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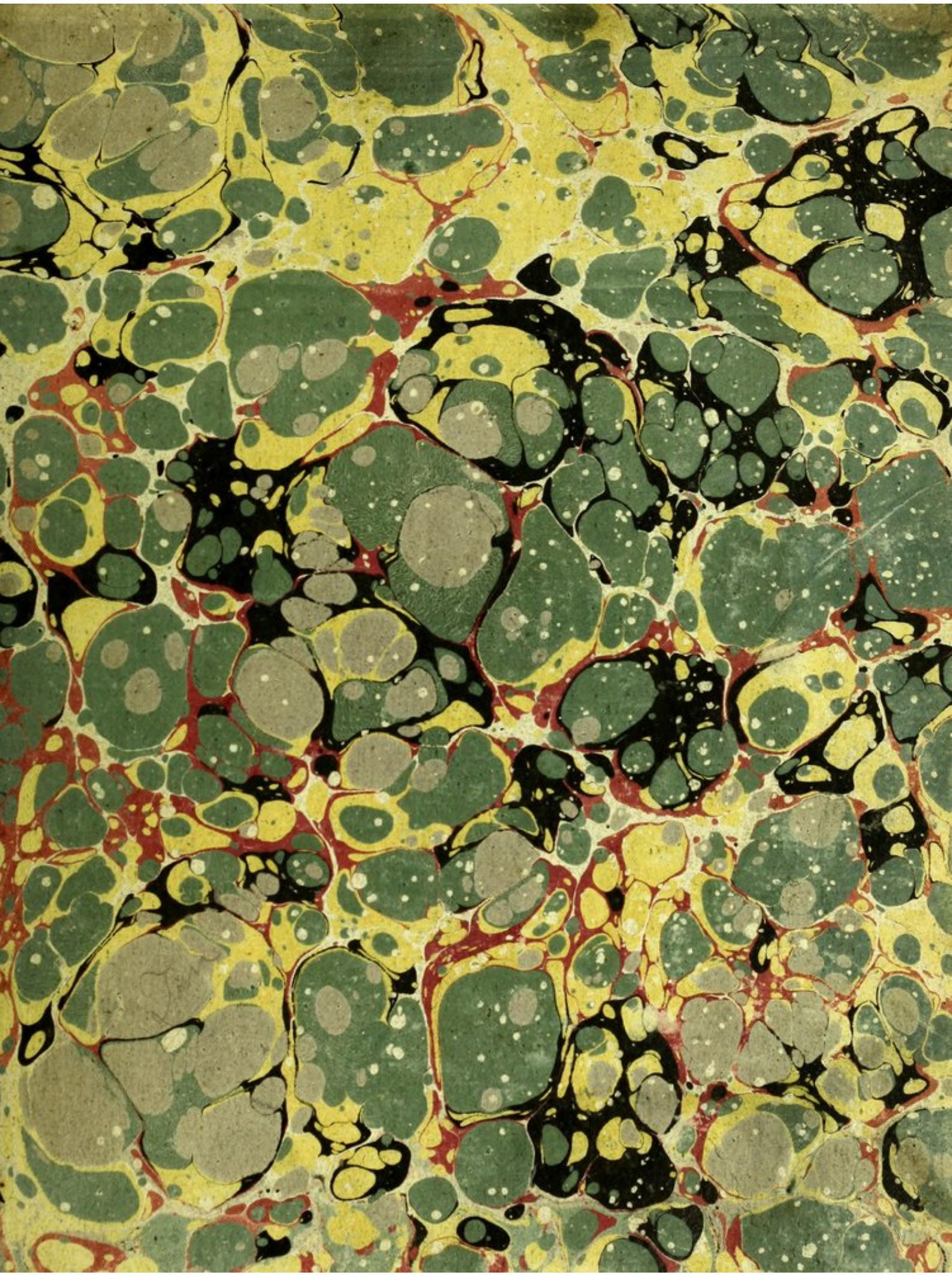

A

COMPLETE TREATISE,

ON THE

ORIGIN, THEORY, AND CURE

OF THE

LUES VENEREA,

AND

OBSTRUCTIONS IN THE URETHRA,

ILLUSTRATED BY

A GREAT VARIETY OF CASES.

BEING A COURSE OF TWENTY-THREE LECTURES, READ IN DEAN-STRET, SOHO, IN THE YEARS 1790 AND 1791.

By JESSE FOOT, Surgeon.

LONDON:

PRINTED FOR THE AUTHOR;

AND SOLD BY THOMAS BECKET, IN PALL-MALL.

1792.

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TO

SAMUEL HOWARD, Esq. F. R. S.

SURGEON TO THE MIDDLESEX HOSPITAL, AND TO THE ASYLUM;

AND TO

JOHN WYATT, Esq. F. R. S.

SURGEON TO THE MIDDLESEX HOSPITAL;

THIS COURSE OF LECTURES

IS ASCRIBED,

BY THEIR FAITHFUL FRIEND,

Dean-street, Soho,
Jan. 20, 1792.

JESSE FOOT.

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ERRATA.

Page 112, *line* 8, *for* anamolous, *read* anomalous
—— 141, *line* 16, *for* was, *read* were
—— 164, *line* 6, this Author, *means* Mr. Hunter
—— 167, *lino* 9, *for* make, *read* makes
—— 317, *line* 2, *for* as, *read* is
—— 451, *line* 5 *from the bottom*, *for* a, *read* as
—— 502, *line* 10, *for* parts, *read* part
—— 516, *line* 10, *for* phagædenica, *read* phagedænica
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LECTURE I.

ON THE

ORIGIN OF THE VENEREAL DISEASE.

THE intention of these Lectures is, to unfold to you the whole which belongs to the Venereal Disease. For this reason, my design is to begin with an Enquiry into the Origin of it, to lay a foundation, before I raise a superstructure.

Although an investigation of the Origin of the Venereal Disease in Europe may not be, at this time, considered as positively necessary for defining the symptoms of the Disease, or for assisting the Cure, yet, upon more general reflection, such an investigation must be judged to be an interesting point of Speculative Philosophy, as it involves, by the enquiry, the nature of Diseases produced through climates and habits of life, which are indigenous in one or other part of the globe, either at periodical seasons, or continually.

I define an indigenous disease, to be a disease prevailing in particular climates, arising out of the climate, which is succoured and promoted by the state of the air, the food, the apparel, and some other accelerating causes: a disease necessarily existing, and particularly belonging to inhabitants of a particular climate.

There is an important distinction betwixt an indigenous disease, and the same disease when propagated in another climate by the circulation of the infecting principle. An indigenous disease, or that disease which is peculiar to a place, will always, or at certain seasons, be found to prevail, because the infecting principle cannot be extinct: it must necessarily continue whilst that particular part of the globe partakes of the same solar heat, and of the same portion of rain; whilst the surface of the earth undergoes no change through revolving years, and whilst the inhabitants subsist on the same food, wear the same covering, and are governed by the same habits.

Quis tumidum guttur miratur in Alpibus—

If the Alpine Mountains were levelled by earthquake, the change of situation must necessarily put an end to the disease.

We know there is such a disease as the Plague, and we know that the Plague never rages in this island by a spontaneous

taneous attack on the inhabitants; that it is not produced through the air of the island, nor through the food, or any other first cause in the constitutions of the inhabitants: but when it has prevailed here, it has been owing to a propagation of the infecting principle, which, so far from being supported by any concurring circumstances in the climate and habits of the people, has become extinct, by those common means of precaution which were prudently taken to prevent the circulation of it. The same may be done with the Small-Pox, and the same with the Venereal Disease.

Both an indigenous disease, and a disease removed and propagated from the original spot where it was indigenous, may be epidemically prevalent at certain predisposing periods. This epidemic prevalence depends, at all times, upon circumstances which favour such a disposition. In extreme hot weather, the Small Pox is found to rage in England; and in extreme wet weather, Fluxes and Sore Throats prevail in low and damp situations. Such is the importance of situation, under the same latitude, and even in this island, relating to particular Diseases, at particular seasons, in Spring or in Autumn, that the Ague is predominant in the Hundreds of Essex, the Fens of Cambridge, and the Island of Sheppy; when, under the influence of the same seasons, it is scarcely or never experienced by the inhabitants of the chalky and mountainous counties.---

That which is termed Epidemic, therefore, might be considered as an aggravated condition of any disease to which a country is liable, and also an aggravated condition of any disease propagated in a country, when conveyed by infected individuals, or any thing else which could convey it.

It is a subject, if unaccountable, yet worthy of our attention, to reflect how diseases which are indigenous in certain specific parts of the world, are supported and propagated in climates, where, before the importation of original infection, no such disease, or any thing similar to it, had ever been experienced. The same disease is perceived the same in the symptoms, as well as in the event; but the acting principle, the first cause, the agency of infection, the distinct property, is as hidden from our comprehension, as the most unexplored subject, or *desideratum* of knowledge, which we long to know, yet cannot discover. What does that infecting medium consist of which is conveyed from one climate to another opposite climate? How does it come to pass that a disease which is generated in a southern climate, can be conveyed to a northern climate, and there produce those effects upon the constitution, which, although in point of time it was many months in conveying, could not have been otherwise ever experienced by subjects of that northern climate?

Philosophers

Philosophers have, with great wisdom, exemplified the wide distinction there is betwixt the Creator and the created, by saying, that the first saw the effects, when he proclaimed the cause; but that we are forced to attempt the tracing of the cause through the effects. It is certain, all climates do not provoke the same dieases; and we know, from consequences, that an opposite climate is not a bar to some. That an opposite climate is not a bar to some diseases, I think, can be proved to a demonstration: for, although the Plague is not generated here, yet it may be continued. The same may be said of the Small-Pox, and of the Venereal Disease. In following up the idea more minutely, it will appear, that, although the predisposing cause for certain diseases be stronger in Summer than in Winter, by which they acquire vigour, and are epidemically promoted, yet the widest difference in the seasons is not found to be adequate to a total extinction of them; for we find the Small-Pox will rage in the Winter, and the Venereal Disease, we know, is not checked in its progress, nor are the effects of it mitigated by any change of the seasons.

There seems to be something very singular in the temperament and composition of the human race, when we contemplate it, in regard to the preservation of the first of all blessings, namely, that of health. In point of true philanthropy, health of body ought to be considered the very first care, the very first study; yet the sacrifice of it

is

is found to be subordinate to, and constantly risqued at the shrine of enterprize, and in the pursuit of riches. Bales of goods have been imported from the harbours of Turkey, when the Plague has raged there. And, strange it is to be told, children, before they arrive to an age to think for themselves, to be the guardians of their own safety, are most unnaturally suffered to die by the Small-Pox, in the natural way, as it is called: instances of this most miserable reflection are constantly before our eyes, when the alternative of almost certain safety is offered through inoculation. Dangers are opposed by the baneful effects of climate, riches are plucked from the very jaws of death, and, by those who survive, the enjoyment of life is sought for, when the very springs of life are fallen into decay. Religious and commercial enthusiasm has introduced Diseases before unknown to our climate. Pilgrims returned from the Holy War, gratified and sated by devotion, and encumbered the inhabitants with the Leprosy. Mercantile crews introduced, by navigation, the Plague into our harbours. The Small-Pox and Venereal Disease found their way by the same channel. Mistaken Bigots, from mistaken notions, have soiled the sacred flame of Religion, by attaching to themselves a cause of a new disease: and restless Merchants, disdaining danger, have risqued their sailors, and the population of an island, by desperate adventure.

It is both matter of praise and reproach, to consider how some of these Diseases, to which I have alluded, are seen no more; and how others are still propagated and continued. At least, we are furnished with a wholesome proof, that it is possible to clear a country of every distemper which is not an *ab origine* disease of that country: that it is possible to destroy every disease imported into a country, which is propagated by the infection being permitted to be conveyed from one to another, and not by any pre-existing cause, either in the air, the soil, the temperament of heat, the nature or manner of the inhabitants of it.

To extirpate the Plague, every man was roused into exertion through self-defence. Necessity taught the salutary means—society founded its existence upon it. Lines of circumvallation were drawn, houses were demolished, the sick were deserted, situations were chosen pure from infection, and the elementary property of fire was brought in aid, to give liberty to those who were obscured in recluse retreats, and to secure them from future danger. Leprous subjects had separate apartments allotted for them; and time, under a persevering regulation, hath effected that extirpation of the disease which Medicine could not. As to the Small-Pox, we seem at length to be reconciled, and to have made terms with that disease; instead of shutting our doors against it, we have compounded with the misery

misery of it, after a miserable manner. Instead of softening the violence of it altogether, it is only partially done, by partial inoculation. It is an important point in human preservation to know, that there is a power of changing a contagious into an infectious disease, and I am sorry to see that power so much neglected. Many lives are still sacrificed to this contagious infection, which might have been saved by inoculation.

The subject of our future enquiry must be confined to the Origin of the Venereal Disease. I have made those Observations, as preparatory to this enquiry. I shall now proceed to lay before you the opinions of former Authors upon this question. I mean to produce to you a fair state of the arguments which have been used on both sides, to submit them to your consideration, and to offer that opinion which I have formed, through the Quotations here produced.

To establish the æra of the Origin of the Disease in Europe, it is necessary to revert to Authors. The chiefs amongst these are, Mr. William Beckett, late a Surgeon in London; and Dr. Astruc, Physician to the late King of France. There was also published, about twenty years ago, a Dissertation on the Origin of this Disease, by a Dr. Sanchez, a Physician, in France, and which has lately been translated, from an original in my possession. But as his arguments

arguments are very feeble, when compared with those of Mr. Becket, who has taken the same side in the question, I have judged it proper to give you the whole which Mr. Becket hath advanced. The papers of Mr. Becket are in the Philosophical Transactions, which are in the hands of a few, and therefore difficult to be come at. Dr. Astruc hath answered the arguments of Mr. Becket, but hath not laid his papers before his readers. An advantage, therefore, of these two Lectures will be, that the whole which is material can be seen at one view, and that you may become judges at once in the question. I shall also, to that end, give you the most material Extracts from Dr. Astruc.

PHILOSOPHICAL TRANSACTIONS, VOL. XXX.

AN ATTEMPT TO PROVE THE ANTIQUITY OF THE VENEREAL DISEASE, LONG BEFORE THE DISCOVERY OF THE WEST INDIES, IN A LETTER FROM MR. WILLIAM BECKET, SURGEON, TO DR. JAMES DOUGLASS, M D. F.R.S. BY HIM COMMUNICATED TO THE ROYAL SOCIETY, 1718.

SIR,

THE undertaking I am at present engaged in, has unavoidably obliged me to consult, among others, a great number of ancient physical and chirurgical books, written by my own countrymen: from these I took the hint, that the Venereal Disease was known among us much earlier than the æra which has been generally assigned for its rise by modern Authors; for it is believed it was not known, at least in Europe, till about the year 1494. Notwithstanding which, I determine, in the following Papers, to make it evident, it was frequent among us some hundreds of years before that date. I could mention several Physicians and Surgeons of eminence, who have been of the same sentiments with me, particularly the learned Dr. Charles Patin, who has written a curious dissertation to prove the antiquity of this disease; which is sufficient to excuse me from the imputation of having started a novelty, or being at the trouble of quoting ancient authorities, before taken notice of, from the most ancient Writers of Medicine; as the great Hippocrates,

Hippocrates, Galen, Avicen, Celsus, &c. and even the Holy Scriptures. I shall, in these and some following Papers, lay aside all those foreign aids and assistances, and trace out the symptoms of the disease, as they naturally arise, from the first infection to the last destructive period; and shew, that by searching into our own antiquities, we may be furnished with instances of the frequency of the distemper among us, in all its respective stages, before ever our modern Authors dream it had its appearance in Europe.

I shall begin with the first degree of this disease, and prove, from authentic evidences, it was anciently called the BRENNING, or BURNING; and that this word has been successively continued, for many hundreds of years, to signify the same disease we now call the CLAP; and that it was not discontinued, till that appellation first began to have its rise. The most likely method to accomplish my design will be, first, to examine those records that relate to the stews, which were, by authority, allowed to be kept on the Bank-side, in Southwark, under the jurisdiction of the Bishop of Winchester, and which were suppressed in the 37th of Henry VIII. For it is impossible but, if there were any such distemper in being at that time, it must be pretty common among those lewd women, who had a licence for entertaining their paramours, notwithstanding any rules or orders which might be established to prevent its increase. But if we shall find that there were or-

ders established to prevent the spreading of such a disease, that persons might be secure from any contagious malady, after their entertainment at those houses (which were anciently eighteen in number, but in the reign of Henry VII. reduced to twelve) we may then securely depend upon it, that it was the frequency of the disease, that put those that had the authority, under a necessity of making such rules and orders; for the same powers that granted a liberty for keeping open such lewd houses, must find it their interest to secure, as much as possible, all persons from receiving any injury there, lest the frequency of such misfortunes should deter others from frequenting them, and so the original design of their institution cease, from the entire sinking of the revenues. Now I find that, as early as the year 1162, divers Constitutions, relating to the Lordship of Winchester (being also confirmed by the King) were to be kept for ever, according to the old customs that had been time out of mind: among which there were some, viz. no Stew-holder to take more for a woman's chamber, in the week, than 14d.; not to keep open his doors upon Holidays; no single woman to be kept against her will, that would leave her sin; no single woman to take money to lie with any man, except she lie with him all night, till the morning; no Stew-holder to keep any woman that hath the perilous infirmity of Burning. These, and many more orders were to be strictly observed, or the offenders to be severely punished. Now we are assured there is no other

disease

disease that can be communicated by carnal conversation with women, but *that* which is venereal, by reason *that* only is contagious; and it is evident the Burning was certainly so: for, had it been nothing else but some simple Ulceration, Heat, or Inflammation, there would have been no contagion; and that affecting only the woman, could not be communicated by any venereal congress, and so not infer a necessity of her being comprehended under the restraining article. These orders likewise prove the disease was much more ancient than the date above mentioned; because, they were only a renewal of such as had been before established time out of mind.

But to confirm this farther, I find that, in the custody of the Bishop of Winchester, whose palace was situated on the Bank-side, near the stews, was a book, written upon vellum, the title of which runs thus, "Here begynne the Or-" dinances, Rules, and Custumes, as well for the Salvation " of Manne's Lif, as for to aschewe many Myschiefs and " Inconvenients that dayley be lik there for to fall owte, to " be rightfully kept, and due Execution of them to be don " unto any Personne within the same." One of the articles begins thus: *De his qui custodiunt Mulieres habentes nephandam infirmitatem.* It goes on, *item*, "That no Stew-holder " keep noo Woman wythin his hous that hath any Syck-" nesse of Brenning, but that she be putte out, upon the " peyne of makeit a fyne unto the Lord of a hundred Shyl-" lyngs."

" lyngs." This is taken from the original manuscript, which was preserved in the Bishop's Court, supposed to be written about the year 1430. From these orders, we may observe the frequency of the distemper at that time; which, with other inconveniencies, was " dayley lik there for to fall owte:" and the greatness of the penalty, as the value of money then was, that is laid on it, proves it was no trifling or insignificant thing.

But the bare proof of there having been anciently such a disease as was called the Burning, may be thought to be insufficient, unless we were perfectly assured what it was, and how it was in those times described: I shall therefore do it from unquestionable authority, which is that of John Arden, Esq. who was one of the Surgeons to our King Richard II. and likewise to King Henry IV. In a curious manuscript of his upon vellum, he defines it to be, a certain inward heat and excoriation of the Urethra; which description gives us a perfect idea of what we now call a Clap; for frequent dissections of those that laboured under that disease, have made it evident, that their Urethra is excoriated by the virulency of the matter they receive from the infected woman; and this excoriation or ulceration is not confined to the Ostiola or mouths of the Glandulæ Mucosæ, as has been lately thought, but may equally alike attack any part of the Urethra not beyond the reach of the impelled malignant matter. The heat before described, which these

persons

persons are sensible of, as well now as formerly, is a consequent of the excoriated Urethra; for the salts contained in the urine must necessarily prick and irritate the nervous Fibrillæ, and excite a heat in those parts of the Urethra which are divested of its natural membrane; which heat will always be observed to be more or less, as the salts are diluted with a greater or less quantity of urine; a thing I have often observed in persons that have laboured under this infirmity in hot weather, when the perspirable matter being thrown off in greater quantities, the salts bear a greater proportion to the quantity of urine, and thereby make its discharge at that time so much the more painful and troublesome.

Thus we see this very early and plain description of this disease among us, to be entirely conformable to the latest and most exact Anatomical Discoveries. Here is no tone of the Testicles depraved, according to Trajanus Petronius; no exulceration of the Parastatæ, according to Rondeletius; no ulceration of the Seminal Vessels, according to Platerus; no seat of the disease in the Vesiculæ Seminales or Prostatæ, according to Bartholin; nor in those parts and the Testicles at the same time, according to our countryman Wharton, and others, who have falsely fixed the seat of this disease, and whose notions, in this respect, are now justly exploded; but a single and true description of it, and its situation,

about

about an hundred and fifty years before any of those gentlemen obliged the world with their learned labours.

Having, I hope, sufficiently made it appear, the Burning was a disease very early among us, and given a description of it, I shall proceed to say something of the ancient method that was made use of to cure it. We are not to expect the measures our predecessors, in those early times, made use of, should be calculated for the removing any malignity in the mass of blood, or other juices, according to the practice in venereal cases at this time; because they looked upon the disease to be entirely local, and the whole of the cure to depend upon the removal of the symptoms: hence it was they recommended such remedies as were accommodated to the taking off the inward heat of the part, and cure the excoriations or ulcerations of the Urethra. The process for the accomplishing of this, I shall set down from the before-mentioned John Arden, who wrote about the year 1380; his words are as follow: *Contra Incendium. Item contra incendium Virgæ Virilis interius ex calore & excoriatione, fiat talis syringa* (i. e. *injectio) lenitiva. Accipe lac mulieris masculum nutrientis, & parum zucarium, oleum violæ & ptisanæ, quibus commixtis per syringam infundatur, & si predictis admiscueris lac amigdalarum melior erit medicina.* There is no doubt but this remedy being used to our patients at this time, would infallibly take off the inward heat of the part, and cure the excoriations or ulcerations of the Urethra, by which means what

what issued from thence would be entirely stopt; and this was all they expected from their medicines, forasmuch as they were entirely unacquainted with the nature of the distemper; and did not in the least imagine, but if the symptoms that first attacked the part were removed, the patient was entirely cured.

I shall now, as a farther confirmation of what I have advanced, proceed to prove, that by this Brenning or Burning is meant the Venereal Disease, by demonstrating that succeeding Historians, Physical and Chirurgical Writers, and others, have all along with us in England used the very same word to signify the Venereal Malady. In an old manuscript I have by me, written about the year 1390, is a receipt for "Brenning of the Pyntyl, yat men clepe ye Apegalle;" galle being an old English word for a running sore. They who know the etymology of the word apron, cannot be ignorant of this. And in another manuscript, written about fifty years after, is a receipt for Burning in that part, by a woman. Simon Fish, a zealous promoter of the Reformation in the reign of Henry VIII. in his Supplication of Beggars, presented to the King in 1530, says as follows: "These be they (speaking of the Romish Priests) that corrupt the whole generation of mankind in "your realm, that catch the Pockes of one woman, and "bear them to another; that be burnt with one woman, "and bare it to another; that catch the Lepry of one wo-

" man, and bare it unto another." But to make this matter still more evident, I am to observe, that Andrew Boord, a Doctor in Physick, and Romish Priest, in the reign of Henry VIII, in a book he wrote, entituled the Breviary of Health, printed in 1546, speaks very particularly of this sort of Burning; one of his chapters beginneth thus: " The nineteenth chapter doth shew of Burning of an Harlotte;" where his notion of communicating the Burning is very particular. The same Author adds, that if a man be burnt with an harlot, and do meddle with another woman within a day, he shall burn the woman that he shall meddle withal; and as an immediate remedy against the Burning, he recommends the washing the pudenda two or three times with white wine, or else with sack and water: but if the matter hath continued long, to go to an expert Chirurgeon to have help. In his eighty-second chapter, he speaks of two sorts of Burning, the one by fire, and the other by a woman, through carnal copulation; and refers the person that is burnt of a harlot, to another chapter of his for advice, what to do, " yf he get a Dorser or two," so called from its protuberancy or bunching out: for I find about that time the word Bubo was mostly made use of, to signify that sort of swelling which usually happens in pestilential diseases.

From hence it appears, the Burning, by its consequents, was venereal, since every day's experience makes it evident, that

that the ill treatment of the first symptoms of the disease, either by astringent medicines, or the removing them by cooling and healing the excoriated parts, will generally be attended with such swellings in the groin, which we rarely observe to happen from any other cause whatsoever.

I shall give a few more instances of this disease being called the Burning, and conclude. In a manuscript I have of the Vocation of John Bale to the Bishopric of Ossory, in Ireland, written by himself, he speaks of Dr. Hugh Weston (who was Dean of Windsor, in 1556, but deprived by Cardinal Pole, for adultery) as follows: "At this day is "lecherous Weston, who is more practised in the art of "Brech Burning, than all the whores of the stews." And again, speaking of the same person, he says, "He not "long ago brent a Beggar, in St. Botolph's parish." The same Author says of him elsewhere, "He had been sore "bitten with a Winchester goose, and was not yet healed "thereof;" which was a common phrase for the Pox at that time, because the stews were under the jurisdiction of the Bishop of Winchester. Mich. Wood, in his Epistle before Steph. Gardiner's Oration, *de Vera Obedientia*, printed at Rhoan, in 1553, gives another evidence of the Burning. And William Bullein, a Physician, in the reign of Queen Elizabeth, in a book he published, called the Bulwark of Defence, &c. printed in 1562, bringing in Sickness demanding of Health what he should do with a disease called the

French Pockes, Health answers, " He would not that any " should fishe for this disease, or to be bold when he is bit- " ten to thynke thereby to be helped, but rather to eschewe " the cause of thys infyrmity, and filthy rotten Burning of " Harlots."

I believe, by this time, I have sufficiently proved what I proposed, that the first degree of the Venereal Disease was very anciently known among us, under the title of Burning; and that you may lose no more time at present upon this subject, I shall reserve my Collections, which shew that the disease, when it came to be confirmed, was no novelty here in those early times, for a further opportunity; and detain you no longer than to express my pleasure in professing myself,

Yours, &c.

WILL. BECKETT.

London, Feb 4, 1717-18,

PHILOSOPHICAL TRANSACTIONS, VOL. XXXI.

A LETTER CONCERNING THE ANTIQUITY OF THE VENEREAL DISEASE. BY MR. WILLIAM BECKET, SURGEON, F. R. S TO WILLIAM WAGSTAFFE, M. D. S. R. AND COLL. MED. LOND. SOC. AND BY HIM COMMUNICATED TO THE ROYAL SOCIETY, 1720.

SIR,

BEFORE I engage in the principal design of this letter, which will be to prove, that the Venereal Disease, when it came to be confirmed, was frequently known among us some hundreds of years before the siege of Naples, I shall endeavour to refute the opinion of those persons, who believe it to have had its rise there, if any such shall remain, who have read over my preceding letter. True, indeed it is, that there have not been wanting several modern Authors, who have asserted it; but I determine to make it appear to be an error, as inconsiderately and hastily received, as started by some chimerical Author; who, because several Writers about that time, observing the disease to begin in the pudenda, separated it from another, with which it was before confounded, must likewise take upon him to assert its being a new distemper, and to assign a certain time and place for its rise. Now one might with all the reason in the world expect, that, if the disease had its original there, it must have been so certainly and infallibly

libly known, that there could have been no doubtful or uncertain opinions about it, but that the Physicians, who resided in or near the place, and those more especially who interested themselves so far as to write of it, must have all of them to a man, agreed upon the certainty of a thing, the knowledge of the truth of which was so easily attainable. But on the contrary, Nicholas Leonicenus, who was the first Italian Physician that wrote of this disease, and who lived at the very time, when Naples was besieged, is so far from acknowledging it to have had its rise there, from the French soldiers conversation with the Italian women, and so little did he know of its true cause, that he does not allow it to be the consequent of impure embraces. About this time it was likewise, that Pope Alexander the VI. engaged Gaspar Torella to write of this distemper. This Pope was in league with Alphonsus King of Naples, against Charles VIII. King of France, to prevent his passing through Italy, when he went to besiege Naples; yet this Author is so far from allowing it to have had its original there, that he tells us, the Astrologers were of opinion, that it proceeded from I know not what particular constellations. Nor does Sebastianus Aquilanus, who lived at that time, allow it to be any other than an ancient disease; or Antonius Scanarolius, who wrote in 1498, which was but four or five years after the before-mentioned siege. Nor do several Authors, then living, say one word about this Neapolitan story. But it seems Ulricus de Hutten, a German

German Knight, who was no Physician, positively affirms this disease to have had its rise there; but how he should come to know this, who lived at such a distance from the place, and they, who were Physicians, residing, as it were, upon the spot, be ignorant of it, will be as much credited, as his following inconsistent relation, which will sufficiently prove how little care he took to be apprised of the truth of what he wrote. This very Author tells us this disease was unknown till the year 1493, or thereabouts; that he himself had it, when he was a child, and so consequently that it was hereditary, or from the nurse. He wrote his book of this distemper at *Mentz*, where it was printed by John Scheffer in 4to, in the year 1519. Now if we allow him to be but 27 years of age, when he wrote, (for he cannot be supposed to be less, who before this took upon him to cure his father of the Venereal Disease, without the assistance of any Physician or Surgeon) he must have had the distemper upon him according to his own account, before ever it was in being. Thus we may see, how persons may be imposed upon by a hasty and inconsistent Writer, no way qualified for such an undertaking, and greedily receive in falsehoods instead of truths, if they will not be at the pains of consulting the original writings of our predecessors, the only sure method of overthrowing such chimerical and imaginary notions.

But to come to what is principally designed in this letter:

If

If I have, Sir, in my former sufficiently proved, that the first degree of the Venereal Disease was very common among us some hundreds of years before it is commonly said to have been known in Europe, there will be no reason for any body to conceive we were at that time in any measure strangers to it, when it came to be confirmed; more especially, when we consider the methods of treatment in those times, which consisting principally in topical applications, many of their patients could not possibly escape having it confirmed on them. Now when it was in this confirmed state, the Writers of those early times looked upon it as an entirely new Disease, and not a consequent of any evil before contracted, because they were not apprised, that the first symptoms being removed, and the disease to appearance cured, it should afterwards discover itself in such a manner, as should not seem to have the least analogy with the symptoms that first attacked a part which had been for a considerable time free from any misfortune. But because the symptoms are the only true characteristicks, whereby we are infallibly able to know one disease from another, it may be expected, that I produce sufficient authorities, to demonstrate they were all of them known and described by ancient physical and chirurgical Writers, just as they appear to be in the Venereal Disease at this day, if I would prove that Disease to be of a much more ancient date than is generally thought; and if I do this, I cannot but think it will be satisfactory, since

since we can have no other way of coming to a knowledge of any one distemper, than by its symptoms. The method of laying down the exact succession of them, will be impossible to be reduced to any certain and infallible rule, there being so great a variety of causes, that obstruct such a regularity; for which reason, I shall take notice of them in such order as they moſt generally appear, which was on no account to be expected from our ancient Writers, for as much as they mention every particular symptom by itself, not knowing but that they were independent of each other, and that each of them was a distinct disease. However, the proving these symptoms were in being in these early times, will be as strong an argument to prove the antiquity of this distemper, as if they had been registered in the most exact order of succession, because we shall, upon the strictest examination, find they are peculiar to the Venereal Malady only. I have, I hope, sufficiently made it appear in my former letter, that the first degree of this disease was anciently known among us by the name of the Brenning, or Burning; and that it was the same thing with what we call a Clap. The symptoms, which are usually its concomitants, are the Phymosis and Paraphymosis, both which are accurately described, and proper remedies for them set down by the before-mentioned John Arden, Esq. in another manuscript of his, curiously written upon vellum, and beautifully illuminated. The imprudent method of cure of the first degree of the Venereal Malady, is sometimes at-

tended with a Caruncle in the Urethra, which was a disease very common among us anciently: For not to mention other early Writers, our before-mentioned Author gives us the case of a certain Rector, that had such a substance, like a wart, growing in the Penis, which in another place he says frequently happens; and of another, which had such an excrescence as big as a small strawberry, which (says he) proceeded from the corrupted matter, which remained in the Urethra.

And indeed there is not any symptom of the Venereal Disease, that I find so often mentioned as this of the Caruncle, insomuch that it seems to have been more common in those early times, than at this day. But this must be certainly owing to the smooth and oily remedies they were continually injecting, which, by their relaxing and softening the fibres of the part, must necessarily dispose the contexture of small Blood-Veſſels, lodged at the bottom of the little Ulcerations, to fill with nutritious juices, and to extend themselves so, as to form such fungous excrescences; and so solicitous were they to remove these inconveniences, that they made use of several ways, by Corrosives and other methods, to accomplish this end; and a very early Writer among us, has given us a very methodical and curious tract on this subject, wherein he recommends the removing them by the Medicated Candle, which we use at this day, and lays down divers other instructions, in relation

lation to it, which makes it probably the best discourse, on this subject, that was ever yet written. The same Author takes notice of those contumacious ulcers which happen upon the Glans, and the neighbouring parts, which we now call Shankers; and the great trouble our ancient Authors found in attempting their cure, sufficiently discover them to have had their original from a Venereal Infection.

These several symptoms of the Venereal Malady our early Writers are very full in their accounts of, and others, when the disease was in a more confirmed state, to which they appropriated particular names, perhaps more significant and expressive than those imposed by modern Authors. Thus, for instance, the Buboes in the Groins they called Dorsers, which I have assigned a reason for before; and the Venereal Nodes on the Shin-Bones, they termed the Boon-Hawe, which gives us a perfect idea, not only of the part affected, but after what manner it was diseased; for the old English word Hawe, signifies a swelling of any part.

Thus for instance, a little swelling upon the Cornea, was anciently called the Hawe in the Eye; and the swelling that frequently happens on the finger on one side of the nail, was called the White-Hawe, and afterwards Whitflaw. The process our last-mentioned Author recommends, for the Boon or Bone-Hawe, is by making use of a plaister which had a hole cut in the midst, to circumscribe it; and

applying a caustick of unslacked lime and black soap incorporated together, which, with plaster and bandage, was to be secured on the part four hours, and longer, if that was not found sufficient; after this, he proceeds to the separating of the Slough, &c. This practice of his seems to have been found out by accident. For he tells us, when he was a young Practitioner, he having applied both the natural and artificial arsenic to the leg of a man, that was his patient, it so mortified his flesh, as surprised him; but by proper digestives, the Eschar coming off, and leaving the bone bare, he scraped it with an instrument for several days, and dressed it with incarnatives, designing to have ingendered flesh on it; but this proving unsuccessfull, he continued to scrape it, till he observed it move under the instrument; after which, having separated it, he found the sore covered with new flesh, and that the bone was four inches in length, two in breadth, and very thick, upon the removal of which the patient was soon cured. Thus it is probable, this observation of this great man, led our predecessors to practise the very same method; and we do at this day in our Hospitals treat the Venereal Nodes on the shins exactly as is here described, where we observe the same appearances, he so long before took notice of; and it is not in the least to be doubted, but the Bone-Hawe and our Venereal Nodes are the same disease. By the appearance of some of the last of the abovementioned symptoms, we infallibly judge the patient has had

had the Infection upon him a considerable time, and that the disease is making its gradual advances, to the corrupting and destroying the whole frame of the body. That this was the conclusion of the miseries of those persons that gave themselves up to the deceitful delights of lewd women, in those early times as well as now, I cannot better prove than by those remarkable instances you quoted from a MS. in Lincoln College, in Oxon, which you kindly communicated to me, after you had mentioned them in one of your learned Lectures in our Theatre. They are as follow, " Novi enim ego Magister Thomas Gascoigne, licet indig- " nus, sacræ Theologiæ Doctor, qui hæc scripsi & collegi, " diversos viros, qui mortui fuerunt ex putrefactione mem- " brorum suorum genitalium & corporis sui ; quæ corrup- " tio & putrefactio, ut ipsi dixerunt, causata fuit per exer- " citium copulæ carnalis cum mulieribus. Magnus enim " dux in Anglia, scil. J. de Gaunt, mortuus est ex tali putre- " factione membrorum genitalium, & corporis sui, cau- " satâ per frequentationem mulierum. Magnus enim for- " nicator fuit, ut in toto Regno Angliæ divulgabatur, & " ante mortem suam jacens sic infirmus in lecto, eandem " putrefactionem Regi Angliæ Ricardo secundo ostendit, " cum idem Rex eundem Ducem in suâ infirmitate visi- " tavit ; & dixit mihi qui ista novit unus fidelis sacræ The- " ologiæ Baccalaureus. Willus etiam longe vir maturæ " ætatis & de civitat. Londonii, mortuus est ex tali putre- " factione membrorum suorum genitalium & corporis sui,

" causatâ

" causatâ per copulam carnalem cum mulieribus, ut ipse-
" met pluries confessus est ante mortem suam, quam manu
" sua propria eleemosynas distribuit ut ego novi anno Dni.
1430." Now what those instances mentioned from Arden, or these from Gascoigne, who was then Chancellor of Oxford, could possibly be but venereal cases, I would be obliged to any body to inform me. Certain it is, no disease was ever known to be gotten by the carnal conversation of women, which first attacked the Genitals, causing a corruption and putrefaction of them, and afterward of the whole frame of the body, but that which is venereal. For nothing is more commonly known at this day, than that after the venereal engagement with an impure woman, the Penis is the part where the scene is first laid for the succeeding tragical appearances; and there, and in the neighbouring parts, do the symptoms of the disease, as its retainers, always first assemble, till the malignant poison taints the blood and other juices; which being conveyed over the whole frame of the human fabrick, if not checked, soon brings about its total corruption.

What I have further to add in relation to this, is, because we do not find the disease mentioned by Gascoigne was distinguished by any particular name, and that great numbers must unavoidably die of the Venereal Malady at that time, from the imperfect knowledge of those who had the treatment of the first degrees of it, it must necessarily follow,

low, that, when the whole frame of the body had received a taint from the venereal poison, so as to occasion its breaking out in scabs and ulcers, almost all over its surface, it must generally be called by the name of some particular disease, whose appearance had somewhat of an affinity to it. Now if we examine the nature of all the diseases that attack the human body, we shall not find the Venereal Malady, when it arrives at this state, to bear a greater similitude to any than the Leprosy, as it is described by the ancients: nay, so great was the analogy betwixt these diseases supposed to be, that Sebastianus Aquilanus has endeavoured to prove from Galen, Avicen, Pliny, &c. that the Pox is only one species of the Leprosy; and Jacobus Cataneus, a Writer almost as early as the rise of the name of the Pox, (1550) tells us, it is not only possible there may be a transition from one of these diseases into the other, but that he saw two persons in whom the Pox was changed into the Leprosy: that is, from having great pockes or pustules on the surface of their bodies, from whence the Pox is denominated to have become ulcerous or scabby. This particular state of the disease anciently, put the Surgeons to a great deal of trouble: for they, finding that these ulcers were of a very contumacious and rebellious nature, were obliged to make use of great numbers of remedies, in order to conquer the evil disposition of them. But they observed that all of them were useless, unless mercury was joined with them. Now the dressing each particular ulcer being

so very tedious, they ordered the patients to daub the ointments over the parts which were ulcerated; which done, they were wrapt in linen cloaths till the next dressing: but after a few days they were extremely surprised to find their mouths began to be sore, and that they spit very profusely; but they tell us, to their astonishment, that in a little time the sores became healed, and the patients cured. And by this accident it was, the method of salivating by unction was first discovered, which is in so much use among us at this day.

From these, and some other instances I have given of the industry and application of our predecessors, and with what sagacity they applied every accidental hint, to the relieving their distressed fellow creatures from the misfortunes they laboured under, we ought to be led to the highest esteem and veneration of them (and so much the more most certainly) forasmuch as they were principally our own countrymen, who, I can prove, not only from several persons coming from foreign parts to be cured of their diseases here in England, but for other reasons, that they excelled most of their cotemporaries in the divine art of healing. Now, although those foreign authorities I before-mentioned, might be looked upon as sufficient to convince any one, how our ancestors blended these two diseases together; yet shall I pursue my designed method, and prove, from our own Writers, long before those, that although the Pox was

not

not only among us, but in distant nations anciently confounded with the Leprosy; yet so exact were our Writers in their observations of the infectious nature of one species of that disease, and describing the symptoms, as was sufficient to lead any person to the distinguishing between them, so as to separate one disease from the other. I shall, therefore, first enquire into the manner how the Leprosy was sometimes said to be gotten in those early times, and then examine the symptoms of the disease that attacked the patient.

John Gaddisden, a very learned and famous English Physician, who flourished about the year 1340, in an excellent work of his, he entitles Rosa Anglica, speaking " de infec-" tione ex coitu Leprosi vel Leprosæ," says as follows:— " Primo notandum quod ille qui timet de excoriatione & " arsura virgæ post coitum statim lavet virgam cum aqua " mixta aceto, vel cum urina propria & nihil mali habebit;" and in another place speaking, de Ulcere Virgæ, he says, " sed si quis vult membrum ab omni corruptione servare, " cum a Muliere recedit, quam forte habet suspectam de im-" munditie, lavet illud cum aqua frigida mixta cum aceto, " vel urina propria intra vel extra præputium." He, likewise speaking still of the Leprosy, recommends a decoction of plantain and roses in wine, to be made use of by the woman, immediately after the venereal encounter; upon which, he tells us, she will be secure. From hence, it is

evident, some of their leprous women (as they called them) were capable of communicating an infectious malady to those that had carnal conversation with them, which proves, the Pudenda of the women must be diseased, for as much as we are absolutely assured infections of that nature only happen when a sound part comes to an immediate contact with a diseased one; for the symptoms always first display themselves in those parts through which the virulency is first conveyed. Now in a true Leprosy, we never meet with the mention of any disorder in those parts, which if there be not, must absolutely secure the person from having that disease communicated to him by coition with leprous women: but it proves there was a disease among them, which was not the Leprosy, although it went by that name; and that this could be no other than venereal, because it was infectious: for there is no other disease that is capable of being communicated this way but the Venereal Disease, seeing the Pudenda are only in that distemper so diseased as to become capable of communicating their contagion. I find the learned Gilbertus Anglicus, who flourished about the year 1360, reasoning concerning the manner how it is possible a man should be infected by a leprous woman; where, if we allow him to call the malignant matter, which is lodged in the Vagina (the woman's seed) we shall find he accurately describes the very first venereal infection, by part of the virulent matters being received into the Urethra; from whence, by the communication

munication of the veins and arteries, it is conveyed into the whole body, after which (says he) ensues its total corruption.

Let us now examine the symptoms of one sort of their Leprosy, for it must be necessarily divided into different species, when another distemper was blended with it, in which we observe such a diversity of appearances; and this I shall the rather do in this place, because it will furnish us with the next succession of symptoms, after those already mentioned, as the Venereal Ozænas, the Ulcers of the Throat, the Hoarseness, the proof of its being communicatable from the nurse to the child by hereditary succession, &c. All which we find to be true in the Venereal Disease at this day. Our countryman, Bartholomew Glanville, who flourished about the year 1360, in his book De Proprietatibus Rerum, translated by John Trevisa, Vicar of Barkley, in 1398, tells us, " some Leprous persons have " redde pymples and whelkes in the face, out of whome " oftenne runne blood and matter: in such the noses " swellen and ben grete, the vertue of smellynge faylyth, " and the brethe stynkyth ryght fowle." In another place, the same Author speaks of " unclene, spottyd, glemy and " quyttery, the nosethrilles ben ſtopyl, the wasen of the " voys is rough, and the voyce is hoarse, and the heer falls." Among the causes of this sort of Leprosy, he reckons lying in the sheets after them, easing nature after them; and

others, which the first Writers on the Pox looked upon to be capable of communicating that contagion: also, says he, "it comyth of fleshly lykyng by a woman, after that "a leprous man hathe laye by her; also it comyth of "fader and moder; ann so thys contagyon passyth into "the chylde, as it ware by lawe of herytage. And also "when a chylde is fedde with corrupte mylke of a lep-"rouse Nouryce." He adds "by what ever cause it comes, "you are not to hope for cure if it be confyrmyd; but it "may be somewhat hidde and lett that it deſtroye so "soon." Thus we see how our Author, under the name of one species of the Leprosy, gives a summary of the symptoms of the Pox, and the several ways whereby it is at this time communicated. Now when these two diseases were anciently blended together and passed under the name of the Leprosy only, it must be the real cause why that disease seemed to be so rife formerly; for two distempers passing under one name must necessarily make it more taken notice of and much more frequent; not but that much the greater number of those who were formerly said to be leprous, were really venereal, seems to be very evident; for since that disease has been separated from the Leprosy, it has drawn off such vast numbers, that the Leprosy is become, as it were, a perfect ſtranger to us.

Those that are acquainted with our English History well, know the great provision that was anciently made through-

out

out all England for leprous persons, insomuch that there was scarce a considerable town among us, but had a Lazar-House, for such diseased. In a Register which belonged to one of the houses, I find there was in Henry VIII.'s time six of them near London (viz.) at Knightsbridge, Hammersmith, Highgate, Kingsland, the Lock, and at Mile End; but about forty years before, I find but four mentioned; and in 1452, in the will of Ralph Holland, Merchant-Taylor, registered in the Prerogative-Office, mention is made but of three, which, with his legacies to them, are as follow: "Item lego Leprosis de Lokes, extra Barram Sti. Geor-"gii 20s. Item lego Leprosis de Hackenay (which is "that at Kingsland) 20s. Item lego Leprosis Sti. Egidii ex-"tra Barram de Holborn 40s." From which it is worth while to note, that the Lock, beyond St. George's Church and that at Kingsland, are at this time applied to no other use, than for the entertainment and cure of ſuch as have the Venereal Malady. Some of our learned Antiquaries have been much concerned to know the cause why the Leprosy should be so common in those early times, and so little known among us now: But I believe the reason will be impossible to be assigned, unleſs we allow, according to the proofs which I have already brought, that the Venereal Disease was ſo blended with it, as to make up the number of the diseased. It seems to have been the same thing with them in France as with us: for the Author of the History of that Kingdom, which was lately published

published here in 2 vols. 8vo. tells us, that the House of the Fathers of the Mission of St. Lazarus, was formerly an Hospital for leprous people; but that disease being ceased in this last age, (since the Pox has been separated from it) these Lazar-Houses have been converted into other uses; and it may not be foreign to my purpose to take notice, that the Writ de Leproso amovendo, containing the Regifter of Writs, was (according to Coke upon Littleton) to prevent leprous persons associating themselves with their neighbours, who appear to be so by their voice and their sores, and the putrefaction of their flesh, and by the smell of them. Well, then, let us examine what method was to be taken to prevent this noisome and filthy distemper, the Leprosy; why, truly, that which would infallibly prevent their getting the Pox after the usual method, and that was Caftration. It is certain that Eunuchs are rarely or never troubled with the Leprosy, according to M. le Prestre, a Counsellor in the Parliament of Paris, who has these words: "Antipathia vero Elephantiasis veneno resistit: Hinc Unuchi & quicunque sunt, mollis, frigidæ & "effeminatæ naturæ nunquam aut raro Lepra corripiuntur; "et quidem quibus imminet Lepræ periculum de con- "silio medicorum sibi virilia amputare permittitur." And Mezeray says, he has read, in the Life of Philip the August, that some men had such apprehensions of the Leprosy, (that shameful and nasty distemper) that to preserve themselves from it they made themselves Eunuchs. Now, it is highly

highly probable that these persons that submitted to such a painful operation; having before observed, that those that gave themselves up to a free and unrestrained use of women, fell at length under such unhappy circumstances; and so found the only measures to preserve themselves from it, was to be disabled from such engagements, which sufficiently proves this species of the Leprosy was infectious; and, for the reasons before assigned, could be no other than venereal; for how the true Leprosy should be prevented by such means, will be, I believe, impossible for any person to determine.

There yet remains one very considerable symptom of the Venereal Malady for me to take notice of, because it is looked upon to be the most remarkable in that disease, which is, the falling of the nose; but since it has been already proved, that the disease, when it had arrived to such a pitch as to discover itself by those direful symptoms as are the immediate fore-runners of this, was, by the ancients, confounded with the Leprosy, and called by that name, it must be among the symptoms of that disease we are the most likely to meet with it, if any such thing as the falling of the nose was known among them. Now the most likely method of coming to a certain knowledge of the infallible symptoms of the Leprosy of the ancients, in its more confirmed state, is, to consult the examinations those unhappy persons were obliged to undergo, before they were

were debarred the conversation of human society, and committed to close confinement. But this being a thing some ages since laid aside, no Author, that I know of, having the particular history of it, and somewhat of it being absolutely necessary in this design, I shall do it, as briefly as I can, from what remains I have met with in records, and other scattered papers. First then, after the persons appointed to examine the diseased, had comforted them, by telling them, this distemper might prove a spiritual advantage; and if they were found to be leprous, it was to be looked upon as their Purgatory in this world; and although they were denied the world, they were chosen of God: the person was then to swear to answer truly to all such questions as should be asked; but the examiners were very cautious in their enquiries, lest a person that was not really leprous should be committed, which they looked upon to be an almost unpardonable crime. They considered the signs as univocal, which properly belonged to that disease, or equivocal, which might belong to another, and did not upon the appearance of one or two signs, determine the person to be a Lazar: and this I find to be the case of the wife of John Nightingale, Esq. of Burntwood, in Essex, who, in the reign of Edward IV. An. 1468, being reported to be a Lazare, and that she did converse and communicate with persons in public and private places, and not (according to custom) retire herself, but refused so to do, was accordingly examined by William Hattecliff, Roger Marcall, and Dominicus

minicus de Serego, the King's Physicians; but they, upon strict enquiry, adjudged her not to be leprous, by reason the appearances of the disease were not sufficient. Some of the questions put to the leprous persons (as they called them) which will more fully confirm what I have before advanced, I shall now give, as I transcribed them from an ancient book of Surgery: "Yf there were any of his lygnage "that he knew to be Lazares, and especially their Faders "and Moders, (for by any other of their kynred they ought "not to be Lazares) then ought ye to enquire yf he hath "had the company of any lepress woman, and yf any La-"zare had meddled with her afore him; and lately, because "of the infect matter and contagyous filth, that she had "received of hym. Also yf his nostrills be wyde outward, "narrow within, and gnawn. Also yf his lips and gummes "are foul, stynking, and coroded. Also yf his voice be "hoarse, and as he speaketh in the nose." Now the signs which are here mentioned, were looked upon to be univocal: and these were they that made the Examiners principally determine the persons to be leprous: but what determinations any one would immediately give from such symptoms now, no person is surely ignorant of. But even these certain appearances would not always satisfy some persons, if we may believe Felix Platenus, in his Medicinal and Chirurgical Observations, lib. 3, who tells us, some did not look upon them to be so, till they had an horrible aspect, were hoarse, and noses fell. Likewise in the Examen

Leprosorum, printed in the De Chirurgia Scriptores Optimi, the Author, speaking of the signs of the Leprosy, relating to the nose, begins thus: "Si nares exterius secundum ex- "teriorem partem ingrossentur, & interius constringantur "& coarctentur. Secundo si appareat cartilaginis in medio "corrosio, et casus ejus significat Lepram incurabilem." And the before-mentioned John Gaddisden, in his Chapter de Lepra, says as follows: "Signa confirmationis etiam in- "curabiliter sunt corrosio cartilaginis quæ est inter fora- "mina & casus ejusdem."

Thus, Sir, have I proved we had a distemper among us some hundreds of years before the Venereal Disease is said to have been known in Europe, which was called the Burning; that this Burning was infectious, and that it was the first degree of the Venereal Disease; that this being common at that time, from their method of treatment, the Pox must be unavoidable: that it had exactly the same appearances it has now, although they were generally called by different names; that the ancients confounded it with the Leprosy; that the vast numbers of leprous persons among us, before the Venereal Disease was separated from it, and the small number we observe at this time, is a flagrant proof of the former; that in describing the symptoms of the Leprosy, they give us those of the Venereal Malady; and, by mentioning how it is communicated, they describe the ways by which the Pox is gotten at this day; that such remedies

medies were by them recommended to prevent the first attack of the Leprosy, as are at this time in use to prevent the first symptoms of the Pox; and that the falling of the nose, which has been looked upon to be the most remarkable symptom of the Venereal Disease, was commonly observed in what they called the Leprosy in former ages.

I am, Sir,
Yours, &c.
WILL. BECKETT.

PHILOSOPHICAL TRANSACTIONS, VOL. XXXI.

A LETTER TO DR. HALLEY, ASTRONOMER ROYAL, R. S. SOC. IN ANSWER TO SOME OBJECTIONS MADE TO THE HISTORY OF THE ANTIQUITY OF THE VENEREAL DISEASE. BY MR. BECKET, SURGEON, F. R. S.

SIR,

I Was of opinion that what I had said in my two former Letters had been so full and satisfactory, that I should have had no occasion to have given myself or any body else any farther trouble upon this head: but forasmuch as I find there have been two objections made against what I have advanced, by one or two learned gentlemen, I shall take upon me to answer them, and endeavour to prove they do not in the least invalidate the authorities I have before produced. The first is, that the Venereal Disease, so well known among us now, and the Leprosy of former ages, could not be the same disease, because the Leprosy is not to be conquered by salivation, which the other generally very readily yields to. In answer to this, I am to observe, that the Leprosy, which we have among us as this time, affects only the surface of the body, the skin generally appears scaley, with a certain deep red colour, or small sores upon removing the scales, and sometimes a scabbiness, with a redness of the skin, which affects different parts of the body. I have known both

both the cheeks only affected, both the arms for the breadth of the palm of the hand, sometimes the breast, the legs, and other parts; but this may continue upon the patient during his life, as it frequently does, and never makes any farther progress; which shews it to be a cuticular disease: in these cases, upon salivating the patients, the scales generally fall off, the redness disappears, and the cure shall seem to be compleated; but in a month or two, the same inconveniencies generally attend them as before. But one ought not to conclude, that because our Leprosy will but rarely be cured by salivation, and the Pox generally will, that many of those persons the ancients judged to be leprous, were not really venereal; for their Leprosy, as they called it, was quite a different disease from ours. Had there been any proof brought that persons had been salivated in their Leprosy, and failed of cure, it would have determined the case; but on the contrary, we are assured by the learned Dr. Pitcairn, in his Dissertation concerning the Ingress of the Lues Venerea, that the Leprosy, before the Neapolitan Disease was talked of, was cured by Mercury, and now since it changed its name, it is no longer heard of. Thus we find that their Leprosy and our Venereal Disease would be cured by the same method; but their Leprosy and ours, being absolutely different diseases, we by no means ought to expect the success, from the same process of cure, should be the same. I dare be positive that no body ever observed our Leprosy to be attended

attended with falling of the hair, hoarseness of the voice, the patient speaking as though he spoke through the nose, consumption of the flesh, ulcers all over the body, corruption of the fleshy parts, and of the bones themselves, filthy ulcers of the throat, corrosion and falling of the nose, all which are reckoned as symptoms of their Leprosy. On the contrary, ours is a mild and almoſt inoffensive disease, which a person may be affected with during his life, and never become worse; whereas the other by displaying itself under the symptoms before enumerated, brings the patient to the most miserable end; besides this, their disease was got by coition, as their Authors assure us, but in our Leprosy, a diseased husband may cohabit with his wife as long as he lives, and he shall never be able, either by coition, or the immediate contact of the diseased parts with those that are sound, to communicate any evil. Had what our predecessors called the Leprosy been the same disease we call by that name now, they had not been so solicitous of making such large provision for them, or shutting them up from humane society; for one of our leprous persons might have been among them, and no body have known he laboured under any infirmity at all. From hence it is evident the disease so common among them, was entirely different from our Leprosy, the appearances of which bear no manner of analogy with the former. It is from the symptoms of the disease, and the manner of its being received, that we generally know one disease from another; but the symptoms

toms of most of their leprous persons, and the manner whereby the disease was gotten, will be found in no other disease that attacks the human body, but in the Venereal Disease only; for here they so exactly agree, that we must in a manner do violence to our own reason, if we deny them to be the same.

I proceed now to answer the second objection, which indeed was long ago falsely asserted by Dr. Fuller, the Historian; which is, that the Leprosy was brought into England from the Holy War, by some of our countrymen, and that the disease was altogether unknown among us before. This, as I take it, does not so immediately concern me, since all I take upon me to prove is, that what they called the Leprosy, is not the same disease we call by that name now, but another. However, I shall in a few words make it appear that this objection is likewise groundless, by observing that the first Englishmen that went over to the Holy War, made their first voyage in the year 1096, as our Historians generally agree, and that some of them returned in 1098, two years after that expedition; but most certain it is we had the Leprosy among us before, for Wharton, de Episcopis Londinensibus, and other Historians, assure us, that Hugo de Orivalle, one of the Bishops of London, died here of the Leprosy in the year 1084, which proves our countrymen did not bring that disease first from the Holy War, because we had it among us before. The account

count William of Malmsbury gives of this Bishop's disease, is as follows: "Is post paucos ordinationis annos in morbum "incurabilem incidit. Siquidem regia Valetudo totum "corpus ejus purulentis ulceribus occupans ad pudendum "remedium transmisit. Nam credens asserentibus unicum "fore subsidium si vasa humorum receptacula, verenda "scilicet, exsecantur, non abnuit. Itaque & opprobrium "spadonis tulit Episcopus, & nullum invenit remedium, "quoad vixit leprosus." Now it is highly probable, had this been a new disease the Bishop died of, the mention of it as such, would not have escaped our Historian; but on the contrary, it seems to have been anciently known among us, because the remedy made use of for it was so, it having been recommended by Ætius, and other Physical Writers, several hundred years before this time; and I think it is very plain that the cutting off the Testicles, and with them the vessels formed for the receiving the humours, as expressed in the former case, was by them looked upon to be of peculiar service, because it is probable that observing the disease to begin in these and the neighbouring parts, they thought the very Minera Morbi would be by this means destroyed, and the disease either cured or the spreading of it prevented.

I am, Sir,

Yours, &c.

WILL. BECKETT.

LECTURE II.

THE TESTIMONIES LATELY PRODUCED BY MR. WILLIAM BECKET, IN FAVOUR OF THE ANTIQUITY OF THE VENEREAL DISEASE, EXPLAINED, BY DR. ASTRUC.

THE last person who has appeared in defence of the Antiquity of the Venereal Disease, is Mr. William Becket, a Surgeon in London, in three Dissertations, published in the xxx^th^ and xxxi^st^ volumes of the Philosophical Transactions, in which he has with great diligence gathered together whatever could be collected from the English Antiquities, whether printed or manuscript, that might seem to favour his opinion.

In his first Dissertation, printed in the xxx^th^ volume of the Philosophical Transactions, Numb. 357, Ann. 1718, he labours to prove that a Venereal Gonorrhœa was known in England some ages before the year 1494, under the name of *Ardor*, *Arsura*, *Incendium*, &c. in English, Brenning or Burning; whereof indeed there is frequent mention made

by the English Historians. And in confirmation of this opinion, he produces several authorities, whereof some are earlier and others later than the year 1494.

The earliest of his authorities are taken, 1st. From a manuscript Treatise of John Arden, an eminent Surgeon in England in his time, i. e. about the close of the fourteenth century, wherein he speaks of a Burning, and, according to Becket, defines it " to be a certain inward heat and excoriation of the Urethra."

2. From certain physical pieces, written, as he supposes, about the year 1390 and 1440, in which are some receipts for the Cure of this Brenning both in Men and Women.

3. From the manuscript rules and ordinances of the stews, that were by public authority allowed to be kept at London, in the Borough of Southwark, and are supposed to have been drawn up about the year 1430, one of which articles begins thus: " Of those who keep women having a " wicked infirmity," and orders under a severe penalty, " that no Stew-holder keep noo woman wythin his hous " that hath any sycknesse of Brenning."

His later authorities are taken, 1st. from the Supplication of Beggars, a book presented by one Simon Fish, a zealous promoter of the Reformation in England, and a bitter enemy of

of the Roman-Catholicks, to Henry VIII. in which, speaking of the Priests, he says, "These be they that corrupt the "whole generation of mankind in your realm, that catch "the Pockes of one woman, and bear them to another; "that be burnt with one woman, and bear it to another."

2. From a book published in 1546 by Andrew Boord, Doctor of Physick, and a Romish Priest, entituled the Breviary of Health, where one of the chapters begins thus: "The 19th chapiter doth shew of Burning of an "Harlotte;" and then he adds, "that if a man be burnt "with an Harlot, and do meddle with another woman "within a day, he shall burn the woman that he shall "meddle withal."

3. From an Epistle placed before Stephen Gardiner's Oration *de Verâ Obedientiâ*, printed at Rouen in 1553, by Michael Wood, in which mention is made of the Burning.

4. From a manuscript work of John Beale, which Mr. Becket had in his custody, wherein Bale speaking of Dr. Weston (who was Dean of Windsor in 1556, but deprived by Cardinal Pool for Adultery) says as follows: "At this "day is lecherous Weston, who is more practised in the "art of brech-burning, than all the whores of the stews."

And again, speaking of the same person he says, "He not "long ago brent a beggar in St. Botolph's parish."

5. From a Treatise of William Bulleyn, Dr. of Physick, called the Bulwark of Defence, &c. printed in 1562, wherein he treats of "the Burning of Harlots."

Here indeed we have several authorities, but few or none of them valid, for they all stand upon an unsure foundation. For not to mention, that these testimonies are chiefly drawn from unpublished records, or such books as are hard to be met with, so that we cannot either examine them as we ought, or take such a view of them as to be certain of their age or the faithfulness of the quotations made from them; omitting, I say, these circumstances, that I may not seem to cavil as distrusting my cause; and granting that Mr. Becket has been exact in his relations, which is a large concession, I do still deny the consequence he has drawn from them, that this Burning in dispute was the same disease with a Venereal Gonorrhœa, or that a Venereal Gonorrhœa, contracted by unclean coition, was formerly signified by the name of Burning. But to explain myself more fully, it will be requisite to enlarge a little upon this subject.

I. Then, the Leprosy of the Arabians, which was formerly a common disease in England, as well as in other parts of Europe, was capable of being communicated, not only

only by living in the same house, but even by visting a leprous person, insomuch that all Lepers were by several very severe edicts separated from the rest of mankind, and prohibited all manner of conversation with them.

II. And therefore, in case any lascivious person had carnal knowledge of a leprous woman, as no contact can be closer than that, the Leprosy could not fail of being thereby communicated, by an almoſt immediate infection. And that it was this way communicated, we have the unanimous testimony of almost all the physical Writers upon this subject; as of Forestus, Observat. Chirurgic. lib. 4, Obs. 8; Palmarius, de Elephantiasi, cap. 2; Paræus, Op. lib. 20, cap. 8; Fernelius, de Partium Morbis & Symptomatis, lib. 6, cap. 19; Valescus, de Taranta, Philon. 7, cap. 39; Gordon's Lilii Partic. 1, cap. 22, where he relates, "That a certain Countess, ill of the Leprosy, came to "Montpelier, and was at last under his care; and that a "certain Batchelor of Phyſick waited on her, lay with her, "and got her with child, and became perfectly leprous;" similar to which you have a story told by Philip Schopffius, lib. de Leprâ, of a Carpenter, who caught the Leprosy by lying with a leprous woman.

III. But if through good fortune the case did not prove quite so bad, yet the private parts, at least, of such persons as had to do with leprous women, or such as though sound themselves

themselves, had lately conversed with leprous men, were for the most part affected with an Inflammation, Erysipelas, Herpetick or miliary Exulcerations, cuticular Eruptions, &c. whence arose a Dysuria, in the language of that time called Ardor, Arsura, Incendium, Calefactio; in English, Brenning.

IV. In confirmation whereof we have several sufficient eye-witnesses to produce. As,

1. Theodoric, a celebrated Physician in the year 1290, who in his Chirurg. lib. 6, cap. 55, says, that whoever converses with a woman who has lain with a Leper, will catch the distemper.

2. The Author of a manuscript Treatise of Surgery, called Rogerina, by some thought to be Roger Bacon's, but in the opinion of the very learned Dr. Friend, Hist. of Physick, Part. 3, more likely to have been written by Roger of Parma, in which, as we learn from Dr. Friend, ibid. we have an account of the disorders which follow from coition with a woman, who had lately conversed with a Leper.

3. Gilbert, an Englishman, who in Compend. Medicin. tam Morborum universalium quàm particularium, gives the same description we find in the Rogerina.

4. Bar-

4. Bartholomew, commonly named Glanville, whose Breviarium Medicinæ, extant in manuscript, agrees so well with the Compendium of Gilbert, that it may seem to be the same work. In this Breviary, lib. 2, cap. 4, according to Dr. Friend, there is the same account, almoſt word for word, concerning "the danger of lying with a woman "who has lately had to do with a leprous person," as in the Compendium of Gilbert.

5. John of Gaddisden, an English Physician, who in Pract. Medicinæ, seu Rosa Anglica, has a distinct chapter, "de infectione ex Concubitu cum Leproso vel Leprosa," where he says, "that he who lyes with a woman, who has "had to do with a Leper, will suffer punctures between the "flesh and the skin, that is, between the Glans and Prepuce, "and sometimes heats all over the body."

6. Lastly, John Manardus, of Ferrara, a famous Physician of his age, in Epiſtolar. Medicinal. lib. 7, Epiſtol. 2, published in the year 1525, says, "That they who lye with "a woman, who has lately had to do with a leprous per-"son (for thus they call one who is infected with the "Elephantiasis) by the semen still remaining in the Ute-"rus, are smitten with the Elephantiasis, and sometimes "not, as also other complaints of a milder, or harsher "nature, according as the woman herself is affected, or the "Leper who infected her."

V. Hence

V. Hence, therefore, may easily be explained the three authorities objected by Becket, which are earlier than the year 1494. For that Burning, which, 1st. is spoke of by John Arden, about the year 1370, and against which, 2d. there are extant certain manuscript receipts, written about the years 1390 and 1440, and of which, 3d. mention is made in the manuscript Rules or Ordinances of the Stews in London, drawn up about the year 1430, is the very same disorder with that which we have proved to arise from having to do with a leprous woman, or one who had lately lain with a leprous man. And as to the *nefanda infirmitas*, which occurs in the same Rules, I take it to be the Leprosy itself.

VI. Farther, it ought by no means to seem strange, that women should formerly be frequently infected by coition with Lepers. For as many who had the Leprosy lay either undiscovered, or were carelesly sought after, and consequently not hindered from conversing with the uninfected; and as many of those, who were separated, were negligently kept, and by that means had sometimes opportunities of getting abroad, it was easy for them to indulge the strong inclination they had to venery from the nature of their distemper, at a time when public stews were allowed by authority, and especially in populous cities, where an unknown person might without much difficulty keep himself concealed. What is most surprizing is, that this complaint should be more common in England than in other

other parts of Europe, and be more frequently mentioned in the Writings of English Historians and Physicians; though this might perhaps be owing to the laws against Lepers being less severe in England than in other countries, or it may be not so strictly observed. But that I shall leave to the consideration of the English Antiquaries.

VII. However, this Burning could not be the same with a Venereal Gonorrhœa, from which it appears to have been manifestly different, for several reasons.

1. Though we should give up the cause so far, and own the Venereal Disease to have been a much earlier distemper than we imagine it was, yet a Venereal Gonorrhœa cannot be conceived to have made its appearance early enough to have been the same with that Burning, which was a frequent complaint in the thirteenth and fourteenth century. For it is evident from the silence of all the Authors, who have wrote of the Venereal Disease before the year 1545, and the express testimony of Fallopius, Tract. de Morbo Gallico, cap. 23, that a Venereal Gonorhœa did not shew itself among the symptoms of this disease before the year 1545, or 1546, that is, fifty years after the period we assign for the first eruption of this distemper, and consequently of much later date than the Burning we have been treating of.

2. In a Venereal Gonorrhœa (as the word itself implies) there is always a large and lasting discharge of purulent semen, which constantly attends the heat of urine, or at least follows presently after it; but nothing like this was formerly observed in the ancient Burning, or at least is not mentioned by any of the Authors who have wrote of it; and yet it is by no means probable, that a symptom of such consequence would have been omitted, if any such had been observed at that time; from whence it is very likely, that this Burning was no other than a bare erysipelatous inflammation of the Glans and Urethra.

3. A Gonorrhœa is a complaint of so stubborn a nature, that it frequently takes up a great deal of time before it can be cured, and is never cured without some difficulty; whereas this Burning was so slight a disorder, as to admit of a cure from a bare fomentation, or at most from an anodyne injection, as we learn from Becket himself, who has given us the following quotation from John Arden:— "Against the inward Burning of the Yard, from heat and "excoriation, use the following injection: take the milk "of a woman who is suckling a male child, and a little su- "gar, the oil of violets and ptisan, mix all together, and "inject them with a syringe. It will be still better if you "add a little almond milk to the above." Which prescription is of the same kind with a receipt for the burning and pricking

pricking of the Lips, recommended by Roger, Chirurg. lib. 1, cap. 35; and Rolland, Chirurg. lib. 1, cap. 21.

4. A woman, who has lain with a person ill of the Venereal Disease, and received the infection, shall not only give a Gonorrhœa the same day she has lain with the person infected, but any time afterwards, unless she is cured, and not only before she has washed herself and the virulent semen remaining in the womb, but even after a thorough cleansing and a total discharge of all the semen; whereas there was no danger of incurring the Burning by having to do with a woman who had lain with a Leper, unless de novo, as Lanfranc expresses it, Practic. Tract. 3, doct .3 cap. 11; or de proximo, as Valescus de Tarantâ phrases it, Philon. lib. 7, cap 39; or unless the semen of the Leper was still left in the womb, "Semine adhuc in matrice existente," according to Gordon Lillii, Partic. 1, cap. 22; and Valescus de Tarantâ, ubi supra. And hence it is, that John of Gaddisden, in "Rosa Anglica, cap. de Infectione ex Concubitu cum leproso vel leprosa," herein copying Gordon, expressly affirms, "That a woman will remain absolutely free from "infection, if after lying with a Leper she dances, goes "down stairs quick backwards, and sneezes by means of "powdered pepper, or of a feather dipt in vinegar thrust "up the nostrils, so that the semen which was introduced "may fall down, and then washes herself with a decoction "of roses or plantain, boiled in wine and bran."

5. Lastly, there were several other symptoms, which constantly followed upon coition with a leprous woman, or with one who had lately lain with a Leper, of which none are at any time joined with a Venereal Gonorrhœa, or ever observed in the Venereal Disease. They are thus described by John of Gaddisden, loc. citat. "He who lyes with a wo-"man (says he) who has had to do with a Leper, feels "pricking pains between the flesh and the skin, and some-"times heats over the whole body, and afterwards cold and "watchings, and a sensation like that of ants running up-"on his face, if it proceeds from a hot cause. The colour "changes from red to white, and the contrary; and they "frequently feel a gentle heat inwardly, which sometimes "breaks out on the skin, when the person who gave the "infection is of a choleric constitution. If it comes after "lying with one of phlegmatic or melancholy tempera-"ment, it appears slower, the face is presently discoloured "and swelled, he feels a weariness over all his limbs, so "that he can hardly move, with a coldness under the skin, "and a pricking like the stinging of ants, first upon the "face, and afterwards over the whole body." Which description we have also word for word in the Chirurgia of Theodoric, loc. laudat. in the Rogerina, and in Gilbert's Compendium Medicinæ.

VIII. The other testimonies produced by Mr. Becket, which are later than the year 1494, may likewise be understood

stood of this Burning, derived from the Leprosy, though, to speak freely, I think they do rather belong to such disorders as were truly venereal. For the earliest of them does not go higher than the year 1530, at which time the Leprosy was grown a less common distemper, and the Venereal Disease had not only spread itself through Italy and France, but had got a firm footing in England also, as appears by the will of Dr. Collet, Dean of St. Paul's, in the year 1518, of which see Dr. Friend's History of Physick, Part 3. Nor is it any objection, that in the passages referred to, the Disease is mentioned under the name of a Burning in the private parts, and not of a Gonorrhœa or Ulcers in the Penis. For it is usual to apply such names as are already in use to new symptoms, and to borrow them from such distempers as may seem to bear some allusion towards them, though in reality of a different nature, of which this is not the only instance of the Venereal Disease; in like manner as the Romans, though in a different case, called the Elephants of Pyrrhus by the name of Boves Lucæ or Lucani; and so it will constantly be, so long as men are obliged to proceed from the known ideas of things they are acquainted with, to the unknown ideas of things they are strangers to.

Secondly. In the Dissertation printed in the thirty-first volume of the Philosophical Transactions, Num. 365, ann. 1720, Mr. Becket takes a great deal of pains to prove the antiquity of the Venereal Disease, and besides certain reasonings,

sonings, which we have already fully answered, has brought two authorities, which seem to be somewhat remarkable.

The first is taken from a manuscript in Lincoln College, in Oxford, in which we have these words of one Thomas Gascoigne, who, Mr. Becket says, was at that time Chancellor of Oxford: "I Thomas Gascoigne, an unworthy "Doctor of Divinity, who wrote and collected these Ob- "servations, have known several men who have died of a "Putrefaction of the Genitals and of the whole body; "which Corruption and Putrefaction, as they said, was "owing to carnal copulation. For that great English Duke, "viz. John of Gaunt, died of a putrefaction of this kind, "occasioned by coition. For he was much addicted to "Venery, as was well known all over England, and when "he was upon his death-bed, he shewed that mortification "to King Richard II. This was communicated to me by "an honest Batchelor of Divinity, who was the only per- "son in the secret. Willis likewise, a citizen of London, "pretty far advanced in years, died of a mortification of "the same kind, occasioned by carnal copulation with "women, which he confessed to several persons before his "death, as he was distributing alms with his own hands, "of which I was a witness, in the year 1430." Thus far Thomas Gascoigne, from whose words Mr. Becket thinks it appears that both John Duke of Lancaster, by the English called John of Gaunt, and Willis of London, were infected with

with the Venereal Distemper before the year 1430, and therefore that this disease was known before the discovery of the West Indies.

His second testimony is taken from certain passages in the manuscript Works of John Arden, a famous Surgeon in England, about the year 1370, where he treats of a Phimosis, Paraphimosis, Caruncles of the Urethra, and a Bubo; from whence Mr. Becket concludes that these symptoms were venereal, and therefore that the Venereal Disease was at that time known in England.

But to me it is very surprising, that Mr. Becket should be so much possessed with his opinion, as to have nothing else in view but the Venereal Disease, at least to discern it where it is not, and indeed upon a thorough consideration, where the slighteſt marks of it are not to be traced.

For, 1st. We have already observed that the Genitals are no less subject to violent diseases than the other parts of the body, that they are alike exposed to all the causes of indisposition, nor enjoy any prerogative above the rest to guard them against the attacks of distempers. 'Tis certain enough, that from the very infancy of Physick, and long before the Venereal Disease was known, several Physicians have treated at large of an Abscess, Ulcer, Cancer, and Mortification in the Genitals, concerning which see Galen, lib. 6, de

de locis affectis, cap. 6; and Cornelius Celsus, lib. 2, cap. 1, lib. 5, cap. 20, and lib. 6, cap. 18, not to mention others: as also that several of the ancient Historians have spoken of them in their works. Thus Flavius Josephus, in his second Book against Apion, relates, that that vile slanderer of the Jews was afflicted with an Ulcer in the Penis, of which, after several incisions to no purpose, he died in exquisite torments, the genital parts being mortified. And again Hist. Jud. lib. 17, cap. 8, he says that Herod, King of the Jews, died consumptive and convulsed, his private parts being putrefied and eaten up with worms. So we learn from Eusebius Hist. Eccles. lib. 8, cap. 16, that Galerius Maximianus died a miserable death from an Abscess and Ulcer in the midst of his private parts, I suppose, in the Perinæum, the disease proving incurable, and the parts affected abounding with worms, and sending forth an intolerable stench. So in the famous story of the Novocomensian, told by Pliny, Epistolar. lib. 6, Epist. 24. "The husband, (says he) had been "long ill of putrid ulcers about the private parts; his wife "desired to examine them, looking upon herself as the "properest judge whether it was possible to cure them, or "not. She saw them, despaired of curing them, and ad- "vised him to die; nay, she attended him in his death, en- "couraged him to do it, set him the example, and even "forced him to it: for she tied herself to him, and threw "herself into the Lacus Larius with him." And lastly,

Palladius

Palladius reports, that one Ero, upon lying with an Actress, had an Anthrax in the Glans, which afflicted him in such manner for six months, that his genital parts rotted off. And yet in all these cases mentioned by Historians and Physicians, nobody, who is not an absolute stranger to physick, can believe or even suspect these disorders to have been owing, at that time, to the Venereal Disease, as the same do now happen in our days, since the Venereal Disease has been grown so common, without being derived from an impure coition.

II. Whoremongers, adulterers, and lascivious persons must, in my opinion, have been most liable to these diseases of the private parts, as from their proneness to lust, they had to do chiefly with common strumpets, who were always very unclean. And as they were unguarded in their choice, and promiscuously lay with all they met with, and often with women who had Cancers, Ulcers, and Abscesses in the Womb, &c. with such as had the fluor albus, or a discharge of acrid and virulent matter, &c. with leprous women, or such as had lately lain with Lepers, they must from hence, without doubt, have more frequently than others contracted Heats, Burnings, Inflammations, Abscesses, Ulcers, and Cancers in the Genitals.

III. But granting that these lascivious persons were to lie only with sound and clean women, which is hardly to

be supposed, yet from the frequent act of coition, they could not but be more liable to fall into these distempers than the chaste and temperate. For the genital Organs being turgid with a more sharp, salt, and hot Semen; being oftener and longer exercised and compressed, as also distended by the over-heated blood, which is poured into them afresh, or was before retained in them; or, which is still worse, perhaps irritated by too frequent use of provocatives, it is not possible but Heats, Burnings, and pustulary Eruptions must more frequently shew themselves in the Glans; and Inflammations, Abscesses, and Swellings, more constantly arise in the prostatæ, vesiculæ seminales, testes, &c. As therefore violent declaimers are most subject to diseases of the Lungs, and such as are obliged to be very intent upon minute objects, to weaknesses in the eyes, so lascivious persons cannot but be most exposed to disorders in the genital Organs, and thus, as the Poet speaks, tho' in a different sense, "artifices arte perire suâ," which the Stoics seem to have well understood, who, according to Tully, upbraided Epicurus with a Disury he complained of, as if it had been "turpis intemperantiæ morbus." Thus Peter Paul Vergerius, the Elder, of Justinopolis, who flourished at Padua in the year 1395, says, in the History of the Carrara Family not yet published, as Johannes Rhodius attests, in Emendationibus et Notis ad Scribonium Largum, No. 235, "That Ubertinus of Carrara, the seventh of that "name, and the third Governor of that City, died at "Padua

"Padua the 29th of March, in the year 1245, of a linger-
"ing disease of his private parts, occasioned by too much
"venery."

IV. And therefore, though I care not here to dispute the genuineness of the Lincoln College manuscript, or the authority of Thomas Gascoigne, who is said to have wrote in 1430, and consequently could only have by hearsay, as he owns himself, what he says concerning John of Gaunt, who died in the year 1398; yet unless Mr. Becket can produce some better reasons, I am persuaded he will never convince any body, either that John of Gaunt or Willis of London had the Venereal Disease, "because they died of "a putrefaction of the genitals, and of the whole body;" or because that corruption and putrefaction, as they said, was occasioned by carnal copulation. For it is plain, from what we have said, that such corruptions and putrefactions of the genital organs might have arose formerly without any Venereal Taint, either from impure coition with a woman diseased in her womb, or from lying with a leprous woman, or one who had before lain with a Leper, or even from an immoderate use of coition with such as were sound, as they may at present likewise proceed from the same causes.

V. Mr. Becket has been no less unfortunate in the passages he has produced out of John Arden's Chirurgical

Works, where he treats of a Phimosis, Paraphimosis, Hypersarcosis, or Caruncle of the Urethra, &c. For it is very certain, as I believe nobody will deny, that the Greek Physicians were well acquainted with those disorders, as indeed their Greek names sufficiently shew, that they have been described by them, and the descriptions taken from them by the Latins, Arabians, and Europeans, from the time of the Arabians down to the restoration of learning. And besides Galen and Celsus, whom we have already quoted, it were easy to produce six hundred Writers more, if there was occasion, who have given a clear and accurate description of them. But then these disorders in the private parts proceeded formerly from an ordinary cause, as must be plain to any one who will be at the pains to consult those Authors, and not from any Venereal Contagion; therefore they ought not to be confounded with the Venereal Disorders of the same kind, which are now to be met with, and though in appearance of a similar nature, and so called by the same names, are notwithstanding in their cause and original entirely different.

Lastly, Mr. Becket's third Dissertation, printed in the same volume of the Philosophical Transactions, Numb. 366, turns upon the Leprosy of the Arabians, which, as in his former Dissertation, he thinks was of two kinds; the one, the Leprosy, properly so called; and the other, the Venereal Disease, under the mistaken name of a Leprosy.

But

But these are mere fancies, the fruits of Mr. Becket's imagination, and have been sufficiently confuted already.

This is the substance of what he has advanced to invalidate the late original of the Venereal Disease, an opinion which most men have now fallen into, and gains ground almost every day; and he thinks he has done his business by producing some few ambiguous scraps from obscure, and for the most part, unpolished Writings, and which most evidently relate to other distempers. A mighty performance indeed! Which if it were thought to be of any weight by proper Judges, I would undertake to prove by a like method, that the Ancients were both well acquainted with and visited the West Indies, that fourth part of the known world, long before the age of Christopher Columbus; which though I think to be a very false notion, yet I could produce for it more and much stronger authorities, than those which are brought in Defence of the Antiquity of the Venereal Disease.

MEDICAL AND OTHER TESTIMONIES.

ASTRUC, the learned and judicious Historian, I may call him, on the subject of the Venereal Disease, has spared no pains in investigating the Origin of it—has done all for us to clear up a question so remote as three centuries ago—has enquired into the History of diseases in the days of the Greeks and the Romans, but has not been able to produce any Author who satisfactorily corroborates the opinion of those who are desirous to attribute its Origin to another and prior cause,—to make it an ancient disease with another name, and to establish the Venereal Disease, an original, venomous, morbid disease of Europe.

He first advances, for the sake of his argument, the silence of all Physicians, who have lived since the age of Hippocrates, i. e. from the infancy of Physick among the Greeks and the Romans, in whose Writings there is not a syllable that can properly be applied to the Lues Venerea.

It is scarce two hundred and ninety-six years since this distemper first shewed itself in Europe, and we have extant upwards of Four Hundred different Treatises upon this subject,

subject, by Authors of great reputation, besides the notice which has been taken of it in almost every book that has been written on Physick.

Then it must be admitted at least to be extraordinary, that they should all to a man, for full two thousand years, omit to mention the Venereal Disease, so violent, so remarkable and common a disorder, if this distemper had been among them, and which, when undisguised by Mercury, would not have been misunderstood. How comes it to pass, if the disease did exist before the æra of the return of Columbus's crew, that no one Author should have written specifically upon it before that æra, and that so many have since?

Nor can there be a stronger argument in favour of the opinion, that this was a new disease, than the silence of the ancient Historians, who have given no account of any person that ever had it, though the lewdness and irregularities of Emperors, Kings, and others, have frequently fallen under the lash of their pen.

It is certain we live in a chaster age, nor is mankind at present so entirely abandoned to all the excesses of ungovernable lust. And yet from the first appearance of the Venereal Disease among us, which is no long period, we have the express testimonies of several Historians, that a considerable

considerable number of Princes and great men have laboured under this distemper. What just excuse can be brought for the ancient Historians, who have not given us one single instance of this disease in Two Thousand Years? Can we imagine that they could find none of the Grecian Commanders to produce, not one of the Roman Emperors, who indulged themselves in all manner of lust, that ever had the Venereal Disease? It was certainly something miraculous, that such a constant course of uncautious lewdness should never have been attended with any such effect. Would Tacitus, for instance, who has been so severe upon the Roman Emperors, or Suetonius, who has written the Lives of the Twelve Cæsars, with the same freedom that they passed them, or any of the Writers before them—would these, from a disposition to flatter, have industriously concealed what they judged might tend to the disgrace and scandal of their Princes? It is not to be conceived.

In the writings of the ancient Poets, whether Satyrists, Epigrammatists, or others, there is no expression that can be properlyunderstood to signify the Venereal Disease. The Poets who have lived in France for these two hundred years, not to mention those of England, have not been altogether so modest. The Satyrs of Reignier, the Epigrams of Marôt, Rabelais, Pope, with many more, confirm the disease to be now existing.

Whence

Whence can this difference arise between the ancient and modern Poets? Shall we imagine that Horace, Juvenal, or Persius—Catullus, Petronius, or Martial, were more reserved, more modest than the Moderns?

Another argument, and which to me appears the strongest and most convincing, may be deduced from the diversity of names which this disease had bestowed upon it, when it made its first appearance in Europe: for had it made the some havock formerly amongst the Greeks, Latins, or Arabians, by whom alone Physick was successfully cultivated, as it does now in Europe, it would certainly have been transmitted to us under some fixed denomination, either in Greek, Latin, or Arabick, the same as other diseases which the Ancients were acquainted with. But on the contrary, the Venereal Disease, when it was first observed, was not distinguished by the European Physicians by any proper name, applicable to it alone; but every one was at liberty to name it as he pleased, signifying that it was a new, and therefore an anonymous disease.

First it was named from those hard and party-coloured tubercles, or those ulcerous and various-coloured pustules which deformed the skin, and which, on its first appearance, were at least the chief, if not the only symptoms of this disease. Hence it was called by the Spaniards, according to Johannes de Vigo, Las Bubas, Buvas, Buas, or Boas; by the

Genoese, le Male de le Tavelle; by the Tuscans, il Malo delle Bolle; by the People of Lombardy, lo Malo de le Brosule; all which different names signify pustules; but the French named it la Verole, signifying pustule. It also derived names from the different countries where it prevailed, and to which it was imagined to have been transmitted. The Neapolitans called it the Mal Francese; the French retorted, and called it Mal de Naples; the Germans, Frantzosischen Pocken; the Flemish and the Dutch, Spaanse Pocken, or Spanish Pox; the Africans and Moors, the same; by the East Indians and Japanese, the Disease of the Portuguese, being carried to them by the crews of the ships from Portugal: and each of them alledging, that it was brought to them either by the nation bordering next upon them, or by one connected with them by commerce.

For the proof of all these assertions, Authors are referred to by Astruc. Some also gave the disease fanciful names—thus, Jerome Fracastorius calls it Syphilis, derived from the Shepherd Syphilis, whom he feigns to have been the first punished with it, for having offended the Gods: it was after him that our countryman Turner called his volumes on this disease Siphilis.

Lastly, we may urge the authority of all the Physicians and Surgeons who lived at the time of the first eruption of the disease, who in general agree, that it was first brought into

into Europe towards the close of the fifteenth century, that in symptoms it differed from every other disease that had ever been known, that the infection was propagated throughout Europe, from the Kingdom of Naples, where it first spread itself among the French and Neapolitans; and lastly, that it was imported to Naples from the West Indies, by the Spanish soldiers who served under Columbus.

It has been asserted, that the Venereal Disease was the same with the Elephantiasis of the Greeks. But I do not think that there is the least foundation in truth for any such assertion. The Elephantiasis of the Greeks is a disease peculiar to Syria and Egypt, and has never but twice been brought into Europe; first by the army of Pompey, before the birth of Christ, when they returned into Italy, upon the reduction of Syria and Egypt, where it continued not long; and a second time, in the twelfth century, during the Crusades, or religious expeditions, undertaken to recover the Holy Land out of the hands of the Infidels, of which our modern histories are full.

