

Observations on morbid poisons, phagedaena, and cancer : containing a comparative view of the theories of Dr. Swediaur, John Hunter, Messrs. Foot, Moore and Bell, on the laws of the venereal virus. And also some preliminary remarks on the language and mode of reasoning adopted by medical writers / By Joseph Adams.

Contributors

Adams, Joseph, 1756-1818.
Hunter, John, 1728-1793.
University of Leeds. Library

Publication/Creation

London : J. Johnson, MDCCXCV.

Persistent URL

<https://wellcomecollection.org/works/chh8exmg>

Provider

Leeds University Archive

License and attribution

This material has been provided by This material has been provided by The University of Leeds Library. The original may be consulted at The University of Leeds Library. where the originals may be consulted.

This work has been identified as being free of known restrictions under copyright law, including all related and neighbouring rights and is being made available under the Creative Commons, Public Domain Mark.

You can copy, modify, distribute and perform the work, even for commercial purposes, without asking permission.

**wellcome
collection**

Wellcome Collection
183 Euston Road
London NW1 2BE UK
T +44 (0)20 7611 8722
E library@wellcomecollection.org
<https://wellcomecollection.org>



Leeds Infirmary
MEDICAL - LIBRARY.

Entered *June 25* 1795

Allowed for reading } Weeks. Days.
the first Year. }

10

—After the first Year

20

Forfeiture per Day for keeping it } d.
beyond the Time. }

1

S.C.-2

*The University Library
Leeds*



*The Library of the
School of Medicine*

LEEDS UNIVERSITY LIBRARY

Classmark:

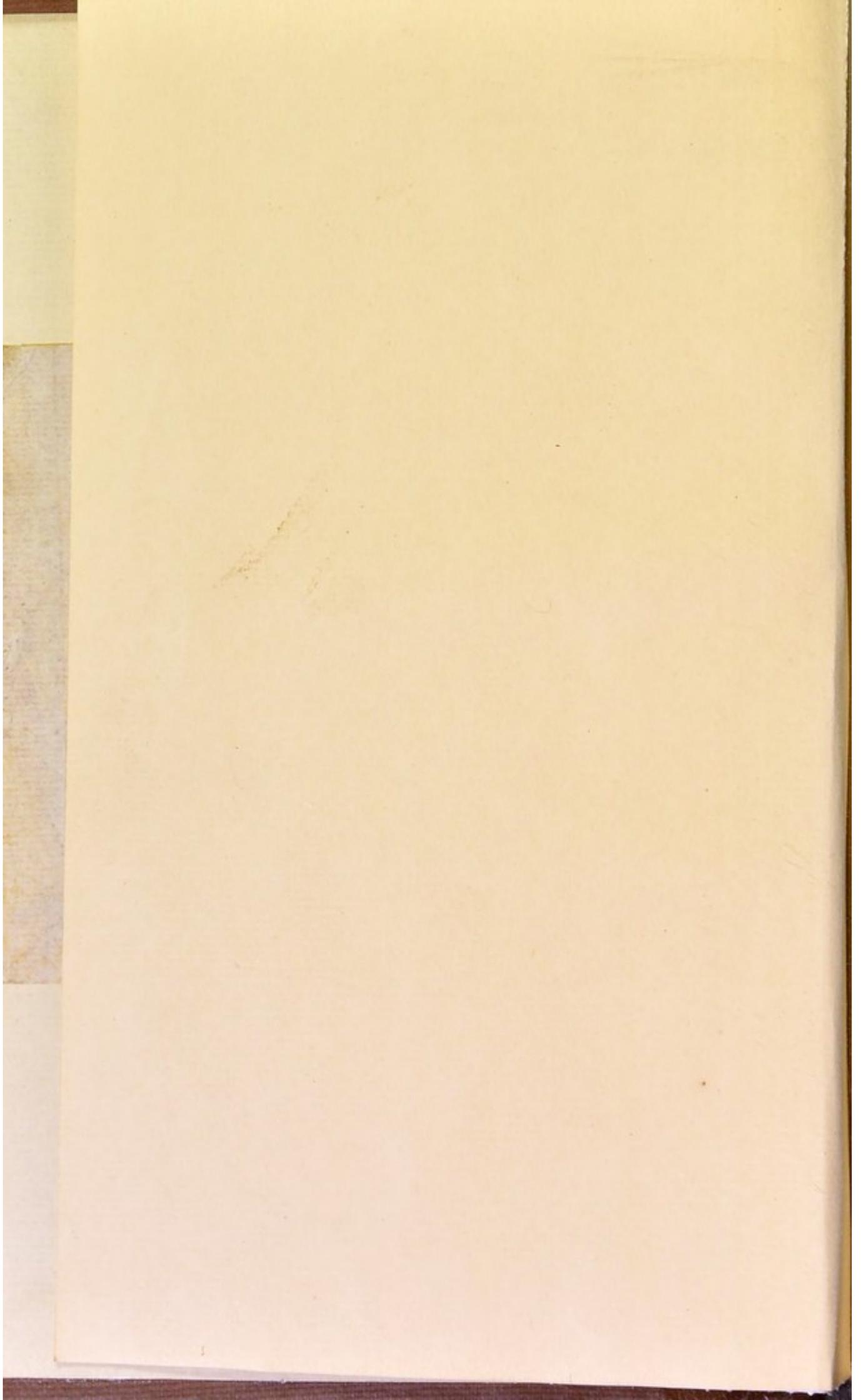
Special Collections

Health Sciences Historical Collection

SC2 ADA



30106016127721

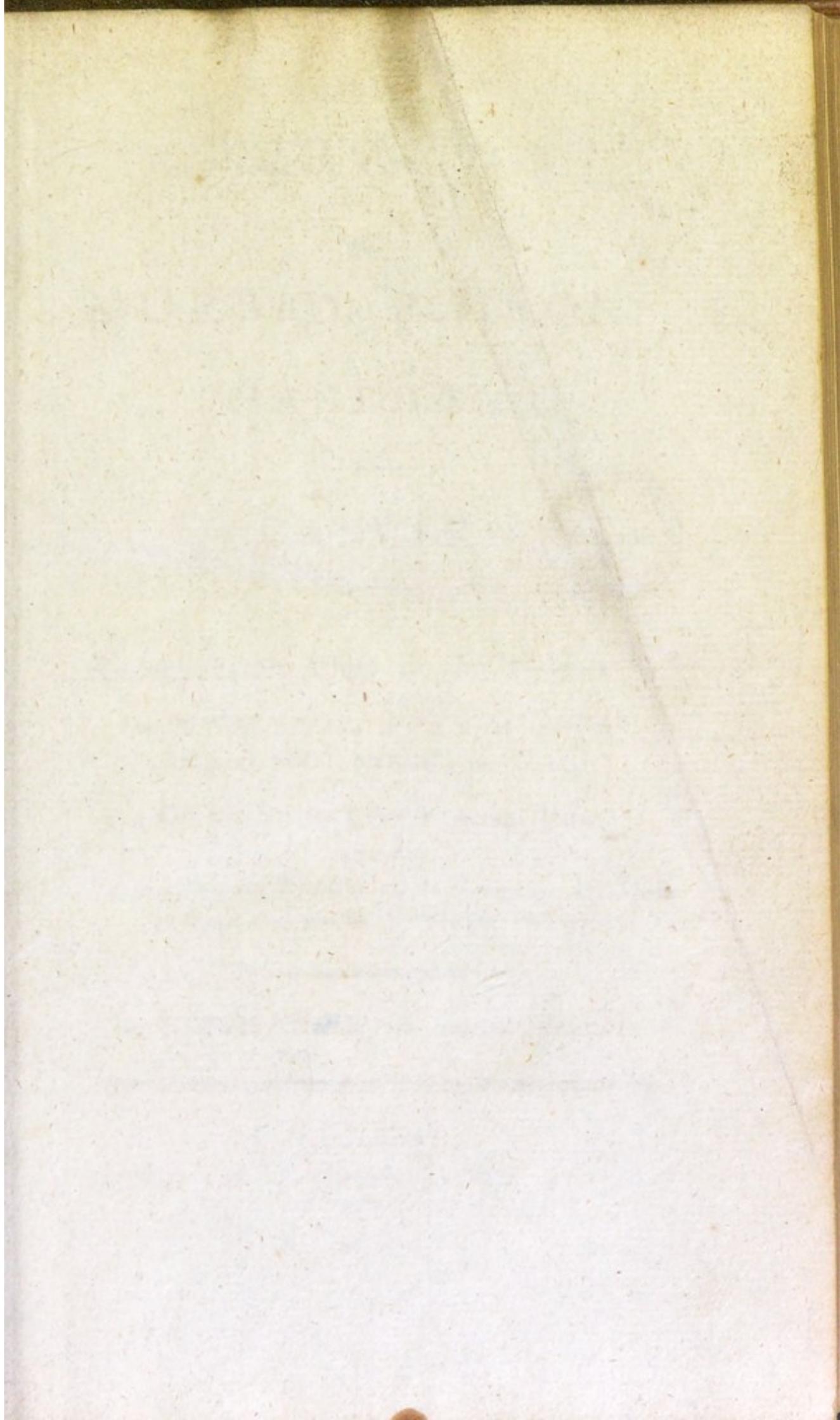






100

NK2



THE
MORNING
STAR
LONDON

Printed and Published by
W. H. & A. CO.,
15, Abchurch Lane, London, E.C. 4.

Printed in Great Britain
by the University Press,
Cambridge.

OBSERVATIONS
ON
MORBID POISONS,
PHAGEDÆNA,
AND
CANCER:



CONTAINING

A comparative View of the Theories of
DR. SWEDIAUR, JOHN HUNTER,
MESSRS. FOOT, MOORE, AND BELL,
On the Laws of the Venereal Virus.

AND ALSO

Some preliminary Remarks on the Language and Mode
of Reasoning adopted by Medical Writers.

By JOSEPH ADAMS, OF LONDON, SURGEON.

LONDON:

PRINTED FOR J. JOHNSON, NO. 72, ST. PAUL'S
CHURCH-YARD.

MDCCLXCV.

DEDICATION.

TO

MORRIS COLLIERS, Esq.

P. H. Chamberlain, Esq.

DEAR SIR,

ALTHOUGH you cannot be offended at seeing these sheets addressed to you yet had I apprized you of my intention you might have expressed a wish that I should wave them. In this case I should have lost the only opportunity that may ever occur of thus publicly showing you with how much gratitude and respect I shall for ever remain

Your faithful Friend and Servant

The AUTHOR.

DEDICATION.



TO

JOHN COLLIER, Esq.

Camberwell.

DEAR SIR,

THOUGH you cannot be offended at seeing these sheets addressed to you, yet had I apprized you of my intentions, you might have expressed a wish that I should wave them. In this case I should have lost the only opportunity that may ever occur of thus publickly assuring you with how much gratitude and respect, I shall for ever remain

Your faithful Friend and Servant

The AUTHOR.

Walbrook,
March 31st, 1795.

DEDICATION.

CONTENTS.

TO

JOHN COLLIER, ESQ.

CHAMBERLAIN.

Presented to the Honorable John Collier, Esq. Chamberlain of the Exchequer, by the Author.

The Author is obliged to the Honorable John Collier, Esq. Chamberlain of the Exchequer, for the opportunity he has given him to publish this work. He is also obliged to the Honorable John Collier, Esq. Chamberlain of the Exchequer, for the opportunity he has given him to publish this work. He is also obliged to the Honorable John Collier, Esq. Chamberlain of the Exchequer, for the opportunity he has given him to publish this work.

CHAM.

CONTENTS.

CHAP. I.

Observations on the Language and Mode of Reasoning adopted by Medical Writers, page 1.

On words to which no precise meaning has been affixed.—
On the term An Hereditary disease—as used by Dr. Cadogan,—by Mr. Hunter, page 2.—By Mr. White.—On the term Scirrhus, as used by different writers, page 3.—Sydenham's Despumation, &c. p. 5.—Fermentation by various Writers, p. 6.—Erosion by Dr. Swediaur, p. 7.—Remarks on Mr. Abernethy's language, p. 9.—On Mr. Moore's, p. 13.—On Mr. Foot, p. 19.—Specimen of Nofology, from Dr. Swediaur, p. 21.

CHAP. II.

Advantages of more correct Language and Reasoning, illustrated in other Sciences and in Medicine, p. 29.

Specimen of the language of the ancient Schools, p. 30.—
Introduction of true philosophic reasoning by Sir Francis Bacon, p. 31.—Less attended to in Medicine than in some other Sciences, p. 36.—Danger of definition and syllogism and advantage of reasoning by Bacon's method of induction.—Illustrated in Haller, Mr. Pott, and Mr. Hunter, p. 38.

CHAP.

CHAP. III.

Observations on Morbid Poisons, p. 47.

Susceptibility, disposition and action, p. 48.—Yaws, p. 57.—Sivvens, and Canadian disease, p. 58.—Of the parts most subject to the influence of morbid poisons, p. 64.—Effects of various morbid poisons on the Genitals, p. 65.—Inducing Ulceration and Slough, *ibid.*—Phagedæna, p. 67.—Sloughing Phagedæna, p. 69.—Remarkable case of the latter, p. 70.—Effects of morbid poisons on the Nipples, p. 82.—In the Gums from transplanted Teeth, p. 84.—Impropriety of classing any of these with the venereal, p. 86.

CHAP. IV.

Of the first local Actions induced by Morbid Poisons, p. 88.

Natural History of an Ulcer, *ibid.*—Scabbing, Ulceration, Granulation, Cicatrization, p. 89.—Callosity of old Ulcers, p. 90.—Effect of extraneous bodies, p. 91.—Application of these to Morbid Poisons, p. 92.—General division of Morbid Poisons, p. 97.

CHAP. V.

Of the mode of cure original and remedial, and of the difference between Primary and Secondary Local Actions, p. 99.

Laws peculiar to Morbid Poisons, p. 99.—Loss of substance not restored by Granulation, p. 101.—Except in Secondary Ulcers, p. 103.—Other differences between primary and secondary Ulcers, p. 104.—When Secondary Ulcers are similar to Primary ones Granulation does not take place

place in either, p. 104.—Exceptions where skinning is prevented by the presence of Slough.—Illustrated in the Small Pox, p. 109.—Causes of Pitting in that disease, and of the advantages of Inoculation, p. 112.—Of its general fatality to the Fœtus, p. 115.—Actions induced by Mercury, p. 118.—Its manner of curing Ulcers from Morbid Poisons, p. 124.—Secondary Ulcers more easily relieved than Primary ones, p. 129.—The former not prevented though cured by Mercury, p. 130.—Of the diseased actions induced by Mercury, p. 133.

CHAP. VI.

Particulars not reducable under the former Heads, with conjectures on the origin of some Morbid Poisons, p. 141.

Reasons why the world should have been alarmed at the appearance of the Venereal, though other Morbid Poisons had been frequently described on the Genitals, p. 143.—Whether any of the latter occur without a broken Skin, p. 145.—Are always attended with fever, p. 146.—Causes suggested, p. 148.—Some advantages derived from the Laws which govern Morbid Poisons, p. 150.

CHAP. VII.

Of Carcinoma and other local Diseases usually called Cancerous, p. 157.

Ambiguity of language, as applied to these diseases, p. 157.—Description of Carcinoma, p. 159.—Difficulty of ascertaining its early Stage, p. 164.—Enquiry into the causes, p. 167.—Analogy between Carcinomatous and other Hydatids, *ibid.*—Is the matter of Carcinoma infectious? p. 172.—Other diseases called Cancerous, p. 174.—Cancer in Utero improperly confounded with Carcinoma, p. 177.—Reflections

fections on the slow progress of the Art in these Diseases,
p. 179.—The only means of improving it, p. 193.

CHAP. VIII.

Comparative View of the Theories of Drs. Foart Simmons, and Swediaur, John Hunter, Messrs. Foot, Moore, and Bell, on the Laws of the Venereal Virus; with an enquiry how far the opinions of Mr. Hunter are confirmed by facts admitted by Boerhaave, Astruc, and other Writers of reputation, p. 186.

Remarks on Dr. Foart Simmons, p. 188.—On Dr. Swediaur, p. 190.—On John Hunter, p. 200.—Difficulties that occurred to Hoffman, p. 213.—To Sydenham, p. 214.—To Boerhaave, p. 215.—To Astruc, p. 217.—To Turner, p. 218.—To Chapman, *ibid.*—To Dr. Swediaur, p. 219.—Discoverable in Mr. Moore, p. 222.—In Mr. Foot, *ibid.*—How accounted for by him, p. 223.—Unnoticed by Mr. Bell, p. 224.—Explained by Mr. Hunter, p. 225.—His explanation confirmed by facts admitted by the best Authors, p. 226, and by analogy with other Diseases, p. 228.—Objections considered.—Of Dr. Swediaur, p. 236.—Of Mr. Moore, p. 238.—Recapitulation of Mr. Hunter's Theory, p. 241.—Objections to some parts, p. 242.—Of Mr. Foot.—Remarks on his Lectures and Observations together, p. 245.—On his Observations, p. 264.—Comparative view of the Characters of Dr. Hunter, John Hunter, and Mr. Pott.—Of Mr. Bell, p. 276.—Conclusion, p. 313, &c.

ERRATA.

- Page 33, in the note, line 5, for Ab. read At.
43, in the note, line 3, for philosophis, read philosophi.
148, 3 lines from the bottom, for we, read the.
171, line 9, for fanies, read lymph.
18, line 21, for it, read them.
251, line 5, from bottom, in the note, dele only.
172, line 5, from bottom, for ungrateful, read ungraceful.

C H A P. I.

OBSERVATIONS ON THE LANGUAGE AND
MODE OF REASONING ADOPTED BY
MEDICAL WRITERS.

IT is a frequent remark that many facts passed current for ages without having their validity enquired into. The same may be said of words without having any precise meaning affixed to them. In common reading we are apt to be satisfied if we understand, or think we understand an author, and can even feel a temporary triumph and a further apology for our own indolence, if we discover that he has evaded a question we could never solve to our own satisfaction by adopting an expression that has never been defined.

How many ages was the term "hereditary disease" current among the learned! A little more than twenty years ago Dr. Cadogan published his celebrated treatise on the Gout. In this, though he shows himself free from the shackles of the former language and opinions of medical writers, we find him strangely entangled in asserting that gout is not an hereditary complaint.

“ If,” says he, “ it were hereditary, it would necessarily be transmitted from father to son; and no one whose father once had it could possibly be free from it.” Yet after this he admits, by implication, that scrofula and madness are hereditary.* If so, what a numerous race of strumous persons and maniacs may we not by degrees expect!

At length Mr. Hunter met the question fairly, and showed there was no such thing in nature as an hereditary disease, but that a disposition to a disease was often hereditary. Though he probably taught this from the time he began to lecture, yet when, in the year 1780, he gave a public deposition to that effect, at the trial of Donellan, for the murder of Sir Theodosius Boughton, the opinion seemed new to most medical men, and to many incomprehensible. That it was not generally adopted till some time after appears by Mr. White's Essay on Struma, which was published in 1785.

The avowed object of this essay, was to prove that struma was not hereditary. As the author had to give his own definition of the word, he did it in a note, and in such terms as there was no difficulty in proving did not apply to the disease in question, nor indeed to any other. Two years after appeared an edition very different from the former.

* “ Diseases really hereditary, I fear, are never cured by any art or method whatever, as is but too true in cases of scrofula and madness.” Dissertation, 7th edition, p. 18.

In this the author gives a definition from people of whom he had made frequent enquiries what they meant by an hereditary disease, easily proves their definition fallacious, and afterwards admits that scrofula is hereditary, according to the language of Mr. Hunter. In the third edition Mr. Hunter's solution is adopted, with little or no reference to any other. A remarkable instance how generally men agree, how unsatisfactory their reasoning is, when their terms are undefined, and how much the stock of our knowledge is improved by the accuracy of our language.

This is an instance of the necessity of accurate expression. Perhaps some of the remarks I may make on words may appear trifling; but I trust every student who wishes for information will be of a different opinion. How often will he feel dissatisfied with his own attainments, if he finds the same word used in various senses by different authors! In some he neither meets with a definition, nor a reference where he may find one. If one is more accurate than the rest, he perhaps attempts to apply *his* definition to other writers, and has the mortification to find himself bewildered, or pursues throughout the passage an erroneous idea. Let me instance the word scirrhus.

If he considers its etymology, he will find it very well applied to any indolent tumour peculiarly hard; in general language, often to any of the

viscera indurated by chronic inflammation. In a closer sense he will find it confined to that species of tumour which agrees with Dr. Cullen's description of it in his *Nosologia*; ordo—*Partis magnitudo aucta sine Phlogosi. Genus. Partis, plerumque glandulæ, tumor durus, non dolens, ægre suppurans.* The other nosologists use for the most part the same language, except Linnæus, who makes no reference to a cancer. Yet Dr. Fordyce considers it as one of the terminations of inflammation. Most authors consider cancer as the sequel of scirrhus. Boerhaave seems to have been so seduced by his own language, that he advises us, by all means, to abstain from stimulating applications to a scirrhus liver, lest we produce a cancer; and Vansweten enforces his preceptor's advice. Yet neither of them pretend ever to have seen a cancerous liver, nor is any better authority produced for such a disease than a dissection, related by Bonetus, from Bellonius. It must be admitted, that so cautious has Morgagni been in this respect, that among the number of cases related of indurated liver, he never once uses the term scirrhus. Some anatomists, who are not less accurate in this respect, do not scruple to apply the term to that enlarged state of the prostate, which is among the calamities of old age. But surely it wants the characteristic mark of ever terminating in cancer. In how few cases where this gland has continued for years,

in

in an enlarged state, is suppuration found; or when ulceration has commenced on the interior surface, do we find an internal suppuration with a circumscribed hardness, like what is met with in the cancerous breast, or testis? Do we hear the patient complain of lancinating pains, or any other uneasiness, than what arises from the mechanical obstruction at the neck of the bladder? Here, however, the etymology of the word being consistent with any one of the senses, in which it is used, we have only to regret, that authors will not be careful to give a definition of their terms, or refer us to such of their predecessors, whose language they are contented to adopt.

But the worst consequences arise when an undefined term not only conveys an imperfect idea, but so far leads the student from the object of his enquiries, as to make him fancy *that* explained, which has scarcely been described. If Sydenham relates the symptoms of a disease, who, that wishes for information, is not concerned when he arrives at the conclusion? If the same author begins to assign the causes of symptoms, who has patience to read of the ebullition of the blood, the concoction, digestion, and separation of the inflamed particles, &c. in the small pox—of the subtle matter of the plague, which being free from the grossness of the variolous matter, requires no previous digestion, nor any ebullition to procure it? If any apology

were necessary for Sydenham, besides the ingenuousness with which he acknowledges his ignorance of the essence of the disease, it may be urged in his favor, that nothing ever diverted him from an accurate observation and description of every symptom, as it occurred.

Nor is this obscurity peculiar to authors of that date. Dr. Cullen, in describing the manner in which matter is brought to the surface, first contents himself with the experiments related by Sir John Pringle and Mr. Gaber, relative to the conversion of the gluten of serum, by stagnation into pus. "This pus however," continues he, "is not the converted gluten alone; for the conversion of this being the effect of a particular *fermentation*, which may affect the solid substance of the part, and perhaps every solid of animal bodies; so it most readily and particularly affects the cellular texture, and thereby a great deal of this is *eroded*, and forms a part of the pus, and it generally happens that some of the smaller red vessels are eroded, and some red blood appears mixed with the pus in abscesses."* Now as far as common language can direct us, and our author has given us no new definition to contradict it, fermentation and erosion are processes performed without the assistance of any living principle, the former being the spontaneous

* First lines, vol. 1. pages 196 and 198.

decomposition,

decomposition, or new arrangement of the particles of dead matter, and the latter the effect of chemical combination. Hence we might expect the same processes to be imitated with the same materials in the dead body. But if it be said that the term erosion is here used only as descriptive of the effect and not of the process, I then repeat that the term is highly improper, inasmuch as it lulls the mind into an indolent satisfaction, by making us fancy ourselves acquainted with what it is our duty to enquire after.

A similar instance of the misapplication of the term erosion we have in Dr. Swediaur's account of the formation of chancres. "The venereal virus," says he, "having been applied to the part, and by its stimulus produced an irritation and subsequent erosion,"—Now these two terms irritation and erosion to produce the same effect are incompatible with each other. The former may produce an action on the living part, the latter would be produced by the same causes in either the dead or living subject. If Dr. S. should say he has admitted irritation as the cause, and that erosion is only a *description* of the consequence, I answer that in this case, *erosion* has no meaning at all, and only misleads his reader, by making him fancy himself in possession of a *process*, the *effects* only of which are described. But, as is often the case, this language is not only liable to deceive the student, but

has actually deceived the writer. For, satisfied with the term erosion, he has overlooked a process between it and irritation—a process which all accurate writers have taken notice of, namely, a vesicle, or pustule, which, when broken, discovers the ulcer.

I was much surpris'd to find that ingenious veteran in our art, Dr. Fordyce,* use the term fermentation, in his account of digestion and secretion, till I met with a definition of the word, which extends to living as well as dead matter. "Fermentation he calls
 " the conversion of one compound into another, by a
 " new arrangement, or manner of combination of its
 " elements."—And afterwards, when speaking of digestion, he says, " The fermentation which takes
 " place, is peculiar to the organs of digestion, and has
 " never been produced by any artificial means." And farther on, " The operation by which they "
 [the secreted fluids] " are formed, is fermentation." By this the Doctor certainly makes us acquainted with the definition he affixes to the term fermentation. But in so doing, he confounds two processes entirely distinct from each other. Fermentation, as we see it, is a process peculiar to matter that never takes place during life, and secretion is a process that never takes place but during life.

But if we carry the Doctor's definition to its full extent, we shall find that it will lead us to call, not

* Elements of the Practice of Physic.

only the secretions he enumerates, but even the growth and propagation of animals and vegetables, a fermentation, since these are brought about by “a conversion of one compound into another, by “a new arrangement, or manner of combination of “its elements.” It seems to me, therefore, with due deference to such respectable authority, highly improper to use the same term for a process that can only be effected by living organs and one that is peculiar to dead matter.

The world is much indebted to the early labours of Mr. Abernethy, and from the instance he has already given of a practical improvement in the art, as well as a correctness of judgement in physiological experiments, we may expect the most important advantages from his future researches; yet even in his first writings, we meet with an instance of the inconvenience that results from inattention to words. In his explanation of the difference between the lumbar and phlegmonoid abscesses, he observes, that in the latter the surrounding parts partake of the inflammation in a sufficient degree to produce a thickening and *adhesion* of the cellular substance, so that there are two causes of the confinement of the pus, viz. condensation of the surrounding cellular substance, which is the consequence of the collected matter, and a thickened unnatural state of that substance, which is the effect of the inflammation it has endured.

dured. “ But when matter appears under the
“ skin very distant from the part where it was
“ formed, the quantity of surrounding inflamma-
“ tion being less, the cyst is formed *almost solely*
“ by the condensation of the surrounding substance.
“ Hence, when the evacuation diminishes the con-
“ densing cause, the surrounding cellular sub-
“ stance is likely to recover its original rarity and
“ looseness of texture, and thus greatly to diminish
“ the cavity of the abscess.”

In this description of the confinement of pus in the phlegmonoid abscess, we are first told of a thickening and adhesion of the cellular substance from inflammation. Afterwards it is said to arise from two causes, viz. the condensation of the surrounding cellular substance from the collected matter, and a thickened unnatural state, the effect of the inflammation which it has endured. Now the first of these propositions, which is nothing more than Mr. Hunter's adhesive inflammation, is undoubtedly true; but the second, if it differs at all from the first, is inadequate to the end proposed. For if the fibres of these cells are sufficiently elastic to admit of being pressed closer to each other, still while the communication of the cells remains, it cannot prevent the effusion of pus, nor is an unnatural thickened state of the substance arising from inflammation a sufficient description of that adhesion, which takes place
from

from a particular species of inflammation; for if inflammation produced only a thickening of the part without adhesion, it would still be unequal to the power attributed to it.

Much of what is here said may be applied to the observation, that in lumbar abscess the cyst is formed almost solely by the condensation of the surrounding substance. Either it is formed solely by it, or it is not. That such a cause is not equal to such an effect I have before shown, and Mr. Abernethy seems aware of it, by his addition of the word *almost*. What then is the other cause? it can be none but adhesion. If condensation from pressure were sufficient, why is it never sufficient in anasarca? If even inflammation were sufficient without adhesion; why is the serum diffused, when thrown into the cellular membrane in erysipelas? In cases of anasarca, we have sometimes inflammation from bandages. Here is inflammation, and such a confinement of the fluid, as might occasion condensation, yet nothing of this kind is permanent.

“But,” continues Mr. A. “in consequence of the
“evacuation of the matter the condensing cause is
“diminished, and the surrounding cellular sub-
“stance is likely, in some measure, to recover its
“original rarity and looseness of texture, and
“thus greatly to diminish the cavity of the abscess.”

Now if adhesion has taken place, the parts cannot

recover

recover their elasticity. If it has not, the matter could not have been confined. But taking the argument as we find it, what could possibly be gained towards filling the cavity if the fibres of the cellular membrane were as elastic as Mr. A. seems to describe them? Only those about the circumference of the abscess could recover themselves: some are absorbed, but many are broken through by the pressure of the matter, and in all the cases, which have been numerous, that I have seen of the cavities of large abscesses exposed after death, the ends of the fibres have been seen hanging loose from the sides of the cyst, so as frequently to line it with a flocculent, or rather fibrous stratum of different shades from the natural colour towards black. But Mr. Abernethy appears to me to have been led into this last error by the first: for having made up his mind that the cyst was caused by pressure, the consequence he relates seemed to follow.

Nor is this the only thing that dissatisfies me in this part of his reasoning. By ascribing the cyst of an abscess in one instance to a condensation, and a thickened unnatural state of the cellular substance arising from the inflammation which it has endured, and in the other to a condensation of the surrounding cellular substance only, he seems to consider one process of nature which we have both been taught, and which by the word adhesion he seems half to admit, rather as a matter of accident

dent than the effect of those laws, the uniformity of which, deserves a better name than "unnatural thickening or pressure." By this early making up his mind too, he has overlooked a very probable cause of the complexion and smell of the pus, which follows the future evacuations of the cyst. The first consisted only of the confined pus, but when the cavity is incomplete, and the sides collapse, the disposition takes place to unite the cyst by granulation. Before this can be done, the fibrous stratum must be cast off, which mixing with the pus discharged at future openings, accounts for the variety of colour and smell so often described, but not accounted for by this accurate observer.

I have been more particular in my observations on these writers, because the subject on which they have written, as connected with inflammation and its consequences, is perhaps the most important and leading one in pathology. There are a few others of less consequence which I shall slightly hint at.

I have already taken notice of some physiologists, who have admitted fermentation to take place in the changes in living animal matter. One of them I observed, has given a definition in such general terms, that if admitted, might include not only secretion, but other processes still more remote. But Mr. Moore, in his consideration on the theories

ries of Mr. Hunter,* is of opinion, that fermentation in its strict sense, as applied to farinaceous substances, is one of the causes of the increase of variolous and venereal virus. He insists there are only two ways in which fluids can be separated from the blood; the first by mechanical straining and filtering, and the other by a change of properties produced by some chemical means. After objecting to the mechanical, he adds, “it seems clear to me from the nature of the circumstances, that the change is a chemical one, yet there is great difficulty; perhaps it is impossible to discover by what particular chemical operation the change is effected, and the structure of the secreting vessels is too minute ever to be inspected.”

“Let it then be taken for granted, that all pus is formed by a chemical operation.” The reader surely cannot be so fastidious as to refuse his assent to such a *proof*.

Mr. M. then hints at the opinion, that fermentation is the cause of the secretion of venereal pus on the application of that virus to the skin, and also at the theory of “the most philosophical surgeon of the age” on that subject. The latter he shows is attended with some difficulties; then returning to the doctrine of fermentation, and stating Mr. Hunter’s objections to it, he concludes:—

* Essay on Materia Medica, p. 217.

“These

“ These reasons, I think, make it certain that venereal pus does not act as a ferment. Mr. Hunter’s opinion must therefore be well founded;” (yet Mr. Hunter never hints at a chemical process).

Mr. M. next considers the effect of the poison when absorbed and taken into the circulation. As he cannot conceive that the small quantity of matter absorbed from a venereal ulcer, or from the arm under inoculation for the small-pox, can be sufficient of themselves to produce such effects as follow, he supposes that the poison must be afterwards increased in the blood.

“ When this is admitted,” continues he, “ it is then to be enquired how the quantity of virus can be increased in the blood. Having endeavoured to prove that the venereal pus could only be formed by a chemical process, I cannot help thinking that this augmentation of the virus must be accomplished in a similar manner.”

“ It appears more similar to fermentation than any other operation in nature; but there is *one striking difference*, namely, ferments only hasten a process that would naturally have taken place. For example, leaven quickens the fermentation of paste, but paste naturally ferments, although it requires a longer time than when mixed with leaven. But common pus or blood has no tendency to form venereal virus. The action of
“ venereal

“ venereal matter upon the blood in that respect,
 “ therefore, differs from that of a common fer-
 “ ment on those subjects on which they operate.
 “ Yet, as we have *no better word than fermenta-*
 “ *tion* to express the change in question, we think
 “ it proper to adhere to it, though some patholo-
 “ gists prefer assimilation.”

Now if this is to be called reasoning, it will
 not be difficult to prove that a man and an image
 of rye dough are the same thing. For though it
 may be right to take notice of *one striking differ-*
ence, namely, that a man can walk and an image
 must be carried, yet as there are other resemblances
 we may prefer calling them by the *same name*. If
 other differences should occur to some readers, the
 same may be said of fermentation. Is there any
 thing like the fermentation of farinacea in animal
 matter? In fermenting farinacea, does it alter the
 properties of the fermented substances, what fer-
 ment we use? If we express the juice of the grape,
 and add the yeast of malt to it, do we make beer,
 or *vice versa*?

But, absurd as this reasoning may appear, it does
 not end with this preference given to the *word* fer-
 mentation. The word once adopted, nothing so
 easy as the transition of incorporating it as a fact
 into the subsequent reasoning.

“ While it seems probable,” continues Mr.
 Moore, “ that venereal virus occasions *something*
like

“like fermentation in the fluids; it is likewise evident that it irritates the solids. A complete explanation of the phænomena of the disease, therefore, can only be obtained by admitting both the theories of fermentation and irritation.”

It is one of Sir Isaac Newton's maxims, in reasoning philosophically, that where one cause is sufficient to account for a phænomenon, we should never look for two. In the present instance we have three; for either fermentation is like other chemical processes, or it is no chemical process at all. If the former, there can be no occasion to make a distinction between that and other chemical processes; if the latter, we have three causes to bring about, what we may conceive to be brought about by one, while the other two not only appear unnecessary, but in all respects inapplicable. For if irritation, as the author admits, is sufficient to induce the first phænomenon, why must we add chemistry to produce an effect which the test of chemistry has never been able to detect? And if the irritation of a particle of venereal virus that escapes our senses is sufficient to produce a chancre, why should we insist that the matter absorbed from this chancre, however little, is insufficient to produce an irritation on several parts of the skin and bones?

C

But

But it is inconceivable through what absurdities our author pursues this phantom of fermentation. Not contented with showing or admitting with Mr. Hunter that mercury, by producing a new irritation, may destroy a former one, and by that means the venereal sores may heal, he goes on thus;—
 “ We have hitherto treated of the action of mercury on the solids only, we now proceed to make some observations upon its action on the fluids.

“ It was formerly observed, that the venereal virus produces *something like* fermentation in the fluids; no permanent cure can therefore be expected, without this fermentation is stopped. Mercury certainly has the power of stopping this fermentation.” Thus a process, the language of which was at first only adopted, is next spoken of as probable, afterwards reasoned upon as proved, and last of all its existence affirmed as a certainty. For it could not *certainly* be stopped unless it *certainly* existed. And this too without a single proof of the fact, and in defiance of positive evidence, which the author is aware of and admits.

In this instance we see a writer adopting a mode of expression, instead of stating a fact; and afterwards reasoning on his expression as a proposition that he had proved. But we shall find another, without waiting for this process, at once openly assuring his readers,

readers, that a definition of one thing is a proof of another.

