulcers in that pas-
 “ sage ; but from observation it was then
 “ proved that this was not the case.” I believe with the Professor, that it is now universally understood that ulcers in the Urethra do not prevail at least in a recent gonorrhœa ;
 or

or in other words, that the discharge, called by the Professor pus, is not produced from ulcers. This last observation of the Professor has afforded him an opportunity of furnishing us with the historick fountain from whence he drew this wonderful store of information.

A little before the above date, Dr. Hunter upon opening the thorax of a child, found in the cavity a large quantity of pus loose; with the surfaces of the lungs and plura furred over with a more solid substance similar to coagulable lymph; on removing this from those surfaces they were found entire. Mr. Sharp was present, who soon after published his Critical Enquiry, in which he introduced what the Professor calls this fact, “that matter may be formed without the breach of substance.” The Professor proceeds further to elucidate this subject, but as I think rather out of humour and somewhat off his guard, I shall drop it, having here already set down what is necessary for my purpose, and proceed to examine whether the discharge from the Urethra in a gonorrhœa be pus, or encreased mucus from irritation? For here I mean to make a considerable stand against

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the Professor, and to call to my aid every authority that I can.

Whoever has considered what was the cause of pus, uniformly agrees that it must arise necessarily out of a solution of the parts where that pus was formed. Now as no ulcer is found in a recent gonorrhœa, as no destruction of the parts are offered in Vindication of so uncommon an assertion, that the discharge from the urethra is pus; why has the Professor so roundly given this secreted fluid the unqualified appellation of pus? The definition of the word will not admit its application here, and the definition of the discharge proves that fact. A gonorrhœa may appear in six hours after infection, or it may be six weeks, so the Professor admits; but no person ever felt irritation in the urethra without looking for, and expecting at the same time to see discharge. The former is seldom present unattended by the latter.

Does our observation upon this, answer to the idea of pus? If without any infectious impuration, what the professor calls a simple gonorrhœa makes its appearance, and if, it has followed almost immediate connection, here

more discharge is instantaneously produced from less irritation and no virus—What was the process to form this pus? Where is the loss of substance? From whence does such a deluge of fluid draw its source? Where was the previous inflammation? The fever? No, says he, “when an irritating matter of any
 “kind is applied to a secreting surface, it en-
 “creases that secretion and changes it from its
 “natural state (whatever that be) to some
 “other which in the present disease is pus.” but in the simple gonorrhœa, unfortunately for the Professor, no irritating matter was applied that either he, or I know of, for I will take upon myself to answer for him in this instance, yet the discharge appears to be the same; for who at sight can make the distinction?

If the Professor had expressed himself somewhat after the following manner, for I would not differ with him about the mere word, “Ir-
 “ritation upon the secreting surface of the
 “urethra may produce discharge without
 “breach of substance; this ought to be called
 “increased mucus from an irritating cause;
 “but as I shall be often under the necessity of
 “using this compound expression, and as I am
 “at a loss for a word that expresses its full

“ meaning, I shall use the word pus ; but at the
 “ same time, I beg to be understood, that the
 “ word pus, is not to be conceived in this in-
 “ stance in its general, but in the abstracted sense
 “ that I have now given to it by this explana-
 “ tion.” This would have satisfied me, but no-
 thing less shall satisfy me ; I shall continue to re-
 fuse my consent to the Professor’s opinion, and
 although I pretend to no uncommon sagacity,
 although I have not made mankind my study
 with that zealous and unremitting ardour that
 the pious anchorite performs his duties in reli-
 gion, yet I know why the Professor persists to
 call this secreted fluid pus ; and my readers shall
 know it, because Mr. Pott says it is not pus. I
 shall content myself with flinging the weight
 of his authority into my scale, and then leave
 it to my readers to determine which of us kicks
 the beam.

“ These two fluids, pus and mucus, which
 “ have been so frequently confounded together,
 “ do really differ so widely from each other in
 “ their nature, constitution, sources, purposes,
 “ and effects, that to distinguish them proper-
 “ ly, and to point out the true character of
 “ each, seems to be a matter of much import-
 “ ance :

“ ance : it would carry me too wide from my
 “ present purpose to attempt it in this place,
 “ and therefore I shall only just mention what
 “ may serve merely to illustrate that.

“ If I conceive rightly of this affair, mucus,
 “ considered in a general sense, is the effect of
 “ a natural secretion made by glands, mem-
 “ branes, or other bodies appointed for that
 “ purpose, and is so far from being originally
 “ the consequence of disease, that, in a due
 “ quantity, it is absolutely necessary for seve-
 “ ral very important purposes, in the animal
 “ œconomy ; which purposes, when this fluid
 “ is deficient, must be ill-executed, and some
 “ kind of disease or defect follow : whoever
 “ will reflect upon the uses of it in the intes-
 “ tines, joints, sheaths, or capsulæ of the
 “ tendons, in the sinuses of the skull serving
 “ the purposes of speech, in the cavity of the
 “ nose, where the olfactory nerves do their
 “ duty, in the prostate gland, larynx, trachea,
 “ urethra, and vagina, will be easily convinced
 “ of the truth of this assertion, both with
 “ regard to its natural uses in a healthy state
 “ and proper quantity, and the share it fre-
 “ quently has in the production of diseases,
 “ when it is either vitiated or redundant.

“ Pus,

“ Pus, or matter, is certainly no natural
 “ secretion; supuration, though it is an act
 “ of nature when some parts of the body have
 “ been forcibly divided from each other, is
 “ nevertheless to be regarded as the effect of
 “ violence and destruction, at least of division;
 “ for, without entering minutely into the
 “ origin or nature of it, I believe I may ven-
 “ ture to affirm, that the dissolution of some
 “ of the solid particles of broken cappillary
 “ vessels, and a mixture of some part of the
 “ juices which should circulate through them,
 “ make a necessary part of its production;
 “ however constant its appearance may be in
 “ the progress toward healing a wound, or
 “ sore, yet it never is produced, even in the
 “ smallest quantity, without some degree of
 “ erosion, some breach in the natural structure
 “ of the parts; and when such breach is heal-
 “ ed the discharge necessarily ceases.

“ On the contrary, mucus may by irrita-
 “ tion, relaxation, or defluxion, on its secre-
 “ ting or containing parts or organs, be in-
 “ creased to a quantity far beyond what is
 “ necessary or useful, and produce thereby a
 “ disease in parts where there is not the least
 “ degree of solution of continuity, as in the
 “ cases

“ cases of tenesmus stone in the bladder, fluor
 “ albus, and simple gleet from the urethra;
 “ as also in that kind of defluxion on the nose
 “ and fauces, producing a catarrh, and in the
 “ immediate effect of all sternutatories.

“ Other differences between the nature and
 “ properties of the two fluids might be men-
 “ tioned; but if these already cited are just,
 “ they will be sufficient to evince the impro-
 “ priety of confounding them together, either
 “ with regard to theory or practice.

“ Nor is this mistake of discoloured mucus
 “ for matter confined to the lachrymal sac
 “ only; the two circumstances of pain, and
 “ yellow colour, having in almost all times
 “ produced the same misconception in the
 “ virulent gonorrhœa of both sexes: this has
 “ been called pus, and being said to proceed
 “ from ulcerations in the urethra and vagina,
 “ though the repeated testimony of those who
 “ have, immediately after death, examined
 “ the parts of persons so diseased, has often
 “ been produced to the contrary, and though
 “ the discharge itself, when properly examined
 “ will always prove the contrary: inflamma-
 “ tion and irritation of the membranous linings
 of

“ of the urethra, and vagina, will fully ac-
 “ count for all the appearances in this disease
 “ in which there is neither matter, nor ulcer,
 “ nor abscess: whoever will attend to the dis-
 “ charge made from a purulent ulcer, will
 “ find it widely different from that which
 “ issues from either of the above parts in the
 “ gonorrhœa.” *

So much for this question, which I cannot quit without expressing my grateful thanks to the author, whose opinion I have just borrowed, for the great information I have received from his works in surgery. His lively imagination, deep penetration, enlightened understanding, and great experience have raised him above my panegyrick; his skill in surgery will be long remembered; “ he has not only lengthened but gladden life” †.

Page 31. *Of the Time between the Ap-
 plication and Effect.*

The Professor examines into the different distances in time, for gonorrhœa to make its

* Vide Pott on Fistula Lacrymalis, p. 313.

† Johnson.

appearance after infection; and, in order that there may be scope enough, he grants a latitude from six hours to six weeks. This I think is allowing a generous come off, both for the patient as well as for his surgeon; and it is also well calculated to make the position unexceptionable to the opinion of every man who thinks for himself. How true it is, that the effect is sometimes six weeks after the application I will not contend, I cannot help my doubts. But I never have yet heard of or seen, as he has, that the inflammatory state took place before the discharge for a considerable time. The Professor would have cause to exult, if it were the case; for upon that principle alone his theory is built. If the inflammatory state did take place for a considerable time before the suppurative, that would be a satisfactory reason for his calling the discharge pus. But with his leave in such an instance only.

Page 32. *Of the Difficulty of distinguishing the virulent from the simple Gonorrhœa.*

This section is so replete with positions of all sorts, and of all times, so cramed with dis-

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charges

charges that may be produced from every possible cause, without a single clew to aid us in distinguishing one sort from another, that it seems to be written for the purpose of confounding all distinctions. And I certainly should have passed it over, had there not been found in it, such materials as clearly assure me, that the Professor's singularities have not even system for their basis.

I congratulate my readers that we recognise here the old word virus, as I feared that the Professor had banished it out of his venereal vocabulary. Through all his distinctions of simple, and virulent gonorrhœa, I cannot follow him. A simple gonorrhœa, and here we agree, may or may not fall to the lot of a man who never had any venereal complaint, or any chance of infection.

But, there is a sentence in this curious section that perhaps may excite the curiosity of my readers, and be worth my observation, “ In
 “ this complaint a discharge, and even pain,
 “ attacks the urethra, and strange sensations
 “ are every now and then felt in these parts,
 “ which is either a return of the symptoms of
 “ the venereal disease without virus, may arise,

“ ass

“ as it were spontaneously, or may be a consequence of some other disease.” I shall not dispute with the Professor upon grammar for many delicate reasons ; and I do assure him, I will never wilfully misconstrue any of his ideas ; I would not accept of a triumph upon any such ground. If I can understand his true meaning, it is all I desire. As the quotation is before us it may not be improper to ask the Professor, If he calls the discharge without virus, pus also ?

Page 34. *Of the Common final Intention of Suppuration not answering in the present Disease.*

In other words, the discharge from the Urethra being increased secretion, and not suppuration, it cannot be reconciled to the doctrine of suppuration. The Professor says, “ that general irritation upon secreting surfaces endeavour to produce their own destruction like a mote in the eye, which by increasing the secretion of tears is itself washed away.” This observation is very

pretty, very obvious, and shrewd. But suppose the Professor as an experimental philosopher, was, instead of the mote, to apply gonorrhœal discharge to the eye, would that be so readily washed away? I have seen two consequences from two accidental applications of gonorrhœal discharge to the eye: The one was attended by me, and the Professor himself was called in to be consulted upon it*. The other was attended by me, and Mr. Pott was called in and consulted upon it. One eye was totally lost, and the other was with difficulty saved.

Suppose a person to be attacked with a cold, and a great discharge is produced from the nose; Is the irritation in this instance got rid of after the same ingenious manner? What is it that produces that irritation? How long is the nasal membrane irritated before the increased secretion appears? Does the Professor call this increased secretion pus, in all its conditions, either when thin and diaphanous like gum and water, or when like an usual secretion only in increased quantity?

* Vide my Critical Enquiry, fourth Edition, page 71.
Case XI.

The Professor says, “ that the Venereal
 “ Disease is not kept up by the pus which is
 “ formed, but like many other specific dis-
 “ eases, by the specific quality of the inflam-
 “ mation itself.” The Professor and I do not
 differ much in this opinion. It is a specific
 irritation that produceth an increased se-
 cretion ; which secretion will be both in qua-
 lity and quantity, generally in proportion to
 the influence of the specific stimulus on the
 surface of the Urethra. The secreted mucus
 must therefore be considered as a symptom
 and not the cause.

At length we are approaching to one of
 those new opinions of the Professor, the right
 of which he is welcome to assert, and so may
 his friends, until they are tired out in the same
 manner, that he says the specific irritation
 tires out the parts on which it acts.

“ As the living principle in many diseases
 “ is not capable of continuing the same
 “ action ; it also loses this power in the pre-
 “ sent, when the disease is in the form of a
 “ gonorrhœa, and the effect is at last stopped,
 “ the irritation ceasing gradually. This ces-
 “ sation will vary according to circumstan-
 “ ces ;

“ ces ; for if the irritated parts are in a state
 “ very susceptible of such irritation, in all
 “ probability their actions will be more vio-
 “ lent and continued longer ; but in all cases
 “ the difference must arise from the differ-
 “ ence in the constitution, and not from any
 “ difference in the poison itself.

“ The circumstance of the disease ceasing
 “ spontaneously, only happens when it at-
 “ tacks a secreting surface, and when a se-
 “ cretion of pus is produced ; for when it at-
 “ tacks a non-secreting surface, and produces
 “ its effects there, that is an ulcer ; the parts
 “ so affected are capable of continuing the
 “ disease, or this mode of action for ever, as
 “ will be taken notice of when we shall here-
 “ after consider chancre. But this difference
 “ between spontaneous and non-spontaneous
 “ cure, seems to depend more on the differ-
 “ ence in the two modes of action, than in
 “ the difference in the two surfaces ; for when
 “ the disease produces an ulcer on a secreting
 “ surface, which it often does from the con-
 “ stitution, as on the tonsils, it has no dispo-
 “ sition to cure itself ; nor in the Urethra, in
 “ a recent case, if ulcers are formed there,
 “ would

“ would they heal more readily than when
 “ formed any where else.

“ The common practice proves these facts ;
 “ we every day see gonorrhœa cured by the
 “ most ignorant ; but in chancre, or the lues
 “ venerea, more skill is necessary . The rea-
 “ son is obvious, gonorrhœa cures itself,
 “ whilst the other forms of the disease re-
 “ require the assistance of art.

“ It sometimes happens that the parts
 “ which become irritated first, get well,
 “ while another part of the same surface re-
 “ ceives the irritation, which continues the
 “ disease, as happens when it shifts from the
 “ glans to the urethra.”

“ From this circumstance of all gonor-
 “ rhœas ceasing without medical help.” &c.
 &c.

I trust that my readers will not attribute
 any quotations that I make to a desire of tres-
 passing on their patience, or of swelling this
 pamphlet. Nor am I ambitious, however
 highly I respect the talents of the Professor
 in general, of idly ingrafting into my humble
 performance

performance what so particularly belongs here to him. But that the Professor may not have a wrong construction put upon his meaning by me, it is as necessary to my character, and to his own, as to the cause of truth.

I have therefore laid the above before my readers that they may judge for themselves, both as to the new opinions and to the language that conveys them.

These assertions of the Professor, that gonorrhœa cures itself, and that all gonorrhœas cease without medical help, are perhaps the broadest, and the boldest that ever were offered by man, from the earliest writer on this subject, down to the present hour. It is here publicly announced as a general position without any exception whatever, that all gonorrhœas cease without medical help; that gonorrhœa cures itself. He has qualified these assertions with nothing like an exception; for if ulcers were formed in the urethra, the disease would be no longer gonorrhœa; it would require medical help; it cannot therefore answer at all to his definition of gonorrhœa; to this he agrees, as he says, “ till
“ the year 1753, it was generally supposed,
“ that

“ that matter in the urethra in a gonorrhœa
 “ arose from ulcer or ulcers in that pas-
 “ sage ; but from observation, it was then
 “ proved, that this was not the case *” This
 therefore cannot even by the Professor be con-
 strued into an exception.

The Professor “ doubts very much the pro-
 “ bability of a person getting a fresh gonor-
 “ rhœa while he has that disease ; or of en-
 “ creasing the same by the application of fresh
 “ matter” I will defy him to prove that the
 gonorrhœa cannot be continued by the appli-
 cation of fresh matter as he calls it ; but the
 Professor has with much modesty asserted his
 doubts upon the subject : At all events, I am
 confident, that as long as the urethra is sus-
 ceptible of irritation, so long will it be cap-
 able of being acted upon by fresh infection.
 That the secreting surfaces may part in some
 measure with their irritability I believe is very
 true. The taking of snuff begins with pain
 mixed with pleasure ; its continuance affords
 pleasure only by gently stimulating the sur-
 face ; the edge of irritability in the nasal
 membrane is blunted but not destroyed. The

* Page 29.

repetition of snuff gives pleasure to the end of life, and the secretion is encreased by every fresh application.

Let the Professor stand forth, and declare, that gonorrhœa is not continued by fresh connection with venereal subjects, for this is his opinion ; but unfortunately for him the cases he has produced in this chapter do not prove all that he wishes, they tend rather to contradict his innovation ; they only prove that the irritability of a fresh subject is greater than the irritability of one in the constant habit of promiscuous venery.

But supposing these cases produced by the Professor had gone further ; such would not be the sort of proof that could so far be relied upon, as to destroy one theory for the purpose of setting up another ; the declarations of the ladies upon this question are governed by their natural propensity to innocence ; they approve so much of purity and modesty, that they assume these virtues, when they no longer have them. Their assertions are sometimes found to be faulty. The moral truth of this I believe will be felt by most of my readers.

I shall

I shall here match two paragraphs of the Professor.

He says, “ that gonorrhœa may be cured
 “ while there is a chancre, and *vice versa*.
 “ Now, if fresh venereal matter was capable
 “ of keeping up the disease, no gonorrhœa
 “ could ever get well while there is this
 “ supply of venereal matter.”

In page 17, he says, “ I have seen cases
 “ where a gonorrhœa came on, and in a few
 “ days in some, and in others many weeks,
 “ a chancre has appeared; and I have
 “ also seen cases where a chancre has come
 “ first, and in the course of its cure a running
 “ and pain of making water have suc-
 “ ceeded*. It may be supposed that the
 “ two diseases arose from the original infec-
 “ tion, and only appeared at different times,
 “ and their not occurring oftner together
 “ would almost induce us to believe it was so,
 “ since the matter is the same in both, and

* Here is inflammation after running.

“ therefore capable of producing either the
 “ one or the other †.”

To explain the theory of his first paragraph, the Professor appeals to the Abbé Fontana's Experiments, an author that I hold in the highest admiration. The Abbé says, that the viper cannot be affected by his own poison. Is this applicable to gonorrhœa not producing chancre? or *vice versa*? The poison of a viper is organised by, and originates from himself: It is only poisonous when applied to another; to the viper himself it is no poison: But the venereal disease is a poison, that the individual who infects another, has received, and already felt the effects of it, either as in chancre or gonorrhœa, or both in the first instance; or the one out of the other. He has received the infection, and he can impart it, still retaining the disease in all its force. Is it not true, if the disease be not counteracted by remedy, that a gonorrhœa alone in the first

† How admirably the latter part of the second paragraph is fitted to the same reasoning in the swelled testicle! Why did not the Professor also presume, that, because a swelled testicle does not occur oftner, it arises from the original infection.

instance

instance can produce phymosis, paraphymosis, chancre, bubo, swelled testicle, affections of the bladder, chordé? still a longer train of symptoms?

This fact the Professor knows as well as I do, and he will prove to us that he knows it hereafter.

It is impossible for him to illustrate the first paragraph by the second, or *vice versa*. To illustrate the first, he produces a most outrageous, unsuitable comparison in the viper; and the second borrows all its illustration from the rarity of the circumstance.

But the Professor is deficient in the very essence of that which constitutes a successful skeepick. For where is that halcyon smoothness in language to charm us! where the dissembling and persuasive argument that assimilates illusion to truth!

Page 41. *Of the Seat of the Disease
in both Sexes.*

The Professor says, “ that the disease seldom attacks the orifice of the urethra without passing some way along the canal.” The reason of this must be very obvious. If the whole glans be smeared with the infectious virus, the secreting surface will be the first irritated, and if the person infected be cleanly, perhaps the only part put into action at all will be that which is out of his reach to prevent.

The Professor says, “ the secretion of pus with so little inflammation (in the beginning of gonorrhœa) is perhaps owing to these parts being naturally in a state of secretion ; therefore the transition from a healthy to a diseased secretion is more easily produced.” I thank the Professor for his nearer approach to the general idea of encreased secretion in this instance

instance than in any before. The professor sat out with telling us “ of the discovery of matter being formed by inflammation without ulceration *.” The assertion is now softened down to a transition from a healthy to a diseased secretion with an inflammation, that is so moderate that it can “ hardly be called so.” This is a compromise that I did hardly expect, after all that has been pledged; was the Professor aware of it? or was it inserted by accident?

Page 43. *Of the most Common Symptoms, and their Order of Appearance.*

Here, he says, with the same singularities in grammar and theory, † “ the effects of inflammation is next observable, and soon after a running appears. There is often no pain till some time after the appearance of the discharge and other symptoms.” I do not mean to oppose the Professor’s enumeration

* Page 29.

† Page 44.

of the symptoms, they must be described as they naturally offer. I therefore only stop to beg from him an explanation of what appears to me now a mystery ; as in the former part of this quotation, inflammation comes before running, so in the latter, pain (that is inflammation) comes after running.

Ignorant and dark as our understandings are, we look up to the only illumined source of all venereal knowledge, with anxious expectation, to know if pus really comes before inflammation, or inflammation before pus ?

Page 46. *Of the Discharge.*

It has never yet, I believe, been doubted by the thinking part of the profession, but that the venereal disease is to be propagated only by infection conveyed in some substantial form or other. The interval of time between the infection being received, and the appearance of a gonorrhœa, may perhaps be filled up with cohabitation without harm, and with pleasure

pleasure unallayed by after ill consequences.

But the Professor has carried this idea to such a length, that if it be followed into practice, cannot in its effects prove otherwise than vicious. “ I could even allow a man
 “ who has a gonorrhœa to have connection
 “ with a sound woman, if he took care to
 “ clear all the parts of any matter, by first
 “ syringing the urethra, making water, and
 “ washing the glans*.” The Professor has asserted, and with truth, in another part of his work, “ that no man can will an erection :” How then can he answer for the man, that he will have compleated his connection, before that additional infecting mucus will be secreted ? This advice from the Professor is the more wanton, because it is unprofitable. The husband who follows it, will be an unfeeling fool. The innocent wife would have cause to exclaim against both. “ This may be sport to you, but it is
 “ death to me.”

* Page 12.

The Professor meets the general opinion of the nature of the discharge much nearer, when he comes to describe it as it is in reality, than when he attempts to paint it from his own prolifick imagination. He is too modest to call it pus ; he calls it discharge ; and, as if he had felt some compunction already for the crime of imposing a wrong sense, on what was before understood in a right one, he as much as says, that if the discharge be not pus, “ it is the natural slimy discharge from the glands, and “ the natural exhaling fluid of the urethra, “ which is intended for moistening that surface, and which appears to be of the same “ kind with that which lubricates cavities in “ general, become less transparent, and both “ these secretions becoming gradually thicker, “ assume more and more the quality of pus.” In other words, it looks like pus, but it is only encreased mucus. I can readily conceive, that if we lived in the days of enchantment, and the charm was so well worked up as to oblige the Professor to speak his mind, we should hear from him an exclamation somewhat to the following purpose. Why would not Mr. Pott agree to call this discharge pus, especially as my brother, and I, had dictated that it should be so called ; did not Mr. Sharp believe us,

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and propagate our doctrine? And would not Mr. Gataker have rivaled us in our claim? How mortifying is it at last that the novelty is ridiculous! that it cannot after all be acknowledged as pus! That however I have claimed, as successor to my brother, the honour of the discovery, a more solid opinion proves at length that my theory was founded upon a false precipitate decision, and that what I call pus deceives me, because it assumes the appearance of pus! “Do you
 “see yonder cloud, that’s almost in the shape
 “of a camel?—Methinks it is like an ouzel
 “—It is black like an ouzel.”

Page 49. *Of the Chordé.*

The Professor says, “that the chordé as
 “in the adhesive inflammation †, unites the
 “cells together, destroys the power of dis-
 “tension of the corpus spongiosum urethræ,
 “and makes it unequal in this respect to the
 “corpora cavernosa penis, and therefore a

* One of his new opinions.

“ curvature on that side takes place in the
 “ time of erection.”

I see no reason why there should be an appeal to adhesive inflammation to explain chordé, nor do I think that such appeal is applicable. To fly from causes that we know, to those we doubt, would be making a surrender of our senses before necessity calls for it.

Inflammation in common being generally understood, I shall undertake to define what a chordé is under the favour of that aid only.

Parts when inflamed thicken and lose the power of action they enjoyed before they were attacked by inflammation. The component parts of the corpus spongiosum urethræ being, in consequence of inflammation, already stretched in their reticular directions have no power to yield to further distension. And when the corpora cavernosa penis in erection naturally extend, being in a state of perfect health, the inflamed corpus spongiosum urethræ cannot favour the disposition; therefore the pain of chordé is felt whilst the acting
 power

power in the one, is antagonising the suspended power in the other.

Page 50. *Of the Manner in which the Inflammation attacks the Uretbra.*

The Professor and I agree, that the infection applied to the lips of the urethra creeps along to the inner surface; and indeed there is no other way for it. To prove this, as if there was a difficulty, the Professor applies a case of a man and a wall*. Every day would have afforded him opportunity of better proof of a man and a woman.

But the Professor has exchanged almost through this treatise his own certain ground of experimental solid proof, for the hearsay gossip of others. A strange tale to prove a fact, is not half so important, as a plain true relation founded upon common usage. I have known a person, who, examining a girl just as he was about to enjoy her, and finding that

* A gentleman caught a gonorrhœa by a bit of plaster, from a necessary, sticking to the point of the penis.

she was injured, declined the embrace ; the same fingers he felt her with, were afterwards employed in handling the penis ; in three days he was attacked with a virulent gonorrhœa. But this, although a better proof than that brought by the Professor, is not the best proof. The true manner of proving the case, and the way that the Abbé Fontana would have gone about it, would have been to charge a finger with gonorrhœal infection, and apply it to the lips of the penis of a sound person. The veracity of a second person, and the episodical trash of a piece of a wall would be here out of the question.

I come now to a singular opinion of the Professor, “ When the disease attacks the
 “ urethra, it seldom extends further than an
 “ inch or two inches at most within the ori-
 “ fice, which distance appears to be truly
 “ specific, and what I have called the specific
 “ extent of inflammation.” I will not be at the trouble of answering this—it is not worthy of an answer. I will refer the position to every man who has had a gonorrhœa, and when I appeal to others feelings, I think I am acquitting myself upon this self-evident question, with more candour as to my own. But
 this

this artful overstrained assertion was meant for the better introducing one of our Professor's new opinions.

The symptoms usually attending a gonorrhœa of irritation of all the neighbouring parts, have naturally given the Professor the long wished for opportunity of attributing the affections of scrotum, testicles, perinæum, anus, hips, and glands of the groin to Sympathy; but of the glands of the groin only, when they do not come to suppuration; “when, says he, “they inflame from the absorption of matter, they in general suppurate.”

“When the bladder is affected it becomes
 “more susceptible of every kind of irritation.
 “The urethra, and even the kidneys, sometimes sympathise, when the bladder is
 “much inflamed, or under considerable degree of irritation.” This may justly be called confusion, more or worse confounded; for we first of all find irritation, then sympathy, and then irritation again; according to the Professor's law of specific extent of inflammation, there was no more virus lodged on the neck of the bladder, than on the
 kidneys.

kidnies. Then why not call both irritation, or both sympathy?

After all the ostentatious embellishment of new opinions, in the introduction to this great work, and after all the usurpation of this mere word, sympathy; in what, either in definition or application, is it preferable to the word irritation?

I trust that I shall make it appear to my readers, that the word sympathy, is not only absurd in every appropriation where the word irritation would have been equally misplaced, but also, that the word itself, in its true simple meaning, is not so apposite as the word irritation.

Where a symptom is the effect of irritation, and has always been understood as the effect of irritation in a gonorrhœa, there sympathy may, although not with as much propriety or energy, be substituted; and the exchange would be so harmless as not to be worthy of a contest. I will venture to assert, that, let the Professor carry his influence of sympathy as far as he will in accounting for the most singular symptoms that the animal
frame

frame is liable to, there irritation must meet the meaning with more force.

When a person has received a blow on the head, and in consequence of it, is attacked with a vomiting, and suppression of urine, a symptomatick fever; I should attribute these effects to irritation. When a person has a virulent gonorrhœa, solicits constantly to make water, has a chordé, feels erratick pains in his thighs, hips, and about the pubis, I should attribute these effects to irritation. When a person is attacked with the tooth-ach, the whole cheek swells, the glands swell also, the aching tooth cannot be always ascertained, all the adjacent parts are involved in the event, even delirium is joined in it; I should attribute these effects to irritation. Any part of the same individual in pain may produce irritation on other parts that are predisposed to be susceptible of irritation from the pain of that part. Irritation is applied to affections of the various parts of an individual only, independent of another. Sympathy has been, and ought always to be applied to affections that one person feels for the condition of another. Those who faint or fall into hysteric fits at the catastrophé of others,

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do

do really sympathise. When Mrs. Siddons, in some distressful scene in tragedy, has fascinated and worked upon the passions of the audience, I have seen one lady fall into fits, she has echoed the shriek of the empassioned actresses, another and another have caught it from the first, and like an electric shock it has been felt through the whole circle.

Page 54. *Swelled Testicle.*

If the Professor had for the sake of being singular only introduced the word sympathy for irritation, I should, after what has already been said, have dropped the subject without making any further comment upon it. For no man can be more disposed to let the Professor off, where he has erred from accidental or venial faults, than I am. But when his new opinions are persisted in, and with all their mischievous errors, are brought forward into practice; when such bold inconsistencies are obtruded upon us, and the only choice before us is, the alternative of defending the right by resisting the wrong; I shall no longer consider myself as a careless spectator,

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but as a centinel upon the watch, as bound to oppose opinions injurious in their effects, and which, if I rejected, would be dishonourable in me ; for silence would be deemed acquiescence, and the mischief would steal into practice by imperceptible gradations.

The Professor we find now mounted on the hobby-horse, that we had only before seen capering before us. But I flatter myself, that he can only gain the plate, by being suffered to walk over the course. He asserts that the swelling of the testicle is not owing to venereal infection, but to sympathy only.

“ From what I have said of its nature
 “ when it arises from a venereal cause, which
 “ was, that it is owing to sympathy only ;
 “ and from what I shall now say, that it is
 “ never affected with the venereal disease,
 “ either local or constitutional, as far as my
 “ observation goes, we must infer that such
 “ suspicions are always ill-founded. This
 “ perhaps is an inference to which few will
 “ subscribe.”

The only true method to describe a disease is to set down the symptoms in the order that

they offer in common, and if an exception should arise now and then out of the common order, that exception should also be noticed, but in so clear and distinct a manner, as that it might always be known as an exception, and never confounded with the common order.

It is a melancholy reflection, that when men, from whatever cause, have gained a reputation with the public, they should sport with their understanding, as well as their safety. I should be sorry to charge the Professor with having done so; but why did he not at once state the manner of the swelled testicle coming on, as it does, I will venture to assert, nineteen times out of twenty, and reasoned upon the most probable cause of this usual attack? Instead of which, he has introduced this prevailing observation, as if it might or might not have been left out of his chapter, and as if it did not weigh a feather in informing us of what nature a swelling of the testicle is, that arises out of venereal infection. As the appearance I allude to so universally prevails, it ought to be considered as most worthy of our investigation, and not what might happen from extraordinary causes.

But

But so plain a truth would not favour our Professor's singularity; it would not have given him the opportunity of sporting his new opinion. He must distort truth, for the sake of new theory. He creates a disease, and then builds a system upon it.

I will now lay before my readers this general observation of the coming on of the swelled testicle, which he had too much modesty to omit, although he has but barely remarked it.

“ As singular a circumstance as any respecting the swelling of the testicle is, that it does not always come on when the inflammation in the urethra is at the height. I think it oftener happens when the irritation in the urethra is going off, and sometimes even after it has entirely ceased, and when the patient conceives himself to be quite well.”

This is all the notice that the Professor takes of what he calls this “ singular circumstance;” and the reason is very obvious to me, why he did not venture to enlarge upon it; because, when he asserts that the swelled testicle

testicle arises from sympathy, he is too cunning and too guarded not to know, that when the swelled testicle comes on after irritation has entirely ceased, and when the patient conceives himself quite well, there is nothing for the testicle to sympathise with. I earnestly request the attention of my readers to this fact: and I now call upon the Professor to declare in such a state of the urethra, what has the swelled testicle to sympathise with? when there is no pain, no inflammation, no irritation in the urethra to provoke a sympathy in any other part: Is it possible that this shadow of the Professor should exist without a substance? But what will he say, when I tell him that this which he calls a “singular circumstance,” is the most general and common manner of the attack of the swelled testicle. I do assert that it is, and I leave it to my readers to determine between us from what has fallen within their own observation.

The swelled testicle must be produced from irritation in the urethra, or from virus conveyed to the testicle. When a gland is irritated from pain in a contiguous part, it never happens but when that pain is at its greatest

greatest excess, and as the pain declines, the gland naturally returns to its healthy condition. This is the case of the enlargement of the inguinal glands, which is clearly symptomatic in a gonorrhœa when at its highest state of irritation. Why is not the testicle always irritated by a gonorrhœa in such a state? or, why is it scarcely ever irritated? Why does the swelled testicle come on without gonorrhœal irritation, rather than when the urethra is irritated? Because it is incapable of being affected without the immediate action of virus. The mild symptoms of a gonorrhœa, previous to the pain of the testicle, and the attack of the epididymis first, prove that the virus has been continued from the urethra to the testicle. Did the attack of the swelled testicles alway come on while the inflammation was at the greatest height in the urethra, and, did the inflammatory symptoms in the urethra actually cease, after the pain was felt in the testicle, the swelling might be said to originate from irritation without virus, or from sympathy, if the Professor likes it better; but the fact proves the contrary. I will put one question to the Professor before I drop this subject, although I know I am anticipating the page that instructs us how

to

to cure this unsubstantial shadow. Upon your honour, do you cure all swelled testicles arising from gonorrhœa in all its stages without mercury internally or externally applied, and pledge yourself for all after consequences, whatever?

Page 57. *Of the swellings of the Glands from Sympathy*

The Professor introduces here the doctrine of absorbents, as it was taught by Dr. Hunter to whom the honour is due. Dr. Hunter's labour in the pursuit of every thing he undertook, and his vivacity and elegance, in the explanation of every subject at his anatomical lecture room, are too strongly impressed on the memory of those who heard him, and his fame is too well known in general to receive any embellishment from my pen.

“ But although we know the manner in
 “ which substances get into the circulation;
 “ and on having learned that many substances,
 “ especially poisons, in their course to the cir-
 “ culation, irritate the absorbent glands to in-
 “ flammation. We might naturally suppose
 “ such

“ such swellings accompanying complaints in
 “ the urethra attended with a discharge, to
 “ be owing to the absorption of that matter,
 “ and therefore if it be venereal discharge that
 “ they must also be venereal. But we must
 “ not be too hasty in drawing this conclusion;
 “ for we know that the glands will sometimes
 “ swell from an irritation at the origin of the
 “ lymphaticks; where no absorption could
 “ possibly have taken place.” Here the Pro-
 fessor instances, that, from a prick in the finger
 from a clean sewing needle, the lymphatick
 glands will swell, but that “ such irritation
 “ soon goes off.” This the Professor points out
 to show, that the absorbent system is as capable
 of being affected by irritation, as by the absorp-
 tion of matter. To all this, I say yes, and
 do congratulate the Professor on his return to
 plain solid argument. If, in the highest ex-
 cess of a gonorrhœa, the lymphatick glands
 swell, and the discharge of gonorrhœa continues;
 If, when the inflammation subsides, the swell-
 ing of the glands subsides also; I should not
 doubt but that such swellings were owing to
 irritation: but if, on the contrary, the swell-
 ings of the glands remain after the inflamma-
 tion and discharge from the gonorrhœa are past,
 whether they suppurate or remain in an indura-

ted state, there is all presumption to suspect absorption, and nothing less should be thought but that it had taken place. But the professor has spared me any more trouble upon the subject, for, under the influence of a kindly disposition, he agrees, that “ however there are
 “ swellings of these glands from actual absorp-
 “ tion of matter in gonorrhœa, and which
 “ consequently are truly venereal; and as it is
 “ possible to have such, they are always to be
 “ suspected.”

In the latter part of this section, the Professor tells us, that it is difficult to account for the nature of those “ sympathetic diseases:” I must confess for myself, since we have agreed that the glandular swellings may arise in gonorrhœa either from irritation, or absorption, and since we know that these swellings go down with inflammation in the urethra, when produced by irritation, and remain after all inflammation has ceased, when produced by absorption, that we know as much as we can, and enough to account for, and cure the one or the other. And that these “ sympathetic diseases,” were always clearly understood by every one in the profession, who had discernment to guide them, confidence enough not to be
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be mislled, and modesty enough not to nickname as “sympathetic diseases,” what were known before as affections from irritation.

Notwithstanding what I have already said might be conclusive as to the absurdity of sympathy, yet I cannot help remarking for the credit of all reputable practitioners, who have taken their directions from observation, that they never betrayed the least jealousy of the effects of swellings of the glands, when they correspond with urethral inflammation. Such have always been considered by them as the consequence of irritation, and without a new opinion, either stolen* before publication, or learnt from the Professor’s work after, have been treated heretofore with security and science. I trust I shall hear no more of the unimportant vanity of sympathy, a newfangled appropriation, and equally discordant here, as the word “accommodate” was to Bardolph: this is my jocular opinion of it; but I am deceived if my readers have not found it out as soon as I, to be seriously a monstrous imposition.

* Vide Introduction to the Professor’s Work.

Page 59. *Of the Diseases of the Lymphatics in a Gonorrhœa.*

The Professor does not dwell long on this subject, only to point out, “ that, when there
 “ is an excoriation or discharge from the pre-
 “ puce or glans which may be called a ve-
 “ nereal gonorrhœa of these parts, a hard
 “ chord is felt leading from the prepuce
 “ along the back of the penis, and the glands
 “ inflame.” Upon these, he remarks, “ that
 “ from the observation of the lues venerea
 “ being seldom produced from a gonorrhœa,
 “ it appears that a whole surface, or one only
 “ inflamed does not readily admit the absorp-
 “ tion of venereal poison ; and therefore, al-
 “ though the venereal matter lies for many
 “ weeks in the passage, and over the whole
 “ glans, it seldom happens that any absorp-
 “ tion takes place.” Here is one of the
 many opportunities, that the Professor seizes
 to sport the valuable treasure of science,
 which he has throughout his work convinced
 us he particularly possesses. The symptoms of
 gonorrhœa are held in contempt by him, and
 he

he is eager at all times to repeat to us, that the discharge is harmless that flows from a gonorrhœa, and that his doubts of bad consequences arising from it, are all at an end. Notwithstanding there are absorbents, yet they do not absorb. Notwithstanding glands suppurate, yet they are not infected with poison. If gonorrhœa remains, there it may with innocence. If testicle swells, if fever runs high, if pain be acute, if matter be formed, I doubt, says he, that venereal poison has produced these symptoms; they cannot be venereal; they do well without mercury. And is there no probable way for venereal poison to pass into the habit? If a whole surface, or one only inflamed, will not permit it; will not wounds of any sort be passports for its admittance? for as we all know that there is such a disease as a constitutional lues venerea, tell us, if venereal poison does not pass through that channel? No; says the Professor, "I am inclined to think that wounds are also bad absorbing surfaces, especially, when I consider, that few morbid poisons are absorbed from wounds."

My

My readers will be pleased to recollect, that the Professor, in page 9, calls the venereal a morbid poison, to distinguish it from animal, vegetable, and mineral poisons. The same Professor, in a note, page 36, compares, without any pretence to analogy in that instance, the virus of a viper, and venereal virus, as, I trust, I have in my observations upon his note made it appear. That there may be comparative reasoning on the effects of all poisons upon the subjects poisoned, I readily agree; but the Professor, to establish his opinion, that wounds are bad absorbing surfaces for poisons, and especially morbid poisons, would not venture a comparative elucidation. I shall with pleasure perform that duty for him, and prove to my readers, that all animal, some vegetable, and even morbid poisons are soonest received into the habit by wounds. Does the Professor call the small pox a morbid poison? for as it may be “communicated to others, “as it is received*”, it can in his opinion be nothing else than morbid. How often does he fail in communicating the infection by wounds when he innoculates for the small pox? I only mean when the Professor is not

* Page 9.

interrupted by another action *. All animal poisons, of mad dog, rattle snake, viper, &c: &c. are infused by wounds. The duct that conveys the poison of the viper perforates the tooth that makes the wound †. The ticuna, a vegetable poison kills the animal as soon as applied to his blood ‡. Where will the Professor's new opinions lead us!

I am told by authorities that cannot be disputed, that the Professor addresses his pupils somewhat after the following manner: "Gentlemen, I observe that many of you take notes, I would advise you to burn them; for although what I deliver now is my opinion to day, in another course, I may change this opinion." A student from the country, under the Professor in the year 1786, will find himself out of all fashion on the return of his medical friend in the year 1787. Like country milleners, they must visit London every spring, for the newest mode of cutting out frippery.

* The Professor says, that no two actions can take place in the same subject at the same time.

† Abbé Fontana, Vol. I. p. 10.

‡ Abbé Fontana, Vol. II. p. 118.

May we not next year presume upon another quarto edition, by way of exposition upon this?

Page 60. *Short Recapitulations of the Varieties of the Symptoms.*

Here the Professor has, as I knew he would, broken in upon his specific distance of an inch and half, or two inches, from the orifice that marks the specific extent of inflammation. “ Sometimes it runs, says he, (amongst his “ other recapitulations) all along the urethra “ to the bladder, and even to the kidneys, “ and spreads into the substance of the ure- “ thra producing a chordée. The Cowper’s “ glands he suspects inflame. Sometimes “ the disease appears after infection in a few “ hours, at other times in six weeks*.” In short, when he describes symptoms as they really are, he cannot but tell us what is known to happen, and what is only information to such as never sought after it before.

* Page 51.

Page 69. *Of the Cure of the Gonorrhœa.*

At length we are approaching to examine how far the Professor is disposed to keep his word, after he has asserted, “ that gonorrhœa cures itself.” The touchstone of his sincerity, in such a declaration, can be only ascertained by his abandoning the disease to work its own cure, by bidding defiance to all remedies, and by pledging himself, that by leaving the gonorrhœa to itself, no ill consequences can happen from it hereafter.

“ As we have no specific medicine for
 “ gonorrhœa, it is fortunate that time alone
 “ will effect a cure. It is therefore very
 “ reasonable to suppose, that every such in-
 “ flammation gets well of itself; yet al-
 “ though this appears to be nearly the truth,
 “ it is worthy of consideration, whether me-
 “ dicine can be of any service in this form
 “ of the disease. I am inclined to believe it
 “ is very seldom of any kind of use, perhaps
 “ not once in ten cases; upon this idea of
 “ every gonorrhœa curing itself, I gave cer-

“tain patients bread pills. The patients al-
 “ways got well, but some of them, I be-
 “lieve, not so soon as they would have done,
 “had the artificial methods of cure been em-
 “ployed.”

I presume that every reader of the above extract will agree with me, that, after such a declaration, nothing in future ought to be dreaded from a gonorrhœa; and for what purpose the after pages are devoted, I am at a great loss to conceive. If what the Professor advances be true, if his opinions, when once examined with candour and judgment, stand the test of general approbation; then is a gonorrhœa, a trifle indeed, and those who treat it as a matter of importance, are deluded ideots, possessed of no power of discrimination, and who have borrowed all their understanding. The Professor himself will be suspected of a prejudication, that his honour will never permit him to deserve; if, after the above quotation, the rest of his work be not engaged to confirm such new opinions. For when it is considered that society in general, and the whole profession in particular, have a stake in the concern; no time surely can then be too long to explore so interesting and
 so

so pleasing a theme ; no work can be too large that explains it ; and no credit too great for him that has made the discovery.

Diogenes dying, was asked by a friend, what he wished should be done with his body ? Let it lie, said the philosopher, where I die. The friend replied, that the dogs would devour it. They dare not, said the philosopher, for my stick will lie by me. But you cannot use your stick after death, replied the friend, nor will you know when the dogs do devour your body. Then, said Diogenes, what signifies what will become of my body ? What signifies what will become of gonorrhœa ? The use of mercury, like the stick of Diogenes, is unnecessary. If all the above be true, and the Professor's quotation stands as pointed as if it had been written in Rubrick, we need no longer trouble ourselves about what can befall us from a gonorrhœa. It is a bugbear in the imagination only ! Common sense will not give it a hearing !

A friend, after reading the Professor's work, humourously called him the Calvin in venereal reformation. But I beg leave to differ from my friend, the Professor may be more

aptly compared to Luther, who retained as much of the pageantry as was innocent in religion, without being idolatrous; just so has our Professor in physic. Pills being formerly given in gonorrhœa, he has kept up appearances, by giving bread pills; he might carry it further; suppose that he gave for an injection some pump water, to be thrown up by a syringe not material as to its being perfect; for, if the injection reached an inch and half, or two inches, that would be quite far enough, for these are the specific bounds of the disease.

If the Professor had meant this new sport of bread pills just to amuse his readers, and had episodically flung it into this part of his work, before he entered upon the more serious and important subject of the best cure for a gonorrhœa, and its supposed consequences; as a clever fellow would entertain us, by a trick with the cards, while the rest of the party were preparing for a sober game at whist, I would have joined him in the fun; but I imagine, he means to be serious in this new opinion, from what he has said in page 35, and in many other parts of his work, the substance of which is this, that irritation from
gonorrhœa

gonorrhœa spontaneously ceases by tiring itself out. But whilst the Professor sits at his ease, an inactive spectator only, can he tram-mel the consequences? can he insure us from all local and all the constitutional attacks that do arise from a neglected gonorrhœa?

Cases, if I were to collect them, that would fill as many volumes, and of as large a bulk, as the records of parliament, may be brought in proof, that all local, and all constitutional venereal affections may be produced from gonorrhœa: It is for this reason that virulent gonorrhœa does not compleat its own cure. The virus that produced the irritation in the urethra, after the urethra can be no longer irritated, is capable of shifting its influence, of producing new effects, and of being ramified into all venereal symptoms that ever were known. This can only be prevented by remedies of art. The application of these remedies destroys the stimulating power of the virus, and puts an end to all irritation. Thus is the immediate attack removed, and the future evils guarded against and prevented.

Does not the Professor know, that by the application of a prophylactic, no irritation will
ever

ever come on ? that, by the same application, as soon as the irritation is perceived, the symptom will more readily give way ? and that, whenever it be applied before absorption has taken place, its success is effectual ? Delay, which should be always avoided as much as possible, will certainly make the cure more intricate and important. From what I have seen I have the strongest presumption to draw this conclusion, that the length of time, in the cure of a gonorrhœa, carries a very corresponding reference to the length of delay, in applying remedies of art, after the time of receiving the infection. Irritation, from many other causes, may act somewhat after the manner that the Professor states this to act. But of all irritating substances that could possibly be offered, perhaps that of gonorrhœal virus was the least to be reconciled, from the nature of its action, to this new opinion of the Professor. For this is virus of a specific quality, and only temporarily local ; it cannot be said that it has no power of shifting situation, and, if suffered to remain unopposed, of acting on every part of the constitution : it always may, because it often does. Therefore, as the irritation in the urethra, that ceases spontaneously, is not the whole of the consequences

ces that we are to look to, as the disease is not final there, who will be found so hardy and conceited, as to pronounce without equivocation or subterfuge, that gonorrhœa cures itself.

I have admitted, for the sake of the argument, the case, that gonorrhœa would cure itself, that it would gradually decline till it totally disappeared ; but what if that which generally happens, should follow such neglect ? what would he have to answer for to the sufferer, who created the evil by the absurdity of his counsel ? would it not be a subject too serious to be so sported with ? would the patient be as careless in his revenge, as he was in his advice ?

I observed, that irritation produced from other causes may determine in that manner which the Professor declares irritation from virus determines in the urethra. And so it may on all parts very susceptible of pain, by blunting the feeling that was before the attack more delicately tender ; by reducing the sensation of real pain, or exquisite pleasure, to the neutral condition of receiving either with indifference. The first gonorrhœa is generally the
I most

most inflammatory, the first swelled testicle, the first fever, the first corrections received by soldiers, the first effects on the eyes, by lime, and foot, of lime burners and chimney sweepers, are all most severely felt: The sensations from pleasure are in the same degree reduced as they are brought into repetition.

From these general observations, which are neither new nor uncommon, it appears, that the Professor's idea of gonorrhœa tiring out itself by the decline of irritation, is only a new opinion, or a strange opinion, when it is applied thus unhappily, where it ought not to be applied at all. I repeat it again, because here is a virus of a specific quality, and which is only temporarily local; and although it produces its first effects by irritation, yet it does not cease to act when irritation can be no longer supported. And although this specific virus may, or may not have transferred its influence, it cannot be said to be inactive, it cannot be said to be extinct; for it always retains the power of quitting the local for a constitutional attack. And last of all what is more self evident, and more important than all I have hitherto said, the local consequences produced

produced by gonorrhœa uncorrected by remedy, may not, and very seldom will be at an end, till the parts on which the virus acts are destroyed, or if not destroyed, till they become so unfit for their destined functions, that the remainder of life will be a miserable entail upon the deluded subject, who fallaciously is advised to wait for his gonorrhœa to cure itself; or who fallaciously persuades himself, that there is no specific power yet known, which cures a gonorrhœa by depriving the virus of the power of action, either in the form of gonorrhœa, or any other form arising out of it. The orthodoxy of the Professor will be found to be heretical, or there is no truth in divination. Suppose a house in flames, how ridiculous it is to bring the fire engines! what an useless set of people the fire-men are! what nonsense it is to take precautions of insurance! when, if you only permit the fire to continue till the house be burnt down to the ground, the flames will not only tire themselves out, but in the end be compleatly extinguished.