But it is a presumption, at this time of day, in the modern Physicians, who have never seen a Leprosy, to say that the Venereal Disease is the same with the Leprosy; when it appears, by the united testimonties of all the Physicians who lived at the first eruption of the Venereal Disease, and were acquainted with the Leprosy, that they were

very distinct distempers, as has been proved, by comparing the symptoms and appearances of both together, by a variety of Authors of the first reputation, from the years 1497 to 1560, viz. Coradinus Gilinus, Nicolas Leonicenus, Gasper Torella, John Maynard, James Cataneus, Peter Maynard, Peter Andrew Matthiolus, Aloysius Labera, John Baptist Montanus, Anthony Musa Brassavolus, and Gabriel Fallopius.

If, after the testimony of these eye-witnesses, any further doubt remains, we may without difficulty find out the difference of these two diseases, by comparing the symptoms of Leprosy, given us by Avicenna, Guido de Cauliaco, and other more ancient Authors: thus, for instance, according to Guido de Cauliaco---

These are univocal symptoms of the Leprosy: "A round-"ness (rotunditas) of the eyes and ears; a shedding of the "hair, and a thickness or tuberosity of the eye-lids; the "nostrils dilated and turned outwards at the extremity, but "contracted within; the lips fetid, and the voice hoarse, as "if the person spoke through the nose; the breath and "smell of the whole body rank; the aspect stern and hor-"rid, like that of a satyr.*

"The

*By the bye, this is comparing a thing which is to a thing which is not.

"The equivocal symptoms (he says) are, a hardness and "tuberosity of the flesh, especially of the joints and ex- "tremities; the colour of the skin freckled and very dark; "a falling off and fresh growth of the small hairs; a wasting "of the muscles, especially of the thumb; an insensibility, "numbness, and cramps of the extremities; scabs and ring- "worms, the face red and full of pimples, and ulcerations "over the body; kernels (grana) under the tongue, the "eye-lids, and behind the ears; heats and pricking pains, "and great sensibility in the body; the skin, when exposed "to air, like that of a goose when it is plucked; when wa- "ter is thrown upon them their skin appears greasy; they "seldom are feverish; they are cunning and deceitful, fu- "rious and quarrelsome; they are oppressed with heavy "and dismal dreams; their pulse is weak, their blood is "black, or of a leaden or dark-grey colour, cineritious, "sandy, and grumous; their urine is palid, thin, and ci- "neritious."

Here we have no Gonorhœa, no Ulcers of the Glans, no Buboes of the Groin, no ulcerated Throats, no Nodes, which are at least the most frequent, if not the most essential symptoms of the Venereal Disease. A confirmed Leprosy all Physicians have judged incurable; but the worst stage of the Venereal Disease may certainly be cured. The symptoms of the Leprosy are heightened and increased by the use of mercury, and never carried off; venereal per-

sons,

sons, on the other hand, may always find a benefit from it, and, if rightly administered, never fail of a cure.

It is not, therefore, strange, that the more ancient Physicians, who lived at the time of the first breaking out of the Venereal Disease, though thoroughly acquainted with all the remedies which were proper in leprous cases, and which were easily to be found in the Writings of the Greek and Arabian Physicians, should yet confess themselves ignorant of a proper method of cure of this new disease, and then, under this difficulty, that they should find it requisite to have recourse to new methods and medicines, entirely different from such remedies as were in use for the Leprosy. For this assertion, I refer you to consult Gasper Torella, John de Vigo, Ulrich de Hutten, Laurence Phrisius, Gonsalvo Fernandes de Oviedo, and Gabriel Fallopius.

Laurence Phrisius says, that the leprous patients were o alarmed, that they refused to converse or cohabit with such as were afflicted with this new disease.

From hence we may conclude that the Lepers were fully convinced of this new distemper being very different from the Leprosy they were afflicted with, and that there was danger of their contracting a new disease, by conversing or cohabiting with such persons as had it; for which reason there were separate houses for them at Paris, named Mala-

Maladaries and Leproseries. Lastly, they have no grounds for affirming, that the Leprosy disappeared of a sudden in Europe, upon the eruption of the Venereal Disease; or from thence to conclude, that the Leprosy then only changed its name with the Venereal or French Pox. The contrary is certain, from the expressed and indisputed testimonies of Physicians, who declare, that the Leprosy was common in Europe during the sixteenth century.

I shall now proceed to the authorities for the strong ground entertained, that the Venereal Disease first made its appearance in Europe towards the latter end of the year 1494. This may be learned from the joint testimonies of all the Physical Writers who flourished then in Italy, and have treated of the Origin of this distemper.

To pass a right judgment, therefore, upon this naked point, it may not be amiss to produce some evidence, from whence it will appear that the Venereal Disease was at that time first seen in Europe, and from thence was propagated into other European territories: that this disease was so far different from any of the diseases of the Ancients, that all persons were at that time unacquainted with it; in so much that the Physicians, astonished at the novelty of the disease, and finding, from experience, that the medicines which were usually given to analagous distempers proved ineffectual,

tual, were at a loss what method to pursue, and, for a time, gave up the cure of this disease into the hands of Quacks.

Joseph Grunpech, a German Physician, published, in the year 1496, "Tractatum de Pestilentiali Scorræ sive Mala de Frantzos," in which he affirms, that it was a disease so lately inflicted on mankind, that it seemed to be a Plague sent down from Heaven—that it was a new kind of disease, hateful to Nature—a most horrid and terrible prodigy, unheard of, hated, and altogether unknown to mortals before that time.

Alexander Benedict, of Verona, who was Physician in the Venetian Army which Charles the Eighth of France destroyed in the Battle of Fornova, in the year 1495, and therefore had the opportunity of observing the first appearance of this new disease, asserts in his Work, "de Omnibus Morbis," published in 1496, that "by the Venereal "Contact, a new French disease, or at least one that was "unknown to former Physicians, owing to the pestiferous "aspect of the stars, had burst in upon them from the "West;"—and in another part of his Work, that "the "French Disease, a new Plague, which was sprung up in "the world, contracted by lying together and contact, was "reckoned, in his time, incurable."

Nicolas Leonicenus, of Vicenza, Professor of Physick at Farrara,

Farrara, the famous reviver of Greek Physick, in a Treatise which he wrote in the year 1496, de Morbo Gallico, observes first, that "New diseases had appeared in Italy, which "were unknown to former ages, after the manner of the "Lichenis, which, according to Pliny, Hist. Natural. lib. 16, "were never known before the time of Claudius." Then he proceeds thus:

"Something like this has happened in this age, for now "a new disease, of an unusual nature, has attacked Italy, "and several other countries; however, this disease has ob- "tained no proper name hitherto by our present Physi- "cians, but they commonly call it the French Disease; as "if the contagion had been imported by the French into "Italy, and that this country was infested both by the di- "sease and the arms of France at the same time. I, for my "part, am forced to believe (nor indeed can I conceive the "case to be otherwise) that this infectious disease, which "has lately sprung up, has harrassed this present age, as it "never did any former one."

Coradinus Gilinus, in his "Opusculum de Morbo Gallico," begins thus:

"Last year (1496) a very violent disease attacked great "numbers of people, both in Italy and the other side of "the mountains, which the Italians call the French Disease,

" affirming, that the French introduced it into Italy; which " the French call the Italian or Neapolitan Disease, because " they say they were first infected in Italy, and espe- " cially at Naples, with this cruel Plague; or because the " disease appeared first in Italy at the time of the passage " of the French over the mountains. And as this disease " is yet unknown to the Moderns, and there have been, and " still subsist great debates about it amongst Physicians, I " have therefore determined to write something upon it."

Bartholomew Montagnana, the younger, of Padua, wrote, in the year 1449, Physical Advice to Peter Zenus, of Venice, for the most illustrious and most reverend Bishop and Vice-Roy of Hungary, who at that time was ill of this disease. " This disease (he affirms) was unknown to Hippocrates, " Galen, Avicenna, and other ancient Physicians; and there- " fore its proper name was not known. For provided Avi- " cenna (proceeds he) had known it, he would certainly " have given some account of it, and called it by its proper " name, as he has done to a great many others." Whence " he concludes, that the French Disease, which appears now " a days, did not exist in the times of Galen, Hippocrates, " and Avicenna."

Gasper Torella, of Valencia, in Spain, formerly Physician to Pope Alexander the Sixth, published in the year 1500. " This disease (says he) was first discovered by the French, " when

" when they entered Italy with a strong army, and remained " there for some time; especially while they kept posses- " sion of the Kingdom of Naples. Wherefore by the Ita- " lians it was called the French Disease, because they ima- " gined that it was natural to the French. On the other " hand, in France they called it the Neapolitan Disease, " imagining that the French had brought it with them from " Naples, because it began first to appear at the time when " King Charles returned with his men into France:—and " as such an extraordinary disease had never been seen by " any person in our days, therefore it could not be methodi- " cally and perfectly cured, by the most skilful, experienced, " and oldest of our Physicians."

Anthony Bonevenius, of Florence, in a book, de Abditis Rerum Causis, printed at Florence in the year 1507, speaks thus concerning the Origin of the Venereal Disease: " In " the year of Christ 1496, a new kind of disease spread it- " self, not only over all Italy, but almost over all Europe. " Beginning first in Spain, it travelled thence through Italy, " and so over France and other countries, seizing great " numbers of people wherever it went."

Wendelinus Hock de Brackenaw, of the University of Bologne, Professor of Arts and Physick, in 1502, writes thus: " From the year 1494 to the present year 1502, during " which time a contagious disease, which is called French, " has raged very much. Of this disease, or its efficient,

" formal, and material cause, neither Ancients nor Mo-
" derns have given any certain account, nor have they said
" nor wrote any thing satisfactory, neither of its name, nor
" of its nature or quality:—and it is not without cause, that
" even the learned men of the Faculty fly from this cruel
" disease, confessing themselves entirely ignorant of it."

Jacobus Cataneus, a Genoese, in a Treatise, de Morbo Gallico, written about the year 1505, thus relates the history of the first appearance of the Venereal Disease: " In the year
" 1494 of the Christian Æra, when Charles the Eighth,
" King of France, invaded the Kingdom of Naples, during
" the Pontificate of Alexander the Fourth, there first ap-
" peared in Italy a monstrous disease, which was never seen
" in any former age, and quite unknown in any part of the
" world."

Peter Trapolinus, of Padua, Professor of Philosophy and Physick, who died in the year 1506, published de Morbo Gallico, and says that " this disease was certainly unknown
" to Hipocrates, Galen, Avicenna, and the rest of the an-
" cient Physicians."

John de Vigo, a Genoese, Physician and Surgeon to Pope Julius the Second, in his Practicâ Chirurgicâ Curiosâ, about which he was employed from the year 1503 to the year 1513, and was first published in 1514, expresses himself thus:

thus :—" In the month of December, in the year 1494, in " which year Charles King of France marched into Italy " with a very numerous army, in order to regain the " kingdom of Naples, there broke out and spread itself " almost all over Italy, a certain disease of an unknown " nature, which different nations have called by different " names. It was necessary in order to cure it, to call in new " helps, and to make trial of new medicines : and indeed " whatever was found to be successful in that disease, was " rather owing to new experiments, than to any assistance " from remedies recommended both from reason and ex- " perience of the Ancients in their Writings."

Peter Maynard, of Verona, about the year 1518, wrote two Treatises de Morbo Gallico—in the former he speaks thus:—" At this present time there has been discovered an " epidemical or fatal distemper :—it is called French, be- " cause there is no remembrance of it before Charles King " of France marched with his army into Italy, viz. about " the year 1496."

Ulrich Hutten, a German Knight, having been infected with the Venereal Disease, and tried the common method of cure to no purpose, no less than eleven times in the space of nine years, was at last restored to perfect health, by the use of Guaicum, about the year 1519, and the same year wrote a learned Treatise de Curatione Morbi Gallici, per Administrationem

Administrationem Lign. Guaic. which he dedicated to the Archbishop and Elector of Mentz and Magdeburg:—He expresses himself thus: "It has pleased God to send diseases "upon us *unknown* to our Ancestors, as far as we can judge. "In the year 1493, or thereabout, there broke out a con- "tagious disease, not in France, but in Naples, though it "was called French, because it appeared first in the French "army, which Charles King of France marched into the "kingdom of Naples. Besides, the Physicians shunned the "very sight of it, much more the touch of the persons who "were infected with it, being more afraid of it than any "other disease."

James à Bothincourt, of Roan, Doctor of Arts and Physick, published a book, entitled Purgatorium in Morbum Gallicum sive Venereum, in which he affirms, "that if this "disease afflicted mankind formerly (as some Physicians "seem vainly to imagine) there is not the least mention "made of it in their Writings. It is certain (proceeds he) "that it only appeared amongst us about thirty years ago; "which æra of the disease, substracting thirty years or so, "from 1527, will make it happen about 1496."

Laurence Phrisius, a German, in a Treatise de Morbo Gallico, published at Basil, 1532, writes thus:—"In the year "1496, there raged a violent disease, making dreadful havock "upon mankind with ulcers, swellings of the glands, and "excruciating

" excruciating pains, and so excessively tormenting, that " all who have ever beheld it, have been quite astonished " at it; for that destructive disease was *unknown* and *unseen* " before, not only by the vulgar, but likewise by men of " learning and well skilled in Physick."

Peter Andrew Matthiolus, of Sienna, in a Discourse de Morbo Gallico, written about the year 1535, expressly asserts, " I can assure you, that before the arrival of Charles King " of France, Italy neither knew the disease, nor had ever " heard the name of it."

Alphonsus Ferrus, a Neopolitan Doctor of Arts and Physick, and Physician to Pope Paul the Third, in a Treatise de Morbo Gallico & Ligni Sancti Naturâ, published at Rence, 1537, expresses himself as follows:—" I do not find that " any of the ancient Writers have at all treated in particu- " lar of that disease which is called the French. Latin Au- " thors have imagined that some of the Ancients have hint- " ed at it in general; but this is only conjecture, and far " from being certain. Let your young Authors torment " themselves as much as they please, in interpreting the sig- " nification of the names and the force of the words, and " put that construction upon it, which is most agreeable to " themselves, yet none of them will be able to shew a par- " ticular chapter upon that subject."

Jerome

Jerome Fracastorius, a Veronese, de Morbis Contagiosis, published at Venice, in 1546, has these observations on the Venereal Disease:—"Among other wonderful phenomena which have happened in this age, we have seen a disease spring up quite new and unknown in this part of the world, which has overspread almost all Europe, and likewise a great part of Asia and Africa, but in Italy it broke out at the time when the French took possession of the kingdom of Naples, under their King Charles."

Anthony Musa Brassavolus, of Farrara, in the beginning of his book de Morbo Gallico, which was printed at Venice, 1553, says, "that the conclusion of the whole affair is this, that the Venereal Disease then took its rise, when the French invested Italy, and Charles the Eighth of France subdued the kingdom of Naples."

Gabriel Fallopius, a native of Modena, in a discourse de Morbo Gallico, which he wrote in the year 1560, having produced in the first chapter several instances of diseases lately known in Europe, proceeds thus:—"The latest of them is that which we now treat of, and is called the French Disease or the French Scab; nor is there any question to be made of its novelty, because in the times of our Ancestors it was not yet discovered, it was not yet heard of; in these days the Elephantiasis was the reigning

" ing disease. The Scab first attacked us in the year 1494, " when Charles, a warrior of great authority, and a very " fiery genius, indulging his natural inclination to war, in- " vaded Italy with a large army, which he had gathered " together that year; having subdued the Dukedom of " Milan, and all Tuscany, the Republic of Florence, &c. " yielding likewise to his arms, he attacked Rome. From " thence he marched to Naples, which he attempted to " attack with a very large army of eighty thousand men. " It was during this juncture that the disease appeared."

Besides the testimonies I have here produced, if there were any occasion for further proofs that the Venereal Disease first of all appeared at the æra that I am assigning to it, many more Authors may be brought to corroborate the fact, if a case, in my opinion, already so plain demanded it.

Leonard Schmai, of Saltzburg, de Morbo Gallico; Nicolas Massa, a Venetian, de Morbo Gallico; Anthony Gallus —Aloysius Labera, of Avilra in Spain, Physician to the Emperor Charles the Fifth; John Baptist Montanus, a Veronese, Tract. de Morbo Gallico; Prosper Borgantius of Padua; Nicolas Macchellus, of Modena; Alex. Trajan Petronius; and lastly, William Rondeletius, Regius Professor and Chancellor of the University of Montpelier, in the beginning of his book de Morbo Italico.

All these wrote between the years 1500 and 1566, and all of them declare with one consent, that the Venereal Disease was a new distemper in Europe, and when it first appeared towards the close of the fifteenth century, was entirely unknown to the Physicians of that day.

LECTURE III.

HISTORICAL TESTIMONIES.

OF so great importance to society was this disease, and so much concern did it excite among men, that we find the Historians of the time were no more silent upon it, than those of other times were of the Sweating Sickness, of the Plague, or of the Fire of London. Historians always mark events which do happen; and sometimes perhaps they either aggravate or diminish the circumstances of them; but I believe it will never be thought that they write on diseases, and describe them as existing, if they are not at the time in existence; nor do they interrupt their more general object by relating of events, unless they are remarkable by being uncommon.

Mark Anthony Coccius Sabellicus died at the age of seventy, in the year 1506, of the French Disease, which he had contracted by venery. This Historian, in his Rhapsodiæ Histor. lib. 9, first published at Venice in 1502, and

afterwards at Paris in 1509, writes thus: "In the year 1496 "a new kind of disease began to spread itself all over Italy, "which took its rise the year before, at the time that the "French first entered that country, and therefore, as it was "natural to believe, was called French; from whence that "horrid disease, which no age was ever afflicted with be- "fore, took its origin, is, in my opinion, far from being "certain. After various tortures, there appeared pustules "upon the body, which, degenerating into foul ulcers, "made it loathsome beyond expression. Neither the Ele- "phantiasis nor Leprosy can be compared with this disease; "and the most melancholy circumstance of all was, that it "was prolonged for several years, and the persons infected "with it neither died nor recovered. Few died, in pro- "portion to the number of those who were seized with it; "but much fewer escaped it: and not only Italy, but like- "wise Germany, Sclavonia, and the whole coast of Ma- "cedonia and Greece, were smitten with this dreadful "scourge."

Baptist Fulgosius, a famous Genoese General, published, in the year 1509, nine books of Memorable Actions, in his native language: it was turned into Latin by Camillus Gilinus, of Milan, and published there. Fulgosius relates, that "Two years before Charles came into Italy, a new di- "sease was sent down upon mankind, for which the Phy- "sicians could neither find a name nor remedies among "the

" the Ancients, and was differently called, according to the " different countries which it visited. It greatly affected the " joints, and in some the whole body was all over ulcerated. " But the most surprising circumstance (adds he) in this " disease was, that it exerted its contagious quality only in " coition, and always derived its origin from the Genitals. " Another remarkable circumstance attending it was this, " that they who were under cure, unless they took parti- " cular care to abstain from venery, even after the disease " seemed quite gone, relapsed into it, and were as bad as " ever. In old persons it appeared to be quite incurable. " This Plague (for such indeed it seemed to be) was brought " from Spain to Italy, but was communicated to the Spa- " niards from Æthiopia, (the West-Indies) and soon spread " itself all over the globe."

In 1529, Jean de Bourdigné, a French Historian, in his History of the Province of Anjou, says, " I must not for- " get to mention that, in the same year (1495) a disease be- " gan to rage in France, which the French called the Great " Pox, and the Neapolitan Scab, because they were first " seized with it in the Neapolitan expedition; and the Ita- " lians called it the French Disease, because it appeared first " in that country at the time when the French made that " irruption into it. It is a dangerous disease, and at first was " as bad as the Leprosy; but in process of time its rage is " somewhat mitigated, and it is neither so violent nor so

" contagious

" contagious as it was formerly. It is certain, however, " that it has killed great numbers of people, and many " persons of distinction, who, for a long time, could find " no remedy against it. But in the islands lately discovered " by the Spaniards, a little while ago was found out a tree, " commonly called Guaicum, which is a sovereign and in- " fallible remedy against this disease."

Francis Guicciardino, of Florence, who wrote the History of Italy, of his own time, from the year 1494 to the year 1532, in the second Book of his History, expresses himself thus, upon the Venereal Disease: " The French " being seized with that disease, while they were in the " possession of Naples, in returning to their native country, " spread it all over Italy. And indeed this disease, whether " it was altogether new, or was only hitherto unknown, ex- " cept in some remote corner of the globe, was so very ter- " rible for a great many years that it seemed the greatest " calamity which in the memory of man was to be trans- " mitted to posterity: for it broke out either in nasty pus- " tules, which frequently turned into ulcers of so malignant " a nature that it was impossible to cure them—or disco- " vered itself in most exquisite pains, racking all the joints " and nerves of the body. Neither were the remedies used by " the Physicians found effectual against it, but frequently " quite the contrary, and such as only irritated it. Great " numbers it killed, sparing neither age nor sex;—still

" greater

" greater it left entirely disfigured, lame, and subject to per-
" petual tormenting pains. Nay a great many of those
" who appeared to have been cured, in a short time re-
" lapsed into the same miserable condition in which they
" had been before. But after some years, whether from
" the direful influence of the stars, from which it had its
" origin, being at last mitigated, or from proper remedies
" having been found out by long experience, it began to
" grow more mild, and was divided into more kinds, and
" put on different appearances to what it did at first. It is
" agreed upon by all who have enquired carefully into its
" nature, that it seldom or never has been communicated
" to any person except by coition."

It appears from History, that the New World was first discovered by Christopher Columbus, under the patronage and protection of Isabel Queen of Caſtile and Leon. In August, 1492, he first set sail with three ships and 120 men, arrived at Hispaniola in the month of December of the same year, and returned from thence in March, 1493. On the 25th of September following, he set out again from Cadiz with seventeen ships and 1500 men, besides mariners and workmen, and arrived in November again at Hispaniola; from whence in the year following, 1494, he dispatched fourteen ships to Spain. In April, 1494, Barth. Columbus, the brother of Christopher, arrived in Hispaniola with three ships, which returned towards the end of the ame year into Spain with Pedro de Margarit, a Catalonian gentleman,

gentleman and Father Bayl, a Benedictine Monk of Catalonia: the former was at that time severely afflicted with the Venereal Disease. In August, 1494, four other ships from Spain arrived at the Island of Hispaniola, under the command of Antonio de Torrez, which returned in the same time that the others did. Lastly, in October 1495, John Aquado, the Envoy of their Catholic Majesties, came into Hispaniola, with four ships to enquire into the crimes laid to Christopher Columbus's charge, and departed for Cadiz the year after, and arrived with Christopher on the 11th of June, 1496, with 200 soldiers infected with the Venereal Disease.

The controversy will be properly said to be at an end, when once we come to this point, as it evidently appears from the testimonies both of Physicians and Historians, who lived at the first appearance of this distemper in Europe, not only that the Venereal Disease was a common distemper in Hispaniola and the neighbouring islands; but that it was thence transported into Spain, on board some of their ships; and that a remedy was sought for it from the same, or some of the neighbouring islands, from whence it sprung, and was pointed out by some of the natives, when no medicine of sufficient efficacy could be found for it in Europe.

Amongst the Physicians who are witnesses to these facts, the

the chief are Anthony Musa Brassavolus, John Baptista Montanus, Gabriel Fallopius, and Roderic Diaz. These confirm the disease to have originated in the West Indies, and to have been brought over by Columbus's men—That it appeared in Spain first in the year 1493, at Barcelona, and there spread immediately through the whole city; but as it was hitherto unknown, and appeared to be formidable, fasts and religious devotions were appointed to appease the destructful progress of the disease—That in the West Indies the disease was very frequent, and familiar to the natives—That amongst them they had found an antidote against it, which was called the Guaicum Wood—That there it was milder than in Europe, where, when it was transplanted, it became so fierce as to affect the head, eyes, nose, palate, skin, flesh, bones, and ligaments. But the Historians of the time appear to me to be more unexceptionable, and represent the first origin of the Venereal Disease, as a fact they were well acquainted with, and of which they were eye-witnesses.

Gonsalvo Fernandez de Oviedo, who was sent into Hispaniola by King Ferdinand, in 1513, to inspect the melting of metals, and made a long stay in that country, about the year 1535 wrote the Natural and General History of the Indies. In this history he relates, "1st. That this disease, which "is called de las Buas, is common to all those countries, "and therefore it had pleased the Divine Providence to

" communicate assistance to them all, and to furnish them " every where with a proper remedy for curing it. But " although," continues he, " that disease is frequent else- " where; yet it is chiefly known amongst the Christians, " and cured by means of a tree called Guaicum in this Island " of Hispaniola. Amongst the Indians it is not so severe, " nor so dangerous, as it is in Spain and the colder climates, " for the natives are cured by means of this tree. In these " Indian countries there are very few Christians who lie " with the female natives, that escape this terrible disease: " because it is really the produce of this soil, and as com- " mon to these Indians, as other distempers are in other " countries." In the Summary of the Natural and General History of the West Indies, which, upon his returning to Europe from Hispaniola, where he had lived twelve years, the same Author wrote in Spanish at Toledo, in 1525, he thus addressed himself to Charles the Fifth, King of Spain, by whose direction he compiled:——" I can assure your " Imperial Majesty, that this disease, which is new in Eu- " rope, is very well known in the Antille Islands, lately dis- " covered, and so very common there, that almost every " one of the Spaniards who lay with the Indian women " contracted it from them. Thus it was imported from " thence into Spain, by those who returned with Columbus " after his first and second voyage. And afterwards, in the " year 1495, when Gonsalvo Fernandez of Corduba, at that " time a famous General, marched the Spanish forces into

" Italy,

" Italy, by order of their Catholic Majesties, Ferdinand and " Isabella, to the assistance of Ferdinand the Second King of " Naples, against Charles King of France, there were seve- " ral of the Spanish soldiers infected with the same disease, " and thus it was quickly communicated with the Neapo- " litans and the French, by means of their lying promis- " cuously with the same women."

To the like purpose the same Author writes in his Natural and General History of the West Indies, in book the second, which is a much larger work than his Summary, and written in Spanish ten years after, viz. in 1535. There he expressly names several witnesses of eminence, who beheld what passed, and were concerned in the first expedition made into Hispaniola, and from whom he declares he received whatever he relates. And more especially he relies upon the authority and testimony of Pedro de Margarit, the Catalonian, whose testimony is of the greater value, as he attended on Christopher Columbus in his second expedition into Hispaniola in 1493, and returned from thence in the year following, infected with the Venereal Disease, of which the Author suspected he was not perfectly cured at the time he wrote; because he was even at that time tortured with continual pains. Upon a thorough knowledge, therefore, of the fact, this Author expressly declares, that the Venereal Disease was brought from Hispaniola into Spain previously to the year 1495, by the Spanish soldiers, who

served under Gonsalvo Fernandez in Italy, and communicated to the French and Neopolitans by promiscuous venery.

Francis Guicciardino, Citizen of Florence, who in the History of his own Times, written in Italy, has carefully related whatever passed in Italy from the years 1494 to 1532: at the close of his second book, having observed that the Venereal Disease was commonly called the French Disease, he thus goes on: "But it is no more than just to remove "this reproach from the French. For it is certain, that "the Venereal Disease, together with a most inveterate "itch, was imported from Spain to Naples. Neither, in- "deed, was it a native of that country, but was likewise "imported thither from those islands, which about that "time were discovered by Christopher Columbus of Ge- "noa. But in those islands kind Nature has provided a "speedy remedy against this disease, viz. only by drink- "ing the juice of an excellent tree that grows there, en- "dowed with many other properties, they are very easily "cured. This tree is called Guaicum."

Francis Lopez, of Gomara, a Sevillian Clergyman, and Chaplain to Ferdinand Cortez, who reduced the Empire of Mexico into a Spanish Province, in his General History of the Indies, written originally in Spanish, and published at Medino del Campo in the year 1553, gives us the following account:

account: "All the natives of the Island of Hispaniola are "infected with the Venereal Disease, and therefore the "Spaniards who had to do with the Indian Women, very "soon were seized with this complaint, than which there "is none more infectious, or more painful. Wherefore, "finding themselves very much tormented, and in no way "of getting better, they returned into Spain, most part of "them on account of their health, and some on account of "business. The disease still lurking in those who return-"ed, was communicated by them to a great many common "women, and by these to several of the soldiers who "marched into Italy, to fight under King Ferdinand the "Second against the French.

"Thus the Venereal Disease was introduced into Italy; "and as it broke out amongst the French, upon their first "entering into that country, it was therefore by them call-"ed the Neapolitan Disease, because they imagined they "contracted it from the Neapolitans: on the other hand, "by the Italians it was called the French Disease, because "they fancied that it was brought to them by the French: "though there were some who likewise called it the Spanish "Scab. Further, as the disease, so likewise the remedy "for it came from the West-Indies; a very strong argument "that the disease came originally from thence. That re-"medy is a kind of wood or tree called Guaicum, with "which the high mountains in that country are covered."

Father

Father John Baptist du Tertre, of the Order of Preaching Friars, in his General History of the Islands of St. Christopher, Guadaloupe, Martinico, and others, says, that "That "foul distemper which they call Epian, and is really the "Venereal Disease, is, in a manner, hereditary to the na- "tives of those islands, who not only contract it by vene- "ry, but it likewise breaks out spontaneously upon them, "owing to their bad manner of living, and the unwhole- "some food which they eat." He then adds, that "to his "certain knowledge there were Spanish soldiers who, upon "their return to Spain, the first voyage with Christopher "Columbus, contracted this disease from the natives, and "carried it with them to Naples, where the infection was "communicated to the French, and hence it was spread "over Europe."

To the same purpose, and with equal strength from authorities, speak Paul Julius, a Student in Physick, who was advanced to the Bishoprick of Nucerias by Clement the Seventh, and died in 1552—John Mariana, a Jesuit, a very careful collector of Spanish History—and Frances Eades de Mezaray, in his Abbregé Chronologique de l'Histoire de France, in the year 1496. But it will be certainly thought unnecessary to quote their words at large, in a case so plain as I think I have made it out.

In the Neapolitan, or rather in the Spanish Army, there were

were not a few of the soldiers who, returning from the Indies, either in the first voyage with Christopher Columbus, in the month of March, 1493; or in greater numbers, in the second, as already observed, with Antonio de Torrez, in the beginning of the year 1494; or in the third, with Pedro de Margarit, at the end of the same year, were as yet infected with the Venereal Disease, or at least had contracted it in Spain, after it had been brought by others into Europe. And therefore it is by no means strange, that many of the Neapolitans should be infected with the same distemper, as they served under the same colours, and had to do with the same women that followed the camp. And, for the same reason, the like contagion could not but soon be communicated by one or both of them to the French; for, as the success of the war continued doubtful for two whole years, and the same towns were taken and retaken by both parties, it is plain that the French also must have had communication with the same women who had lain with the Spaniards and Neapolitans, and thus the seeds of the Venereal Disease must have mutually passed from one to another.

But though this disease appears to have spread itself very quickly from Spain, Italy, and France, where it first began, into the countries of Europe, yet its progress was gradual, and slower in proportion to the distance of those countries from the places where it first shewed itself. Of this

this we have the testimony of Daniel Sennertus, about the year 1632, concerning the Upper Saxony, who, in the sixth book of his Works, after relating that Cappavacius, in Italy, had got above eighteen thousand crowns, solely by curing of the French Disease, ingenuously owns, that, "for thirty-four years, during which space he had practised physick, not without profit, he had not made so "many single crowns by curing this disease, as Cappavacius had made thousands, owing to the small number of "those who laboured under it in that country."

However, at the same time that the infection of the Venereal Disease passed from Spain into Italy and France, and from thence into the other European countries, it appears likewise to have spread itself along the coasts of Africa and Asia, that were adjacent to the Mediterranean Sea; which we think was owing to the following causes: to the banishment of the Jews and Mahometans, who, upon the conquest of Granada, were driven out of Spain by Ferdinand and Isabella, and such of them who fled into Africa carried with them the Venereal Disease, which they had before contracted in Spain.

Of this we have a remarkable witness, John Leo, a Mahometan, and a native of Granada, who, being banished with the rest, removed to Fez, in Morocco, where he applied himself to the study of the Arabian learning, and being

being afterwards taken by Pirates, and presented to Pope Leo the Tenth, embraced the Christian Religion. Now in his Description of Africa, which he wrote in Arabic, at Rome, and which was turned into Latin by John Florianus, he expresses himself thus: "Even the name of this disease "was not known to the Africans, before King Ferdinand "banished all the Jews and Mahometans out of Spain, "who being returned to their native country, the Æthio- "pians lay with their wives, and thus the disease spread "over Africa, like a Plague, so that there was hardly a fa- "mily that remained free from the Infection. They are "firmly persuaded that it was transplanted to them from "Spain, and on this very account (that it might not be "without a name) they called it the Spanish Disease: but "at Tunis, and all over Italy, it is called the French Disease; "and the same name is given to it in Egypt and Syria;— "whence the common proverb of cursing—may the French "Disease kill you."

The Merchants and seamen, who brought goods daily from the ports of Italy, France, and Spain, to the ports of Africa and Asia, carried over also a very bad kind of merchandize, viz. the Venereal Disease, which, by degrees, spread itself farther into the country, from the coasts bordering upon the Mediterranean. Nor did the contagion, spread by the Europeans, stop here, but extended wide into the remotest corners of Asia; and, by repeated voyages,

penetrated into the East-Indies, where, by the Persians, &c. it is called the Disease of the Christians. It is also certain that the first seeds of this disease were imported by the Portuguese, not only to the coasts of India and the Empire of China, but likewise to the Kingdom of Japan; for Engelbert Kempfer, in his Natural, Civil, and Ecclesiastical History of Japan, says, that "the Venereal Disease, which "is common amongst them, is called by the Japanese, the "Portuguese Disease."

THE AUTHOR'S SUMMARY.

AFTER having produced for your consideration all the proofs and authorities which serve to direct us to the true source and origin of the Venereal Disease, I am naturally called upon to give my opinion. I am irresistibly induced to believe, that this disease was conveyed into Europe from America by the crew of Columbus.

The silence of all Authors upon it from the days of Hippocrates, Galen, and Avicenna—the disease bursting forth at once, and committing an uncontroulable havock among the human species—the Faculty at a loss how to treat it—the consternation which is expressed by all the Writers of the time—the inability which they confess to account or assign a cause for it—it attacking first of all the soldiers, men in the habit of promiscuous venery—All these circumstances conspire to prove the novelty, the importance, and dreadful effects of the disease.

The natural action of the Venereal Poison, the distance of time between the contraction of the first infection and the appearance of constitutional symptoms, operated very strongly against the success of the first enquirers after

the origin of it. They did not then know, that a soldier or sailor, ab origine infected in America, could convey the infection to a woman in Europe; that this infection could be afterwards diffused and multiplied in the armies. They were then incapable of forming any opinion how such a progress of the disease could come to pass. For to assign a true medical cause, it must first of all have been known, that it is the natural action of the disease to be contracted in one form, and after a long process of time to appear in another. Those who were ignorant of this fact, could not possibly imagine that the original symptoms would produce different symptoms on other parts, long after the appearance of the original symptoms. This reason will operate always very forcibly against the probability of the patients, or of medical men who saw them, being capable to assign a true cause for such a disease, when it was first discovered.

As at first they were ignorant of the true cause and natural effects of the disease, so they applied to find them out. They were attributed to the influence of the stars, to rainy seasons, to Divine Vengeance for the scourge of those who were concerned in the ravages of war, to poisonous wine, to poisonous water, and to various other fabulous causes, which are the truest and strongest arguments of the novelty of the disease. To assign preternatural causes for natural events, is generally a mark of ignorance. A plain

fact

fact here presents itself:—we find a disease raging which they could neither cure, call by a name, nor account for. But when the general consternation had subsided, when the mind was engaged in a free investigation, Physicians and Historians developed the mystery—the fatal truth soon became fixed, that the new disease which had destroyed thousands in Europe, was conveyed by the crew of Columbus from America.

The appearances of disease on the same parts will be often exactly similar, although produced from different causes. Every individual symptom that is similar to the venereal symptoms, existed before the Venereal Disease. The same parts have always been liable to many diseases, or all sore throats would be from one cause. Diseased appearances on mucous and internal surfaces, can be only correspondent with their anatomical constructions. Stimuli of various classes may operate upon them, and produce very similar effects. It is no wonder, therefore, that the Ancients as well as Moderns should be often, from an immediate decision, erroneous in their opinions. But the Ancients more especially confounded diseases, because the symptoms of them were confounded, from want of true, precise, anatomical ideas, and of that discrimination which is acquired by enlarged knowledge. The time has been, when every sore throat was treated similarly, because the difficulty of swallowing was common to all. But even in this age, now that

that we are so nice in our distinctions, the judgement will, a priori, be staggered, to determine what might have been the stimulus which produces a discharge from the Urethra, whenever it comes on, because we know it might be provoked by many causes.

It cannot be disputed, therefore, that a discharge from the Urethra must have been a symptom from other causes, before the Venereal Poison was ever experienced in Europe. The same observation will apply to other parts: the eyes will trickle down tears from affections, from motes, from common and virulent inflammations; little ulcers will be formed on the edges of the lids, the conjunctiva will be inflamed, from various causes; the secretion from the Glans Penis will be increased, small ulcers will be found on the internal part of the Prepuce, and the Prepuce will swell, in consequence of a vitiation in the parts, from various causes. All which symptoms, I presume, have existed, and will be continued without the addition of venereal infection, from the beginning of the history of man to the final conclusion of it.

That such diseases, in those parts, have existed from the beginning, both holy and heathen histories confirm to us. The operation of circumcision, which is freely discussed in the History of the Jews, was intended to guard against predominant diseases in these parts: and that operation confirms

confirms to us, that the cause was not owing to venereal infection; for if it had, neither when the disease was actually acting on the parts, nor for the prevention of it, could the operation of circumcision have availed any thing. The removal of the Prepuce could not have then cured, nor will it now, the Venereal Disease; neither will it prevent the parts which remain, from any future contraction of it. There cannot be a doubt but the object of the Jews, by performing this operation, and enforcing the performance of it by an holy edict, was to prevent disease incurred through redundant and vitiated secretion, which was produced from foul living and uncleanly habits; or in other words, from a defluxion on the parts, and which was not of an infectious venereal virulent nature.

If we pursue the idea throughout the whole of the human frame, we shall find that, not only mucous surfaces, but every other part, is liable to the same symptoms, from many causes; and we shall also find that there is so strong a resemblance in symptoms which are produced from various causes, that so far from our being able, a priori, to find out the true cause, it is often left to be decided by the effect of the remedy. Let men say what they please in theory, we know this to be the truth in practice. If I only heard a subject cough, sneeze, or hiccup, could I, a priori, assign a cause for it? If I were told that a subject had a profuse discharge from the Urethra or Vagina, could I assign a

cause

cause for it, without previous enquiries? The skin is exposed to many sorts of eruptions, besides those which are venereal; ligaments are exposed to nodes, bones to caries, glands to tumours; nocturnal pains are symptoms of other diseases besides the Venereal. From the venereal symptoms, taken distinctly, being so complicated and equivocal, from the strong resemblance which every separate symptom bears to symptoms from other causes, and from anamolous appearances also, we are very often confounded, and find them nice and intricate points to be decided upon.

Without the idea be well inculcated, and continually impressed upon the mind, that every separate symptom belonging to the Venereal Disease is to be found through other diseases, we should hesitate, and be left in some uncertainty from this cause, whether the desired truth on the Origin of the Venereal Disease lay in the arguments of Mr. Becket, or of Dr. Astruc, both men of abilities, equally industrious, erudite, and argumentative, and who have done honour to their professions.

There cannot be a doubt that every symptom singly of the Venereal Disease, may be a resembling symptom of another disease; but there is no other disease which embraces all the symptoms of the Venereal Disease. There is no other disease where the symptoms of it go progressively on in resemblance to the Venereal Disease. It appears,

pears, at least to my understanding evident, that these Authors quoted by Mr. Becket were not knowingly describing the Venereal Disease, but that Mr. Becket has put that construction upon their words. They described the condition of parts, as the cases came before them, without imputing to these cases such consequences as must naturally have ensued if they had been venereal. For if they had been truly venereal, the description of them could not have been equivocal; a doubt of the true cause could not have remained; the disease needed only to have been seen, to be truly described, in that full manner in which it has been by Authors who have written avowedly on it, in the beginning of the sixteenth century.

From Vigo, Fallopius, Massa, and Berengarius Carpus, we can now know what are the true symptoms of the Venereal Disease, so strongly do their definitions correspond with the present appearances of it. At some of the periods to which we are referred in the authorities produced by Mr. Becket, mercury was not then known as a specifick for the cure of the Venereal Disease; and I may therefore be permitted to remark, that a few solitary cases, and those none of the most complete, or aggravated in their nature, would not have been the only traces of the disease which the accuracy of our ancestors would have left behind them.

If the Venereal Disease had existed in Europe long prior to the knowledge of the power of mercury over it, we

should have been furnished in volumes with fatal arguments of the fact, instead of faint doubts, expressed in a collection of rare and scattered papers. Knowing, that in all societies, the intercourse, by which the infection is conveyed, must be continued from an irresistible impulse, and presuming the disease to have been in existence, without the knowledge of the only medicine which can cure it, would not the ancient Writers have expressed in their works a continual alarm from the consequences of it, before, as those Writers did in particular on the first appearance of it, after the return of Columbus?

If the disease had actually prevailed in Europe so long as some Authors have declared it to have been, the consequences must have been dreadful to an extreme, as every specifick for the cure of it, both real and supposed, as mercury, guaiacum, China root, and sarsaparilla, are discoveries made since the return of Columbus.

Those, therefore, who favour the arguments of Mr. Becket, and who deny that the disease was first introduced into Europe on the return of Columbus from America, will be pleased to point out to us, from whence the origin of infection began in Europe; for it must have been either imported, or it must be indigenous. If they say it was imported, but not at that time which Dr. Astruc asserts, we expect to be told from them when the time was. If they say it is indigenous,

genous, their part is, to shew at what æra, and from what extravagance in lust, or from what other cause the Venereal Disease was then created, which is now only propagated.

I shall appeal to the arguments which are adduced by Dr. Astruc, in proof that the Lepra could not have been the Venereal Disease; for if it had, those which were the symptoms of the Lepra formerly, would be the symptoms of the Venereal Disease now. A change of names cannot alter the symptoms of a disease: the one may be the capricious act of a man, the other will be an uniform act in Nature.

It is not incumbent upon me to say how the Lepra has passed away, and is now scarcely heard of: I am only to assert that the symptoms of the Lepra are described to be different from those of the Venereal Disease; and that the Physicians, in the beginning of the sixteenth century were perfectly acquainted with the Lepra, but were at a loss to account for, or define the Venereal Disease. This was a period when their similarity might have been compared with some pretensions to truth, when the comparison might have been made with success, by the two diseases being brought face to face, if there had been any suspicion then, that their strong resemblance afforded a probability of their being the same. But it was the irreconcilable distinction there was between them, and the despair of a cure of the Lepra by mercury, when the Venereal Disease very rea-

dily yielded to it, which has ever since established them to be two distinct diseases.

It is said, the Venereal Disease appeared when the Lepra disappeared :—if they were never the same disease, that argues no more, than the Small Pox remaining, when the Plague is extinct.

The possibility of the disease being brought from America to Europe, has been formerly also denied, but we have seen in our time, that it can be conveyed in a longer voyage; for it has been imported to Otaheité from Europe. There we find the disease committing dreadful havock, proceeding through all its stages in the fullest extent, from not being disguised or restrained by mercury.

It remains at present, and probably it ever will be a truth unascertained, whether Bouganville, the French circumnavigator, or Wallis, first introduced through their crew the Venereal Disease to the unfortunate inhabitants of Otaheité. As the act reflects no glory, as there is nothing in the deed which can be honourably boasted of, I presume neither of them have been industrious to assist the Historian in discovering or fixing the fact. If a prize had been the bone of contention, the claim would have been long ago made out, and the right adjudged. If the anxious Historian seeks to ascertain how the disease found a way

to

to Otaheité, a repugnance to that which must be ever considered as an unpardonable neglect, and an indelible disgrace, will stifle the enquiry. For we have found, the more the question has been agitated, the more has art been practised to darken it.

The only account which we are furnished with is, that the disease was actually there since the European philosophic Captains touched upon the Island; that Captain Cook found it there, but he made no mention of any particular symptoms, such as Gonorrhœa, Chancre, or Bubo. He presumed "they had found out a specifick for it," although in that I believe he was mistaken, as will hereafter appear. "He "had seen some upon whom the disease had made a rapid "progress, and one who went away infected, returned in a "short time in perfect health." In another part of this voyage, he says, "that 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, in his last voyage, "they had 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 without the loss of the nose, or the best part of "it." Afterwards he tells us, "that the disease is now al-"most universal: for this they seem to have no remedy. "They allow that in a few cases, Nature without the assist-

"ance

" ance of a Physician, exterminates the poison, and perfect
" recovery is produced; but they also say, if a man is in-
" fected with it, he will communicate it to others in the
" same house, by feeding out of the same utensils, or hand-
" ling them."

It would have been more accordant with the distinct provinces of men, if Captain Cook had delegated to his Surgeon the office of describing the symptoms and the progress of the disease. We then should have been enabled to have comprehended the account of it in a more minute and professional manner. An ignorance of the nature of the disease could not be now pleaded in apology for such an omission, with the same pretence as it might have been by Columbus; and therefore it was a duty due to a country, that a more clear information should have been given. But imperfect as they are, these accounts by Captain Cook, of the progress of the Venereal Disease, correspond with the progress of it when it first appeared in Europe. The symptoms described in general terms by Captain Cook, and those by Historians after the origin of it in Europe, when the crew of Columbus returned from America, are similar, both in respect to rapid progress and uncommon extent. —And it might be thought, because the Venereal Infection was fresh and new to a particular set of inhabitants, in a part of the globe where the disease had never raged before, that this would be a true reason why so rapid a destruction

struction of parts, and so general a havock from the disease prevailed more in these instances, than they do in common amongst us. But I do not think there is any reason for this opinion. I do not think that what has been advanced relative to the Venereal Disease being milder in its symptoms now than formerly, has the smallest foundation in truth. I am aware that my opinion is contradicted with the authorities of Sydenham and Astruc. I think the distinction consists in the art of curing it, and not in the moderated virulence of the disease.

If mercury was not now known, if mercury was not now always applied, if those who know how to cure the disease, and if those who know not how to cure it, did not all apply more or less of mercury, I believe the disease would not be found less wanting in power now than ever. I believe there is not a single instance to be produced of late years, where the Venereal Disease has persisted upon a subject, without a resort to mercury at some time or other during its course. And I believe if mercury was withheld, from what I have often seen, that the force of the venom on the constitution is in general as powerful as ever.

Many a gay youth in the pride of vigour would have been laid low in his first career, were it not for mercury. The morning sun of many a beautiful girl would have been untimely set with her innocence, without the aid of mercury.

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In many married families, where desire has wandered from the nuptial bed, and returned with tainted effects, there would have been experienced an immediate ruin of future happiness, a fatal privation of domestic bliss, and disease irreparable, if the inestimable power of mercury had not been known amongst us.

LECTURE IV.

ON THE

NATURE AND ACTION OF THE VENEREAL DISEASE.

THE Venereal Disease appears to be a poison, sui generis, —an animal poison peculiar to the human subject. Every poisonous disease is found to be propagated either through morbid effluvia, or through poisonous fluid conveyed from a diseased subject, and lodged with a sound one. The Plague and the Small-Pox are instances of the former description, and which may be termed contagious diseases: the Venereal Disease and bites of mad and venomous animals are instances of the latter, and which may be termed infectious diseases. This distinction between these diseases is evident, for although a contagious disease can be propagated like an infectious disease by inoculation; yet an infectious disease cannot be propagated by effluvia, at least it has not yet been proved to our satisfaction that what I have defined to be an infectious disease, is possessed of that power.

There is no disease which ever was defined, so full of variety as the Venereal Disease is. The reason of this variety will not be found, I flatter myself, too difficult for some additional explanation. In order to account for venereal action on the various parts, both local and constitutional, we must consider the quality of the parts on which it is acting: we must consider that the venereal infection has the singular power of acting on every component part of the human body: and we must further consider, that, independent of a local and constitutional power, which evidently belongs to it, and which is in general acknowledged, there is a relative distinction to be explained, by which the true nature and action of it will be better understood. Although in this country we have no instance to establish an opinion that the Venereal Disease can be propagated in any other way than by contact of venereal fluid, conveyed from a diseased subject to a sound one, yet, if its nature does not permit it to be propagated by effluvia, the subtilty of the poisonous fluid, when once it has taken possession, cannot be disputed. It is owing to the uncommon subtilty of the venereal poison, and the power which it has of attacking every component part of the body, to the irregular action of the poison on each part in point of succession, to the length of time it will remain without discovering itself, to the progress of it not being definable under any fixed periods, to its assimilation on various parts to other diseases which such parts are liable to, that so many disputations have

have engaged Authors, so many erroneous opinions have been given, such various theories have been adopted, such contradictory practice in the treatment of it has been suggested, such false conclusions drawn, and such impositions on credulity practised. It is owing to the subtilty of the venereal poison, and to the power it has of lying dormant, that the introduction of it into Europe was first disputed.

As the Ancients knew no disease like it, and were not aware that it could be transplanted from the West-Indies, they assigned the origin of it to preternatural causes, not being able to comprehend a natural one. I can always conceive the errors arising from ignorance to be venial, when I cannot with so much readiness find in my duty an apology for skepticism. Modern Theorists, who do not believe that this disease was first brought from the West-Indies, or that it could be, might as well doubt that it has since been conveyed from Europe to Otaheité. And those who doubt that a virulent Gonorrhœa is the venereal poison acting locally on a mucous membrane, might also doubt that it is produced, either in consequence of a connection between a sound subject and a diseased one, or in consequence of the infecting fluid being conveyed from a diseased subject, and lodged on a mucous surface of a sound subject, so as to take effect. If we were to make a surrender of our own reasoning upon this question, and build our faith upon the as-

sertions of some, we should become the slaves of constant deception.

The truth is, that Gonorrhœa and Chancre are both the result of venereal poison acting on parts, under different modifications. Skeptical Theorists have not, as yet, carried their distinctions into practice; they only continue to assert, because they know they cannot be positively contradicted. We cannot, a priori, positively avow a discharge from the Urethra to be a virulent discharge; the subtilty of the poison will not admit of it. We make up our minds, from the state of the case, the time of connection, the suspicion of that connection, and the appearance of the symptoms; we also search into the condition, where we can, of the subject who was supposed to infect. We only know, a priori, that a discharge from the Urethra is a true local symptom of venereal infection, and such a symptom as must be necessarily produced, from the anatomical nature of the part, whenever venereal fluid is so lodged there as to act upon it. The particular construction of the surface of the Urethra, will as necessarily produce a discharge from that surface, in an increased quantity, by the application of venereal fluid, as the application of venereal fluid to the cuticle or cutis will produce an Ulcer or a Chancre there. Those, therefore, who deny that a discharge from the Urethra is a symptom of the Venereal Disease, I answer, that a Chancre upon the cuticle or cutis is not more so; for, in consequence

quence of venereal infection, the symptom of Gonorrhœa is not seen on the cutis; nor is the symptom of Chancre seen within the Urethra. The cause of both symptoms is the same, and the effects will be corresponding to the anatomical nature of both parts: venereal fluid applied to the Urethra, produces a discharge; that fluid lodged on the cutis, produces a Chancre.

The Urethra, like every other mucous membrane of the body—like that of the nose, the fauces, and the eyes, is by Nature so formed as to yield from its surface a constant moisture, and the surface which secretes this constant natural moisture is more immediately susceptible than a part covered with skin. That which will irritate the membranes of the nose and fauces, the coats of the eyes, and the Urethra, will not produce the smallest effect on the common skin—Like a spark of fire, which falling on gun-powder will produce an explosion; but when falling on sand, it will there become extinct. Any little innocent extraneous substance fastening on these mucous surfaces, will excite from them an additional discharge, will stimulate them beyond their general action; and it seems to be quite compatible with the animal œconomy, that the particular or preternatural action of every part, is a possession of power enjoyed to indulge or become free of that which is acting upon it, but more especially on parts of this description, which are both inlets and outlets of the constitution; when any

any thing which is obnoxious stimulates them, such stimulating extraneous qualities are diluted away by an increased secretion; this increased secretion diminishes the acrimony of that which stimulates; and from this cause, an extraneous substance stimulating a mucous membrane, may be carried away in the secretion, and if it be, then all irritation being at an end, the discharge of secretion, in course, ceases; but when that extraneous substance happens to be venereal infectious fluid, when the stimulus, through that cause, excites an increased secretion, we are taught, in consequence, and nothing less is expected from this poisonous cause, than that that which most commonly follows should follow, and which is, inflammation and increased secretion, carried on to an unlimited extent, and creative of effects worse in degree, and for a longer continuance, than through any other stimulant which has been experienced ever to engage the attention to those parts in a state of disease.

On whatever part this infecting fluid lodges, if its action does take effect, that action will always be the same, for otherwise it would not be uniform venereal action; the symptoms of its action will only vary, according to the anatomical nature of the parts on which the fluid finds a lodgement. The application of venereal fluid to any part of the body, will necessarily produce a symptom conformable to the part on which it is applied. I take upon me to say,

say, that this position will generally be found to be uniformly true; and that if venereal fluid be applied, and if it acts, without seeing the action, but with being told the part on which it is acting, one might to a certainty ascertain its action and describe its effects.

Independent of the subtilty of the venereal poison being a cause of our want of a clear and decisive knowledge of the various symptoms of the venereal infection, the want of explanation of what we daily see, will always leave a door open for disputation to enter in; and where the room for enquiry is large and dark, every one will bring out of it what comes first to hand, and producing it to light, flatter himself that he has brought forth the truth. The Ancients, when first the Venereal Disease appeared in Europe, did not, among the first symptoms, enumerate a Gonorrhæa, and the symptoms concomitant with it. Almost fifty years elapsed before a Gonorrhœa was classed by them amongst the other symptoms of this disease. This has been a strong argument with modern Writers; and this, in conjunction with a few other objections, has formed a ground for them to build various opinions upon. With some, Gonorrhœa has no relation with the Venereal Disease; with others, if Gonorrhœa has any relation with the Venereal Disease, it is so remote, so comparatively innocent, so free of every thing obnoxious to the constitution, so easy to be cured, or so unnecessary to be cured, that no harm can follow the contracting

ing it, no bad consequences are in the least to be dreaded. We are positively told that Gonorrhœa is not a venereal symptom, or if it be, that it will cure itself, and that it cannot or will not infect the constitution.

As the first Writers did not class Gonorrhœa with the first venereal symptoms, it becomes us to ascertain why they omitted it—whether the omission was the result of their perfect knowledge of all the true venereal symptoms—whether from conviction arising out of a perfect understanding of the subject, which we are now in doubt of—or whether it had ever occurred to them for the first fifty years after the discovery of the disease, that Gonorrhœa was a venereal symptom. I firmly believe the question had never occurred to them; and I will shortly explain my reasons for that persuasion. Independent of the general confusion which prevailed on the first eruption of this disease, their want of reflecting rightly upon this point is also to be attributed to another cause. When the venereal symptoms, such as Chancres, Ulcers, Nodes, and Caries Bones, presented themselves before the Ancients, they did not know the true progress even of these symptoms at first; they did not know from what original source they sprung, but, primâ facie, they saw alarming symptoms—symptoms new to their eyes, and other senses—symptoms which they could not cure, nor account for: they sought their origin in divine judgment, in poisoning wines, in poisoning water, in deluges

deluge of rain, and in various other false causes. To their own ignorance and astonishment at these symptoms, they could plead in plain language; and here they speak through their writings perfectly intelligible. They tell us what they saw, but not how that which they saw was produced; they tell us such were the symptoms, such was the havock of them upon the human constitution, that they were new to them in their nature, and in this, they told us all that was then known by them. But a discharge from the Penis and Vagina at that time was not a new symptom—a discharge from either is perhaps as old as our race; and when it appeared upon a subject then, produced in all probability less alarm and less enquiry than it would now: for previous to the origin of the Venereal Disease, the Urethra and the Vagina were liable to every disease which they now are, excepting that produced by venereal irritation; and if the symptom of discharge be the same from other irritating causes, as well as venereal, it will be no longer a wonder, that they should overlook this at first, as a venereal symptom, and more especially considering, that as the consequences of the constitution being infected were so remote from such a local symptom, it would have been almost amounting to a miracle, if it had at first occurred to them. Besides, they overlooked this symptom as a prima ordo of the disease from another unsuspecting cause: they did not know that venereal fluid could be insinuated up the Urethra, and there lay a foundation for infection, which would act, not

only on the Urethra, and not only spread its virulent effects there, but would also be continued on and conveyed from one to another. We knowing the truth, are constantly upon the watch; we wash and we wipe, we wait and we suspect: common sense treats this complaint, as if it were of a serious nature; prudence will not permit us to listen to the tongues which persuade us, nor to learn from the books which teach us, that ourselves and our women are as safe now, as the Roman Cæsars, the Pompeys and the Anthonys were in their promiscuous connections with the followers of their camps, at a period when amongst them the disease was unknown.

Another reason which I have to assign, why the symptom of Gonorrhœa did not occur to them as a venereal symptom at first is, that as soon as they found it out to be a venereal symptom, they had not the least doubt, but it in reality was so. And as they did not at first annex the symptom through ignorance, so did they afterwards annex it through information. If they had first of all annexed it through ignorance, and afterwards rejected it through information, then the case would have been widely different: for it would have been apparent, they were apprised of the fact from the very beginning, had weighed it, and had decided to reject it. But from the very first time that Gonorrhœa was adjudged to be an increased discharge, flowing from the Urethra, in consequence of venereal infection,

tion, and was considered as true a symptom on that part as a Chancre was on another part, that fact has not, until very lately, been resisted. In the whole of the historical tracts on the Venereal Disease, which have been written year after year, that fact, so far from being doubted, has been always established: not even Mr. Becket, the strongest advocate for the existence of the Venereal Disease in Europe prior to the voyage of Columbus, has attempted to separate Gonorrhœa from being a true venereal symptom; so far from it, he endeavours to establish his position of the priority of the disease to the voyage of Columbus, by bringing more and stronger proofs through the former existence of Gonorrhœa, than through any other local or constitutional venereal symptom; and he was most assuredly justified, in proving that a discharge from the Urethra and Vagina existed before the voyage of Columbus; but other venereal symptoms, having nothing so strongly resembling them, could not be so easily compared, nor so innocently mistaken.

With respect to the power which there is in the infecting fluid, whilst acting on the Urethra in man, or Vagina in woman, of producing either diseased effects on the parts locally, or of being from them absorbed into the constitution, and of producing thereby confirmed venereal symptoms—this will be a future subject of consideration. But whilst it occurs to me, I will beg leave to make one more

remark, as it may yet be found necessary for those to whom I mean to reply. They may ask, if Gonorrhœa be as true a symptom of the disease on one part, as Chancre is on another, how came our Ancestors not to have made the discovery before? I answer—There are many medical and other truths, which appear to be floating and unsettled upon the common surface, thrusting upon our view, inviting our attention, and yet passing away for successive years, but which, as soon as known, strike our conviction, command our assent, become with great readiness settled facts, and lasting ornaments of useful and general information.

The common way by which the propagation of the Venereal Disease is supported and carried on, and has been from the first history of it in Europe, it is almost needless to say it, is by connection between the sexes. But although the propagation of the disease is continued by this intercourse of the sexes, and although the act of copulation between the sexes is the common medium for transferring the disease from one to another, yet that is not the only manner by which it can be conveyed. Venereal fluid, when conveyed through any medium from an infected subject, and lodged with a sound subject, will, if it acts, as completely infect the sound subject, as if it were contracted through an amorous intercourse.

So complex and difficult I have ever found it to determine, a priori, whether a discharge either from the Urethra

thra or Vagina possesses a power of conveying infection, that as I cannot fix in my mind a general rule to direct me, so am I at a loss to explain it to you. An opinion upon this question is very often an opinion of serious importance; no less than the future happiness or misery, riches or poverty, virtue or infamy of individuals are found to be depending on it. The only guide which I have to conduct me for ascertaining my doubts, is to weigh well, to be cautious, and never to be positive, but on the happy side of the question; as it is well known that a woman who is suspected, and with just reason sometimes, to have injured a man, in general denies it, and persists in her innocence with steady obstinacy; so should we be guarded in not involving the innocent part of the sex with her. Much is depending upon our opinious at such a crisis, and therefore in this sense only, the importance of discriminating well, is not to be considered as a trifle which might be passed unnoticed.

But we even should be cautious in aspersing or fixing a criminality upon many of those who, knowing they have been injured, do convey an injury without any intention, or without any belief that they could, when they did convey it. For if the most observing of us all be deceived with respect to the point of time when the infecting power is extinct, before the discharge which conveys it ceases; if we plead ignorance upon this question, and I am certain that, a priori, we ought and we must, how much more ought women to

be

be excused, or at least ought they to be indulged with a latitude of apology, who are not of the profession to know, and who are, in the mean time, debarred from intercourses, and depriving themselves of the only profit which their short-lived pleasure prompts them to aspire after? There are many who innocently infect in this manner, deserving of more generous treatment than they receive. An attention to this part of the subject must therefore be fixed; for it is at a period of the disease which we least suspect, when the violence of the symptoms have subsided, and some time after even, that such mistakes have happened, and that such an injury has been produced, by the premature hazard of enjoyment.

I am glad it occurs to me to make an observation, which I wonder no Writer has made before—I have constantly found, and I dare say it has not escaped the remark of others, that we never scarcely read of a rape, without the additional crime of infection: "that such a one has made "an attempt upon a young girl, and has not only gratified "himself by force, but has given her the foul disease."—She is examined, and I believe, too often the conceit gains consent; sometimes perhaps from truth, but oftentimes, I believe, from error.—As the aggravation in crimes should produce an aggravation in punishment, and as it always will act with additional horror upon the conscience of a man of sense, this observation, whenever such a case again occurs, might have its use.

I shall

I shall now endeavour to explain myself, and give opinions, which, however erroneous, I will take care they shall only err on the side of caution and safety. I will not permit a man (as Mr. Hunter does) to have a connection, in a state of disease, and bare the woman harmless. I would not carry this opinion so far as he does, and "allow "a man who has a Gonorrhœa to have connection with a "sound woman, if he took great care to clear all the parts "of any matter, by first syringing the Urethra, making wa-"ter, and washing the glans." My reason for this interruption is, that the parts will go on in secretion during the connection; and that no punishement is already fixed for me, which can bear any proportion to the injury my theory would do, if I did permit it. Every man who reflects, and who loves truth, knows that the Urethra will yield an increased secretion during the act of copulation, and with many, that increased secretion will be discharged, whether the act of emission be gone on with or not. And now I will avail myself of the opportunity which offers of pledging to you, that I am not fond of novelty, unless it appears to be for the improvement of science, and that I will never advance an opinion merely as a new one, unless I think it preferable to any former one on the subject.

In order to proceed to a right understanding of the true state of this important enquiry, let us ground our arguments upon the common and usual circumstances attending a Gon-

a Gonorrhœa, and make ourselves, from the habit of reflection, familiar with those points which ought to be best known, and most observed upon, because they are the most likely to come to pass: for I do not think myself confined to the cure of the Venereal Disease only, the exposition of its nature, so far as it assists to the prevention of its mischiefs, is as important as any other consideration about it. When a subject has had a suspicious connection, and a few days after, a Gonorrhœa appears, attended by some degree of inflammation, there the case is perfectly clear; there will be no doubt about the power of conveying infection. But when a Gonorrhœa has been protracted for six weeks or two months, or beyond that time, when all the inflammatory symptoms have subsided, when nothing but a thick discharge remains, and a subject, thus conditioned, becomes desirous to form a connection where he must not injure, is it safe? I take upon me to say that it is not safe. This is generally the predicament in which many well-intending women are found; for they, from the nature of the parts, cannot with so much accuracy decide, whether any infectious discharge remains or not: they have most of them some discharge when they have never had any infection; and it is in this doubtful situation that, to their confusion, they do sometimes ruin their prospects by so unfortunate a discovery. If you ask, where are we to draw the line betwixt security and danger? I answer, that it is the presence of any discharge, so circumstanced as I have

stated,

stated, which will make it dangerous; and the absence, alone, perfectly safe. It is allowed that the sexes are subject to an increased discharge from the mucous parts, without being provoked through any venereal infection, and which is called a Gleet, in man, and Fluor Albus, in woman. A man may have a Gleet, who never has had a Gonorrhœa; and a Fluor Albus may most certainly be found upon a woman, where the foulest suspicion dares not fix a worse idea. A man may marry, with a Gleet upon him; and a woman, with a Fluor Albus. But it behoves us to beware that the latter end of an infectious discharge be not construed into a Gleet or Fluor Albus; and whilst a discharge remains, after a confirmed Gonorrhœa, on both sexes, it behoves them to get completely rid of it, or at least to remember that unfortunate discoveries have been made.

If you ask, what local symptom of the Venereal Disease retains the longest power of infecting? I shall most certainly answer, that it is Gonorrhœa: for if it was not this symptom of the Venereal Disease which laid the foundation of that general disease, as it now prevails at Otaheité, I know not how otherwise the infection could have been conveyed by a ship's crew to that part: there might, indeed, have been a constant renovation of infection, if women were on board, and then the state of the case, in that particular, is quite altered, as that puts all conjecture out of the question.

The two general local first symptoms of the Venereal Disease are said, by me, to be Gonorrhœa and Chancre: the former consists of a virulent discharge of fluid from the Urethra in man, and Vagina in woman; the latter may be an ulcer on any part of the Penis of a man, or Labia of a woman; or on any other part of the cutis or cuticle, where the venereal fluid which is necessary to infect, has been so lodged as to produce that effect.

It has been a question of late years, whether the infecting discharge from the Urethra is capable of producing a Chancre on another subject—or whether chancrous discharge is capable of producing a Gonorrhœa on another subject—or whether a subject, having a Chancre, can infect another with a Chancre and Gonorhœa—or whether a subject, having a Gonorrhœa, can infect another with a Chancre and Gonorrhœa? Whether these two symptoms be produced from virus, the same in its nature but only acting on two different anatomical parts—or whether the virus which produces them be not of a different nature, so that Gonorrhœa from one cannot produce Chancre on another; nor can Chancre from one, produce Gonorrhœa on another?

This question has already been touched upon once by me, when I was observing upon the subtilty of the poison: but I did not then go at large into the enquiry, and offer those observations, which are the result of practice. It must be

be confessed, there are many subjects so intricate in their nature, and some of them so far out of the reach of human understanding, that all disputation upon them must be founded upon conjecture, and it is most probable that the real cause which produces them may never be ascertained: as for instance, the nature of the Small-Pox—of the poison of the Lues Venerea—of the poison of the Viper—of the poison from the bites of mad animals—what all infections consist of—and many other intricate questions, through a variety of metaphysical speculations; such as the load-stone—electric ether—the composition of the body of the sun—the age of the earth:—These are subjects through which, those who delight to wander, fancy they have found a direct path, and can trace the real causes of certain effects; they enjoy their theory, but it is such a theory as never remains unanswerable. It cannot obtain a general assent, and therefore the truth cannot be fixed.

But it seems astonishing that a doubt should remain at this day in the mind of any man who had adverted to the subject, whether the infection from Gonorrhœa, and the infection from Chancre be the same in their nature or not. By those who have doubted they were the same, gonorrhœal fluid has been applied to the cuticle and cutis, for the purpose of proving whether Chancre could be produced by it or not; and chancrous fluid has also been applied to mucous parts, for the purpose of proving whether Gonor-

rhœa could be so produced. When this experiment was made by one person it succeeded, and when it was made by another it failed. A third person, and who was apprised of the two former experiments, still doubts whether the point in question is clear or not, and therefore thinks, that the fact, to be compleatly established, wants a stronger confirmation.

Too often candour and cool deliberation are excluded from those enquiries which depend for the truth most upon them; and we are all too willing to incline to the arguments which favour our own opinions, and to neglect, or treat carelessly what might be adduced on the contrary side. All experiments which are made to ascertain whether the poison of Gonorrhœa and Chancre be the same, are artificial means for ascertaining a fact, better to be ascertained by natural events. But it should not be forgotten, that a fact may fail of being proved, through an error in the experiment. In this question the truth could be well ascertained, if one experiment succeeded out of an hundred; for if gonorrhœal fluid could not produce Chancre, nor chancrous fluid produce Gonorrhœa, if that were true, artificial experiments might be made to eternity without prospect of success. But it only remains that one should succeed, and the fact is proved. A man, for instance, may deny that a single ball discharged from a gun will kill a bird flying, and the trial might be repeated by a very bad shot, at least a thousand times, without success; yet we know the position

position to be true. Therefore, if the experiment which proves that chancrous and gonorrhœeal fluid are both the same in their nature, has been once found successful, all contest upon this question must be at an end, if you really give due credit to the person who tried it.

But is it not wonderful that those who with so much curiosity have given us experiments of art to ascertain the sameness of the poison, should have said nothing about what might be observed upon the natural progress of the poison? Or do men expect, when they want to be obliged by Nature, and have their own wishes gratified, that she will yield more information by being tampered with through art, than by being left to her own action, and to be viewed in her proper light in which she presents herself? Have they told us they were induced to make those experiments, because there was no other means of ascertaining the fact? And is that the case? Is it impossible to examine a woman, who from a natural intercourse has infected a man, and from that examination to decide, whether she infected him from Gonorrhœa or from Chancre? Is there a difficulty in discovering a Chancre, if she has one—and if there be no Chancre, must not such infection have been from Gonorrhœa?

If women who infected men with Chancres, all necessarily possesed Chancres, certainly those Chancres would

go

go on increasing to such a degree, that they could not long continue to receive embraces, and would be obliged to keep to themselves, at least till their Chancres were healed.— One might expressly declare, without being deep in venereal knowledge, that women so conditioned would be in retirement for a little while; for if they continued to appear in company, whilst they took mercury, their breath would betray them, and their intentions would be defeated. But in Gonorrhœa, the case is widely different. Many men who have been infected with Chancre, by women who had only a Gonorrhœa, have been obliged to abandon their charges against them, after the women have submitted to examination, and their avowed innocence has been confirmed, by their never being out of the way, nor by the least change of their health: but a Chancre, which is important enough to infect, never can be concealed, whenever it be looked for. But there is a new observation which I have to make, and which is, that the effect of infectious secretions from one subject, acting upon another, differs essentially from the infectious secretions of a subject taken from one part and applied to another part of the same subject. Those who have been making the experiments I have alluded to, knew nothing of this distinction.

The fact, that the fluid of Gonorrhœa and the fluid of Chancre are both the same, I have no more doubt about, than I have of my present existence, or any other self-evi-

dent

dent proposition. The men in general who present themselves to me, refer me to the women whom they know to have infected them; and the men in general who present themselves to me, are such as I can depend upon for their veracity—for there cannot be the least inducement, nor the least point gained by their concealing or confounding the truth. Young men have no guilt to answer for to me, nor do they care about their condition, otherwise than as it is injurious to their health, and to their pleasures—Then why should they deceive me, by telling me, they had since the opportunity offered of being in the way of infection, connection with one woman only, if they had had connection with more? Or what other proof or stronger can I have of putting this question to the test, than by repeated references from men of veracity, thus situated, to the women who had infected them? The result of such enquiries have been in every instance invariably strong to authorise me positively to declare, that the fluids of Gonorrhœa and Chancre are both from the same origin. What insight can an artificial experiment give, farther than this appeal supplies? What other proof is wanted, than to know the men who come before you thus situated, and to be referred by them to inspect the condition of the women? When Chancres present themselves they must be seen; if they be not seen, Gonorrhœa, which is certainly the most general symptom, will always be conclusive to my expectation whenever I search.

One

One reason, and I think, upon the face of it, the most plausible of all others, why the fluid of Chancre and Gonorrhœa may be said to be not the same, is that a man may have a Gonorrhœa without a Chancre, and a Chancre or Chancres without a Gonorrhœa. And it might be also held that this is in general the case, and also that the one does not produce the other upon the same subject, and that the one is not commonly found, when the other is present. It is upon this point, I think, the advocates for the opinion, that the fluid of Gonorrhœa and Chancre are not the same, may make a stronger stand, than at any other. Is it not strange (they may say) that, if both the fluids possess the same virus, Chancres do not inevitably accompany Gonorrhœa, and Gonorrhœa Chancres, on the same subject? They might also add, if both infections were not receivd at first, yet if they be of the same quality, why does not the continual flowing of Gonorrhœa produce Chancre upon the Glans and Prepuce; and why does not chancrous fluid produce Gonorrhœa?

I have placed this objection in the strongest point of view, because I think it my duty to do it; for I should be sorry to be thought either positive, or mean in argument. He who has not the candour to listen to the arguments of others, must be confident that he cannot retain enough of his own to command their conviction.

I ſhall

I shall answer this objection under two separate heads:

First, what I have to say with respect to the local action of the infection, as it is received from another subject; and secondly, that which offers to me on the local action of the infection upon other parts of the same subject, after it is received.

The two first symptoms, which are Gonorrhœa and Chancre, are already confessed; but when they both are found on the same subject, and from the same contraction, their order of appearing perhaps may not be so generally known. When a person has contracted both symptoms from the same connection, Gonorrhœa will be the first to betray the infection—the Corona Glandis, Frœnum, and the internal part of the Prepuce will be the next—and the cutis, or the external part of the Prepuce, will be the last. I have known the space of time between the appearance of Gonorrhœa and that of Chancre on the cutis of the Penis to be three weeks; and yet as there was but one connection to produce both, I am confident the one did not arise out of the other; but the true cause of the distance of periods between them was, that it would take but a short time for the venereal stimulus to produce a discharge from the Urethra, and a longer time for the same stimulus to produce a Chancre on the cutis; and also, that it would take an intermediate time between the former and the latter for the same stimulus to

produce a Chancre on the glans, internal part of the Prepuce, or Frœnum.

When all these symptoms do appear from one infection, such is the general order of their appearance; I say general, because I do not suppose but there might be exceptions. It is also very well known to those whose practice affords them the opportunity of obtaining that knowledge, that a Gonorrhœa is the most general first symptom, when it comes on alone; that Chancres on the Frœnum, and internal part of the Prepuce, are the next most in common; and that Chancres on the external part of the Penis, and parts adjacent, are the most uncommon. Why this difference is found, it is not owing to the virus from the woman not coming into contact with the one part more than the other, but to the difference in the construction of those various parts—to the more susceptibility of the one than the other—to the one being more disposed to be stimulated by the virus than the other—to the one being capable of being acted upon by the virus when the other may not—to the virus being lodged and remaining undisturbed—to the faint power which the virus might at the time possess when the infection was given—or to the difficulty there is in the subject who is infected, to be infected.

For if a man be connected with a woman who has a Gonorrhœa, every part of the Penis may be presumed to take up

up the moisture; but as the cutis is more difficult to be infected than the cuticle, and the cuticle than the Urethra; and as the fluid lodging on the external part of the Penis is more liable to be wiped away, so does it happen that that part is the most exceptionable. The Urethra being a part liable to be stimulated by a stimulus which would not stimulate the skin, is, of course, liable to be stimulated by a power which in its nature may be too faint to stimulate or act on the Prepuce: besides, the natural formation of the Urethra prompts it to throw off any thing which is obnoxious to it, or at least, it is readily prompted to discover whatever produces an irritation upon it: and therefore also, it might very naturally be inferred, that if there be stages of the Gonorrhœa when the power of the infecting fluid is weaker, and less active, the Urethra, from its nature and construction, will be liable to be acted upon by it, when other parts cannot.

In order that I may be well understood, I shall explain what I mean by the power of the virus being faint at a certain stage of the disease, and how far that faintness or defect of power might operate or affect a person who is infected from virus so conditioned. I mean, if a woman possesses a Chancre or Gonorrhœa, or both, and the fluid of which, at the period when the man cohabits with her, is so conditioned, that the discharge is very kindly, that the Chancre is healing, that the Gonorrhœa is almost gone—I mean that

the fluid thus conditioned, when it does infect, is not capable of infecting the cutis, when it is capable of stimulating the Urethra. I mean to point out, by this observation only, the probability there is that the virus, at such a stage, cannot act on any other part, less irritable than the Urethra, or a surface similar to it. This appears to me to be a reason why a person may have a Gonorrhœa, without a a Chancre; and why a Gonorrhœa prevails much more than Chancre does. But I believe it often happens, that the undisturbed lodgment of the virus is the cause of the appearance of the first infection on any part.

Although I have said that a subject may be infected with a Gonorrhœa, who, through the faintness of the virus, could not be infected with a Chancre, yet I do not mean to be understood, that the symptoms of a Gonorrhœa, upon the subject so infected, will be in the least influenced from the greater or less degree of virulence in the infection of the subject who infects: for as the infection is proved to have taken effect, the symptoms will not depend upon the condition of thevenereal poison, as it was in another—they will depend upon the nature of the constitution, and upon the degree of irritability of the subject who has received the infection. And thus is this observation, in its nature, correspondent with the more general laws of every other infecting poison. This manner of reasoning will be found universally true, both as to the local action of poisons, as well

well as to their constitutional effect. If I were to inoculate a person with the Small-Pox, and took the matter from another who had only a single pustule, it does not follow, but the inoculated subject may sink by the load of the disease. In the Jail Distemper, as it is called, the prisoner who is able to appear in Court to take his trial, may circulate that infection, which has often proved fatal to those who caught it.

The last observation which I have to offer is—the difficulty there is in a subject who is in the way of infection, to be infected—and this is a cause why such a subject may be liable to contract a Gonorrhœa, when he may not a Chancre. This difficulty of receiving the infection may be either natural to the constitution, in the first instance, or be produced from habit. In the natural constitution, if it be in general irritable, parts of it will also partake of that general irritability, and will of course be more susceptible of the action of virus, than where such irritability is not predominant; such a constitution is more exposed to infection, or at least, suffers much more from it, than one of an opposite description. But the common habit of intercourse with women liable to give the infection, will so far take off from the irritability of the parts, that in many instances men have gone on for years, from one woman to another, without the least sign of infection; whilst a stranger to the town, on his very first connection with them, will immediately feel the virulent

virulent effects of the embrace. This fact is so indisputable, that whenever the argument is started in conversation, it never fails to be remarked upon; I do not mean by medical men alone, but by all those who have been in the way of proving the fact. And last of all, to prove that the Urethra as well as the whole of the substance of the Penis, is sometimes not in a disposition to be acted upon by venereal virus, notwithstanding the fluid has evidently been lodged there, I shall call to your recollection, that the first sign of infection has been often pointed out by a Bubo in one or both groins.

Having said what I here intended, with respect to the local action of the infection, as it is received from another subject, I shall now come to the second head, and consider that which offers to me on the local action of the infection upon other parts of the same subject, after it is received.

It has been said, and very truly said, that if a Gonorrhœa and Chancre be found upon the same subject, they do not in their progress interfere with each other; that they appear to be so very distinct, and require such a different treatment, that those who at first suspected them to have not originated from the same virus, will yet, from these observations, have their suspicion more strongly confirmed. If a subject has a Gonorrhœa only, I am of opinion that the Gonorrhœa will not produce a Chancre on the external part

of

of the Penis, nor within the Prepuce. I will go farther, I am confident that a Chancre is never produced from a Gonorrhœa of the same subject; and I am also confident that a Chancre on the Penis will not produce a Gonorrhœa on the same subject. This may, in the opinion of some, be still widening the difference in the nature of the two fluids, which, according to my theory, ought to be united. I will go farther and say, that I have seen a Chancre destroy almost the whole of the glans Penis, without bringing on a Gonorrhœa; and I have seen a Gonorrhœa produce a swelling of the Penis—an excoriation and discharge upon the glans—an abrasion of the whole of the surface within-side of the Prepuce, without there being the least appearance of a Chancre.

But the truth of all this does not alter the question, because it is no proof, but the infection which produced either or both symptoms might still be the same. This certainly is no proof, that the twos timuli were not excited from the same virus; but it is a proof that the two stimuli, when once excited upon parts whose secretions naturally will be different, and upon the same subject, will not interfere with each other.

I can put this question in a much more logical, and in a much stronger light than I have hitherto done—for hitherto it has been only presumed that the virus is not the same, because

because Gonorrhœa will not produce Chancre, and Chancre will not produce Gonorrhœa on the same subject. I say hitherto it has been given only as presumptive proof. But I will give a positive proof, that this difference may exist, as to their action, and the virus yet be the same. For as I have observed that Chancre is not produced out of Gonorrhœa on the same subject, so do I now observe, that on the same subject, one Chancre will not produce another. This is a positive proof, that the first infection received has produced a Chancre or Chancres, but that this Chancre or Chancres will not generate, or, at least, do not generate other Chancres on parts which come into contact with them; for if that were the case, the whole of the Penis would, time after time, become the seat of a succession of Chancres, arising out of ab origine ones. But, on the contrary, the original Chancre continues on, increasing in its size, and growing by degrees worse and worse, until, in all probability, without the interposition of art, the whole of the Penis would be destroyed by it.

If the virus from a Chancre could produce Chancres on the same subject, then Chancres would be found wherever the fluid touched and lodged on a sound part, and the whole of the glans would shortly become a galaxy of Chancres.

I will state a case for the illustration of my argument. A gentleman that I was attending at the time of my writing this, first of all perceived a Chancre on the Frœnum, and another

another about the eighth of an inch from the Frœnum. He had discovered both about three days. On the fourth day, after he applied to me, a pimple was discovered on the dorsum Penis, which was observed for three days, before the truth of its nature was ascertained. This also proved to be a Chancre, and the late appearance of it after the two former, was most assuredly owing to its being on the skin, as the two former were more early, by being on parts not cloathed by skin. They were all three contracted at the same time. These three Chancres never produced a fourth. The two first could not possibly produce the third. The third was quite out of the way of the fluids of the other two; besides, it was discovered too soon after the other two to be produced by them. I cannot take upon me to say whether the woman who infected this gentleman, did it through Gonorrhœa or Chancre; but I will give my reason, why I think he was infected by Gonorrhœa and not by Chancre—because two of the Chancres were about the Frœnum, and the other at a distance from them on the dorsum Penis. One Chancre could not have produced this infection from the woman—there must have been more—but the fluid from Gonorrhœa could effect it with facility.

I will now rest this argument upon the result of general experience, and which I am confident is the only solid method of reasoning upon this or any other question; for how are we to know what the Venereal Disease is, or what any

other disease is, but from the result of its action. And first, it is evident to my senses, from common observation, that the virus of Gonorrhœa and Chancre are one and the same:—secondly, that a man may be infected with a Gonorrhœa and a Chancre, or with a Gonorrhœa only, or with a Chancre only, by once cohabiting with a woman infected with either:—thirdly, when I find that Chancres appear all at the same time around the glans Penis, as they often do, I am of opinion that the infection which produced them was from Gonorrhœa; or whenever they appear on the Penis, either all at one time, as they will on parts of the same texture, or whenever they appear at stated periods on parts different in their texture—then I am of opinion they were produced from Gonorrœha:—fourthly, I do not believe that gonorrhœal fluid does ever produce Chancre on the same subject, nor do I believe that Chancre ever produces Chancre on the same subject, or Gonorrhœa. This is, so far, my venereal creed—and this is confirmed by the concurring testimony of natural occurrences, as they offer through the first infections of Gonorrhœa and Chancre.

I have gone no farther with my theory in this Lecture, than what is barely necessary to explain my ensuing Lecture. To make theory and practice clear to your understanding, I mean that mine shall go hand in hand.

LECTURE V.

ON

GONORRHŒA.

VENEREAL infection is always conveyed in some substantial form—either by gonorrhœal fluid, or chancrous or ulcerous fluid: it is in these forms imparted and continued from one subject to another, and which is the very support and existence of the disease. This fluid applied to any part of the body, from one subject to another, will act, although with more difficulty on skin, than on a mucous surface.

Whatever part of one subject comes into contact with the infectious fluid of another, that part is exposed to be infected; and as from the nature of the disease, the Penis in coitu is the part thus exposed, so is the Penis in general the first local part infected. The infection there shews itself either in the form of Gonorrhœa or Chancre. But it is of Gonorrhœa that I am now about to treat, for I mean to take another opportunity of enlarging my opinions upon the subject of other modes of infection.

It is obvious to our constant feelings, that when any thing is applied of an irritating property to a mucous surface, it increases a discharge of mucus beyond the common degree of secretion; and when the Urethra is stimulated by venereal infection, the discharge of venereal mucus is then called a Gonorrhœa.

I am not fond of changing the names of things, after they have been long understood to convey certain senses, although they may not critically convey the true idea of that which they are meant to convey; for a word can only convey the name of a thing, but never the full meaning or description of it; and therefore if we do but know the thing by a name, that is all which is wanted. I consequently shall continue to call the increased discharge of mucus from the Urethra, when brought on by venereal infection, a virulent Gonorrhœa, although the semen has nothing to do with it, because the disease is already understood by that name, and not so well by any other.

The application of venereal fluid to the Urethra produces a stimulus on the surface of it, and that stimulus must produce a discharge, and so it would, if it were applied to any other mucous surface of the body. It is a consequence which must follow, and the symptom of discharge is in this instance the natural effect of the venereal stimulus on the part.

It is not uncommon for men, from their admiration of novelty, to detect one error and to plunge into another—to know what a thing is not, before they know what it is—to be able to convince us from certain reasons, that the thing cannot possibly be what it is asserted to be, and yet to be still at a loss to say what it really is—or at least, he who says with truth what a thing is not, does not by that establish what it really is. The Ancients called this discharge brought on by the application of venereal virus, a flowing of semen —the Moderns have clearly disapproved it, and we are now convinced that it is not a flowing of the semen. But, although we all now disavow the discharge from the Urethra in a virulent Gonorrhœa to be a flowing of the semen, yet there is a difference of opinion amongst the Moderns about the true nature of this discharge: some will have it to be an increase of mucus from irritation, and others to be pus—to be the true conditioned pus—whilst others take the middle path, denying that it is mucus, and only asserting that it is resembling to pus.

In giving my opinion upon the nature of the discharge, as it is always seen in a virulent Gonorrhœa, I mean to do justice to my own understanding, and to abide by such truths as I can and do see, instead of looking after and producing for evidence, arguments which are hidden, and proofs, which are obscure. No chemical processes can prove pus and mucus to be the same, if they really be not so—nor

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can any other mode of experiment, be it either the result of ingenious inquiry, or singular vanity—for Essays upon this subject, which have treated it in all its ways, have been published, and them I have read.

If from any irritating cause, the mucus, which is naturally secreted, and is spread over the surface of the Urethra, be increased—if the glands, which secrete this mucus, from the same cause be also irritated—if there be an increased action in the secreting power of the glands—if the mucus, from that increased action, be not so perfectly elaborated as to assume the appearance of true conditioned mucus, in a healthful state—but if from that increased action it be so elaborated as to assume the true condition of such mucus as constantly flows from glands thus irritated—I shall certainly call such a discharge increased mucus from irritation. It has as great a claim to, and it might with as much propriety retain the appellation of mucus, in this state, as saliva is still called saliva, when the effects of mercury has stimulated the glands of the mouth, and thereby increased the quantity, and changed the taste, smell, and appearance of that fluid.