“Whoever,” says Mr. Foot,* “has considered what was the cause of pus, uniformly agrees that it must arise necessarily out of a solution of the parts where that pus was formed. Now, as no ulcer is found in recent gonorrhœa, as no destruction of parts are offered in vindication of so uncommon an assertion, that the discharge from the urethra is pus; why has the professor so roundly given this secreted fluid the unqualified appellation of pus? The definition of the word will not admit its application here, and the definition of the discharge proves the fact.” It would be very difficult to place this sentence in any logical order, so as even to comprehend, much less to answer it. However, as the proof rests on the definition, let us enquire what this definition is. We are told, that “pus must necessarily arise out of a solution of those parts where it was formed;” but what is the solution of parts on a granulating sore, where we have the truest pus? We are at last referred to Mr. Pott’s definition. But we shall find Mr. Pott much more cautious in his language; and his definition, as far as it goes, is, like that of most other writers, not of pus and mucus, but of the parts secreting them. Hence it is, that these

* Obs. part 1, page 18.

fluids have always accommodated themselves to every fresh discovery that has been made of the nature and seat of a disease. While gonorrhœa was supposed to arise from ulcers, it was pus. As long as fistula lachrymalis was deemed a fistulous ulceration, the discharge was pus. When an improvement in anatomy taught us the contrary, it became necessary either to alter the name of the discharge, or to admit that it might be secreted without any loss of substance. Mr. Pott, whose work on fistula lachrymalis appeared as early as the year 1758, had evidently his doubts, even at that time. But as his object* was to reform a cruel and erroneous practice, in which he succeeded, he perhaps did right not to entangle himself in a dispute which would not at all affect the theory of the disease. After observing, therefore, that it would carry him too far from his present purpose to attempt pointing out the true character of the two fluids, he contents himself with showing the different offices and conditions of the parts from which they are supplied. Even this is done with that caution which usually distinguishes a writer who has a well-earned reputation to lose. His account of mucus is introduced by "*If I conceive rightly of this affair.*" When he speaks of pus, it is in a manner not less guarded.—"Without,"

* Earle's Life of Pott, p. 18.

says he, "entering minutely into the origin and
 "nature of it, *I believe I may venture 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 pro-
 "duction." This language, which was more than
 admissible at the time it was written, is somewhat
 different from "Pus must necessarily arise out of
 "a solution of the parts where it was formed;"
 an assertion which can only be maintained by
 another equally unintelligible, that "the definition
 "of the discharge proves the fact."

We may expect from the labours of nosologists
 a gradual reform in our physical language; but
 that this science is at present far from perfection,
 appears by the confession of the last writer on that
 subject, who admits, *Nosologos successus optatos non-*
*dum obtinuisse.**

To those who wish to correct a part of the wri-
 tings of these laborious authors, without accurately
 and diligently examining the whole, Dr. Swediaur
 has left a very useful lesson. After the hacknied
 objection against the word gonorrhœa, he thinks
 proper to substitute in its stead blenorragia, from
 βλεννος (it is usually written βλενω), mucus, and
 ρεω, fluo. There can be no other objection to this

* Culleni Præfatio.

word, than that it is of too general an import, and is as applicable to catarrh or dysentery as gonorrhæa. But it is fit our author should speak for himself, as he seems well aware of the objections that may be raised against his term.

THE FOLLOWING IS HIS

NOSOLOGICAL TABLE.

Class. Locales

Ord. Phlogoses or Mucifluxus

Genus. *Blennorrhagia*. *Anglis*, Clap. *Germanis*, Tripper, *Gallis*, Chaudepissie. *Italis* *Gonorrhœa*

Character in viris. Titillatio, præcipue in urethræ parte anteriore, in lacuna Morgagni sub frænulo; subsequente post biduum aut quatrimum, phlogosi locali cum ardore & dolore in mingendo; accedente stillicidio *materiei purulentæ vel puriformis ex urethra*; corpore cavernoso urethræ præternaturaliter turgescente, plerumque cum erectionibus membri virilis solito frequentioribus, dolorificis

In Feminis, &c. —

Variat ratione sedis

Blennorrhagia balani
 _____ urethralis
 _____ vesicalis
 _____ labialis
 _____ vaginalis
 _____ uterina
 _____ nasalis

Materia fluente

ex glande
 ex urethra
 ex vesica
 e labiis vulvæ
 e vagina
 ex utero
 e naribus

Spe

Species sunt

Blennorrhagia syphilitica

α. Simplex

β. Complicata, s. ulcerosa

Blennorrhagia a manuſtupratione

———— ab acri externe applicato

———— a ſtimulo interne applicato

———— arthritica

———— a calculo veficæ

———— a cancro uteri aut vaginæ

Though what follows is hardly leſs objectionable than what I have tranſcribed, yet as this is ſufficient to relieve me from any imputation of partial quotation, I ſhall intrude no longer on the patience of my reader.

The firſt remark that muſt occur to every one after the inaccuracy of the latinity, is the looſe manner in which the *order* is deſcribed; *phlogoſes*, or *mucifluxus*, two words that have not the moſt diſtant affinity with each other. If we take the firſt, it will not include all the ſpecies, *blennorrhagia e manuſtupratione*, being unattended with inflammation. If we take the ſecond, we ſhall find it only a Latin tranſlation of the word uſed to expreſs the genus.

It is well known that the deſcription of the genus ſhould be applicable to all the ſpecies and varieties.

rieties. As our author has thought proper, contrary to all systematic writers, and in defiance of common sense, to enumerate the latter first, let us see how far he has been careful to confine his description of the genus to what may be applied to all the varieties. The French name for his genus, he tells us, is *chaude-pisse*; and in the description we have *cum dolore & ardore in mingendo*. How is this applicable to *Blennorrhagia nasalis*? When I had read all the varieties he enumerates, and among the rest *vaginalis e vagina, nasalis e naribus*, I could not help being surprized at not finding *ocularis ex oculis*. It is a common adage, “*qui plus lachrymat minus minget*,” and Ovid, who is said to have described some of the operations of nature with peculiar accuracy, somewhere uses the phrase “*lachrymas tepidas*,” which Dr. Garth, a physician, translates “tears—which scalded as they fell.” But these are poets, let us confine ourselves to medical writers. Mr. Hulston,* Member of the Corporation of Surgeons, Fellow of the Society of Antiquaries, and of the Medical Society of London, and Surgeon to the Philanthropic Reform, in his pamphlet, entitled *Sketches of Facts and Opinions, &c.* assures us, that a *gonorrhœa* may be contracted in the eye by the application of ve-

* This gentleman is engaged in some experiments on venereal matter, and hopes by *chemical* analysis to elucidate the enquiry, whether the matter of gonorrhœa and lues venerea is the same.

venereal matter to that organ; and the industrious Astruc, who fed a dog six months with dog's flesh, to see whether it would give him the venereal disease, tells us of a young man who got a venereal ophthalmia by the application, not of pus, but of the excretion from which the French name the disease.

It should seem, indeed, from a *rare case* related by Mr. Jesse Foot, as if the venereal fluid had a peculiar attraction to the eye, or as if the eye had a particular power of analyzing that fluid when considerably diluted.*

* " In a rare case which I saw of a gonorrhœa of the eye, and where the eye was lost, it evidently appeared that it arose from the following accident, and that it was produced through the application of the 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 that 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 became thus infected; because that 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."

See Foot's New Discovered Fact, printed for Becket, 1790, and entered at Stationer's-Hall, p. 8.

It

It is true all these facts, except the chemical one, are a little at variance with Mr. Foot's *New Discovered Fact*,* and his reasoning upon it; but as they all

* “ And I mean to prove from what I shall say hereafter, “ that the local fluid of a gonorrhœa, or chancre of a *subject* “ thus only locally infected either with the one or with the “ other, will not or cannot venereally infect any other part “ to which it can be applied of the same *subject*. In “ other words I mean to prove, that the 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, 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 ever produce them. And further I mean to “ prove—but that I believe will upon reflection be found “ to be unnecessary, for every day's observation does “ prove it, that gonorrhœal fluid on the same *subject*, se- “ creted by the same *subject*, never produces a chancre up- “ on the same *subject*, and that chancrous fluid on the same “ *subject*, secreted by the same *subject*, never produces a “ gonorrhœa. I mean to prove more, or at least, I mean “ to declare positively, that if in consequence of the ori- “ ginal fluid, which was received from another *subject*, “ and which took effect, only that part where this *ab origine* “ fluid acted, ever was impressed or acted upon through this “ single connection by this venereal fluid; and that if there “ were not the least grounds or possibility of any other in- “ fection being contracted, so that every other power was “ precluded, but that if a chancre on this part was ever fol- “ lowed by another chancre, or by a gonorrhœa, that such “ fresh local symptom must be produced out of the original “ chancre

all rest on historical evidence, the reader is at liberty to take which he pleases. But why need we look so far for authorities? Our author himself tell us, that the consequence of a *retropulsed* gonorrhœa is among others, a *venereal ophthalmia*. Now to be *retropulsed*, is to be driven back again, that is, to the place from whence the *subject* came. Hence it appears the *chaude-pisse*, or *blennorrhagia cum ardore et dolore in mingendo, originates in the eye*; and that our author has been highly negligent in not mentioning *ocularis* among the other varieties.

If the reader is not satisfied with this specimen of nosology, let him consult the rest, which he will find not less edifying. This accurate etymologist, who is so much dissatisfied with the word *gonorrhœa* as to substitute *blennorrhagia* for it, tells us that its *sequela* or gleet is *blennorrhœa*, as if there were any difference between the two words, except in

“chancre which was the seat of the action of the fluid of
 “the other *subject*; if it was at all, I mean to be understood
 “in this instance, to say that it is impossible to come to pass,
 “and that it never did come to pass.”

Foot's New Discovered Fact, p. 12.

If the reader does not perfectly understand all this, I recommend him to the Pamphlet, where he will find the *subject* so often repeated, and placed in so many *new* lights, that it will be impossible it should escape him.

found :

found: and among the species of this blennorrhœa, are enumerated in women,

Ab ulcere aut cancro uteri vel vaginae,

A manuſtupratione.

Hence not only *cancer in utero*, but *manuſtupratio*, is the effect of the venereal diſeaſe, though it has been generally thought its cure; or perhaps this, as is ſaid of ſmall-pox, is a diſeaſe people may be frighted into.

C H A P. II.

ADVANTAGES OF MORE CORRECT LANGUAGE
AND REASONING, ILLUSTRATED IN OTHER
SCIENCES, AND IN PHYSIC.

IT may seem strange that a science so important should still labour under disadvantages, from which others of more remote application to human happiness, have in great measure been rescued. But authors do not always write with a view to the improvement of knowledge. Hence their works are often addressed to those who are but incompetent judges. Readers, incapable of ascertaining the validity of a first principle, readily give the writer credit for its truth; and when this is got over, all subsequent reasoning appears so demonstrative, as often to make us forget where we set off.

Nothing is more flattering to the vanity, or favourable to the indolence of the human mind, than that language which seems to teach us the result of a proposition, without the necessity of attending to the demonstration. For strange as it may seem, the more simple a proposition is, the greater difficulty oftentimes the mind feels in attending to its demonstration, or comprehending its solution. How many ages did men rest satisfied with what they fancied a solution of mercury rising in the
Torricel-

Toricellian tube! The language of the schools was, "Nature abhors a vacuum. The *fuga vacui* is the cause of the suspension of the mercury." The fact itself, which ought to have been accounted for, was brought as a proof of the doctrine. Now though this *fuga vacui* was a mere hypothesis, while it passed current it was likely to be more popular than the true solution of the phænomenon, inasmuch as it saved the mind the trouble of thinking, and attending to those laws on which atmospheric pressure depend.

To give a further illustration of the subject.— If in the less improved state of chemistry, a professor should exhibit to his pupils the spontaneous ignition that takes place on the mixture of the nitrous acid with spirit of turpentine, and should seem to explain it to them as arising from no other cause than the force of their mutual attraction, which by the rapidity with which it acts, excites so much motion as to produce heat enough to set them on fire; no one will now say that the cause was in any respect explained. Yet how congenial to young minds, might such an apparent solution prove; and how much more popular than the long process by which a true philosopher would explain the sudden decompositions that take place, and the extrication of fire that was before in a state of combination.

I am

I am aware it may be urged, that in what is called the true explanation, we are still far from arriving at primary causes; that in the first instance we have still to learn the cause of gravitation, and in the last what the matter of fire is, and why it is not active under every combination. But in both we can trace those LAWS by which the phænomena are governed; that is, in the first instance we can show, that the mercury only keeps its height by a pressure on one part, and a want of resistance on the other, which we see constantly occurring in all other bodies; and in the latter, we can so far trace the laws of chemical attraction, and are so well acquainted with the constituent parts of the subjects of the process, as to account for the phænomenon, by its agreement with those laws when applied to the same bodies, under different circumstances.

Sir Francis Bacon, the father of modern philosophy, has shown in his *Novum Organum*, the causes that impede the progress of natural knowledge. Among others, he mentions the too great impression our mind feels from those properties in bodies which most forcibly act upon our senses, and dwells much on the errors men have fallen into, in explaining all the phænomena of nature, by the laws of that branch of science to which they have principally attended. Thus the Aristotelians applied logic to natural philosophy; the chemists expected to

to discover all the arcana of nature by furnaces, and Gilbertus undertook to solve every thing by the magnet.

Speaking afterwards of the great influence words have over the mind, he observes, that even a definition of them in our pursuit after *natural knowledge*, is hardly sufficient to keep us from error. "Because," continues he, "definitions consist of words, and words give birth to words; so that it is necessary to *recur to certain facts, their series and order*—as will be explained when we come to the manner of constituting notions and axioms."*

This he afterwards explains, by shewing the insufficiency of the common manner of reasoning by syllogism, which by stating only a few facts, and reasoning from them without attending to the objections, is always inconclusive. † "In constituting

* Quæ tamen definitiones, in naturalibus et materiatis; huic malo mederi non possunt; quoniam et ipsæ definitiones ex verbis constant, et verba gignunt verba: adeo ut necesse sit ad instantias particulares earumque series et ordines recurrere; ut mox dicemus, quum ad modum et rationem constituendi notiones et axiomata deventum fuerit.

Nov. Org. lib. 1. Aphor. 59.

† In constituendo autem axiomatico, forma *inductionis* alia, quam adhuc in usu fuit, excogitanda est; eaque non ad principia tantum (que vocant) probanda et invendienda, sed etiam ad axiomata minora, et media, denique omnia.

Inductio

“ing an axiom,” says he, “a new form of induction must be thought of, from what has hitherto been in use; one that will apply to the principal and all the intermediate axioms. The induction that will be truly serviceable in demonstrating the arts and sciences, ought to analyze nature by rejecting and excluding as far as is necessary, and afterwards forming conclusions on what may be ascertained.” In the succeeding book he proceeds to what he calls the art of interpreting nature; and, after expressing the insufficiency

Inductio enim, quæ procedit per enumerationem simplicem res puerilis est, & precario concludit, & periculo exponitur ab instantia contradictoria, & plerumque secundum pauciora quam par est, & ex his tantummodo quæ præsto sunt, pronunciat. Ab *inductio*, quæ ad inventionem & demonstrationem scientiarum & artium erit utilis, naturam separare debet, per rejectiones & exclusiones debitas; ac deinde post negativas tot quot sufficiunt, super affirmativas concludere; quod adhuc factum non est nec tentatum certe nisi tantummodo a Platone, qui ad excutiendas definitiones & ideas, hac certe forma inductionis aliquatenus utitur. Verum ad hujus inductionis sive demonstrationis instructionem bonam & legitimam quamplurima adhibenda sunt, quæ adhuc nullius mortalium cogitationem subiere; adeo ut in ea major sit consumenda opera, quam adhuc consumpta est in syllogismo; atque hujus inductionis auxilio, non solum ad axiomata invenienda, verum etiam ad notiones terminandas, utendum est. Atque in hac certe inductione spes maxima sita est.

Nov. Org. Lib. 1. Aphor. 105.

D

of

of the human powers to arrive at the primary causes of things, observes; * “ For although
 “ throughout nature nothing really exists but in-
 “ dividual bodies having simple individual ACTIONS,
 “ according to a LAW, yet in every theory that
 “ law, and the tracing, finding out, and explain-
 “ ing of it, is the foundation of science and expe-
 “ riment.”

It is hardly possible to conceive a more concise yet perspicuous distinction than this between true theory, which, I believe, the late Sir Joshua Reynolds defined to be “ the knowledge of what nature is,” † and that fondness for reasoning which teaches us for every effect to assign a cause, without the labour of tracing facts or considering the various objections that may occur. The great philosopher, however, whose opinions I have been endeavouring to trace, was well aware of the difficulties that must attend his mode of reasoning by

* Licet enim in naturâ nihil vere existat præter corpora individua, edentia actus puros individuos ex lege; in doctrinis tamen illa ipsa lex, ejusque inquisitio & inventio atque explicatio pro fundamento est tam ad sciendum quam ad operandum.

Nov. Org. Lib. 2. Aph. 2.

† Probably from the *το ον* or *τε οντος ζήτησις* of Plato.

induc-

induction.‡ In the passage already quoted, he adds, that for justly and fairly drawing such induction or demonstration, many facts must be established, concerning which we are at present entirely in the dark; so that more time must be spent than the syllogism requires. From this he seems to admit, that in the then imperfect state of science, men must be contented with the latter, but points out the precise manner in which natural knowledge must hereafter be studied.

His method was pursued by those great luminaries who appeared about the close of the seventeenth century, when the laws of matter and of motion were defined in a manner that will stand the test of all succeeding ages. But in physiology we find Bacon himself failing in the only attempt he made. In his "History of Life and Death," his first enquiries are entirely directed to the causes that contribute to the preservation and corruption of dead animal matter. When he afterwards comes to reason from the more obvious properties of living animals, there is hardly a conclusion he wishes to establish but is contradicted by so many facts of his own stating, as are sufficient to invalidate any position, how generally soever it may be laid down. Nor was this to be wondered

‡ See the conclusion of the quotation in the note of page 33.

at in one who wrote in an age that furnished him with so few facts. About this period, however, we owe the discovery of the circulation to Harvey; a few years after of the lacteals to Aselius; since that of the thoracic duct to Pecquet; and to the anatomists of our own days the office of the lymphatic vessels, as a system of absorbents.

When we take a view of the powers of the human mind, as we see them displayed in all the other arts, we shall rather wonder that nothing more has been done in our own, than that these discoveries should have been made. When we further consider how little advantage has been derived from them towards forming a rational physiology, our surprize must increase.

In all our physiological researches, the most striking fact, and what one should think would be perpetually obtruding itself on every thinking mind, is the change produced on dead animal and vegetable matter, by its becoming part of a living animal. The first causes of this will probably forever elude our enquiries; but the fact must be admitted by every one who sees that an animal is fed, that he grows, and that there is a difference between a dead and a living muscle. Is it then credible, that though the circulation was discovered more than a century and a half ago, till our own days no physiologists have ever made it a subject
of

of enquiry! And so little had our minds been accustomed to such researches, that when Mr. Hunter undertook to show, that the change from dead to living matter commenced in the blood, the new theory became a matter of wonder or ridicule. So easy was it thought to laugh away the idea, that *the blood is alive*, that hardly an experiment was instituted to prove the contrary, or still less, where this vital principle commences, if not in the blood. Dr. Fordyce's definition of fermentation will, as I before remarked, solve, as far as words can, this and almost any other difficulty, but without bringing us forwarder in our enquiries.* Hence, and from many other instances in the preceding essay, we see the danger of trusting to *definition* in physiological researches, and I shall now attempt to illustrate, by a well known fact, Bacon's objection to the syllogism; his observation, that it is necessary to recur to certain facts, their series and order; to reason by such induction as will meet every objection; and that the foundation of science is to trace that law by which simple,

* In the Essay on Digestion, Dr. Fordyce drops the term fermentation, and uses only the definition. In this essay, also, instead of considering the blood (see his Practice of Physic) as constantly tending to putrefaction, which it is only kept from by its motion in the vessels, he admits the probability of its life.

individual actions take place in individual bodies.

The dispute concerning *hernia congenita* is familiar to every one. Let us then trace the history of it in a manner no one can object to. Dr. Hunter, and probably other anatomists, before the enquiry was instituted, saw the testicles within the abdomen of a foetus. This was one of those facts which, as the discoverer neither communicated nor enquired further into, might be said to amount to nothing, and the subject might have passed for a monstrous production. Mr. Sharp after this acquaints Dr. Hunter that he had seen in an old rupture the testicle in contact with the gut. Here was an important fact, and the more so as it was communicated. Had such been the case with Dr. Hunter's, Mr. Sharp might have pursued his enquiry into the cause of the rupture he met with, and the whole difficulty might have been solved. About this time appeared Baron Haller's *Opuscula Pathologica*, in which he gives a case of *hernia congenita*, shows, that the testicles are originally formed in the abdomen, and adds that their descent is about the time of birth, when he conceives they are somewhere about the ring of the external oblique muscle, and by the breathing and crying of the child, and the action of the abdominal muscles, are forced into the scrotum.

Mr.

Mr. Pott, who wrote a year or two afterwards, describes the fact pretty much in the same manner. Haller afterwards, in his *Primæ Lineæ*,* of 1764, from which the Edinburgh edition is printed, ascribes their descent to increased weight from the blood rushing into them. Either of these hypotheses might be supported by syllogism. To prove the latter it might fairly be urged, that the weight of the testicle is increased by the increased quantity of blood it receives from the new course of circulation, which respiration produces; that by the well-known laws of gravitation, all bodies move to the centre or downward, in proportion to their weight, and that from the erect posture of the human body, the scrotum is lower than the abdomen. Hence the cause of the phenomenon. In favour of the other opinion, it might be urged, that such are the consequences of pressure on the contents of the abdomen, that when violent, even in the adult, it will frequently cause a protrusion of the intestines through apertures before not discoverable. That respiration and the new action of the abdominal muscles must be violent in proportion as the parts are unaccustomed to such a pressure; and that when a child

* Denique sub inguine in plena ætate ponuntur, situ mutato, vi forte sanguinis irruentis.

Prim. Lin. 807.

cries, which it usually does, and is very likely to do, from the peculiar situation of so sensible a part as the testicle, this pressure is very much increased. Hence the solution of the difficulty.

But it is impossible it should escape Haller, that the gravitating cause might be prevented by the boy being kept so continually in an horizontal position as infants usually are; or Mr. Pott, that if the process depended at all on the action of the abdominal muscles, *hernia congenita*, instead of a rare, must be almost an universal disease. But the inconvenience of the mistake could not end here. For these great men, one of whom spent a life in experimental philosophy, and the other had procured a number of subjects, the appearances of which he had noted, were so well satisfied with their explanation of the difficulty, as to prevent either of them inquiring after the period at which the testicles descended into the scrotum; a circumstance which had they attended to would have shown them the fallacy of their reasoning.

This last task was reserved for one who neither reasoned by definition nor proved by syllogism, but who considered, that though throughout nature nothing really exists but individual bodies having simple individual *actions*, according to a *law*, yet in every theory, that law, and the tracing, exploring, and explaining of it, is the foundation of science and experiment. Rejecting, therefore, the

sylo-

syllogism which never stops to obviate objections, and every induction which would not apply to the intermediate, as well as the principal axiom, we find him attentive only to facts, their series and order, and forming no conclusions but what could be well ascertained. By a close observation of the state and situation of the testis in every age of the *fœtus*, he discovered the *time* which led to the mode in which the testicle descended, not only without the accidental aid of weight, posture, or temper, but without the danger of the intestine descending with it. In a word, he traced nature throughout every part of the process, and showed she had as regularly provided for this change, and as much by laws suited to the actions to be produced, as she does for the changes from the *fœtal* to the state of respiration, from childhood to puberty, from the virgin to the impregnated uterus. When these laws were traced, it was easy to see the consequences that must follow the interruption of any of them; and by these means to account for the orifice remaining open, and the descent of the intestine, which sometimes, though rarely occurs.

But it is hardly credible how much the original error in which Haller entangled himself, warped all his future reasoning on this subject.* Not

* *Errores radicales & in prima digestionem mentis; ab excellentia functionum & remediorum sequentium non curantur.* Nov. Org. Aph. 50.

contented with endeavouring to make it appear that the descent of the testis during or after birth, is very common, though every other anatomist dates it at least a month before that period, we find him still looking for a mechanical cause to produce an effect, the greater part of which he is forced to refer to a specific operation of nature. We have before seen him impute it to the action of the lungs, since that to increased weight. But at length the point being admitted, that for the most part the descent is before birth, these are both given up, and the whole is referred to pressure. One might think, says he, that the increase of the lungs, and their action, is the cause of the progress of the testicle; but as it most commonly happens before birth, we must refer the whole to the single efficacy of the abdominal muscles.* And why must we have recourse to either the action of the lungs or the abdominal muscles to produce an effect, the whole of which they are unequal to, and the only part of which, were they to perform it, they could not do without danger? Can the action of the abdominal muscles bring the testicle from the loins forward to the ring of the external oblique?

* Elem. Physiologia, Lausanne, 1778, vol. 8, p. 311. *Pressio* Eo testium iter refero, qui paulatim, de renum sede, in scrotum descendunt. Crederes pulmonum augmentum, & respirationem ejus itineris causam esse; cum tamen plerumque ante partum testes id iter relegant, oportet id totum musculorum abdominis simplici efficaciam tribui.

or after it has passed through the aperture, can those muscles close the orifice of the peritoneal process, so as to form the future *tunica vaginalis testis*? Above all, can they act in this manner on the testis, without affecting the other contents of the abdomen, which, during a considerable part of the progress, lie anterior to the testis, and which when the testis descends later in life, are very apt to come down with it. It must then be admitted by all, that to produce part of this change in the situation of the testis, nature has provided certain laws, that only take place while that action continues. The question that remains is, would any thing be saved if one part of the operation were performed by an action that is always going on, viz. that of the abdominal muscles? On the contrary, the operation is only so much the more complicated; * because, besides that of directing the testes in their passage through the ring, another process must be set up to prevent the escape of the intestines along with them. Thus it is that men incumber themselves, when they attempt to prescribe laws to the operations of nature, instead of recurring to facts, their series and order, and reasoning by induction from them.

* *Causæ rerum naturalium non plures admitti debere, quam quæ et veræ sint & earum phænomenibus sufficiant. Dicunt enim philosophis natura nihil frustra fit per plura quod fieri potest per pauciora. Natura enim simplex est & rerum causis superflua non luxuriat.*—Sir Isaac Newton's Principia.

I have

I have chosen to confine myself to this single fact, because it is the more pointed from the two names it opposes to Mr. Hunter, and because I have printed authority for all that is asserted. But though Baron Haller is certainly entitled to the highest respect for his genius, industry, and readiness of communication, it must be admitted, that his physiology is too often tainted with mechanical reasoning. Let me instance his account of the wasting of the thymus gland. *Eadem a pulmonibus aëtis elisa & ab aorta nunc majori sensim evanescit.** Is this the language of poetry or philosophy? Of the latter it cannot be. Let me ask, among all the whimsical theories of which Mr. Hunter is accused, has he ever been heard to evade a question in such a manner as this, or to accuse the Divine Artificer of suffering one of the contents of a circumscribed cavity, when all are in their natural or healthy state, to disappear by the dashing, or overgrowing of the rest? I am not ignorant, it may be urged, this is only a relation of what actually takes place; for that the lungs are in constant motion, the *aorta* is increased, and the *thymus* gradually disappears. But let me ask, is it not given

* Primæ Linæ, D ccccxxi. This chapter closes with an account of the elongation of the cæcum, and the increased size of the feet, which are thus accounted for. Cæcum intestinum nascitur a fæce ad dextra appendiculæ pondere suo deorsum nitente, & pedes insigniter augentur a sanguine a ligatis umbilicalibus arteriis repulso, Dcccclii.

us by Haller as a cause producing an effect? Now is such a cause equal to such an effect? Can the motion of the lungs wear the *thymus*, when the thin *pleura* that is interposed escapes, or could the increased *aorta* lessen the size of any neighbouring organ, if the œconomy had not provided absorbents to take up parts no longer necessary to the system?

In these instances I have only endeavoured to show, how much advantage has been derived from pursuing Bacon's method in physiology. In pathology, which must be considered as a branch of the former, it would be difficult before our own times to find a single instance of the application of this mode of reasoning, except in such cases as depend on mere anatomical investigations. In all these it would be great injustice not to acknowledge the industry of the French; to whom, whatever may be said of the difficulty of giving credit to all their histories of cases, we certainly owe the foundation of practical surgery. Is it, that this conscious superiority renders them inattentive to the improvements of other nations, or that a Hunter has not yet appeared among them to show, that in enquiring into a disease, we are not to have recourse to definition and syllogism, but trace pure individual actions, and the law by which they are governed? I have already shown the deficiency of many well received writers in this respect, and shall now attempt

attempt to illustrate it with more precision, by contrasting their mode of reasoning in a single disease. But first let me premise a few general remarks on morbid poisons; which, without perpetually bringing forward the same name, I wish my reader to understand, I derive principally from Mr. Hunter.

C H A P. III.

OBSERVATIONS ON MORBID POISONS.

POISONS are substances which change the action of a part, or of the whole constitution, from a healthy to a diseased state. They are either animal, vegetable, or mineral. The animal poisons may be divided into the original and the morbid. The former are the secretions of animals, as the viper in a state of health, the latter, the effect of disease; these last are since Mr. Hunter's time, pretty generally called Morbid Poisons. The morbid poisons are those which convey a diseased action from one animal to another of the same, or a different species. This may be either by vapour, contact, or wound. The first usually produce fever; which, if the patient is able to struggle through the stages of it, subsides of itself. Those which affect by contact or wound, produce a local disease, which sometimes only extends itself by the diseased action being kept up on the part; at others, affect various parts of the body by absorption. The hydrophobic poison is an exception, being given by contact, and producing a critical fever. The small-pox, and perhaps most of the poisons producing eruptive fevers, may be communicated
by

by contact, or vapour. In the morbid poisons, the quantity applied, provided it be sufficient to produce the consequent disease, does not seem to lessen or increase it.

For a morbid poison to produce its full effect, the subject that receives it must be susceptible of the diseased action it occasions. The part to which it is applied, or the constitution must take on a disposition to the diseased action, and nothing must interfere to prevent the action taking place.

The susceptibility depends on the constitution, or the state of it at the time the poison is applied.

If the constitution is susceptible, the local *disposition* will take place on the application of the poison, and the action follow in a certain period. From this, as soon as matter is absorbed, the constitutional disposition will follow; but the diseased *action* will not take place till a certain period, according to the laws of each individual poison.

The susceptibility is confined for the most part to distinct species of animals, as the mange to dogs, the rot to sheep, and a great variety to man. This is, I believe, liable to only the single exception of hydrophobia. But all men, and probably other animals, are not susceptible of the poisons peculiar to their species. Some men pass through life without feeling the effects of the variolous infection, and still more without that of the venereal, though exposed to it as much as the greatest sufferers.

ferers. Many are susceptible at one time, and not at another. Some have even shown a susceptibility of the local, and not of the constitutional infection, or absorption has produced no consequent disposition to the disease. In some, a pimple has appeared on the arm after inoculation for the small-pox, such as could not have arisen from a mere puncture without infectious matter; this has gradually subsided, and the constitution remained uninfected, though exposed to the effluvia of the disease. In the venereal, how many do we find admitted into the hospitals in whom the local disease has existed long enough to commit the most formidable ravages, yet no symptoms from absorption have followed!

That there is a period between the time the infection is received, and the diseased action shows itself, is evident in every instance. And as in most we can perceive no alteration during that interval in the actions of life, this state of the constitution has been called by Mr. Hunter, a disposition to take on the diseased action. In the casual small-pox,* we find a space of about twelve days between the reception of the poisonous effluvia and the first symptoms of the disease. But the disposition to the disease, must have existed during that whole space, because the subject may be re-

* See Dr. Haygarth.

moved from all the means of infection from the time he first received the effluvia, yet this will not prevent the disease from appearing at the appointed period. Travellers frequently leave the country where they received the miasma of ague long before any symptoms of it appear; yet the disposition having been given, a removal into better air does not prevent the diseased action taking place.

BUT even when the disposition is given, the action may be superseded for a time. Of this we have several instances, related by Baron Dimfdale, of persons under inoculation, in some of whom the period of eruption was protracted, and in others the pustules after eruption rendered stationary by the action of other diseases.* These are

* IN the first case described of those attended with erysipelatous rash, the eruptive fever began on the eighth day; but erysipelas supervening, the small-pox eruption did not show itself till the eleventh day, when the former subsiding, the latter began and completed its progress.

Dimfdale's Present Method of inoculating for the Small-Pox. 1767. Page 100.

IN other cases the eruption appears to have been stationary. See Cases 15, 16, 17, 18.

Page 104, and seq.

By Dr. Haygarth's collection, the most common period of eruption after inoculation, seems to be the eighth day. Whether those which happened later arose from the inter-

are instances where the action is suspended in morbid poisons, producing their effect by symptomatic fevers, having their stated crisis and termination. The same happens to those which affect locally, and by absorption, without a crisis. The venereal never shows itself on the skin, or in the bones, while the first local symptoms are yielding to mercury given in any form. But the disease will appear in the skin or bones, after the first local symptoms have been removed, and the effect

interruption of any other disease, does not always appear, except in one case related by Dr. Clarke, where the eruption was protracted to the twenty-fifth day. In this, seven days after the incision, an erysipelalous fever and eruption attacked the patient. It is very remarkable, how seldom the common progress of the disease is interrupted in hospitals or inoculating houses. Of five hundred cases given by Dr. Woodeville, four hundred and seventeen had the symptoms on or before the eighth day, fifty on the ninth, thirty on the tenth, two on the eleventh, and one on the fourteenth.

Of three hundred and eleven given by Dr. Clarke from the Newcastle Dispensary, eight had the disease on the twelfth day, on the thirteenth three, on the fourteenth three, on the sixteenth, seventeenth, and twenty-third, one each. (See Dr. Haygarth's Sketch of a Plan, &c. page 397.) Does this difference arise from the patients in the hospital being kept more free from any other infection or accidental cause? This appears more probable, because inoculation in private practice is by no means so uniform in its phenomena as stated by Dr. Woodeville.

of mercury on the constitution has ceased. In this case the diseased action has been suspended, after the disposition had taken place. This will be at once admitted by every one who reflects, that the source of infection no longer existing since the cure of the original local complaint, the disease that afterwards appears must be in consequence of matter absorbed before the local was cured; and if its appearance is later than the stated time, it must be because the constitution was occupied by another, namely, the mercurial irritation.

IN the small-pox these phænomena are more obvious, because, since the introduction of inoculation, the periods of infection, fever, and eruption, have been marked with greater perspicuity, and the uniformity is more striking from those periods being short, and all the symptoms acute. But if we allow for the difference between eight days, which is the medium the inoculated small-pox requires, and six weeks, the medium of the venereal for showing the effects of absorption, we shall not find the variation in any greater proportion. For if, as the same author observes, the former varies from one to ten, or even twelve days, without any apparent constitutional impediment, the latter may be supposed to vary from one to as many weeks, and even longer, because in proportion

tion to the length of time there is a greater probability of some accidental circumstance happening, to suspend or forward the action.

AN objection of some force has been raised against this language. "Action," says an ingenious writer, "must have taken place or not: if it has taken place, there is disease; if not, there is none. I have no conception of any intermediate state. What is named disposition, appears to me impossible; for no part of the body is disposed to disease, although all are susceptible of it." *—There is at first sight an appearance of accuracy in the language of this quotation. But as I trust I have sufficiently shown the difference between susceptibility, disposition, and action, I shall only endeavor to defend the term disposition.

THAT this disposition is a modification of action, or, in other words, that some alteration has taken place in the action of the part or constitution, is highly probable; but as, to use the language of a writer of equal accuracy and elegance, † "this is often without any perceptible injury to the ordinary actions of life," it is surely proper to dis-

* Moore's Mat. Med. p. 273.

† Jackson on the Fevers of Jamaica, p. 134.

tinguish it from that action which is obvious to our senses, and which constitutes the disease. Till, therefore, we are better acquainted with the real state of the part or constitution between the period of infection and the formation of the disease, it is surely justifiable to call that intermediate state the disposition to take on the diseased action.

FURTHERMORE, we have great reason to believe, that not only an action may be suspended, but also that a disposition may be prevented from taking place, even when the poison is applied. I have already observed, that the constitution of itself may not be susceptible at the time; and it appears that other causes may prevent its being so. Of this a very striking instance is afforded us in the effect of the Harmattan wind.* Seventy negroes were inoculated for the small-pox three days after the Harmattan set in; none of them had any symptoms of the disease. In a few weeks afterwards, fifty of the same were inoculated and had the disease; the rest had taken it in the natural way. Here, though the infectious matter was applied to seventy, all of them, as appeared afterwards, constitutionally susceptible of the disease, yet even

* See Norris's Memoirs of the Reign of Bassa Ahadee, King of Dahomy. Also Philosophical Transactions, Vol.

the local disposition was superseded, and of course the constitution was not infected.

At the time of any raging epidemic, the morbid miasma must be generally diffused; yet it is universally allowed, some ages and constitutions are more susceptible of the disease than others, and also that a particular mode of living more generally prevents or favours this constitutional susceptibility. It is true, that this can never be reduced to demonstrative evidence, because we never can ascertain whether those who escape would have taken the disease, had none of the above circumstances occurred to prevent it.