Whether the Professor be right or wrong in his conjecture, that gonorrhœa was the venereal form with which the crew of the voyag-

ers infected the unfortunate inhabitants of the islands in the south seas ; it must remain doubtful, until some good authority positively declares, that the disease was spread there from gonorrhœa. But, notwithstanding the Professor asserts from no other authority than his bare surmise, and that is to me poor authority indeed ! yet I claim the right of convicting the Professor, by the strongest evidence that can possibly be brought into an English Court of Justice ; which is, by his own confession, by what proceeds out of his own mouth ; and I shall make the appeal immediately to my readers, whether I have not brought such convictions home to the Professor, or rather, in this instance, home to the Confessor ; but I should not lay so great stress upon this, if it were not a matter of the first magnitude ; and not because, this is the only contradictory blunder that I find throughout that part of his performance I have hitherto perused ; for truth bids me tell it, that the scattered differences in opinion blaze forth every where to the eyes of the observer, as obvious, as stars are in a clear night.

“ But as we find in Cook’s last voyage,
 “ that the disease in every form is now there,
 “ and as we have no intelligence of a gonor-
 “ rhœa

“ rhœa being since introduced, we must suppose, that every form of the disease has been propagated from one root, which most probably was a gonorrhœa *.

“ If it is true, as is asserted in a voyage round the world, that the disease was carried to Otaheité; It shews that it can be long retained after all ideas of its existence have ceased: and when it is retained for such a length of time, it is most probable in the form of gonorrhœa †.

“ A young woman from the Magdalen Hospital was received into that house two years before, and continued there all that time. The moment she came out, she was picked up by one who was in waiting for her with a post chaise to carry her off immediately; she gave him a gonorrhœa ‡.”

From whence I took the above quotations, there I find innumerable others, of cases that prove the bad effects of gonorrhœa; it is not material to me for what purpose the Professor

* Page 15.

† Page 40.

‡ Page 38.

brings forward these cases ; for if they be true, he may apply them to elucidate one point in theory, and I may apply them to elucidate another ; for all true cases being but instruments in our hands to be applied for our advantage, one may employ them to one end, and another to another end. These so far prove, that the Professor believes, and knows that gonorrhœa does not cure itself ; and that gonorrhœa can commit all the disastrous mischiefs that I have attributed to it. After my readers have satisfied their information, by looking at the above picture of gonorrhœa given by the Professor, I beg that they would regard its reverse drawn by the same artist ; for, *reddere personæ scit convenientia cuique**, and their own comment shall spare me from any further intrusion of my observations on this point at issue.

“ As the living principle in many diseases
 “ is not capable of containing the same
 “ action, it also loses this power in the pre-
 “ sent, when the diseases is in the form of
 “ a gonorrhœa, and the effect is at last stop-
 “ ped, the irritation ceasing gradually.

* Horace.

“ The

“ The circumstance of the ceasing spon-
 “ taneously, only happens when it attacks a
 “ secreting surface, and when a secretion of
 “ pus is produced,

“ The common practice proves those facts ;
 “ we every day see gonorrhœas cured by the
 “ most ignorant ; but in chancre or in the lues
 “ venerea more skill is necessary : the reason
 “ is obvious, gonorrhœa cures itself ; whilst
 “ the other forms of the disease require the
 “ assistance of art.

“ From this circumstance of all gonor-
 “ rhœa ceasing without medical help, &c.
 “ &c. &c*.

“ As we have no specific medicine for go-
 “ norrhœa, it is fortunate that time alone, will
 “ effect a cure : it is therefore reasonable to
 “ suppose, that every such inflammation gets
 “ well of itself.

“ I am inclined to believe that medicine
 “ can be very seldom of any kind of use.

* Page 35.

“ Upon

“ Upon this idea of every gonorrhœa curing itself, I gave certain patients bread pills ; the patients always got well : but some of them I believe not so soon as they would have done, had the artificial methods of cure been employed*.

“ Whatever methods are used for the cure, either locally or constitutionally, it is always necessary to have in view the possibility of some of the matter being absorbed, and afterwards appearing in the form of lues venerea ; to prevent which, I should be inclined to give small doses of mercury internally †.”

Thus have I brought a few of the Professor's scattered new opinions, and such as are referable to the subject before us, to a focus, and set the collection in so advantageous a light, that the variety gives a pleasing effect of each upon the other : like a variegated landscape ; here are hill and dale, wood and water, lawn and heath.

It is not the first time that I have endeavoured to possess my readers with a suspicion, that

* Page 69, 70.

† Page 86.

has all along clung to my belief, that the Professor plays off his new opinions in theory, and reconciles himself to adopt our old ones in practice. This accounts for his cautious prudence, though reluctantly set down, of betraying an inclination to give a few doses of mercury. As I proceed further in this remarkable chapter, a chapter that cannot be outdone by Quivedo himself, I find that the Professor, instead of filling up the remaining pages of it, with more successful cases of bread pills, with more cases effected by constitutional dispositions without artificial aid, has already discovered an intention to abandon his new ground, and to fall into the ranks; familiarly speaking, to become one of us; to engage the enemy with the same weapons that we do; and to be subject to our discipline. I shall watch this manœuvre, and if I find it a feint, my readers shall know it. The fact is, that the Professor proceeds to treat of the different modes of cure, amongst all of which, I see no novelty, that I would stoop to pick up: All methods, that every body knows, or that every body has tried sooner or later, that have followed the old fashioned practice of curing this disease, are served up to our view, like an exhibition of pictures, and so judiciously remarked upon by the Professor,

feffor, as to borrow new splendour by the fresh varnish he lays upon them.

I shall proceed to give my readers the best idea I can of the Professor's opinions, and rules to be observed, in the treatment of the gonorrhœa.

“ The first thing to be considered is, the
 “ inflammation itself, whether violent or
 “ mild, whether common or irritable; yet
 “ even when this is ascertained we have not in
 “ all cases the cure in our power.”

“ When the symptoms are violent, but of
 “ the common inflammatory kind, which is
 “ to be collected from the attending circum-
 “ stances, particularly the extent of inflamma-
 “ tion not exceeding the specific distance, the
 “ local mode of cure may be either irritating
 “ or soothing till the original violence is
 “ over. Irritation in the present case, may be
 “ attended with less danger than in the irrita-
 “ ble inflammation, and may alter the specific
 “ action, but to produce this effect it must be
 “ greater than the irritation from the original
 “ injury.”

After

“ After all, however, I believe the soothing
 “ plan is the best at the beginning.”

Here we are treated with a sublime flight of the Professor, “ irritable inflammation,” he has undertaken to explain this new opinion by a note, but, until he adds a second note to explain his first, my readers must remain in the same ignorance that I do, and be content to guess at his meaning ; for like every mystery it is enveloped in dark obscurity, and hidden from the reach of vulgar understanding. As the reader may perceive from the quotations I have already given, it is impossible for me to explain to them the ideas the Professor entertains of the cure of a gonorrhœa, now he brings his new opinions into play : Nothing can be more undetermined than the system he adopts : It can hardly be called a system : The word chance is much better to call it by. All that I can collect from the above extracts is, that the irritating plan is not dangerous, and the soothing safe. But directly after, he says, “ Nothing
 “ should be done that may tend to stop the
 “ discharge.—The constitution should be
 “ altered if possible. If the constitution cannot
 “ be altered, nothing is to be done but to al-

M

“ low

“ low the parts to tire themselves out by a
 “ continuance of the same action.”

What a forlorn hope has the Professor hung out to his patient ! a gonorrhœa may remain upon him for years ; he must be shut out from the most alluring enjoyments ; he cannot be cured ; there is no specifick in the catalogue of remedies. Irritable inflammation may come on, and then the disease becomes so unruly as to break over its specifick bounds. It is vain for him to cry out, that he is tired before the parts attacked are ; for they must be allowed
 “ to tire themselves out by a continuance of
 “ the same action.” Go, says he, to the patient, you have got a runaway horse, I can make you no bridle that will govern him ; when he gallops away with you, spur him on ; tire him and he will stop of himself ; you may coax him to stop if you can ; but if neither will avail, I say again, keep your seat, and tire him out.

I shall just lay before my readers one more extract, and then get rid of this chapter of difficulties, as soon as I can, for I may, with truth, that cannot be impeached, affirm, that Locke himself, in his essay on human under-
 stand-

standing, is not half so abstruse as the Professor is, in his essay on animal gonorrhœa. There may be more in his reason, than is dreamt of in my philosophy, but I cannot find it out, I confess my incapacity.

“ When the inflammation has considerably
 “ abated, the disease only now remaining
 “ in a mild form, it may be attacked locally,
 “ violence is still to be avoided; because it
 “ may bring back the irritation. At this pe-
 “ riod gentle astringents may be applied with
 “ a prospect of success; or if the disease has
 “ begun mildly, and there are no signs of an
 “ inflammatory disposition either of the com-
 “ mon, or irritable kind; in order to get rid of
 “ the specific mode of action quickly, an irri-
 “ tating injection may be used, which will in-
 “ crease the symptoms for a time, but when it
 “ is left off, they will often abate, or whol-
 “ ly disappear. In such a state of parts, astrin-
 “ gents may be used; for the only thing to be
 “ done, is to procure a cessation of the dis-
 “ charge, which is now the principle symp-
 “ tom.”

I am confident, that, if a man of superior
 talents to me, and with a stronger disposition

to satire, had taken upon him the task of proving the theory of the Professor by the test of criticism, the subject before us would have warmed him, and he would have proceeded in his triumph with more than common rapture; the brow of the Professor would have been deeply furrowed, and such an irritation would have been raised on his feelings, as would have reached my sympathy. I am only angry at his mistaking himself all this while; from his situation, we had a right to expect, that every innovation would have been clear and distinct. This is not an age for absurd theory, and the Professor's powers are not equal to charm us into a wrong persuasion. Experiment, the touchstone for truth, is the high road for the Professor. But as his friend, the Abbé Fontana says, after all, I fear that man loves better to be admired than useful; marvellous than true; difficult than important.

Page 72. *Of the different Modes of Practice---Evacuants---Astringents.*

The Professor divides the remedies commonly recommended in gonorrhœa, into two kinds, internal and local. The internal remedies are divided into evacuants and astringents. “ The evacuants are principally of the purgative or diuretic kind, every practitioner supposing that he is in possession of the best remedy; mercury, neutral salts, nitre.” The Professor has no great opinion of these remedies. “ For what idea” says he, “ can we form of irritation produced all along the intestinal canal curing a specific inflammation in the urethra?” “ Yet” for it is impossible for the Professor to be positive in any opinion but in his new ones “ there are cases where a brisk purge has even performed a cure.”

“ Astringents though often given, yet have been always condemned by those who call themselves the judicious and regular practitioners.” The Professor, “ believes that they

“ they do not in any case lessen the venereal
 “ inflammation, but certainly they often lessen
 “ the discharge.” “ As that effect however
 “ does not constitute a cure, it is not necessary
 “ to produce it.” The professor “ can con-
 “ ceive that the balsams, with something else,
 “ may help to lessen the discharge, as the in-
 “ flammation abates.”

Page 74. *Of Local Applications---different
 Kinds of Injections---irritating---Se-
 dative---Emollient---Astringent.*

“ Local applications may be either internal
 “ to the urethra, external to the pubis, or
 “ both; all of which will be in many cases
 “ necessary. The internal, or those applied to
 “ the urethra, would seem most likely to cure
 “ this species of disease, by coming immedi-
 “ ately into contact with the diseased parts;
 “ for if they have any power of action, what-
 “ ever that be, it must be in opposition to the
 “ venereal irritation; therefore we might sup-
 “ pose, that most irritations that are not ve-
 “ nereal, would tend to a cure; but certainly
 this

‘ this is not the case. If on the contrary, the
 ‘ applications are such as quiet irritation, they
 ‘ must also be of service——These applica-
 ‘ tions may be fluid, or solid.”——“ By fluid,”
 he means “ a wash to the fore,” by solid “ a
 ‘ bougée” and at length, he tells us, “ that
 ‘ fluid applications are called injections, and
 ‘ which are without number; but the great
 ‘ variety of injections, and every venereal in-
 ‘ flammation getting well during their use,
 ‘ which was likewise observed to happen when
 ‘ internal medicines were given, are strong
 ‘ corroborating circumstances in favour of
 ‘ the opinion, that every such complaint
 ‘ will in time cure itself.”

‘ This last observation of the Professor shall
 not escape my notice for a moment. And is
 this all the foundation he has for unqualified
 assertions, that injections do not cure a gonor-
 rhœa, but that gonorrhœa cures itself? Be-
 cause a variety of injections are given, and
 every venereal inflammation gets well during
 their use? Is this the source from whence
 the Professor draws his strong conclusion, that
 every such complaint will in time cure itself?
 The presumption may carry him further than
 he was aware of; for proof he cannot call it;
 It

It is presumption arising out of a determined obstinate principle of doubting every theory but his own opinions ; for, he might also have asserted, and it would have been much more consonant to reason : That the great variety of injections given, and every inflammation getting well during their use, are strong corroborating circumstances in favour of the opinion, that they may all tend to the same end, and that the complaint yields to their specific power. Is there but one way to cure a chancre ? Does the Professor not give us hereafter to understand, that there is more than one ? I ask him this, because I do not wish to forestall the after page that will tell it. Does he not in bubo advise different means ? Does he not aid, or change the preparation of mercury in blotches on the skin ? In nodes in the bones ? In short, does he depend upon mercury solely, and in one form in the cure of all venereal cases, where he admits of the specific power of mercury to cure them ? If, in these instances, he goes through his cure of all symptoms, and every stage of them, without calling into his aid, as new symptoms offer, other remedies, or trying mercury in other forms, I say, if he does do so, then do I yield all resistance to all his new opinions,

opinions, and upon these terms alone do I admit, that he has argued more for the honourable cause of investigating truth, than for the contemptible vanity of being singular.

But the Professor might as well have said, you can only convince me that there is a specific for gonorrhœa, as there is for chancre or constitutional venereal disease, by giving me proof that there is but one specific, that you have that specific in your possession, and that I will see the application of it, and that it shall rest with me to determine, whether it be a specific or not. This is what I call a specific in the strictest sense ; if the Professor means as much, I can only say, that when he asserts that there is no specific for gonorrhœa, he calls for more proof than he requires for chancre, or constitutional venereal disease.

But immediately after this the Professor assumes such a system as I know nothing *simile aut secundum* to it. He begins to treat of the cure of a gonorrhœa according to the old opinion, whilst he is combating the doctrine by his new one. The favourite maxims inculcated by the ancients and sanctioned by the moderns, of adhering to established rules unless better can

be provided, are spurned by the Professor; he has built a new house, but will not quit the old one; he has raised up a new theory, but dares not conform a practice to it; he has built upon a sandy foundation indeed! The Professor may depend upon it, that he is erring from the rule of right when he finds fault without being able to amend. This is what my suspicions all along led me to, and I find them well founded. Does the Professor send away those unfortunate victims he devotes to be tired out by a gonorrhœa? If he does, and for conscience sake, I will be obliged to him to send them to me; for although I have a conscience also, yet I believe it is not so delicately wrought as the Professor's; I should endeavour to cure them, I should still try to prevent all evils (as he calls them) that arise from neglected gonorrhœa; notwithstanding the comfort he has held out to the contrary, which must be at least as grateful to the feelings of the unfortunate patients, as a wet blanket wrapped over them. But I am sure the Professor will not part with one of them: the pious fraud of bread pills was meant to amuse us; to detain his patients, something more, as hereafter will appear, will be attempted at least by him; and perhaps
 what

what others attempt, but I trust with more confidence and success.

“ I shall divide, says he, injections according
 “ to their particular effects upon the urethra,
 “ into four kinds; the irritating, sedative,
 “ emollient, and astringent. The specific, I be-
 “ lieve is not yet discovered, although a mer-
 “ curial injection, in some form or other, is
 “ by most people supposed to be possessed of
 “ such a power, and of course this mineral
 “ makes part of many of the injections now
 “ in use.”

His irritating injection is composed of a so-
 lution of sublimated mercury. His sedative, of opium,
 saccharum saturni, advising at the same time
 deluting liquors. His emollient, of gum
 arabic, milk and water, and sweet oil. And his
 astringent, of the gums, as dragon's blood, the
 balsams, and the turpentine dissolved in wa-
 ter; the juices of many vegetables, as oak
 bark, peruvian bark, tormentil root, and per-
 haps all the metallic salts, as green, blue, and
 white vitriols; the salts of mercury, and also
 alum.

“ External applications, poultices and fo-
 “ mentations—when the glands of the ure-

“ thra are fwelled, the application of mercurial ointment may be proper, after the inflammation has subsided. Indeed mercurial ointment is often applied when in a state of inflammation,” but he says “ he is not perfectly satisfied of the utility of such practice.”

Such is the catalogue of remedies that the Professor has made out for the gratification of our eager curiosity, and quenching our uncommon and pressing thirst for information. Such is the inexhaustible treasure from this fountain. Here are calculated remedies for all constitutions, from the rich king down to the poor beggar. But I would not wish to turn my back upon my old acquaintances ; and I hope, although I find them in company with the Professor, that I may be permitted the honour of recognising them ; for, I firmly believe, that there is not a pupil of two months standing in this city, who does not know all the remedies remarked upon by the Professor, as well as I do. That they may choose to throw aside the worser part of them, I will not deny, and perhaps they may be in the right if they did.

“ Irritating

“ Irritating injections do not agree with all
 “ inflammations arising from venereal poison ;
 “ it may be asked, in what cases are the ir-
 “ ritating injections to be used with advan-
 “ tage ? This I have not been able to de-
 “ termine. Where the perinæum is very sus-
 “ ceptible of inflammation, nor where there
 “ is a tendency in the bladder to irritation ;
 “ in such cases I have not succeeded with
 “ them ; they do not only do no good, but
 “ they often do harm, for I have seen them
 “ make the inflammation spread further in
 “ the urethra, and I think I have reason to
 “ suspect that they have been the cause of
 “ abscesses in perinæo.” This last observa-
 tion the Professor qualifies with a note to ex-
 plain. “ It is however to be remarked, that
 “ this symptom is not always to be attributed
 “ to injections ; for it often happens when
 “ none are used.”

Of sedative injections—The Professor de-
 clares for “ opium, being perhaps the best
 “ sedative we have ; but even opium will not
 “ agree, or act as a sedative in all parts ; on
 “ the contrary, it often has opposite effects,
 “ producing great irritability. Saccharum fa-
 “ turni

“turni makes a good sedative astringent injection.”

Of emollient injections—he says, “they are the properest when the inflammation is very great; they are probably first useful by simply washing away the matter, and then leaving a soft application to the part.” I shall just put the Professor in mind, and bring to the recollection of my readers, that the Professor has all along persisted, that the matter could do no harm; and here is my authority for the observation: “Washing of a sore I believe unnecessary; for I imagine that matter from any sore whatever, is always such as cannot stimulate the sore into any action*.”

Of astringent injections—“They should only be used at the latter end of the disease, when it has become mild, and the parts begin to itch. But this should be according to circumstances, and if the disease began mildly, they should be used at the very beginning; for by gradually lessening the discharge without increasing the

* Page 76.

“inflammation,

“ inflammation, we compleat the cure, and
 “ prevent a continuation of the discharge,
 “ called a gleet.” I am at a loss to determine
 to which of the five words, “ But, probable,
 “ seldom, happen, and believe,” the Profes-
 sor owes the most obligation. They certainly
 are his coat of armour, which he puts on when
 he combats old opinions ; but his new opi-
 nions want neither coat of mail, breast-plate,
 or falchin ; they are bold in their own na-
 kedness, are truth itself, best to be admired in
 the strongest light, and “ unadorned, adorned
 “ the most.”

Such is the disposition which the Professor
 has made for the attack of a disease, that, if
 but let alone, would cure itself. But if any
 of my readers be desirous of knowing how
 and when these remedies are to be applied, he
 will be disappointed in that expectation from
 me. For, when the Professor conferred upon
 us his ideas, he ought to have furnished
 us with intellects to comprehend them.
 He tells us that the remedies do not always
 act alike, that they do good and they do harm :
 Sometimes this first, and so *vice versa*, ring all
 changes upon them, and their harmony will
 still be the same ; if you regulate them by the
 scale

scale of a country dance, thus, let sedative and emollient set corners; figure in irritating; and lead down astringent; then all four hands round; right and left: This disposition may do, or it may not do; these are my adopted remedies, and I have nothing better to offer.

But to be a little more serious; when a man comes forth with a voluminous work full of innovation in theory only, without any thing like the shadow of improvement in practice, which I protest to be the case in point, as far as I have now perused; to what good can such work tend? If the disease be not better understood, if contradictions and doubts take place of matters of fact, if the mark of cure be removed at a greater distance from us in proportion as the theory is attempted to be changed, and no new remedy preferred; where is all that advantage which is gained from true knowledge? Is it to be found in this performance of the Professor? I have heard of, and I have lived long enough to be a witness to a theory that has helped to practice, and to a practice that has helped to theory: such have been no sooner given out by their respective authors, than they flashed
I
conviction

conviction on all, and became generally adopted in spite of mean opposition. The practice of treating putrid fore throats in a new manner was generally approved from the theory of Dr. Fothergill. The theory of treating the small pox in a new manner, was conceived from the practice of the Suttens. The giving opium in mortifications, not to mention his other valuable improvements, was introduced by Mr. Pott. Do we derive any such profitable information from the book before us? Is there any thing in the eighty-three pages that I have hitherto examined, which bids fairer to remove the dreadful symptoms of a gonorrhœa? Or has the Professor done himself credit, or treated with candour the general system already adopted, when he denies its efficacy, without being able to produce one more efficacious; when he doubts every thing, because he would not be seen to acquiesce in any thing; and when, for the sake of deviating from the path hitherto trodden, he has gone out of his way, and bewildered himself and his readers by constant errors and palpable contradictions? The result of my new opinion is, for I in my turn have a right to a new opinion, that the Professor is more at home with his knife than his

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pen;

pen ; that, by a perverse application of his mental endowments, he has diminished and wasted his reputation ; that his anatomical comparative knowledge was a wedge of gold to him, the advantageous application of which he has neglected ; and that he has, by employing his labour in expanding the leaves before us, exchanged solid wealth for feeble splendour. This ought to have been told to him in the closet, but I trust the opportunity may not be now too late.

Page 84. *Of the Treatment of the Constitution in the Cure of the Gonorrhœa.*

“ So capricious sometimes is this form of
 “ disease in its cure, that I have seen by an
 “ accidental fever coming on, the discharge
 “ stop, the pain in making water go off, and
 “ the gonorrhœa finally terminate with the
 “ fever.” As the Professor makes no remark upon this, it is to be presumed, that he conceived the whole venereal concern to be at an end. In the only instance, where I

ever saw a gonorrhœa disappear in this manner, the consequence was, that the patient endured the most confirmed lues venerea that ever I have seen in all my practice. A lieutenant of a man of war was seized with the small pox, whilst a gonorrhœa was on him; he had the small pox so very bad, that it was with great difficulty he recovered: the gonorrhœa left him altogether. He went, as soon as he recovered from the small pox, to sea, was absent from England more than twelve months, and returned in a most emaciated state, with nodes upon his bones, attended with incessant pains, and a large ulcer over all the palate, fore-teeth out, and the bone was a caries. Much mercury had been given at sea to him, but to no purpose. He recovered at Dr. Kelly's at Knightsbridge, by warm bathing, milk diet, decoction of sarsaparilla, and a little mercury was given, when his constitution had been previously mended. Dr. Wright attended him with me. So much for the security of gonorrhœa curing itself. I am inclined to think, that the more a person is out of health, the more likely the venereal virus is to be absorbed. At any rate, the case above proves directly the contrary to an opinion the Professor would fix us with: "It

“ appears to me beyond a doubt, that no two
 “ actions can take place in the same constitu-
 “ tion, at the same time.” Here we find
 the Professor positive, but this is one of his
 new opinions; perhaps he is in the right.
 We ought to be proudest of our own children;
 and the more rickety they are, the more pa-
 ternal tenderness they require from us. Does
 the Professor pretend to say, that the virus
 was dormant whilst it was absorbing from the
 urethra into the habit?

“ Unfortunately there are cases where no
 “ known method lessens the symptoms; eva-
 “ cuations have produced no abatement, the
 “ strengthening plan has been as unsuccess-
 “ ful, sedatives and emollients have procured
 “ no relief; and time alone has performed
 “ the cure.”

I am still of opinion, that much more might
 be done, than, as the Professor tells us, hath
 been done. And with all submission to his
 superiour power, I shall repeat an assertion
 that I made a few pages back, and although
 contrary to the opinion of the Professor, I

shall not shrink from the credit I stake upon it. The Professor in very pathetic strains, and with much seeming candour, finding that he can no longer guide at the helm, abandons his post, and commits his bark to the storm. And could he leave his cases to the apathetick event of time, when he had only called to his aid the above resources? All of them seem to me calculated to bring the patients to the very predicament he is about to leave them in. Let any man turn to the particular articles in the separate classes above recited, and see if he can find what he himself terms any thing like a specific in the strengthening plan, the sedative, or emollient; evacuations the Professor has no more opinion of than I have. Where was mercury all this while? Where was his irritating injection? Why did he play with the cases, and suffer them to arrive to such a desperate height? I say that a mercurial injection is a specific in the cure of a gonorrhœa, and that the cure is only made difficult and important from procrastination; by permitting the virus to extend its ill effects, and by widening the inflammatory surface. Such must always be the consequences, where the application of mercury is to wait upon the departure of inflammation;

for

for the disease must not only be protracted, but aggravated, if the specific power of the virus be not destroyed. And I cannot impress this momentous concern too strongly upon the minds of my readers, that delay in the cure is the general cause of all the untoward symptoms that arise out of gonorrhœa. The sedatives, the emollients, and the astringents, set down by the Professor, ought to be considered as adventitious aids selected to act under the all controuling power of mercury; as slip slops to James's powder, and as varieties of adjuncts are to bark. I hope to hear no more of this milk and water system: Bread pills were bad enough.

Page 88. *Of the Treatment of occasional Symptoms of Gonorrhœa.*

The Professor says, “ as the following
 “ symptoms are only occasional consequences
 “ of venereal gonorrhœa, being the effects of
 “ an irritation in the urethra, and therefore
 “ not venereal; they are to be treated in the
 “ same manner as if they arose from any
 “ other cause.”

All that I shall observe upon the above opinion, is, that these occasional consequences arise from the irritating virus on the urethra, and that what began in virulent inflammation, if permitted to continue any length of time, will end in a fixed disease on the parts, after all the virus that gave cause to it is no more. In other words, I say that these consequences will always be found in an aggravated condition, where there has been a neglect of removing the original irritation, or where a gonorrhœa is left to cure itself.

Of Bleedings from the Urethra.

These, the Professor observes, always go off in the usual time of the cure of a gonorrhœa.

Page 89. *Of the Chordée.*

The Professor and I have already offered to the reader our differences on this opinion; I shall not revive them here; but proceed to
 enumerate

enumerate the remedies he lays down for the relief of the complaint. These are bleeding in the arm ; leeches to the part ; poultices with camphire in them ; steam of hot water ; opium ; mercury ; cicuta. Electricity, he says, “ may be of service.” The efficacy of electricity I am disposed to doubt in a more general sense. From what I have seen, and from more information that I have collected out of the disinterested reports of others, I am convinced, that this sublime elementary science is more calculated to enlighten philosophick pursuits, than to restore diseased parts to a state of health. From out of ten thousand trials of electricity, if we hear of a few scattered cases where patients have done well, these are not enough to authorise us to attribute the cure to the power of electricity, or to waste the time of the patient in useless project.

Page 90. *Of the Treatment of the
Suppuration of the Glands of the
Urethra.*

These diseases of the glands the Professor instructs us to treat the same as we should a venereal ulcer. “Mercury, he believes, must
“be given.” I shall make no comment on this, but refer my readers to what is past.

*Of the Treatment of the Affection of
the Bladder.*

The Professor suspects, “that this symp-
“tom sometimes lays the ground-work of
“future irritation in that viscus, which may
“prove very troublesome, and even danger-
“ous, but this is only conjecture” with
him; but with me it is more than conjecture.
This is one of the local evils out of many
more, not to mention the evils that arise from
absorption, that will most certainly succeed

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from

from neglected gonorrhœas, or from such as are abandoned to their own cure. This is one of those symptoms whose evil consequences may be so attached to us, and so deeply rooted in us, that it cannot be shaken off; that may not only attend us, but hasten us to our end. This is such an evil as is too complex for the ignorant to cure, and too important for the vain to despise, that ought to be guarded against, and not waited for, that ought to be resisted by anticipation. For the Professor, after all his parade about the harmless simplicity of gonorrhœa, confesses that, “ in
 “ spite of every attempt, the affection of the
 “ bladder often continues for a considerable
 “ time, producing other sympathies in the
 “ neighbouring parts.” The cure, he says, consists in opiate clysters, warm bath, bleeding, but with caution if it arises from sympathy—leeches to the perinæum—opiate plaster to the region of the pubis—blister to the perinæum.

Page 91. *Of the Treatment of the Swelled Testicle.*

For the cure of this the Professor instructs us to bleed; foment; poultice; to apply leeches; vomits may be thought on; opiates; mercury; but the last when hardness remains after inflammation has subsided, &c. &c. &c.

It may be a matter of some curiosity, if not of importance, to be able to distinguish whether the professor is serious in the opinion, that all gonorrhœas cease without medical help; or whether he is so only when he prescribes the most important remedies that are to be found in all the whole *Materia Medica*, for the cure of the dangerous consequences arising out of gonorrhœa. If he takes up the one serious, the other must be of course a jest. If gonorrhœa cures itself, if it needs no medical help, if the most ignorant succeed in their treatment of it, why does the Professor draw forth so powerful an array of remedies? Is it not as equally

ridiculous, as it would be in a commander in chief to lead forth a whole army to destroy a gnat for giving a sting?

“ It is generally a long time before the
 “ swelling of the testicle entirely subsides.
 “ It is still much longer before the epidydi-
 “ mis comes to its natural state, and indeed
 “ often years before it returns to its natural
 “ size and softness, and sometimes it never
 “ does.”

The reader will be pleased to mark the counter paragraph to the above from page 58.
 “ A swelled testicle, in consequence of the ra-
 “ dical cure in the hydrocele, does not sub-
 “ side after inflammation is gone, in as many
 “ weeks, as the swelled testicle, in conse-
 “ quence of its sympathy with other parts,
 “ does in days; and probably the reason of
 “ this is, that it arises from sympathy.”
 Only let men alone who set off upon a wrong theory, and they will entangle themselves. If I were invidiously to seek for contradictions, I should despair of coming ever to a conclusion.

United

United with the subjects of the affection of the bladder and swelled testicle, we find the new opinion on sympathy again brought forward by the Professor; if it had not been here obtruded upon us by him, I certainly should not have provoked its revival: And as I am already too sensible, how the Professor must feel upon the occasion, I would not wish in the least to be accessory to further disgrace; and much less therefore, would I aim to make so ignominious a stain indelible, by enlarging upon the theme. I will therefore only say, in addition to what I have already said, that sympathy is a corporeal effect from a mental cause, and that such authorities as South, Lock, and Johnson define it to be, the quality of being affected by the affection of another. Yet the Professor hugs his new opinions, and hugs his new idea of sympathy, as a barren woman hugs the infant of another mother; both alike soothing the infirmities of their nature by the same false and flattering illusion.

If an expression has escaped me, that may be construed personal to the professor, I desire once for all to say, that I never can mean it;
for

for the Professor has my esteem and respect.

To resist useless and flimsy innovations, to do away empty new opinions, to penetrate into the design of contradictory assertions arising out of confused ideas where I could; and to restore truth to its original pristine vigour, ought not, and could not be done with tameness; Juvenal tells us, that in such a case, *stulta est clementia*.

I shall only stay my further observations for the present. I shall persist in them throughout the Professor's work at my first leisure hours. I have already gone through ninety-three pages, observed upon, as well as my abilities and time would permit, his general system, and gonorrhœa in particular. The task may be thought by some invidious, but I will do my duty.

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NOVEMBER 1871

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I have the honor to acknowledge the receipt of your letter of the 11th inst., with reference to the above-named work.

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Your obedient servant,
J. W. Alden

OBSERVATIONS

UPON THE

NEW OPINIONS

OF

JOHN HUNTER,

IN HIS LATE TREATISE

ON THE

VENEREAL DISEASE,

TREATING ON STRICTURES IN THE URETHRA, HIS CURE
BY CAUSTICK, HIS APPARATUS FOR CONDUCTING THE
CAUSTICK; AND HIS CHAPTER ON IMPOTENCE.

PART THE SECOND.

TO BE ENDED IN THE NEXT.

By JESSÉ FOOT, SURGEON.

As far as they carry conviction to any man's understanding, my labours
may be of use; beyond the evidence it carries with it, I advise him not
to follow any man's interpretation. LOCKE.

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M DCC LXXXVI.

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WALLES, &c.

1801.

I did not think mischievously wrong,
 not have I imposed upon my reader
 one that was not my sincerest. Be-
 fore the great work of John Hunter
 appeared, our ears were constantly
 flattered with the magick of his new
 opinions; we were to see, what we

I Should have fulfilled my promise
 at any rate, but since my first part has
 been so well received, I am now dou-
 bly bound to do my duty. He, who
 is insensible to the pleasure arising from
 a fond ambition for honest fame, must
 be a stoick of no happy condition of
 mind. If to think, and to act as we
 think, had ever any accordance, I
 trust that my reader, and it is upon
 him that I place all my confidence,
 will believe; in this instance, they
 are in the most perfect harmony. I
 have not opposed one new opinion that
 I did

I did not think mischievously wrong, nor have I imposed upon my reader one that was not my sincerest. Before the great work of JOHN HUNTER appeared, our ears were constantly stunned with the magick of his new opinions ; we were to see, what we could not see, and we were not to see, what we did see ; but now we find the tone materially changed. I shall take leave of this great work in my next ; and that time to write, which I exact, shall not be beyond the indulgence which my reader would grant to me, before I asked it.

Dean-street, Soho, }

Nov. 9, 1786. }

OBSERVATIONS, &c.

CONTINUED.

I HAVE almost persuaded myself that I have not since my last publication kept silence till now, but only to write with more preparation.

If it be true, that he is a greater man who has assumed the right of new opinions, than he who has deserved it, the Professor has gained his ends in being the greater; and he has also compassed all that he can desire, not coveting any more than to prove that he is the greater man. The Professor had a reputation before his new opinions came forth to the world, so that he had already acquired upon the stage what he now attempts to carry in

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the

the cabinet. If he be guilty of a false theory, it is of a crime for which he has already been honoured; and if that theory must needs be condemned, it will yet be after he has obtained a triumph. Hitherto he has not a little cause to rejoice; or at any rate, he is welcome to this consolation, such as it is.

It might have been expected from the general pretensions of the Professor, from his particular distinctions and minute ramifications of all the symptoms of the venereal disease, in the volume before us, that the affection of the eye, produced by the application of virus to that organ, would have been an object worthy his consideration. But whatever was the real cause for such an omission, as it is the most dangerous and alarming of all venereal attacks, his silence must be the more reprehensible. But not to interrupt my readers with reproofs against the Professor upon a subject that is now incorrigible, I shall take upon myself, at some future time, to offer my information upon this attack of the eye, and directly proceed to examine what the Professor has done, rather than dwell longer upon what he ought to have done, but has left undone.

First

First of all, I must beg to be indulged with a moment's attention of my readers to what refers to myself. It has been more than once insinuated to my friends, that I must have been induced to these observations on the new opinions of the Professor, from some previous provocation. Such a suspicion cannot be better answered than by referring my readers to my last publication, where, I flatter myself, they will find sufficient cause from the palpable objections I have offered to his theory, even if I had stopped there. Let those who are disposed to suspect that I am actuated by impure motives, recall for a while their attention from the principles of others, the better to examine into their own; and when they have refined upon their own morality, when they have conquered their own prejudices, and when they have so far expanded their own narrow minds as to enable them to entertain more generous opinions, then they will judge in all probability with more purity of the springs that direct the actions of others. If this be a crime, I glory in it, I am infinitely apprehensive of all injuries that abuse my reason. I stake the little reputation I have earned, upon the purity of my motives and the

strength of my assertions. I never will substitute cavil for argument; I will not dwell upon exceptions for the sake of singularity; nor will I oppose exceptions to general rules, in order to procure the birth of a new opinion; I will not forsake the general standard of doctrines grounded upon true theory, and sanctioned by successful practice; I will not abuse because I cannot persuade, nor will I yield when I ought to engage. If I were guilty of any such deeds, I should expect to be despised even by the Professor, discarded by the faculty, slighted by my partial friends, and hated by myself. Let it no more be then said, that I either seek the enmity of him whose new opinions I oppose, or the ridicule of his friendship; and let it be remembered, that as long as the Professor and I publish opinions, we have both of us the right to think and to act, and that it is with our readers to determine which of us ought to succeed or to suffer.

Page 109. Part III. Chap. I.

“ *Of Diseases supposed to arise in consequence of*
 “ *Venereal Inflammation in the Urethra of*
 “ *Men.*”

“ Gonorrhœa either produces, or is sup-
 “ posed to produce, many disorders besides
 “ those already mentioned, and which are to-
 “ tally different from the original disease.
 “ How far they do all, or any of them, arise
 “ in consequence of this disease, is not clear;
 “ but as they are diseases of the urethra, and
 “ are both numerous and important, I mean
 “ to treat fully of them in this place. If any
 “ of these diseases arise from gonorrhœa, they
 “ are most probably not the consequences of
 “ any specifick quality in the venereal poison,
 “ but are such as might be produced from
 “ any common inflammation in those parts,
 “ as was observed of the continued symp-
 “ toms.” It is impossible there can be any
 man of the least feeling or observation, but
 must perceive the choice of difficulties that
 engage at this moment the mind of the Pro-
 fessor. Here sympathy for his situation is
 claimed

claimed in the strictest sense, for the alternatives that offer to his judgment in its happiest vigour for decision, must be all against him. The struggle is at length made, and he frees, or supposes that he frees himself from the difficult predicament, by not positively saying that the diseases of the urethra arise from gonorrhœa; for if he were to say so, according to his own description of the case, which is certainly a true one, and I will give it to my readers, we should find that he would condemn himself that way. “ There is a series
 “ of complaints. Thus a stricture of the ure-
 “ thra produces an irritable bladder, a dila-
 “ tation of the urethra between the bladder
 “ and the stricture, ulceration, fistulæ in pe-
 “ rinœo, dilatation of the ureters, and en-
 “ largement of the pelvis of the kidneys, be-
 “ sides other complaints that are sympathetic,
 “ such as swellings of the testicles and glands
 “ of the groin.” Now if these symptoms arise out of gonorrhœa, what is become of the flimsy fabrick that gives novelty to the work of the Professor? where has the bold assertion in the early part of his performance hidden itself for shelter from the contempt of the observer, that “ all gonorrhœas cease without
 medical

“ medical help, and that gonorrhœa cures
 “ itself?” I predicted all along, and so ob-
 vious and common an observation is no com-
 pliment to the sagacity even of a tyro, that if
 all gonorrhœas cease without medical help,
 and if gonorrhœa cures itself, none of these
 symptoms immediately enumerated can possi-
 bly originate from, or arise out of a gonor-
 rhœa, and I thank the Professor for the words;
 for if they be just, and if posterity approve of
 his doctrine, they will serve to record my a-
 postacy when his new opinions are taken into
 practice, and when the venereal disease that
 begins with gonorrhœa, must inevitably end
 harmless without remedy, without an imposed
 pretence for cure, and consequently without
 any ill effects for ever after. I should rejoice
 at my conviction upon these terms. But un-
 fortunate for mankind that hour is not yet
 come, and is far out of the reach of human
 prediction, when the patient that has been
 unfortunate will venture to take his chance
 without the help of medical aid, or risk the
 comforts of a happy family by one of the Pro-
 fessor’s harmless connections in a state of dis-
 ease. Obstinate error, or a misconception of
 the disease on our part, will make the Profes-
 sor’s

for's reformation halt however as yet. But he has still another advantage to console himself with, which my generosity has given him, and to which he is heartily welcome. Here are two strings to his bow. Now if his work dies away, mine may record this innovation, and may so far preserve its fame and value, that although this new opinion may fall into decay, a future race of fatalists even may adopt it through choice.

So far it is accounted for why the Professor could not positively assert that obstructions in the urethra, and the other train of diseases, were actually in consequence of gonorrhœa. But there was a necessity for his finding his way out, if he fought his way through the wood; if he had gone through fire and water, yet he must get through, and account for the cause of these diseases in the urethra. For he says, “ if any of these diseases arise from
 “ gonorrhœa, they are most probably not the
 “ consequences of any specifick quality in the
 “ venereal poison, but are such as might be
 “ produced by any common inflammation in
 “ those parts, as was observed of the continued symptoms.” This is, in my opinion,
 very

very true, and we have only now in order to investigate the question fairly to ask, what prevailing inflammation is the urethra liable to, so as to confirm to our reason beyond a doubt that these diseases can owe their source in preference to the inflammatory effects originating out of gonorrhœa? does any body know of any such other prevailing inflammations? has any single authour, whose opinion he is shy to quote, hinted at them? can he produce to my readers any one single authority, and if he can, why has he declined it altogether? He tells us only that, how far these diseases do all or any of them arise from gonorrhœa, is not clear; and this is the explanation of an eminent logician, who is a candidate for fame through the way of new opinions!

Now as the Professor has put gonorrhœa out of the question being the cause of the diseases of the urethra, and as every other authour that I am acquainted with, both ancient and modern, has uniformly and fully agreed that such diseases were owing to former gonorrhœa, it only remains with me, as he has fled from this general opinion, to follow him up, to
C watch

watch him, to shew to my readers what a figure he makes in the new one he is about to adopt, in opposition to all established authorities. The happy address of the Professor, in his management of this new opinion, must, and I am sure will prove to be an object worthy to engage our most curious attention. And the moment is at length arrived that we are to be gratified in full with the ingenious mode of reasoning by which we are persuaded, that the theory of all former men of science was wrong, and that this is right !

“ It may first be observed, that the urethra
 “ in man is employed for two purposes, and
 “ on this occasion I may be allowed to make
 “ the following general remark, that nature
 “ has not been able to apply any one part to
 “ two purposes with advantage, as might be
 “ illustrated in many instances in different ani-
 “ mals. The animals whose legs are adapted
 “ both to swimming and walking, are not
 “ good at either, as seals, otters, ducks and
 “ geese ; the animals also whose legs are in-
 “ tended both for walking and flying, are but
 “ badly formed for either, as the bat ; the
 “ same observations are applicable to the fly-
 “ ing

“ ing fish, for the flying fish neither swims
 “ nor flies well ; and whenever parts intended
 “ for such double functions are diseased, both
 “ are performed imperfectly. This is imme-
 “ diately applicable to the urethra, for it is
 “ intended for two purposes, as a canal or
 “ passage, both for the urine and the semen.”

Thus is the Professor harassed in new re-
 sources to form new opinions, and thus is he
 driven to hunt after associations, that when
 they are found, do not associate, and to console
 himself with the “ badly formed ” singularities
 of the birds of the air and the fish of the sea.
 After having deprived himself of the power of
 accounting for the diseases of the urethra, ori-
 ginating from inflammation brought on from
 gonorrhœa, why did he not examine whe-
 ther this canal, calculated for a double pur-
 pose, and in a state of exemption from the in-
 flammation of gonorrhœa, was as often dis-
 eased where the cause could not be attributed
 to gonorrhœa ? Would such an enquiry,
 founded upon more consonancy in reason, pro-
 mote, or baffle his views ? Would it tend to
 confirm, or destroy his innovation ? How many
 instances of strictures in the urethra can he pro-

duce from the peasants of this country, who reside in their native villages, and where there has been less chance of gonorrhœal infection? As the urethra of other animals are more comparative with that of man, than the feet and wings of beasts, birds, and fish, would his conceit find favour there? But I do not mean to stop here, ludicrous as the argument is, I will follow it up, and my readers must pardon me if I become ridiculous, for the subject cannot be treated with any other temper that is reserved for us as a gift in human dispensation.

Let us see what sense is to be made out of the analogy of the two purposes of the swimming and walking of ducks and geese; seals and otters we know not so much of, them, for that reason only, I put out of the question. I do not know where the Professor found out that the duck was an awkward fowl at swimming. I have heard of a proverb, whether it is recorded in Ray's Book of Proverbs I know not, but it is familiar in the mouths of the most common amongst us, and the Professor need not be told that it is by the pointed truth it conveys, and by the familiarity of any expression, that it becomes proverbial. Now

this proverb, unfortunately for him, makes directly against his new opinion ; it contradicts it flatly, for it goes pointedly to the excellent quality of a duck in swimming. For example, if one of his domesticks were asked whether the Professor was not a great anatomist ? The answer in all probability would be, Can a duck swim ! But if the same domestick were asked if he be a learned writer, and a man of great judgment ? There this very proverb would fail in the application, it would stick in his throat, for it would not confirm him in these excellencies. So much for one purpose of the feet of ducks and geese, and now let us enquire into the other, and there I am fearful that the conceit of the Professor will turn out equally unfortunate. Not having made the experiment myself, I cannot speak from my own knowledge ; the Professor, fond of experiments, might perhaps in his zeal have gone upon the pilgrimage, nor do I know what sporting calendar, or in what year I can refer to for information upon the fact, but I have heard of a match betwixt the travelling of a flock of geese and turkeys from York to London, when in the jockey phrase, the geese were said to beat the turkeys hollow !

The

The Professor says that “ the animals also
 “ whose legs are intended both for walking
 “ and flying, are but badly formed for either,
 “ as the bat.” When he spoke of animals, I
 expected with some anxiety a long list of them;
 when animals turn out to be an insignificant
 solitary bat! a poor exception in the order of
 the creation to be placed in a parallel with the
 double purposes of the urethra of man! and
 one of them the most exalted of all purposes!
 The claws of the bat are annexed to its wings;
 it was never seen to walk; it sticks by them
 to the walls. The bat can be much better
 compared, and the parallel will be found ger-
 man with the book of the Professor. A bat,
 according to naturalists, belongs to the order of
 primates, it is a monster, it has a character of
 its own, so has the book. A bat sculks in
 holes, seldom is seen, and avoids the light*,
 so does this book. A bat is torpid, dull, and
 can scarce make a flight, so is this book.

Ah down it falls, in vain for fame it springs,
 A perfect lifeless bat, with leather-wings!

* *Tectaque, non Sylvas celebrant; lucemque perosæ
 Nocte volant; seroque trahunt a vespere nomen.*

OVID.

Such

Such I say are the miserable shifts the Professor is reduced to in assigning a cause for the diseases of the urethra, when he denies the obvious cause from gonorrhœal inflammation; and such is the piteous predicament of an author who has been regardless of all history of surgery, who has spurned at all authorities, and assumed the right not only of forming a system from the very foundation out of his own imagination, but has attempted to pass that system upon the world. What can be said with either temper or decency of a man who, without a single authority, without argument or reason, asserts that diseases in the urethra do not originate from gonorrhœa, when he assigns so lame a cause in its stead, and that in the face of general history and common observation! Is all illumining knowledge confined to the Professor! Was it decreed, that we were to remain in ignorance of the causes of the diseases of the urethra till he was prepared to illustrate the truth by seals, otters, ducks, geese, bats, and flying fish!

Let us appeal to the bar. Suppose a man in the character of a lawyer appeared at Westminster-hall, and in defiance of all precedents
and

and established authorities, attempted to dictate his new opinions to the judges and the jury, what would be his reception? *Solventur risu tabulæ.* Suppose a man, in the habit of a divine, ascended the pulpit, and preached on the principles of christianity, but at the same time denied every text in the New Testament, would not the orthodox in divinity recommend to him the open air? Would not the fields near Bloomsbury or Bedlam be deemed a more fitting situation, where the ears of the groundlings would catch the sound, whilst the tale told by such an idiot would be scattered by the winds? Can the Professor produce in his defence any one authour that has made an arrangement of the diseases of the urethra, and which bears a prior date to the history of gonorrhœa? Or can he produce any single authour, since the introduction of gonorrhœa among Europeans, that has written on diseases of the urethra, and assigned the general cause of them to be independent of gonorrhœa? I will not go more largely into this question, if I were, I should but retail an obvious fact: And supposing now that every surgeon had read these assertions of the Professor, that gonorrhœa cures itself, and of course that diseases

eases in the urethra could not proceed from gonorrhœa ; I say, supposing every surgeon to be well acquainted with this information from the Professor ; I shall be glad to know how many of them will join with him in opinion, first of all reminding them, that they have a dignity in character, and reputation in profession at stake ? Who is there amongst the whole profession that will declare that gonorrhœa, or the inflammation arising from gonorrhœa, is not the general cause of diseases in the urethra ?

“ There are five modes of obstruction (says
 “ the Professor) four of which are diseases of
 “ the passage itself. The fifth is a conse-
 “ quence of the diseases of other parts. Three
 “ of the former are a lessening of the diameter
 “ of the passage ; the fourth an excrescence in
 “ the passage ; the fifth arises from the sides
 “ being compressed, which may be done either
 “ by exterior contiguous swellings, or by a
 “ swelling of the prostate gland.”

Perhaps there is not a medical subject for these last two hundred years, that has more engaged the attention of the profession than

this immediately before us ; not only in settling the characters or causes in the urethra that produce the lessening of the stream of urine through it, but also in finding out the best method of cure. Those who have enquired strictly into the history of these diseases, and looked back with a view of acquiring a thorough knowledge of the characters and cure, will find that the present age are little indebted to their own ingenuity, either in the analysis of what these diseases are, or how they are to be cured. The Professor, since he has amused himself with assigning new causes for these diseases, descends from his stilts, and in his method of cure, as in the gonorrhœa, falls nearly into the general system at first ; therefore we may expect something more consistent, if the present calm be not “as Halcyon’s brooding in a winter sea”*. He has given us the opinion of others, but has told us nothing new ; some things that are essential he has forgotten, and what he has revived and brought forward to the view of the young and unread practitioner would pass for his own, if the pen of justice were not to

* Dryden.

mark the intention, and restore the obligation to whom it is due.

I shall consider his five modes of obstruction as they come before me under their separate heads, for I would not wish to turn over the page that I am observing upon, until I had said all that I intended upon it; for every page will be sufficient for its own comment!

Page III. 1. *Of Strictures.*

“ The three first (modes of obstruction) I
 “ shall now consider, of which the first is the
 “ true permanent stricture, arising from an al-
 “ teration in the structure of part of the
 “ urethra. The second is a mixed case, com-
 “ posed of a permanent stricture, and a spasm.
 “ The third is a true spasmodic stricture.
 “ Most obstructions to the passage of urine, if
 “ not all, are attended with nearly the same
 “ symptoms, so that there are hardly suffi-
 “ cient marks for distinguishing the different
 “ causes.”