If the discharge from a virulent Gonorrhœa be not increased mucus, because its colour, smell, and every other appearance vary from mucus which is secreted without irritation, it does not follow, that it should be said to be pus; for this would be admitting that because a thing is so changed,

ed, and cannot be believed to be the same thing that it was, it must now become a thing which it cannot be.

The whole of the membrane which covers the fauces, larynx, and internal parts of the nose, the antra of the jaws, the sinuses of the sphenoid and ethmoid bones, and the sacculus lachrymalis, yield a mucus, limpid in colour, small in quantity, and almost without any smell; but this fluid, either by being confined beyond the necessary time, or by inflammation, or irritation of the gland or membrane which secretes or contains it, or even from general affection of the habit, will put on a yellow purulent colour, where there is neither abscess nor ulcer in the part from whence it flows. So many instances of this are producible, as to put the matter beyond a doubt. The lining of all these parts is constantly embued with a mucus, naturally clear, and no more in quantity than is necessary to keep the membrane moist; but either inflammation or irritation does immediately so add to its quantity, and so alter its colour, that the discharge from these parts has been taken for purulent, as if it was produced by ulceration.

As a Surgeon, whose memory is endeared to us, and whose fame will not soon yield to the lapse of time, says, "These "two fluids, pus and mucus, which have been so fre- "quently confounded together, do really differ so widely "from each other in their nature, constitution, sources, "purposes

" purposes, and effects, that to distinguish them properly,
" and to point out the true character of each, seems to be a
" matter of much importance.

" Mucus, considered in a general sense, is the effect of a
" natural secretion made by glands, membranes, or other
" bodies appointed for that purpose; and is so far from be-
" ing originally the consequence of disease, that, in a due
" quantity, it is absolutely necessary for several very im-
" portant purposes in the animal œconomy; which pur-
" poses, 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 intestines, 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 Prostrate
" 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 frequently has in the produc-
" tion of diseases, when it is either vitiated or redundant.

" Pus, or matter, is certainly no natural secretion; sup-
" puration, 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 or
" destruction, at least of division; for, without entering mi-

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" nutely 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 capillary 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 towards
" 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 healed, the discharge necessarily
" ceases.

" On the contrary, mucus may, by irritation, relaxation,
" or defluxion, on its secreting or containing parts or or-
" gans, be increased to a quantity far beyond what is ne-
" cessary or useful, and produce thereby a disease in parts
" where there is not the least degree of solution of continui-
" ty, as in the cases of tenesmus, stone in the bladder, Fluor
" Albus, and simple gleets from the Urethra; as also in that
" kind of defluxion on the nose and fauces, producing a
" catarrh, and in the intermediate effect of all sternutato-
" ries.

" Other differences between the nature and properties of
" the two fluids might be mentioned; 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.

" Inflammation and irritation of the membranous linings " of the Urethra and Vagina, will fully account for all the " appearances in this disease, in which there is neither mat- " ter, nor ulcer, nor abscess: whoever will attend to the dis- " charge made from a purulent ulcer, will find it widely dif- " ferent from that which issues from either of the above " parts, in the Gonorrhœa.

" In cases of strictures in the Urethra, the discharge occa- " sioned by bougie, properly and judiciously used, is a dis- " coloured mucus, and not matter, though it is generally so " called: it is from the discharge of this mucus, and the di- " latation of the passage, that the relief is obtained, not from " any destruction or division of the parts: the bougie which " produces true matter, does much more harm than good, " and makes a sore where there was none, and where there " ought to be none. How often do catarrhous defluxions " on the trachea and larynx wear, towards the close of them, " a deep purulent colour, so as to deceive the unknowing " into an opinion, that it is matter upon the lungs? But " no judge of these things ever had recourse to abscesses or " ulcers, for a solution of such appearance. The argument " drawn from the quantity of these discharges is as erroneous

" as

" as that taken from its colour; as an inflammatory de-
" fluxion on the part does generally occasion the latter, so
" mere irritation will produce the former, which does also
" generally cease when the irritating cause is removed or
" appeased. How immediately is a most troublesome tenes-
" mus cured by a clyster of starch and opium? What large
" fetid discharges are made from behind the prepuce of ma-
" ny persons, not only free from all venereal taint, but with-
" out any ulceration of the parts, by a kind of exudation?
" To what length of time will they not continue, if neglect-
" ed; and how immediately do they cease by the use of a spi-
" rituous or vitriolic wash? How often is the Fluor Albus,
" even in some of its worst circumstances, moderated, not
" to say cured, merely by washing away the acrid mucus,
" which, lodging in the rugæ of the Vagina, continually
" irritated the parts to a fresh discharge, and perpetuated the
" disease? What quantity of slime is there in the urine of
" those who have a stone in the bladder? And how totally
" does it cease, upon that stone being discharged or taken
" away? Whereas, neither cleansing of parts, nor removal
" of irritating bodies, does or ever can procure an immediate
" cessation of a discharge of true matter, which being occa-
" sioned by a solution of continuity, an erosion or division
" of the parts whence it proceeds, must decrease gradually,
" and at last can only cease by such part becoming whole
" again.

" In short, the two fluids are so absolutely different and

" distinct, that the blending them together, in our ideas of " disease proceeding from, or producing either of them, " cannot be too industriously avoided." *

The difference betwixt pus and mucus, in my opinion, can never be reconciled; and I am at a loss to conceive how, besides this Author, others have fallen into so outrageous a conceit—a conceit that militates against all common sense, and common observation. It must have originated from the lowest order of reasoning, from the mere similarity of colour of the two fluids: and upon the same pretence, as well might milk of roses, as it is called by the Perfumers, be applied to the purposes of cow's milk, because the eye may be deceived in the distinction.

Although so much has been said upon the subject, merely as an answer to what has been advanced on the other side of the question, yet the true distinction, and all that ought to be regarded in order to point out the difference betwixt pus and mucus, lies in a very small compass indeed, which is, that increased discharge of mucus can only be found to flow from the surface of mucous membranes and the glands thereof, without any solution of continuity, but barely as an increased secretion; whereas pus will be found on parts all over the body, where there is a destruction or solution of parts. This is the exact state of the case.

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* Pott, on Fistula Lachrymalis, vol. 1.

A virulent Gonorrhœa, therefore, I define to be an increased infectious discharge of mucus, from the membrane which lines the Urethra in man and Vagina in woman, and from the glands thereof. This discharge is occasioned by the stimulus of the venereal virus communicated to these parts, generally at the act of coition. When this discharge is perceived from the Urethra, it commonly appears within three or four, to twelve days, and sometimes longer, after the infection was received.

First of all, there is an unusual aching heavy pain about these parts, attended by a rotation of the testicles; afterwards a fluid distills, by little and little, from the orifice of the Penis, and appears in very small spots on the linen; or if the Penis be pressed by the finger and thumb, this fluid will be forced out, and the Urethra will feel painful and sore about the glans. As this fluid comes away, it occasions a titillation in the part; which sensation is likewise felt in making water in an increased degree, until at length it arises to a sensible heat, and a sharp cutting pain. This heat and pain of making water is at first also felt near the orifice of the Penis; and the lips of the orifice appear turgid, red, and open, and are exquisitely tender to the touch. The glans Penis also becomes hard, swelled, and glassy, and puts on a redder hue.

As the irritation spreads on the membrane, these symptoms

toms are followed by others. The discharge increases, the sensation of pain is more acute, erections are involuntarily excited, or rather excited from a stimulus in which the mind has no concern, and are more frequent and lasting than what are termed natural erections. They are also attended with much uneasiness oftentimes, and the distended Penis feels at that time as if it were squeezed on both sides by some strong power. This symptom is most intolerable in the night time, when the patient begins to be hot in bed: it often awakes him from his sleep, and he is obliged to rise from his bed to alay it.

About this time also a Chordée is felt, which begins by a spasmodic action on the Frœnum, during erection; the pain of which is extended all along the Urethra, as if the Frœnum was upon a constant stretch; and sometimes in consequence of this strong action the substance of it gives way, and a considerable hemorrhage follows, which, for a time, mitigates the symptoms. The discharge, at this period of the disease, generally increases; and especially after straining to make water, and just at the close of making water, the pain becomes more sharp and pungent. The discharge recedes daily from the natural appearance of mucus, changes both as to colour and consistence, and puts on the exact appearance of a discharge from the nose, in a cold.

When the disease is arrived at its most aggravated stage, all

all those symptoms become yet more acute; the heat of urine is so extremely intense, that the patient dreads the evacuating of it, when at the same time he is constantly soliciting to evacuate it, by a teizing uneasy sensation about the neck of the bladder. There is also, at this time, a constant heat and fullness all along the Urethra, and of the glans. The patient cannot sit down easy in his chair; pain is felt about the anus, and the preternatural action of the acceleratores urinæ make them tender when they are pressed upon; and the glands in the Urethra are swelled, and are often to be easily felt. The urine comes away in a small stream during the inflammatory height of the disease, owing to the general inflamed state of the Urethra; and when the swelling of the glands intrudes upon the Urethra, the stream of urine will be scattered, and sometimes forked, owing to the irregularity of the surface of the duct through which it passes, and to the irregular action of the muscles in the expulsion of it. When the disease is arrived to this height, the most favourable of all appearances is a profuse quantity of discharge: for if the discharge was at this time to cease, other and worse symptoms, which will be hereafter described more at large, such as stranguary, inflammation of the prostrate, of the vesiculæ seminales, and of all the deep seated parts about the neck of the bladder, would follow; and that they sometimes do, it is known from experience; and it also known, that a kindly return of the discharge, is

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the most certain sign of their favourable and early abatement.

I have stated, in as concise a manner as I could, the gradual effects of an inflammation brought on from a venereal stimulus upon a mucous membrane, from its commencement at the orifice of the Urethra to the remotest part of it; and it appears that the discharge from the Urethra during the first inflammatory stage of the disease is expressive of the various degrees of inflammation, as well as of the extension of the inflammation along the surface of the Urethra. And as it is the case in other inflammations on other mucous membranes, so it is in this instance, that sometimes the inflammation proceeds to such an increased degree, that the surface of the Urethra and the glands within it, are rendered incapable of yielding any secretion, and that during this stage of the disease, all discharge is of course checked. This is also the case in a severe cold; and the safety of the patient, and his speedy restoration to health, is prognosticated by a return of discharge from the nose, and by a plentiful spitting, when the trachea and lungs have been oppressed. This is, in my opinion, the true path for pursuing the theory of inflammation on mucous parts; and by this mode of reasoning we readily are enabled to assign a cause why a discharge from the Urethra is all at once disconsinued, when at the same time the inflammatory symptoms are higher than ever.

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I shall often allude to the theory of a late Writer on the Venereal Disease, without, I trust, being in the smallest degree personal; for at any rate I would not be suspected of personality. My former publications were not only designed to inquire into his theory, but also into the motives which induced him to adopt some parts of that theory. But now I beg to be considered as an individual travelling on through the several stages of this disease, with the most anxious and heart-felt desire of acquiring honest fame from what I explore—to be considered as a bigot to no theory whatever—not so positive as to have the bias of my mind fixed necessarily to any other theory but that—which I sincerely think the true one—not so positive as to adopt any innovation, but such a one as reason will support me in—and as deviating, with delicacy, from former opinions, when the research will justify me in it, because I doubt their truth.

A late Writer has said, that the specific distance of the disease, or specific extent of it, is not farther than an inch or an inch and a half within the orifice of the Penis: for he says, "as the Gonorrhœa is commonly an inflammation, it "is accompanied with pain, and the formation of matter; "in such a state, neither the sensations of the patient, nor "the actions of the parts themselves, are confined to the "real seat of the disease." And then he goes on in shewing what other symptoms follow those original specific symptoms, and states them thus: "In consequence of the "neighbouring parts sympathising, a variety of symptoms

" are produced, many of which do not exceed what may " arise from an irritable state; an uneasiness, partaking of " soreness and pain, and a kind of weariness is every where " felt about the pelvis: the scrotum, testicles, perinæum, " anus, and hips, become disagreeably sensible to the pa- " tient; and the testicles often require being suspended; " and so irritable are they, indeed, in such cases, that the " least accident, or even exercise, which would have no such " effect at another time, will make them swell. The glands " of the groin are often affected sympathetically, and will " even swell a little, but do not come to suppuration: when " they inflame from absorption of matter, they in general " suppurate. I have seen cases where the irritation has ex- " tended so far as to affect with real pain the thighs, the " buttocks, and the abdominal muscles, so that the patient " has been obliged to lie quiet, in an horizontal position; " the pain has at times been so considerable as to make him " cry out, and the parts have been sore to the touch; they " have even swelled, but the swelling has not been of the " inflammatory kind, for though there was a visible full- " ness, yet the parts were rather soft."

It would be needless to quote farther, in order to exemplify what I wish to remark in my answer to this, for I have quoted sufficient for that purpose already.

I agree with this Author, that many of the symptoms, which I have stated from his publication, are not specific symptoms

symptoms of the disease, but are the consequences of high inflammation of the specific parts upon which the venereal virus is acting. Inflammation or pain carried to a certain point cannot fail of affecting other parts of the same body besides that which is the principal in suffering. This must be readily admitted, and this is not a new observation, nor is it a new belief. But I am at a loss to know what has induced this Author to suppose that the venereal virus, which produces Gonorrhæa, only specifically acts an inch or an inch and a half up the Urethra? What is there in that part of the same mucous membrane which is continued through the whole of the urethral canal, so particular as to stop the progress of the true specific venereal action? If the real action of the virus has proceeded an inch and a half, why should it not proceed farther, when it is to continue acting upon the same anatomical membrane? If it can go an inch and a half, it surely proves that it has the power of going on.

But what are our feelings upon this question, and what are the feelings of our patients? If some of us do believe that the virus can extend an inch, or an inch and a half only, will others be persuaded that what they feel beyond that mark, is only the effect of what is in reality acting up to that mark? Does not the inflammation throughout the Urethra appear to be the same? Is not the progress of inflammation through the Urethra regularly continued? Is not

the discharge excited as well beyond as up to this mark? Is there any anatomical change in the membrane which commences there? What can be the inducement for such a new doctrine? It is very true, that when the disease is mild, and when the inflammatory symptoms are but of little moment, the inflammation may be confined to the part nearest to the orifice of the Urethra; but this must be only said to be the milder and incipient condition of the disease—and thus it will be with a slight cold on another mucous membrane: but it is as true, that the severity of a cold consists in the more general inflammation of that membrane, and that the severity of a Gonorrhœa also consists in a more general inflammation of the membrane of the Urethra.

Had this Author considered (for that is the present object of consideration) how it happens that any discharge was produced by a local application of venereal virus, a few days before—I say, had this Author considered that no such symptom as a discharge could possibly have been produced in such quantity and of such a nature, if the same venereal virus which excited it there, had been lodged and left to stimulate any other part of the body but that which was mucous membrane, he then would have been convinced, that the stimulus which had excited inflammation in a mucous membrane, and had increased the secretion of it, might proceed the length of that membrane: but beyond the length of the mucous membrane, in all probability, some of the symptoms

toms which I have enumerated in my quotation, might be, and are the effects of the specific inflammation on the mucous membrane; for I am certain they never accompany a Gonorrhœa, when the inflammation does not extend more than an inch, or an inch and a half up the Urethra, but only when the inflammation throughout the Urethra is very high and alarming.

The Urethra is undoubtedly liable to inflammations, independent of those brought on from venereal virus; but these inflammations are not attended with such high symptoms as those which are produced from a virulent Gonorrhœa. One of the most troublesome of all is that which is called a Chordée, which takes its name from the Penis being curved downwards, and the glans drawn in as it were by a chord. This symptom is most generally felt when the inflammation runs high, and is much more troublesome to the patient in his first Gonorrhœa than in any contracted afterwards. This symptom is chiefly experienced in bed, and when the Penis is in a state of erection. It is a circumstance of no little mortification, that the Penis should have the strongest propensity to erection at the time when only it were wished that such a power would fail—for the rest is disturbed, the symptoms are aggravated, and the disease generally protracted to some length, whenever a Chordée becomes one of the symptoms of a Gonorrhœa.

This

The Chordée is certainly produced from an inflammation of the substance of the Urethra, from the inflammation having penetrated beyond the surface, and affected the corpus spongiosum Urethræ. The corpus spongiosum Urethræ being inflamed, becomes painful and tumid, the circulation is impeded in it, and the power of extension restricted. The corpus spongiosum Urethræ, in a state of health, confirms and corresponds in action with the corpora cavernosa Penis. When the corpora cavernosa Penis are in a state of flaccidity, so is the corpus spongiosum: and when the blood is thrown into the corpora cavernosa, so as to increase their bulk, and cause thereby an erection of the Penis, the corpus spongiosum also receives its share of sanguineous aid, and completes the intention. This is often the effect of the will, often the consequence of venereal reflections, sometimes an act from the heat of the bed, from an inflammation of the part, and from a stimulus on other parts which correspond with the Penis. It is a venereal stimulus from a mental idea which produces the erection in a state of health—and it is an inflammatory stimulus of the parts which produces the erection in a state of disease.

In this instance of Chordée, the corpus spongiosum Urethræ is so highly inflamed, that it cannot bear distention, and it is the act of distention which produces the pain that is felt; for the corpora cavernosa are not at the same time inflamed

flamed, are capable of their usual distention; but when the blood is flowing into the reticular substance of the corpus spongiosum, the vessels of it are found to be inflamed, tumid, and too much loaded; the effort, therefore, cannot be even attempted, without producing great pain, and without sometimes lacerating the reticular substance, from which a considerable hemorrhage ensues, and which is, in general, found to be a relief. It is when the corpora cavernosa are swelled with blood, that the compression of them upon the corpus spongiosum produces a feeling as if the Penis was strongly squeezed between the finger and thumb. This is my Theory of Chordée.

The ardor urinæ, or heat of urine, is a sensation produced, not from the urine being more hot at that time than at any other, but from the change of condition in the canal through which the urine passes. This sensation is brought on from a change in the condition of the mucus, owing to the action of the venereal stimulus on the membrane. It is not only felt all along the Urethra, but more especially at the sphincter vesicæ. It is there so exquisitely sensible, that a constant dread of making water is always uppermost; and then it is that drops of blood are seen to follow the urine, and that they are also seen mixed with the discharge upon the linen. Besides the pain and bleeding attendant upon this state, the sphincter vesicæ is so swollen, that it cannot, when the effort is made for closing it after urining, fall completely into

into those regular folds which it does in a state of health; and from this cause some drops of water are found to issue for some time after the intended action of urining ceases, and some drops also precede that intended action, from the difficulty being as great in its dilating as it is in its contracting. It is owing to the sphincter not being completely closed, that a little of the urine constantly finds its way out of the bladder, and keeps up a successive solicitation to be discharged.

I have before remarked, that the worst stage of a Gonorrhœa consists in the inflammation of the membrane of the Urethra, of the glands within it, and of the neck of the bladder being so very powerful as to produce a cessation of the discharge. When the secretions are, from this cause, at an end, there is in general, in addition to great heat along the membrane—to the tumid appearance of the visible part of the Penis—to the constant propensity of making water—a dull continual uneasy sensation, seated near to, and affecting the anus, which I suspect to be inflammation of the prostrate gland. The patient at this time cannot bear to sit down, nor scarcely turn in his bed; and when he moves, he walks with his legs straddled. Throughout the whole symptoms of the Venereal Disease, I do not know a more critical situation than the patient is in, thus circumstanced; for whatever lasting injury is done to those parts, either to the neck of the bladder, or to the prostrate gland, it is often entailing

tailing continual disease, which baffles our power totally to remove for ever after.

There is another symptom attendant upon a suppression of the discharge, and which is also a symptom during the inflammatory condition of the parts, when the discharge continues, namely, a swelling of the inguinal glands. The swelling of the inguinal glands has been said by some, to be symptomatic, by others, sympathetic, (a term become now very familiar to the ear in Surgery, more so than it ever will or can be to the understanding) and by others, to be the effect of absorption of the venereal virus. It is observed, when the glands become tender and swell, during a continuance of the discharge, that they generally abate of both, as the inflammatory symptoms in the Urethra decline, and as the discharge assumes a more kindly condition—I say, this is the case, in general: and also, when these glands are found to become tender and swell, whilst there is a total suppression of the first discharge from the Urethra, they have, upon a return of the second discharge, and as it becomes more kindly in condition, been found, in general, to abate of both these symptoms. But in both these instances, exceptions do happen to the contrary. The glands will sometimes come to abscess, even whilst the discharge from the Urethra continues; and they will oftener come to abscess when the discharge does not return any more. Those who with me suspect that Buboes thus forming, and thus

coming to abscess, are venereal, will doubtless treat them after a serious manner, and consider them as the seat of infection, advanced at least a stage beyond the original point where the first local infection took its date, and treat them accordingly. Whilst those who consider them as a symptom which is the effect of corresponding affection or irritation, or sympathy, will follow up their theoretic opinions in practice, and treat them as common simple glandular abscesses, as innocent tumours, neither requiring mercury, because they are not venereal, nor any other species of alterative, because they were not produced from Scrophula, Scurvy, or any other obstinate, indurating, chronic cause, but merely from the effect of sympathy in parts.

There are two other symptoms arising from Gonorrhœa, which a late Author has said nothing about. A Phymosis and Paraphymosis, which are excited from a Chancre on the Prepuce, and where a Chancre was the first cause, he has treated with the usual perspicuity that he has treated every other subject, but not that Phymosis and Paraphymosis which arise from Gonorrhœa. Where a Phymosis arises from Gonorrhœa, it begins without any apparent provoking first cause but Gonorrhœa, no Chancre is ever to be seen, but in all other respects the symptoms of it are equally aggravated as when a Chancre was the first cause.

A Phymosis is in part a soft œdematous intumescence of the

the duplicature of the Prepuce, which is supplied by exudation of lymph on the parts, and lodged there in too redundant a proportion. In consequence of this intumescence, the Prepuce is so thickened that it cannot be slipped back, hangs in a considerable portion before the glans Penis, and prevents it from being denudated or seen.

A Paraphymosis is in consequence of the like intumescence, and from the same cause. It occurs where the Prepuce is naturally very short, and where it has been slipt back whilst the Prepuce was commencing to swell, and before the swelling had increased to any great degree. The swelling still continuing to increase the difficulty, a return of the Prepuce over the glans is found afterwards to be impracticable. If the Prepuce be suffered to remain in this state, it will produce so much stricture on the substance of the Penis, as to cause it to slough entirely away.

Thus a Phymosis and Paraphymosis are the same complaint, in different positions. I said that the tumid feeling of the Phymosis was partly œdematous only, for there are parts of it which feel much harder than others, which I conceive to be the swelling of lymphatic glands; and it oftener arises from Phymosis or Paraphymosis, than from any other cause, that we trace hard and knotty chords along the dorsum Penis. These will remain for some time after the Phymosis has subsided, and will sometimes terminate in

small abscesses. These hard chords are lymphatic glands, either irritated from corresponding inflammation, or from the infectious lymph passing through them. These hard chords are not confined to Phymosis or Paraphymosis only, but are also, in consequence of exudation of lymph around the glans Penis, and of Chancre.

A young gentleman, about two years ago, applied to me, for Gonorrhœa: the symptoms of it were of the moderate degree; the prepuce of his Penis was long. It was in the Summer time, and he went, about a fortnight after his first application to me, upon the water, when he was caught in a shower of rain, and the next day he found himself feverish, and chilly: at that period his discharge disappeared, and his prepuce began to swell. It continued to swell as much or more than in any Phymosis I had ever seen. I could feel, amidst the œdematous swelling, on two or three points, hard tumours, and those I suspected would become Chancres, for I did not then reason from my newly discovered theory. But although he was confined to his house for near a fortnight, and although he had a second return of the Phymosis from indiscretion, yet there was no appearance of an ulcer from the beginning to the end of his cure. In this case there were three large hard chords along the dorsum Penis, two of them had at first made up one tumour; but as the tumour subsided, the two knotty glandular substances could be with ease distinguished.

The

The cause of this Phymosis was as wide from that brought on through Chancre, as it is possible for two causes to be. This was positively an inflammation, extended to this part, in common with that inflammation in the Urethra, which gave cause, for a time, to the total suppression of discharge from the membrane within the Urethra.

LECTURE VI.

ON THE

SWELLING OF THE TESTICLE.

DURING the progress of a Gonorrhœa, a swelling of the testicle sometimes comes on as a symptom, which in itself is of a very troublesome nature. It is scarcely ever attended with the loss of the testicle, nor does it often leave behind it any hardness which does not subside, or which prevents a restoration of those functions it is the property of the testicle to perform. That Gonorrhœa is the cause of the swelling of the testicle, is a self-evident fact; but how the effect is brought about, is a question still agitated, and perhaps, as every reason which has been hitherto assigned fails in the explanation of satisfying doubt, it might be suspected that the true reason is not as yet satisfactorily laid down, in accounting for this important event.

I shall first of all endeavour to describe at what period of time, and in what condition of a Gonorrhœa, a swelling of the

the testicle generally comes on. It may be remarked, that I constantly cover every assertion which I advance, by saying that it generally happens, or that it is a general symptom. I certainly do so, and I justify myself in it, because from things as they generally occur, we are to form our conclusions, if we mean to come at the truth. Nothing leads men more into error, and especially and constantly into medical error, than drawing conclusions for a general scale of reasoning out of rare cases, and rare circumstances accompanying rare cases. It becomes now the fashion, and I will venture to say, that the only road to error is to lose sight of common occurrences, and to draw conclusions from uncommon cases.

A swelling of the testicle generally comes on at the latter stage of a Gonorrhœa, after the inflammation has marked its progress through the surface of the whole of the Urethra: it generally also comes on at a time when the patient thinks himself getting well apace, when inflammation has subsided, where it had been most felt, and when the discharge has lessened also. This phænomenon in the animal œconomy has excited various opinions, for developing the cause which produces it; and yet I do not know if that the true cause of the attack of the testicle, in this instance, were exactly defined, whether it could be prevented oftener from coming on, or whether it could be cured by a more simple and compendious manner, than it now is, when properly treated.

As

At the period when pain is first felt in the testicle, and when the epidydimis begins to swell, the discharge from the Urethra generally ceases. If it cease wholly, the inflammation in the testicle goes on; but if at that period when the testicle feels tender to the touch, the discharge has but partially ceased, there then is a probability, by the omission of injections, if they were in use, by suspending the testicle, by horizontal position, by opening the pores, and by moderating the diet, that the discharge may return, and the tenderness of the testicle abate. But when the contrary is the case, the progress of inflammation is as follows. A pain is felt in the epidydimis, which is exquisitely sensible to the touch; it is swelled and hard; the body of the testicle does not for a few days appear in some to be affected, in others it does immediately after the epidydimis: but the most exquisite sensations of pain are felt when the testicle itself swells, and this pain I am certain is owing not so much to the swelling of the substance of the testicle, as it is to the distention of the coat of the testicle. The scrotum sometimes will also partake of the inflammation, and appear red and thickened.

After the testicle has experienced the effects of swelling and inflammation, the spermatic chord swells, and is also exquisitely sensible of pain, the pain is traced all along the direction of it, and the loins and kidnies become in their turn also sensibly affected. The pain in the back often ex-

cites the necessity of shifting positions in vain for ease, a general symptomatic fever prevails, the pulse is quickened, and sometimes the patient will be delirious; in short, the symptoms in some irritable habits do become so very alarming, that if we were not able to assign a cause for them, the fever would assume every appearance, and be judged to be of a dangerous nature. But as it is the case, in all fevers excited from an irritation on a specific part of the body, the pulse, although extremely quick, (for I have felt it beat to one hundred and twenty strokes in a minute) will be irregularly so.

The most disagreeable symptom of all in a swelled testicle is a sickness of the stomach, with vomiting. This action of vomiting, when the fever runs high, becomes additionally aggravating by the pain in the loins, and by the increased pain of the testicle. It very rarely happens that the testicle comes to abscess, but it sometimes does, scarcely ever, I believe, but from neglect or wrong treatment. If any thing independent of constitutional disposition can bring on a swelled testicle, I think riding on horseback, excessive walking, stimulating purges, sharp injections thrown up in the height of inflammation, and obstruction of the natural perspiration will effect it. But nocturnal emissions, or any act which produces seminal discharge, during the inflammatory stage of a Gonorrhœa, will, above all other contributory

butory causes, tend to bring forward a swelling of the testicle.

Here we see a certain train of inflammatory and consequent symptoms following each other, from the first irritation which is in the testicle, to a more general irritation of the whole of the body, corresponding with, and existing so long as the pain and inflammation of the swelled testicle continue, and no longer—and this is the true idea that I have of a symptomatic or corresponding irritation or affection. But we do not ever find that this inflammation exists, and is continued in the Urethra at the same time that the testicle swells; but on the contrary, the inflammation, pain and discharge in the Urethra all cease, and the Urethra appears to be free of every degree of irritation at the very time when the swelled testicle is at its highest and most exasperated state of pain. And moreover than this, when the inflammation of the testicle abates, and the swelling subsides, when the spermatic chord is no longer affected, when the pain of the loins is no longer felt, when the pulse is calm and the stomach at ease, although the swelling of the testicle be not gone down, it is then we find that the discharge from the Urethra returns, attended sometimes with as many symptoms of inflammation, as were attendant upon the discharge before the testicle had commenced to swell. If this description be the true and general description of the condition of those parts whenever the testicle does swell, I

cannot think such effects are to be attributed to sympathetic affection, or to what is termed sympathy; for the fact seems to be so opposite to such a doctrine, that it may rather be termed antipathy than sympathy. When I consider the state of the Urethra, with the state of the testicle, when the testicle first inflames; or, when I consider the state of the testicle, when the Urethra inflames a second time, after the discharge again returns, so little does the condition of the one correspond with the condition of the other, that they never appear to suffer together, but to suffer alternately. The one part seems to be eased by the pain of the other: when the Urethra is in pain, the testicle is at ease; when the testicle is in pain, the Urethra is at ease. They are the Castor and Pollux of each other. I say this is not the general effect of corresponding irritation, nor of sympathy. This is not similar to the effects of swelled testicle on the parts which I have lately enumerated, nor similar to the effects on the spermatic chord, on the loins, on the stomach, or on the pulse. These were not the most affected when the testicle was the least, but were most affected when the testicle was so, and particularly and only at that very point of time.

This is my idea of corresponding irritation, and this must be the only true idea of sympathetic affection, whenever those who chuse to adopt that term in reasoning upon this subject, or any other similar to it, can adopt it with truth. I therefore give it as my opinion, for the above reasons, that when

when the testicle swells from a Gonorrhœa, it is not owing to a sympathy with the Urethra. If it were owing to sympathy in parts, whenever it came on, it would be always when the Urethra was most inflamed, when the scalding of the urine was most acute, when the Chordée and erections were most troublesome; and the swelling of the testicle would abate, as these symptoms abated:——but unfortunately for the Author's opinion, in this instance, who first avowed this doctrine, the testicle never scarcely is known to swell whilst the Urethra is found in this condition.

The English Authors highest in fame, for purity of diction, as Locke, South, and Johnson, are known to define sympathy—to be an affection of one person for the condition of another. Now if in addition to this power of sympathy, we apply the term to the affection of one part for the condition of another part in the same person, and vainly annex to sympathy, in this instance, not a mental idea, but a real corporeal action, that part which sympathises, must necessarily be suffering for the condition of the other, at the time it suffers. For it cannot be said to be sympathy, when this reciprocity ceases, provided the mutual suffering even began in sympathy. I can only say thus much with truth, that the pain of the testicle most effectually goes off, that the swelling of the testicle most rapidly declines, when the second return of the discharge from the Urethra is the most profuse. It is not material to the progress of the

reduction

reduction of the testicle, whether there be a scalding of the urine, or any other inflammatory symptom attendant upon the second return of the discharge; but it is solely dependant upon the plenteous return of discharge, either with or without (indifferently which) inflammatory symptoms, that the success of the reduction of the testicle depends; for when the discharge ceases before the swelled testicle has subsided, the remaining swelling is generally a long time before it goes down, and sometimes it never does. We ought to attend to this circumstance, and to do nothing to stop a discharge, whilst a swelled testicle is subsiding. And it is in such instances that ingenious men, knowing this to be the fact, have devised stratagems to bring on a discharge.

A certain modern Writer on this disease has also asserted, that he has known the testicle to swell, when the inflammation in the Urethra has never extended above an inch or an inch and a half up the Urethra. I confess I have known no such instance; but if I had, I should draw no conclusions from it which would affect more constant and more general observations to the contrary. Testicles, he says, are known to swell from other causes, besides that from Gonorrhœa. Nothing, in my opinion, from this circumstance, can lead to inform us what it is which produces the swelling in this case. When the testicle swells from the use of bougie, and the circumstance is very singular, and worthy our notice, it is then generally one only that is affected. In case

of

of a scirrhus, it is also in general one only that is affected. And in the case of swelling from Gonorrhœa, we find that although sometimes the inflammation does shift from the one to the other, yet if the attack be serious, and if the inflammation proceed the whole of its extent, there is seldom an instance when both testicles are highly inflamed at the same time. We find also when the testicle swells from any cause, unattended with a discharge from the Urethra either before or after, that the hardness continues sometimes for ever after all inflammatory symptoms are gone.

Such being the natural occurrences which offer to our observations upon the swelling of the testicle, it appears that Nature has been very provident with respect to this double security for propagation. And although it seems almost unreasonable to suppose that the orifice of one vas deferens can be affected from a venereal stimulus, and by that means that the testicle to which it leads can be, in consequence, affected, whilst in the very midst of inflammatory stimulus, the other vas deferens and the other testicle shall escape the effects of venereal stimulus—I say, although this appears strange, yet we know it to be a fact; that is, we know that one vas deferens will feel tender and affected, and that one testicle will swell, without the other being in the least affected at the same time. Such is the fact, let it be ever so difficult to be reconciled to our understanding: and I should suspect that although the orifices of the vasadeferentia may

be

be a bar to the actual progress of the virus in general, and although from an extraordinary instance, that bar, in the provision of nature, might prove to be inefficient, and a swelling of the testicle, from that circumstance, does ensue, yet if sympathy had any concern in the condition, both must swell when one only does.

Besides, when the use of bougies brings on a swelling, seldom more than one testicle swells. This seems as if it were an original design in the disposition of the parts, from the very nature of them, that both should not be seriously affected from the same cause, at the same time; or how otherwise can we account for the attack upon one, and the escape of the other? The swelling of the testicle cannot, in my opinion, be affected from a bare stimulus of the venereal virus acting on the orifices, and no farther, of the vasa deferentia; for if that were the case, a long-protracted Gonorrhœa, and such as had excited the full effects of inflammation on the neck of the bladder, on the prostrate gland, and on all the parts deep-seated in the Urethra, would surely be a sufficient cause for provoking both testicles to swell, whenever such inflammation occurred: but this we know is not that which usually happens, for I believe I may venture to assert, that a swelled testicle does not occur to one in twenty of those thus conditioned.

It evidently appears, from these observations, that there

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is a difficulty in the effects of a swelling of the testicle being brought about. I am, therefore, of opinion, that the orifices of the vasa deferentia, which open into the Urethra, are shut against the effects of all stimuli; and that it is from accident alone venereal stimulus can possibly be admitted. But if it once gain admittance, I am of opinion that the venereal stimulus can act as well along the vas deferens, as it does along the Urethra; and that it is the positive presence of the venereal stimulus which produces an inflammation on the internal canal of the vas deferens, and which proceeds to the epidydimis, and affects that and the testicle. If virus can pass through lymphaticks, by what is termed absorption, there is no difficulty to presume that it may pass along the vas deferens, by capillary attraction. And I am also of opinion, that part of the discharge which follows a swelled testicle, and to which a swelled testicle from a venereal cause owes its restoration to a sound state, flows through the vas deferens, and that it is poured from thence into the Urethra. It does not follow, from this mode of reasoning, that this discharge should carry with it any different mark from that which flows from the Urethra; for an increased stimulus upon the internal membrane of smaller tubes will produce an increased discharge from them, alike as it does from the Urethra. The more the discharge which follows the swelled testicle, the sooner will the testicle go down: and this is a reason why a swelling of the testicle, from a venereal cause, in general subsides

sooner and safer than a swelling brought on from any other cause, and where no discharge follows. I am also of opinion, that when the venereal stimulus gains admittance within the orifice of the vas deferens, the progress of the inflammatory symptoms are as slow there, as they are through the Urethra after gonorrhœal infection has been first received.

From the above statement of the cause of a swelled testicle, I think we have been able rationally to account for the various circumstances attendant on the swelling of the testicle brought on by Gonorrhœa—how it happens that sometimes, notwithstanding long and great inflammation in the Urethra, the testicle is not affected at all—how it is from accident when it is affected—how one may be affected without the other—how that affection may come on after inflammation in the Urethra has subsided—and how its own cure consists in the return of the discharge.

Having taken notice of most of the important symptoms attendant on a Gonorrhœa in man, I shall proceed to a few observations on a Gonorrhœa in woman.

GONORRHŒA IN WOMAN.

GONORRHOEA in woman is not frequently attended with symptoms so complicated or so violent as those in man. The cause is assignable only to the different construction of the parts which are exposed to an attack of this disease. But although the symptoms are neither so complicated, nor so lasting, nor do they bring on other after consequences, which are in themselves more alarming in their nature than the original cause which produced them, yet sometimes enough of inflammation and enough of pain are found to accompany this diseasse when it first appears in the Vagina of woman.

It need not be observed, that as it is the most usual manner for man to receive the infection on his Penis from the Vagina of a woman, so does the Vagina of a woman receive it from the Penis of a man. In some instances, a simple Gonorrhœa may be confined to the Vagina, hardly any other part will be susceptible of the stimulus, and a woman will have a Gonorrhœa without knowing that she has one. This simple mild appearance of the disease is generally experienced by women in the common habit of venereal intercourse. But women of a contrary way of life are more sensibly irritated by venereal virus, although with them,

according to their constitutions, there will be a sensible difference in the affections produced by inflammation. The most general symptoms of a Gonorrhœa are an inflammation along the Vagina, with swellings of the labia, clitoris, nymphæ, carunculæ myrtiformes, excoriations on the perinœum and on the meatus urinarius. These are attended with pain, itching, scalding of urine, immoderate discharge, fever, uneasiness in sitting and in walking: the labia are enlarged, become rigid and protrude. Such are the symptoms which appear in the first instance, and which seem specifically to embrace all the effects of venereal stimulus at one and the same time, and are, excepting the general fever which they produce, independent of any corresponding irritation arising out of one of them simply for the condition of the other. And here also it may be remarked, that let those symptoms grow to any height, they do not produce chancres on these parts, nor on any other contiguous parts to which the gonorrhæal fluid might have easy access, although the discharge will often excoriate.

Sometimes the inflammatory symptoms are not confined to these parts alone, but are found to extend to all the region of the lower part of the abdomen, producing pain, and so strong a sense of tenderness, that the patient cannot bear the least weight of cloathing, also violent intestine commotion, flatulencies and vomitings, attended with a symptomatic fever. It has been presumed that these effects are owing to the uterus being affected, as well as the ovaria.—

It

It may be possible that both may be affected from the disease: But where I have seen those symptoms, I was inclined to attribute them to an inflammation on the bladder, for the chief seat of pain was directly described to be above the pubis.

Where the inflammation has gone on for a considerable time, the glands withinside of the labia and other parts will remain enlarged and hard, and sometimes never return to their healthful size, and sometimes they will come to abscess. It also happens in a woman as it does in a man, that either from irritation or absorption, the glands of the groin will swell and proceed to abscess.

These are the chief effects of a Gonorrhœa in woman; and as the inflammation on these parts is to be accounted for upon the same ground of reasoning as it has been in my former Lecture, and as the same method of treatment is requisite for both, a further enquiry into the subject of what has been already described will perhaps be unnecessary.

The most important topic for medical enquiry into these affairs of women, does not consist in a clear, avowed knowledge of gonorrhœal symptoms; for these being once confessed, the case is plain, and the path for removing them will be as plain also. It is not in this instance that address and penetration are so requisite, as it is—where the case is doubtful—where the possibility of giving infection is denied—

denied—where a medical man is called upon to decide upon the condition of a woman, whether from that condition she be in a state for conveying infection or not—where he may have, from his discernment, her arts to baffle, or her innocence to protect—or where her ruin or her good fortune is at stake upon his judgment. This is a confidence, a part of medical jurisprudence which carries with it importance, and a man of sense will feel it in a high degree; he will aim to be furnished with every distinction which may possibly lead him to a right determination, and which may teach him to discern between the tricks of art and the distress of innocence. The only proof I know of a woman being infected, when all inflammatory symptoms have disappeared, is, that she retains the power of infecting; for a discharge from a Fluor Albus, and a gonorrhœal discharge, when free from all inflammation, will not afford a distinction for me to conclude them to be different. Perhaps those who know the one to be pus, and the other to be mucus, may be able to discern a difference.

It has been said that a woman will go on with a Gonorrhœa for a long time—that the power of infection will be demonstrable to a stranger, a long while after it ceases to affect her more constant friend. This I believe to be true; and in this instance the woman cannot be blamed, although she can and does infect a stranger. A woman might also have been free from all infection, and have known a man who

who was infected; she might have received an infection from this man; immediately afterwards she might have known another man who was not infected; she might have imparted the infection which she received from the first to the second, without being conscious of having done so: for the gonorrhœal fluid which had been lodged with her had not time to produce any stimulus, and it might be shifted altogether to him who had the second connection. In this instance, the woman cannot possibly be blamed, as not knowing herself infected. But on the other hand, a woman may know that she can infect, because, although her inflammatory symptoms are just gone away, she has still a considerable quantity of discharge. It is in this situation that she will often keep back the truth, avow her innocence, and set detection at defiance.

ON A

GONORRHŒA OF THE EYE.

THERE are two distinct conditions of venereal affections of the eye, the one arising out of constitutional infection, and the other from a local infection. It is the Gonorrhœa of the Eye, or the local venereal infection, which I am now about to treat upon. The Gonorrhœa of the eye is one of the most acute, inflammatory, painful, and destructive attacks which can be endured: in comparing it with the whole class of acute diseases that locally affect human parts, I am not able to recollect another equal to it, in rapidity, torture, or destruction. Two cases of this unhappy attack have come before me: in the first the eye was lost, and in the second, from timely treatment, and from being aware of the nature of the complaint, the eye was with difficulty saved. The virulence of the attack of the latter was nothing like so formidable as the former.

This attack of the eye has been attributed to a metastasis, from a repelled recent Gonorrhœa; and it has been described as if the Gonorrhœa from the Penis ceased, when the Gonorrhœa from the eye came on. That when the pain in the eye becomes intolerant, the pain in making water, and other inflammatory symptoms of the Urethra will be sensibly

sibly diminished, it is consonant to general observation in other similar instances, which present themselves in the animal œconomy. In these two cases the discharge from the Penis was not discontinued, but the inflammatory symptoms were not high. I do not believe this doctrine of metastasis. I believe the venereal virus was conveyed by the fingers; for both my patients were previously subject to weak eyes; and the former, he who lost his eye, was in the habit of washing it with his urine, to cure it of the weakness.

I am now to point out to you, how it happens, that out of so many which we constantly see infected with Gonorrhœa, who are totally indifferent and thoughtless about any consequences, and who therefore do not make use of any precautions, none infect themselves through wounds and sores, and chops in their lips; and that this complaint of the eye is so seldom experienced. The fact is certain, that the instances of Gonorrhœa of the Eye are very rare indeed, and that the instances of wounds, or sores, or chops on any parts of the body, being infected, notwithstanding they are exposed to such infection, if it could act, are rarer still. We all know that not only we have the habit, but that it is a natural addiction in us to be constantly fingering the parts which are diseased. And yet I cannot help remarking, for the observation intrudes itself upon me, that we are never infected on fresh parts in this manner. Notwithstanding, if the in-

fectious discharge could act, we are more exposed to infect ourselves, than others are to infect us. I must here explain myself, in order that my true meaning might be only impressed. I am assured, in both the cases which I saw, that the fluid from the Penis of the first or second patient did not produce the inflammatory effects on their eyes: for I know, that the gonorrhœal fluid of the same subject is not capable of producing that effect. And therefore this theory accords with the observations I have made before, on the nature of both gonorrhœal and chancrous fluids, as they affect the same subject. For, if a Gonorrhœa from the Penis of the same subject can produce a Gonorrhœa of the Eye, the Gonorrhœa of the Eye can produce a Gonorrhœa of the other eye, and the infection can go on in an endless round of diseased parts. Wherever the discharge gained a lodgment, if it were virulent, there it would commence an action conformable to the part, and the whole of the body would be one scene of Chancre and Gonorrhœa. But this is not the character natural to this or to other poisons. Where a subject has a Chancre or a Gonorrhœa, as a first local symptom, the virus which produced it was received from another, that virus necessarily proceeds in its action if it be not extinguished; and it is that virus, imparted from another, which is absorbed into the habit, and there again it produces all those symptoms which occur in the history of the disease, and to which the subject who imparted it was in like manner liable. Of this theory, however new, as applicable

cable to the present subject, I am more convinced of the truth, than I am of any idea I ever found myself adequate to form. The poison of a viper does not affect the viper which secretes it, but it will affect another viper, or any other animal into which it is suffused. It is an excreted fluid, lodged temporally in an excretory bag, in order to be discharged whenever the viper is provoked to it.

I shall close this subject with an account of the case of a gentleman who lost his eye from a virulent Gonorrhœa. A gentleman was recommended to me for a virulent Gonorrhœa: the first time I saw him he complained also of one of his eyes being more inflamed and more painful than it ever had been, and this was the next day after the Gonorrhœa had discovered itself. I bled him for this inflammation, and gave him the necessary remedies for the Gonorrhœa Urethræ.—I saw him the next day, when I found his eye more inflamed, and the inflammation of the Urethra lessened. I bled him again, fomented and poulticed his eye. The next day the pain from the eye was increased, he complained of a violent throbbing, the discharge from it was in increased quantity, and looked purulent, the tunica conjunctiva was in a higher state of inflammation than I ever had seen one before: I applied leeches to his temples, blisters behind his ears, injected milk and water, renewed poultices, &c. The inflammatory symptoms in the Urethra at this time were of no importance, and the discharge from it was kindly. The

morning following I found the pain of the eye had not in the least abated, the conjunctiva assumed one continued fleshy appearance, which protruded so much as to leave the eye-lid more than half open. The same remedies were repeated, with the addition of merc. calcin. and opium. The next day, the pain and discharge were yet more increased. The inflammation not yielding to any of these revulsive means, I repeated all the remedies which I had applied the day before. The next day, the symptoms continuing without the least abatement, I solicited a consultation; but the Surgeon did not meet me till the day after, when every appearance was yet more aggravated. We proposed touching the carnous fungus of the conjunctiva with fine powder of mercur. calcinat. mixed with soft pomatum, by a camel brush. This increased the pain so much, that we directly desisted, and returned to our former method a day or two longer, when the pain gradually abated, and the carnous appearance began to decline. We then gave him bark, and continued the merc. calcin. which succeeded very well in taking off the inflammation, but the issue was, that the cornea was thickened, the crystalline became opaque, and the patient lost all vision. The Gonorrhœa from the Urethra by this time got also well. At the time when this case happened, I found no assistance from any Author how to treat it, or how it had been described by others, excepting from Astruc, and he advises the tunica conjunctiva to be freed from

from this fleshy fungus by the knife.—If I were to treat this case again, I should certainly make a puncture through the conjunctiva, in order to give an opening for the discharge of extravasated lymph. But how this fungus is to be dissected away, during the torture and pain which the patient undergoes, I know not. It seems to me to be impracticable.

It appears in this case, that the symptoms of the Gonorrhœa of the Eye were as forward or early as the symptoms of the Gonorrhœa of the Urethra. It must be also remarked, that though both eyes of this patient had been previously weak, yet only one of them was thus affected. The patient had been in the constant habit of washing his eyes with his own urine, for this weakness, and he had washed his eyes half an hour after he had had connection with the woman who gave him the Gonorrhœa. My opinion is, that it was the positive fluid whichhe received from the woman, that infected his eye, and that the other escaped, because it was the last which he washed, and because none of the original infection came into contact with it.

ON

IMPOTENCE.

I SHALL, before I dismiss you from this Lecture, beg leave to say a few words upon the subject of Impotence. And perhaps this may be brought forward with a better grace in a lecture-room, before an audience who are assembled to reflect upon the cause and cure of diseases, which they are to attend to and treat in others, during their professional practice, than in a publication, that may be open to curiosity and ignorance, as well as to discretion and judgment.

Impotence from an excess of venery, and impotence from a debility in the muscular power, are two separate questions. And so far as there is an impotence in the muscular force, brought on in consequence of long inflammation from a Gonorrhæa, or in consequence and in union with a debility of the muscular force of the bladder, the accelaratores urinæ, and other muscular powers which are applied to expel the urine and the semen also, so far does a consideration of this subject of impotence come in course fairly befors us, without going out of our line to drag it into debate. It is therefore impotence from debility, and from a temporary paralysis of that muscular force necessary to eject the semen

semen through the Urethra, which is the immediate object of our present consideration; and for the cure of which I know nothing preferable to sea-bathing, moderate living, and temperate indulgence of the venereal passion. The process of secreting semen, and that of secreting urine, will both go on; but the discharge of both are not so absolutely necessary—the urine must be discharged, the semen may be retained; and therefore, during this debility, it should not be provoked by any venereal excitements. This is all that will be found necessary for me to say upon impotence, as it is brought on from a debility of the muscular power.

But as a late Writer has indulged himself with a reverie, and has not hesitated to declare in an open publication, his opinion, that excess of venery, from self-pollution, is not attended with any prejudice to the constitutional vigour, and to the venereal power of natural gratification, I hope that if I detain you a few seconds, in discussing this point, it will not be altogether considered as unprofitable, although it might otherwise be deemed irrelevant to our present subject. If it be considered as relevant at all to the present subject, that consideration must refer to an answer which I intend to the arguments of Mr. Hunter, who has introduced the subject in his Treatise upon the Venereal Disease, and who has there set up an opinion so singular, so different from any Author that has gone before him, and so fraught with

with mistaken theory, that no opportunity which offers should be missed, in order to express dissatisfaction with it.

With respect to the morality or immorality of self-pollution, that is not the question before us—although I am convinced, that as much as it is repugnant to the evident path of Nature and reason, it will have morality opposed to the mischievous effects arising out of it: for the mind will be debilitated alike with the body, it will be exposed to vicious assaults, from want of fortitude to stand up against them; and where our actions are at variance with reason, I think the morality of them will and must be ever disputed. To say that excess of self-pollution does not debilitate both the corporeal powers and mental faculties, is to deny a self-evident fact. For it is apparent to every inquirer, both from the confessions of those who have, from want of resolution, yielded to this selfish gratification, as well as from the change that is observed in the condition of their health. There never was, I may further remark, an Author who ever dwelt upon this subject, but held out the strongest warnings against the habit of it, and pointed the evil tendency of it, as productive of the most ruinous consequences to the constitution in general, to the venereal powers in particular, and to the mental endowments. Only let the symptoms arising from self-pollution long indulged, be referred to, and let the indifference for that natural propensity which youth is prompted to gratify with woman by an irresistible impulse,

when

The latent scintillæ may, by these means, be re-kindled, and emit a pleasing warmth to cherish life. But the bold fiery qualities of youthful vigour, being already consumed, the blaze of life will never more shine out strong and splendid.

LECTURE VII.

CURE OF GONORRHOEA, AND ITS CONCOMITANT SYMPTOMS.

THE cure of a Gonorrhœa must naturally include a consideration of the nature or quality of the part attacked, and a consideration of the nature or quality of that which attacks it, and produces an inflammation upon it. Any common stimulus, which is not of a virulent nature, applied to a mucous membrane, will produce some degree of irritation; but such irritation will cease by the stimulus being carried off in the secretion. The ardor urinæ excited by the action of cantharides can only last for a short space of time; because, from the moment the sensation is felt, the secretions are diluting the stimulus, and carrying it out of the constitution, and when the action is totally spent, the sensation from it is discontinued also.

Mucous membranes are placed as coverings to parts which are inlets and outlets of the constitution, and in their construction are so framed as to express a sensation whenever any

any extraneous injury is offered to them, or whenever any extraneous body is lodged upon them. Effluvia which is inimical to the welfare of the body, passes over the nasal membrane, not without notice of disgust, by the smell which is impressed upon it; and irritating properties lodged upon it, are gotten rid of, or an endeavour is made to get rid of them, by the additional secretion which is excited. Mucous membranes seem to be, by destination in the animal œconomy, centinels to the rest of the body, and endowed with power to alarm the constitution whenever any thing obnoxious is liable to assail it.

It has been said that a Gonorrhœa will cure itself, and it has been also said, that a Gonorrhœa cannot be the cause of the Venereal Disease in the constitution. Both these assertions, if they were founded in truth, would be very fortunate ones indeed, and if they were founded in truth, there would not be so strong a reflection upon the morality or importance of a profession in all the round of human occupations. That a Gonorrhœa may cease to be a Gonorrhœa, if let alone to its own action, it may be very true, but it may also be as true, that it might not cease to be Gonorrhœa, until it had reduced the organization within the Urethra to a condition which could not afterwards be restored to a sound state. The Urethra being endowed with the faculty common to every other mucous membrane, of getting rid of that which irritates, and of being a barrier against whatever

whatever might, if it got into the constitution, annoy it, does certainly contend very strongly with that which irritates, and as often gets rid of the irritating cause; but in the instance of the stimulus excited upon it by venereal virus, it appears from evident facts before us, that the difficulty is greater than from any other cause whatever; for what other stimulus, applied to a mucous membrane, is there, which will continue so long, and which will baffle all medical means so effectually, as the stimulus which is excited by venereal virus? What other stimulating property is there within our knowledge, that by once applying it to the Urethra, will afterwards support a stimulus in the Urethra for months and years, which I have known to happen in the instance of Gonorrhœa? If we refer to the very action of a sharp injection, which, from the sensibility of the Urethra whilst in an inflammatory state, has sometimes proved so very painful as to excite for a time more inflammation than had before been experienced from the venereal stimulus, we shall find that such increased inflammation, produced by the injection, will not be able to support itself long; for unless the injection be repeated, the effect first produced by it will subside in a very short time, but the original inflammation excited by venereal virus will still continue, for having once commenced, it retains a power of long supporting its own irritation—much longer than any other stimulus can or does. And further, I shall remark, that in a recent case, if a sharp injection be repeated for some

some time, and if at the intervals during the use of it, the inflammation appear to have subsided, if the discharge be thickened, and if it approach nearer to mucus in a healthful state, yet if the use of the injection be withdrawn before the discharge has totally ceased, and that for some days, the venereal stimulus will again recover its force, and will again produce all the inflammatory effects which it had produced before the injection had ever been thrown up. For if the venereal irritation be not compleatly extinguished, it retains a power of collecting again in force.

I think I have truly asserted, that the stimulus excited by venereal virus has a power of supporting itself beyond any other stimulus that can, from one application alone to the Urethra, be brought into competition with it. I shall endeavour also to impress upon your recollection, that the discharge from the Urethra is not the effect of any ulcer, that it is not produced from any other change in the condition of the Urethra but inflammation on the membrane, and of the glands which are distributed throughout various parts of the membrane. Such being the plain matter of fact, if it were possible to a certainty at all times, by any remedy, to subdue this inflammation, the disease would terminate like any other inflammation; and the analogy which has been drawn by some, betwixt the inflammation excited by venereal virus, and any other inflammation which was not produced by virus, would be successfully confirmed. But this is not the fact.

fact. An inflammation excited by one act, and by a cause which was not venereal virus, unless it be constantly supported by additional acts, will readily yield to proper remedies applied for the removal of it.

I have said that, as mercury is a specifick in the Venereal Disease, in all other modifications of it, why is not mercury a specifick in Gonorrhœa? I shall take pains to be as explicit upon this question as possible. It does not follow in reason, because mercury destroys the action of the Venereal Disease on the constitution in general, that it should be a specifick against the local action of venereal virus, when topically applied. As well might it be expected, because the pulvis antimonialis possesses the property of abating a fever, that by laying it on an inflamed part, it should possess the property of abating that also. Mercury possesses no chymical power of neutralising venereal virus, if mercury be mixed with venereal virus abstracted from the constitution. The power which mercury possesses, is that of producing a change in the constitution, by which the venereal stimulus is extinguished. One poison prevails over the other, by superior predominant activity. But in the instance of Gonorrhœa, no such revolution takes place; the venereal virus is only acting upon a surface, by so far stimulating it as to increase its general secretion. The venereal virus is not absorbed into the constitution, and whilst the discharge continues, I believe, the absorption of it seldom takes place; for as the

whole of the membrane might be considered to possess a secreting power, it is natural to conclude that the absorbents do not much prevail throughout it. That mercury is not a specifick against venereal virus, merely as venereal virus, is evident from other instances besides Gonorrhœa; for if a Chancre be dressed with mercurial ointment, spread on lint, and laid upon it, and if there be not enough of the mercury absorbed, so as to act effectually on the Chancre from within the constitution, that will not cure the Chancre. A Chancre which is disposed to heal by this application, will heal also by dry lint. But mercury cures the Chancre by its internal action, which destroys the venereal action. Mercury, from its action, brings the constitution into such a state, that the virus cannot act any longer, and becomes evanescent.

I shall now proceed to lay down as general rules as I possibly can for the cure of a Gonorrhœa, and assign my reasons for every particular, as near as I can. And first of all, I cannot too strongly enforce an early application of remedy, as soon as the disease is discovered; for experience tells us, it proceeds by degrees through the Urethra; and experience also tells us, that those who wash the parts, and keep them clean after every act of doubtful connection, seldom or never are infected at all; for if the fluid imparted in consequence of connection be washed away, nothing of course will follow. And this will be found as successful from any other

other detergent wash, as from a mercurial one. The best remedy I know for preventing the infection, is the act of urining, and washing the prepuce and glans repeatedly well, and wiping them during that act. But as this is not always at our command, it is necessary that a substitute should be found, and which can be had at will. A solution of the vitriolic fluid, the preparation of which I shall proceed to demonstrate, is the most successful. It should be applied as soon as possible, before the moisture on the Penis is dry: the whole of the Penis should be also wiped dry, for a Chancre has sometimes appeared at the very root of it. It has been said by some that the preventive applications often fail—that they disappoint—that it does not follow a man is safe although there be neither Gonorrhœa or Chancre, for a Bubo is sometimes found to be the first symptom—and that sometimes the Venereal Disease is discovered in the constitution, where there has been previously no local symptom. All this may be and is very true; but it has no weight, in my mind, against the use of preventives, for whether they be used or not, none of those symptoms, happen when they will, can be treated medically before they are discovered, nor can any infection be imparted through them before they have appeared; for there must be fluid, venereal fluid, to infect, and that is the only medium of infection. But the preventive method, I will venture to assert it, might set aside every mode by which infection can take place, which otherwise would. For if the absorbents

take up the venereal fluid, as it is lodged on the Penis, and thereby a bubo or lues is produced, so by washing away that fluid, both or either will be prevented. I am confident neither can be encouraged or promoted by it.

As soon as it appears that, from a suspected connection, a virulent Gonorrhœa is the consequence, there are two methods which present themselves for procuring a rational and more speedy cure, than if it were left to cease spontaneously by the dilution of discharge which is excited by the virus—the one is by assisting that dilution by sedative injections, and the other is by exciting a stimulus which will suspend the former stimulus, and by repeating that excitement until the former stimulus be extinguished; for in all injections, if their intentions succeed, the success must be obtained by a repetition of them. The degree of inflammation excited by the virus must be our guide in the adoption of our system, and if we do gain time by the interference of topical applications, it must be by availing ourselves of the variety of treatment which is indicated from the appearance of symptoms.

The following solution I have found to be very successful, in the first stage of a Gonorrhœa. The proportion must be considered according to the degree of inflammation, which will be expressed by the feeling of the patient. It should be thrown up six or seven times a day—it cannot be used

used too often. If the progress of the venereal inflammation be at a stand, and if the discharge be kindly yielded, there can be no doubt but the Gonorrhœa will soon be at an end, especially if this injection be applied, as I mean it should, early in the attack of the disease.

Dissolve blue vitriol in a sufficient quantity of spring water, precipitate the solution with a sufficient quantity of lixivium tartari, (which may be known by the effervescence ceasing) suffer it to separate, and pour off the clear liquor —then wash the precipitate with warm water, set it by to subside, decant the clear liquor, and repeat the process with fresh quantities of warm water, till it become insipid and tasteless, at least of the salt—then filter the solution, and reserve the precipitate. Dissolve as much sal volat. sal. ammon. in distilled water as it will take up, and filter it. Mix as much of the above precipitate with the filtered solution as it will dissolve, which reserve for use.

This will be found to be a very preferable injection. I do not mean to attribute to it any other effect, than that it possesses a predominant power of extinguishing the venereal stimulus, and can be used where there is a high degree of inflammation

inflammation with more security and success than any thing else.--I mean to offer it as a preparation which acts safe and effectual. I mean to be understood in saying, that if it be applied properly in time, it will prevent the Gonorrhœa from coming on at all; and if it be applied as soon as a Gonorhœa is discovered, it will prevent a progress of every symptom, provided the patient takes care to inject often, and observes the strictest temperance. Five drops of the fluid to an ounce of water is the proportionate medium for injection. As a preventive, it may be made stronger to wash the parts externally, a dram of the solution to half a pint of spring water, is about the proportion. At the same time I recommend a grain of mercurius calcinatus with half a grain of opium to be taken every day during the cure.

There is another stage of the Gonorrhœa, which I think requires another consideration, and for the cure of which other methods are to be taken—I mean where the inflammation proceeds along the Urethra, in consequence of neglect—where, from tardiness in the application of the patient, the stimulus has pervaded the whole of the membrane. When this is the state of the disease, it is not found to be always safe to attempt removing the original stimulating cause by the substitution of another. The plan for abating the inflammation must be soothing. Constant injections of warm milk and water, with the application of the steam of hot water, and a grain of mercurius calcinatus

taken

taken internally with half a grain of opium every night, will not only be necessary to guard the constitution, but will in process of time so far affect the secretion of the mucous membrane, as to render it less exposed to the irritability of the virus. From the little danger there is of the constitution being liable to be infected, where the virus is only acting on the surface of a mucous membrane, and from the length of time it will take for mercury to produce its effect on this part, it cannot be presumed that the end obtained from such a quantity of mercury is to bring on a salivary affection. No such idea is meant to be inculcated. It is meant only to produce such an effect, as the circulation of the mercury within the constitution will produce without going all the length of stimulating the glands of mucous membranes. I have never known an excess of mercury abate an inflammation in the Urethra, and I have always experienced the discharge protracted, and the inflammation supported from a wanton continued use of mercury. I therefore consider mercury to be, in this instance, productive of good, by meliorating the disposition of the mucus, through a change which it procures in the action of the virus, by which the stimulus of it is enfeebled, and at last becomes evanescent.

If in a reasonable time these symptoms do not abate, but on the other hand the ardor urinæ, chordée, increase, and the discharge becomes thinner, and mixed with blood, if a deep-

deep seated pain be felt towards the anus, and the patient cannot sit down without expressing great uneasiness, still the soothing plan ought to be more rigorously pursued. Bleeding and the warm bath, with a few grains of the pulvis antimonialis with an opiate, should be given; and discretion will teach us how these remedies are to be repeated. An opiate clyster will also have its share of good effect.

Sometimes, in consequence of high inflammation, the membrane and glands will be so inflamed, that the discharge altogether will be checked. In this case, the method last laid down exacts our most earnest attention—for this, I look upon it, is the worst condition of a Gonorrhœa. The origin of most of the obstructions in the Urethra is from this inflamed condition of parts. From the thickening of the corpus spongiosum a Chordée will be continued, even after the inflammation has subsided—from the stretch which it is put to in a state of erection, a hœmorrhage often comes on, and this I am never sorry to see, for it generally brings on favourable symptoms, by unloading the parts.

After the inflammation has subsided, by adopting the means already laid down, there will remain enough to be done for the consequences that are left behind it; for when once the whole of the Urethra becomes thus highly inflamed, it will often be a long time before an attempt can be made to put a stop to the disease by injection. And it is for this reason

son that, to treat a Gonorrhœa successfully, I have always recommended to the patient early applications, when injections will be of use, and when the same injections which would then produce a cure, will not be safe to be hazarded till some time after. The termination of the inflammatory state may be known by a return of a kindly discharge, which will be ropy and thick, leaving a daub upon the linen like white paint, and which, gradually becoming less and less, will cease altogether. The parts will again enjoy their natural functions, and then the disease will be at an end.

But as this is not always the case, when the parts have been severely attacked, I shall proceed to consider what is the nature of the symptoms which are left behind, what are the ravages which the virulent stimulus has produced throughout the Urethra, and endeavour to follow up those consequences which do actually happen from this disease, although a Gonorrhœa has been said by some, and although it is believed by others, to cure itself.

On the contrary, so far from this disease curing itself, it appears to me to be one of the most intricate subjects either to be cured or rationally discussed. Glandular parts and secreting surfaces, once deranged, may never be restored—their organization is so delicate and fine, their orifices are so

nice, their tubes and sphincters having once lost their functions, where is the power or art that can restore them?

After the disease has continued beyond the reasonable time it might be expected to cease, after the inflammation has declined, if a discharge remains, it will be necessary to consider whether this discharge be continued by the positive action of venereal virus, or whether the parts have sustained an injury, and whether any particular diseased part, or indurated glands, support it. If the discharge be supported solely by a continuance of the virus, that will be discovered by the freedom of the stream of urine, by no particular part being affected, by the discharge being regularly distilled, and by the sensation which is felt being generally diffused along the Urethra: or it will be discovered, a posteriori, by the discharge kindly declining, through the use of the vitriolic injection, or by the inflammation increasing upon ceasing to inject it. This condition of a Gonorrhœa is very often that which puzzles a Practitioner, and makes him fly to various and contradictory remedies. I shall therefore dwell a little upon it, and describe it as it generally happens: for, independent of all injury which has been done to particular parts, from no other cause than barely from that of the stimulus excited by remaining virus, the discharge will go on for a long time, and more especially so, if there were any neglect or omission, either at the com-

commencement of the disease, or, what is not uncommon, during the period of the cure. I will explain what I mean, by stating a case.

A person applied to me for a Gonorrhœa, as soon as he discovered it. I gave him the injection, which answered very well, and in ten days he crossed the water to Boulogne. He took no injection with him, because he thought himself well. Whilst he was absent, an inflammation came on afresh, and doing nothing to abate it, before he could again reach me, the inflammation had gone to such a length, that all discharge was checked. I treated him after the manner I have already stated for such a condition, and then, by a return to the injection he got quite well. Patients are apt to cease injecting immediately when they find the discharge is at an end; but I always caution them to proceed many days beyond that point. For if the injection which abates the symptoms be followed up, there is no doubt of success; but success cannot be obtained unless it be regularly followed.—This is the state of the case in one instance. The state of the case in another is, where, although an injection does keep the inflammation under, yet whenever it be withdrawn for any length of time, the discharge will become acrid, and the inflammation will increase. I do not, in this case, change the injection, I only increase its force, and repeat it oftener, which I find generally successful; but if it

should not succeed directly, as long as all inflammation is kept down, I am not fearful of consequences; I inculcate patience, and in time the cure takes place. I avoid, as much as possible, the change of remedies in this intricate and obstinate continuation of discharge, whilst I see the ground is safe, though the cure is slow. If I dishearten the patient by persevering in this manner, it would dishearten me, in foregoing my own reason, to oblige his conceit; and I render him more obligation by adhering to this system, than by yielding to any other.

I therefore wish to impress upon your recollection, that the virulent inflammation will repeatedly return, after it has been partially extinguished, and that it is owing to the means, which first brought the cure almost to a conclusion, being omitted—That a return to those means, where the violence of the inflammation does not forbid it, will again succeed, and that it must not, for some length of time, be concluded, the stimulating power of the virus is at an end, when an inflammation does return after the remedy has been withdrawn. I flatter myself this observation will demand some credit, will be found to be generally true, and will preserve those who give it attention from some miscarriages and some contradictions in practice; for it must always be remembered, what it was which gave rise to the inflammation, and what it is which most probably is the support of it. This is a ground which should constantly

stantly be made good, before any conjectural cause be started for the continuance of the discharge, lest by leaving the true cause behind, a wrong scent be pursued, and the practice of error take place of the practice of reason.

An opportunity is now offered for me, to make some observations upon the remedies which are in use for the cure of Gonorrhœa. And first of all, I shall speak upon the use and abuse of injections. If those who condemn the use of injections, drew the line for fair argument, by asserting, that no injection, of any preparation, is proper to be thrown up the Urethra, during any stage of a Gonorrhœa, I should reply to them, that such assertion is not commendable, and that it is injurious to the advantages which are obtained by the contrary practice. If they assert, that there are injections which are proper at the commencement and at the close of the disease, which, on account of the inflammation, cannot be used with safety at the height of the disease, where, except milk and water, or what is as simple, nothing can be ventured by way of injection; in that case I perfectly agree with them. There cannot be a stronger proof of the superior advantage of injections, than their keeping the inflammation under; for as it is the inflammation continuing for a length of time, which only brings on the derangements in the Urethra, if that be shortened in its period, or lessened in its effect, if only this point singly were gained by injections, they would be of sufficient value to re-

recommend and justify them. But more than this can be done by injections. Repeated injections can prevent the extension of the virus over the Urethra, by an early application of them, and save the parts from any inflammation; and injections can close the disease, when no human power by any other means can effect it. A gentleman of Plymouth sent to me his case, in September, 1788: he had had a Gonorrhœa upon him for two years, had been salivated twice, (this is strange to be told in the year 1788) had made use of the hot bath, had taken the balsams, gum Arabick, opium, and bark. I advised him to inject, and bathe in the sea; and in a fortnight I received a letter from him that he was well. From what I have already observed it will appear obvious, that it is not the use of injections which should be decryed, but the abuse of them. Those who object to injections, because they can do mischief, should be requested not to forget, that every good in life might be equally proved to be an evil, through a perverse application of it.

I shall only make one more observation, for I am not fond of bringing forward a number of remedies, of shewing them on purpose to expose their inefficacy, or of reviving a catalogue of those remedies already rejected—it must be obvious that I know they have been in use, and that if I had selected them through choice, I should through choice produce them. But it might not be a waste of time to reflect, whether the ancient method of cure, by purging, has any

any preference or pretensions to be continued. If it be asked, during the inflammatory state of a Gonorrhœa, whether it be proper to keep the body cool, or whether from high living and a costive habit, a Gonorrhœa will not be protracted, I answer, yes. But if a cure be undertaken, and continued upon the purgative system, if a constant irritation be kept up, by repeated purgative medicines, I to that answer, no. There is no method of cure I have ever seen adopted which protracts a Gonorrhœa so long as this; and there is no method of cure which exposes the patient to an absorption of the venereal virus more than this; a swelled testicle often comes on from this cause, a dysury yet oftener; and the discharge from this treatment will be spun out to a tedious length. I think upon the whole it is as safe to do nothing as to attempt a cure by purging medicines.

I have already suggested what I think a necessary and prudential caution, that it should not be too soon taken for granted, the virus is extinct towards the close of a Gonorrhœa; and I have endeavoured to make a distinction (both from the symptoms and from the effect which remedies have upon them) between the actual presence of virus, and the consequent effects of it. These consequent effects I shall now consider, for at present I am pursuing and defining the true nature of the discharge, and endeavouring to discriminate where infection ends, and where that which is termed a gleet, commences.

As far as my observation has ever taught me to discriminate, I am of opinion that that which may be said to be a simple gleet, is an innocent discharge, flowing from the Urethra, yielded from the membranous secreting surface of it, and from the glands within it. It will sometimes be changed by intemperance and increased by inflammation. After a Gonorrhœa a gleet may arise, from a long inflammation of the parts, from the orifices of the glands being relaxed, from the debility of the whole of the membrane, and from an abatement of inflammation of the corpus spongiosum Urethræ, of the glands within the Urethra, and of the swelled testicle. It may be a sine qua non of the restoration of the whole of the parts which have been in a state of disease. But if it were a matter of doubt whether such a discharge possesses any power of infecting or not, I know not how to clear it up, either from a sight of the discharge, or from hearing the symptoms described which accompany it. The fact I cannot ascertain a priori. The importance of the question depends upon whether, by connection, infection be or be not given. I should be very cautious in permitting a man to have a connection who has a discharge in consequence of a former virulent Gonorrhœa.

If it were possible to discriminate with accuracy between a discharge produced from the mere habit of the parts, in consequence

conequence of former inflammation, and if it were known that the continuance of that discharge is not a necessary act in nature, in order that the thickening of the corpus spongiosum may go down, that the membranous surface may recover its former sound state, and that the induration of the testicle may all be dissolved—if it were possible to distinguish between a gleet, and a discharge which is a necessary act in nature to restore parts to a healthful state, we should be rewarded for our sagacity in knowing to a certainty that the one ought to be stopped, and the other let alone. This distinction is not so difficult but some clue can be given to assist us in making it out. If the patient be of a delicate habit, and of a pallid complexion, if he has practised an excess of venery, and is otherwise debilitated, if the discharge be accompanied with pains in the loins, if during the Gonorrhœa a Chordée was not very troublesome, if there be no symptom of it remaining, if the testicle did not swell, if the stream of urine be large, but not strong, I should conclude that the discharge ought to be stopt. I look upon it, that as the constitution in this instance has some concern in the protraction of the discharge, so ought the treatment for the cure of it be constitutionally considered. Bark and steel, the cold bath and the injection, may be first of all tried; if these do not succeed, the balsam copaiva should be taken. But all of them will not sometimes succeed.

The Chordée in general goes off with the rest of the in-

flammatory symptoms, but it is sometimes experienced to be a continued symptom after inflammation has disappeared. I think but little of it in point of importance, nor should I now have dwelt upon the subject, had it not furnished a late Writer with the opportunity of misapplying a new term in Surgery. He says that the symptom of a Chordée is felt from what he calls adhesive inflammation. This expression of adhesive inflammation by him, is what I should express, an adhesion of parts in consequence of inflammation. But neither of these terms, if they mean the same thing, or if they do not, are applicable to the condition of the corpus spongiosum, when the symptom of Chordée is felt; for if it were produced from adhesion of parts, I should consider the consequence to be unalterably permanent. I should conceive there could not be any change from disease to health, when once there was a compleat adhesion of parts. My reverence and admiration of the wonderful works of Nature are very high, but I cannot be persuaded that a part can be again restored to a state of perfect organization, when, for a length of time, it has been obliterated by adhesion in consequence of inflammation. A Chordée, from the thickness of the corpus spongiosum, will remain for some time after other inflammatory symptoms; but if an hæmorrhage should ensue, or if the discharge continues, it will gradually go off, at least I never saw it remain unalterably fixed—I never saw a Chordée that time did not cure.

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The Phymosis I have described to be an affection of the prepuce as well from Gonorrhœa as from Chancre, and I have described it also to be a redundant collection of extravasated lymph, together with an enlargement of the lymphatic glands. This disease will require some patience and rest. The Penis should not be permitted to hang down, leeches should be applied to the part, mercurius calcinatus should be taken internally; it should be fomented with a decoction of poppies, to which one third of spirit of wine should be added, and a poultice should be applied with spirit of wine in it also. If the fever runs high, the patient should be bled, and he should take every night the pulvis antimonialis. A continuance of these means will certainly reduce the inflammation. To prevent a return of inflammation, and to brace up the part (for the vessels having been loaded, are liable to a topical congestion) the Penis should be frequently washed with aqua ammoniæ acetatæ, or with ammonia dissolved in water.

The Paraphymosis is a symptom of the same nature, but with aggravated circumstances. If the stricture be not taken off at a certain critical time, the glans Penis will mortify. The Paraphymosis is always at first a Phymosis. The prepuce has been at some time slipped back, and could not afterwards be flipped forward upon the glans, but remained in that state. If there be danger of mortification, an incision on either side of the Penis, through the stricture,

will relieve it, and in other respects it is to be treated as a Phymosis.

I have already noticed upon the subject of gleets, that the discharge which was the effect of a relaxation of the whole of the parts, should if possible be removed by topical injections, and by a constitutional regimen. But this is not the only cause, as I have also remarked, for a long continued discharge from the Urethra. There are others by far more obstinate, which demand our attention to be separately investigated, and which after they are investigated will sometimes baffle our art. Diseases in no part of the human body are found so difficult to be discriminated and cured, as mucous membranes and glandular bodies when deranged. Such cases become additionally intricate from their situation being in a narrow canal, remote from sight, especially as the soundness of that canal and parts contained within it, is important, and necessary to life itself.

THE CURE OF A SWELLED TESTICLE.

A Swelled Testicle has been already fully described by me, I therefore shall proceed directly to the cure of it. The true method is very simple, and the consequences safe. The cure of a swelled testicle consists as much in forbearance from the former practice of some means, as in the selection of others—as much in what we should not do, as in what we should. It is a complaint which exacts some patience, and some passive obedience to it. If it comes on at the same time that there is a discharge from the Urethra, the symptoms of it will be more moderate, and less will be required to be done for preventing or curing it. In this case, the patient should be in bed, suspend the testicle in a bag truss, and lay upon the testicle folded linen dipped in aqua ammoniæ acetatæ. No injection of any sort should be used at this time, nor should any other means be taken to stop the discharge from the Urethra.

If the discharge returns in full quantity, depend upon it the swelling of the testicle will not proceed to any alarming degree; but if the discharge does not return, and as long as no discharge returns, the testicle will continue to swell, and every symptom will be increased. In this situation of the case, bleeding in the arm, or leeches on the part, fomenting

ing with decoction of poppies, to which spirit of wine should be added, poulticing, and suspending the testicle, and keeping constantly in bed, both with a view to the advantage of horizontal position, as well as to keeping the pores open, taking a grain of merc. calcinat. with half a grain of opium once in the day, and small doses of pulvis antimonialis at repeated intervals, will all be necessary. A repetition of these means will abate the pain and inflammation; and when they are abated, to prevent a return of them, to which, by any indiscretion, there will always be a propensity, and also to prevent a topical congestion on the part, from a relaxation in the dilated capacity of the vessels, linen should be dipped in aqua ammoniæ acetatæ, and laid on the testicle. This application should be frequently renewed.

If the swelling does return a second time, it is seldom equal to the former in point of violence, for the coat of the testicle, which is the seat of pain, having been once distended, complies with more readiness again than at first. Sometimes the testicle will come to abscess; but this is generally the result of neglect in the patient, or inexperience in the Surgeon—more often owing to the latter than the former—for the pain is so great, and the difficulty of going about so forbidding, that the patient, if both of these were necessary to a cure, would hardly submit to either.

But after all, a return of the discharge from the Urethra

is

is of more importance, and more accessary to the cure of a swelled testicle brought on from a venereal cause, than every medical art that can be devised. This simple act of Nature is superior to all the combined power of medical ability: and this act is cherished, by with-holding injections and purging stimulating medicines; by keeping the patient in a horizontal position, and his pores open. This is the reason why a testicle which swells and inflames through venereal infection, more rapidly than through any other provocation, subsides and recovers with more facility than from any other first provoking cause whatsoever.

CURE OF GONORRHŒA IN WOMAN.

FROM the seat of a Gonorrhœa being more upon the surface of the parts in woman than in man, from the injuries done to parts by the effects of virus not being so serious or complicated in their nature, less is required to be said upon this subject; and from what has been already laid down for the treatment of the same complaint in man, less is therefore necessary to be said for that reason. All the superficial parts, and the Vagina throughout, are exposed to the action of the virus in a Gonorrhœa; and most of the symptoms which are experienced by a man, may, in like manner, be experienced by a woman.

In the beginning, the symptoms will be, a pain in making water, a tumid fullness of the labia and nymphæ, a soreness of the Vagina, and a plenteous discharge, the condition of which will be conformable to the degree of inflammation. For the cure of this stage of the disease, I should recommend the injection which I have already proposed, to be thrown up five or six times a day, and a grain of mercur. calcinat. with half a grain of opium to be taken once in every twenty-four hours. It must not be forgotten, that in all those cases where the injection succeeds, it should not be discontinued too soon; it should be gone on with at least

least a week after every actual and every suspected symptom has disappeared.

But of this I am confident, that an early attention to consequences and a timely precaution will prevent the symptoms of a Gonorrhœa from ever coming on after an infectious connection. If the patient, after a suspicious connection, washes effectually all the superficial parts, and throws the injection a few times in the course of the first and second day up the Vagina, there will be nothing in future to be dreaded or cured.

When the symptoms which I have described are aggravated, and when the inflammation of parts is very extensive, the following means should be adopted. The parts should be fomented with a decoction of poppies, and the patient should take the pulvis antimonialis in small proportions, dilute plentifully, continue in the bed chamber, and the mercurial pill with opium should not be omitted. I have seen instances, from the violence of inflammation, where a mortification has taken place. As such consequences cannot happen but from the most palpable neglect, and when nothing has been done by way of remedy, the same means which I have already prescribed, only with the addition of some spirit of wine to the fomentation, and some to a poultice, will eventually succeed.

Sometimes the symptoms of a Gonorrhœa are not limited to the parts already described; I have known the affection to extend over the lower abdominal region, stretching round to the loins and hips. I have seen an inflammation from this cause equal to any that any other could produce. A gentleman applied to me for a Gonorrhœa, who from the little discharge that remained, fancied himself safe enough to lie with his wife, who was then residing sixty miles in the country. In consequence of the connection she was infected with a Gonorrhœa. The inflammation continued to increase more and more, until she experienced a great tension above the pubis, so that she could not bear the bedding which covered her to touch the part. The discharge was very thin and acrid, excoriating wherever it touched. He sent an express for me to come down to her, which I obeyed. I bled her often, fomented the parts, kept her body open, and gave her pulvis antimonialis with opium. When the inflammation had subsided, she used the injection, took a grain of mercur. calcinat. with half a grain of opium every night, which effected a cure. I saw another case in consequence of a Gonorrhœa, which I attributed to the affection of the same parts from venereal virus. This case also proved successful, from a similar treatment.

I think it necessary to observe, that whenever women apply to Surgeons for complaints seated in these parts, it becomes important to find out the true cause of complaint;

plaint. It might be either Gonorrhœa or Chancre, or neither—it might be some disease about the meatus urinarius, it might be an affection in the uterus, or it might be a Fluor Albus. There are many causes productive of similar sensations, which they, not seeing, can only describe from feeling, and which they, not conversant in, can only describe indefinitely. Prudence, and regard for our own reputation, ought to direct us to investigate those causes, and to obtain a true knowledge of them; although with the most squeamish, or with the most modest, we may often find much difficulty of getting their acquiescence to an ocular inquiry. But if I could not gain their permission, I would rather give up the cure of the case, than incur the chance of not understanding it.

A Fluor Albus may be in consequence of stimulus excited on the parts from venereal inflammation, or may be the remaining effects of virus. But here the same observations and the same precautions ought to be remembered, which were given in my former definite statement of Gonorrhœa and Gleet. Whilst any discharge is continued, in consequence of an original venereal stimulus, I cannot take upon me to fix any limited time when the power of infection ceases, before that discharge ceases also. I should be unwilling to mislead, or to defer pleasure that might without harm be indulged; but I know that the infecting principle will continue for more than twelve months; and I know fur-

ther, that the man who constantly cohabits with a woman thus conditioned, may not feel any effect of the stimulus, when a fresh man will.

When a woman has been infected with a Gonorrhœa, who before such infection had no more discharge from the Vagina than that which was necessary to health, it behoves her to attend to her symptoms, until she be restored to that state again; for whatever discharge remains more than there was before she was infected, is to be suspected. The Fluor Albus is common to many women, who have never been in the way of contracting a Gonorrhœa. The constitution, the passions, the manner of living, have a share in a Fluor Albus which did not originate from a venereal cause. But this being foreign to my purpose, I shall return to the gleet remaining after Gonorrhœa—for the cure of which, I recommend the injection, the cold bath, and powder of rhubarb, with gum olibanum, or balsam copaiva.

LECTURE VIII.

ON

OBSTRUCTIONS IN THE URETHRA.

I HAVE hitherto only treated of the effect of inflammation upon the surface of the Urethra, as it is brought on by the stimulus of venereal virus, and as it ceases, the discharge also ceasing and leaving the parts in as sound a state in every respect as they were before any inflammatory attack, through a venereal cause, had been made upon them. But this is not always the happy issue of a Gonorrhœa. The continuance of the inflammation, from wrong treatment, from intemperate habits, from natural dispositions in the constitution, will often fix more lasting injuries, which will remain destructive of all comforts, and dangerous to existence, after the disease that created them has been for a long time at an end. Such injuries will naturally be expected, and such varieties of them are naturally to be experienced. They become intricate and obscure, because they are out of all ocular reach; and they are various, from the variety of the parts which are exposed to inflammation. In order

order that some true idea may be formed of the anatomy of the parts which are subject to affections throughout the Urethra, I shall give a sketch of them, and of their situations and functions. This will assist us in making our observations with more truth, and enable us to account for the varieties of symptoms which occur from a particular derangement of any particular component part within the Urethra.

The inside of the urinal canal is covered by an exquisite fine and delicate membrane, very susceptible, and full of capillary blood vessels. On the surface of this membrane are found a great many little orifices, oblong, known generally by the name of lacunæ, various in their sizes. These lacunæ are the excretory ducts of as many small glands spread up and down the urethral substance. They pass into the spungy substance of the Urethra, to the convex side of the internal urethral membrane, and open obliquely from behind forward into the great canal. Their edges are of a semilunar, or crescent form, on account of their oblique opening. There are two of these Glands described by Morgagni, which are more considerable than the rest, situated near the glans Penis; the one under the frœnum, the other near the fossula navicularis. The gland near the frœnum often inflames and comes to abscess, discharging itself externally, by two orifices on each side of the frœnum. Further up in the Urethra there are three considerable lacunæ,

described

described by Cowper. Two of these lacunæ or ducts make a considerable rout through the spungy substance of the Urethra, directing their course to two glandular bodies, of the size of a horse-bean, of an oval figure, flat, and situated on the two convex sides of the spungy urethral substance, near the ends of the crura Penis. These two glands are covered entirely by the muscles called the acceleratores urinæ. The third duct leads to a gland, smaller than the two described, placed in the spungy substance of the Urethra, near the curvatures which the canal describes under the pubis, and often opens, by two orifices, into the Urethra.

In the cavity of the Urethra, towards its extremity, from without inwards, several anatomical points offer, very essential in themselves, and which demand our strictest attention. The prostrate gland is of a shape like a chesnut, and of the size of a small chesnut; it is situated between the bulbous part of the Urethra and the bladder, having its basis towards the bladder, and apex towards the Urethra. It yields a limpid glutinous secretion, which is conveyed into the Urethra by several excretory ducts. At the bottom of the urethral canal, lying within the prostrate and near the neck of the bladder, an oblong ovular eminence offers, terminating forward in a point, called caput gallinaginis, or verumontanum. This portion of substance is commonly perforated by one, sometimes two or three orifices, which are the excretory ducts of the vesiculæ seminales. There is

a valve

a valve to each of these excretory ducts. The vesiculæ seminales are two bodies, appearing like little bladders, situated under the bladder of urine, and on each side of the prostrate: each of them can be drawn into one single duct. They dicharge into the Urethra by the one or more orifices above-mentioned. Such is the anatomical description of those parts, which, when diseased, demand our present attention, and which will engage a considerable portion of our time to discuss, to explain, and to cure.