It is a law with most of those morbid poisons which produce their effect by a critical fever, that a constitution which has once gone through the action excited by them, is no longer susceptible of it. This is the case, with very few exceptions, in the small-pox, in scarlatina, and probably in most infectious fevers. I say with very few exceptions, because some cases to the contrary have been so well authenticated, that I am not sufficiently sceptical to doubt that the susceptibility may be so strong in some constitutions as to admit of the disease a second time, after a certain period. But poisons which produce diseased actions without critical terminations, as the venereal, do not

leave the constitution less susceptible of their influence, except for a time.

FROM being able so well to ascertain the immediate causes of such diseases as are to be traced from poisons, and from an accurate observation of their progress under different combinations of events, the laws peculiar to some of them have been ascertained with as much precision as pathology can be expected to arrive at. It is to be regretted, that the same cannot be said of some others, which, no doubt, when well understood, will be found to observe the same regularity as all the other operations of nature. Though the venereal has existed for three centuries, and a specific remedy for it been known almost as long, yet, till our own days, no writer has undertaken to trace the series and order of the two, so as to detect the laws by which each of them are governed. Nor is it less remarkable, that in a disease said to assume such a variety of forms, as to be reducible to no law, the ablest of the profession, forgetting the fallacy of ocular demonstration, should so often have taken upon themselves to determine by the eye, and sometimes on a superficial glance, whether an eruption or old ulcer was venereal or not:—a node on the shin bone has usually been condemned at first sight. Such has been the facility with which this *insidious disease*, as it was often called, has been detected;

detected; while those which are better known have required an accurate statement of their history and symptoms before they could be ascertained.

ANOTHER morbid poison, which, from its observing something like critical stages, might have been better ascertained, is the yaws of the African negroes. This disease, as far as I can learn,* has its crisis and termination, though its periods, from being longer, appear more irregular. It seems to have the singular property of never inducing suppuration. Such at least is the general law of the disease. It sometimes happens, that a large yaw suppurates after the fungus is formed; but at this time the disease has run through its stages. The first appearances are pimples which, when the cuticle is separated, discover flougs or sordes. Under these arise funguses of various magnitudes, according to the situation and nature of the disease. These funguses gradually increase, some to the size of a small wood strawberry, others of a raspberry, and some exceeding that of the largest mulberry, "which berries they very much resemble, being knobbed as they are." If the cure goes on well, the funguses scab over, and by degrees, the scab falling off, discovers the sound skin underneath.

* Edin. Med. Ess. Vol. 6, p. 312.

IN this fungating property, it shows some resemblance to the *fiyvens* of Scotland. But in the latter the funguses rise after ulceration. In this disposition to form fungus, *fiyvens* differs from the Canadian disease, and both of them from the venereal in the phagedenic nature of the ulcers. The Canadian disease, too, is often cured by the unassisted powers of the constitution, which never happens with the venereal. How far such is the case with *fiyvens* does not appear from the accounts I have been able to collect. Mr. Bell,* and Dr. Swediaur,† consider *fiyvens*, the Canadian, and the venereal, as varieties of the same disease. But this only increases the puzzle; because the venereal, never yielding but to mercury, contrary to the Canadian, leaves us more than ever in the dark concerning *fiyvens*. Mr. Bell tells us too, on the authority of a practitioner, whom he does not undertake to refute, that *fiyvens* yields more readily to corrosive sublimate, than mercury in any other form. This is contrary to the now established practice in the venereal.

Dr. Gilchrist ‡ describes some of the phagedenic ulcers in *fiyvens*, as healing in one part while

* Bell's Treatise on the Lues Venerea, vol. 2, p. 442.

† Swediaur on the Venereal, page 175.

‡ Phys. Essays, vol. 3, p. 154.

they

they spread in another; which would lead us to suppose, that the disease might cure itself, were we certain these ulcers did not arise from the remedy, which is usually mercury. The same author speaks of the fungus as a rare appearance in his own neighbourhood. Mr. Bell describes it as characteristic of the disease which both allow derives its name from the resemblance of the fungus to a wild raspberry.

DR. Gilchrist says, the disease does not attack the larger and harder bones. By his description, indeed, it does not appear to attack any of them, but by the spreading of the ulceration from the soft parts, as the bones of the nose from the throat, the palate from the roof of the mouth, and other contiguous parts. Mr. Bell has seen it in both the bones of the leg, and in the cranium. But as he conceives *sivvens* to be syphilis, he may mistake syphilis for *sivvens*. Both authors describe ulceration as spreading much faster than in the venereal. Mr. Bell says, when the disease is first received at the mouth, as soon as the throat begins to ulcerate, the *uvula* and *amygdalæ* will be sometimes destroyed in a few days; and Dr. Gilchrist says, children at the breast, seized with it in the mouth and throat, sometimes perish with hunger, not being able to suck or swallow. Mr. Bell tells us, the part first infected has the same appearance as a venereal

nereal ulcer; but his incorrect manner of describing a disease, well known in the Southern metropolis, makes one doubt his accuracy in this. He conceives, it never originates in the genitals, because those who are infected abstain from coition. This is a great deal to answer for, especially as we are told, that when the virus has entered the system, it first commonly breaks out on the genitals and neighbouring parts. We are not told whether the virus, in this case, has entered the system by absorption from a previous local ulcer, or without that effect. If the latter, we may be allowed to form our own conjectures on the originality of the ulcers. At present, however, we must remain in the dark on the most important phenomena of a disease, we should have expected would have been well ascertained, in a country of which it appears to be indigenous, and which has held such a deserved reputation for the improvement of medical knowledge.

If any practitioner or student, after the recess will favor me with an accurate detail of the symptoms, their series and order, and the effects of the remedy, I shall be anxious to acknowledge the obligation, and to do the writer that justice his endeavors will entitle him to. The Encyclopædia Britannica disappointed me much, in making no mention of *shivens*, under its medical article. But

as the letter S is nearly in order, we may expect to be gratified in this particular, in a work which challenges a sort of national reputation. If, in the mean while, any gentleman should be disposed to undertake the enquiry, I would recommend to him the following method of directing it.

FIRST, Whether the disease ever appears without producing a local effect on the part where it might be supposed to have been received.

SECONDLY, Whether there is any, and what, difference between the primary and constitutional ulcers.

THIRDLY, Whether mercury cures the disease without producing those effects on the system which we find necessary in the cure of inveterate chancres.

FOURTHLY, If the primary symptoms are cured before the constitutional ulcers have appeared, whether the latter ever show themselves without the recurrence of the original ulcer.

FIFTHLY, Whether, in cases of phagedena, the parts usually heal by granulation, or skin over without the lost substance being renewed.

SIXTHLY,

SIXTHLY, Whether the disease is ever cured by the uninterrupted efforts of the constitution.

SEVENTHLY, If this be the case with the phagedenic ulcers, whether, if the fungufes can be destroyed by applying escharotics as low as the found part, the disease will cease in that part, and healthy granulations rise, no other remedy being used at the same time.

LASTLY, Whether mercury ever fails to effect a cure.

IF this last point can be ascertained with the same certainty as in the venereal, there can be no danger in the experimenter's inoculating himself, which he should do with the matter of both primary and secondary ulcers.—Dr. Swediaur refers to Mr. Bowman's account of the Canadian disease, which I should not have failed to consult, but that all my enquiries of the medical literati and book-sellers in this great town have been fruitless.

IN all these morbid poisons we find a disease communicated similar to the parent stock. But it appears as if the healthy secretions of one person may, under certain circumstances, be deleterious to another. Whether any of these poisons originated from such a cause, cannot now be determined;

terminated; certain it is, that many of them are of recent date, which in some we can ascertain with tolerable accuracy. Though all of them may be communicated through the pores of the common cuticle, yet they are more readily conveyed where that membrane is either broken or particularly thin. It is well known that the cuticle is incapable of ulceration. This is proved in a variety of ways. When abscesses approach the surface, we see every part partake of the suppuration till they arrive at the cuticle, which is elongated into a bladder, and bursts. When broken through, it is for the most part so thin as to be lost in the dressings, or whatever approaches the part. Where the cuticle is thicker, as in the palms and soles, this is much more obvious.

THE author to whom we are indebted for the account before given of the yaws, observes, that when any of them are seated on the sole of a negro, there is no other remedy than by paring the thick cuticle to come at the yawy fungus, and afterwards treat it in the manner he proposes for those situated in other parts. Among the labouring people of this country, it is not uncommon to find similar inconveniencies from the variolous matter confined under the cuticle of the palms of the hand. In this case we find a large sub-cuticular collection, which breaks the cellular membrane,

brane, but never escapes through the cuticle, unless opened by art. This property in the cuticle is the great protection of the body from morbid poisons acting by contact only. It is not indeed always sufficient, because we find the venereal and variolous will produce their effect by passing through the pores of the cuticle, and ulcerating the skin beneath it. In this case, as in that of abscesses, the cuticle does not partake of the ulceration, but is elongated into a vesicle.

BUT where the cuticle is broken, or is particularly thin, the effect of morbid poisons is much more certain. It even appears, as was before observed, that the healthy secretions of one person may, when applied under these circumstances, prove deleterious to another. The parts over which the cuticle is thinnest, are the genitals, the papillæ, and the mouth. In all these we have instances of disease implanted, which cannot be traced from the parent stock. The ancients, who were ignorant of the venereal, could not fall into our error of imputing every ulcer on the genitals to that source; but then their ignorance of the effects and progress of morbid poisons, made them consider these diseases as peculiar to the parts, without ever suspecting they were received by coition. This is the less remarkable when we consider, that these ulcers are seldom, if ever, met
with

with in the *vagina*. Celsus,* whose accuracy can only be exceeded by his brevity, describes no less than nine appearances of ulcer on the penis, which he distinguishes by their progress and situation. I shall avail myself of the order in which he has arranged them.

SOME of them may be imputed to the secretion of the glands hardening and changing its properties; an inconvenience said to be much more considerable in warmer climates than in our own; we need therefore only take notice of those which have been confounded with the venereal. The first of these is an ulcer that occasions phimosis, the second an eating ulcer near the *frænum*, painful, with a sanious discharge. All these he considers as readily cured by gentle remedies and detergent washes.

THE next species is by no means uncommon in this country, but has too generally been considered as venereal, and treated as such. It is either seated on the glans or inner part of the prepuce, spreads very rapidly, and in a short time the glans or a part of it mortifies and sloughs off, after which a cicatrix forms so readily as sometimes to endanger the urethra. Celsus was so well aware of this,

* Lib. vi. chap. 18.

that whenever the case happens, he advises instant circumcision, and a probe to be introduced, lest the prepuce should adhere to the glans, and even glue up the urethra.

OF this kind of ulcer I shall direct my reader to three cases. The first he will find in the Edinburgh Medical Effays.* The surgeon, observing the mortification, thought it necessary, according to the practice of those days, to amputate the penis above the slough; after which granulations rose to such a height as to form something like a new glans. Another case of the same kind is related by Turner,† who, as soon as he perceived the gangrene, thought it right to cut down to the living part, and apply hot dressings. In spite of this, however, the patient recovered, but was less fortunate than the former, nothing remaining after the cure but the *stump* of a penis. The case related by Mr. French, in Mr. Hunter's Effay,‡ was of this kind. That none of these were venereal is probable, from their being so well known to Celsus, and from their healing readily without mercury. In the first case only small doses of calomel with purges were exhibited: but here the part being cut off might

* Vol. 1. art. 19.

† Syph. page 248.

‡ Hunter on the Venereal, page 385.

account for the disease ceasing. In the second no mercury was given till the slough had cast off, and the parts began to heal. The last case related by Mr. French was much exasperated by mercury. When that remedy was laid aside, what was left of the glans penis sloughed off, and the fore healed.

THOUGH it may seem certain these cases were not venereal, it might still be doubted whether they arose from impure coition. But the last case places the matter beyond a doubt, exostoses appearing on the scalp and tibia. The cure of these without mercury is a further proof that the source was not venereal, and that other poisons may by inoculation produce effects apparently similar to that disease.

IT is a remarkable proof of the accuracy of Celsus, that he particularly distinguishes these species of ulcer which heal either by common applications or after sloughing, from those which spread either by phagedæna or a succession of sloughs. It is well known that all local diseases which spread without any circumscribed termination, are by this writer called cancer.

THE reader is not to be offended at meeting with *ulcer exedens* among the first, and φαγεδαίνα in

the second. He should remember that Celsus derives his definition and division of cancer* from the Greek physicians, which, after explaining, he accurately follows. That the chapter *de obsecanarum partium vitis* is intended more particularly for the use of such patients as through over delicacy could not apply for relief, and to such no description could be more intelligible than *ulcer exedens*. But this kind of ulcer, which so readily gives way to common remedies, is very different from his definition of *cancer*. *Non solum id, quod occupavit, corrumpit, sed etiam serpit, &c.* The erysipelatous inflammation is for this reason among his species of cancer. But what I particularly wish to remark here is his description and division of phagedæna into two species. The first is the common phagedæna, for the cure of which he advises the actual cautery. The other he describes as beginning with a blackness or slough, and if not prevented, spreading to the bladder, in which stage no assistance can be given. If this is seated on the glans near the urethra, he advises the same remedy, with proper care to preserve the orifice of the urethra, but if the disease has penetrated deep, that the knife should be used.

THE first species is by no means uncommon. The third volume of the London Medical Trans-

* Lib. v. cap. 26.

actions contains a case by Dr. Donald Monro of a phagedænic ulcer, in the space of five or six weeks eating away the whole glans penis. By degrees it spread so as to lay bare the muscles of the abdomen, and, destroying part of the epigastric artery, produced a hemorrhage of which the patient died before any relief could be given. During the patient's life Dr. George Monro was consulted, who pronounced the disease, like six others he had seen, to be of a cancerous nature, and produced by the too free exhibition of mercury in its early stage. But this was so far from the case, that no mercury was given, except as a drastic purge, before the doctor directed a mercurial friction.

ADMITTING that purging was improper, it is not likely that no improved mode of treatment should ever remedy the inconvenience of it in a juvenile constitution. It should also be remembered that the doctor himself found the inflammation so considerable as to induce him to advise bleeding. In a word, if the disease was venereal, why did it not yield to mercury? If cancerous, how should mercurial purges produce cancer? or why should cancer assume so unusual a type as to ulcerate without any surrounding callosity, or without a previous scirrhus?

THE other species of phagedæna is, I believe, described only by Celsus. Of the accuracy, how-

ever, of his description, I have lately had a remarkable instance, which I shall now relate.

A GENTLEMAN accustomed to little pustules from the sebaceous glands about the face, was in the habit of pressing out what he called the maggot, as soon as it appeared. Observing one of them on the under part of the prepuce, he treated that in the same manner, and while the skin was broken continued his intercourse with a woman with whom he had been long connected. Finding the pustule did not heal like those on the face, he applied to a physician, having previously used a solution of lunar caustic, and the mercurial friction. The physician, before the slough was cast off, recommended him to a surgeon more in the habit of seeing such cases. After the eschar came off, no disposition to heal appearing, the mercurial friction was advised. The sore very soon spreading, and the constitution showing but slight symptoms of being affected by the mercury, the friction was continued, and the quantity increased. But in spite of all the ulcer spread, insomuch that while the mouth was sore, it had extended laterally so as to be seen without raising the penis, and downward to the scrotum. In this situation he sent for me. On hearing the history, I had no difficulty in making up my mind, that, whatever the case might originally have been, it was not then venereal.

Nor

Nor was this opinion founded on the presumption of any better knowledge of the subject. The gentlemen before consulted were my superiors in professional rank, in age, and in their opportunities of seeing cases of every description. But I came in after them, and had their facts and experience to reason upon. The physician was likewise of opinion that the friction should be discontinued. On laying aside the use of mercury, and attending to good diet and bark, the ulcer put on a clean, granulating appearance, and there seemed no doubt but its healing would be in proportion to the rapidity with which it had spread. But at the very time when a beginning cicatrization might have been expected, a paroxysm of fever came on, with violent pain in the part, and an effusion of serum. This, as well as the consequent increase of the ulcer, was imputed to some accidental irregularity, or one of the changes common in an irritable habit. In a short time all these unfavourable symptoms disappeared, and the wound assumed its former clean and florid complexion. But in a few days the former symptoms recurred, with similar consequences. At each paroxysm of fever the ulcer not only put on this unfavourable appearance, but gained ground in all directions. The same train of symptoms succeeded each other so often, that I could now tell when to expect them, and observed before the febrile attack a livid ap-

pearance about the skin at the edges of the wound. As the fever went off, all that part of the skin disappeared, and left a clean sore underneath, which soon after assumed a healthy granulating appearance.

THE prospect of so long an attendance made me desirous of the assistance of another surgeon. Having therefore warned my patient not to expect any amendment till the whole prepuce was sloughed off, Mr. Cline was consulted. From the alternate changes in the sore, he made no scruple to assert, it could not be venereal. The usual remedies were tried for the constitution, and the dressings varied according to circumstances; but the ulcer increased as before. The fever was however less at each returning paroxysm; and as the edge near the *corona* became attacked, the true phagedæna appeared more and more, with but little previous blackness of the skin, or slough. This continued till the whole prepuce, and that part of the scrotum which forms a part of it when the penis is erect, entirely disappeared. Till this was nearly accomplished, though we had frequently the appearance of beginning granulations, no disposition to form skin or cicatrix ever showed itself.

THE

THE process of skinning now began at the upper edge next the pubis, and increased rapidly till the whole was nearly covered. Soon after the skin began to form, a copper spot appeared on the inside of the right thigh, and others on the hands; and in a day or two afterwards the patient discovered some *bumps*, as he combed his head: he had also an ulcer in his throat. When these were diligently examined, and we had retired, Mr. Cline expressed his suspicions, that they were venereal; and his surprize at the same time, that the *cranium* should have given no pain. About the same time appeared a protuberance on the right tibia; and the patient remarked, that he was become bandy. It was however agreed, not to give any mercury for the present, lest the healing process of the penis should be interrupted, nor to alarm the patient with the discipline he was likely to go through. But in a short time the blotches began to ulcerate. That on the thigh, in the space of four days, became as broad as a crown piece; another appeared on the sternum, which was itself elevated, and a new tumor arose on the forehead, which gave the fingers every sense of fluctuation. It now appeared no longer doubtful what course to pursue. Five grains of crude quicksilver, rubbed down with conserve, were given daily for three days, and then increased to eight grains for eight days.

AT

AT first the success answered all we could expect; the ulcers took on a kinder aspect, no new ones appeared, and the tumors in the cranium lessened. But at the end of eleven days his constitution began to feel some of the effects of the mercury, as was evident by the smell of his perspiration and breath. Immediately the ulcers, from a sound granulating surface covered with good pus, became of a dusky red, with bloody fannies. This was, without exception, the case with all of them. The bones remained the same.

I NOW ventured to suggest to Mr. Cline, that the disease might still not be venereal. He expressed some surprise, but offered to put it to the single test of the bones, which, if they should recover without more mercury, he would admit were not likely to have been tainted with that virus. At all events, the mercury was to be discontinued. The bones did recover without more mercury, and before all the ulcers healed.

AT this time the patient, finding his health better, wished to try the effect of country air, which, as mercury could not then be repeated with safety, we made no objection to. On his return, in about a fortnight's time, his throat was again ulcerated, and such of the old external ulcers as had not healed threw up a kind of fungous granulation, resem-

resembling the accounts we have of *sivvens*. The sore on the penis, which had never completely skinned, was spread to about the size of a shilling, but, instead of a phagedænic, had only an indolent appearance, such as is common with parts newly cicatrized in a debilitated constitution. His health, however, being better, mercury might have been exhibited with safety. But, by this time, like many invalids, he seemed disposed to consult every body, and was satisfied with nobody. He brought with him a prescription from a physician in the country, for a decoction of the woods, which prevented our doing more for the present than giving him a gargle of corrosive sublimate. Before two days trial of these remedies, another physician was consulted; and after that the case transferred to another surgeon. This gentleman, having no doubts of the venereal origin of the disease, exhibited mercury freely, and the ulcers healed. After this, some pieces of bone were exfoliated from the nose; and almost as soon as he recovered from the mercurial irritation, blotches appeared in his face, a fresh ulceration on the throat, and the tibia again enlarged without any pain. He is now under his fifth mercurial course.

THIS case, in its first stage, is exactly described by Celsus, as the phagedæna, distinguished by *quæ-*

dam

dam nigrities quæ non sentitur sed serpit. It was indeed seated on the prepuce, and his directions are applicable to the glans, because his principal object in them is the preservation of the urethra. When phagedæna spreads in this manner, it is impossible to say how far the disease may have extended before the loss of substance shows itself. The propriety of the direction, therefore, to cut beyond the diseased part [*præcidendum*] in this instance, and to cauterize in the other, is easily comprehended.

THAT this case was the effect of a morbid poison introduced from the broken skin at the lower part of the prepuce, is most probable; and that it was not venereal, is to me equally so. Is it consistent with what we know of the latter, that an ulcer should increase while mercury is showing its effects on the constitution? Even if mercury were exhibited in too great profusion, there should have been a period during its use when the ulcer should have showed a healing disposition; but the contrary was invariably the case till the remedy was laid aside, and indeed long after the constitution had recovered from all the common effects of it. Was the phagedæna, then, the effect of mercury? This is more probable, especially when we recollect, that a caustic having been very early applied to the part, the action from the virus might have been destroyed, and that of mercury have
been

been uninterrupted in a crude wound. But the wound showed no disposition to heal when the eschar cast off, before the second exhibition of mercury; and when that remedy was discontinued the phagedæna was not of the kind that has ever been described as consequent on the use of mercury, nor indeed such as existed while the constitution was under the strongest mercurial irritation. The constitutional symptoms were much more suspicious, but the blotches ulcerated much earlier, and the progress of the ulcers also was much more rapid than that of the venereal. The nodes were unattended with pain, and the rapidity with which, after the formation of matter, they yielded to small doses of mercury, is at least new in the history of the venereal. Nor have those spongy granulations ever been described as consequent on venereal ulcers. Lastly, have we ever before heard of venereal ulcers assuming that unfavourable aspect at the time when the mercurial irritation had just arrived at such a height as would induce us to expect the most favourable issue?

I SHALL not now insist on one argument drawn from Mr. Hunter's observations on the laws of the venereal, namely, that when the disease is once cured in the skin or bones, it never returns to those parts from the same stock of infection. If the disease in other respects resembled lues venerea,
this

this circumstance would only prove the fallacy of Mr. Hunter's theory. But I will venture to say, that, excepting its being a morbid poison producing a local effect, and also effects from absorption, it had no single character of the venereal. That the first local ulcer which came under my observation was not the effect of mercury, there is the fairest presumption. That it was not venereal is certain, if the chancre has a character which every accurate writer has described, and which alone distinguishes it from all other ulcers.

THE reader must have observed, that among the great variety of ulcers described by Celsus, there is no mention of a thickened edge and basis to any of them. That Celsus was not inattentive to such appearances, every one accustomed to trace his accuracy will admit, and I shall have occasion hereafter to show. But if we allow the concurrent testimony, not only of Mr. Hunter, but of every accurate writer of respectability before his time, the thickened or hard edge and base are the true characteristics of the venereal ulcer, or chancre. It is therefore rather a mark of Mr. Foot's inattention than of his critical acumen, when he asserts, that "ulcers are only found out to be venereal from situation or suspicion."*

* Observ. on Hunter, part 3, p. 8.

Astruc,† whom he is perpetually quoting, describes them with an exactness which always accompanies that author, and adds a theory which, considering the time it was formed, shows an ingenuity equal to his industry. Sydenham,‡ and indeed all other writers of established reputation, make the same remark. It is the absence of this edge and base in the ulcers of most other morbid poisons, that produces, I suspect, such a rapidity of ulceration as is very properly denominated phagedæna.

THE language of Celsus would lead us to suspect, that these species of ulcers were much more common in his time than at present. But the truth probably is, that since the knowledge of the venereal has taught us the use of mercury, all such as give way to that remedy have passed for venereal, and only those that resist it are now taken notice of. It is plain, however, from the methods proposed by Celsus, that some of these ulcers of his date resisted all the then known remedies. This argument may be extended to his severe manner of treating diseased bones.‖ His division into *vitium, caries & nigrities*, which exactly answers our node, ulceration, and caries; his choice

† Astruc, vol. 1, p. 407.—Barrowby's translation.

‡ Swan's translation, p. 308.

‖ Lib. 8. cap. 2.

of the word *vitiatum*; and his description, *primæ fere pingue est*, would lead us to suppose he had an imperfect idea that the bone was in some way contaminated. This opinion is strengthened by the great anxiety he shows, that the whole of the diseased part should be removed, either by the hot iron producing exfoliation, or by rasping or scraping. *Qui radit hæc audaciter imprimere ferramentum debet, ut agat aliquid & maturius desinat.* By the last part of this sentence we can perceive, that he had met with cases which were neither relieved by a spontaneous cure, nor by any known remedies, nor by any thing short of an entire removal of the vitiated part, the smallest portion of which being left, would re-produce the disease. It is also worth remarking, that when the caries is seated in the *cranium*, *sternum*, or ribs, he considers all attempts at burning as useless, and that nothing short of cutting away the part can be efficacious. It is now well known, that this severe treatment is not necessary. Our knowledge of the venereal and its remedy, has taught us to apply the latter to bones infected with this, and probably with other morbid poisons. It has also taught us to distinguish between bones enlarged from the stimulus of a poison, and diseases arising spontaneously, or from a peculiarity of constitution. But the state of surgery in Celsus's days was not such as to suggest this difference. Finding therefore
some

some cases which could never be relieved but by the removal of all the vitiated part, it is not surprising if he considers this *vitium* as inherent in all diseased bones. These inferences, I am persuaded, will be admitted by every candid peruser of the passage, as well as the probability that all the ulcers under the division of *cancer in cole*, were the effects of morbid poisons, which still occasionally show themselves, and which, though at present confounded with the venereal, are really distinct from it.—If the reader still retains any doubt on this subject, I would recommend him to peruse Mr. Becket's paper in the Philosophical Transactions, on the antiquity of the venereal disease, where he will not only meet with a description of ulcers on the genitals infecting the rest of the body, before that poison was known, but with an account of some that were cured by mercurial salivation. Astruc gives a very good abstract of this paper, and answers it with much learning and good sense, so far as he confines himself to the supposed antiquity of the disease.

I COULD relate other instances of ulcers of this kind, but my wish is to confine myself to such as do not rest on my own authority. I shall now therefore direct the reader's attention to a few cases of morbid poisons which have been communicated by the nipples.

IN the third volume of the Edinburgh Medical Effays,* we have an account of several ladies whose nipples and other parts of the body were infected in consequence of having their breasts drawn by a woman who had an ulcer in her mouth. Though the writer of the paper considers the disease as venereal, yet there is no evidence of its being such. The woman in whom it originated had no complaint in any part of her body, excepting two ulcers in her mouth, one of which healed with no other application than a gargle of infusion of woodbine. Many of the patients were not cured by mercurial courses; and the author remarks, that the disease was different from that given in the common way, both in the malignancy and *quick progress of the symptoms*. But the poison was applied only to matrons, and on a part from whence there was less danger of the infection spreading. Had it first occurred in a camp, and affected only the genitals, it is difficult to say how extensive the contagion might have proved.

INSTANCES of sore nipples in nurses, and often without any apparent disease in the children, are so numerous, that every practitioner must have met with a variety of them. Happily they are, for the most part, attended with little ulceration,

* Fifth Edition, p. 297.

and readily heal by discontinuing the application of the child's lips. But there are too many instances of the contrary. The most remarkable case that has come within my knowledge was that of a woman sent from the parish of Worth, by the Rev. Dr. Bethune, to St. Bartholomew's Hospital. She was suspected by the village practitioner of being poxed, and in consequence salivated by a mercurial friction. The disease not making any great progress in the child, no remedies were used for it. When the two arrived at the Hospital, they were under the care of Mr. Pott. But on hearing the history of the disease, he determined the case not to be venereal, and referred it to the physician. Dr. David Pitcairn had then the care of them. The woman had lost her nipple, with a considerable portion of the breast; a phagedænic ulcer was creeping along her neck and face, and had destroyed part of one eye-lid. Cicuta and decoctions of several of the woods were tried, as well as the mercurial salts in small doses; but the disease continued to gain ground, and even appeared on the inside of the other breast. In the mean while, the breast originally infected, and part of the neck, healed, but without the lost substance being filled up by granulations. The child took no kind of medicine, and recovered in the air of an hospital; while its mother was reduced to this wretched spectacle. She returned uncured

to her parish, where she died. That this case was the effect of a morbid poison, cannot be doubted. That it was not venereal is evident, from its not giving way to mercury, from a part healing while the disease gained ground in another, and from the child being so slightly infected.

THE cases related by Mr. Hunter* are instances both of the nipples and mouth being affected by two species of morbid poison, evidently distinct from each other, and from the venereal. Most of the cases recovered without mercury; and where that remedy was successful, it was in preparations and doses that are never found equal to the cure of the original venereal ulcer.

I SHALL now make a few remarks on the tooth cases. That many of these were not venereal is evident, because they recovered without the use of mercury. This was the case with one in which a node appeared on the tibia. Most of those which yielded to mercury were affected by that remedy much earlier than the venereal ever is, or, what is more to the purpose, before the constitution showed any symptoms of mercurial irritation. In the fatal case the ulcers spread with a

* See Treatise on Venereal, p. 386, and seq.

rapidity

rapidity never known in the venereal, a convincing proof that there was no callous edge or base. For the same reason, as will be hereafter explained, the ulcers yielded to a short exhibition of calomel,* part of which, by the manner in which it affected the bowels, probably escaped by stool.

AN accurate examination of all these cases will convince the attentive reader that none of them were venereal. The five first in order were of the common phagedæna. The sixth exactly resembled that species of ulcer which Celsus describes by the glans falling off, after which the parts heal by common applications. Of this kind I have related three cases from different authors,† and I trust convinced the reader that neither of them were venereal. In every one of them the sloughing of the part produced the cure. In the present instance we are told, “about a month
“after the operation an ulceration of the gums
“took place, which increased to a very great de-
“gree, till the edges of the gum sloughed off, after
“which the part healed as fast as any other ulcer.
“But, though the gums recovered perfectly, they
“remained considerably shorter.” In the fatal case related by Sir William Watson, and referred

* Medical Transactions, vol. 3, p. 325. Hunter ut supra.

† See page 66.

to by Mr. Hunter, the original ulcer was so constantly attended with pain and fetid discharge, and extended with such rapidity that it was most probably the second species of the phagedæna of Celsus, which spreads by a succession of floughs, like the case described in page 70.

I NEED not apprise the reader, that this attempt at classing anomalous morbid poisons by the local actions they produce must be very incomplete. The facts are few in number, and often imperfectly related. Many of them can only be traced in a single individual, so that we are unable to ascertain whether all the appearances are to be ascribed to the peculiarity of the constitution, or the laws of the poison. Those which were communicated are now extinct, and in such as were treated with mercury we cannot at present ascertain what part of the symptoms is to be ascribed to the disease, and what to the remedy. I trust, however, this arrangement will not be altogether useless, and that if it does not teach us always to ascertain what a disease is, we may at least learn what it is not.

To call a disease by the name of that which it seems most to resemble, would be at any time unphilosophical, and in the present state of medical improvement is, in my opinion, inexcusable. This
want

want of accuracy has, on former occasions, been the cause of very serious evils. When the venereal first appeared, the physicians of those days called it by the names of known diseases, and attempted to cure it by remedies efficacious in them. To the unrestrained boldness of empiricism we owe the only remedy we can now rely on. Almost in our own days we have been witnesses of the fatal consequences of confounding the putrid sore throat with other diseases in that part; and as soon as the white speck was found to characterize the new complaint, many seemed to forget that before its existence any white appearance had ever been perceived about the throat. By degrees it has been found necessary to trust no longer to the sight, the most fallacious of the senses; still less to the descriptions of others, always unsatisfactory, and at best depending on the accuracy of the spectator, both in observing and relating, as well as on the impression of mind under which his observations are made. It has been found necessary that our descriptions should be so minute as, by showing precisely what appearances were present, to show what were not; and to connect with all this a most exact detail of every concomitant symptom, their series and order.

C H A P. IV.

OF THE FIRST LOCAL ACTIONS IN-
DUCED BY MORBID POISONS.

LET us now keep the cafes above related in view; and if, at the fame time, we confider the natural hiftory of an ulcer, we fhall be able to form a juft idea of phagedæna, and the other appearances we have enumerated in morbid poifons.

WHEN a divifion of any folid part on the furface of the body takes place, the moft fimple operation of nature is to form a kind of fcab over it. By this means the whole is defended from the external air; the blood or other interftitial fluid is abforbed, and the fides unite by contracting clofer to each other, or by the appofition of new formed fubftance; after which, the fkin recovering its fmoothnefs, the fcab falls off. But the injury may be of fuch a nature, or the parts may be kept fo long divided, as that the edges muft die for want of a due circulation. The neceffary confequence of this is, that before any other procefs can com-

mence,

hence, the dead part must be separated, which can only be effected by the sound part in contact with it being absorbed. This process, attended with purulent discharge, is called ulceration; and in this stage of a wound nothing can be more true than that "a dissolution, or rather diffusion, 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 the discharge." This will appear more obvious if we take a view of the consequence of applying a caustic. Observing the separation of the dead from the living part, and pus between them, we are apt to consider this pus as a solution of that part of the solid substance which occupied the space of separation. But nothing can be more inconclusive than such reasoning. For if the dead part is dissolved, why does this process only take place at the edges? If the living, it must be the effect of a peculiar organization. But this only increases the difficulty, because the loss of substance is not confined to the living part, but is observable in the slough, which is dead and unorganized. We are therefore forced to admit an organization that will dissolve slough and living cellular membrane blood-vessels, and their contents, into one uniform fluid. But how much more complicated is such a process, than to admit that the lost substance, both of the slough
and

and living part, is absorbed, and that pus is secreted. Absorption, we know, is constantly going on in every part of the system; and the secretion of pus continues after the loss of substance ceases, and granulations begin to rise.

WHEN the dead substance is completely separated, the fore is in a similar situation to the loss of substance from suppuration in cases of abscess; and, if nothing interrupts the healing process, we may expect granulations to rise. But this healthy operation may be interrupted in a variety of ways. The constitution may be engaged in a diseased action; in which case the ulcer may remain stationary, or spread according to the nature of the disease. If, from negligence or mistaken cleanliness, the pus and rudiments of the future granulations are wiped off, the surface of the wound will be again crude, and die, from the causes above-mentioned. Hence absorption and fresh ulceration must commence. If the same treatment is continued, the same effects will follow; till by degrees the ulcerating process may become habitual, and spread some time after the first cause has ceased. Such is usually the origin of those unhappy cases of fore legs, so common among the poor. But sometimes, especially in older subjects, or where the disease is seated at a distance from the source of circulation, an ulcer will continue almost stationary

ary for a great length of time. This is another resource of the œconomy to provide against the effects of injury or disease. In these cases, instead of ulceration, a hard, thickened edge is formed round the wound, which must be absorbed, or removed by art, before any healing process will take place.* The operation of casting off the dead edge in one instance, and absorbing the callosity in the second, are both attended with purulent discharge, and usually called digestion.

EVEN when ulceration has completed its object, and we might expect granulation, and consequent cicatrization, these processes may be interrupted. If an extraneous body is present, which cannot be dislodged, as often happens with a piece of dead nail, or as was described with the issue; instead of true granulations, we shall have a soft, spongy,

* INTERDUM vetustas ulcus occupat induciturque ei calculus, & circum oræ crassæ livent. Post quæ quicquid medicamentorum ingeritur parum proficit, quod fere negligenter curato vulnere supervenit.

CÆLS. lib. 5. cap. 37. sect. 51.

THE last part of this sentence is a very exact and elegant description of the cause of this kind of ulcer. The medical reader will not be deceived by the term *curato*, which, relating only to the dressing, with the addition of *negligenter*, reduces the whole to the cause I have mentioned above.

pallid

pallid fungus, which will not only rise much higher than the surrounding skin, but often grow over it. Whilst this continues, nothing like a cicatrix will be formed, though in some instances a sort of thin caducous skin will cover the fungus.

BUT any one of these interruptions may happen from the presence of a poison, and that with a uniformity which characterizes all the operations of nature, wherever we can detect those laws by which they are governed.