These are three of the five modes which the Professor points out as obstructers of the urine in its passage through the urethra. And these three are only one, and that one is a permanent stricture sometimes in a state of irritation, which he calls spasm, and this irritability of the stricture and contiguous parts depends upon the situation of the stricture, the manner of living in the patient, and the treatment of the case. What I understand by spasm is, that it is an instantaneous convulsive attack upon any part, that comes and goes without previous inflammation or irritation, and not that continued irritation sometimes felt for years instead of minutes, even where a bougie passes without finding any resistance. I will venture to assert this, that spasm is never a continued symptom; but irritations are continued from some other first cause, and in the instance before us, the stricture is that first cause.

But notwithstanding the Professor is disposed to reject some causes for obstruction, and introduce others of his own, yet my readers must not be too implicitly yielding; before they judge decisively upon the subject, they

3

should

should reflect that other causes of obstruction opposed by the Professor were insisted upon by men of equal respect with him in anatomy, and far before him in judgment. Men with equal claim to veracity, men that would no more depend upon hearsay evidence than himself*. Astruc has given such a history of the venereal disease, has entered so minutely into the origin of it, has recorded the new symptoms of it as they appeared in different periods after its introduction amongst us, has so completely collated the very numerous authors that have written upon the subject, and hath with so much erudition and candour given us their different opinions and different modes of cure, that his authority must always be looked up to so far. It is itself a library on the disease.

The Professor observes, and perhaps not improperly, “ that there are hardly sufficient “ marks to distinguish the different causes ;” and I will add, that as the method of cure will be nearly the same, it is not so material that

* Alderetus, Ametus, Fabricius ab Aquapendente, Col de Villars, Parré, Dionis, Wiseman, Saviard, Le Dran, Turner, Astruc, Sharp.

they should be exactly known. The passage is obstructed, and the opening must be regained. Since that it is found that every fresh authour sets up fresh opinions contradictory to the former ; since from the year 1550 to the year 1786 this has been the case, it is rather fortunate that the cure is not very much affected through it ; not but if we knew exactly what sort of obstruction we had to oppose, we may be better able to facilitate the cure, or in a more compendious manner pronounce the contrary ; we should not then perhaps be unreasonably betrayed by our inability, in flattering with hopes an incurable disease.

Page 113. Chap. II.

Of the Permanent Stricture.

The Professor should have called this a simple permanent stricture, for in reality it is so when there is only a contraction of one point of the urethra, as if it had been girted (not surrounded as the Professor terms it) with a packthread ; and when there is no enlargement of parts externally to be felt as hard and callous

callous as ossification. The Professor obeys the natural dictates from the patients in his description of the symptoms, and hence it must be obvious that his description cannot vary from others before him. He says also, and with truth, that the bulbous part of the urethra is the most liable to be attacked with strictures, and that these so situated prove the more obstinate and difficult of cure. The Professor gives us to understand, that these strictures are “ generally slow in forming, it being
 “ often years from their being perceived before
 “ they become very troublesome.”

And now for another repast of conjecture, conceit, and hypothesis, from the Professor.
 “ It is impossible to say what is the cause of
 “ that alteration in the structure of the ure-
 “ thra, which diminishes the canal : it has
 “ been ascribed to the effects of the venereal
 “ disease, and often to the method of cure ;
 “ but I doubt very much if it commonly, or
 “ even ever, arises from these causes, yet as
 “ most men have had venereal complaints
 “ some time or other, it may be difficult to
 refute this opinion.” The Professor seems determined not to yield up his pretensions to originality ; he perseveres in skepticism, let the
 reforma-

reformation arising out of it be ever so trivial; and notwithstanding it might have been presumed that he had already marked out his strong ground to fortify this new opinion by his allusions to ducks, geese, seals, bats, &c. yet we now find him abruptly breaking up that camp, and trenching himself round by another. Here we cannot but feel for the shifts he is reduced to; we see him overwhelmed with difficulties, sinking under the weight; the task is given up, and I will answer for him that he would now rejoice to compound, by making any atonement for his temerity. I shall separate and answer his new objections.

“ Yet as most men have had venereal
 “ complaints some time or other, it is natural
 “ to ascribe the stricture to them; and there-
 “ fore it may be very difficult to refute this
 “ opinion.” Yet he doubts “ if they ever
 “ arose from that cause.”

This only proves an obstinacy in disposition. It has been for a long time remarked, that strictures prevail more in common than ever, and it must be very obvious why they should: all intercourse with the metropolis being great-
 ly

ly increased, most men sooner or later are infected with gonorrhœa, and of course the proportion of obstructions will be increased. “ Strictures are often to be found in the oesophagus, intestines, prepuce, and lacrymal duct.” All these put together bear not the least comparison or proportion with those found in the urethra. “ They sometimes happen in the urethra, where no venereal complaint had ever been.” “ I have seen an instance in a young man of nineteen who had had the complaint for eight years.” “ I have seen an instance of a stricture in the urethra of a boy of four years.” Does it not appear that the Professor is now at his old system again, making general rules out of exceptions; it must be obvious to every one that these instances are rare, and it appears by his manner of relating them that he knows them to be so. I never meant to assert that the urethra was not liable to diseases independent of gonorrhœa. But I will still support the assertions of all the profession as well as my own, that the general diseases in the urethra arise from gonorrhœa.

“ They are generally slow in forming, it
 “ being often years from their being perceived
 “ before they become troublesome.” I mean
 that this quotation shall be the only answer to
 another of the Professor’s objections in the
 next page to it. “ There have been thirty,
 “ and sometimes forty years between the cure
 “ of a gonorrhœa and the beginning of a
 “ stricture.” I take it very kind in the Pro-
 fessor in saving me this trouble.

And now for a very material objection in-
 deed. “ If they arise in consequence of ve-
 “ nereal inflammation, we should expect to
 “ find them most frequent in that part of the
 “ urethra which is most commonly the seat of
 “ the venereal disease.” The Professor should
 recollect, that it is he only that presumes to
 mark the specific extent * of gonorrhœa in
 the urethra; but that the patients yet feel, and
 that surgeons yet hear them declare, that the
 pain is all along the urethra, in spite of the
 commands from the Professor; but probably
 strictures may be equally rebellious, probably
 they would not be controuled by his magic

* Vide 1st Observ. p. 46.

spell; indeed we do not find that he has attempted to mark their specific extent within an inch and an half or two inches of the urethra. Why did not the Professor, if he needs must persist that strictures in the urethra, as well as other supposed consequences, as he calls them, do not arise from gonorrhœa, treat upon the subject boldly in a separate work, give it any other title but venereal, and assert his right to an independent revolution, instead of remaining in a condition of disobedience and rebellion. Why may not these strictures come on with a westerly wind, as mares have been said to conceive without copulation with horses * ! Let him keep a diary of the westerly winds, and a journal of the diseases of the urethra. When the former prevail, let him look out for

* ——— Furor est insignis equarum

——— illæ

Ore omnes versæ in zephyrum stant rupibus altis
Exceptantque leves auras; et sæpe sine ullis
Conjugiis vento gravidæ, mirabile dictu.

VIRG. GEORG.

The mares to cliffs of rugged rocks repair,
And with wide nostrils snuff the western air;
When, wondrous to relate, the parent wind,
Without the stallion, propagates the kind.

DRYDEN.

the latter, attribute them to any cause but gonorrhœa!

Page 116. 1. *Of the Bougie.*

The Professor begins his history of bougies from the year 1750, and makes the following remark upon the practice of that time, “ When I compare the practice of the present
 “ day with what it was in the year 1750, it
 “ hardly appears to be the same disease we are
 “ treating.” Whatever might have been the condition of the Professor’s mind at that time, I neither wish to know, or is it of the least importance if it could be known; but unenlightened as it may be, it could not have been said of others, that they laboured under the same degrading imbecility. The fault and complaint lie both with the Professor; for Mr. Sharp’s Critical Enquiry was then published.
 “ Daran was the first who improved the bou-
 “ gie, and brought it into general use. He
 “ wrote professedly on the diseases for which
 “ it is a cure, and also of the manner of pre-
 “ paring it; but he has introduced so much
 “ absurdity

“ absurdity in his descriptions of the diseases,
 “ the modes of treatment, and of the powers
 “ and composition of his bougies as to create
 “ disgust ; yet this absurdity has been much
 “ more effectual in introducing the bougie in-
 “ to universal use than all the real knowledge
 “ of that time, directed by good sense, could
 “ have been.” Here follows a most curious
 position ; as if the present mode of inocula-
 tion was a wrong thing introduced into prac-
 tice, not from preferable utility, but from
 enthusiasm only. “ Such extravagant recom-
 “ mendations of particular remedies are not at
 “ all times without their use. Inoculation
 “ would have still been practised with caution,
 “ if it had not been for the enthusiasm of the
 “ Suttons.”

My reason for giving this long quotation I
 hope my readers will applaud. It is to sepa-
 rate the vile impositions of Daran from the en-
 thusiasm of the Suttons, as he terms it. Da-
 ran forced his way into popularity by the
 meanest of all arts. The Suttons took the
 ground of men convinced of the truth of their
 innovation ; and conscious of the rectitude of
 their system, they persevered with true cou-
 rage

rage and a laudable enterprize. Their success proved the value of their pursuit; and although they had not received the best of education, yet of so much importance was their practice to society, that they fairly triumphed in spite of opposition from men of education, who wrote pamphlets then, which they would be glad to disown now. All I know of this family is, the publick good they have done; and as an individual who am convinced that the present system of inoculation is by far the noblest of all modern improvements, I hope that their rewards have been at any rate adequate. The idea could afford but a fleeting profit to them, for it was no sooner known than it was universally adopted. I am not worthy to do justice to this great act of utility; it would be an ornament to the best heart and ablest pen. But the preservation of lives and beauty ought to be considered with publick gratitude.

The Professor has not given himself the least trouble, notwithstanding the voluminous extent of his work, to inform his readers of whatever is said on the diseases of the urethra, or on the methods of cure, by ancient authors :

thours : The reason must be obvious, because the origin of these diseases would make against his theory, as I have already proved, and I will also prove, that there is not a single novelty in any of the methods of cure adopted by the Professor, I shall only defer my proceeding to such proof until the page offers that treats of the cure. It is sufficient in this place that I have given my readers a hint that I knew why the Professor did not unmask to us the concealed mysteries before the year 1750 of the cause and cure of the diseases of the urethra. The science of treating this subject has been advanced to the present perfection by the emulous diligence of successive authours, and the gradual discoveries of one age improving on another. Cicero remarks, that not to know what was transacted in former times, is to continue always a child. And if by not reading of books a man ascribes to himself the invention of arts generally known, he is guilty of a fault a child cannot commit.

Page 117.

“ Of the Treatment of the Permanent Stricture.”

The Professor says that “ the cure is either
 “ a dilatation of the contracted part, or a de-
 “ struction of it by ulceration, or escharotics;”
 that “ the dilatation is performed by bougies;
 “ the ulcerative process is also affected by a
 “ bougie, and the destruction by escharotics
 “ is by means of caustics, as the lunar
 “ caustic.”

When the Professor divided the three first modes of obstruction, I foresaw that in his treatment of the first, he would confound at least the second, for it could not be otherwise, if he adhered to the nature of the complaint; and as he has now so properly, if accidentally, included them, I shall just point out to my readers what he does to “ quiet the spasms,” or, in other words, to abate the irritation that accompanies the permanent stricture. In such cases, therefore, he says, “ we must have
 “ recourse to temporary relief; such as the
 “ warm bath, which ” he archly remarks,
 “ coun-

“ counteracts the effects of cold, and quiets
 “ any spasms that may have taken place in
 “ the parts; and clysters also with opium,
 “ which have still more effect.” “ Producing
 “ of stools often lessens the spasms—probably
 “ a blister to the perinæum will be of singular
 “ service, if there be time for its operation.”

Before I proceed further, I choose to contemplate the necessary information the Professor has afforded us for quieting of the spasms, if they should accompany a stricture. The blister to the perinæum is amongst the best of his new opinions. I do not recollect having ever seen in print before, the idea of a blister being applied to that part. We all knew of the planet, but did not think to look for it in this hemisphere. The Professor has my hearty congratulation, but I wish he could have been more lavish in its praises, he but coldly compliments so warm a remedy, there should be something more than “ probably ” to recommend it. He talks of doubting if there be time for the operation of a blister: In the repeated irritations brought on by confirmed strictures, there will be time enough for the operation of successions of blisters.

“ It is not an easy matter in cases where
 “ the passage is very small, to tell whether
 “ the bougie has entered the stricture or not ;
 “ for such slender bougies as must generally be
 “ used at first, bend so easily, that the intro-
 “ ducer is apt to think it is passing whilst it is
 “ only bending.” If the Professor had been
 aware that this paragraph would have attracted
 my particular regard, I dare say he would have
 studied in it more of elegance than common.
 If the introducer was apt to think it was pas-
 sing, whilst it was only bending, would he not
 see that it was twisted after he had withdrawn
 it ? But my comment shall not end here. I
 have perused the whole of the Professor’s chap-
 ter of the treatment of the permanent stricture,
 and I find that one of these weak bougies is the
 only remedy the Professor makes use of to
 cure the stricture by dilatation, but if this fails,
 the mode of cure by dilatation is to be aban-
 doned, and his ulcerative method, or rather
 his method by escharotics, are to come next
 into play. And here I hope my readers will
 permit me to recall their attention, whilst I put
 a few questions immediately to the Professor ;
 they will have the opportunity of seeing the
 enquiry fairly investigated, and their opinion,

as to the result, is the boon I expect for my reward.

The Professor says, that “such slender bougies as must generally be used at first, bend very easily.” Whoever has made the attempt must agree to the truth of this assertion, and there never was a more obvious reason given why the defect should not be supplied by some method already known to be preferable; by some other bougies yet more slender, but not so easily bent. The observation I am going to make was not thrown in my way as treasures are sometimes found by a ploughman, but it is an acquisition obtained without reproach to me, by a justifiable condescension to seek for and acknowledge that information from reputable authors, which my own understanding could not supply.

Has the Professor never heard of cat-gut bougies being in use? Has he never heard it hinted that they had a preference on many accounts to the feeble, slender bougies which he recommends? Has the Professor never read any authours who have recommended them? Does not he know that they are now brought

F 2

into

into general practice from their preferable advantages? If he had not heard of them, he is yet to be informed. If he has heard of them, he has abused his reader by his contemptuous silence. And if he does not know of their application, and more certain success in overcoming the most contracted strictures, he has yet to learn what the whole body of surgeons are already acquainted with.

I shall now produce my authority. A quotation from Mons. Le Dran will serve to strengthen my assertions, and point out to my readers where they may refer more at large. As my intention is never to lose sight of the new opinions of the Professor, I am always watchful to detect myself when I am episodically deviating into extraneous enquiries.

“ All these intentions may be frequently
 “ answered by the use of bougies, armed with
 “ medicines agreeable to the state of the dis-
 “ order, and carefully introduced through the
 “ urethra into the bladder. The medicines
 “ made use of on these occasions should by no
 “ means be irritating; and for this reason I
 “ should absolutely reject the use of any caustic
 “ introduced into the urethra on pretence
 “ of

“ of destroying the carnosities, since, as I
 “ have learned from experience, they serve
 “ only to eat into the canal. These bougies
 “ are made either of cat-gut, of linen dipt in
 “ wax and rolled; or of linen waxed and
 “ rolled upon the cat-gut. The second kind
 “ of these are improper to be used, as they
 “ soften in the canal by the heat of the part,
 “ and cannot readily be passed into the blad-
 “ der.”—“ The cat-gut bougie, which, by
 “ the moisture of the part, swells in the ure-
 “ thra during its continuance there, in some
 “ measure removes the strictures it passed by
 “ at its entrance; and on pulling it out in an
 “ hour, or thereabouts (for it must not be left
 “ in longer), you will find it twice as large as
 “ when it was put in.”—“ This second bougie
 “ swells in the same manner, and enlarging the
 “ canal still more, the urine begins to discharge
 “ with greater freedom. When the bougies of
 “ cat-gut enter readily, we must use those
 “ made of linen rolled upon cat-gut, &c.”*

I will not pass over an obvious remark that sets the consequential, self-important assertion

Le Dran's Operations in Surgery, translated by Gataker. 4th edit. p. 275, 276, 277.

of

of the Professor in its true light : “ When I
 “ compare,” says he, “ the practice of the
 “ present day with what it was in the year
 “ 1750, it hardly appears to be the same dis-
 “ ease we are treating.” I have repeated this
 once before, and I will again and again. These
 are the traits that discover palpably how he
 would arrogate, if he were permitted. If what
 I have already quoted be not sufficient, let
Monf. Le Dran be fully read, and his treat-
 ment be compared with the present by the Pro-
 fessor. Let *Monf. Saviard* also be produced. Let
 this assertion be fairly investigated, and then
 let it be seen, what credit an authour, who has
 been bold enough to risk it, deserves in return.
Hudibras says, there is no argument like mat-
 ter of fact.

I am inclined to think, that had Mr. Pott
 favoured us with his opinion upon this question,
 he would have judged the use of cat-gut bou-
 gies to have been of some importance in over-
 coming the narrowest strictures in the urethra;
 and I am induced to think so, not from any
 knowledge I have of his practice, but because
 it is conformable in analogy to the similar treat-
 ment he adopts in his observations on the fis-
 tula

tula lacrymalis. “ The point to be aimed at
 “ is, if possible, to render the nasal duct per-
 “ vious to the lacrymal fluid ; and we must
 “ endeavour to obtain this end by such means
 “ as give the least pain, excite the least in-
 “ flammation, and leave the parts as near as
 “ possible in their natural state ; that is, we
 “ are to endeavour to dilate the passage from
 “ the sac to the nose, by some means which
 “ will gradually distend it without destroying
 “ its texture, in the same manner as the dila-
 “ tation of the urethra ought to be affected in
 “ the case of strictures, by passing either a
 “ probe, or a piece of cat-gut, or a bougie,
 “ gently into it, as far as it will easily go, and
 “ repeating it occasionally, until it is got quite
 “ through, and the passage is free.” In a
 note to the same page he further remarks,
 “ This caution is very necessary to be observed
 “ in the cure of strictures of the urethra, in
 “ which case, the proper intention is gradually
 “ to dilate the passage, and to procure an en-
 “ creased discharge of mucus from the lacu-
 “ næ ; this should be always done gently,
 “ and by means which give as little pain as
 “ possible ; whatever irritates or gives pain
 “ will

“ will certainly do mischief, will add to the
 “ obstruction, and encrease the dysfury*.”

Thus have I sanctioned my assertions, and made good my ground in the first quotation, which I call point blank to my purpose, from the most eminent surgeon in all France, who flourished and published before the dismal year of ignorance 1750. And as he has not expressly written upon the subject, but introduced this question where the only opportunity offered to him, thus is also in the second quotation my opinion sanctioned collaterally by the most eminent surgeon of every country.

And I might be permitted to remark, that in this quotation which I have taken the liberty of applying from Mr. Pott, there is another lesson, and that not the least essential to my readers, as well as myself; the Professor also may hereafter profit from it, if he pleases, and would, if my influence could avail; but I fear that all I can do for him, will be to give him room in my charity. The freedom I profess will not permit me to dissemble, and, I hope

* Pott on Fistul. Lacrymal. vol. i. p. 343.

at least, he will always esteem my praise for honest, whenever I bestow it, as I do now my advice. “ We must endeavour to obtain this
 “ end by such means as give the least pain,
 “ excite the least inflammation, and leave the
 “ parts as near as possible in their natural
 “ state.”

Has the Professor called to his aid all such means? Has he not employed the acuteness of his knowledge sparingly, and used his professional strength by halves? Has he insinuated the least hint, or encouraged the least thought of any other bougie than this which others have discarded from despair of success from it? One would almost think it a miracle that he should have been fortunate, or that the choice of fortune, or the arrangement of chance, had in this instance crowned the fame of a surgeon!

Now that I have grounded the preferable pretensions of the cat-gut bougies by such strong theoretic authority, I shall proceed to enquire how the present practice stands, whether the imbecile texture of a piece of linen dipped in plaster, and rolled into a small form,

is the only instrument in present use with surgeons both in London and Paris. The fact is, that the cat-gut bougies are deemed preferable in both places*.

For my own part, I have carefully avoided throughout my observations resting any assertion upon my bare authority; for which reason, whenever I have objected to the theory of the Professor, I have chosen rather to oppose him by the opinions of others, than to be too confident in my own experience; I know its fallability, but I am not afraid or ashamed to add my attestations to the general opinion, and further to declare, that the cat-gut bougies will overcome many strictures by the first and most desirable means, where the others must fail; and that without a trial of them, modern practice is not justified in appealing to more painful and dangerous resources.

So much for the cure of the permanent stricture by dilatation; a mode of cure that

* Although the cat-gut bougies made in France are sold by Mr. Savigny at two shillings each, and the other bougies at five shillings the dozen, yet he confesses that he has a very great sale of the former.

should

should always be aimed at, and reluctantly abandoned; for ever bearing in mind the admonitory caution of Mr. Pott, which I have already quoted, and which will be considered as an aphorism as long at least as we practice from reason.

Page 122.

3. “ *Of the Cure of Stricture by Ulceration.*”

“ The cure of a stricture by means of ulceration is likewise effected by a bougie; this method may be employed both in cases, where a bougie will pass, and where it will not.”

It is not necessary for me to confess that I know the method by ulceration may be employed in both cases where a bougie will pass, and where it will not. The possibility is amongst the most obvious of the Professor's positions; so that at any rate such a possibility cannot be entitled to the credit of a new opinion. But I am still under some anxiety least

I should prove too importunate in asking the Professor a question or two ; my thirst for information must apologise for my forwardness.

Could I dare to employ this method by ulceration, where a bougie will pass without it ? Or if I dared, should I be permitted to put it into practice ? Have I a friend that would so far indulge me, or an enemy that I would wish so much to inflict ! A garrison town, when the gates are shut against the enemy, is sometimes stormed by a breach ; but did the Professor ever know, and it is possible he may, especially as he draws all his conclusions from singular cases, that any person through choice pulled down part of his house in order to gain an immediate entrance, when the outer door was already open to him ?

The Professor confesses afterwards, that even where this process is attempted, “ few patients will submit to the practice ; and indeed few will be able to bear it.” And I say that it ought not to be attempted but from the necessity of the case, and that then it has the preference to all other painful resources.

“ If

“ If the smallest bougie which can possibly
 “ be made with some degree of strength,
 “ cannot pass, dilatation becomes impracti-
 “ cable, and it is necessary that something
 “ else should be done for the relief of the pa-
 “ tient ; and the destruction of the stricture
 “ is to be effected some way or other. In
 “ many cases, it may be proper to attempt
 “ this by ulceration of the part, for we find
 “ from experience, that a stricture may be
 “ removed by the simple pressure of a bougie.”

There is a certain rationality in this quota-
 tion that I am not always in good fortune to
 find when I seek it. In a work so volumi-
 nous, and in a system so new, it must not be
 indeed expected, that every sentence we peruse
 should attract our admiration, or that every
 opinion that is given should improve our un-
 derstanding. The remark here of the Pro-
 fessor is just, if it be not new ; and I know he
 will pardon me, when I say it is just, because
 it is not new ; for which of his new opinions
 is just ! My praise is here bestowed on the
 Professor for the tranquility of his disposition.
 He has made use of an acknowledged truth,
 and I was glad to recognise it. But yet I
 hoped

hoped to have been for a moment at least gratified in full without allay. He says, “ in
 “ many cases it may be proper to attempt this
 “ by ulceration of the part.” I looked forward with some mortification at my disappointment in search of these cases, to obtain information of the line the Professor was about to draw ; when we were to set about this mode, and when we were to resolve upon another ; for if he has not as yet made up his mind upon such distinctions, he should have referred us to Mr. Sharp’s Critical Enquiry, or what would have been better for himself, he should not have written.

In the next immediate paragraph, the Professor indulges us with something of his own, and this is to account to his readers, and to mine also, how the destruction of the part by ulceration is affected by a bougie. Here I must beg their particular attention.

“ This effect must arise from the irritation
 “ of absorption being given to the diseased
 “ part, which from the stricture not being an
 “ original formed part, nor having any power
 “ of resistance equal to the original one, is
 “ more

“ more susceptible of ulceration, and thereby
 “ is absorbed.”

Will my readers permit me to instruct them?
 It is, that they read over the above at least
 three times, and if they can then make any
 sense out of it, will they compare it with the
 succeeding paragraph which I shall now lay
 before them? I must enjoin them to read this
 three times also.

“ The force applied to a bougie in this case
 “ should not be great; for a stricture is the
 “ hardest part of the urethra, and if a bougie
 “ is applied with a considerable degree of pres-
 “ sure, and continued there, it sometimes
 “ happens that the end of the bougie slips off
 “ the stricture before there is time for ulce-
 “ ration, and makes its way into the sub-
 “ stance of the corpus spongiosum by the side
 “ of the stricture; and if the pressure be
 “ continued still longer, it will make a new
 “ passage beyond the stricture in the corpus
 “ spongiosum urethræ.” To elucidate the
 last observation, the Professor furnishes us with
 a plate.

Here

Here is so much matter crowded in upon me at once, that I must beg time and patience for the separate investigation of it. As to “ the “ irritation of absorption being given to the “ diseased part,” the Professor ought not, and here I condemn him very seriously in having taken such a palpable advantage of our unenlightened condition ; so exalted an expression would have been becoming in the Roman age, but then only at Rome itself, where that which was out of the reach of human understanding, could have been explained by oracular divination.

Let us proceed to examine if the flat contradiction expressed in the two quotations be more within my power to explain to my readers ; and I think if I cannot make out the Professor’s meaning by his words, yet I can by his intentions.

To reconcile the contradiction, as it appears upon the face of it, would be offering an affront, which I am not prepared to do to my readers. But if they did but consider, that the first paragraph embraced part only of the system which had been adopted by Mr. Sharp,
and

and continued ever since; and if that the Professor had given us the whole of the action of the bougie by pressure, which gains a passage by ulceration, when it cannot gain it by dilatation, then he would have told us the whole truth, that the point of a strong bougie so continually presses upon the stricture that it meets in its way in the urethra, as to effect an ulceration of the obstructing part. Then he would have told us, that the obstructing part is only exposed to such an operation, as it stands exactly in the way of the force that is applied intentionally against it. Then he would have told us, that this is the act of a bougie that passes a stricture by ulceration, and that this was Mr. Sharp's system, by continuing the pressure of the bougie. The obstructed part only offering directly to the point of the bougie, is the fact upon which the cure by ulceration was established by that reputable authour, and persevered in ever since. It does not depend upon which is the strongest, or which is the weakest; not whether the Professor is right in his first paragraph, or wrong in his second; as "no such stuff was ever in our thoughts;" not whether the diseased part will give way before the sound

H

part,

part, or *vice versa*. The only question is, which is most in the way of the pressure of the bougie?

And cannot my readers guess at the motive that induced the Professor to half explain the first, and so suddenly to contradict himself in the second paragraph? He could not design any underhand system, for if he had, he would have guarded it with more cunning. It is not congenial with his disposition. Common precaution had no concern in it. The contradictions come so quick upon us, that we ought to take credit to ourselves for finding them out, as they might have been easily passed over. An umpire in the most critical nicety at any game could not boast of more adroitness.

Let me ask my readers, was not the subject of the next chapter, was not the innovation of caustick, was not the new opinion, or rather the revived old opinion from Wiseman, and an old opinion which Wiseman himself had revived from a time as old as the history of strictures, a temptation irresistible? Was not something, not done in this age, a grand
idea

idea open to the Professor? The coming out in a new character, or in an old one revived, is an object for every actor in life. He knows the advantage of a first impression, that it is snatching a grace beyond the reach of common art. Besides, did not the affrighting dread of a bougie making a wrong passage pave the way to a new case? Did not that case enrich the work with a plate? We may envy vaulting ambition, but we cannot blame it, when we see it so towering, even if it cries havock, or if by chance miscarriage in the attempt makes the object ridiculous.

I shall close this chapter with one more quotation from the Professor, and as it is seriously of importance, I hope my readers will not call it frivolous. “Bougies, in all cases, “from their shape, and from the action of “the parts, readily slip out, which retards “the cure, but it is much worse when they “pass into the bladder.” Here follows a case; so that I beg they would take warning by this unhappy case, and take care that the bougies may never slip out, or never slip in!

Page 126.

4. “ *Of the Application of Caustic to Strictures.*”

If it hath not struck the notice of my readers already, it will evidently from the smallest attention appear that the Professor flurred over the last chapter, and aimed to get rid of the system contained in it, for the sake of this we are now upon. Thus men send their sons in law out of their families, in order that their own sons may enjoy the whole of their favours and affections. The Professor should recollect, that he had undertaken to treat fully the former system, and that, as he had so far condescended, he ought to have bestowed upon it all the advantages it could fairly have born; and that in as many instances as he has neglected the whole of his duty, he has brought down dishonour on himself.

The system of ulceration by the pressure of a bougie was to be made inadequate to the end, for the sake of the application of caustick.

The

The application of caustick, the Professor wishes to insinuate and palm upon his readers as a new opinion ; that, will be a matter of consideration for me hereafter. I shall proceed directly to consider the cure by caustick, and examine the statement of it, as in his wisdom he has laid it before us.

“ When a bougie can readily pass, there
 “ is no necessity for using any other method
 “ to remove the stricture.” And so say I.
 But how did the Professor’s former chapter begin in “ the Cure of a Stricture by means of
 “ Ulceration ?”—“ This method may be employed both in cases where a bougie will
 “ pass, and where it will not.” It is not my business to reconcile this contradiction. Here it is. I have set the two paragraphs down by each other, and there I will leave them !

The Professor states three causes that may require a cure by caustick. “ First, the
 “ stricture may be so tight as not to allow the bougie to pass. Second, the orifice of the
 “ stricture may not be in a line with the urethra, which will make it uncertain, if not
 “ impossible, to pass a bougie. And third,
 “ there

“ there may be no passage at all, it having
 “ been obliterated by disease, and the urine
 “ passed by fistulæ in perinæo.”

The Professor asserts, “ if the obstructions
 “ are any where between the membranous
 “ part of the urethra and the glans, where
 “ the canal is nearly straight, or can easily be
 “ made so by the introduction of a straight
 “ instrument, it becomes an easy matter to
 “ destroy them by caustic.”

I shall only remark to my readers what has
 occurred to me, and I do not doubt but that
 they, who are conversant with cases of ob-
 structions in the urethra, will agree with me
 in the opinion, that obstructions in the favour-
 able part alluded to by the Professor are easily
 removed without the aid of caustick.

“ It does not always happen in cases of
 “ obstruction to the passage of the urine, that,
 “ when the obstruction is removed by the
 “ caustic, and the water of course passes
 “ freely, a bougie will pass. This I appre-
 “ hend arises from the caustic not having
 “ destroyed the stricture in a direct line with
 “ the

“ the urethra, so that a bougie cannot catch
 “ the sound urethra beyond.”

Here we are told of advantages from caustick, which common sense would have construed into evils. First, that when a caustick succeeds, a bougie will not always second that success. And second, that a caustick may make a new way with impunity, where a bougie must not.

First, if a bougie will not pass after the caustick has removed the obstruction, the patient, that is advised “ not to go a journey, “ even of a week, without them*,” may as well leave them at home. And second, if a caustick may be permitted to destroy the stricture, or rather the substance of the urethra, not in a direct line, with impunity, then it must be indifferent what route the bougie may take, and the caution, and the case given by the Professor, might as well be erased, and so may the plate, where he said that a bougie passed into the corpus spongiosum urethræ †,

* Page 124.

† Page 123.

and which gave birth to a new case, and plate the second.

While the Professor was dealing out with all the force of oratory his instructions to his attentive pupils, did it never occur to him, that
 “ it is easier to teach twenty of them what
 “ were good to be done, than to be one of the
 “ twenty to follow his own teaching * ?

I shall now proceed to examine the instruments by which the Professor conveys this formidable but effectual power to the obstructed part of the urethra.

“ The caustic should be prevented from
 “ hurting any other part of the canal ; which
 “ is best done by introducing it through a
 “ canula to the stricture, making it protrude
 “ a little beyond the end of the canula, by
 “ which it acts only upon the stricture. The
 “ caustic should be fixed in a small port
 “ crayon. It is necessary to have a piece of
 “ silver of the length of the canula, with a
 “ ring at one end, and a button at the other

* Shakespeare.

“ of

“ of the same diameter with the canula,
 “ forming a kind of a plug, which should pro-
 “ ject beyond the end of the canula that en-
 “ ters the urethra, by which means it makes
 “ a rounded end ; or the port crayon may be
 “ formed with this button at the other end.
 “ The button being introduced into the ca-
 “ nula, it should be passed into the urethra,
 “ and when got to the stricture the silver plug
 “ should be drawn, and the port crayon with
 “ the caustic introduced in its place ; or if
 “ the plug and port crayon are on the same
 “ instrument, then it is only withdrawing it,
 “ and introducing the port crayon with the
 “ caustic, which will destroy the stricture.”

The Professor proceeds to remark, “ if the
 “ stricture be in the bend of the urethra, the
 “ canula may be bent at the end also ; but
 “ it becomes more difficult to introduce a
 “ piece of caustic through such a canula,
 “ for the plug and port crayon must also be
 “ bent at the end, which cannot be made to
 “ pass through the straight part of the ca-
 “ nula ; but this I have in some measure ob-
 “ viated, by having the canula made flexible,
 “ except at the end where it is to take the
 I “ curve.”

“ curve.” The third plate given by the Professor demonstrates this piece of machinery.

I must, before I proceed to make my observations upon the presented plan of the Professor before us, candidly confess to my readers, that I have been strongly prejudiced in favour of a trial to overcome obstructions in the urethra by caustick ; and that, when I perused the antient authours who had written upon the subject, I lamented the inadequate power of conveying the caustick with safety to the obstructed part. And what was yet to be more dreaded, the mischief that might be committed by so potent an application acting on parts excluded from our sight, and perhaps destroying such as did not conduce to the removal of the obstruction. Besides, I further dreaded the alarming effects of inflammation, and retired from the undertaking, because I found that they, whose opinions I looked up to, were not so satisfied of the possibility of its safety, as to set the example. On the contrary, the most eminent of the profession that had last written upon the subject, execrated the idea, and flung it out of practice *.

* Le Dran, Saviard, Astruc, Sharp.

Having

Having premised to my readers, that it was my hearty wish that a safe and effectual mode may be found out for destroying a stricture in the urethra by caustick, I shall proceed to examine whether this given by the Professor promises to be that safe and effectual mode. And as, under the sanction of his recommendation, I have already made a trial of it, I mean to make a true report of the result. I before remarked, that I never found an obstruction in the strait part of the urethra, which was of so serious a nature, as to require a new resource to be called to our aid; therefore I put obstructions thus situated out of the present question. If caustick will succeed there, so will subordinate means; means more innocent, and therefore preferable, as effectual.

The case on which I attempted the trial was an obstruction of ten years standing, situated in the bulbous part of the urethra. The stream of urine was very small, and evacuated with much difficulty, and sometimes it came away by drops. On account of the regular life of the patient, who was fifty-two years of age, the irritation was kept a good deal

under, he could enjoy an interval of three hours without making water. A tumour in the perinæum was to be felt as large as a walnut, callous and immoveable. I provided myself with the apparatus of the Professor, and having by the previous use of the cat-gut bougies gained upon former advances near half an inch, as we found by measuring the length of the cat-gut with the length of the bougie the patient had been in the habit of wearing, of course we advanced further through that part of the urethra where it is curved. On my introducing the strait canula, I found it could not pass so far up the urethra as the cat-gut bougie by near an inch, and upon my enquiring afterwards for a flexible one, I found that the first of this invention was but just made, so that the one given in the plate in the work before me might be said to be the pattern draught by which the maiden one which I saw was made. But if the flexible canula had been ready at hand, it would have been impossible to have conveyed the caustick through it with any safety; for the port crayon embraced so small a portion of the caustick, that the least resistance would loosen it, and it was

im-

impossible to pass it through the flexible canula whilst fixed in the urethra, without its meeting with more resistance than when the experiment is made out of the urethra. Besides, the ring that tightens the caustick in the port crayon would be slid back when the caustick was attempted to be introduced through the canula. But, resolving that it should not be said that I raised unfair objections, I passed the caustick through the strait canula, after withdrawing the plug, and I permitted it to remain more than a minute. The effect upon the patient was not in the least to be dreaded, and we certainly should have persisted in a repetition, had we not been prevented, by proving that it would not have been safe; for upon the slightest touch, the caustick dropped from the port crayon, and thus ended the experiment.

This complex, intricate apparatus is found to be deficient in so many circumstances, that I do not hesitate to pronounce that it should never be tried; and here follow some of my reasons.

First,

First, that a strait canula, and of that size, will never pass any thing near home to a stricture that is an object for caustick,

Second, that it is difficult to break or cut a piece of caustick into so exact a size and shape, as to be received by the port crayon, and not to overstrain it.

Third, that the ring has scarce any hold, and is apt to slip back, by which the caustick may escape.

Fourth, that if the flexible canula be substituted for the strait, when it has passed the curvature of the urethra, it would be running an unwarrantable risk to introduce the caustick through it, fixed as it is in the present instance; and I pledge myself upon the assertion, that it would sometimes be broken off, and sometimes loosened. For although the caustick may be passed through the flexible canula safely upon making the experiment out of the urethra, where it has all the freedom of flexibility; yet let it be remembered, that the canula, after it is passed into the urethra, and conformed to the curvature of it, is no longer flexible,

flexible, but fixed, and of course the caustick must rub against the sides of the canula in passing to the obstruction.

Fifth, on account of the large size of both canulæ, when compared with the contraction of the urethra, and so far also up the urethra, the caustick must be passed far beyond its guard, in order to reach the obstruction. The mischief it may commit on unnecessary parts, cannot be prevented; and as it is at so great a distance from our fingers, and will bear so little pressure consistent with safety, I found it very difficult to determine what degree of force was necessary to be applied to it, so that that force may be both safe and effectual.

And Sixth, to sum up all, and my comparison in this instance shall not be said to be hyperbolick; this piece of machinery is found to be so laboured, so ticklish, and so dangerous in all its component parts, that I look upon it, if, upon trial, it succeeds in every necessary repetition so as to perfect a cure and do no mischief, that the miracle would be as great as a man walking with safety to and from

from a tight rope fastened at both ends to the tops of St. Paul's and the Monument.

I have thus coolly examined the use and abuse of the Professor's new apparatus, that flames so much in print, and that stands foremost amongst the first exertions of his genius ; the subject of gonorrhœa and sympathy only excepted. Not that I myself thought, that his apparatus merited so moderate and so minute a consideration, but because I was convinced, that if the instructions he had given were not profitably right, they must in the end be mischievously wrong.

For although it may be admitted that such an apparatus was safe in his hands to overcome a stricture in the strait part of the urethra ; for I can hardly even in his hands admit that it would be safe to risk it further. For although, in case of an untoward accident, the caustick should be disengaged far up in the urethra, the Professor may have the address to clear himself of such an embarrassing situation, and such an ascendancy over the temper of his patient, as to be pardoned

and acquitted; yet what would become of the inferior order of the profession, of those who hold but inferior offices in the temple of fame! Would not the honest reputation they had gained by unwearied assiduity, their acquirements from uncommon constancy and stubborn perseverance be crushed in a moment, and tumble upon them altogether? Let us be bold and daring, but let our enterprises be safe; let us not be surprised into dangerous dilemmas for want of prudence in guarding against them. Let us not be too implicitly confident in the new opinion of any man, let his reputation be ever so high, for very often, like a false glare, it hinders us from discovering the insignificance of the power that it shades.

The Professor's judgment in the instance of this work before us proves to be deficient in the extreme; and we have a right to conclude, that, on other subjects upon which he has not written, the same defection will be found. Hume remarks, that where several known circumstances are observed to be similar, the unknown also will be found to be similar. Thus if we see the limbs of a human body, we are sure there belongs to it a head, if it be

hid ; or if a small part of the sun shines through a chink in a wall, by removing that wall, we are sure to see the whole of that body.

But why should I dwell longer upon this subject it may be asked, as I know that the Professor is constantly now altering that very apparatus which is described in his book, and exhibited in a plate. The reason must be obvious ; because it is that very apparatus described, and that very plate, which are to direct the practitioner, and not the alterations of them that he carries into his own private practice. Both ought either to be torn out of the book, or left like rocks in charts, to warn us how to avoid them ; to be as monuments of human insufficiency, or broad hints of disappointed arrogance.

But if the Professor had modestly failed in a modest attempt to conduct caustick with safety and effect, so as to destroy such strictures as could not otherwise be destroyed, and such strictures as we have sure and certain hopes that the caustick will destroy, when once we have found out a sure but safe method

thod to apply it, I should not have been disposed to have blamed him. Such an attempt would have been laudable, and miscarriage would have been misfortune.

Some, it may be, will say, why do you not, as you have endeavoured to destroy this method, propose one of your own? or, in more general terms, as you have found fault with the theory of the Professor, why have you not given us a better?

Let such remember that I am not pledged to any such undertaking; that I stand upon the firm ground of modern practice; that I am maintaining that ground only; that I am maintaining the system that has the solid opinions of the whole of the profession for its basis; and that I am guarding it against the romances of new theory, and the fanaticism of new opinions; that I am resisting a positive individual who assumes the power of standing alone, one who never acknowledges the obligations he owes to others, and one who denies excellence in all but himself. This is next what I shall undertake to prove, and if I do not prove it, let the censure recoil upon

me, and I will make the atonement as publicly as I now exhibit the charge*.

The Professor's book must be before my readers while this cause is at issue, and so far is he obliged to me in recommending it; for every part is involved in the guilt. But as it is incumbent upon me to specify some particular charges, I will begin by enquiring how he describes himself to have come at the idea of applying the caustick. And here, as it will be found by my readers, it is curious to watch how slow the mind of man advances by progressive steps, from the first simple idea

* The fact is, if caustick must be used, I have certainly adopted a method more safe and effectual than that we have already considered. And my reason for inserting this note here is, to prevent mischief from the use of the other apparatus. I shall not dwell upon its merit. Its simplicity is not the least of the perfection. I have embodied some caustick into the point of a small common bougie, which may be conducted to the stricture through a canula. It may remain about four minutes, and may be repeated occasionally. Mr. Sharp observes, that escharoticks, mixed in plaster, lose their corrosive property; but by constant trial, day by day, I think I have prevented this, as I keep the bougie in glass well stopped. Those so disposed are welcome to see or try it.

to the perfecting of any particular grand object* ! It happened in the year 1752, that the Professor attending a chimney-sweeper for a stricture, the first patient he ever had with that disease, and not succeeding after six months trial, conceived that he might be able to destroy the stricture by escharoticks. His first attempt was with red precipitate †. He applied to the end of a bougie some salve, and then dipped it into red precipitate. This he passed, but it brought on a considerable inflammation all along the inside of the passage ; which he attributed to the precipitate being rubbed off in passing the bougie. It was then that he had a silver canula made, and introduced it down to the stricture, and through this canula passed the bougie with red precipitate as before. But finding, however, that he made water no better, and not yet being able to pass the smallest bougie through the stricture, he suspected that the red precipitate had not sufficient powers to destroy it ; he therefore took a small piece of lunar caustick, and fastened it on the end

* Page 128.

† Vide Sharp's Critical Enquiry, published 1750, p. 173.

of a wire with sealing wax, and introduced it through the canula to the stricture. After doing this three times at two days interval, the chimney-sweeper came to the Professor and told him that he made water much better; and in applying the caustick a fourth time, the canula went through the stricture. A bougie was afterwards passed for some little time, till he was perfectly well. This success encouraged the Professor to apply his mind to the invention of the apparatus we have already described; but when the mountain produced this labour must remain a doubt; as far as conjecture can go, it must be presumed to be not long before the publication of the great work, as I have already observed, that the flexible canula is but just finished.

The Professor being aware that neither the application of the precipitate, or caustick through a canula, was a new opinion, and being from his very commencement in life determined upon the character he meant to maintain, that of being a novelist and an asserter of new opinions, would not be seen to have read a book first, or to ask the boon
of

of information from any man. This is what he wishes to insinuate; this is what he would have his readers to understand.

For although the same methods had been not only practised long before, and had been noticed in a publication, not such as is forgotten with the year that it is printed, but in a publication familiar to every surgeon, and in a publication that was the standard of surgery at the time it came out, in the reign of Charles the Second, for Wiseman was sergeant surgeon to Charles, yet the Professor would feign be seen no more to draw information from the works of the dead, than he would from the living. For it appears from two notes that he has annexed to the history of the rise and improvement of the practice of the caustick in the case of the chimney-sweeper, that he is fearful of being suspected, that the precipitate, the caustick, and even the application of that caustick through the canula were not new opinions of his own. Therefore he has artfully inserted these notes, which have answered fully his purpose, he knowing perfectly well, that it was not in human power positively to contradict him.

The

The first note refers to the idea of the application of precipitate, and runs thus: " Lately looking over some authors on this " disease, I find that this is not a new idea." And the second thus: " Wiseman had the " same idea, but probably the clumsy way " in which he attempted to put it in execu- " tion, might be the reason why he seems " not to have pursued it."

I have just observed, that as the Professor has been pleased to tell his own story in this manner, that he conceived both ideas himself originally, put them into practice, and afterwards in reading for his amusement found that they were not new, it is impossible for any one to contradict him. But thus far I may be allowed to remark: It is very singular that a surgeon, at a loss for six months how to proceed, and as long ago as the year 1752, when he was young in his profession, of a plodding turn of mind, ambitious for making the most of himself, greedy for success, and jealous of eminence in others, should remain so long at a stand still in a case, without referring back to learn what authours had said upon it. But all this is not half so
strange

strange as that the same surgeon should have read one authour since, and in that very authour should have found all the exact ideas that he had adopted. This is a phenomenon worthy to be recorded, and worthy to be told for the filling up a long winter night. And I do not doubt, but, if it falls in the way of the editor, that it will command a place in the next collection of remarkable occurrences. Even a man whose genius qualifies him for great undertakings, must be content to learn at least from books the present state of human knowledge; that he may not ascribe to himself the invention of arts generally known, weary his attention with experiments of which the event has been long registered, and waste, in attempts which have already succeeded or miscarried, that time which might have been spent with usefulness and honour upon new undertakings*.

Thus have I proved that the Professor has been morally wrong both ways. First, if he did form the idea before he read the book; and second, if by reading the book, he found

* Johnson.

the idea that so exactly corresponded to his wishes, and afterwards appropriated to himself the exclusive credit of it. The last of these historick doubts is certainly the most probable; but the truth remains only in the breast of the Professor, and he alone has the power to clear it up. But there is one privilege which we enjoy; we may form conclusions, and these make against him on both sides of the question.

I shall proceed to give my readers the promised treat from Wiseman, and from which I have furnished the repast in argument, that I have hitherto offered for their consideration.

“ I then made a search with a candle, and
 “ having marked the main stop (in the ure-
 “ thra) I proceeded in the use of French
 “ candles, which were made taper fashion,
 “ and passed one over the great earuncle,
 “ bound it on; and left it lying upon it, and
 “ directed the patient to take it out upon oc-
 “ casion of urining, and afterwards to pass it
 “ again. By the use of that, there was room
 “ made for the application of my own can-
 “ dles, which I armed with the composition
 of

“ of mercur. precipitat. &c. and thereby did
 “ wear off the caruncle. Afterwards I met
 “ with others, which I also extirpated the
 “ same way*.”

“ But if after the use of emollients you
 “ cannot pass the caruncle, you may con-
 “ clude it callous. In which case you may
 “ pass a canula into the urethra to that ca-
 “ runcle, and whilst you hold that there
 “ steady, you may convey a grain of caustick
 “ into the canula, and press the caustick to it;
 “ and whilst you hold it there, you will per-
 “ ceive its operation by the pressing forward
 “ of the canula †.”

The Professor sets out with complaining of
 the ignorance of the state of surgeons in cu-
 ring of strictures, thirty-six years ago.
 “ When I compare the practice of the pre-
 “ sent day with what it was in the year 1750,
 “ it hardly appears to be the same disease we
 “ are treating ‡.” And now, alas ! the year
 1786 is to be graced with a manner not pre-

* Wiseman. Folio, 4th edition, page 534.

† Ditto ditto ditto page 532.

‡ Page 116.

ferable to what was practised in the seventeenth century ! This artful plea of the ignorance of the practice before the year 1750 was to throw us off our guard, in order that we may not look back and find old ideas in old books, before that time, vamped and passed as new ideas, in a new book in the year 1786.

I will again repeat the Professor's vague and ill-timed note on Wiseman, for the purpose of remarking further upon it : and although I despise the monkish proverb of, *De mortuis nil nisi bonum*, yet more than humanity teaches me to justify, *de mortuis nil nisi justum*.

“ Wiseman had the same idea, but probably the clumsy way in which he attempted to put it in execution, might be the reason why he seems not to have pursued it.” If the way of Wiseman was clumsy, that of the Professor is more so. The serjeant was provided with a canula, and weighed a grain of caustick to be passed through it to the stricture ; the Professor provides himself with a canula, and passes through it an undetermined quantity of caustick to the stricture. The serjeant's determined grain of caustick
 exacted

exacted a proportionate destruction of the obstruction, the sergeant's analysis was consistent with safety ; the Professor exacts destruction out of all proportion, and has formed no analysis out of any system. As to the " probably " and the " seems," these are only the hackneyed words the Professor has all along made use of to damn the theory of others, and with as much pretence here also, for it does not appear, I declare to my readers, that Wiseman ever gave up the caustick, or discovered the least inclination to do it. But as they can have recourse to the book, I wish they would rely upon their own inspection for determining this point.

I have now closed my evidence upon this charge I exhibited against the Professor. If he was the real parent of the idea, he was ignorantly industrious, for the idea had already been registered ; but if, as I suspect, for such a concurrence is but just within the scope of human probability, he was guilty, I should wish to close my observation upon it, in such words, as his best friend would upon the same occasion express his dissatisfaction ; not in such harsh terms as Mr. Wedderburne applied to Dr. Franklin in
the

the memorable philippick at the Cockpit ; but something after the manner of Hamlet to Polonius ;—“ I took thee for thy betters.”