By all the permanent diseases, the several parts in the Urethra are liable to, from the commissure of the glans to the neck of the bladder, the stream of urine will become more or less impeded: and it is this impediment, with its concomitant symptoms, that proclaim the serious importance of such diseases. The original causes which produce them, have furnished variety of doubts and conjectures. Much ingenious argument has been spent upon this topick; for frequently these complaints come on at a period very remote from every previous assignable first cause. When they are present, they are too self-evident; how they were produced, what gave rise to them, cannot always with so much precision be ascertained. Whether the difficulty of micturition originated in a diseased state of the bladder, or in the Urethra, was formerly not easily distinguished. Before that could be decided by the passage of a bougie or catheter,

theter, most of the complaints which we now find originate in the Urethra, were attributed to gravel, stone in the bladder, ulcers, or some other cause or defect within the bladder. The symptoms of obstruction, by the impediment which the urine finds between its exit from the bladder, and the part which is obstructed in the Urethra, will often produce such effects on the bladder, as if the original complaint had had its rise there. And since the seat of obstruction is now so easily ascertained by the introduction of a catheter, or bougie, I am very sorry to remark, that I have frequently known the true nature of the complaint mistaken, and wrong treated for years, which, if it had been judiciously ascertained, would have yielded to right treatment in the progress of a month.

I shall, for the better ascertaining the true cause of these diseases in the Urethra, for the better distinguishing the varieties of them, and for the more rational appropriation of remedies, inquire strictly into their origin, into the difference between them, and also into the various contrivances which have been adopted for the cure of them, and their consequences.

It has been advanced by a late Writer, that he does not believe obstructions in the Urethra are ever in consequence of former Gonorrhœa. But he has not, in the place of this general accepted cause—a cause which has been received,

and tacitly assented to for a series of years, substituted any other, which will, with so much plausibility, account for them. If a more true cause for these obstructions cannot be assigned, than that which has always been suspected—if another be suggested, that has not a stronger foundation in truth, it is not only natural but just to adhere to that which has been already adopted. But where one cause is attempted to be totally set aside, without the least shadow of pretence, and where no other cause is assigned, to account for these obstructions, such sort of argument can never carry with it any credit; for who will follow the opinion of a man that attempts to set aside one adopted cause, and furnishes not another in its stead? Skepticism does not only imply a bare positive denial of any adopted system, but it goes farther—skepticism consists also in attempting to prove a former cause to be weak, because it feels itself capable of advancing another, which appears in reason to be more flattering to truth.

The remote period at which obstructions are experienced after a Gonorrhœa has been cured, is the chief objection he throws in the way. Besides, he has found out an instance or two where boys have had obstructions in the Urethra, who never could have had a Gonorrhœa. It is sometimes a long while after a Gonorrhœa that an obstruction appears in the Urethra, and I do not believe any person can be said to be exempt from a liability to an obstruction,

of

of one sort or another. But all this proves nothing against the ground of assertion, that Gonorrhœa, or in other words, a long inflammation continued on the substance of the Urethra, is not the most general cause of obstructions in it. My argument does not go so far as to advance, that Gonorrhœa is the sole cause—my argument only goes to advance, that the Urethra is more generally inflamed through Gonorrhœa than any other cause—that it is inflammation of the Urethra which lays the foundation in general for obstructions. If the Urethra has been exposed to inflammation from any other cause, that would lay a foundation for obstructions; and in all such instances, they could not be attributed to Gonorrhœa. Obstructions are not a consequence of virus, but of inflammation produced through virus, when they follow a Gonorrhœa. Inflammation either brought on by virus, by injection, or by any stimulating cause from within the constisution, such as stone, gravel, gout, or scrophula, (whatever produces inflammation) will hazard an obstruction.

When glands are deranged by disease, inflammation will not be the cause but the effect; for the inflammation is there excited by the current of urine not being permitted to pass. This is proved to be the case, for we very commonly find the whole of the irritation confined to that part which is between the neck of the bladder and the stricture; so that if there were no inflammation at the first commence-

ment of the obstruction, yet, from the constant irritation which is for a series of time kept up by the detention of urine, this secondary inflammation continues to increase, and parts become involved in disease, from that cause, which were not from original obstruction: and very often those parts so diseased will be of more serious concern, than that which was the original obstruction, for their situation is always more important—they must be situated between the original disease and the neck of the bladder.

These sensations of disease within the Urethra, when it appears that they were solely excited by a former Gonorrhœa, may be attributed to some of the following causes: to the swelling of a gland or glands within the Urethra—to a protrusion of any other part within it—to a thickening of the membrane of it—to an ulcer having formed on the surface of the membrane—or to a thickening of the corpus spongiosum Urethræ. But be the original obstruction from whatsoever cause, or be it on whatsoever part within the Urethra, it does not follow that the obstructed part, as an obstructed part abstracted from the act of urining, would excite any inflammation; and it is my opinion that it never would. I think the inflammation in consequence of an obstruction in the Urethra, is often excited from the manner of life in the patient, which otherwise, notwithstanding the obstruction, would not be excited. For colds, hard drinking,

ing, high living, violent exercises, secretions of gravel—of stone, acrid urine, strong venereal habits, sitting up late at nights, will always aggravate the obstructed Urethra, will always excite additional inflammation, when the contrary mode of living will procure a free discharge of urine, without either pain or inflammation being annexed to it.

I mean these positions to be considered as general ones. I do not mean to say that the case will be always and invariably so, for I know, sometimes the stricture is so irritable that if the urine were as innocent as simple water, the retention of it in the Urethra, and the effort to pass it, will constantly not only dispose the parts to inflammation, but will fix them in more disease—I mean, those parts which are between the stricture and the bladder. But when this is the case, those who live the most regular, will fare the best, and some must live low, to live at all.

The compound purpose of the Urethra has furnished a late Writer with a curious reason, why the Urethra is so liable to disease. He says, where Nature has furnished a part, and allotted to it a double purpose, both will be badly discharged. The fact is not so, nor is the reasoning just. Nobody can doubt but the Urethra, in a state of health, is complete for both the purposes of conveying urine and semen. To advance an opposite position, would be implying, that these parts are not now so perfect as they ought to be,

and

and that Nature has been lame in this instance. Through the common course of life, unless the Urethra be stimulated from assignable causes, and such as can be avoided, and are not necessary acts in nature, no organ of the body, either in man or beast, continues in a more uniform, perfect state of health. If we set aside intemperate acts, the stone, gravel, and Gonorrhœa, we never, or at least scarce ever find, from any other cause, a derangement of the Urethra. I even do not know that the continued pain from the stone will produce an obstruction in the Urethra, for I have never seen or heard of a case where a man had a stone, that it could not be come at from an obstruction of the Urethra. I refer you to search for instances through all the peasants in all the villages, where Gonorrhœa has not reached them, and to enquire from them whether this double purpose, so arraigned by a late Writer, is not well performed. But I shall pursue this argument no farther, than also to assert, that among the whole animal creation, it is very rare we find the Urethra deficient in the discharge of this double purpose allotted to it by Nature.

I therefore shall take upon me, agreeably to the concurring opinions of all former Writers, to declare, that a most common cause of obstructions in the Urethra is inflammation brought on it through Gonorrhœa. This is not, in my opinion, a matter of faith, as something more than probable; it is a matter of fact, which reason tells us cannot

cannot be otherwise. If there were any other general cause which produced inflammation within the Urethra, it would be known, well ascertained, and obvious beyond all conjecture. A continued application of corrosive injections will also prove a cause of obstruction in the Urethra, and I believe these two are the only true assignable causes, as they act from without inwards.

The causes which produce an obstruction by exciting an inflammation from within outwards, may be attributed to sharp urine, either through hard drinking, cantharides, gravel, excess of venery, or through a scorbutic, scrophulous or gouty habit. These which I have enumerated are the chief causes for obstructions in the Urethra, and these, in my opinion, will almost all be found to be the effects of abuse of constitution, of luxury, and intemperance.

In searching into ancient Authors, we find only those who treat of Gonorrhœa and the Lues Venerea, treat of the subject of obstructions in the Urethra. And it was not until some time after the first appearance of a Gonorrhœa, which was more than thirty years after the appearance of the Venereal Disease, that any Author advanced any thing upon the subject of obstructions in the Urethra; so that we can trace this disease as a disease regularly growing out of an inflammation in the Urethra, excited by Gonorrhœa, from the beginning. Alexander Trojanus Petronius, of Castile,

in

in the year 1565, is the oldest Author I have read upon the subject: and every Author who has written since on a Gonorrhœa, has said also something of obstructions in the Urethra, so strongly possessed were they that Gonorrhœa was at least a chief cause of obstruction in the Urethra. Ambrose Parré describes the symptoms of obstructions with fidelity and accuracy. He attributes them to two causes, to a venereal, and to an affection of the prostrate gland, independent of venereal. Our countryman, Wiseman, treats of them in his Chapter upon Gonorrhœa; also Bartholinus, Wharton, Mullins, Boerhaave, Dionis, Le Dran, Savoyard, Astruc and Sharp—all these concur in attributing the general cause of obstructions to Gonorrhœa, and all these have laid down systems of cure, which I shall consider hereafter.

Perhaps there is not a medical subject for the two last centuries, which has more engaged the attention of the profession than this immediately before us; not only in developing the characters of obstructions and the causes by which the stream of urine is lessened and impeded, but also in defining the best means of removing them. Those who have enquired strictly into the history of these diseases, and referred back to remote Authors, with a view of acquiring a thorough knowledge of the characters and causes, will find that the present age owes much to their ingenuity, both in their analysis of these diseases and in their method of

of cure. All these diseased conditions of the Urethra have been seen by the eye of the Surgeon. Some of them can be known, and defined from their peculiar symptoms, a priori; others are constantly disguised and concealed, and the true nature of them cannot at first be developed. The situation of them in the Urethra will always be of much relative importance, as the nearer they are to the neck of the bladder, the greater will be the difficulty in removing them, and the more will be the danger from them, if not removed.

Spasm has been said to be a cause of obstruction of the stream of urine. Those who have lately advanced this opinion (for it is an opinion of no long standing, or at least the Authors are now living who have ventured to risk it) should have been more explicit, if they were anxious to be well understood, and if they were serious in their endeavours to impress upon our judgment the opinion of spasm being a cause of obstruction in the Urethra. Do they mean to say that a spasm in the Urethra, independent of any thing else, is a sole cause for obstructing the stream of urine? Or do they mean to say, that in consequence of an obstruction in the Urethra, some will be liable to spasmodic affections, and that the difficulty in urining and the difficulty in removing the obstruction will be increased by this additional affection, which is annexed to original obstruction, and which renders it a case more complex? If they mean to say, that spasm, abstracted from every other cause,

will produce an obstruction and a lessening of the stream of urine constantly and unalterably, I deny that it will. For if spasm did ever affect the Urethra, abstractedly as spasm, the stream of urine would only be lessened during the spasmodic action; that being off, the stream would be again restored to its natural fullness. This would always be the true state of the case, whenever it was owing to spasm.

But if they mean to say that spasm is an affection, which the Urethra takes on in association with an original obstruction, in association with an original substantial cause which constantly and invariably lessens the stream of urine; this requires some consideration, and may be not unworthy of our discussion. It remains to be considered whether, from the general effect, the application be just, whether the term spasm conveys the true idea which is intended to be conveyed by it, or whether there must not be a new definition annexed to that term, before it can be received, as explaining what in reality is meant to be explained by it.

If a person tells me, his complaint is nothing but spasm, I should ask him, how he knows it? And if he replies, that he knows it to be so, because his urine always flows from him in a full stream, unless it be impeded by this affection, I should agree with him that the definition is proper. But such a definition is not applicable to the following equivocal symptoms which accompany a permanent

manent obstruction; when the stream of urine will be found to flow sometimes with more freedom, sometimes with increased difficulty, and when it will sometimes totally be stopped; when a bougie, fitting to the stricture, will sometimes be passed, and at others cannot; when the stream of urine will come forth flush, although no bougie can be passed, and when a bougie can be passed, although no urine will follow. Such equivocal symptoms do often occur, and if they were produced from spasm, in such instances the term would properly be applied. But the accepted idea of spasm as it affects any part of the human body, is not ample to express all these varieties of symptoms which I have stated, and moreover, the Urethra is not a part liable to be affected with spasm.

For if the Urethra were a part generally exposed to spasm, it would be found to be so at other times, and in other conditions besides that from a bare obstruction in it. It would be found to be so at the height of a Gonorrhœa, at the time when cantharides are acting upon it (by the influence they have upon the urine) when the patient is racked and tortured by the stone or gravel, and from that irritation which is brought on by immoderate and stimulating venereal repetitions. But in all these conditions by which the most violent symptoms are provoked, we see nothing of spasm as long as the Urethra is free of any obstructing obstacle, and yet we are to be told, because there

is a difficulty in accounting for intricate occurrences, which offer during the cure of obstruction, and more especially during our first attempts to cure it, that such difficulty which occurs is owing to spasm! The fact is, that it is an attempt to account for the difficulty in one word, either to avoid all further explanation, or to escape from that which could not be explained. Spasm, in this instance, is an unmeaning metaphor, a vague substitute for truth, a something indefinite, a cloak for ignorance, a shield borne upon the arm of empiricism. Whenever a complaint is intricate and difficult to be accounted for, whenever there is a loss for a real definition of it, either spasm, sympathy, or nervous is always ready at hand.

If I were to venture at a definition of the nature of spasm, I should call it a muscular or tendinous convulsive action. The Urethra is possessed of no muscular power whatever. The Urethra is to be considered as a membranous passive tube, which conveys semen and urine, and is acted upon by muscles. It is wholly from the action of muscles, that the semen and urine are ejected through it; so that if spasm has any share in this disease, it must arise from the convulsive action of the acceleratores urinæ, and other assisting muscles. But the Author of this opinion has not supported his hypothesis by the concurring testimony of spasm accompanying venereal acts, or other violent actions of the parts, when no obstruction prevails. Let us call our attention to that part of the Urethra which is pendulous from the

the body, and to which no muscle whatever is attached, and let me put this question openly to those who hear me: Was ever a passage denied to a bougie or catheter, at least only so far up the Urethra as where it hangs pendulous from the body, from any other cause than from a palpable obstruction, and which was pre-existing? Or at the moment when the tremulous patient is brought into the operation room to be cut for the stone, and when there is no suspicion of obstruction, would not the spasmodic affection to a certainty be called forth then? And would there not be some signs, some tokens of such suspected action demonstrable upon so important and critical an occasion? When and how often has this been the case? Where there are muscles, we may suspect spasm, the sphincter of the bladder is governed by a muscle, and has been often known to deny a passage for the urine, and I believe from bare spasm, without solid impediment, without palpable inflammation on it or about it. Discrimination is the very essence of science. By discrimination truth is weeded from error. Discrimination is that illuminated focus which enlightens every study.

I shall now attempt to account for those perplexities which offer during the cure of an obstruction. I shall attempt to reason upon the following cases. How it happens that a stricture, situated in any part of the Urethra, will admit of a small bougie at sometimes, and deny it at others—how the urine

urine will pass, when the bougie sometimes will not—how to-day there is every expectation of success, and to-morrow nothing can be done—how the bougie must be suspended, for fear of violence, and we must be content to wait and to watch a relaxation of those paradoxical symptoms. Such are the conditions which have been termed spasm.

I have already remarked, that an obstruction in the Urethra, barely as a point obstructed, is not in general the seat of the whole of the inflammatory effects which are experienced by it, and that if it were not necessary for the current of urine and semen to find a way through it, in all probability it would not be of sufficient importance to engage our attention. If that which obstructs the passage in the Urethra was, for instance, to exist in a similar membrane, as within the lips or cheeks, not a thought would be wasted about it. We very often find loose fungous substances protruding there, often inequalities and pendulous fungi, which we bite off, and treat as trifles. But trifles as these are, thus situated, yet if such obtain in the Urethra, and if, in consequence of them, the urine is obstructed, the whole of the parts between the obstruction and the bladder (because of a retention of the stream of urine) will in process of time become altered from their natural state, irritated and inflamed.

The urine is often liable to variation, from the condition of the patient, and the sensations of the Urethra will experience

perience a change from that cause also. The state of the constitution of the patient, and the sensations of the Urethra, will afford great scope in accounting for the paradoxical symptoms which are so frequently experienced. Obstructed perspiration, hard drinking, and even the first introduction of a bougie, are all likely to bring on these equivocal symptoms, when the passage is so narrow as barely to permit the urine to pass. I have known, in the Winter time, that if a patient thus situated only changed his bed, or continued abroad too long in the air, or went from town to country, or so delicate was his state, if the smallest obstruction in perspiration was experienced from the slightest cause, he would be obliged to have recourse to the pulvis antimonialis, and to keep in bed for some additional hours, in order to be able to procure a passage for his urine. For at this crisis, a bougie could seldom be passed, and if it passed, no point, but a momentary one was gained from it, and that was when the urine, as the bougie was withdrawn, immediately followed, before the disposition in the part to close had been carried into effect. There is an incredible affinity between the symptoms of a diseased Urethra and obstructed perspiration. That which should be carried off by perspiration being retained, the urine will be increased in quantity, and more acrid in quality. The natural secretions of the Urethra are also affected by obstructed perspiration. And this is a cause of those equivocal symptoms to which I have alluded.

But

Another cause is, where the stricture is very narrow, and where the parts beyond it are very irritable—where, from the narrowness of the stricture, and from the irritability of the parts, a bougie, upon the first attempt, will not always be successfully introduced through the obstruction, and when it is introduced, cannot be regularly continued and repeated sufficiently to dilate the obstruction: for if it could, in proportion as the bougie dilated the obstruction, so would the inflammation beyond it subside; and the oftener, in that case, the bougie was introduced, the more the irritable symptoms would abate. But by the constant interruption of this system, so far from the symptoms answering favourably, they will frequently become more aggravated, and oblige one to desist. The bougie which is first passed through a stricture, is only fitted to that exact capacity which the stricture will admit. The stricture itself, and the parts beyond it, already irritable, will become more so, by the aggravation of an extraneous substance introduced there. Now if constant and enlarged bougies cannot be followed up, one after the other, both the stricture and the parts beyond it will become worse than before; for the bougie, once only introduced, has not opened the stricture, but has inflamed it; and the stream of urine has not been improved, but lessened. A second attempt will not succeed, because the passage is narrowed; and when the inflammation is abated, a bougie is again introduced, it finds a passage, but when it is withdrawn, the same inflammatory

tory effects are experienced, and the same time must be again allowed for them to subside.

As every impediment to the stream of urine may be owing to a different cause, it must ever be an object of importance, to know what is the nature of the disease which produces that impediment. For, although the inflammatory consequences, which are brought on from every impediment, will be found to be of equal injury to the parts between the impediment and the bladder, and although the description of all of them, as given by the patient, will be the same, because the symptoms of all will be similar, yet it will be expected from him who practises, that he should be able to discriminate the particular sort of impediment, and be able, a priori, to give some satisfactory opinion upon the nature of the case.

As I before observed, the inflammatory symptoms which arise from a retention of urine will be the same from every obstruction; but the nature of every particular obstruction has its own particular definitive marks, which the finger of observation will be able to point out, and which will assist in drawing a prognostick. Some knowledge of this may be acquired from a description of the obstruction; but more may be with accuracy ascertained, by observing the stream of urine, and by an introduction of a bougie. The situation of the obstruction is first to be ascertained, for the nearer it is to

the bladder, so much the worse, as it is further out of our reach, and as the most important parts have a concern in it.

It is necessary also for me to remark, that the greatest irritability does not always arise out of those cases where the stream of urine is smallest; for very often we find, when the stream of urine is very narrow indeed, that the patient does not solicit to discharge his urine more frequently than when the Urethra was sound, only he is a longer time in discharging it. And so, on the contrary, have I seen, when a bougie of a moderate size can be passed without much hesitation, such great distress, from a constant solicitation to urine, that the first glass of wine, or the slightest provocation, will disturb the patient, and his urine must be directly evacuated. These very essential differences ought to be attended to, and it will be expected from us at least, that we should attempt to account for them. I have already given my opinion, that an obstruction which is the nearest to the neck of the bladder, will excite the greatest irritation; and from observation I am disposed to judge, in many of the instances when a constant solicitation to urine prevails, that it is not owing to a constitutional irritability in the patient, (that idea being too remote and vague) but to the particular part which is affected, to the nature and duration of that affection, and to his habits.

In order to discriminate properly, and to know how to speak

speak upon each particular case, and to treat it in the best manner, we should never lose sight of the two first leading points, which I have before insisted upon: these are, to ascertain the first cause of the obstruction, whether it were produced from without inward, or from within outward; whether from a local inflammation on the part, either through Gonorrhœa or injections; or whether from bad conditioned urine, scrophula, scurvy, diseased bladder, diseased prostrate, venery, or hard drinking. To cure the former will be much more easy than to cure the latter. But before any attempt be made, it is rational to aim at this knowledge of the case. I shall first of all endeavour to discriminate between an obstruction brought on from a local cause, and a constitutional one; and then point out to you what may be the nature of either.

I conceive, that an obstruction produced from a local cause, must have had its origin either from inflammation, or from some impediment which has grown out of the membrane of the Urethra; that it may be produced from a thickening of the membrane, from a thickening of the spungy substance of the Urethra, from pendulous fungi which hang loose in the Urethra, from warts in the Urethra, from an enlargement of some of the glands, or from a diseased state of the lacunæ. I conceive, that when an obstruction is produced from one of these local causes, and in which the constitution has no concern, there will be nothing like the

difficulty in curing it, as when an obstruction is owing to a constitutional cause.

To ascertain the first cause, it will be necessary to inquire whether the patient has had a Gonorrhœa, and when he had it, how long it remained upon him, what was the degree of inflammation, and where it was chiefly seated. The distance of time between a Gonorrhœa and the time of the first appearance of an obstruction should also be ascertained. The condition of the health of the patient should be inquired into, his habits of living, the apparent state of his health, his age, and whether any particular constitutional disease be predominant in him at this time in particular, or has been. If the patient be young and healthful, if the obstruction has followed the inflammatory effects of a Gonorrhœa, if there be no particular constitutional disease, which leads to an obstruction, or if he has had a constant discharge from the Urethra since he had a Gonorrhœa, the case is clear, that it is owing to Gonorrhœa, and that some of the glands, or some parts of the membrane are affected. If he had ever been subject to warts, they may be suspected, and when the obstruction is produced from warts, or from pendulous fungi, the urine will pass out, irregular, in a scattered and confused state, the force of the stream quite broken, especially at the latter end of urining, when the bladder is nearly empty. For when the bladder is full, the force of the stream will be strong enough to press down such

such loose substances, and it will come forth with a fullness and strength almost equal to a stream in health. If it be owing to a thickening of the membrane of the Urethra, or to a thickening of the corpus spongiosum, there will be seldom any discharge; the stream of urine will come forth very thin but strong, and it will be uniformly so, never varying into a large stream at any time, when the bladder is ever so full, or when the inflammatory symptoms are ever so trivial.

Such are the simple traits of these diseases, and as they come before us, our judgment upon them may be either confirmed or corrected by the great assistance which we may gather from an introduction of a bougie. When the disease is owing to glandular affections, attended with discharge, a bougie will, on being introduced, and of such a size as is fitting to the opening, hesitate a little, but after it is passed, and has been lodged there for some time, it will, when withdrawn, be particularly loaded with mucus on those parts which touched the disease, and it will be withdrawn with facility. If the patient's urine directly follows the bougie, his stream will come forth amended; for the urine occupying the same space that the bougie did, the stream will keep in possession of it until it becomes faint for want of more supply, and then a protrusion will take place again.

When the disease is owing to loose substances in the Urethra,

Urethra, a bougie will be passed with very little difficulty, and almost of any size. The stream of urine will never be improved from it, but only when it follows the immediate withdrawing of the bougie, and in that instance it will come forth in a stream equal to the size of the bougie, as long as it is impelled by a continued force, so that the stream be constantly kept up by the same supply, and by an uniform expulsive force; for if that force be once interrupted, although there may be a sufficient supply remaining, yet the stream will not again regain its fullness. The force from the current of urine must be strong enough to press down the fungous substance and to keep possession, in order that it may flow uniformly. When this uniformity of the stream is interrupted, the stream will come forth divided; these divisions will intersect each other, and preserving their separate courses, pass out in two or more, and sometimes in scattered, confused streams, and these weak and dripping, from a well known principle in physicks, that a fluid passing from a narrow canal into a wider, will move proportionably slow, to the narrowness of the canal from whence it flowed; hence the urine no longer describes that arch, when it is discharged, as it formerly did, where the passage was uniformly equal.

When the disease is owing to a thickening of the membrane or corpus spongiosum Urethræ, it will only admit a bougie which is smaller than the obstructed passage; and if there

there be force made use of in passing it up, it will require as much force to withdraw it: for as the obstruction is not produced from any yielding substance, so will the bougie remain up the Urethra, as if it were enclosed in a tight case: it will neither be advanced forward, nor can it be withdrawn, without as much force as was applied for the introduction of it. Whilst the bougie remains in the Urethra, the thickened part can be felt externally, and when it is touched, it will convey to the finger a sense of having touched something possessed of solid hardness.

These are the chief obstructions which originate in the local causes I have already assigned. It must be evident, when they arise from a cause simply local, that as long as these obstructions remain, so long will the inflammation and retention of urine be supported; and that they will only abate when the obstructions are removed. And it must be also evident, that if the obstructions be not removed in due time, the continuance of the inflammation will increase a diseased alteration in the parts which are suffering. These conditions I deem to be curable, because they were not originally either produced, provoked, or supported, by any predisposition in the constitution. But I only mean to say, they are curable, as long as the parts, when released from the obstructions, retain a power of being restored to a sound state; for if from any of these causes the disease has gone on, up to that point when the parts, after opening the stricture,

stricture, cannot be restored, palliation of the case, in that instance, must stand in place of a cure. You will please, therefore, to remember, that the aggravated condition of parts, arising out of a disease in the Urethra, which is merely local, could have been prevented; when the same, produced from a constitutional disposition, might not; for the former is the consequence of neglect, the latter is the progressive effects of that in the constitution, which cannot always be foreseen, checked, nor amended.

LECTURE IX.

OBSTRUCTIONS IN THE URETHRA, DISEASED BLADDER, AND OTHER PARTS.

I SHALL proceed to enquire into such causes as may produce an obstruction in the Urethra from within outwards. I mean to speak of the diseased state of the Urethra, when it is not brought on from any external violence.

A diseased bladder will have very great influence on important parts within the Urethra, and will bring on obstructions of a most serious nature. The consequence of irritation on the internal surface of the bladder, or about the neck of it, is, that when the urine is poured into it from the kidnies, it must be almost immediately again poured out. The bladder not being in a state to retain a due quantity, the urine will come away dripping through the Urethra, and will erode and inflame the membrane and glands within it. Thus the bladder cannot retain enough of urine to be expelled as it is in a state of health, nor is the bladder in a condition

for the expulsion of it, if there were a full quantity contained within it. The internal membranous coat of the bladder will be thickened by inflammation, the capacity of it will be lessened, and it will be deprived of all power of yielding to distention. I have seen the bladder after a long inflammation more than half an inch thick, and I have seen the cavity of it reduced so small as scarcely to admit a pullet's egg, or to contain an ounce of water.

Irritation, from a constant propensity to get rid of the urine, on the sphincter of the bladder, and parts around it, will produce an enlargement of the prostrate gland, a thickening of the membrane of the Urethra, and many more deplorable symptoms. A bougie can most commonly be passed, but it will afford only a temporary relief. It will only take off the pressure of the enlarged gland, whilst it remains up the Urethra; on withdrawing it, the gland will again obtrude upon the passage, and block it up. When the disease has not commenced within the bladder, but in the prostrate gland, the same symptoms will be experienced, and the pressure of the prostrate on the sides of the Urethra, and on the sphincter of the bladder, will render the case so complicated that it will be difficult to say whether the disease began in the prostrate or in the bladder, whether it was without or within the sphincter of the bladder.

As the prostrate, whenever it is diseased, only obstructs the

the stream of urine, by compressing the sides of the Urethra, it rarely happens that a bougie cannot be passed, and a bougie, in such an instance, will contribute more than any thing else to assist our judgment in determining whether the seat of the disease be within or without the sphincter of the bladder. For if it be the bladder which is diseased, or the sphincter of it, whenever a bougie be passed, very little urine will come away; but if it be owing to a compression of the prostrate on the sides of the Urethra, and if the bladder as yet enjoys its healthful functions, whenever a bougie be passed, the urine will then flow in its proper quantity and stream, because the pressure of the prostrate gland which gave cause to the obstruction is taken off. When the seat of the disease is doubtful, when it has not been as yet ascertained whether it be within or without the bladder, if, upon passing a hollow bougie or a catheter, or even a common bougie, the proper quantity of urine which is in common contained within the bladder be any time discharged, the inference from it is, that the state of the bladder is either sound, or not so far gone in disease, as that the condition of it cannot be remedied. This simple discrimination is of great importance, it will conduct you to form a true prognostick of the state of the bladder, without which, by every other assistance, you may not be able to ascertain it.

It may not be improper to remark, that the bladder is not only incapable, when diseased, of containing the destined

portion of urine, and of retaining it for a certain time, but that sometimes the portion of urine may be defective from another cause, namely, from the urine not having been poured into the bladder. And I state this fact, as it is so far relevant to the subject of our enquiry, that an error of the true cause may produce a method of treatment which would degrade our reputation and injure the patient. An inflammation on the kidnies, or a fixed diseased state of them, will stop the secretion of urine, and unless that fact is not attended to, an error in judgment may lead to an error in practice. A puncture might be made into the bladder, and that which is sought for might not be found. A stone might be imagined to lie in the neck of the bladder, and to retard the passage of the urine. And as the urine does not pass, as this is the result of the case, be the cause of it what it may, it is always necessary to find it out, in order to be able to procure if possible a discharge of the urine, or to say in a few words that it cannot be effected.

An ulcer on the surface of the Urethra, will be a very troublesome and dangerous cause of obstruction of the urine; and more especially if the ulcer be situated on any part near the neck of the bladder. The same irritation, the same solicitation to urine, the same dripping of the urine, the same exacerbation of symptoms which intemperance could produce on an ulcer within the bladder, will be produced on an ulcer without, the same thickening of parts con-

contiguous, and the same particular affection of the prostrate gland, will be predominant in this as it will be in the other instance. One distinction between an ulcer without the bladder and an ulcer within, is, that the discharge issuing from the former will precede the urine, if there be any thing like a regular act in urining; whilst the discharge from the latter may follow the urine. In the case of an ulcer in the Urethra, the bougie will cause more pain, and pass with more difficulty through the Urethra, than when it is seated within the neck of the bladder. Before the bougie is passed into the bladder, the diseased part will be sensibly affected when touched by it, and some discharge will be brought away upon it.

The parallel which I have drawn between them, as well as the little distinctions which there are between them, have been pointed out, on purpose that it may be seen by you how they may be confounded together, and how, whilst they are thus conditioned, they may also be rightly defined. But if they should not be defined, when in that state which I have already described, in all probability a short time will convince us of the difference there is between them, for a formidable, and often a fatal symptom will arise out of an ulcer in the Urethra, which cannot arise out of an ulcer within the bladder: and here the parallel statement ends between them, and the ulcer in the Urethra is avowedly declared.

Before

Before I proceed, it will be right to remark, that during the tremendous state of an ulcer in the Urethra, the patient will experience a constant lurking little fever, his tongue will be parched, he will be thirsty, and his spirits will be depressed. He will be much altered in his temper, his rest will be broken, his appetite will fall off, he will become thin and wasted by profuse nocturnal perspiration.

It is scarcely necessary to inform you that, if this ulcer cannot be cured, if its progress cannot be checked, if it be owing to an obstruction which may be between it and the external part of the Urethra, and which obstruction either is not or cannot be removed, or if it be owing to the lodgment of the urine, in consequence of the affection of the prostrate gland, be the cause whatever it may, the event of the case will be of the most serious importance, for the urine will continue to aggravate those symptoms which have been already commenced, the ulcer will penetrate deeper and deeper into the substance of the Urethra, and the urine, which cannot find a passage by its natural channel, will seek it where it can be found.

The urine will seek to be evacuated through the perinœum, a tumour will be formed, and may be seen externally, the protrusion will most commonly terminate in a fistulous opening, through which either the whole of the urine will pass, or it will pass partly through this way, and partly through its natural channel, if that be not wholly obliterated.

I have

I have seen patients endure this condition for years, that could not be relieved from it; and I have seen some recovered from it, and perfectly restored to health. I do therefore pronounce this to be the most favourable issue of such a case of extremity. Or, instead of this opening into the perinœum, the urine will find a passage into the cellular substance of the Penis, will be insinuated throughout the Penis, will be collected in so redundant a quantity as to stretch the prepuce, and the whole of the integuments of the Penis and scrotum, to such a size that they could scarcely be thought capable of yielding to. A mortification generally ensues, attended with the most excruciating pain, and the patient dies. For when the disease has gone this length, it is in vain to seek a passage for the urine by the Urethra. The glans Penis will be out of all reach, and such destruction of parts has already gone on in rapid progression, that every attempt for the preservation of life, I fear, will be hopeless.

I shall, for the better explanation of this state of the disease, give all I recollect of a case, which fell to the unhappy lot of a gentleman I not only knew as my patient, but as my intimate acquaintance, and who died in the year 1784. He had, three years before that time, applied to me for an obstructed Urethra, which was attended with a great deal of discharge. I was then able to pass a bougie, and I am certain, if he had persevered and availed himself of that advantage

vantage, he would have been preserved. He only used the bougie for a month or two, by which he found his case much amended, and that induced him to decline a continuance of it, which ought to have encouraged him to proceed. He was a man of a very companionable turn, full of anecdote, and of a most retentive memory: he was courted by company, used to sit up night after night, and this naturally produced a lazy negligence of all concern for his health. He was, at the time of his death, between thirty and forty; and otherwise to appearance of a firm constitution.

For some months before this extravasation came on, the irritation about the neck of the bladder became more and more vexatious to him, and his urine was discharged with increased difficulty. He could hardly sit on a chair, and had been obliged to spend his evenings at home, although very contrary to the bent of his inclinations. No bougie would find admittance through the obstruction, which appeared to be within an inch of the bladder. The discharge which flowed from the Urethra, was thin, and tinged with blood. Mr. Pott had been consulted, but as no bougie could be introduced, and as the urine then came away constantly drop by drop, it appeared to Mr. Pott that as a bougie could not be passed, there was nothing which a Surgeon was authorised to do, in such a state of the case, to be done; for as the urine did at that time flow, although it was only

only drop by drop, a Surgeon would not, in that case, recommend a puncture to be made into the bladder, or if he had, I am certain this patient would never have voluntarily submitted to it.

During the present alarming state of the case, an increase of the febrile symptoms, with other constitutional affections, came on, and a Physician took the charge of the patient. It was whilst he was under the care of the Doctor, that an evacuation of his urine through the Urethra was totally prevented, and an extravasation of the whole of it pervaded the Penis, the cellular substance of the scrotum, and the surrounding parts. Mr. Pott saw him twice in this situation; he gave him large doses of opium, declared the case to be lost, and judged, in such a state, every attempt to be an unprofitable torment. This ungovernable patient fell a victim to an idle and fatal inattention to himself, in the course of three days.

This unfortunate gentleman had been married many years, at least ten, and had gotten children. During his marriage, I can venture to affirm that he had lived chaste; but before his marriage, he had had a long-protracted Gonorrhœa, after which a discharge continued, and an obstruction in the Urethra succeeded; neither of which was ever after removed. I do not say that this would have brought on his death, if he had not sat up late at nights, obstructed

his natural perspiration, and if he had not drank, which he constantly did, too freely of diluted spirits.

I shall proceed to give some further definition of the other event from obstructed urine, where it finds a passage through the perinœum, and shall also endeavour to pass an opinion how it happens that this difference is eventually experienced. It may be observed, that in the case I have just given, the passage for the urine had totally stopped before there had been any process commenced for obtaining a passage through the perinœum; and in that case, the patient being weak and emaciated, the ulcerated part gave way, and the urine became diffused throughout the contiguous parts all of a sudden, it not being impeded by any thickening of the parts, through previous inflammation. The force of urine, in the above case, was all applied to one action, which, in the instance I shall hereafter state, is divided, as the urine will be found partly to pass through the Urethra, during the time that it is forcing a passage through the perinœum.—When the urine forces a passage through the perinœum, the natural passage is not totally obliterated; a pain in the perinœum will be felt for some time before-hand, and the perinœum will be externally inflamed; a tumour will present itself, very hard and red, and there will be, to the eye of inexperience, all the appearance of an abscess forming; it will become larger and larger, until it be the size of a hen's egg; it will begin to shine, and by degrees the skin will appear

pear so thin as if it would burst; and will feel as if the contents were an accumulation of matter. Sometimes, even after it is arrived to this state, it will gradually disappear, and either from a cold, or any other aggravating cause, it will return again. For if there be an enlargement of the natural passage, so that the urine can find its way, then it goes down; but when that is not the case, it protrudes again in the perinœum, and it is certain only to burst from that cause. The fluid which passes forth is always urine. Now as this process of Nature takes up some considerable time, there being always some resource for the urine by a partial passage through the natural channel, the inflamed parts, about the fistulous opening which is made in the perinœum, have undergone a complete change, they are thickened and hardened, and the urine is prevented from being diffused throughout them, and thereby confined to the fistulous opening. This is not an incurable case; the cure of it depends upon the following circumstances: if it be brought on merely from an obstruction in the Urethra, which has been never attempted to be opened, and which is situated on the exterior part of the Urethra; or in other words, if the fistulous opening be between the obstruction and the neck of the bladder, if the prostrate be not diseased, and if the other parts between the fistulous opening and the bladder be sound, if the bladder also be sound, and if nothing but the mere stricture exterior in the Urethra to the fistulous opening has produced it, there is no doubt but removing of the

stricture will produce a cure. And when I treat upon the cure, I will give a case in confirmation of it.

I shall now proceed to assign an enlargement of the glands within the Urethra, and an affection of the lacunæ in consequence of it, to be a cause of the impediment of urine. And when this comes on without any assignable local cause, by referring to the constitution, it will be generally found that a scorbutic habit will furnish ample argument to attribute it solely to that. The disposition of scorbutic habits is generally known to be slothful and venereal, and the propensities of such a constitution subject it to sit long and to drink hard, to seek an exhiliration of spirits by this habit of life, and to become the slave of sensuality, from want of vigour to shake it off. Seafaring men are most liable to obstructions from this cause, and those turned of forty find them advance upon them. Such as have eruptive faces, sore eyes, thick livid lips, rough skins, and impoverished constitutions, will be the most liable to them: their secretions and their urine will be more acrid, and if these were not the first cause, they will aggravate the symptoms. There is almost always a discharge from the Urethra attending this state of the parts, and which is thin and sharp. The glans Penis will be red, or will be livid; the sebaceous glands will yield a thin discharge, and the prepuce will sometimes be thickened. The stream of urine will come forth strong but thin, and sometimes forked, and the Penis will swell during the

the act of urining. The orifice of the Penis will be glued up, and little ulcers will be found about it. Upon passing a bougie, we shall find that it will hesitate at every part of the Urethra where the glands are situated, but we shall also find that we seldom fail in passing it. An exacerbation of symptoms will be experienced in this kind of obstruction, as it will be in any other, from intemperance.

The prostrate gland, of all other glands, is when diseased most contributory to the worst and the most lasting symptoms. This gland is frequently liable to affections from a double cause, it will either participate of a general inflammation, which may be found to pervade the whole of the Urethra through a Gonorrhœa, or it will become more seriously diseased from a constitutional cause, from scrophula, scurvy, hard drinking, and from excessive venereal addictions. When an inflammation is excited on it from venereal stimulus, if the symptoms be attended to, and if there be no constitutional disposition to support the diseased state of it, it will most commonly be restored to its former state of health, with the rest of the parts, at the close of a Gonorrhœa; but when a Gonorrhœa has been neglected, I have been able to trace from thence a continued disease of the prostrate, and which was habitually fixed on the patient as long as he lived.

From the effect which mercury has upon diseased glands, I should

I should be afraid to make free with it, where I find a disease of the prostrate, for I am apt to conclude, that if an injudicious quantity of that be given, the gland will be enlarged from that cause: at least, I can assert from experience, that mercury will stimulate and excite an enlargement of glands, which are previously obstructed, and will remove them further from a state of health, than they otherwise would have been, if so much mercury had not been introduced into the constitution. I therefore suspect, that an injudicious quantity of mercury will dispose the prostrate to be more affected, when it has been previously affected through an inflammation brought on by Gonorrhœa. I shall go into a fuller explanation of this opinion hereafter.

Whether a continued disease of the prostrate, which originated in Gonorrhœa, be supported by a constitutional disposition, or whether it was originally brought on from constitutional disposition, it is not very material to be known, so far as the cure has any concern in it; for if the prostrate be in a diseased state many years after a Gonorrhœa was contracted, no knowledge of the circumstance of the remote affection can benefit the present condition of it, and no idea of the original disease can be annexed to the present predicament. When the prostrate is affected from a constitutional cause only, it generally comes on at the latter part of life, and generally from amongst those who have

have been subject to venery, and are subject to hard drinking.

The first case I saw was of a Gentleman betwixt sixty and seventy years of age, who on account of this complaint had retired from the fashionable world, in which he had formerly lived with much splendour. This Gentleman told me, that he could trace his first attack to a Gonorrhœa many years before, but that the symptoms which I am about to give of his case, did not appear until within six years of the time that I saw him. I examined the parts, and found a fistulous opening in the perinœum, through which a part only of his urine sometimes passed; this fistulous opening would often become highly inflamed, and at other times it would appear as if the parts were quite free of complaint, and no urine would pass that way at all. Whenever he caught cold, or whenever a fit of the gout was coming on him, he would then experience an exacerbation of symptoms. The part between the fistula and the anus was always hard and protruding. The enlarged sate of the prostrate could be easily felt from the anus. His living was very high in general; he would often drink a bottle of wine at supper. He seldom experienced a dysury after the fistulous opening, but before, he had been often in danger from it. Bougies irritated, he therefore never used them but when he was forced, in order that the urine might be evacuated. But there was a symptom attending this diseased

eased prostrate, which I believe accompanies sooner or later every diseased prostrate, and this was, a discharge of mucus as thick as the white of an egg, and almost as much; it comes away sometimes after urining and sometimes before. This mucus has a faint nauseous smell, and it will be noticed by those who sit in company with the patient. Whenever this discharge comes on in any quantity, the patient will become tabid, pale, and dejected. It has been suspected that the semen has a concern in this discharge. I cannot take upon me to say positively whether it has or not: but I do not believe that it has, for at a late time of life, and at the latter debilitated stage of it also, it would be a phænomenon indeed, if there should be so redundant a quantity of that secretion from disease, which could not be produced in a state of health; besides, this mucus comes away without pleasure or pain, without any sensible effort whatever. It did not appear that this gentleman died of this disease, but he constantly grew weaker and weaker, from being exhausted through the great discharge.

Another instance of affection in the prostrate gland, I saw on a gentleman who died in consequence of it, about eight years ago. He appeared wan, feeble, and emaciated at the time that he called upon me, and suffered great distress from his lamentable condition. His nights were passed without rest, and his urine was never evacuated at stated times. No bougie could be passed, and from that cause all his

his symptoms were greatly aggravated. The check to his passing a bougie was on the exterior side of a fistulous opening which he had through the perinœum.

I must remark, that there is generally a stricture on the exterior side of a fistulous opening, but whether the stricture, in this confirmed prostrate case, existed prior to the fistula, or whether the fistula, through inflammation and thickening of the parts, produced it, is a point that cannot be decided, unless the progress of the case had been watched. When the prostrate gland is the seat of disease, the passage will generally admit of a bougie, unless there be a thickening of the parts, and an adhesion of them from inflammation. These will produce an obstruction on the exterior side of the fistulous opening, and through which no bougie will possibly be passed. It is a matter for inquiry, how it happens that a fistulous opening is to a greater certainty found to accompany a prostrate case, although that fistulous opening be on the side exterior to the prostrate, than from any other case of impediment within the Urethra. For it appears very reasonable to infer, that a fistulous opening will be the consequence of an obstruction which is exterior to it, yet in the instance of a prostrate case, it is exterior to the prostrate gland, which is the seat of obstruction. To explain this, it is to be observed, that the external opening of the fistula from without the perinœum will not be necessarily opposite to the internal

opening from within the Urethra. The urine will find a passage where there is the least resistance, and that least external resistance may not be always directly opposite to the internal opening within the Urethra. This patient also had a discharge of nauseous smelling mucus in profuse quantities.

Another prostrate case which I saw was not so far gone as the two former, already described. It was the case of a Clergyman, who, independent of this disease, appeared to have a tolerable stamen. He was about fifty-five years of age, had been frequently attacked with the gout, but never had had a Gonorrhœa. When I first saw him, a tumour had been for some time advancing in the perinœum, but had not bursted, although it had often threatened. He was not conscious of the cause of this tumour, and was greatly alarmed at it. It had been treated for a length of time as an approaching abscess, and it would very frequently appear on one day as if it would burst, and on the next it would again subside. This variation was entirely under the influence of his discharge of urine through the Urethra. When that discharge became more difficult, the retained urine would protrude, and when the impediment was obviated, it again passed through the Urethra. This patient could almost always pass a bougie, which accounts for his escaping so long from his threatening fistula. The same discharge of mucus accompanied this case also.

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I could detain you a whole day with details of cases of diseased prostrate glands; but as one is in general a fac simile of another, I perhaps might be told that I have said too much upon the subject already.

LECTURE X.

ON THE

CURE OF DISEASES IN THE URETHRA.

I Shall now proceed to treat of the cure of diseases which arise within the Urethra. But first of all, it may not be unprofitable to inquire how these diseases were treated by those who originally furnished us with the history of them.

From the very beginning, it will appear, that the idea of a bougie was not wanting to those Authors who first wrote on these diseases, as Alexander Trojanus Petronius, of Castile, has said, that "the Urethra must be cleaned by a wax "candle, or some such instrument." Nor was the idea of corrosives or causticks wanting to this ancient Writer, for he says also, "in case of a caruncle," by which is meant a protuberant projection of fungus, "gentle corrosives are to "be introduced in a dry form, or by injection, such as one "scruple of precipitate with two ounces of barley-water. "This," he says, "is an effectual remedy in such a case, and "operates

" operates with the least pain of any corrosive." So that we see two great points in practice established as far back as the year 1565, namely, the application of the bougie, and the application of the caustick, both in a dry form as well as by injection.

Ambrose Parré describes very fully the method of treatment in use by the Surgeons at Montpelier, who prepared a corrosive ointment, spread over very fine rags, wrapped it about a wax candle, and passed it up the Urethra; they afterwards drew out the candle, and left the end of the rag hanging out of the Penis. Others made a wax candle very slender, but with a stiff wick, the end of which was medicated with escaroticks, and which was worn until the caruncle was consumed. Also rods of lead were thrust into the passage, as thick as it would permit, which served to dilate the stricture. These rods of lead were besmeared with quicksilver ointment, and kept in day and night.

Wiseman, who was Serjeant Surgeon to Charles the Second, and whose works on Surgery will be read by every man that is not a pretender to the art, or that is not fastidious of the opinions of others, has treated very minutely upon the subject of diseases in the Urethra. Wiseman was a very bold Practitioner, and appears to have gone every length in the treatment of obstructions, even much further than any Surgeon either before or since his time—for in more

than

than one instance, he divided the Urethra, throughout the whole passage, to the neck of the bladder; but then he has the candour to tell us, that it was to no good effect, as the urine never afterwards passed by the Urethra, but flowed from a puncture made, previous to this general operation, in the perinœum close to the neck of the bladder. He also practised the use of the bougie and the caustick. He used French candles, which were made taper fashion, passed them to the obstruction, bound them on, left them lying upon it, and directed the patient to take them out upon occasion of urining, and afterwards to pass them again. By the use of these, he tells us, that there was room made for the application of his own candles, which he armed with mercurius precipitatus: but he says, if, after the use of these, he did not succeed, he had another resource, which was that of passing a canula into the Urethra, and of conveying through it a grain of caustick up to the obstructed part.

Dionis, a Surgeon of the first eminence, in his Demonstration of Chirurgical Operations, enters into a discussion of obstructions in the Urethra: their cure, he says, is performed by conducting a caustick to the diseased parts, by means of a wax candle, which will destroy the old callosity, remove the stricture, and give an opening to the wax candle to pursue its rout into the bladder. The obstruction being overcome, the sore is to be healed, and a leaden probe continued until the passage becomes free and open.

Thus

Thus we see that the system of curing, or attempting to cure obstructions in the Urethra, began in the year 1565, and continued to the year 1740, with very little variation in practice. Wax candles, leaden probes, and causticks, both in a liquid and dry form, were the chief remedies to which the cure was indebted. And as this practice became more diffuse, as quackery prevailed through France, before it reached England, and as the chief resource amongst quacks consisted in the various modifications of caustick, so were the mischievous effects arising from caustick more generally alarming, until Surgeons of reputation began to take the lead in decrying the use of it, and in banishing it from practice.

This reprobation of caustick for the cure of obstructions, began about the year 1740. In Savoyard's Observations in Surgery, we find that method of cure strongly reprobated. "This bougie," says he, "armed with a strong caus-
"tick, has made a considerable eschar in the sphincter of
"the bladder, and much inflamed the canal of the Urethra.
"A Priest of St. Genevive des Ardents came to the hospital
"at the same time, cruelly tormented by the effect of such
"another bougie; but the accidents were so sudden and
"violent, that there was no time for relief, the poor Ec-
"clesiastick dying in four and twenty hours. These dismal
"accounts sufficiently explode," says he, "the danger of
"using too powerful cathereticks. I dare even advance, in

"this

" this place, the treatment by caustick ought to be ba-
" nished the practice of Surgery."

The famous Le Dran, in his Operations in Surgery, followed up the censure of Savoyard. " The medicines," says he, " made use of on these occasions, should by no means " be irritating, and for this reason I should absolutely reject " the use of any caustick introduced into the Urethra, on " pretence of destroying the obstructions, since, as I have " learned from experience, they serve only to eat into the " canal."

Astruc was also decisive in his opinion upon this serious and important question. " But it is not worth while," says he, " to quote more Authors, upon the infinite number of " contrivances and forms of applying cathereticks, since this " mode of practice has been some time in disuse, for many " reasons: first, because it is calculated solely for warts and " caruncles which beset the Urethra, but by no means for " other obstacles that may straiten that duct, by which the " strangury, which succeeds a Gonorrhœa, is most com- " monly produced, as is now confessed by every one. Second- " ly, because it is more safe; for cathereticks, which have " force enough to consume caruncles, will, at the same time, " inflame, corrode, and ulcerate the sound parts of the U- " rethra. I know that former Surgeons have endeavoured " to obviate this inconvenience, by an apparatus of various

" kinds of instruments and remedies; but I know, at the " same time, that all their cautions have been for the most " part fruitless."

So far has Astruc, in his theory, militated against the application of caustick. To the above opinions I shall also subjoin that of our own countryman, Mr. Sharpe, who says, " However, at all times there have been enterprising men, " who have endeavoured, by escharotic applications at the " extremity of their bougies, to make way through these " obstacles which resist the bougie or the leaden probe; and " to say the truth, this practice has been avowed by the " ablest Surgeons of the two last centuries; but at present " it is universally condemned, and indeed it has been so al- " most ever since Savoyard's time. The objections to the " use of caustick were, the difficulty, and almost impossi- " bility of directing them, so as to eat through all the di- " seased parts of the Urethra, without destroying the sound " part; the impracticability of preventing the Urethra from " contracting when it healed, as much if not more than it " was at the time of applying the escharotick. And lastly, " the pain was so excruciating, and perhaps the application " was sometimes so poisonous, that an immediate mortifi- " cation of the scrotum, Penis, and bladder, were sometimes " known to ensue; upon these accounts, the use of escha- " roticks seems to have been entirely rejected."

They

They who knew Mr. Pott's practice, must have also known that, upon this question, he held the same language with the Authors I have already quoted; and I have it in my power to say, that I have heard him reprobate, in the warmest terms, the application of caustick to remove obstructions in the Urethra. He ever considered the mildest treatment, as that most fitting to cases thus conditioned.

Having given an historical account of the progress of the treatment of diseases of the Urethra, up to the present æra, it must naturally be expected, if I am not prevented from some conjectural motive, that I should offer some remarks upon the mode of treatment of obstructions in the Urethra, as it is laid down by Mr. John Hunter, in his late Treatise on the Venereal Disease. I look upon every publication to be open to fair criticism, and therefore do not see how there can be any objection against my inquiring into the merits of his theory and practice, as the cause of truth, the art of Surgery, and the welfare of patients, are the intended objects at issue in the question we are now upon. I have already considered Mr. Hunter's singular opinion upon the causes of obstructions, as they are found in the Urethra; I have already expressed my difference in opinion with him, when he asserts, that he does not believe any kind of obstructions are owing to former Gonorrhœa. I shall now consider how far his practice has pretensions to preference—how far it is likely to benefit the patient upon whom it is prac-

tised, or to do credit to the judgment, and enlarge the reputation of that Surgeon who follows his instructions. First, I shall remark, that Mr. Hunter has controverted the idea of any caruncles being ever a cause of obstruction in the Urethra; and further, he has, in his description of the obstructions which do really prevail, stated, that a thickening of the membrane, and of the corpus spongiosum, are general causes of obstruction in the Urethra. Admitting this to be the truth, how are we to expect that the application of caustick to a single spot in the Urethra can, by being barely applied there, remove such causes of obstruction? For in order that the caustick, if the practice of it were admitted, might prove successful, it must be previously conclusive, that it is to act upon the obstructing part, and thereby free the Urethra from the stricture. And if it does produce that effect, it must necessarily follow, that the stricture be confined to that point where the caustick is acting. To advance, as he does, roundly and boldly, that the application of caustick is generally successful, without pointing out to us any distinctions, without discriminating to our satisfaction, what are the sort of local obstructions which the caustick is so successful in curing, is, in my opinion, nothing like any argument or proof, which I have been used to receive as such, or can be conscientiously invited to follow.

As Astruc very properly observes, local obstructions, such as caruncles or warts, seem to be the only obstructing conditions

conditions in the Urethra, which the caustick bids fair to remove. The existence of the former Mr. Hunter totally denies, and that of the latter he is silent upon. Then it behoves me fairly and candidly to put this question: If the glands be diseased, if the Urethra, as it is to be seen in his plate, be thickened, if the prostrate be diseased, if an ulcer be formed, if there be neither caruncles nor warts, how does reason tell us that caustick is to act, so as to act beneficially? There should be certainly some fair promise, some plausible prospect far more flattering to our reason than assertion without argument in support of it, to induce us to adopt indiscriminately the application of caustick, knowing at the same time that its action is severe, and must go on, when once it is conducted to that part upon which it is intended to act.

I have already stated the opinions of Surgeons of the first reputation, upon the safety and admissibility of the application. I have shown how their objections to it grew out of the practice of it. I therefore shall point out, not without some little astonishment, two remarks, as they occurred to me, when I first perused the Treatise of Mr. John Hunter. The one was that, not having read upon the subject, as he confesses, he should have suggested from his own mind the application of caustick, and afterwards by reading find out that the idea was not new. The other is, that when he found the idea was not new, when he found it was an old idea,

strongly

strongly marked and reprobated by such professional characters as are in the highest reputation, as Savoyard, Le Dran, Astruc, and Sharpe, he took not the least pains to enforce, by any new argument, the merited revival of this justly discarded project, he offered no willing apology for flying in the face of those practical precepts, nor gave the least hint even, that Authors of reputation had ever said a syllable upon the subject. This I must say, is not treating him who reads his book with candour, nor has he done justice to the opinions of those Surgeons whom he so silently passed by. Even a man of genius must be content to learn from books the present state of human knowledge, that he may not weary out his attention in finding out things already well defined and well understood, and tire the recollection for researches already registered; for it must be considered as an unprofitable study, as waste of time, which might have been otherwise bestowed with usefulness and credit upon new undertakings.

Mr. Sharpe has given us to understand, that at all times there have been enterprising men who have endeavoured by escharoticks to make way through obstructions in the Urethra. And Dr. Astruc tells us that Surgeons have endeavoured to obviate the inconvenience of the application of caustick, by an apparatus of various kind of instruments and remedies, but he also tells us that he knows, at the same time, that all their cautions have been fruitless; and I take upon

upon me to speak for this present age, and to assert, that we have not any reason for changing that opinion which a former held upon this subject. If you be satisfied that in saying this much, I have only assumed to speak the language of truth, you will indulge me with the pleasure of following my natural propensity, by passing over the odious task of examining into the apparatus of Mr. John Hunter for conducting the caustick to obstructions. For I should feel all the chagrin of an interested reader of lectures, and my spirit would be depressed, if I were suspected of departing from the path of true science, in the dishonourable pursuit of aiming a fruitless criticism on the opinions of Mr. Hunter relative to the application of caustick.

I myself know but of very few instances where caustick, if it could be applied safely, and if it could be brought to act on the intended parts, can effect a cure. When the Urethra is thickened for some length, that cannot be said to be a case for caustick. When the prostrate gland is diseased, that cannot be said to be a case fitting for caustick. When there is an ulcer formed near to the neck of the bladder, the caustick would only precipitate the danger of it. When the glands in general throughout the Urethra are scrophulously affected, and when the lacunæ of them are diseased, in such cases caustick could do no good. Caustick, it appears to me, could only be of service, with any pretence, when the stricture which it is to surmount is only on a certain

tain point of the Urethra, and when nothing else that has been devised can surmount it; and in this case the caustick must be applied exactly to the stricture. On warts and fungi it may have its use; but we ought to know that it is these which produce the obstruction, we ought to know that we can apply the caustick to them, and we ought not even upon any terms to attempt it, until we have tried the success of every other means. It must appear very strange, and the importance of some must be not a little sunk, when it is considered that this application of caustick is recommended indiscriminately by them to every stricture that offers within the Urethra.

As in a foot-path, through fields, which cannot be totally shut out, the proprietor erects a stile, not from choice, but necessity, so shall I give an apparatus for conducting the caustick to a stricture, for those to apply it with the greatest safety whom I cannot prevent by my own will and argument.

In this simple apparatus which is before us, some powder of lapis septicus is embodied into the point of a small common bougie, which may be conducted, part of the way to the stricture, through a canula. This may at any rate have a chance of reaching the obstruction, and of acting upon it and about it. It may remain for six minutes, and may be repeated according to the degree of inflammation

tion which it excites. Mr. Sharpe observes, that escaroticks mixed up in plaster, lose their corrosive property ; but from observations which I have made upon this point, I think I have prevented that effect, by keeping the armed bougie in glass well stopped.

Thus we see the application of caustick, for the cure of obstructions, has been in practice from the remotest history. This, the bougie of Ambrose Parré, and the leaden probe, have been the chief resources, until catgut was recommended by the famous Le Dran, and until the bougie of Daran was moulded into that neat and useful form which we now find it.

Of the application of prepared catgut to open, by the gentlest means, strictures which are very narrow, I have availed myself, and it was owing to the success which I drew from this practice, that I was induced to recommend it to the public attention, by a pamphlet about twelve years ago, since which time I have had an opportunity of seeing a great variety of cases of obstructions in the Urethra, have been able to make my observations upon them, to learn what has been done for them by others, have been enabled to enquire into the various causes which have given birth to them, to ascertain, so far as it is possible to ascertain that which is remote from sight, the peculiar symptoms attending upon each peculiar obstruction, and to adopt the best

and mildest method of curing them, where that can be done, or to make use of the best palliative means that are fitting for incurable cases. I shall in my future instructions for the cure of obstructions, point out the share which a prepared catgut has in promoting it. Of the bougie which Daran has to claim the merit of preparing, I am now to speak.

From the history of bougies that I have lately given, it is apparent the idea of Daran was not new, for the same instrument had been more slovenly applied by Ambrose Parré. I shall not consider Daran at all in the sense of a Surgeon; I shall only consider him as the maker of a bougie, the application of which to the purposes of obstruction is amongst the first of modern improvements that the art can boast of. If you read Daran to know what his bougie was composed of, you cannot find it out. If you read him to know any thing about obstructions, you will not understand him, for he neither addresses you as a scholar, a man of reason, nor a man of truth. If you seek to know from what he says, how it comes to pass, that the bougie which he brought into practice, enjoys such preferable advantages to all former ones, you will find yourself so harrassed and bewildered with unintelligible jargon, that you will give up the pursuit.

The fact was, that he had made an instrument which was best calculated, by a proper temperament of pliabili-

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ty and resistance, to pass up the Urethra—to pass over obstructions with the most facility—to remain up the Urethra with the least pain—an instrument which either possessed or ought to have possessed no other qualities than barely the mechanical ones of smoothness, pliability, and strength, for the purposes of passing up the Urethra without danger, and remaining there without irritation. But if Daran had attributed to it alone its real properties, every person having once seen the instrument, could have made another similar to it, and the emoluments arising to him out of the invention, would have found at first, as they did at last, various channels; for in spite of the tricks and falsehoods which were employed by him, in endeavouring to possess the publick with an idea that there was something specific in the composition of his bougies, which acted upon obstructions only so as to destroy them, and left sound parts in the Urethra unaffected, yet the truth was not long thus counteracted by such absurd doctrine.

Mr. Sharpe, as it may be seen in his Critical Inquiry, made bougies, the materials of which he has given to the publick, and those bougies which he intended should act mechanically only, answered in every respect as well or better than those which Daran made, and thus it was that the delusion could be no longer supported. Mr. Sharpe saw clearly that a bougie which possessed any other quality, than that which was the mildest of all possible substances, was not a fitting instrument to remain in the Urethra. Since that

time, men who have learned the art of making bougies, employ themselves in it, and sell them to the Surgeons; and we who buy them, seeing them in their true light, have only to examine whether they be good or bad, whether they be well or ill made, whether they be too soft or too hard, whether they be finely polished or too rough, whether the linen be not rotten, or whether the composition has not been worked up too hot, so as to affect the linen, and endanger the breaking of the bougie within the Urethra. These points should be attended to.

There are many advantages we avail ourselves of in the cure of obstructions by bougies, from the variety of the sizes they can be made of, and from the safety with which we can push them forward into the Urethra. For it must not be forgotten, that it was owing to injuries brought upon the patients from caustick and rough instruments, that these bougies, as soon as they were invented, became so much sought after, that they are now in constant and general use, and that, in my opinion, they still maintain their preference, notwithstanding they have been attempted to be supplanted by a revival of the method of cure by caustick. Bougies of this composition can also be made hollow, a stillet can be passed through them, and the urine can be drawn off through them: they will often pass when a silver catheter cannot; and if it be necessary to let them remain up the Urethra, for the urine to be discharged through them, they are, in

that

that case, far preferable to the silver catheter. But the most preferable of all catheters is that which has been lately made of the elastic gum, and which may be had from Mr. Savigny. No Surgeon should be without an assortment of them.

It is a circumstance of no small gratification, notwithstanding, as I have already shewn, obstructions in the Urethra arise from various causes, and the nature of those obstructions are also various, yet, from experience, I have ever found that bougies do either cure, or serve to assist every one of them. The cure of those obstructions which arise from Gonorrhœa only, and which are experienced early in life, are the first I shall treat upon. The facility of effecting this depends upon two circumstances chiefly—whether the obstruction be far up in the Urethra, or whether the stricture be not too narrow to admit a bougie of any size and sort, for if a bougie cannot be passed, depend upon it, the patient will never get well, or at least I never saw a stricture disappear spontaneously.

After having made the previous inquiries, the patient should evacuate his urine before you, and the stream of it should be marked. A bougie should then be passed up to the stricture, and if it will not go through it, a smaller should be tried; but if it goes through, it should be suffered to remain as long as the patient finds he can bear it without much pain, or until he wants to evacuate his urine.—

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Upon withdrawing the bougie, his urine will come out more flush, and the patient will feel that it is discharged in a shorter time, and with less exertion than he used to give to the act.

It sometimes happens, upon attempting to pass another, or the same again, that we are foiled; but if it does pass, it should remain as the last, and be repeated according to the degree of irritation which it brings on: for with respect to the repeated and continued use of bougies, they cannot, where they do not irritate, be too often repeated, or too long continued: but in both these instances it must depend upon the feelings of the patient. In order to effect a cure, the size of the bougie must be gradually increased, until the stricture will admit one of the largest; that must be continued also for some time, and must be left off gradually, but not totally discontinued, for it will be necessary to pass it up from time to time, once in a week for instance, a long while. By this it will be ascertained whether the ground which has been gained is preserved, or whether there be a disposition in the part to become narrow again.

After the bougie has remained for some time up the Urethra, there will be a considerable quantity of mucus about it. This is always considered as favourable to a cure, because it shews that the membrane enjoys its natural secreting

ing power, and that the lacunæ yield the contents of the glands; for if the obstruction be owing to a thickening of the membrane, or of the corpus spongiosum, this discharge may be reasonably considered as a main cause, as a sine qua non of the cure. It sometimes happens, that the parts are so exquisitely irritable, that a bougie cannot be endured; and it oftener happens, that after passing a bougie for a few times, the irritation which was great before it was applied, is very much lessened in the course of the use of them.—This abatement of the irritation is to be expected, if the cure goes on as it ought; for as the irritation which existed prior to the use of a bougie, was brought on through a detention of urine between the stricture and the bladder, so by opening the stricture will that irritation subside. But where, in consequence of irritation, the parts cannot bear a repetition of the bougie, and where the bougie is not passed often enough, or long enough, or where it cannot through pain be passed, then the Urethra will have to endure irritation from the stricture, and from the fruitless introduction of a bougie. For these reasons, if the bougie should increase the irritation at first, it may by a repetition procure a contrary effect, when the stricture becomes more dilated.

It will not be long after the familiar use of bougies before it be discovered, whether a cure will be effected through them or not. If the stream of urine keep pace with the bougie,

bougie, if the stream continue to increase as the size of the bougie is increased, if there be no other irritation remaining than that which may fairly be inferred from the use of the bougie, one may reasonably expect that a cure will be perfected. But if, notwithstanding any bougie can be passed, the stream of urine be not improved as the bougie is enlarged, and if the irritation which was experienced before a bougie was applied, does not abate, it must be inferred, that the disease is of that nature, which the bare use of a bougie has not as yet removed, or possibly may not. For if the Urethra be beset with warts, or fungi, and if these be not destroyed by pressure—if a thickening of the parts have only yielded to the immediate pressure of the bougie—or if the glands which are enlarged have been only pressed down whilst the bougie was passing——if these, after withdrawing the bougie, again protrude and obstruct, the bougie will require to be used more frequently, and for a longer continuance, on account of the difficulty which is attending upon these circumstances, and on account of the doubts which may arise, whether a complete removal of the obstructing cause can be effected or not.

In these cases, where a bougie only procures temporary relief, it is of more importance that it should from time to time be passed, than we are at first aware of. To possess the power of always being able to pass a bougie, even in an incurable case, is of the greatest moment. That power, once

once gained, should never be relinquished. For, although we may not be able by a bougie to remove the obstructing cause, yet, as whatever danger ensues, or whatever injury arises, will be in consequence of irritation from detention of urine, so will a bougie always have the power of palliating such effects, and of bringing away the urine upon a pressing occasion, when otherwise an ischury would detain it. I have seen many live comfortably for years, who could not have existed a day without passing a bougie.

For where the bougie, by being passed, only puts aside the obstructing cause, and where it is passed when the bladder is full, merely that the urine should come away, this is the manner by which that effect is produced. The bougie is passed into the bladder, or up to the neck of it, at any rate it must be passed beyond the obstruction; it is then withdrawn very slowly, and, during the time of withdrawing it the patient is keeping up the effort of urining, the stream follows close to the point of the bougie, and the force and strength of the stream retains the same opening which the bougie has provided for it; but as soon as the urine is almost evacuated, and the force of the current diminished, the obstructing cause again presses forward, and must be removed by a repetition of the operation, in order for the act of urining to be again repeated.

In these cases the bougie can only procure a temporary

relief, as the obstructing cause yields to its immediate pressure, but returns again when that pressure is taken off. I think I may venture this inference, and it will highly shew the important office of a bougie, that where it can be passed, the life of that patient may be said to be safe, and sometimes through the very act of passing it; and where it cannot, the patient is in jeopardy every hour. Therefore, it is found to be an essential point gained, to be able to pass a bougie; for if the difficulty, on the first attempt, be ever so great, provided that difficulty be overcome, there is as much probability, a priori, to expect that such a case will be effectually cured, as when the stricture was not so narrow; for the irritation was not increased merely from the stricture, but also aggravated from detention of the urine. If the small bougies which are in common use, and made from composition, cannot be passed—if they should be found too feeble—if when withdrawn they be bent, and come forth twisted at the top, a prepared catgut should be then introduced.

It is a truth, and a matter for reproach, that the mind of a person will be alert in adopting all the purposes of an useful invention, when his tongue will be palsied to its praise. Catguts possess some preferable qualities, which I perceive the dullest Practitioner begins to discover; of their superior advantage more avail themselves, than of the candour and generosity in expressing it. Since I have brought them

them into practice, those who prepare and sell them, find their account in it. The best are made in France, and sold by Mr. Jones, Chymist, near the Monument. A small prepared catgut should be attempted to be passed very slowly, and the fingers of the Surgeon should, in passing it, be kept close to the Penis. It will stick sometimes at the orifices of the lacunæ, as it is passing through the course of the Urethra, but by withdrawing it a little, and by exercising a little patience, that will be readily obviated. If it be passed through the stricture, it will succeed without difficulty. No force must be used. And if it does not succeed in a few minutes, it should be withdrawn, and another introduced, for as it becomes moist, it loses its elasticity; but if it be passed, that moisture is of great service, for the catgut will swell, and the stricture will be dilated by it. Upon withdrawing one, another should be immediately introduced, and that rather larger than the former. Catgut will remain up the Urethra without exciting the smallest irritation, for as soon as it becomes moist, it becomes also flaccid, and as it parts with nothing to stimulate, after it has lain up the Urethra a little while, it cannot be perceived to be there. When withdrawn, it may be placed straight, and as soon as it is dry, it will be again fit for use. When it has been passed in increased sizes, so as to admit, by the opening that has been gained through it, a bougie of the composition kind, its preferable advantages cease, and the cure may be gone on with by the other.

I must premise, that success in introducing of the catgut does not always attend the first or second attempt; but as it is of so much consequence that an opening be obtained, we must not despair and give up the pursuit, for as all the urine from within does pass out at the opening, so must there be an opening from without inward, but that is found sometimes very difficult to be hit—perhaps in some cases as difficult as threading of a needle in the dark. It being practicable, it ought not to be abandoned. There is something also in the choice of opportunity, to warrant or baffle success.—If the irritation be more than common, that is not the time to try, or to expect success. When the parts are most free from it, is a period more happy for this experiment.

It behoves me to say something about that which is to be done to keep down the irritation, whilst we are attempting to pass a bougie, and before one has been passed. The cure of an obstruction certainly consists in opening the passage, and I know of no other permanent relief: but irritation may be kept under, and the danger of a total stoppage of the urine prevented by keeping the pores open, by the warm bath, and particularly by the pulvis antimonialis (which acts very favourably in those complaints) by bleeding, by keeping the body gently open, by clysters of laudanum, forty or fifty drops in water-gruel, and by taking a grain now and then of the extract of opium. When a warm bath is found inconvenient

inconvenient to be had, sitting over the steam of a decoction of poppies as a good substitute.

A blister has been advised by a late Author to be laid on the perinœum. Blisters have been and are applied to inflamed parts, or near to them, with great propriety; as in a Plurisy, for example, where an opportunity offers for inflammation to subside, and that is the event of the case when a Pleurisy terminates without coming to abscess, all inflammation ceasing. But such sort of reasoning from analogy will not hold good; as, in this instance, the effect from a blister cannot be the same, cannot be happy, for the inflammation will never finally abate until the stricture be removed, and all the revulsive means that can be devised, cannot remove the stricture. Therefore, the only true method of treatment consists in quieting the inflammation, in suspending the violence of its effects, until that which gives cause to it be removed. But a blister, both with respect to its local action on the parts, as well as to the effect of cantharides upon the urine, will irritate the parts more than before, without the least chance of removing the obstruction; for that, as I said before, can only be done completely by one way, namely, by opening the passage. If it were possible that the cause of inflammation were unknown, and if nothing but opening the obstruction to remove that cause were unknown also, then, from ignorance, a blister might have been suggested, as promising some service when applied to the perinœum,

rinœum, as being the nearest part to the seat of inflammation to which it could be applied; but in relation to a case and to a condition of parts well understood, the idea is as ridiculous, irrelevant, and irrational, as possibly could be suggested.

I hope that what I have hitherto said upon this subject may be well understood, that the ideas which I intend to convey are so plainly and perspicuously expressed, as to make my meaning perceptible, and then it will appear, that gaining a passage for the bougie home to the bladder, is the great desideratum; that to effect this requires all the indulgence which human patience can grant—that if the smallest composition bougies do not effect it, catgut must be tried—that whether it succeed or fail on the first onset, it must be followed up, as the only commendable resource—that when it succeeds, the patient is out of all immediate danger—that the irritation must be kept down—that blisters are improper—that when a bougie can be passed of any size, although we have gained a great point from such an acquisition, yet it does not follow that a cure of the case is to a certainty to be the consequence—that the nature of an obstruction may be such, as the bougie can only relieve temporarily—that it cannot always remove the cause, but by its pressure, it will generally palliate the effect.

I shall conclude this Lecture with leaving this impression on your memory, that a bougie ought always to be introduced,

duced, whenever a disease is apparent within the Urethra, or about the region of it—that by a bougie, we derive an improved knowledge for defining whether it be a positive obstruction, or some other cause, which, from its nature, produces equivocal symptoms, and renders the case doubtful—that by the action of a bougie on every obstruction, in all the varieties of cases, we are enabled to draw a prognostick of the event, to demonstrate with precision the true nature of the case, to ascertain, in the clearest manner, whether it be curable, and to say with authority, that if it cannot be cured, it can be palliated; but without a bougie, the life of the patient would be insupportable.

LECTURE XI.

ON THE

CURE OF DISEASES IN THE URETHRA,

(CONTINUED.)

I SHALL now take into consideration that obstruction which is attended with a fistulous opening into the perinœum, and through which all or part of the urine passes. A fistula might be produced either in consequence of an obstruction brought on from a local cause, or from a constitutional cause: and also either in consequence of an obstruction exterior to the stricture, or from a prostrate, or some other glandular disease.

When it is produced by the passage of urine being blocked up, through an obstruction which is between it, and the exterior orifice of the Urethra—when this is the only local cause, by removing the obstruction, and restoring the fistulous parts to a healing condition, there is not the smallest doubt but the case will be cured. But when it is produced

from an incurable obstructing cause, then the fistula will remain, in spite of every attempt to prevent it: and indeed it would be in that case wrong to attempt to heal it up, as it may endanger a total stoppage of the urine; for such opening is to be considered as an operation in Nature for the discharge of urine, which could not find a passage any other way. Therefore, at all events, the first step to be taken is to pass a bougie, and, until that be effected, nothing can be done for the fistula. After the passage for a bougie is gained, and after a large one is with facility introduced, it will be then seen, whether the obstruction between the stricture and the exterior orifice of the Urethra was the immediate cause of the fistulous opening; for if it were, the fistula will relax of its hardness, the tumour will subside, and when the bladder is full, the urine will flow flush, in a stream corresponding to the size of the bougie, and little or none of it will pass through the fistula.

But it may so happen, and it will be generally found to be the truth of the case, that when the prostrate gland is the seat of disease, or when some other incurable disease produces this fistula, there will be also an obstruction between the fistula and the exterior orifice of the Urethra; and then the callous thickened edges of the fistula itself will prevent the urine from passing, either partially or wholly, and will also, in many instances, prevent a bougie from passing into the bladder. So that, at any rate, whether the obstruction between the fistula

fistula and the exterior orifice of the Urethra were the cause of the fistula, or the consequence of the fistula, there being an obstruction, in order to cure the fistula, it must first of all be removed.

If, after the passage is made free, the urine be evacuated as it ought, and if no other cause can be traced, which might have originally produced the fistula, there will not be found much difficulty in reducing the callosities, in dispersing the indurated and thickened parts, in disposing the fistula to heal, and in obliterating every remnant of the disease. For after the passage is regained for a natural current of the urine, when obstruction solely has produced the fistula, it is astonishing to see with what ready compliance the parts are disposed to recover their original condition. Not long since, a gentleman applied to me, thus conditioned. He was about thirty years of age, and from his appearance I very much doubted if success would have attended any endeavour of mine, for he had as much eruption in his face as I had ever seen; and I concluded from thence, that his complaint would last as long as his scorbutick disposition did, and that the cause was in the constitution. But the issue turned out much more favourably than I expected.

Upon examination, I found there was a very large tumour in the perinœum, which was very hard round about it, but soft and red, and shining in the center. He made

water, not at stated times, but it flowed from him by drops continually, as from a leaky vessel. I was told that he had had a Gonorrhœa about a twelvemonth before, that it spun out to an extreme length, that the inflammation attending it was very great, and that once he had had an attack of an ischury, by which he was near losing his life. He had often attempted to pass composition bougies, but never succeeded. In the state I then found him, with great irritability of the parts, with a large tumour, which I knew to be contained urine, and although it had not yet bursted forth through the integuments, yet I saw that it would not be long first, I would not attempt to pass a bougie, because I knew it would be vain, but preferred waiting until the urine should make its way through the fistula, when the inflammation would be less, and the bougie would have the fairer prospect of success. I applied a poultice and fomentation to the perinœum. I gave him opiates and pulvis antimonialis, and in the course of a week, the opening of the fistula gave a vent for the urine that way. I then attempted to pass the smallest composition bougies, but failed after various repetitions. I then repeated my efforts with small catguts: with these I succeeded. After having increased the size of them, so as to procure a fair stream of urine, and after being assured that little or none found its way through the perinœum, I joined my assistance, in support of the natural disposition in the fistulous induration to subside and to heal; I continued the poultice, and dressed the external opening with

with fine powder of red precipitate; and in the course of three weeks, he was perfectly well, from the time that it first bursted. I do not chuse to enter into a panegyrick upon the preferable pretensions of the catgut, in gaining a passage where any other instrument cannot—for if what I have already said has, as it ought, engaged your attention and belief, I have said enough: and to say more, at any rate, may be deemed superfluous.

When a fistula has been of long standing, one may necessarily imagine that the parts diseased do not so soon conform to a state of healing, after the obstruction is overcome, as those in a fistula which has lately commenced. There may be a little more pains necessary, and the case may be a little more tedious; but all this is of no great moment. The callosities may be either dissected away, or destroyed by caustick, and then intervening granulations will effect an union of the parts.

Mr. Hunter has remarked, and I think most probably he is right, that sometimes the urine does not pass through an obstructed part in a direct line, for when the stream is very small, or when the urine comes away by drops through an obstructed part, and even when the stream of urine is of a tolerable fullness, the bougie will not find a way in, although the urine will find a way out. This I often have seen, and I think the reason assigned by Mr. Hunter is not an improbable one. In that case, it is impossible to conceive any thing

thing more fitting to follow the crooked canal, in all its mazes, than the catgut. And if the catgut does not succeed then, I am certain nothing else can.

A diffusion of the urine throughout the contiguous parts, comes next under our consideration to be cured. I have already observed, that whenever this deplorable condition unfortunately befalls a patient, the danger from it will be truly alarming; for it must be brought on from some part of the texture of the Urethra giving way to the urine. It comes on very unawares, without giving the least notice, no tumour pointing in the perinœum, as it does upon an approaching fistula; but the patient is generally feverish, and the symptoms of irritation, and difficulty of urining, are in a more aggravated state than common. Before a diffusion of the urine comes on, and at the time of its being on, there is an impossibility of passing the smallest instrument up the Urethra. Why, when the urine cannot find a passage through the proper canal of the Urethra, it should in some instances be diffused through the contiguous parts, and in others find a passage by an opening through the perinœum, it may be difficult to determine with any certainty, unless we can suppose that the texture of the parts, when the urine is diffused, gives way all at once, from its diseased condition, it not being possessed of that soundness and stability for inflammation to act on, as it is in the instance of a fistula, when from that reason the urine is confined by adhesion of the parts

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to the artificial canal which opens into the perinœum. I believe, in general, in cases of diffusion of the urine, that the substance of the Urethra has sphacelated; for, as I before remarked, it is always attended by a low and quick pulse, and a feverish heat.

If this idea be just, all hopes of success, from the most prudent precautions, are to be considered as proportionably distant. But this I can venture to affirm, in corroboration of it, that in two days after a diffusion has shewn itself, all the parts which are exposed to our view will put on a gangrenous aspect. And I know, that it was such an opinion as this which Mr. Pott held of the state of the parts, in the case which I have already quoted, when I described the symptoms of a diffusion, and which induced him to give opium in large quantities, and to leave the rest to the termination of Nature. For the conclusion cannot be far from just, and the inference which follows appears to be too natural, that when the urine is diffused, in consequence of a destruction of the parts at least, if not in consequence of a mortification already commenced, some miraculous change in them must be brought about, before any promising prospect of life can be hoped for. From what I have said, it must appear, that I have not so much faith in the art we profess, as to promise that any treatment which may be laid down, will answer our wishes. But as something will always be exacted from us, while life is hesitating at departure,

ture, I shall endeavour to point out that which seems to be the most rational; for sometimes, it must be confessed, it does so happen, that cases which we judge to be lost, are restored; and when they are not restored, the reflection of having done our utmost, will not be unpleasing.

I consider the Urethra, where the urine has been thus diffused, to have partially mortified; and I suspect, in most instances, the sphincter of the bladder and the contiguous parts, are also affected. I consider that what is to be done should be done without hesitation, on the instant the suffusion of urine is discovered, and that an opening should be made into the bladder, as in the operation for the stone. The not being able to introduce a staff for a guide, and the general diseased condition of the parts, being, in this case, additional obstacles, perhaps it may be remarked, that making an incision into the bladder is a display of theory which cannot be applied in practice. But it is not absolutely necessary for the purpose which is intended, that the incision should penetrate the bladder; for if it penetrate the Urethra, near to the sphincter of the bladder, and between the obstructing part and the bladder, an outlet may be thereby gained for future urine, and a catheter or bougie may be introduced by this opening, through part of the Urethra into the bladder.

If this be done immediately, on the first discovery of the diffusion, if the bladder be sound, and its sphincter unaffected,

fected, I see no reason why the case should be despaired of, if a mortification on other parts hath commenced. By the opening made, after this manner, some of the diffused urine will find a way, and be discharged: but what cannot, must be evacuated by incisions being made upon the parts so loaded. Spirituous fomentations should be applied, and opiates with bark and wine should be liberally administered. The passage once gained into the bladder, should never be surrendered. Hollow bougies, for this purpose, are preferable. When the parts so loaded have subsided, a bougie should be introduced through the natural canal, and if it cannot be made to pass, after repeated trials, to that part which is already dilated, the Urethra should be farther dilated, until the bougie can be either seen to pass, or sensibly touched by the finger. For if the obstructing part be not dilated, every idea of a solid cure will be impracticable: and if the first point be gained, namely, that of obtaining an evacuation for the urine by the means of the first incision, yet without overcoming the obstruction which is exterior to the incision, the utmost success which can be expected must be, that the first incision will generate into a fistula, as if the urine had naturally found a passage that way.

I shall now revert to the consideration of that which is to be done when a passage is denied for the evacuation of the urine, and when there is not as yet any signs of a diffusion of the urine, or any pointing protrusion of it to be

seen or felt in the perinœum. In short, I shall state the treatment that must necessarily take place for procuring an evacuation of the urine, which cannot be effected, on account of a stricture in the passage, by any bougie. These cases often present themselves, and no condition of patients requires more watching, more to be done, and more critical decision.

In order to do justice to this subject, we should consider it in two views: whether the detention of urine be owing to an inflammation on the neck of the bladder; or whether it be owing to a stricture having so completely closed up the Urethra, that it cannot pass only from that cause. The first cause is frequently experienced, both from obstructions, as well as from the use of bougies in removing of them; and it is very often experienced when the stream of urine had before been tolerably full, and when a bougie of some size can be made to pass. I mention these circumstances more particularly, because more time can be allowed for the return of urine, and less doubt will be entertained of the success of those means which will be employed, where inflammation almost solely appears to be the cause of detention, than where, from a mechanical impossibility, the urine is prevented from passing.

In the first instance, bleeding, opening the pores by the pulvis antimonialis with opium, and opiate clysters, will, by

by repetition, succeed. I lay great stress upon the pulvis antimonialis, and I think it preferable to the warm bath, for in the process of preparing the warm bath, and passing to and from it, the patient is harrassed, and the effects of the bath do not last so long as those brought on by the pulvis antimonialis and opium. Opium should be also not sparingly given at intervals, for as sure as the patient falls asleep, so sure will he discharge his urine. And I can venture to assert, that I never found this prognostick fail, where inflammation was the first cause. It is very remarkable to observe, what an affinity there is between the degree of some obstructions, and the season of the year and the state of the weather. In the Summer months, when the wind is not easterly or northerly, such complaints are always best, because then the natural perspiration is more predominant. In the Winter, or when the sky lowers, and the wind is in the east or north, in many of these cases, inflammation comes on to such a degree, that without keeping to the chamber, and taking of the pulvis antimonialis, no relief could be obtained. This always succeeds, and this success warrants my assertion of the great affinity which subsists between obstructed perspiration, and urethral inflammation and obstruction.

But if, in consequence of all the means which may be pursued, no passage for the urine be obtained, and no immediate prospect appears that it will be obtained, an arti-

ficial opening must be made into the bladder, for the evacuation of it. And first, in treating upon this, I shall also take it for granted, that the detention of urine is not produced from a mechanical impossibility in the urine to pass, for in that case I think the Operator has no choice where to make the puncture into the bladder. But he has a choice of parts for making the puncture, when he expects that, after the patient is once relieved from the danger which threatens a longer retention of urine, and after inflammation has subsided, the urine can return again through the Urethra, and find a free passage that way. The puncture into the bladder may then be either made in the perinœum, where it is made in cutting for the stone; or above the pubis, according to the former way of cutting for the stone; or through the rectum.

Mr. Sharpe has given the preference to a puncture being made above the pubis, in that part of the bladder where the high operation for extracting the stone is performed. I must observe, that he did not, when he wrote upon this subject, take into consideration at all the puncture made through the rectum, but only weighed, in his arguments, the comparative advantages of the puncture in the perinœum, or above the pubis. Speaking of the latter, he says, "this method has been occasionally followed by some emi-
"nent Surgeons for many years, and is still approved of;
"but it is not recommended, as having those superior ad-
"vantages

"vantages 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."

It was just about the time Mr. Sharpe published his Critical Inquiry, I believe in the same year, that Monsieur Flurant, of the hospital de la Charité, at Lyons, practised first the opening into the bladder by a puncture through the rectum. An account of this case was given to the publick, together with another case in the year 1752, and another in the year 1757, in a memoir published by Monsieur Poicteau, in the year 1760, entitled, Melanges de Chirurgie. The first was successful, the second failed, and the last was successful. In the year 1774, Dr. Hamilton, of Lynn, sent a narrative to the Royal Society of a successful case performed by him, by a puncture into the bladder through the rectum. In the year 1778, Mr. Reid, Surgeon to Chelsea Hospital, published a pamphlet upon the comparative benefits arising out of these various choices for emptying the bladder.