I HAVE already described ulceration as the means of separating a dead from a living part. The same may take place from other irritations. Pressure usually produces it. Suppuration, which is only an internal ulceration, is the means used by nature to bring any substance to the surface. In all these cases, a process takes place to prevent the effusion of the matter, which Mr. Hunter very accurately describes, and which is one species of his adhesive inflammation, forming a thin lamina over the whole surface of the abscess or ulcer. In the case of morbid poison, ulceration seems the attempt to separate the diseased from the sound part. In this, as in all the other cases, the parts are stimulated beyond a state of health, and a degree of inflammation takes place, partly depending on the nature of the stimulus and partly on the peculiarity

culiarity of the constitution. In some cases of external violence, we find the part so peculiarly affected, that without any destruction of vessels, to prevent a free communication, gangrene takes place, and the part is afterwards separated. In some instances the bare inflammation is so considerable that this consequence follows, though the first injury was not in itself sufficient to produce such an effect. The same happens in morbid poisons. Mr. Hunter has remarked, that the variolous pustule* is always attended with a *Slough*, which must be from the peculiarity of the stimulus. The same happens from the yaws without inflammation in proportion to the effect produced. Of this kind too were the cases† described by Turner, by Mr. French, and in the Edinburgh Medical Essays, and also of the West Indian whose gum ulcerated and sloughed from a transplanted tooth. In these cases as soon as slough commenced the first ulceration ceased, and when the slough cast off cicatrization immediately followed. Under such circumstances it is not easy to determine whether the cessation of the disease arises from the constitution being no longer susceptible of the irritation of the poison, or from the slough extending beyond the contaminated part. Certain it is, how-

* Philosophical Transactions, vol. 72.

† See pages 66 and 85.

ever, that where the slough is smaller the cure depends on the susceptibility of the constitution to the poison. In the small pox though the slough is inconsiderable, the cure follows, because the constitution is no longer susceptible of the variolous irritation. The same happens in yaws. But if the constitution remains susceptible of the virus, and the slough be at the same time inconsiderable, we may expect that during its separation the sound part will be irritated by the diseased pus, and that the same consequences will follow as from its first application. Hence, as Celsus observes, we shall find blackness or slough spread without our being aware of it, and alternate sloughing and ulceration will follow each other. This diseased action I shall take the liberty of calling, till nosologists invent a better term, the *Sloughing Phagedæna*, or *Nigrities Serpens* of Celsus.

IN some other morbid poisons which usually produce only vesication or ulceration, a peculiarity of constitution will induce slough. This sometimes, though rarely, occurs in a few early pustules of chicken pox; it more frequently takes place in the first appearance of chancre. In the latter the slough is generally small, and when cast off infects the base from which it is separated. But the law of the poison being to induce ulceration, the disease afterwards spreads by that process,
the

the first sloughing only arising from a peculiarity in the constitution, which was more than usually irritated by the new stimulus.

A MORE common and simple effect of morbid poison is, to produce ulceration, in order to separate the diseased from the sound part. But if the pus secreted in the attempt partakes of the same property that first occasioned the ulceration, the cause still continuing, the effect will continue also, as long as the constitution remains susceptible of the disease, or till the nature of the pus is altered. If this last is within the power of the constitution, she will continue the attempt; and the only doubt will be, whether, before the nature of the pus is altered, so much mischief will be done as the constitution must sink under. It is not, however, always necessary that the ulceration should be continued by the presence of a poison, or by the secreted pus partaking of the property that first occasioned the disease. We shall hereafter find that, like other diseases, it may, when continued for a certain time, be kept up by habit.* In this

* SEE the first paper of the transactions of a society for the improvement of medical and chirurgical knowledge. This paper is written by Dr. Fordyce, and contains many ingenious illustrations of constitutional diseases, kept up by habit, after the cause that produced them has ceased.

case we may expect to see ulceration extending in one direction and a healing process in another. But this will be best illustrated when we come to consider mercury as a remedy.

As long, however, as the constitution retains its susceptibility, and the pus the property of the poison, the irritating cause will be constantly present, and ulceration or absorption, with purulent discharge, will continue without interruption, and with such rapidity often as to prevent the formation of the adhesive lamina, or to absorb it as fast as it is formed. Hence we have the true *Phagedænic Ulcer*.

BUT if the poison is such that the powers of the constitution are unable to resist, or if no healthy action of the vessels can alter the nature of the pus, the attempt at healing being given up, we may expect a *hard thickened edge and base* to be formed similar to what we observed of those ulcers, the healing process of which is suspended from being frequently interrupted.† But if the constitution is not only unequal to the cure of such an ulcer, but remains for ever susceptible of the irritation of its pus, we may expect a hardened edge and base from the first cause and a continuance of ulceration

* See pages 90 and 91.

from the second; hence the edge will be absorbed, and a new one formed. Of this kind are the cancer and the venereal. The slow progress of the former is such that the callous edge is long in forming, and when formed increases in an irregular manner, from ulceration and callosity proceeding at the same time in different parts of the cancer. Hence the ulcer by degrees assumes a rough, irregular appearance. In the venereal the progress is quicker, the callous edge and base are formed with more regularity; for the same reason they are as quickly ulcerated, and new ones formed. But though the progress of ulceration is quicker, and consequently more regular than in the cancer, it is much retarded by the thickened edge and basis which distinguish it from the phagedænic ulcer.

If this distinction of the local actions induced by morbid poisons be admitted, it will resolve itself into the following division;

1. SLOUGH, with consequent fungus and scab,
as in yaws.

2. ———, with suppuration and scab, as in
small-pox.

3. ———, preceded by ulcer, and when
separated followed by immedi-

ate skinning, as in several anomalous poisons.

4. ————— with ulceration, and each in succession, as in the Sloughing Phagedæna.

5. **ULCERATION**, kept up by the irritation of the secreted pus, as in common Phagedæna.

6. —————, with a thickened edge and base, as in the venereal.

SOME of the varieties have already been noticed, and others will occur in their order, when we come to consider the mode of cure.

C H A P. V.

OF THE MODE OF CURE, ORIGINAL
AND REMEDIAL, AND OF THE DIF-
FERENCE BETWEEN PRIMARY AND
SECONDARY LOCAL ACTIONS.

WE have hitherto considered the action induced by the stimulus of morbid poisons only as it contributes to keep up the disease ; and traced, I trust, satisfactorily, the analogy between these and other *stimuli*. We are now to detect the process by which the parts are restored, either by the original powers of the constitution, or when assisted by a remedy.

AND here we shall find a set of laws, which, if not peculiar to these substances, are at least not to be traced with the same uniformity in any other. I need not repeat that what has hitherto been described as the process of digestion in common sores, whether it be the separation of a dead part, or the recommencement of action on an old ulcer with callous edges, is only preparatory to granulation, by which the lost substance is restored. It is well known, that when the granulating process is completed, instead of the whole being covered with a new skin, the surface of the wound is con-

tracted; so that, the surrounding skin is gradually stretched over part of it, by which means we have usually a cicatrix, inconsiderable, when compared with the original ulcer. These processes, with the variations from them, and the causes of those variations, are very minutely described by Mr. Hunter, to whom I must refer all those who wish for information in this most important branch of Physiology. My business is only to trace the deviation from these laws, when the disease arises from the stimulus of a morbid poison. Even in these there at first sight appears a variety; but a closer observation will convince us, that these varieties are reducible to laws, with an uniformity that is truly striking.

WHEN a loss of substance is induced by the stimulus of a morbid poison, whether from slough or ulceration, as soon as the diseased action ceases, instead of granulations rising, to fill up the cavity, we find it first skinned over, and, if the lost substance is restored, it is by a subsequent process under the skin. In cases of common ulcer or abscess, we have found nature, as it were, sparing of skin,* while in the present she seems prodigal of

* I use this word for want of another, to express the common covering, though it is probable the cuticle only is renewed. The term prodigal is fairly applicable to this process.

of it. No sooner does the stimulus of the poison cease, than every cavity is instantly lined, like what is observed of the pustules about the face in the small-pox. Such is the case in chancres, where the ulceration has acquired any depth, and we have traced it in all the anomalous morbid poisons. Turner tells us, that the stump of a penis was all that remained after the cure of the slough, the history of which he relates. The lost substance was never renewed in the case related by Mr. French.* In the sloughing phagedæna described above, no part of the prepuce was ever restored by granulation; but in all these cases the whole skinned over, without that previous process.

It is probable that this skinning, which seems to consist of nothing more than the cuticle, is derived from the surrounding fibres or vessels of that membrane, which I have before observed does not appear susceptible of the influence of morbid poisons; and if so, it is certainly a most important

process. In the small-pox it is renewed in such profusion, as to scale and even sometimes peel off more than once before the surface recovers its original smoothness.

* For this piece of information I am indebted to Mr. Simpson, of New North-street, successor to Mr. French, and his assistant at the time the case occurred.

provision of nature in the cure of ulcers or abscesses, arising from the stimulus of those substances. This will appear the more obviously when we reflect, that as often as the granulating process is attempted on these surfaces, it is invariably unsuccessful. As we shall hereafter have occasion to enlarge on this subject, I shall at present only remark, that in the primary venereal ulcer or chancre, if nothing interrupts the healing process after the action from the virus has ceased, and the surface is become clean and florid, the whole is skinned over with a rapidity greatly exceeding that of a common sore of equal magnitude. Such is the process on the glans. In the prepuce the sides of the ulcer contract together so quickly and firmly as often to impede the future denudation of the glans. But if this process is interrupted, by a sloughiness remaining about the sore, or by the application of such dressings as prevent the cuticle from extending over the glans, or contracting in the prepuce, an attempt at granulating will commence, but the granulations will be always some way so ill-conditioned as to render cicatrization extremely tedious.

It may be urged that in the genitals, this peculiarity arises from the structure of the parts. But in the case recorded in the Edinburgh Medical Essays, for which an amputation of the penis was performed,

performed, probably above where the influence of the poison had extended, granulations rose so rapidly that the surgeon treated them as fungus till the pain his escharotic applications produced obliged him to desist. After which, the glans was in a certain degree "regenerated."* Nor is the fact itself confined to the genitals. The women whose cases are described with accuracy as suffering by phagedænic ulcers from morbid poisons applied to the papillæ experienced the same fate. In most of those related by Mr. Hunter the whole or part of the nipple was lost. I have observed the same of the woman in St. Bartholomew's; a considerable part of whose breast was healed without granulation, and the same may be remarked in some of the tooth cases.

STRIKING as this uniformity may appear in all primary affections from morbid poisons, where loss of substance is induced, it is not less remarkable than the regularity with which granulation takes place in all their secondary or constitutional ulcers. This is always the case in the venereal. It was the case in the sloughing phagedæna; in the case related by Mr. French, and may be easily traced in all the morbid poisons of whose second-

* The history of a case contained in Appendix, No. I. will further illustrate this.

ry symptoms we have any correct history. As this difference must convince us that the poison undergoes an alteration in the circulation, it might have led us to doubt whether the pus secreted in the secondary ulcers was infectious like that of the primary ones. Mr. Hunter has reduced this to experiment in the venereal, and shown that the poison in the course of the circulation loses its infectious property. Leaving this to rest on the authority of those who witnessed the fact, * as nothing of the kind has been attempted in the anomalous poisons, I can only remark those differences between their primary and secondary symptoms which might lead us to form a similar conclusion.

THE most obvious is the different appearance and progress of the symptoms. Where the law of the poison is to induce slough on the part to which it is first applied, we find nothing but ulceration follow the secondary or constitutional symptoms from absorption, and where the primary symptoms have been phagedæna, the secondary have usually been blotches slowly ulcerating if at all. The small-pox is an exception to this rule, producing slough not only in the part inoculated but in every consequent pustule, and here we find the primary

* See Appendix, No. II.

and

and constitutional ulcers or abscesses equally infectious. In the chancre we remarked that notwithstanding the callous edge and basis the ulcerating process is still continued by the pus retaining its virulent or irritating quality. If, therefore, the pus of the secondary ulcers be no longer infectious, we might expect ulceration would be proportionably slower, and such we actually find it. So slow indeed, that instead of an open ulcer, we have for a very considerable time only a discoloration of the skin under the cuticle, and when that at last breaks, so little is the loss of substance and so sparingly is the pus secreted that the whole usually hardens into a kind of scab, and this often continues for a length of time that would be sufficient for the destruction of the glans penis by a chancre. If it be admitted too, that after the venereal has been cured in the skin it never returns to that part from the same stock of infection, this is at least a presumptive argument that matter from these pustules is not infectious; because, as it is well known that the constitution remains for ever susceptible of the venereal poison, the absorption of this pus might sometimes reproduce the disease. And this is further illustrated by what we see actually take place in some other poisons, whose secondary ulcers, from spreading with greater rapidity, may be supposed to secrete infectious pus. In some of these we see the disease a second time in the skin,
though

though under a different appearance, that is, with blotches that continue longer without ulceration, and heal more readily, either spontaneously or by the exhibition of a remedy, than the secondary ulcers did. Mr. Hunter mentions two instances of this kind.* The sloughing Phagedæna (of page 70) was of this kind, and a brief recapitulation of the history of that case may serve to illustrate all the various appearances and progress of many of these poisons.

THE disease seems to have been inoculated first, by continuing an habitual connection after the skin of the prepuce was broken. The consequence was an ulcer, always exasperated by mercury, and which when left to itself spread by a succession of sloughs. It stopt spontaneously when it had extended over all the part similarly structured and organized with that on which it began, viz. the prepuce. This is analagous to what has been often observed of the glans penis and papillæ.—It healed without granulations.

THE secondary symptoms were blotches, which very soon became ulcers, purely phagedænic, or spreading entirely as such. In this they differed from the first, no less than in their yielding very

* Treatise on the Venereal, pages 389 and 393.

early to mercury, and healing by granulation. When this remedy was resumed, on account of some of the ulcers throwing up spongy granulations, all the remaining symptoms soon disappeared; but it was thought necessary, to push the mercury further, in order to eradicate the latent virus. The salivation was raised so high, that the patient spat near three pints in twenty-four hours, and the smell from him was so intolerably offensive to himself and all about him that it was necessary to change the room several times a-day. All the other symptoms of mercurial irritation were in proportion.

BUT soon after he had recovered from this severe discipline, blotches appeared on his face, which had hitherto escaped the disease. They remained stationary much longer than the former ones, and gave way very early to mercury without ever ulcerating.

HERE were three stages of the disease. The first, on the part infected, which resisted the use of mercury, and might be said to heal spontaneously, and *without granulation*.

THE second were blotches, soon becoming phagedænic ulcers, which, though not prevented by a full course of mercury unsuccessfully given to cure the first; yielded after their appearance, very early to that remedy, and healed *by granulation*.

THE

THE third,—blotches, which, though not prevented by a still more violent course, yet after they once appeared, readily gave way to a short exhibition of mercury.*

FROM which it follows, First, that a disease arising from a morbid poison, may, in three stages, show itself in three different forms.

SECONDLY, That a remedy useless in the first form, may cure the second, and also the third.

THIRDLY, That as far as facts will direct us to determine, no quantity of the remedy which easily cures the disease in these two forms will prevent its appearing in them.

BUT except in the venereal, which we shall hereafter consider by itself, we have not a sufficient number of facts to ground any other reasoning upon, than, that all primary ulcers from morbid poisons, heal without granulation, and where the secondary ulcers are different in their appearance, they heal by granulation. Let us now trace the

* THAT this account may be as little incumbered as possible, I omit the bones, the progress of which cannot be traced with the same accuracy. The most remarkable circumstance attending them is, that the first swelling yielded to very little mercury. That a complete exhibition of it did not prevent a return of the swelling, and lastly, when returned, it was soon relieved by the same remedy.

progress

progress of those poisons, whose ulcers or pustules are similar in both stages.

OF this kind is the small-pox, and probably the yaws, if that disease should ever be inoculated. Till this is the case, we must confine our remarks to the small-pox. The pustules of this disease are equally infectious, whether primary or secondary. They are all of them distinguished by a slough, and no way differ from each other, but by a higher degree of inflammation, which though constantly observable in the primary pustules, is not entirely confined to them. On this single circumstance of the degree of inflammation depends the consequent pitting. It is well known, that all inflammations, whether they terminate in suppuration or slough, run through these stages as well as granulate and cicatrize, with a rapidity in proportion to the violence of the first symptoms. With the same regularity in this disease, if the inflammation is considerable, all the stages of sloughing, separation of the slough and skinning are quickly run through. Hence the law that distinguishes morbid poisons is preserved, the part is skinned over without previous granulation, and the concavity or pitting remains. But where the inflammation is slight, the slough not being cast off so early, the healing process, which in this instance would be skinning, cannot commence.

The

The consequence is, that the pustule approaches nearer to a common ulcer, and from the presence of the slough, as an extraneous substance, a fungus rises up, which with the remaining pus, slough and cuticle, hardens into a scab. If this rests long enough, the influence of the poison will so entirely cease, before the healing process is completed, that the lost substance will be renewed, as after any other abscess. Hence it is, that in the casual small-pox, we have usually pits only in the face, or where the inflammation is most considerable. In the inoculated, we have invariably a cicatrix in the arm, and if the inflammation is considerable in that part, rarely any pits in the face, and never, excepting in pustules, which show high inflammation. It is not a little curious to trace the gradations of these appearances. Where the disease has been confluent we observe seams. Where it has been distinct and the inflammation rapid, we see the corrugation of the pits exactly resembling the shrivelling which the slough assumes, as it begins to be separated round its circumference. If the separation on the side has preceded that at the centre long enough to produce a beginning fungus from the presence of the slough, as an extraneous body, and at this period, the centre should be suddenly detached, either by force or the rapid progress of all the changes, the consequence is, that the whole is skinned over, and the lateral fungus is either absorbed,

absorbed, while healthy granulations supply its place ; or is rendered firm by granulations filling up its cellular interstices. All this is performed under the scab, and when completed, we find a small smooth pit on the spot where a large pustule matured. This is the species of pustules, remarked by Sydenham of the regular small-pox, which appeared for six months of a period he traces with his usual exactness. “The pustules of the face are succeeded,” says he, “by a scurf or branny scales, and those sometimes by pits or pock marks; for when the pustules fall off, the skin looks smooth; but these scales coming on and falling off alternately do at length make those pits that frequently continue visible long after the recovery of the patient in this disease, though the distinct small-pox very rarely leaves any marks behind it.”* This phænomenon, so accurately traced by Sydenham, arises from the bottom of the foveolus being skinned over, while its surface is crude, and while the fungus of the sides is covered only by the thin skin mentioned in page 92.† The consequence of this interruption to skinning the sides seems a perpetual attempt at accomplishing it, which produces the succession of branny scales, probably cuticular processes, that could not pass unnoticed by one who suffered nothing to escape him. It very commonly occurs after inoculation, where the erup-

* Swan's Sydenham, page 98.

† See also note in pages 100 and 101.

tion has been considerable, so that I have frequently been able to distinguish by the pits, especially if numerous, the manner in which an individual had the small-pox.

ON the single circumstance of the higher degree of inflammation, attending parts first irritated by the novelty of the stimulus, seem to depend all the advantages of inoculation. We have already seen in the chicken-pox and venereal, the early symptoms will sometimes be slough, though contrary to the law of the poison. But no sooner have the constitution or parts recovered from the first shock, than the true character of the poison discovers itself. The character of the small-pox is slough, with circumscribed pustules, or as Mr. Hunter would call it, adhesive inflammation. But sometimes so violent is the inflammation, consequent on the first shock, especially in irritable habits, that it degenerates into the erysipelatous, and spreads like it. The disease, however, soon recovers its character, the subsequent pustules are properly circumscribed, and those in the face, which at first showed a disposition to spread, remain stationary. But the mischief is not over, innumerable sloughs are to be separated, which cannot be done without fresh inflammation, and though the disposition to spread has subsided, the parts have not recovered that perfectly

fectly healthy action that distinguishes the true ad-
hesive inflammation. Hence it is observed in
confluent cases, the pustules of the face do not
rise with the convex figure which marks that com-
plete circumscription, so distinguishable in other
parts of the body. On the number and condition
of those on the face depends the violence of the
secondary fever.* This symptomatic fever runs
proportionably high, therefore, according to the
number and condition of the early pustules.

HENCE it appears, that if we could introduce
the poison in such a manner, that the first action it
produces shall be confined to a smaller spot, we
should have little to apprehend from the subsequent

* See Sydenham on the small-pox. The reader will per-
ceive, that the respect I bear to such names as Sydenham
and Hunter have induced me to use the word *secondary* in
this chapter in two different senses. Every one knows
that Sydenham uses it to express the fever at the turn of the
small-pox.—Hunter, to distinguish the local ulcer or pustule
on the part to which a morbid poison is first applied, which
he calls *primary*, from those which are the effect of absorption,
which he calls *secondary* or *constitutional ulcers*. As in the
natural small-pox (the only one Sydenham was acquainted
with) we can have no distinction of primary and secondary,
but only early and subsequent pustules, and in the inocu-
lated we have usually no secondary fever, there can be no
danger of confounding the terms; and perhaps this apology
was unnecessary.

stimulus. And such is actually the case. For though the inoculated part frequently shows every symptom of confluence, yet being small compared with the face on which the casual first fixes itself, the subsequent or secondary fever is hardly observable. If this reasoning be admitted, it will not only show why the disease is so generally milder, but account for the occasional deviations from that rule; especially as Baron Dimsdale and others have remarked, that in proportion as the inflammation in the arm is considerable, the subsequent symptoms are generally milder.

THIS reasoning may be extended to the foetus. The superior danger attending pustules in the face, as remarked by Sydenham, and as comes within every one's observation, can only arise from the higher inflammation attending them as early pustules; and this danger in inoculation is lessened by the number of early, or, as they may here be called, primary pustules being lessened. Whatever the reason of the disease in the casual way appearing first in the face is, I pretend not to determine; but it is probably some peculiarity which cannot exist in the foetal state. Accordingly we find, in all the accounts we have of children born with the small-pox on them, that the maturation has been equally forward in every part of the body. Mr. Hunter observes of the one he examined, that the pustules, though

though numerous, were more generally distinct in the face than any other part. Dr. Pearson tells me the state of maturation appeared similar in every part of the fœtus he examined. Such was the state of fœtus in the Small-pox Hospital. By which it appears, that, all the pustules being coeval, the shock of primary or early ones is extended over the whole body: and this circumstance may with propriety be added to those remarked by Dr. Pearson * as accounting for the general fatality attending the disease at that age. This is confirmed by Mr. Lynn's valuable case.† The child lived five days, during which time it was free from all those inconveniences described by Dr. Pearson as attendant on the fœtal state. Notwithstanding which the secondary fever proved fatal. Though the pustules were all distinct, they were probably attended with higher inflammation from the cause above assigned; and though dispersed over the whole body, from being all of them early pustules, they were likely to produce similar consequences as if the same number had fixed on the face.

THERE appears a remarkable similarity between small-pox and the yaws; which, excepting the specific nature of the respective poisons, seem to differ only in the slow progress of the latter. The constitution is susceptible of each only once dur-

* In a paper read before the Royal Society. † Ibid.

ing life. Each begins with a pimple. The law of each is to induce slough, and each has its period and decline, the powers of the constitution being in each equal to their cure. The small-pox, from the inflammation being considerable, differs from yaws in throwing out lymph, which from the absorption or evaporation of the thinner particles, thickens into a fluid resembling pus.* But when the inflammation is very considerable, the pimples are often unattended with any fluid, exhibiting a horny appearance, and going off with a scab like the yaws, without showing any signs of suppuration or leaving any pits. In the yaws the inflammation is scarcely perceivable, and the process proportionably slow, the cuticle is, therefore, separated as soon as the progress of the disease has arrived at the stage of sloughing. After this the fungus shoots, and by degrees hardens into a scab, under which the separation of the sordes or slough and consequent granulation take place.

WHENEVER a part which was the seat of a poison throws up this fungus, the attempt of separating

* THIS is further assisted by the pus secreted in the separation of the slough, and to this mixture is probably to be attributed the well-known circumstance, that pus taken late from a pustule is less irritating or certain to succeed in inoculation than when in a cruder state. To this also may we not ascribe some anomalous appearance which sometimes follow inoculation for the small-pox?

the

the diseased from the sound part by ulceration has ceased, and the parts are prevented from skinning by a slough acting as an extraneous body, or by the sore still retaining a morbid disposition. The yaws is so evident an instance of the first, that when all the sores are fungated, the disease is understood to have completed its progress. The same, by close attention, may be observed of the small-pox, and we have traced it in the secondary ulcers of a sloughing phagedæna. What the state of sivvens is when the fungus shoots we have yet to learn—that of cancer will be considered in its order.

IN the small-pox then, and probably all other morbid poisons, whose primary and secondary local actions are similar, and the law of which is to induce slough; the progress of both stages is

FIRST, sloughing and ulceration, in order to separate the slough.

SECONDLY, skinning over the part that has sustained the loss of substance, or if this is prevented by some of the slough remaining or any other impediment,

THIRDLY, to generate a fungus which is for the most part a prelude to healthy granulations.

HITHERTO we have only considered the laws by which the parts injured are healed or restored, our next business is to trace the operation of remedies, where the original powers of the constitution are unequal to the cure, or where the progress of the disease may be shortened. Let us attend to the actions induced by

MERCURY.

IN warm climates or in the warm air of a hospital we see the effects of this mineral most obviously. If exhibited to a constitution while there is a crude wound we find it acting as a poison, that is, producing ulceration beyond what is necessary for dislodging the dead edges of a cut. If discontinued as soon as this effect is perceived, as the ulceration cannot, like those from *morbid* poisons, produce a pus irritating like the substance which first occasioned it; the ulceration will cease, and the healing process commence. If mercury is persevered in, the constitution will be proportionally long before it recovers from its irritation, and there will not only be danger of the ulceration continuing from the present irritation, but of its being kept up by habit after the mercurial irritation is over.

IN

IN this instance we have supposed mercury exhibited before any healing action has taken place on the wound, that is, where no suppuration has commenced, nor of course any adhesive inflammation to prevent the effusion of pus. The operation of the poison then being unrestrained will be equally sudden and certain, but temporary if managed with address. We may therefore in some instances expect, where a morbid poison keeps up phagedæna by the pus producing a similar irritation to the one which first caused the ulcer, that by inducing the mercurial irritation we may supersede the former by a phagedæna which will not be permanent. In this case the only difficulty will be to determine when we have superseded the first, because by continuing the latter too long we run the danger of producing a second and often more dangerous disease, inasmuch as we have no longer the benefit of our remedy. But even this danger may for the most part by accurate attention be prevented. For certain it is, and for reasons which will presently appear, that in most instances before the phagedænic action from mercury is extended to the sound part we find the suspension of the action of the first poison produces a healing disposition. This is more obvious in the venereal, whose callous edge and basis we see disappear as the mercury produces its new action, and often cicatrization or rather skinning instantly follow.

WHEN funguses arise where a morbid poison has produced a loss of substance by ulceration, as in syvens, or by slough, as in yaws, we may ascertain that the poison no longer excites its specific action, but that the surface remains diseased. In this case mercury seems of all others the most obvious remedy. By its property of inducing phagedæna we may expect the surface of the former ulcer, which is now the basis of the fungus, to be absorbed, after which there will be nothing to prevent the healing process commencing. This being the only object, we need not excite that irritation which is necessary in the venereal, where the influence of a poison induces ulceration, callosity, and probably disease, beyond the surface of the ulcer. Instead, therefore, of mercury in substance which produces a slower but more lasting effect, some of the mercurial salts which act more suddenly and less permanently, may best answer our purpose. In this way it may be safer to treat all phagedænic ulcers without a callous edge and basis, whether arising from the immediate action of a morbid poison, or from habit after the influence of the poison has ceased. It may happen indeed, that the poison may be of such a nature as that the constitution shall be more susceptible of its influence than of the mercurial. If so, the action of the first may not only remain, but be much increased from the known property of mercury to induce absorption,

forption. But where the contrary is the case, a cure may be always expected from mercury inducing a new action, which, however, should be done with caution, where a disposition to phagedæna exists. It will, therefore, be better to discontinue the remedy as soon as we perceive the first symptoms of mercurial irritation, and after a time to return to it if necessary. In this manner we may expect by degrees to supersede the action of a poison, the influence of which, from the rapidity with which ulceration spreads, is not likely to extend much beyond the edges of the ulcer, or if the disease is only kept up by habit, we may by often exciting a new stimulus, without extending it too far, frequently interrupt and gradually destroy the habit.

EVEN if the poison be such as the constitution is more susceptible of than of the mercurial, the novelty of the latter may excite at first an action contrary to that which has been produced by a stimulus, to which the part has long been familiarized. But here, as in the former case, we should be careful not to push our remedy too far, because as soon as the stimulus of mercury ceases to be new, we shall lose all the advantages of it. We should, therefore, be attentive to stop as soon as we are convinced by any change in the ulcer, that the mercury has begun to excite a fresh action, and

not

not return to the remedy till we conceive it likely again to stimulate from the same cause. It is from the necessity of treating some morbid poisons in this manner that I suspect some practitioners have been induced to suppose the venereal should be cured in a similar way, which in cases of inveterate chancre is known to be unequal to the intended end.

IN this manner of accounting for the cure of the ulcers of morbid poisons, one difficulty will occur to every reader. If the cure is in consequence of phagedæna induced by mercury, why does it ever happen that a venereal ulcer shall skin before the mercurial irritation has ceased? I answer, if a chancre is at all inveterate, particularly if seated on the prepuce, it rarely does heal till the constitution begins to recover from the mercurial irritation. In more recent chancres, especially when on the glans, we find the cure complete while the mercurial irritation remains. But it is obvious that the action of mercury will be more powerful on a part previously ulcerated than on the healthy original formed part. As, therefore, in the cure of ulcers from morbid poisons, skinning instantly takes place without the previous process of granulation, we may expect that, in recent cases on a part so sanguiferous, and where the actions are so quick, the irritation of mercury may have extended as far as
the

the ulcer before it has arrived at such a height as to affect the sound substance; and cicatrization, or skinning having taken place, we have no proof that the action of mercury is sufficient to produce a new ulcer. That this is really the case appears the more probable, because in chancres of a longer date, where the disease may be supposed to have extended its local influence much beyond the immediate surface of the ulcer, we do not find this disposition to early skinning. The parts contiguous being infected keep the ulcer open till the mercurial phagedæna reaches them, by which the whole constitution is so saturated with mercury that though the ulcer looks clean, being free from its callous edge and basis, yet it remains stationary, or even spreads till the constitution begins to recover from the mercurial irritation.

By attending to all these facts we may learn,

FIRST, Why mercury will be often serviceable in ulcers that do not arise from morbid poisons.

SECONDLY, Why less will cure an ulcer arising from a morbid poison, without a callous edge and basis, than where these are present.

THIRDLY, Why it is frequently unsuccessful in ulcers from morbid poisons attended with slough,

AND

AND lastly, Why a later application of mercury will cure an ulcer from a morbid poison, which at first resisted that remedy.

IF the property of mercury in curing local diseases, arising from morbid poisons, depends on the actions it produces on the extreme vessels, where there is a division of the solid part of the surface, it ought to prove efficacious in local diseases, or ulcers arising from other causes: and so we actually find it. But we must not expect the relief to be so uniform; because where an ulcer does not heal nor arise from a poison, we may suspect some constitutional peculiarity, which cannot be so readily subdued, and which there will be reason to apprehend may recur after the irritation from mercury has ceased. In cases of morbid poisons, we have less to apprehend from this cause; especially, as the parts in general, as soon as ulceration has ceased, heal rapidly and without the process of granulation.

IF this reasoning be admitted, we shall see also why for the most part less mercury will often cure an ulcer, arising from a morbid poison, which has no callous edge, and also why no quantity of mercury will ever cure a cancer. In the former we have every reason to believe, that the powers of the constitution are equal to the cure of the ulcer. All, therefore,

therefore, that is required of mercury is to interrupt the action excited by the poison. But where a callous edge is present, we must apprehend that the cure is not within the power of the constitution.* It is, therefore, not enough to excite a new action, but every part engaged in or disposed to the former action must be destroyed. Add to this, that where ulceration proceeds with such rapidity, as in common phagedæna, it is probable that every part is ulcerated as soon as affected by the poison, so that if the mercurial irritation can supersede it at all, we may expect it to do so as soon as its action begins. But in cancer, the interruption to the ulcerating process is still greater than in the inveterate venereal, the callosity being more extensive. From the irregularity of this callosity too we may expect the phagedæna produced by mercury will spread in an irregular manner, as it will meet with the least interruption where the callus is thinnest, or where ulceration is previously going on. The only state of cancer, in which we can expect mercury to be efficacious, is after an operation; when we find unhealthy granulations rise, which show that some parts contaminated have not been removed. In this case the mercurial phage-

* THIS is certainly not a necessary consequence, because, as was before observed, a callous edge may be formed from the constitution not being at the time disposed to heal. But in cancer and the venereal the fact is established.

dæna might be serviceable, if we could ascertain that the cancerous would yield to it: but when we consider the slow progress of cancer, it is impossible to determine how far the disposition to the local disease has extended beyond where we can trace the disease itself. And if we could, it would not be safe to extend the mercurial phagedæna much beyond the surface; lest we should produce an habitual ulceration. As, therefore, nothing would be gained in a disease over which the constitution has no power of itself, while any part diseased or disposed to be so remains, it would certainly be safer to remove the whole by the knife than attempt it by a mercurial ulceration. It is also well known, that all stimulating remedies greatly encrease the pain, and forward the progress of cancer. There is, therefore, reason to fear, that notwithstanding the usually slow progress of the disease, such a remedy might greatly forward and exasperate all the symptoms.—But this subject will be considered more at large hereafter.

WE may also, by thus tracing the progress of mercury, perceive why it never succeeds if administered early for the cure of those morbid poisons, whose law is to produce slough after ulceration. In all these cases the disposition to slough extends so much beyond the ulcer, that the mercurial phagedæna cannot, without great danger, reach the full

full extent of the disease. Yet in some instances of this kind, mercury seems for a time to alter the character of the poison. In the case related by Mr. French,* mercury produced its genuine phagedæna on the ulcer, which continued as long as the stimulus of that mineral remained. When this ceased, the poison recovered its true character; a large sloughing took place and was succeeded by skinning. The same happened with the sloughing phagedæna (of page 70). While the mercurial irritation remained, it produced its true phagedæna, though somewhat more rapid than usual. When the constitution recovered from that irritation, the character of the poison showed itself, and alternate sloughing and separation followed. In such cases we can expect no advantage from mercury, unless where the disease is kept up by habit. When arrived at this stage, a very slight exhibition of mercury may be sufficient to induce a new action, which may be followed by a permanent cure. And this is the more probable, as the cure of the disease does not appear beyond the power of the constitution.

ANOTHER remark deduceable from this is, that, as the character of these poisons is so much changed in the course of the circulation, that their se-

* Hunter's Essay, page 385.

condary

condary ulcers are unattended with slough, we are not to be discouraged from attempting the cure of the latter by mercury, though it has failed in the primary.

THERE still remain two other observations highly deserving our attention.

FIRST, that less mercury will cure the secondary than the primary affections induced by morbid poisons.

SECONDLY, that mercury will not prevent the secondary actions of those morbid poisons, which it will cure when they do appear.

THE first proposition has been proved in every instance of morbid poison, to which I have referred the reader, where the disease has shown itself in a secondary form ; and even where it has appeared a third time on the skin, it has been more readily cured than in its secondary stage. But the two following cases, in which mercury was not exhibited till after the appearance of secondary symptoms, show the fact still more clearly.

IN the tooth-case related by Sir William Watson,* the attentive reader will be struck with the

* Medical Transactions, vol. 3. page 325.

differ-

difference between the primary and secondary or constitutional ulcers. The first, as I before observed, seem, by their *fætor*, to have been of the nature of the sloughing phagedæna, and appeared about a month after inserting the tooth; the second, or blotches, more than a month later, and became ulcerated painful sores, “increasing daily.” But what is most to our present purpose is, how much more readily the secondary ulcers yielded to mercury than the primary ones. During the exhibition of one or two pills a-day, containing two grains of calomel in each, and before fourteen of them were taken, which produced no other apparent mercurial irritation, than upon the bowels, “the ulceration of the mouth and cheek did not spread, was less painful, and of a milder appearance; while such of the blotches as had ulcerated, healed apace.”* If we suppose that the poison had produced its full effect at the primary ulceration, and that the diseased action was only kept up by habit; this case will be very similar to the instance I have related of sloughing phagedæna. Whether subsequent blotches more indolent than the secondary ones would have appeared as a third form of the disease,

* By this difference of expression too, there is reason to believe, that the secondary were filling up by granulation, while in the primary the ulceration only ceased previous to skinning.

cannot be ascertained, on account of the premature death of the patient.