I shall proceed now to further observations upon the subject of caustick, and as it is in my opinion both serious and important, we have a right to all the information before us which we can get, not with a view to a fashionable practice, but to what ought to be preferred as a solid advantage. Whilst the Professor appears as the determined advocate and strong panegyrist for the application of caustick, we can find in his work nothing but success upon success, here we behold a clear sky without a single cloud. Whilst other writers reprobate the use of caustick, condemn those who bring it into their practice, and consign it to banishment for its mischievous effects ; there we see nothing but tempestuous storms, and inevitable wrecks. When such combatants enter the list, the game is worthy the attention of the spectator. It must not be forgotten, that the Professor has not dropped a single hint of the opinions of the authours I shall produce in opposition to his favourite adoption.

“ I have

“ I have seen several cases where it was
 “ thought necessary to follow this practice,
 “ and it succeeded so well, that after a few
 “ touches with the caustic, the bougie could
 “ be passed, which was all that we wanted.”

“ Most strictures I ever examined after
 “ death appeared to be in the power of such
 “ treatment; however I have seen one or two
 “ cases where the contraction was of some
 “ length, and irregular, which would have
 “ puzzled me, if I had attempted the cure
 “ by caustic.”

But immediately after, the Professor, in the
 actual practice of the application of caustick,
 pronounces so vehemently upon the success of
 it, as to set aside his theoretick puzzle, over-
 turn all doubts in his way, establish in the
 most compleat manner the advantages of caustick,
 by attributing to it even unexpected ac-
 complishments.

“ I have tried this practice in strictures
 “ where there were also fistulæ in the urethra,
 “ and where the water came through disse-
 “ rent passages. Such cases I should suspect
 “ were

“ were not the most favourable, yet I suc-
 “ ceeded in the greater part of them; that
 “ is, I got through the stricture, and could
 “ pass a bougie freely. I have seen several
 “ cases of fistulæ of these parts, where the
 “ natural passage was obliterated by the stric-
 “ ture, in which I have succeeded with the
 “ caustic, and the fistulous orifices have rea-
 “ dily healed up.”

These are the warm commendations of the
 Professor, and this is the abstract and brief
 chronicle of the report of an individual, that
 is to set aside the final resolves of the most
 eminent surgeons in Paris and London, that
 were agreed to unanimously forty years ago;
 and so tenacious have the most eminent sur-
 geons since been of such a deliberate resolu-
 tion, that they have implicitly complied with
 it. A resolution fixed in consequence of accu-
 mulated evils from the use of caustick. This
 state of the case is necessary to be known; all
 the evidence shall be laid before my readers,
 that they may judge with impartiality, whe-
 ther this method of cure by caustick, revived
 by the Professor in so commendatory a man-
 ner, should be adopted, whether his appa-
 ratus

tus for introducing it be the safest yet known, or whether the opinions of those, who from experience and full conviction of its abuse, stood forward to banish it out of practice, should still be maintained.

In the quotations that I last gave from the Professor's work, in praise of caustick, I presume that he had an eye to an assertion of his own in another place in this work.

“ Such extravagant recommendations of
 “ particular remedies are not at all times
 “ without their use*.”

That it shall not be said, that I, in my turn, make it necessary for a man to commit dishonour, even for obtaining an honourable end, I will not say any thing more of myself; I will not say which method ought to be adopted; I will but observe in behalf of the eminent authours, whose opinions I shall silently lay at the feet of my readers, that they pretended to no extravagant recommendations, or *vice versa*; that they were compe-

* Page 116.

tent judges of the nature of such evidence as would convince rational men fairly, and beyond which they would not presume.

“ This bougie being armed with a strong
 “ caustick, had made a considerable eschar in
 “ the sphincter of the bladder, and much
 “ inflamed the canal of the urethra.”

“ A priest of St. Genevieve des Ardents
 “ came to the hospital at the same time,
 “ cruelly tormented by the effect of such an-
 “ other bougie; but the accidents were so
 “ sudden and violent, that there was no time
 “ for relief, the poor ecclesiastick dying in
 “ twenty-four hours.

“ These dismal accounts sufficiently ex-
 “ plode the danger of using too powerful ca-
 “ thereticks. I dare even advance in this
 “ place the treatment by caustick ought to be
 “ banished the practice of surgery*.”

“ The medicines made use of on these oc-
 “ casions should by no means be irritating;

* Saviard's Observations, published ten years before the Professor's critical æra, 1750. Page 167, 168.

“ and for this reason I should absolutely reject
 “ the use of any caustick introduced into the
 “ urethra on pretence of destroying the car-
 “ nosities, since, as I have learned from expe-
 “ rience, they serve only to eat into the ca-
 “ nal *.”

“ But it is not worth while to quote more
 “ authours (upon the infinite number of con-
 “ trivances and forms of applying cathere-
 “ ticks) since this mode of practice has been
 “ long in disuse for many reasons.

“ First, because it is calculated solely for
 “ caruncles and warts which beset the ure-
 “ thra, but by no means for other obstacles
 “ that may straiten that duct, by which the
 “ strangury, which succeeds a gonorrhœa, is
 “ most commonly produced, as is now con-
 “ fessed by every one.”

“ Second, because it is never safe; for ca-
 “ thereticks, which have force enough to con-
 “ sume caruncles, will at the same time in-

* Le Dran's Operations in Surgery, 4th edit. p. 275.
 He wrote also before the critical æra, 1750.

“ flame, corrode, and ulcerate the sound parts
 “ of the urethra. I know that former phyfi-
 “ cians have endeavoured to obviate this in-
 “ convenience by an apparatus of various
 “ kinds of instruments and remedies, but I
 “ know at the same time, that all their cau-
 “ tions have been for the most part fruit-
 “ less *.”

“ However at all times there have been en-
 “ terprising men, who have endeavoured by
 “ escharotick applications at the extremity of
 “ their bougies, to make way through those
 “ obstacles which resist the bougie or the
 “ leaden probe; and to say the truth, this
 “ practice has been avowed by the ablest sur-
 “ geons of the two last centuries, but at pre-
 “ sent it is universally condemned, and indeed
 “ has been so almost ever since Saviard’s
 “ time. The objections to the use of cauf-
 “ ticks were the difficulty, and almost im-
 “ possibility of directing them, so as to eat
 “ through all the diseased parts of the ure-
 “ thra, without destroying the sound part;
 “ the impracticability of preventing the urethra

* Astruc, Edit. 1756. Page 321.

“ from

“ from contracting when it healed, as much,
 “ if not more, than it was at the time of ap-
 “ plying the escharotick : And lastly, the
 “ pain was so excruciating, and perhaps the
 “ application sometimes so poisonous, that
 “ an immediate mortification of the scrotum,
 “ penis, and bladder, were sometimes known
 “ to ensue. Upon these accounts the use of
 “ escharoticks seems to have been entirely re-
 “ jected*.”

Having performed my promise to my rea-
 ders, in order that they should not be hood-
 winked into this opinion, either by receiving
 it as a new one from the Professor, or, without
 knowing the disadvantages set forth, by be-
 lieving that the caustick was adequate to all
 things, and dangerous in none ; I shall keep
 my resolution, and, without any further com-
 ment, leave the question thus fairly before
 them.

But be it remembered, that I cannot carry
 my authorities that I am so lately obliged to,
 into a court of appeal upon the subject of the

* Sharp's Critical Enquiry, 1750. Page 151.

Professor's apparatus for conveying caustick ; that opposition must rest with me only ; and I rely upon the judgment of my readers, from what I have already said, with perfect confidence. If the application of caustick must needs be revived, let it be sanctioned with the approbation of the Profession. Pliny very handsomely says, that when we attempt perilous enterprizes, we should implore the charity of the world. But I am afraid, as we have already been forewarned of the dangerous effects of caustick from such high authorities, that if we failed now of success, in the revival of the attempt, or if fatal consequences now followed the use of it, we should not come within the pale of that gracious plea of the Roman.

Page 131. Chap. III.

“ Of Strictures in Women.”

The Professor here goes over the same ground, and follows up the same opinions that he asserted in his chapter on strictures “ in the urethra of men.”

“ How

“ How far a stricture in the urethra of this
 “ sex is really a consequence of the venereal
 “ inflammation I am not certain, but should
 “ suppose it was not ; and for stronger rea-
 “ sons still than those given in speaking of
 “ the cause of strictures in men.” “ I have
 “ observed before, that in most women who
 “ have the venereal disease in the form of a
 “ gonorrhœa, it seldom attacks the urethra.”
 This last quotation from the Professor is an-
 swered by himself in another immediately
 following. “ Strictures are not near so com-
 “ mon in women as in men, and this is ow-
 “ ing to the great difference there is in the
 “ length of the two canals.”

I have already said as much upon the sub-
 ject of the first quotation as I ought, and
 much more than the absurdity of the Profes-
 sor's opinion merited from me. And although
 I despair of preventing him from returning to
 his former sophistry, yet I am always proud to
 applaud him when I can ; and so do I now
 greet him with my warmest commendations,
 for having the modesty to keep out of com-
 pany his comparative monsters of bats, seals,
 and otters, when we are treating on the de-
 licate

licate parts of woman ; it would have been too affrighting !

The condition the Professor feels himself in ; the bar that he has himself put against his attributing these effects to gonorrhœa, must necessarily press him onward, he must persevere ; men do not always plunge directly deep into error, and it is not till they find how much they are entangled, that they feel the difficulty of extricating themselves. Then why should I take a pleasure by insulting him in his present dilemma ? Wrong impressions on weak and prejudiced minds end in irremediable bigotry.

Page 133. Chap. IV.

“ Of Stricture attended with Spasmodic Affection.”

“ There are few strictures that are not
 “ more or less attended with spasms ; but
 “ some much more than others.” “ In all
 “ cases that I have seen of this kind, when

“ not attended with spasms, the disease is not
 “ formidable; but when the parts are in a
 “ spasmodic state, the symptoms are as vio-
 “ lent as in the simple stricture.”

I confess that I am quite at a loss how to find out the Professor's meaning here. Common sense heretofore would have naturally concluded, that a simple stricture, unattended with spasms or any other symptom, for otherwise it cannot be defined to be a simple stricture, would prove to be the mildest of all strictures; but now we are to understand, for the first time, that when the parts are in a spasmodic state, the symptoms then are but as violent as in the simple stricture.

Has the Professor first of all defined to us what he means by spasms? But in turning to the page that he adorns with this new opinion a little further on, I find there the secret comes out, and there I shall wait upon it. Other men would have set out with the explanation of the word, before they had assigned to it so weighty a cause; but the Professor may well be shy of his opinion; and I promise him it shall not be overlooked. We

all know that a stricture, accompanied with much irritation proves more difficult to cure than that which is milder ; that irritation may be greater at one time than at another, and that the bougie may pass more readily to day than to-morrow.

I have before me many pages so loaded with rubbish, so many useless sets of distinctions, sections so narrative and inapplicable stand in my way, as Hercules himself would turn from, and consider in the comparison his Augéan task, a pleasure. A confused treatment of consequences from obstructions that can only be cured, by removing such obstructions, and that can only be palliated by powers in every body's knowledge, whilst the obstructions remain uncured. It will soon be seen if my remark be just.

Page 135. Chap. V.

*“ Of some Circumstances attending the Use of
Bougies. Their figure and composition.”*

The Professor puts a question, “ Whether
“ it is better to pass the bougie the whole
“ length

“ length of the urethra, so that the end shall
 “ be in the bladder, or only to pass it through
 “ the stricture a little way, so that the end
 “ shall remain in the urethra.”

The Professor has before remarked, that
 sometimes more than one obstruction are
 found in the urethra, and if he means to cure
 one at a time, certainly the mode to which he
 gives the preference should be adopted. Be-
 sides, it would afford much amusement to the
 patient to be constantly pushing the bougie
 forward, and he would, especially if he were
 fond of the use of bougies, amuse himself
 with the comforts from them a longer time.
 No one could doubt but that the Professor
 would give the preference to the passing the
 bougie only a little way beyond the stricture,
 for this obvious reason, because every other
 surgeon practises otherwise. The danger ari-
 sing from the bougie being the cause of gene-
 rating calcalous matter is more in idea, than
 in reality; but if it were, it would carry it
 away with it, when it was drawn out; the
 length of time it remains in, places the idea in
 a frivolous and ridiculous light. From the
 form of the bougie, the further it is passed,

the more the stricture will be dilated ; and from its form, if it be passed but a little way beyond the stricture, it will continually be slipping out.

Now I shall treat my readers with a most curious remark of the Professor, and I would have recorded it as a neat observation, if it had not some relative connection with his new opinions. “ When treating of the stricture, “ I observed that it was often the cause of a “ swelling in one or both testicles ; and further, “ that the passing of the bougie often removed “ that complaint. I may now observe, that “ a very common consequence of the passing “ of a bougie is a swelling of the testicle ; “ this also arises from sympathy, and is the “ common effect of all irritations of the ure- “ thra.” Here stricture produces swelled testicle ; bougie cures it. Bougie produces swelled testicle ; why then stricture out of gratitude should cure it. They arise from sympathy, but are the effects of irritation. “ Which be “ the malefactors ? Marry, that am I and my “ partner *.”

* Much Ado about Nothing.

Page 137.

1. Of a new Passage formed by Bougies.

When this circumstance attending the use of bougies opened to my view, I must confess, what I thought the Professor meant by “ a new passage ” ; that the bougie had made its own way into the bladder. But I now find that this “ new passage ” is where the bougie is forced from the natural passage of the urethra, either on the outside or on the inside only, through the substance of the urethra itself.

The Professor remarks in very few words, all that need be said upon the subject.
 “ When a new passage is made, I know of
 “ no other method of cure but to open the
 “ part externally, and the opening must be
 “ made in that part of the urethra which is
 “ most convenient for coming at the stricture,
 “ regard being had to the other external parts,
 “ such as the scrotum, &c.”

The

The Professor proceeds to instruct us how to perform the operation, and furnishes us with a case of a foldier in the guards, upon whom it was performed with success. As the process was tedious, and the instruments out of sort, and as I find some difficulty in comprehending the long-winded narratives of the Professor, not being used to his manner of telling his own histories, not being as yet perfect master of his idioms, my intemperance not being tame enough to wait upon his prolixity, I shall refer my readers to the whole in a lump as they find it; and I shall congratulate them if they prove more fortunate than I am. Yet I must observe to my readers, that he appears to me to cut those knots he cannot untie; and, in this instance, I follow his example.

But I shall not so readily put up with reflections he has passed on the action of the bougie here, and which I think require a very ample explanation. In his first paragraph he says, “ the worst consequence arising from
 “ the improper use of the bougie, and the
 “ most dangerous is, where it makes a new
 “ passage. I mentioned before that this ge-
 “ nerally

“ nerally arose from the attempt to produce
 “ ulceration by the application of the end of
 “ the bougie to the stricture, in cases where
 “ a bougie could not pass ; for in those cases
 “ where a bougie passes, there can be no dan-
 “ ger of such an effect.”

My readers will first of all be pleased to observe, that, when the bougie, in the phrase of the Professor, does make a new passage, he says, it is owing to the improper use of the bougies. I say that mischief may be committed by the improper use of any, and of every thing. But these are the main questions I want to put to the Professor. How often is the bougie thus improperly used ? How many more cases has he seen besides that of the soldier ? How great was the injury that was committed in that case ?

Now suppose we substitute, for I see his drift, the application of caustick to open such obstructions as we attempt by bougie, and then let us ask such as apply it according to the instructions that the Professor has given, what would be the mischief committed by an improper use of caustick ? If caustick were
 3 generally

generally substituted for bougie, would accidents from improper use of it obtain so rare? What would be the event of it, should it act on either side of the obstruction? What if the piece of caustick be left behind? What if the inflammation should go further than we can controul? Are not these evils more likely to happen from caustick, and are they not such as will overbalance all that can arise from improper use of the bougie? This only required to be fairly stated, in order that the obvious truth resulting from it should have its due weight. And this is an act of justice due to established practice.

Page 143. Chap. VI.

“ Of Diseases in consequence of a Permanent Stricture in the Urethra.”

This chapter gives us a bill of fare, out of which we are furnished with a sixth part of the Professor's work to feast upon, a description of diseases arising out of obstructions, which can only be palliated, without removing

ing the obstructions. What an ambition such an authour must have for a large book ! He ought to pray for length of days, that he may make up his account for the loss of his time, and be again reconciled to himself and his readers.

“ Strictures in the urethra produce almost
 “ constantly diseases in the parts beyond
 “ them ; that is, in the part of the urethra
 “ between the stricture and the bladder. They
 “ bring on in most cases a gleet, and often a
 “ considerable distention of this part of the
 “ canal ; also inflammation and ulceration,
 “ and in consequence of them diseases in the
 “ surrounding parts, as in Cowper’s glands,
 “ the prostate, and the surrounding cellular
 “ membrane, forming abscesses there, and at
 “ last ulceration, for the purpose of making a
 “ new passage for the urine. The bladder is
 “ also often affected, and sometimes the ure-
 “ ters, with the pelvis of the kidneys, and in
 “ some cases the kidneys themselves. All
 “ these are the effects of every permanent ob-
 “ struction to the urine ; some of them are
 “ methods which nature takes to relieve the
 “ parts from the immediate complaints.”

“ Of these complaints I shall take notice in
 “ their order.”

1. “ *Of the Enlargement of the Urethra.*”

“ The urethra beyond the stricture is en-
 “ larged; it is naturally passive while the
 “ bladder is acting, by which means it be-
 “ comes distended in proportion to the force
 “ with which the bladder acts, and the re-
 “ sistance of the stricture. It is also more ir-
 “ ritable, the distention becoming often the
 “ immediate cause of spasms in that part;
 “ and these spasms are most probably excited
 “ with a view to counteract the effort pro-
 “ duced by the action of the bladder.”

The urethra beyond the stricture is here observed to be enlarged, to be passive, to be capable of proportionate distention, to be irritable, and to be spasmodick. The spasms also are said to have a view in their action, a mental, intellectual design.

The

The plain simple fact is, that in the act of urining, the acceleratores urinæ prefs forward the urine faster than it can pass through the obstructed part in the urethra, and that, that part between the obstruction and bladder is dilated in consequence.

Page 144.

2. “ *Of the Formation of a new Passage for the Urine.*”

“ The mode by which nature endeavours
 “ to procure relief (when by reason of the
 “ stricture the water cannot pass) is by ulce-
 “ ration on the inside of that part of the ure-
 “ thra, which is enlarged and within the
 “ stricture.” “ When the internal mem-
 “ brane and substance of the urethra is re-
 “ moved by absorption, the water readily gets
 “ into the loose cellular membrane of the
 “ scrotum and penis, and diffuses itself all
 “ over these parts, from their not having
 “ been previously united by the adhesive in-
 “ flammation : And as the urine has confide-

“ rable irritating powers, when applied to the
 “ common cellular membrane, the parts in-
 “ flame and swell considerably. The pre-
 “ sence of urine prevents the adhesive inflam-
 “ mation from taking place ; it becomes the
 “ cause of suppuration wherever it is diffused,
 “ and the irritation (“ not sympathy”) is of-
 “ so great, more especially in cases where the
 “ urine has been allowed to become very
 “ stale, that it produces mortification in all
 “ the cellular membrane, and then in several
 “ places of the skin ; all of which, if the pa-
 “ tient lives, will slough away, making a
 “ free communication between the urethra
 “ and external surface, producing fistulæ in
 “ perinæo.”

I consider the subject immediately before us
 as an exception to the frivolous observations
 of other sections ; it is worthy of our most se-
 rious concern, but not so much for the relief
 that can be given when the symptom of dif-
 fusion of urine becomes general, as to excite
 the patient to have done, and his surgeon to
 do all in their power timely to remove the
 obstruction. For I fear that, when the case
 be arrived at such a state, as that the urine is
 suffused

suffused through all the surrounding parts, when the penis assumes the appearance of a most enormous and shapeless tumour, when every contiguous part is loaded with the urine, and when the violence of pain is equal to any that can be endured, the destruction of the whole of these parts must be the consequence, and the only release is death. Such a case I once saw in the last scene of it.

The Professor has here furnished us with a case which succeeded in his hands. But it should be remembered that he attended the patient before these symptoms came on, that he saw the growing evil before it had arrived to any great height. For he says, that when the patient had just recovered, the penis in erection was bent to the side that had suffered, but in time the parts recovered gradually their natural form. So that it must be clear that the mischief could not have been great, or is it to be expected that it ought, for as the Professor actually attended the patient before these symptoms had come on, it would, where a patient was favoured with so much skill to guard against the coming evil, as well as to cure it when it was come, have been unfor-

tunate

tunate indeed; and it would have proved more, that when these symptoms do ever even threaten, the art of the profession must yield to their havock.

But although it is very true that, when the obstruction hinders the urine from passing, it forces its way *qua data porta*, and is sometimes, instead of being evacuated by a fistulous opening externally, lodged in the surrounding substances of the urethra; yet I shall not, after acquiescing with the Professor upon this fact, which I readily do, because it is a serious one, drop the long quotation that I have last copied here. There is still some difference as to new opinions which must be cleared up, and they shall as often meet with my resistance as I find them in my way; and this was another reason for giving so full a quotation.

“ When the internal membrane and substance of the urethra is removed by absorption”—We already have had a smack of this jargon in page 123, which I then treated as it deserved; what he means is, that the urine, not being able to pass in the natural way, on account of an obstruction that hinders

ders it, makes its way by constant action or attrition, through the substance of the urethra behind the obstruction.

“ The presence of urine prevents the adhesive inflammation from taking place.” This new opinion of “ adhesive inflammation ” first made its appearance in page 49, in treating on the chordé. I then paid my respects to the stranger, and this visit shall be also returned. If this adhesive inflammation takes place in a chordé, how happens it, that the chordé goes away with one gonorrhœa, and returns with another? If this be true of chordé, it can only be temporary adhesive inflammation; again it is adhesive, again, and a little while, and it is not adhesive. Now in the case before us, the presence of urine prevents this adhesive inflammation; but if the adhesive inflammation, and that is very unlucky, had been so good as to have lent its assistance beforehand, it would have prevented suppuration from the urine having been diffused. I had almost forgotten to explain to my readers my only idea that I can form of what he means by adhesive inflammation. When parts inflame that do not suppurate, such adhere together

together after inflammation is subsided. In other words, adhesion of parts after inflammation.

But how came sympathy to be left out, in all these violent effects from irritation, in this general muster of new opinions, by the Professor? We see nothing of her. We see the cuckoo, but where is the little bird which should attend it? Sympathy is a capricious goddess; hath the Professor affronted her, because he hath abused her? Is she no longer one of his *lares*? Why was she absent upon this occasion? As she is a votarist to pleasure as well as to pain, was she present at a courtship, a wedding, or a christening? Where was she? We should all be sorry to part with sympathy! But as for the other new opinions; if we could regulate the laws surgical, by an imitation of those military, they ought to, and would be drumm'd out of the company.

Page 151.

3. "*Of Inflammation in the Parts surrounding the Urethra.*"

The Professor very properly remarks, that
 " the inflammation affects the prostate gland,
 " the membranous part of the urethra, the
 " bulb, and probably Cowper's glands, with
 " other parts of the urethra between the bulb
 " and the glans." And that " when suppura-
 " tion takes place, the matter points exter-
 " nally according to the parts that suppurate,
 " and the situation of the stricture." " But
 " inflammation in the surrounding parts of
 " the urethra is not always a consequence of
 " distension or stricture; it arises from other
 " irritations in this canal, such as violent go-
 " norrhœas, and very irritating injections."

Now I take all this kind in the Professor,
 and if this paragraph did not militate against
 the whole of his former language, I should
 almost persuade myself to become his panegy-
 rist. But these lucid instances crowd in upon

us, for here I find another as candid as the former, and which in a great measure does away all the quaint notions that we remarked upon in the former section. “ These ulcerations
 “ and suppurations are to be considered as effects of nature ; or to speak more physiologically, as a natural consequence arising
 “ from such irritation, by which as the urine
 “ cannot pass by the old passage, a new one is
 “ made to prevent further mischief.” This last, however it be set down with some confusion, candour must allow, comes near to the point.

Page 153.

4. “ *Of the Treatment of the Inflammation in the surrounding Parts.*”

The first line of the Professor runs thus :
 “ The inflammation of these parts is to be
 “ treated like other inflammations.”

Page 155.

5. *“ Of the Effects of Inflammation in the surrounding Parts upon the Constitution.”*

The Professor here, notwithstanding the successful case he gave us, where there was a diffusion of urine, is obliged to confess my predictions to be true ; for he says, that if the patient “ be not soon relieved, he sinks, and “ mortification comes on ; that we should not “ wait (he believes) for the sloughing of the “ parts, but make an opening early.” All this is surely very true, and he who flatters himself that success will attend a spontaneous slough of the mortified parts, resigns his judgment with the life of the patient to the ravages of the disease. The Professor further says, “ that there accompany these symptoms “ a smart sympathetic fever ; that the bark “ gives strength, and lessens irritability * ; “ and that opium will add to its effects.”

* The reader will remark how he places sympathy and irritability here.

Page 156.

6. “ *Of Fistulæ in Perinæo.*”

The Professor says, that “ I apprehend that
 “ the bougie may bring on an inflammation
 “ on the urethra at the obstructed part, and
 “ produce adhesions there.” Here adhesions
 from inflammation on the urethra comes con-
 fessedly forward, and the new opinion sinks,
 and is lost in the general acceptation.

Page 157.

7. “ *Of the Operation of Fistulæ in Perinæo.*”

What does the Professor mean by an operation for fistulæ in perinæo? Does he mean one operation for all; and that one instruction will suffice for every fistula in every situation? If he wrote till doomsday, description would not avail; the various adoptions must depend upon the situation of the fistula, and ingenuity of the surgeon.

Whoever enjoys the independent power of reflection, must grievously feel the insult offered to his understanding by the tedious drawlings of the Professor. And I am extremely sorry, that by the desertion of his friends, or the want of firmness in them, this unwarrantable prolixity hath not been prevented. It is not only the subject before us, but the whole of the work that drags so heavily. Did he search for no precedent, as a novice in literature? Was he ignorant of the character of his countrymen? Did he not know that they would accept of no other information but that which would bear reflection from reason? Is not conciseness the nerve of sense? And had he not for his example, from among many others, the works of Sharp, Cheselden, and Pott, who in particular have improved the system of surgery, and exalted it above dependance on foreign superiority? But what is strange to tell, they have written very full on almost every subject*, yet the whole of their works put together do not much exceed in quantity this book of the Professor!

* I am warranted in saying that there is not a subject but what they have written upon.

But I will be the last to yawn ; I am become his voluntary watchman ; what faculty I have shall be bestowed as his commentator ; and where professional truth calls for it, I will yet encircle his system, let his lines of amplification be ever so extensive.

The whole of the tedious case he gave us in a former section, and all the sections of many pages passed, and many to come, might have been compressed, and included in this before us. The Professor confesses that the mode of opening, and the circumstances attending the operation, will vary according to the situation of the fistulæ.

He says, “ that as little of the sound
 “ parts as possible of the inner surface of the
 “ urethra may be opened, and that the dis-
 “ eased be fully exposed, it is necessary to be
 “ directed to the inner orifice. To effect this,
 “ a staff, or what else can be passed, must be
 “ introduced into the urethra to be pressed
 “ forward to the stricture, and a probe intro-
 “ duced into the fistulous opening to meet it
 “ as near as it can. The whole of the fistu-
 “ lous canal must be laid open, and the cal-
 “ lous

“ lous parts fully dissected.” “ It will,” he says, “ most probably always be proper ;” and I say, in order to render the operation effectual, that it must always be necessary, “ to
 “ introduce an instrument into the bladder,
 “ and keep it there almost constantly, so as
 “ to preserve the passage in the urethra in a
 “ regular form, while the openings made are
 “ healing*.”

There has been already so much said upon this operation, and the system for performing it is yet so depending upon the stricture of the fistulæ, that one may almost as well judge of a beautiful woman, by a lock of her hair, as to form in the mind what is fitting to be done by the description of one situation for all. Much must depend upon the firmness and ingenuity of the surgeon, and many of the resolutions must wait upon the condition of his patients. Such as are far advanced in life, whose feelings can be made comfortable by palliative means, and whose systematick adop-

* My readers will find this operation already in practice in Wiseman, Dionis, Palfin, LeDran, Astruc, and Sharp.

tions secure them from rapid changes, and imminent predicaments, were better to be content under the evils that they suffer, than fly to others that they know not of.

“ When the urethra has suffered so much
 “ that abscesses have been formed beyond the
 “ scrotum, the patient should ever take great
 “ care to avoid a fresh gonorrhœa, for he seldom
 “ in that case escapes a return of the
 “ same complaints.”

Without the least distortion of truth, without this warning to the patient being capable of bearing any other construction, than an implicit confidence that these mischiefs had arisen, as I predicted, from former gonorrhœa, what can the Professor otherwise attribute them to? What other cause prevails to bring down upon these parts such constant effects? Effects corresponding not only to the history of gonorrhœa, but that cannot be given to any other general cause whatever.

Surely some relenting friend of the Professor, wounded like me at the waste that had been committed upon time and truth, slipped
 in

in this last confession into the press unknown to him.

Page 162. Chap. VII.

“ Of some other Affections of the Urethra.”

The Professor sets off with asserting, that
 “ the only proof we have of the urethra be-
 “ ing muscle, is, that it is subject to diseases
 “ peculiar to muscle.” That is to say, in
 order that my theory might be my own, in-
 dependent of any possible rival, I shall begin
 with an opinion that no one else ever dreamt
 of, that muscular action and muscular disease
 can exist where no muscle can be found ; and
 that I will have it to be so, and will draw all
 my conclusions from such premises, in spite
 of the impossibility.

“ In a sound state of parts these muscles are
 “ never excited to violent actions, acting sim-
 “ ply as sphincter muscles ; but when irri-
 “ tated, they are capable of acting violently,
 “ as is best seen in some cases upon the first

Q

“ use

“ use of injections, the urethra often refusing
 “ them entirely.” Nothing can be, and
 there never was a more fallacious argument
 than this to prove the action of muscle in the
 urethra. The first injections are generally
 thrown up when the urethra is inflamed, (and
 sometimes injections are repelled because the
 syringe is not held in a direct line with the
 urethra) the pain from inflammation being ex-
 quisite, the muscles which act in the expulsion
 of the contents of the bladder and seminal
 vessels become so irritated from pain and in-
 flammation in the urethra, as to be engaged
 beyond their natural action, and so much do
 these muscles by their powerful constriction,
 when thus irritated, compress the urethra, as
 to prevent partially or wholly any fluid either
 from passing in or out of the urethra. And
 this accounts for plainly, why a bougie or a
 catheter may or may not pass into the bladder,
 when urine cannot be evacuated in such a state
 of irritation, because their pressure against
 these muscles may or may not be more than
 the counter resistance by the muscles. I beg
 my readers would particularly consider of the
 above, for he is here fighting for a new opi-
 nion.

“ But

“ But there are often spasmodic contrac-
 “ tions of these muscular fibres in different
 “ parts of the canal, shutting up the passage,
 “ and obstructing the course of urine, often
 “ not allowing a drop to pass ; that this is al-
 “ so owing to spasms upon the muscular fi-
 “ bres is evident, because a large bougie will
 “ sometimes pass when it was at the worst.”

This I have already accounted for above, and
 I hope satisfactorily. But I will here make
 one more remark. If these sphincter muscles
 obtain in the urethra, why, when the ure-
 thra is irritated from another cause than a per-
 manent obstruction, is not the bougie or ca-
 theter as well stopped from passing in one part
 of the urethra as in another ? Because the real
 hindrance to the passage is only where the
 acting muscles upon the urethra compress it ;
 and their action is confined to that part of the
 urethra nearest to the bladder.

“ Spasmodic strictures often bear so strong
 “ a resemblance to the cramp, that one would
 “ be apt to attribute them to the same cause
 “ as that which produces cramp. In such
 “ cases, the spasm also goes off by tickling

“ the part, similar to the removal of the
“ cramp.”

This quotation I have purposely given, in order to ascertain to my readers the importance of the subject the Professor means to illustrate in this chapter, and what, he wishes to insinuate, should be considered as spasmodic affections. If the Professor had asserted that the urethra, without any previous cause, was sometimes attacked with a spasm similar to the cramp in other parts, which produced a temporary obstruction of the urine, and which, as he asserts, would go off by tickling, I would be the last to deny him the privilege of this innocent gratification, or would I deny that the urethra was not liable to it ; it would be a common harmless attack on the urethra in common with other parts of the constitution, and it would be as harmless in the Professor to treat upon it, and an amusement he ought to be, and should be indulged in. But when he sat out with assigning fresh causes for obstructions, with turning away some that had been established by former writers, when he undertook to tell us what was not one cause ;

cause; it surely ought, and it would have been becoming in him to have told us what was another true and serious cause. And this another true and serious cause turns out to be a spasm similar to cramp in other parts, that is to be cured by tickling! Why then I will leave the Professor to amuse himself with the remedy, and pursue the analogy of spasm or cramp with a serious affection in the urethra.

When we meet in our practice, an irritation in the urethra that denies a passage for the urine, and upon introducing a bougie of any size, we find at some times that it passes very readily, and at other times that it will not pass, it naturally alarms us for the condition of the patient, because we know that there is a serious latent cause, which we are at a loss to account for; the only part of the case that we know is, that there is irritation, and that, that irritation is at times more or less violent. It often happens also, that, after going into the strictest examination of the condition of the contiguous parts, we cannot fix the cause of this irritation upon any of them, yet notwithstanding we find such affections continue for years on the patient, and we find that

that, almost every day, and even at every time he solicits to discharge his urine, he must first pass a bougie, before he can affect it. This is the serious condition that is worthy our investigation, and this is a case not to be cured by tickling, that cannot be called a cramp, and which is something more than a spasm, or there is no truth in the definition even of the Professor; for the word is not fitting to the disease, nor the disease to the word. Sometimes the bougie will not answer the end of exciting the patient to urine, and then the case requires the same attention as a dysury from another cause. Such a complaint often continues years, commonly to the close of life. Spasm is an instantaneous affection of parts, without any first known cause, and never continues for any length of time.

Page 163.

2. “ *Of the Cure of the Spasmodic Affection of the Urethra.*”

He recommends a blister to the perinæum, and gives this curious reason for it; “ that
“ the

“ the parts concerned in the expulsion of the
 “ urine sympathise strongly with the skin of
 “ the perinæum.” Blisters on these parts
 can only do good where the case is acute, but
 in a chronic one their power must always
 prove inefficacious. Applying them so near to
 parts affected is consonant to the general pur-
 pose of blisters ; yet the effect of strangury
 produced by blisters should not be forgotten,
 nor has the Professor forgotten it, no more
 than he had the bad effects from caustick ;
 truth and the whole truth are very distinct.
 His other remedies are dipping the penis in
 cold water, opiates and turpentine.

Page 166.

3. “ *Of the Paralysis of the Urethra.*”

This he says is an opposite condition to the
 foregoing, but I believe that no such disease
 ever existed, and that it is the chimerical re-
 sult of his own prolifick imagination. De-
 bility of the bladder and muscles that assist to
 expel the urine, is the paralysis here meant.

4. “ *Cure*

4. “ *Cure of a Paralysis of the Urethra.*”

The cure would be the same for debility of the bladder and muscles. His remedies are blisters to the loin and perinæum, tincture of cantharides, spices and steel, bark, cold water, and one of his “ perhapses.” If electricity should fail, I will furnish him with another “ perhaps ” ; he is fond of comparative science. What says he to the idea of retaining the ablest whistler to horses from Newmarket ? Horses hard ridden have been thus relieved. I wish the Professor seriously to weigh the importance of this hint.

Page 167.

5. “ *Of Caruncles, or Excrescences in the Urethra.*”

“ From the familiarity with which authors
 “ talk of them, and the few instances in
 “ which they really occur, one would be apt
 “ to suspect that this cause was originally
 “ founded

“ founded in opinion, and not observation,
 “ and afterwards handed down as matter of
 “ fact.” I did not expect to find so unfair an
 insinuation from the Professor. I do not think
 it altogether so decent from a man who all
 along asserts what he cannot prove, that he
 should attempt to deny what has been proved.
 Such authours have asserted that caruncles pre-
 vail; they have also asserted finding them in
 the urethra by examination; their existence
 have been proved by the eyes, and not sur-
 mised. The Professor himself confesses after-
 wards to have seen two instances of caruncles.
 But what adds to the aggravation of this in-
 delicate skepticism in the Professor is a confes-
 sion of his own, “ that most probably it will
 “ not be possible in the living body to distin-
 “ guish caruncles or risings in the urethra,
 “ from a stricture.” Admitting this to be a
 fact, I may then draw this inference: that
 others, to know that caruncles actually existed,
 must have seen them after death; that the
 Professor has also seen them, but that he has
 insinuated that others had only guessed at
 them; and that we do not know how of-
 ten they prevail, because we cannot without
 examination after death distinguish between

the cause of obstruction produced by caruncles, and other causes; we know only that they do prevail.

6. “ *Of the Cure of the Excrescence, or Caruncle.*”

This cure is either by the pressure of a large bougie, so as to ulcerate the carnosity, or by the caustick; and from the practice of the caustick he does not doubt of a cure. “ But,” says he, “ the difficulty lies in distinguishing “ the disease from a true stricture; for al- “ though authors talk of caruncles as com- “ mon, and give us the method of treat- “ ment, yet they have not told us how we “ are to distinguish them from strictures.” At any rate the Professor mixes with the herd in this state of ignorance. He has searched into authours in vain to find a distinction, but which, if he had found, would have been probably transplanted, as was the case of the caustick, into this work, and came forth from his hands glittering with all the polish of another new opinion! But why does the Professor seem so out of humour at not being able to find

find a distinction in other authours ; it is only unfortunate ! But it would have been yet more so, if that cunning and rapacious animal, the fox, had not found a credulous crane when he sought for one to pluck out the bone which he pretended to be stuck fast in his throat, we then should have been deprived of the most beautiful fable in all Æsop !

Page 169. Chap. VIII.

“ Of the swelled Prostrate Gland.”

The Professor says with truth “ that this
 “ disease is often very formidable, of more
 “ serious consequence than any of the for-
 “ mer causes of obstruction, and that we
 “ have fewer methods of cure, for we can-
 “ not destroy it as we can the stricture, nor
 “ can nature relieve herself by forming new
 “ passages.” What nature can or cannot do,
 it is beyond my presumption. But so far I
 will grant, and cheerfully too, what the Pro-
 fessor has said. When he comes to his con-
 jectural causes for the action of the diseased

prostrate in producing the obstruction to the urine, there I fear we shall find him in his usual mood, beginning with inexplicable hypothesis, and ending with contradictory assertion. “ When the prostrate gland swells, it
 “ does not lessen the surface of the urethra at
 “ the part similar to a stricture ; on the contrary, it rather increases it.” “ From the
 “ situation of the gland which is principally
 “ on the two sides of the canal, and but little if at all on the fore part, as also very
 “ little on the posterior side ; when it swells,
 “ it can only be laterally, whereby it presses
 “ the two sides of the canal together, and at
 “ the same time stretches it from the anterior
 “ edge or side to the posterior, so that the
 “ canal, instead of being round, is flattened
 “ into a narrow groove.” “ Besides, the effect
 “ of the lateral swellings, a small portion of
 “ it which lies behind the very beginning of
 “ the urethra swells forward. It sometimes
 “ encreases so much as to form a tumour,
 “ projecting into the bladder some inches.”

“ The effects of these swellings are very
 “ considerable, for they squeeze the sides of
 “ the urethra close together, and the project-
 “ ing

“ ing point hinders in some degree the urine
 “ from entering the passage, and in many cases
 “ stops it entirely.”

These are a few of the proofs which the Professor brings to make good his hypothesis, that the surface of the urethra is encreased. The rounded surface of the urethra being flattened into a narrow groove, the projection of the process of the prostate gland, and the effects of the lateral swellings of the canal, we all know differ from the obstructions that we have hitherto described; but I have yet something to learn before I am convinced that one and all of these causes do not tend to narrow the surface of the urethra, and by that means only to obstruct the urine. Is the surface of the urethra widened when it is flattened into a narrow groove? Is it so, when it is blocked up by the process of the prostate gland? Or is it so, when it is almost closed up by the lateral swellings in it? Let the glands of the throat be our comparison, and then let it be asked, whether, when they are swelled, the act of swallowing is not impeded, or obstructed.

This

This chapter has produced two cases and two plates, embellishing ornaments that I would no more touch, than a modern artist should a painting by Raphael. Cases I look upon to be sacred truths that help our reason. By them the medical world have obtained more real knowledge than all the finest imaginations of every age, if their applications had been concentrated to this one point, could have produced without them. But it behoves us to see that our reason be not abused, and that the advantageous current which flows from cases be not turned awry; and it is with this regard, that I wish my readers to reflect that two cases cannot establish what general diseases the prostate is liable to, no more than they can the general action of that gland when diseased, by which it obstructs the passage of urine. In this respect it will be found that the Professor did not escape me in my first publication.

Page 174.

I. " *Of the Treatment of the swelled Prostrate Gland.*"

" A certain cure, I am afraid, is not yet
 " found." " Opiate clysters, sea bathing,
 " hemlock, mezereon root." A seton in the
 direction of the perinæum had been passed by
 Mr. Earle, which took off the irritability, and
 reduced the size of the prostrate to its natural
 state, but the complaint returned again, and
 the same resources were not so effectual on the
 second attempt. This is the substance of the
 Professor's remedies.

Page 176. Chap. IX.

" *Of the Diseases of the Bladder, particularly
 from the before-mentioned Obstructions to the
 Urine.*"

I shall not spin out my observations by go-
 ing further into the fantastick theory of the
 Professor on the sphincter muscles in the ure-
 thra, and of the acquisition of " encreased
 " strength

“ strength of the bladder, when it becomes diseased.” Such wild romances cannot exist an hour, will never be found but in the book before us, and at any rate they are too glaringly ridiculous to do harm. It is sufficient for me to have given in my protest against these new opinions, that these new diseases exist not in the urethra, but in the bladder and the contiguous muscles. His remedies are appropriated for either condition, so that all the mistake lies in the theory, and so far it is fortunate. I should have heartily rejoiced, if that, when these half perfected dreams came over the Professor in an exacerbated degree, he had got rid of them after the same manner that Gil Blas did. His reveries only roused a stranger in the next chamber from a sound sleep ; that was the troublesome extent of them !

Page 184. Chap. X.

“ *Of a Suppression of Urine, and Operations for the Cure of it.*”

This is a subject that requires our most serious attention, both with regard to the time
that

that it is fitting to perform the operation, and the fittest manner of performing it.

“ In cafes of total fuppreffion of urine,
 “ arifing from ftricture, or any other caufe
 “ where a catheter cannot be paffed, and
 “ where every other method recommended is
 “ impracticable, an artificial opening muft be
 “ made into the bladder for the evacuation of
 “ water. There are three places where an
 “ opening may be made, and each has had its
 “ advocates.”

“ The opening may be firft made in the pe-
 “ rinæum, where we now cut for the ftone;
 “ fecondly, above the pubis, where cutting
 “ for the ftone was formerly practifed; and
 “ thirdly, from within the rectum, where
 “ the bladder lies in contact with the gut.”

I fhall juft remark, that although we underftand what was meant in the laft quotation, yet whoever refted on the construction of his words only, would prefume that the fame fubject was to undergo all three operations. But all this is venial, the error cannot be mended. Moft of the chapter before me is

made up of the various modes of treatment from other authours, and the little from the Professor does not alter their opinions, or improve our understanding. As all the essential points are borrowed, and some of them not acknowledged, it behoves me to refer my readers to the very pamphlet from which the Professor drew all his information upon this subject, from which even the Professor, after all the elaborate torture of the imagination that he has exerted in his theory, as if something big with welfare for mankind were to have sprung from it, has condescended to raise recruits for the present service, but has not wilfully mentioned names by halves with the whole of the theory, when that very pamphlet has mentioned all the names with the whole of the theory*. The candour of the authour, and the general utility of this pamphlet, deserve commendation.

The opening above the pubis, and a consideration on the opening in perinæo, have been accurately and minutely treated on by Mr.

* The pamphlet I allude to was written by Mr. Reid, surgeon at Chelsea, and published in the year 1778.

Sharp, in his Critical Enquiry, and the preference given to the former.

“ This last way of drawing off the urine
 “ is by a puncture above the os pubis, in that
 “ part of the bladder where the high operation
 “ is performed. This method has been
 “ occasionally followed by some eminent surgeons
 “ for many years, and is still approved
 “ of; but it is not recommended, as having
 “ those superior advantages which in my opinion
 “ belong to it. It is an operation of no
 “ difficulty to the surgeon, and of little pain
 “ to the patient, the violence done to the
 “ bladder being at a distance from the parts
 “ affected, &c. &c.*”

Just about the time that Mr. Sharp's book went to press, I believe in the very year, Monsieur Flurant, of the Hôpital de la Charité at Lyons, practised first the opening into the bladder by making a puncture through the rectum. An account of this case was given to the publick, together with another case in the

* Critical Enquiry, pages 122, 123.

year 1752, and another in the year 1757, in a memoir by Monsieur Pouteau, in the year 1760, in his *Melanges de Chirurgie*. The first was successful, the second failed, and the third was successful. In the year 1774, Dr. Hamilton, of Lynn Regis, sent a Narrative to the Royal Society of a successful case, performed by him, by a puncture into the bladder through the rectum.

Having just given this brief state of facts to my readers, and pointed out to them where they may refer for a more general information, it remains with me now only to consider two things. The first is, why did not the Professor do the same act of justice to Mr. Sharp, that was done by Mr. Reid, and that is now done by me? Why did he not name Mr. Sharp as the authour who favoured the operation above the pubis, as well as he has named Messieurs Flurant and Pouteau, and Dr. Hamilton, who reported favourably of the operation through the rectum? If he means to excuse himself by saying that he knew not of Mr. Sharp's favourable report, or that surgeons knew of this opening into the bladder before it was described by Mr. Sharp, neither
can

can avail him as an excuse, because his name was mentioned with the other three by Mr. Reid, and the Professor confesses to have borrowed from his pamphlet. The strong manner in which Mr. Sharp recommends this operation above the pubis, and his forcible arguments to convince our reason, as that they cannot ever fail of having their due weight in surgery. But hitherto we have not been able to assign a reason why the Professor was silent upon the name of Sharp. Dr. Hamilton was not the first who broached the idea of a puncture through the rectum, the world had been in possession of it fourteen years ; yet the Professor did not forget the name of Hamilton, but omitted the name of Sharp ! The truth is, he had not forgiven the subject alluded to in my first publication, thereby hangs the tale. “ I am afraid of gun-powder Percy, tho’ he
“ be dead.”

The second is an observation which I offer to my readers with great diffidence. Supposing that we were reduced to the necessity of making an opening into the bladder from the case of the urine being diffused, or beginning to diffuse, into the corpora cavernosa penis and
the

the cellular contiguous parts, and knowing the threatening fatality of such symptoms, would it not be preferable in that case to attempt an opening into the bladder through the perinæum? If the opening was made above the pubis, or through the rectum, into the bladder, neither way would prevent some of the urine continuing the diffusion when once it had found that way into the cellular contiguous parts. Whilst the life of the patient was attempted to be saved at one point, it would be lost at the other. But it appears to me that an opening into the bladder in this instance through the perinæum, would put an end to the diffusion at the same time that we drew off the water.

Page 192.

2. “ *Of the encreased Strength of the Bladder.*”

This is the first time that we were ever told that disease produced additional strength on any particular part. This is indeed a new opinion! Such singular assertions make our observations upon them almost inevitable, although

though we know that it would be more discreet to pass them over in contemptuous silence, yet it is found difficult to adopt that plan.

Frequent irritations thicken the whole substance of the bladder, and lessen its capacity. The internal surface of it becomes rigid and fasciculated. This is known to be produced from long pain endured from a stone in the bladder. About ten years ago I extracted a large stone from the bladder, after death, of a very old gentleman, a famous mathematician, who had been a sufferer for many years. The cavity of the bladder was so diminished by the thickening of its coat, as to embrace the stone closely on every side. Will there be another man found, besides the Professor, to declare in such a state that the strength of the bladder is encreased? or are the limbs of men who bear about the elephantiasis stronger than those without that disease? If they be, why then in the beginning of another war, let us recruit our army from out of the hospitals!

Page 197. Chap. XI.

“ *Of the Discharge of the natural Mucus of the Glands of the Urethra.*”

“ It would seem that the contest between
 “ the mind and the body increases this secre-
 “ tion, for it cannot be considered as a dis-
 “ ease of the parts.” What a pity it is that
 they cannot agree, especially as they are
 obliged to live together ! for if a divorce should
 take place, then probably both may become
 nothing !

1. “ *Of the Discharge of the Secretions of the Prostrate Glands and Vesiculæ Seminales.*”

The Professor has described this disease with becoming accuracy. I have seen many instances of it. The discharge when I have seen it always accompanied a disease of the prostate. It comes on at the latter time of life, it exhausts the strength of the patient, he generally

nerally appears tabid, and wasting away, his eyes are weak and dim. It is increased by free living and a costive habit, it follows the urine. The Professor says, " it has hardly " any smell." I have found it to smell, in-
 somuch that the patient has been obliged to keep out of company. It appears to me to be a glandular discharge somewhat similar to that which old men discharge by the mouth from habitual defluxions. It may be palliated by temperate living, moderate exercise, and keeping the body open. The Professor advises a soothing plan, to prevent all violent actions, and to keep the body open.

The reader perhaps will not a little wonder that we see no longer any thing of pus for mucus, either here or in the former section. Pus seems now to be *hors de combat* ! Nothing goes down but mucus, mucus ! The fact is, that in the description of this disease, there was no competitor, and then the Professor hunts after no new opinion !

Page 200. Chap. XII.

“ Of Impotence.”

“ This complaint is by many laid to the
 “ charge of onanism at an early age. I think
 “ I may affirm that this act in itself does less
 “ harm to the constitution in general than the
 “ natural ; that the natural with common
 “ women, or such as we are indifferent about,
 “ does less harm to the constitution than
 “ where it is not so selfish, and where the af-
 “ fections for the woman are also encreased.
 “ Where it is only a constitutional act, it is
 “ simple, and only one action takes place ;
 “ but when the mind becomes interested, it
 “ is worked up to a degree of enthusiasm,
 “ encreasing the sensibility of the body and
 “ disposition for action ; and when the com-
 “ plete action takes place, it is with propor-
 “ tional violence ; and in proportion to the
 “ violence is the degree of debility produced,
 “ or injury done to the constitution.”