The preference which each method has over the other, may be considered abstractedly and relatively. If it be considered abstractedly, there is no doubt but that which is in itself the most simple in performing, and the most easy to the patient, that which commits the least violence on other parts,

parts, must be considered as claiming the superiority. And where the operation is barely performed for the purpose of procuring a discharge of urine, and in expectation that it will again reassume its former channel, I have no doubt but the puncture above the pubis claims the preference over that in the perinœum, or through the rectum. But in a very corpulent subject, I do not know whether I may not prefer the operation through the rectum. Either of them may be performed by a curved trocar, the canula of which must be continued in the bladder until the urine comes its natural way. Through this canula a catheter or hollow bougie will be at all times easily introduced into the bladder.

When the patient is not extraordinarily gross of habit, I should certainly prefer the method adopted and recommended by Mr. Sharpe, as the most simple and most safe: in addition to whose opinion, we have the recommendation of it from Monsieur Petit, tom. iii. who assures us, that he has never seen any accident happen in consequence of this puncture above the pubis. "I regard it," says he, (comme un coup d'epeé dans l'eau) "like plunging a sword into wa-"ter." I have practised often this operation, and I am astonished that it is not so generally resorted to as the merit of it exacts. If Mr. Sharpe recommends it, and if Mr. Petit thus declares, that this operation, in the abstract, is never dangerous, as in performing it on that part we are more remote

remote from injury which may be done towards the neck of the bladder, I see no reason why these strong arguments, from such Surgeons as Sharpe and Petit, should be resigned, why that which is safe should be changed for that which is doubtful, or why the rectum should be wounded, when it can be avoided: in short, when nothing forbids, and there is nothing forbidding but corpulency, I see no reason why that which is not only safe, but easy, should not be continued in practice. I have twice performed this operation, and met with no difficulty whatever. I found it a very simple affair. At any rate, a passage for the fæces is molested, by performing the operation through the rectum. If this operation be as safe, (and that I do not deny) it cercainly is more inconvenient and complex. I should not omit, for it is my duty to be explicit, that there is a successful case given of the puncture through the rectum, for a suppression of urine, as performed by Mr. Harris, Surgeon to the Charter-House, in the presence of Mr. Watson, F.R.S. and Surgeon to the Westminster Infirmary, in the first volume of the Memoirs of the London Medical Society.

I shall now proceed to consider what is the best method to procure a discharge of the urine, when it cannot gain a passage from a mechanical impossibiiity, as there is no passage in the Urethra, it being completely obstructed. When this is the case, if an effort in the natural powers of the parts be successful and supply the defect, it is most commonly procured

cured by a fistulous opening into the perinœum. This opening is obtained by a long process, by the attrition and pressure of impeded urine forcing a way through it: but where, from the suddenness of this stoppage, such a process has been incomplete, an opening made through the perinœum into the bladder, or between the stricture and the bladder, will obtain the purpose intended.

Whether it will be necessary to cut into the bladder or not, it must depend upon the close connection of the obstruction to the bladder, or to the obstruction being at such a space from the bladder, that by making the incision into the Urethra between both, the urine will be by that means evacuated. The intention of the operation being not only calculated to draw off the immediate urine, but to obtain a passage for it hereafter, through its original and natural channel, both of these objects must be kept in view in all our proceedings: therefore, having once obtained the former, it behoves us directly to advert to the latter, and after having introduced a bougie of a large size, it should be passed as far as it can for obstruction, and there it should be kept as a guide to the obstructed part. The incision having commenced immediately upon that spot, should be carried on towards the bladder, until the urine follows the knife; for a bare puncture made by a trocar will not succeed in this case, nor would a puncture avail any thing more than that of granting a temporary respite.

After

After having commenced the incision near to the point of the bougie, and after having obtained a discharge of the urine, it behoves us to revert back to the obstructing cause, and to make good a passage for the bougie. The bougie should be conducted forward into the bladder, and should remain there at any rate for a certain time. If afterwards the case should put on a curable aspect, and if the urine should be disposed to pass by the Urethra, perhaps from constantly keeping a bougie within the bladder that intention might be baulked, unless either a hollow bougie, or a prepared catgut, be that in use—either of which, the former as a tube, and the latter as a compressible substance, when moist, will favour the intention; the former, by the urine passing through it whilst it remains up the Urethra; and the latter by not incommoding the process of healing in the Urethra during the cure, and not subjecting it to a permanent fistula. I refer you to the twentieth case in the succeeding Lecture, for an illustration of this.

I have already observed, that the choice of performing the operation for the discharge of urine, which otherwise could not be discharged, was precluded in an obstruction: and I think, that now I have stated the method which I adopt to procure a passage, where there is a mechanical impossibility otherwise existing, it must obviously appear that neither a puncture above the pubis, nor a puncture through the rectum, can any ways effect the purposes which I have

thus pointed out; for there is a relative concern in the one instance, and in the other there is no other concern but in the abstract safety of the operation. The one may be the best, and the other the worst; but performing the best could not remove the obstruction. And indeed it appears, by reverting to the history of the cases, when punctures have been made either above the pubis or through the rectum, that none of the obstructing causes were produced from an impossibility of the urine to pass in consequence of a permanent obstruction.

The cure of the prostrate gland is, of all others, the most difficult, the disease itself being incomprehensible.—As I before remarked, when it is diseased in consequence of venereal inflammation in young people, provided there be no strong predisposing cause to the contrary, it will, by care and long attention, by doing every thing salutary, by keeping the part totally at rest, recover its former state of soundness. The requisites for effecting this are, opiate clysters, pulvis antimonialis and opium, sitting over a vapour bath, abstemious living, and moderate venereal intercourse. A grain of mercurius calcinatus, upon the first appearance of this attack, may be given every night for a month or more; and the greatest care should be observed to keep the constitution cool, and the parts at rest. Such serious attention, employed at first, will, in all probability, obtain a cure, where a disease of the prostrate is provoked solely

solely from an inflammatory cause. But if any induration be left after inflammation has for a time subsided, the same means, employed at a distant period, will fail in reducing it, because the diseased parts are thickened, hardened, and the vascular system may be obliterated beyond all power of restoration. No part of the human body is so complex as the glandular. A diseased gland, in general, is not obedient to any system of cure. It is a part over which we have very little medical ascendancy, and to relieve which, when diseased, no theory has hitherto been certain.

Scirrhous tumours, in cold low habits, will remain in an indurated state sometimes for years, and no medicine will affect them. Indurations of the testicle, of the breast, of the tonsils, will frequently defy the power of medicine. So arbitrary are glandular diseases, that we are always glad to leave them at rest, by attempting no violent means, lest we should rouse them, and more especially when we find them free from pain; and so fearful are we of consequences, that, when we can prevail upon the patient, we are apt to extirpate them at an early period, lest they may become yet more formidable.

With respect to topical applications to a diseased prostrate brought on from a constitutional cause, I do not promise much hope in any. When it comes on at a late period of life, as it often does, and when every assignable cause is very re-

mote, what can the power of any topical application avail in such a case? If we knew which of the vices of the constitution, or which of the vicious practices it was that first brought on an induration of the prostrate, we might then, with some plausible reason, point out the means, if not of curing, at least of palliating the case. As for instance, if it were brought on and be supported from hard drinking, an abatement in that will be the first indication; if it were brought on from venery, the practice must be moderated. But I believe, that generally an inflamed prostrate, brought on from any stimulating cause, and neglected at first, will become scirrhous; and I believe scirrhous glands are owing to the vascular system of them being obliterated; and from that cause, medicine has not, nor can have but little or no influence over them.

Although it must be obvious from what I have said, that I have not much faith in any system for the cure of a long continued diseased prostrate, yet it will be expected from me at least to point out that which appears reasonable to be done. A bougie will always be necessary for assisting a discharge of the urine. The parts should be kept quiet by a very regular mode of life. The discharge of mucus will be moderated only by abstemious living. The body should be kept open, and the patient should be guarded against catching cold. Opiates should be occasionally used. Lime-water may be tried. Sea bathing is proper. These

are

are the best palliatives that I know. Some have flattered themselves that a cure is to be obtained by hemlock internally and externally administered, blistering the perinœum, and by passing a seton through the parts adjacent.

In consequence of retention of the urine, and in consequence of a long irritation on the parts in connection with the bladder, it often becomes, from the one, stretched beyond its natural tone, and deprived of the power of expelling of the urine; and it often becomes, from the other, thickened, the vascular system of it destroyed, the capacity of it lessened, and incapable of containing its due portion of urine. The inner surface of the bladder instead of being smooth, will be fasciculated, and sometimes ulcerated, yielding a discharge which comes forth with the urine, and making it an offensive and turbid mixture. This will be the effect of a long continued irritation.

To distinguish these differences is absolutely necessary, both for defining a disease which may be cured, as well as that which cannot: the one may be curable, but the other is not. Of the curable I shall first treat. The external membrane of the bladder is muscular, the destination of it is to expel the urine, to conform the bladder to contract whenever the urine be emptied, and to support it in repletion. The sphincter of the bladder is connected to this muscle, and acts in conjunction with it, similar to the drawing string of a purse.

whenever

whenever the bladder be over-stretched from retention of urine, it sometimes has happened, that not only the inner membrane of it has bursted, but the outer muscular membrane also. When this is the case, it will always be fatal. It has at other times happened, that the inner membrane only has given way. When this is the case, I do not know that it is often fatal; but I know not what the art of Surgery can do for it.

When neither membranes have bursted, but when both are stretched beyond that tone which they cannot immediately recover, they may be restored again; but it will sometimes require care, attention and time, to be effected. If the sphincter of it, when the bladder is thus affected, be closed, it cannot of itself be relaxed; and if it be relaxed, it cannot of itself again contract. In the one instance, the urine will not be discharged at all, and in the other, it will not be retained, but will be poured out of the bladder as soon as it is poured into it from the ureters. The urine, when it is retained, must be drawn away by a catheter or hollow bougie; the latter may remain within the bladder, especially if there should be a difficulty to introduce a bougie. Bark and opium should be given internally, a fomentation of water impregnated with aqua ammoniæ acetatæ, should be applied cold to the abdomen and loins, they should be rolled with a flannel roller, and, as soon as the patient can, he should bathe in cold water. If these means should fail, or

be

be inefficient in confirming a cure, tincture of cantharides may be given, beginning with ten drops in gruel; or the balsam copaiba should be taken, and a blister applied to the loins.

But when the bladder is in a state of disease, in consequence of a long and fixed inflammation, I do not know a cure for it. In this instance the urine will always be discharged in very small quantities, and often, and it will be for the most part turbid and offensive. The strongest criterion of this condition of the bladder is, that at no time whatever will it or can it retain that full portion of urine, which the bladder does in a healthful state. It is this symptom alone that will distinguish the disease of it, in this particular, from all other diseases which it may be exposed to suffer. The assistance that art can bestow, will be most beneficially found in the mildest diet, in keeping the mind calm and composed, and the body open; in quieting irritation by every means, and preserving the Urethra free of obstruction.

The ureters and kidnies do also suffer in consequence of obstruction in the Urethra. For these consequences, and the cure of them where it can be effected, I refer you to the fourteenth, twenty-fourth, and twenty-fifth cases, which will be found in the subsequent Lecture.

LECTURE XII.

ON THE

CURE OF DISEASES IN THE URETHRA,

ILLUSTRATED BY CASES.

IN the course of my Lectures I have endeavoured constantly to impress upon your recollection, that your particular attention ought to be engaged to the discovery of the cause and the seat of a disease; and I have not omitted puting you in mind, that diseases of these parts are very often mistaken, and that from want of discrimination more errors are produced in practice than from any other or every other cause.

Many of those cases which have come before me as difficult cases, might have been cured in a very short time, if their true cause and real situation had been ascertained.—The simplest of all possible cases, if it be not rightly defined, and well understood, will be found as obstinate to be cured, as a case in its nature more tedious and complex.

The remedy is much more certain than the judgment. The remedy is most commonly successful, when the judgment has, in due season, given it the proper bias and true direction.

That which Mr. Pott remarked at his Lectures, in my opinion is very true, that Students attend to capital operations, and to remarkable cases, and do not sufficiently inform themselves of the common occurrences in Surgery. I have already said, that the introduction of a bougie into the Urethra helps the judgment in defining the cause of diseased symptoms in these parts. And I have already said, that a neglected disease in the Urethra will bring the bladder, the kidnies, and other relative parts, into a state of disease: and that when the true cause has been neglected, and when the disease has gone on either without any treatment, or under wrong treatment, it will be difficult to say which was the cause, and which was the effect. A subject may die of an inflammation of his bladder, or of his kidnies, from an obstruction, which was the cause, in the Urethra. A simple bougie might have effected that cure, when applied under the instruction of a true knowledge of the cause, when every other means, for the contrary reason, will not only fail, but will, through wrong application, be creative of additional disease.

If a Patient has a discharge from the Urethra, together with great irritation in all the contiguous parts; if this be produced

produced from an obstruction in the Urethra, if the judgment has not discovered that an obstruction was the cause, if a bougie has not been thought on, if the stream of urine has not been watched, such a case cannot be cured, but will become worse and worse, the inflammation will increase, the bladder will become diseased, and what is not unusual, the kidnies will, without great care, inflame and come to abscess.

Some of these cases, when they are found out in time, will with the removal of the obstruction do well; the kidnies, the bladder, and the contiguous parts to the obstruction, will be restored to their separate actions, and their organization will become again perfect. But that is not the case of all, for when the cure has been neglected, and when the organization of parts is obliterated by disease, when the coats of the bladder are thickened, and the internal surface of it lessened and ulcerated, when an abscess is formed in the kidnies, and when the prostrate gland is become scirrhous, or when an ulcer is formed in the Urethra near to the neck of the bladder, when these symptoms are evident, the opening of an obstruction in the Urethra, may not restore them again to a state of health, although it ought in every sense to be attempted. But nothing like the whole of these bad consequences is to be attributed to the want of information in the Surgeon; many of them arise from the neglect, the conceit, and the ignorance of Patients.

I shall proceed to lay before you a variety of cases, which have lately occurred, and I shall not omit selecting such as are simple, because it is necessary to be informed of the distinctions there are among them, and to acquire a general knowledge of their treatment. The simplest case is not to be passed by with inattention, as in ascending a ladder to gain the summit, you must begin with the lower steps. It is general knowledge that I am ambitious to inculcate: a comprehensive idea of diseases in these parts may be formed, by referring to the true nature of the cases, to the causes which produced them, to their supposed situation, to the remedies by which some of them were removed, and to the fatal cause by which a cure of others has failed.

CASE I.

A Gentleman returned from India had a Gonorrhœa many years before he first departed out of England, and which was a long time in being cured, the discharge and inflammatory symptoms remaining much longer than they ought. Previous to his return, from the symptoms of which he complained, the nature of his disease had been suspected by the Surgeon to whom he applied; but from want of an assortment of bougies of various sizes, or from some other cause, he was not able to succeed in passing one through the

the Urethra. The Gentleman experienced at different times an aggravation of symptoms, and was once very near losing his life from a total stoppage of his urine: finding that he did not obtain the desired relief, and that he was threatened with continual danger, under such a constant dread he returned to England, and applied to me.

The symptoms then were a hardness above the pubis, a discharge from the Urethra, a frequent desire to make water, a soreness along the perinœum, and a pain in the loins. I saw his urine pass, it came away drop by drop, but when he griped the Penis so as to collect it in quantity, after it had passed beyond the obstructed part, as much as was then collected would come forth in a full stream. I remark this, to shew you a distinction, which is, that to ascertain the true condition of the stream of urine, it should be permitted to flow out, if it can, without the Penis being griped; whereas, if the urine be retained for a time, by the fingers, the stream will deceive you whenever it be suffered to come forth. I attempted to pass up the smallest of the composition bougies, but failed. I then attempted a small catgut, and succeeded: itwas permitted to remain until he wanted to make water, when the stream was evidently improved; another was then introduced without difficulty, and was continued much longer than the former; and when this was withdrawn and the urine was again discharged, a larger catgut was introduced, which also opened the passage sufficient

sufficient for a proportionate increase in the size of the succeeding catgut.

By this means, without any interruption, in the course of three days, a small bougie was passed, and the patient himself, from this time, was enabled to proceed in his cure. He passed a bougie once or twice in a day, as it suited him. He ascertained that he could bear the bougie up his Urethra two hours at a time without irritating. He took care to live abstemious, to observe how long he could continue without a bougie in the Urethra, and yet return to the use of another, without the difficulty of introducing it being from that cause increased. He gradually enlarged the size of the bougie, and found the stream enlarged in the same proportion. He proceeded in this method, till he could pass a bougie calculated to pass up a sound Urethra: he persevered with a bougie of this description two hours every day, for a fortnight.

This was considered by him as a bougie of a standard size for keeping the Urethra in a sound condition. He continued to pass it afterwards in the following manner, in order to ascertain whether he kept his ground: at first every other day, then twice a week, and, for a long time after, once a week, not chusing to leave the bougie off altogether, lest the obstruction should again come on, and he should have to do more for the removal of it, than could possibly

possibly be necessary by these precautions; and knowing also, that if the obstruction did return, he must return to the use of a bougie of such a size only as could be passed.

At the close of this Case, and not before, I applied an injection of the vitriolic fluid, which completely stopped the discharge; but it had previously, during the cure, become more kindly and less, and every other symptom was removed.

CASE II.

A Naval Officer came from his station to England, to be cured of this complaint, because he could no longer do his duty. This case was also in consequence of a former Gonorrhœa. He was a very young man, and although he was able to pass at times a small bougie, yet he did not find that relief which rendered him capable to perform his duty. He had lived freely, and I believe had carelessly applied the remedies proposed for him.

When I first saw him, his stream of urine was very small, he had very little discharge, the solicitation to evacuate his urine was constant, and the irritation of the contiguous parts was very great. I could pass a small bougie, but it was done with difficulty, a great portion of the bulbous part

part of the Urethra appeared to be thickened and inflamed, so that the bougie required force for passing it, and force also for withdrawing it, as if the Urethra closed upon it and griped it. Finding that the bougie gave him great pain, and that he could not bear it to remain up the Urethra, I advised him to be bled, to live very sparingly, to go into a warm bath, and to throw up an opiate clyster every night. By these means the irritation had so far subsided as to permit a bougie to be passed with more facility, and to remain up the Urethra for a longer time. That which was feared to be at first impracticable, was afterwards, in the course of this case, found to be procured with the greatest ease.

In a fortnight he could pass a bougie himself without much pain, he acquired an increase in his stream of urine, his calls for the discharge of it were less frequent, he was furnished with bougies of increased sizes, he followed up the use of them, and returned after two months to his station again. This patient was also advised to pass a bougie from time to time, in order to keep his ground, and to ascertain the future state of the Urethra.

CASE III.

A Gentleman about thirty-five years of age, of a strong constitution, who drinks at least a bottle of wine every day, and sits up late at nights, found that he had more frequent calls

calls to make water, and was a longer time in discharging it than formerly. He had also a teazing sensation in his glans Penis, and an unpleasant feel along the Urethra. Upon seeing his stream of urine, I found it much diminished from that of health. I succeeded in passing a small bougie, but the irritation from it was too violent for me to proceed, until he had gone a few times into the warm bath, and had for some time abated of his habits of drinking, and sitting up late at nights. When I returned to the use of bougies, I found my expectations highly answered, and he his discipline well rewarded, for by gradually increasing the bougie, he soon got well. And although my advice to him was, to pass a bougie now and then, for the better ascertaining the state of the parts, and for preventing a return of symptoms, yet, as I have had the opportunity of knowing the fact, I find he has not since passed a bougie, nor has he as yet had any occasion for it.

CASE IV.

A gentleman about fifty years of age applied to me, when he was in extreme pain, from an inability to evacuate his urine. I found him with some bougies by his side, which, he said, he had been in the habit of passing, but now could not. He had had this obstruction for many years, and he thought it was produced from a long-protracted Gonorrhœa. I made an attempt with the same bougies, but failed. I

hastened home, and returned with some catguts. One was introduced, and it succeeded in procuring an ample discharge of urine. This gave my patient great encouragement, and he being conversant in the method of introduction, I left him some catguts to proceed with: with these he so far opened the passage as to be soon capable of introducing bougies—with these he got so well as to be able to go about his professional concerns, in which he appeared to be very anxious, and deeply engaged. From that reason, as soon as the inconvenience and danger in his case was removed, he neglected a perseverance in his cure, and became as bad or worse afterwards than he had ever been, the irritation having increased to a greater extent, and the stricture having diminished the stream more than when he first applied. In this situation I saw him the second time. Our meeting was not a little interesting to us both; for he thought he had the claim of applying whenever he pleased, without expecting any remonstrance for neglect; that the consequences were all to himself; and I thought that no credit, no self-applause, no conscious pleasure, no gratification, the result of a zealous attention to the discharge of duty, could be obtained through a patient with such a sentiment, and therefore I told him, that it was expected he would be more attentive to his case, if he desired my future instruction. He submitted to my resolve, because it was for his interest. With some difficulty I passed a catgut again. If I had failed, the cure would have been by me despaired of. A repetition

petition of the former means were adhered to, with more diligence, and continued with similar attention. He submitted to continue in the use of a standard bougie, after the passage was opened, and I am assured that he now continues very well.

CASE V.

A gentleman, aged twenty-two, applied to me with symptoms of the following description: an uneasy sensation along the Urethra, a frequent desire to evacuate his urine, some discharge from the Urethra, and a diminution in the stream of his urine. He had had a Gonorrhœa twelve months before, and presumed that his complaints were a continuation of that disease. He had not, from the beginning, ceased to inject. I must here remark, that the constant solicitude of patients seems to be bent to the importance of the discharge. They, not knowing causes from effects, judge that the discharge is the cause of all their other complaints; and the discharge being seen by them, when the other complaints are hidden and obscure, their anxiety goes no further, and their faith rests upon the belief, that if the discharge be stopped, the whole will be at an end. But the truth is, that the discharge is the effect of obstruction, and of irritation produced through it; that the obstruction being once removed, the discharge will cease of course; and that every power which can possibly be applied to stop it,

will be vain and ineffectual, previous to the removal of the obstruction.

The smallest bougie could only pass through the obstructed part, and this being effected, the consequences were fully answered. The size of bougies were graduated, and their application regulated. He could bear one up the Urethra two hours, twice a day, without irritation. His stream became enlarged, his irritation abated, and he got well in two months. This gentleman was also told to apply, from time to time, a standard bougie, which he has also neglected; but I can take upon me to say, his obstruction has not returned.

CASE VI.

A gentleman, aged fifty, had, for four years, repeated attacks of the ischury, and twice a swelling of one of his testicles. At each time these attacks came on, he imagined he had contracted a fresh Gonorrhœa, and that the high symptoms of inflammation were all in consequence of the virulence of each Gonorrhœa. It was at the time of one of the inflammatory paroxysms being on him, that I first saw him, and it was not till then that his complaint was ascertained. I saw him with an inflamed testicle, with shivering fits, with a constant desire of making water, and with only the ability of discharging it in the smallest quantity.

These

These were the effects of a long-neglected obstruction, and I very much doubted, in my prognostick, whether the parts, thus brought into disease from such long irritation, could have been ever again restored to a sound condition, even after the obstruction, which was the cause, had been removed. But the event of this case will shew, what can be sometimes endured, what parts will sometimes undergo in suffering, and yet again recover their pristine state of health, when once the cause of their disease is discontinued. I fomented the parts with decoction of poppies, to which I added camphorated spirits. I kept the testicle suspended, and the patient to his bed. I gave him the pulvis antimonialis, with opium, and I succeeded in passing a small bougie. On account of the irritation I did not permit it to remain long in the Urethra. He drank barley-water mixed with mucilage of gum arabick during the time of an exacerbation of the symptoms, and in a short time they were so far abated as that he could bear a more frequent and longer application of the bougie. As the passage became enlarged, his irritation became less; and in process of time, under the strictest regulation in the use of bougies, and temperance of living, he got completely well. My chief apprehension, in the event of this case was, that the bladder would not have recovered its organization. But where the disease is not too far gone, it is wonderful to see how the alarming symptoms do suddenly disappear.—This gentleman also never recurs to the use of bougies, although he was likewise instructed to do so.

CASE

CASE VII.

A gentleman, fifty years of age, very corpulent, and who had drank hard, for some time had experienced a constant irritation in the Urethra, and a dilatory diminished stream of urine. Upon his first application to me, I found he had been advised to take the tincture of opium, and opiate clysters. His language also was, that he could not bear bougies, for that, whenever one was passed, every symptom was violently increased. I desired him to let me see his stream of urine, and pass a bougie. With the latter he complied with some reluctance. I then told him (what from experience I knew would be found to come true) that his case was in its nature very simple, under right treatment, but that the consequences of neglecting or omitting that treatment, more especially to a man of his corpulency, would be fatal; that if no catheter could be passed, when an ischury was on him (and it was most probable that it could not) every other resource was fraught with danger of life—that the best opportunity for commencing to open the passage by bougies was, when the symptoms of irritation were at the mildest—that notwithstanding what he had advanced against the use of a bougie, his not being capable of bearing it, from its increasing the irritation, yet a bougie was his only remedy—and that to bear the use of it, he should live very temperate, use the warm bath, and by degrees try to bear it up the Urethra gradually longer and longer, in point of time

time. I told him also, that a bougie at first which could only pass through the opening which there was in the obstructed part, must naturally aggravate the symptoms, which, after some use, would as naturally, by the passage being more open, abate them; and what could not be effected by the first week, would be found to be done only by a longer continuance of the process, under a proper regulation; for if he found himself getting better, there was no occasion for haste; the passage being opened, that point being once secured, and the irritation having abated, he would then to a certainty get well at his leisure. He submitted to the use of bougies, with the other instructions I before laid down, and his cure was gone on with to the utmost expectation.

CASE VIII.

A man from the country, aged twenty-seven, was recommended by letter to me from a Surgeon. He told me that he had had a Gonorrhœa for some time on him, which, notwithstanding he had constantly used injections and other remedies for it, was apparently worse than ever. For some time he had a constant desire to make water, both by night and day, he was often obliged when on horseback to dismount, and sometimes, from his situation, he had undergone such misery, by retaining his urine, that a most profuse perspiration had bursted forth at once from him,

him, and which for a time afforded him relief; he also declared himself to be a most miserable man in another respect, for that he was under promise of marriage, but dared not to fulfill it. This case had never been treated otherwise than as an obstinate Gonorrhœa, and in all probability had he not applied where the practice is habitual to refer, from suspecting motives, such symptoms to the cause of obstruction, he would have never got well. I passed a small bougie, and found a palpable obstruction. I wrote to his Surgeon, and gave him some bougies, with the necessary instructions how to proceed in the use of them. I heard from the Surgeon soon after—that he proceeded successfully in the cure, that the passage was open, that the irritation had subsided, and that only some discharge from the Urethra remained, which was effectually afterwards stopped by an injection of the vitriolic fluid.

CASE IX.

A man about fifty, from the country, applied to me, and seemed under very great agitation. He told me that he was a married man, and father of a fine family; that he had laboured with a great difficulty in the expulsion of his urine; that, by night as well as day, he had unremitting inquietude; that his belly was hard; that he had a nervous fever on him; that he had been for more than twelve months under the care of a Physician in the country; that he had tried

tried various kinds of remedies to no salutary end; that he had been a fortnight in London; that he had, during that fortnight, been under the care of a Physician, who had treated his complaint as if it were a dropsy, and administered some strong purgative medicines; that on the day I saw him, he found so increased a difficulty in evacuating his urine, that his Physician was induced to draw it off, but failing in the attempt, and the man being alarmed at the sight of a slight hemorrhage, it was this which brought us together. I found him nervous to a degree, his belly was very hard, and sounded hollow; I saw him attempt to make water, which he could not effect. I attempted to pass a very small bougie, and succeeded, but no urine followed. I directly gave him forty drops of tincture of opium, and sent him home to bed. I called on him a few hours afterwards, and found that he had evacuated more than a pint of water. This I was very glad to see, because I knew the bladder could not be in a very bad state of disease, when it can contain a pint of water. I desired him to be fomented with a strong decoction of poppies, and to have an opiate clyster, his body also to be kept open by castor oil. I found that in order to expel his urine, he had been in the habit of taking forcing diureticks, and especially gin and water. These I desired should be omitted, and, in their stead, I gave him barley water with mucilage of gum. I passed daily a small bougie, which sometimes, in the first week, would go through the obstruction, but at others would not. Well

knowing that perseverance soon overcomes this difficulty, which very often baffles thus in the beginning of the progress of the case, I found in the course of a fortnight, there was no more check from that cause. The patient himself was by that time capable of passing a bougie; he had a proper assortment with instructions for his future proceeding. Before he left town, he could ride on horseback, his belly became softer, his appetite and spirits increased, the discharge from the Urethra which he had, was greatly diminished, and I know he is well at this time.

CASE X.

A person, aged thirty-five, married and sober, applied to me, after having been under the care of others before; he therefore was apprised of the nature of his complaint, and that the seat of it was in the Urethra. His residence was out of London, and from that cause I was curious to ascertain how such a disease could originate with him. He told me that many years since he had had a Gonorrhœa, that it was a long time before the discharge stopped, and that ever since he had progressively been afflicted with his present complaint. His symptoms were, a great difficulty of making water, with a very thin stream, and as the urine was passing, his glans Penis swelled, as much as with an erection; he was better in the Summer than the Winter, and always at the worst in dry easterly winds; if he caught cold

cold he was certain to feel the bad effects from it, or if he drank rather too much, which for that reason he seldom indulged. He said, he had experienced many severe attacks of the ischury, from which he had very narrowly escaped; and also added, that unless he could find more relief than he had hitherto experienced, at succeeding attacks, he should be lost. He told me that his present system of treatment was to pass up a bougie as far he could, and there to leave it, but that as yet no bougie had gone further, which appeared to be about three inches up the Urethra, and this was effected by a soft bougie, badly made. I then attempted to introduce a small catgut, and succeeded. I kept him in town for three days, until he himself was capable of passing a small bougie through the Urethra. I told him that the object was to endeavour by every means to keep the ground he had gained, that he should gradually increase the size of the bougie, that he should pass it at every opportunity, that he should foment the perinœum with decoction of poppies, and that if he were attacked again with an ischury, he should take ten grains of the pulvis antimonialis with one of opium, and also a clyster with forty drops of tincture of opium. I saw him a second time a month after, found that he had diligently passed the bougie, and that he had had no ischury, but I was surprised to find what little progress he had made in the increase of size of the bougie which he could pass, there being scarcely a shade of difference between the first and the last at the end of a

month's constant application. I passed one myself, and ascertained, that a great part of the Urethra was thickened, for it was with some force that I could introduce it, and it remained up the Urethra quite tight and fixed, as if the Urethra griped upon it. With unusual difficulty I also withdrew it. I then told him that his hope of cure rested upon a faithful attention to instruction, that nothing could be done but through perseverance, and that by unremitting attention to the use of bougies, in proportion as the Urethra was distended, and when a discharge from the Urethra was seen upon the bougie (for the bougie at this time came forth dry) he then would find his industry daily rewarded. I provided him with some very stiff bougies, and he returned a month after, considerably improved in his case; a bougie of alarger size could be introduced with more facility, and some discharge was beginning to come away upon the bougie. I desired him to rub in a little quicksilver ointment along the perinœum, in order to assist a discussion of the thickened part, and to proceed as before in the use of the bougie. In a month after, I saw him so much improved, as to pass, with ease, a bougie of a very fair size. I told him to keep to one of that size, and to regulate the use of it so as to keep his ground. A few months after this he returned to me, almost as bad as ever, for he had been out of bougies, and thinking himself well, he had carelessly neglected the introduction of one for more than two months; but by a return to the same means, with much less difficulty, he soon regained

regained his advantageous ground, and has been prudent enough ever since to keep it, by now and then remembering to pass a bougie.

CASE XI.

A Physician did me the honour of consulting me for a complaint in these parts, but it was not as yet ascertained what was the true nature of it. He had experienced a frequent desire to evacuate his urine, he found that he was longer in passing it, he felt a constant teasing sensation about the region of his bladder, he had some little discharge, his mode of life was exemplarily sober, and he was not more than thirty-five years of age. He had consulted other Physicians about his case, who were of opinion, that the seat of complaint was in the bladder. It is strange to be told, he had never as yet passed a bougie. This circumstance will bring to your recollection what I have often dwelt upon, that whether the seat of disease be in the Urethra or not, the introduction of a bougie helps the judgment to define it; and that many a disease terminates fatally, which could be cured in a very little time, and in a very simple way, when the true cause and seat of it are ascertained. I passed a very small bougie on the first attempt. I found his stream very small, and the obstruction very simple in its nature, for from that time my patient became his own Surgeon, by only recurring, from time to time, for more bougies, and these

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in increased sizes. The last visit he paid me, I had the pleasure to hear that every disagreeable symptom was declining. He further remarked, that he had a brother, remote from London, who had been for a long time ill of what was supposed to be a complaint in his bladder and kidnies, and that he would take care, from the insight which his own case afforded, to have the true cause of the case of his brother ascertained.

CASE XII.

A gentleman consulted me for a very curious case, and one which I have not been able either to ascertain the nature of, or radically to cure. With the assistance I have given, he seems to be more satisfied than myself, as he is content to let it remain in the imperfect state of cure in which I now consider it. Whenever he catches a cold, whenever the wind is easterly, whenever he drinks hard, or lies in strange beds, he becomes exposed to very severe attacks of the ischury; and when these attacks are over, his stream of urine returns as flush and full as that of any man whose urinary parts are in a state of health; and he finds himself as well, in every respect, as any sound man whatever. It is now near three years since I first saw him. All his attacks of the ischury have been removed by the warm bath, by the pulvis antimonialis with opium, and by opiate clysters. The smallest of all bougies can, with some difficulty, be passed,

sed, whenever he is at the worst, and the assistance of a bougie, at that time, is found to be very essential; for, independent of the use of it, the other means will not be effectual. But the strange part of the case consists in this, that when he is to all appearance well, when his stream of urine flows full, and when he has not the least irritation, no bougie has yet been passed into the bladder, not even a catgut. The attempt has been often made by him, by myself, and by another Surgeon also, and each without ever succeeding.

CASE XIII.

A person, in the parish where I live, aged forty-five, applied to me for a difficulty in making urine. He had no discharge from the Urethra. He was a married man, and in general sober, but not strictly so. He was constantly solicitous of making water. His rest at night was very much disturbed, and his occupation through the day interrupted. I attempted to pass a bougie, but did not succeed. I then had recourse to a catgut, and with this I did succeed. I repeated the catgut, until such time as I was enabled to introduce a bougie, and with that, by a gradual process, I obtained a perfect passage; his diseased symptoms disappeared, and he became perfectly recovered. This case was attended in the Summer of 1789, and in the Autumn of 1790 (it may not be unworthy of the record to state it) his wife was brought to bed of three living children at one birth.

CASE

CASE XIV.

This is a continuation of the second case which I gave in my Critical Inquiry, and it serves strongly to prove in evidence, the consequent effects of indolence and neglect. This gentleman had experienced, for eight years before he first applied to me, which was at least fourteen years ago, a very narrow stream of urine. It was with the most difficult efforts and strainings that he could evacuate his urine. When he first applied to me, I remonstrated with him on the danger of his condition—that a man so corpulent as he was, and who lived very high, would, without every exertion, run the risk of his life; and that if, as a seafaring man, he should be attacked with an ischury, whilst upon his voyage, then his fate would be inevitable. He used the warm bath, he was bled, and as the perinœum was thickened and indurated, quicksilver ointment, in small proportions, was rubbed along it. The smallest catgut then found a passage on the first introduction. There proved to be three obstructions, one a little way up the Urethra, and the others at a distance of about an inch and a half from each other, further on. Upon his withdrawing the first catgut, he made his water with more happiness than he had for the last two years. A larger was immediately introduced, and the size of the catgut graduated. Within four days the stream was natural, and he could pass with ease a bougie of any size.—His hour of departure was then come. He was furnished with

with bougies, and also with catguts, in case from his natural propensity to indolence, he should be reduced to apply to them again. This was the state of his case, at least fourteen years ago; and if he had been then only at the pains of now and then passing up a standard bougie, there would have been an end of the case. In the Autumn of 1789, he sent for me into the City, when I found him in very great pain and inflammation. The region of his kidnies was the seat of his complaint. Upon examining the stream of urine, I found it as small as it formerly had been. He had a great discharge from the Urethra, he made bloody urine, his pulse was very strong, and he could scarcely turn in his bed. In this condition also it was expected that he was either to sail, or lose his voyage. A Physician saw him with me; and the conclusion of his case at this time was, that after going into the warm bath, and losing at least seventy ounces of blood, with other antiphlogistic means, he was enabled to go upon his voyage. Whilst he was away, he experienced another attack of his kidnies, which, from the same treatment, terminated also favourably for him. As by this time he was convinced that his own indolence was the cause of all these dangerous attacks, on his return, in the year 1790, the means for opening the obstruction were repeated, that had been put in practice fourteen years before. These were submitted to, soon after his arrival, so that there was time for me to see them carried into effect before he sailed again; and I can say, with truth, that he is now completely well.

CASE XV.

A gentleman, aged twenty-five, had contracted a Gonorrhœa near twelve months before he applied to me, during which time he had been his own Surgeon, and the chief applications which were made use of consisted of an injection, in which calomel formed the basis, and pills of calomel. But as he found that the calomel injection did not stop the discharge, he sometimes had applied an injection with Goulard's extract. I must premise that, during the history of the case, before he applied to me, I could not understand that the inflammatory symptoms had ever subsided; but that this noviciate (for so I am authorised to call him, because he did not seem to be even in possession of that theory which a man of education could readily acquire, in one afternoon, from books) had, in the very face of inflammation, gone on with the calomel injection, for the most part of the time. When I first saw him, I found on him the following symptoms: a profuse discharge, and very thin and bloody; the point of the Penis appeared turgid and red; the orifice of the Urethra had serous vesicles on it, he made his urine with difficulty, his erections were painful, his emissions were so replete with pain, that he felt at the time as if he was cut by a sharp instrument; his stools also excited a similar sensation, he complained of pain in his loins, and about the pubis; he could not bear riding on horseback, and even sitting at table was

creative

creative of great uneasiness. For the first six weeks I was not apprised that he had any obstruction in the Urethra, because he had misled me, by saying that his stream of urine was very full. I therefore contented myself with using antiphlogistic means, but not in the manner that I wished, nor all those which I expect the greatest benefit from. For this gentleman being with his father, and being also of opinion that, if he was desired to apply more than one remedy, it was enough that he made choice of one only, I was forced to do as well as I could, but not as well as I would. In short, I never met with a case that gave me more trouble. I desired him to foment the perinœum with poppy decoction, I gave him the mucilage of gum, I kept his body open with castor oil, and I recommended opiate clysters. But this was not complied with. I then gave him tincture of opium, at stated times. The fomentation was troublesome, instead of which he used the warm bath. His diet I requested to be low, and that he should drink but little or no wine. I found out that he sat up late at cards: this I also desired that he would not do. When I was flattered with the idea that the inflammatory symptoms had abated, I ventured on an injection of the vitriolic fluid; this stopped the discharge whilst he used it, but still the inflammatory symptoms remained. Finding myself foiled, I determined to pass a bougie: but whether it was from fear, or pain, my patient could not give me the opportunity then of ascertaining if there was an obstruction or not. On another day,

fact was proved, that all this irritation proceeded from obstruction. I persisted, as well as I could, with my sedative means, and gave him instruction how to pass a bougie, after having myself shewn him that a small one would pass. This, instead of doing, as I directed, morning and night, he went from home, and did in the middle of the day, in a slovenly manner; for in the morning he wanted to sleep, and in the evening he still played at cards. However, in process of time he was able to bear the bougies up his Urethra, and he was disposed to use them night and morning. His symptoms all abated, his discharge spontaneously stopped, the whole of the irritation declined, and the bougie, which at first came forth dry from the Urethra, at last was covered with well-conditioned mucus. We persisted in the use of bougies, which effected a cure. It must not be forgotten, that, at the latter end of his case, he was also advised to bathe in cold water, and to wash the perinœum, when he did not bathe, with sal ammoniac, dissolved in water.

CASE XVI.

A gentleman, fifty years of age, applied to me with the following symptoms: a discharge from the Urethra of a thin acrid nature, a redness of the glans Penis, and a turgidity and soreness of its orifice: he had pain in his loins, a hardness above the pubis, and a soreness along the perinœum. So irritable was the condition of this patient, that the first glass

glass of wine at dinner was no sooner drank, than he was obliged ever to quit the table, and evacuate his urine. The gentleman had laboured under this complaint for a long time, and being in the habit of indulging promiscuous venery, he thought his case was a succession of Gonorrhœas. He was one of those also who asserted the right which they most undoubtedly posses of doing themselves mischief, for he was his own Surgeon; but being about to marry advantageously, he did me the honour to consult me. For more than a month I trusted to his own report of his case, and, during that time, I found every means that were tried ineffectual. I then requested that he would evacuate his urine before me, which I saw come forth in a small stream, and scattering. I attempted to pass a small bougie, but failed. I then attempted a catgut, but did not succeed in that. I waited for a few days, and applied leeches to the perinœum, sent him to the warm bath, and prescribed him a very low regimen. I had him to lodge near me, and for two more days I was foiled in passing the catgut. On the third day I succeeded, and was so happy also, which is not always the case, to introduce the second soon after the first was withdrawn; his stream of urine improved with the increasing opening, and in a few days we were able to pass up a bougie; but his cure was for a long time retarded, owing to the great irritability of the parts, which we all along endeavoured to keep under by poppy fomentations, and by tincture of opium; the pain in his loins was also very troublesome:

blesome: however, the passage of the Urethra was fairly opened, and my patient married. After his marriage he neglected his case, and in a short time died of an inflammation in the kidnies, out of London.

CASE XVII.

A gentleman, turned of sixty, applied to me for a known obstruction in the Urethra, but was not able himself to pass a bougie. Upon my making the attempt I also failed, but succeeded with the catgut, and as the irritation was not very great, we soon obtained a very fair opening. This gentleman was also of that negligent disposition, that he would not have been at the pains of attending to his state, had he not been about to marry: he did marry in three weeks after his first application to me. The point of this case is found to be, that he can only obtain relief through bougies, but no cure; that he cannot live without them, for very often he cannot evacuate his urine without first of all introducing a bougie.

CASE XVIII.

A gentleman who was very temperate in his manner of living, about fifty-five years of age, applied to me, with a long train of complaints, and I think had gone through most part of the materia medica in his recital of remedies for

for his case, before I was permitted to examine him; and when I shewed him a bougie, and asked his leave for introducing it, he was astonished at the novelty of the attempt, for he had never before seen such an instrument, although he had consulted Physicians for a long time, and had taken the variety of medicines, through their prescriptions, which he had already described, for the cure of this tedious and vexatious case. His symptoms were, a very great tension of the abdomen, pain in the loins, a constant desire to make water, which came from him very often inconsciously; for the immediate cause of his application to a Surgeon, at this time, was a tumour which was advancing in the perinœum. There was also very much induration, which extended as far as the anus. The tumour itself appeared soft, and shining, very red, and almost disposed to burst. I acquainted him with the nature of his case, that his urine was about to pass through where the tumour pointed, and that the event of the case wholly depended upon circumstances, whether he would be got well or not. I attempted to pass a catgut, and succeeded with a very small one. I told him if I could obtain an opening, that his tumour would subside; for which purpose, as one catgut was withdrawn, another was directly passed, and the urine flowed in an improved stream. The tumour subsided, as the obstructed part was opened; and as bougies in increased sizes were introduced, it disappeared: but still there remained an induration in the perinœum, nor would the urine, when an opening was obtained of the obstruct-

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ed part, ordinarily flow without the introduction of a bougie. When the patient solicited to make water, he passed a bougie, and then it would immediately follow it; when, examining into the cause, I found the prostrate gland was very much enlarged. This gentleman has since been often under the necessity of drawing off his water by a flexible catheter, as often as the urine will not follow a bougie as it is withdrawn. Thus it is that this gentleman passes a life as comfortable as the nature of his case will admit, evacuating his water by passing a bougie first, by introducing himself a flexible catheter when a bougie fails, and by living exemplarily temperate.

CASE XIX.

A gentleman, aged twenty-five, sent to me for a large tumour which he had in the perinœum, and which he attributed to having rode a considerable distance on horseback, and very fast. He told me that he had had a Gonorrhœa upon him for three years, which nothing could ease, and that during the greatest part of that time, his stream of urine had diminished, but that at present it came from him as through a dripping stone, which he received in a bottle made of elastic gum. He did not then suspect that the tumour in the perinœum was in consequence of the obstruction in the Urethra. When I interrogated him upon the careless neglect of himself, he replied, that his present state could not be prevent-

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ed, for that bougies had been attempted, but would not pass; and that he had not compounded with the misery of his case, until he had despaired of a cure of it. He was a very active young man, and appeared to be in as good a state of health as the nature of his complaint could admit. I told him, that the tumour in the perinœum was an effort of Nature to convey the urine that way, when it could no longer pass through the common canal. I attempted to pass a catgut repeatedly, but failed. I told him, that the tumour would soon burst, and when the urine had found a passage that way, the inflammation would subside, and then a trial might be made again, of introducing a catgut, with a prospect of more success. The tumour shortly after did burst, and the urine came away in part through that opening, and in part through the Urethra. After making use of every sedative means, and by keeping the patient upon very low diet, I succeeded in passing a catgut, and followed up that success, by constant repetitions, until, in a very few days, I was enabled to introduce a bougie. After I could introduce a bougie, I then drew off his water with a flexible catheter, and taught him to effect the same, so that no urine might pass by the diseased opening. By these means he recovered his natural passage in a most rapid manner, and the diseased opening, without any art or difficulty, healed. And as there was no other provocation for all this exacerbation of symptoms, but mere obstruction in the Urethra, and which a bougie, when introduced and repeated, did remove, so did he get completely well by this treatment.

CASE XX.

I was called up in the night to a gentleman, aged thirty-six, who could not evacuate his urine. He had been ill, and was attended by a Physician. He was lately come from Paris, and had brought with him a powder, which was to form the basis of an injection, for the cure of a supposed Gonorrhœa: but this was not the cause of the Physician's attendance; the patient had a fever, and it was for this that he was sent for. I found him in great pain, and that he had not discharged his urine for the whole day. I passed a small bougie, hoping that the urine, if I could introduce it, would follow. I found a very obstinate stricture in the passage, the bougie passed into the bladder, but no urine followed. I waited and tried again, still no urine followed. I then introduced the smallest of all flexible catheters, made of elastic gum; I passed this, but not without partly withdrawing the stillet. When I had got the flexible catheter into the bladder, I passed up the whole of the stillet, and then withdrew it. His urine followed, but the stream was so small, that it was an hour in the whole of it coming away. I took from him more than a quart of water. By this time the Physician came, who was also sent for. We left our patient with a bougie up the Urethra. We met the next day, when he had had a rigor, and he also had another in the evening. His pulse was very quick, and his mind was disturbed. His rigors were more severe than any

any I had ever seen, but once. We suspected formation of matter some where, but there was no evident sign; he made his urine tolerably well. In four days, a tumour was discovered in the perinœum; and in three after, a large abscess was opened, from the anus to the scrotum: from this issued a great quantity of matter, and it was discovered that the greater portion of his urine came this way also. We then gave him bark, in full quantities, for his fever was at an end, and he was very low and reduced. I dressed the abscess with dry lint, and (according to those admirable directions of Mr. Pott) in a very light superficial manner. I drew his water off by the flexible catheter, and as he recovered, I advised him to do the same in the night time. The obstruction was removed by the daily use of a bougie, and in the course of three months he got completely well.

CASE XXI.

A gentleman, aged thirty-five, sent for me at twelve o'clock at night. I found that he could not evacuate his urine. He had drank that day a great deal of wine, and the wind was cold and easterly. He had an obstruction in the Urethra, of which he was previously conscious, but had never done any thing for it. He did not recollect to have made water since his dinner. He was in great pain; his abdomen was very hard, and his Penis was almost shrunk up, and scarcely any part of the pendulous part appeared.

His pulse was very quick, and he was shivering. I could not pass the smallest instrument up the Urethra. I made him sit over warm water. I gave him opiates in great proportion, both by the mouth and by clyster. He was soon after seized with an incessant vomiting. I began to question him, whether he had before been in this predicament? He told me, that he had not. I asked him, if he usually evacuated his urine in any quantity? He said, that he made as much at a time as he ever did, only that it was longer in coming. I was very glad to hear the last answer which he made me, because I was certain that his bladder was not materially injured. I then told him, that if he did not succeed in making water, in the course of two hours, I should take it from him through a puncture, and that I had not a doubt of the safety of the operation, for that there was not the least danger in the operation itself, it being scarcely to be felt, and, as a French Author expresses it, "like plunging the point of a sword into water." At seven in the morning I made a puncture above the pubis, and evacuated a large quantity of water. I then gave him two grains of opium, a draught of wine whey, and left him to rest. At two, I passed a small bougie, with little difficulty. He had perspired freely, and slept sound. His urine had, during his sleep, dribbled from him. His bladder did not immediately recover from distention. I applied aqua ammoniæ acetatæ to his abdomen, proceeded with bougies, and with them perfected a cure.

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CASE XXII.

A gentleman, aged forty-five, robust and active, a generous liver, but not a drunkard, consulted me for an uncommon complaint. He had been, for more than twelve months, obliged to get out of his bed, sometimes four, five, six, seven, eight, and even nine times a night, in order to allay painful erections. If he lay awake, these did not come on; the pain of them awoke him, and they would not abate until he evacuated a portion of urine. This disease, of course, harrassed him very much for the whole of the time of its being on him. He rose very early, to avoid it, and in lieu of this defect of rest, he used to sleep in his chair after dinner. During the whole of the day, he was as well as any man could be, nor had he these erections when he was sleeping in his chair. I must observe, that these erections were not attended with any lustful passion—his power, and his gratification, in that way, was fair, equal, and proper; and he made his water, during the day, in the usual proportion, and at the usual distances of time. I passed a bougie, but did not find much obstruction, nor were his symptoms truly indicative of such an expectation. But knowing, as I do, that a little obstruction, on a free liver, will be often productive of great irritation, I chose to make that ground good, and advised him to persist with bougies; which he did for three months, until the case was clear, that they did him no benefit. During the use of the bou-

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gie, he had a swelled testicle. I also, during this time, advised him to the warm bath, to the antimonial powder with opiates, and to live abstemious; but that he did not strictly attend to. We then consulted with an eminent Physician, upon the nature and the cure of this singular case; the result of which was, that he should apply leeches to the perinœum twice a week, that he should go to the warm bath twice a week, that he should take opiate clysters every night, keep his body open, take saline draughts with camphire, during the day, live almost upon vegetables, and drink no wine. This system was pursued, with very little infringement, for more than two months. Our patient was lowered, but his disease was not conquered. To live on thus, he could not bear, and when he returned to wine, he was as bad as ever. Even at the time of his abstemious regimen, he was obliged to get out of his bed three or four times in the night. As he had gone on in this low system for a sufficient time to procure all its effect, we were induced to try what a bracing system would do, both in a general and local sense, for which bark was given in liberal doses; and, as it was in the Winter season, a strong solution of sal ammoniac in water (instead of the cold bath) was applied with a spunge about the seat of the disease. In these he found essential service, and with these he proceeded. But if he took the bark, and omitted the sal ammoniac wash, his painful erections returned. On the contrary, if he applied the wash, and omitted the bark, they did not. By a continuance of this system

system for a long time, I do not hear him complain, for I have the honour of knowing him intimately; although I am persuaded, that he is not now quite free of the complaint, and that neglect of his remedy, together with too free living, will provoke a return of it. I was anxious to know what Authors had said upon the nature of this disease, but I have not yet been able to find a similar case; the nearest to it is in Dr. James's Dictionary. It does not come at all under the description of a Satyriasis. The disease appears to me to have been first excited by too constant a habit of uniform free living, and the warmth of the bed provoked an irritation, which habit made familiar. So that the disease, in the end, may be said to be, what Dr. Cullen expresses, a congestion where vessels are dilated and filled, and by which they are stimulated to a stronger action.

CASE XXIII.

A gentleman, aged fifty-eight, had applied to me two years before this last attack, which proved fatal to him.—On the first, I was called up to him early in a morning, and found that he could not make water. I immediately recognised his person, having very often seen him in the streets of this metropolis. He was an active man, and with a strong constitution; his mode of life, in point of eating and drinking, was rather abstemious. I found him in great pain, under a suppression of urine. He told me, that it was a

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case of many years standing, and that he had not (through careless inattention) been able, for many years, to pass a bougie. He said, that he was in the habit of making water very frequently, and that he always made a very little at a time. He had not, at this time, evacuated a drop for several hours. I directly bled him in the arm, gave him forty drops of tincture of opium, and sent him to the warm bath. On his return home, I gave him an opiate clyster, and a grain of opium to be directly swallowed. He fell into a sleep, and his urine came from him whilst he was in it. I endeavoured then to pass a bougie, but failed; and then a catgut, but with no better success. However, by means of the antimonial powder, of opium, of mucilaginous drinks, and of low living, he overcame this exacerbation, and returned to his former state. When I took leave of him at that time, I told him, that, from the long while his obstruction had been on him, his bladder was far gone in disease—that, the only prospect he had, was to relieve the bladder from its distress, by opening the passage in his Urethra—that the most favourable opportunity for it was, when he was freest from inflammation; and to effect this, I left him some catguts, and some small bougies; and I have not a doubt but if he had followed my advice, he would not have died from this cause. When he sent to me the last time, which was also very early in a morning, I found him exactly in the state I had seen him two years before. I directly pursued the same means I did before, and his urine also this time

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came from him when he was asleep. I then also attempted to pass both bougie and catgut, but with no success. I had the mortification to see those I left, two years before, untouched. After he awoke, I requested that the urine he made in future should be put by: this I found to be more than half mucus, which adhered to the bottom of the pot, and so closely united as to form one mass. I found that he did not recover from this attack as he did from his former—that the disease was more pressing—that he had a symptomatic fever—that his return of urining was not without great pain—that his spirits were very low—and that he seemed to despair. His following night was very much disturbed, and his urine mixed with full as much mucus as the day before; his solicitations for the discharge of it more frequent, and his febrile symptoms not abated. Towards the night, he could no longer evacuate his urine, his pain increased, and a vomiting ensued. From this time, that symptom never left him, his pains became worse and worse, his shrieks were heard all over the house. Another Surgeon saw him with me, when it was debated, whether or not a puncture into the bladder could be attended with any prospect of success? The result of our opinion was, that it would not. At the close of this evening, there were evident signs of his approaching catastrophe, and the next morning he expired. Upon examining the state of the parts, the Urethra was found to be contracted for some length, to the neck of the bladder, and the parts in the highest state

of inflammation; a bougie of the smallest size could but find admittance through the contracted part. The prostrate gland was sound, the bladder very far gone in disease, the coats of it very much thickened, and the internal surface of it irregular, and fasciculated—in some parts ulcerated. The kidnies were enlarged, but sound. Upon the whole, I have not a doubt but this gentleman would not have died of this complaint, had he followed my advice when he sent to me the first time.

CASE XXIV.

A gentleman, aged twenty-five, applied to me. I found him in the following situation: he was almost worn to a skeleton, he could make but about a tea spoonful of water at a time, it was evacuated with great torture, and looked at first like starch and water; when it stood for some time, there was a separation of the matter from the urine; after pouring off the urine, the matter would follow, and break off so short as to be poured off also in drops. His voice was so very low, that I could scarcely hear him speak; he was so affected, that he could not bear any one to walk across the room; he had been taking of opiates, in large doses, both by the mouth and by clysters; he had ceased to pass bougies, from the pain which they gave him; and when the action of the opiate was spent, he was so irritable as to break out into the most extravagant bursts of passion. By attending

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to his story of himself, which he seemed very anxious to acquaint me with, I learnt, that he had had a Gonorrhœa two years before, the consequences of which had never ceased—that he had since had an obstruction, for which he had used bougies, and which he could once pass, but that he had cut off from them more than two inches, and of course they were not passed into the bladder, nor perhaps beyond the obstruction. Being a Lieutenant in the Navy, whilst this complaint was on him, he had got wet upon the watch, and his symptoms, from that time, had grown worse and worse. From necessity, therefore, he was obliged to quit his duty, and had been for some time confined to his chamber. He had been attended by an eminent Physician since his return, who had done all for him that he possibly could. I was at once convinced, in my own mind, that this case was incurable; but as Nature will sometimes effect surprising things, especially when the subject is young, I told him that his only chance was, to open the passage, to facilitate the evacuation of his urine; and that probably, the distress being taken off from the bladder, and parts in disease, there might be a chance of their recovery—at any rate, it was right to try; for it is very well known, that the long irritation produced from stone in the bladder, and which brings it into disease, most commonly ceases when the stone is taken out, and the bladder recovers. The patient, at this time, had a very good appetite, and indulged it. He accordingly was reconciled to make the trial. A small bou-

gie did pass, and his urine flowed the better for it; but the pain which he experienced, and some exacerbated symptoms that followed, and which were attributed to the bougie, made him desist, in the hopes of an happier opportunity, for he did not suspect himself to be in danger. His bad symptoms still increasing, his body, if possible, wasting, his appetite at length failing, his pulse quickening, a diarrhœa came on, his urine flowed insensibly from him, and he died. I was very anxious to know the part which yielded, for more than four months, such a source of matter; for I had all along asserted that, from the anatomical nature of the bladder, the seat of abscess, from whence such pus flowed, could not be in the internal membrane of that part. The internal surface of the bladder, whilst that membrane remains, will yield mucus: but I had all along advanced, and contrary to the opinions of others, that the source of so much matter was not there. The sequel of this inquiry will settle the point, and prove that prognostick to be true, which was drawn from the supposition of the bladder not being capable of affording complete concocted pus, and which I mean to be understood in saying, that it is not. Upon opening the abdomen, the first inquiry was, the state of the kidnies; both of which were full of matter, and on the surface of the left, there was an ulcer as broad as a shilling; the ureter of the left, with very little resistance, separated from the kidney, it being also in a state of disease. The coats of the bladder were thicken-

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ed to a great degree, the internal surface of it was fasciculated, and I am certain would not contain a hen's egg. There was an incisted abscess between the bladder and the pubis, the Urethra was thickened and inflamed near to the neck of the bladder, and the prostrate was sound.

CASE XXV.

A strong man, aged fifty-eight, was brought from the Mary-le-bone Workhouse to the Infirmary, complaining of a violent pain in his bowels, which he first felt four days before, and became gradually more violent. From the day after the attack, he had not been able to make water, and said he had had formerly a Gonorrhœa. The Physician to the Infirmary being present, ordered a turpentine clyster to be immediately injected, and a bougie to be passed as soon as the Surgeon came, which was accordingly done, and met with no resistance till it came near to the neck of the bladder. This however was readily overcome, upon making a stronger pressure, and entered the bladder: upon withdrawing it, a few drops of urine only followed. An apperient mixture was now ordered, of which he was to take four table spoonfuls every four hours, till stools were procured; the first dose was almost instantly rejected, and soon after succeeded by a yellow-coloured fluid, similar to fluid fæces, which partook strongly of the smell of turpentine. The vomiting of this fluid continued, with intervals,

vals, for more than an hour, but nothing more was done this day, except that a grain of opium was given him in the evening. The next day there was no abatement in the symptoms, but they were on the contrary aggravated. Two Surgeons of eminence saw him, and made an attempt to pass a catheter, in which after much perseverance they succeeded, but no urine followed, only a few drops of blood, which made them suspect the instrument had not entered the bladder. They did not make another trial, but directed an anodyne clyster, and the grain of opium to be continued in the evening, determining the next day, if no urine was passed in the course of this, to perform the operation for the suppression of urine; but death put a period to his miseries early in the morning. The following appearances presented themselves, on opening the body next day. Upon laying open the cavity of the abdomen, the omentum came in view, very much on the stretch, and drawn downwards, as consequently was the stomach, so as to be in the umbilical region. This, at first sight, was suspected to be adhesion, arising from inflammation, with which the peritoneum appeared to be generally affected; but upon more minute examination, it was found to be strangulated by the abdominal ring, on the left side: upon laying open this part of the scrotum (which did not appear to be much more bulky than that of the other side) we found a considerable portion of the epiploon highly inflamed, and that it was of the congenial kind, being in contact with the testicle, the body

of

of which was very much diminished in its size, and not bigger than a small hazel nut. The epididymis was as we usually meet with it. The peritoneum, just above the spine of the left os ilium, was, with the subjacent muscles, in a gangrenous state. We were naturally led next to turn aside the omentum, and examine the bladder, which was in a contracted state, containing no urine, and its coats thicker than they usually are. The small intestines were considerably inflamed, and small pieces of coagulated lymph were seen floating in serum (of which there was about a quart in the cavity) between them. Turning aside the colon, and detaching the peritoneum, we looked at the right kidney, which had the natural appearance, but was soft to the feel, and its pelvis distended with urine, the inflammation having been so great as to obliterate the canal of the ureter. We then turned aside the stomach, spleen, and colon, to examine the other kidney, when we were astonished at seeing a considerable tumour extending from the diaphragm to below the spine of the ilium, of a white, irregular, lobulated appearance, oblong in its shape like that of a kidney, and hard to the feel. Upon pressing it hard with the finger and thumb, it seemed as if there were a number of calculi grating together, which, as it will hereafter appear, was owing to the ossification of the internal coat. It was dissected carefully out, together with the ureter and bladder, then making an incision into it, upon which a quantity of turbid urine came out, which was collected in a

phial,

phial, and measured thirty-six ounces. The tumour was not examined any more this day, but put by for the inspection of the Physician on the next, when it appeared to be the tubuli uriniferi, augmented to this enormous size. The ureter was likewise considerably enlarged, so as to be about the size of the small intestine in circumference, to within an inch of where it perforates the bladder, where it was of a natural size, and where was found a calculus about the size of a pea, covered over with a brown membrane, which had obstructed the passage of the urine into the bladder. The internal surface of the enlarged pelvis was covered with a brownish substance, the whole of which was supposed by the Physician to be calcarious matter; but, for the following reasons, it was believed to be bone, covered with a thin incrustation of this matter; for when some of it was separated, it admitted of being bent, which calcarious matter will not admit of; it was perfectly white on the side where it adhered to the membrane, was fibrous, and, in fine, was evidently bone. Besides, calcarious matter will not adhere to a membranous surface, and it is natural to suppose that it was ossification, for it is found that Nature commonly, and in most cases of preternatural enlargement of canals, takes on this process as the last she is capable of, as is the case in anurisms. There was not the least vestige of the glandular part of the kidney remaining, which shews that the arteries can secrete the urine independent of it. The resistance the catheter met with, must be attributed to irritation, as the sound

passed

passed into the bladder easily after death. Now to this calculus is to be attributed the preternatural enlargement of the pelvis of the kidney; for by its getting into the ureter, it must have prevented the urine passing into the bladder, as fast as it was secreted; consequently the pelvis must enlarge itself to contain it, and as this became greater and greater, the mere pressure in consequence of such distention, induced the absorbents to carry the glandular part totally away. Had this man at first mentioned that he had a rupture, his life perhaps might have been prolonged, but it is likely he was ignorant of it, as it was congenial, and of which he was never known to complain. The inflammation that ensued from the strangulation of the omentum, throughout the cavity of the abdomen, obliterated the small opening there was for the passage of the urine; for previous to this it could but have flowed guttatim, or had he lived much longer, this calculus would, in all probability, have got into the bladder, but how, (for it might have become the nucleus of a larger one) that cannot be determined. From this case we learn, that we cannot be too particular in our inquiries and examinations of patients, for it was natural to suppose, from the complaints this man made, that he had no other disease than a suppression of urine from inflammation on the neck of the bladder, and as such only, it was treated.

LECTURE XIII.

ON

CHANCRE.

CELSUS his celebrated passage of ars longa, vita brevis, was never more applicable than to the Venereal Disease. The subtilty of the poison, and the interesting concern which it constantly has kept alive, for near three hundred years, render it a subject yet of continued importance, and unabated enquiry. The Venereal Disease is proved to be the most difficult of all diseases to be discriminated. Cases are often suspected to be venereal when they are not really so, and cases are in the event found to be venereal, when they were not suspected to have been so. These mistakes cannot be said to arise out of our want of attention, for to speak generally, there is no one disease in particular which employs our attention more than the venereal; as every practitioner is under a daily necessity of furnish-

ing the mind of his patient with some idea or other of the Venereal Disease, and therefore he is under the necessity of adjusting in his own mind some opinions upon it. It must necessarily be so, whilst he continues to practise; for if he should not inform himself through the pride of obtaining information, he must through the force of necessity; and if he be not a proficient from the love of science, he will become so from the passion of interest.

A Chancre is to be considered as a primo ordo of venereal infection. It is sought for and found on the glans Penis, behind the glans Penis, upon the frœnum, upon the internal duplicature of the prepuce, or externally upon any part of the Penis. As the most general mode of conveying the venereal poison is by a venereal intercourse between the sexes, so are the effects of that intercourse, when either any part of the Penis in man, or labia, vagina, or nymphæ, in woman, is in possession of the venereal virus, imparted from the one to the other. It is by the intercourse of a sound person with a diseased, that the infecting principle is constantly supplied, supported, and conveyed.

The infecting fluid must be parted with, and left to act undisturbed, in order to infect a sound person; for without the medium of fluid, it has not been found that infection can be imparted. Venereal poison does not consist of those fine light and volatile particles, which may be dispersed in air,

air, be carried at a distance, or will speedily pervade the pores, but in order to take effect, it must be by a fluid left to go on in an uninterrupted process. On whatever part of the skin this fluid be lodged, it will there produce a Chancre. The skin of the Penis is as much defended from infection as any other part of the body, and so is the scrotum which is annexed to the Penis, and nearest to it; but these will not be found to be exempt from the action of venereal fluid, as Chancres are not only constantly seen on the external part of the Penis, but on that part of the scrotum also nearest to the Penis. Now it is evident, there is not that difference between the skin on the Penis and the skin on any other part of the body, which will induce the fluid to act upon the one, and prevent it from acting upon the other: it must therefore be obvious, that if any other part of the skin were in the way of receiving that infection which operates upon the skin of the Penis when it is received there, it would have likewise operated upon that other part.

The glans Penis may be considered as a part in anatomy sui generis. The parts behind the glans Penis are replete with small secreting glands, which discharge a sebaceous substance, and this is generally affected according to the state of health; sometimes it will be thin and acrid, at others it will be redundant without acrimony; and sometimes there will be a soreness, with inflammation. The frœnum is a portion of the prepuce annexed to the glans. This and the whole of the internal part of the prepuce, which,

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in the natural pendulous state of the Penis, serves as a covering over the glans, are both either devoid of cutis, or it is evidently thinner on them than elsewhere.

As it so happens that the Penis, which is a part exposed to the first act of venereal infection, is a member made up of various constructions in anatomy, so must the venereal symptoms be absolutely different when the virus is lodged on any part of it; for it must, when it operates, produce symptoms of its action peculiar to that part. Thus will the virus received from another, if lodged only within the commissures of the Penis, produce a Gonorrhœa, when the same virus, lodged behind the glans, or under the frœnum, will produce a Chancre: and thus will the same virus, when lodged withinside of the prepuce, or under the frœnum, or behind the glans, produce a Chancre in a much shorter time, than when the same virus lodged on the glans, the external part of the scrotum, or on any other perfect cutaneous part, will produce a Chancre there. The glans Penis appears to me to be a part difficult to be infected, and I draw my conclusion from seeing that it is not so often the seat of Chancre as other parts equally exposed, but which cannot be more so. The appearance of Chancre most in common is either under the frœnum, or behind the corona glandis; the next in common is withinside of the prepuce; and the rarest appearance is on the external part of the Penis, on parts adjoining to the Penis, on the corona glandis,

and

and every other part of the glans. Sometimes a Chancre will commence behind the corona glandis, and spread upon the substance of the glans; but if the glans be not affected after this manner (and from its contiguity to a part more liable to receive the impression of infection, this may be a cause of its being infected) it will seldom happen that it will be infected at all; although it be in the way of being most infected, as I before observed.

On all these parts, when affected by venereal fluid, the affection will be a Chancre; but if the Chancre be situated behind the frœnum, it will seldom there excite more discharge or inflammation, than what may be said to be in consequence of the Chancre itself. If it be situated behind the glans, amongst the sebaceous glands, or withinside of the prepuce, the former situation will provoke an additional discharge from other parts, the same in their texture, and near to the original seat of infection; and the latter will be swelled, become hard, and thickened, sometimes throughout. When a Chancre is situated on the skin of any part of the Penis, or at the root of the Penis, or about or on the scrotum, the inflammation and thickening of the parts are most commonly confined to the seat of the Chancre itself. But whenever a Chancre appears, and on whatever part it appears, the strongest criterion of a true venereal Chancre does not consist so much in a separate and distinct ocular appearance of a palpable ulcer, as it does in that

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certain thickening of the parts, which, soon after the first appearance of it, is always to be felt. I am so much guided by the feel of the surrounding parts, in my decision, whenever I am to define a suspected ulcer to be a Chancre, or not, that if my reputation were at stake upon a right decision, I would rather trust to my fingers than to my eyes, provided I had my choice of either, but was debarred from the use of both.

I have already observed, that the original symptoms of venereal infection, when it was first discovered in Europe, were not at all changed by time; that the symptoms will always be the same, although an accurate discrimination of them, as the same venereal fluid affected different parts, might not have been so successfully made by our ancestors from want of experience, as they can now be made by us. A discrimination of symptoms could never have been made, with any success, at any time, if the subject which is attempted to be discriminated were not subservient to one general rule, or universal law. The symptoms, therefore, of this disease, or any other, will be the same, as well before we are able to discriminate them, as they will be after. Although discrimination of symptoms cannot alter the symptoms, yet it will not only assist us to understand them, but also, where it be possible, assist us to cure them, or prepare us to submit to the event of such as cannot be cured.

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In the year 1494, the process of a Chancre is thus described by Marcellus Cumanus: "I observed," says he, "many of the Officers and foot soldiers at Milan, whilst I "was in the camp at Navarre, to have several scabs or pus- "tules breaking out on the face, and spreading all over the "rest of their bodies; the first of which appeared usually "under the preputium, or on the outside, like a grain of "millet; sometimes behind the glans, with a small itching; "at other times a single pustule would arise, like a little "bladder, without much pain, but itching also. If rubbed "or scratched, there arises an ulcer, corrosive, and some- "thing like the sting of an ant."

Vigo, in 1513, describes them thus: "The Venereal Di- "sease has its origin in the genital parts, namely, in the "Vulva of women, and Penis of men. There almost al- "ways appear small pustules, sometimes of a livid colour, "sometimes black, and sometimes whitish, with a callosity "surrounding them."

I think these quotations sufficiently prove, by indubitable testimony, that the original symptoms of pustule or Chancre were just the same at first as they are now; and it does not appear that Astruc, who knew more of the Venereal Disease than any Author which is upon record, and who marked, with the exactest accuracy, every particular, has doubted in the least but the symptoms at the

origin of this disease were the same then as when he described them: and his description, which I shall here present you with, will serve to confirm my position.

"Chancrous ulcers, from impure venery, are small superficial ulcers, very little hollow, round, callous, and obstinate, occasioned by the venereal poison, which breaks out upon the pudenda. They are situated, in either sex, upon different parts of the genitals; in men, frequently upon the internal surface of the prepuce, and upon the back or the sides of the glans; most frequently upon the corona or frœnum of the glans; sometimes too, upon the extremity of the duct of the Urethra: in women, oftentimes upon the internal surface of the labia pudenda, very frequently upon the clitoris, the root of the clitoris, or on the prepuce with which the clitoris is covered; upon the nymphæ, carunculæ myrtiformes, and Vagina; nay, sometimes upon the extremity of the Urethra, near the lacunæ. At first there arises a small miliary, red, pointed, hot, itching pimple, the top of which, by degrees, grows whiter, and more even, and at last opens with a small aperture, from whence a small quantity of ichor is discharged. The erosion of the lips continuing, the ulcer increases, is dilated, and forms a small cavity; it is surrounded with callosities, more or less hard and thick; it abounds with a thick, viscid, tenacious pus. Ulcers of this kind differ; first, in situation, for some belong to

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" the glans, as those which occupy its back or convex part,
" or, which is more frequent, the corona; others to the
" prepuce, as those which cover its internal surface, or its
" extremity; lastly, others belong to both, as those which
" are situated here and there upon the frœnum, by which
" the prepuce is connected to the interior part of the glans.
" Sometimes they are few and distinct, sometimes numerous
" and confluent; sometimes they are crouded together, and
" collected in one area; sometimes they surround, with a
" circular ring, the corona glandis, or the roots of the pre-
" puce. Some are of the benign kind, which are round, su-
" perficial, very little callous, white at the bottom, discharge
" a laudable pus, and have lips which are neither red nor
" enlarged; others, on the contrary, are malignant, of an
" irregular angular figure, with a black, livid, dark, pur-
" plish-coloured cavity, have hard, callous, prominent, red,
" inflamed lips, discharge matter rather of an ichorous kind
" than pus, and spread deep and wide."

Thus it appears, that every part of the Penis is exposed to the action of venereal virus. The malignancy or the mildness of chancrous ulcers will depend, in the first instance, upon their situation; and in the second, upon the condition of the parts. If a Chancre be seated either upon the frœnum, upon the internal surface of the prepuce, upon the glans, or upon the external cuticular part of the Penis, the inflammation will not be so general as if

it be seated behind the corona glandis; for if the sebaceous secretions there be fetid, and out of condition, previous to the appearance of a Chancre, it will be provoked to appear with more inflammation, with more thickening of the parts, and with more malignancy and obstinacy, than if these parts were in good condition. The next part where a Chancre is seated, which is also susceptible of additional irritation, is on the internal surface of the prepuce, and this from experience, we find will produce an inflammation so extensive, as to prevent its being slid back, when it is forward; and from being slid forward, when it is back.

It very often happens, that the sebaceous glands yield their secretions so much out of condition, as to inflame every part adjoining to them, namely, the glans and the prepuce, insomuch, that such an appearance may be taken for venereal infection. But although there will be an excoriation and inflammation, in consequence of this state of parts, and sometimes a thickening of the prepuce, so that it can with difficulty be slid back, yet after cleaning the parts, no ulcers will be seen similar to chancrous; the inflammation will be more universal from this cause; but by cool diet, and by washing the parts with some mild astringent frequently, the whole will disappear.

The date of appearance of Chancres on these various parts, from the same contraction, will be different. For the internal

ternal surface will be more readily acted upon by venereal virus, than the external; and the virus will more likely be lodged on each side of the frœnum, and behind the glans —will more likely remain undisturbed within and about the cryptæ of the sebaceous glands, than upon the glans, or upon the external parts of the prepuce and of the Penis. But if, from the same contraction, the virus should find a lodgment on the frœnum and about it, behind the glans, upon the internal surface of the prepuce, and upon the external surface of the Penis, and if the virus should remain on these parts, so as to produce Chancres, they would not appear all at the same time. The first appearance would be behind the frœnum, behind the glans, and upon the internal surface of the prepuce. The appearance of those on the glans, and on the external part of the Penis, will not be until some time after. Chancres, situated behind the glans, are not so dangerous to infect the constitution from the absorption of virus, as Chancres situated on any other part; and Chancres situated on the skin of the Penis, have, in general, infected the constitution at the very time they are first detected. I scarcely ever saw a Chancre on the external part of the Penis, which did not, in spite of mercury, terminate in a bubo; for the inguinal glands have been inflamed at the moment of time the Chancre is first felt, or the virus has past them unmolested, and is already acting upon the constitution.

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With respect to the distance of time from receiving the infection to the appearance of Chancres, it is too vague to be regularly ascertained, or for any general rule to be made out of it. If the Penis, on which the virus acts, be exceedingly dry, and all parts of it, when in a flaccid state, appear palish, the virus will not be found so rapid in its action on one thus conditioned, as it will on a Penis disposed to a redundant exudation of acrid secretion, and which looks red and tumid. The appearance of Chancres after connection, in the former instance, will be six or seven days, or later, on those parts which favour the earliest appearance; and perhaps three weeks or a month, on other parts; and the appearance of Chancres, after connection, in the latter instance, will be in a day or two, on parts most susceptible of infection. But when a Chancre is detected on the external skin of a Penis, where the internal surface of its prepuce and its sebaceous glands yield a redundant portion of acrid discharge, I will not take upon me to say that a Chancre so situated, will from that cause become more malignant than on the skin of a Penis of the former description.

I shall state the condition and appearance of Chancre, under three separate heads; but it must not be forgotten, that the difference in the appearance of Chancre is never owing to a difference in the quality of the virus which produces it, for the virus is always the same, but to the length of time that it has acted, to the difference of parts on which it

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is acting, and to the condition of the natural health of the parts. When an incipient Chancre is detected on the frœnum, as soon as it appears, it will be a little glassy acute sore, smooth and even with the rest of the surface; it will be very sensible of a smarting pain upon being rubbed, or from slipping back the prepuce. And when it is detected behind the corona glandis, at its first appearance, it will be found to have excoriated the thin covering of the part where it is situated, and will appear also either shining, and in a small circumscribed circle, or it will inflame at some distance around it. A Chancre, thus seated, will not be so painful as the former. Sometimes there will be many of them appear, at first very distinct, which will in time run into each other, just as though the virus received, from uncleanliness, had never been washed off, but had acted upon the cryptæ of the sebaceous glands, and had ulcerated the whole of them. When a Chancre is found upon the internal part of the prepuce, it will there appear as a little pustule, such as I have described in my quotation from former Authors. The cause of its appearance always as a pustule, containing a watery fluid, is, that the thin cuticle which covers the internal part of the prepuce is yet thicker than on the frœnum and behind the glans, but nothing like so thick and substantial as the cuticle on the external part of the Penis, but yet thick enough not to be destroyed before a Chancre upon it may be detected. At the first appearance of a Chancre there, it will be found to itch and to smart, and when the

cuticle

cuticle is destroyed, an ulcer will be seen, and the surrounding part will be somewhat thickened.

The first appearance of Chancre, both on the external part of the Penis, as well as on the glans Penis, will plainly point out that which I am endeavouring now to make appear, namely, that one description will not answer for the appearance of Chancre on every part, it will only answer for the appearance of every Chancre on parts of the same nature; and still there will be a reference to the previous condition of the natural health of the same parts. When a Chancre appears on the skin of the Penis, it will be a red, hard, round pimple, with a streak of red leading from it; it will be in this state for some time, and the parts about it will appear at first thickened, but will harden also by degrees. It will sometimes not open, and yield a discharge for a considerable time after it be detected, and I have known it to remain in this indolent state for more than a month. When it does discharge, it will be a thin ichor, and it will be found to shew a strong disposition to ulcerate, and the parts around will inflame and redden. It is neither so smarting as a Chancre on the internal part of the prepuce or on the frœnum, and nothing like so rapid in its progress as those which I have before described. A Chancre on the glans Penis, considering how much that part is exposed to infection, is very rare; I mean a Chancre not connected with the prepuce, but one which arises solely out of the glans.

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The cuticle of it will be raised, and a fluid will be discovered under it, as if it were confined within a bladder; and when this membrane is broken, and the fluid discharged, there will be a red spot remaining, which will in process of time become an ulcer, but it appears to be very slow in its action. The external orifice of the Urethra will be sometimes the seat of a Chancre, and which is also very slow in its action. But Chancres which are found on the corona glandis, are generally chancrous spreadings of venereal ulcers, which began behind the glans, and therefore their origin belongs not to the corona glandis, but to the sebaceous parts behind the glans.

I have given the various incipient symptoms of the first local attack of venereal virus, and I have bespoken your attention to mark, that the virus can only act according to the construction of the part upon which it is found to be acting; that it will act sooner on that part where the sebaceous glands are distributed, than on the external part of the Penis, and that on the intermediate parts between these two extremes, it will be found to act also intermediate; that there will be a greater inflammation at first excited by its action, on those parts most disposed from anatomical situation and anatomical construction, than there will be on parts more difficult and less concerned in receiving the infection at any rate.

If the progressive course of Chancres be left uninterrupted, some parts will be sooner destroyed than others, and

some concomitant symptoms of an alarming nature will be found to await on Chancres in one situation, that cannot happen from Chancres in another, although they may be all contracted at the same time, and although the predisposing constitutional affection be exactly equal in its influence on all. No person who reflects for a moment, can fail of seeing a difference in the effects of a Chancre proceeding to destroy parts about the corona glandis, and behind the corona glandis, and in the effects of a Chancre proceeding to destroy internal parts of the prepuce only, or in the effects of a Chancre proceeding to destroy the frœnum, or in the effects of a Chancre proceeding to destroy the covering skin of the Penis. The action of the first must always be considered not only more serious, as relative to the preservation of the member itself, but the life of the patient may be sometimes at hazard; for if the Chancre has began mild, because the parts on which it is continuing its action, were in a sound state, yet the progress of it will only be lengthened from that cause, it will still proceed, and find its way into the corpora cavernosa, the corpus spongiosum, and the Urethra: so that it will not only destroy the Penis, but in its destruction, sometimes an hemorrhage will be experienced, which will endanger the immediate life of the patient.

This will be the consequence of virus left to act on this part, if the whole of the parts were previously in a healthful condition; but if the parts were previously not in a healthful con-

condition, the symptoms of venereal infection would be proportionably more rapid, more aggravated, and more dangerous, for a mortification might ensue, and the whole substance of the Penis might be destroyed, before any assistance could be brought through medical aid to release it from the ravage of the virus, when connected with a previous constitutional derangement of the parts.

It necessarily follows, where a Chancre on one part will produce all its worst effects, that if it had fallen on another part, the worst effects would have been produced by it there also. But if a Chancre be situated on the internal part of the prepuce, it will only destroy that loose substance; or if it proceed to greater lengths, the whole of the prepuce might be spared without a feel for the loss of it; and the frœnum also might be spared: if it proceed to destroy the under part of the glans, and to penetrate into the Urethra, that inconvenience may be compounded with: if it proceed to destroy the skin of the Penis, it will there be like an ulcer which is venereal on any other part of the skin. The danger from an ulcer thus situated does not consist in the mischief it can locally produce, but in the inevitable power there is, through it, of the virus being conveyed to the inguinal glands and into the constitution. These being not only the partial effects of particular Chancres acting on particular parts, but also the general effects of all Chancres acting on every part, I shall just remark, that before they arrive to

this stage, they will be found to be acting progressively as Chancres, different from each other on different parts, but similar always in their consequences on similar parts.

In the progression of Chancres situated behind the glans, the appearance of them will be like an honey-comb: and if the same Chancre spread, so as to extend to the corona glandis, which is contiguous to it, the appearances on the one and the other will be widely different; for that part of the Chancre situated on the corona glandis will not be so eaten down as the other, and will be apthous at its base, the edges of it will bleed, and they will not thicken like the edges on the sebaceous parts. When the white apthous appearance at the base becomes clean, the cavity, which is made by the destruction of the Chancre, does not fill up, but the loss will be always apparent. In the progress of ulceration, the frœnum will be often eaten away: but that which is destroyed of the prepuce will be again supplied, unless it be of very great extent, and then the prepuce will become so contracted, as to continue to be a stricture on the glans. An ulcer on this part is seldom or never apthous, but of an honey-comb nature, and eating deep into the surface; and so will an ulcer also on the external skin of the Penis.

There is one symptom of a venereal ulcer, which is common to all, and which is particular to a venereal ulcer, namely, that it will be surrounded by a thickening of the parts,

parts, and as long as this thickening remains, so long is the ulcer proceeding to destroy the parts, or the virus is proceeding into the constitution by absorption. Although it be necessary to speak of the whole of the consequences that may happen from venereal Chancres, and although it be necessary to ascertain the utmost local devastation that may be committed by them, yet these appearances do not always occur; when they do, they bring with them enough to alarm us for the consequences, and enough to excite us to employ our attention to guard against them. But this most general symptom of a thickening and induration of the surrounding parts must ever excite our attention, as an object which refers to future constitutional infection; for if the Chancre heal, and a hardness remain, it will either break out again, when the constitution becomes infected, or the hardness will still be increased, as ominous and indicative of a constitutional infection. This symptom, therefore, will always explain, by its presence, that the local infection is not radically removed; and by its absence, that it is. If by embracing the part which was the seat of the Chancre, the appearance be thin, so that the finger and thumb do almost meet, the cure may then be concluded to be perfect; but if a hardness and thickness remain, although it be healed, and if there be a scale upon the part where the Chancre was, then the case must be deemed as not cured, and as requiring much more to be done for it. This observation cannot be too closely attended to; for, depend upon it, a Chancre

cre will sometimes heal, and yet the virus will not be extinguished. It must always be remembered, that the constitution may be hereafter infected, in consequence of that original inattention and escape of that original infection into the constitution.

During the inflammatory state of the glans and prepuce, which is often excited by Chancres, both these parts, as well the frœnum, will be subject to hydatids, or crystallines.—The blood-vessels, in this instance, are not only turgid, but also the lymphaticks become so beyond measure from obstructing lymph, and therefore the lymph will forcibly transude the over-distended vessels, and be collected under the cuticle, in one or more hydatids, or crystalline vesicles, prominent, broad, or narrow, just in proportion to the quantity of lymph collected. These vesicles will be filled with a fluid, thin or thick, pellucid or opaque, according to the quality of that which transudes. If the vesicles remain long entire, at length you will find them become flatulent; for the airy bubbles which are blended with the transuded lymph, as in all other fluids, being by the continued force of the inflammatory heat by degrees let loose, will occupy the space of the limphous fluid which is constantly and gradually evaporating. Every injury done to the prepuce is observed to produce, in some measure, a redundancy of accumulated lymph.

Sometimes,

Sometimes, in consequence of high inflammation, and at other times in consequence of former Chancre, when inflammation has not been high, and even after as much mercury has been thrown into the constitution as can be warranted by the warmest expectations, there will yet remain hard chords, which are indolent and uninflammable. At length they may or may not inflame, become painful, and discover signs of a malignant disposition.—These will be found upon the frœnum, behind the glans, upon the pendulous part of the prepuce, upon any external part of the Penis, and also upon the scrotum. When not inflamed, they will bear touching, and will be found as hard as a dried pea. If they cannot be discussed, they should all be extirpated before they discover signs of malignancy.

Warts or porri are generally found behind the glans, upon the glans, about the frœnum, and sometimes within the internal part of the prepuce; but their chief seat is behind the glans and upon the glans, most commonly beginning behind the glans. They spring up in consequence of porous exudation, and are always the effect of a redundant exudation from the sebaceous glands. They are animal mushrooms, and will be found to grow out of these parts, independent of any venereal infection. The same cause which now and then produces a change in the condition of the sebaceous secretions, by which the whole of the parts will partake of a temporary inflammation, when there are

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no warts, will, by such a change in the condition of the sebaceous secretions, produce a temporary inflammation when there are warts, and then their condition will be aggravating, and productive of much fetor, pain, and inconvenience; for the warts will become moist, will harbour fluid, will increase and turn into a bushy appearance, and will yield a nauseous discharge of fetid mucus, mixed with blood—they will bleed at the touch, cause the prepuce to be slid back with pain and difficulty, almost deprive the possessor of venereal intercourse, and will, to a certainty, absorb any moisture that may come in the way of them.