THE tooth case, related by Dr. Lettsom, in the Memoirs of the Medical Society,* was evidently the sloughing phagedæna. The primary symptoms are so accurately described, as to need no comment. They commenced about six weeks after inserting the tooth. Dr. Lettsom saw them fourteen days after, when “the ulceration had “an irregular, jagged, loose appearance, with livid sphacelated interstices.” For the succeeding week, the ulceration continues nearly stationary, and no mention is made of sphacelus. The two following days the ulcerations have not increased, but rather diminished. Four days after, the ulceration is more extended, jagged, and sphacelated. In four days more, the complaints are not augmented, but if any alteration is perceptible, it is on the favorable side. Two days before this, a mercurial friction was begun; whether the last alteration arose from this cause cannot be ascertained. If it is thought too early for that remedy to have taken effect without any apparent mercurial irritation, it is an additional recurrence of alternate sloughing and ulceration which marks this poison. If this last change is to be imputed to the mercurial friction, it is unnecessary to add, that it was earlier

* Vol 1, page 330.

than

than is ever experienced in chancres of that date and extent. In the course of ten days more, all the symptoms had nearly subsided, and two days after, the remedy is laid aside: the ulcerations being perfectly healed, and every other symptom of indisposition vanished, and this, as far as the account informs us, without ptyalism, or any other mercurial irritation having been excited. The secondary symptoms occurred between a fortnight and three weeks after the primary ones. They were blotches which never ulcerated, and disappeared some days before the primary symptoms.

IN these two cases mercury was not exhibited till the appearance of the secondary symptoms. I have, therefore, reserved them as a more striking proof, that secondary symptoms yield earlier to mercury than primary ones, which may fairly be accounted for by the disease constantly recurring in a milder form.

THE second proposition; that mercury will not prevent those secondary symptoms which it will cure, is so entirely Mr. Hunter's discovery, that I shall reserve the proofs of it till we come to review his theory of the venereal. At present I can only direct the reader's recollection to those cases in which the fact has occurred.*

* See page 108. Also the cases referred to in the note of page 106.

IT appears then, that mercury is a remedy we are justifiable in trying in all cases of ulceration, that resist common topical applications and restorative remedies, particularly if unattended with slough.

THAT where ulceration is unattended with a callous edge and base, mercury should be exhibited with greater caution, and the mercurial salts for the most part preferred.

THAT the secondary ulcers of some morbid poisons yield to less mercury than their primary ones.

THAT in some instances, where mercury has been freely exhibited before the appearance of secondary ulcers, it has not prevented them. Yet in these same cases, when secondary ulcers have appeared, they have yielded to a much slighter mercurial irritation than was ineffectually raised to prevent them.

THAT blotches or ulcers, which appear after the cure of secondary ulcers, seem in the manner in which they yield to mercury, to bear the same analogy to secondary ulcers, as secondary ones do to primary.

AND

AND lastly, that if a primary ulcer, whether of the sloughing or true phagedæna, should at first refuse to yield to mercury, we may be justifiable in attempting it a second time with great caution, either when we conceive the disease kept up by habit, or so far familiarized to the constitution, that the novelty of the mercurial stimulus may be sufficient to excite a new action, however temporary.

HAVING thus far treated of mercury as a remedy, let us now attend to the diseases it produces. We have already seen, that on a crude cut it will induce phagedæna. This is often the more evident in cases of bubo, particularly when opened by art. Here the edges being crude, or not contaminated with the venereal poison, mercury will frequently produce so sudden a disposition to phagedæna, that an experienced practitioner will direct the exhibition of it to cease as soon as the lips look particularly clean and florid. If this is not done, phagedæna soon spreads throughout the whole ulcer, if the venereal action has been superseded.* But if the bubo was not originally venereal, which is much oftener the case than we apprehend, the

* When it was more the custom to open buboes by the knife, this effect was better known: some of the older surgeons used to advise, that the mercurial friction should not be begun till the lips of the wound had commenced suppuration.

consequences will be much more serious and lasting. We shall have a mercurial ulcer seated deep in the cellular membrane, which, on account of the rapidity with which it spreads, not being provided with the adhesive lamina of common abscesses or ulcers, nor with the callous edge of the venereal, will extend in all directions. If the surgeon, from observing the ill condition of the wound, should think it necessary to push the mercury with proportional activity, in order *to eradicate the venereal poison, or to conquer its peculiar acrimony*, both he and the patient will, in the end, find cause of repentance. If observing the spreading of the wound in spite of his diligence, he should be willing to intermit the mercury, and return to it when the constitution shall be more susceptible of its irritation: he will, perhaps, find some advantage by this respite, provided the former exhibition has not been continued so long as to render the disease habitual. But if on the appearance of amendment and better health, it should be thought necessary to return to mercury, in order for ever to extirpate the virus, the disease will be again exasperated in proportion as the mineral is exhibited. When the remedy is again laid aside, the wound will in different parts assume different appearances. In some we shall have the thick lips consequent on frequent interruptions of the granulating process, in others, a clean edge from
the

the continuance of phagedæna. In the lower surface the same difference will be observable, and different sinuses forming from the deep seat of the original ulceration, some parts will be undermined in such a manner, as to sphacelate from being deprived of connecting vessels. When this dead part casts off, a clean surface will be exposed, and the surgeon who is unaccustomed to such appearances, will fancy the ulcer going to take on a healing disposition.

IN the course of all this the patient, particularly if of the inferior class, grows impatient, and applies to another practitioner. If to one who judges by the eye, and conceives it unnecessary to trace the long history of a disease *so obvious*, or who has made up his mind that every venereal patient is a liar, and that it is in vain to attend to their stories, the remedy is again brought forward. At first all the advantages that were expected seem at hand. Phagedæna attacks the thick indolent lips, which appearing clean, florid and ulcerating, give the surgeon the idea of digestion and beginning granulations. The patient, however, being more accustomed to this appearance, and being at the same time sensible of most intolerable pain, ventures to suggest a doubt, but this it is not thought necessary to attend to; and the whole process of salivation being gone through, the disease is some-

thing worse than before. This subject might be pursued much farther if it were necessary; but it is more to our purpose to find a remedy.

THERE is only one situation of such an ulcer in which mercury can be serviceable. When the lips are pretty universally thick, very little appearance of phagedæna can be traced, and the constitution has well recovered from the mercurial course: the local application of mercurial ointment, and the use of the mercurial salts inwardly may induce some action on the surface, and even correct a disease only kept up by habit. It is needless to say this must be done with great caution; but it may always be attempted, and will often be serviceable. The moment ulceration commences, this treatment should be laid aside, and nothing thought of but common topical dressings, with such remedies as will restore the constitution. If mercury has been lately exhibited, or if the smallest quantity is found constantly to exasperate the disease, we should first endeavour to lessen the extreme irritability of the parts by cicuta; and if this, as is often the case, should in a little time cease to produce the beneficial effects that followed its first exhibition, we should try the efficacy of different minerals—most of them have a powerful effect on the extreme vessels. That of antimony is too well known to render any remarks necessary. Lead

is found serviceable in internal hæmorrhage, and iron is known to produce an hæmorrhagic disposition. Arsenic given internally, in such doses as the stomach will bear, produces ulceration in the sound skin. Where phagedæna extends fast and regularly, some advantage may be expected from lead, some of the preparations of which may be given internally with the greatest safety; or where the whole surface has an indolent appearance, the use of steel is not only likely to produce a salutary action on the ulcer, but very much to assist an exhausted constitution. I have in two instances of this kind found considerable benefit from *flores martiales*. Arsenic has hitherto disappointed most practitioners: I cannot myself say I have seen any advantage from its use in external diseases. The fourth volume of the Medical Observations and Enquiries contains a case of this kind, cured by the carrot poultice and malt infusion.—

WHERE these remedies fail, we must attend principally to the constitutional disposition of the patient. If this is peculiarly irritable, we should return to cicuta as often as the ulcer partakes of that disposition. Cicuta, though an uncertain, is an important medicine, and should never be overlooked in cases that resist common remedies, especially if attended with high irritation. It is needless to dwell on the well-known advantages of sea-bathing, particularly in scrofulous constitutions.

WE have hitherto only considered the more obvious effects of mercury; but its influence is extended over every part of the body. What particular action it excites in the bones, it is impossible for us to ascertain, where the parts are not exposed to our view. But we find where a bone or periosteum is thickened by a morbid poison, that under the use of mercury it usually first softens, and then recovers its form. When an open ulcer exists, we see much the same effects as on the soft parts, allowing for the difference of structure. If the constitution is under the influence of a morbid poison, which has its crisis and termination, mercury seems, in some instances, to render the bones peculiarly susceptible of the irritation of the poison. Most of the accounts I have been able to collect of yaws show that if its progress is interrupted by a too early exhibition of mercury, the disease may be for a time diverted from the skin, but that there is great danger of the bones at the same time taking on a morbid disposition. What effect it has on the action induced by other poisons in the bones, has not been hitherto ascertained, excepting in the venereal.

HITHERTO we have considered only the local actions induced by mercury. But its effects on the constitution are not less deserving our attention. The fever it produces may be truly called speci-

specific, from its uniformity and total difference from all others. Hence we find it often superseding diseases kept up by habit, having arisen from causes which are no longer present. Dr. Donald Monro * mentions a case of intermittent fever, which resisted all remedies, till after a mercurial salivation, it was readily cured by bark. It is not uncommon to find obstinate habitual headaches give way to a much slighter exhibition of the same remedy: and many weak constitutions, or such as have long laboured under chronic complaints, have found, after a severe mercurial irritation, all those advantages, which often follow the energy excited during convalescence from acute diseases. These remarks are certainly foreign to our present purpose. I have, however, introduced them because of the error some few practitioners have fallen into, of considering many diseases venereal, merely from their giving way to mercury.

Such is the operation of mercury on the constitution as a remedy. The constitutional diseases it produces are not less remarkable. Besides its well-known determination to the salival glands, which is neither constant nor always necessary, we find it producing head-ache, debility, and a total inca-

* Medical Transactions, vol. 2, page 325.

capacity for application of any kind. For the most part, these gradually subside after the cause is discontinued; but in some constitutions, they end in gutta serena, epilepſy, mania fatuity, and a train of other nervous ſymptoms. Hence it ſeems to follow, that while it remains a doubt, whether mercury will prevent the ſecondary ſymptoms of a morbid poiſon, and as far as our knowledge extends, thoſe ſecondary ſymptoms, when they appear, gave way to a comparatively ſlight exhibition of the remedy, it may be ſafer, till the laws of ſuch poiſons are better underſtood, to reſt when we have relieved the primary ſymptoms; and watching attentively the acceſs of any ſecondary ones, attack them as early as they can be fairly aſcertained. But this will be beſt illuſtrated when we attend to the laws of a poiſon, which, from the frequency with which it appears, are better determined.

C H A P. VI.

PARTICULARS NOT REDUCEABLE UNDER THE FORMER HEADS, WITH CONJECTURES ON THE ORIGIN OF SOME OF THE MORBID POISONS.

IN the preceding chapters, my object has been to detect the mode in which, from the stimulus of a morbid poison, a loss of substance takes place, and the part is afterwards restored. It was my intention to have pursued this subject farther, by tracing the precise character of those poisons, which producing no destruction on the surface beyond the cuticle, do not come within any of the former descriptions. But though I have prepared pretty large materials for this purpose, as they are not necessarily connected with the reasoning in any other part of the work, I am unwilling to swell the volume more than is necessary till its fate shall enable me to judge of the encouragement I may expect. Another reason for withholding these remarks for the present, is a want of opportunity to reduce two important facts to the test of experiment. The first is; the slough in the early pustules of chicken-pox;

pox; the second, whether the lost substance induced by slough, in the putrid sore throat, is ever restored by granulation. The first I have taken on the authority of Mr. Hunter.* But besides my wish to reduce as much as possible to the test of my own experiment; on a re-examination of the passage, Mr. Hunter appears to me rather fearful of denying the fact than ready to assert it. Whether the pitting arises from slough or ulceration, does not at all affect the reasoning I have founded on it; because either of them is equally contrary to the law of the poison; † and confined to the early eruptions. We can, therefore, only impute it to the constitution or part being unusually affected by the novelty of the stimulus. But in tracing the exact history of a disease, so as to discriminate it from others, nothing must be left to doubt, and an opportunity has not occurred, of dissecting the pustules or vesicles, since I have instituted these enquiries.

DR. FOTHERGILL, who may justly be stiled the Sydenham of the putrid sore throat, speaks of the ulcers consequent on the sloughs, as *filling up* on the decline of the other symptoms. My observa-

* Philosophical Transactions, vol. 1xx.

† See a paper by Dr. Heberden, Medical Transactions, vol 1, page 437.

tion would lead me to describe them differently. But conscious of the accuracy of all his other remarks, and of the danger, lest my opinion should be warped by a theory of my own, I wish to suspend this decision till the re-appearance of the disease shall subject it to more impartial observers. I could indeed make some remarks on other passages of the doctor's work, which might seem to favour my opinion, but this would engage me in an enquiry I profess at present to avoid. This, therefore, with some observations on the varieties of scarlatina, and its true discrimination from the former disease, as well as the measles, and its varieties, I shall reserve to correct or confirm by a closer examination: and conclude the present chapter with a few particulars, which though perhaps somewhat too speculative, I crave the reader's indulgence for, after the laborious investigations in which we have both been engaged.

It may be asked, if there is this variety of poisons to be traced in the account of different diseases in the genitals, and above all, if for the most part they yield to mercury, either in their primary or secondary stages, how should the world have been so alarmed at the appearance of the venereal, which at most could be only an addition to those calamities it had always witnessed? I could answer, that the venereal, as far as we can ascertain,

is

is the only poison that produces a disease formidable, by the probable extent of its spreading, and by the situation of all the infected. In the other three, viz. the *ulcer* terminating in flough, the *floughing* and the *common phagedæna*, the symptoms are too acute to admit coition, and as soon as they begin to subside, the parts heal rapidly. In the venereal, the actions are all slow, from the causes before enumerated, so that coition in either sex may for a time be continued after ulceration has commenced. It may also, as will hereafter be shown, produce only gonorrhœa, the discharge from which is infectious.

SECONDLY, the constitution has no power over the diseased action this poison occasions, nor indeed has any remedy but mercury, the use of which was unknown in Europe when the venereal first appeared.

LASTLY, we have not yet ascertained, that either of the other poisons produces that uniformity which can be traced like the venereal from the infector to the infected. Though chancres are precisely similar in women and men, we have no accounts transmitted of floughing ulcers in the vagina, so that if they occur at all, it is at most but rarely.

THIS

THIS last circumstance naturally leads us to enquire into the origin of a poison which cannot be traced in more than one sex. It is extremely to be regretted, that more attention has not been paid to the situation of the penis when these diseases have been contracted. In the case I have so frequently referred to, coition was continued while the skin of the prepuce was broken. In a case which Mr. Wadd very kindly showed me, knowing the enquiry in which I was engaged, two very considerable sloughs had followed ulcers on the glans penis, after which, the parts began to skin without the use of any other remedy but cicuta. On enquiring of the patient the history of the case, I found, that at the time there was the strongest reason to suspect he contracted the disease, he had an open sore on the glans, from an application by which some warts were destroyed. If this could always be ascertained, it might not be difficult to account for sloughs, or ulcers, by what we see repeatedly happen from the application of animal matter to the broken skin of a living animal. These accidents are so common in dissecting rooms, that it is unnecessary to dwell upon them. But it is a great mistake to suppose, that it is necessary the subject should be putrid, in order to produce such consequences. Mr. Abernethy assures me, he never dissects the most recent subject without having an ulcer in whatever part of his hands the

skin is broken, and frequently without his being conscious of any previous division of the cuticle. The fate of Mr. Jones, his late dissector, is well known. Dr. Latham relates the case of a servant who on casing a putrid hare brought part of it in contact with an ag-nail; the consequence was a local ulcer, with swelling in the axilla, succeeded by an efflorescence in the skin with fever, which in an intemperate habit proved fatal. If we examine carefully the accounts of all the tooth-cases, we shall find most of them attended with fever during their whole stage. Sir William Watson and Dr. Lettsom mention it particularly in the cases they relate; and Mr. Hunter in most of those, the whole progress of which fell under his inspection. Mr. French particularizes the circumstance of fever in his case of the sloughing of the glans before referred to. I have remarked it also in the case of sloughing phagedæna. The same may be traced in the case related in the Edinburgh Medical Essays; and Turner describes his patient as delirious with fever, and a miliary eruption.

THIS last circumstance may lead the reader to suspect that the case was merely that of fever, attended with a peculiar determination to the genitals. But it happens that the law of morbid poisons was so scrupulously, if I may so say, adhered to, as to leave the matter beyond question. "Seve-

" ral

“ral weeks after,” continues Turner, “when
 “I was again desired to visit him, I observed the
 “stump of an ill-favoured penis without *glans* or
 “*præputium* on one side healed; on the other
 “raw, with the flesh callous, or rather chancrous,
 “a fungus arising thereon, which though endea-
 “vours had been used to keep under by strewing
 “on the precipitate, yet did it still increase, *even*
 “*before the slough could be digested off.**” In few-
 er words; where the slough cast off readily the
 part skinned over, and where that process was inter-
 rupted by the remaining slough, a fungus rose,
 as we have traced in every case of morbid poison.
 At the same time there was a bubo in the groin
 with scabby pustules on the scalp and other parts of
 the body†. It is not a little remarkable that Tur-
 ner should propose curing the penis by local appli-
 cations before he salivated for the other symptoms.
 The plan, however, succeeded. The first local
 disease yielded to topical applications, and the
 secondary ones to mercurial salivation. All these
 circumstances, particularly those of the primary
 disease, amount to a conviction that the case,

* Syphilis, page 248.

† TURNER imputes the *second infection* to some previous
 ill-cured clap. But as several weeks had intervened be-
 tween the slough and these secondary symptoms, there can
 be no reason for such a suspicion.

though the effect of a morbid poison applied to the penis, was not venereal.

It may be urged, that the disease in the genitals and teeth have seldom proved fatal, compared to those in dissecting rooms. Were this really the case, a variety of causes might be suggested for such a difference; but till we have more facts, and those better ascertained, I shall content myself with calling the attention of the profession to these important subjects, and feel particularly grateful for any communications that may lead to ascertaining the laws of these poisons. That neither the cases on the genitals or teeth are exactly similar to those which have been received by contact with putrid animal matter is certain. Those of Mr. Abernethy are very similar to the little ulcers we sometimes find on the glans, and particularly about the corona, which spread rapidly and superficially, and for the most part heal so readily, that we know not how to impute their cure to the mercury exhibited. Such is probably the *ulcer exedens* of Celsus. When the custom of putting the thumb over the orifice after bleeding was more general, this kind of ulceration or festering, as it was called, was much more common. I well recollect one in St. Bartholomew's which spread so considerably that we students readily made up our minds the patient had been bled with a lancet stained with venereal pus.

Mr.

Mr. Crane probably had his doubts, as he directed the calomel pill. In three days the ulceration stopt, and in another the whole was healed. This confirmed *our* suspicions, and removed Mr. Crane's doubts, who, to the surprize of his young auditors, determined the case not to be venereal.

It is most probable when more serious diseases have arisen from coition with a broken skin, that the secretions of the vagina or uterus have either not been perfectly healthy, or by stagnating too long have some way been altered in their properties. But my business has hitherto been rather to point out the differences between well-known poisons and others, which, though palpably distinct, have been confounded with them. This appears to me the first step towards knowledge, and if I have done any thing towards so important an object, I shall feel happy in having pursued a path almost untrodden till Mr. Hunter directed us to it.

BUT the reader must indulge me with one speculation. We have found in every morbid poison, arising from whatever cause, or communicated in whatever manner, one law invariable, that as soon as ulceration ceases, instead of granulations, as after loss of substance in all other cases, we have immediate skinning or fungus. To what are we

to attribute this peculiarity? The more closely we inspect the operations of nature, the more we are satisfied of design in all. The object of some seems solely to preserve that order which was originally instituted. The mule offspring, perfect in all other parts but those of generation, is the most striking instance of this.

It is well known that two exposed surfaces of the same animal, if brought in contact, will unite either by an union of minute vessels, by mutually attaching themselves to the coagulum of the blood, or by granulation. Were this to take place with as little difficulty between different animals, as it does between different parts of the same animal, very serious inconveniencies, physical as well as moral, might be introduced into the world. To prove the first, we need only reflect how very rapidly this inosculation of vessels, or union by coagulable lymph, sometimes takes place. Even where these fail, it is not improbable but with animals who are torpid during the winter, an union by granulation might sometimes be the consequence of long continued contact.

THESE are some of the inconveniencies that may arise from those original laws, by which the race of animals is preserved. But when we consider the ingenuity of man, his fondness for novelty, the infinite diversity of his wants and caprices,

prices, it is impossible to ascertain what attempts at distorting nature might not have been introduced. Most of the animals, supported for his pride, convenience or security, can bear witness of this. Hitherto, in his own race, it has extended no farther than the rich and crafty taking advantage of the poor and simple, by inducing the latter to undergo a painful operation, and to disfigure themselves, in order to add to the ornament or convenience of the former. Had the plan succeeded, it is not easy to say, what might have been the next attempt; and though, in what we call a state of civilization, men may be induced to barter their teeth, and perhaps other parts, for money, yet if there should be a condition of society in which this medium is unknown, or if the stronger should be envious of any part belonging to the weak and unprotected, another source of discord might have arisen, besides those which have hitherto distracted mankind. It would not be difficult to suggest a variety of probabilities on this subject. But I shall content myself with the facts before us, and leave the reader to determine how far he can impute to any other cause the extraordinary uniformity of these laws, and their exclusiveness to morbid poisons: whether the same are to be traced in the local disease, arising from original animal poisons, it has not been in my power to determine; but there is nothing in the accounts delivered us by authors

to lead us to suppose it. Certain it is, that the loss of substance is in all these cases much less, compared to the constitutional mischief produced. Nor is it less remarkable, that where the secondary symptoms arising from morbid poisons have been different from the primary ones, how great soever the loss of substance may be, it is restored by granulation.

IN whatever way, then, the component substance of one animal is applied to another, where the cuticle is divided, we find danger of its producing ulceration or slough, either of which effectually prevents union; where the contact is continued, the parts might be familiarized to the stimulus, and an union might take place by granulations. But this can only happen where the life of both is preserved, and whenever such is the case, the rapidity with which skinning follows the cessation of ulceration, must prevent the possibility of union. Even where we have traced fungus, followed by granulation, we have always found it preceded by slough, and in most cases soon covered by a scab, which continuing till the cuticle is formed underneath, would be sufficient to prevent the possibility of union. It must, however, be admitted, that this provision of nature, if it really is such, cannot be traced with absolute certainty beyond ulceration, sloughing and immediate skinning. If fungus takes place, and each part preserves its life, as far as our knowledge

ledge hitherto extends, union may happen from consequent granulation. But then, we must admit, contact for a length of time greater than is ever likely to take place, and that in neither subject skinning or scab preceded granulation. But it must be admitted, that transplanted teeth have appeared to retain their life, by which it seems to follow, that under every circumstance this contact does not prove poisonous. In the case related by Dr. Lettsom too, it appears that the stimulus of the poison may after a time be superseded by a remedy. But before I enter farther into this subject, I could wish the fact of the tooth retaining its life to be well established. It is well known that some of them more than two, and even three years after they were supposed to have retained life and acquired stability, have gradually loosened, and without any previous uneasiness or apparent caries, dropt out. As this is different from what we meet with in the original teeth, it is impossible not to doubt whether their vessels have ever inosculated with those of the enclosing gum or socket.

It is true that the tooth inserted into the coxcomb, and the testicle into the abdomen of a hen, were found after death to have vessels communicating with the surrounding parts. But it has been found that the comb is scarcely susceptible of the
irrita-

irritation of original poisons,* and though we have traced these laws on the surface, it does not necessarily follow that the same are observed in internal cavities. It is not, however, necessary for me to reconcile every objection; it is enough that the uncertainty and danger attending the experiment of uniting parts of different animals is such as to prevent its ever becoming general.

I WAS curious to know how Taliacotius, the celebrated nose-maker of Hudibrastic memory, had succeeded in his attempts. But though the facetious poet has given him the credit of cutting noses from one subject, and inserting them in another, he himself disclaims it, for many reasons, and among the rest, the uncertainty of an agreement between the constitutional characters of the two parties.† I pretend not to determine what credit is to be given to this writer, but certain it is, there is nothing in any of the operations he speaks of at all inconsistent with the well-known laws of the animal oeco-

* The Abbè Fontana found that the poison of the viper produced, for the most part, no effect on the coxcomb when applied to it, but swelling and inflammation in the gills. *Fontana on Poisons, vol. 1.* This shows a degree of insensibility in the comb to the irritation of poisons, and we have no instance of morbid poisons producing disease without affecting the part to which they are applied.

† *De Curatorum Chirurgia.*

nomy. A contemporary writer* also speaks of the practice, without attempting to refute it: and if we may believe the accounts of impartial travellers, something of the kind is at this time frequently practised in the East Indies. †

THUS far we have only traced the poisonous effects of matter applied from one animal to another of the same class, and when under the influence of no specific disease. Where a diseased action exists, if it be not such as arises from an original conformation, it is not to be wondered if similar consequences follow. But it seems very difficult to conceive how diseases, which at present never appear to originate with the sufferers, should have first sprung up. Mr. Hunter, as far as the obscurity of his language will enable us to judge, seems to conjecture, that the venereal might owe its origin to some unnatural connection. Certain it is, that diseases in one class of animals, when communicated to another, seem to alter many of their properties. It has not yet been exactly ascertained, what is the peculiar situation of the dog, or other rabid animal, when his bite produces hydrophobia. Certain it is, that the same symptoms have not uniformly appeared as those which have followed his bite.

* Alexander Benedictus.

† See Gentleman's Magazine for November, 1794.

The cow-pox is a disease well known to the dairy farmers in Gloucestershire. The only appearance in the animal is a phagedænic ulcer on the teat, without any apparent inflammation. When communicated to the human, it produces, besides ulceration in the hand, a considerable tumour of the arm, with symptomatic fever, both which gradually subside. What is still more extraordinary, as far as facts have hitherto been ascertained, the person who has been infected is rendered insensible to the variolous poison.

WHETHER any of the morbid poisons, which at present so much diminish the period of human life, arose from such causes, it is impossible to ascertain. It would be easy to suggest many arguments that might favour such an opinion, derived from the countries from which some of these poisons are said to have originated. But as nothing satisfactory can be ascertained, it is much more to the purpose to direct our attention to the laws by which every poison is governed, till an accumulation of facts shall enable us to form rational conclusions.

CHAP.

C H A P. VII.

OF CARCINOMA AND OTHER LOCAL
DISEASES USUALLY CALLED CAN-
CEROUS.

THOUGH these diseases may be traced as far back as any correct annals of medicine, few modern writers have been so accurate in their descriptions as to ascertain exactly what is meant by cancer. It may, therefore, be better to leave the term to its general sense, and consider *carcinoma* as one division. As this, with some others, has hitherto proved beyond the reach of medicine, it may be thought of less consequence to make these distinctions. But this want of accuracy has very much encreased that scepticism regarding remedies which must for ever retard the healing art. Whoever takes the trouble of perusing Stork* will find very few, if any, of his cases such but what might yield to hemlock, aided in some instances

* OF this work I have only seen a translation, entitled, An Essay on the Medical Nature of Hemlock, &c. Printed for Nourse, 1760.

by the diet and rest of an hospital. Dr. Akenfide,* in his account of the effects of sublimate and other remedies, in curing cancers, confounds steatoma, carcinoma, struma and phagedæna. By his language and theory, he seems to derive them all from the same source, and supposing no difference but in degree, conceives all may be cured in their early stage. The second volume of the Medical Observations and Enquiries†, contains a number of valuable communications from Edinburgh, on the cure of phagedænic ulcers by corrosive sublimate. They all go to prove, that in many cases this preparation of mercury is preferable to all others: but none of them are fairly referable to carcinoma. Mr. Gooch‡ has also several important cases of cancer, some of which he was successful in curing by medicine; but none of these were true carcinoma. Dr. Rush, of Philadelphia, gives us some valuable cases of cancerous ulcers cured by arsenical preparations,§ but observes that the same was never successful in cancerous breasts; probably the only true *carcinomata* in which it was attempted. From this confusion of language, the names

* Medical Transactions, vol. 1.

† Medical Observations and Enquiries, page 218 and seq.

‡ See Gooch's Work in 3 vol. published by Johnson.

§ Medical Enquiries and Observations by Benjamin Rush, page 188.

names of respectable practitioners have been brought into question, remedies highly important have been much undervalued, and some of a stimulating property, when applied to carcinoma, have greatly exasperated the disease.

To avoid ambiguity as much as possible, I shall begin with a description of *carcinoma*. This term may serve us for every stage of the disease, by which we shall keep clear of those of *scirrhus* and *cancer*, which I have already shown have been used in too general and undefined a sense. In my description of carcinoma, I shall be obliged to differ from most of our nosologists, and I fear from some very accurate writers. Scirrhus, or, as I would call it, the early stage of carcinoma, has usually been described as a glandular affection, and some who have taken notice of the cysts, seem to consider them as accidental appendages to the disease. From all the observations I have been able to make, aided by the preparations I have had access to, and even all the more accurate accounts of authors, it appears to me, not only that the cyst is constantly found in cases of true carcinoma, but that it constitutes its character. Celsus, who was no theorist, could scarcely mean any thing else, when he derives the disease from the liver or spleen† parts, in which he must often have met with hydatids, as his observations

† Lib. v. Cap. 28, Sect. 2.

ons were so much confined to the anatomy of brutes. Dr. Monro and Mr. Gooch take notice of these hydatids, but neither of them seem to consider them as constituting the disease. I have heard Mr. Pott remark, that in all truly scirrhous testicles, how clear soever the spermatic cord may remain, he had never failed to find a fluid in some part of the testicle. The same may be remarked of some diseased-breasts, described by Mr. Fearon, for which the operation was performed very early: and in conversation, that gentleman has assured me, he has often found the only appearance of disease to consist of small cysts filled with a fluid of different complections, some of them apparently containing matter. I can say precisely the same from my own observation, but it is more satisfactory to have the authority of others. Mr. Hunter spoke of these cysts as *cancerous hydatids*, but whether as forming the disease, I am unwilling to determine from my notes.

IN what manner or from what cause these hydatids first arise, is beyond the present state of science to determine. When they are formed, they have the property of encreasing in different degrees, from causes equally beyond our knowledge. It appears, also, either that their number augments, or that some which were altogether imperceptible to the touch, and the patient's sensation, encrease in such

such a degree as to equal the size of the largest. This enlargement of a foreign body in a solid substance, and so extremely sensible as the breast, cannot but be attended with intense pain, and frequent inflammation. The increase of the cysts is not towards the surface, like matter in a common abscess, but in every direction. Another peculiarity in this disease is a disposition to fungate before the skin is broken. This is generally to be discovered in a certain degree in breasts that have been amputated before ulceration. But if the disease is suffered to proceed till the skin ulcerates, the usual appearance is a fungus, which being no longer confined, very soon grows, sometimes to a considerable extent beyond the lips of the ulcer. It appears as if its previous confinement gave it a hardness unusual for substances of this kind. At least I have observed in several instances a difference in the firmness of the fungus, where the skin has been destroyed by art or by ulceration. To this fungus we are to impute the discoloration and unequal surface of the skin which preceded ulceration.

SUCH is the progress of the ulcerated carcinoma, or open cancer as it is called, when the cysts are small and deep-seated with a quantity of fungus between them and the surface. After ulceration has commenced, the progress is extremely uncertain, depending upon the patient's health, the situ-

ation of the cists, their size, and often on the remedies. Sometimes the fungus will remain nearly stationary for many months, occasionally bleeding, or discharging matter of various consistence, till a large cyst, or probably several below its base, suddenly bursting, or gradually separated by ulceration, detach the whole, and expose an ill-conditioned surface, consisting partly of the tunics of the cists. In other cases where the cists have been nearer the surface, the fungus ulcerating or sloughing away in various parts, exposes at different periods several roundish *foveoli*, apparently filled with sloughs, but which are nothing more than the cists with their contents, changed from their original appearance to that of an opaque pale, somewhat resembling half dissolved leather. When any of these are exposed, the fungus bed in which they are deposited, seems to recede from them by ulceration, and on removing some succeeding dressing, a cleaner surface appears. If this happens near the edges of the ulcer, and no other cists, or fragments of them remain near; the lips which before were reflected on the sound skin, being forced back by the increasing fungus, will very soon take a different direction, and the wound will contract.*

THESE are among the appearances ulcerated carcinoma assumes when the hydatids are deep seated or

* Such appear to have been the eighth and eleventh cases, related by Stork. These are the only two that a careful

or small. But sometimes it happens, that one or two particularly large, or a cluster of them, extends from the surface very deep. When this is the case, very soon after the skin has separated, a considerable evacuation of fluid follows, as described by Dr. Monro,* or a perpetual dripping, which will last till all the hydatids exposed by the opening are evacuated. Even then it seems doubtful whether the distillation is not kept up from the sides of the cysts, as it sometimes continues for a considerable length of time, and in great quantities. This evacuation of the cyst or cysts, very much relieves the patient, without rendering the breast at all flaccid, but producing a most ghastly cavern of an irregular surface, lined in many places with the tunics of hydatids, and emitting a very peculiar smell. In the meanwhile the edges, as far as they are free from hydatids, make an attempt at granulating, which produces an exuberance of fungus that reflects the skin back in such a manner that it may be almost said to curl.

careful reader will be able to satisfy himself were true carcinomata. The second and fourth were certainly suspicious, but in the former we evidently detect a scrofulous habit; in the latter, the *collapsing* of the breast, after a copious discharge of *matter*, gives us no other idea than that of common abscesses. How much is it to be regretted, that a physician, who has enriched the Pharmacopœa with so valuable a remedy, should have lost any part of his reputation by withholding his other unsuccessful cases.

* Medical Essays, vol. 5, page 339.

IN this description I have confined myself to the appearance of the disease in the breast, because we see it here on the largest scale. In other parts of the body too I have not had the same opportunity of tracing all the different stages. When carcinoma attacks the testis, the disease is either extirpated in time, or if the spermatic chord is affected, soon puts a period to the patient's suffering. Mean while the interposition of the scrotum prevents external ulceration till a very late period. The reader will meet with some cases of this kind related by Turner,* under the title of Hernia Carnosa. Le Drant† has also some well marked cases of cancerous lip, which may be easily distinguished from a steatoma extirpated with success.‡ In the former carcinoma was marked by the previous slow enlargement, sanious discharge, subsequent excavation and fungus, as distinctly as can be expected, where the contents of the tumour are not described.

BUT in the advanced stage of the disease it is not easy to mistake it—if in the breast scarcely possible. It would be much more to the purpose, if at an earlier period we could distinguish a carcinomatous from a scrofulous or any other tumour.

* Turner's Surgery, vol. 1, page 280 & Seq.

† Observations in Surgery, translated, third edition, page 48 & seq. ‡ Id. Page 43.

But though some of us are in the habit of pronouncing boldly on these subjects, I fear this readiness rather arises from an incapacity to offer any satisfactory reason than from any certainty of the fact. It must be admitted that carcinomatous tumours are usually harder to the touch and more circumscribed. That the pain they occasion is in some measure peculiar, and that the breast has for the most part an unusual weightiness, even for its increased size. But there are periods of the disease in which nothing satisfactory can be determined, all the symptoms not being present; while in some instances scrofulous tumours assume many of them. The changes the breasts undergo, at different periods of the constitution,* and in the early stages of gestation produce tumours very liable to be mistaken for carcinoma. So that if we except the pain, a description of which must be attended with great uncertainty, I know of nothing that can discriminate early carcinomata from other tumours. On account of this uncertainty, the term scirrhus might perhaps be useful in a more general sense, to denote any circumscribed hardness, the nature of which we are unacquainted with.†

* VID. Morgagni De Causis sed. Morb. Epist. L. Article 39 et seq.

† DISCERNERE autem cacoethes quod curationem recipit, a carcinomate quod non recipit nemo scire potest nisi tempore et experimento. CELSUS.

IN describing the more advanced stage my object has been not the mere diagnosis of carcinoma, but by tracing the series and order of facts to show the impropriety of confounding such a disease with many others which will be hereafter enumerated, and, if possible, to lead the reader into such an inquiry as may direct us in the application of remedies. The first suggestion that will offer itself is, are these hydatids similar to those in the cavity of the abdomen? If they are different in themselves, is their increase in size and number similar? Hydatids in the abdomen grow more rapidly, multiply faster, and perhaps run through their stages quicker. But, besides that, there are probably varieties in these, as we find there are in cancerous hydatids, while we confess our ignorance of the natural history of such bodies, we must not pretend to ascertain what contributes most to their increase. Hydatids in the liver or spleen produce no apparent change in them—but other foreign bodies will remain or pass through the substance of those viscera, without stimulating to suppuration or ulceration.* This disposition to fungate before the skin is broken, is in great measure peculiar to carcinoma, and is much more exuberant in the breast, than in any other part.

* I leave the reader to determine whether the cancerous tumour described by Morgagni's Epist. L. 4, originated in a string of hydatids multiplying from the thorax to the axilla.