Here we find three degrees of debility, or injury, done to the constitution from an exhaust of feminal discharge. Great in the unnatural act, greater in the natural act with a common woman, but greatest of all with those we most love ! If I thought that this was the fact, I must confess that I would not be so squeamish as to withhold my assent to this doctrine of the Professor. In philosophick, or physiologick speculations, the truth of things is the object we all look after, and it behoves us on every subject, when the mind discovers a disposition to search for it, not to check, from a false delicacy, such a pursuit. And I hold this observation to be, on all occasions just, that immorality can no more be promoted from a minute investigation of the true knowledge of things, than morality can be supported by a purposed forbiddance of enquiry into a falshood, which is imposed upon our senses for a truth. The danger to a traveller is always greatest on a road that he is ignorant of. An enlightened understanding gives a firmness to our actions, and tends to secure the welfare and happiness of society. The mischief of the theory of the Professor, I am confident arises out of the false princi-

ples of it. We have all along seen erroneous new opinions advancing in their tendency to do harm, and he seems to have wantonly urged them on where the necessity of his argument he had to prove did not exact them. Where he allows a man who has a gonorrhœa to have connection with a sound woman, and bears her harmless, there he was contented with only injuring that sex ; but here he abandons the females, prefers the unnatural act as the least injurious, and they have nothing left for them but to go to a nunnery !

Perhaps the Professor, amongst the rest of his new opinions, may think that it is high time that the human race should be annihilated ; and let us suppose, for one instance, that it were, would another order of superior beings planted upon this very spot of the earth in its stead, know what the venereal disease was, from the book that the Professor had written ? I mean provided there was no other written trace of the disease remaining, but in the great work before me, and that this other order of beings be 'alike infected as we were !

I will

I will not treat this subject as if I had any serious dread that this new opinion of the Professor would be any tendency to evil. It is a subject which more than medical men can judge upon; no false credit for professional reputation can screen the Professor here; besides what has been said against the vice of onanism, by medical authours, from the days of Galen down to the moment that I am indicting the Professor, besides what every poet and every historian have said in execration of it, every living man and boy, and all nature cry aloud that the Professor has adopted a singular, a false, and an ignorant theory. His attempt upon this question was both vain and criminal here, for impotence has no more to do with the venereal disease, than that it produces debility in common as other diseases; it has no more to do with it, than it has in a nunnery!

I can easily figure in my mind the Professor enthroned in his elbow chair at his lecture room, surrounded by his pupils, not exactly such a group as Apollo in company with his nine muses. I can figure the Professor not having exactly the front of Jove, nor the eye
of

of Mars, but with a contour of his own, about to expound a theory of his own, and in a language of his own. I seem to see the attentive pupils with their greedy ears devouring up his discourse, catching the poisonous sweets as they are distilling from his lips, their faculties entranced by the power of his oratory, and so transfixed and lost to all external alarms, that if Heaven's own thunder rattled in loudest peals over their heads, the sound would pass away as unaffecting to their senses, as the buzzing of a fly. I seem to see the conscious glow of exultation mantle in their cheeks, flattered as they must be, that, that which they had through shame shunned or concealed as a noisome vice, was now transformed into the fairest virtue. I seem to see how impatient they waited for the nod of dismissal to hasten home to their solitary chambers !

But now it is high time for me to mark the dreadful consequences that follow an excess of this "wasting of manly marrow," and here follow most of the immediate symptoms. Privation of desire for the enjoyment of woman, a constant propensity to the same unnatural

tural habit, pallid complexion, flabby muscles, lassitude, weakness, gleet, pain in the loins, contortions of the testes, trembling hands, dim eyes, head ach, drowfiness, startings in the sleep, numbedness of the limbs, shy in company, timidity, inactivity, mind inert, no desire for manly diversions, preferring solitude, chillness, epilepsy, madness, “ *terribiles visu formæ.*”

Let it not be said that I have aggravated the list of evils, for were I to sit down to add from authours to these I have already recollected from my own knowledge, I should but prove my oversight. I shall close this hateful subject with what I can recollect from a poem of our immortal Shakespeare.

Th' expence of spirit in a waste of shame
Is lust in action.——

Enjoy'd no sooner but despised strait,
Past reason hunted, but no sooner had
Past reason hated, as a swallowed bait,

On purpose laid to make the taker mad!

All this the world well knows, yet none knows well,
To shun that heaven, which leads men to that hell!

Page 209.

“ Of the Decay of the Testicle.”

“ It would appear from some circum-
 “ stances, that the parts of generation are
 “ not to be considered as necessary parts of
 “ the animal machine, but only as parts su-
 “ peradded for particular purposes ; and there-
 “ fore only necessary when these particular
 “ purposes are to be answered ; for we ob-
 “ serve that they are later of coming to ma-
 “ turity than any other parts, and are more
 “ liable to decay.”

They are so far necessary to the animal machine, that a man without them is a monster ; and relatively so necessary as life itself, as the means without which the human race cannot be propagated. Their attributes come forward at so early a period as any other corporal perfection of man, and continue so long perfect, as the rest of the corporal powers do ;

do; indeed they exhibit tokens of maturity in a subject long before in other respects, one would be capable of writing him “ a man.”

So unfortunate is the Professor in all his assertions. He says that testicle is more than other parts liable to decay, and now he is going to give us the rarest of all instances! This is the natural genius of man!

“ Testicles have been known to waste in
“ cases of rupture, probably from the con-
“ stant pressure of the intestine. Mr. Pott
“ has given us cases of this kind.”

I just remarked, that the Professor is singularly unfortunate in all his assertions; it gives me great pain to be constantly contradicting him, but I do here flatly; because it must place the one or the other of us in a mean and most disgraceful situation.

Mr. Pott has given us three cases of the wasting of the testicles, and not one of them

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was

was in consequence of rupture, nor had either of the patients ever a rupture*.

But the Professor, not content to find out in the name of Mr. Pott, that the cases he had given us of the decay of the testicle were owing to rupture, when Mr. Pott had never made any such assertion, has also helped him out in accounting for the rupture producing the decay of the testicle. To illustrate this singular vein of ingenuity in the Professor, I shall refer my readers to a scene in the School for Scandal.

Curiosity will ask, how it happens in the only instance hitherto where the name of Mr. Pott has been mentioned, that it was abused thus by a wrong and false construction? Was it in order that it may be said, that the Professor should be the first to describe the decay of the testicle without a rupture? If it were, this detection has proved so strong a moral against him, that, I will answer for him, nei-

* Vide Pott, Vol. II. from page 423 to 428. Cases 36, 37, 38.

ther his weakness nor vanity will again seduce him into such a plot.

He has given us but two cases of his own, one indeed cannot be called his own, for it equally belongs to Mr. Pott. The Professor has here again introduced his name, and a curious situation he has placed so great a man in, a helpless spectator to the decay of a testicle ! somewhat similar to a mute at a funeral !

When the Professor recommended Vanbutchel*, I inwardly applauded his humanity, such an alliance was natural. What would Don Quixotte have been without poor Sancho ! And this naturally leads me to make a few remarks on the manner which the Professor has treated authours who have already closed their scenes on the stage of life, and such as are now acting conspicuous parts upon it. In a book so voluminous as this before us, treating on a subject that has at-

* Page 159.

tracted the attention of more authours than any other perhaps that the literary world can again afford, a book arranged with the most studied care, a book which, if it hath any value when cleared of the new opinions, must draw that value from the various authours out of which the present system of treating the venereal disease is formed, is it not monstrous, that not a single authority is acknowledged to any good purpose, that not a single character is praised or recorded for any valuable improvement? What should be said to this? If the Professor were jealous of the reputations of the antients; if he had an intention by concealing some of their useful hints; if he meant to show off his diamonds ready set, without telling where they were to be found in the rough; yet amongst the moderns he ought not to dread competition. It was his very plea for writing that he was
 “ desirous to have an opportunity of asserting
 “ his right to some new opinions, that have
 “ made their way into the world under other
 “ names.” Then surely it would have been but fair in the Professor, as he discovered such jealousy for keeping what was his own,

as

as he was so greedy after the fame of these his new opinions, to have given some little share of credit to those who might put in their claim for here and there an old opinion ! Whilst he was erecting pyramids to himself, from out of their materials, surely he should not have grudged a head-stone for them !

I am very sorry that I must for the present withdraw my attention from further observations upon the new opinions of the Professor ; but it will be, I hope, to return to them with even additional pleasure. I am uneasy also that my next is to be my last ; for, although at first I thought the task unwelcome, now I am diverted. This speculation has proved to me so pleasant a one as any in my life. To see the man of learning and genius imitated ; to see wrong ideas, more wrong expressed, piled each upon the other ; to hear a man endeavour to reason abstruse, when he cannot reason plain ; to hear him talk of objects that exist not, and see him blind to such as are palpable ; to see illiberal traits which he had with studied art concealed, with common ease detected ; to see pride and vanity consume them-

themselves by ineffectual efforts. These cannot excite any other emotion but a pleasant one. Thus we are told of a vain philosopher, who ran frantick into a society, to shew it that he had discovered living monsters in the sun; which proved in the event to be only insects in his telescope.

(To be ended in the next.)

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PART III.

BY JESSÉ FOOT, SURGEON.

We appear astonished when we see the multitude led away by sounds ; but we should remember that, if sounds work miracles, it is always upon ignorance. The influence of names is in exact proportion to the want of knowledge.

PALEY'S PREFACE TO MORAL PHILOSOPHY.

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ORRERVATIONS

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VENEREAL DISEASE

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BY JESSE BOOZ, SURGEON

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CONTINUED.

PART III.

THERE are some to whom in my Observations I seem too bold.* Countries have always abounded with people of this character; being inactive spectators only, they do not consider how inevitable it is to write with enthusiasm, and to repel with some strength of expression, indignities that are offered to common understanding. If such remarked upon a battle, the gashes would be made too deep for them; if upon a horse-race, the animals would be pressed too hard; or if upon a boxing-match, the blows would be exchanged with too savage brutality; they would turn pale at the sight; let them keep to their chambers, or retire where their principles already are, and be enclosed within a cloister.

A

There

* Sunt quibus in satyris videor nimis acer—

There are some who annex so great a sanctity to names, as no more to be persuaded that reputation may be false, than that Achilles was vulnerable; such will not be convinced that I have, so far as I have already gone, exposed all the most material errors of the Professor; nor that the ox is yet slaughtered, whose mangled quarters are already hung up on the shambles of his butcher.

There are some, whose professional judgment affords them the ability of weighing fully the arguments which I have advanced, of determining with precision on the observations which I have made; before them I feel proud to appear, and I do assure them that I do not go beyond the dictates of my honour, when I declare how anxious I have been to gain their good opinion, and how zealous I still am to preserve it, by advancing only what my mind prompts me to be nearest the truth.

And there are others, who, amongst the partial friends of the Professor, knowing him to be guilty, wish not that he should be condemned; they whose prudence and humanity arrest the

the

the arm of justice; they who would rather that a whole country should be exposed to the pernicious effects of false and dangerous opinions, than that the Professor should experience their dereliction; they who are all to a man cautious, and with whom beyond their own circle all is hush! they whose very shoes are shod with velvet! and who deal forth whispers amongst each other, just like gossips in a sick chamber conversing on the rotten condition of a dying patient!

I do not wish to prejudice the minds of my readers, by forestalling any of the observations which I have yet to offer upon the new opinions of the Professor. I cannot conceive more gratification than to be indulged by them with an impartial review.

In order that I may be rightly understood, it is necessary for me just to give an outline of my future intentions, and to shew to my readers my system of arrangements; for without some system, the complicated subject which I have before me could not be gone through, either with credit to myself or advantage to them. I shall make a point of understanding the Professor in the most favourable construction.

struction. I shall always be upon my guard to correct myself against quibbling upon his language, as I would punish a pointer for standing at larks. I will continue to watch his new opinions, only so far as they apply to practice. I will remark upon that practice either as it is useful or new; and that practice which he adopts as new, and I know to be old, I will trace back to its original author, restoring thus the honour to whom that honour is due. Such expressions as are asserted to be facts in one page of the great work of the Professor, and contradicted in many other pages, will be only brought to one point of conviction, when I judge them to be of sufficient importance in practice. By all these endeavours I hope to methodise something like a system, to collect some simple scattered truths from out of a compound and incoherent mixture of systems. Sauvage and Cullen, as nosologists, Camper, as an experimental philosopher, and all the theorists of the present age, may claim their mutilated opinions in every page; these are so injudiciously chosen, and so unaptly applied, so mangled and perverted, as to tend rather to obstruct and confound the harmony of science, than to help the mind in the elaborate investi-

investigation of useful knowledge. He has made a sacrifice of himself to vanity. A confident Professor of the science of comparative anatomy, without the judgement of applying it to practice. The lower orders in the animal creation have afforded him an amusement somewhat similar to that which children derive from Dutch toys ; the fancy is awakened, but the understanding sleeps. I challenge the strongest panegyrist which he has, to produce one single solid advantage that the whole of the great work derives from his comparative anatomical pretensions. That he should conceive himself fitting, by education or genius, to become a dictatorial theorist, it must be unfortunate! "*stultè prava judicat sententia.*"* But our immortal bard has referred us for his apology to the infirmity of human nature. "We, " ignorant of ourselves, beg often our own " harm, which the wise powers deny us for our good."

Page 215. Part IV. Chapter I.

Of Chancre.

" Venereal ulcers commonly have one character, which however is not entirely peculiar

* Phœdrus.

“ peculiar to them, for many sores that have no
 “ disposition to heal, which is the case with
 “ a chancre, have so far the same character. A
 “ chancre has commonly a thickened base,
 “ and although in some the common inflam-
 “ mations spread much further, yet the spe-
 “ cific is confined to the base. The future or
 “ consequent ulcers are commonly easily di-
 “ stinguished from the original or venereal,
 “ which will be described hereafter.”

That the reader may not presume that
 I have selected this paragraph in order to
 expose the remarkable circumstance of
 each period opening with “commonly,” I
 shall vindicate myself by referring him to the
 paragraph immediately before this, where he
 will find six more “commonlys.” But here
 follows the observation which I have to offer
 to this paragraph, that it aims to distinguish,
 and to give a separate definition of chancre,
 in contradistinction to all other ulcers ; but
 it fails. First, “ Venereal ulcers commonly
 “ have one character, which however is not
 “ peculiar to them, for many sores that have
 “ no disposition to heal, which is the case with
 “ chancre, have so far the same character.”
 That is as much as to say, venereal ulcers
 are

are like all other ulcers having no disposition to heal. Such being the definition to the first period, now for the second.

“ A chancre has commonly a thickened
 “ base, and although in some the common
 “ inflammation spreads much further, yet
 “ the specific is confined to the base.” This
 might have been said, to have given us a
 distinction betwixt an ulcer produced from
 specific virus, and an ulcer from a common
 injury, had not the Professor just before
 confounded that distinction; for then he
 said, that the “ character of chancres is not
 “ peculiar to them.”

And now for the third, which is accom-
 panied with a most sagacious confession in-
 deed! “ That consequent ulcers are com-
 “ monly easily distinguished from the ori-
 “ ginal or venereal.” As these three defini-
 tions of the Professor pull three separate
 ways, I shall leave them to the penetration
 of my readers to regulate the power of each
 upon the other, to the end that they may
 obtain from them that information, which
 I am confessedly incapable to explain.

The truth is, that ulcers are only found out to be venereal from situation or suspicion, by being either seen on such parts as induce us to prejudge them to be owing to venereal infection, or by uniting and combining circumstances when seen on other parts, so as to persuade us that the strongest probability goes to their being venereal. When ulcers are found upon such parts as there is reason, upon their first appearance, to believe that they owe their source to a venereal cause, it is upon the first blush decided that they are so; but when they appear upon a part not suspected, then it often happens, that other opinions are given, and other remedies are tried, and these not succeeding, mercury is at last administered, and the disease is proved by the success of the remedy.

Let men say what they please in theory, this we know to be the state of the case in practice; and it will be hereafter seen, that there is not an additional chink in the Professor's dark constitutional cottage, through which new light is let in upon the subject.

“It

“ It is an invariable effect, that when
 “ any part of an animal is irritated to a cer-
 “ tain degree, that it inflames and forms
 “ matter, the intention of which is to re-
 “ move the irritating cause.”

I shall, before I proceed, give the true definition of “ invariable,” as it always has been, and as it will be ever understood. I say, it is that which cannot be changed, and must always be constant. It now behoves me to prove that the Professor’s logick is no logick, and that his invariable aphorism is subject to great variation. Instances in contradiction to this invariable position of the Professor are to be found, both as to the effect and to the intention; both from constitutional causes and external applications; both as they affect the constitution in general or partial; in short, in every sense the Professor’s invariable effect and intention are foiled. To begin with the constitutional causes.

Do the ear-ach, tooth-ach, continued vomitings, stones in the kidnies, stones passing from the kidnies to the bladder, stones in the bladder, stones passing from the bladder

through the urethra, gout, rheumatism, and spasms, invariably in their effect, inflame and form matter with intention to remove the irritating cause? Do the hydrophobick symptoms and lock-jaw, from local causes, produce such invariable effects with such intentions? Do barbarous punishments inflicted on the human body by the authority of tyrannick laws, produce from such causes, such effects, and with such intentions? What would the poor soldier say to the Professor's invariable effect and intention, when standing upon spikes? For God's sake, Mr. Professor, do not persuade yourself that, although I am now irritated to the very "certain degree" which it is possible for man to be, I shall get rid of the irritation by the formation of matter; if you do, I shall certainly be dead before your intention will be fulfilled! But if, continued the poor soldier, you will permit me to instruct you, pray, loosen my hands which are tied to the halberds; I will then step off from the spikes, and shall by that means shew you my intention to get rid of the irritating cause!

But let us pursue this neat observation of the "intention to remove the irritating cause

cause," and then let us see how the "invariable effect" is produced in chancre; and there I fear that the intention and effect are invariably contrary to the Professor's logick; for admitting the intention, and it will be found to be in destroying the part which is the seat of the chancre, and to this he has agreed; "for it was almost impossible to carry a chancre a long voyage without its destroying the whole penis."*

It is to be presumed that it was the same intention that taught the Professor to cure gonorrhœa with bread pills; and it was the same intention and the same ignorance in discrimination, that made him confound common irritations with specifick irritations. To illustrate this subject, I will state the exact distinction which there is between irritation from a common and a virulent cause. Suppose a thorn to be stuck in any part of the body, and there left to work its way out, the part would be inflamed, and matter would be formed, the offending substance would be loosened, and in time be discharged; and the parts being no longer irritated, would heal of course. But let us

also suppose that the same thorn had been dipped in venereal virus, and that the infection from it had actually taken effect, would the parts then heal up as before, when the thorn was withdrawn?

Immediately after the aphorism last quoted we find the following: “ This process is
 “ easily effected, when it is on a surface
 “ whose nature is to secrete; but when on
 “ a surface whose nature is not to secrete,
 “ it then becomes more difficult, for another
 “ process must be set up, which is ulcera-
 “ tion. This is not only the case in com-
 “ mon irritations, but also in specific irrita-
 “ tion from morbid poisons, as the vene-
 “ real disease and the small-pox.” Again we are told, that venereal irritation, as well as variolous and common irritations, terminate alike in three processes, on non-secreting surfaces; inflammation, formation of matter, and ulceration, the intention of all which is to get rid of the irritating cause. I have already pointed out to my readers my distinction betwixt common and specifick irritation; and I have often observed to my readers how wanting in judgement the Professor always is, when he draws his comparisons.

rifons. Do ulcers arising from small-pox and venereal infection terminate alike? Are they alike got rid of? The variolous matter and the venereal act so far alike as to produce each of them an ulcer by inoculation; but the utmost stretch of human ingenuity cannot afterwards bring the least parallel between them. The action of variolous ulcers ceases spontaneously, after a certain process; but the action of a venereal ulcer is continued *ad infinitum*, as the Professor has confessed in page 14, and, I will answer for him, as he will again hereafter confess. So much for his similar effects from common and specifick irritations; and so much for his comparative knowledge, and his skill in the application of it!

“ There are three ways in which chancre
 “ are produced. First, by poison being in-
 “ fected into a wound; second, by being ap-
 “ plied to a non-secreting surface; and third-
 “ ly, by being applied to a common sore.”

“ The poison much more readily conta-
 “ minates, if it is applied to a common
 “ wound, than to an ulcer, in this resem-
 “ bling the small-pox.” The Professor in
 another

another place has given the very opposite opinion to that which we find here. " I am
 " inclined to think that wounds are also
 " bad absorbing surfaces, especially when I
 " consider that few morbid poisons are also
 " absorbed by wounds." My readers will
 be pleased to recollect, that this last quotation was not overlooked in my First Part.* And now I will treat my readers with some selected opinions that will better explain themselves than I can for them.

" Whether there are any parts of the skin,
 " or any other part of the body, more susceptible of this irritation than others, is not
 " yet ascertained."

" I have seen a chancre on the prolabium
 " as broad as a sixpence, caught the person
 " did not know how." To this is added a note,
 " That this fore was a chancre, I made no
 " no doubt, for besides its diseased appearance,
 " he had a bubo forming in one of
 " the glands under the lower jaw on the
 " same side."

" Its affecting the glans penis, &c. arises
 " from the manner in which it is caught, and
 not

* Pages 61, 62, 63.

“ not from any tendency these parts have to
“ catch it more than others.”

“ But as the cuticle cannot be affected by
“ the poison, this covering acting as a guard
“ to the cutis, it is often prevented from
“ coming into contact with it.”

“ If the venereal poison should be ap-
“ plied to the skin, where the cuticle is more
“ dense than that of the glans penis, such as
“ upon the body of the penis, or forepart
“ of the scrotum, then it generally appears
“ first in a pimple, which is commonly al-
“ lowed to scab,” &c. &c.

Let the Professor, for it is his business and not mine, reconcile these various and contradictory opinions to his pupils, if peradventure he should hereafter have the opportunity; for as his train of followers, which like to poor King Lear's hath been an hundred, now is less than twenty, it may be a unit! And does the noble science of the Professor “sink in the
“ ground? I thought it would have mounted!”

I cannot, because of my limited condition, take notice of every absurd new opinion; indeed

deed, I am often necessitated to be very brief with those I do observe upon. My condition is somewhat similar to that of a ghost, whose appearance is permitted to disclose some momentous enormities, but is warned away at the third crowing of the cock!

“ I have known cases where the chancres
 “ have appeared twenty-four hours after the
 “ application of the matter, and I have known
 “ them seven weeks. A remarkable case of
 “ this kind was in a gentleman who had not
 “ touched a woman for seven weeks, when a
 “ chancre appeared. That this was a
 “ venereal chancre was proved by his hav-
 “ ing had the lues venerea from it, and be-
 “ ing under the necessity of taking mercury.”

All the remark that I shall make upon this quotation, in the present instance, is, that the Professor does not pretend to prove the above to be a chancre by his own original definition; but that he proves it to be a real chancre by its consequences, by its having diffused infection, and by its yielding to mercury. And again he says—

“ An

“ An officer in the army had a chancre
 “ broke out upon him two months after he
 “ had a connection with a woman. After
 “ the last connection he marched above an
 “ hundred miles; the chancre broke out,
 “ and only gave way to mercury.”

Here is another instance to shew how the
 Professor, even in a doubtful case, proves a
 chancre. But these two cases will hereafter
 be appealed to in a cause of much more se-
 rious importance.

Page 221.

Of the Phymosis and Paraphymosis.

“ These happen generally in consequence
 “ of a chancre in the prepuce. This irrita-
 “ tion however and inflammation sometimes
 “ attack the prepuce, even when the disease
 “ is in the form of what I suspect to be a go-
 “ norrhœa of the glands of the prepuce, and
 “ sometimes even in the common gonorrhœa,
 “ but most frequently of all from a chancre
 “ in the prepuce.*

I request the particular attention of my
 readers to the observations which I am about

PART III.

C

to

to lay before them. These will help to explain that idea I have of such intentions of the Professor, which with his usual left-handed cunning he would wish to conceal. By his not saying a word about phymosis or paraphymosis arising from gonorrhœa, when he was treating upon gonorrhœa, he was desirous it should have been forgotten that these symptoms had been ever found with gonorrhœa, as well as with chancre; and the reason why he did not observe upon phymosis and paraphymosis, as being often the immediate effect of gonorrhœa, only must have been, because in such an instance gonorrhœa could not have been said to have cured itself; because he must have been reduced in such an instance, not only to have prescribed bread pills, but also bread poultice, with something more. This silence, and this strong instance of instinctive sagacity in the Professor, are yet more apparent, as he has exhausted almost a fourth of his great work upon what he calls, the supposed consequences only of gonorrhœa. And even now, it is curious to remark, how he arranges and how slightly he touches upon the the subject of phymosis arising from gonorrhœa. I appeal to men of candour, whether by the Professor's statement of the case,

it

it would not be understood by a tyro, that phymosis and paraphymosis were as frequently liable to arise out of his conceited and visionary opinion of a virulent gonorrhœa of the glans penis, as out of a common virulent gonorrhœa. What shall be said to these deviations from truth? Do they spring from vanity or ignorance? Were phymosis and paraphymosis not noticed by him, when he treated on the consequences from gonorrhœa, because he knew they would tend to destroy the cobweb which he had spun out in theory? Or were these symptoms then omitted, because he was yet to learn, that they often did arise from gonorrhœa? It could not be the latter, unless it was presumed that he had acquired the knowledge since he treated upon gonorrhœa. Here it is confessed, but reluctantly confessed; not observed upon as a common symptom, not as a man of ingenuous candour would observe upon such a truth, but it is privately slipped into this page, in so cunning, so secret and indirect a manner, as the arrantest Jonas of them all would pass a card to deceive us! I know that phymosis and paraphymosis may be the effect of gonorrhœa; I know that phymosis may produce chancre; I know that chancres may destroy

ad infinitum; and I know that the whole constitution may thus become diseased.

Page 226. Chap. III.

General observations on the treatment of chancres.

“ Chancres as well as gonorrhœa are
 “ perhaps seldom or never wholly venereal,
 “ but are varied by certain peculiarities of the
 “ constitution at the time. The treatment
 “ therefore of them, both local and constitu-
 “ tional, will admit of great variety, and it is
 “ upon the knowledge of this variety that
 “ the skill of the surgeon principally de-
 “ pends.”

In my first outset I told my readers, that I would never forestall any of the Professor's contradictions, because I could always collect a more than sufficient supply from the very ample and superabundant store that had already passed in my review. The Professor here says, “ That the treatment of both gonorrhœa
 “ and chancre, both local and constitutional,
 “ will admit of great variety, and that it is up-
 “ on the knowledge of that variety the skill
 “ of the surgeon principally depends.” Was the Professor always of the same opinion? Or was the great work so long in hand that he had here forgotten what he had advanced there?

there? Was truth yesterday, falshood to day? I will now illustrate the Professor's consistency in as strong a light as his own anxiety for singularity of opinion could hope.

The common practice proves these facts; we every day see gonorrhœas cured by the most ignorant; but in chancre, or the lues venerea, more skill is necessary. The reason is obvious; gonorrhœa cures itself, whilst the other forms of the disease require the assistance of art.*

“ The great variety of injections, and
 “ every venereal inflammation getting well
 “ during their use, are strong corroborat-
 “ ing circumstances in favour of the opi-
 “ nion, that every such complaint will in
 “ time cure itself.”†

Once for all I may be permitted, with all due respect for the Professor, to remind him in a very humble way, of an opinion which he knows, but which, because it is not new, he will not adopt; and this opinion now understood between us, will save many illustrations in future, when the Professor and I are found again at variance, from simi-
 lar

lar causes. Do the same diseases affect all all subjects in an equal degree? Is there not in every constitution an idiosyncrasy? May not two brothers, even with the same complexions, the same habits of living, be attacked with the small-pox at the same time? And may not the one be so fortunate as to have no occasion for the skill of the Professor, because he had not two eruptions all over his his body? And may not the other, in spite of such skill, die of the disease?

The question which naturally arises, is, how the Professor in this instance would draw his conclusions? Would he assert as roundly here as he did upon gonorrhœa? No. If the small-pox in the one be slight, small-pox cures itself; if it be severe, it may kill. If gonorrhœa be slight, it may cure itself, or at least bread pills may cure it; if it be severe, and no other remedy applied to it but the harmless one prescribed by the Professor, then the whole contiguous parts may become diseased, and more disease may follow, so as to kill the deluded subject!

Admitting that such may be the event of two brothers attacked with the small-pox,
and

and I defy the Professor to contradict it, what further argument is it fitting that I should hold, when it is known that the whole process of small-pox closes at a certain specifick period, and when it is also known that the effects of gonorrhœa may continue *ad infinitum*? But such are the doctrines of the Professor, “as who should say, I am Sir Oracle!”

“It is to be observed, that in the cure of
 “chancres we have two points in view, the
 “cure of the chancre itself, and the preven-
 “tion of a contamination of the constitu-
 “tion.” So far, so good. “The first, or the
 “cure of the chancre, is to be effected by
 “mercury, applied either in external dres-
 “sings, or internally through the circula-
 “tion, or in both ways. The second ob-
 “ject, or preservation of the constitution
 “from contamination, is to be obtained, first,
 “by shortening the duration of the chancre,
 “which shortens the time of absorption,
 “and also by internal medicine, which
 “must be in proportion to the time that
 “absorption may have been going on.”——
 “If the power of a chancre to contami-
 “nate the constitution, or which is the
 “same thing, if the quantity absorbed is as
 “the

“ the size of the chancre, and the time of
 “ absorption, which most probably it is,
 “ then whatever shortens the time, must
 “ diminish that power or quantity absorbed;
 “ and if the quantity of mercury necessary
 “ to preserve the constitution, is as the
 “ quantity of poison absorbed, then what-
 “ ever lessens the quantity absorbed, must
 “ proportionally preserve the constitution.”—
 “ For instance, if the power of a chancre to
 “ contaminate the constitution in four weeks
 “ is equal to four, and the quantity of mer-
 “ cury necessary to be given internally, both
 “ for the cure of the chancre and the pre-
 “ servation of the constitution, is also equal
 “ to four; then whatever shortens the dura-
 “ tion of the chancre, must lessen in the
 “ same proportion the quantity of mercury;
 “ therefore, if local applications, along with
 “ internal use of mercury, will cure the
 “ chancre in three weeks, then only three
 “ fourths of the mercury is necessarily want-
 “ ed internally.”——“ If four ounces of mer-
 “ curial ointment will cure the chancre, and
 “ preserve the constitution, in three weeks,
 “ then only three fourths of the mercury is
 “ necessarily wanted internally.”

I make no apology to my readers for the length of this quotation; they as well as I had seen the Professor all this while affirming impossibilities, realising visions, reviving old ideas, and propagating them as new ones. Certainly then we ought to congratulate ourselves, and be thankful to him for the new talent that he has here disclosed to us. After having exhausted his old characters, he is now to shew us a versatility of genius in the display of a new one. Our conviction to his system must now be inevitable, for the solidity of his arguments is to be confirmed by arithmetical calculations and mathematical deductions. And the truth is, that if every machine to which the Professor applies these mathematical rules for measuring the size of the chancre, and these arithmetical calculations for ascertaining the quantity of absorbed virus from the chancre, were the same; if there were the same proportionate ratio found in the size of all chancres, and if the absorption from every one of them were the same in quantity, in the same given time; if every machine to which the Professor applies his local as well as his constitutional powers, were also uniformly the same, and at all times; if every

machine did not differ, and if each did not vary, which all machines constantly do; if absorption of virus were not undetermined and capricious, which we constantly find that it is; if chancres were not to prove more or less obstinate in their cure, which we constantly find that they do; if the absorption of mercurial ointment by friction were not uncertain, and if its action on the constitution were not various in the extreme, as one may rub in an ounce, and another three ounces, to the same effect; if these circumstances, separate or combined, were not diametrically averse to the mathematical and arithmetical conclusions of the Professor, which they constantly are, then he might have boasted, and we might have profited from his superior knowledge of the mathematical scale for the measurement of chancre, and for his arithmetical calculation by the golden rule direct, in the cure of it and the constitution. For example, thus: If a chancre of a certain size yield a certain quantity of poison, and if that certain quantity of poison be absorbed in a certain time, and if a certain quantity of mercury, given internally, (for he calls the external friction of mercurial ointment, internal) preserve the

the constitution from contamination, then, by applying part of the mercury to the chancre itself, instead of throwing it all into the constitution, the chancre will heal the sooner, and less mercury will be thrown into the constitution, because the duration of chancre is shortened. If constant effects could be thus insured, we might then exult indeed, and the patient also would have great cause for triumph; we need visit him only once, just to take measure of his chancre, to calculate from its size the quantity of virus absorbed, to ascertain the time, to work our problem, to produce our scales, to weigh out our mercury, and to take our leave! For the Professor says, "This is not speculation, but the result of experience." I sincerely beg his pardon for disputing so strong an assertion, and for having so lately quoted from his own authority, "that the treatment of
 " chancres, as well as gonorrhœa, both local
 " and constitutional, will admit of great
 " variety; and it is upon the knowledge of
 " that variety that the skill of the surgeon
 " principally depends."

1. *Of the Destruction of Chancre.*

At length we come to the practical part of his opinions upon chancre, after having wandered with delight through the flowery path of theory, laid out with all the luxuriant fancy that the vigorous and fertile genius of the Professor could lavish upon it.

“The simplest method of treating a chancre
 “is by destroying it, whereby it is reduced
 “to the state of a common sore or wound,
 “and heals up as such. This only can be
 “done on the first appearance of the chancre.”
 —“It may be done either by incision or
 “caustic.”

To effect this the Professor recommends either the lunar caustick or lapis septicus. As for incision, it appears from what is here said, to be recommended from one instance only. “I have dissected a chancre out, and
 “the sore has healed up without any other
 “treatment but common dressings.”

But in the very next paragraph, and all the succeeding ones, this simple practice is
 thrown

thrown aside ; and as absorption may be suspected, he contradicts his first position and encourages specifick applications both internal and external. He only meant by these assertions to shew himself off, to point out to us what he could do, how he could apply caustick to a chancre, how that chancre would heal afterwards, or in other words, how he could stand upon one leg for a time, although for walking it was necessary to use both legs. As this is a practical subject of the first importance, it behoves us to ascertain whether any novelty in practice be here offered by the Professor ; whether in this instance there be any display of new opinions worthy the great author of a great work. Is the application of caustick to a chancre a new opinion ? Is the application of caustick to a chancre a proper method of treatment ? And first, the application of caustick to extirpate a chancre is not a new opinion ; although the Professor has been silent upon this point, for this was his reasoning upon this delicate question : There are they who will read my book, who will know how old this practice is, and who now follow this very practice. There are others who take up the history of the cure of the disease

disease from the gospel in my great work only ; they lean so implicitly upon my honour and judgement, and are in point of numbers so superior to those who can detect me, that it will be clearly for me politick to be silent ; no matter how I get credit, but I shall get credit ; therefore the names of any authors shall not be hinted at by me, not even whether they had the “ same idea,” nor whether they were “ clumsy” in applying it, nor whether I found out the idea, and saw it in their work afterwards when reading for my amusement ! Wiseman, Astruc, Turner, and authors more modern, have observed upon the application of caustick in the cure of chancre, particularly Howard and Swediaur. The Professor at least keeps always one eye employed to one object ; it is never taken off, but fixed habitual to interest as the lungs to air.

Second, the application of caustick to a chancre is a proper method of treatment. But I defy any man to determine from what the Professor has said, whether the mere act of destruction of chancres be safe or not, as he has said that it is, and also said that it is not

not safe; I shall therefore, with all deference, just offer my opinion upon the subject. I have practised the application of caustick for the cure of chancres, but have never been so hardy as to depend upon that alone. And the reason for my mistrust of so simple a cure will be more obvious, when my readers take into their consideration one notorious and indisputable fact; a fact which has never been contested by the most stubborn skeptick of them all, namely, that absorption of virus may take place without the first appearance of chancre; that the venereal disease does sometimes make its first appearance in the symptom of a bubo; that at the same time when the infection received has shewn itself in one part of the penis, in the form of a chancre, other parts might have absorbed the infection without its having left such a trace of it on the penis; adding also the probable supposition that notwithstanding the chancre does heal, absorption might have taken place there, which is the case, when a part is bitten by a mad dog; that part heals, yet the constitution has been found to be infected. I presume therefore that we are not justified in trusting to simple destruction of chancre only;

only ; but hitherto I have been convinced that in the early stage of a chancre, the application of caustick to it promises to be the most compendious method of treatment, at the same time mercury should be given internally, and in small quantities, for a length of time ; and in this I am in some measure guided, by there remaining or not any hardness where the chancre was, or by the state of the glands of the groin.

Page 230.

2. *Of the Cure of Chancre—Local Applications.*

“ The cure of chancre is a different thing
 “ from its destruction, and consists in de-
 “ stroying its venereal disposition and ac-
 “ tion, and then the parts heal of course,
 “ as far as they are venereal.”——“ Chan-
 “ cres may be cured in two different ways,
 “ either by external applications, or inter-
 “ nal applications through the circulation ;
 “ the same medicine is necessary for both
 “ these purposes, that is, mercury.”

I just now remarked, that the Professor had left us undetermined, whether to give mercury when the chancre was removed by
 what

what he calls destruction ; but it now appears from his own confession here, that you must not believe a word he has said there, about giving mercury in that case, for the cure by mercury is “ a different thing.” By the cure of a chancre, mercury must be given externally, or internally ; by destroying a chancre, “ the thing is different.”

My readers will not forget that the Professor, when the mechanical fit was upon him, told us, that the application of mercurial ointment, by friction, was giving mercury “ internally.” In this last quotation he has corrected himself, by contradicting his former expression. It shall not be said, that this is vainly cavilling upon words. How is a book to be understood, if one expression be given for another ? Or how are facts to be ascertained, when an author there says internal, and here, to convey the same meaning, says external ? Let not the Professor’s apologist explain this instance of inattention away, as if wrong were right, within without, or presence absence ! Sir Francis Wronghead thus said no for aye, and lost his prospect of place !

My readers shall be left to judge for themselves as to the topical applications. I shall but lay them before them, they then may pick, choose, and assort. There they are, collected from all authors and from all times. “ Mercury in poultices; mercury
 “ rubbed down with conserve in the room
 “ of ointment; calomel, and all other prepa-
 “ rations mixed with mucilage or honey;
 “ calomel mixed with common salves; solu-
 “ tions of blue vitriol, verdegrease, calomel
 “ mixed with spiritus nitri dulcis, and ma-
 “ ny others. When the sores are irritable,
 “ mercury with opium, or “perhaps” lead.”

“ Chancres, after having their venereal
 “ teint corrected, often become stationary,
 “ and having acquired new dispositions, in-
 “ crease the quantity of disease in the parts,
 “ as will be taken notice of hereafter.” I do assure the Professor, that his new opinion of new dispositions, shall be taken notice of hereafter. And as the challenge is now given, I shall avail myself of the earliest opportunity of time and place for the full investigation of new dispositions, that do not partake of “ venereal teints.”—“ When they become
 “ stationary,

“ stationary only, they may often be cured by
 “ touching them slightly with a lunar cau-
 “ stick.” Just before he said, “ This can only
 “ be done on the first appearance of chancre,
 “ when the surrounding parts are not as yet
 “ contaminated.”——“ And it is surprising
 “ often, how fast they will heal after being
 “ touched, and probably once or twice will
 “ be sufficient.” How wonderful, as if he had
 heard of a world ransomed, or one destroyed!

Page 232.

3. *Of the Treatment of Phymosis in consequence
 of, or attended with Chancre.*

My readers will remark how shy the Pro-
 fessor is of phymosis from gonorrhœa. He
 will not even be seen in the title-text to ad-
 mit, that, while he is curing phymosis ari-
 sing from chancre, the same treatment, as
 far as is necessary, may also be applied to
 phymosis from gonorrhœa. I just request
 again, that my readers will mark this un-
 common shyness in the Professor. But to
 begin. Astruc has advised, that “ the end
 “ of the penis should be kept up to the
 E 2 “ belly

“ belly if possible;” therefore the Professor’s instruction upon this point cannot be new.

“ As the fores cannot be dressed in the common way, we must have recourse either to dressings in the form of injections, or the operation for the phymosis.”

“ Washing of a fore, I believe, is unnecessary; for I can imagine, that matter from any fore whatever is always such as cannot stimulate the fore into action.”*

Of injections ; “ they should be mercurial, either crude mercury, rubbed down with solution of gum arabic; calomel rubbed with the same, with a proportion of opium. No nicety is required. But if a solution of sublimate is made use of as an injection, some attention is to be paid to its strength. Poultices with linseed, and bread with laudanum; the penis to hang over hot water with a little vinegar and spirits of wine,” which the Professor observes to be “the neatest way of applying fomentations.” Is it the most serviceable fomentation, and the most effectual way of applying fomentations? No.

Decoction

Decoction of poppies is better, and spirits of wine is better to be applied by itself. I never even suspected before, that the Professor was considered as a pattern for neatness. “ When the chancre bleeds, oil of turpentine, and sometimes opium.”

Page 235.

4. *Of the common Operation for the Phymosis produced by Chancres.*

“ In many cases it will be found, that so violent an operation is improper ; for it often happens, that while the inflammation is so very considerable, there is danger of increasing it by this additional violence, of which mortification may be the consequence ;—while on the other hand, there are cases when a freedom given to the parts would prevent mortification ; so that the surgeon must be guided by the appearances, and other circumstances.”

If I understand the true spirit of the intended instruction contained in this paragraph, it is, that the Professor submits the time for judiciously performing the operation, to the judgement of the surgeon ; but his reason

son

son for so doing is so very curious, as to excite investigation at any rate. The surgeon is to form his opinion, whether it be right to perform the operation from consequences : For, if the operation be performed while inflammation is very considerable, there is danger of mortification ; or if the operation be not performed, and a freedom given to the inflamed parts, mortification comes on from that cause also. But, as it is known that this operation is necessary to be performed to prevent inflammation ending in mortification, why has the Professor thus equivocated ? For, as it is only in extreme cases that this operation is judged to be necessary, and as, in my opinion, it is only in such rapid cases, where we find the chancre destroying the substance of the penis itself, without discovering the least “ intention, by its matter, “ to remove the irritating cause ;” where we cannot come at the seat of the chancre that threatens so much destruction, and where our power is otherwise too much cramped ; where the chancre bleeds profusely, and where we discover that its seat is on the glans, it is then only necessary to make this last appeal ; it is then only, that we should not hesitate to perform this operation. But
if

if the chancre be on the prepuce, I do not think that much is gained by the operation. In the necessary instance, much is at stake ; therefore, something should be risked ; much might be gained, and the condition of a spreading corroding chancre on the glans, or behind the glans, cannot be so alarming when exposed to our view, when we have the power of applying topical remedies to it, as when it was concealed, and making rapid strides to destroy the whole of the penis. Either this operation is necessary, or it is not. If it be not necessary, why did the Professor treat upon it ? If it be necessary, why did he not draw the line ? Why has he equivocated ? Or, if he could not annex the proper time to perform it, why did he write ?

“ A mortification of the prepuce is sometimes the consequence, when attended with violent inflammation, even without any previous operation ; and I have seen cases, where the glans, and part of the penis, have mortified, while the prepuce has kept its ground.” Even without the operation, says the Professor. Does he mean to say, that the intention of the operation was to promote, and not to prevent mortification ?

cation ? Does he mean to say, if, in this instance he had, at a fitting time, performed the operation, that such mortification and destruction of parts would have so completely gone on ? If he does, I do not. In my opinion, and I appeal to my readers, the Professor has betrayed much chirurgical ignorance : He seems to have had no settled opinion upon the subject. For, if ever there were a case when the operation should have been submitted to, that was the very case which he has here noticed, and where it was not performed. The danger of an operation is a subordinate consideration to the danger of a concealed chancre, conditioned, as I have already stated, and must be hazarded.

Page 237.

5. *Of the Constitutional Treatment of Phymosis.*

“ Is mercury to be given freely, to get rid
 “ of the first cause ? Or does that medicine
 “ increase the effect, while it destroys the
 “ cause ? Nothing but experience can deter-
 “ mine this.”---“ Bark is the medicine that
 “ probably will be of most general use ;
 “ opium, in most cases of this kind, will be
 “ also

also of singular service. The bark should be given in large quantities along with mercury, while the virus is still supposed to exist ; but if the inflammation be very great, “ bark must be given alone”

Page 238.

6. *Of the Treatment of the Paraphymosis from Chancres.*

The relief that is to be given is so obvious to every practitioner, that it is needless to say more, than that a stricture which threatens danger to the parts, should be divided. But when the symptoms do not exact this resource, the same methods recommended for phymosis, are fitting for this.

Page 239.

7. *Of the Cure of Chancres, by Mercury given internally.*

And here we have, perhaps, not only as they refer to this subject immediately, but also as they bear relation to past opinions, such a string of contradictory assertions, so unqualified, and so exposed to obvious detec-

tion, that it can hardly be decided to what cause to assign them. If the Professor be right in one paragraph by chance, it is opposed in the next. If we are instructed to follow this system here, it is contradicted by another immediately after. We are kept in a state of suspense betwixt both, like the tomb of Mahomet, continually antagonised by contrary attractions.

“ In every chancre, let it be ever so slight,
 “ mercury should be given internally; even
 “ in those cases where they are destroyed on
 “ their first appearance.”

“ The simplest method of treating a chan-
 “ cre, is by destroying or extirpating it,
 “ whereby it is reduced to a common sore
 “ or wound, and heals up as such.”*-----
 “ I have dissected a chancre out, and the sore
 “ has healed up without any other treat-
 “ ment than common dressings.”†----“ The
 “ cure of a chancre is a different thing from
 “ its destruction; medicine is necessary, that
 “ is, mercury.”‡

“ When

* Page 228.

† Page 229.

‡ Page 230.

“ When the sore has put on a healthy
 “ look, when the hard basis has become
 “ soft, and it has skinned over kindly, it
 “ may be looked upon as cured.”----“ But
 “ in very large chancres, it may not be al-
 “ ways necessary to continue the application
 “ of mercury, either for external or internal
 “ action, till the sore is healed.”---“ A large
 “ chancre may be deprived of its venereal
 “ action long before it is skinned over ; but
 “ a small one may probably skin over before
 “ the venereal action is entirely subdued.”
 ----“ It will be erring on the safe side, to
 “ continue the medicine a little longer.”

Chap V. Sect. I.

Of Disposition to new Diseases taking place during the Cure of Chancres.

“ The ulceration on the inside of the pre-
 “ puce will sometimes increase, and run be-
 “ tween the skin and the body of the penis,
 “ and eat holes through in different places,
 “ till the whole is reduced to a number of
 “ ragged sores. The glans often shares the
 “ same fate, till more or less of it is gone ;
 “ frequently the urethra at this part is
 “ wholly ulcerated away, and the urine

“ comes out some way further back. If a
 “ stop is not put to the progress of the
 “ disease, the ulceration will continue till
 “ the parts are entirely destroyed. I suspect
 “ that some of these cases are scrofulous.
 “ As this is an acute case, immediate relief
 “ should be given, if possible.” As the Pro-
 fessor has not yet launched out into a theo-
 retical explanation of what he means by
 “ new diseases,” I shall, and it will here suf-
 fice for me to flatly deny, that the condition
 above described is a “ new disease.” It is an
 assertion arising out of the lowest of all ideas,
 and signifying nothing. He declares, that
 “ no rational method of treating it can be
 “ determined ;” which is as much as to say,
 that he, not knowing the nature of the
 disease, of course cannot determine upon a
 rational method of curing it. Mercury is
 given up entirely, and the disease is aban-
 doned to chance. Sarsaparilla, German diet-
 drink, extract of hemlock, and sea-bathing,
 are substituted ; opium also. But even bark
 is left out of the catalogue. I shall not
 enter into a minute investigation of this
 stage of the venereal disease here ; I know a
 yet more fitting time for it. But now I
 have an opportunity of clearing off another
 score

score with the Professor. In treating of gonorrhœa, what was then the language? Did he not then hold out to us a promise, that mercury was a specifick for every other form of the disease but gonorrhœa? * And did he not then endeavour to prove, that “ the great variety of injections, and every “ inflammation getting well during their “ use, were strong corroborating circumstances in favour of the opinion, that “ every such complaint will, in time, cure “ itself.” † And did I not then ask the Professor, if there were not more than one way sometimes necessary to cure a chancre? ‡ What is the answer now given to the question I then put to him? Does he not aid, or change, or omit mercury altogether, for the cure of chancres in the condition before us? And because he is so driven to such a necessity, does he draw such an inference in the case of chancre, as he did in gonorrhœa? That the great variety of remedies, and every chancre at least getting well during their use, are strong corroborating circumstances in favour of the opinion, that every such complaint will, in time, cure itself? No, says the Professor, I have cured the chancre; it is no longer

longer venereal ; mercury is a specifick for the cure of chancre ; and although you now see an ulcer much more formidable than it was a week ago, and although you see it spreading at such a rate, that the destruction of the whole substance of the penis is threatened, yet I have cured the chancre, and mercury has been my specifick. And what does he call this present disease, this continuation of chancre ? Why he calls it “ a new disease !”

Let us hypothetically put the case, that Saturday is the day on which the Professor first announces to the patient, in the hearing of the nurse, that the chancre is now too bad to be any longer considered as venereal ; that in fact it is not now venereal, but that that which the patient feels, and the nurse sees, is now “ a new disease.” The consternation of the patient, the curiosity of the nurse, the profundity of the Professor, form a striking group ! On what day, asks the nurse, did the venereal disease leave my poor master ? Aye, says the patient, recovering a little from his desponding dilemma, was it on the same day that the new disease came on upon me, or yesterday, or the day before yesterday, or when ?

when ? How was this invisible revolution brought about ? Was the old king of terrors dethroned, or did he abdicate, or was he strangled by mercury ? And who is this new king of terrors, this worst of usurpers, who sat him up, and what are his pretensions ? It is “ a new disease,” says the Professor. It was “ to be,” says the nurse.