If they be not originally venereal, they will be sure to become so, whenever a chance of contracting the disease readily offers. I do not know a more difficult, more nice, and more critical state of a case than this, prima facie to be truly decided upon. For a redundant exudation, and that exudation vitiated from any cause, will certainly produce these effects independent of virus; and the linen will be as much stained at times, as if the patient had a virulent Gonorrhœa, in its worst stage. And also when the warts were previously dry, and when they were supported by a little stem, after they have imbibed venereal virus, they will, from that cause, degenerate into a virulent state. They will also, from either a venereal cause, or any other, continue thus for a long while, appearing better by dry repellents, worse from an omission of them, and from high living; for if they be supported by a venereal cause, as long as they continue to yield

so plenteous a discharge, absorption of virus will not be so rapid in it progress, no more than it will from a Gonorrhœa in the Urethra; but these effects will continue to become more and more troublesome, as I have seen, for twelve months at least, and no other venereal symptom has arisen out of them; notwithstanding, at length they have been cured through mercurial friction, when nothing besides could effect it.

I am convinced, it is owing to a plenteous discharge, that, in these cases, other venereal symptoms, when warts are venereal, do not so readily appear. As long as a discharge continues, unchecked by repellents, and a long as no mercury has been at the same time administered, I do not think other symptoms will appear; for whenever absorption does take place, either from this original cause, or from a Chancre, the discharge from either will no longer be yielded with freedom. In a Chancre the discharge will be considerably baulked, the Chancre will remain stationary, hard, and dry, and if it spread, there will not be a proportionate discharge to its increasing size; or if it heal, the process is never found to be kindly, there will be an ungracious malignant hardness remaining, and a short time will make apparent and confirm the lurking suspicions which such insidious symptoms excite.

I am therefore confidently of opinion, whenever Chancres have made any progress on any part of the body, and

wherever they may be found, that every endeavour should be used to promote their discharge, knowing, that if the venereal stimulus be wholly occupied upon a local part, it will be found less at leisure to be absorbed into the constitution, and that the constitution may be preserved wholly by its action being all thus locally engaged: time may be gained by the administration of mercury to perform a cure by thus radically extinguishing the virus on the local part, and by preventing it from ever being insinuated into the constitution.

I have already stated, that the distance, in point of time, of the appearance of Chancres upon parts, when they are all contracted from one connection, will be according to the nature of the parts on which they appear, and on which the process of them is proceeding; I have stated, that the coverings of some parts are thinner than the coverings of others; and that the thinner a covering of any part is, the sooner will virus lodging upon it, destroy it, thereby producing an earlier Chancre: I have also stated, that parts concealed, and parts the least in the way of being rubbed upon, are parts the most liable to harbour and entertain uninterruptedly the venereal fluid, so that its action will be more generally found, to a greater certainty, in a shorter space of time, and in a more multiplied degree, on such parts, than where the fluid has found a lodgment by accident; and where, when it is found to act, it is also proved to be an uncommon

common occurrence, and which is to be considered as a circumstance out of the ordinary way in expectation.

When we find many Chancres surrounding the parts behind the corona glandis, we see the natural consequences which may be expected from a promiscuous connection, and from an omission of washing them; and even when those parts are washed, we often afterwards detect a lurking Chancre behind the frœnum, because the washing was slovenly done. Some virus might have been left also upon the skin of the Penis, and which, when it be not washed away, will lay the foundation for a future Chancre there. But if from the same connection, there should be virus lodged behind the glans, and virus lodged also upon the skin of the Penis, and if the virus act effectually on both these parts, I will take upon me to say there will be a distance of many days between their appearance; for that on the Penis will be long after that behind the glans, before it be discovered, if the eye could be possibly as intent upon the part where a Chancre will be, as it can where it has already been detected.

I have before stated, that the first local attacks of venereal virus, are only so far confined to the Penis in man, and the privities in woman, as the usual mode of contracting and carrying on the infection is through that channel.

There are abominable symptoms from abominable practices, which may also arise, but upon them I shall be silent. But there are other channels by which the infection will find its way from one subject to another, for it is the local mode of infection that I am proceeding to treat upon.

These I shall now take into consideration, not for the purpose of informing you, but for you to recollect, that venereal fluid received from another subject, will produce local venereal effects upon other parts besides those of generation; for that important truth is too obvious, too commonly known, to be considered as a new truth, and too well recorded in conjunction with a train of such continued, severe, and exacerbated symptoms as cannot otherwise be found throughout the whole of the history of cases, which have been given of the progress of the Venereal Disease.

My present purpose is to prepare your minds, to engage your attention, and explain to you one of the most important theoretic points in the nature of this disease, which in my opinion has been for many years offered for discussion, and which is—that venereal fluid produced through infection imparted by another subject, will be harmless to the subject who secretes it. That venereal fluid thus produced, neither generally nor particularly, will ever act to the prejudice of that subject whose secretion it is; when

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it appears and when it is known, that venereal fluid of one subject by being imparted to another subject, and by there remaining so that such fluid may take effect, is the only means through which the venereal disease can be propagated, and can be supported in society.

I shall, for the further elucidation of what I am hereafter to offer, make a few observations upon the liability of every part to receive a local infection of the venereal disease by other means than venereal intercourse betwixt the sexes, when by any extraordinary manner the venereal fluid of one subject is conveyed, and left to act upon another.

In the rare case which I gave of a Gonorrhœa of the eye, and where the eye was lost, it evidently appeared that it arose from accident, and that it was produced through the application of venereal fluid of another subject to that part; for the subject who lost his eye through it, had knowledge of a woman half an hour before he went home, and when he came home, washing his eyes, because they were weak, as he had been in the habit of doing, with his own urine, he by that means imparted to his eye some of that venereal fluid which was imparted to him just before by the infected woman. The eye began to inflame before the venereal stimulus was discovered in the Urethra, and each Gonorrhœa after kept its own course: and although both of his eyes were washed, as both were weak, yet only one of them

them became thus infected, because none of the fluid of the woman, by being all consumed on the eye infected, or by some other failure in the action of the venereal fluid, took effect on the other eye.

Venereal fluid, imparted from another subject, has the power of producing a Chancre upon the skin; and when it is there lodged, and remains uninterrupted, it does produce it. In general, after examining women, or delivering women, who have a Chancre, or a Gonorrhœa, the hands are washed: it does not follow, therefore, that that which is possible to happen will happen, because the hands are generally washed, and the adhering virus is by this act removed. But when there has been a cut or a sore upon the finger, in that case, notwithstanding the washing of the hands, and notwithstanding the insignificance of the cut or sore, for if it were important, it would certainly be from that cause bound up, the subtilty of the fluid has taken in many instances effect, a Chancre has been produced in consequence, and that Chancre has been seldom known to be cured, without a more decisive action of mercury, than where the infection hath been contracted in the usual manner, and on the usual parts. I know four practitioners in midwifery, who have experienced this condition, and the case of one of them became so desperate, as to baffle all art for near seven years; when, during that time, all the medical men of this and other countries were employed to it.

A tooth

A tooth in transplantation, which was selected for its beauty, and for its soundness, and which to the individual who first possessed it, was to all intents harmless in its original situation, and which although the subject who originally possed it, was venereally infected, would have never, as a venereal tooth, been productive of ill effects to that infected subject who was the original possessor; yet by this same tooth being transplanted into the socket of another subject, the local venereal infection was immediately spread, a general ulceration of the surrounding parts did follow, and the Venereal Disease was sooner constitutionally imparted in this instance also, than in the common and ordinary mode of its being communicated.

I mean to infer from what I have advanced, that by the local application of the infected fluid of one subject so remaining upon another as to have an opportunity of taking effect, it will always take effect on any part—that it will from the law of poisons be a necessary consequence. And I mean to infer, as I shall explain hereafter, that the local fluid of Gonorrhœa or Chancre of a subject thus only locally infected either with the one or with the other, will not nor cannot venereally infect any other part, to which it can be applied, of the same subject. In other words, I mean to say, that venereal fluid, so far as it refers to the same subject, and so far as it may be suspected to be capable of acting upon, and of producing either Chancre or Gonorrhœa,

rhœa, or both, when conveyed to another part of the same subject in such a manner that it would, if it could produce them, will not produce them.

Gonorrhœal fluid on the same subject, secreted by the subject, never produces a Chancre on the same subject; and chancrous fluid on the same subject, secreted by the same subject, never produces a Gonorrhœa.

Those who from constant observation have declared, that the original cause which produced a Gonorrhœa and Chancre was not the same, were led to this opinion from observing, if they were produced by the same virus, the natural consequences, in their opinions, must have been, that chancrous fluid, when it was within contact of the Urethra, would generate Gonorrhœa; and that the continued lodgment of fluid from a Gonorrhœa upon the glans, withinside of the prepuce, and around the corona glandis, must to a certainty produce at least those symptoms which are found to be produced from one single connection held with a venereal subject, so locally diseased as to impart them. The fact is certain, that the one does not arise out of the other; but the manner in which they attempted to account for it, was not the true one. Their observation, that one symptom did not arise out of another upon the same subject, was right; but their denial, that the first cause of the two symptoms were the same, that they originated from the same

same fluid, was wrong. They reasoned to account for an evident and palpable truth upon false premises. Their candour would not permit them to withhold the fact, although they could not rightly account for it. They saw a truth in common occurrence, which they wanted to explain, but could not explain.

Others seeing that the fluid from Gonorrhœa did not produce Chancre, naturally concluded, because it possessed not the power of producing any local effect, no constitutional infection could be imparted from it. Even amongst those who admitted, for instance, that the infection from both was the same, some of that opinion could not be brought to believe, that the constitution could ever be infected through Gonorrhœa, because they found it did not produce Chancre as the fluid passed out of the Urethra and lodged on the parts without the Urethra. For, say they, if this fluid possesses a power of infecting, prove to us, that it locally does so, before we are requested to grant to you, that it can, or does constitutionally infect? And for this reason, a Gonorrhœa has been pronounced by them to be harmless in its nature, to be alike to any other inflammatory attack, having no relation to virulence, for that it could not produce another venereal local symptom, and in the end cured itself. But fortunately for the truth of my theory, some of that opinion to which I last alluded, have proved to their conviction by experiments, that gonorrhœal and chancrous

fluids are from the same origin. As my theory is also founded upon the same basis, if it stood in need of any support from them, I am certain their usual candour and alacrity in promoting the truth, from whomsoever it comes, will not reluctantly be bestowed on this.

It was the want of rightly discriminating, that the venereal secretion of a subject was inactive upon the same subject, that it was a harmless secretion to the subject secreting it, which induced modern theorists to go into erroneous venereal experiments—and where these experiments have been tried, the result from them has, from the same want of discriminating, involved the question into yet more doubt, and yet more difficulty, and which nothing but the true knowledge of this theory could ever have solved, and this does plainly solve it.

Where these experiments have been tried, the result of them has not been accordant with intention, but dependent upon accident; where gonorrhœal fluid has been taken from one subject and inserted into another, there a Chancre has been the consequence; but where gonorrhœal fluid has been taken and inserted into another part of the same subject, there no Chancre would or could have been produced by it—no more than it can or does produce a Chancre anywhere about the parts of the Penis from the natural and continued flux of the gonorrhœal fluid from the Urethra.

Those

Those who tried these experiments with the idea of ascertaining and confirming that Gonorrhœa and Chancre were produced from the same virus, were more at a loss than ever—were wider from the mark in accounting for the reciprocal innocency of these fluids, when found in their natural action upon the same subject—for their experiments brought them to confess, that the same fluid does indiscriminately produce both symptoms, and that the distinction between them consists only in the anatomical distinction of the parts on which the fluid is secreted. They of course knew this fact, but still the result of their experiments was at variance with the natural progress of both fluids upon the same subject. I have ever condemned this mode of ascertaining facts. An experiment made upon a principle in error, can only be an echo of that principle in error. It appears from this instance, that experiments in their result ought to be with caution listened to—for as these were originally founded in error, as they were injudiciously planned from the very beginning, whether they succeeded in proving the two fluids to be the same or not, that success depending upon the accidental and precarious circumstance of the experiment being made out of the fluid of the same subject, or that of another, they could not have ever been successful in fixing their fact, nor in proving why gonorrhœal fluid and chancrous fluid were harmless to the same subject who secreted them. For the experiments which prove gonorrhœal and chancrous fluids to be the

same, are not calculated to account for the innocent action of both upon the subject who secretes them. There cannot be the least doubt but they were induced to make these experiments solely because they saw that gonorrhœal fluid did not produce Chancre on the same subject; but in their attempt to explore the true cause, they offered a violence to Nature, and the common order of the case, by referring to another subject for the proof of that which was particular to the same subject, which other subject, in this instance, ought to have been totally out of the question.

If they had followed the dictates of the natural action of the poison, if they had watched the effects, if they had traced effects up to the cause, and if they had scrupulously informed themselves of those which in a common course of the natural operation of the poison were the most obvious and natural occurrences—this would have been the result of such an attentive and watchful observation.

They would have found from examining the woman who infected the man, after repeated proofs from repeated facts, that the two fluids were the same in their nature, and that the distinction between them was only a difference in the action of the same poison on different anatomical parts.—They would have also been convinced beyond a doubt, that gonorrhœal fluid on the same subject would not produce a Chancre, and that no one Chancre would produce another

Chancre

Chancre on the same subject; but that if there be one or ten Chancres upon the same subject, and upon a variety of anatomical parts, if some appeared early and others late, every one of them were contracted and produced from the fluid which was imparted by the infecting subject. And further, they would have been convinced that a Gonorrhœa was never found to have been produced from the fluid of Chancre on the same subject. They would have seen a Chancre making its way upon the glans into the very aperture, eating away the commissures of the Urethra, and no Gonorrhœa ever produced in consequence.

By thus watching the natural action of the poison, and its natural and relative progress, they would have seen the extent of the power of it in the strongest light. By departing from this natural action, and this natural and relative progress of the poison, and by going in consequence into outrageous experiment, they turned themselves out of the only path which could have conducted them onward in the way to truth.

They have all along avoided meeting the whole of the question fairly, for those misgiving skepticks have been only seen to doubt one half of the question; they have all of them confined their doubts to Gonorrhœa being the same in origin as Chancre, that Gonorrhœa would ever produce a

Chancre

Chancre after any manner whatever, because no Chancre broke out in consequence of Gonorrhœa upon the same subject. But they were never heard to say a syllable upon the other half of the question, which is much the strongest in the confirmation of my theory, namely, that one Chancre on the same subject will not produce another Chancre, nor will it produce a Gonorrhœa.

But now it remains for me to place the truth of my theory in a more conspicuous view, if it be necessary or possible, than I have yet hitherto done. I have shewn, and I trust the fact is satisfactorily established, that if the disease be imparted from one subject to another, from a subject infected to a sound subject, it is only necessary that the fluid from the infected subject should come into contact with any part of a sound subject, that thereby suffering it to remain undisturbed, a possibility for infection will be given—and by this means it has the power of acting even upon sound skin. But I do not exact so much as this in my postulatum. I only desire your acquiescence to the belief of common facts, that from a cut or sore upon the finger, practitioners in midwifery have been infected in consequence of their examining women who were pregnant, and who at the same time possessed one of the two symptoms, or both Gonorrhœa and Chancre. And I desire your acquiescence to the many fatal instances, to the fatal ravages which have been committed in consequence of the local venereal action from transplanted teeth, which

teeth

teeth to all appearance were sound and beautiful in the sockets of the original possessor, and which teeth might have been shifted from one socket of the original possessor to another, without bringing forward any baneful effects by the bare removal of them. I trust you are in want of no further arguments to convince you, that the venereal poison may be propagated by inoculating a sound subject with the venereal fluid of a subject locally infected; and that it is from the want of opportunity only, and not from the impossibility of the case, subjects are not thus more commonly infected. But in the observations which I am now about to prefer before you, it will be apparent that the opportunity is always present, but that the ability never is.

If it were possible that our own venereal secretions could infect ourselves, how few young men or women could escape such action, when the gonorrhœal and chancrous fluids are discharged in such profusion for weeks or months, and when the patient is in the constant habit of fingering them? Where a pimple or a cut may absorb them? Where that which is received by the linen would infect, and where it would, to a moral certainty, if it were possible, how does it come to pass that we find no instance of it upon record? Where the same fingers which handled these parts are applied to the lips, the nose, and the eyes? Where, from the slovenliness of some, and the poverty of others, the same handkerchief is often employed to receive the discharge,

and

and to wipe the nose, the eyes, and the face? Where are the instances in consequence of this unwary indulgence of the experiment, if that any consequences of other infection, on other parts of the same subject, could be produced through it? I say for myself, I never saw infection fairly transferred after this manner; and I never read or heard of a case where it was clearly proved to be so. But if the infection could be transferred in this manner, who could escape infection on other parts through the infecting fluid discharged for a continuance of time?

These arguments are referable to the same unwary indulgence of the infection of not only gonorrhœal and chancrous fluids, but also of the fluid from a bubo. I have rubbed these fluids on sores of the same subject over and over again, and never have been able to trace the smallest effect from them.

Having explained my newly-discovered theory so far to you as was necessary to convince you of its truth, in a local sense, and as it is confirmed by the relative innocency of the two fluids of Gonorrhœa and Chancre on the same subject, I shall reserve what I have more to say upon this theory for the occasion which calls for it, and where it will also plainly tend to confirm and illustrate, in a constitutional sense, the natural action of the venereal poison.

LECTURE XIV.

ON

CHANCRE IN WOMAN.

WOMAN, as the counterpart of venereal connection with man, is exposed to similar venereal affections, but from the different structure of the parts, the same symptoms on woman will not be attended by such violent effects as they will be on man. The simplicity of the construction of their parts, and the parts being more exposed, together with the general temperance of a woman, prevent her being subject to those dangerous consequences which are often experienced by a man. No Chancre any where on the private parts of a woman can produce such effects as a malignant Chancre on the Penis of a man. But a woman is not exposed to a Chancre so frequently as a man, and it is not found so much in common with her as Gonorrhœa. There are two reasons for this—the one is, that the parts within the labia are secreting surfaces, and are more disposed to take on the symptoms of Gonorrhœa and to be inflamed, when the Vagina with her

is infected; and the other is, that the Penis deposits most commonly the infecting fluid beyond the parts susceptible of Chancre; for when a woman has a Chancre it is generally on the labia, although sometimes on the outer part of the nymphæ. A Chancre may also be situated where the perinœum divides to form the labia.

The nature and relative importance of a Chancre in woman, as referring to constitutional affections, being exactly counterparts of the same complaint in man, I shall not detain you longer upon this head. A Chancre neglected, or a Chancre acting upon habits predisposed to aggravate venereal symptoms, will be found to become desperate and malignant, not from any difference in the virus, but from a difference in the constitution on which it is acting. Nothing is more common to be said, and nothing can be more founded in ignorance when it is said, than the observation which we often hear—that because a man finds he has a malignant set of Chancres, attended with all the aggravating circumstances possible, a woman who infected him must necessarily also possess symptoms equally at least, if not more malignant. This is not necessarily so; a woman might, from a very small discharge, and when there is no inflammation, infect a man, who will be, in consequence, as severely attacked as it is possible for him to be. And this is just also in analogy with the Small-Pox—one may be inoculated from the fluid of another who died of it, and the one inoculated

oculated may have the disease in the mildest form. It is generally owing to the idiosyncracy of the individual who possesses a disease, therefore, and not to the subject who imparted it, whether a disease be mild or malignant, rapid or slow, mortal or safe. And independent of this idiosyncracy, it may also be owing to the bare condition of health; as from intemperate habits, sometimes the symptoms will be more severe than they would otherwise have been on the same subject at another season, when he had not committed intemperance.

What I have lately so much dwelt upon, namely, that our own venereal secretions are harmless to ourselves, holds as good, and is as strongly confirmed by referring to Gonorrhœa and Chancre in woman, as it does in man, and if possible it is more confirmed, for the discharge is more diffused, and more lodged about the parts; and the skin of a a woman is much thinner and softer than that of a man. Now, if from one single introduction of a wounded finger of a Midwife, the disease can be imparted to another, or if from one single venereal connection of a man, the disease, as it is, can be imparted—how much more certain would the constant flux of venereal discharge, during a Gonorrhœa, produce a Chancre on the same individual, on any part where the fluid may be attached, if it were by the law of Nature possible to be effected?

Astruc, the very indefatigable and very ingenious Astruc, has never lost sight of any circumstance which has occurred to him, either from personal observation, or from that of others. And Astruc says,—"The disease is contracted by the "introduction of the finger or hand into the place contami- "nated by a venereal ulcer or efflux; by examining, for in- "stance, venereal ulcers with a finger, or discharging the "midwife's office towards an infected woman, especial- "ly if the Surgeon's finger, or midwife's hands have any "wound or fissure, by which the poison may be more "easily imbibed."

So Anthony Gallus relates, that he knew a midwife, who by delivering a woman ill of the French Disease, was taken with it herself. So the Surgeon who translated Charles Musitanus into French, in his notes upon the fourth chapter of the second book, has produced two cases of the same kind, and so well attested, that both of them seem to be unquestionably true. So James Varcellinus, in his Treatise de Pudendorum Morbis, et Lue Venereâ, declares, that he knew a youth, who being afraid of having to do with an infected woman, only wantonly griped the pudenda with one of his hands, and yet that hand swelled very much, and he afterwards broke out all over in pustules, so that he had like to have suffered from a confirmed disease, for a very transitory enjoyment, if he had not called in the physical art to his assistance.

A Surgeon

A Surgeon of eminence in this metropolis invited me to a sight of his patient, whom he is now attending for a venereal ulcer on his right fore-finger. The patient is a tooth-drawer; he had a sore upon his finger, which became venereally infected by drawing the tooth of a man who was constitutionally infected with the Venereal Disease. These instances have been, and are so common, that if I were to go about collecting the whole of them, the labour would be infinite.

ON

BUBO.

FROM the acquired knowledge which has been so eminently displayed by more ancient Anatomists, and from the additional acquirements which have been obtained by the ingenious and indefatigable labours of more modern Anatomists, a more accurate comprehension of the course, distribution, and terminations of the absorbent or lymphatic system in the human body, has proved of the utmost importance in the study of the animal œconomy, and in the practice of Surgery. The true cause of Bubo, as referable to venereal infection, has been since rightly defined, and properly understood, and more especially since the lymphaticks have been proved to be a system of absorbing vessels. Surgeons and medical professors who have since written on the Venereal Disease, have availed themselves of this newly promulgated discovery, and additional light is thrown into their theory.

The doctrine of lymphaticks being a system of absorbing vessels, every where dispersed upon the skin and the internal surfaces of parts, brings more assistance with it to lead us onward, not only to account for the progress of the Venereal

nereal Disease into the constitution, but also for the progress of its antidote (mercury) into the constitution, and thereby affecting it, by the same channel, by the same method of conveyance. It is in reality by the absorption and progress of venereal and other poisons, that the doctrine of lymphaticks has been farther confirmed. The lacteals were discovered, traced, and their use ascertained, from the circumstance of a manifest and particular colour in their contents, upon some occasions at least. We had not the same advantage with respect to lymphaticks; but in them, what we could not trace by the eye, we have found out by the effects of venereal poison. We know, from observation, that this poison may be absorbed on any part of the body, and thence diffused throughout the constitution. We must suppose it absorbed by the same vessels that absorb its antidote mercury, or any thing else that is carried into the mass of blood by absorption. Many things, of a more inoffensive nature, pass unobserved; but this poison, from its inimical, irritating, and destructive quality, is disposed to raise a distubance in its passage, before it reaches far enough to mix with the blood. Hence the lymphatic glands, through which every absorbed fluid must pass, are so often affected by venereal poison, when it is proceeding onward with its contagious effects into the constitution.

If infection be received in the common way, a Bubo is found on the groin, because the lymphaticks of the genital parts

parts pass through the inguinal glands; but if the infection be received on the hand, a Bubo, for the like reason, is formed in the arm-pit; and when the disease is communicated by the lips, the glands of the neck inflame and tumify.

In perusing Authors on the Venereal Disease far back, we find their ideas of the theory of Venereal Bubo quite obsolete, from want of this knowledge. But the doctrine of lymphaticks was not unknown to Astruc, as it has been unfairly said to have been by a late modern Author: and although it may be possible that Astruc did not apply the doctrine to account for all circumstances which occur in this disease, but if he had so applied it, a more clear idea would have been formed by him; yet because he erred in some particulars, it must not therefore be inferred, that he was ignorant of the system, and of the application of it altogether. Mr. Hunter should have recollected, in common with us all, that we shall ever owe a tribute of praise to this wonderful Author, for the most laborious and most ingenious history of the origin of the disease, and of the symptoms, that ever was written; and so far from any modern Author going beyond him upon these points, on the contrary, without him, without our referring to him, the whole of his historical account would have been no where else found in one point of view, for he collected it from every where. We feel ourselves impelled by gratitude towards Astruc; and as Mr. Hunter has chosen to produce quotations from Astruc,

Astruc, to prove he knew nothing of the lymphatic system, I shall not hesitate to declare, he does not merit such an implication of ignorance; and in proof, I will produce a quotation from him, plainly shewing that he did know the true theory of a Bubo, that theory being well known in his time, being publicly taught at the foreign universities, of which this great man did not fail to avail himself.

" Lastly," says Astruc, " the venereal poison is introduced " by transmission through the lymphatic vessels, when, " the virulent particles being insinuated through the pores " of the parts into the lymphatic vessels which are spread " upon the skin, are carried, together with circulating " lymph, into the nearest conglobate glands, and there ex- " ert their power. Thus, tumours in the inguinal glands are " apt to follow upon impure coition, Chancres in the pri- " vate parts, or a slow-discharging Gonorrhœa; so swel- " lings in the maxillary and parotid glands succeed venereal " apthæ, or ulcers of the gums, tongue, palate, or jaws, in " such as have contracted the disease from sucking or kiss- " ing: so tumours of the axillary glands are apt to attend " venereal chaps, fissures and ulcers in the nipples, part of " the poison being carried from the private parts to the in- " guinal glands, from the inside of the mouth to the paro- " tid or maxillary, and from the nipples to the axillary " glands, according to the laws of circulation, which the

" lymph observes." It is not possible for more truth to be expressed in fewer words: not only a perfect knowledge, as far as he has gone, of the system of lymphaticks appears to be familiar to him, but also the knowledge of its being a system of absorbing vessels; for he gives to the lymph its own law of circulation, and he tells us also, and which is very true, that, from ulcers only locally contracted, the glands will swell, and not from ulcers which arise out of that constitution which is already infected; but that the absorbed virus, from ulcers locally contracted, by passing to the first glands belonging to the absorbing vessels, will inflame and irritate such glands, and such only as stand in the way of its passage into the constitution. If Astruc has not availed himself of all the knowledge which this new system could have led him to, and has not applied the beneficial purposes of it in accounting for many points which were but badly accounted for without it, if Astruc has erred on this side, if he has not pushed the new idea far enough, others since have erred on the contrary side, for they have, from the knowledge of the theory, applied it to a wrong purpose, have abused it, and laid the foundation of a wrong practice from that cause. This shall be hereafter explained to you.

Buboes are found on the inguinal glands from a venereal cause, which may be either Gonorrhœa or Chancre; and also when neither Gonorrhœa nor Chancre has previously appeared; when there is no trace, from the minutest examination

amination of any one first infection to be found on the Penis, neither Chancre, Gonorrhœa, warts, nor any other primordial symptom; and in this instance, a Bubo may be said to be a primordial symptom, when in the other instance it may be said to be only a secondary symptom. A Bubo is a symptom which proclaims, that the virus has passed so far in part only (as Astruc observes) from the Chancre or Gonorrhœa with which it originated; for the virus has certainly in part only passed, or the Chancre would be disposed to heal; but so far from a Chancre healing, it will never, by any act, take on such a disposition, until it be cured by mercury, but in that instance it will often heal, when its Bubo does not. A Chancre will become stationary, as the virus is absorbing, and it will remain so for some time when the groin becomes affected, neither changing for the better or worse. The discharge from it, in that case, will be small, and as if it were unwillingly yielded. I have seen a small abrasion of the cuticle of the glans remain stationary until such time as two Buboes came to abscess, but from the effect of mercury the red spot on the glans has disappeared, and this was the only symptom which I could detect.

We are often told that a Bubo may be produced in the groin from other causes besides venereal; and we are also told, when the Venereal Disease is the first cause which produces it, that a Bubo, may notwithstanding be not venereal. There are such doctrines. The foundation up-

on which one of these assertions is built, is from a knowledge that glands may be irritated by lymphaticks being irritated, and that glands may be also irritated by lymphaticks being wounded, and that this may come to pass, independent of any influence through either venereal or any other poison. The theory is undoubtedly true, and it is also true, that we very often see obstinate tumours in the maxillary glands of men, women, and children, from what is termed scrophula; that we see the glands in the groins tumified and tender only from a wound in the foot, hip, or thigh; and that we also see the sub-maxillary glands swell and come to abscess, from a whitlow on; a finger but in all my practice, I never saw the glands in the groins yet come to abscess without a venereal cause. I mean not to deny the possibility of the case, but I mean by this to exemplify the rarity of it. If these possible cases, because they are barely possible, be constantly lugged into observation, whenever we are treating upon venereal Buboes, it will be like a truth told out of season, misguiding and confounding exceptions with general rules If I saw two Buboes in the groins, and could not discover any other assignable cause for them, it would take me some time to be satisfied that they were not venereal, unless there were a moral impossibility against their being so.

I have said, it has been also asserted, that a Bubo may not in the second instance be venereal, when in the first instance

Gonorrhœa

Gonorrhœa and Chancre were apparent. This idea was first propagated as an ingenious apology for that obstinate fact, which it seems to confirm, of the sameness in the nature of the poison which produces Chancre, and which produces Gonorrhœa; and also as an ingenious apology or escape for those who agree that the poison is the same, but who deny that Gonorrhœa can produce a venereal Bubo, alike partaking of virulence as that produced by Chancre. Both these opinions being so contradictory to the common progress of the disease, and so irreconcilable to the common nature of it, that they who adopted them, observing the fact that Buboes did arise in consequence of both, were therefore obliged to assign another cause for the appearance of one sort of them, namely, that it was in consequence of sympathy, and that such a Bubo did not possess any venereal power of infecting the constitution.

A Bubo sometimes is produced in consequence of an infectious embrace, without any previous first symptom. In this we all concur; this knowledge has obtained universal assent; and in this instance, a Bubo must be considered as the primordial attack, and for the provocation of which there was no other first symptom, neither Gonorrhœa nor Chancre, neither a Gonorrhœa to invite a Bubo from sympathy, as some are pleased to call it, nor a Chancre to provoke a true venereal Bubo. Then will not that be thought curious argument, which grants that a Bubo may be truly venereal, when it is not produced by any primordial symptom

symptom, and which denies that it may be truly venereal, when it is demonstratively produced by a primordial venereal symptom? Which grants that a Bubo is from a venereal cause, when no prior symptom leads to the confirmation of it, but denies that it may be so when a prior symptom does confirm the fact? Which is not believed to be venereal when the first cause, which was venereal, palpably indicates a second to be so, but which is believed to be venereal, when the first is a hidden cause, exposed to conjecture and uncertainty, and dependent upon faith, which in this case is the only evidence upon a circumstance unseen? This is that sort of refined logical reasoning, which, I own it, I am not as yet prepared to receive for true argument, it not being sufficiently resembling to plain matter of fact. A very ingenious young Surgeon consulted me in September, 1791, for a Bubo which he had on each groin, in consequence of an infectious embrace. These Buboes came to abscess, in spite of every attempt to discuss them; and these were the first and the only symptoms which he had. Two very important points are illustrated of my theory by this case. The one is, that it must have been the infecting fluid of the woman which was absorbed, without leaving any local impression; and the other is, that sympathy could have had no other concern in the case, than having brought, in the first place, the lady and gentleman together.

Irritation does certainly, from many causes independent of venereal virus, tumify glands in connection with parts thus

thus irritated; and glands will also feel the force of this influence in common, from the pain only of Chancre, and from the pain only of Gonorrhœa. But there is nothing so very excessive in the pain of a Chancre, or in the pain of a Gonorrhœa, considering it abstractedly from all relation to virus, as to provoke such uncommon inflammation in the inguinal glands, as to bring them to abscess, and sometimes in consequence become the seat of the most obstinate, the most continued, and the most dangerous effects that can be adduced by the action of venereal poison.

Glands which are affected from evident common causes of irritation, as evidently subside when the first cause is done away: this is the consequence of irritation on glands from common obvious causes. But glands inflamed by the absorption of virus from a Chancre, grow more inflamed by the abatement of the original symptom which gave rise to the inflammation on them, at least the original Chancre does not increase, as the inflammation increases on the glands, but it seems to be niggardly sparing that virus to the glands, which cannot be comprised within the limits of its own action. I therefore, for this reason, do not hesitate to pronounce, with the fewest of all possible exceptions, every Bubo which comes to abscess, or that is with the greatest difficulty dissolved without coming to abscess, by a thorough decisive mercurial influence exerted upon it, to be truly venereal, to be truly possessed of the presence of, and under the immediate influence of venereal virus.

rus. In order, therefore, to clear my ground well as I go, I shall say that I take no farther notice of such Buboes in the groins, which disappear spontaneously, when the irritating cause which provoked them to swell abates; but that my object for consideration are Buboes, which from venereal virus remain sometimes hard and uninflamed, or which come forward to abscess; such Buboes which owe no relative obedience to any influence of any common stimulus independent of virus, which will remain progressively advancing; and such Buboes which will become more and more formidable, which will to all moral certainty impart the infection to the whole of the constitution, and which cannot be weaned from the constitution by any general mode of treatment appropriated to glands diseased from any other cause—these are the Buboes which I am to define, and such only are produced by venereal infection.

It is always an object which will have its use in theory, and of course also in practice, to know how every local symptom of the Venereal Disease will eventually appear, and how it will eventually terminate, when left to proceed, without any check or controul from mercury. And from what I have seen in these instances, it appears to me, that if the virus, by being left to its own uninterrupted action, proceed through the lymphaticks, from a local infected part, onward into the constitution, the force of its effects will not operate so strongly on local parts, as it will when it is

not

not with so much facility absorbed into the constitution.—For, in the first place, if the virus from a Chancre be absorbed, and the glands of the groin swell, that Chancre will not proceed with so much rapid malignancy as it had done before; and if the glands of the groin remain tumid and hard, yet uninflamed, it is most probable that the virus is passing from thence into the constitution; for whenever the constitution is discovered to be venereally infected, the glands of the groin are found to be more or less enlarged.

But as every subject who contracts the Venereal Disease is not aware of the whole of the symptoms of it, as he only knows them from the whole of the consequences which the disease produces, as sometimes the venereal virus will pass from the Penis through the absorbents, without leaving any impression upon it, or if it does, it will be such an impression as will excite no attention from pain, or from such palpable appearances as will attract regard; so will the absorbents conduct the virus sometimes to the inguinal glands, from whence, with a very small sense of pain, it will find its way into the constitution, and shew itself in an unquestionable form, either by corroding ulcers in the tonsils, or blotches on the skin. This is known to come to pass, and when it does, if the Surgeon refer to the glands of the groin, he will find them more or less enlarged, although that circumstance was not before known to the patient.

I consider the lymphatic glands distributed upon the surface, in the light of barriers to the constitution, as centinels which let into the constitution every thing that ought to pass by that channel for the good of the constitution, without the least restraint or difficulty, and without betraying any disgust whatever; but if any thing poisonous be offered to pass through that channel, and by which the constitution can be materially affected, if any thing attempt to pass through, which should not, because the constitution would in consequence suffer from it, an alarm and an annoyance are first expressed by the lymphatic glands, they become inflamed, and the sense of the injury which the constitution would receive, is imparted and forwarned by these glands, before the constitution ever receives the injury. But whether the cause of diseased glands be known or not, whether their disease be accounted for either from the action of venereal poison, or from a hidden disposition in the constitution, it is found by experience, which the result of practice demonstrates beyond a doubt, that glands, when diseased, owing to their anatomical structure, are not so easily cured, or reasoned upon as other parts of a different anatomical structure. There is nothing from analogy to that which is the best method of restoring other diseased parts of other anatomical descriptions, which will assist us in the cure of diseased lymphatic glands. As they are proved to be the glands of a system of absorbent vessels, so have they

they their own peculiar diseases as well as peculiar functions, and so will they require a peculiar treatment for the cure of them: what that treatment ought to be, I am at a loss to define. I feel a deficiency in the art, which I cannot well supply; for if a gland be hard and scirrhous, and if the cause be venereal, mercury does not always restore it, although mercury will restore every other part which is diseased from venereal virus. I am almost ashamed to confess the little ascendency which surgical or physical art has over obstructed glands.—A discovery of the cure of lymphatic diseases has not kept pace with the discovery of the system of lymphaticks. Anatomical researches have, in this instance, taken the lead of medical improvements.

I have one more remark to offer upon the subject of the lymphatic system, which is, that it is found to be more active in the young than the old—that when the subject is young, he is more exposed to Buboes than when he is old—and that when the subject is youthful, these glands are more conspicuous than as he grows old—and that as the skin shrinks, and becomes loose, the lymphaticks and the glands seem to have done their offices, and become in some degree obsolete.

I shall forbear to enter more particularly into every circumstance attendant upon hard and scirrhous Buboes, and

more especially upon Buboes, which, when they come to abscess, assume the most dangerous appearances that offer in the whole of the history of the Venereal Disease, until I come to consider the Cure of them.

LECTURE XV.

THE ACTION OF THE VENEREAL DISEASE ON THE CONSTITUTION.

I HAVE already endeavoured to impress upon your minds, that I wish to give as pure and natural an history of the progress of the Venereal Disease, as I possibly can, independent of any interference of mercury with it—that I wish to observe its progress, and to observe upon the future effects of the disease, when neither disguised, checked, or obscured by any interruption which might be given to it, from any knowledge that the disease was actually acting on the constitution. Opportunities for such observations offer very frequently after the disease is gotten into the constitution; for, as it often comes forth from the constitution, when it is not in the least suspected, and as the symptoms which are produced from a constitutional infection are various, as they are not so familiarly known to the patient who suffers them, as local symptoms are, as they are often experienced at very remote periods, as the patient, during the

the progress of them, may be so situated, that from ignorance of the symptoms of the disease, the true nature of it might not have been discovered by him, or from the poverty of the patient, he might not have been able to avail himself of the advantages of professional assistance, so will the disease be often found to have been acting upon the constitution for a long time, and to have demonstrated all its effects without any controul by mercury, and without any disguise whatsoever in the process of it.

Such are the effects which I am now about to consider, which I also shall watch and remark upon, not only to explain to you the nature of them when they appear, the parts on which they appear, and the symptoms and complexion of them, but I shall also watch and remark them, as they may have a relative and immediate concern with the original virus, by which they were produced, and as they are the result of a general infection on the whole of that constitution, on which they are acting.

It has been already asserted, that it is by absorption of the original virus, the infection is conveyed to the constitution; that sometimes it is found to pass into the constitution, without making any impression upon that part which receives it from the subject which gave the infection; that sometimes it is found to pass from the local impression of Gonorrhœa, from the local impression of Chancre;

Chancre; and that sometimes it is found to pass from the local impression which it first makes on the inguinal glands.

As this is the natural progress of the disease in the first stage of it, an opportunity fairly offers for me to remark, that it is most probable the absorbents are commencing to convey the infecting fluid into the constitution at the very first period it comes within contact of them. But that on anatomical parts of a certain description, this process is more retarded than on others; that a Gonorrhœa in the Urethra is not so disposed to admit the absorption, as a Chancre behind the glans; that a Chancre behind the glans is not so much disposed to admit the absorption as a Chancre upon the skin of the Penis; and moreover, that when any of these local symptoms are apparent, the virus may not be absorbed from them alone, but it may be also absorbed where it has been lodged upon other parts, and on which parts it has not left any impression. The last observation which I made, and which goes to assert, that the virus received from another subject sometimes proceeds into the constitution, and there produces constitutional venereal symptoms, without having made any local impression, has never been denied by any Writers upon this disease, and therefore it stands upon record as a fact, which nobody dreams of disputing.

The importance of this truth, upon reflection, is more than we are aware of. It proves, that the first local symptoms,

toms, when they do appear, of any denomination, are not necessary for infecting the constitution; it proves, that the secretions excited by these local symptoms, have no necessary share in infecting the constitution, as I before remarked; and it also proves, that it is the original virus, which, being absorbed into the constitution, actually produces the venereal effects; it proves, that the constitution, both locally and generally, is only acted upon by the foreign fluid, which is imparted by another subject, and that it is the actual pursuit of that foreign fluid, which, pervading the constitution, is productive of all the venereal affections which arise out of such infection.

It is not only always the foreign fluid received from another subject, which infects the subject who receives it, but it is also the permanent action of this foreign fluid which supports the disease, and keeps alive its existence as long as it remains unextinguished. The fluid which is produced by the local action of the foreign fluid will be, to another, that which it was to the subject who gave it. It is therefore self-evident, that as our own secretions do not interfere, so that gonorrhœal fluid shall locally produce Chancre, or vice versa, no more will the progress of the foreign fluid, when absorbed, be either influenced in its action, be either advanced or retarded, be in any ways affected from the fluid, barely as a fluid which arises in consequence of a Chancre, Gonorrhœa, or Bubo, on the same subject. But all future venereal constitutional consequences depend totally

tally and solely upon the actual absorption of that venereal fluid which was imparted from another subject; and that so far as it refers to the subject who possesses them, the symptoms of Gonorrhœa, Chancre, and Bubo, are only arguments of the foreign virus being present, and acting upon these parts. But if the fluid of either be imparted to a third subject, it will act on that subject with the same power, as the fluid which was received from a prior subject acted upon the subject who is infected by it, and who imparts it to a third subject.

C is infected by the impression which the venereal fluid has made on some part or other of *C* locally, or constitutionally. This venereal fluid was imparted to *C* from *B*. This fluid to *B* was a fluid produced in consequence of the venereal action of a foreign fluid which *B* received from *A*. The fluid secreted by *B* is the effect of the stimulus of the foreign fluid which *B* received from *A*; and to *B*, as a fluid secreted on a local part of *B*, through the action of the foreign stimulus received from *A*, that fluid will be harmless, will be merely the result of the action of the foreign stimulus communicated from *A* to *B*. But when *B* transfers this fluid to *C*, which was secreted by *B*, in consequence of the action of the foreign stimulus imparted by *A*, this fluid so transferred from *B* to *C*, will produce similar effects as a foreign stimulus acting on *C*, as that fluid which was transferred from *A* to *B*, produced on *B*; and so will the system

of infection go on from *C* to *D*. The fluid which *B* secreted, must therefore be considered as a secretion proper to *B*, and therefore harmless to *B*; but as it was produced from a poison imparted by *A* to *B*, so does *B* transfer that poison, the same in its nature, to *C*, which *B* received from *A*.

The importance of this truth will solve most venereal phenomena, and lay them open to plain comprehension, with as much facility as it has, by the knowledge of it, explained already the sameness of the nature of Gonorrhœa and Chancre; and by this we know, they cannot interfere on the same subject. As the controversies which have been caused, from the phenomenous appearance of these two local symptoms, failed in the true explanation of the matter of fact, so did every attempt which has been offered, in every essay which has been written upon the subject, fail not only of satisfying the judgment, but did also induce theorists to wander from the very path of truth, induce them to lose sight of the actual effects arising from the local action of the disease, induce them to deny a self-evident truth, that Gonorrhœa was a symptom of the Venereal Disease, and induce them to found new controversies upon the errors arising out of a wrong definition, and upon a misunderstanding of the first fundamental points which were attempted to be explained, but not with success equal to the attempt.

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In order to prove the truth of a theory, there should be an accordance in the whole of it with the symptoms of the disease upon which it is founded. If that theory, which seems to explain one part of a disease, in one particular symptom fall wide of the mark in explaining another part, and another material symptom, and if, in consequence of that want of power of explanation, a part of a disease which such theory could not explain, be slovenly passed over, or designedly be unnoticed, depend upon it, the theorist himself considers it not well founded in truth. Such a theory ought not to be persisted in, it ought not to be countenanced; by beginning in error, it will admit of no change for the better. A true theory will go on in a constant parallel with the symptoms it is to describe. Not a link of the one, nor a link of the other will be at variance. The whole will be in perfect coincidence. But the most material of all mischiefs arising out of a wrong theory being pushed to any length, is, that the most obvious and the most positive symptoms, and more especially of the Venereal Disease, have been attempted to be set aside through it, because these could not be brought to conform to that theory avowed by its theorist, in accounting for the natural progress of venereal infection. The disease, in short, must be what the theorist says it shall be, instead of the theorist saying what the true nature of the disease is.

Before I proceed to describe the symptoms of the Venereal Disease, and to prove them by the evident effects of the disease on the constitution—before I proceed to restore that credit to former Authors, such as Astruc, and others, who must be always amongst the foremost, whenever we make our appeal for what does really happen in consequence of the progressive action of the Venereal Disease—before I proceed to display my theory, which is so simple as only to confirm observations on the real effects which the disease is capable absolutely of producing—I shall lay before you one general rule, which must be considered as an axiom, as a truth which pervades the whole of the theory I shall hereafter enlarge upon, and which must be considered as the only basis of it—That when the foreign virus which was imparted by another, is absorbed into the constitution, and when the constitution is confirmed in the infection, which I define it to be, when any outward visible signs of that confirmation appear, and which outward visible signs may be ulcers on the tonsils, blotches upon the skin, opthalmia of the eyes, ozœna of the nasal membrane, ulcers on the interior part of the œsophagus, gummata or deep-seated cavernous sloughs on the aponeurotic surfaces of the muscles, nodes upon the periosteum of any bone, caries of any bone itself, &c. —when one or all these symptoms do appear, or if it were possible to know what the disease was, whilst a previous symptomatic fever was on, and before the cause of that symptomatic fever was known to be venereal, by none of those

venereal

venereal symptoms having yet appeared, I should not, even in that instance, hesitate to declare, that the constitution was completely possessed of the influence of the foreign virus, and that, in all such cases, any or the whole of the fluids taken from a subject infected, will impart the infection to a sound subject, provided it were so applied to that sound subject, as to give the fluid an opportunity of taking effect. It matters not whether the fluid appear to the eye in a sound wholesome condition; it does not depend, for infection to take place, whether that fluid which is expected to act effectually, be taken from an ulcer on the tonsils, or any other part of the constitutionally infected subject, and which has not the immediate signs of venereal infection; I do not assert, that the fluids from constitutional venereal ulcers, are the only medium by which a subject, so conditioned, can impart the disease; but when the subject who imparts it, is notoriously infected with it, I do assert, that all the fluids, all the secretions are capable of imparting the disease to a sound subject, provided the full opportunity be given, that it may act as it can act, and provided it has the power of being left to do all that it can, and to produce what effects it can, by its action, when engrafted on another subject.

Another point I must also recall your attention to, but which need only be recollected, because the truth of it is self-evident. Nothing is more common than to see an infected

infected subject, who by having had some of those symptoms, which I lately stated to be venereal, has undergone a course of mercury, has been imperfectly cured, and the symptoms have, in consequence, disappeared for a short time, but yet the disease has shewn itself afresh: in this instance also, in this intermediate space, when the patient is clear of ostensible symptoms, but when the constitution is not clear of real infection, the fluids of the infected constitution are capable of imparting the disease, under the same prescriptive regulation which I have in the instance before exacted.

If the theory which I am about to confirm, by the self-approving natural occurrences in the history of venereal infection, wanted strength for its confirmation from analogy, I could support it by the effects of the bite of a mad dog; and so far as the general law of the action of animal poisons is in accordance, it may, for that reason only, be justly appealed to. A mad dog does not become a mad dog because his saliva is possessed of the power of conveying canine infection, but the saliva becomes possessed of the power of conveying and imparting the infection, because the dog is mad; the dog being mad, his fluids possess the morbid influence; there is no more change in the appearance of the saliva, than in the appearance of any other fluid within the dog. A transplanted tooth, taken from the mouth of a subject constitutionally infected, and whose

whose blood is venereally tainted, and selected, as I observed before, for its perfect beauty and soundness, on which, nor on the sockets of which, not the least trace of the disease can be seen, by such tooth being applied warm and moist to the socket of a sound subject, it will bring rapidly forward the morst confirmed symptoms of the Venereal Disease. Neither the saliva of the mad dog, nor the tooth of the diseased subject, can be said to be apparently under any change from the disease of either, although in reality both are in full possession of all the poisonous influence which they are found to impart.

These broad outlines of the action of venereal poison, which I have given, intirely correspond with not only the natural progress of the venereal poison, but they are warranted and supported by every Author who has written on the disease, unto the time of Mr. Hunter's publication, who took upon himself to oppose them. Whenever I read an Author upon the Venereal Disease, I try to find out what his object may be in writing, and I give him my credit, or not, accordingly. When I read Astruc, I do not expect from him an improved method of cure, nor do I read for that purpose—I expect from him no more than a true plain narrative of the action of the venereal virus, how it is imparted from one subject to another, and what is the result of its action. I expect to find in his book a zealous account, founded upon the most incontestible arguments, of the whole

whole of the natural progress of venereal infection, and of the whole of the forms in which it may be communicated. As he devoted his attention to this particular object—as his publication, when it came forth was open to the eyes of all Practitioners—as he had the full opportunity of availing himself, both as a man of letters, and a man of the highest estimation, not only of that knowledge which Authors of every country could furnish him with, not only of that which the result of his own inquiries illustrated, but also of the cases of other Practitioners who lived at his time, and who partook of his friendly conversation, upon his favourite topic, at the time he was forming his Treatise on the Venereal Disease—so does he deserve full credit for what he has said on the action of the poison.

It must be confessed, however Authors may differ about their theory of diseases, the symptoms of them will be, notwithstanding, always the same; and however opposite theory may be in two different Authors, the true nature of diseases, and the natural progress of them will always be the same. Any idea which may be formed, does not, nor cannot alter the natural course of a disease; but if a true idea be formed of it, that will assist to account for the most leading occurrences in a disease, and will also assist in the cure of it. At the same time, I must impress upon your recollection, that, whatever the venereal poison from its nature can effect, must be independent and prior to every theory

theory about it; and that the most ignorant man upon earth can proclaim a disease, when he cannot account for it. Who is there in this country that does not know the Small-Pox? And at the time when it was undetermined, whether the cool or hot treatment were preferable, when the opinions of men were changeable, the disease could never change. True theory, which, in other words, is an enlarged idea of the nature of any disease, has wrought a melioration in the symptoms, but not changed the nature of the disease.

Astruc has attributed to the Venereal Disease as broad an outline, as great an extent of power, and of natural action, as I have. Astruc has laid down every form by which the disease can be communicated: from the incontestible proofs of cases which came within his own knowledge, and from the natural accordance of venereal action, he established his theory upon the truth of observation. These new observations which I have subjoined, I am able to declare, tend to confirm the assertions of Astruc, tend to place the natural extent of the disease in a strong point of view, tend to fix such a standard of the true extent of power which the disease possesses, that no theory will hereafter diminish, because it cannot, without palpably forsaking the truth.

When the venereal poison has taken full possession of the constitution, not only the parts which indicate the di-

sease to be venereal, by their apparent morbid vitiations, but also every part of the constitution is venereally vitiated. This vitiation of the whole of the constitution is not brought on from any absorption that may take place of that discharge which is produced by the local action of the foreign virus, but it is the absolute action of the foreign virus alone, which, by being absorbed, brings it on. My reasons for this position I have before stated at large; they are, because the foreign virus will pass into the constitution, and infect it without any local action. The local action of it, therefore, is no sine qua non of infection. And in the next place, the absorption of our own discharge, being an absorption of a harmless fluid, it can neither act for or against the constitutional infection. I shall not hesitate to assert, that the venereal infection, from its nature, is not so limited as Mr. Hunter declares it to be; and I shall prove, that from want of knowledge of this leading first principle, namely—that our own secretions cannot infect ourselves—many of his experiments which tend to limit the natural action of the disease, and to narrow the power of it, and that in the very face of cases to the contrary, were founded in error, and could not, from that cause, demonstrate either the force of the disease, or its true natural action, either on the subject who possessed it, or on one to whom it may be imparted.

I shall now present to you my pure opinion, and support it by facts drawn from the history of cases, of that which I understand

understand to be the power of action of the Venereal Disease, both as to the subject who possesses it, and to the subject to whom it may be imparted. And in these leading points, I shall be found to differ totally from Mr. Hunter. In all these differences I beg to remind you, that where I do not prove him to be wrong, there I wish him to be believed to be right; for I do assure you, I am impelled from no other motive, than that of the exalted esteem I have for the truth and honour of our profession. Foreigners have condemned his theory, and perhaps they may think it contains a catechism of the general opinion which we, in this country, entertain of this disease. It is therefore incumbent upon us, from that cause, to be more alert in not implicitly yielding to delusive impressions.

I need not remark to you, to bear in your minds, that whatever is possible, may not always come to pass. No one ever expects that possibilities should be, in every instance, realised. But you must be aware if a disease has the power, that is all which is exacted by a possibility; and that is the sense which I conceive of what may be possible. I therefore say, that all and every part of the constitution is vitiated, when the avowed symptoms of the Venereal Disease are apparent, and when they are not apparent, from having been obscured through mercury. And I do in consequence say, it is possible that a subject thus infected, will get a child in whom the venereal rudiments will grow, with eve-

ry other natural quality which is imparted to the child from its father. I do say farther, that the mother of a child venereally infected, will produce a child venereally infected; and that these occurrences are very common to those who seek for them in this populous place. There are two cases of the Small-Pox, which are strong in analogy to this, if this fact wanted such for its support. I will point them out to you; the one is the most incontestible that could be adduced. It was a case which occurred to Mr. Lynn, Surgeon to the Westminster-Hospital. A child was born on the twenty-second day after the Small-Pox had appeared upon the mother, and the child, at the time of its birth, was covered with distinct pustules all over its body. Some pus was taken upon a lancet from one of these pustules on the face, and with this lancet another child was inoculated in both arms. The infection began to appear, on the seventh day, on each arm. On the eleventh day, the child sickened, and on the twelfth, the pustules, of the distinct sort, appeared. Another similar case is recorded in the London Medical Journal, vol. iii. part 2d. page 294, and which was given by Mr. Jenner, of Painswick. But all Authors have asserted such to be the natural power and the extent of the Venereal Disease, excepting Mr. Hunter; and our countryman, Wisman, has given us instances of it, in his folio edition, page 491.—And so has Astruc.—

I shall also assert, that chaps in the lips may be the vehicle

hicle of infection, by kisses received from a subject who is constitutionally vitiated—that the repeated venereal intercourse of a sound person with a person whose constitution is venereally infected, will impart the disease—and that the disease is often produced this way, when, upon examination, nothing is seen about the private parts of a woman to warrant a cause of infection; when at the same time she may have, on other parts, strong proofs of constitutional infection. The following is a case in point.

A gentleman of fashion applied to me, in May, 1791, with ulcers in his throat, and which had been there for some time. His constitution seemed to be very much deranged, his rest was disturbed, his pulse was quick, his skin was parched, his appetite failed, his spirits were sunk, he was grown thin, and his mind was indolent. Upon looking at the tonsils, I began to question him, whether he had any indicative symptoms any where else? and found his prepuce thickened and horny, it could not be slipped back, a profuse nauseous discharge was yielded of the sebaceous secretion from withinside of the prepuce, and the inguinal glands were distinct, enlarged, and knotty. I then made known to him the certainty of his case being venereal. He then told me, that for more than nine months, he had only connection with the same woman, and who also had at this, and for a long time before, a sore throat, and some spots on her skin; but that since he had taken her to himself,

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she had neither a Gonorrhœa nor Chancre. I examined the woman, upon whom I found no other symptoms but those already related: but there was an induration on the labium, where a Chancre had formerly been, and the glands of the groins were enlarged. The fact also was confessed, that she had been taking mercury at times, so as to baulk the progress of the disease, but not extinguish it. It was the absorption of her vitiated secretions which infected the gentleman. Both submitted to a regular process of cure, and the symptoms of both were completely eliminated.

I also assert, that a nurse infected, may impart it to a sound child who sucks her; and that an infected child may impart it to a sound nurse, by sucking her. Venereal vitiations, from these causes, constantly offer. And those who approve of Mr. Hunter as a theorist, will not so far pin their faith upon him, as to become indifferent about the state of a nurse who is to suckle their child. Nor would Mr. Hunter himself deny his attendance, as unprofitable or unnecessary, from the strength of his opinion in the impossibility there is of this or any other disease being thus imparted. For at the very time when his theory taught him there was no danger to be dreaded, from the knowledge he had of the power and extent of action which the Venereal Disease was capable of, and did possess, he hesitated not to examine the condition of girls who parted with their teeth, to be transplanted into another mouth, although,

though by that theory he gave us to understand, from the nature of the virus, and from the nature of its action, no danger could arise, if the girl were infected, for it was impossible to come to pass, that, at any rate, such infection could be conveyed.

I am also convinced, as much as I ever was of any fact that ever came before me, so much does the venereal vitiation pervade the whole of the constitution which possesses it, that even the blood of one infected being engrafted upon a wound of another, which other was previously a sound subject, will most completely impart the disease, and will impart it with more rapidity, than if it were introduced by the usual mode of infection, namely, through the absorbents. Upon this point I shall dwell a little, and if I do make this apparent, if I do so impress it upon your senses as to become willing converts to my belief, if the proofs which I bring to support my assertions, be believed, if the cases, which are from every where collected, be not forgeries, then must it be admitted, that every fluid of the constitution of one venereally infected, does virtually and actually possess a venereal vitiation; and that venereal vitiation, although not in the least apparent to the eye, is capable of imparting the infection to another, when properly applied for that purpose; even, I say, if there be no local venereal affection upon the part apparently, from whence

whence the fluid be taken, and which does convey the infection.

In the days of Astruc, modern refinement had not gone so far as to tempt beauty to purchase, for the sake of ornament, a tooth out of the mouth of a poor girl to be placed in her own, and to supply the place of one more ugly, or one that was decaying. That refinement was reserved for our time: and as good, in the common round of human events, is often found to arise out of evil, so has this new practice, from the unsuspected evil consequences which have been found sometimes to attend it, furnished us with a stronger insight into the hidden and occult nature of the Venereal Disease, and the power of its action, than all the cases that have ever before occurred, and so has it given us such an insight into the true theory of it, that if it were not pursued to advantage, the cause would be in our lethargic supineness. If this new truth be ever forsaken, if it be not established to the satisfaction of those who seek to know the true nature and extent of venereal action, they will surrender an opportunity, which, through every other known means, through all the other various cases, will not afford them so ample a scope for ascertaining the true nature and extent of the disease.

I shall repeat to you a strong case, which has already made some noise in the world, and produced an alarm in the minds

minds of those who are interested in safety or in danger from transplanted teeth. This remarkable case has already appeared three times in print, by the late Sir William Watson, by Mr. Hunter, and by myself, in my third part of Observations upon the new Opinions of the latter. There I have been more explanatory upon the case, for that reason I shall only now barely state it, and refer you for the comment.

In the year 1784, a lady aged twenty-one, was attended, at the day appointed, for transplanting an incisor tooth on the upper jaw, at the desire of a dentist, by Mr. Hunter. He examined the girl from whom the tooth was to be taken, inspected the tooth when drawn, and delivered the tooth to the dentist for insertion. A little uneasy sensation continued for two days; at the expiration of that time, the sensation was found to increase, and to such a degree, that the lady applied to the dentist for relief. The dentist having examined the tooth, 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 increasing; 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 eventually ending well; but begged, in case of its growing worse, that she would come to town, and be in the way of constant at-

tendance. This was accordingly complied with, and she remained at the house of the dentist for ten days. During this time Mr. Hunter saw her, and found the edges of the gum beginning to ulcerate, and to which he applied lint, dipped in a strong solution of corrosive sublimate. At the end of ten days the lady removed from the house of the dentist to Knightsbridge, and the late Mr. Pott was sent for to her, who applied something to the ulceration, gave her bark in decoction and substance, and also opium. The tooth which from the beginning had never fastened, was now out of her mouth. There was a consultation between Mr. Pott and Sir William Watson. The ulceration continued to spread under the use of bark; it had extended along the palate towards the uvula, and the maxilla superior became a caries. She had a symptomatic fever, and eruptions followed it; some of these formed into ulcers, and those on the scapula and the knees were corroding and cavernous. Nodes appeared then upon the bones, on the radii, and also on the tibiæ. She had exquisite nocturnal pains. She went through a complete course of mercury; it was administered in the form of pills and of unction, and the whole of the symptoms yielded to the action of mercury. She was removed into Hampshire, the place of her nativity, and died shortly afterwards in a hectic state. Mr. Pott never hesitated to declare, that if ever there had been a venereal case more strongly marked by true

true venereal symptoms than any other—this was that case.

But I must observe, that the proof of the virus being thus conveyed, is not confined to this case, nor are you obliged to pin your faith upon the question, which I call a question at issue, relative to the extent of power of venereal virus, from this instance alone. I say you are neither confined to this case, nor to the opinions of those who have seen it or decided upon it. There are other cases gone forth into the world, from other parties and other channels, all of which yielded to mercury, and upon which there was but one opinion also, namely, that they were truly venereal.

I have at length closed my argument upon this question, and I have been studious to place it before you, that you may rightly and fully comprehend it. It is a question of the highest importance, for if it be true, that the venereal virus thus pervades the whole of the fluids, as I say it does, and as all Authors have said who preceded Mr. Hunter, from seeing avowed cases which came before them, why then we must be the more awake to its consequences, as we shall by this theory fix the power of the virus to be much more highly important, and much more to be guarded against, than that limited power of it which is asserted by Mr. H. And it is the more necessary that

the question be determined, or at least that attention should be paid to these cases, which offer through transplanted teeth, as our knowledge of the extraordinary power of the action of virus can be only ascertained from extraordinary cases. These cases shew what the virus can do, when they do happen, but an opportunity does not often serve for us to ascertain what the virus can do from extraordinary cases. We all know what the virus can effect, when we see a Chancre upon the Penis, because this is the ordinary mode of communicating the disease. There never was such an opportunity offered as this from transplanting teeth, and at such an enlightened age also, to ascertain the exact acmé of venereal power: and I, for my part, have no more doubt but that the virus may be thus conveyed, as I have stated, than I have of my present existence. We have been ever taught, until the publication of Mr. Hunter's Treatise, to be aware of the subtilty of the poison. We have read many cases from former Authors of its breaking out without knowing when or how it was caught. We have the accounts of children being infected from a nurse by sucking, and of the nurse from children. We have often been told, that by kissing diseased women, the disease has been contracted, when there were chapped lips. These assertions must have had some foundation in truth. But as some Authors copy their accounts without searching into the truth of them, so might cases have been handed down from

from one to the other, and have been notwithstanding errors in theory.

But it must be remembered, that if we who declare these cases, from virus being conveyed by transplanted teeth, to be positively facts; then the assertions of our ancestors will of course no longer be doubted. The subtilty of the disease will be hereafter more attended to, and the history of the cases which proclaim this subtilty will not be so liable to be hereafter impeached, when it becomes more generally understood what the power is, which venereal virus can exert, by referring to that power which it has exerted, where opportunities were favourable for the exertion of it.

There are two medical gentlemen within the sphere of my acquaintance, who have been infected by contracting Chancres on their fingers. These cases are within my own knowledge. One of them had bitten off the cuticle which hung loose on his right fore-finger, and which generated into a Chancre, from handling an opened body after death; so that he was contaminated, not by ulcerous fluid, nor chancrous fluid, but by a general venereal vitiation of the fluids. This Chancre on his finger did not nor would not heal, and within three months eruptions broke out over his body, and which, with the

Chancre

Chancre on his finger, gave way to mercury. The case of the other proceeded farther, so as to affect the periosteum of cylindrical bones; and as this gentleman dreaded a decisive mercurial course, and depended on the alterative, he was more than two years in getting well.

LECTURE XVI.

ON

THE ACTION OF THE VENEREAL DISEASE ON THE CONSTITUTION,

CONTINUED.

IT must be remembered by you, that which is the aim and intent of these observations, namely, to shew the extent of influence which the disease possesses over the constitution upon which it is acting; and also, how that influence may be imparted, and how it will act, when imparted, upon another subject. If there be now among you an individual who doubts that the disease does possess this power which I have attributed to it, I should be very glad to know his reasons, and to hear his proofs to the contrary. It is not enough for a man to say, that he does not believe this, or that—this is no answer to proofs and arguments, no answer to strong cases and to strong opinions. It is fitting that I apply here, what has been said by Tully upon a like occasion—that there is nothing more unbecoming, not to say a Physician or Surgeon,

geon, but even any person, than to object to his adversary, which, in case it be denied, he can bring nothing to prove it.

I shall now proceed to corroborate some truths of the action of venereal virus upon the same infected subject, to consider its action as relative to the possessor, and by applying my theory and my observations to this object, to see if I cannot explain to your satisfaction, that which appeared to be impossible before. If I succeed, you will partake of this success: or, at any rate, you are to be the judges, how far I make good my assertions, how far the nature of venereal poison, and of other poisons, will become more or less confirmed from what I shall advance, and how far this theory surpasses other theories, by such explanation of points, which former theories neither did nor could explain. Mr. Hunter, in his outset, has given me a fair opportunity of ascertaining this fact, and of placing it in the strongest light for you to view it in. Before I proceed to lay it before you, I shall make but one observation, which is this—that whatever is true, will always be easily understood; and that whatever is false, cannot be but with difficulty received, and reluctantly acknowledged for a truth.

Mr. Hunter says, "When the matter is got into the con-
"stitution, it from thence produces many local effects on
"different parts of the body, which are in general a kind
"of

" of inflammation, or at least an increased action, occasion-
" ing a suppuration of its own kind; it is supposed that the
" matter produced in consequence of these inflammations,
" similar to the matter from a Gonorrhœa or Chancre, is
" also venereal and poisonous. This, I believe, till now
" has never been denied; and upon the first view of the
" subject, one would be inclined to suppose, that it really
" should be venereal: for first, the venereal matter is the
" cause, and again the same treatment cures both diseases;
" thus mercury cures both a Chancre and a Lues Venerea.
" However, this is no decisive proof, as mercury cures ma-
" ny diseases besides the venereal. On the other hand,
" there are many strong reasons for believing that the mat-
" ter is not venereal. There is one curious fact, which
" shews it is either not venereal, or, if it be, that it is not
" capable of acting in some respects on the same body, or
" same state of constitution as that matter does which is
" produced from a Chancre or Gonorrhœa. The pus from
" these latter, when absorbed, generally produces a Bubo,
" as has been described; but we never find a Bubo from
" the absorption of matter from a pocky sore; for instance,
" when there is a venereal ulcer in the throat, we have no
Buboes in the glands of the neck; when there are vene-
" real sores on the arms, or even suppurating nodes on the
" ulna, there are no swellings of the glands of the arm-pit;
" although such will take place if fresh venereal matter is
" applied to a common sore on the arm, hand, or fingers.
" No swelling take place in the glands of the groin from

" either nodes or blotches on the legs and thighs. It may " be supposed, that there is no absorption going on from " such sores; but I think we have no grounds for such sup- " position. It's mode of irritation, or the action of the " parts affected, is very different from what happens in " the Chancre, Gonorrhœa, or Bubo, being hardly attended " with inflammation, which in them is generally violent. " It might be supposed, that a constitution truly and uni- " versally pocky, is not to be affected locally by the same " species of matter; but from the following experiments it " would appear, that matter from a Gonorrhœa or Chancre " is capable of affecting a man locally that is already poxed; " and that matter from pocky sores, arising from the con- " stitution, has not that power. A man had been affected " with the Venereal Disease a long time, and had been se- " veral times salivated, but the disease still broke out anew. " He was taken into St. George's Hospital, affected with a " number of pocky sores; and before I put him under a " mercurial course, I made the following experiments: I " took some matter from one of the sores upon the point " of a lancet, and made three small wounds upon the back, " where the skin was smooth and sound, deep enough to " draw blood. I made a wound similar to the other three, " with a clean lancet, the four wounds making a quadrangle; " but all the wounds healed up, and none of them ever ap- " peared after. This experiment I have repeated more than " once, and with the same result; it shews that a pocky " person cannot be affected locally with the matter pro-

" ceeding

"ceeding from the sores produced by the Lues Venerea. "But to see how far real venereal matter was capable of "producing Chancres on a pocky person, I made the follow- "lowing experiment: A man who had venereal blotches on "many parts of his skin, was inoculated in sound parts with "matter from a Chancre, and also with matter from his own "sores. The wounds impregnated with the matter from the "Chancres became Chancres; but the others healed up. "Here then was a venereal constitution capable of being af- "fected locally with fresh venereal matter. This experi- "ment I have likewise repeated more than once, and always "with the same effect."*

By this quotation which I have produced to you from Mr. Hunter, it appears, that from want of knowing that important discovery which I have preferred before you for your consideration, all his observations were vague and indefinite; that he was making experiments in expectation of coming at a truth, but could not, through them, find it out. Had he already discovered the truth, these experiments would have tended to confirm it. Had his opinions upon the nature of venereal virus, as it affects its possessor, been sound, these experiments would have illustrated them. But so difficult was it found by him to hit upon the truth, that nevertheless it was by these experiments tempting his understanding, yet he could not embrace it.

* Pages 292, 293, 294.

Mr. Hunter says that, in consequence of these experiments, and in consequence of observation, "there are many strong reasons for believing, that the matter from venereal constitutional ulcers is not venereal." If this were true, there would be a strange falling off indeed of that extent of power in virus, which I have attributed to it. For if I say, that the cases of the teeth were venereal, and that the cases were produced from a venereal vitiation in the fluids of the subjects who imparted the infection; and if he says that even pus from venereal ulcers is not venereal, and will not impart the infection—one or other of us is very wide indeed of the mark. Mr. Hunter says, that "the pus from Gonorrhœa or Chancre, when absorbed, generally produces a Bubo, as has been described; but we never find a Bubo from the absorption of matter from apocky sore," &c. I say that it is not the absorption of fluid secreted from our own Gonorrhœa or Chancre, which produces a subsequent Bubo, and that it could not possibly happen from the absorption of our own secreted fluid; but that it is always produced from absorption of the foreign fluid. Mr. Hunter was aware, if Buboes followed ulcers which were in consequence of venereal constitutional infection, that the lymphaticks must first be affected through these ulcers, and that the fluid of these ulcers must be absorbed. He, not knowing at the same time that it was never the absorption of our own secretions which did inflame the lymphatic glands, took it for granted that the venereal fluid from a constitutional ulcer possessed

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no venereal power of infecting another subject, because it did not, through absorption, stimulate any gland which was in the way of being stimulated, if it really possessed any such power. Mr. Hunter, not being in possession of this fact, took it for granted, that such ulcerous fluid was innocent, because it was innocent to the subject who possessed it, and who secreted that very fluid which Mr. Hunter applied in making his experiment in order to ascertain, not the relative innocence of this fluid upon the subject who secreted it, but the innocence of the fluid, by its not propagating venereal infection to another, upon whom it might be applied. That is, in other words, Mr. Hunter wanted to clear up a point which he was not decided upon—whether venereal fluid, from a constitutional ulcer, was or was not equally venereal as the fluids from Gonorrhœa and Chancre. And very fatal for the truth of the question which he was seeking to ascertain, he just did that by his experiments, which could not ascertain the point he was aiming at. For he applied the ulcerous fluid of the same subject upon a part where no ulcer was, and found that it would not re-infect; and applied the chancrous fluid of another subject, and found that it would infect. Now, if he had applied the ulcerous fluid (by ulcerous fluid, I mean to express the fluid from an ulcer arising out of a constitutional infection; and by chancrous fluid, I mean to express the fluid of an ulcer or Chancre from a local infection) upon another subject instead of the same subject, he would, by that means, have found

found what he sought for, namely, that it would have conveyed infection: and if he had applied chancrous fluid to any part of the same subject, then he would have found that the chancrous fluid no more than the ulcerous fluid did or would impart any infection, would in the least affect a part locally on the same constitution, no more than if the operation had been made with a clean lancet. But in these two experiments, the ulcerous fluid of the same subject was applied to ascertain its malignancy, and the chancrous fluid of another subject was applied to ascertain its malignancy also. Can there then be any longer a wonder that the former should fail of taking effect, and that the latter should succeed?

But I will now point out to you how he should have made these experiments in order to acquire new knowledge, and in order to acquire true knowledge. In the first place, his experiment with chancrous fluid was no more an object of his choice, than his experiment with ulcerous fluid was; for he was, by not dreaming of this distinction which I have pointed out, seduced into an error. The subject upon whom he pitched to make the first and the second experiment, being possessed of ulcerous fluid, he thought, very unsuspectingly, that there was no difference, as it was ulcerous fluid, whether it were tried upon the same or another subject; and therefore when he tried it upon the same, he took it for granted that it was all the same thing, that it would

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produce exactly the same effect as if it were tried upon another sound subject. In a moment of triumph, he seised upon the opportunity of ulcerous fluid being at hand, for ascertaining this fact, and by it, for ascertaining a general theory, and a general effect of that venereal action of virus, by which it imparts the disease from one subject to another. For he means to say, if this ulcerous fluid taken from a subject, and properly placed on a sound part of his skin, does not there produce Chancre, why then the ulcerous fluid does not partake of venereal infection, and it is not like chancrous fluid, for chancrous fluid, thus applied, does convey venereal infection. Therefore, says he, chancrous fluid is venereal, but ulcerous fluid is not venereal. But how was a part of his experiment proved? I mean that part of his experiment which refers to Chancre?—Why, thus it was proved—The subject, having only ulcerous fluid, he made use of his own ulcerous fluid, but he borrowed the chancrous fluid from another subject. For he says, that "he inoculated the man from chancrous matter, and took also the matter from his own sores; and that the wounds impregnated with the matter from Chancres, became Chancres, but the others healed up." I do not see, by making this experiment with chancrous fluid taken from one subject and applied to another, much more insight could be given into the nature of venereal action, than by the common mode of infection, for in reality it is no more. And if this experiment had failed, and it most certainly would

would have failed, had the circumstance of the experiment or the convenience of it suited the present state which I am about to give of it, had that been put in practice, it would have most assuredly failed: and then I suppose we should have been told, and we might have been so told with exactly as much truth, that chancrous fluid would neither infect. For if the subject on which he made these experiments had had local venereal Chancres upon himself, instead of venereal constitutional ulcers; if he had exactly reversed what he did, if he had inoculated the subject on another part with his own chancrous fluid, and had also inoculated him with the ulcerous fluid of another, then he would have found that his own chancrous fluid would have left no impression, but that the ulcerous fluid would have in consequence produced a Chancre, because this ulcerous fluid was the production of another, and from a constitution which was venereally infected.

In short, had he founded his experiments upon true theory, instead of founding his theory upon his experiments, he would not have misled himself, nor would he have misled others; for I do not believe, that in this instance, Mr. Hunter meant to mislead, for if I thought so, I should not hesitate to declare it. But this will shew the vanity of that theory which is formed out of experiment—experiment will confirm true theory, but no true theory can be obtained

tained by experiment, for experiments may thus again be wrongly made. The circumstance of not being awake to, and of not having the mind open to receive an impression of that natural distinction which there is in a relative action of one's own fluid upon ourselves, and in its general action, when imparted to another, will make all the difference in a true or false theory, in a true or false idea of poisons, in a true or false opinion which may be given of common venereal occurrences, in the danger or safety of a patient; and moreover than that, in ascertaining whether we know any thing about the matter or not. Another lesson is inculcated by this, that we should not be so sufficient in ourselves, as not to think any thing of the sufficiency of all the rest of mankind.

The natural distinction which ought to have been observed between fluid, as it relates to the subject which secretes it, and to the subject to whom it is transferred, is a keystone to all the theory, which can be in truth received for the confirmation of the nature of this poison, and of all others. Every ancient Author sufficiently impressed upon our minds, by the strongest cases, that which the venereal virus was capable of effecting. The venereal virus was always uniformly the same. Authors knew before us, and we not only were taught by them, but daily practice confirmed to us, that such being the subtilty of the disease, it appeared at very remote periods, after a probable oppor-

tunity were given for contracting it—that children were infected with it, we knew not how—that husbands and wives were attacked with it, they knew not how—that chastity were in danger of being calumniated, from the imperviable subtilty of the poison—and that from many and established histories of unaccountable cases which have occurred, suspicion were always awake to fear and dread of its subtilty.

I have said, histories of the natural progress of the disease convince us of these truths. But notwithstanding these were known to come to pass, yet from want of this distinction, of this key-stone, of this knowledge of relative action in the poison, neither our ancestors nor our cotemporaries have been able to form that theory, which exactly corresponds with and demonstrates its truth by proof, and which was known to exist, but not how it came to pass. It was to prove these assertions of our ancestors, it was to ascertain what was a possible power in virus, it was to elucidate that which occurred in the history of the disease, that induced Mr. Hunter to appeal to experiments. Mr. Hunter failed laudably, because his intentions were commendable. Mr. Hunter with truth saw that these occurrences, which were said always to have happened, and which certainly can and will happen again in the history of cases, were never accounted for: he therefore appealed to experiments, in order to account for them. He did that which

which the best and first of all mankind can only do.—None of us can reason, but from what we know; and through that limitation of human power, Mr. Hunter failed. He made those experiments wrong, which were to confirm a circumstance already known to be true. This is the point at issue. These extraordinary circumstances of the subtilty of the poison were known and confirmed, in instances indisputable. Experiments were only wanted to explain them. They were not wanted to prove whether they had or could occur. Nobody that knew any thing of the history of the disease, nobody that had read, nobody that had practised, ever doubted this fact, but nobody could or had explained it.

Having said thus much for Mr. Hunter, I will now point out to you where he was to blame. He was to blame in affecting to despise an acknowledged power in venereal action. He was to blame in presuming to suppose, that his experiments were right, and that histories of the subtilty of the poison, in all the great variety of cases, were false. He was to blame in concluding that the nature of venereal poison must bend to his experiments. And he was to blame, in not paying more regard to what had been well attested of the power of venereal action; in not seeking his instructions from the history of cases, and from the most common symptoms, as they most ordinarily occur. Had he been faithful in observing the natural action of the di-

sease, had he been true to Nature, knowing that she is always constant, that she never deceives, and had he only attended to natural events, and reasoned upon them, he would have been right.

If he had reasoned thus, he would have made this discovery—

If I take fluid, which is secreted by a subject, and insert it upon any other part of his skin, will that effect more than it does by always being liable to be absorbed from a part upon which it is constantly dwelling? Can I expect, that by removing the fluid from one part to another, it will any more act from that cause, than it has been known to act, when opportunities for its action have constantly offered? And yet in all the various cases of long and protracted Gonorrhœa, have I ever seen a single instance where it has so acted? From the same reasoning, why should I take chancrous fluid, which dwells upon the Penis, and which neither produces another Chancre, nor Gonorrhœa on the same subject? What am I to expect from such an experiment, if I did transfer this fluid from one part of the same subject to another? Can I expect to confirm the true nature of the disease by it, if it should, thus transferred, excite another Chancre? Does not the true nature of the disease revolt at the idea? Did I ever see that the fluid does generate fresh Chancres? Or if it did, should I not then see constant succession of Chancres on the same parts, and would not the whole

whole of the glans and parts contiguous thereby become a galaxy of Chancres? What can I expect in consequence of my transplanting ulcerous fluid from one part to another of the same subject, but disappointment—seeing that parts surrounding ulcers are constantly exposed to the action of their fluid, if it had any—seeing that lymphatic glands would become inflamed, if absorption were virulent—and seeing that another Chancre was never generated from it?

Such must have been the result of his observing the natural action of the virus. Such is the natural effect of the fluid upon the individual who secretes it. Not only the disease owes its support, but it cannot be conveyed from one subject to another, without a transfer of the fluid, in the most ordinary way of infection, being absolutely made over from him who secretes it, to him who is infected through it. Such is the fact, that the whole of the secretions, as it is exemplified in the case of the tooth, which, to all appearance, are not affected locally by the disease,—that even the natural secretions of one constitutionally infected, do, when engrafted upon another subject, in extraordinary cases, produce such symptoms as all those who have seen them, know them, and positively define them to be venereal.

In making our observations for the discovery of truth, we should be conducted implicitly by Nature—she only can point out the right path, which we are in search after; we

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we are blind, she alone possesses the sight; we are ignorant, she alone can instruct. We therefore should always follow her, and not attempt to pursue our way without her conducting hand.

If Mr. Hunter had done this, he would have found out this desideratum instead of me, he would have avowed what the disease was capable of effecting, he would have acquired fame for explaining and confirming the truth, instead of darkening the subject which he attempted to illuminate, perplexing the question he meant to explain, and narrowing the power of action in virus. Wrong theory is a prejudice to practice, a libel on truth, and a blasphemy on Nature.

I have now established the extent of the power of venereal virus, as it influences the individual who secretes it; and I have also established the extent of power of venereal virus, when it is imparted to another: here I mean to rest the question.

LECTURE XVII.

ON

ULCERS ON THE TONSILS, AND ERUPTIONS ON THE SKIN.

FROM what I have already said, I have no occasion to go minutely into questions which may hereafter arise, and which bear relation to the theory of our own secretions having the least influence in supporting the disease, or of having any concern with the action of it on the same constitution. It is sufficient for me now to offer to you that opinion which I have founded, from the result of what I have already endeavoured to inculcate. I am convinced by unerring observation, that the Venereal Disease is not supported or aggravated on the same subject by any of that fluid, which is either produced in consequence of local infection, nor by any of that fluid which is produced in consequence of ulcers on the tonsils, of ulcers on the skin, or of ulcers situated on any other part of the body whatsoever. This position is not only just, not only proved to be true, in consequence of our researches into the effect of this

this fluid, but it is also in accordance with the general nature and action of all poisons.

Supposing we pursued the question, in order to try by it what would be the consequence to the same individual, if his own fluid, from any venereal ulcers, had any power or concern in supporting or renovating the disease. Such fluid must be either re-absorbed into the habit, and by passing through the lymphatic glands, stimulate them, or it must generate fresh ulcers wherever it were lodged so as to have an opportunity of generating them. Or, if the fluid, for example, which is discharged from venereal ulcers on the tonsils, possessed a power of acting malignantly upon a constitution which secretes it, what would be the consequences locally and constitutionally, which we may have a right to expect, from that being a fact? They would be nothing less than that the whole of the mouth would become chancrous, that the contiguous glands would become Buboes, that the fluid trickling down the throat would excite ulcers, that the swallowed fluid would ulcerate the stomach, and so very preposterously would the argument upon this principle lead us on, that we must expect, by giving it all its scope, that every secreting surface locally infected will produce Gonorrhœa, and every lymphatic gland a Bubo; every part of the skin will be ulcerated; on every aponeurosis there will be a node, and on every bone a caries.—But if none of these consequences do occur, what temptation

tation have we for reasoning after this manner? We are bound to reason upon symptoms as they do occur, and must confine ourselves to that which we see, and which is the only true nature of the disease.

Notwithstanding we find the latest Authors upon this disease, deciding upon effects of absorption of our own fluids, as if these fluids really possessed a power of feeding the disease. When it is found, on the contrary, that every venereal symptom, which is a result of constitutional infection, which is a result of a dissemination of original virus received from another subject, is an apparent relief to the constitution. This is evident, by the symptomatic fever, and by manifold debilities which I shall shortly treat upon, being in some measure abated, after the constitution has thrown forth venereal eruptions, and after they have generated into ulcers. And although the constitution cannot by such effects become freed of the foreign venereal poison, yet its existence under that influence is protracted, and the efforts of it are in this instance similar to more general efforts by which it is governed, in endeavouring to free itself of that which is obnoxious to the living principle of it.

If I appeal to the action of other poisons, my argument will not be found to lose strength by such analogy, it will be by that not only strengthened but confirmed. In the Small Pox a violent fever prefaces the eruption, as does a

general debility also, and no one eruption in the Small-Pox is generated from the infection of another; nor will matter taken from one pustule, and by wounding a part of the skin, and by applying that matter where there was no pustule, produce another upon the same subject. This experiment I have made.

I therefore infer, that it is the actual absorption alone of foreign venereal fluid imparted by another, which progressively pervades the constitution, which acts more slowly on some, and more rapidly on others, and which is disseminated through every integral part of the constitution, whenever it be under the whole of the influence of venereal virus. And in order, that this foreign virus should thus be disseminated, I do infer, that it is not absolutely necessary there should be any local first symptom—that it is not to be presumed a constitution may not be as completely possessed of the Venereal Disease, without any local symptom, as it may be in concurrence with a first local symptom—and that therefore any discharge from the same individual has no relation to the venereal action of that fluid which is absorbed into the constitution, and which was imparted by another. I also infer, that a local symptom upon any part from whence lodged venereal fluid was absorbed, and which in fact was the venereal fluid of another subject, is not a sine qua non for venereal infection fully taking effect upon that constitution into which it was after this manner absorbed.

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I shall now proceed to give a statement of symptoms which are produced in consequence of the action of this foreign virus, after, through absorption, it has gained possession of the constitution. And also it must not be forgotten, that constitutional symptoms will be most materially affected, and undergo a change, when they have been influenced by mercury. The time of the constitutional appearance of symptoms, after the virus be completely absorbed into the habit, has been one of the most indefinite and most irregular circumstances of the disease, and I will explain to you why it must have been so.

When a Chancre or a Bubo was a first symptom, and when the time has been strictly ascertained of the appearance of one or other of them, then venereal symptoms from an infected constitution will follow in regular succession, both in point of order and of time. In general, there will be a symptomatic fever, succeeded by ulcers on the tonsils, with or without eruptions on the skin, in six weeks or two months after the date of either. But when mercury has been imperfectly administered for the cure of local symptoms, the regular succession of constitutional symptoms will be interrupted; or, when the virus of another subject has been absorbed into the constitution, without leaving any local impression, and in consequence of which the patient does not suspect that he is infected from that connection, then ulcers on the tonsils, or eruptions on the

skin, will appear within two or three months afterwards. This last possible form of infection being little known, and as little attended to, it has been a general practice to recur back to that period when the patient had a Chancre, Gonorrhœa, or Bubo, and to attribute the cause of the constitutional symptoms to one of them, if it were ever so remote. But whenever I see a patient on the immediate appearance of ulcers on the tonsils, and eruptions on the skin, and who neverhad any local symptom, I am of opinion, that the disease was contracted by him within three months.

The signs of constitutional infection must be various, as it is found by experience. Some of these signs may be said to be commemorative of the disease, to be assured tokens that the virus is actually present in the constitution, notwithstanding it has not yet avowed itself by any constitutional symptoms. Some commemorative signs not only betray a positive presence of the foreign virus in the constitution, but they tend to confirm those symptoms to be venereal, which, when they appear, might have been said to be equivocal, if such commemorative signs were not taken into consideration.

It has been wisely and truly said by ancient Writers—that it is necessary, whoever would form a true prognostick, they should strictly define and properly judge of all the signs, and comparing their forces together, examine which prevails;

vails: and there is no instance, in all the round of diseases, when this advice claims more attention, when it can be so applicable, as in the Venereal Disease. Because it will, and it necessarily must produce similar symptoms to other diseases, and which symptoms will be various, from an anatomical variety in the parts on which the disease is liable to fall. The nature of this disease is of so wide an extent, and it comprehends such an infinite number of different symptoms, that it rather appears to be every disease than one disease. Therefore, it were vain to attempt a definition of that which cannot be comprehended within the narrow limit of a definition. It is much better to place, in successive views, the nature, form, and disposition of the disease, and the series and connection of effects which it produces, by an accurate description and enumeration of its most general symptoms. The whole of the symptoms of the Venereal Disease do never occur at one time upon the same subject. The whole of the symptoms never can be produced at once, since some of them are repugnant to others. But it is, notwithstanding, necessary, that the whole of them should be known, that they should be placed before you in successive views, not as they must, from a relative bearing towards each other, absolutely be found upon a subject, but as they might, when any number of them are found together, more clearly establish and point out a disease to be truly venereal. So strongly am I convinced of the power of the Venereal Disease, that it exceeds mine, and has exceed-

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ed the abilities of any man to follow it through all the mazes which the disease pursues, as if it were designed to harrass reason in developing them. For, as the fluids in general will partake of the infection, so will every part sooner or later become influenced by it. As every part is liable to disease from other causes, and as every particular part diseased will have its own particular symptom, and its own particular appearance, necessarily according with the anatomical formation of that part so will venereal symptoms on every particular parts be equivocal with other symptoms from other causes.