I am fearful of suggesting all that occurs to me on this subject; I shall therefore request the reader to re-peruse Dr. John Hunter's valuable paper on the subject of hydatids, and to bear with me a little longer.* It may appear as if there were no connection between different cancerous hydatids, like what is found in intestinal ones. But we ought to consider the carcinomatous breast not as we see it, but by comparing it with the found one of the same subject. We shall then find that the great distance between the hydatids is entirely filled up by the fungus which grows after the cyst is formed. Intestinal hydatids are frequently found contained in one large cyst, filled with a fluid in which they float. At other times the connection seems looser, and the multiplication to extend without any circumscription. The latter seems most commonly the case with cancerous hydatids, but unfortunately, though this disease so often occurs, we have not a sufficient number of descriptions of the contents of amputated *mammæ*. In the Paris Memoirs† we have however a very exact account of a cluster of these hydatids in a carcinomatous breast, which, by their compactness, appear to have been all contained

* Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge, Article 3d. See also Dr. Bailie's Morbid Anatomy, page 150.

† Mem. de l'Academie Royale de Chirurgie, vol. i. page 684.

in a single cyst, and of this opinion seems M. Faget, the author of the paper. Perhaps hereafter the success of the operation may be estimated by the probability of the hydatids being inclosed in a cyst, and by the proximity of that cyst to the surface.

THE contained fluid is more variously described in carcinomatous than in abdominal hydatids. But we have much to learn on this subject. It is well known that the contents of many of them are similar. If some carcinomatous hydatids appear to contain bloody sanies, we must satisfy ourselves that these are not filled a second time by ulceration of the surrounding fungus. Carcinoma, in its early stage, or scirrhous state, it is supposed may be cured; but we have no reason to suppose we have any remedies against abdominal hydatids. Let us first ascertain what can be done for true carcinoma in any state. Mr. Fearon has proposed a very rational, and doubtless, in many respects, successful practice, for indurations of the mammæ, in their early stage.* It does that gentleman no small honor that his cases are so few and well related, and is sufficient to convince us that his object was the inquiry after truth, and not the mere recommendation of a practice in a great measure his own. But all the cases are not such as would generally

* Essay on Cancers, page 57 and seq.

have been considered as cancerous, and his own importunate advice in favour of early amputation proves his opinion of the precariousness of any other remedy*.

But hydatids are found in other parts of the body besides the viscera, without producing cancer. I shall not be expected to take notice of incised tumours, as they are called, filled with a glary kind of fluid, but containing none of those regular globules distinguished by the name of hydatids. The Philosophical Transactions, however, contain two well marked cases, one by Tyson, † of hydatids, found in a large cyst, extending from the occiput to the scapula: the other by Watson, ‡ of a cluster, that escaped from the uterus. Before I undertake to reconcile these phenomena with the opinion I have ventured to bring forward, let me beg the reader will permit me to submit the following inferences from the authorities already produced;

1st. By the case mentioned in the Paris Memoirs, it appears that carcinomatous hydatids may be confined in a cyst, so that how great soever their multiplication may be, it can only tend to encrease that cyst as long as it remains entire. That in this

* Id. page 70.

† Philof. Transac. vol. xxv. page 2344. ‡ Id. vol. i. 711.
case,

case, as far as we can judge, the fungus is confined to the space between the cyst and the surface of the skin. Hence if the cyst were immediately under the skin, we might expect its increase to produce no other consequence than a dilatation of the skin. Tyson's case seems of this kind.* Immediately on opening the skin, innumerable hydatids rushed out, swimming in a thickish liquor. But at the root of the tumour, which from the situation of the latter, must have been near the surface of the skin, there was a large fungus [*sarcoma*] which was destroyed by escharotics. The wound readily healed. 'Tis to be regretted we have no account how long this or M. Faget's case continued well.

SIR W. Watson's paper is not less remarkable. After swelling and tension of the abdomen, about the period of the cessation of menstruation, the patient was relieved by the escape of a number of hydatids, *per vaginam*, attached, like grapes, to a spongy substance, answering the purpose of a placenta. After this statement may I not be allowed to ask,

1st, HAD Tyson's hydatid-cyst been situated deeper, like that described by Faget, should we not have found a fungus between it and the skin.

2^{dly}, HAD the hydatids multiplied like Watson's, without

without being confined to a cist, would not this fungus have shown all the appearances we meet with in true carcinoma? It is scarcely necessary to add, that the hydatids, described by Watson, wanted nothing but to have been generated in a solid substance, instead of the uterine cavity, to give them every property of true carcinomatous hydatids. Nor should I omit that the resemblance was at least heightened, by some of them containing bloody sanies.

IT has been assumed, perhaps a little too generally, that blows occasion cancer. But the insignificance of many of these injuries, the immense distance of the period, and the very frequent occurrence of the disease without any such cause, are sufficient to render it at all times equivocal, especially when we recollect the prevalent, but equally vague opinion, that a cancer from violence may be more easily cured than one arising spontaneously. If this conclusion proves any thing, it can only be that such diseases are rarely carcinomatous, though confounded with it. At the same time this ill-founded notion has taught almost every sufferer to recollect some little incident in her existence, which might have produced a disease, the fatality of which she is too well acquainted with. One objection still remains, whether a decisive one, I must leave to the

* Id. p. 79.

deter-

determination of those who have the largest opportunities of tracing diseases during life, and their consequences after death. The stomach, and probably other parts of the intestinal canal, are liable to carcinoma. I say nothing of the rectum, because, in these cases, the disease finds its way to the surface. I shall not take upon me to determine how far these parts are subject to the same laws as govern those on the surface. But if we admit, from this single objection, that carcinomatous hydatids are different from all others discovered within the cavity of the abdomen or thorax, it must still remain to be ascertained whether their growth and multiplication depend on the same laws. It may be said too, that carcinoma contaminates the neighbouring parts by the absorbent vessels. Wherever inflammation exists, it is well known the neighbouring lymphatics will be affected. But it has not always appeared that this affection has been carcinomatous. As far as we can judge, these hydatids increase in number wherever the texture of the parts allow them the most room. Thus in the more advanced stages we find all the cellular substance between the mamma and axilla partaking of the disease.

MUCH has been written on the subject of the matter discharged from carcinomatous ulcers. It has been thought to have a corrosive quality, by
which

which the coats of arteries have been destroyed, and large hæmorrhages produced. The test of chemistry has been applied to account for this.* But the fact should first have been ascertained, which could only have been done by applying the sanies to dead animal matter, and seeing how far any chemical combination followed. That this matter, like all other that flows from ill conditioned ulcers, is highly stimulating when applied to a living part deprived of its cuticle, or covered by a thin one, cannot admit of a doubt. Some well authenticated facts enable us to form those conclusions which might have been expected. The fluid of the hydatids,† when limpid, was found to produce no other sensation on the tongue than that of most other animal secretions—namely, a slightly saline taste. The sanies from the ulcer acted like a morbid poison,‡ producing a phagedænic ulcer. Two cases of this kind, related by Gooch, were relieved, one by corrosive sublimate, the other without an operation. It is unnecessary to add that neither could be true carcinoma. One of them being on the lip of a married lady, proved contagious and fatal to her husband. Here we

* See a paper by Dr. Crawford, Phil. Transac. vol 80. p. 391.

† Medical Essays, Vol. v. p. 339.

‡ Gooch, Vol. iii. p. 169. also Turner, in his Diseases of the Skin, mentions cases of this kind.

have

have an instance of a morbid poison originating from an incurable disease, and relieved in the first instance by mercury. The husband was abroad, or might probably have experienced the same relief. That many of the phagedænic ulcers on the lips, which are usually called cancerous, are the effect of some morbid poison I have already hinted. The situation renders them suspicious, from being constantly liable to contact with animal matter, and the well authenticated cure of some of them by internal remedies or escharotic applications is sufficient to distinguish them from carcinoma. Where ulceration proceeds very slowly, a thickened circumference is sometimes formed round it. This differs considerably from the venereal and the carcinomatous. From the former, by the edges instead of being ulcerated exposing towards the ulcer a surface covered by the cuticle. From the latter, by the absence of that fungus which produces the retorted edge.*

CELSUS is more accurate than most writers in his division of what may be called local diseases, arising without violence.† It is not my wish either to avail myself of his language or division. In the former it has been my endeavour to vary as

* DR. MONRO in Medical Essays as above.

† LIB. 5, Cap. 28—quæ interius corrupta aliqua corporum parte nascuntur.

little as possible from that in general use, and many of his divisions are foreign to my present inquiries, or have been already anticipated. It is impossible but the attentive reader must discover, under the title of *ignis sacer*, the secondary ulcers of some morbid poisons. The description is too accurate to admit of a doubt, and the mode of cure is still more remarkable—*Medicamentum ejus fortissimum est, uno die febris, quæ humorem noxium absumat*. In the cure of secondary ulcers from most morbid poisons, we have seen that it is quite sufficient to interrupt their action by the slightest degree of mercurial irritation, after which they heal readily, the constitution not appearing unequal to the task in any but the venereal. Nothing can bear a stronger analogy to such a remedy than an ephemeral fever, which would be just sufficient as in the other instance to excite a new action, after which we might expect to see the ulcerations heal with a rapidity that might fairly induce Celsus to call this fever *remedium fortissimum*. Nor is it any objection that Celsus enumerates the soles of the feet as particularly liable to such complaints. Parts most exposed are usually soonest affected by the secondary symptoms not only of the venereal, but other morbid poisons. The Roman dress and habits of life rendered the feet much exposed, as we might have observed of the ribs on a former occasion.*—But this is foreign to our present purpose.

* See page 80.

THERE is only one other cancer that I shall detain the reader with, which, from its frequency and constant fatality, may well arrest a medical writer in his career of undertaking to inform the world. Of all the calamities to which a sex that seems destined to support the largest share of human misery is obnoxious, the cancerous uterus is the heaviest. This will not, I apprehend, be disputed, whether we consider the situation of the disease, almost any one of its attendant symptoms, or the certainty of a life more wretchedly protracted than by any means that the ingenuity of Indian torments can boast. We are ready enough to call the gout *opprobrium medicorum*, because we know, in most instances, it may be called *opprobrium divitiarum*. But what shall we say of a disease that is every day occurring, and the medical progress of which has hitherto extended no further than to confound it with another, which it only resembles in being painful and incurable; and to apply remedies which have derived their reputation from curing diseases different from either? The latter part of this proposition I trust I have already proved. If the former rested on my own authority, I might have only ventured to ask, where are the cysts, where the fungus, where the increased size in the most common of all the cases of cancerous uterus. I might ask, do we discover more than a sloughing phagedænic ulcer, with thickened
hard

hard lips, which slowly destroys that organ and all the neighbouring parts, till the kindest of physicians puts a period to the disease and the patient's sufferings.

THAT true carcinoma may attack the uterus as well as any other part, is a position I shall not attempt to deny; but that the above, the most common species of what is called cancerous uterus is improperly confounded with the disease in the breast, is a remark for which I was first indebted to Mr. Hunter. While I am ready to admit, that a partiality I take no pains to conceal, may have warped my judgment, without my being aware of it, I am at the same time ready to confess the triumph I feel, in being able to introduce my master in such respectable company. A writer,* whom he could only know by name, but with whose works every physician practising in London must be well acquainted, takes some pains to correct this error, which existed in his time; and Morgagni describes two cases, in one of which this ulcer had committed its usual ravages on the cervix, in the other it had extended to the neighbouring parts, while the rest of the uterus was sound.†

* Aretæus, cap. xi.

† De Causis & Sedibus Morborum Epist. xlvi. 8. & xxxix. 33.

BUT neither a Hunter, an Aretæus, or a Morgagni, is any authority for a plain matter of fact, on which any one may satisfy himself. It must therefore depend on the observations of those who have the best opportunities of seeing such appearances. Dr. Bailie* should stand among the foremost of these; and from the general accuracy of his descriptions, and the universality of his enquiries, we might expect a solution of this important difficulty. Strengthened, however, as I feel myself by such authorities, I shall not scruple to say, that his account of the diseases of this important viscus is much too short and confused. That if the section of the cancerous uterus does sometimes exhibit an appearance similar to other parts in what is called a scirrhus state, this is neither a necessary nor the most common attendant on the sloughing phagedæna of the cervix and neighbouring parts. I had an opportunity of inspecting in one winter the uteri of four women, who died of the complaint, and none of them exhibited the least appearance of disease a quarter of an inch beyond the ulcer. None of them appeared enlarged, nor was there any "whitish firm substance, intersected by strong membranous divisions." In the melancholy collection preserved by Dr. Lowder, few appear enlarged, and of these very

* Morbid Anatomy, p. 245.

very few beyond the size that this uncertain viscus often exhibits in a sound state.

It may be said that parts differently structured and organized may be expected to show the same disease in a different form. Without waiting to dispute this question, let me only be permitted to ask, Is this a sufficient reason for calling two diseases the same, when they only resemble each other in those points, which should teach us to redouble our industry in detecting the peculiarities of each? There is, indeed, one part of the description of either, that is, by general consent, common to both; I need not add their constant fatality. It would be to little purpose to deplore the insufficiency of our art; or to accuse the industry of its professors. Causes can only produce their effects; and perhaps the vast storehouses of nature may not contain more than a palliative for these devoted sufferers. But when we consider the slow progress of the maladies, the frequency with which they occur, and the ample opportunities some of us possess of detecting every stage and form of them, can we say these opportunities have been improved with an industry proportioned to the magnitude of the object, and the sacredness of the functions we profess to be engaged in? If remedies have been unsuccessfully varied, and operations proved precarious; have we yet marked

any of the causes by which we are to judge of the probable success of either? Have we discriminated those appearances in the trophies of our disgrace which may teach us to regulate our future practice, or in more successful operations have we learned the causes to which we are to attribute this desirable event? Have we yet agreed what is a cancer? Is there an author to whom an uninformed student can apply, that will teach him how to distinguish this most formidable complaint? and are not the definitions of the present day less perspicuous and wider from the mark than the description of a writer who has been two thousand years before us?*

“ Is goodness no part of wisdom; that while we seek to be wiser, we neglect to be better? Is it well that the study of philanthropy is precluded the schools of philosophy?” Such are the words of one† who probably emerged from his study to wonder that men did not invariably pursue their truest interest. If by this time, he has mixed with those who having completed their researches are exchanging them for the commerce of the world, will he not ask, “ Is science no possession, that

* Compare the definitions of modern nosologists with the description of Celsus.

† See Mr. Young's Introduction to the Character of Socrates. Spirit of Athens, page 232.

“ wiser?”

“ while we seek to be richer, we neglect to be
 “ wiser?” But let me not seem sullen, while I
 mean only to lament the weakness of human na-
 ture. Life is short, and our art much too extensive
 for the limitation of our capacities. Objects of dis-
 tress are always painful, and doubly so where
 they associate the idea of our inability to offer the
 looked-for relief. We naturally hasten to those
 scenes which flatter our vanity, and relieve our
 feelings. Perhaps it is best for most of us that
 we do. The scope of professional enquiry is suf-
 ficiently extensive, and the path to fame and
 emolument is not by the intricacies of a labyrinth
 that may be only half explored, when the period
 of our usefulness ceases. In the midst of our en-
 quiries we find ourselves overtaken with new pas-
 sions, new fears, and new wants. The prospect
 of old age and penury very early alarms us, or
 views of ambition and aggrandisement present
 themselves; and at the period when the faculties
 of the mind are the strongest, they too often
 receive a bias fatal to all further improvement.

MUST then true science for ever languish; or
 must the end be for ever sacrificed to the means?
 Must that industry which might be directed to the
 most useful of arts be wasted on the “ filky texture
 “ of a flower,” or be consumed in the furnaces of
 the chymist? I mean not to undervalue those arts,

which, while they embellish life, might contribute to soften its calamities. But is the same accuracy conspicuous in the discrimination of diseases, as is displayed in the classification of plants, or the arrangement of fossils? Is the same patient investigation discoverable; the same progressive improvement—and if to these considerations we add the superior importance of the former, may we not be allowed to pause a moment to enquire into this apparent inversion of the order of things?

IT is to little purpose, however, to dwell on a self-evident proposition—rather let us regret that, hitherto, the only practitioners who have professed this branch of surgery, exclusively, have not been such as could discriminate diseases with that accuracy which even a regular education does not enable every one to undertake. But what is still more unfortunate, the secrecy with which they have conducted their practice, has prevented benefit to the world by that accumulation of facts from which some information cannot but result. We have indeed to thank Mr. Fearon for some improvements in the operative branch, and for a judicious method of treating tumours at a period before their true character can be discovered. Dr. Ewart has also favoured the world with an account of his successful manner of treating a case, the cure of which we can only hope may be

as permanent as the disease was indisputably characterized. But how slow, how feeble must be the aids of such as are engaged in general practice! May we not indulge the hope that the faculty will select from among themselves some individual, in whom integrity, industry, and accuracy are so far united, that by a life devoted to such researches, and the communication of them to a class, we may at least learn the full scope of the human intellect in diseases hitherto deemed incurable?

WHENEVER this desirable event is accomplished, the following questions will be no longer matters of doubt.

FIRST, Is the simple hydatid the first form of carcinoma?

SECONDLY, Is there any difference between abdominal and carcinomatous hydatids, except in the slow progress of the latter, the number of their tunics,† and the contents of some of them?

THIRDLY, Are carcinomatous hydatids, like those of the abdomen, divisible into such as mul-

† The tunics and contents of carcinomatous hydatids may, I think, be explained; the first by frequent inflammation producing strata of coagulated lymph, the latter by these bodies increasing in a substance so sanguiferous as the fungus. The tunics of Fagel's incised hydatids were thin, and some of Watson's were filled with bloody lymph.

tiplly

tiplly within a cist, and such as multiply without any circumscribed cavity to confine them?

FOURTHLY, Does the permanent success of the operation for carcinoma depend on the hydatids being confined within one common cist; in consequence of which, when the tumour is removed, no hydatids can be left imperceptible to the eye, but whose subsequent growth and multiplication may perpetuate the disease?

FIFTHLY, Where no operation is performed, is the period of the patient's existence to be estimated by the magnitude of individual hydatids, and the rapidity of their growth?

SIXTHLY, Have these hydatids a life independent of the subject in which they grow, excepting as parasites?

SEVENTHLY, If so, should the means of cure, where an operation is not submitted to, be directed to the extinction of that life, with as little injury as possible to the patient's health?

EIGHTHLY, If hydatids possess the principle of vitality during their transparent state, and their opacity is the effect of the loss of that principle, would they not in the latter stage stimulate the parts in which they are situated to suppuration, as we find the case with the guinea-worm when dead?

NINTHLY,

NINTHLY, Would not this suppuration prove the destruction of all the neighbouring hydatids? or should carcinomatous hydatids produce absorption of the internal coat of the pylorus or cardia, as they do of the liver and spleen, so as to find their way into the cavity of the stomach, would not this be equally destructive to them?

TENTHLY, May not the fungus generated between the hydatids and the surface of the skin, or towards the cavity of the stomach, be for the purpose of preventing suppuration in one instance, and absorption in the other?

I AM aware the boldness of these queries may subject me to the charge of rashness in venturing them to the world. Let it, however, be remembered they are founded on facts of which I have myself been witness, though I have thought it a tribute due to public opinion to produce authorities for them all. I have only, therefore, to beg that before I am censured or ridiculed, all these authorities may be examined, and that then it may be recollected, I am only proposing enquiries, without venturing to form theories: and that these enquiries are directed to a disease, against which all the improvements of our art have hitherto proved ineffectual.

CHAP.

CHAP. VIII.

COMPARATIVE VIEW OF THE THEORIES
 OF DRS. FOART SIMMONS AND SWEDI-
 AUR, JOHN HUNTER, MESSRS. FOOT,
 MOORE, AND BELL, ON THE LAWS
 OF THE VENEREAL VIRUS; WITH
 AN ENQUIRY HOW FAR THE OPINI-
 ONS OF MR. HUNTER ARE CON-
 FIRMED BY FACTS ADMITTED BY
 BOERHAAVE, ASTRUC, AND OTHER
 WRITERS OF REPUTATION.

THE reader who has had the patience to tra-
 vel with me thus far, need not be informed of the
 labour with which my materials have been collect-
 ed. Most of the diseases described in the former
 sheets have been hitherto little noticed; or con-
 founded with others, or the description of them has
 been too obscure to direct our researches. Though
 this cannot be pleaded of the venereal, yet in the
 early accounts of it, we have a number of symp-
 toms enumerated which never appeared since
 more authentic documents have been produced;
 either because men were credulous enough to be-
 lieve

lieve any thing of a disease ill ascertained, or because whatever appearances were uncommon, were ascribed to that cause. In later days, has the disease been marked with that precision which might have been expected from the frequency of its appearance, and the immense tribe of writers that have undertaken to inform the world?

IT would be to little purpose to dwell on the strange opinions of ancient writers, the inaccuracy with which they confounded the symptoms, and even the seat of the disease. Those who wish for information on this subject may consult Astruc, from whom most of his successors have borrowed their authorities. By the kindness of Dr. Sims, in allowing me the full scope of his library, and directing me through it, I have had access to all the early writers, till I was wearied with fruitless researches, and unsatisfactory enquiries. By means of the same gentleman, and Mr. Wadd's Tracts, collected by himself and the late Dr. Luke Wayman, I had also an opportunity of seeing many of the productions of a later date. But it is hardly credible how little information is to be gained by any. In none between Astruc and the present times could I discover a close description of symptoms, and of the effects of remedies. Astruc has certainly great merit as an industrious investigator, and if he had not been tainted with the
false

false physiological reasoning of his days, would have done more. From his time almost to the present we find nothing added to the pathological knowledge in this disease. In some of the writers of our own days, if we meet not with a more accurate description of the symptoms, we have at least a juster account of the seat of the disease, and by degrees, a true discrimination between the two species of it.

BUT, where publications are so numerous, we can only select those which have acquired the highest reputation. Of such, before Mr. Hunter's appeared, there are two particularly recommended by Dr. Cullen, as so complete in themselves, as to render many remarks of his own unnecessary. These are Dr. Foart Simmons and Dr. Swediaur. The former has written a tract containing many practical remarks, the utility of which cannot be doubted by such as are acquainted with the author's abilities. Though the conciseness of the work is such as to preclude much theoretical reasoning, yet there is one observation on the chancre that must not be overlooked:

“ Objections have been made to the cure of a
“ chancre by topical applications, on a supposition,
“ that if the ulcer is healed by such means, the virus
“ will be carried into the blood, and the patient will
“ be

“ be in danger of constitutional lues. That this may,
 “ and sometimes does happen, I am convinced by
 “ several striking instances of lues from this source,
 “ that have fallen under my own observation: but
 “ in those cases, the chancres had been large and
 “ spreading, affording a copious discharge of mat-
 “ ter, and of course a considerable absorbing
 “ surface: they had likewise been improperly
 “ healed, and the means of preventing infection
 “ had been neglected: for the fact is, that if we
 “ apply mercurial ointment, or any other stimu-
 “ lating or astringent substance, to a sore of this
 “ sort, and so dry it up, we must inevitably infect
 “ the habit of the patient; whereas, if the chancre
 “ is small, and without any considerable inflam-
 “ mation, and we touch it repeatedly with the
 “ lunar caustic, so that the sore shall throw off
 “ several sloughs, and at length appear clean and
 “ disposed to heal, we, by such a process, *destroy*
 “ the venereal virus, instead of *repelling* it, and
 “ in this way, may, in a few days remove an evil
 “ which would otherwise, perhaps, be the work of
 “ several weeks.*” Now, if it were possible, by
 infecting the habit, to relieve a part; this reason-
 ing might be admissible; or if there were any
 other way of curing a chancre than by destroying

* Observations on the cure of Gonorrhœa, and some other effects of the Venereal Virus, second edit. page 37.

the local venereal action. If we were possessed of a topical remedy that would do this, we should as effectually cut off the source of infection, as if we destroyed the part by a caustic. But to me, the caution against drying up large spreading chancres by the application of mercurial ointment, or any other irritating or astringent substance, would appear unnecessary, as I never knew it succeed. But it is not my intention to interfere with practical remarks, unless immediately connected with the theory of the disease.

DR. Swediaur's "Practical Observations" are on a larger scale. This gentleman has been accused of anticipating some of Mr. Hunter's opinions, which he is said to have collected from his lectures. For the credit of his head as well as his heart, I trust the aspersions are not true. His general arrangement has some similarity to Mr. Hunter's, and some of the practical remarks are the same. But when Mr. Hunter had lectured for so many years, one may fairly suspect that his opinions were floating abroad, and imperfectly collected by many, who might be unable to trace the origin of their own ideas. Dr. Swediaur's theory, if it can be called such, is every where confused, often inconsistent with the laws of the animal economy, and the only passage in which I can trace any analogy with Mr. Hunter, is in his discrimination

tion between the two species of first local infection: Here if I could suspect him of any thing so disingenuous, I might fancy he had a confused notion of Mr. Hunter's opinions; in endeavouring to trace which he has fallen into an absurdity one could hardly suspect a man of common understanding could be guilty of. "In ninety-nine out of a hundred claps, perhaps, (says he) there is no such thing as an ulcer; but the disease is merely a superficial erysepelalous inflammation of the internal membrane of the urethra.*" Now, if the inflammation were erysepelalous, it would be extremely difficult to conceive how an ulcer could be prevented, in ninety-nine cases out of a hundred. The characteristic of the erysepelalous inflammation, next to its disposition to spread, is vesication. The first is quite contrary to the gonorrhæal inflammation, which is usually confined to a specific distance from the orifice of the urethra; and though the second is not a necessary attendant on erysepelas, yet if the inflammation were of that kind, ulceration from the broken vesicles would be a very frequent, instead of uncommon consequence. To this passage we have a note, to teach us that the author's friend, Dr. Stoll, of Vienna, opened a person who died whilst afflicted with blenorrhagia. Mr. Hunter, it is well

* Practical Observations on the Venereal Disease, by F. Swediaur, third edit. page 23.

known, more than forty years ago, opened the urethra of two men who were hanged under the same circumstances.

“BUT,” continues Dr. Swediaur, “the reason
 “ why claps do not, like chancres, constantly pro-
 “ duce lues, is that most of them, if not ill treated,
 “ excite only a superficial inflammation in the in-
 “ ternal membrane of the *urethra*, without any
 “ ulceration. Hence absorption cannot easily
 “ take place, the poison being out of the course
 “ of the circulation.*” It is not difficult to give
 this or any other reason to that part of the public
 who never heard of absorbing vessels, and who
 may perhaps fancy they are the same as the blood
 vessels. But a medical man will ask what neces-
 sity there is that red blood should circulate in a
 part, in order that absorption should take place.
 He will also recollect, that so well is the urethra
 supplied with absorbing vessels, that in a healthy
 state, almost all the mucus that is secreted, is ab-
 sorbed by them. But this author, like other men
 who reason without facts, is never at a loss, and
 furnishes us in the next page with another and
 still more absurd cause, why lues should rarely hap-
 pen from blennorrhagia. “As long,” says he,
 “as this mucus is secreted in such abundance, the

* Id. page 26.

“poison

“ poison is inveloped, the urethra defended, and
“ thus the formation of ulcers effectually prevent-
“ ed ; but if either from the violence of the irrita-
“ tion, or from any other cause, this secretion is
“ diminished, or if by improper injections the mu-
“ cus be washed away, while some of the poison
“ remains ; I am of opinion from more than twenty
“ instances which occurred to me, that in nine such
“ cases out of ten, an excoriation or ulceration of
“ the urethra, and subsequent pox will be as cer-
“ tainly the consequence as from venereal ulcers in
“ any other part of the body.”

THE last limb of this sentence is ingeniously made up of two inferences ; one of which would be incontrovertible, if the other were proved ; so that, when both are put together, some readers may fancy them both established. If we have venereal ulcers in the urethra, no one can doubt but they may as well infect the constitution, as those of any other part. The business, therefore, should be to prove the existence of ulcers from the causes here assigned, and not by a string of inferences, originating in an incomprehensible notion of washing the mucus away from the poison, to lead us to suppose every thing proved, when nothing is stated. If venereal ulcers were to exist but a short time in the urethra, without being attended to, they must, as far as we can judge by analogy

from other parts, perforate that thin membrane, and thus their existence would be established beyond dispute. But this error does not originate in the present passage. The reader has not forgotten Dr. Swediaur's ingenious reason, why gonorrhœa rarely infects the system—because, “where there is no ulcer, the poison is out of the circulation.” If, therefore, the constitution is admitted ever to be infected from gonorrhœa, it became absolutely necessary *to fabricate an ulcer* for that purpose, and the doctor, by the stroke of a pen, by developing the poison, while he enveloped a little sentence in a long paragraph, has taken the easiest method of accomplishing this necessary object.

IF we expect any better information from comparing this history of the disease with the method of cure, we shall find ourselves equally disappointed. Here we are told, “sometimes the poison seems to be of a more *exalted* *acrimony*, or rather meets with constitutions of a more irritable nature.” Is this loose manner of writing at all consistent with philosophy or common sense? Can it be a matter of such indifference, whether the cause of this irritation is from the acrimony of the poison, or the nature of the constitution, as that the expressions should be synonymous? This is the less excusable, because
the

the method of cure for the two is different; bark and opium being advised in one instance, and the procuring an increased discharge in the other.

As unsatisfactory is the reason given us why we have ulcers so much more frequently in the glans penis, than in the urethra. "If," says the author, "there was the same quantity of mucus between the prepuce and the glans, as there is in the cavity of the urethra, we should as seldom meet with ulcers there."* But this is so far from being the true state of the case, that in some countries circumcision has been found necessary to prevent the inconveniences arising from the great quantity of mucus collected on the glans, which even in colder climates, is oftentimes troublesome to men who live chaste.

AFTER some further arguments to show that the venereal virus may cause blenorrhagia, we are next led to what, if I understand the author, he seems to consider as a discovery of his own.† "I proceed," says he, "to the *main point*, viz. to prove that local inflammations of the urethra, accompanied with running, commonly called gonorrhœa, are not always, either in men or women, of a venereal nature. This idea first occurred to me from having frequently observed in stone-horses a running of this kind;" and also "in dogs more frequently."

* Swediaur, 27. † Id. 27.

It is remarkable that this same observation occurred to Dr. Bracken, about sixty years ago; and about the same time to the venerable Daniel Turner. Dr. Bracken, from observing this discharge in dogs and horses, draws a conclusion, that "gonorrhœa, or the venereal disease," as he styles it, "though so terrible to London apprentices, that many of our profession live very comfortably upon their gains from this single article, is more easy of cure than drawing of teeth and cutting of corns."* Daniel Turner too, in the fourth edition of his Syphilis, published sixty years ago, observes, "I well remember a lean cur, in the house I lived, that was always running after the salt and proud bitches in the streets; being frequently lost for a week together, would then come home with a dripping of purulent matter from his penis, which seemed also to be attended with a *stranguria*, by his perpetual *mixturitio*, observable in lifting up his leg (as the manner of the creature is) against the steps or door, when he could do nothing, or only a few drops; then presently lying down again, he would gently howl for a few minutes, &c.†

THESE extracts, concerning poor unfortunate falacious curs and stone horses, show, that though Dr.

* Bracken's Farriery, vol. 1.

† Syphilis, page 5.

Swediaur can trace the history of his own ideas on the subject, he has not taken the trouble of tracing the history of the opinion. Without doubt we must admit his great accuracy in correcting, though perhaps not the first time, the errors of these sages. But still it seems odd he should think it so necessary to prove, that in men a discharge from the urethra, with inflammation, is not always venereal. When did he ever see a stricture without such a discharge? Did he never read Tissot? But even his own remarks, extracted from Astruc and others, of the *Appegalle* and *the brenning of the pyntal*, before the venereal was known; or if these authorities were doubtful, the running of the reins, mentioned in the Levitical law, might have satisfied his doubts, saved him much trouble, and a painful operation, he thought it necessary to perform on himself. Every one who reads the description of his sufferings on this occasion* will regret much his oversight, and will be ready to excuse the pique he seems to feel at being so little regarded. "These facts," says he, "which I do not

* "IT [a solution of caustic alkali] occasioned a more severe pain than I ever remembered, yet I retained it for very near the space of a minute; when the pain became so excruciating, that I could bear it no longer. I therefore withdrew the syringe.—I lay down on my sofa, and waited the event with patience; but so violent was the pain, that it was near an hour before I was able to move." Swed. Page 39.

“recollect of having been ascertained by any writer
 “before or after my publication, seem to me of
 “great importance to mankind in general.*” We
 cannot therefore wonder, if the Jewish legislator
 was so attentive to them.†

ALL these remarks, which the candid practitioner will allow are not only just but obvious, occur in the space of three or four pages. It would be tedious to pursue the subject through the whole volume; which, in almost every part, where the author undertakes to assign causes, is hardly less exceptionable. In the beginning of this work, I took notice of two other passages which mark the hastiness of this writer, and shall now conclude, for the present, with his observations on the nature of the virus and its remedy. “The
 “smallest particle of this poison is sufficient to bring
 “on the most violent disorder over the whole body.
 “It seems to spread and diffuse itself by a kind of
 “fermentation and assimilation of matter; and,
 “like other contagions, it requires some time, after
 “being applied to the human body, before it pro-
 “duces that effect. It is not known whether it has
 “different degrees of acrimony and volatility, or
 “whether it is always the same in its nature, varying
 “only with regard to the particular part to which it

* Id. page 43.

† See Leviticus, chap. 15 and 22.

“ is applied, or according to the different habit and
 “ constitution, or particular idiosyncrasy of the per-
 “ son who receives the infection. We know that
 “ mercury possesses a certain and specific power of
 “ destroying the venereal virus: but we are quite
 “ uncertain whether it acts by its sedative, astringent,
 “ or evacuant quality; or if not, perhaps rather by
 “ a chemical elective attraction, whereby both sub-
 “ stances uniting with one another, are changed into
 “ a third, which is no more hurtful, but has some
 “ new properties entirely distinct from those which
 “ any of them had before they were united.”*

THIS is driving post haste, it must be acknow-
 ledged; but I cannot help arresting the reader to
 ask if he knows *quid vult aut num quid*, this “ dif-
 “ fusing itself like a fermentation,” these “ de-
 “ grees of acrimony or volatility,” or whether ei-
 “ ther of them exist. Respecting the sedative astringent
 or evacuant qualities of mercury, some would wish
 to ask why other sedatives, astringents, or evacuants
 are not equally successful; and lastly, as to chemi-
 cal attraction, one should at least wonder if that
 had any thing to do with the cure; why ever so
 large a quantity of mercury, when it does not pro-
 duce a certain effect on the constitution, very dif-
 ferent from either sedative or astringent, or most

* Swediaur, page 2.

other evacuants is not equally successful in forming this innocent *tertium quid*.

I COME now to consider Mr. Hunter's treatise on this disease, which, while it contains every practical lesson, guards the reader against all the objections that may be made against the theory, and contains no reasoning but what is perfectly consistent with the well known laws of the animal economy, is yet for several reasons less popular than many lighter performances. As I before observed, to comprehend the demonstration of a proposition* requires a greater effort of the mind, than to admit what to a superficial enquirer appears like the solution of a difficulty. Thus, though nothing like fermentation has ever been traced in the living body, and assimilation is only a name for a process which ought to be described, yet too many readers will prefer such a hasty manner of escaping difficulties to the laborious task of following a true philosopher, while he traces those facts, their series and order, by which, and by which only we can detect the law that governs them.

ANOTHER great disadvantage in Mr. Hunter's book, is the awkwardness of his style. From the habit of reading little, and in some measure from

* See page 29.

the necessity of using a new language to explain theories entirely new, his expressions are often unintelligible to those who had not the advantage of having heard his course of lectures. Of this it is well known he was so sensible, as to request the assistance of his friends before he offered so large a work to the public. It is a general remark, that it is much easier to write one's self than to correct another. Had Mr. Hunter given his opinions to either of his coadjutors, and requested him to prepare them for the world, there is no doubt but we should have had a complete performance. Either of the gentlemen was more than equal to the task, and one in particular has given the world many proofs of the closeness of his reasoning and the fluency and correctness of his style. But this style could never accommodate itself to the author's, and where the manner of reasoning was original, it might perhaps have been better to have left the mode of expressing it to the inventor. If the object was to enable those to comprehend the book who were ignorant of Mr. Hunter's opinions, the men selected should have been such as had been less accustomed to his conversation: we should not then have had an introduction of only seven pages, in which it is attempted to explain Mr. Hunter's peculiar opinions on the animal economy, as referred to in the work. This short introduction, while it may be sufficient for those who know the
opinions

opinions before hand, is not only unequal to the talk of explaining them to others, but from its brevity, cramps the author in many passages, by refusing him the full scope of a language which only could express his sentiments. The consequence is, obscurity in many parts, much increased no doubt by the different lights in which three men, under different impressions, might view the same passage.