Let us also hypothetically put the case, how the pupils of the Professor would reason upon this ? Says the first, I cannot understand what he means by “ a new disease.” Says the second, the Professor tells us that it is “ scrofulous.” Says the third, I thought scrofulous complaints were chronic, and mostly hereditary ; but the Professor tells us, that this is an “ acute disease,” in continuation of the same sentence where he suspects it to be “ scrofulous.” Says the fourth, what arrogance it is in you three, to presume to understand him in the first course, when I, who have been his perpetual pupil, have not as yet caught half his inspirations !

Page 253. Part V. Chap. I.

Of Bubo.

The Professor commences this chapter with preparing the minds of his readers, by seduction, instead of argument, with presuming, that former anatomists had not acquired enough knowledge of the lymphaticks, and that surgeons, in consequence, did not know enough before the present æra, to treat upon the true cause of bubo, and to deduce from that true cause a practice, founded upon a system so rational as this he has here offered to our consideration. “ Prior to the
 “ knowledge of the absorbent system, we
 “ find writers at a loss how to give a true
 “ and consistent explanation of many of the
 “ symptoms of this disease.” The Professor has not left us to conjecture, at what time this dark ignorance prevailed; but here, with a confidence in his literary powers, and with a triumph from his theoretick and practical pre-eminence, his vanity has got the better of his prudence: In those prudential and cautious moments, he declares, that he never reads; yet, if there be any honours to pluck,
 if

if other authors are to stoop for him, to raise his fame upon their shoulders, why then he reads. Yet he may find himself disappointed even here, as when he attempts to soar by his own innate genius, by his own and new opinions ! What advancement in the doctrine of absorbents has he made ? We hear nothing of him, or read nothing more of him, than that he was an evidence for a brother, in his disputes with Pott and the Monros.* I beg his pardon ; I had almost forgotten : He tells us, that since the knowledge of the absorbent system, he has discovered, that ulcers may be produced from absorption,† and that “ the solids of animal
 “ bodies are equally capable of being ab-
 “ sorbed as the fluids.”‡ Absorbents may take up what was solid, that is, when bones are in a dissolved state. Naturalists know, that, if a centipede be confined with her young in a stopple bottle, without any food, the young will devour their mother, and that they afterwards will go on in devouring each other ; but before the days of the Professor we had yet to learn that we devoured ourselves !

PART III.

G

The

* Vide Dr. Hunter's Comment. † P. 144.

‡ Sheldon on the Absorbent System, p. 29.

The names of the respectable authors whose ideas did not reach, for want of anatomical science, that exact *acmé* which the Professor thinks he has arrived at, in order to account for bubo and to cure bubo, are Heister, Astruc, Cowper, Drake, Boerhaave, Freke, Gataker. When he comes down to Chapman's second edition, 1770,* there he stops, for then Dr. William Hunter had taught enough; for he says, at that time the knowledge of lymphaticks being the system of absorbents, was generally known. It is only necessary for my argument to prepare my reader's attention to two objects. What was known of the lymphaticks by the most eminent anatomists when these authors wrote; and what of that knowledge they have displayed in their several publications. By these, it will appear to my readers whether they failed of the necessary information both to account for bubo, as well as to cure it; and whether the boasted pre-eminence of the Professor in both these objects, respecting the cause and cure of bubo, be true or false.

I shall

* The first edition was in 1755. If there be nothing new advanced in the second edition, why did the Professor quote the second instead of the first? The intention is obvious.

I shall, for the general history of the absorbent system, refer my readers to Sheldon, and for the knowledge of that system at the period that Haller took it up, to the works of Haller. The former begins to be universally known; his genius and candour my pen can only permit me to admire, it can neither adorn nor exalt. The fame of the latter is already fixed. A Tour to the Continent made by Dr. William Hunter, whose industry and professional love of fame would never permit him to be outrivalled, qualified him with all the knowledge of the absorbent system that was then taught. He brought home all the improvements that were there made. His lectures in London were enriched by them. The Doctors Monro did the same in Scotland. The concealed ground of their disputes is thus proved,* that as they each boasted of the same ideas and at the same time, so each had imported them from the Professors on the Continent.

The only way to ascertain what Astruc knew, is by what he has written; for the Professor positively, but I will prove, falsely
 G 2 says,

* Vide Dr. Hunter's Commentaries.

says, "That his ideas are become now unintelligible." Astruc says that a cause of venereal bubo is "from the venereal infection
 " just admitted, which being absorbed in
 " certain parts, is carried with the reflux
 " lymph into the glands, to which that
 " lymph is determined by the laws of
 " the circulation."* A stronger definition of the present known idea, of the infection being conveyed by the absorbents, cannot now be given by the most eminent logician in the world. What the Professor quotes from Drake, to prove that he knew not enough of the system of lymphatics, he found in Turner.† Turner quotes that from Drake, to prove that he knew the true cause how bubo was produced from absorbed virus, which the Professor has taken out of Turner to prove the contrary. He begins where Turner begins the quotation, and ends where Turner ends it. Here I shall drop his literary fame, and proceed to something else, first observing, that when I come to his eminent method of cure, I shall compare that also, by referring back again to those authors.

The

* Vide Astruc, book III. p. 338.

† Page 92, Comment. on Astruc.

The Professor calls “ every abcess in the
 “ absorbing system, whether in the vessels
 “ or glands, arising in consequence of the
 “ absorption of venereal matter, a bubo.
 “ This matter, when absorbed by four dif-
 “ ferent surfaces, which are common sur-
 “ faces, wounds, inflamed surfaces, and ul-
 “ cers, is carried along the absorbent ves-
 “ sels to the common circulation, and in its
 “ passage often produces the specific in-
 “ flammation in these vessels; the conse-
 “ quence of which is, the formation of
 “ buboes, which are venereal abcesses ex-
 “ actly similar in their nature and effects
 “ to a chancre, the only difference being in
 “ size.” The Professor also says, “ That
 “ as this system of vessels may be divided
 “ into two classes, the vessels themselves,
 “ and their ramifications, and convolutions,
 “ called the lymphatic glands, he shall fol-
 “ low the same division in treating of the
 “ inflammation.” And it is my duty to
 wait upon the Professor, in order to deter-
 mine what is original, what is profitable,
 and what is preferable to the knowledge and
 practice of those who have already gone be-
 fore him, and also of his contemporaries.
 And here I cannot help remarking, that the
 Professor has, throughout his theory on
 bubo,

bubo, and especially in page 261, borrowed from Astruc, in his section the sixth, treating “ on inferences drawn from the etiology “ of buboes.” Let any of my readers compare the two, and they will find my assertion to be true. Notwithstanding Astruc’s “ ideas “ be now almost unintelligible,” yet the Professor discovers enough of the virtuoso to expound a good thought in him, in spite of the rust with which it is enveloped. Astruc tries to account for the irritation of external glands by venereal virus. “ External glands,” says he, “ are more exposed to cold air ; besides, the internal glands are guarded from “ blows, attrition and pressure ; by the reverse of which circumstances, we see the “ lymph is frequently excited to make a “ descent upon the external glands.”

“ The Professor says, “ That he has seen a “ chain of these buboes, or little abscesses, “ along the upper part of the penis through “ its whole length.” And so have other practitioners and other authors. But this is the point which I want my readers to attend to, whether Astruc, whom the Professor has charged with being now unintelligible from want of knowledge of the lymphatick system, has

has not only seen them but accounted for them also. " Sometimes," says Astruc, " they
 " are extended into scirrhus chords. If the
 " chancres which they succeeded were in a
 " continued series, the vessels are turgid
 " with the collected lymph; therefore the
 " thinner parts of the lymph will be
 " obliged to transude from the force of the
 " pressure."* But Turner, in his Commentary on Astruc, explains this as clear as demonstration can make it. " Cylindrical
 " chords, extending from its apex along the
 " dorsum penis to the pubes, made up of
 " the lymphatick vessels, hardened by the
 " lymph inspissated therein, and feeling like
 " a piece of whipchord, as Astruc rightly
 " demonstrates them, small chords."† What says the Professor more or better than this?
 " The thickening, or the formation of this
 " hard chord, probably arises from the
 " thickening of the coats of the absorbents
 " joined with the extravasation of coagulable lymph." And yet we must swallow the belief that " the ideas of Astruc are
 " now unintelligible;" then so are Turner's,
 so

* Page 378, book III. ch. 8

† Turner's Comment. on Astruc, p. 73.

so would Pott's have been on the Decay of the Testicle,* and so would Camper's, and so would many others now living, if the Professor's illiberality were not constrained from that very cause, and if it had not actually been constrained ! But the Professor, like the old obstinate Knight in Rabelais, swallows a chimera every morning for his breakfast !

Page 263. Chap. III.

Of the Inflammation of Buboes, and the Marks that distinguish them from the Swellings of the Glands.

I shall only attend to buboes ascertained to be venereal. “ The true venereal bubo, “ in consequence of a chancre, is most commonly confined to one gland.” This I do not think is true ; but as it is matter of opinion only, there let it rest. “ It keeps “ nearly its specific distance till suppuration “ has taken place, and then becomes more “ diffuse.” I know that this is not true ; for, when suppuration does take place, the
tumour

* Vide Second Part of Observations, p. 144-5-6-7:

tumour which was previously diffused, now begins to be concentrated ; more liberty is given to the circumjacent inflamed parts ; the vessels are unloaded, and the bulk of the tumour grows softer, and diminishes of course.

“ It is rapid in its progress from inflammation to suppuration and ulceration. The

“ suppuration is commonly large for the size of the gland, and but one abscess.

“ The pain is acute ; the colour of the skin where the inflammation attacks, is of a

“ florid colour.” Here the Professor stops in his description of a true venereal inflammatory bubo. But I believe, if I were to give

an outline from nature ; if I were to take the whole of the symptoms arising from inflammatory bubo into my description ; if I

had no other intention, but that of laying before my readers, not only the truth, but also the whole of that truth ; if I had no

view in concealing that which would not answer my interested purpose to disclose, that

I should have acted more the part of an honest man, I should have discovered more

of true knowledge, I should have been exposed to fewer exceptions, I should have run

less risk of being betrayed by the suspicion and judgement of my readers, than the Pro-

essor

PART III. H

fessor has, by paying some regard to the violence of the symptomatick fever, to the profuse night sweats, which reduce the patient often so low, that a spectator who did not know the cause would be apt to suspect, from the apparent condition of the patient, from the quickness of his pulse, and from his great inquietude, that he was in the most imminent danger. Why has not the Professor also given us a case in point, of the shortest time that he has known a bubo from inflammation come to abscess? The Professor, who has discovered such a minuteness, such a nicety in other subjects, to become of a sudden so slovenly in this! He, who calculates gonorrhœa, calculates chancre, calculates mercury, makes no calculation upon the rapidity of bubo coming to abscess! Does not the Professor know that these circumstances attending bubo, superadded by me, do obtain? And has he not a design in concealing of them? That will, I believe, come out hereafter to be the truth, upon trial.

“ I have known cases, when the venereal
 “ matter, like a cold in fever, has only irri-
 “ tated the glands to disease, producing in
 “ them scrophula, to which they were predif-
 “ posed.

“ posed. In such cases the swellings com-
 “ monly arise slowly, give but little pain,
 “ and seem to be rather hastened in their
 “ progress, if mercury is given to destroy the
 “ venereal disposition. Some come to sup-
 “ puration while under this resolving course,
 “ and others are so indolent, that mercury
 “ has no effect upon them, and in the end
 “ get well, either of themselves, or by other
 “ means, which I imagine may have induced
 “ some to think that buboes are never vene-
 “ real.” He who sat out to write a book
 upon the leading principle of new opinions,
 should have at least guarded that principle
 by a closer attention to uniformity than has
 fallen to the lot of the Professor. This last
 quotation applies against him, both with re-
 ference to past assertions, as well as to future;
 but the past I shall only observe upon here.
 The former logick of the Professor, which
 he applied in treating on gonorrhœa, again
 rises up in judgement against him, and haunts
 him here, as it did on chancre, “The great va-
 “ riety of injections given, and every inflam-
 “ mation getting well during their use, are
 “ strong corroborating circumstances in fa-
 “ vour of the opinion that every such com-
 “ plaint will in time cure itself.”* Why has
 the

the Professor deserted this logick? Why does he not favour the opinions which have induced “ some to think that buboes are never venereal,” and that they will in time cure themselves, because of “ the great variety” in their treatment! “ *Nil fuit unquam sic impar sibi.*”

But I trust that the Professor, although the bubo hath not totally disappeared, does not mean to infer, that, in such an instance, had he known what would have been the event, he would not have given mercury at all. For a bubo so conditioned as he describes the above, certainly was as much venereal, as the more inflammatory bubo; and as it is owing to the idiosyncrasy of the person infected, and not to the different quality of the virus, that the bubo is more acute in one and sluggish in another, so in both, the bubo is the effect of venereal virus, and so in both, must the cure be performed by mercury.

Page 270. Chap IV.

General Reflections on the Cure of Buboes.

I shall set out in perfect harmony with the Professor, for supporting the same opinion, “ That when it is well ascertained, “ that a bubo is venereal, resolution is certainly to be attempted, if the bubo be in “ a state

“ a state of inflammation only.” And here we agree again, “ That the resolution of “ these inflammations depends principally “ upon mercury.” So far we accompany each other, but no farther. We are now arrived to that point in our journey where the road to the same place divides ; the Professor takes one, I the other. And notwithstanding his long stories, so pleasingly told, might serve to beguile the time, yet I must resist such charms of seduction for the object of exploring the truth, and yield up the gratification of good fellowship for the accomplishment of my end!

The Professor says, that the resolution of these inflammations depends not only upon mercury, but “ almost absolutely upon the “ quantity that can be made to pass through “ them ; and the cure of them, if allowed “ to come to suppuration, depends upon the “ same circumstances. The quantity of mercury that can be made to pass through a “ bubo, depends principally upon the quantity of external surface for absorption beyond the bubo.” What does he mean by “ beyond?” “Mercury is to be applied in the “ most advantageous manner, that is, to “ those

“ those surfaces by an absorption, from
 “ which it may pass through the diseased
 “ gland; for by destroying the disease, there
 “ the constitution has less chance of bei g
 “ contaminated. The powers of mercury
 “ may often be increased from the manner
 “ in which it is applied. In the cure of
 “ buboes, it should always be made to pass
 “ into the constitution by the same way
 “ through which the habit received the poi-
 “ son; and therefore, to effect this, it must
 “ be applied to the mouths of those lym-
 “ phatics which pass through the diseased
 “ part, and which will always be placed in
 “ a surface beyond the disease. But the situ-
 “ ation of many buboes is such as not to
 “ have much surface beyond them, and there-
 “ by not to allow of a sufficient quantity of
 “ mercury being taken in in this way; as
 “ for instance, those buboes on the body of
 “ the penis, arising from chancres on the
 “ glands or the prepuce.”——“ It is first to
 “ be observed, whether the absorbent ves-
 “ sels on the body of the penis are affected,
 “ or the glands in the groin. If the disease
 “ be in the groin, it must be observed in
 “ which of the three situations of the bubo
 “ before taken notice of, it is; whether on
 the

“ the upper part of the thigh and groin, on
 “ the lower part of the belly, before Pou-
 “ part’s ligament, or near to the pubes. If
 “ they are on the body of the penis, this
 “ shews that the absorbents leading directly
 “ from the surface of absorption, are them-
 “ selves diseased. If in the groin, and on
 “ the upper part of the thigh, or perhaps a
 “ little lower down than what is commonly
 “ called the groin, then we may suppose it
 “ is in the glands common to the penis and
 “ thigh. If high up, or on the lower part
 “ of the belly, before Poupart’s ligament,
 “ then it is to be supposed that those absor-
 “ bents of the penis and skin about the pu-
 “ bes pass that way. The knowledge of
 “ these situations is very necessary for the ap-
 “ plication of mercury for the cure by reso-
 “ lution, and for the cure after suppuration
 “ has taken place.” Whilst the genius of
 the Professor were thus upon the wing, I
 could have wished it had flown a little far-
 ther before that it had settled. I could have
 wished that it had not found a resting place,
 till it had asserted that buboes were never
 dissolved nor cured before this great informa-
 tion came forth to the world ! I should now
 hasten with alacrity to examine, to satisfy
 myself,

myself, to make up my mind, and to impart to my readers the high advantages that were to be gained from these wonderful effects of his genius, to hold out objects worthy of him who designed them, and to join in the fullest admiration of their merit, if I conscientiously could; but as I cannot, I shall proceed reluctantly to produce my arguments from which, without fear, I am induced to condemn them. For it is a maxim with me to reason thus: That the man who holds out to me a false and delusive hope, flatters my passion at the expence of my understanding, which, as soon as I detect, I detest.

Page 273.

1. *Of Resolution of the Inflammation of the Absorbents on the Penis.*

Here the Professor says, “ that the surface is not large enough to take in a quantity of mercury sufficient to prevent the effects of absorption, and therefore recourse is to be had to other means. Yet this application is not to be by any means neglected.”——“ As this surface is too small, and as it is necessary that a larger quantity should be taken in, it becomes
“ proper

“ proper to give it either by the mouth, or
 “ by friction on some larger surface.”

Then at any rate, the new project in this instance is confessedly, from its inventor, to yield to the old plan of cure. And now let us examine how near to this plan of cure recommended through necessity by our celebrated Author, the “ unintelligible” Astruc approaches, and in the cure of this very venereal condition! “ But I would have it
 “ remarked,” says Astruc, “ that plaisters,
 “ by virtue of the sharp discutient medicines,
 “ of which they in part consist, and from
 “ their glutinous and sticking quality, by
 “ which they stop up the pores of the skin,
 “ and obstruct perspiration, frequently bring
 “ a morbid heat upon the part; and for these
 “ reasons, less safe than mercurial un-
 “ tions.” * The Professor has said, and
 falsely, that ’till lately, the method of treating
 buboes was to apply plaisters to them. Thus also
 Turner in his Commentary on Astruc: “ As they arise gradually from in-
 “ spissated lymph, so also they disappear, at
 “ least by the help of a little unction and a
 “ course of mercury, unless complicated

PART III. I with

* Astruc, p. 381, chap. v. ib. iii.

“ with some other symptoms which may re-
 “ quire a stricter regimen.”* Last of all,
 I will appeal to the Faculty, from the oldest
 to the youngest, whether any other method
 was ever thought on, and whether this be
 not the exact practice now in general use,
 and that which has been constantly taught
 from the days of Astruc to the moment
 of my writing.

Page 274.

2. *Of the Resolution of Buboes in the Groin.*

“ The inflammation of these glands, is to
 “ be treated exactly upon the same principle
 “ with the others; but we have in general a
 “ larger surface of absorption, so that we
 “ can make a greater quantity of mercury
 “ pass through the diseased parts.”---“ The
 “ length of time the friction should be con-
 “ tinued, must be according to circumstances.
 “ If the bubo gives way, they must be con-
 “ tinued until it has entirely subsided, and
 “ perhaps longer, on account of the cause of
 “ it, a chancre, which may not yield so soon
 “ as the bubo. If it still goes on to suppu-
 “ ration,

* Turner on Astruc, p. 73.

“ ration, the frictions may, or may not be
 “ continued ; for I do not know for certain
 “ if any thing is to be gained by their conti-
 “ nuance in this state. The quantity here
 “ recommended may affect the mouth, and
 “ it must also be regulated accordingly.”

I request my readers particularly to attend to the last quotation. If this method doth not disperse the bubo, why then it suppurates ; and if it should suppurate, the Professor does not know whether mercury should be gone on with. But notwithstanding the Professor's new intention of cure is by a remedy locally conducted to a local disease, it must be curious, and it is almost ridiculous to think, that the Professor should recommend so strongly this important new opinion, and yet dare not trust to it ; for the mouth must be affected, and the bubo may still come to suppuration, and the method pursued after must be “ regulated accordingly.” I suppose accordingly here must mean “ accordingly.” -- It certainly is a word full of minute instruction ! If such is to be the result of this promised improvement, the novelty is all a fiction, an unprofitable conceit ! Astruc and Turner did as much, and as well. They took surface enough,

I 2

they

they applied “mercury to both thighs,” and
 “Turner also to the legs.” They made the
 mouth fore; some of the buboes under their
 care were dispersed, and some came to ab-
 cess; some were fluggish, and some were
 acute.

Page 276.

5. *Of the Quantity of Mercury necessary for the
 Resolution of a Bubo.*

“ If the reduction is obstinate, the mer-
 “ cury must be pushed as far as can be done
 “ without producing a salivation.”---“ If
 “ there be a bubo on each side, in such cases
 “ we must not so much mind the foreness of
 “ the mouth, as when there is but one in the
 “ second and third situation of buboes. If we
 “ find that most probably a sufficient quan-
 “ tity of mercury does not pass through them
 “ for their resolution, it may be continued
 “ to be thrown in by the leg and thigh, to
 “ act upon the constitution, as has been al-
 “ ready observed.” In other words, if this
 conceit of mine does not succeed, do not de-
 pend upon it, but have recourse to that which

is

is known never to fail, and which every body is already in full possession of.

I have been thus profuse in my quotations from the great work, to the end that my readers may become perfect masters of the point at issue betwixt the Professor and me, or rather betwixt the Professor and all the Faculty in the world. For it cannot be concealed, that I had all along the following quotation from the Professor in my view; and now I think that I am prepared to meet this declaration of the Professor, and I hope that I have not been wanting in preparing the minds of my readers for the reception of it also; for a more serious assertion, as far as it relates to the reputation of an author, never yet came from the pen of a man!

“ This method of resolving buboes occurred to me at Belleisle, in the year 1761, where I had good opportunities of trying it upon the soldiers; and I can say with truth, that only three buboes have suppurated under my care since that time, and two of them were in one person, where a small quantity of mercury had considerable effects on the constitution, and therefore a sufficient quantity could
“ not

“ not be sent through the two groins for
 “ their resolution; but in both cases the
 “ suppurations were small in comparison to
 “ what they threatened to be, which I im-
 “ puted to the mode of treatment.”

Here I think it will be necessary for us to pause awhile, to reflect before we give judgement upon this pledged fact, to examine ourselves upon its probability, to suffer the naked independent assertion to rest upon its own validity, to put its author out of the question, to call back our attention to attempts that we ourselves have made to disperse some rapid inflammatory buboes, to ask ourselves, whether we think, that if we had the same cases before us again, we now should be enabled certainly to succeed, where, in spite of our best endeavours, we before had failed in dispersing of certain buboes, provided we now followed this very system which the Professor so glories in! And I say so far for myself, that if I could be brought to believe that so great was the success attending this practice of the Professor, and that he could say with truth, that only three buboes from under his care had suppurated since the year 1761, and indeed

indeed these three are no exceptions, for he could not apply that requisite power to disperse them which was necessary, therefore I consider them as no exceptions, I would be foremost directly in declaring that this one single act of science by far atoned for all his more material errors ; I would instantly cease to make another observation upon his wrong opinions ; I would employ my pen in gratitude and praise of so much genius, so profitably exerted ; and I would be proud to grace his triumph, and to assist in placing the laurel on his brow.

But if on the contrary, it should hereafter appear, that the constant practice has been to choose the surface on the same side with the bubo, to administer there the ointment, where the violence of the fever did not forbid it, to push the friction to an extremity ; and if it should be notwithstanding self-evident to the knowledge of every other practitioner but of the Professor, that all buboes cannot be dispersed even where mercury can be used with freedom, that the same means shall be employed to two buboes in their earliest stages, that these means shall be exerted in the most pressing manner,

ner, that even merely to indulge an idea that promised nothing, but in order to put it to the test, that even all these little little-nesses dwelt upon by the Professor have been followed in the exactest and nicest manner; I say, if this should appear, (and I know that it can be made to appear) and after all, if that buboes do in the same proportion continue to come to abscess, then let the Professor look to himself, then let him look back to what he hath pledged himself to, and to what he hath lost! Has he not already provided us with instructions for treating buboes that resist this power of his? And has he not declared already that “some come to suppuration while under this resolving course?” Then let him take care that in no future part of his work he does not further contradict this solemn declaration; that in future passages we find no cases of buboes under his care coming to abscess, but three; for if it should hereafter be proved that there are instances to the contrary, he surely, who has been so regardless of his own honour as to sport with it in such wanton looseness, cannot expect that others who sit in judgement upon it, will conceal what he has taken so much pains to expose.

We have been already told, that all gonorrhœas cease without medical help ; we have been told of the safe application of caustick in all instances whatsoever ; we have been told of harmless connections in a state of disease ; we have been told that extravagant recommendations of any particular remedy are not without their use ; we have read a chapter on onanism ; we have been told that this very book hath been forbidden abroad, and we know that it has gained no profelytes at home : considering therefore this declaration from the Professor as it stands alone, I positively declare that I do not believe it ; and, taking in other circumstances that he stands as strongly pledged upon, if I had my doubts before, these and the general disregard which he has throughout shewn to uniformity, would make me fling it out of my belief altogether.

Let us for a moment examine what it is that the Professor does, (now that we have seen what it is that he has said) to procure such extravagant success. Is it by pushing mercury through a gland, inflamed and irritated by venereal virus ? So far it only appears, that he is applying a local remedy to a local affection. But does he know that mercury

will pass through this gland ? Has he proved to us that it will ? When he boasts of the power of passing mercury through the diseased gland, and when he boasts, that by this method, he disperses the bubo, does he himself trust to it altogether ? When he talks of taking a large surface, does he mean to insinuate that he, at the same time, intends only to push the mercury through the diseased gland ? Supposing that every gland on one side were absolutely in a state of inflammation, so that the mercury could not pass into the habit, but through diseased glands, does the Professor conceive that mercury would not find more difficulty in passing into the habit, than it would, if some of these glands had been in a sound state ? But these are the material questions I mean to put to the Professor. He says that a bubo is local, and I agree to it. If he finds that the application of mercury directly through the gland dissolves the bubo, why does he do, as we are all in the practice of doing ; why does he take a large surface ? Why the thighs and the legs ? Why does he take the chance of passing the mercury through the sound glands, as well as the diseased ones, which we do ? Why does he push mercury as much, if not more than we do ?

do? Why does he make the mouth fore, as well as we do? Why does he talk of local power, and not trust local power? Why does he boast of the pre-eminence of his local theory, and give us its success in words, when he dares not rely upon it? And why does he follow in deeds that other method, to obtain the same success in practice, which we all do?

Here it is accounted for, why the Professor was so abrupt in his description of inflammatory bubo; and here it is also accounted for, why he concealed the shortest time that a bubo may come to abscess; and why he sunk altogether the impossibility of rubbing in mercury, on account of the violence of the symptomatick fever. "I know this man better than any of you."*

Page 277.

7. *Of the Treatment of Buboes when they sup-
purate.*

"After every known method has been
"used, buboes cannot in all cases be re-
"solved, but come to suppuration." Does
not this assertion come too close after a former? Others may be astonished, but I am not. "Buboes must be allowed to go to
sup-

* Junius.

“ suppuration rather than affect the constitution too much by mercury.”—“ Some buboes come to suppuration whilst under a resolving course.” *

“ It may admit of a dispute, whether the application of mercury should be continued or not through the whole of the suppuration?” I think that the Professor is less able to answer this question than any man; he who can and hath dissolved every bubo but three. With respect to opening the bubo, that the Professor very kindly leaves to those who suffer them to come to abscess. The Professor tells us, that he once opened two buboes in the same person, the one with caustick, the other with lancet, and that the patient preferred the former. “ Giving mercury in these cases answers two purposes; it assists the external applications to cure the buboes, and it prevents the effects of the constant absorption of the venereal matter from the fore.”

Page 280. Chap V.

Of some of the Consequences of Buboes.

“ It sometimes happens that these sores when losing, or entirely deprived of the
“ venereal

“ venereal disposition, form into a fore of
 “ another kind, and most probably of va-
 “ rious kinds. How far it is a disease
 “ arising from a venereal tint, and the
 “ effects of a mercurial course jointly, is
 “ not certain, but most probably these two
 “ have some share in forming the disease.”
 “ I am apt to suspect something scrofulous
 “ in these, especially as they are diseases of
 “ the lymphatic glands.” By this little spec-
 imen which I have laid before my readers,
 it must obviously appear to them that the
 Professor, with all his pretended penetration,
 with all his comparative powers, with all
 his borrowed knowledge, obtained by con-
 ferences and correspondences, is as much
 at a loss as ever man was, to account for
 these appearances not only in bubo, but
 also in chancre. For the Professor hath al-
 ready avowed, and in that I agree with him,
 that these ulcers, so conditioned, are the
 same in their nature, both in chancre and
 bubo. “ Buboes are venereal abscesses exactly
 “ similar in their nature and effects to a
 “ chancre, the only difference being in
 “ size.”* Having ascertained so much, I
 shall make a few observations, in order to
 give my readers the opportunity of judging
 whether

whether the Professor be serious when he calls these appearances “ a new disease ;” or whether also he has acquired reputation for acute intuition, by suspecting that this “ new disease is a scrofulous disease.” I am aware that this is a very essential enquiry, and which, since the time of Astruc, hath been much overlooked. When we have obtained a true knowledge of a case which can only even be palliated, that knowledge not only directs us to the fitting remedy for palliation, but hinders us from applying random remedies, and saves us also from exposing ourselves to those, who, from having better memories, catch us in palpable contradictions. The Professor is thus detected and caught, his theory is contradictory, and his remedies are contradictory. He very emphatically calls this “ a new disease,” because he has not talents sufficiently comprehensive to embrace general circumstances, and to draw such an inference from them altogether, that will, by combination, form an extensive chain, and give an assurance of the nature of this stage of the venereal disease, which baffles the power of mercury, and threatens the loss of parts, or even life of the patient. I
will

will first endeavour to prove to my readers, why they ought not to think that it is scrofulous, and why the Professor does not think so, and why he does, for he thinks both. He has assigned as a reason for suspecting this new disease to be scrofulous, because its seat is in the lymphatick glands; and he has already confessed, that buboes and chancres thus conditioned, are the same. Then I will ask the Professor, whether chancres are seated in the lymphatick glands? Does the Professor say, that when mercury ceases to do good, that the venereal disease is at an end? Yes. Has not the Professor given us a case of a gentleman from Ireland, to prove that such complaints do well without mercury? * Yes. Has not the Professor asserted, that mercury is a specifick against chancre and bubo, and that it will cure every one that is truly venereal? † Yes. Does the Professor, when mercury ceases to do good, and the ulcers are still daily growing more and more alarming, say, that the venereal disease is not at an end? Yes. “ Such diseases make the
 “ cure of the venereal much more uncertain,
 “ because when the sore becomes stationary,
 “ or the mercury begins to disagree, we are
 “ ready

* Page 247.

† Page 230.

“ ready to suspect that the virus is gone ;
 “ but this is not always the case ; the virus
 “ is perhaps only less powerful than the new
 “ formed disease, and as it were, lies dor-
 “ mant, or ceases to act, and when the other
 “ becomes weaker, the venereal begins to
 “ shew itself again.” Has not the Professor
 given us a case to prove also this last opi-
 nion ? Yes. Was it not of a gentleman who
 had a gonorrhœa, and in a fortnight after,
 under the Professor’s own care, had two bu-
 boes, which two buboes came to abscess under
 his own care, (this must have been, I presume,
 before the year 1761) and which abscesses,
 under his own care, assumed all the malig-
 nant appearance which he calls a “ new
 “ disease ?” Yes. And has not the Professor
 told us, that in this case he suspended mer-
 cury, but did not entirely throw it by ; that
 in process of time, when he suspected that
 the new disease was decamped, and that the
 venereal was returned, he returned to mer-
 cury again, and that then the buboes got
 well ?* Yes. What are the remedies which
 the Professor resorts to for curing this new
 disease ? Hemlock, salt water poultices, sea-
 bathing, opium. Has he not given us a case
 of

of a gentleman under his care, who took hemlock till he dropped dead from his chair ?*

Yes. Are hemlock, opium, and sea-bathing consistent or compatible with each other ?

No. Does hemlock act as a corroborant ?

No. And do not sea-bathing and pure air produce all their good effects from their being corroborants ? Yes. But when these cases are acute, for the Professor defines them to be both scrofulous and acute, can a poor patient set off at all times of the year, with his penis half eaten off, to bath in the sea ?

Would the Professor give over a patient, and send him to the coast, so conditioned, in the winter months ? Does the Professor, because the cold sea-water, to bathe in, is perhaps the best of all restoratives, from analogy presume, that a hot poultice made with it and bread, will convey to the ulcerated parts that quality ? What does he mean ? Has he any meaning ? Either all these questions are necessary to ascertain his meaning, or there is no meaning in a single assertion that he has advanced ! So much for the new disease ! So much for the gentleman from Ireland getting well of it without mercury ! So much for the Bellisle infallibility ! So much for gonorrhœa

curing itself, and so much for the new disease doing well with mercury, in the case of his own patient, whose gonorrhœa, from under his own care, produced buboes, which buboes produced suppuration, which suppuration produced ulcers, which ulcers produced some yet more malignant ulcers, and which malignant ulcers produced “ a new disease !” And so much for the patient who was sent to the sea for the new disease, who left off mercury, who returned from the sea, who arrived in town, and who returned to mercury to cure the venereal disease, which returned again also ! “ This is the house that “ Jack built !”

As these ulcers spread under the use of mercury, and whilst mercury is pushed to a great extremity, as mercury proves to be the most general cause of these malignant appearances, it is of the first importance for us to consider what are the fittest means for their cure. That they are not scrofulous, I am convinced, as they will be found on parts not glandular, as well as glandular ; on subjects never disposed to scrofula at any time of life. I can readily reconcile to my reason, that they assume all the qualities of ulcers which
are

are termed highly scorbutick. Mercury administered with freedom on constitutions pre-disposed to a scorbutick affection, will accelerate and promote that pre-disposition. That the action of mercury upon the blood, tends to destroy its healthful texture, and to debilitate the habit, it has been long insisted upon ; but a late author has very satisfactorily elucidated this subject.* That mercury, applied to subjects so conditioned as I shall hereafter enumerate, may accelerate a disposition in the constitution exactly similar to, if not the scurvy itself, I can very readily conceive. From mercury given to a hectic person, to a person at sea, to immoderate drinkers, to such as commit excess in venery, where by any of these causes the constitution is debilitated, no benefit is to be expected, but on the contrary, an aggravation of the disease, as is known from experience. It appears to me as satisfactory as reason can make it, that mercury is necessary to promote a disposition truly scorbutick, and that the constitution, in such a state, is exactly as feeble, in a situation as much to be dreaded, and similar in every sense, to the state of a sailor returned from a long voyage, upon whom

* Howard on Mercury.

the scurvy hath made rapid advances. The obvious remedies are such as tend to correct the scorbutick habit, having at the same time always in view the effect of venereal virus upon the constitution; for the venereal virus may not be destroyed, and it has seldom or never been found to have been destroyed. The remedies are bark, elixir of vitriol, sarsaparilla, opium, oranges, malt tea, spruce beer, milk, and vegetables, cold bath when the patient can bear it. To topical applications we should be encouraged, by the experience of their success: When I saw these appearances coming on in buboes, I should directly apply a tight bandage, with the same view that I would to a foul ulcer in any other depending part. On the groin this may be done, and as I have already tried it, I can speak to its good effects. The dressing which I then used was lint, dipped in a solution of vitriol with rose water. Mercury should be also administered in small quantities, as soon as the condition of the patient will permit it.

Page 287. Part VII. Chap. I.

Of the Lues Venerea.

WHEN I first began upon the subject of the great work of the Professor, my readers may recollect I promised them that I would make my observations upon none other of the new opinions, but those which were carried perniciously into practice. I wish that I could, in future, adhere closely to this promise ; but I find that these material errors are more abundant than I could have expected. On account of my limited condition, I must be content to remark upon the most important of these material errors ; following thus a practice that Mahomet pursued, who decimated the infidels whom he could not reform. Besides, the Professor himself has served me very essentially in many instances ; for when I find some fantastick opinion in one page, I find, in another, a case applied to another purpose directly contradictory to that fantastick opinion, and which makes it at once to vanish from our belief, as the sun dispels those imaginary figures which are formed in a clouded sky.

Th^c

The Professor sets out with stating, how the virus gets into the constitution, which is in general, he says, “ from gonorrhœa or
 “ chancre, the matter being absorbed from
 “ them, and carried into the constitution.”
 —“ But when applied to some particular
 “ parts of our body, such as may be called
 “ a half internal surface, as the glans penis,
 “ the matter appears to be capable of being
 “ taken into the constitution, without first
 “ having produced either gonorrhœa or
 “ chancre; as when a bubo appears without
 “ a previous appearance of either gonorrhœa
 “ or chancre,” So far the Professor and I agree,

“ I think it is probable that it is not ca-
 “ pable of being taken into the absorbents of
 “ the sound skin.” I am not more assured
 of any existing fact, than that it is capable
 of being taken in by the absorbents of the
 sound skin. The Professor says, “ at least
 “ he knows no instance of it.” Any part of
 the body, where the matter has been per-
 mitted to soak, may produce a chancre; the
 common skin of the penis, the pubes, the
 scrotum, the thighs being most exposed, we
 find oftener chancres there. But how was
 the

the opinion of the Professor upon this point, in another part of his work ? “ Any part
 “ of the body may be affected by the appli-
 “ cation of venereal matter, especially if the
 “ cuticle is thin :”* And then follows a case of chancre on the prolabium. But in that very page he contradicts that very instance.
 “ But as the cuticle cannot be affected by
 “ this poison, this covering acting as a guard
 “ to the cutis, it is often prevented from
 “ coming into contact with it.” And in the very next page he says, “ Its affecting these
 “ parts arises from the manner in which it is
 “ caught, and not from any specific tendency
 “ these parts have to catch it more than
 “ others.”†

“ The frænum, or the termination of the
 “ prepuce, are parts more easily affected than
 “ either the glans, common skin of the penis,
 “ or scrotum.” At least he here admits, that the common skin may be affected.

I shall permit this question to rest where it ought, when I have produced one more quotation. “ If the venereal poison should
 “ be applied to the skin where the cuticle is
 “ more

* Page 217.

† Page 218.

“ more dense than that of the glans penis,
 “ or frænum, such as that upon the body of
 “ the penis, or fore part of the scrotum,
 “ parts which are very much exposed to the
 “ application of this matter, then it gene-
 “ rally appears first in a pimple, which is
 “ commonly allowed to scab.”*

It now remains with my readers to form
 their opinions, whether my positive assertion
 be true, or whether the Professor's doubts and
 assertions, first of all produced, appear to
 them to be nearest the truth ; or whether
 the doubts and assertions next produced, in
 direct opposition to his first, and strongly
 according with my opinion, shall be by them
 adopted : Whether my opinion, the Profes-
 sor's first opinion, which is against mine, or
 the Professor's second opinion, which is with
 it, shall be their standard of faith.

“ It is likewise capable of being taken into
 “ the constitution by being applied to com-
 “ mon ulcers, although not necessarily ren-
 “ dering these ulcers themselves venereal ;
 “ also by wounds, as has been observed, but
 “ I believe always previously producing
 “ ulce-

“ ulceration in the wound.” What has the Professor formally said about wounds ? “ I am inclined to think that wounds are bad absorbing surfaces, especially when I consider that few morbid poisons are absorbed by wounds.* How it is possible for him to suspect that an ulcer will receive the venereal matter, be its passport into the habit, and not become a venereal ulcer, I cannot yet find out: This mystery further on may be cleared up. To distinguish, *prima facie*, when an ulcer is venereal or not on any part of the body, I know he cannot ; and I would prove it, if it were not too obvious in every page of his work. How then does he pretend to know, or at least suspect, that an ulcer not venereal, may absorb venereal virus, and yet not itself become a venereal ulcer. He might as well assert that an ulcer can convey that quality which it never possessed.

Page 289.

Of the Nature of the Sores, or Ulcers, proceeding from the Lues Venerea.

“ In consequence of the blood being contaminated with venereal pus, it might naturally

PART III. M

“ turally be expected, that the local effects
 “ arising therefrom, would be the same with
 “ the original which produced them; but
 “ from experience and observation, I have
 “ reason to believe that they are not.”---“ If
 “ matter, when in the constitution, was to
 “ act upon the same specific principles with
 “ that which is applied, we should have go-
 “ norrhœas when it attacks a canal, sores or
 “ chancres when it attacked the surfaces; but
 “ it has never been yet known to produce a
 “ gonorrhœa from the constitution, though
 “ this has indeed been suspected.” I can
 scarce believe it possible, that the strongest
 man could exist long enough for his diseased
 constitution to produce those effects which
 the Professor exacts to satisfy his doubts. If
 every part of the body of a person constitu-
 tionally infected, were to produce symptoms
 in the following manner, glands to produce
 buboes, canals gonorrhœas, skin chancres,
 bones caries, it would prove to be a disease
 more formidable than even it now is. In this
 country, where the venereal disease is not in-
 digenous, and where its virulence is in a great
 measure checked by the climate, and where
 every individual suppresses that virulence by
 taking more or less of mercury, the power of
 virus

virus becomes less easy to be ascertained. What its progress might be, and what its effects are in common, these are two different questions. All diseases are governed by specifick symptoms, and these specifick symptoms point out to us what the nature of the diseases are, and instruct us how to class them. Every disease hath its characteristick marks, in the beginning, the middle, and the end. It would be contrary to the laws of nature to look for incipient symptoms at the end of disease. To look for local complaints, which produced the constitutional disease, again to be reproduced out of the constitutional disease, would be absurd indeed, unless disease were like a circle, where every part was a continuation ; or unless a man was to die of the lues venerea, and to rise again with a gonorrhœa ! But does not the Professor know the reason why the local effects are different from the constitutional ? Is it not because the disease is received into the blood through the absorbent system, and thrown again upon the surface through the continuation of blood vessels ?

“ The sluggishness in the effects of the
 “ poison is more or less according to the na-

“ ture of the parts which become diseased ;
 “ for when the tonsils, uvula, or nose, are
 “ affected, its progress is rapid.” Ulcers arising from the lues venerea in the mouth, the throat, and the nose, differ from local venereal ulcers only by their being in general less active, and by the parts not being operated upon with all the force of powerful virus, locally acting upon a person more liable to become irritable, because he has been as yet less irritated, and upon a person sound in every other respect but the part or parts locally infected.

“ It has been supposed that even all the
 “ secretions from the contaminated blood
 “ could be affected so as to produce a like
 “ poison in them.”--“ That the testicles and
 “ vesiculæ seminales may be affected with
 “ the disease ; the semen may become venereal, may communicate the disease to
 “ others, and after impregnation may even
 “ grow into a pocky child : But all this is
 “ without foundation ; otherwise, when a
 “ person has the lues venerea, no secreting
 “ surface could be free from the state of a gonorrhœa, nor could any fore be other than
 “ venereal. Contrary to all which the secretions

“ tions are the same as before ; and if a fore
 “ is produced by any other means in a sound
 “ part, that fore is not venereal, nor the
 “ matter poisonous, although formed from
 “ the same blood.” As it is a settled datum
 betwixt the Professor and me, that the blood is
 contaminated by venereal virus, it is impos-
 sible for him, *prima facie*, to deny that the
 secretions are not in consequence contami-
 nated with venereal virus. But if any one
 secretion be more likely than another to par-
 take of all the qualities of the constitution
 from which it was secreted, it is certainly
 this very seminal secretion, as it contains the
 very essence of that, from which, at that time,
 it was produced. If the constitution were
 then under the influence of the venereal dis-
 ease, it could convey that infection by the se-
 men. If the constitution were under the in-
 fluence of scrophula, or king’s evil, it could do
 the same. Both these, and many other diseases
 of the constitution, we know, are thus con-
 veyed ; and as it is known to be a fact, I shall
 not take up more time to prove it. The Pro-
 fessor appeals to the instance of a mad dog.
 A dog is bitten, a dog is infected, and this
 infection is carried on *ad infinitum* in this
 form. The Professor says that other secretions
 of

of the mad dog are not infectious : How does he know this ? It must have been very lately, for a twelvemonth ago, in a letter to Dr. Hamilton, he declares that he was as ignorant, and as much at a loss to account for this disease, as “ they were a thousand “ years ago.”* But the Professor, as far as my memory tells me, for I have not the book before me, in that very letter declares, that he knew an instance of a mad dog biting twenty persons, and that only one of the twenty became hydrophobick. That this may be true, I do not doubt, because Dr. Vaughan hath said the same.† This will teach the Professor not to deny the existence of a power, because it fails in many instances ; not to rely upon one experiment, when he attempts by that to prove, that venereal ulcers, produced from constitutional infection, do not convey the infection, or that contaminated blood does not either. But how does the Professor really know, that other secretions of a mad dog do not convey the infection ? Did the Professor ever see a mad dog copulate, and has he ever looked after the consequences ? *Risum teneatis !*

“ The

* Vide Hamilton on the Hydrophobia.

† Vaughan's Two Cases.

“ The milk of the breast is supposed to
 “ be capable of conveying the venereal poi-
 “ son, and of affecting the child who sucks
 “ it ; but there are several reasons which
 “ overturn these opinions.” To prove this,
 amongst other reasons he has recourse to the
 following case.

“ A gentleman who had chancres which
 “ discharged largely, used to wash the parts
 “ with milk and water in a tea-cup, with
 “ some lint, and generally let the lint lie in
 “ the cup in the milk. A little boy in the
 “ house stole the milk, and drank it ; but
 “ whether or not he swallowed the lint, was
 “ not known.” This boy was well watched,
 and found not to be infected.

I declare I am at a loss, whether to laugh
 only at the extreme weakness of the Profes-
 sor, or whether to pity it ! Virus, thus
 diluted with milk and water, received into
 the stomach, and there diluted further ; could
 it be expected, or was the Professor amused
 with the idea, that the boy would be infect-
 ed ? Does he not know, that all poisons may
 be so diluted, as to be deprived of their ef-
 fects ? The infant, who sucks a diseased
 nurse

nurse, does not suck in the milk diluted accidentally once, as the boy did, but is constantly supported by this infectious milk, and may be infected.

“ We may observe that the blood of a
 “ pocky person has no power of contami-
 “ nating, and it is not capable of giving the
 “ disease to another, even by inoculation ;
 “ for if it were capable of irritating a sound
 “ sore to a venereal inflammation, no person
 “ that had this matter circulating, or had
 “ the lues venerea, could escape having a
 “ venereal sore whenever he is bled, or re-
 “ ceives a scratch with a pin, the part so
 “ wounded turning into a chancre ; for if
 “ venereal matter had been on the point of
 “ the pin, or on the point of the lancet, the
 “ punctures would have become chancres.”

And does the Professor reason thus ? Does he attempt to prove, that blood contaminated will not convey that infection to a sound person, because a common scratch of a pin or lancet does not degenerate into a venereal ulcer, whenever it is made on the diseased person's skin ? The Professor has been formerly ready enough to admit, that
 venereal

venereal matter did not affect the part that secreted it. But to serve another purpose, to create another new opinion, he finds no difficulty to assert, that, if blood of one infected with venereal virus could contaminate another, it must contaminate itself; that is, if blood be drawn by a pin or lancet, the wound must necessarily degenerate into an ulcer, or the blood is not venereally infected: or, if a person in the small-pox has a puncture made on any part, with a pin or a lancet, and if a pustule does not arise out of it, which it positively will not, then, says the Professor, the infection cannot be communicated to a sound person. Is it so? Shall I not hereafter prove, that the infection may be conveyed by a part even where the disease hath not been active, although the constitution has been infected? Does not the Professor know that I shall be able to prove it? When he reads this, he will know to what I allude! and if I were to look him in the face whilst he reads it, I should be assured that he knew it! for “ hereby hangs a tale !”

2. *Of the Matter from Sores in the Lues Venerea, compared with that from Chancre.*

This may be said to be a continuation of the same subject. The Professor sets out with denying, what he has not dared to bring to the test ; with denying that matter from ulcers, in consequence of lues venerea, will infect a sound person by any means. When the Professor gave credit to the rest of the relations of Wallis and Cook, why did he exclude this from out of his venereal faith ? “ They also say, if a man is infected with it, “ he will often communicate it to others in “ the same house, by feeding out of the same “ utensils.”* When the Professor falsely asserts that these voyagers said, that the disease “ was in every form,”† to serve his own purpose, I beg him only to believe as much as they have said, for mine. I dare him to take the chance of repeated trials by inoculations, with matter taken from persons who have, in consequence of local infection, become constitutionally infected, and whose constitutional symptoms are active and virulent, whose

* Cook's Voyage, vol.ii. p. 148. Dublin Edition.

† First Part of Observations, p. 12.

whose constitutional ulcers spread, and make rapid destruction of parts, and who, for the cure of them, have taken no mercury. I dare him to give this experiment all the scope which I exact; for, is the Professor aware, that, if in one instance only the infection by this process does take place, as in the case of one person out of twenty, from the bite of a mad dog, all his visionary new opinions on this subject vanish for ever. Constitutional ulcers do only so far differ from local ulcers, that they are less active, less inflammatory, less virulent; and that they are so, is known by their more sluggish action, in comparison to the local ulcers. The action of all poisons depends upon their strength. The Abbé Fontana's experiments prove this. The poison of one viper will destroy a small animal, but it requires the aggregate poison of many to destroy a larger. But the Professor, to put this question to the test, has not exhibited one single faint experiment. We have no other authority for believing that constitutional ulcers cannot convey infection to a sound person, than his bare assertion. But he is liberal enough in proving a truth which every body will admit, "that a subject constitutionally infected,

“ may contract a local infection :” And by some unhappy fatality, he has produced a case to prove this fact, which has overfet all his former assertions in this and the last section ; and all those new opinions which I have so lately opposed, this very case has totally rejected and destroyed.

“ A woman, aged 25, came into St.
 “ George’s Hospital, August 21st, 1782, with
 “ sores on her legs, and blotches on her body.
 “ Her husband was a soldier ; he gave her
 “ the venereal disease December, 1781. Her
 “ symptoms then were a discharge from the
 “ vagina, and a small swelling of the glands
 “ of the groin, which were painful. She
 “ had taken some pills, supposed to be mer-
 “ curial, to the number of thirty. Febru-
 “ ary, 1782, about three months after being
 “ infected, the discharge stopped, but the
 “ swelling, which had been gradually in-
 “ creasing ever since its first appearance, had
 “ now suppurated. She applied some oint-
 “ ment to it, which was brought her by her
 “ husband, and in two months it got well ;
 “ that is, in April, 1782. After the bubo
 “ got well, a discharge from the vagina came
 “ on, for which she took more of the same
 “ pills

“ pills she had taken before, to the number
 “ of thirty. After this time blotches came
 “ out over her whole body ; some of which,
 “ about her legs, under her arms, and upon
 “ her nipples, ulcerated. Twins which she
 “ bore at eight months, in March, 1782, at
 “ the same time the bubo was healing, had
 “ blotches upon them at their birth, and
 “ died soon after. Another girl, about two
 “ years old, whom she suckled, was covered
 “ with blotches when she came to the hos-
 “ pital. The mother and the child went
 “ into the Salivation Ward, October 21,
 “ 1782. The child took no mercury. It
 “ was supposed that its gums became a little
 “ sore, and the blotches got well. The mo-
 “ ther got well also.”