It must not therefore be imagined, that the symptoms which I shall relate will be all the symptoms that ever happen in the Venereal Disease, and that there are no more than will be selected byme : I shall select the chief, and those that are most commonly present, with other more subordinate ones. For if I did otherwise, I should be obliged, not only to go into all the diseases incident to the human body, but likewise into their distinctions. I am well aware, that I am enlarging the extent of power in virus upon the constitution of the subject infected; and that in what I shall hereafter advance, I shall still have in view that perviable power which it possesses of bringing into diseased action any part of the constitution. But it must be remembered always, as I have before now remarked, that I am not introducing to you any innovation, that I am only confirming

those

those attributes which have been said to belong to the disease by every Author who has given a history of it, from the first account of its introduction, till lately.

Since the publication of Mr. Hunter's Treatise, I do not imagine that those who have not confined their studies to this disease in particular, have recurred to any prior opinion upon it. They therefore will be impressed with an idea, that mine, in comparison with his, is too extreme; and that if he fail of the truth, by narrowing the natural limited power of the disease, I am extending its power beyond that point which the history of true venereal cases will support me in. I shall only ask such, if they believe that the cases of the transplanted teeth are truly venereal? And if they believe that the accumulated opinions of former Authors are also true? I know, as well as any man who reflects can know, that the prejudice from first impressions is hard to be done away—that the mind must be prepared for such a revolution, must be engaged de novo—and that a change in opinion is not the sudden result of a stronger argument—but that the mind must be disencumbered of a wrong habit of thinking, before it can be free and open for receiving the impression of a right one. The most energetic mind that ever was possessed by man, is capable of being imposed upon, is capable of receiving an impression from early prejudice, which, like a weed in a rich soil, will root more deep, and grow more rank.

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I therefore know, that I am not only to affirm my facts, and to impress those with them, who are open to receive them, but I am also to take off from others the prejudice of a wrong theory, and to invite them, from habitual reflection, to examine into the merits of a true one, before they will be prepared for receiving it, and firmly adopting it. It is necessary, from that motive, for me to assure you, that by observations which I have made from cases upon the nature of the disease, as well as by that strength which my opinion derives from the authorities of others, I am at liberty positively to declare, that there is not a visible part of the constitution that is not exposed to affections from the Venereal Disease.

I do not intend to inquire farther, how the Venereal Disease gets into the constitution, by being absorbed from that part to which the venereal fluid, received from another, first adhered, as that point has been, I flatter myself, already fully discussed, and strongly exemplified. It will be sufficient for me now to remind you, that there are two ways by which the infection is communicated to the habit. The most ordinary and common way is by the absorbent vessels, and through which the disease is communicated to the blood. The most extraordinary and uncommon way is by the infection being immediately communicated to the blood, and without the intervention of absorbent vessels. By the ordinary way, is meant, that natural intercourse of the sexes

sexes, through which it is produced; and by the extraordinary, that natural consequence which, as I have shewn, might produce it from a transplanted tooth, vitiated by venereal infection. In the former, it has to pass through the system of absorbing vessels into the blood; and in the latter, it is communicated more directly to the blood, from not passing by lymphatic circulation. In the former, its consequences are slow, and appear at a greater and more uncertain distance of time; in the latter, its consequences are more rapid, more violent, and to a certainty in a much shorter time, the whole of the leading features of the disease being particularly strong and decisive.

Although it is manifest from experience, that the whole of the fluids (as they hold a communication with the blood, in every relative sense) must be infected when the blood is infected, yet it will be seen that the venereal poison affects some fluids sooner, or more apparently, than others, that it more readily produces undeniable symptoms of its influence upon them, and that it more visibly betrays its action on some particular parts than on others. And although any part is liable to be vitiated from other causes, and the evident morbid state of it cannot be ascertained to be positively venereal by one instance alone, yet there may be apparent symptoms, at the same time, on other parts, and from comparing them together, they will confirm a disease to be venereal. Consequently such fluids, and such

parts, which from their anatomical construction, have the strongest disposition for first attaching the disease to them, will be most in common liable to betray the actual presence of it in the constitution, by their affections. I shall proceed to an enumeration of the most leading, and the strongest signs, which both indicate the disease to be venereal, and by which we are to know it to be venereal.

I have already hinted at a sign of the presence of the disease, before the constitution has betrayed it, and which I term a commemorative sign: it is, when a local part, which was the part to which the virus was imparted from the infecting subject, is not restored to a perfect sound state; when, in consequence of a Chancre, for example, the part where it was seated is thickened, and when there is in it a disposition to ulcerate afresh; when the lymphatic glands of the groin are enlarged, and when they are tender, or not, to the touch; or when they are swelled beyond their natural proportion in a state of prior soundness. These are strong commemorative symptoms of the disease being in the constitution; as are also, redundant discharges from the sebaceous glands behind the glans Penis, and excoriations, and increasing warty excrescences. If equivocal symptoms of venereal infection come forth from the constitution, and in the presence of these commemorative symptoms, the conclusion must be, that such equivocal symptoms are truly venereal. It is not always certain, that these commemorative

memorative symptoms will be present, when the disease appears from the constitution; for I have already said, that sometimes the absorbents take up the deposited virulent fluid, without there being any local impression left upon the part where it was deposited: but this is rare, and even when it happens, I am doubtful whether or not the glands of the groin, if they were watched, would not betray the escape of the virus into the constitution.

Every subject, when locally infected, is attempted to be cured by more or less of that mineral, which is now universally given in this disease. And from this cause, the action of venereal poison, when it is not completely extinguished, may be, and is for a time only enfeebled, and the influence natural to it thereby protracted. A longer time will be in consequence afforded for the disease to diffuse its baneful effects, for it to renovate and collect again in force, for it, by long uninterrupted exertion, to demonstrate that power it fully possesses, and for it to come forth with all its true symptoms. When the operation of mercury has been unsuccessful, when the application of it has been injudicious, the action of the poison might have been only suspended, but not destroyed; the snake might have been scotched, but not killed; the disease then will be certain to be betrayed by the effects of it, if not so regular in point of time, yet as true in point of symptoms and action. I shall to these causes attribute that tardiness in its appearance,

which is found to prevail more now than it did on the first apearance of the disease in Europe.

Prior to the appearance of cutaneous eruptions, from venereal infection, the patient feels an uncommon depression of spirits, and a languor that cannot be described. He feels erratic pains on every part of his body, and an aching pain in his cylindrical bones, darting through them from without inwards; and he feels also frequently a pain on the pericranium, as if it were bound tight upon the bones of the head. When these pains are not severe in the night, they generally cause restless tossing and inquietude. These seem to be very different from the excruciating and boring pains which attack cylindrical bones in long habitual venereal infection, and which constantly thicken the periosteum of them. The former pains may be said to be merely erratic, and to be confined entirely to the periosteum, the muscular, aponeurotic, and ligamentous surfaces. They are sometimes so slight as scarcely to excite a complaint about them, but at the severest, they are evidently milder than the other species. A languor and lassitude are not only experienced during the day, when the patient is up, but are more experienced in the morning after rising, the sleep which he had, and the bed on which he lay, affording him neither enjoyment nor refreshment. The fever which accompanies these symptoms is not of the inflammatory class; the pulse is quick, the tongue is streaky, the shoulders

ders are sore, the small of the back is aching, and the patient evidently wastes.

These symptoms preface ulcers on the tonsils, as well as eruptions on the skin. The more general and complete these eruptions follow, the more apparent is the remission of the pains, and the abatement of the other foregoing symptoms. The whole of the complexion of the skin will be found to be changed to a tawny hue. Plain spots appear, not protuberant, especially upon the breast, and upon both shoulders, of a red colour, purple, yellow, or livid; sometimes distinct, small, circular; sometimes broad, and spreading wide. They appear frequently in the hair, with a scab on the forehead and on the cheeks, dry, running, furfurose, and frequently like an herpes, and also deep and ulcerating through the true skin, making a large cavernous ulcer on the forehead, which is called a corona veneris. In the palms of the hands and the soles of the feet, these eruptions will generate into clefts, which will become hard, callous, itching, and discharge a thin ichor, and the cuticle being loosened from the subjacent skin, separates from it in small pieces like scales. These spots will also deform the skin, with hard, callous, circular tubercles or pustules, not rising very high, ulcerating at the top, for the most part dry and without matter, but sometimes moist and running, scaly, furfurose, and yellow. They are common in the corners of the mouth, and on the alæ of the nose,

nose; and this sort is frequently found about the forehead, the temples, upon the hands, the wrists, and upon the thighs and the buttocks, and upon the loins; and sometimes they are dispersed all over the body.

Other primary symptoms are, an affection of the tonsils, and other internal parts of the mouth, throat, and the nose. The uvula, tonsils, and the whole arch of the fauces, suffer pain, heat, and inflammation, are eroded by ulcers, and a caries is communicated to the bones of the palate, by which they are in a very short time destroyed. Tubercles and pustules arise in the palate, which degenerate into round phagadenic ulcers, by which the whole of the bony roof of the palate is perforated with a caries as far as the nose. The membrana pituitaria, partaking of disease, either is thickened, is fungous, ulcerous, or callous, terminating in ozœna or malignant ulcers, from which the spongy bones, and the two triangular bones of the nose, with the vomer by which it is supported, becoming a caries, sink. This is the cause of that manifest depression of the nose from venereal infection. The speech in consequence becomes changed, the voice is hoarse, and sometimes the subject cannot be ever after understood, unless lint, or something else be stuffed in to supply the vacuum. The gums will be eroded, and the teeth will drop out, the breath will be fetid, and hot and raking. As the affections of the skin and tonsils, and sometimes the palate, but not always, for

for these appearances will there be generally later, are synchronous, I thought it fitting that they should be mentioned together. But those of the palate would soon succeed the former, if they were not prevented by mercury. The ulcers on the uvula will have at first a white appearance, as if a piece of hog's lard were lodged upon the part, which in time will erode deeper and deeper, whilst those on the tonsils will be deep, and, in their appearance, apthous. To the eye that is conversant in observing them, they can hardly fail of being well defined, though not so easily described; but mercury will make always an alteration in them. Thus much I can proclaim, without hesitation, that whenever the tonsils and uvula are ulcerated from a venereal cause, the ulcers will never spontaneously heal, but will continually spread, if that be not prevented by mercury.

LECTURE XVIII.

ON

OTHER VENEREAL CONSTITUTIONAL SYMPTOMS.

By recurring back to ancient Authors, the constitutional symptoms will be found to be defined exactly correspondent with the present action of the virus, whenever it be truly left to exert it. Thus Vigo, after he has treated upon a Chancre, proceeds in a relation of the following symptoms, in consequence of the foreign virus having been absorbed into the habit. It is found in his Practica Copiosa in Arte Chirurgica.

" Quamobrem non ita hujusmodi pustulæ beneficio me-
" dicinali, intus et extra applicato, poterant a medico cura-
" ri, ut non semper earum malitiâ totum corpus amplecta-
" retur, cum ulceratione partium genitalium difficillimæ
" curationis et facilis recidivæ: quare, licet predictæ pustu-
" læ deponantur, tamen etiam atque etiam solent recidivam
" facere: quare post earum sanationem quamprimum pus-

" tulæ crustosæ et interdum cum carnositate elevata ad mo-
" dum verrucæ super frontem, caput, collum, et faciem, bra-
" chia et tibias, et feré per totum corpus diffusæ fuerant:
" et usque in hodiernum diem hujusmodi morbus hunc or-
" dinem servat. Similiter quoque interdum cum dictis pus-
" tulis, vel saltem post earum ortum, inde ad mensem cum
" dimidio vel ultra patiens laboravit, doloribus clamosis in-
" terdum circa frontem, interdum in scapulis, et humeris
" et brachiis, et aliquando in tibiis, coxis et anchis. Quibûs
" doloribus, aliquando multum post tempus, viz. post an-
" num et ultra, quædam oriebantur schirrhositates ad in-
" star osseum; à quibus ægrotantes doloribus clamosis vex-
" abantur presertim tempore noctis, et sedabantur die. Pro
" quorum, sedatione anodynæ medicinæ à doctoribus com-
" memoratæ sedativæ doloris administratæ nullum affectum
" doloris sedativum ægrotanti præstabant. Cujus doloris
" finis feré semper fuit, os, et almoschatin corrumpere et
" vitiare quemadmodum in ventositate spinæ accidit."

This history of the disease was given by Vigo, in 1513; and thus it appears, that as it was at first, so it now is, not on the decline, but exactly the same. Eruptions then, from a constitutional infection imparted by a Chancre, were produced all over the skin, in about six weeks. These were preceded by acute and erratic pains; And in a few months afterwards, these pains became more and more intolerable, and particularly at night; they then attacked the bones, and made them

them caries. If I were to rest the present definition of symptoms of the disease upon the faithful report of ancient Authors, perhaps you might not be so likely to be misled, as you will be by me. For although I am as anxious after the truth as they could have been, and although I defy the preferable purity of any man's motives, yet their opportunities were more favourable than mine. For Vigo was among the first who introduced mercury: he therefore, and others before him, must have frequently seen the natural symptoms and progress of the disease when not disguised by mercury, It appears from the concurring testimony of every Author, that nodes and caries of bones were the latest symptoms of the disease: and that it was the same formerly as it is now, in that respect, I have shewn from Vigo, who ends his description with symptoms of its attack on these parts; as does also Fracastorius, in his treatise de Morbo Gallico, whose description of constitutional venereal symptoms I will also lay before you.

" Tandem, quod in majori parte inerat, ulcuscula quæ-
" dam circa pudenda oriebantur, iis non dissimilia, quæ so-
" lent ex fatigatione contingere, quam, curiam vocant; sed
" natura longe impar, nam hæc et emori contumax erat, et
" victa una parte, alia regerminabat immortali propagine.
" Post hæc crustosæ quædam pustulæ per cutem erumpe-
" bant in quibusdam quidem à calvariâ incipientes (quod ut
" plurimum erat) in quibusdam in aliis locis parvæ primum
" eæ apparebant, mox augebantur paulatim ad magnitudi-

" nem cooperculi glandis, et similitudinem etiam iis non
" adsimiles quæ in pueris achores vocantur. Differentiæ
" earum multæ visebantur, quibusdam parvæ et sicciores
" quibusdam majores et pinguiores, nonnullis lividæ, aliis
" exalbidæ leviter pallentes, aliis duriores et subrubentes.
" Omnes autem paucis post diebus aperiebantur, ac muco-
" re quodam mucilagineo fœtido manabant, nec dici potest
" quantus ille mucor perpetuo affluebat, quanta sordities.
" Exulceratæ deinde exedebant more eorum ulcerum quæ
" phagædenica appellantur, atque interdum non solas car-
" nes sed et ossa etiam ipsa inficiebant."

From what I have here quoted, and from what constantly has occurred, in the most general sense, in this disease, it appears that the bones are the latest parts affected: and I flatter myself, that I shall not be found to depart from the truth, when I lay down some few positions, which are not only confirmed by the history of the disease, as it was described by our ancestors, but also by the appearances of it, as it ordinarily occurs at the present period.

A languor, prostration of bodily strength, quick pulse, erratic pains, loss of appetite, restless nights, wasting of the habit, and an undescribable change in the countenance, precede the eruptive state. The eruptions generally appear in six weeks after the habit is tainted, they are different in their appearances on different parts of the body, and on some they

now

now and then disappear spontaneously, leaving their mark behind them. There will be a constant succession of them, and they will in time generate into ulcers. Ulcers on the tonsils, and on the palate and nose, will go on to destroy every part to which they are attached. In some time after, the pains will become more fixed, tormenting and constant; particularly at night time, the rest will be broken, and the disease will then begin to shew itself by nodes on the periosteum, by gummata on the aponeurosis of muscles, and after these a caries of the cylindrical bones and of the head will follow. The softer and most sanguineous parts become the first victims, and so on to the harder, which will be the last to be attacked, when the disease has in process of time collected in force, and is enabled to vanquish obstacles, which in the beginning it could not effect. For the erratic pains which precede every appearance, may be considered as an effort in the constitution to get rid of the disease, and as an effort in the disease to obtain a lodgment: the softer parts, being with more facility overcome, more abounding with blood-vessels than tendonous and bony substances, are consequently the first to experience the effect of poisonous action.

The order in which the disease appears is accounted for from the different construction of the parts. They are all from the first equally exposed to be attacked by the disease, and it is not improbable that it is acting on every part, from the very beginning of the primary constitutional symptoms.

For,

For, as the periosteum and tendons betray signs of disease by nodes, in some months, which signs are betrayed on sanguineous and softer parts, by ulcers, in six weeks, so by comparing these different constructions, it is natural to infer, that such would be the effect if the date of original diseased action on every one of them were the same. Besides, it must be remarked, that the pain, which is so intense in the end, comes gradually on the periosteum and tendons; that by their resistance to extension and thickening, the pain is created, and that the disease must have made already some progress before such effects could have been produced. I am, therefore, strongly inclined to believe, that the disease on these parts commences with other earlier appearances; for, as I first remarked, I have seen them come on from a malignant and spreading Chancre, in six weeks after the Chancre healed, and they never shifted, but continued to increase for near twenty months, until the patient was cured by mercury in extraordinary quantities, and by many repetitions. The pains at first were somewhat erratic; but what I mean to say is, that from the first attack of these pains, which was six weeks after that the Chancre healed, for twenty months, the patient had no interval of rest, nor had he any other symptom but nodes, and was unremittingly bored by these pains. All this can be very readily accounted for:—He took at times enough of mercury for preventing a return of the symptoms on the softer parts, and which usually precede the pains and nodes on the tendons and periosteum, but he took not enough to eradicate

and

and extinguish the venereal action on these parts, and on the constitution in general.

In order to prove to you that my theory of the disease commencing on the harder parts at the same period that it commences on the softer is true, and that its protraction is only in consequence of mercury, I shall give another quotation from Fracastorius.

" Præter prædicta omnia, quasi parva illa forent, ingentes " lacertorum dolores accedebant, sæpe cum ipsis pustulis, " interdum ante, nonnunquam post, et ipsi quidem diutur- " ni; quibus nihil crudelius aderat: affligebant præcipuè " noctu, dolor autem non proprie in juncturis inerat sed cir- " ca lacertos ipsos et nervos. Verum quibusdam nihilomi- " nus, sine dolore ullo oriebantur pustulæ; quibusdam, " sine pustulis dolores; major pars utrisque affligeba- " tur."

I do therefore repeat, that in common these symptoms of the disease may remain after mercury has been given effectually for removing symptoms on the softer parts, but ineffectually for the prevention of those on the harder, and that this is generally the cause of their appearing, and continuing, when no other symptoms are apparent. And I do also repeat, and my authority for it is corroborated by this quotation from Fracastorius, that the rudiments of these

symptoms

symptoms commence with those on the softer parts, and that it is only from a difference in the anatomical construction of them, which makes the difference in point of time in their appearance.

The disease, when it shews itself on the aponeurosis of muscular parts, produces a tumour which is at first hard, and which often afterwards becomes soft, containing within it a glary mucus; the part will be exquisitely tender at the touch, and will, in process of time, generate into a spreading cavernous ulcer. The ligaments and tendons on the joints often suffer from this cause, and the pain is like that in the gout, from its being produced on the same parts. When the pain comes on upon the periosteum of a cylindrical bone, it will be found to extend the whole length of it, and to be only confined by each epiphysis of the bone. There will be exquisite tenderness experienced on touching the part, the periosteum will be thickened, and the skin which covers it will be œdematous, retaining the impression of the finger. The periosteum will at length inflame the skin, it will ulcerate, and sometimes slough away, exposing thereby a caries of the bone to view. But when this is not the case, and it is not commonly so, owing to the disease not being permitted to proceed, the node will be found to be hard and circumscribed, extending itself up and down the surface of the bone, and sometimes there will be distinct nodes on the same bone.

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The ischiatic parts will be often materially affected; they will become enlarged, and the whole of the external part of the hip will be painful. A lumbago will also affect the loins. The pains which affect the bones and the membranes will be such as if they were continually boring through by a trepan. The bones will be subject to the worst effects, when the disease has been suffered to go on, and when the power of it has been long continued so as to produce them: the thickest parts of them will swell out into greater or lesser exostoses, sometimes with soft excrescences, and sometimes with hard; and these at certain times will excite clamorous and raging torture; at other times they will give the patient very little trouble, just as the parts in succession are changing from a state of health to that of disease. The bones at their extremities, where they are spongy, and less compact, will by degrees be enlarged, with a slow accretion of matter, through their whole substance, but unequally, according to an inequality in their thickness. This is owing to the periosteum having been previously destroyed. The pain that is experienced, will not be so intense as on harder parts of bones; and after they are enlarged, there will be an anchylosis. A caries will penetrate the whole substance of the bones, it will be found to affect the meditullium, as well as the superficies, and to be so universal as to destroy the living principle of them; from whence they become incapable of bearing the least force, and are broken without the least, in the end.

The lymphatic secretions, and the glands that secrete them, also experience the ravages of the disease. The eye-lids will be thickened, rough, red, itching, watery, callous, and ulcerous; the tunica conjunctiva will appear œdematous, inflamed, watery, and ulcerous, discharging continually a sharp acrid fluid; and the cornea will become opaque, and sometimes it will be ulcerated. The very humours will be vitiated, and vision will in consequence be destroyed. The caruncle situated in the greater canthus of the eye will be much enlarged; and the saccus lachrymalis will be ulcerated, producing by it a venereal fistula lachrymalis.

The ears also will not be found to escape. There is not a more common, manifest, indicative symptom of venereal affection, and especially when associated with other affections, than an hissing noise, an aching pain, thickness in hearing, and deafness. The internal part of the ear, the cavity of the tympanum, the mastoid sinus, the canals, and the labyrinth, are exposed to suffer by inflammation and ulceration, attended with violent and continual pains. The small bones which assist in the action of the ears, and the bony arch of the ears, will all be liable to be destroyed by a caries. A sanious ichor will flow from the ears, as from a fistulous sinus: the cartilage of the ears will be thickened externally.

The functions, as well as the parts which produce them, become impaired or destroyed, by which a period is put to the

the existence of the patient, sometimes by colliquative fever. A wasting in the solids will expose the glandular parts, which will be found to be enlarged and knotty. The secretions every where will proclaim the disease: as will also ragged excrescences on the verge of the anus, thickening of the prepuce, redundancy of discharge from behind the glans, encreasing warts, scirrhus of the testicle, thickening of the spermatic chord, ulcers on the scrotum, falling off of the hair and eye-brows. The nails will be diseased, lose their shining appearance, and will become rough and uneven; and there will be a redness on the skin at their roots. Such are general effects of venereal infection. In short, the whole of the fluids and the whole of the solids will be found to undergo a diseased change, from venereal constitutional infection. It will be apparent, by the known progress of the disease, and by its universal action, that sooner or later every visible part of the body, the whole of the solids, and the whole of the fluids will be exposed to suffer, to be changed, and to be contaminated by it.

I have related such symptoms which are certain to be the effects of venereal infection, and which will progressively go on, without an intervention of mercury. But it must not be imagined, that any relation which is or can be given of the venereal poison, will be perfect. Every patient who is confirmedly under the influence of venereal poison, will be possessed with some of these leading symp-

toms in the disease, but the whole of the symptoms are scarcely to be expected upon the same subject; nor will the desperate ends of the most important be seen, but when the disease has gone on, from error in judgment of its true nature, or from its not having been treated in any way. No point in practice ought more to be inculcated, than care in ascertaining the true nature of any disease, in the first instance; for if a diagnostick be wrong, nothing after, that can be done, will be right. A Surgeon perhaps might conceive, that the pains which attend venereal affections of the bones and periosteum were from rheumatism, and this wrong definition might be never corrected. The patient will never, in consequence of treatment for rheumatism, experience ease, nor obtain a cure; but on the contrary, his disease, under that misconception of it, will proceed, will become more and more aggravated in its symptoms, and the torments from it must be as long endured by him, as the true nature of it lies concealed.

Irregularity, and impatience in submitting to regimen, will be a cause of the Venereal Disease being left behind after the use of mercury. And this will happen, when a Surgeon is chosen particularly for his reputed intuitive adroitness: for although he can always answer for his own sound opinions in practice, yet he cannot be responsible for the event of instructions which are dictated with a certain probability of advantage, and which are absolutely necessary for securing the safety

safety of the patient from future return of symptoms, if such advice be imprudently neglected, or only carelessly followed. This is a cause of most anomalous symptoms. The interval between an original complaint and a return of it may be a long time, when mercury has been used but ineffectually. Many are the instances which may be brought, to prove a recurrence of venereal symptoms, year after year, when the disease has been imperfectly cured. Such interval between original infection and the recurrence of symptoms may not, to a common observer, betray the least sign of any remaining infection; the latent sparks of it when unextinguished, may not directly rekindle, and collect sufficiently for discovering a true cause by which they were produced.

I would not advise any person to imagine, that he must be free from a venereal taint, because no symptoms directly appear after an irregular use of mercury: for it must not be forgotten, that although all the symptoms which I have enumerated, be strong affirmative proofs of the action of the disease, whenever they appear, yet an absence of them is but a very weak negative proof of the constitution being entirely free of the disease. It is a monster, capable of assailing the constitution in a variety of shapes, and which, as long as it remains in it, will again and again peep out on any visible part.

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When the disease has frequently occurred, and at very distant periods, it will become so assimilated and habituated with every part of the constitution, that it is scarcely possible to eradicate it. But if, by a quantity of mercury, and that continued long, the disease should yield, and the symptoms of it retire, a knowledge of that effect ought to be the strongest inducement for encouraging a perseverance, from time to time, in that remedy, which at once betrays the disease it is intended to cure. It seems impossible to reflect, without horror, upon the miserable lot of that subject, who is possessed of this disease, but who is not impressed with a true idea of the nature of it; who appeals for relief, but can obtain none; and who flies from remedy to remedy, from want of the original cause of it being detected.

The attention should ever be awake to a possibility in recurring symptoms; for if that be not the case, the most certain venereal symptoms that ever were defined, may be overlooked, and may be treated after every other manner, but as venereal. The whole of the Materia Medica will then be in vain appealed to for effecting a cure. This possibility, by those who are the most conversant in the nature of the disease, is by them the most strongly impressed. And it therefore should be never forgotten, that if a disease does not yield to all other powerful remedies, and that if a disease has the least similarity in symptoms with those

those which may be not only defined true venereal symptoms, but also with equivocal symptoms, such a disease might be a recurrent Venereal Disease.

The importance of this question, I must repeat it, is very high; for if it be taken for granted, that the case be not venereal, even when the disease, and the possibility of it has been recollected, but otherwise decided and otherwise treated, and when at the same time the disease was venereal, then the patient will be shut out of every prospect before him, but that of a diseased and miserable existence. To prove this argument, I could instance to you the mistaken proceedings of many cases, and the events of them. It will be extremely necessary for to you to attend to this circumstance of the Venereal Disease, and to be awake to the possibility of it. The importance of your knowing this fact, and of giving it its due weight, will be found very essential. Such cases, rightly defined, will gain you more credit than those which occur early, and at regular periods, from the first local infection; for recent symptoms carry with them no suspicion of the disease being any other but venereal. Such speak for themselves, and plead their own cause. You, without hesitation, pronounce them to be so, and the patient receives your report, accepts the truth of it, and implicitly yields to a cure through mercury. But when the fact is doubted, a cure, through a long course of mercury, is submitted to, from want of confidence, with great reluctance.

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An Officer, when quartered in Ireland, was attacked with a Chancre, more than two years before I saw him. His Chancre was healed, in consequence of mercury; but his mouth becoming sore of a sudden, mercury was left off, while the Chancre was healing. A hardness and thickening of the parts remained, and there was a second tendency on the chancrous part to open afresh. In a short time afterwards it opened afresh, and he was attacked with pains, which were truly erratic pains, such as precede eruptions. Eruptions soon followed, in consequence of which he applied to mercury again, and his mouth soon became sore. Because his Surgeon and Physician judged that he should have enough of mercury, they rubbed in near an ounce at once of ointment, made of equal parts. His mouth again being thus suddenly made sore, he desisted from mercury, and his pains disappeared, as did also the eruptions, one of which had left a large scar on his forehead, and another below his left eye, on his cheek. In about twelve months after this, his pains returned, and arrived to such a pitch, that he had no remission of them at night. He had gummata on his elbow, and nodes on his tibiæ. He was emaciated. He had an ulcer just within sight, behind his uvula. And moreover than this, he had a considerable node on one of his parietal bones.

Now I beg to know, how it was possible that this case could be mistaken? But the sequel of the case will prove

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to you not only that it was mistaken, but how it was mistaken.

The Surgeon and Physician thought they had done enough with mercury, and the Officer, despairing of a cure there, with difficulty reached London last August was twelve months, and applied to the same Surgeon to whom I have so often alluded. That Surgeon sent him down to the salt water. He bathed in both the hot and cold baths, and took bark, elixir of vitriol, and opium at night. After remaining there for six weeks, he returned to town, and was but just capable of crawling with the assistance of crutches. He paid his Surgeon a second visit, who then told him there was no farther call for mercury, and who prescribed for him encreased doses of opium, and gum guaicum. The Officer finding that his case was no longer considered as venereal, and from that cause, judging he could from the practice of a Physician obtain that relief which he had hitherto sought in vain from his Surgeon, applied to Dr. Smith, of Black-Friars. The Doctor, upon hearing the opinion of his former Surgeon, prescribed bark and steel, for the case. But on his second visit, he told him of his suspicions that the disease was yet most likely to be venereal and desired him to consult a Surgeon again. It was at this period that I first saw him. After hearing his case attentively, I told him, that I could do nothing for h m, until my opinion were confirmed by another Surgeon. Mr. Shel-

don then saw him with me, and we were both confirmed in the opinion, that the case was venereal. The symptoms then were those which I have already stated. The result of this case, in a few words, was, that by a gradual introduction of mercury for two months, the patient became quite restored to health; whereas, if mercury had been hurried into the habit, that restoration to health would never have been obtained. In this consisted the whole of the difference—a difference which in its consequences was very serious and alarming.

I will make but one remark. If in this palpable case every possible idea of its being venereal was renounced, how much more might it have been so, if the symptoms had been more anomalous. The only staggering circumstance in the case was, that the ulcers on the forehead and the cheek had healed. This proves, that symptoms on soft parts will give way, when pains in the bones do not, and that with less mercury success will be obtained in the former instance than in the latter. I do not call this an intricate anomalous case, nor any thing so equivocal; but if in such a case as this, the true disease be left undecided, what will be the fate of those infected, whose symptoms are more intricate and anomalous?

I attended an irregular lady, who was attacked by boring fixed pains in the bones of her legs and arms. The periosteum was thickened on the tibiæ and radii. She rubbed in mer-

mercurial ointment, four times, at four different periods. Each time she rubbed in, the pains relaxed, and the thickening went down: the last mercurial process, by being made more protracted and by her being closely confined to her chamber, completely removed her complaints.

A gentleman, who was much involved in business, and who travelled about more than any other man, perhaps, in the kingdom, contracted a Chancre, for which he applied to me, in the year 1781. I prescribed for him mercurial pills, which he took with him into the country. In a month after, I heard from him, telling me, that he could not get his Chancre to heal, until a Surgeon in the country had washed it with vitriol, and that then it healed. This was before Christmas. In the Spring he found his body covered over with spots, and his throat sore. When these appeared, a Physician in the country prescribed for him some solution of sublimate—that famous panacea of Van Swieten. The solution baulked the progress of the venereal symptoms, but did not cure the disease; it did enough to change the appearance of it, to make it stationary, and to make it more doubtful; it did so much as to induce the patient to think that his complaints were not venereal but scorbutic, to turn him away from the idea of his being cured by mercury, and to flatter him with the belief that it was not necessary. In the Autumn he came to town with a sore throat, and with eruptions thus stationary, and then applied to me. I told him

that in my opinion his case was venereal. He wanted faith, and I invited him therefore to take the opinion of Mr. Pott, who confirmed my opinion. The patient, dreading what he was to go through, and being involved in business, and obliged to travel, still indulged the hope of escaping from mercurial inunction and confinement. He therefore applied to Mr. Hunter, who gave it as his opinion that it was not venereal. From him he went to Mr. Howard, from Mr. Howard to Mr. Wyatt and Mr. Davenport, who pronounced his case to be venereal. From them he went to Sir Noah Thomas at Bath. I must observe, that he had again re-assumed his solution of sublimate, during the use of which the pains had come on upon his scapulæ, and he had an hissing noise in his head and ears; but his throat was better, and his cutaneous spots were scaly. Sir Noah told him that the bath would not cure him, nor would solution of sublimate. He soon after returned to town, impaired in his health, which was visible to his friends, one of whom sent the elder Dr. Pitcairn to him. He ordered him nervous medicines for these affections in his head, and a seton. It was for making this seton, that I saw him a second time. He was shy of talking about his case to me, and I would not begin it with him. After going through the nervous processes of blisters, seton, cuppings, and valerian, he still grew worse and worse; his head was giddy, his pains increased, he became deaf, and could not sleep. Another friend sent Dr. Smith to him, who gave him steel and bark; and dried up his seton. It was during this stage of his case that he sent a third time to me;

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for I must remark that we had long been friends, but that now I was rather cool with him, on account of his former versatility and want of confidence. When I came to him, I was not surprised in finding one of his testicles enlarged and indurated, the spermatic chord thickened, the inguinal glands enlarged, and the seat of the original Chancre hardened. I told him, in a peremptory manner, my opinion of his case and his conduct. I told him, that if he had taken half the pains in pursuing that which was right, he had taken in pursuing that which was wrong, he would have been well a long time since: that now I would, if I were again to attend him, insist upon his seeing Mr. Pott with me, and that if he did not follow the result of our opinions, there would be an end of my future interference. Mr. Pott met me, and it was determined by us that he should confine himself, and seriously apply to mercurial inunction. With this he strictly complied, and not a single vestige of his complaints were experienced by him in the process of nine weeks. This is not only an history of an anomalous case, but it is also an history of a troublesome unpleasant patient.

In the year 1787, I was applied to, to visit a lady who came from the North of England to be advised for a fixed pain which she had on both her tibiæ, but more especially one of them was uncommonly painful at night-time. The skin was œdematous, and the part was so exquisitely tender, that

that she could not bear the bed-cloaths. I knew her history—that she had been in promiscuous venery in this town, that she had been married three years, and that she had ever since privately lived in the country, and had conducted herself exemplarily sober, and in every sense properly. I found no difficulty in persuading either her or the husband into a belief of my opinion of the nature of her case. I understood that every antispasmodic had been long tried, that she had endured pains for twelve months, that they increased every night, and that they were always most boring in wet weather. I gradually insinuated mercury into the habit. In a fortnight her pains began to abate; in a month she could bear the parts to be touched; in two months only a little hardness and thickening remained on the periosteum of the tibiæ; and in three months I desisted from mercury, which had been continued daily to that time, during which she positively grew better in her appearance of health, recovered her flesh, and to my knowledge she has remained perfectly well.

It was the state of her husband, which first awoke a suspicion of the case of this lady being venereal. He, from being an healthful handsome subject, had grown thin, had a nauseous discharge from behind his prepuce and which was thickened, had ulcers on his tonsils, eruptions on his skin, and two nodes on his forehead. For these appearances he for some time had been taking advertised drops, and which,

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it may be presumed, had made the disease so long stationary. He submitted to a mercurial process, after a very regular manner, and also got well. The manner in which he was infected, was most assuredly by the venereal vitiated secretions from his wife.

I need not relate more details of mistaken and anomalous cases. It is enough for me to apprise you of the value of these reflections, an improvement from them must be a result of your own.

LECTURE XIX.

HISTORY OF REMEDIES FOR THE CURE OF THE VENEREAL DISEASE.

MERCURY had been applied to the purposes of medicine by Arabian Physicians, prior to that knowledge of its use amongst us. Rhazis, who was the Arabian Author that gave all the insight of the practice of the East to European Physicians, recommended an ointment in which quicksilver was an ingredient, for the cure of cutaneous eruptions. It was from analogy to this, that Vigo, Berengarius Carpus, and Fallopius, with other Physicians, who practised at the time the Venereal Disease appeared in Europe, first tried the effect of mercury, in the form of an ointment and in the form of a plaster, for the cure of cutaneous eruptions in consequence of venereal infection.—These eminent Authors on the Venereal Disease did not take the idea immediately from the Arabian Physician, and were not the first who used the ointment for cutaneous pur-

The disease having been propagated to India by the banishment of the Moors from Spain, they introduced China-root, and adopted it as a cure, which, in process of time, found a recommendation, in the like manner, owing to a general disgust to mercury. In consequence of the disease being propagated to the Spanish settlements in South America, the effects of sarsaparilla, a vine of that climate, were discovered, and that vine was brought over to Europe, and adopted from the success of it there. But whether from a difference which there may be in the woods being brought to Europe dry instead of green, or whether from a difference which climate will produce, on subjects afflicted with the Venereal Disease, none of them were found to answer those high expectations which their virtues flattered. A similar instance to this has been of late experienced in hemlock.

As soon as it was discovered that mercury, under the influence and preparatory correction of pharmacy, and under the various changes which the art of chymistry effected, could be given internally, then every country abounded with panacea for the cure of the Venereal Disease. Swarms of secrets infested every town of every country in Europe, as if it were intended that by this disease society should experience additional scourges, from the abuses of that only medicine which may be justly said to be an antidote to it.

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From that time to the present, every secret which is avowed for the cure of the Venereal Disease possesses for its basis one of those chymical preparations, not excepting such professed remedies which are extolled for their pretended power of curing the Venereal Disease without mercury. Mercurius dulcis, the common Æthiops, and abundance of other Æthiopic minerals, according to the nature of the medicine joined to mercury, were introduced into practice. Mercurius alkalizatus ground with oyster shells; mercurius antithisicus with dry balsam of Peru; mercurius antiscurbuticus with gum guaicum; mercurius dulcis with manna; mercurius diureticus with juniper gum; and mercurius catharticus with scammony.

But trials of these generally fell short of expectation, as the mercurial globules, which only adhered to the substances they were joined with, soon got loose in the stomach, and ran into larger globules, which passed through the prima via, without affecting the habit. These were the innocent and ineffectual inventions which followed the first internal application of mercury. To them succeeded many rough and pernicious tortures of this mineral. Mercurius precipitatus albus, solution of red precipitate in aqua fortis corrected, red precipitate, turbith mineral, green precipitate, besides many high-sounding panacea, such as to our ears are now a days familiar:—Panacea solaris, lunaris, arcanum corallinum, aurum vitæ, &c. Nor

was an internal use of mercurial waters, in which sublimate was the basis, wanting to them. Eight, ten, or twelve drops of solution of corrosive sublimate, given in eight ounces of barley water or water gruel, was execrated as "the vile practice of London quacks," by our countryman Wiseman, in the last century.

Such were the successive exhibitions of mercury, and such were the various forms adopted, when once it was found safe and practicable for being taken internally. That danger apprehended by Rhazis, and other Physicians, who confined themselves to the external use of this poisonous mineral in the genuine state of it, and which is the form most innocent to the constitution, being once, in the opinions of succeeding Practitioners, overcome--that fence, kept up through fear, being once broken down, the communication became open, and common to all. The great variety of impositions which were practised by this medicine, in former days, is equally applicable to the present. We have now pills, professed to be made without mercury, which salivate; we have now the disease cured without salivation, but the mouth is made abominably sore; and we have now our conserves and our vegetable syrups, which beguile the taste and other senses. But it is my part to inform, not to reform; I shall therefore spare my remarks.

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The whole history of the Venereal Disease demonstrates a constant rebellion in the inclinations of men for adopting mercury. From the frequent efforts that have been made to set the application of it aside, and from the abuse of it through error in practice, it has been in repeated danger of being decried. If there were within the scope of human invention any other certain remedy for the cure of this disease, I am inclined to believe that gratitude, which the reputation of mercury exacts for past services, would not be an inducement for retaining it a moment longer in practice. If mercury did not still possess that superior influence over the disease, which no other known power can with any pretension in truth lay claim to, it would not almost instantly have recovered its temporary disgrace, nor by a fresh display of its power, procure, after every disgrace, new admiration of its pre-eminent qualities. It is my business to furnish you, to the best of my abilities, with every argument for ascertaining the truth.—When we decry the use of mercury, because of the baneful and poisonous effects resulting from it, we are only in fact arraigning our own conduct, and impeaching our own understanding and want of practical experience: for mercury cannot be considered otherwise than a powerful instrument put into our hands, for the purpose and application of which we are answerable. A knowledge of that power is to be acquired, and confidence

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is in course implied, that we adjust the application of it to the benefit of the patient, and that we watch and guard against that pernicious counterpart of the two-edged power which we know it possesses, and which we know is often laid to the disgrace of the medicine, when, if I were to speak out, properly is, and ought to be the disgrace of him who so vilely administers it.

It is from a discrimination in the use and abuse of it, that we are to take credit to ourselves, for it is well known, and the idea is common, that mercury will cure the Venereal Disease: if, therefore, there be no art necessary for obtaining that effect, if there be no art to be acquired for discriminating in practice, and if art does not consist in the mode of application of it, so as to have all the good, and as little as possible of the bad effects of it—if this be not the fact, then that which has been the contention of ages, has been about nothing; and that which is the contention of this day, is of as little importance. For at this time there are schisms in opinion, about the application of mercury, and of which I shall hereafter take notice, when I come to the cure of the disease.

I shall not detain you with observations upon the preferable action of this or that particular chymical preparation of mercury for internal purposes. Those have been already better expressed by Dr. Swediaur, in his treatise on the Venereal

Venereal Disease. His preferable pretensions to chymical analisations have strong claim for arresting your attention.

The principal forms in which I apply mercury are the following :

My mercurial ointment is made of equal parts of quicksilver and hog's lard without any turpentine.

My quicksilver pill is made of ten grains of quicksilver, the globules of which are best extinguished by conserve of hips, with or without opium.

My chymical mercurial pill is made of a grain of calcined mercury, with half a grain or less of opium.

ON

THE APPLICATION OF MERCURY.

WHAT I have already said in the short history of remedies for the cure of the Venereal Disease, will be sufficient to convince you, that however reluctant every age has been to adopt mercury in preference to every other remedy which has been attempted to supersede it, yet from a necessity, from a superior power in mercury of eradicating the Venereal Disease out of the constitution, it has been enabled to maintain its importance as the first medicine that ever was introduced. This powerful medicine appears now to be more established than formerly; for amongst those who are the most capable of judging, there is not a dissentient voice against it. Their differences do not consist in denying or asserting the superior virtue of the medicine, but in the manner and proportion in which it should be administered.

During the whole of the sixteenth century, it was a debated question, by persons of great name on each side, whether mercury was a safe medicine for the cure of the disease. This might have been excusable in those times, for they thought that it was their duty to adhere scrupulously to the opinions of the ancients, and that it was blameable in them

them to make use of a medicine which had been disapproved. But at length these prejudices were removed, and experience from practice prevailed. It was found by repeated instances, that the woods were absolutely unable to cure a confirmed Venereal Disease; and on the other hand, that the use of mercury was safe, certain, and most effectual, if rightly administered: so that those who had adhered so firmly to the opinion of the ancients, being convinced of their mistake, and yielding to the force of truth, were at last obliged to give the preference to mercury, and unanimously to own, that in the cure of the Venereal Disease, it ought always to be preferred to a decoction of any of the woods.

I do not for my own part believe there is the least antivenereal power in any of the woods, neither guaicum, China-root, sarsaparilla, nor mezerion. When mercury, by too long and improper use, had (as it was thought) failed in curing the Venereal Disease, then the woods were administered, and the disease was found to abate. The woods were then said to have cured that disease which mercury could not: for mercury, having been administered before the woods were, and mercury not succeeding, but on the contrary the disease becoming worse and worse under the influence of it, the woods in decoction were administered directly after mercury had been, and by them the disease got well. Whenever it may be found necessary, from any cause, but more

especially from a suspicion that enough, if not too much mercury has been already administered, to substitute a medicine from which some good may be expected, but no harm can be dreaded, I should in such a recess adopt sarsaparilla. I believe its effects are beneficial as a restorative, and at the same time, it should be considered, that as the reputed properties of this medicine have been generally presumed to be antivenereal, the patient will be more disposed to yield to the judgment of him, who, when he stops from persisting in mercury, substitutes a medicine to which an antivenereal power is annexed. For, although my pure opinion may be, that in this recess from mercury, I am only gaining time for the constitution to recover from the mercurial effects, yet it does not follow that the patient would give me implicit credit for such an opinion, whilst his symptoms remain. But if I were to administer such medecines as were purely restorative, and to which no antivenereal power was annexed, there would be, in that instance, a necessity for my saying something to the patient; for something must be said, if mercury be desisted from, in the very face of symptoms growing worse and worse, and when, in its stead, no other reputed antivenereal is preferred.

It being now a days admitted that mercury is the only antivenereal medicine yet discovered, I shall consider it as a self-evident truth, as a datum which will not be disputed.

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But although this be the fact, and although the fact be as well known as the disease which it is appointed to cure, yet the manner by which this effect is to be brought about, the quantity which may be necessary to produce a cure, the mode of introducing it into the constitution, the length of time that may be necessary to be taken for introducing it, the guarding against the baneful effects of this powerful medicine, and the regulation of the patient during the administration of it, form one of the most intricate studies that ever engaged the attention of men of science.

The intricacy which is annexed to the proper application of mercury, so as to effect a cure of the Venereal Disease, by the safest and the surest means, is most strongly confirmed, by appealing to the difference in opinion, which has subsisted and does subsist to this day, about the most preferable and most certain method of administering mercury, in order to obtain that end which is aimed at by all, but in the completion of which the difference is wide. That method which is certain to cure, if one be more certain than another, is not so generally impressed upon the minds of men, as to become, by conviction, universally adopted. I do not say, that I am able to account to you, why there should be this difference in opinion about that which is the most preferable method of administering mercury, because I cannot in truth reconcile myself to a belief, that there ought or can be a difference in opinion

about the administration of it, without one of the opinions being at least egregiously wrong.

It appears to me, that very often the difference in opinion about the administration of mercury, arises from that antipathy which Theorists have derived by now and then seeing the baneful effects of mercury, in cases where it has been improperly gone on with. The power of mercury must be always the same, and if every constitution to which that power was applied were the same, then it would be possible to lay down a rule, prescribing the exact proportion of it which would be necessary to procure the safest and the most effectual cure. If that were the fact, there would be a given rule, to direct the due administration of the medicine, so as to procure, to a certainty, the desired good effects from it. But since every subject possesses his own proper idiosyncracy, since every subject is exposed to varieties of changes from varieties of diseased conditions, since the venereal symptoms will be more or less rapid, malignant, and obstinate on one subject than on another—it cannot from these causes be expected, that an exact proportion of mercury will ever be ascertained, which, by curing one subject, will thereby be the standard in truth of that proportion which may be necessary for curing another. This is not to be done through any given quantity of mercury. It is enough to know, before the cure of the disease on any constitution be began upon, that there is a power in mercury

cury adequate to that end: as for the rest, experience must be the guide. It is a knowledge of the action of mercury upon the constitution in general, a knowledge of the action of it upon a constitution in particular, it is a knowledge obtained from a variety of cases, which will alone teach us how to regulate the administration of mercury to the best advantage. The true line of practice must be a result of observation and experience.

It will be therefore of some importance for us to reflect, what those Authors have said upon the subject, who have been most in the way of ascertaining this knowledge; for it is to them I should look up for an opinion upon the question, if I wanted to have my doubts satisfied when they were wavering; it is to them I would rather appeal for the truth, than to a Theorist, who, by seeing that mercury is capable of injuring the constitution, thinks the proportion given of it is, from that cause, always too great; and who keeps alive a controversy, by laying down such rules and distinctions, which those who are more conversant in practice must know, from their experience, cannot nor will not answer; and which those who are conversant in practice must know, that observations like these are the result of theory alone, and not of practice. It is from conclusions drawn in the closet, persuasions have gone forth to the world, that the use of mercury is abused, by being too profusely administered. And it is from want of

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an enlarged knowledge of this subject, that such false conclusions have been drawn. The Theorist should be told, that they who are instructed by practice to be liberal in the application of mercury, presume from former success in similar cases, and are authorised again to expect the same: he should reflect, that the most steady and discerning Practitioner sometimes finds, from a peculiarity in the constitution on which the mercury is applied to act, such pernicious effects which are not in general produced on other constitutions. The Theorist, in some degree, would have then suspended his censure of the system, for the sake of the necessity of it: whereas he has adopted the censure, for the want of knowing that the system was founded in necessity.

It is necessary for mercury to be pressed forward to a certain point: it is to be wished, that the point may be obtained without any bad constitutional effects. It is necessary that the venereal symptoms be extinguished, and for that end, that certain effects, arising out of the operation of mercury on the constitution, must come forward: it is to be wished, that as soon as these effects do come forward, the venereal symptoms may retire, and that the constitution be left with this specific difference, in consequence of the exchange of a mineral poison for a morbid one—that the mineral effect will cease, when the application of it be withdrawn from the constitution; but that the morbid effect will never cease, until the operation of mercury has gone

gone on to a certain acmé, and until it has brought about this great and necessary revolution.

This difference in opinion, relative to the certain proportion of mercury which is requisite forthe cure of the Venereal Disease, in all the stages of it, may be truly said to be a warfare between the Theorist and the Practitioner. I mean to place the question in the strongest point of view. I mean to take it for granted, that the wisest arguments of the most intelligent Theorist are applied against the wisest conclusions of the most general and successful Practitioner. I do not mean to shelter this question under any infortunia, that may be the result of the want of faculty or prudence on either side: I mean to prove, that this question is not a question which can be decided by a Theorist, but that it is a point-blank question to be decided from practice alone. I mean to prove, that the same Theorist who opposes, as long as he continues to be a Theorist, that freedom in the use of mercury which the Practitioner adopts, will support that freedom in the use of mercury as soon as hereafter he becomes a Practitioner.

The Theorist always presumes that mercury can cure, in small quantities, the Venereal Disease, because he has seen instances where the patient has got well, who has gone abroad in the air all the time: and he presumes that large quantities of mercury do not cure, because he has seen instances

stances where the effects from mercury have brought the patient into a worse condition than he appeared to be in from those venereal symptoms which the mercury was appointed to cure. These are the two leading arguments, which every Theorist has furnishsd himself with, from the beginning of the application of mercury for the cure of the disease, to the present time. Every objection which has been urged against that free use of mercury which is adopted by the most experienced Practitioners, is founded either upon the possibility of the patient getting free of the disease with less mercury than is used, or upon the mischief which is produced on the constitution, in consequence of an improper freedom in the use of it.

Such presumptions, so far from their being founded in envy, are founded in philanthropy; and so far from their being founded in truth, are founded in error, for want of knowing the truth. The Theorist and Practitioner are urged on from the same purity of motive, to express their opinions. But the difference consists in the advantages which the Practitioner has over the Theorist, and which tend to render his presumptions abortive: for the Practitioner finds that, unless the virus be extirpated, the infection will be again re-inforced, and again return, with aggravated symptoms: and the Practitioner is taught from experience, that it is absolutely necessary that some outward visible signs of the action of mercury be apparent, before any of its known

power

power over the symptoms of the Venereal Disease will be demonstrable; and that therefore he cannot reconcile his expectations to that dose of mercury which is to be found in a formula, to that exact proportion which may be allotted to every subject. He must, to be successful, press forward mercury, until its efficacy be apparent. Nor must he be regulated by any other rule, than by the consequent effects which the mercury produces. From observation he knows where the good of the medicine ends, and the evil commences. He knows not only how to press forward the one, but to correct the other.

Practice authorises him to persist in his opinion—that the motive which induces the theorist to cure the Venereal Disease by small regulated doses of mercury, may be founded in benevolence, but the end of it will be contrary to the intention:—that the disease will be often left behind the remedy, and the time will be only deferred, when, with less advantage, and with less ease, the whole of the cure, with more inauspicious symptoms, will be to be gone through again.

Every young man, when he begins practice, possesses an idea, that the disease can be effectually cured with a less quantity of mercury than he finds will be necessary, as he proceeds in his practice; and every practical instance will convince him of the necessity, from the nature of the case, of

changing his opinion. He must, if he means to be successful, not consider the weight of mercury that he applies, but consider its effect, from an application of it, upon the symptoms and upon the constitution. I do not vouch for the truth of this position from the result of my own observations in practice alone, but I appeal, for the confirmation of it, to every practical Author, from the first use of mercury, to this time: I appeal to that opinion which has uniformly been given—to that opinion which Authors were obliged to give from necessity: because, if their practice had not been to press forward mercury, until the venereal symptoms were completely eliminated, they could not have been said to have been successful Practitioners; and if they had not given to mercury such a scope for action, they could not, by it alone, have cured the disease. They could neither have maintained their high characters as Practitioners, nor their assumptions in behalf of the omnipotent power of mercury over the Venereal Disease.

The Theorist who condemns a liberal use of mercury, and who urges the infortunia which arise in consequence of it, as his justification for thus condemning it, is silent about the consequences which must inevitably follow, from the Venereal Disease being left to continue its baneful progress upon the constitution. He does not recollect, that if mercury be necessary to be fresh applied, more rather than less must be required, in proportion as the disease has continued its action—he is silent in comparing those transitory

effects

effects which are produced by mercury with those permanent effects which are produced by the Venereal Disease—and he is silent about the necessity of the disease being cured at all events, and that for effecting it, mercury must be pursued until the end be obtained, because no other known power besides can procure it. This happens to be one of those permanent truths, which neither ancient nor modern refinement has ever been able to do away—a truth as fixed and immutable as the nature of the disease, and the nature of the remedy which is appointed for the cure of it.

The respectable Authors who have urged the necessity of pressing forward with mercury until a change in the constitution be apparent through it, will be seen to add weight to the arguments which I have advanced. The names of Vigo, Massa, Fallopius, Berengarius Carpus, Sydenham, Astruc, Turner, Howard, all concur, unâ voce, in support of this opinion. Astruc has given cases where the disease was left behind, and where the patients would have been utterly lost by it, if they had not fallen into his hands, and if he had not gone on with mercury.

One was the case of a young nobleman who was infected with the Venereal Disease in Holland, and after repeated unskilful treatment, he went to consult Doctor Boerhaave.

At that time he had erratic pains in his joints, and painful exostoses on the upper and lower parts of the right arm; but his chief complaint was a putrid, malignant ulcer in the upper part of the nose, with a caries of several appendages of the œthmoidal bone, of which several small fragments had come away. Dr. Boerhaave, according to the method which he proposes in his preface to his third edition of his Aphrodisiacus, prescribed for him a decoction of guaicum, to be drank according to the strictest rules of diet. The complying patient pursued that method with the greatest exactness for three months, till being quite emaciated, squallid, and withered, he was hardly able to stand, or even to draw his breath, nor in a condition even to suffer that remedy any longer. However, by that method the pains left him, the exostoses diminished, his nostrils grew less fetid, and discharged pus in a smaller quantity: wherefore, having considered the case, Dr. Boerhaave judged the patient to be perfectly cured, and ordered him to return by degrees to his usual manner of living. Three months after the patient came to Paris, and applied to Dr. Astruc, who found the exostoses remaining of the arm, and which were painful to the touch; the discharge from his nostrils was offensive, a good deal of pus mixed with mucus came from them; his nose was red, swelled, and painful to the slightest touch; caries bony fragments were often discharged; the septum, being gradually wasted, came away. A continuance

ance of inunction for three months induced these parts to heal, dissolved the exostoses, and the patient was restored.

Thus the variance of the theory of Boerhave and the practice of Astruc were at issue; and as it will ever be the case, so was it in this, perseverance in mercury prevailed: it was experience alone which could have induced Dr. Astruc to proceed with mercury. From the like experience we have ascertained, what reason would never have suggested, those different effects which are produced through mercury, whether it be taken inwardly in chymical preparations, or mixed with lard and externally applied.—We have ascertained also, that, under necessary regimen, the Venereal Disease, in process of time, is certain to yield to the action of mercury—to the predominant power of that mineral which possesses neither taste nor smell.

LECTURE XX.

ON

THE APPLICATION OF MERCURY,

CONTINUED.

I HAVE been heard already to say, that the good effects produced by mercury do not arise out of the use of a given quantity, but from its apparent action upon the constitution. Sydenham, one of the first and most sagacious observers of the animal œconomy, not only watched with accuracy the symptoms of diseases, but also the symptoms which remedies produced. He does not hesitate to say, that he believed the Venereal Disease was not curable by any quality in mercury, purely specific, but by salivation alone. When he said so, he was not very wide of the mark: for Sydenham reasoned after this manner, before he came to so declaratory a conclusion:—If mercury be given in any quantities, and continued for any length of time, I do not see that mercury, as mercury, cures the Venereal Disease: but when I see from the consequence of mercury, that the salivary

salivary glands become affected, I then, upon observing the venereal symptoms, find that mercury, not as mercury, is curing the Venereal Disease, but that mercury, as affecting the salivary glands, is curing it.—So far Sydenham.

And I say that there must be a necessary mercurial disease produced on the constitution by mercury, before the Venereal Disease can be affected by it. There are two of the strongest reasons for this conclusion to be given, that can possibly be adduced; the one is the result of practice, and the other is founded in necessity. Can it be thought that our ancestors, from observing that mercury, when it did act upon the constitution, produced strong symptoms of a poisonous nature, and which, when carried to excess, would certainly poison and thereby destroy the patient—I say, can it be thought that they would, in the face of such effects, have persisted in pursuing the same system of applying mercury for more than two centuries, that they would have persisted in avowing these symptoms must come forth before mercury could be fairly proclaimed triumphant over the disease? Can it be thought, that they, as well as we, would have persevered in looking after the symptoms which are produced by mercury, instead of looking after the quantity, if that quantity availed any thing in the cure, without mercurial effect?—Certainly not. Experience told them, that it was effect and not specific mercury: and that which experience told to them and to Sydenham, it has alike imparted to us. There is a necessity for the effect to be produced,

duced, or any curative expectations from a specific power in mercury will be hopeless and deceitful.

I shall relate to you what I understand to be the positive and the presumptive effects of the poison of mercury upon the constitution. The positive effects are, an acceleration of the pulse from a putrescent diathesis, an universal prostration of strength, a giddiness of the head with swooning, a bleeding at the nose, an increase of saliva, which will be viscid, and attended by pain and swelling of the maxillary glands, a brassy taste in the saliva, a stubborn costive habit, an aching head, a throbbing in the temporal arteries, and the eyes hot and reddened. If these symptoms be supported by repeated additions of mercury, the affections of the mouth will be increased, the discharge of saliva will be more in quantity, the gums will become sore, will bleed and ulcerate, the teeth will be loosened, and the insides of the cheeks will be furrowed by ulcers forming on the sides of the teeth. Ulcers will also be formed behind the teeth, which will give pain, and often prevent the mouth from being opened. The tongue will swell, ulcerate, and fill up the whole of the mouth—parts of the tongue and parts of the cheeks which come into contact with the teeth, will particularly be ulcerated. By additional mercury these symptoms may proceed to a dangerous excess. The tongue will protrude out of the mouth, without there being a possibility of returning it, and it will be indented by the teeth. The viscid saliva

will perpetually flow. The patient will be incapable of taking any nutrition, and of expressing his hard condition by speech. His pain will be exquisite, no sleep will come over him, the whole of his face will be greatly swelled, and he will become impatient, irrascible, and frequently furious. If mercury were farther pursued, and if such a condition in the patient were in the least aggravated, the consequences might then be fatal.

Such are the positive symptoms which arise from the action of the poison, and such are the effects which a profusion of mercury will ordinarily produce upon the constitution. But it sometimes happens, that by any quantity of mercury these effects cannot be produced. To be able to produce them, it is always to be wished; a certainty of producing them is not always to be obtained. When mercury has been pushed to a certain point, if the positive symptoms of salivary affections do not follow, there are generally some extraordinary symptoms which will be discovered, and which may be presumed to be alike declaratory of the action of mercury upon the whole of the constitution. These are, fetid breath, putrescent diathesis, prostration of strength, throbbing of the temporal arteries, heat of the eyes, profuse perspiration, diarrhœa, fetid urine, and swooning. Whilst the perspiration or the diarrhœa continue, salivary affections will not come on: but the mercurial diarrhœa will be attended with as much serious importance,

when

when carried to excess, as those salivary affections which I have been describing; and what is still worse, the venereal symptoms will not so readily yield to the action of mercury falling on this channel, as to the action of it falling on the salivary glands.

Independent of all these effects from mercury, both positive and presumptive, there are presumptive indications of the action of mercury, which seem to arise out of a concurrence of effects, rather than out of a simple effect of mercury. It has been presumed, when mercury has been administered in large proportions, and when the patient has gone forth in all weather, and has eat and drank freely in society, that from these causes, mercury has brought on an ungovernable frenzy, and which has either continued without intermission, or has subsided, leaving the patient with a perfect or imperfect restoration of faculty. This is a presumptive effect which has been attributed to mercury. I know no reason why such a suspicion may not be presumed.—Seeing that mercury is arbitrary in fixing upon parts, and seeing that when perspiration is checked, and the blood is in an irritable state from excess in drinking, I think it might be concluded, that such an effect is possible. Other effects from mercury are, cholic, attended with paralysis of a limb, or general paralysis. Such have been known to follow an excess of mercury, but have not been known as incipient symptoms

of the poison, but rather as symptoms in consequence of the poison and of indiscretion together.

I have thus briefly stated to you the effects of mercury upon the constitution, when carried on to any excess. I do not mean to point out to you, that these effects are absolutely necessary to be present for a cure of the worst symptoms of the Venereal Disease. I do not think they are: but it is necessary always, that some, if not all of them should discover a disposition for following. It is necessary that the constitution should be in a state preparatory for their coming on, before the action from mercury will be sufficiently capable of eradicating venereal virus. It is necessary that there should be a putrescent diathesis, fetid breath, prostration of strength, affection of the gums, and increase of saliva; and that these appearances should be supported for a considerable time, even longer than the venereal symptoms remain, in order to eradicate the disease from the constitution. This may be almost said to be that desirable point of mercurial operation upon the constitution, which is calculated to effect every purpose of eradicating venereal symptoms. Without these mercurial appearances, without these positive inferences, that the whole of the fluids have undergone this necessary mercurial change, and have been continued, for some time, by a regular succession of mercurial supply, I should not expect

that

that a fixed, obstinate, and inveterate venereal infection could be eradicated.

This mercurial change which is brought on the constitution, is clearly of a putrescent nature. The whole of the symptoms concur in proving it to be so. The crasis of the blood is attenuated, the eyes look fiery, the pulse are quickened without being high, the chamber of the patient is strong of a putrid stench, the urine is fetid and of a deep dirty red, the body is either costive, or colliquative, voluntary hœmorrhages flow from the nose, and these dispositions are always increased by an exclusion of fresh air. The prostration of strength, the fretfulness of the temper, the dulness of the senses, and the inactivity of the mind, are declaratory of such a disposition from the action of mercury. And this is confirmed by reversing the scene—for, as these symptoms are only supported by a constant supply of mercury to the constitution, so whenever that supply be withdrawn, it is wonderful to see how the effects decline, especially if the patient change his situation to pure air, cleanse his body by a tepid bath, and take the bark, and gentle exercise: in the course of three weeks, there will not remain one solitary trace of the mercurial symptoms.

It must be evident, upon reflection, from what I have said, that that which is deemed to be the most objectionable point in the action of mercury, namely, the excess and violence

violence o f its action upon the salivary glands and mouth, is, of all other points, the most devoutly to be wished. To be able to procure this point, is the desideratum of every Practitioner: for by it, he will feed the constitution with such a supply of mercury, as will always produce a certain, regular, and safe effect—he will be enabled, after having brought its action forward to a certain point, to continue it there—he will be enabled to obtain a cure of the disease, and a commanding controul over the unnecessary action of its antidote.

But if, on the contrary, the disposition of the constitution be not favourable to the production of these symptoms, if that necessary change in the secreted fluids cannot be brought about, and cannot be made known through the positive effects of mercury, there is every reason to presume, that the obstinacy in eradicating the venereal virus will be exactly in proportion to the obstinacy of the constitution in resisting this necessary mercurial change. Therefore, I shall take upon me to say, that the most favourable circumstances we can hope to attend a cure of the constitutional Venereal Disease, are, when a fair proportion of mercury, by being gradually absorbed into the habit, as gradually excites a salivary discharge—when by continuing a supply of mercury, these effects are uniformly supported for some time after the venereal symptoms have totally disappeared—and when, by withdrawing the mercurial supply,

supply, the symptoms of it as gradually decline, leaving the patient freed from both the Venereal and mercurial Disease, and the constitution in every respect convalescent.

This is the desideratum that, in every case, we should have in our view; and the more every case is found to deviate from this, the more untoward it will prove, the more the patient will be harrassed with the disease, and the more will the baneful effects of the remedy prey upon him. If mercury does not cure the disease, when it is applied for the purpose of curing it, the constitution of course will be exposed to endure a double violence, a double excess of poisons. It is therefore absolutely necessary, when the constitution is venereally infected, that every measure be taken, that every accelerating power be brought in to assist the kindly operation of mercury, such necessary adjuncts being too material to be dispensed with.

Notwithstanding it has been the fashion to decry confinement during a serious application of mercury for the cure of an inveterate venereal constitutional infection—confinement must be adhered to, it must be so:—and whoever gives up this point in practice, whoever makes a surrender of it, in compliance with the opinion of any man, whether patient or not, will have cause to repent. It will not prove so prudent or safe to attempt that which is impracticable and return aftewards to that which is practicable, instead of beginning with

with that which is practicable. Whatever be done against the conviction of our own internal evidence of things, is not honestly done. There is a necessary confidence arising out of conviction, which never should be surrendered. If there be a truth which I am more than ordinarily interested in, I am in this—that it is much more easy to the patient, and much more safe—that less mercury will be required to effect a certain cure of the Venereal Disease, by confining him to his chamber, than can be dispensed with when he goes at large, putting the safety and the certainty of his cure out of the question, for that must always, from wrong treatment, be doubtful and hazardous.

When the constitution is evidently under the influence of venereal virus, that influence not only extends to such parts as betray the immediate and apparent symptoms of venereal action, but at the same time the whole of the solids, the whole of the circulating fluids, and the whole of the secreted fluids, are also possessed of its influence. The great and predominant power of mercury consists in that quality which belongs to it as a poison, being capable of pervading every part of the constitution where the venereal virus can pervade, and of overcoming and radically destroying it. If, therefore, it be necessary that these evident effects which arise out of the operation of mercury upon the constitution, must be produced before the constitutional influence of it can overcome the venereal influence, if this decisive mercurial

curial change in the whole of the component parts of the constitution must take place before a cure of the Venereal Disease can be effected, it is not only a professional duty, but it is exacted from us to pursue the safest and most effectual means for obtaining that end. Every means should be adopted for conducting this mercurial process with safety and effect; every measure which tends to accelerate that process should be encouraged, and every precaution which is suggested by long acquired wisdom in practice, should be taken, for guarding against those poisonous and baneful effects. They will be always liable to interrupt a cure, will be certain to follow an injudicious treatment, and they will as certainly be prevented by adhering to rule and to regimen. If these decisive effects from mercury consist in a temporary disposition of the constitution to a putrescent diathesis, and if that cannot be dispensed with—if that must exist as long as there be a necessity for supplying the constitution with mercury, and until the venereal virus be subdued, it then must be to a certainty acknowledged, that the more effectually this process is supported and conducted, the more promising will be the safety of the issue, not only with regard to the extirpation of venereal virus, but with regard to the future restoration of the health and the faculties of the patient, who may be reduced to the necessity of enduring for a time such a conflict.

Notwithstanding the plea for permitting a patient to be at large, under a course of mercury for the cure of the Ve-

nereal Disease, be plausible and pleasing, yet it must be obvious that it is a false one. Besides, there is an indelicacy, which is the smallest of all objections, in a patient obtruding himself upon company with a fetid steach emitted upon every expiration, and with a countenance every thing but agreeable.

I shall proceed to consider what these auxiliary aids are, which may be deemed necessary and consistent for promoting a salutary operation in a mercurial process, and for conducting the patient in the safest and most effectual manner during a cure of the Venereal Disease—Confinement to the chamber, a regular diet, a gradual introduction of mercury by friction, an exclusion of fresh air, are the most important points to be attended to. The frictions must be began in moderate quantities, until symptoms of mercurial action, from a repetition of these frictions, be discovered: and when these symptoms are of the positive description, the frictions must be so repeated as neither to decline nor advance in too rapid a proportion. When the symptoms advance too rapidly, nothing is gained, but much must be lost, from the regularity of the process being interrupted: for the cure consists in persevering with a regular supply of mercury, until the venereal symptoms be extinguished: and there will be often some venereal symptoms, which will not nor cannot be brought to yield, under a process short of two months: then it must be easily understood, that, if the

symptoms

symptoms arising from mercury be so alarming as to forbid a perseverance in it within the first fortnight, the process will thereby be considerably checked, time will be lost, and the case will be rendered more uncertain. Therefore this must be attended to. It must also be observed, that on the other hand, the mercurial supply must be constant and uniform: for, as soon as it be discovered what proportion of that supply is necessary for keeping up an equality of effect, for supporting that uniform ptialism, which will be necessary to be continued for some time after the symptoms of a long prevailing venereal infection have disappeared—then the difficulty as to the future regulation of the proportion will cease, and the process will be conducted with as much facility and security as the nature of the case will admit. Mercury, therefore, must not be hurried into the constitution; the action of it will be various upon various constitutions: the business of the Practitioner is to ascertain, from consequences, the particular power of mercury upon every particular constitution, and to repeat and regulate the supply of mercury accordingly.

The beneficial effects which are produced by the action of mercury, when that action is kindly, must always be the same upon every constitution: but the proportionate quantity of mercury, which will be found requisite for producing such effects, will be various upon various constitutions. From the advantages obtained by confinement to the cham

ber, and by exclusion of the external air as much as possible, the cure will be conducted within less time, with less mercury, and with greater certainty: the natural action of mercury will meet with fewer interruptions, as the diet will be regulated, and the putrescent diathesis promoted and controuled. As the whole of this process consists in a combination of the most favourable means for rendering the operation of mercury for the cure of the Venereal Disease speedy, safe, and effectual, so will any practice, adopted in opposition to this process, increase the difficulty and the danger. The more the system which I have insisted upon be deviated from, the more will obstacles arise to the cure. I mean to say, that it is impossible for a patient to be cured of an inveterate constitutional venereal infection, when he is, during the attempt, going into the open air; and it is dangerous even to attempt it. That necessary mercurial change in the constitution will be constantly resisted, and more mercury will be required from that cause. The longer time this necessary change be producing, the more uncertain it is of being produced. Longer time will be necessary for continuing the mercurial supply; that supply will be more interrupted in its action, and the prospect of cure will be more uncertain and distant.

In order to effect a radical cure of the most stubborn venereal symptoms, I do say, that the change which is wrought through mercury, must be complete and decisive: and I do say,

say, that this change cannot be generally brought about without confinement, and without every accelerating power being employed to promote and conduct the mercurial influence. If these arguments were strictly adhered to, and if the practice of them were always carried into effect, I am confident that the histories which we daily hear of cases—such as the Venereal Disease returning after repeated salivations, and such as are calculated to degrade the action of mercury, both as a baneful poison and an ineffectual remedy, would be much more rare than they now are found to be.

From the opinion which I have given, of the necessity there is for the whole of the constitution being thus impressed with a complete change through mercury, of the necessity there is for the positive symptoms of mercury being produced and supported by constant supplies, not only until the Venereal Disease be eliminated, but also for some time after—from the opinion which I have given, that the strictest attention must be engaged to controul and regulate the mercurial action—it will be obvious, that any hopes or any expectations, founded in reason, of these events being either produced, or guarded against if they could be produced, must be fallacious, by the pursuit of any other method different to that which I have exacted. And although there may be still a difference in opinion, how these effects by mercury are produced, and how that change is brought

brought about by mercurial action, which I have attempted to account for, yet the adoption of this prudent and natural system of curing the disease is neither new nor strange: it is that system which the most experienced appeal to in the first instance, and it is that which the most experienced must adopt, after having tried other systems (or rather no system) before the intention will be completed. Why do those appeal to this resource in the end, if, without confinement, the Venereal Disease can be cured by mercury? Why should this be considered as the last resource, by those who have given the preference, in former trials, to its opposite treatment? If all these changes could be effected by an open treatment, that would long ago have banished this out of practice. If every fluid in the constitution could have been so mercurially possessed as to defeat the venereal infection by this open treatment, the possibility would have been before now ascertained. Or, if an irregular salivation, conducted either by an open method of treatment, or a confined one, were sufficiently effectual for the intended purpose of eradicating the Venereal Disease, there would have been no appeal for new trials of mercury, no repetitions of the remedy, no changes introduced in the treatment, importing a more serious and attentive concern to the action of the remedy, to the time of continuing it, and to the closer confinement of the patient. And independent of my arguments upon the necessity of this confinement, in order that such a necessary change should be produced, that circum-

stance

stance alone, of every Practitioner at last resorting to this serious method, after the default of others more carelessly conducted, proves beyond a doubt the necessity, when it is not at first adopted, and that it must be adopted in the second instance, when it has been omitted in the first.

There has been, of late years, an alterative method recommended for the cure of the Venereal Disease, when the constitution is infected. This alterative method consists in giving such small doses of mercury, accompanied with sarsaparilla, as two grains of calomel, or a grain of mercurius calcinatus, or ten grains of quicksilver in a pill, every day, or at most twice a day, so that none of the emunctories shall be affected through it. Whoever thinks that the method which I have laid down be right and true, must, upon the face of it, condemn that theory which has given encouragement to any successful expectations from so impotent and feeble an attempt. I am able to say, that it finds no support from those who are the most conversant in practice. It must be as ineffectual to the intention of cure, as a shot from a cannon would be in the intention of hitting a mark, when the distance of it was too remote for the ball to reach. This alterative method, is that which is adopted for local infection; and, even in local infections, it is sometimes known to fail. It sometimes leaves the Venereal Disease behind it, owing to the resistance which is given to the action of mercury, by the patient being in the open air, and by the

irregularities

irregularities which he may be frequently tempted to give into, through the company with which he associates. For to give this method the best praise, when it does succeed in local cases, it does not so well succeed as a less quantity of mercury would, under a stricter regimen; and that stricter regimen (by which I mean to include confinement, although not with all its concomitant rigidities) which I have exacted for the cure of a constitutional venereal infection, is sometimes found necessary to be adopted even for the cure of desperate Chancres.

At any rate, every Chancre will require an internal use of mercury for the cure of it—at any rate, that mercury, in order to be of any effect, must have had an influence on the part by its internal action, before the Chancre will betray any change, by its ceasing to spread, or commencing to heal. Then it must follow, and there cannot be a doubt of the fact, that, in this instance, the whole of the constitution must have undergone an incipient influence from mercurial action, when a particular part of that constitution evinces an evident proof of that action. And therefore I may venture to declare it as my opinion, that the mouth should be somewhat mercurially affected, and that affection continued for some time after the Chancre has healed.

There is a prevailing practice, during mercurial frictions, of making use of the warm bath, in the course of a cure under

under a strict regimen. I do not think that the practice can be justified by rational theory. This was borrowed from the French Surgeons, and I know they indiscriminately persist in it. But so far from its promoting the intention of a proper action of mercury for a cure of the disease it is appointed to cure, it tends to baulk it; for the effect from it is to open the pores and thereby to divert the action of mercury from its natural and proper channel. It hinders the progress of mercury in proceeding to accomplish that change which must take place in the fluids, and disappoints the Practitioner from ascertaining true conclusions of the particular action of mercury upon each particular constitution: a point which I have said, is always important towards conducting the cure. But if a salivation has been found to exceed those bounds which may be judged to be safe and to be necessary—if from any cause an abatement of that immoderate flux be the object—in that instance both the warm bath as well as opening medicines ought to be in practice for obtaining that end. A costive habit, from mercury, will be often so very stubborn, that almost a double purgative potion will be necessary. The warm bath, bark, and opening medicines, are essential means for reducing exacerbated symptoms from mercury.

Sulphur has been named, as having a specific power of neutralising quicksilver: but that argument, a priori, is foreign to any reason which is annexed to the action of mer-

cury upon the constitution. It is improbable upon the very face of it: nor does the administration of it strengthen the idea by its effects, for I have never seen any good or any harm from it. Keeping the mouth clean with barley-water and honey, in the first place, and, when the sores will permit it, adding thereunto tincture of myrrh, will avail something. Bark, at any stage of these symptoms, will accelerate the cure of them: but the constitution, after a mercurial process that has been well conducted, will stand in very little need of any other restoratives than free air and wholesome diet; for though a kindly patient, immediately after a mercurial course, will be thin, yet his spirits will be high, his countenance will be chearful, his skin will be clear, his constitution will feel relieved from a burthen, and these will be expressed in every sense.

The action of quicksilver upon the constitution cannot be that which Dr. Astruc has attributed to it—it can neither be owing to the specific gravity of it, nor to the power which there is in the mineral of being so minutely divided into globules, as to be enabled, by these subdivisions of globules, to pass through vessels which are too minute for admitting blood, and by that means remove obstructions imperviable to any other medicine whatever. I say, that the experienced action of quicksilver is not produced by either the one or the other of these causes, and I prove my assertion by strong circumstances in the action of quicksilver,

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or any chymical preparation of it. That it is neither owing to its specific gravity, nor to the subdivision of its globules, is proved by observing, that sometimes, from a chymical preparation of it, a salivation can be excited, and that sometimes from a very small portion of it. I have seen six grains of calomel swell the face, make the mouth sore, and excite a spitting. I have seen three grains in solution of sublimate, taken at three doses, do the same. These proofs are so irrefragable, that we must necessarily appeal to another cause for such effects. It is a natural antipathy which subsists between the poisonous mineral and the constitution, and which is expressed by a certain order of symptoms. I need not say, that this natural antipathy, which is another word for poison, would extend, provided that the supply were equal to the action, to the perfect extinction of the living principle. It is not to be presumed, because the action of mercury destroys the venereal action, that the action of mercury is not therefore as inimical to the constitution as the venereal action. The action of mercury would never be considered as a relief to the constitution, by destroying the action of venereal virus, provided mercury, when it were once admitted into the habit, had a power of supporting its action, independent of additional supplies of mercury. If the action of mercury were not of stated duration, if it were a continued action, like the venereal, even though it did destroy the venereal action, I do not believe that any one would be induced to make an exchange of the latter for the

former: for of the two, and whilst it continue, the stimulus excited through the mineral poison is far more excessive than that excited by the animal poison. It is not only by the perception in our feelings that this superiority is proved, but also by the stimulus of mercury being superior to the venereal stimulus, and thereby destroying and extinguishing it.

The action of mineral poisons upon the human constitution cannot be supported without constant supply, but the action of animal poisons can. The action of mineral poisons is acute and almost immediate. Mercury, once imparted to the constitution, will not be capable of supporting its action beyond a certain duration; but venereal virus, once imparted to the constitution, endures for ever. The action of mineral poisons is of a certain duration. The action of venereal poison is supported by the original poison being multiplied, from an atom of venereal fluid.

The most preferable mode for the application of mercury I consider always to be by inunction: I consider it the most innocent and the most certain action. The violence which it commits upon the constitution in general, is the only violence which is experienced by it from this mode of introduction. No violence is done to any local part. But when mercury is given internally, there is a greater probability of a double violence being incurred through its action:

tion: a local violence, besides the constitutional violence. It will nauseate upon the stomach, it will stimulate the bowels, it will injure both, it will produce griping, and the dose will be obliged to be limited. In desperate cases, or when from any other cause the proportion of mercury is necessary to be increased, that cannot be so well and so safely done by internal application of any chymical preparation of it, as by external frictions. But notwithstanding generally there may be no hesitation about the preference being given to the external application of mercury, yet an internal application has its use, and may be sometimes employed to great advantage, as it will appear from the truth of the observation that I am about to offer.

In impoverished habits, we know that absorbents are sometimes rendered incapable of doing their office, in consequence of a long and symptomatic fever, such as is supported by the venereal stimulus. It may, without a stretch of reason, be presumed that their power may be impaired, if not totally suspended from that cause; and more especially, if the patient who is venereally infected be also in the constant habit of drinking spirituous liquors. These, I think, are causes why sometimes the largest proportion of mercury, applied in unction upon the surface, does not procure an apparent effect in any degree to the quantity applied. In such cases, and in such conditions, I have found this difficulty in the action of mercury most to prevail. It appears

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to me, that little out of the much which has been administered has been absorbed, and that little which has been absorbed, has not been equal to excite a mercurial stimulus, much less, therefore, has it been equal to excite a stimulus superior to the action of venereal stimulus. When this is the condition of the habit, the most remote absorbents, which are those that are dispersed upon the cutaneous surface, will of course, by being remote and more minute, feel the effect of such a temporary disability, of such a privation of action, in a greater degree than the larger absorbents—than those which are nearer to the centrical part of the animal machine: for, if the absorbents throughout the prima via were also thus equally debilitated, the constitution could not then be supported. In such instances, therefore, the internal application of mercury, either partially or wholly, has a preference.

Whether it be prudent to attempt a continuance of mercury, in any form, in a constitution thus conditioned, and when such is the result of its application, that must depend upon the necessity and pressure of the occasion. If the symptoms of venereal virus be rapid, either locally or constitutionally—if there be no time to be lost—if there be an immediate necessity to put a stop to them, an endeavour must certainly be made, by every possible means, to excite a complete mercurial stimulus, as the only means for effecting that end. But I believe that, in many instances, the effort will be vain,

vain, and the end cannot be obtained: and I know, and I feel that such conditions are the most nice, dangerous, and intricate, of any which the disease and the remedy are exposed to. When mercury can be dispensed with for a time, it certainly ought to be; and pure air, milk diet, with light nutritious food and gentle exercise, must be the intervening resources. Bark and steel will be found to be the truest medicines to the purpose.

The chief causes of the constitution being brought into such a state, are, constant use of mercury, in small and ineffectual doses, in conjunction with hard drinking during the time of thus administering it: an ineffectual stimulus is thereby excited, which provokes, but never produces any change upon the venereal symptoms. No power which mercury can impart, can be received with radical effect, because the absorbent system is rendered incapable from debility, and because wanton and injudicious stimuli have been, for a length of time, mischievously and irrationally excited.

In all cases, therefore, it is of the utmost importance, when the administration of mercury is began with for the cure of the Venereal Disease, to observe temperance: it should be scrupulously exacted: and in all cases where mercury is once began upon, it should be regularly pursued, until such a change in the constitution through it be produced,

duced, as proclaims it to be equal to the purposes of extinguishing venereal symptoms, either locally or constitutionally. The dose began with should be the smallest, and that should be daily increased every rubbing, until the fetid breath, the prostration of strength, and soreness of the mouth, demonstrate its action on the fluids. The mercury should be followed up until the venereal symptoms have disappeared, and all thickening of the parts and induration have subsided. But if, on the contrary, the doses be not repeated daily, if they be not increased, if there be lapses of time between their repetition, if the stimulus be partial, but not effective, the constitution is thereby harrassed, but not relieved: it has to endure a continued non-effective stimulus from mercury, and venereal stimulus, at one and the same time.

It is to be remembered, that the generality of those whose venereal cases are complicated with bad-conditioned habits, provoke their own condition by intemperance and turbulent resistance to salutary advice: and that those who are not disposed to do well for themselves, will shew the least inclination to do justice to you. You will frequently be condemned by such, for what human foresight could not have prevented. It is always in your power to be prepared against unjust reflections, and to be justified by the suffrage of reason. But if you make the praise

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or blame of others the rule of your conduct, as a great Author has expressed it, you will be distracted by a boundless variety of irreconcileable judgments, be held in perpetual suspence between contrary impulses, and consult for ever, without determination. Think for yourselves.

LECTURE XXI.

ON

THE CURE OF CHANCRE.

THE two most important points in which a Chancre is to be viewed, are, the date of the Chancre, from its first appearance, and the condition of the parts. The virus will always be the same, but the state of the parts on which it is acting may not be always the same: for the natural temperament of the patient may be more or less irritable, or that temperament might have become irritable which was not otherwise irritable, by an improper manner of life. The whole of the difference between mild and irritable Chancres consist in this.

Let us begin with the most simple of all venereal Chancres, and then by degrees go on to those which, from longer continuance, and upon worse habits, were not in the beginning so simple, from that cause, and which will not in their progress assume any other more favourable appearances than those they sat out with.

When the substance of the Penis is pallid and dry, a Chancre will not be so disposed to spread and inflame the parts around, as when it is red and moist. The cure of every Chancre, be it ever so mild and simple, will exact the internal action of mercury. If a patient apply the very instant that he discovers a Chancre, and if that Chancre be a little pustule, which just begins to discharge, which shines as if a pure fluid were lodged upon it, and which has not eaten any depth, and even before there be any apthous bottom to it, can or ought that Chancre to be cured without mercury given internally? I say, certainly not: for it is most probable that absorption has already commenced. Admitting that there is a doubt, which the most accurate judgment cannot solve, whether the virus has been absorbed, or whether it be confined to that little point where inflammation and destruction of the cuticle has just marked out the seat of an incipient Chancre—what in this instance is the safest and surest method for securing the constitution from the possibility of future infection, and for inducing the Chancre, which is the part on which the foreign virus is acting, to heal kindly from its natural disposition, it being no longer stimulated or acted upon? Can this end be obtained by any topical application which is now known and applied in practice, and independent of the internal action of mercury? I am certain that it cannot. There are certain topical applications which may sometimes so succeed; but that is not such a success as I would wish to prescribe

scribe or to practise, because of the insecurity and uncertainty of it. Do those who favour the application of topical remedies trust to them? Or do they at the same time annex, for a better security, an internal use of mercury? That class of Practitioners generally annex to topical applications an internal use of mercury. Then for what purpose, when there is no inducement from an irritable inflammation on the parts, are those topical applications — what is the motive for which they are applied, and of what do they consist? They are applied for the purposes of preventing the Chancre from spreading and ulcerating, by destroying the action of the virus, and obtaining thereby an earlier cure: they consist in excision of the part, in applying caustick to the part, and in laying mercurial ointment on the part. Is it fitting that any of them should be put into practice in this simple stage of the case—or will they answer fully the intention? I think they are neither fitting to be put into practice in this stage of the cure, nor will they answer fully the intention for which they are adopted: and I will proceed to give my reasons.

First, of excision. Where that is practicable, it must be done immediately upon the first discovery of the Chancre, and the situation of the Chancre must be favourable for the operation, for there are many parts exposed to Chancres, where it would not be practicable; not to mention, that often there will be many Chancres on different parts. If a Chancre be situated under the frœnum, it could not be conveniently

veniently cut out; or on the frœnum, without dividing it; or on the glans, without the operation being severe; or behind the glans. But if the excision be put into practice, the virus might have previously escaped beyond that part; and as the object is too important for any doubt of certain security in the case, mercury would at the same time be given internally; and there would be a sore from the operation, which would require time for healing. Therefore, I look upon the method of excision to be something talked of, but which will never be in common practice.