BUT the greatest inconvenience attending the work is its size. By including such a variety of practical remarks, many of which are familiar to every medical reader, the attention, which is apt to be equally interested by every part of a long book, grows weary before it arrives at the most important passages. Hence, theories entirely new, yet founded on the most accurate observation, and by which alone the phænomena of the disease are to be accounted for, are read with as much indifference as recipes for a swelled testicle, or the manner of treating a bubo.

IT is not easy to determine what the opinions entertained of the discharge in gonorrhœa were, before the dissections made by Mr. Hunter in the year 1753. The name gives us reason to suppose the ancients considered it as the seminal liquor; and Astruc, taking it for granted that such was the case, determines, that the seat of the disease must

in consequence be in the vesiculæ feminales, the prostate and Cowper's glands; another instance of the inconvenience arising from ill defined terms.

IN a translation of Boerhaave's Academical Lectures on the Venereal Disease, by Mr. Wathen, dated 1763, is a passage which might lead us to suppose Mr. Hunter's discovery as old as the time of that professor. The words are, "Neither is it
" [gonorrhœa] a natural secretion, but a preterna-
" tural discharge of a singular kind, at first neither
" excoriating nor eroding the parts, *for in men who*
" *have died suddenly during this disorder, no ulcers*
" *were perceived on dissection."*

As Boerhaave died in the year 1739, it became a matter of historical enquiry, *when* these posthumous lectures, of which I had only a translation, were published. For this purpose I applied to Mr. Wathen, mentioning the object I had in view. The present deranged state of his library preventing his having access to the original, he very kindly satisfied my enquiries as far as memory could instruct him; the result of which was, that the date was very little antecedent to that of his translation. But the resources I derived from Dr. Sims's collection, put the matter beyond all doubt. In an edition published at Gottingen, in the year 1751, (two years before Mr. Hunter's discovery)

discovery) I find the ulcers of the urethra traced with an exactness which men usually assume when without the incumbrance of facts they describe a creature of their own imagination.* If there were any reason to suspect Boerhaave the author of this work, some apology might be necessary for thus treating so respectable a name; but the work alluded to is printed after his death, and has not even a name of any kind except the bookseller's, to sanction or introduce it to the world.

THE pamphlet of Gataker, whom Mr. Hunter accuses of publishing a fact discovered by himself,† is now before me. It is dated in the year 1754, and written in an easy epistolary style, addressed to a young surgeon. After showing the improbability of so much matter being furnished by an ulcer in

* Si inveteraverit, substantia cellulosa *semper* læsa erit. Ex ea vero gonorrhœa neglecta suppressa, male tractata vitium proximum est quod ex dilatatione rheumatica, *ut recte* vocatur, gonorrhœa simplicis *ulcera* in minimis lacunarum osculis hæreant et osculorum corruptione existant *ulcuscula*. Duplex est urethræ membrana quorum oscula inter duplicaturam membranæ decurrunt; hinc virus lento gradu primam membranam corrumpit donec secunda quoque erosa lamella per cellulofam urethræ substantiam se diffundat et eam expandat.

H. Boerhaave Præl. Academ. de L. V. Gottingæ apud Schmidt. 1751, LXX.

† Hunter's Treatise, page 20.

the urethra, he illustrates the idea of mucus by the increased discharge from the nose in catarrh. The internal membrane of the eye-lid is also said to have frequently a purulent as well as watery discharge without ulceration. That the opinion was then new is probable, by the answer published the following year by "George Key, surgeon, author of a "Dissertation on the Effects of Mercury on Human Bodies in the Cure of the Venereal Disease." This performance bears no kind of comparison with Gataker's, in style or argument, particularly the former. But the principal hold the author has of Gataker is in his illustrations. Had the latter been ingenuous enough to acknowledge the sources of his information, in the dissections, by Mr. Hunter, he must at once have silenced his antagonist, by a fact which could not be contradicted. But by withholding this, and incumbering himself with the term *mucus* for a substance which had all the properties of *pus*, he gives Key a double advantage over him. First, it is insisted that the discharge from the nose is in all respects different from that of the urethra; that it is therefore unfair to form any analogy between a diseased part that affords a substance having all the properties of *pus*, and one from which nothing but *mucus* ever flows. That in the eye-lid the case is different, the thick matter there found must be admitted to have all the properties of true *pus*, and that whenever it appears,

an ulcer always attends it. That a physician whom he well knew, (but whom he is of course under no obligation to name, as Gataker's authorities were nameless) had opened the urethra of several persons who died of fevers while under gonorrhœa, and in all had found ulcers. It is very remarkable how strong the attachment remains through life to words and definitions. In this instance the matter from the urethra is allowed to have all the properties of pus, and by most authors it is called purulent. Dr. Swediaur fancies himself extremely ingenious in coining a new word, *puriform*. The same may be said of the fistula lachrymalis, the disease probably referred to by Gataker and Key. The discharge from this was admitted to be *pus*, till it was discovered that no breach of the solids existed, after which it became mucus. Thus, as Mr. Foot observes, the "definition of the discharge proves the fact;" or, in other words, we have no better proof of the fact than our own definition. But it is the business of philosophy to admit no distinctions without real differences. The discharge from ulcers varies as much as that from the urethra, in colour, consistence, and quantity, and under the same circumstances. When the inflammation is great, the discharge in each is thinner and in greater quantity: as the inflammation subsides, and the parts show a disposition to return to their natural state,

state, the quantity lessens; it grows thicker, and at length ceases.

THE irritation produced by the application of venereal matter produces, like all other irritations, increased action in the neighbouring vessels. If it happens on a non-secreting surface, or one covered with a cuticle, the effect will usually be a collection of a thin fluid under the cuticle, forming a pustule. If this rests any time the consequence will be a loss of substance underneath, from the pressure of the fluid, and when the cuticle breaks an ulcer appears. But if the matter is applied to a secreting surface without a cuticle, or where the cuticle is so thin as to make no resistance, an increased and altered secretion follows. One should suspect this would oftener be the case on the glans; but here the cuticle is somewhat thicker, by which means a fluid is collected under it, a pustule produced, and a consequent ulcer or chancre formed.

It is probable the same variety of consequences follow when variolous or the matter of other morbid poisons is applied to, or affects secreting, or non-secreting surfaces. We well know in the small-pox eruption, instead of pustules in the mouth, we have commonly a salivation, and Dr. Mead tells us of a female convict, to whom he applied variolous pus, after the Chinese manner, up the nostrils.

nostrils. This girl had the fever and eruption, like the other convicts who were inoculated in the usual way, “ but suffered much more than the rest, “ being immediately after the poison was applied, “ miserably tormented with sharp pains in her head, “ and a fever, which never left her till the eruptions “ appeared.” Here we find symptoms in many respects different, from the different structure and function of the parts; viz. pain coming on immediately on the application of the pus, and no account of a pustule on the part inoculated.* I have not, however, been able to trace in a single author this cause of the different appearances of the disease from the nature of the parts before Mr. Hunter’s time, if we except Dr. Swediaur, and how far he can be said to have succeeded, I leave the reader to judge.

BUT, though this is a means of ascertaining the manner in which the same irritation *may* produce two different effects, it does not determine that the two diseases *are* the same. I shall not recapitulate all Mr. Hunter’s arguments on the subject,

* FOR this experiment we are indebted to Dr. Mead. It seems to have made no part of the original intention of the legislature, and was not attended to by Maitland, who published an authenticated account of the other six criminals, on whom the practice was tried. See Mead on the Small-pox, page 83.

till

till I come to consider Mr. Bell's objections. Nor is it necessary to say more than what I have anticipated on the nature of the venereal poison, and the manner in which it produces its effects: let me only in general observe, that all the arguments are drawn from those actions we see constantly going forward in the animal œconomy; and although the cause of these is beyond our researches, we can trace the laws by which many of them are governed; the causes that impede, and the means of restoring them.

FROM this manner of explaining the difference between the two diseases, it at once appears why gonorrhœa should cure itself, and chancre require the assistance of art. In the former case an irritation is excited, which, producing no alteration in the structure of the parts, will gradually subside, as they become familiar with, and insensible to, the irritating cause. The mode and time of this must, however, depend on the irritability of the constitution, and its susceptibility of the virus. Thus it is usually observed to be the most violent the first time, or when the parts are the most susceptible of the irritation from being unaccustomed to it. But in chancre the case is different; a breach is made in the solid parts, which cannot be healed, while the venereal action remains. The property of this action is to produce other

venereal matter, which irritates the sides of the ulcer, and produces the same action on parts not before irritated. Hence the spread of the ulcer, till the application of such a remedy, as by producing a new irritation alters the venereal action. But this new irritation arising from mercury is different from the former, in the very circumstance that renders the venereal permanent; namely, that the irritation is kept up by a substance which no action of the vessels can generate. But this has been already taken notice of, when the actions induced by mercury was considered.

THOUGH Mr. Hunter does not expressly remark that chancre heals without granulation, yet we may, I think, without straining his language; infer, that he had observed what I have stated as the law in morbid poisons, viz. that when a loss of substance is induced, the parts are skinned over, without granulation; or if the granulating process is attempted, that it is invariably unsuccessful. His words are, “Chancres, after having their
 “venereal taint corrected often become sta-
 “tionary—when they become stationary only,
 “they may often be cured by touching them
 “slightly with lunar caustic. They seem to re-
 “quire that the surface which had been conta-
 “minated, or the *new flesh which grew upon that*
 “*surface should be either destroyed or altered* before
 “it can cicatrize; and it is surprizing often how
 “fast

“fast they will heal after being touched, and pro-
 “bably once or twice may be sufficient.”* ‘Tis
 curious to observe the caution of this accurate
 observer: As if not having perfectly made up his
 mind on the subject, but satisfied that the new
 substance could not be called granulation, we find
 him speaking of it under the general terms of *new*
flesh, but such as could not cicatrise. With the
 same circumspection we shall find him describing
 the more common mode, in which chancres heal.
 “When the sore has put on a healthy look, when
 “the hard basis is become soft, and *it has skinned*
 “*over* kindly, it may be looked upon as cured.”
 “But, in very large chancres, it may not always
 “be necessary to continue the application of mer-
 “cury till the sore is healed; for the venereal
 “action is just as soon destroyed in a large
 “chancre, as in a small one; for every part of the
 “chancre being equally affected by mercury,
 “is equally easily cured. But the *skinning* is
 “different; for a large sore is longer in *skinning*
 “than a smaller one. A large chancre, there-
 “fore, may be deprived of its venereal action long
 “before it is *skinned* over; but a small one may
 “probably *skin* over before the venereal action is
 “entirely subdued.”† Here, though the precise

* Hunter's Treatise, page 231.

† Id. 240.

manner in which chancres heal, is not expressly pointed out; yet it must occur to every reader, that in describing the more common mode of healing, the terms granulation and cicatrization are cautiously avoided, and nothing is spoken of but skinning.

I HAVE already remarked, that to Mr. Hunter we owe the well-founded observation that secondary or constitutional symptoms will yield earlier to mercury than primary ones or chancres.* But what we are most of all indebted to him for, is the industry and accuracy with which he has traced the LAWS of the poison, and its remedy throughout all their stages local and constitutional. It is hardly credible how entirely these laws were overlooked till he undertook to inform the world. That the constitution was sometimes infected was well known, but this had produced no other reasoning, than that sufficient mercury had not been exhibited to *eradicate* some latent particles. Yet it could not but occur to every practitioner, that no quantity of mercury had in some instances been sufficient to prevent the re-appearance of the disease, while in others, nothing of the kind had happened, though the supposed imprudence of the patient had

* A lues venerea shall in many cases be perfectly cured before chancres have made the least change. Id. page 227.

prevented

prevented the continuance of the remedy as soon as the first symptoms had yielded to it. It will, however, be found, that every author of established reputation, has in some way admitted the difficulty of ascertaining when the disease is subdued: and if each had not unfortunately attempted to account for, before he traced the fact with philosophical exactness, it would not have remained for Mr. Hunter to confer this obligation on mankind.

HOFFMAN has left nothing expressly on the disease, but in several parts of his works we can see the very confused idea he entertained of it. In his experiments on mineral waters, as quoted by Dr. Swan, he imputes its return after being apparently subdued, to certain impurities in the blood, which are to be discharged by the glands of the skin. "We could," says he, "produce numerous instances, where, after mercurial salivations, the symptoms have abated for a season, but after a while returned with greater violence, because the taint was not discharged; but part remaining behind, gradually prevailed and acquired fresh force: But by nearly the same course of mercurials, and drying decoctions, together with a proper use of warm bathing, the cure has been completed, and the virulent matter discharged from its innermost seat of the nervous parts."* We shall hereafter

* Swan's Sydenham, page 321, note, second edition.

consider whether this second use of mercury, *after the symptoms showed themselves*, would not have been sufficient without the assistance of bathing.

It is much to be regretted that Sydenham, to whose accuracy we owe so much in other diseases, should have been so concise on this. Probably he had not the same opportunities of tracing its various stages. We can only collect from him that the distemper was too stubborn for the physicians of this country, which he imputes to the thickness of our atmosphere. But whether in this instance he spoke of the difficulty of curing the disease when it showed itself, or of preventing its return, does not appear with certainty.

THOUGH the lectures I have referred to, have so little title to be considered as Boerhaave's, they have preserved one part of his theory, namely, that the seat of the disease is in the fat, the whole of which must be dissolved before the patient can be cured.* Even then the author conceives it may lurk in the bones, from which mercury will be unable to dislodge it, for want of having that free circulation through them which the looser texture of the cellular membrane admits. He therefore conceives, that though mercury may occupy the cells

* Prefatio ad Luifini Aphrodisiacum. L. B. 1728.

emptied

emptied by the corruption of the cancellous and medullary fat, it will be necessary to use other remedies to expel the virus entirely. That this is his meaning is evident, by comparing the beginning of his preface to Luifinus's collection, with the passage quoted by Astruc. In the introductory part he acknowledges, that the most experienced physicians in all other diseases are mere tyros in this, and reproaches himself and them with the re-appearance of the disease in the throat, palate, nose, &c. after they conceived the virus conquered; confessing that they not only knew not when they were to expect such mischief, but how to prevent it.

ASTRUC, in his remarks on one of these passages, has no difficulty in showing that Boerhaave was mistaken in supposing mercury could not cure the disease in the bones. But for want of completely seeing the intention of the author, he confutes the theory, without attempting to remove the difficulty. Boerhaave's object was, to account for the disease appearing in the bones after it had been cured in the softer and more sanguiferous parts. By a mercurial friction he found, that he cured all the venereal blotches and ulcers that appeared, and prevented the return of the disease in these parts, but not in the bones. Hence he conceived, that mercury would cure not only the apparent but the latent mischief in the cellular membrane,

but not in the bones. That such was his idea, will appear more obviously when we attend to the distinction he makes between *lucm dominantem; et mala venerea quæ latitant.* Comparing the membranous structure of the urethra, on account of the small quantity of red blood that circulates through it, to the bones, he says mercury will not cure gonorrhœa, though it perfectly *lucm eo tempore dominantem tollat.* Such being the case with the *prædominant* stage of the disease, he infers the same may happen with the *latent*, that is, that though it may lurk in every part of the cellular membrane, while it appears only partially, yet mercury will entirely expel it, except in the bones, for the reason above mentioned. Unsatisfactory as this reasoning may be, we may collect the following remarks from it: First, That Boerhaave made a just distinction between the latent and the apparent disease, and even admitted the possibility of the one requiring a different treatment from the other: next, That he had discovered that the disease, when cured in the skin, never returned to it without a fresh infection; but that there was no certainty it might not show itself in the bones, the frequent occurrence of which induced him to look for other remedies besides mercury. However unnecessary this reasoning may be, it is a strong proof of the closeness of his observations, and his desire to be serviceable in the profession he was engaged in.

ASTRUC fancies he has removed all these difficulties, by proving that mercury will cure the disease in the bones, as well as in other parts. But he is not aware that Boerhaave's error is to be traced further back; and we shall presently find that when Astruc attempts himself to account for the re-appearance of the disease, his theory is not more satisfactory than the former. He first proves it cannot be accounted for by supposing the poison deposited in certain cells; but conceives it may be readily explained by the manner in which the blood and humours are renewed. "Hence," says he, "the poison admitted may be sometimes
 " by degrees increased and multiplied, and some-
 " times by degrees decrease and disappear, and
 " sometimes keep such an even tenor, as constant-
 " ly to be renewed in the same degree."

" BUT to this purpose it is requisite that the
 " poison, which is admitted or left behind, should
 " hold such measures in quantity and force, and
 " the blood likewise keep such a temper in qua-
 " lity, and the manner of its generation, as to
 " permit a renewal of the poison, but such a re-
 " newal as is constantly one and the same, with-
 " out addition or diminution. For otherwise, if
 " the quality of the blood be vitiated by a fever,
 " or any adventitious disease, if by errors in diet,
 " immoderate watchfulness, drunkenness, &c.
 " then

“ then by the same means as the blood departs
 “ from its natural disposition, the before latent
 “ poison will presently gain ground, both in quan-
 “ tity and strength, be restored to its natural
 “ fierceness, and, like another *Pandora's* box
 “ opened, bring on a terrible troop of grievous
 “ symptoms, which will end in a manifest *lues*.”*

THOUGH this theory cannot be satisfactory to any reader of the present day, yet it certainly redounds much to the credit of this as well as the former author, that he acknowledges himself incapable of knowing when to expect the re-appearance of the disease, or how to prevent it.

DANIEL Turner, who, like a modern exhibitor, always appears in the character of a divine and moral philosopher, as well as a physician, acknowledges the great difficulty of ascertaining when a person is cured. “ Nor,” says he, “ can any man have any other security than the absence of all the symptoms, which continuing through a double solstice, it is to be hoped he will hear no more thereof.” He concludes, therefore, with advising men to chastity, in the words of a *royal apothegmatist*, as the only certain preventive of a disease, the cure of which is so uncertain.

CHAPMAN gets rid of the question like too many other writers, by ascribing the secondary symp-

* Barrowby's Translation, vol. 1. page 159.

toms to the neglect or ill treatment of the primary ones, without attempting to teach us how we are to prevent them. Of all the arts of medical quackery, this is the most disgraceful. If indeed there were a single pathological fact that could be brought to mathematical precision, there might be some apology for this easy method of reducing things to a certainty, or reasoning upon them as such. But lest my reader should think me too severe on one no longer able to defend himself, let me transcribe his own words. "A virulent gonorrhœa, *taken in time, and properly treated, never degenerates into a confirmed lues, so long as the running continues to flow freely, nevertheless it must be observed, that how copious soever the discharge may be, if the disorder be totally neglected, nothing can hinder some portions of the infectious particles from being taken up by the absorbent lymphatics, and conveyed into the blood.*"

DR. Swediaur, after exhibiting one of his usual flourishes, concludes with acknowledging his incapacity to determine when the lues is *radically exterminated*. "To know whether the lues is radically exterminated, is a nice point of practical judgement: and if I say that from a carelessness on the part of the patient, or from want of knowledge on the part of the practitioner with regard to this point, a great many patients are unhappy and sufferers,

* Chapman's Treatise on the Venereal Disease, p. 212. & Seq.

" I ad-

" I advance nothing but what we see daily con-
 " firmed. If we were in possession of a remedy,
 " which having the power of rendering the least
 " particle of the venereal virus concealed in the
 " body active, and thus enable us to discover its
 " presence, in like manner as the loadstone disco-
 " vers the presence of iron, there would be no-
 " thing necessary but to administer that remedy the
 " moment we think the patient had taken mercury
 " enough. I have made some experiments on this
 " subject; but the number of facts are not yet suf-
 " ficient to enable me to form a conclusion."

WHEN we hear of *radical* extermination, I wish
 not to be accused of a useless scrupulosity in asking
 what we mean by the *root* of the disease. It may
 be answered, that though the root of a disease is
 merely a metaphor, it is what every body well
 understands. I answer, it is what nobody under-
 stands, and the metaphor, whether it leads us astray
 or not, at least supercedes our enquiry. We have
 got an expression which serves our purpose, and
 we fancy that we have described a thing as it is.
 But the author proceeds by another metaphor, of
 the magnet, to acknowledge, that no means has
 hitherto been discovered of knowing when a pa-
 tient is free from syphilis, and concludes with ob-
 serving, that he has made some experiments on
 the subject. Now, what is meant by experiments

* Swediaur, page 169.

here?

here? If he had acknowledged, that in spite of the closest observation of the phenomena of those cases which had come before him, he was still unable to determine when he might ascertain that a patient is free from syphilis, we should have understood him, and the enquiry would still have been open. But it will be said, Dr. Swediaur admits this. I acknowledge he does; but it is in such a manner as shows he had not arranged his own ideas before he gave them to the public; and that this was the case, appears by another and similar passage.* “As soon as the mercury affects the
 “mouth, we are sure of the most essential point,
 “viz. of its having entered the mass, which, as was
 “before observed, is a point absolutely necessary
 “for eradicating] the] poison.—“*Of its having*
 “*entered the mass.*” “If that were the point
 to be ascertained, we need only observe, whether
 it is absorbed during friction, or in giving it by the
 mouth, whether it runs off by stool; and if the
 business were to destroy the *root* of the disease,
 and mercury had that property, its entering the
 mass would be sufficient. But the author is well
 aware this is not enough, for we find him, imme-
 diately after, using a different language. “The
 “disappearing of internal venereal symptoms, and
 “more so that of external ones, is another not un-
 “equivocal sign that mercury has exerted its action
 “on the venereal virus. If venereal ulcers, which

* Id. page 167.

“arose

“ arose from an infected mass, begin to mend or
 “ heal ; if pains or tophus’s of the bones begin to
 “ disappear, &c. under the use of mercury, we
 “ are sure of its having entered the mass, and
 “ removed the effects of the venereal poison, but
 “ we are not sure of its having eradicated entirely
 “ all the poison in the body.” In these two sen-
 tences mercury is said first to have exerted its
 effects *on the venereal virus* ; secondly, to have
 removed the effects *of the venereal virus* ; and
 thirdly, to act by *rooting the poison out of the body*.
 And all this the consequence of the first error, viz.
 ascribing that to the action of mercury on the *poi-
 son*, which ought to be ascribed to its action on the
 constitution. But all my present business is to
 show that though Dr. Swediaur conceives it possible,
 by a long-continued use of mercury, to *eradicate* the
 disease, as he calls it, or to prevent the re-appear-
 ance of symptoms after they have been sub-
 dued ; yet he acknowledges all his observations
 on the subject have hitherto proved unsatisfactory.

MR. Moore, having fewer scruples, is much
 more decided in his language, but not in the
 least more accurate in the arrangement of his
 ideas. “ It appears to me,” says he, “ that the
 “ few cases where the venereal disease returns,
 “ after a thorough course of mercury, may
 “ be

“ be accounted for in a much more natural manner,
 “ namely, that the mercury was stopped before
 “ all the virus was thrown out of the excretories,
 “ which it would *certainly* have been, and the dis-
 “ ease quite extirpated, if the medicine had been
 “ continued longer.”* This mode of reducing
 things to a *certainly*, is certainly very agreeable
 to those who are not fond of close reasoning. Such
 may fancy they can form an idea of virus circulat-
 ing and remaining active for any indefinite time.
 As to the *extirpation* of the disease, it is only ano-
 ther branch of the eradicating and exterminating
 theory.

MR. Jesse Foot, whom I shall hereafter have
 occasion to consider more particularly, observes,
 “ that if he knew, or could by any divine intuition
 “ know, of a venereal disposition being formed,
 “ he could destroy it by mercury.” This is admit-
 ting all that any one can desire, viz. that the disease
 may appear after he conceives it cured; so that he
 knows not how to ascertain when the patient is safe.
 But, by his lectures, published a few years after,
 it appears that he has been favoured with *this di-
 vine intuition*. His language is a little confused;
 but if I understand him, his object is to teach us
 when the constitution is safe. “ If,” says he, “ by
 “ embracing the part which was the seat of the

* Essay on Mat. Med. page 275.

“ chancre,

“chancre, the *appearance be thin*, so that the fin-
 “ger 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 will some-
 “times heal, and yet the virus will not be extin-
 “guished. It must always be remembered, that
 “the constitution may be hereafter infected, in
 “consequence of that original inattention and es-
 “cape of that original infection into the consti-
 “tution.”*—In a future edition, perhaps, Mr.
 Foot may explain his meaning; but, at present,
 like other recent divine intuitions, there is a mix-
 ture of mystery in this business inscrutable to pro-
 fane readers.

MR. Bell has not thought this peculiarity of
 the disease worth his notice.

IT may be said, that as it is well known mer-
 cury cures the disease, it is of little consequence to
 enquire whether it is by eradication, or by what
 other means. I answer, if this were uniformly the
 case in every instance, there might be some apology
 for such indolence. But it may be fairly traced from
 the acknowledgment of every writer, and every can-

* Lectures, page 413.

did practitioner will admit, that the disease does sometimes show itself a second, and even a third time, though in a different form, after the most careful exhibition of mercury: it surely then becomes us to trace the progress of the disease and remedy. By such means only can we expect to correct our practice so as to prevent the re-appearance of the disease; or if that cannot be done, to regulate our treatment according to laws which can only be detected by tracing the actions induced by the virus and its antidote. It is the neglect of this that has led us to talk of the poison floating in the blood for years, and then showing itself, as it were by accident. Though nothing can be more absurd than such an idea, to those who know that the blood, juices, and even the solids, are in a perpetual state of renewal; and also that the most deleterious substances may, by constant application, be familiarised to the constitution: yet either this must be admitted, or that during the existence of the first local disease, some parts were poisoned from the matter absorbed, and did not show the disease till afterwards.

THIS last is analogous to what is observed of other morbid poisons, which require a certain time after the application of infectious matter before the action excited by them can take place. The enquiry then will be—Why were these parts not
Q cured

cured with the first, which had been longest infected; and whether an additional quantity of mercury, given before the appearance of secondary symptoms, would have prevented them?

It cannot be still necessary to remark, that many of our physiological errors arise from the looseness or figurative style of our language. We talk of the extirpation, extermination, and eradication of a poison, till at last we reason upon it as if there were really roots which we were to destroy, or at least a certain quantity of some substance, which we have the power of discharging from the system. But what are the proofs of it? If the operation of mercury were to discharge the virus from the body, and secondary symptoms arose from particles of virus, which gradually multiplied till the disease becomes apparent, the inference must be, that these particles will be with greater difficulty discharged, or the disease be with greater difficulty eradicated, in proportion as a part has been longest under its influence. Hence we should conclude, that the quantity of mercury sufficient to cure the primary symptoms would be more than sufficient to discharge the virus deposited in parts so slightly infected as not to have shown the disease. But the concurrent testimony of the best authors goes to prove, first, That the highest degree of mercurial irritation, or the fullest exhibition of
that

that remedy, is no certain security against the secondary symptoms of the disease. Secondly, These secondary symptoms appear not in the genitals, the parts first infected, but on the skin, throat, or bones, where particles of virus must have been deposited later than on the genitals, and from which they should consequently be more readily removed. Thirdly, when the disease does appear in its secondary stage, it is more readily cured than the primary ones were; or if the two forms of the disease are present together, the secondary symptoms yield earlier to mercury than primary ones.* The first and second of these propositions are admitted by all authors; if the third is not taken notice of by any one but Mr. Hunter, it arises from the very high degree of mercurial irritation which it has been thought necessary to excite, in order to extirpate the disease after its second appearance. The fact, however, has been traced in the secondary symptoms of some other morbid poisons, and is not less certain in the venereal. But without insisting on this for the present, we may, from the first and second propositions, draw the following inferences: First, That mercury cures every form of the disease obvious to our senses; Secondly, That it does not always prevent

* In this statement it is hardly necessary to say, the bones, from the slowness of all their actions, cannot be included.

the disease appearing in a part, which it readily cures when the diseased action shows itself.

HENCE we may conclude, that mercury will cure those symptoms which it will not prevent, or that the disease must have made a certain progress before the remedy will cure it.—I shall now endeavour to shew that this property of mercury may be traced in the cure of other diseases arising from morbid poisons, and that mercury is not the only remedy that will prevent the return of a disease which it will cure when it does return. When we take a view of those morbid poisons whose laws we are acquainted with, we shall not find any of them exactly similar to the venereal; most of them produce a paroxysm of fever, which, if the constitution can struggle through, subsides of itself, and all the symptoms with it. We have, however, traced some poisons which have many symptoms very similar to the venereal, and which yield to mercury precisely in the manner I have described the latter. But none of these occur frequently enough to be accurately traced, except yaws. This is a disease arising from a morbid poison, the effects of which gradually subside; but mercury produces a considerable effect on it. If given in any stage it will often suspend the disease; and if its exhibition is delayed till the disease has produced its full effect, it will facilitate and hasten

its

its cure. "If you salivate your patient before the yaws are at their height," says the author quoted above, "the best that you can expect is, their appearing again soon after the salivation is over."* By this it appears that mercury will suspend the action of the disease at any time; but that it will not cure it till it is arrived at a certain stage. This is analogous in many respects to what is well known of the venereal. The secondary symptoms never appear while the constitution is under the mercurial irritation. They are, however, only suspended; for, if the parts are contaminated, soon after the mercurial irritation is over the disease will appear, and in this stage it will readily yield to mercury.

THE intermittent fever is the effect of a poison of some kind, though it does not produce any visible action but on the whole constitution. Bark is its well known remedy. But it is not yet a determined point, that it will prevent the action, after the disposition is given. † Before the
ague

* Ed. Med. Essays, vol. vi. p. 319, Edit. 1771.

† A space of time intervenes, various indeed, according to circumstances, but always such as gives room to believe that the cause requires, and actually undergoes a modification, before it is capable of producing a fever or a paroxysm of a fever. The circumstances connected with the approach of fevers, particularly the cause of the disease, so far from producing the fever immediately when applied to the body, often lurks for a considerable time in the constitution, with-

ague shows itself by a regular fit, we have frequently anomalous symptoms, under which it is thought by some not advisable to begin the bark; but if it is begun, we do not prevent the ague; the most we can do is to render the paroxysm more regular. At this time we may say, the ague is formed, or its action has begun; and in this state the bark will cure it. But if the ague has been of long continuance, the disposition is more fixed; and though the action may be stopped, the disposition will remain; and usually on the eighth or tenth day, after being apparently cured, commence its action with as much regularity as before. To prevent this the most experienced practitioners find it advisable, after continuing the bark previous to the period of two succeeding paroxysms, to intermit the use of it for four or five days.* This brings

them

out perceptibly injuring the ordinary actions of life. Sometimes it gives rise to affections which are apparently very different from their nature. Thus a person often languishes for days, weeks, or even longer. The indisposition suddenly vanishes, and the apparent recovery from health is soon followed by a paroxysm of regular fever.

Jackson on the Fevers of Jamaica, p. 134.

BLEEDING was often found useful in particular cases.—It seemed not to be without effect in removing a certain state of the system which resisted the successful operation of the bark. *Id.* Page 314.

* THE bark is to be continued during the time the subsequent paroxysm should have continued, and then is to be

bc

them exactly to the time above mentioned. By this intermission not only is the patient relieved of swallowing what would be useless in the intermediate time, but the constitution is rendered more susceptible of the effects of the bark. The quantity taken at this latter period seldom fails to prevent the paroxysm; whereas the same and much more, taken six days earlier, would have produced no effect whatever. This is the usual course of intermittents in Great Britain; but I need not add there is an endless variety of them. In warm climates the disposition is sometimes so strong, that bark will only cure or suspend the action till a change of season removes the disposition. Dr. Jackson, the elegant writer and accurate observer above alluded to, makes this remark of the intermittents of America.

“ It is universally known, that the powers of
“ bark seldom fail in the cure of intermitting fe-
“ vers, when given in sufficient quantity; yet I
“ must also observe, that its virtues do not extend
“ farther than to a temporary suspension of the

be repeated in the same quantity and manner, especially if any symptoms of the fit should have recurred, provided that the paroxysm has been greatly lessened. The same measures are to be pursued in the third period. Afterwards the medicine is to be omitted for four or five days, and then repeated for twenty-four hours.—See also Dr. Jackson.

paroxysms

“ paroxyfms. That bark does not *eliminate* or
 “ *destroy the actual* cause of the disease, appears
 “ plainly from this fact, that relapses are frequently
 “ the consequences of those circumstances which
 “ occasion debility, or which counteract the effects
 “ of this tonic remedy. To which we may add,
 “ that though *relapses* are often of a *different type*
 “ from the original fever, yet as they generally
 “ happen on an even day from the suppression of
 “ the paroxyfm, there would be little room to doubt
 “ that the old complaint again resumes its course,
 “ though it probably in the mean time *loses several*
 “ of its *original symptoms*. It is a fact likewise
 “ which we ought not to omit mentioning, but
 “ which in general does not seem to be much at-
 “ tended to, that some periods are more remarka-
 “ ble for the relapse of intermitting fevers than
 “ others. I observed before, that relapses almost
 “ constantly happened on the even days. I now
 “ add, that the most remarkable of these days are
 “ the sixth, eighth, twelfth, fourteenth, twentieth,
 “ twenty-second, twenty-eighth, and thirtieth. The
 “ fourteenth is remarkable for relapses above all
 “ the others; next to it we may rank the twelfth,
 “ twentieth, and twenty-second; unless in times of
 “ very prevailing sickness, where the sixth and
 “ eighth often come in for a great share. If we
 “ take pains to examine the particular circum-
 “ stances of the patient, and attend to the nature
 “ and

and degree of the prevailing epidemic, we may be often enabled to form a tolerable conjecture with regard to the most probable period of return. Having therefore acquired from observation some general ideas of the different propensities to relapse, in different situations and in different subjects, I usually began to give the bark in quantity, and to use other precautions, on the fifth day after the suppression of the paroxysm, in cases where there were the strongest suspicions of a speedy return; while this was delayed till the eleventh, nineteenth, or twenty-seventh, in others, in proportion to the different degrees of healthiness. This practice was continued for the space of three days, or till the suspicious period was past.*

THE fact, that bark does not eliminate, or, as we might as well say, exterminate the cause of the disease, is proved with much perspicuity by this accurate observer. But it supercedes its action, and the cause, whatever it is, being no longer present, the disease ceases. It is true, in these cases neither the form of the disease, nor manner of exhibiting the remedy, are the same as in the venereal. But the analogy holds good in the main point. The disease is cured for a time, is expected to return, and a perseverance in the remedy at the

* Jackson on the Fevers of Jamaica, p. 326.

time of cure is not sufficient to prevent that return. If it be said, that the bark is still exhibited before the diseased action shows itself, I answer, we have not yet ascertained at what time this action commences. All physicians allow before the shivering takes place; and Dr. Jackson, who experienced it in his own person, dates it earlier than the debility. But besides this, the manner of curing the ague in the first instance was during the absence of the paroxfym. It is precisely the same in the second. This author also remarks, that the disease on its return has lost many of its original symptoms. The same has been remarked of the secondary symptoms of the venereal.*

If in yaws the circumstances are not exactly similar to the secondary appearance of the lues, it must at least be acknowledged they bear the same analogy as the disease and remedy do to each other. In truth, there is no disease that exactly resembles the venereal in all its phenomena, and the manner of its cure. But it has, I think, been proved, that there are remedies which will cure diseased actions, and not cure dispositions to those actions. I trust also this difference will appear no way paradoxical, when it is admitted, that mercury never cures but when it excites a peculiar action in the constitution. This action, the testimony of our senses

* See page 104, and Appendix No. II.

flows

shows us, alters the venereal action, and that the two are never present at the same time. But though, as I before observed, we can ascertain the laws of the actions of this disease and remedy, our present knowledge does not authorize us to determine what their laws are before the action of one of them has become apparent. If it should be said, that what we call a disposition must either be an action, or else is nothing, I answer, this will not remove the difficulty; because, if it be an action, it is a different one from that which has been the subject of experiment, and therefore, as in the instance of the bark with some species of intermittents, or mercury in the earlier stages of the yaws, it is still such an action as we have not yet ascertained that mercury will cure: and as there is for the most part no action obvious to our senses, at least none of those local actions which afterwards show themselves in the skin, throat, and bones, it is surely justifiable to call the then state of the parts a *disposition to take on the diseased action.*

HAVING thus far, I hope, cleared the ground, I shall proceed to state Mr. Hunter's theory upon the disease. A theory which, for reasons I cannot pretend to determine, has been overlooked by many, and misunderstood by others, who seem to have attended to it. For this reason perhaps it may appear in a more striking light if we attend

to the arguments which have been brought against it by his antagonists. It has been said, that “if mercury, prior to the action, will not remove the disposition, what reason have we to give mercury internally, during a clap, bubo, or chancres?” * Without entering into the question, whether these latter symptoms can be cured without the internal use of mercury or mercurial frictions, I shall only answer in Mr. Hunter’s words, “Mercury hinders a disposition from forming, or in other words, prevents contamination.” ’Tis strange Dr. Swediaur, in his haste, should not see the difference between hindering a disposition from forming, whilst the virus is absorbed, and curing that disposition when it is formed. Nor is this peculiar to the venereal. During the prevalence of any epidemic, or of the ague, in humid situations, prudent people often escape the contagion by the frequent use of bark. If this is not uniformly successful, it is universally allowed, that those who have the advantage of better diet more commonly escape than their poorer neighbours. As I before observed, this is one of those opinions that can never be reduced to certainty; we can therefore only reason on the probability of it as far as inferences may be drawn from facts.