Thus do moles, digging under ground,
 turn up hillocks of earth upon the surface,
 not conscious that this very act of blind
 industry instructs their enemies when and
 where to ensnare them ! ! !

3. *Of the local Effects arising from the Constitution, considered as critical---Symptomatic Fever.*

“ I observe, that a gonorrhœa might be
 “ produced by a general law in the animal
 “ œconomy, by which it endeavours to re-
 “ lieve itself of the irritation, by producing
 “ a discharge ; and that in chancres a breach
 “ is made in the solids for the same purpose,
 “ although this purpose is not answered in
 “ either, nature not having made a provision
 “ against poison.” This single paragraph is
 a text for a comment of a large volume, in
 order that its author may be degraded as he
 deserves. The very animal œconomy is
 wrong, but yet the Professor is right. Na-
 ture is to blame, but yet the Professor’s the-
 ory is right. Nature hath made no provi-
 sion against poison, for if she had, the Pro-
 fessor would have been right, for then go-
 norrhœa would have cured itself. And does
 this come out at last to be the truth ? Nature
 has made no provision against poisons, yet a
 person having once had the small-pox, never
 has it again. There the Professor is wrong
 again,

again, for nature, as it appears, has made provision against certain poisons. But the fact is, that no stage of the venereal disease can be gotten rid of after that manner. And in this instance the invariable effect of the Professor, and the invariable intention are foiled ; he now confesses it, and I have already foretold it.* And now, that the Professor is arrived at his last confessions, as he finds it no longer necessary for him to sport the insincerity of gonorrhœa curing itself, and of the constitution getting rid of the venereal disease intentionally and invariably by the formation of matter, let us see what the same Professor, in these present moments of contrition, has to say for himself. “ But the venereal matter, when taken
 “ into the constitution, produces an irrita-
 “ tion which is capable of being continued
 “ independent of a continuance of absorp-
 “ tion, and the constitution has no power of
 “ relief, therefore a lues venerea continues
 “ to increase.” And has the Professor found out this at last to be the truth ? And who is there that could not have told him so long ago ?

Why

* See the beginning of this Pamphlet.

Why did the Professor keep back this confession till now ? But since that he hath here produced it, I trust that we shall not hereafter hear any thing more of his insufferable jargon, that had so long wearied our attention in its refutation. I have followed up the Professor, overtaken him, and gained over him, on this very spot, a compleat victory !

I shall give to my readers two more confessions from the Professor. In continuation of the last quotation he says, “ This circumstance is perhaps one of the best distinguishing marks of the lues venerea, for in its blotches and ulcers, it is often irritated by other diseases, which not having this property, will therefore heal, and break out again in some other parts. Diseases in which this happen shew themselves not to be venereal ; however, we are not to conclude, because they do not heal of themselves, and give way to mercury only, that therefore they are venereal ; although this circumstance, joined to others, give a strong presumption of their being such.” In the next page he says, “ Many of these symptoms give way to mercury, which is probably the only concurring circumstance

“ stance attending this complaint, that is a
 “ proof of its being venereal. But if mer-
 “ cury always cured them, it would not be
 “ very material what they were called.”
 Does this, or does this not prove, that the
 Professor knows not how to distinguish ve-
 nereal ulcers from other ulcers ? Hereafter
 I shall have more use for these questions.
 And as to the symptomatick fever, he not
 having chosen to observe upon it, when he
 was treating upon bubo, I suspected that we
 should never have been given to understand,
 that it ever arose from any other cause than
 that brought on through the use of mercury.
 But I thank him for this confession also to
 the contrary. From what we have seen in
 this section, it appears that there is another
 way of extracting confessions from a man
 besides by that of putting him to the tor-
 ture ; and that other way is, by his writing
 upon false opinions, and contradictory asser-
 tions, a great work !

Page 302.

4. *Of the local and constitutional Form of the Disease never interfering with each other.*

“ If a man has a lues venerea, and gets
 “ either a gonorrhœa or chancre, or both,
 PART III. O neither

“ neither of them affects the lues venerea,
 “ nor are their symptoms worfe.”—“ But
 “ the chancre has this advantage, that the
 “ constitution cannot be cured without its
 “ being cured also.” We know very well,
 that a gonorrhœa cannot be more than viru-
 lent, if the constitution be diseased at the
 time it is contracted ; and we know also,
 that a chancre cannot be more than a chan-
 cre, therefore it is not possible for the Pro-
 fessor to prove, that they do not interfere, by
 the way that he goes about to prove it. If
 the constitution be diseased, and the symp-
 toms very virulent and active, any external
 injury then making a wound on the skin,
 that wound may be converted into a chancre,
 and that chancre may continue as long as
 the venereal disease remains uncured, just as
 a satellite attends upon its planet. This is
 possible.

Page 303.

*Of the supposed Termination of the Lues Venerea,
in other Diseases.*

On the diseases which follow the lues ve-
 nerea, in consequence of the action of mer-
 cury in curing it, also on the rheumatick
 pains,

pains, and other affections which will be often experienced, I shall refer my readers to compare the new opinions of the Professor with the old opinions of Astruc, and to make up their minds from such evidence, before them.

Page 304.

Of the specific Distance of the Venereal Inflammation.

I have already denied that there exists any such limitation in the venereal disease as specifick distance, either as relatively to gonorrhœa, chancre, or lues venerea; and if the Professor pretends to limit the venereal action by any general rule, I shall continue to treat it as a conceit not warranted by general observation, but as mere ideal rubbish. These specifick distances, as he calls them, are in continual variation, both in local, as well as constitutional symptoms. The effects of gonorrhœa, in some habits, if not interrupted by cure, may go on, *ad infinitum*, to destroy all the parts; of chancres the same, and of lues venerea the same. These effects, vary in different constitutions, only by being mild or strong, slow or rapid. These effects, and the variation, of them are owing to the

constitution, and not to the disease. Let me ask the Professor, how many pustules in the small-pox are specifick, and how many are the consequences of irritable inflammation? What is his specifick distance betwixt each, when there are but twenty, and what is his specifick distance when there are twenty thousand? Which are the specifick eruptions, and which are the irritable? Is it not self evident, that these effects are owing to the nature of the constitution attacked, and not to the disease, for that can have no specifick limitation?

Page 305.

Of the Parts most susceptible of the Lues Venerea.—Of the Time and Manner in which they are affected.—What is meant by Contamination, Disposition, and Action.—Summary of the Doctrine,

“ As persons fly to relief upon the first
 “ and second order of venereal appearances,
 “ it may be supposed that the whole disease,
 “ in the parts actually affected, is cured before the other parts had time to come in-
 “ to action, which will be cured under the
 “ state

“ state of a disposition only, if we can conceive that a cure can take place before parts come into action.” That, when apparent symptoms are effectually cured by the action of mercury on the blood, those symptoms, which would otherwise have followed, but do not, are prevented and wholly extinguished by that action, to me is very clear, or mercury deceives us as a specifick ; or those we know to be infected, could never be cured.

There is not an external part of the human body, but what may be liable to the venereal disease. But the Professor has here so far met a former opinion of mine, in contradiction to a former assertion of his, that as we fly to remedy, the symptoms have not an opportunity of going all lengths ; and of course, it is not to be ascertained what the venereal disease would be in general, if it were left to its own uninterrupted action.

The Professor, and there cannot be a stronger symptom of an inert mind, is always methodising. He is a methodist in specifick distances, in irritable inflammations, in ulceration from absorption, in sympathetick
con-

connections, in new and old diseases, and in invariable effects. If these should now and then fail, or always be false, the fault is not with the Professor, the fault is in nature, for nature has made no provision against poisons! What provision has nature made against every class of fevers?

We now meet with another of his methods, the method of "order of parts!"—"A first and second order of parts!" By these are meant, that the venereal disease shall or must appear with the first order, in one set of symptoms; and with the second inevitably from the first, in another set of symptoms. But this, like his other doctrines, is not "invariable;" for he says, quicker than I could have thought it, "however, it is not universally the case, that the parts which I have called first in order, are always so; on the contrary, we find that this order is inverted in some cases."

The first order of parts affected is the "skin," (I suppose that he means any part of the skin, for I insist upon it, that any part of it may be affected) "tonsils, nose,
"throat,

" throat, inside of the mouth, and some-
 " times the tongue." The second order of
 parts is " the periosteum, fasciæ, and bones.
 " Perhaps the bones come into action
 " from the membrane being affected." Thus
 far the first and second order of parts.
 To account for these successions, as if man
 were a pine-apple, the Professor appeals to
 heat and to cold. He appeals to the affec-
 tion of parts, in the same succession as,
 he thinks, they are found in comparative
 order and degree from heat to cold, as if the
 bones of the leg (particularly the fore part
 of the tibia) were not as much exposed to
 cold, as many of the parts where the symp-
 toms are said to appear, in the first order,
 although this symptom of affection is there
 set down amongst the second, and the latest
 of the second order of parts. Can there
 be more superficial argument advanced, than
 that the tibia is not superficially situated?
 But this doctrine, of reasoning from the ef-
 fects of cold, I have always considered as
 chimerical. I have all along asserted, that
 every disease is marked by its own idiosyn-
 crasy, and that it is by the symptoms only
 that we arrive at the knowledge of the na-
 ture of the disease. This idea of heat and
 cold

cold has been hinted at by Astruc, yet I cannot give the smallest credit to it. But the Professor obliges himself to believe it, notwithstanding he hath in his possession one of the same general arguments which I shall produce; not because he hath only remarked it, but because it strikes my conviction as much more founded upon reason, than that upon which he hath solely relied. I say, that these orders of symptoms, even if they did so occur, and which I deny that they generally do under any given rule, could not occur from the cause of cold; and here is my obvious reason, that the venereal disease derived its origin from, and is endemial in that climate where the specifick heat of the external air is greater than the heat of the blood of man who breathes that air. But we know that the disease is generated in parts of the body not very susceptible of cold; and we have been told by Wallis and Cook, that at Otaheite its ravages are rapid. The Professor is, perhaps, the worst logician that ever undertook to make the best of a bad argument. Does he not know, that, if he fail in his great outlines of truth, all his subordinate conclusions will fail also, as the *plus* must include the *minus*? Do not all
morbid

morbid diseases, and do not all poisonous diseases originate in warm countries? And is not cold air the grand specifick against them? I am aware that it will be said, that the throat is constantly fanned with cold air; but notwithstanding, I cannot help insisting upon it that the throat is not the coldest of parts, but that it is a part where the blood vessels come home to the very surface; and as it is the general effort of nature to throw that upon the extremities of blood vessels, which is obnoxious to the blood, so I can conclude that the disease will be more rapid and more early there than any where else. If this be the true theory, at least it accounts for why the fasciæ, periosteum, bones, and parts in which the circulation of blood is not so compleat, come last in the order of disease, as already asserted; and it also accounts for why these are found the more difficult of cure.

The difference betwixt the Professor and myself seems to be this: He, to destroy an edifice of human construction, (and such is the theory of the disease in question) begins where he should end, with rashly dragging away its foundation stone. I creep slowly

on to trace effects *gradatim* up to causes, and unresistingly remove the loosened stones, in order to fix them firm, and make the edifice yet more solid ! And now for the “ summary doctrine” of the Professor, which I shall consider in a light somewhat similar to articles of capitulation betwixt us. He says, “ The above account of the lues venerea may be reduced to the following heads :

“ First, That most parts, if not all, that are affected in the lues venerea, are affected with the venereal irritation at the same time.” No. Previous to any parts being constitutionally affected, irritation of the blood, where the venereal contamination is circulating, is first to be observed upon ; and this is the symptomatick fever, or constitutional effort, by which the venereal eruptions are thrown upon the surface ; after which some irritation may, or may not accompany the parts affected.

“ Secondly, The parts exposed to cold are the first that take the venereal action ; then the deeper seated parts, according to their susceptibility for such action.” No, for the reason which I have already assigned.

“ Thirdly,

“ Thirdly, The venereal disposition, when
 “ once formed in a part, must necessarily go
 “ on to form the venereal action.” No.
 We know nothing, nor can we know any
 thing of disposition ; we are given no warn-
 ing of venereal attack. We know nothing
 of it, until we see or feel its action.

“ Fourthly, That all parts of the body,
 “ under such disposition, do not run into
 “ action equally fast, some requiring fix or
 “ eight weeks, others as many months.”
 No. The action, whether rapid or slow,
 will greatly depend upon the parts first lo-
 cally affected, upon the degree of virulence
 of this first attack, and upon the susceptibi-
 lity of the blood for receiving the infection,
 and expelling it by fever upon any parts of
 the body. The time is indefinite.

“ Fifthly, In the parts that come into
 “ action first, the disease goes on increasing,
 “ without wearing itself out ; while these
 “ that are second in time, follow the same
 “ course.” Granted, that the venereal dis-
 ease does not “ cure itself.”

“ Sixthly, Mercury hinders the venereal
 “ disease from forming, or in other words,
 “ prevents contamination.” Granted.

“ Seventhly, Mercury does not destroy a
 “ disposition already formed.” If I knew, or
 could by any divine intuition know, of such
 disposition, I could destroy it by mercury.

“ Eighthly, Mercury hinders the action
 “ from taking place, although the disposi-
 “ tion be formed.” Granted, upon the con-
 ditions recited in the seventh article.

“ Ninthly, Mercury cures the action.”
 Granted, that mercury destroys the effects of
 venereal virus on the blood, and then the
 action is cured of course ; but that some-
 times mercury requires, to effect this, adven-
 titious aids.

Page 316. Chap. II.

Of the Symptoms of the Lues Venerea.

The Professor, in this chapter, sets off
 afresh, with acknowledging general truths,
 and disclaiming his former assertions. We
 now

now hear nothing more of the methodist. He proceeds to describe the disease as it is; and we shall find, that he tells us a different story to that which he has been lately telling us; and we shall find, that now the disease is the same always, but that it is the constitution which causes the variation in its symptoms, both with respect to their order of appearance, to the effects produced when they have appeared, and to the time of their appearance. The trammels of method are flung all aside as trash. The certainty that the symptoms are venereal, *prima facie*, is exploded. For, to ascertain that fact, “ We
 “ are often obliged to have recourse to the
 “ preceding history of the case, before we can
 “ form any judgment upon it.” Specifick distance tumbles down altogether also: “ For
 “ I can easily conceive, that a peculiarity of
 “ constitution may make a very material
 “ difference in the appearance of the same
 “ specific complaint; and I am certain that
 “ the solids, according to their different na-
 “ tures, produce a very different appearance
 “ when attacked with this disease.” The specifick time for the appearance of symptoms, and their order of appearance, tumble down next: “ The difference of constitu-
 “ tion,

“ tion, and of the same parts at different
 “ times, may have considerable effects in
 “ the disease appearing sooner or later. This
 “ I am certain of, that the different parts
 “ of the body produce a very considerable
 “ difference in the times of appearance of
 “ the disease.”——“ In many cases the lo-
 “ cal effects are much later than six weeks,
 “ and in many much sooner,” Order of
 parts, in their turn, not only tumble down,
 but are so crushed in their fall, and so jum-
 bled together, as never more to be again set
 up under any distinguishing order whatever,
 neither Corinthian, Dorick, Ionick, Tuscan,
 nor Composite: “ I have seen cases when the
 “ periosteum, or bone was affected prior to
 “ any other part; whether in the same
 “ cases it might in the end have affected the
 “ skin or throat, I will not pretend to say.
 “ But it is possible that the second order of
 “ parts may be affected, without the first
 “ having ever been contaminated.” Who
 hath not seen Hogarth’s tail-piece?

The chimerical idea of the influence of
 cold is imperceptibly stolen away, never
 having been introduced as a prominent cha-
 racter in the venereal drama of the Profes-
 sor ;

for ; but only as a little episodical trait, to prove that the Professor could now understand Astruc. It therefore was not necessary that it should be again revived. But as the criticks say of the volatile Mercurio in Romeo and Juliet, the author killed him, because he had no further occasion for him.

“ We may also observe, that similar parts
 “ come sooner into action, and appear to
 “ go on more rapidly with it, as they are
 “ nearer the source of circulation.” This appears to me to be a reason which approaches much nearer to the truth than the doctrine of cold.

“ The circumstance of its being very late
 “ in appearing in some parts, has made
 “ many suppose that the poison lurked in
 “ the solids ; and others, that it kept circulating in the blood for years.” When the venereal disease does appear, after a certain length of time, from original infection, and when the Professor doubts that it did neither lurk in the solids nor blood, where does he think that the venereal disease, betwixt the hour of first infection and the hour of its second coming in the form of
 lues

lues venerea, did all that while reside? What other natural habitation is there in the human frame, and which is neither solid nor fluid? But what is this new opinion which immediately follows?

“ We never find that a man had a chancre a twelvemonth ago, and that it broke out after in venereal scurfs upon the skin, or ulcers in the throat.” I will make the Professor answer to this. “ The lues venerea generally arises from gonorrhœa or chancre.”*---“ From the latter oftener than the former, by one hundred to one.”†---“ That the parts first affected are the skin, throat, &c.”‡---“ The time for its appearance, after it has got into the constitution, is not certain.”§ It may not be improper now to ask the Professor, what he calls the constitution? What part of the human frame it is, when he excepts the solids and blood? “ Oh that mine enemy would write a book!”||

Page

* Page 287: † P. 288. ‡ P. 307.

§ P. 317. || Solomon:

1. *Of the Symptoms of the first Stage of the Lues Venerea.*

The first symptoms after absorption, are either on “ the skin, throat, or mouth.”----
 “ These differ from one another, according
 “ to the nature of the parts affected.”—
 “ The appearance of the skin I shall call the
 “ first, although it is not always the first
 “ appearance ; for that in the throat is often
 “ as early a symptom as any.”—“ The ap-
 “ pearances of the skin generally shew them-
 “ selves first in discolourations, making the
 “ skin appear mottled, many of them disap-
 “ pearing, whilst others continue and in-
 “ crease with the disease.”—“ In others in
 “ distinct blotches.”—“ The appearances of
 “ the skin generally shew themselves in
 “ every part of the body.” Are all parts of
 the body equally cold ?

The Professor recites many other differences which authorise me to declare, that he cannot definitely describe these eruptions.

“ How far certain appearances are peculiar
 “ to the venereal disease, I know not.” This

verifies my last assertion, and proves, that he cannot always determine whether eruptions, which vary in their appearances, be venereal eruptions, or not ; and I am confident that he cannot, without previously asking questions leading to ascertain that fact.

“ The second part in which it appears, is
 “ most commonly the throat, sometimes the
 “ mouth, and the tongue.” The Professor
 himself shall also prove in this instance, that
 he cannot define to a certainty, what ulcers
 are venereal, and what are not. “ This spe-
 “ cies of ulcer is generally tolerably well
 “ marked ; yet it is perhaps in all cases not
 “ to be distinguished from others that attack
 “ this part, for some shall have the appear-
 “ ance of being venereal, and what are really
 “ venereal shall resemble those that are
 “ not.”* But here the Professor hath fur-
 nished us with a case, which at once destroys
 three of his positions, and cripples a fourth.
 “ A gentleman had a chancre in May, 1781 :
 “ In the same month next year, 1782, he
 “ had a gonorrhœa ; and in May, 1783, he
 “ had a fore throat. He had no connection
 “ with any woman from September, 1782,
 till

“ till May, 1783, which was about a fort-
 “ night before his throat became sore, and
 “ had no immediate local complaint. When
 “ I saw the fore throat first, I said it was not
 “ venereal ; and he being of a hectic ha-
 “ bit, was desired to go to Bristol. When
 “ at Bristol, an ulcer appeared at the root of
 “ the uvula. When I saw this ulcer, I said
 “ it was venereal. He went through a
 “ course of mercury, and was cured.” My
 readers cannot have forgotten an assertion of
 the Professor a page or two before. “ We
 “ never find that a man had a chancre a
 “ twelvemonth ago, and that it broke out
 “ after in venereal scurfs upon the skin, or
 “ ulcers in the throat.” God of Heaven !
 is it possible that a man should be so incau-
 tious, that he should so sport with consis-
 tency ? If chancre produced this lues vene-
 rea, the specifick time was two years ; half
 that time he denies. If gonorrhœa produced
 it, the specifick time was one year ; that
 power he denies altogether. If the virus
 were absorbed, without leaving any local
 impression, if that were the mode by which
 this infection was conveyed, and if virus can
 make so rapid a progress in a little fortnight,
 as to produce a fore throat, and if that fore

throat be the primary symptom of infection, why then at least six weeks is not an early period ; and virus may not only be first discovered in the symptom of bubo, but it may go further ; an ulcer in the throat may be the first symptom. And the advice which he gave, that yet a little while, and it is not venereal, and then that it is, proves that he has no definite judgement in that instance also.

More is yet proved by this case, that it is ascertained by the Professor to be venereal, because it was cured by mercury, and not a doubt has he advanced to the contrary.

Page 324.

2. *Experiments made to ascertain the Progress and Effects of the Venereal Poison.*

“ Two punctures were made on the penis,
 “ with a lancet dipped in venereal matter
 “ from a gonorrhœa ; one puncture was on
 “ the glans, the other was on the prepuce.”
 —“ This was done in May, 1767.”—“ Both
 “ these experiments took effect, and in a
 “ fortnight after the chancres in consequence
 “ had gone through the local treatment, by
 “ the

“ the repeated application of caustic; and
 “ by dressing them with mercurial oint-
 “ ment, they were allowed to heal up, and
 “ which they readily did.” On this I shall
 make a few observations before I proceed
 further in the case, thus clearing my ground
 as I go. And first of all I will prove that
 the Professor has treated these chancres in
 exactly the same manner, as he has treated
 chancres when he never expected that the
 virus should ever after return. “ The sim-
 “ plest method of treating a chancre is by
 “ destroying or extirpating it, whereby it
 “ is reduced to the state of a common sore
 “ or wound, and heals up as such.” “ The
 “ caustic should be continued till the sur-
 “ face of the sore looks red, after having
 “ thrown off the sloughs; after it has ar-
 “ rived at this state, it will be found to
 “ heal like any other sore produced from
 “ caustic.”—“ However, as our knowledge
 “ of the extent of the disease is not always
 “ certain; and as this uncertainty increases
 “ as the size of the chancre, it becomes ne-
 “ cessary in some degree, to assist the cure
 “ by proper dressings; and therefore it may
 “ be prudent to dress the sore with mercu-
 “ rial ointment. From such treatment there
 “ is

“ is but little danger of the constitution
 “ being infected, especially if the chancre
 “ has been destroyed almost immediately
 “ upon its first appearance, as we may then
 “ reasonably suppose there has not been
 “ time for absorption.”* It already appears
 that the caustick was applied and repeated
 in time, for the chancres healed. It now
 only remains for me to compare the time
 that the caustick was applied here intention-
 ally not to cure, with the caustick that is
 applied with the intention to cure. The
 caustick here was applied eleven days after
 the infection, and just as the chancres began
 to discharge some matter. “ The distance
 “ of time in its application and effects upon
 “ the parts is uncertain; but upon the
 “ whole rather longer than the gonorrhœa.”
 “ I have known cases where the chancres
 “ have appeared twenty-four hours, and I
 “ have known them seven weeks.”† So
 that I may fairly infer that the caustick was
 as soon applied here to prevent absorption,
 as effectually applied here to destroy and to
 heal the chancre; and the method to all in-
 tents as exactly followed to cure a chancre
 compleatly, as to make an experiment. And
 now

* Pages 228, 229. † Page 218.

now let us again revert to the case. " Four
 " months afterwards the chancre on the
 " prepuce broke out again, and very stimu-
 " lating applications were tried; but these
 " seemed not to agree with it, and by letting
 " it alone, it healed up. This it did several
 " times afterwards, but always healed up
 " of itself. That in the glans never re-
 " turned." Here we find that the venereal
 poison was not destroyed by caustick, that
 although it first healed up as a " common
 " fore," it broke out again four months
 after; it would be no longer amused with
 stimulants, but healed up of its own accord,
 without dressings, or mercury in any form;
 nay, that it played the same prank repeat-
 edly, and healed up spontaneously. Here
 we find an inversion of venereal facts. A
 chancre, locally produced, healing up, re-
 maining healed four months, breaking out
 again, and healing spontaneously, over and
 over again. Before I stir one single line
 further in the case, I will first declare my
 opinion that none of these last recited symp-
 toms were venereal; and that, during the
 four months, the virus did not sleep in the
 healed chancre; and that the person upon
 whom the Professor tried the experiment
 amused

amused him, if he told him that he had been chaste; and that if he told him to the contrary, the fable is all at an end. For I persist in the assertion, that the chancre which broke out at the end of the four months was not from the first infection, if it were venereal, and that the succeeding ulcers were not venereal at any rate. But to proceed.

“ While the fores remained on the prepuce
 “ and glans, a swelling took place in one of
 “ the glans of the right groin.” The Professor (after flourishing away with his new opinion of dissolving bubo) tells us, that the fores upon the penis were healed before the reduction of the bubo was attempted; and that then, with scientifick adroitness peculiar to himself, he “ rubbed in as much mercury as to reduce the gland considerably;
 “ it was then left off, for the intention was
 “ not to cure it completely at present. The
 “ gland some time after,” for he does not say what time, “ began to swell again, and
 “ as much mercury was then rubbed in as
 “ appeared to be sufficient for the entire
 “ reduction of the gland; but it was meant
 “ to do no more than to cure the gland
 “ locally, without giving enough to prevent the constitution from being contaminated.”

“minated.” Here I must recall the attention of my readers again, to see whether the intention of experiment and the intention of cure, be not one and the same system with the Professor. But of the two, the intention for experiment will be proved to be more effectual for radical cure, than what he has adopted for radical cure itself. “Rubbing
 “in half a dram of mercurial ointment
 “every night, pursuing this course till the
 “gland is reduced to its natural size, will be
 “sufficient, and probably will be a good
 “security for the constitution, provided the
 “chancre, which may have been the cause
 “of the bubo, heals at the same time.”*

In this case of experiment, the chancre, the imaginary chancre healed first of all; but in the intention for a cure as above recited, it appears that if the chancre got well while the bubo dissolved, stop at the dissolution of the bubo, and the constitution is safe. And now to proceed: “About two months after
 “the last attack of the bubo, a little sharp
 “pricking was felt in one of the tonsils in
 “swallowing any thing; and on inspection
 “a small ulcer was found, which was al-
 “lowed to go on till the nature of it was

PART III.

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“after-

“ ascertained, and then recourse was had to
 “ mercury. The mercury was thrown in
 “ by the same leg and thigh as before, to
 “ secure the gland more effectually, al-
 “ though that was not probably necessary.”

To cure the constitution of venereal infection, it is, I believe, very immaterial whether the mercury be rubbed in on the right leg and thigh, the left, or on both; and the gland where the bubo had formerly been, but where it now was not, required no particular mark of the Professor's attention, although he did shew it such partial regard. The poison had already gone into the constitution, and the Professor, unless he thought that it would return again to the groin after its entire reduction, as well as it did four months afterwards return to the chancre, after it was healed, performed a harmless act of superstition; for he might have as reasonably looked for the track of a transient arrow in the air, as that the venereal virus dislodged as it was from a part locally infected, and healed for four months, or two months even, should return to that very part again. Supposing the surface of the body to be one hundred thousand times more than that very spot where the local complaint was,
 and

and supposing, after the infection be gone into the constitution, that this same spot be again infected, and this spot only, then such were the odds against the Professor's experiment in this instance of the return of chancre, and such the proportionate odds of a bubo coming on afresh, and of the same gland in the groin being infected from the constitutional infection; for the one was before completely healed, and the other was completely reduced, and as much so as if he intended radically to cure both, as I have already proved, but yet I will here produce another voucher to the fact. " A part once
 " perfectly cured is never irritated again by
 " the same stock of infection, though pro-
 " bably some other parts in the constitution
 " are still under the venereal irritation."*
 How does the Professor like this quotation? But to the case: " As soon as the ulcer was
 " skinned over, the mercury was left off,
 " it not being intended to destroy the poi-
 " son, but to observe what parts it would
 " next affect." Let us compare the inten-
 tion here, with another intention in another part of the great work. " When a sore has
 " put on a healthy look, when the hard

R 2

" basis

“ basis has become soft, and it has skinned
 “ over kindly, it may be looked upon as
 “ cured.”* In the same page he says, “ In
 “ very large chancres it may not be neces-
 “ sary always to continue the application of
 “ mercury, either for internal or external
 “ action, till the sore is healed.” Then here
 I think that no more real intention is ac-
 tually discovered in this stage of the grand
 experiment, than there has been in any of
 the former. But to proceed: “ About three
 “ months after, copper coloured blotches
 “ broke out on the skin, and the former
 “ ulcer returned in the tonsil. Mercury was
 “ now applied the second time for these
 “ effects of the poison from the constitu-
 “ tion, but still only with a view to pal-
 “ liate. It was left off a second time, and
 “ the attention was given to mark where it
 “ would break out next, but it returned
 “ again in the same part.” The unfortu-
 nate Professor, for so I must call him, for
 unfortunate he is, that like Actæon, he
 should be devoured thus by his own hounds
 never leaves me at a loss for condemnation
 from out of his own new opinions. “ We
 “ never find that a man had a chancre a
 “ twelvemonth

“ twelvemonth ago, and that it broke out
 “ either in venereal scurfs upon the skin,
 “ or ulcers in the throat.”* What, is not
 the twelvemonth yet expired? But we shall
 immediately see whether it be or not, for I
 find that I am come already to the conclu-
 sion of the case. “ It not appearing that
 “ any further knowledge was to be pro-
 “ cured by only palliating the disease, a
 “ fourth time in the tonsils, and a third
 “ time in the skin, mercury was now given
 “ in sufficient quantity, and for a proper
 “ time, to complete the cure. The time the
 “ experiment took up, from the first infer-
 “ tion to the complete cure, was about three
 “ years.” It is not to be presumed that the
 Professor gave more mercury now than be-
 fore, because he has all along asserted that
 these complaints are cured in a shorter time
 than local ones, and require less mercury.
 “ For the most part a much smaller quantity
 “ of mercury is sufficient for the cure of
 “ such symptoms, than what is necessary for
 “ the cure of local complaints.”†

In casting up the registered account of
 time which the Professor has been pleased to
 annex

annex to the case, not more than twelve months out of the three years are recorded. Besides, as he appears to have left off his correct description with the third coming of ulcers in the tonsils, and as “ we never find “ that a man had a chancre a twelvemonth “ ago, and that it broke out either in vene- “ real scurfs upon the skin, or ulcers in the “ throat,” it is not to be presumed that there was in this instance an intended complaisance in the disease for this great event and it is not to be presumed also that the disease took up two more years for the fourth return of ulcers in the throat and scurfs upon the skin! And as I have already proved, (and I defy the strongest advocate he has to say I have not) that any intention of continuing the disease must have been impossible, the Professor having treated, or told us that he treated it, which is all the same to him and to me too, every symptom in the case exactly as he treated the same with intention to cure; therefore either the intention to protract the disease, for the sake of experiments, is all an imposition, or his mode of cure is false. Both cannot be right. And as I am confident that the case is not so told, that any advantages are to be derived from
it ;

it; as it bears strong marks of improbability, as it is inaccurate to a degree, as the register of time and of symptoms is loose and vague, and as there is “ *Hiatus valdè* “ *deslendus*,” of at least two years, I shall not hesitate, for I scorn to deceive, to fling the experiments out beyond all question, as unprofitable, as impossible, and as another “ tale told, signifying nothing.”

“ Ludicrous nature which at once can show

“ A man so very high, so very low !”*

Page 327.

3. *Of the Symptoms of the second Stage of the Lues Venerea.*

“ This stage of the disease is not so well
 “ marked as the former, and as it is of more
 “ importance, it requires all our discern-
 “ ment to determine what the disease is.” I
 have already proved from the Professor’s own
 words, that he cannot decide upon the venereal symptoms, *prima facie*, in what he quaintly calls the “ first order of parts.” And now the “ second order of parts,” (for I hug the distinction, because it is a distinction

* Churchill.

tion often confounded) appear to be
 marked with a distinction far less compre-
 henfible and definite. Then how is that to
 be described upon system which is not yet
 defined? Which is not yet understood? Can
 the Professor point out the spot where the
 lost object was, before he has found it? I
 do not mean to insinuate that the Professor
 is the most ignorant in the knowledge of the
 symptoms arising out of what he calls the
 second order of parts; for although he has
 thought proper to drop the phrase, I like it
 so well, that I shall adopt it; I only mean
 to assert that he is not more particularly ex-
 pert than others in defining the symptoms
 arising in his "second order of parts." "The
 " deeper seated parts affected are the peri-
 " osteum, tendons, fasciæ, and ligaments;
 " however, what the parts affected may be
 " when the disease is in this stage, is not
 " always certain; I have known it to pro-
 " duce total deafness, and some of these
 " cases to end in suppuration, attended with
 " great pain in the ear and side of the head."
 " They have very much the character of
 " scrofulous swellings and chronic rheuma-
 " tisms, only in this disease the joints are
 " not so subject to it as they are in the
 " rheumatism."

“rheumatism.”—“When they attack the
 “periosteum, the swelling has all the ap-
 “pearance of a swelling of the bone, by
 “being firm and closely connected with it.”
 —“When matter is formed, it is not true
 “pus, but a slimy matter.” How could
 the Professor look for pus, when the bone
 and periosteum were to be the only parts
 from out of which the venereal matter was
 to be formed? Where did he ever find, and
 what was the disease in which it was found,
 that pus was produced in any quantity out of
 periosteum and bone? “These pains are
 “commonly periodical, or have their exa-
 “cerbations, being worst in the night; this
 “is common to other aches or pains, espe-
 “cially of the rheumatic kind, which the
 “venereal pains resemble very much.”—
 “When the pain is the first symptom, it
 “affords no distinguishing mark of the dis-
 “ease; it is therefore often taken for the
 “rheumatism.”

Page 329.

Of the Effects of the Poison on the Constitution.

I really thought, and do believe his read-
 ers thought so also, that the Professor had

been for some time treating on " the ef-
 " fects of the poison on the constitution."
 " The poisonous matter produces fever,
 " which is of the slow kind ; and when it
 " continues a considerable time, it produces
 " what is called a hectic disposition ; arising
 " from a cause which the constitution can-
 " not overcome. While this exists, it is
 " impossible that any thing salutary can go
 " on in the constitution. The patient loses
 " his appetite, or even if his appetite is
 " good, loses his flesh, becomes restless,
 " loses sleep, and looks fallow."

Vulgar as the Professor has described this
 symptomatick fever, yet I acquiesce to the
 truth of it. But I mean to ask him a ques-
 tion before we proceed, and I mean to put
 this question seriously home to the Professor ;
 I mean to probe him to the quick ; I mean
 to try if I can at any rate extort from him-
 self that confession, which I have already
 exposed for him, he not voluntarily treat-
 ing upon it where he ought, and where he
 knew, from his own internal evidence of the
 truth, that he ought to have treated upon it.
 Why did he not treat of a symptomatick
 fever accompanying a bubo ? Why said he
 nothing

nothing of a harrassed constitution then? Why said he not then, that “ while this exists, it is impossible that any thing salutary can go on?” Why this was his reason, and I challenge him upon it: I must not here tell these truths; it is impossible for me in the same breath to attest, in the solemn manner which I do, that I have dispersed every bubo but three, and go liberally, as I ought, into the description of symptomatick fever accompanying bubo. The truth will come so near to the falsehood, as to gall its kibe. The common truth will jostle the falsehood.

“ There are a number of local appearances mentioned by Astruc and his followers; the cancer, scrofula, rheumatism, and gout, &c.” And do not the Professor and his few remaining followers mention something similar, but worse? Whenever the venereal disease produces pain similar to rheumatism, Astruc, in order to define that symptom, calls it venereal rheumatism; implying that the disease is venereal, and in this instance like to a rheumatism. The Professor says, that the venereal disease predisposes the constitution to these rheumatick

affections. How can that be, when the pains come on as venereal, when nothing but the venereal disease itself provokes them, and when the venereal disease is the cause? Astruc is right, and the Professor is wrong. Astruc is the substance, the Professor the shadow, which pursues the substance, and proves it to be true!

Page 331. Chap. III.

General Observations on the Cure of the Lues Venerea.

“ To cure the local and visible effects of
 “ the disease, we must attack it through
 “ that medium by which it was communi-
 “ cated, that is, the blood, without how-
 “ ever considering the blood itself as a dis-
 “ eased part, or containing the poison, but
 “ as the vehicle of our medicine, which
 “ will be carried by it to every part of the
 “ body where the poison was carried, and
 “ in its course it will act on the diseased so-
 “ lids.”

I am pleased to find, that the Professor has been so gracious as to permit me to ap-
 peal

peal to so round a declaration of his opinion, that the venereal disease is at least communicated to the blood. But how are we to reconcile the paradox, that the blood itself, in consequence, is not to be considered as a part diseased? What has produced the hectic or symptomatick fever? What does the Professor himself define this fever to originate from? Does he not say that it "arises from a cause the constitution cannot overcome?"* But I promise him that he shall hear more of this hereafter. If the poison passed through the same vehicle, and mixed with the same fluid that afterwards carried its antidote, and if it be confessed by the Professor, that the action of mercury brings on a diseased habit, how can he deny this action in the former, and assert it in the latter? If the venereal disease, by the natural constitutional effort to get rid of it, out of the constitution altogether, shews itself in the throat, or in any other part of the body, it proves to common understanding, that it is acting on the blood itself, and that the blood in the extreme vessels betrays the contamination, both in the throat by ulcer, and on all other parts by appearances tending to ulcerate, as well as by the hectic symptoms

toms already described; and that these hectic symptoms, instead of arising "from a cause the constitution cannot overcome," arise, as it is proved, from an effort in the constitution to overcome the cause, and which effort is successful, when there is an end to them, and when the patient recovers; for if it were otherwise, if these hectic symptoms were not, or could not be overcome, every patient must die, who was once attacked by them.

"In the first stage of the disease," says the Professor, "before it begins to show itself externally, the patient has generally rigors, hot fits, headachs, and all the symptoms of an approaching fever." Yet the blood is not diseased! Is it possible that the blood is not diseased, and yet that it is diseased? If that be possible, why then the Professor may be right! If the blood were a tube instead of a fluid, if the blood were a vessel, and not the fluid contained within the vessel, why then the Professor may be right! But the fact being the direct contrary, it must be the blood which is contaminated, and it is the blood which affects the vessels, and acts upon them so as to produce
ulcers.

ulcers. But then the Professor may ask, if it be the blood which is contaminated, if it be the blood which produces these effects upon the surface, why are such effects found on particular parts, and not on general parts? I will answer him, for I mean not to leave him a loop-hole. It is confessed by us both, that the disease may appear on any part of the body; but the reason why it appears more frequent in the throat, the mouth, and nose, is, because the circulation of the blood is carried more home to the surface of these parts, than on any others of the body. Without anatomical assistance to teach us the fact, we know that the slightest external violence will produce bleedings there, and sometimes these bleedings there are spontaneous. And now that I have answered him in this particular, I will ask him a general question. Why does the constitution endeavour at all to throw off obnoxious qualities? Why has every disease that we know its own particular symptoms? Why are the small-pox, measles, fevers of all classes, scrophula, scurvy, all so particularly marked? But throughout the great work we are not reconciled to this new opinion of the Professor. How it is possible
for

for a fluid to receive an ingredient, without partaking of that ingredient. There is one way how that might happen, which is by two fluids not incorporating. When it is considered how the virus gets into the blood by the finest of all vessels, when it is considered how it circulates in that blood, and when the length of time that it circulates is also taken into the account, I believe there will not remain another being who even pretends to be a reasonable one, that will adopt this new opinion of the Professor.

I should be very sorry to put this new opinion to the test after a certain manner, because the regard that I have for the person of the Professor is much more exalted, than that which I have for his new opinions. Suppose that a load of sublimate was thrown into the river head, and suppose that the Professor was in earnest with this theory, and that he was to insist upon drinking of this water, because though "it conveyed the poison, it " was not contaminated by it;" should I not instantly call some powerful assistance to me, should we not seize him, bind him, and restrain him at any rate from committing such an act of desperation!

But

But now it is time for us to drop argument, and appeal to facts, by which the truth of this great question is ascertained; and another truth, formerly contended for by me, will be ascertained also, namely, that the blood is infected, and that that infection can be imparted, by the laws of circulation, to a *fœtus in utero*, from the infected mother.* This is what the Professor denies, and this is what I am now about to prove. Does the Professor not know the name of Lynn, and that he is a surgeon to the Westminster Hospital?—Does he not know that a child was born, on the twenty-second day after the small-pox had appeared upon the mother, and that the child at the time of its birth was covered with distinct pustules all over its body? Does he not know that some pus was taken upon a lancet from one of these pustules on the face, and that with this lancet another child was inoculated in both arms; that the inflammation began to appear on the seventh day, on each arm; and that on the eleventh day the child sickened; and that on the twelfth the pustules of the distinct sort appeared? Was not this very case put into the hands of the Professor; and did he not refuse to read this

PART III.

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* Vide, Wiseman. Fol. Edit. p. 491.

well-attested fact, before the Royal Society, because he knew that it would at once effectually destroy the flimsy fabrick in theory, which he was erecting, and render his real prospect of reputation from so feeble a foundation, as deceitful as a dream? Did the Professor not know of this case, before this great work came forth to the world? Are there not two more cases of children born with variolous eruptions, whilst their mothers had laboured under this very disease, recorded in the London Medical Journal? * The Professor's friends thus rise up in judgement against him. Nay, he even condemns himself, for he tells us, as plain as his words can tell it, " that a diseased woman bore twins that had blotches upon them at their birth, and died soon after ;" and that " another girl, about two years old, whom the diseased mother suckled, was also covered with blotches when she came to the hospital." He yet goes farther, that " the mother and child went into the salivation ward ; that the child took no mercury, but that its gums became a little sore ; and that the blotches got well !" † Whenever the Professor

* Vol. 3d. Part 2. Mr. Jenner, Painswick.

† Page 294-5.

feſſor affects to exerciſe his faculties in papers, upon a “new opinion,” and upon “aſ-
“certained experiments,” let him read them hereafter to his grandmother !

“ I have aſſerted, that what will cure an
“ action, will not cure a diſpoſition ; if ſo,
“ we ſhould puſh our medicine no further
“ than the cure of the viſible effects of the
“ poiſon, and allow whatever parts may be
“ contaminated to come into action after-
“ wards.” My readers ſurely cannot yet
have forgotten, the famous experiments made
upon the ever to be remembered caſe, which
was ſo lately remarked upon ; there they
will find, that the Profeſſor cured the local
actions firſt for four months intentionally,
and then altogether ; but then intentionally
he gave not enough of mercury to cure the
conſtitutional action that was to be. No,
ſays he, “ it was meant to do no more than to
“ cure the gland locally, without giving
“ enough to prevent the conſtitution from
“ being contaminated.”---“ Mercury was
“ now applied the ſecond time for thoſe ef-
“ fects of the poiſon from the conſtitution,
“ but ſtill only with a view to palliate.”---
“ Mercury was now taken in ſufficient

“ quantity, and for a proper time to complete the cure.”*

Feeble as my powers are, in producing attestations from one page, to contradict attestations in another ; yet so many have I brought, and so many have I overlooked ; such violations of truth, such inconceivable prevarications have I confronted with each other, that even the credit of cases, and the credit of experiments, which ought to be considered as sacred, are shaken ! But enough of the alarm that I express ; I heartily wish that when reflection came over me, I could treat it more lightly. I should rejoice if that these new opinions and assertions of the Professor were, in their effect, only laughable or amusing ! If they could only be considered as a mere play game ! If his arguments were but like nine-pins in a Dutch skittle-ground, first set up to be knocked down, and then to be set up again !

Page

*Of the Use of Mercury in the Cure of the Lues
Venerea.*

“ Mercury, in the lues venerea, is the
“ great specifick, and hardly any thing else
“ is to be depended upon.” To this I agree.
“ It is necessary that we should always con-
“ sider well the effects of this medicine,
“ both on the constitution at large, and the
“ disease for which it is given,” To this I
agree also,

“ Mercury is carried into the constitu-
“ tion, either externally by the skin, or in-
“ ternally by the mouth.” So that it now
appears, that we should, in another page
already remarked upon, have read for inter-
nal by the skin, external by the skin. *---
“ Sometimes it happens, that the absor-
“ bents on the skin will not readily receive
“ it; at least, no effect will be produced
“ either on the disease or constitution, from
“ such application.” And has the Professor
found out this truth also at last? It would
appear that this page was intended to apolo-
gize for, and correct a former page; for there
it

it is not only asserted, but attested also, that a given quantity of mercury will cure a chancre of a given size, in a given time. And this, says the Professor, "is not speculation, but the result of experience."---

" Dressing a small sore with red precipitate, produces salivation; it shows that sores are good absorbing surfaces, especially too, when we know that lues venerea generally arises from a chancre." If any person were to ask me what is understood in surgery by the word "sore," I should not be able to inform him, it not being a term of art; of course, it conveys no idea of any part of that art. It is a vulgar expression, meaning to point out generally any part aggrieved. But the real injury done to the part, must be defined not by the word "sore," but by that word which is fitting to define the exact condition of the part. But the Professor hath made use of this low, vulgar, indefinite word to wounds, abscesses, ulcers, excoriations, and blotches on the skin. Sometimes he says, "these sores or ulcers," and sometimes, "a wound or common sore." Which does the Professor choose that it should be here? If he chooses that it should mean

mean wound, why then I will inform him that in another page he says, “ I am inclined to think that wounds are bad absorbing surfaces, especially when I consider that few morbid poisons are absorbed from wounds.” * And yet this very Professor inoculates for the small-pox with the point of a lancet! These are some of his bright, rare, and happy thoughts in the great work! These are his “ *rari nantes in gurgite vasto!*”

“ The visible effects of mercury are of two kinds, the one on the constitution, the other on some parts capable of secretion.” To illustrate these actions, I shall treat my readers with another case from out of the great work; and I do assure them, that I begin to dread a perusal of his cases. For if they succeed to illustrate the truth for which he appeals to them, a thousand to one but that they go point blank against some other solemn attestations, which he has laboured to impress us with the belief of, and which afterwards prove to be gross imposition. I am aware of my expressions; I will never recall them. It shall soon be
seen,

seen, if my justification be not ready at hand. It shall soon be seen, if I do not prove him personally guilty of this very charge !

“ A gentleman rubbed in mercurial ointment for the reduction of two buboes. He had only rubbed in a few times, when it affected his constitution so much, that it was necessary to leave it off. He was seized with feverish complaints of the hectic kind, a small quick pulse, debility, loss of appetite, no sleep, and night sweats. He took the bark, with James’s powders, and asses milk, and gradually got rid of these complaints.” Before I proceed, I shall positively declare, that none of these symptoms were brought on by the little mercury, but they were absolutely symptoms from the venereal bubo ; and this is further proved by what he says next. “ As the buboes were advancing, it was necessary to have recourse to mercury again ; and I told him, that now it would not produce the same effects so quickly, nor so violently as before. He rubbed in a considerable quantity, without his constitution or mouth being affected, but the buboes suppurating, made me order

“ der

“ der it to be left off a second time, and
 “ without producing any disagreeable ef-
 “ fects.” This still further, if it were ne-
 cessary, goes to prove, that the first febrile
 symptoms were symptoms from bubo. But
 I beg pardon for interrupting the Professor
 in this interesting history. I think his story
 left off with the suppuration of buboes, and
 favourable effects of mercury. Now let him
 proceed. “ The buboes took on a healing
 “ disposition for a while, and then became
 “ stationary, showing that a new disposition
 “ was forming. He was then directed to
 “ leave off the ointment, and to bathe in
 “ the sea, which he did, and the buboes be-
 “ gan to heal. In about three weeks, how-
 “ ever, it was thought necessary to rub in
 “ again, and when he began, which was the
 “ fourth time, it had almost an immediate
 “ and violent effect upon his mouth. He
 “ left off again, till his mouth became a lit-
 “ tle better, and then returned to the mer-
 “ cury a fifth time, and was able to go on
 “ with it.” The Professor has adduced this
 case to shew the visible effects, and I may
 say, the poisonous effects of an improper
 quantity of mercury injudiciously thrown
 into the constitution. And so far as it proves

that it was injudiciously administered, I think the case is much in point. But now it is my duty to apply this case against the Professor, to prove him by it guilty of intentional omissions, of contradictory opinions, and of what is yet more painful, as it is impossible to separate, in this instance, the man from the act, some false attestations. And to begin with his omissions. Why did the Professor, in his definition of the incipient symptoms of inflammatory bubo, omit these very symptoms which he has here described, and which he has here wrongly carried to the account of mercury?

Why has the Professor so roundly asserted, that the venereal disease was at an end, when the new disease came on? That mercury had cured the venereal disease, so far as it was venereal; and that so far as it was not venereal, sea-bathing, hemlock, opium, and salt-water poultices would be the only finishing system? Has he not here resorted to mercury five times? If mercury were not necessary in his former theory, how happens it that it should be necessary in his present practice? What sort of traffick does the Professor call this trading voyage, of sending his patient

tient to sea with the new disease, and of the return of the same to his custom-house, with the old disease ? How does he class it ? It certainly is not so profitable to the capital, and of course not similar in its effect, but in appearance, this dispatch of the Professor is not unlike that of a country farmer, who sends hay to London in the same cart that returns with manure !

And now for the false attestations ! Has not the Professor formerly attested, that, by his own peculiar art, he could say with truth, that since the year 1761, he had dispersed every bubo but three ? And that these were not two out of the three, I will prove, because he says, by virtue of that very art, although they did come to abscess, yet so insignificant was that abscess, that they soon got well. How came this potent power of the Professor ! this Bellisle infallibility ! to desert him in this instance before us ? Or was his memory so weak, as to betray him into this dilemma ? But not only this attestation, but many more throughout the great work, are easily seen through. Their intentions were never but to one object, and that was, *ad captandum vulgus*, a miserable expedient !