Secondly, of the application of caustick to the chancrous part. This is much more common in practice than the former, and has been adopted as long a time as almost the cure of the disease. The precipitate of Vigo went to the same object. But Astruc speaks of the caustick itself; and Mr. Hunter also rather encourages it. I have applied it, but I now see so many objections to it, that I really think it ought to be condemned, and banished from practice. It must be remembered, that this application will only be safe, when it is applied to a recent Chancre, and a Chancre unaccompanied with an inflammatory disposition in the parts. But when there are many Chancres, and those on different parts, or when there is a disposition generally in the parts to inflame, the ulcer or ulcers produced by caustick may amount to a serious importance: and therefore my reasoning upon the question shall be confined to where it can be applied, and where it is most commonly applied.

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If the question were entirely local, if the only object were that of an expeditious healing of the Chancre, it would not signify by what means that end was obtained, and that which did to a certainty dispose it to heal the soonest, would be the most preferable. But the whole of the importance of the object is still beyond that argument, for this is the moment when the constitution is to be guarded against the poison; and if the applied caustick does not ensure that beyond a doubt, which it most assuredly does not, then why should the patient be thus exposed to doubt and to danger? If the caustick be laid on and act, of course a larger sore is incurred, some time must be taken for it to heal, and when it is healed, there will, from the nature of the action of caustick, be a hard cicatrix: this will perplex the understanding in determining whether the hardness be supported from the consequences of caustick, or the remaining virus which may be escaping into the constitution. But this is not all—both the stimulus from the caustick and the stimulus from the virus will irritate and enlarge the lymphatic glands of the groin, and then another perplexity and doubt will be raised, to determine from which of these two causes this symptom is incurred.

Will any of the Faculty take upon themselves to say, that the use of mercury internally may be spared, when caustick is thus applied, and that if it be spared, the patient will be as certainly safe? Will the mind be satisfied, and will there be

be no after-dread of danger? From the knowledge that we have of the nature of venereal poison, it is impossible that the mere application of caustick alone can always ensure the patient from the future action of the virus. Then is not that method a wrong one, which does not procure a positive safety against future infection, and which does not set the mind at rest, and dissipate the dread of future danger? The idea, that from shortening the duration of the Chancre by destroying the virulent action which produces it, the duration of giving mercury internally will be, and may be also shortened, and the quantity also from that cause lessened, is founded not upon a sound knowledge of venereal action, and therefore it would be extremely dangerous to admit it. The Author who has asserted this idea, has adopted a mode of reasoning incompatible with the truth of venereal action. He asserts that the quantity of virus absorbed will be in proportion to the size of the Chancre, and to the time for absorption: that is to say, the action of venereal virus upon the constitution infected with it, will be more or less influenced according to the quantity of that virus which is absorbed: as if an atom of foreign virus once imparted to the constitution, would not infect the constitution as effectually as an hundred or a thousand atoms: as if the most divisible drop of Small-Pox would not infect as effectually as the whole of the fluid extracted from a thousand pustules. Where would such a speculative idea as this lead us in the cure of a Chancre!

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The question does not consist in the quantity of venereal fluid which escapes into the constitution, but whether any escape or not. There will not be less mercury required, because more or less fluid escapes into the constitution: that is the proportion of mercury required for the cure of a local infection, which produces such a change in the fluids of the constitution, as will necessarily oblige a cessation in the action of the foreign virus on the part to which it was imparted, and thereby also prevent a presumed constitutional infection.

The erroneous idea of this Author to whom I allude, and to whom I have so often alluded, arose from want of distinguishing the nature of action of foreign fluid on the subject infected by it. He reasoned upon a presumptive idea, that the constitutional Venereal Disease drew its infection from the secreted fluid discharged by a Chancre, and from that being absorbed: for he says, the quantity absorbed will be as the size of the Chancre, and the time of absorption, and the quantity of mercury must be correspondent to that. But surely, if this fluid will infect when it is absorbed, if this fluid be the fuel to bait the venereal flame in the constitution, a profusion of the same fluid would to a certainty excite other Chancres on the surrounding parts where it is lodged. This and the true nature of the action of it has been already laid down by me.—We are furnished with another proof, how necessary a true knowledge of the re-

lative action of the poison is for the better ascertaining that which is necessary both for a cure of the local part infected, and for guarding the constitution against infection: from a false idea, that the constitution was infected more or less, according to the size and the proportionate discharge of a Chancre, the conclusion was—that there must be a local discharge existing prior to constitutional infection—that the mildness or virulence of the constitutional infection must depend upon the quantity of discharge—and that the mercury which may be necessary, must bear a proportion to the size of a Chancre, to the quantity which it discharges, and to the time for absorption whilst it continue to yield that discharge. Not one single iota of this speculative theory is founded in truth. I will repeat these facts again, and conclude my argument: that the secreted fluid from a Chancre of any size, or of any duration, is the fluid of that subject by whom it was secreted, and is perfectly harmless to him—that a Chancre is the result of the action of venereal fluid of another subject—that this is the fluid which is not only acting locally on a part so as to produce Chancre, but is also the fluid which is absorbed into the habit, or which is presumed to be absorbing, and for the cure of which mercury is given, for the purpose of extinguishing its local action, and of course its constitutional action—that the quantity of mercury for a cure must not be regulated by the size or the time of the Chancre, but must be regulated by such change in the fluids of the constitution as will necessarily oblige a

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cessation in the action of the foreign virus on the part to which it has been imparted, and necessarily prevent its action on the constitution when any infection has been presumed to be absorbed.

Thirdly, of the local application of mercurial ointment. That has been recommended, both when the Chancre has not been previously extirpated, after it has been extirpated or destroyed:—the mercurial ointment has been applied as a dressing to the part. By this it seems as if there were a conscientious mistrust, a mental misgiving, lurking behind the securities of excision and caustick, which have been thus topically taken. "However" says Mr. Hunter, "as our "knowledge of the extent of the disease is not always cer- "tain, and as this uncertainty increases as the size of the "Chancre, it becomes necessary, in some degree, to assist "the cure by proper dressings; and therefore it may be "prudent to dress the sore with mercurial ointment." These "are Mr. Hunter's own words, page 229.

In order to be better understood in what I now mean to explain, I shall first impress upon your recollection, that it is not possible for me to object to the application of mercury, as soon as I discover a Chancre: because I know, and my theory goes to prove it, that mercury ought to be immediately thrown into the constitution, for its preservation against the presumed progress of the foreign poison, as well as for the extinction of the local action of it; and that the

mercurial effect must go on to a certain point, for obtaining both these ends. This I have said, for this is my doctrine: therefore I cannot object to mercury; on the contrary, I enforce its necessity. But I must confess, it strikes me that there is something very inauspicious or suspicious in the theory and practice of Mr. Hunter—and that neither of them are well founded, when we see such an application as mercurial ointment applied to the seat of a Chancre, or to the seat where the Chancre was, but from whence it has been extirpated, either for a cure of the Chancre, or the consequences which it might have left behind it. There cannot be, in my opinion, but two reasons assigned for dressing that part with mercurial ointment, and these reasons argue an absolute deficiency in a true knowledge of the nature of the disease, and of the nature of mercurial action.

Mr. Hunter has assigned his reasons why he dressed the sore with mercurial ointment: it was so to act upon the part as specific mercury, positively to prevent that which may happen if it were not thus applied. This was one intention: and this must have arisen from a belief that a little mercury, applied on this part, would, both locally and preferably, operate against the virus, by being approximated to it, if any virus remained: that it would preferably operate upon the presumed progress of the virus into the constitution, by being also introduced into the constitution through the same channel as the virus was. I could

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be as soon induced to believe any other impossibility in Nature, as be brought to believe that mercury, merely as specific mercury, has any influence upon venereal virus, in any form or denomination whatever. If the one and the other were mixed together, they would, so mixed, retain in full effect their original properties. But the local application of the one for the local extirpation of the other, or for the local security of a part that was infected by the other, argued at least, that he was not wholly disposed to surrender up such an idea. And the consequent idea of supposing, that by sending mercury after the presumed absorption of virus, and by the same channel, has as narrow a foundation in reason (if it be any thing like reason) as the former idea: for it looks at least suspicious, that the former idea was pursued, and that it was the same theory which led him to promise something particularly specific, in dressing a part locally with mercury, for the object of extirpating the remaining virus, if any did remain. From want of a true knowledge of the nature of the action of mercury, the practice of dressing a Chancre with mercurial ointment has been long in use; but it is so dissonant to reason, that at any rate, if any other dressing be at hand, it should not be applied, lest by such a practice it may falsely be presumed that it was done with a specific motive.

I shall now proceed to treat upon that method of cure, which I not only judge to be founded on true reason, but

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I am also sanctioned in the opinion by the soundest Author that ever has written upon this important subject, I mean Mr. John Howard.

There are two points only to be considered and to be distinguished in the cure of a Chancre: the one is, whether there be no more inflammation than what may be fairly attributed to the action of the foreign fluid, and independent of any disposition in the part to be otherwise irritated; or whether the specific stimulus of the foreign virus be annexed to a part already in an irritable condition, and by which there will be produced an aggravation in the symptoms. The first point of distinction I mean to treat first upon. I mean to explain to you, the mode of treatment of a Chancre, at its earliest appearance, which is free of every other inflammation but that which is specific to the action of the foreign fluid. As it is the presence of a Chancre which alone imparts to us a knowledge of the disease being absolutely contracted, so must it be an extinction of the Chancre, which will evince to us that the disease is no longer existing—that extinction of the Chancre must be effected by the internal action of mercury, by such an action as will induce the Chancre spontaneously to yield, and to heal. This action of mercury should be indubitable, and in order that it may be proved to be indubitable, the contest should be fair, nothing should be done to the Chancre for obscuring or disguising the appearance of it: there

there it should remain until mercury cure it, and when mercury has so acted upon the constitution, as by that action to extinguish the foreign fluid, the Chancre then will heal of course; therefore, nothing but dry lint should be laid upon the Chancre, and that should be only done with an intent of taking off the soreness of it.

If it be said, (and from that cause I am provided with an answer) that more mercury will be necessarily given internally by this method of cure than by any other; seeing that the cure consists entirely in the action of mercury upon the constitution, and that for the cure of a local complaint, and when there is only a presumption of the virus being absorbed —I answer that by leaving the cure to the internal action of mercury, I am certain it will be radical, whenever a Chancre thus heals. That is not my only expectation: for there will not, nor there cannot be by this method a doubt entertained but the constitution will be also safe from future infection, and there will be no future inducement for suspecting that the disease is left behind, and that it may constitutionally appear hereafter. Whereas the case will ever be doubtful and protracted, by its being made more complex— by any hardness remaining on the original seat of the Chancre—by an hardness from the action of caustick—and by an enlargement of the inguinal glands, which the inflammation from caustick will frequently excite. The process by caustick is neither safe, decisive, nor final: more mercury

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may be given, than is necessary to be given where the Chancre is left undisguised; and the difficulty of healing the part destroyed by caustick, will be fully as great, the time as long as that of healing the original Chancre. When a Chancre heals by the action of mercury on the constitution, that method is sure to be final, and the constitution is sure to be safe: so much cannot be said when a cure is conducted by any other process—and this may be an answer in full to every argument to the contrary.

Another consideration is, the quantity of mercury, and the form of the mercury. With respect to the quantity, that must depend upon its action. It should be given with freedom when nothing forbids it, for as soon as it acts, there is an end of the case; and it may be given with more freedom to produce that action, because there will be wanted but very little supply for the continuance of it. It will succeed sooner if the patient stay at home, than if he went abroad: it will be more preferable in the form of unction, than in the form of pills. If the patient be abstemious, both forms will act the more kindly and effectually: if he live freely, the pills will not agree with him: if he omit the pills for a day or two, from that cause, the mercurial process will be interrupted, and the whole of the action and effect of mercury will be broken in upon. It is necessary, therefore, that the patient should be observant of regularity, and that the process of mercury should be regularly conducted.

A drachm

A drachm and half of the mercurial ointment is to be rubbed on the thighs every night going to bed. To do this effectually will take up some time, at least ten minutes. The patient need not wash his hands, but put on a thin pair of gloves, and wash his hands in the morning. Or a grain of mercurius calcinatus with half a grain of opium, in a pill to be taken every night and morning, when it can be taken without violence to the intestines: or ten grains of quicksilver, rubbed down with conserve of hips, to be taken every night and morning. But if the action of mercury be not apparent, and be not perceptible on the face of the Chancre within ten days, the mercurial ointment must be increased to two or three drachms, and this must be continued according to its effect: or the drachm and a half of mercurial ointment may be gone on with, and the quicksilver pill may be taken at the same time. If after the effect of mercury be evident on the constitution, and on the face of the Chancre in course; and if then the Chancre should not heal kindly, you may dress it with lint, dipped in the vitriolic solution, purely for disposing it to heal, and the mercury should be continued for some days after the Chancre has healed.

You will readily see, that when the virus is imparted to an inflamed constitution, through an inflamed part of that constitution, there will be a necessity for attending to the natural inflammation, as well as to the specific inflammation

excited by the action of the foreign poison. In that case, not only the constitution but the diseased part will exact assistance, will exact that which is best to be done for the vitiated state of the constitution and the Chancre, before it can be brought to granulate and heal. And I must farther remark, that from the habits of those patients who commonly contract this disease, this double concern is more frequent, and is generally a consequence of a round of irregularities, not only throughout one day, but throughout a succession of time. The vitiation of the fluids, from intemperance and continued irregularities, together with omissions after the Chancre is contracted, is the cause of all the untoward symptoms which arise in Chancre, and which oblige the patient, in the end, to retire to his chamber, and to adopt a stricter discipline than would have been necessary without such povocations.

But when this manner of living in the patient be known, it behoves us to be the more strict and peremptory in our instructions. Whenever the parts betray this vitiation, it must not be imagined that a change for the better will be sudden; it must not be concluded, that the most prudent correction of the constitution which can be adopted, will effect a change of such vitiation, without time being allowed: so that this condition of the constitution, and the aggravated condition of the Chancre also, will be objects throughout the cure. Moreover than this, there might have

have been a prior taint in the habit, and a Chancre from this might be the more obstinate, knowing that a subject constitutionally infected can contract a local infection. But when a Chancre, thus conditioned, heals under the pure internal action of mercury, the constitution by it might also be freed from former infection.

Before I proceed to lay down the method for treating this double case, I shall just observe, that when a simple Chancre or Chancres, independent of any other inflammation but the specific inflammation, have not been attended to, and when mercury has not in the first instance been given, such Chancres will proceed slowly to eat deeper and deeper, and their circumference will be as slowly enlarged. To the shining inflammatory red appearance, which was the incipient appearance, will succeed an apthous appearance: the treatment which I have laid down already, is always that treatment which will be necessary and proper, only with this regard, that mercury must be gone on with until the Chancres heal, and for some days after; and by its effects the requisite quantity of it will be ascertained better than I can, a priori, prescribe it to you.

The usual appearances which indicate a compound attention, are, Chancres acting very rapidly, spreading unusually, of a livid sloughing aspect, of an irregular form, eating very deep, and proceeding to destroy the parts with

uncommon perseverance. The appearance of the Penis will be tumid, the prepuce will be thickened, the glans will be red and hard, the discharge from the sebaceous glands will be profuse, and from the Chancres it will be bloody. There will be generally a fever attendant upon those symptoms. The two first points that will be indicated are, resisting the natural vitiation, and resisting the venereal progress. As neither of these are to be immediately accomplished, an aggravation of all the symptoms will naturally go on, until a correction of both be carried into effect.

The patient must therefore submit to stay at home. If his pulse indicate it, he should be bled. He should take an opening draught, and observe the strictest regimen. His pores should be opened by pulvis antimonialis, and mercury should instantly be began with: a drachm and a half, or two drachms, if the febrile symptoms will permit it, should be rubbed in every night for ten nights: and then the result should be sought for in the appearance of the Chancre. After perspiration has been profusely obtained, and the patient is in a state for it, bark with opium should be freely given. The propensity to sphacelation should be resisted by every means, and the local treatment should be all to this regard. The parts should be fomented with a strong decoction of poppies, to two-thirds of which a third of spirit of wine should be added. Linen wet with this should be wrapped round the Penis, and a poultice laid on of linseed,

seed, meal, and yeast. The Chancre, if the prepuce permit it, should be dressed with lint, moistened with spirit of wine, at least twice a day; but if the swelling of the prepuce forbid it, an injection of this should be thrown up. The Penis should not hang down.

If the seat of the Chancre be on the prepuce only, it is not so material to divide the prepuce; but if the seat of the Chancre be on the glans, and there are apprehensions that it is still proceeding, and that an hœmorrhage will ensue, the prepuce should be divided, and the Chancre exposed. Mr. Hunter has recommended an application of turpentine for an hœmorrhage from a Chancre, for it must be observed, that sometimes this hœmorrhage is serious. I have tried the turpentine, and found the good effects of it; but the spirit of wine will prevent the hœmorrhage coming on. I have also found advantage from an application of leeches to unload the prepuce. If such propensities be armed against in time, there is no doubt but from this mode of treatment the symptoms will, in the course of twelve days or a fortnight, give way.

But if the treatment be later adopted, and if mercury be not given at all, or so given as not to produce its necessary action, if the constitution has not undergone the preparatory means which I have exacted, and if, in consequence, such unfavourable symptoms are advancing more forward, still the same means are to be pursued, only with additional

ditional diligence. But if under all these adopted means, after the expiration of a fortnight, evident symptoms of convalescence do not appear, but a contrary effect is observed from the action of mercury on a constitution thus conditioned, time must be given for restoring the constitution, for assisting the state of it, by pure air, exercise, bark, milk diet, and by every means which tend to take off the irritability arising from a habit thus abused.

The symptoms, although they have not been observed to give way, will not be so rapid and alarming in this stage of the cure as they were at first, and after that which I have laid down has been adopted. The condition of the constitution being so restored as to receive that full impression from mercury which is necessary for the cure of the venereal part of the case, and the time having been allowed for bringing it into that condition, through pure air, wholesome food, observance of temperance, and the assistance of restorative medicines, then by returning to the mercurial course it will be found to be effectual: the Chancre will heal, and every venereal constitutional symptom will be hereafter out of the question, provided that mercury, when it be found capable of acting, be not negligently and slovenly applied, or that it be long enough persevered in for obtaining that wished and necessary end.

I have but two remarks to make before I dismiss this important

portant subject, the more important, because it is one of the passports for future and sometimes long disease; and because, by it being effectually opposed in this stage, either when the attack is in the most simple form, or in that compound and intricate form which I last observed upon, no other future bad consequence will follow.

One is, why I have recommended as a dressing to Chancres thus conditioned, lint dipped in spirit of wine. The first idea of applying spirit of wine was imparted to me by Doctor Nooth, Physician-General to the Army. After repeated trials, I am warranted in saying, that the local action of it deserves preferable attention: it resists a sphacelating disposition in the part, and yields nothing from itself that will become acrid; it collapses the vessels, and takes off inflammation. To Dr. Nooth I am also indebted for the vitriolic fluid, in page 221.

The other is, that I have not noticed the exhibition of hemlock, from a perfect conviction of its being not in the least serviceable in any stage of that case we have been treating upon. It appears at best to be but a medicine possessed of strong power without a proportion of good arising from it, even when applied to those cases where it has been found to be most applicable.

Beirken says that it aggravates a Chancre. I never saw any

fair prospect even from it in any form or for any disease, and have therefore nothing to say in its praise. A valuable medicine will leave no doubt amongst men who practise of its good effect; and as this is more and more doubtful, I am apt to think that the efficacy which it possesses has not done any certain good in practice.

It is not possible to treat upon every circumstance which may arise in the cure of this part of the disease: there will be discriminations in every case which may be necessary to be made, but which might not be thought on, but from the pressure of the occasion. If my outlines be just, the subordinate considerations will follow in course. The main point is to take care that no venereal atom be left behind the desisting from mercury—to see that no hardness remain when the Chancre is cicatrized—that no fresh symptoms appear on other parts—to watch the affections of the inguinal glands, of the throat, and the skin—and if there should be pains on the tendinous parts, or on the periosteum of any bones—not to forget that they might have arisen from a venereal cause.

It must be remembered, that wherever these compound cases obtain, the specific effects from mercury are not so thoroughly certain as in more simple cases. And it must also be remembered, that these compound cases are produced generally from the intemperate habits of a patient; and that the

the same patient is most likely to be rebellious to regimen, and to submission in a continuance of that necessary quantity of mercury, and that necessary time for the action of it, which will be found to be indispensable for the perfect extirpation of virus out of the constitution. Nor must you be staggered, become irresolute, and fly from opinion to opinion, from medicine to medicine, because a Chancre spreads, or because it does not yield to the adopted system immediately: for if the mode of treatment be just, every deviation from it will be of course wrong. If you be not prepared before-hand, and do not from deliberate reason reflect, that you have the constitution to amend, and the disease to correct, before the symptoms will in the least abate, impatience and confusion will take the lead, and precipitate and contradictory treatment will be the result: remember always, that you can only impart that confidence which you feel you possess.

From what has been said, it will be evident to you that the main distinction for your consideration is, the relative conduct and condition of every patient who thus is committed to your care: that a temperate patient will experience from a Chancre the simplest consequences, and an intemperate the reverse. Observation will soon point out to you the truth of my given rule. If the patient be a stranger, you must discover his habits; and if he be your

acquaintance, you will draw your conclusions without interrogation, and prepare for success by admonition. The virus is always the same, and there generally is no other cause of difference in its action, than that which I have explained.

Some time since, a patient applied to me, who had a Chancre concealed under his prepuce, which was swelled too much for me to denudate the glans, and expose the Chancre. He was an intemperate liver. His pulse were quick and strong. I bled him, fomented the Penis with decoction of poppies and spirit of wine, injected the same under the prepuce, poulticed the Penis with oatmeal mixed with yeast, which I think a good antiseptic; applied leeches, and gave him the pulvis antimonialis mixed with opium. These remedies were administered on the instant. In the course of two days I began with frictions of the mercurial ointment, and continued the whole of these means without intermission. Notwithstanding, the fever increased, the Penis became generally inflamed, uncommonly swelled, and the Chancre, at the expiration of a fortnight, had destroyed the prepuce, part of the glans, and more than half of the skin of the Penis, before any check could be given to its progress. Bark with opium had, during this stage of it, not been omitted. At the end of a fortnight the symptoms began to be favourable: the discharge became kindly,

kindly, granulations shot forth, the tumidity of the Penis abated, the mouth became sore, and the whole healed in due time, according to the highest expectation. Here ends the case, so far as it refers to the subject we are now upon. The sequel of this case is so important for the illustration of constitutional infection, that I shall give you the whole of it, when I come to treat upon that subject.

LECTURE XXII.

ON

THE CURE OF BUBO.

I Have already premised—and the experience of every day's practice confirms the fact—that an enlargement of the inguinal gland or glands will be often the first indicative symptom of venereal infection: and I have already premised—that it is the foreign fluid only, imparted by another subject, which, by being absorbed, acts and contaminates the constitution—that this poisonous foreign fluid sometimes leaves local impressions, at the commencement of its progress into the constitution—that these local impressions are not a sine qua non of venereal infection—that the constitution may be infected, without a single instance of such local impression—that the discharge from these local impressions is merely a result of the action of the foreign fluid—that that discharge is a secretion proper to the subject who secretes it—and that any absorption of it is of no importance, nor has any reference to that poisonous infection

of

of another subject, which does proceed in contaminating a constitution. Nothing can confirm these truths more strongly, than that this local impression is arbitrary—than that the disease may yet go on—than that the constitution may be as effectually infected without there being any local discharge from any local action of the foreign virus to be absorbed, as when there is discharge in consequence of this local action of the foreign virus, and when it might be absorbed. For if absorption of our own fluid supported the disease, and if the urethral membrane had not an absorbent power, there would be always enough of it lodged upon every part out of the Urethra, which we know does absorb foreign virus, and which we know does thereby contaminate the constitution. The same may be said of the fluid of a venereal Bubo, and the same of the fluid of a Chancre. You will therefore be pleased to reflect, that I am now advancing a fact, which confirms my theory to be also a fact strongly and positively enforced by the evident action of the disease.—There is no analogous reasoning in this question; but the fact of my theory is proved by the action of venereal fluid.

When a Bubo arises in consequence of a previous local infection, which will be either Gonorrhœa or Chancre—that Bubo may be known to be venereal: but when it arises without such primary impression, and without any other apparent cause but a suspicious connection—that will be a sufficient inducement for determining it to be from a venereal

real cause: for I believe inguinal glands seldom come to abscess but from a venereal inducement. And when there has been a suspicious connection, it would be madness, and not the reason even of a skeptick, that would lead one in search of another cause.

When the Venereal Disease has completely tainted the habit, when the constitution is in consequence labouring under the disease throughout, when it is in an hectic state, and when the habit is wasting fast—the whole of the lymphatic glands will then be found enlarged. This is not that part of venereal effect which I am proceeding to treat, but it is purely the local impression of the foreign virus upon inguinal glands from recent connection.

There are two points which are most materially to be discriminated—the anatomical nature of the part, and the nature of the stimulus:—that it is venereal virus acting upon and disturbing a glandular body, and aiming through it to obtain a passport into the constitution;—and that the knowledge of treating glandular inflammations and glandular indurations, be the cause what it may, is not so certain, and is not so well defined, as the same knowledge of treating other anatomical parts badly conditioned. A gland, once disturbed, does not possess that direct and ready disposition for recovery, which other parts possess, after the cause which disturbed it has been completely removed.

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The management therefore of a Bubo must be regulated with a continual reference to the nature of the part, and to the possible power which the foreign virus has by it of infecting the constitution.

It must be observed, that a gland may, when once diseased by venereal virus, afterwards be continued in disease, from too long a continuance in mercury. And it must be observed also, that because a gland does not yield to the mercurial action that is calculated in general to be sufficient for removing venereal actions on other parts locally infected, that from hence more mercury will be often wrongly employed in the cure of a Bubo, than will be necessary for curing the most confirmed venereal symptoms. And it must be observed, that from an increasing sedulity, arising out of the obstacles which offer in consequence of venereal Bubo, more is often done than is necessary to be done; and often that which is done for promoting a cure, instead of answering that end, continues the disease, and produces every contrary effect to that which was solicited. Every day's observation convinces me of this truth: and the same observation shews to me, that the common people never experience so many distressing situations from the venereal Bubo, as those do to whom mercury is more profusely given for dispersing it, and to whom, when it be not dispersed, mercury is still given to the end of the case, or as long as the patient will permit it, seeing that his case is growing worse and worse

by

by it. In this instance, the zeal being founded upon a wrong theory, that zeal persisted in will clearly multiply the wrong.

The application of mercury, previous to a Bubo coming to abscess, for an intention of dissolving it, demands a particular consideration from each particularity in the symptoms attendant upon the Bubo. For if, from a certain idiosyncracy the Bubo inflame very rapidly, if the fever run high, if the pulse be extremely quick, and if the stimulus excited produce at night profuse perspiration, I do not think that is a favourable state for any fair promise of success in the action of mercury for dissolving such a Bubo. The additional irritation which the mercurial stimulus will excite, the time which this stimulus will require for effectual action, the obstacles which it will meet in that action, from the venereal stimulus already in action, must terminate to the prejudice of the constitution of the patient, and of a cure of his Bubo. For it must not be forgotten, that a regard should all along be paid to the condition of the patient. If a profuse application of mercury should, in this stage of the case, fail of the intention, and if the Bubo (which is most probable that it will notwithstanding) should come to abscess, a foundation then is laid for future malignant symptoms, and for future doubts whether mercury will hereafter be necessary or not. The case will be protracted, and be made more dangerous and complex.

When a Bubo betrays signs of coming rapidly to abscess, and sooner than the action of mercury upon the constitution could take effect, we are assured that the fever will subside with the formation of matter—and when that is discharged, the mercurial process may fairly be began upon, the action of it can fairly be ascertained, and the effect in consequence will be fairly defined; less time will be lost to the patient in the cure, and no dread or doubt of necessary or unnecessary perseverance in mercury will be confounded in the question.

If there were no such distinctions as these, the same means that would dissolve one Bubo would dissolve another; the same fever would be attending upon each, and the same result would be conclusive for all. But, independent of these reasons which I have produced against the probability of mercury dissolving a Bubo, when it is thus acute in its consequences, and which are the result of practice, I have every reason to urge the same arguments from the result of true theory; not only from my general opinion of the action of mercury as a powerful stimulant, but from that particular distinction which I have made between the pure local action of mercury upon the local action of virus, independent of constitutional action, and which I deny to be any action in any way relative to venereal virus; for that action must consist in its constitutional affection. Therefore, if mercury cannot be thrown into the habit before the Bubo has already

already come to abscess, cui bono should mercury then be administered?

I am here naturally led to make a few observations upon the contrary opinion to this doctrine, and I am sorry that I am again at issue with Mr. Hunter. I am almost ashamed to make his opinions so often the subject of our investigation. Mr. Hunter adopts the same system for every Bubo, and he has positively declared, that throughout his practice he has, by that means, dissolved every Bubo but three.—This cannot be contradicted in any other manner than by positively asserting, that the same success, by any other Practitioner, neither will nor can be obtained. For the very intention which Mr. Hunter professes, he does not trust to; and if he did, he would ever be defeated. If Mr. Hunter, or any one else, were to apply only so much mercury, to the glands of the groin, through the same absorbents which absorbed the venereal virus, and were to expect that, by the mercury coming into contact with the virus when it was locally acting, the mercury, as specific mercury, would locally destroy the local action of the virus—then such success would also prove, that the application of mercury upon the local seat of a Chancre, would cure it or a Gonorrhœa in the like manner; but so far from the idea being true, mercury will betray its genuine local power, by an aggravation of every local venereal symptom.

But although such is the theory of Mr. Hunter, he is found not to depend himself upon it; for he still persists in the use of more ointment, and procures, as soon as possible, the constitutional action of mercury for the cure of this, much sooner than for any other venereal symptom whatever. And thus he has done, without either discriminating the rapid inflammatory Bubo, the fever attending it, or its immediate approach to abscess. He adopts the same treatment alike to all Buboes. Can that be, either in theory or practice, the wisest system for obtaining the most successful end?

Having stated, that when the symptoms of Bubo are truly inflammatory, and in consequence of which it will necessarily come to abscess, and having urged from that cause not only the improbability of preventing it through the action of mercury, but also the pernicious effects of an ineffectual stimulus which would arise from mercury thus applied, so far as it regards the promotion of future embarrassments in the case, I shall proceed to the treatment of a simple Bubo, and which is not attended with such inflammatory symptoms. Mr. Hunter, in the same page where he has avowed a positive certainty of success by the treatment which he had adopted, also avows, that after every known method has been used, Buboes cannot, in all cases, be resolved, but come to suppuration. And as we all know that it is not in human skill to dissolve every Bubo before it comes to abscess,

cesss, so we all are convinced that there is not a method yet known which will or can to a certainty effect it. The rapid progress of a Bubo, and the fever which accompanies it, appear to me to be the only obstacles against attempting the cure of a Bubo before it comes to abscess. And therefore, I see no other reason why it should not be attempted, for if the attempt succeed, time will be saved by it; as it will also when the Bubo is sluggish and hard, and when, without the virus being destroyed, it will be likely to remain so, until other venereal symptoms may follow from it. But the method for effecting this should be attended to, and in my opinion it is of no little importance.

Whatever virus be lodged on the gland which is the seat of the Bubo, cannot be influenced, as a local poison, by mercury, unless it be influenced upon the same principles, the same regulations, and the same true laws of mercurial action, by which other local parts, venereally affected, are influenced, namely, by the action of mercury upon the constitution—by the change alone which it produces upon it: and as the gland which is the seat of the Bubo will only be additionally stimulated by a passage of mercury through it, without locally destroying the virus, it therefore is not only preferable but just in true theory to introduce the ointment into the habit by the other thigh, that the gland may not be vexatiously and vainly irritated, when the object is that it should not: for if the gland be not provoked by any

other

other cause than venereal stimulus, so when the part is freed from that, the tumour may and will the more readily subside. In the cure of a Chancre, no one presumes that a local application of mercury will produce any friendly effect; on the contrary, it will only aggravate. It has been tried time out of mind, and it has been abandoned from experience. Then why should it be presumed that mercury will effect more on one local part, towards the destruction of virus, by chymically neutralising the virus, than it will on another? The reason is still the same, and as strong against the possibility in one instance as in another.

But so far from the action of mercury being certain in reducing a diseased venereal gland, under the most favourable mode of applying it, the event will always be uncertain. And this uncertainty in the event is always dependant upon the idiosyncracy of a patient, and the state of his health at the time of the application of mercury. For if the stimulus of mercury inflame the Bubo, or if an increase in its quantity for bringing on the necessary mercurial change should prove to be requisite, then by the former a sluggish indurated Bubo will be brought to abscess, and by the latter it will remain obstinate in its sluggish state, for a longer time; the constitution of the patient temporarily suffering an ineffectual stimulus from the mercurial action.

To

To ascertain how far it be safe, prudent, and justifiable to urge the action of mercury for dissolving a Bubo, and curing the disease, without the Bubo coming to abscess, the mercury should be administered with the same expectations as in the case of a Chancre, and with always a reference to constitutional condition: the similarity of the state of the patient will exact similar effects from mercury in each complaint: when the case is simply venereal stimulus upon a pure constitution, and that in its coolest state, as much salivary effects from mercury as will cure a Chancre will destroy the venereal stimulus on a gland: and at any rate, if such effect does not succeed, mercury should not be gone on with; but rather the patient should do nothing more than endeavour to amend the habit, and leave the future consideration of his case to future eventual circumstances. But it cannot be conceived how many Buboes will subside, when the mercury is withdrawn, after the inflammatory stimulus by it has been perhaps supported from a contrary expectation too long a time.

I trust that I have made hitherto my intention obvious to you. My intention is to impress you with these truths—that the venereal virus will in this instance exact no greater effect from mercury, than for the cure of any other local complaint—that the case is compounded with a glandular affection—that the stimulus from mercury will inflame glands—that when the stimulating cause is taken off, glands will

will subside with more facility by the patient being restored to a state of health, and by his living in such manner as to promote and support that state of health—that there is not essentially so much to be dreaded from the original cause, which is the venereal attack on the part, as there is from a continuance in mercury after a cessation of its requisite good effects—and that too much sedulity in reducing the gland, through mercury, has brought on all the bad conditioned Buboes, which are found in practice, or marked in the history of cases.

A prevention of all these bad conditioned cases is more important by far than a cure of them. Precautions must be taken, in the first instance, for preserving the constitution from being exposed to malignant consequences. A right beginning will preclude a wrong event; so will the neglect of it oblige us hereafter, when these bad conditions do obtain, to desert the wrong and begin with the right. If a patient with a Bubo has lived freely, if his habit be heated and inflamed by drinking and other irrregularities, these should be corrected, and the effects of them repaired. It is always an object, that mercury shall have no other stimulus to contend with but the venereal. A system in practice—a true observance of that system—a strict regard to the patient's health—and patience in observance of the indicating intentions in the constitution, will do much for the cure of a Bubo. As little mercury as can be only necessary, will only

be

be justifiable—to discriminate when its good effects cease, and its bad commence, is a line not difficult to be ascertained, when the truth of such a natural event is known and attended to. In short, it will be much safer to give too little than too much mercury, in the diseased state of a Bubo: the latter cause will be more tedious to be removed, and more dangerous than any venereal symptom, purely as venereal, which may hereafter occur. The constitution for the cure of a Bubo will exact particular attention—for it must be remembered that the symptomatic fever is often of serious importance, and that time and proper treatment should be exacted for its recovery of debility from that cause. I have seen a patient as much reduced by an inflammatory Bubo, as he could have been in the same given time by a consumption. Then, without amending such a state, is there any good to be hoped for from mercury? Certainly not.

I shall now proceed to treat a Bubo which does come to abscess. When it is evident that a Bubo is coming to abscess, and that it cannot be prevented—do nothing medicinally, wait the event—let the patient enjoy pure air—let his food be nutritive, and his habits such as should be practised for perfect health—let him be rendered fitting to support the process, and to be in a future condition for the kindly action of mercury—let the Bubo remain as long as it can without being opened—let as much of the indurated part as possible be dissolved, and the Bubo will plainly demonstrate

the choice of time for a discharge of the matter by its natural approach to effect it. The assistance which is necessary for effecting this will be by the application of lapis septicus, by only rubbing it up and down, so as to obtain an opening on the most dependant part about the size of a thumb nail, by laying on a poultice to the part and by waiting the continued discharge of matter for a week, when all fever will be at an end. Then is the time for considering what mercury has been given, if any has, and what must be still given: by referring to the effect which a former application of it had, you may better ascertain any further relative necessity for it. In the course of a fortnight the sore will be clean, and will take on a disposition to heal. Mercury, when it has done so much, should still be continued, unless it be observed that the process of healing hesitates, and which it will always do after it has looked most promising, where mercury has been pursued too far. If such hesitation take place, mercury should be directly left off, and the constitution left freely to recover itself. Change of air, and simple diet will then finish the cure. But if mercury be gone on with, when the face of the Bubo indicates a change for the worse, every symptom will be aggravated, and the promising sore, from an almost certain appearance of immediate healing, will be regenerated into a bad conditioned ulcer, which no topical application can serve; but which, whenever it is the case, must be cured by abstaining from mercury, and by giving the constitution an exacted time for the bad state

of

of it to be repaired: and that sometimes will be found to be an intricate and arduous task, a tedious and dangerous process, because mercury has been so injudiciously continued.

In my opinion, when such is the case, there is nothing so effectual as pure air, milk diet, warm bath occasionally, and bark and opium, for obtaining a cure. Opium should be given freely at first, and gradually left off in the end, seeing that it suspends the stimulus which is irritating the habit, and that the effect from it can only be supported by repeated doses: but if it were continued longer than the stimulus which it was intended to suspend, it would be found to act prejudicially, and the constitution would of course be alike injured from that cause; or the recovery of it to a pure state would at least be protracted. From the positions which I have laid down, it will be evident, that it is either owing to the inflamed condition of the constitution by irregularity in the patient, or to the effect of the febrile symptoms attendant on a Bubo, or to an excess of mercury--that it is owing to each, or to all these causes, that a bad conditioned Bubo ever obtains---and that when these causes are watched, and when they are previously guarded against, there never will be an embarrassment of that nature. When a constitution is heated or inflamed by irregular living, preparatory to any attempt of mercury, that should be corrected; and time should be allowed for the patient to recover from the effects of a symptomatic fever, during a Bubo coming to abscess---

for if mercury be given instantly, the febrile pulse will never subside, nor will the fever abate; as the constitution will be additionally oppressed by the influence of the mercurial stimulus being immediately joined to the venereal.

It is a fact of the first importance to be attended to, and which proves to you better than volumes written upon the subject can prove it—that when a bad conditioned Bubo appears early after the opening of an abscess, either mercury has been previously given for dissolving it—or the constitution of the patient has been otherwise harrassed by excessive high living. But in the former instance the case will be still more plain---in that the indicative cause of a change in the condition of a Bubo points out itself: for if the patient has gone on regularly, and if the sore has been almost brought to an immediate point of healing—what other proximate cause can there be for its degeneracy but that mercurial system which has been injuriously persisted in?

Topical applications are next to be considered, both as they will be necessary to healing a Bubo, as well as for a Bubo bad conditioned. I will in the first place remark, that I am never in dread of an induration from Bubo, knowing that every stimulus will support it, and knowing that it will be gradually dissolved, whenever the stimulus from any cause

cause which is the support of it, be completely removed. It will be by the operation of time alone that an indurated Bubo will subside. When a Bubo has began to granulate and look clean, the poultice which was continued, from the time it was opened, may be then omitted, and the dressing may be dry lint; or if occasion should indicate it, vitrioliated lint in the mildest form. I have now the opportunity of pointing out to you, that by my viewing this state of a Bubo in the same light which I do a common sore on any dependant part of the body—by my being aware and provided against a pernicious abuse of mercury—and by my exacting from the patient, that he avoid any irregularity, I am ever perfectly at rest about any future dread from a bad conditioned Bubo. A Bubo in this state will experience a miraculous change, by the application of a flannel bandage to it:—but I will urge the observation no farther. It is for those who through my recommendation adopt it in that state of a Bubo, which I have here pointed out, to praise or blame me for the idea hereafter. When a Bubo is situated low down the thigh, the application of a flannel roller will not be difficult; but when it is situated in the groin, there will be some necessary care for fitting it. Care must be taken, that it does not partially press; for if it should, it will inflame, instead of keeping off inflammation. This idea I took from analogy in the cure of ulcers on the thighs and legs, and you will experience how true it is to reason.

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With respect to a bad-conditioned Bubo, nothing can be done, or expected to be done effectually for it, until its amendment evidently arises out of a correspondent amendment in the constitution. But a progress of it may be checked, and a treatment of it topically may be conducive for guarding and repairing any destruction of parts, and for advancing them to a progress of healing. Fomentation of poppies, to two-thirds of which spirit of wine should be added; lint dipped in spirit of wine should be laid on the ulcer, and over that a poultice of oatmeal and yeast. When a favourable change in the ulcer is apparent, it may be dressed and rolled, as I have before recommended. I will close this subject by the application of practice to theory, in two cases which have recently been the objects of my attention.

A gentleman applied to me in June, 1790, with two Chancres, extremely simple, situated one upon the frœnum, and the other at a little distance from it. By dry lint alone, and by mercury given in the form of ointment, these Chancres proceeded to heal without much spreading, and with no other circumstance worth remarking. But before they had entirely healed, a pimple was discovered on the external part of the prepuce, upon that part which is facing the abdomen. This pimple was at first only red, and the part around it was thickened. I was rather surprised at the sight: by mercury having been previously given for the other Chancres, the appearance of this was changed from

from the form in which it would have been seen to be truly venereal, into another form; it was the appearance of a Chancre influenced by mercury, but not destroyed, and deprived of venereal action. It was the disguise of the Chancre which made me hesitate, and therefore I stopped a continuance in mercury, for two reasons; because I saw that the constitution was a good one, and because I doubted whether this was a Chancre or not. The ascertaining that fact, in the first instance, I judged to be of more importance than securing the cure of the first Chancres. By discontinuing mercury, the suspected Chancre appeared evidently a true one; and then by returning to mercury, the two first Chancres healed; but this last stood out for a longer time, discharging ichor, and reddening around it. The gland of the groin began to swell, and was rather painful. I continued mercury very freely for ten days, and found at the expiration of that time that a Bubo had come very forward, and that the fever had a considerable effect upon the health of the young gentleman. I therefore dropped mercury, and did not see him for seven days after, when I found that the Bubo was ripe for opening, and that the third Chancre was red, hard, but not ulcerous. I then applied lapis septicus to the Bubo, rubbing it horizontally along the dependant part, so as to procure an oblong eschar, in the direction of the inferior part of it. I desired him that night to lay on a poultice—to take only some elixir vitriol three times a day, in cold water, and to live in other respects in such a manner

manner as was most conducive to health. I saw him the next time ten days after, when I found that his fever was gone, and that his spirits and appetite were mended. The ulcer had not as yet a clean appearance, but there was no hardness about it. I then began with mercurial frictions, which were continued on for near three weeks, until the sore was just upon the point of healing, and until the third Chancre upon the prepuce had for some days been quite healed. At this period the patient became drowsy, he had languid swoonings, his pulse were feverish, his eyes were hot, and the Bubo was swelling. The little sore appeared glassy; I withdrew the roller, and applied a poultice to it, desiring him to take lodgings in the country, to go once into the warm bath, to return to his elixir of vitriol, and also to wash off and refrain from mercury; and if he added any other medicine, he might, if he pleased, take sarsaparilla: I farther told him, that when the renewed inflammation subsided, he might then return to the use of the roller again. This he did, and soon got well, as soon as from reason it could be expected.—The leading points which claim our present attention in this case are, that the application of mercury, fairly and regularly administered with a view of dissolving the Bubo, failed; and that after the Bubo was opened, a continuance of mercury really produced that mercurial change in the sore when it was just about to heal, and which indicated a necessity for then discontinuing the application of it. The patient had

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all along been submissive to proper regimen, and obeyed instructions with exactness.

A gentleman was at Plymouth when he first perceived a Chancre, for which he took mercury in the alterative manner. The Chancre soon after appeared to be healed, when he desisted. Five weeks afterwards, he came to town, and then I saw him. Upon examination, I found that the seat of the Chancre appeared red, felt hard, and upon pressing it discharged an ichor. I found that one of the glands of the groin was swelled, but was not tender. The gentleman was grown thin, and had febrile symptoms upon him, and I dare to say, that in a very short space of time he would have experienced constitutional symptoms. The inguinal intumescence, as I before observed, was free from pain, and had been stationary a long time, and I am clearly of opinion, that it would have continued in that state, if mercury had not been necessarily given. As this gentleman was going abroad at a stated time, I proceeded to the cure with mercurial inunction, and the Bubo as regularly, during the application of mercury, came forward to abscess. The Bubo, after coming to abscess, assumed a very kindly appearance in consequence of my desisting from mercury, upon finding that it was certain of coming to abscess. But as the gentleman was going abroad, and as the disease had been long upon him, I again returned to mercury, during the healing process of the Bubo, and continued it for fifteen days. As soon as

the effects of its action became evident, the Bubo again, although just healed, betrayed signs of fresh inflammation. I desisted then from mercury, and my patient went to sea; the inflammation subsiding, the Bubo healed kindly without mercury, and no other symptom afterwards appeared.

Having been studious to exactness in explaining minutely to you the causes of bad-conditioned Buboes—that they are either produced by an abuse of mercury, or by intemperate habits of the patient—it may fairly be said, that my duty is discharged. But before I wholly dismiss the subject, it will not be improper for me to remind you--that whenever such a case arises, you must be prepared against that dissatisfaction which will be expressed by the patient, although generally by his irregularities the case has become thus desperate. Nothing is more commonly said, and no tale is received with more avidity than that told by a patient—of having deceived his Surgeon. But you will only hear of this sarcastically, when the patient, notwithstanding, gets well: ---if his case prove otherwise, you will find the pleasant sarcasm exchanged for open reproach. The reputation of a Surgeon—like the sensative plant—fadeth at the touch; and therefore, it cannot be too much sheltered and protected from the inclement and blasting tongue of slander. No success in practice can be obtained but by an implied submission in the patient to rule and to regimen.

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I attended a case during the last Summer, with a very ingenious young Surgeon, where a Bubo, just as it was upon the point of healing, betrayed the most malignant symptoms, and where the blame of it was clearly with the patient. This bad case continued during the Winter. Every means which I have stated were tried. But his constitution became so depraved, that no method of treatment appeared to claim a preference. Wearied, at length, by ineffectual remedies, we recommended the patient to go into the country, and in process of time his ulcer healed.

LECTURE XXIII.

ON THE

CURE OF VENEREAL SYMPTOMS FROM CONSTITUTIONAL INFECTION.

I Shall now proceed to remark upon the cure of constitutional symptoms. I observed, when treating upon the cure of Bubo, that sometimes it was the first symptom of local venereal infection—that the first impression of the disease was found on that part—and that the first knowledge of the disease being present, was first imparted by that symptom. I shall now again repeat what I have advanced, and which is, that sometimes the first knowledge of the presence of the disease will be discovered from constitutional symptoms of venereal infection. This truth plainly indicates a necessity of discarding the idea, that secretions, arising from local venereal symptoms, have any concern either in promoting the disease on the constitution which secretes them, or on the part by which they are secreted. And this truth indicates also a necessity of admitting the idea, that it is the foreign

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virus, only in the form of fluid, which, by being thus absorbed into the constitution, infects the whole of the constitution —thereby producing signs of infection, evident, demonstrable, and proper to the Venereal Disease alone. After what I have already said upon this discovery, it may be unnecessary to push the argument any farther---but it is evident, that the truth of it pervades the whole of the venereal action.

The cure of every venereal symptom alike consists in a due action of mercury upon the constitution; therefore much more upon the subject cannot be necessary. Judicious discriminations in the use and abuse of mercury, can be somewhat farther observed upon: the constitution, independent of the disease, will exact them; and the disease, independent of the constitution, will also exact them. For there is a time when the disease must be protracted, for the sake of the constitution; and when the constitution, for the sake of its preservation, must necessarily get rid of pressing venereal symptoms. And although the disease and the cure of it are, upon a general scale, as familiar to almost every person as any two facts whatever, yet so difficult has it been ever found to form certain principles for eradicating the one by administering the other, that neither time nor practice have as yet established a given direct rule for successfully effecting the intention: because every case is dependant upon each constitution proper to every subject, and therefore

therefore every case has its own particularity. Without seeing the case---without trying the effect of mercury upon the constitution---and without looking to the event, it is impossible to pronounce in a decisive manner whether the disease be eradicated, when the symptoms of it are extinguished ---or what will be the effect from mercury on a constitution, before it has been tried.

From our present knowledge of the nature of the poison, and from our present knowledge of the action of mercury, we are enabled to discriminate true venereal symptoms, and to ascertain a judicious application of mercury---we are enabled to conduct the process of mercury, by observing its relative action upon the venereal symptoms and the constitution---and we are enabled, as far as the limitation of human power permits us, to know how long mercury ought to be continued, so as to eliminate the disease. But from that limitation of human power---from our being forced to trace causes through effects, we find it will not be always certain, that the disease--when it has been a long time associated with the constitution--when it has been assimilated with every part of it---when every fluid and solid may be said to be under the influence of the poison—by no one single mercurial course can be perfectly eliminated, although mercury might have eliminated the evident symptoms of it. In such cases, mercury should be administered in a longer process---three or four months should be taken for the action of it, and the

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constitution should be, during that time, supported under this continual conflict.

When symptoms of the most obdurate infection have been completely done away, the patient should be allowed time for recovery, and the return of symptoms should be carefully watched:—always recollecting, although the action of mercury might not as yet have completely eradicated the disease, that it is notwithstanding the only specifick for obtaining that end—and that the default is never in mercury, but in the constitution, with which the disease is so completely interwoven. Although it would be imprudent to persevere in mercury, long beyond the time of a disappearance of evident symptoms of the disease, so should it be reassumed whenever there be a palpable return of those symptoms. I cannot put this argument in a more strong situation, than by comparing it to the case of a fire. When an house is on fire, the engines are brought: from these engines are poured as much water as the firemen presume will extinguish the fire: but although the firemen are induced to desist from pouring on more water, because it appears to them that the fire is already extinct, and which is the only way they have for directing them how to act, yet they do not directly shut up the plug-holes, nor withdraw the engines from the spot; but they wait to see if the fire break out afresh, and if it should, they do not then infer that water is inadequate to the purpose of extinction of fire—they do not fly to any other power

power for that purpose, but they begin again in pouring on more water, and in the end the fire is extinguished by it.

It appears the most improbable of all expectations, that for the most trifling symptom of constitutional infection, the alterative plan of curing the disease, whilst the patient associates in company and goes about in the open air, can be depended upon. It appears, that the action of mercury, where the alterative method has been ineffectually practised, will fail of its due effect—will be acting upon a debilitated constitution, when afterwards the true method of applying it is adopted, immediately upon a default of the alterative method.—It appears, that the alterative method is only winked at against conviction by those who permit it, and by those who are conversant in practice.—It appears, that if there were no better prospect from future resources—no alternative after the alterative method has failed—that the disease could never be cured.—It appears, therefore, that there are but two rules to be constantly observed in the application of mercury, if you would hope for success—That the quantity of mercury which is required to perform a perfect cure, may act upon the constitution with a force that is necessary for accomplishing the desired effect—and that the quantity of mercury which is necessary may continue to act with an equal force, and may support that change which it produces for a sufficient length of time, until there be no part ineffectually acted upon---until no atom of infection be latent be-

behind, by which a relapse of symptoms may follow. They offend against the former rule, who administer mercury in too small doses, or at too great a distance of time, by which there is not a sufficient quantity conveyed into the habit for producing the requisite effect; and they offend against the latter, who discontinue too soon the application of mercury, by which the change which is produced in the habit, is not supported for a sufficient length of time for eradicating the disease. In the alterative method of administering mercury—it is common to offend against both rules: hence it appears, that this method so often fails of success; for the violence of the symptoms being mitigated, the disease only remits, the infecting principle is not destroyed, nor consequently the disease extirpated: and hence proceed those anomalous cases, where the symptoms vanishing, the patient indulges the hope that every thing has succeeded to his wishes; but, after a short respite, he relapses into a condition of disease worse than ever.

From all that has been said, it will naturally be concluded, that the chief discriminations which the cure of every constitutional venereal symptom exacts, are those which arise out of the obstinacy of the symptoms—out of the various parts diseased—out of the length of time that the disease has remained—and out of the immediate deranged health and deranged condition of the patient.

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The primary symptoms of an infected constitution generally appear on the softer parts, as I before remarked, shewing themselves by eruptions on the skin, and ulcers on the tonsils. These are also generally the result of preceding febrile symptoms, and evidently denote the infection of the whole of the constitution. For the cure of the eruptions, the mercury should be introduced in the form of unction, as evidently preferable in every respect; and the perfect cure of the eruptions, as well as the prevention of future symptoms, exact a strict attention to every minute circumstance, as if the symptoms had remained longer and were more multiplied. By increasing the proportion of mercury according to the action of it regularly every day—by bringing on a mercurial change—in the course of a fortnight the symptoms of the disease will begin to vanish—and by continuing mercury for the support of this change a fortnight longer—the whole of the eruptions will be effectually removed—leaving the skin smooth, over which a new cuticle will soon be formed; by this the hardness of eruptions in harder parts, and clefts in the hands and feet, will also become smooth and healing. This symptom of the disease is the most pleasant of all venereal cases to be cured. The contest will be fair between the mercurial action and the venereal: and mercury strictly and successively administered, will gradually extinguish the venereal action, without an interference of any topical applications.

There will, in these recent constitutional affections, be commonly some trace of disease on the original local part that was first the seat of the disease, and previous to its being absorbed into the habit---there will be either an hardness upon the seat of the Chancre, a weeping ulcer, warts on the part, a thickening of the prepuce as if it were horny, or an enlargement of the inguinal glands, or an ulcerated Bubo, or an ulcer any where else. All these commemorative signs of prevailing constitutional disease will surrender their virulent marks as the eruptions on the skin disappear. The dulness of the patient, the ringing in his ears, the loss of appetite, the emaciation from fever, will also be totally evanescent. And although he may appear wasted through mercury and regimen, the first fair day of fresh air will shew a change in the whole of his complexion—the constitution being freed from the agency of both the poisons. All this is procured through that specific power of one medicine alone, which the single or joint power of the whole materia medica could not besides effect.

But although mercury possesses a sole power of eradicating the disease, yet that cannot be obtained without its action being continued for some time after the venereal symptoms are eliminated—not even on a constitution in the most favourable state for its action.

An Officer who had been quartered in Birmingham, applied

plied to me in the Spring of the year 1790. He had febrile symptoms, was wasting, his appetite failed, and a strong constitution was impairing. Eruptions were apparent on his forehead, shoulders, arms, and thighs. An hardness remained on the part where the Chancre had been, and the glands of the groins were knotty. The Chancre had healed about six weeks. He had been taking mercurial pills from time to time, but had never confined himself. We began with mercurial frictions, a drachm and a half every night was rubbed on the thighs, and in the course of ten days his mouth became affected. The same proportion was continued three days longer, when his mouth became so affected, that he only repeated his mercurial frictions every other night. During the process, the eruptions gradually disappeared, and at the expiration of three weeks, the Officer being impatient to return, and tiring of his mercurial state, left the mercurial symptoms to subside, and joined his party at Birmingham, more satisfied with his condition than I was. In a short time after, he went to the West Indies, and after remaining there four months, returned with the following symptoms. Eruptions more generally over his body, pains in his cylindrical bones, nodes on his forehead, and a constitution more wasting than by the former attack. He very chearfully submitted to a second process. His experience pointed out to him, that my opinion was right. He gradually continued mercurial frictions for seven weeks, and

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owing to a kindly action in mercury upon a constitution radically good, he got then completely well.

Had this patient, in the first instance, but persevered a week longer in the use of mercury, he would have prevented a return of the disease, and second sufferings of a cure. The folly of being too impatient, and desisting from mercury too soon, is as great in a patient, as in him, who, in order to arrive at a place of destination, sets off to perform his journey---turns back before he has reached it —and is afterwards forced to set off again.

The next symptoms which I mean to treat upon, are venereal ulcers on the tonsils, and which, if neglected, will also persist in destroying the uvula. Sometimes both tonsils are affected, and sometimes eruptions on the skin are concomitant with ulcers on the tonsils. An ulcer, thus situated, first attracted the attention to fumigation, but which is now, in my opinion, with great justice exploded. My wish is, never to disguise any venereal symptom---never to apply any topical remedy to it, if I can avoid it. I wish to be apprised of the worst of the venereal action, and the whole of it---and I wish to see the pure action of mercury take effect. For that reason, instead of topical applications, I exact from the patient to submit to rule---that the full force of mercury may not be interrupted, neither through omission nor through intemperance. By leaving the ulcer to the event

event of the action of mercury solely, I see when the venereal part of the disease ends, by the ulcer healing under the kindly action of mercury; and having once seen the sanative power of the action of mercury, I know that if the ulcer—after it has commenced to become clean, and after it has almost been healed---again shews a disposition to increase, inflame, and become a spreading ulcer---that it then is no longer a venereal ulcer. Without leaving the parts to the prevailing action of the two poisons, this could not be known: nor without the first effects from mercury, which were kindly, being succeeded by second effects, which were marks of the abuse in the power of it, I should neither know when to distinguish the predominant action of the one from the other, nor how to treat that ulcer---where from no peculiarity in the proper appearance of it, nothing could be by it discovered. Therefore, the whole of such a case should rest upon the prudent administration of mercury, and that should be conducted as I have already pointed out. All those badly-conditioned ulcers, which clearly arise out of a degenerated state of the constitution, in consequence of mercury being pressed too far, should be considered as requiring an amendment of the habit by bark, air, pure diet, and regularity in every respect. Bark in decoction with a little tincture of myrrh, will be a proper gargle for washing the mouth.

On the tonsils there will be sometimes an horny induration,

tion—somewhat similar to that on the prepuce—and the uvula also will be indurated and rigid when an ulceration on it is stopped by mercury. But when mercury has not been completely gone on with, and when restringent washes to the parts have been improperly tampered with—to cure these it is necessary to pursue mercury farther: for, without the glands be restored to their original secreting character—without the symptoms of the venereal infection, which are evident, be completely removed—it must be concluded, a priori, that the constitution cannot be said to be freed from that which, through it, produced its disease. The tonsils will also be found to be enlarged sometimes during the action of mercury; but when that action is withdrawn, these enlargements will go down, or at least if they should not, I know no art that will conduce to promote their subsiding.

A Member of Parliament applied to me in the Spring, 1791, with the following symptoms. A thickened prepuce, a foul discharge from behind the glans, an hectic heat, an emaciated aspect, and his tonsils were uncommonly enlarged and irregularly ulcerated. He had experienced a soreness in his throat for more than two months, and had been taking a nauseous syrup, known by the name of Velno. Some of this syrup, which he had by him, was tasted by me and another Surgeon of eminence, and we both were convinced that it contained solution of sublimate. I began with mercurial frictions, in larger proportions than I wished,

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two drachms every night. As my patient, on account of public business, was impatient, this was continued for ten days, but producing not the fair effect, three drachms were repeated for ten days more. From this as little of the action of mercury was evident: that action from it which was apparent, being deleterious instead of kindly—for his mouth was unaffected, and his head was giddy, and he was disposed to swoonings. After a continued application of mercury ten days longer, these symptoms from it were more increased, but the venereal symptoms were almost evanescent. The prepuce was quite thin, the discharge behind it had ceased, the tonsils looked clean, but their enlargement had not abated. Desisting then from mercury, my patient went a little way out of town, took bark, and did every thing for the restoration of his health. In a short time afterwards, the tonsils assumed again their former state of ulceration, but the health of the patient was mended. Mercury again was administered, in almost as full proportions: in the beginning it acted kindly, and affected the mouth, which deterred me from persevering with repetitions of such proportions. It was then administered in half proportions, by which the salivary symptoms declined, and could never be brought on again, although the mercurial frictions were again increased to as full proportions as in the former process. After pursuing this mercurial system for four weeks, during this second course, the tonsils had for some time appeared very clean; but their enlargement still remained,

mained, and every other venereal symptom had been long extinguished. The patient was very much debilitated by the action of mercury, and by confinement; I therefore advised him to desist from mercury—to pass his time quietly by the sea side—and to wait the operation of pure air, regular living, and sea bathing. This method then succeeded, and he continues well, at least to this time; but I know not whether the enlargement of the tonsils has yet subsided.

In the treatment of all the venereal symptoms, on every part of an infected constitution, there are two material discriminations: the one is, if the treatment be commenced for the cure of them, when the symptoms are recent; and the other is, if the treatment be commenced for the cure of them, when the symptoms have gone on, and when, through that long progress of them, other parts of other anatomical descriptions situated in contiguity to them, have fallen in for their portion of venereal destruction. The treatment of the first will require less time, and will be more simple; and the same treatment for symptoms at their early appearance, will be almost equally effectual, on whatever parts such symptoms appear.

Then supposing, in the first case, that there is an indication of venereal symptoms on any of the parts which are most in common subject to venereal symptoms—if upon the first discovery of them, mercury be immediately and effectually began

began upon, the progress of such symptoms will be stopped, and the parts on which the symptoms appeared, will need no other assistance for their restoration than the bare constitutional action of mercury; whereas, if the disease be permitted to continue its action on these parts, they will be found to be in such state of disease as is peculiar to the anatomical nature of them: for although mercury will be in this instance more necessary, and a longer time must be exacted for the cure of the protracted disease—and although mercury will cure the Venereal Disease which was the cause of the disease on these parts, yet mercury will not restore them to a sound state, nor will the parts themselves become restored to a sound state, not after the Venereal Disease is eradicated, nor after mercury is withdrawn. Each particular part will then exact particular chirurgical treatment for the cure of it; and without chirurgical aid, diseases on particular parts will go on, merely as diseases of parts, independent of any constitutional cause whatever.

Having premised this—I shall proceed to state all those incipient symptoms which I deem to be curable, without any other interposition than mercury insinuated into the habit; or at least very little conjunctive assistance will be required for this stage of the cure, in addition to mercury; and what that assistance is to be, must depend upon the nature of the part. An incipient ulcer on the palate, oesophagus, uvula, tonsils, nasal or auricular membrane, a commencing

opthalmy, ulcers on the forehead, a thickening of the spermatic chord, enlargement of the testicle, ulcers on the scrotum, sanies discharge from the anus, rhagades or loose fungi about the anus—all these require the full and decisive operation of mercury. And as the progress of the disease on many of these parts would be fraught with not only local mischief, but would also bring on diseases on contiguous parts, so should the utmost attention be exacted, that the mercury be always liberally applied, and that the state of the patient be always such as will be most conducive to a decisive operation of it. Two drachms should be rubbed in of the mercurial ointment the first week daily, and three in the succeeding week, until evident signs of its action appear, when the quantity may be reduced, so that the action of it may be supported for ten days after the symptoms are become evanescent. In this instance, when the promised success is fully answered---by leaving the symptoms to speak for themselves, by leaving them untouched, the action of mercury upon them will be better discovered; and at any rate, when that action is as it ought to be, they will need no other assistance for the effectual cure of them.

Supposing, that if any part derived any benefit, on the first commencement of disease, from topical applications, it must be a commencing venereal opthalmy; but what can stop its progress but the action of mercury? But if---in consequence of the rapid progress of the disease, and before the action

action of mercury could take effect when it has been began with as early even as the nature of the disease could be ascertained---the symptoms have notwithstanding spread, and other parts are involved in disease, I shall state in that particular, which I term the second mode of treatment, what chirurgical assistance must be exacted, independent of the action of mercury.

Ulcers on the palate—within the nose—within the ears---on the head or forehead---and on the internal parts of the upper and lower jaws, may so proceed, that not only the softer parts will be destroyed, but also the covering of the bones will be destroyed, and the bones adjacent to the ulcers will become caries: these bones once becoming caries, will naturally protract the disease, and will never recover from disease; the caries must have time to be exfoliated and separated from the sounder parts of the same bones; or the bone altogether, if the whole be entirely a caries, will come away. A caries bone is to be considered as a dead part, as an extraneous part, as obnoxious to the living principle of remaining sound parts: it will, and must be separated, and time must be allowed for that process, after the disease which caused the caries be entirely done away.

In my observations---upon the cause which produces a caries of bones, in the instance of a recent venereal constitutional infection, and upon the cause which produces a caries of the cylindrical bones, and the epiphyses of them, in the

the instance of a long-continued and inveterate infection—there will be a most material and important distinction. The former bones become caries in consequence of the destruction of the softer parts about them; and the latter become caries in consequence of the venereal action within them. The former will be the effect of bones being deprived of their covering, deprived of nutrition, deprived of attachment, and of being exposed to air—the latter, will be the effect of a long and complete venereal action radically on them—will be the positive action of the poison—will be the process of months, and sometimes of years—cannot be effected but through exquisite pain—cannot be removed but by a long mercurial process—and cannot sometimes be removed but by parting with the diseased part through large and repeated exfoliations.

When the softer parts are destroyed, and the covering of those bones are so also—when the parts to which the bones are annexed, are also destroyed, and the bones are exposed, either in part or wholly, whether the disease which produced this destruction were venereal or not—the bones would alike be caries. It is therefore incumbent upon us, in this instance, to be particularly guarded in observing the action of mercury; and to know, that as soon as mercury has effected the intended purpose of extinguishing the venereal action, which it will do in almost the same space of time that it would have cured the symptoms, before they had gone on so far in destroying the parts—then the quantity which is ad-

administered should be gradually diminished, and the balance should be kept so even, that the patient may not fall into a contrary disease, from the preponderating action of the mercury, and in consequence of its being pushed too far. Before the whole of an exfoliation is complete, it will be some time; and during which the constitution should be restored by bark, air and diet.

If the disease be in the mouth—the parts may be washed with barley water, honey of roses, and tincture of myrrh; and the nose and the ears may be also syringed with the same fluid. After the venereal action has been destroyed, lamina of the bones of the cranium will sometimes be exfoliated from a caries there: until the exfoliations are completed, none of the parts will heal, although, when the constitution is freed from the virulent cause, and when air, diet and strict temperance have been observed, the propensity for healing will be discovered to be very strong.

A naval Officer, when at sea, was attacked with a venereal ulcer on his palate, and which continued to spread to so alarming a degree, that he was obliged to quit his station, and return to London. When I saw him, he appeared to be in the last stage of emaciation: the ulcer had destroyed the palate, had extended to the gums of the upper teeth, all of which were dropped out, and his voice (or rather noise) was so inarticulate that he could not be understood. This ulcer had made its appearance six months before,

fore, at which time the patient had been living freely: he told me, that at no time mercury had done any good for his cure: but upon more strict inquiry I found, that at no time there had been any serious attempt for perfecting his cure by a regular or judicious administration of it. He had not lain by—his vitiated state of constitution had not been corrected—nor had he for two months taken a single grain of mercury. On his passage home he was attacked by continual boring pains in his cylindrical bones; he had nodes on his elbows, and on one of his parietal bones there was a soft tumour, containing fluid. He had a violent cough, and his pains in the night were most tormenting. There were two objects to be most attended to in this case—the delicate state of the patient, and the disease with which he was possessed. It appeared to me, that if mercury were pressed too rapidly, the patient would sink; I therefore put him upon milk diet, and began rubbing in three drachms of the ointment in a week. This system was persevered in for the first month, at the expiration of which time—his health was better—the ulcer looked favourably—his pains were abated—and his nodes disappeared. During the next month, the proportions of mercury were increased to a drachm and a half every night, the patient using the warm bath to check the too rapid progress of its symptoms, which were strongly disposed to come on. At the expiration of this time, the whole of the venereal symptoms were evanescent; but the sockets of the upper teeth were not exfoliated. It was to explain to you

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the following points that I have given this case :--They are---that the Venereal Disease, which was the cause of the ulcer, being at an end, proceeding farther with mercury would have been injurious---that the sooner the patient was restored to health, the more kindly would the exfoliation follow---that in a month after, a very large exfoliation did follow---and that granulations soon covered the sound parts of the bone. The patient always applies lint to fill up the cavity in his palate, and by this he articulates very well, his upper lip being very long: and I question whether he may not, if he pleases, conceal the whole of this unfortunate calamity from the observation of his most particular acquaintance.

When a venereal opthalmy has made any progress, the inflammatory symptoms may exact rather particular attention: at least, it will be expected that something topically should be gone on with during the mercurial process, and even after, for the injuries done to the component organs of the eye. Blisters, leeches to the temples, fomentations of warm water with opium dissolved in it, avoiding the light, and at the latter end of the cure, a vitriolic wash, or brandy and water, for the restoration of the distended vessels, should be applied.

Whenever the lachrymal sac and the lachrymal duct suffer from venereal infection, the extinction of that infection may not procure their restoration to a sound state. The art of Surgery peculiar to that disease must be appeal-

ed to; the fluids must either find their natural passage restored to them, or an artificial passage must be made for them. The same may be said also of the necessity of operations which are particular to the cure of a diseased state in other parts. A fistula in ano, from a venereal cause, will not be cured as a fistula when the virus is extinguished. But in all these cases mercury must be pursued, as I have before laid down, until an elimination of the Venereal Disease be complete. The constitution, by being freed of the original cause, and of the mercurial process, will be then in a better state for any operation necessary for the cure of a disease particular to a part: and during that operation no other appeal need be made to mercury, unless appearances demonstrate the necessity, which can never be the case, when the Venereal Disease is entirely eradicated: but rather bark and restoratives ought to be then administered, that the operation and cure may proceed with success.

The testicle will also require attention: if, from long Venereal disease, the testicle and the spermatic chord be indurated, if the vessels be no longer in a state for vascular offices, no power can again restore the testicle to its original state, nor fit it for its former functions:—its extirpation or its remaining---its recovery or not, must be an object of local attention, when the Venereal Disease, from a thorough action of mercury, is out of the question. In short, as the constitution becomes deeper and deeper infected with the disease, so will the

disease

disease betray itself on every part:—the lymphaticks will betray it, and the whole of the secretions will be vitiated.

Venereal warts and excrescences will also exact some chirurgical treatment: but that should not be put into practice until the latter end of a mercurial course, when mercury be fully acting. I attended very lately a case of this nature, and as the circumstances of it are rather singular, I will state them to you. The patient had the following appearances: an emaciated sickly countenance—weak eyes—hissing noise in his ears—a node just perceptible on his forehead—and a large fimbriated excrescence around the anus, discharging a sanies, and obliging the patient to straddle in his walking. A Surgeon in the City pronounced the case to be venereal, and my opinion was solicited to make his the more decisive. I did not hesitate to declare the same. The cure commenced by confining the patient to his chamber, and by mercurial frictions of a drachm and half every night: the mercurial action was remarkably kindly, and at the expiration of three weeks, by a continuance of the mercurial frictions the fimbriated excrescence began to slough away: but as it held fast at the root, and as it grew out upon separate stems, four or five distinct ligatures were then passed around them at separate times, and the whole soon sloughed away. Mercurial frictions were continued for a fortnight longer, and the patient got well. When I first saw this patient, I asked him how long it was since he had a venereal complaint? He replied, not for seven years. I asked him if he had knowingly taken any mer-

cury duringthe last seven years for any other cause? He replied that he believed he had for a sore leg. The question is—when this disease was contracted? It must have been contracted seven years ago---or the virus must have since been absorbed in consequence of an infectious connection, without leaving any local impression. I believe that the present symptoms were in consequence of the known disease seven years ago—and I am induced to think so, because, if the virus had since been absorbed without leaving any local impression, the probability is, that the first constitutional symptoms would have been more notorious and rapid--or that they would have been in the form of ulcers on the tonsils, or eruptions on the skin. Or he might have contracted the disease during his taking mercury for his sore leg, and by that means have interrupted the regular progress of the first symptoms natural to the disease. For the removal of those soft warty excrescences which are found behind the prepuce, and upon the glans penis, sabine powder sprinkled upon them will frequently succeed, and more especially when they bleed at the touch: but when the stems of them are strong, and when they are grown hard, nothing but ligature will extirpate them. As they are generally produced by a redundant vitiated secretion, so should the parts be frequently washed with a weak solution of vitriol, to prevent their growth.

A variety of other parts, which may not be within my recollection to enumerate, will require the assistance of

Surgery

Surgery for the reparation of them, after the disease which was the cause be already eliminated; therefore, it is unnecessary to add to those instances which I have already adduced: The proper chirurgical acts which are proper to each particular part, must be so appropriated as I have already stated, and the consequences will equally evince that truth in the theory and the practice which first urged me to the recommendation of it.

There are but few more symptoms remaining, which are objects for our present observations: for I consider subordinate symptoms always included in more important ones (as minora within majora); they will always yield to that judicious treatment which effects the cure of those in point of local consequences, and in point of local diseases more seriously deserving our notice.

A venereal hectick, attended with ulcerations of the lungs, is an object for particular observation. This disease has not exacted all that attention which it ought; and this disease very often proves fatal, for want of discriminating it to be from a venereal cause. When it arises on a constitution not otherwise disposed to a consumptive diathesis, the effects of mercury will act upon it like a charm: but when it is in combination with a consumptive diathesis, although the venereal cause may be removed, and may be cured by mercury, yet there is nothing in that medicine which will conduce to correct a constitutional disposition to pulmonic

monic affections. But as this cannot be known a priori, so must the case not remain unattempted to be cured by a trial of mercury, whenever there be indicative symptoms of a prevailing venereal infection, to lead us in forming such a judgment on the case. As this case may arise from the constitution being feeble, and it is an argument of a feeble constitution, so must these indicative venereal symptoms be not expected to be either numerous or very striking. Eruptions on the skin, and slighter symptoms of venereal infection which might have formerly fallen to the lot of the patient, will be sufficiently demonstrative of a cause for such a suspicion.

There is also another reason why a venereal pulmonic infection may not be judged to be venereal, and why it may not be treated as such when it is really venereal, and when it would be cured, if it were known to be venereal. I will point out the reason to you. In consumptive cases, it generally is the Physician who is applied to, and not the Surgeon. In venereal cases, the Physician may not be said to be so conversant as the Surgeon. From that cause, and from a want of attention to it, a true nature of the case, and of the indicative symptoms which may lead to a discovery of it, may be and are frequently overlooked. When I was House Surgeon at the Middlesex-Hospital, Doctor Huck Saunders then was a Physician to it. I remarked that he always, in the beginning of consumptive cases, gave mercurial pills to the out-patients. Upon my asking him his intention---he told me, that

many

many of these cases were restored by it, because the disease was venereal. He also shewed me some with eruptions upon them, adding at the same time---that by being assured that many with this complaint have been in the way of venereal infection, he made this ground good, and began with mercury, and with some it acted to their effectual cure.

I shall now point out the peculiar mode of treating a pulmonic venereal hectick: as in such cases the constitution is in a feeble state, the cure must not consist in exciting a rapid mercurial action: at the commencement mercury should be gradually introduced, and the action of it as gradually ascend to an acmé. The patient's health should be the first consideration, and mercurial action should be subordinate to that. He should enjoy pure air, milk diet, and he may take the sarsa decoction. After the process has been thus gone on with for a month or more, the indicative symptoms should be remarked, and if they shew a disposition to retire, and if the constitution gather strength, then may mercury be farther increased, in order to procure the more effectual action of it, and to produce that change necessary for the perfect extinction of the venereal action out of the constitution.

The last important symptoms, on the cure of which it will be necessary for me to treat, are, gummata on the aponeurosis of muscles—nodes on the periosteum—and caries

of

of bones. When I was defining the symptoms of the Venereal Disease, I observed, that before any eruptions, or any other first venereal constitutional symptoms appeared, there were, in this incipient state of infection, pains felt on various tendinous parts, particularly on the shoulders, the arms, the loins, and the hips; and I farther observed, that these pains were not permanently fixed, but that they were merely erratic, and that they were foreboders or precursors to venereal eruptions on the skin, and to other incipient venereal symptoms, and that these pains subsided, as soon as such eruptions, or such other incipient venereal symptoms advanced and became critical.

These are not the pains which I now shall consider, as they are not produced from the same cause, namely—not in consequence of the action of venereal foreign virus strongly impressed, and that for a series of time, on the tendons, the periosteum, and the substance of bones. This is that stage of the disease which I am now about to consider, and to cure: that last and late action of virus on the constitution —that which marks the power of it over the constitution as conclusive—that which, according to the descriptions of Vigo, of Fallopius, and most of the Authors up to the present æra, has been considered, as it actually is, the last and the most obdurate of venereal action.

I also before observed—that as soon as the constitution

was

was proved to be infected by the symptomatic fever, the erratic pains, and the subsequent appearance of venereal eruptions---a foundation for all future venereal symptoms was laid from that immediate point of time, and the constitutional infection then also commenced: but that the harder parts, by their being of that peculiar anatomical construction as not to admit of, and as not to yield to the force of venereal destructive power with so much facility as the softer parts do, consequently could not betray that destructive venereal power over them so soon as the softer parts. The peculiarity in the construction of these parts anatomically prove, that the disease must be later in betraying symptoms of venereal action on them, than on softer parts. They are composed of vessels far more minute and compact, and very little sanguineous circulation is discovered to pervade them: the living principle of them is supported by fluids which are circulated through minuter vessels than where sanguineous globules can find a circulation: and so rigid and so inadequate are tendinous parts to distention, that it is effected with the utmost difficulty, and attended with excruciating torture whenever such distention or diseased action on them is provoked even by any other first cause than venereal. Diseased action on them, either by Gout or Rheumatism, will produce all those painful effects from over distention and inflammation.

It therefore is evident, that any other diseased action on

these parts will excite symptoms similar to the venereal; but there is always this difference between them, that the venereal action will never be discontinued, without mercury; it being produced by a foreign poison and an endless cause, whereas other causes and other diseases may decline and pass away of themselves. Nocturnal pains, from every cause, originate by the pressure of circulation, in consequence of additional warmth from the heat of the bed. The venereal cause produces the most intense and the most constant pains, and these will continue: the common irritation on these parts from Gout or Rheumatism may decline, and the nocturnal pains may subside; but the specific irritation produced by foreign virus, will never be discontinued; and therefore, as a poison it may fairly be said to be similar to Virgil's description of Fame—

"Mobilitate viget, viresque acquirit eundo."

Having renewed these recollections, I shall proceed to farther remarks, by saying—that the time of the appearance of these symptoms is very uncertain—and that the uncertainty generally is in consequence of mercury being given, but not effectually, for eradicating the disease. That the rudiments of the action of the Venereal Disease commence on these parts, with the rudiments of the same action on softer parts, is proved by the following observations: first, that when there is no interference of mercury for curing or retarding the venereal action, these symptoms, although last,

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will immediately succeed to symptoms on softer parts, as it was evident in the case that I gave you, of the lady who was attended by Mr. Pott, and when no mercury had been given, until these symptoms had actually appeared: and secondly, I prove it more familiarly to your observation, by stating that which is constantly the case when these pains and these affections do come on: and thus it is—

Enough of mercury has been administered on a subject, for the cure of any of the constitutional symptoms which were apparent on the softer parts; and these symptoms, in consequence of that mercurial action, ample for that purpose, will disappear, and the patient is concluded to be perfectly well, and free of infection. At this period of time, no symptoms of the action of the disease, from the same infection, will be apparent on any tendon, periosteum, or bone;—the virus on the harder parts having not proceeded so far as to excite evident effects of commencing destruction, but the rudiments of the disease on these parts are commenced, by the virus having pervaded the circulation. The action of mercury, when it cured the venereal symptoms on the softer parts, pervaded also the circulation through the harder parts; but by its action being discontinued too soon, the venereal action on the latter is not eliminated, but retarded for a time, in consequence of which it in time again recovers its crippled power, and indiscriminately returns at earlier or at remoter periods. The rudiments of its action there-

fore are laid long before that action may be apparent; for so uncertain is the time, in the appearance of symptoms on the harder parts, that it may be one, two, three, or more years, before the venereal effects are perceived. Nothing can more clearly prove, that the rudiments of the infection commence on harder parts as soon as on softer parts, and that the difficulty in removing it is greater than that in removing the infection on softer parts—than the following continuation of a case, from page 611.

The patient, as soon as the spreading Chancre healed, became rebellious to a continuance in mercury. He washed it off, and retired into the country, it being in the Summer months, and there remained till October. On his return to town, he applied to a Physician, having at that time nocturnal pains in his arms, shoulders, hips, and also enlargements on his elbows, and on his forehead.

If this patient had not, from necessity, during the protracted state of his spreading Chancre, continued the application of mercury for a longer time than usual, eruptions on the skin, or ulcers on the tonsils, would have, in their common order, appeared. But these primary symptoms of constitutional infection were baulked from that cause. There was enough of the action of mercury produced for the prevention of them—but not enough for eliminating the rudiments of infection already commenced on the harder parts.

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The Physician attended him for some time—he gave him hemlock, but that not succeeding, he gave him mercurial pills. The pains became relieved by mercury, but the disease was not removed. When the patient was at ease, he discontinued the pills; and as often as the pains returned, he returned to the pills again. By this, he kept the disease at a stand, until the next Summer advanced, when he went to the sea side, and bathed in the warm bath and in the open sea. In the Autumn he returned to town, with all the former symptoms: but by then submitting, from conviction, to a regular process of mercury during seven weeks, he got completely well.

For the effectual cure of gummata, nodes, and carious bones, there will be some necessary discriminations.—These consist in considering the various stages of the disease, the duration of it, and the effects produced upon the parts by mercury: these consist also in the state of the parts, when by mercury the cause is removed, but the diseased condition of parts still remain. As long as the disease has spared enough of the living principle for supporting the parts, the venereal part of the case is curable by mercury; but when the disease has destroyed the whole substance of a bone, or the epiphyses of it, neither can be again restored. That which is once a caries will be always a caries; as in the instance which I before stated, and from the same necessity in the nature of things. But it must be observed, that so strong

strong is the resistance of these parts against the action of the disease, that it will be found acting on them a long time, and yet by mercury the parts may and can be again restored to a sound state. This is that stage of the cure, which I shall first commence to treat on.

At the commencement of the cure for at least the first six weeks, the mode to be adopted should be an intermediate one between the alterative and the decisive and more effectual mode: mercury should be gradually insinuated for six weeks, and the warm bath should be used twice a week. The patient should keep within doors, but not to his chamber: and for the ensuing six weeks, the process of mercury should be so advanced, and so supported, as to keep up constantly the mercurial change, and this should be continued almost a month after the venereal symptoms have been eliminated. The patient may take on opiate every night, and if he pleases, he may drink decoction of sarsa through the day. By this method, many of these venereal attacks have been cured, purely owing to the action of mercury being decisive, and owing to its being continued long enough for procuring all its effect. All the cases, the symptoms of which yield to mercury, may thus be cured; and although they may return again and again, by mercury not being sufficiently persevered in, yet it must not be forgotten, that since mercury has once relieved the parts from the venereal

nereal action, it is the strongest presumptive proof that the case is within the pale of a cure through it.

It has been a question---what local applications are necessary to nodes on the periosteum, and to enlarged and diseased bones? I will give you my opinion upon it.---The practice has been with some to make an incision through the node--and the practice has been with others to lay a caustick on the part. When the skin is sound—or when it is discoloured—or when it is a little broken—or when there is even an ulcer on the part affected---provided that these appearances precede any use or action even of mercury, they should be suffered to remain as they are, until the evident effects of mercury, through the constitutional action, be observed upon them; and it will be found very often that these threatened operations will be unnecessary. The skin, from modern observation, is proved to be more and more an object for preservation, and he who divides it, when it might be dispensed with, does more harm than good, or than he can often after repair:---that labourer who can pull down St. Paul's, may not be able to build an hovel. If an incision be made when the integument be whole, the diseased part under it will not suffer it again to heal---if caustick be laid on, if the periosteum be destroyed, and the surface of the bone be exposed---whether that bone were a caries or not before, it will most assuredly become so, and most assuredly an exfoliation from it will take place. But if, after a complete

mercurial

mercurial process, the integuments inflame, or ulcerate' there is then a presumption for concluding that the bone is in part caries---that the living principle of the remaining part is excluding it---and that it is an obnoxious substance foreign to life, about to be separated---and that by the best means it ought to be separated from the living part, before that part can become sound, and before the integuments will again be healed. When such is the case---I cannot object to the application of caustick---not but I am convinced, that in the end, the integuments would continue to give way, until such time as the extraneous part be completely separated.

I shall lastly observe---that although the Venereal Disease may be eliminated in all its stages by mercury; yet there are frequently instances from mistakes in cases, where the bones on various parts of the human body will be in that state of confirmed disease, from long venereal action on them, and where such will be the destructive effects, that they cannot be expected to be restored, neither by mercury, nor by any additional art that may be annexed to it. It seldom happens, that where the living principle in one bone is so destroyed, as that there be not enough, if there be any of the sound part remaining, to give it support, and by which it in consequence gives way---I say it seldom happens that the disease is confined to that bone in particular---others will be found advancing in disease from the same cause. The golgothas of diseased bones, collected into anatomical

anatomical cabinets can be produced for proving this fatal truth, and for placing it beyond a doubt. Bones not suspected, will be sometimes forward in disease: this cannot be more strongly evinced, than in that case which is related by Mr. John Howard, of a girl whose chin dropped suddenly on her breast, and who expired, whilst she was under a salivation in St. Bartholomew's Hospital, and whilst her cure was in consequence looked for. It appeared, on examination, that one of the vertebræ of her neck was a caries—and had thus suddenly given way.

My Course of Lectures is now closed, and my intention is to publish it. You will have the opportunity of perusing it to more advantage in the closet. But before I take my leave, I wish to be indulged with a few reflections.

I am not disposed to flatter with a single compliment the taste or the virtue of the present age. The merit or demerit of this production cannot be truly ascertained but by comparing it with former ones upon the same subject. Astruc has recorded nearly one thousand Authors, and if I add one hundred more, who have written on the disease since his time, there will be abundant choice from many to amuse the fickle, but not so many as there are of the fickle yet to be amused—"Stultè prava judicat sententia."

Criticism is the most exalted effort of the human mind. ---It is a power possessed but by few.----It is art, taste, and learning, associated with genius.---It is an ermined Judge, in the supreme Court of Literature, deciding upon merit.--- It is the patron of Authors when alive, and their immortality after death.---It is the noon-day Sun emblazoning truth. By the reflecting mirrors of a Longinus and an Addison the sublime beauties of ancient and modern Authors have been illustrated. It is something to be said, that this production could never have met the eye of the world, without intense application, and without some practice--that the most brilliant imagination that Nature ever formed, in her happiest mood, and from her choicest mould, unpractised, could not have atchieved it. If I have explored with any success a subject dark and occult by nature—if I have in the least adorned an art, eminent and necessary in the grand scale of humanity---if I have gladdened but a single hour that must have been otherwise endured in pain and in anguish-- if I have plucked the down, and softened the bed of disease, I have but done a duty. The blessing flies to those generous minds who, by entrusting their health to my care, furnished the investigation through practice--to those who dared, even in this age of fashion, to reflect and to judge for themselves ----to those who ventured their confidence in a man without fame, without fortune, without patronage, without protection---

tection---to those who gave, what even downcast moping merit often vainly waits for, the opportunity--to those who fancied my value, like a lot at an auction, took it home and examined it afterwards.

FINIS.