These are the traits that exonerate me. No man shall soften my rigour, but by satisfying my mind that it has been unjustly applied. The crimes which I have attacked could not be punished by any other means than through the press. Such the Professor hath published, and such professional crimes have I exposed.

Apropos. Having now closed the account of the blood being, and not being contaminated, of the Bellisle infallibility, of the new disease, and of the extraordinary circumstance of fever accompanying bubo; let us no longer insult the "two actions" by silence. "I have formerly laid it down as a principle, that no new action will take place whilst the body is under the beneficial operation of mercury*."

If there be really a partnership formed when venereal buboes assume the scrophulous disease during the use of mercury, how does the Professor conduct himself in such a dilemma? What is his process? Does he follow the legal method? Does he bring his two actions separately or jointly? Or is his

his cause in so bad a condition as to suffer a nonfuit !

Page 341.

2. *Of the Quantity of Mercury necessary to be given.*

“ Two circumstances are, however, strictly
 “ to be attended to in the administration of
 “ the medicine; which are, the time in
 “ which any given quantity is to be thrown
 “ in, and the effects it has on some parts of
 “ the body, as the salivary glands, skin, or
 “ intestines.” I think to these two circumstances that I should have added a third, which is the effect that mercury hath upon the disease for which it was given: and when the professor omitted this, I wonder that he did not forget mercury also, as well as the disease for which it was a cure. Many times I have seen the disease give way to mercury before I have seen any other effect from it; and I have been often left with a persuasion that mercury was acting upon the constitution by this solitary instance. I will go farther in saying, that I should rejoice if I saw always, that the venereal symptoms yielded before
 other

other symptoms of the action of mercury came on, as I should by that means be able to continue the same quantity with the pleasing prospect of doing all that was good, of effecting a cure of venereal symptoms without incurring a disease of the constitution by mercury. For I consider mercury in over doses as a poison to the constitution. The Professor goes so far as to confess that mercury is capable of affecting the body very materially; that it produces local diseases; and that it is “also capable of retarding the cure
“ of chancres, buboes, and certain effects of
“ the lues venerea.” But then he says, as if he had forgotten the case which I last gave, where he returned to mercury five times,
“ after the poison has been destroyed.”

Page 342.

3. *Of the sensible Effects of Mercury upon Parts.*

The sensible effects of mercury upon parts are too well known. Therefore I shall only remark, that all these effects, and the condition of the blood itself, prove to my satisfaction, that mercury tends to reduce the constitution into a state very similar to
that

that in which a patient with strong scorbutick symptoms is found. And that this idea, as far as a speculative idea can lead us to ascertain a fact, does go to the ascertaining of this fact. For by the strength of this knowledge, we suspend the use of mercury, we proceed in restoring the constitution by every remedy that is the most powerfully antiscorbutick. And when the constitution is thus restored from such reduced condition, we have recourse to mercury again, but not in the large quantity which we had before been giving; for if we did, and if mischief again followed, it would appear that our ignorance was more than common, that it was incorrigible, and that we could not profit from the experience of our former misfortune. For it is but reasonable to suppose, that, if we were to push mercury so far, as to bring on again symptoms similar to to those which induced us before to suspend it, we are gaining no ground in eradicating the venereal disease; but that we are, by such a practice, only debilitating the constitution, and protracting the disease.

Of the Action of Mercury.

I am extremely sorry, that my fixed limitation will not permit me to enlarge upon this *thesis*. I have perused the opinions of many authors,* I have weighed their arguments, and I have watched, with a jealous eye, the opinions of the Professor; I have also compared these opinions, and measured them by the best of all standards, by the result of practice,

“ Mercury can have but two modes of
 “ action; one upon the poison, and the other
 “ upon the constitution; we can hardly
 “ suppose it to act both ways.”---“ If the
 “ first were the action of mercury, then we
 “ might reasonably suppose, that quantity
 “ alone would be the thing to be depended
 “ upon; if the second, that the quantity of
 “ evacuation would be the principal circumstance.”---“ But if it act upon the principle of destroying the diseased action of the living parts, counteracting the venereal irritation by producing another of a
 “ different

* Howard, Kirkland, Swediaur.

“ different kind, then neither quantity alone,
 “ nor evacuation, will avail much ; but it
 “ will be quantity, joined with sensible ef-
 “ fects, that will produce the quickest cure,
 “ which from experience we find to be the
 “ case.”

I can find no other way of reasoning upon
 this question, than by referring my readers
 to the real effects, both of the venereal poi-
 son, and of mercury, upon the constitution.
 And as I have already endeavoured to prove,
 and I trust, with some satisfactory, if not
 convincing argument, that the blood cannot
 but be infected by venereal virus ; and as it
 appears to our senses as strong as any fact
 possibly can, that by the action of mercury,
 the blood, in particular, and the constitution
 in general, are also affected ; then there can
 be no longer a mystery about the action of
 mercury in effecting a cure, so far as that
 cure is evident to our senses. For if we
 know that the blood is under the venereal
 influence, and if we know that the action of
 mercury destroys that influence, then this is
 all that it is possible for us to know, and per-
 haps as much as is necessary to be known.
 This is also still further evident to our senses,

that neither specifick given quantities of mercury, nor certain evacuations produced by it, will always cure the venereal disease. The quantity to be given depends upon the constitution that is to receive it, and the condition of the constitution at the time of receiving it. It is by the action of mercury, that we are to be guided ; by its relative action, in removing the venereal symptoms, in particular, and by its action upon the constitution, in general. If we find that the venereal symptoms encrease, whilst the action of mercury upon the constitution in general is encreased, we are not justified in pursuing the remedy, until we have amended the constitution. But seeing that it seldom occurs, that the venereal symptoms do not retire, as the visible action of mercury upon the constitution advances, I do not hesitate to say, that we are authorised to push mercury until we find, at least, that our intention is disappointed. For if we were not to adopt this practice, the power which we depend upon may not have been exerted with sufficient force for obtaining the intended end ; and we may be thus pursuing the disease, without ever overcoming it.

This

This leads me to make some few remarks upon the opinions of those who have lately, with much philanthropy, advised the giving of mercury in very small doses, to cure the venereal disease. The pleasure resulting from the exhibition of a mild remedy, to destroy a dreadful disease, cannot be confined to the theorist who has enforced it, nor to the suffering invalid. It is a general *desideratum* in humanity. And so far every surgeon will always have in view the administration of as little mercury, and so guard, with unremitting attention, its action, that as little mischief shall be produced from the administration of it as possible. But I trust that these humane and sensible advocates will not blame me for declaring, that I cannot go with their opinions, so far as I could wish. As long as I see that I can controul the effects of mercury, and as long as I experience that the symptoms of the lues venerea, in general, decline, when the effects from mercury advance, I shall continue to push mercury. And my reason for so doing is, because I see no danger to be dreaded from it; because in general, the venereal disease is cured by these means, and without any bad consequences; because, if I gave mercury in small doses, I

must pursue the course a longer time ; because, by such delay, the virulence of the disease will triumph over the weak efforts of its antidote ; and because I must, after all, appeal to an increased quantity of mercury, and which increased quantity, given in this late stage, will not operate with that beneficial effect, it would in an earlier. For at any rate, the disease and the cure are a bloody battle of poisons, and the poor constitution is the field of action,

The Professor has not fallen into this proposed method of curing the venereal disease, by mercury in small doses, neither locally nor constitutionally ; for the local symptoms, he hath formerly said, require for their cure, more mercury than the constitutional. “ The
 “ practice must vary, according to circum-
 “ stances ; if the disease is in a violent de-
 “ gree, less regard must be had to the consti-
 “ tution, and the mercury is to be thrown
 “ in in large quantities.”---“ To cure the
 “ disease, whether in the form of chancre,
 “ bubo, or lues venerea, probably the same
 “ quantity is necessary.”---“ I believe that
 “ the recent, upon the whole, are more dif-
 “ ficult

“ difficult of cure.”* Here I do not find so much to blame in the Professor, as I do to praise. To resist doctrines inculcated by strong theoretick arguments ; to withstand appeals to humanity, persuasively enforced by a Kirkland and a Swediaur ; to be able to keep our ground, in spite of such seducing temptations ; to be able to pursue truth, notwithstanding formidable consequences may sometimes affright us ; to follow it through difficulties, unawed by censure, require a firmness, not always to be found where it is most commonly looked for. And in commendation of this theory of the Professor, of his keeping up to the spirit of the intention of cure by mercury, I prove myself to be more actuated by the force of truth, than by any other principle that may be alleged against me. And here if I had acted otherwise, the friends of the Professor, and my enemies, if such there be, might have exclaimed with the immortal bard, “ The adder
“ hisseth where the sweet birds sing.”

5. *Of the different Methods of giving Mercury externally and internally.*

“ Mercury is best applied “ externally,”
 “ in form of ointment.”---“ If the symptoms
 “ are mild in the first order of parts, and
 “ the patient not accustomed to mercury, or
 “ it is known that he cannot bear the medi-
 “ cine in great quantity, and it is intended
 “ to conduct the cure by almost insensible
 “ means, it is proper to begin with small
 “ quantities.”---“ One scruple, or half a
 “ dram of an ointment made of equal parts
 “ of quick-silver and hogs-lard, rubbed in
 “ every night for four or six nights, will be
 “ sufficient to begin with. If the mouth is
 “ not affected, the quantity may be gradu-
 “ ally increased, till two or three drams are
 “ rubbed in at each time. But if the first
 “ quantity has affected the mouth, we may
 “ be almost certain, that the glands of the
 “ mouth are very susceptible of the mercu-
 “ rial stimulus. Therefore it will be proper
 “ to wait two or three days, till that effect
 “ begins to go off. When we begin the se-
 “ cond

“ cond time, the quantity may be gradually
 “ increased, at least a scruple every time, till
 “ two drams, or more, is rubbed in each
 “ night, which may be done without affect-
 “ ing the patient a second time very confi-
 “ derably, as has been observed. If all the
 “ symptoms gradually disappear, there is no
 “ more to be done, but to continue this
 “ practice a fortnight longer by way of se-
 “ curity.”

But why “continue the practice a “ fort-
 “ night longer,” if mercury will not cure a
 disposition ? I thought it proper to give the
 whole of the Professor’s method of adminis-
 tering mercury by friction, in the simplest of
 all venereal attacks, in order that my readers
 may be able to ascertain the true and intrin-
 sick merit, the superior and successful effect
 of the practice of the Professor, as acquired
 in the year of our Lord 1761, during the ex-
 pedition to Bellisle. And now I leave those,
 whose sagacity penetrates deeper than mine,
 whose powers are better calculated for ren-
 dering insignificant measures important, and
 whose highest quality is to annex infallibi-
 lity to names, to extol to the skies, as they
 ought, the above instructions of the Profes-
 for ;

for ; and to prove to the world what is done in this instance by him, that has not been as well done, and better taught, by authors before. Let them refer to the ingenious pamphlet by Howard, on mercury, and there they will find more than ever were dreamt of in the philosophy of the Professor.

For an analysis of the different preparations of mercury, I think I shall hardly rely upon the chemical powers of the Professor ; not but that I am confident he thinks himself so sufficient, as not to doubt his own sufficiency. But for the analysis of the different preparations of mercury given internally, the proportionate doses of each, their preferable qualities, and their various actions on the venereal disease, I shall refer my readers to the late ingenious publication of the learned Swediaur, whose chemical powers are really equal to any pretensions, and whose independence is superior to all venal seduction.

6. *Of the Cure of the Disease in the second or third Stage.*

“ In the more advanced stages of the disease, the mercurial course must be pushed further. The greatest quantity of that medicine that the patient can bear at a time is to be thrown in, and continued with steadiness till there is great reason to suppose the disease is destroyed. It will not be possible in these cases to prevent the mouth from being considerably affected, the quantity of mercury necessary to be thrown in for the cure of these stages of the disease, being such as will, in most cases, produce the effect.”

“ Friction will answer better than giving it internally ; for in this way we are surer of throwing in a larger quantity in a given time, than could be taken internally, without hurting the stomach.”---
 “ How long the course is to be followed is not exactly ascertained; it may be thought proper to continue it till the local appearances, as nodes, have subsided ; but

“ I suspect that this is hardly necessary, ex-
 “ cept they give way readily ; for in such
 “ cases, the local complaints, or tumefac-
 “ tions, &c. generally require a longer time
 “ to be removed than the venereal action ;
 “ and local applications must be of service,
 “ especially if such tumefactions are ob-
 “ stinate.”

Having stated to my readers the outline
 of the Professor's administration of mercury,
 in what he calls the second and third stage
 of the disease, and it appearing evidently
 that he encreases the power of mercury by
 an encrease of its quantity, in order to over-
 come the disease in this stage of it, I shall
 just recall the attention of my readers to
 former theoretick assertions of the Professor
 respecting the quantity of mercury necessary
 to be given, and the comparative difficulty
 of curing the different stages of the venereal
 disease. “ To cure a chancre, if large, we
 “ may suppose that the absorption will be
 “ proportioned to the surface, and if long
 “ continued, the absorption will be accord-
 “ ing to the time ; and if they have been
 “ many, large, and continued long, then
 “ the greatest quantity of mercury is neces-
 “ sary.”

“ fary.”* “ The quantity of mercury for
 “ curing a local effect will be nearly as
 “ much as the cure of a flight lues ve-
 “ nerea.†”---“ A chancre may be sometimes
 “ cured in two weeks, and others require as
 “ many months. The lues venerea may in
 “ general be cured in one or two months.
 “ This shews the regularity and irregularity
 “ as to time in the cure of each form of the
 “ disease.”‡

Seeing then that such is the confession of the Professor in practice, and such is the other and contrary confession in theory, it remains for me only to draw this conclusion. That, whether the great work of the Professor be read or not, whether it be ever opened again, or continued shut up for ever, we derive, in this instance, neither profit nor loss. Seeing then that it appears that chancre still requires as much or more mercury for the cure of it, as it is a longer or shorter time in curing; and that in lues venerea, it is still also the same; seeing then that mercurial ointment is still rubbed in in largest quantities for the cure of the most obstinate symptoms, and in the smallest for the cure

* Page 240.

† Page 241.

‡ Page 331.

of the mildest symptoms; seeing then that mercury applied externally, has still the preference, and that mercury is still only to be depended upon as the grand specifick for the cure of the venereal disease; it is surely
 • high time for me to dismiss this unprofitable enquiry, and to ask my readers whether it be not high time for us all to make up our sentiments upon the general subject; and whether we should prove ourselves honest in our profession, firm in the cause of truth, and exalted in the value that we set upon our time, if we did not, without hesitation, declare, that “ Gratiano speaks an infinite
 “ deal of nothing, more than any man in
 “ all Venice---his reasons are as two grains
 “ of wheat hid in two bushels of chaff;
 “ and when we have them, they are not
 “ worth the search!”

I cannot close this subject without paying my tribute of gratitude to the memory of the unknown author, who, through the benevolence of Providence, first taught us the application of mercury for the cure of the venereal disease. With such an acquisition of knowledge, so necessary to the very being of society, is it possible not to feel,
 and

and not to express our sense of the bounty, without emotions of ecstasy? I may be permitted, without any superstitious imputation, to indulge the same in revery which Tully put into practice, when, from the same fervent zeal, he sought his way through brambles to the tomb of Archimedes,

*Of Diseases supposed to be Venereal, produced
by transplanted Teeth.*

Having gone through all the material new opinions, and illumined them with such conspicuous embellishments as my abilities would permit, I congratulate myself and my readers also upon it. And now that my exposition upon the venereal text of the Professor is compleat, it only remains for me to observe upon the supposed venereal disease produced by transplantation of a tooth from out of the mouth of a subject, whose blood is contaminated by venereal virus, into the mouth of a subject, before perfectly sound. This I shall call the hypocrypha to the great work of the Professor; because he has introduced it to his readers not as a true venereal disease, but as a doubtful disease resembling the venereal. Of this I am as certain as I can be of any truth, where my internal evidence of that truth is my only director, that if Sir William Watson * had not given to the publick one of the fatal instances of lues venerea being conveyed

* Vide London Medical Transact. Vol. III.

veyed by transplantation of tooth, we should not have read a single syllable upon the subject from the pen of the Professor. Few men accuse themselves. Why did not the Professor read some of these cases before the Royal Society? Why have not our medical magazines teemed with them? or why has not a single hint upon the subject escaped the accurate observations of these medical journalists? The Professor, whose fame sounds so far and near, not to catch the opportunity of another breath from the trumpet upon this important occasion! I will give my reasons for this my positive opinion, that had it not been for Sir William Watson's case, the Professor would have been silent upon this serious and important question!

When the fact which I have before related, of a *fetus* being infected with the small-pox in the womb of its mother, was proved after the birth of the infant, by a child being inoculated and infected from it, the Professor refused to read that fact before the Royal Society; not because it was a fact, but because it militated against that theory which was the fundamental principle
of

of his great work ! For if the blood cannot contaminate, that case could never have happened. And if the blood infected with the lues venerea cannot contaminate, some of the cases recited by the Professor could never have happened. Therefore the Professor must of course deny that all the seven recited cases are venereal, or reject his former theory as false. I will here produce to my readers a note from the great work upon this point. “ It is to be remarked here, “ that I do not in the present case lay any “ stress at all on my opinion of the lues “ venerea not having the power of con- “ tamination ; and I believe we must allow, “ if these are venereal, it must have been “ the lues venerea in the person from whom “ the tooth was taken ; for chancres are “ not common in the mouth, and they “ would be seen on examination. I be- “ lieve few discharges similar to gonorrhœa “ take place there.”

But the concern of the Professor is still more extensive. For it is he who hath almost alone been constantly attending upon transplanters of teeth ; it is he who hath for many years embraced most of this honourable

nourable branch of the profession. Whether it was revived by him or not in this country, I will not pretend to say, for I care not about it; but I have a professional concern in clearing up the point at issue betwixt the Professor and the dentist, on the one part, and the professional judgement and honour of all the independent part of the faculty, on the other. My readers cannot be strangers to the frequent allusions that I have made to this fatal subject. And I do assure them, in the most solemn manner, that, in aid to my own opinion, from every enquiry which I have made, I have not as yet found more than one * professional man who hath read these cases, that is not satisfied beyond a doubt, that the case remarked upon by Sir William Watson, and some of the other cases, were truly venereal; and that the venereal infection was conveyed from the persons from whom the teeth were extracted.

Let any man read the seven cases furnished by the Professor, and attend to his remarks upon them; I am confident that he cannot then hesitate to declare, that the Professor,

PART III.

Z

instead

* This gentleman said, that he knew that mercury was the only cure for them.

instead of illustrating the subject, has laboured to obscure it; instead of coming boldly to an explanation of the matter of fact, has written a defence; instead of confessing his errors in opinion, and the evils from them in practice, has laboured to protect them; has studied with more art, than I could have suspected him of, to confound the true venereal cases with those that were the effect of simple irritation; has evidently collected and given in upon the same scroll, the cases which bark cured, with the cases which bark could not cure, although it was confessedly tried. This distinction alone explains all the intentional difficulties which the Professor has thrown in the way.

The third case given by the Professor, which continued three years, was clearly venereal, and more clearly so than his former case of three years for “ ascertaining experiments.” I shall produce it before my readers.

“ The third case was of a gentleman,
 “ where the transplanted tooth remained,
 “ without giving the least disturbance, for
 “ about a month, when the edge of the
 “ gum

“ gum began to ulcerate, and the ulceration
 “ went on till the tooth dropped out. Some
 “ time after, spots appeared almost every
 “ where in the skin; they had not the truly
 “ venereal appearance, but were redder,
 “ or more transparent, and more circum-
 “ scribed. He had also a tendency to a
 “ hectic fever, such as a restlessness, want
 “ of sleep, loss of appetite, and head-ach.
 “ After trying several things, and not find-
 “ ing relief, he was put under a course of
 “ mercury, and all disease disappeared ac-
 “ cording to the common course of the cure
 “ of the venereal disease, and we thought
 “ him well; but some time after, the same
 “ appearances returned, with the addition
 “ of swelling in the bones of the metacar-
 “ pus.” Before I proceed in the case, I ask
 the Professor, how he came not to specify
 what appearances returned? Did the ulcers
 in the gum return, as well as the spots upon
 almost every where on the skin? “ He was
 “ now put under another course of mer-
 “ cury more severe than the former, and in
 “ the usual time all the symptoms again
 “ disappeared. Several months after, the
 “ same eruptions came out again, but not
 “ in so great a degree as before, and with-

“ out any other attendant symptoms.” Here then we find at least no fresh ulcerations on the gum. Perhaps there was none left to be ulcerated. But to the case. “ He a
 “ third time took mercury, but it was only
 “ ten grains of corrosive sublimate in the
 “ whole, and he got quite well. The time
 “ between his first taking mercury and his
 “ being cured, was a space of three years.” The Professor only doubts that this case was venereal, because, after two compleat courses of mercury, it yielded to ten grains of corrosive sublimate. And cannot he for once reconcile himself to so obvious a truth? Cannot he conceive that a disease returning the third time, and much more faintly than the two former, and after two compleat mercurial courses, shall at last yield to even less than ten grains of corrosive sublimate?

But how could the Professor flatter himself that, by mixing these seven cases in the manner that he has, the venereal would not be distinguished by professional men from those which were not venereal? That professional men could not discern in the true venereal cases, the local effects of virulent irritation, and the constitutional affection
 in

in consequence from it? And that they could not also discern the local effects of common irritation, where no constitutional affection was in consequence produced, and where bark did cure, because such did not partake of venereal virulence? Did the Professor, for I persist in it, that he has here published a defence of himself, a defence of the fatal consequences of his erroneous opinions, and not an illustration of the virulent effects produced from transplantation of teeth, conceive, however men unformed may not distinguish the difference, that professional men could not point out the stricken deer panting with the poisonous shafts in their sides, from those that had received a common accidental injury, when mingled thus by him in the same herd? This I can smile at, as well as I can at his query upon the weak effect of ten grains of sublimate given at that stage of the disease that they were, and as well as at his defining what is the true venereal shade of colour in eruptions upon the skin. These I call subterfuges not worthy to be controverted, and these only prove the verdict more inevitable, that must be given against him,

I shall

I shall now proceed to give my readers my account of the case of the young lady, and which is also given by Sir William Watson in the London Medical Transactions. But as there are some facts which I think essential to be ascertained, and which were overlooked by Sir William, he, I suppose, not then thinking that they were so essential, I have been at the pains, and have succeeded through the favour of a very valuable friend, and who is not of the profession, of coming at those facts. I preferred this opportunity of ascertaining them, because I was thus enabled to give my pure opinion upon the subject, that it may not be said, I either imbibed the prejudices or partialities of others.

This unfortunate transaction passed in the beginning of the year 1784. A lady, aged 21, was attended at the day appointed for transplanting an incisor tooth on the upper jaw, at the desire of the dentist, by the Professor. The Professor examined the girl from whom the tooth was to be taken, inspected the tooth when drawn, and after having rubbed it a little with his finger, delivered the tooth to the dentist for insertion.

A little

A little uneasy sensation continued for two days. At the expiration of that time, that sensation was felt to encrease, and to such a degree, that the lady again applied to the dentist for relief. The dentist having examined the mouth, conceived it to be a cold caught after the operation, and recommended treating it accordingly, saying, that he could no otherwise account for it. The lady went then into the country, the pain constantly existing and encreasing; she was from thence obliged to send to town for the dentist. The dentist, after examining her mouth, said, he had still hopes of its turning out well; but begged, in case of continuing to grow worse, that she might come to London, and be in the way of constant attendance. This was accordingly complied with, and she remained at the house of the dentist for ten days.

Here comes in the time when the Professor says, “ The dentist being alarmed at
 “ the first appearance, desired me to go and
 “ see her upon his own account. The edge
 “ of the gum was just then beginning to
 “ ulcerate. As I did not know well what
 “ was best to be done, I desired him to make
 “ a strong

“ a strong solution of corrosive sublimate,
 “ and let the mouth be often washed with
 “ it, also some lint soaked in it and applied
 “ to the part; but as this did not stop its
 “ progress, she applied to Dr. Watson, to
 “ whose account I must refer the reader;
 “ and from that account I must take my
 “ materials to reason upon.”* So far the
 Professor. And before I proceed with my
 case, I shall clear up a few points with him.
 How came the Professor to be so much at a
 loss, as not to know well what was best to be
 done? He who could in a case before (the
 fourth) stop the hand of another surgeon
 from giving mercury, and who cured another
 case (the fifth) which he calls a similar,
 and which I deny to be a similar case, by
 bark only? He who in the third case, and
 which I have already quoted, could try se-
 veral things, and not find relief, till mer-
 cury was resorted to? He who can reason so
 fine, so clear, so minute, and so distinct?
 He who can correct others? He who has
 seen almost all the cases, and under whose
 own immediate wings, the fatal system of
 transplanting teeth has been fostered? He
 not to know well what was best to be done,
 therefore

therefore applies a strong solution of corrosive sublimate! The Professor did not apply a strong solution of corrosive sublimate, because he suspected the ulceration to be venereal; for how could he, when he denies that the infection could be thus conveyed; but because he knew not well what was best to be done! The practice of surgery is arrived to a fine pass indeed, when we are to put up with such reasons as these!

Having for the present dismissed the Professor, I shall again revert to the history of my case.

From the house of the dentist, the lady was removed to Knightsbridge. Mr. Pott was sent for immediately to attend her.

And here I must interrupt my narrative of the case again, to make way for a few more remarks. I am astonished that Sir William Watson never mentioned Mr. Pott by name. Sir William only says, “that she
“ was put under the care of an able and ex-
“ perience surgeon, who exerted his utmost
“ for her relief.” And the very ingenious editor of the London Medical Journal * may
PART III. A a also

* Vol. vii. Part II. p. 213.

also profit from this information, by correcting, where he says that Sir William Watſon means by this description Mr. Hunter; as the truth now turns out to be, that Sir William, under this description, could not mean Mr. Hunter, but Mr. Pott. Singular as this circumstance is, yet it is a truth, that, notwithstanding the various publications this very case has produced, the name of the surgeon who almost solely attended it, has never till now transpired before the Publick. This piece of information is very material indeed; because, here is the name of the surgeon of the first professional eminence brought forth to our knowledge, and whose concern in the case could not have been publicly known, neither through the Observations on this case, made by Sir William Watſon, nor through the Remarks made by Mr. Hunter. But now again to revert to the case.

That Mr. Pott applied remedies to the ulceration, gave her bark in decoction and in substance, and also opium. That the tooth, from the beginning, never fastened, and that it was now out of her mouth. That the Professor never saw her at Knightsbridge. That

Sir

Sir William Watſon was acquainted in the family. That Sir William Watſon ſaw her at Knightsbridge. That it is a certain aſcertained fact, that Mr. Pott and Sir William Watſon conſulted together five or fix times only upon the caſe. That the ulcerations did ſpread under the uſe of bark. That the ulcerations extended along the palate to the uvula. That the maxilla ſuperior was a caries. That a fever was on her. That eruptions came out. That ſome of theſe formed into ulcers. That on the ſcapula and knees, theſe ulcers were corroding and cavernous. That nodes appeared upon the bones. That theſe nodes appeared on both radii, as alſo on both tibiæ. That ſhe had nocturnal pains. That Mr. Pott gave her mercury internally, and applied it afterwards by friction. That ſhe was in a downright ſalivation. That all the ſymptoms yielded to mercury. That ſhe was removed into her native country, perfectly free of all venereal ſymptoms, and there died hectic. That Mr. Pott conſtantly attended her from the beginning to the end, and that he hath not the ſmalleſt doubt, but that the caſe was truly venereal.

Before I proceed to make more general remarks, I must first of all give my reasons to my readers, why, although opportunities have offered, I did not apply to Mr. Pott, though I knew he attended almost wholly this case, for a more minute information of the coming on of the symptoms, of the destruction of the parts, and of their yielding altogether to the effect of mercury. I have already asserted, that I chose rather to state my general facts, collected from a gentleman who was not of the profession, but nearly connected with the family, and who had taken much pains to inform himself of the case, because he was more than coldly interested. And these facts, thus collected, are enough to satisfy me. These general truths, together with those given in by Sir William Watson, prove all that I wanted, that the bark and opium, and other remedies, given under “an able and experienced surgeon,” as Sir William very emphatically expresses him to be, failed. That mercury was then appealed to, and that mercury in this, as in all other venereal cases, effectually removed and cured the disease.

I have

I have here ascertained, that the ship struck upon a rock, owing to the ignorance or obstinacy of her original pilots on board. I have here ascertained the rock that the ship struck upon. I have ascertained in general the damage that she sustained, the order in which the rigging gave way, and how she was at last got off. I have here furnished myself with such material evidence, as to justify me in the most solemn manner to declare, that the man who now doubts that this case was venereal, must be a despicable ideot, or a self-important, audacious, and dangerous impostor,

Sir William Watson says, “ that the progress of this putrid disease not being impeded by the most powerful antiseptics, in liberal doses; and its giving way to mercurials, even in small ones, cannot but furnish room to suggest, that the taint was truly venereal,” This, I think, proves what his opinion was of the case. But Sir William afterwards, with great diffidence, leaves the case for discussion, and offers it to professional men, as a proposition.

And

And now for my remarks upon the Professor's defence of the case as it was given in by Sir William Watson; for to "that account, he says, he must take his materials to reason upon."

"The case," says the Professor, "appears
 "to be supposed at last to be venereal, what-
 "ever might have been the first opinion,
 "and for the two following reasons; first,
 "from the mode of catching the disease
 "being possible; and secondly, from its
 "not giving way to medicines which are of
 "no service in the venereal disease; and this
 "opinion appears to have been confirmed
 "by the disease giving way to mercury."

I shall first of all request that my readers will take notice, that the Professor is at last driven to the necessity of admitting, what he has never before admitted throughout the great work! that "the mode of catching
 "the disease was possible!" I am aware that here he speaks the opinions of others; but he has already confessed, "I believe we must
 "allow, if these are venereal, it must have
 "been the lues venerea in the person from
 "whom the tooth was taken." That surely
 is

is admitting the possibility. But I care not whether he admits it or not. I lay no stress upon it. We do not want the confession from the person charged, to prove his guilt. So unfortunate is the Professor, that here he cannot even make use of his former plea, and which he invoked in his fourth case ; for the tooth was out long ago, and others also. What he with his usual shyness calls “ medicines “ which are of no service in the venereal “ disease,” were bark and opium. If they be of no service in any condition of the disease, the Professor then himself has most grossly insulted his readers. Bark was given, the symptoms increased under its action. All the freedom that can be exacted by the Professor was given to other remedies, but without the least abatement of the virulent symptoms. The symptomatick fever raged. Eruptions and other symptoms then followed. Mercury could then be given ; mercury was then given, and the venereal symptoms yielded to it. If mercury had been given in the first instance, and the symptoms had then increased, the Professor would have denied that the disease was venereal ; he would have then called it the “ new disease.” If mercury had been given in the first instance,

and

and the symptoms had yielded to it, then the Professor would have said, that the disease would have done well without mercury. For thus has he said in his Comment upon the Fourth Case: " If that the lady had gone
 " through a course of mercury, she would
 " have in all probability also got well." But here other remedies had their scope. They failed. The disease rapidly increased during their use, and only gave way to mercury. Where then is there a twig for the Professor to grasp at?

The Professor complains, that " attention," by Sir William Watson, " had not
 " been paid to the necessary circumstan-
 " ces sufficient to determine it to be vene-
 " real." But yet we find that the Professor is informed of enough from him to ground disputation upon, and that it arises out of the reports, as given of the case by Sir William. For the Professor, now that he has yielded up the first point, now that he has so far given way as to admit that " the
 " catching of the disease thus may be pos-
 " sible," seems determined to fall like a hero, to fall in the field of battle, and in the last ditch!

" The

“ The case does not perfectly agree with
 “ the common attending circumstances of
 “ the venereal.”-----“ The progress of the
 “ ulceration in the mouth, which was the
 “ first symptom, was by much too rapid for
 “ a venereal ulcer in common ; for it must
 “ be considered, if venereal, simply as a
 “ chancre, or local affection.” I agree with
 the Professor, that in the first instance the
 case was local. And in order to indulge him
 as much as I ought in his defence, I will see
 how soon he defines a chancre to come on.

“ I have known cases where the chancres
 “ have appeared in twenty-four hours, and
 “ I have known them seven weeks.” As to
 the rapidity of the symptoms, if he means
 virulence of symptoms and in a short time,
 why then I am extremely sorry, for his own
 honour, to find that he is driven to such a
 subterfuge; for argument I shall not call it.

“ Now,” says the Professor, “ let us trace
 “ the progress of the disease into the con-
 “ stitution.” And here his desponding spi-
 rit seems to revive. He here quotes this pas-
 sage from Sir William Watson’s Case. “ When
 “ the local disease was making rapid progress,
 “ blotches appeared on her face, neck, and

“ various parts of her body ; several of those
 “ became ulcerated painful sores”---“ Now,”
 says the Professor, “ this date of the consti-
 “ tutional affections following the local, is
 “ by much too soon to be venereal. We
 “ know if a lues venerea arises from a go-
 “ norrhœa,” (Does he talk of gonorrhœa !)
 “ or chancre, it does not appear in common
 “ till about six weeks ; often much later,
 “ but seldom sooner.” And does the Pro-
 fessor pretend to be serious in this argu-
 ment ? Does he presume, that such an ar-
 gument can have the least weight with pro-
 fessional men ? Has he such an idea of hu-
 man ability, that the scale of it will be found
 so low as to descend to such a servile and
 implicit submission, to receive such an igno-
 rant or mean opinion, because that it is dic-
 tated by him ?

The tooth which was drawn, exposed the
 the part from whence it was taken. The in-
 fection conveyed by the transplanted tooth,
 was immediately active in a double capacity.
 Instead of waiting the process of the com-
 mencement of destruction of parts, which is
 the beginning of chancre on the glans penis,
 when it was previously in a sound state,
 here

here the virus was instantaneously communicated to lacerated blood vessels. The effect from that communication as instantaneously took place. The common medium of introduction of poison, by absorption, perhaps had but a part of concern in diffusing the infection into the blood at first. No bubo came on. But the local spreading virulent ulceration, still added to the constitutional affection. And hence it appears, why that the corrosive sublimate, applied in a strong solution by the Professor, failed. For his view was similar to the same, in his treatment of chancre. He meant to destroy the local infection, conveyed to the part by the transplanted tooth, in a private compendious manner ; by the first intention ; by a *coup de main*. But the disease had already gone beyond the Professor's " specific bounds ;" it had already gone into the blood. The Professor was ensnared by one of his own toils. For the symptomatick fever soon after was found to be rapid in its advances. The Professor's secret intention was thus defeated. And although unfortunate indeed for the unhappy victim whose fate I deplore, yet I hold it fortunate for society, that this case hath thus come to light ; for, what further

proofs might have been exacted to convince determined skepticism, or a man so overheated in the pursuit of interest, as to lose sight of humanity, I cannot fathom !

The Professor assigns, as another reason for his not believing that the disease was venereal, because that the symptoms, which were rapid, yielded to too little quantity of mercury. If he means to proportionate the requisite quantity of mercury by calculation, instead of by effect, I shall assure him, that the calculation may be erroneous, when the effect cannot be so. It is with much more truth, that we can determine upon the necessary quantity of mercury to be given, by the effect that it hath on removing the venereal symptoms, than by any previous assertions of calculated quantity that may be necessary. This point has already been contested. The appeal is beyond the reach of us both. It is now before the profession, to be determined upon from the result of their theory and practice. Calomel was given for ten days, in small quantities. As much mercury was rubbed in as the lady could bear, for twelve days longer. She was salivated by mercury. The venereal symptoms, in consequence

sequence, all disappeared, and when nothing but mercury could cause them to disappear. Let then the Professor, if he can, "rail the seal from off this bond!"

"When we consider, that the girls from whom the teeth were taken, had not the least appearance of disease at the time, and had none when the disease broke out in the person who received the teeth, it becomes strange, that it should break out in the receiver, and not in the giver." This is the sheet-anchor upon which the Professor rests his hope. If the blood, when contaminated, could not impart that contamination to another, it would then be impossible that the disease could break out in the receiver on a local part, when no local part was found to be affected in the giver. But as contaminated blood, from the facts that I have already given, can convey the infection, it only remains for me further to prove, that poisons received by a fresh person will be more active and rapid, in their effects, than they were with the giver. These "young girls," as the Professor calls them, exposed to venereal infection, remain a long while without any formed attack, whilst that the infection is cir-

circulating with the blood, yet a fresh person receiving the infection from them shall suddenly feel all the virulence of the disease. This is consistent with the laws of all poisons, or when putrid diseases prevail, no nurses would ever be safe; on the contrary, they would be early victims. Yet these nurses, apparently well, may infect a fresh person, not accustomed to breathe the infectious air of prisons or foul hospitals; and that person may experience the full force of the virulent infection. *

But the Professor dwells very much upon this; that as the local symptom of infection was a chancre on the gum, near to the transplanted venereal tooth, so the gum from whence the tooth was drawn, should have had a chancre on it also. That the infection might have been conveyed that way, I will admit; and that it was not so conveyed, I will admit also. But because it was not conveyed that way, is every other way precluded? Does not the Professor feel his mistake? or hath not yet his dentist drawn the error out of his head? Is he not aware that

* Vide the Paper by Dr. John Hunter, London Medical Transactions, vol. iii.

that, in this fatal instance, the local infection could not have been received in the same form that it was given? In the giver, the blood was contaminated. In the receiver, the gum locally partook of the contamination. But where is this young girl? Hath she been produced in evidence by and for the Professor? Have we been assured after her tooth was drawn that her gum healed up sound? Why had she not been fought out? Why had she no part allotted in the defence of the Professor? Why was she not seen even in the back ground of the case? She, at least, owed obligations for the Professor's handsome report of her, and at any rate through him received, "*dulce munus pro munere*" "*amaro.*"

Is it not experienced every day in hospitals, that lues venerea is often first found lurking in the habit from a wound, which had been received by accident, assuming an ill condition? Even when a venereal symptom is local in the giver, must it necessarily be the same local symptom in the receiver?

One more quotation from the Professor, and then perhaps our literary contest upon
this

this subject is done for ever. For the great work has long enough lain exposed in publick state, I am now about to surround it with a winding sheet, and to screw down the lid of the coffin! “ But it must be nearly
 “ the same thing to those who want to have
 “ teeth transplanted, whether my reasoning
 “ is just or not; for a disease in consequence
 “ of the operation, most certainly has taken
 “ place; and in some cases this has been
 “ worse, or cured with more difficulty than
 “ the lues venerea in common.” Through whose theory was it that this disease was created? Under whose new opinion was this system of transplanting of teeth revived? Hath not the Professor suffered another evil to escape from out of the box of Pandora? Cadmus transplanted teeth, and propagated warriors.—The Professor and his dentist transplanted teeth, and propogated lues venerea!—The Professor and his dentist have not even the *Cadmea victoria* to boast of! But how came the Professor to be the obsequious attendant upon transplanters of teeth? What business had he there, of all professional men? When he denied the possibility of infection, at the same time that he received pay for guarding that possibility!

Let

Let him reconcile this as a man of honour ought !

The Professor's whole system of the lues venerea is founded upon this basis : That blood contaminated with venereal virus cannot contaminate a sound person. It only now remains for me to declare that that system is both false and dangerous ; and having proved as much, I trust that the Professor will hereafter insult us with no more of his new opinions. Fallen ambition hath often experienced a safe retreat, and received an unexpected comfort in lowly humiliation. The rash pilot that is wrecked at sea, consigns himself to a plank, and prolongs his life by implicitly yielding to the power of the waves ; for if he struggled, they would no longer pass smoothly over him, and break beyond him ; cheered by the sight of a friendly bark, he is submissive until he be picked up ; he will not dare the proud power of the waves ; they would dash against his vain but feeble resistance, and indignantly precipitate him to the deep !

I must now address myself in particular to the transplanters of teeth in this country.

To them I shall only give my charge. They are not to expect that I shall go into argument with them. They are to take their instructions from that which I have already preferred before their master, the Professor. He being already defeated, they must of course surrender at my discretion.

Tigris vince, levemque passerinum.

*Nulla est gloria præterire asellos.**

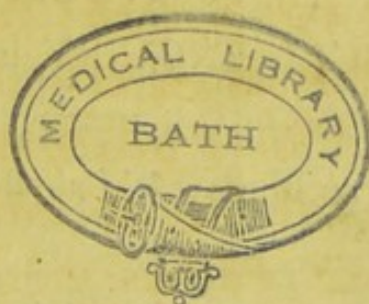
I must insist that they do either abandon this operation altogether, or take upon themselves to extract the tooth to be transplanted only from a person in whom they can by means of family, character, residence, and age, absolutely confide; that there may not remain in consequence a cause for suspicion of thus conveying the infection. If it be said, that only loose young girls, not befriended, nor protected, offer themselves for such a sacrifice; if it be said, that young girls, who have parents that feel for them, as they feel for themselves, that spurn at the humiliation as they would spurn at their seduction; that would as soon consent to the immolation of the whole of their persons,

as to such a part, in such a cause ; that would, like the father of Calista, in the Fair Penitent, rather lead their daughters to death than permit them from whatever necessity, to degrade thus human nature whilst living ; if it be said, that from these reasons, no other subjects are offered up as sacrifices to pride and avarice, but young and unprotected girls, why then I shall be able to fix my charge, and to deliver that fixed charge without hesitation. For it is neither the dread of an individual, nor of any particular class in society that shall swerve me from preferring the truth. For to the cause of truth, to my own honour, and to publick safety, all lower considerations must give way. For if at my time of life I were not equal to support this pretension, I should be unworthy to write, unworthy of the confidence of the world, and so far from profit being derived from the struggle that I have made hitherto in the support of truth against strong opposition, if that my actions had not been in harmony with my feelings, I must long since have given up the contest.

I shall now give in this my charge to these transplanters of teeth. Notwithstanding they have hitherto, from want of knowing better, been buoyed up and countenanced in the practice; notwithstanding the authority of the Professor at their elbow may have hitherto exculpated them in the opinions of moderate men; and notwithstanding hitherto they are, as they ought to be, pardoned and acquitted; yet seeing now that the case is altered, that the authority of the Professor is no longer of any validity, that the whole of the Profession revolt at it, because that the errors, which he is charged withal are proved against him, it only remains for me to forewarn them and friendly to instruct them, that, whether they are hereafter instigated by the continued presence of the Professor or not, if they pursue the same practice, they then pursue it with their eyes opened; that if they do not give in a previous declaration of what may happen, not only to the persons who apply to them for the transplanting of teeth, but also to their relatives and friends, and that if bad consequences do after follow, then they must not expect that that which they had taken on themselves, in defiance of general professional

professional opinion, on the hour of trial, should meet with professional support. They can neither then come within the pale of their compassion nor of their protection,

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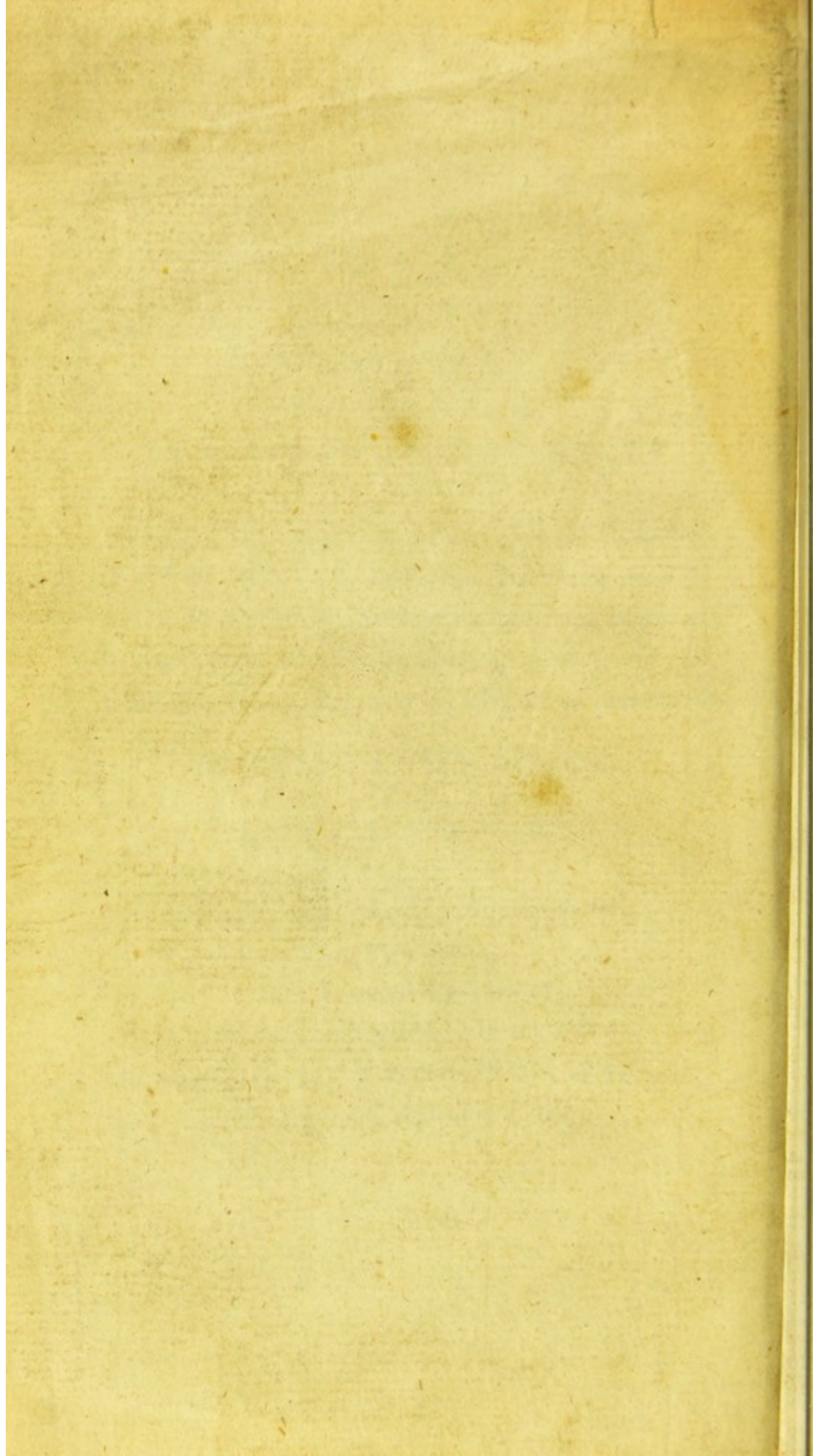
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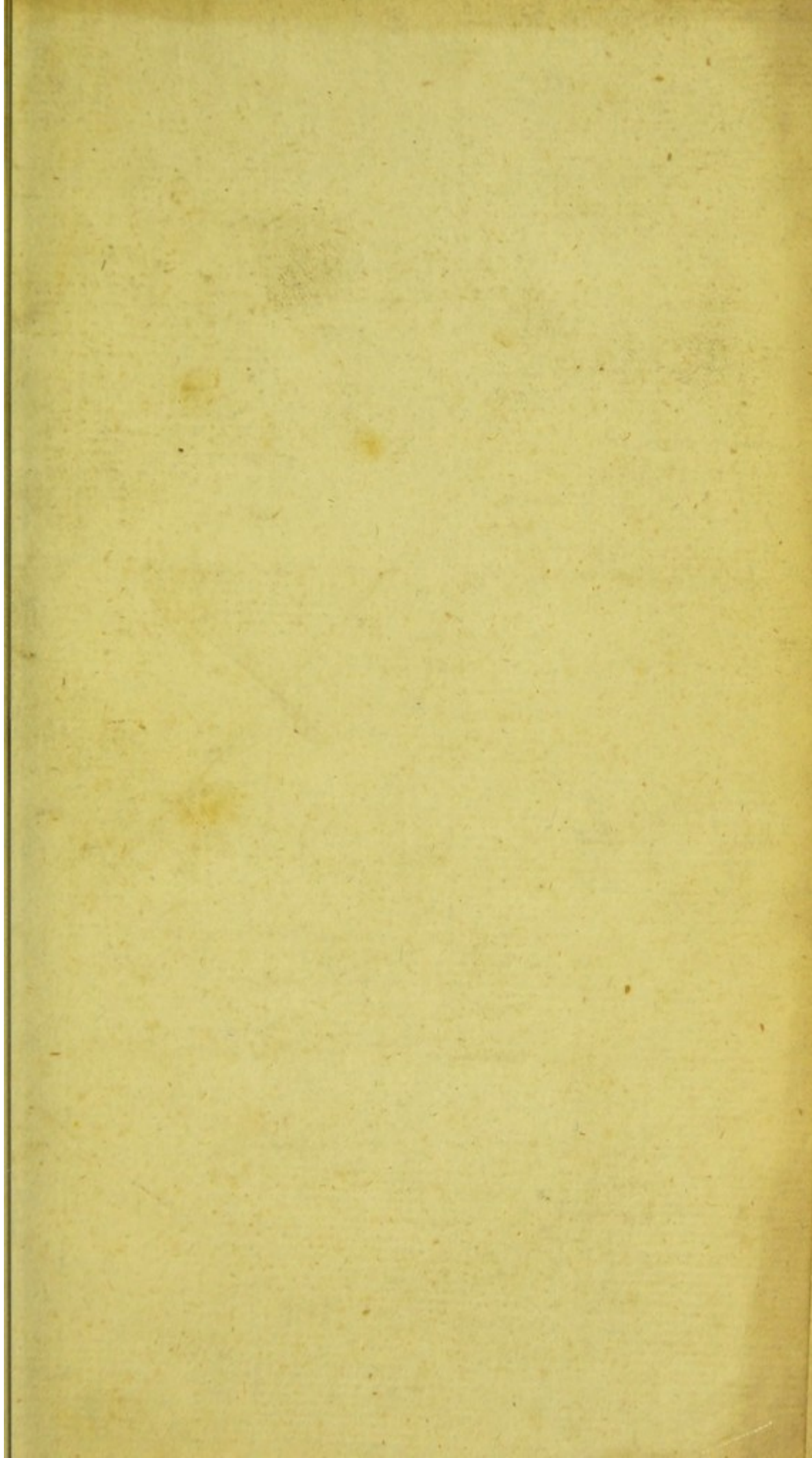
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