* Swediaur, p. 309. 3d edit.

BUT

BUT another and more common error in those who pay a proper respect to Mr. Hunter's opinions, is not distinguishing between the diseased action having begun in a part, and having produced its full effect in that part.

MR. MOORE, who writes much too rapidly to do justice either to his own abilities or the arguments of others*, runs bolder into this error than

* As a proof of this let me transcribe the following passage, which I the rather prefer, because it is not directed against Mr. Hunter.

“ IN all its qualities it [the gluten] very nearly resembles the solid matter of the body, while in other parts of the fluids there is no such resemblance.”*

“ HERE,” says Mr. Moore, “ Dr. Cullen seems to have forgotten, that there is in the serum a portion of matter which coagulates by heat, or by being mixed with ardent spirits, or spirits of wine, or essential oils, which when coagulated, resembles the solids as much as the gluten.” [To show how far Dr. Cullen forgot it, I shall continue the transcript a little lower than Mr. Moore has done.]

“ THEREFORE, this gluten we hold to be the chief part of the fluids; and considering how much of it is diffused among the other fluids, and how much of it is dissolved in the serum or serosity, it is certainly next to the water, the largest portion of the common mass.”

Moore's Essay on Mat. Med. page 41.

MR. MOORE, in his haste, has made no distinction between the fluids and the parts of the fluids; it is true there are obscurities in the subsequent passage in Cullen, but no man should be charged with more than he is guilty of.

* Cullen's Mat. Med. Vol. I. page 219.

most

most of Mr. Hunter's opponents. "It is evident," says he, "that mercury prevents the morbid effects of virus which is in the habit but not come into action. Indeed, if mercury had not this power it would be of little use. For if every particle of virus began to produce disease before the infection could be expelled, no man once infected could ever be cured. The constitution of the strongest would be ruined, and his life terminated before all the particles of virus absorbed from a chancre or bubo could successively come into action and be destroyed, by the same number of courses of mercury.*"

IN stating this *evident* proposition, either the theory of Mr. Hunter, or Mr. Moore's own should have been adhered to, because it certainly is *evident*, that if the opinions of the one are applied to the theories of the other, the conclusion must be unsatisfactory. But there is nothing in Mr. Hunter's book like the opinions here fastened upon him: nothing about "*particles of virus coming into action:*" or even "*particles of virus beginning to produce disease.*" When the venereal matter has got into the circulation, Mr. Hunter conceives, to use his own language, that it acts on all parts of the body with equal force. But the

* *Materia Medica*, page 274.

† See *Treatise*, page 307 & seq.

parts that receive the disposition, and afterwards come into action, he divides into two orders : First, the throat, fauces, and skin : Secondly, the bones. The order in which I have placed them is that which they usually follow ; but this is not constantly the case. Sometimes the throat shows the disease first, and is cured ; after which it appears in the skin. In this case the orders may be divided into three. The bones have also in a few instances shown the disease first. But when the diseased action has taken place in one order, the skin for instance, and has been cured, it never appears again in that order of parts from the same stock of infection. The analogy between the venereal and other morbid poisons is very remarkable in this instance. The part susceptible of the variolous irritation is the skin, or the cellular membrane immediately under it. And though we cannot prevent the action from taking place after the disposition is given, yet we can prevent the full action ; that is, by evacuants and exposure to cold we lessen the number of pustules that would otherwise appear ; yet the susceptibility for the disease is as completely destroyed as if the full effect had been produced. In the same manner, when in the first instance the chancre is cured by remedies applied to the constitution, the disease does not show itself on the genitals a second time from the same infection, but on the skin or fauces. If no mercury is exhibited,
the

the blotches will encrease in number, and ulcerate, but if the first are cured by mercury no fresh ones will appear. But if the bones have taken on the disposition, then, in a given time after the mercurial irritation which cured the skin has ceased, the disease will appear in one or more of them. If no attention is paid to these they will ulcerate, and other bones, if contaminated, will show the disease likewise: but if mercury is exhibited so as to cure the first, no others will ever show the disease from that stock of infection. Mr. Moore, therefore, has only mistaken *particles of virus* for orders of parts in an infected constitution. If Dr. Gilchrist's account be just, that the bones are insensible to the virus of siveens, we may easily account for the continuance of mercury producing a perfect cure of that disease. For if the remedy is not applied till the secondary ulcers have shown themselves, there is no other order of parts for the disease to attack.

THE theory I have now traced is in every respect formed on that mode of constituting an axiom which Sir Francis Bacon advises, which he acknowledges had not been attempted in his days, and which I will be bold to say has scarcely been attempted in pathology till Mr. Hunter's. We shall have occasion to illustrate it closer, by answering some further objections which have been
made

made to it. But as I trust every difficulty is now cleared, and every objection answered, as far as this stage of the argument produces either, I shall conclude with refreshing my reader's memory by a general recapitulation of the doctrine.

FIRST, That the matter which produces chancre may produce gonorrhœa only.

SECONDLY, That the matter absorbed from either circulates with the blood, and is thrown out by the common emunctories; but in its progress may contaminate other parts of the body, and give them a disposition to a disease different from the first.

THIRDLY, That when this disposition is given, the diseased action does not follow till a certain time, which varies according to the constitution, and other circumstances, but never happens while the constitution is under a mercurial irritation.

FOURTHLY, That when the disposition has taken place, the action may be suspended by mercury, but the disposition will remain, and the action show itself at some period after the mercurial irritation has ceased.

FIFTHLY, That when the action has begun in

an order of parts, it may be cured, and will not return in the part or the order of parts to which it belongs, from the same stock of infection. But

SIXTHLY, That the diseased action will take place in another order of parts, if that other order has been contaminated; and in this order it must be treated as in the former.

SEVENTHLY, That when the diseased action has taken place, and been cured in the part first infected, in the throat and fauces, the skin, and the bones or periosteum, the subject may be said to be free from the disease, as far as our knowledge has hitherto traced it.

EIGHTHLY, That the usual time of the skin or fauces taking on the diseased action is, on a medium, six weeks after the mercurial irritation that cured the first symptoms has subsided; in the bones about twice that time, but this period has varied like other morbid poisons, and apparently in a greater proportion: but this has not hitherto been accurately ascertained, especially when we take into account the proportionate ratio in the varieties of other morbid poisons.

I SHALL NOW take the liberty of starting an objection or two, not against Mr. Hunter's theory, because

because twelve years experience, and the concurrence of every practitioner who is well acquainted with the subject, and has had opportunity of reducing it to practice, is sufficient to satisfy me of its truth---but against a few concessions which the inventor seems to me to have made to former opinions. The first of these is, that mercury will not cure virulent gonorrhœa. That it will not stop the discharge I will readily allow; but cannot feel myself convinced that it will not alter the action of the parts so that it may be no longer virulent. Mr. Hunter's arguments on the subject are conclusive as to the first point, but the latter cannot be ascertained while there is so great an uncertainty of the nature of gonorrhœa. All I have to urge is, that mercury alters the secretions of other glands, and where it does not cure a venereal ulcer, it alters its action so far as to render it no longer venereal, for we find it may be cured by other remedies. This point does not therefore appear to me to be reduced to a law.

My next objection is to the number he supposes would receive the secondary disease, were it not prevented by the exhibition of mercury. I cannot help thinking these would be much fewer than he states them. I have before observed how many we see in hospitals who must have laboured for a great length of time under the local disease, yet

show no constitutional symptoms. In private practice those on whom they appear have usually attended very early to the disease. One remarkable instance Mr. Cline favours me with, which was witnessed by himself, another surgeon, now living, of high professional rank, and Dr. Warren, in which a chancre was in the first instance cauterized, mercury instantly exhibited internally, and continued till it produced an effect on the mouth. Yet in this gentleman the disease afterwards showed itself on the skin. This was cured with still more caution than before, that is, the ptyalisms, though gentle, were kept up longer; yet at the end of about two months after the last had ceased the *offa palati* took on the disease, which soon spread to the nose, and before the mercury could arrest it, took off a part of each. It must indeed happen, that where ulceration exists, absorption must always be going on, as the only means of accounting for the loss of substance. If therefore the parts usually attacked by the secondary disease, are not susceptible of the impression of the virus, when first applied to them, it seems unlikely they should ever be so afterwards from the same stock. It is possible indeed cold, or some other exciting cause, may induce this susceptibility; but this is in my opinion not sufficient to authorize us to suppose so many would show the secondary symptoms as Mr. Hunter states.

MR.

MR. JEFFE FOOT comes next in the order of time. This gentleman, almost as soon as Mr. Hunter's treatise appeared, was prepared with an answer. A few years afterwards, he published a course of lectures on the same disease. As each of these works contains remarks on Mr. Hunter, and also opinions of the author's own, it is extremely difficult to separate them: I shall therefore take a view of the lectures and the remarks as they are connected, and then answer some of the objections met with in the latter only.

THE first lecture, comprising forty-eight quarto pages, is principally confined to Mr. Becket's Letters on the Antiquity of the Venereal Disease, printed in the Philosophical Transactions. These are republished by Mr. Foot, because, he observes, the Philosophical Transactions "are in the hands of a few, and therefore difficult to be come at." Astruc's answers follow, because they are not inserted in the Philosophical Transactions. The advantage of this is, that the readers may see the whole at one view, and become judges for themselves: subjoined are the most material extracts produced by Astruc from the early writers; with medical and historical evidence and remarks by Mr. Foot. This brings us to the 120th page.

THE fourth lecture is on the nature and action of the venereal virus: the former the author acknowledges himself ignorant of; the latter, he observes, with Mr. Hunter, depends on the different structure of the parts. To show the similarity of their opinion, I shall quote Mr. Hunter's words from Mr. Foot's observations, and Mr. Foot's own from his lectures. The passage in the observations is as follows: "The Professor says, that general irritations upon secreting surfaces endeavour to produce their own destruction, like a mote in the eye, which, by increasing the secretion of tears, is itself washed away." "This observation," continues Mr. Foot, "is very pretty, very obvious, and shrewd."* (We shall see by the lectures how well Mr. Foot likes it.) "The urethra, like every other mucous membrane, the nose, fauces, and eyes,† &c."—"Any little innocent extraneous substance fastening on these mucous surfaces, will excite from them an additional discharge." "When any thing obnoxious stimulates them, such are diluted away by increased secretion."† These are not the only passages in which this *obvious* observation is repeated, but as it is *pretty* and *shrewd*, as well as *obvious*, this may be an apology for bringing it forward so often.

* Observations, Part I. page 27.

† Lectures, page 125 & seq.

I SHALL not enter into the enquiry, whether gonorrhœa will cure itself, because the opinion is too general to be called Mr. Hunter's; the contrary, as held by Mr. Foot, is in the present day more like a novelty.

THE next enquiry, which occupies the greater part of the lecture, is, whether the matter of gonorrhœa and lues is the same, a question which at one time was not thought deserving notice: for in the "Observations" we have the following remarks. "This enquiry he [Mr. Hunter] introduces to prove, that the poison is the same in gonorrhœa and chancre. I should be glad to be informed by the Professor who ever doubted it."* Again, "To prove that gonorrhœa and chancre arise from the same poison, he has made a voyage to the South Seas, and, as *I before observed, no one ever doubted the fact!* But no such inference is to be drawn from any of the accounts of the state of the disease, as it was found in the islands of the South Seas."

BUT Mr. Hunter's object was to show, that though chancres exist in Otaheite, the disease must have been first carried thither by a gonorrhœa, "because," says he, "it is almost impossible to car-

* Observations, page 10 & 12.

“ry a chancre so long a voyage without its de-
 “stroying the penis.”—It must at least reduce
 it to a situation not likely to be very mischievous.
 Though Mr. Foot takes another method of prov-
 ing the similarity of the discharge, yet his partiality
 for Mr. Hunter’s opinions will not permit him to
 pass over the South Sea voyage. But in this, as in
 most other instances, unfortunately for himself, his
 wish to avail himself of Mr. Hunter’s arguments,
 and his determination to differ from him, betray
 him into some inconvenience. Yet it is odd how
 exactly Mr. Foot falls into the same idea in almost
 the same words, only with a little less caution. “If
 “you ask,” says he, “what local symptoms of the
 “venereal disease retains the longest power of in-
 “fecting. 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
 “the general disease, as it now prevails at Ota-
 “heite, I know not how otherwise the infection
 “could have been conveyed by a ship’s crew to
 “that part.”* Now, though this is certainly a
 very fair way of accounting for the disease being
 carried to Otaheite, it is no proof that this local
 symptom retains longest the power of infection.
 The contrary is more *certainly* the truth, because
 the matter of a chancre, as long as it retains its

* Lectures, page 137.

chancrous appearance, is always infectious, while that of gonorrhœa ceases to be so often before the discharge stops. This lecture concludes with those odd opinions of the author, which we have before given in a note,* and which are to be explained hereafter.

THE fifth lecture on gonorrhœa, after repeating Mr. Hunter's very pretty, obvious and shrewd observation once more, proceeds to the old question concerning pus and mucus. The same arguments are repeated, and the proof still rests with the long quotation from Mr. Pott. In one place we read, "If the discharge from a venereal 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."† Thus far we see the discharge is in every appearance different from mucus. What then does it resemble? We are afterwards informed. "This conceit (that the discharge is pus) must have originated from the lowest order of reasoning, from the mere similarity of colour of the two fluids."‡ Thus much for the difference between the gonorrhœal discharge and mucus, and for its resemblance to pus; now for the true difference between

* See the note in page 26.

† Lectures, page 158.

‡ Page 164 et Seq.

pus and mucus. " 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 mu-
 " cous membranes and the glands thereof, without
 " any solution of continuity, but barely as an
 " increased secretion ; whereas pus will be formed
 " on parts all over the body, where there is a de-
 " struction or solution of parts. This is the exact
 " state of the case."* This proposition, assertion,
 conclusion, definition, or whatever the sentence
 may be called, is certainly very strong, and only
 deficient in argument. But if we place the ques-
 tion as stated by Mr. Foot, it amounts to this,
 The altered secretion resembles pus in some ap-
 pearances, mucus in none. What then shall it
 be called? The answer is, as we find in the Ob-
 servations, " The definition of the discharge
 " proves the fact."† Neither resemblance nor
 properties have any thing to do with the question.

I SHOULD weary my reader, as well as my-
 self, were I to take notice of every other passage,
 even as cursorily as I have done these. For this
 reason I shall not dispute, whether because John-
 son defines sympathy to relate to two persons, it
 may not, with still more propriety relate to two parts

* P. 164 et Seq.

† See page 19.

in the same person. I shall also pass over all the lectures on stricture, as not connected with my subject; and for the same reason, the practical remarks on swelled testicle and bubo.

IT is not a little extraordinary that when I meet with any thing perfectly intelligible in Mr. Foot, I should always fancy myself reading John Hunter. I have already remarked Mr. Hunter's description of the thickened base which circumscribes the ulceration of chancres, and Mr. Foot's inaccuracy in his objection to that description.* In his Lectures, however, he makes up for his former inattention. "There is," says he, "one symptom of a venereal ulcer, which is common to all, and which is peculiar to a venereal ulcer, that it will be surrounded by a thickening of the parts."† At the conclusion of this lecture, the author's new theory is brought forward, but I shall reserve my remarks on it till it occurs in its order, and now direct my reader's attention to the Lecture on Buboës.

JOHN HUNTER—"A knowledge of the absorbing system, as it is now established, gives us considerable information respecting many of the

* See page 78. "Ulcers are only found out to be venereal only by situation or suspicion." Observations, part 3, p. 6.

† Lectures, page 412, and compare pages 6 and 7 of Observations, part 3, in which the same passage of Mr. Hunter's is quoted differently, in order to produce a palpable contradiction.

"effects

“ effects of poisons, and illustrates several symp-
 “ toms of the venereal disease; in particular, the
 “ formation of buboes.—The discovery of the
 “ lymphatics being a system of absorbents, has
 “ thrown more light on many diseases than the dis-
 “ covery of the circulation of the blood.—It leads
 “ in many cases directly to the cause of the dis-
 “ ease.”*

MR. FOOT—“ From the acquired knowledge
 “ which has been so eminently displayed by more
 “ ancient anatomists, and from the additional ac-
 “ quirements which have been obtained by the in-
 “ genious and indefatigable labours of more mo-
 “ dern 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 lymphatics have been
 “ proved to be a system of absorbing vessels. Sur-
 “ geons 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 the-
 “ ory.”†

* Treatise 253.

† Lectures 438.

THE reader will perhaps suppose by this similarity of language and sentiment, that Mr. Foot is going to confirm Mr. Hunter's opinions by fresh arguments of his own; but unfortunately for himself, he seems to think it his duty to differ from him. As Mr. Hunter remarks, that Astruc was ignorant of the lymphatic vessels being absorbents; Mr. Foot fancies he answers him, by ascertaining that Astruc was not ignorant of the *lymphatics*. After, therefore, expressing the obligations we owe Astruc for collecting all the early accounts of the disease, and, he should have added, enabling him to fill a hundred quarto pages, Mr. Foot adds, "We feel ourselves impelled by gratitude towards Astruc, and as Mr. Hunter has chosen to produce quotations from Astruc to prove he knew nothing of the lymphatic system ——."* Mr. Hunter never hints Astruc's ignorance of the lymphatic series of vessels; but that he knew nothing of their being the absorbing system.

'THE only way to ascertain what Astruc knew,' continues Mr. Foot, 'is by what he has written, for the professor positively, but I will prove falsely, says, "That his ideas are become unintelligible." Nothing can be fairer than such a proposal; let me therefore transcribe his account of the cause of bubo, as translated by Barrowby.

* Lect. page 441.

CAUSES OF BUBOES.

“THE inguinal glands cannot be enlarged after
 “ impure coition, and indurated into a bubo, unless
 “ the lymph, which is conveyed to them from the
 “ neighbouring parts, as to a common reservoir, and
 “ ought to flow through their cells or vesicles, in
 “ order to be carried off again, shall stagnate there
 “ and be accumulated in them. But the lymph
 “ cannot stagnate and be accumulated in the ingui-
 “ nal glands, unless it shall be thicker and more viscid
 “ than usual. It follows, therefore, that the lymph,
 “ which circulates through the inguinal glands, is
 “ rendered too thick and viscid by impure coition,
 “ and by being accumulated in those glands pro-
 “ duces venereal buboes.

“ BUT nothing new happens to the body from im-
 “ pure coition, except the simple admission of the
 “ venereal infection. That infection therefore be-
 “ ing admitted into the body, and blended with
 “ the lymph of the inguinal glands, renders it too
 “ viscid and thick, for the producing of which effect
 “ it is very fit, since it is of a salt acrimonious na-
 “ ture, whose efficacy in coagulating sulphureous
 “ humours is well known, of which kind the lymph
 “ is.”*

THOSE

* Vol. 1. p. 258.

THOSE who are ignorant of chemistry and “the additional acquirements which have been attained by the ingenious and indefatigable labours of more modern anatomists” may fancy they understand this. Indeed, from the great haste in which Mr. Foot’s observations were brought out one might almost fancy he had accepted the assistance of one better acquainted with classical literature than medicine. The remarks on the increased muscular fibres of the bladder, in cases of dysuria, much confirm this opinion. So little idea has the writer of the necessity of increased strength, to overcome increased resistance, that he confounds the muscular fibres of the bladder, with the thickened state of a limb in elephantiasis, and proposes, if the bladder really is strengthened in proportion to its thickness, that we should for the future recruit our armies from the hospitals.* Mr. Foot should check his friend Classic in these fallacies, and explain to him, that if we wish for a particularly strong arm, we should chuse that of a gold-beater, or a pair of strong legs, those of a chairman, which are always increased in proportion to the labour of the owners, and that in strictures of the urethra, or even calculus in the bladder, the disease is not in the muscular coat of the latter, which is only thickened in proportion to the additional force required to ex-

* Observations, page 134.

pel the urine. But while this oversight makes me suspect, that part of the "Observations" were suggested by a scholar not of the profession, I give Mr. Foot the full credit of his "New Discovered Fact," his "Lectures," and even his learned quotation from Celsus or Hippocrates,*—'tis all one—But to return to our subject :

THE 15th lecture commences with "the action of the venereal disease on the constitution," a subject which is continued through eighty pages, but with so little argument, point, or any thing but verbosity and illustration, that the whole might have been included in a paragraph, were it not for a few useful hints from Mr. Hunter, or as the writer chuses to suppose, from Fracastorius—Let the reader judge.

JOHN HUNTER.—"When the venereal matter has got into the constitution in any of the ways beforementioned, it has the whole body to work upon, and shows itself in a variety of shapes." And in the same page, "I have already endeavoured to show that it is most probable all the parts affected are contaminated nearly at the same time. This difference in the times" (of the dis-

* "CELSUS his celebrated passage of *ars longa, vita brevis*, was never more applicable than to the venereal disease." Introduction to Lect. 13th. page 393.

ease appearing) “is either owing to some parts
 “being naturally put into action more easily by the
 “poison than others, or that they are naturally
 “more active in themselves.”*

MR. FOOT.—“The order in which the disease
 “appears, is accounted for from the different con-
 “struction of the parts. They are all, from the
 “first, equally exposed to be attacked by the dis-
 “ease, and it is not improbable that it is acting on
 “every part, from the very beginning of the pri-
 “mary constitutional symptoms.”†—“In order to
 “prove to you, that my theory of the disease com-
 “mencing 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 om-
 “nia, quasi parva illa forent, ingentes lacertorum
 “dolores accedebant, sæpe cum ipsis pustulis, in-
 “terdum ante, nonnunquam post, et ipsi quidem
 “diuturni; quibus nihil crudelius aderat: afflige-
 “bant præcipue noctu, dolor autem non proprie in
 “juncturis inerat sed circa lacertos ipsos & nervos.
 “Verum quibusdam nihilominus, sine dolore ullo
 “oriebantur pustulæ; quibusdam sine pustulis do-
 “lores; major pars utrisque affligebatur.”‡

* Treatise, p. 316. † Lectures, p. 517. ‡ Id. p. 519.

A WRITER who proves by definitions and by quotations, should do it at least by definitions of the things to be proved, and by quotations that confirm what is stated. But this is not the first time the subject has been brought forward by Mr. Hunter, nor of course by Mr. Foot. In a previous chapter we meet with the following words from

JOHN HUNTER:—“ We cannot suppose that
 “ this difference” [of the periods at which the
 constitutional symptoms appear in different parts]
 “ arises from any active power in the poison, nor
 “ any particular direction of it, but from proper-
 “ ties in the parts themselves; for it may be allowed
 “ us to suppose, that when the matter has got into
 “ the circulation, it acts on all parts of the body
 “ with equal force, &c.”*

MR. FOOT. “ When the venereal poison has
 “ taken full possession of the constitution, not only
 “ the parts which indicate the disease to be vene-
 “ real, by their apparent morbid vitiations, but al-
 “ so every part of the constitution is venereally vi-
 “ tiated.”† Here Mr. Foot sets off very well, but
 in the progress, forsaking his guide, he loses himself.
 For though the poison must, by being applied in-
 discriminately to every part, if it has any action,

* Treatise, page 307.

† Lectures, page 465.

act equally on all ; yet, as we find some parts never are affected by it, we are not authorised to say “ *every part* of the constitution is venereally vitiated.” Mr. Foot should have attended to the rest of Mr. Hunter’s sentence, which continues thus : “ That is, it is not determined to any one part more than another, by any general or particular power in the animal machine, nor is the nature of the poison such, as will fall more readily on one part of the body than another, when they are all in similar circumstances.” Thus we find, as the fact proves, that though the poison must circulate every where, and in the same manner, only those parts are affected by it, which from particular circumstances are susceptible of its irritation. That this is really the case we have every reason to believe, because there are so many parts, which under every circumstance of neglect have never shown this *venereal vitiation*.

BUT this passage from Mr. Foot is only introductory to the author’s discovery of a doctrine, which we are afterwards told is “ the key-stone to all theory, which can be in truth received for the confirmation of the nature of this poison and of all others.”*——“ 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

* Lectures, page 489.

“ 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 rea-
 “ sons for this position I have before stated at large ;”
 [very much so] “ they are, because the foreign virus
 “ will pass into the constitution, and affect 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 in-
 “ fection, 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 prin-
 “ ciple, namely, that our own secretions cannot in-
 “ fect 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 not here stop to determine whether
 the constitutional symptoms ever appear without
 the previous local affection, as this will be con-
 sidered hereafter. But if, as Mr. Foot asserts,
 the

the constitution is never affected but by foreign matter, as it is called, how comes it that lues is so often produced by chancre, and so seldom by gonorrhœa; the foreign matter being in both cases the same? I know not whether the following passage is meant to account for this, but to me it is absolutely unintelligible in any other way than that of matter being infectious to the subject that secretes it. “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 in-
 “fecting 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; a go-
 “norrhœa 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.”* But this opinion
 of matter secreted, being no way infectious to

* *Leat.* page 455.

the person secreting, is so contradictory to all former observations, that before it is seriously considered, it must rest on better experiments and more conclusive facts than those related in the pamphlet.* On the manner in which mercury cures the disease, Mr. Foot's opinion so exactly coincides with Mr. Hunter's, that it is enough to read either of them.

JOHN HUNTER.—“ Thirdly, It may produce
 “ an irritation in the constitution which counteracts
 “ the venereal, and entirely destroys it. The third

* “ I have rubbed these fluids [gonorrhœal and chancrous]
 “ on the sores of the same subjects, over and over again, and
 “ never was able to trace the smallest effect from them. I
 “ have seen these fluids rest upon pimples long enough for
 “ effect, without any effect. Within these few days a young
 “ gentleman, who was sore from riding, and who had a
 “ gonorrhœa, applied to me; I rubbed his gonorrhœal fluid
 “ on the galled part, and he, from confidence in my opi-
 “ nion, did repeatedly the same, but not the least sensible
 “ effect was produced by it.”† It is admitted by all that
 we have no certain diagnosis to distinguish a venereal from
 a gonorrhœa arising from any other cause; and at the time
 Mr. Foot made his discovery, he probably knew not how to
 distinguish a venereal chancre from a common ulcer, but by
 “ situation or suspicion.” We are not therefore to take up
 theories from such authorities, when they contradict those
 made in public, and by one whose accuracy was never dis-
 puted by his opponents.

† New Discovered Fact, page 28.

“ or

“ or last mode of the action of mercury seems to me
 “ most probable, for many reasons.”

MR. FOOT.—“ Of the two, and whilst it conti-
 “ nues, the stimulus excited through the mineral
 “ poison is far *more excessive* than that excited by
 “ the animal poison. It is not only by the percep-
 “ tion in our feelings that this superiority is proved,
 “ but also by the stimulus of mercury being supe-
 “ rior to the venereal stimulus, and thereby de-
 “ stroying and extinguishing it.”

THUS far Mr. Foot keeping his guide in view
 travels safely, but without being aware of it him-
 self, in the next paragraph he loses sight of him, and
 is as much bewildered as ever. In accounting for
 the cure by this new irritation, he observes, “ Mer-
 “ cury once imparted to the constitution will not
 “ be capable of supporting its action beyond a
 “ certain duration. But venereal virus once im-
 “ parted to the constitution, endures for ever.
 “ The action of mineral poison is of a certain
 “ duration. The action of the venereal poison is
 “ supported by the original poison being multiplied
 “ from an atom of venereal fluid.” If now mer-
 cury only cures by exciting a stimulus greater than
 the venereal, it can only cure the parts previously
 stimulated, and the action of mercury having a
 finite duration, whilst that of the other endureth
 for ever, why does not the virus resume its activity

as soon as the mercurial stimulus ceases? This difficulty will appear greater when we consider the last sentence. "The venereal action is kept up by the poison." Thus the poison and the action it excites, are two things, and the curing the latter is no proof that the former ceases to exist, as before. It is not enough that we are told before this of the *eradication* of venereal infection, and the *extirpation* of venereal virus: because when Mr. Foot takes upon himself to explain the effect of mercury, he only assigns a cause that destroys the action of the poison, and not the poison itself.

I SHALL now take notice of some passages in Mr. Foot's "Observations" that could not be so well introduced into the review of his lectures. On the subject of gonorrhœa curing itself.— "Cures," says he, "if I were to collect them, that would fill as many volumes, and of as large a bulk as the records of Parliament, may be brought in proof, that all local and all constitutional venereal affections may be produced from gonorrhœa. It is for this reason that gonorrhœa does not complete its own cure. The virus that produced the irritation in the urethra, after the urethra can be no longer irritated, is capable of shifting its influence, of producing new effects, and of being ramified into
" all

“all venereal symptoms that ever were known.”* This capacity of “shifting its influence,” is certainly a very extraordinary one; and, I confess, not quite intelligible to me. The ramifying property is also Mr. Foot’s discovery. Other authors have seen the root and stem, but only he the branches. We may understand what is meant by absorption from a virulent sore producing effects in a distant part of the constitution, and that in the mean while the first symptoms may be cured; or, if the structure of the parts is uninjured, that the first irritation may cease of itself! It is true, which ever way we state the fact, the author’s own reasoning somewhat contradicts his new opinions, the “key-stone to all true theory.” But at the time he wrote the above he was not acquainted with the true theory. Then, probably his ignorance, as is often the case, made him so positive, and talk about volumes of precedents that would fill books as large as parliamentary records! But *does* he mean absorption of matter, *from the same subject, infecting the same subject?* We shall see in his further observations.

THE following quotation contains a passage from Mr. Hunter, with Mr. Foot’s remarks. “I have seen, by an accidental fever coming on, the discharge stop, the pain in making water

* Observations, part 1, page 69.

“ go off, and the gonorrhœa finally terminate
 “ with the fever.”* “ As the professor,” says Mr.
 Foot, “ makes no remark upon this, it is to be
 “ presumed, that he conceived the whole venereal
 “ concern to be at an end. In the only instance
 “ where I ever saw a gonorrhœa disappear in this
 “ manner, the consequence was, that the patient
 “ endured the most confirmed lues venerea that
 “ ever I have seen in all my practice. A lieute-
 “ nant of a man of war was seized with the small-
 “ pox whilst the gonorrhœa was on him: he had
 “ the small-pox so bad, it was with great difficulty
 “ he recovered: the gonorrhœa left him altoge-
 “ ther. He went, as soon as he recovered from
 “ the small-pox, to sea; was absent from England
 “ more than twelve months, and returned in a most
 “ emaciated state, with nodes upon his bones, at-
 “ tended with incessant pains, and a large ulcer
 “ over all the palate, fore-teeth out, and the bone
 “ was a caries. Much mercury had been given at
 “ sea to him, but to no purpose. He recovered at
 “ Dr. Kelly’s, at Knightsbridge, by warm bathing,
 “ milk diet, decoction of sarsaparilla, and a little
 “ mercury was given, when his constitution had
 “ been previously mended. Dr. Wright attended
 “ him with me. So much for the security of go-
 “ norrhœa curing itself. I am inclined to think that

* Hunter’s Treatise, page 84.

“ the more a person is out of health, the more likely
 “ the venereal virus is to be absorbed. At any
 “ rate the case above proves directly the contrary
 “ to an opinion the professor would fix us with :
 “ It appears to me beyond a doubt, that no two
 “ actions can take place in the same constitution
 “ at the same time. Here we find the professor
 “ positive ; but this is one of his new opinions :
 “ perhaps he is in the right. We ought to be
 “ proudest of our own children ; and the more
 “ rickety they are, the more parental tenderness
 “ they require from us. Does the professor pre-
 “ tend to say, that the virus was dormant whilst it
 “ was absorbing from the urethra into the ha-
 “ bit ?” *

THIS is a long quotation, and I shall take the
 liberty of making a long remark on it.

MR. BELL, who does not admit the virus of go-
 norrhœa to be venereal, would say, the lieutenant
 became infected at some port during his voyage,
 which does now and then happen. But taking the
 case as it stands, it confirms Mr. Hunter's opi-
 nion in every point of view. Mr. Hunter only
 contends for the local action being cured by a fe-
 ver. The case also shows precisely that two dis-

* Observations, part I. p. 8.

ceased actions cannot take place in the same part at the same time; for the venereal ceased at the access of the variolous, and did not return till the latter was over. "But," says Mr. Foot, "does the professor pretend to say, that the virus was dormant whilst it was absorbing from the urethra into the habit?" I trust not; because such language is too loose and figurative for one who had precise ideas. I could almost adopt Mr. Foot's mode of reasoning, and ask, whether the poison might not as well be conveyed sleeping as awake. But I wish to avoid verbal, or even grammatical criticisms, where they are not necessary, from the effect words and grammatical construction have on the reasoning.

It has been a dispute among English grammarians, whether the want of a present and imperfect passive is any defect in our language.* It is most probable that precise ideas may be communicated in any language where the writer possesses such ideas himself, and wishes to communicate them. But the present is an instance how a writer may be deceived by the too frequent misapplication of the terms of his own language. "Can the virus be dormant whilst it is absorbing?" By dormant it is plain Mr. Foot means *not in action*, whilst he

* SEE Mr. Pickbourn's ingenious dissertation on the English Verb.

seems to add, it is *in the act of absorbing*. But Mr. Foot knows the virus has no power of absorbing, and if it had, what could it absorb? Had he written in Latin, he would have said *Dum absorbebatur*, but the refinement of our ears, not admitting the circumlocution of *whilst it was being absorbed*, and custom having authorised the other mode of expression, has deceived the writer so as to make him fancy that the virus was in the act of absorbing, and consequently, that it could not be passive or dormant. But after all, are the virus and the constitution the same thing? certainly not, the constitution was engaged in the variolous fever, but the venereal virus might be present, and the action of the absorbing system could no more be suspended during a fever than respiration. What then would Mr. Foot signify? Whenever he has fixed precise terms to his meaning, he will say, "At the time the variolous fever commenced, the gonorrhœa ceased. At the accession of the fever, I conceive the virus was absorbed before the gonorrhœa quite stopped, (for it could not be afterwards) and, when absorbed, gave a disposition to certain parts of the body, to take on the disease, but which diseased action did not take place while the constitution was engaged with the variolous." This, he will be convinced, was the only way in which the virus could be said not to be inactive or dormant, and in this, or any other intelligible

telligible way of stating his fact, I thank him, from the respect I bear to the memory of Mr. Hunter.

IT was my intention to have passed over every thing not immediately connected with the disease in question, but in turning over the leaves of the second part of "Observations," the name of Mr. Pott caught my eye.

"THE professor has again introduced his" [Mr. Pott's] "name,* and a curious situation he has placed so great a man in, a helpless spectator to the decay of the testicle!"

I KNOW not why men, who chose to forget their former animosities, if they had any, should be brought before the public as rivals. Mr. Hunter, in the passage under consideration, pays respect to Mr. Pott's observation and to his judgment. In another part of the work, "Mr. Pott," says he, "is, I believe, the first who showed the world the use of opium in mortifications." I have myself heard Mr. Pott adopt Mr. Hunter's phrase of adhesive inflammation in his lectures, have met them together in consultation, and on these, as well as all other occasions, have remarked with pleasure, how much the urbanity of truly well bred and

* Observations, Part II. page 147.

well informed men, softened those asperities which are characteristic of vulgar and little minds. Nor was this surprising, when it is considered how little Mr. Hunter was disposed, in writing or lecturing, to lose time in proving what is best shown by a regular statement of facts. More than this would have interrupted those pursuits which were always uppermost in his mind. In all these respects he differed much from his brother: and Mr. Pott always showed his consciousness of it by the different manner in which he treated their names. Such was the petulant waspishness of the Doctor's temper, that, not satisfied with defending himself, he must always leave a sting in the side of his adversary: and so frequently would he vellicate, cauterize, cut, abrade, and by every means irritate the tender granulations that were disposed to rise, that if ever a cicatrix was formed, it was of such a complexion as must always keep alive the memory of the ancient injury.

FAR different was the character of that philosopher, whose name I have so frequently quoted: a name that will be dear as long as memory and gratitude exist, and as different was the language of Mr. Pott, when he had occasion to speak of either.

BUT if it was improper thus to oppose characters
to

to each other who were disposed to live in harmony, it is not less unfair to make comparisons between talents so different. Mr. Pott is never so aptly described as when he is called the ornament of the profession: he certainly adorned every thing he took in hand. While Mr. Hunter was diligently and attentively examining the ground-plot, and seemed fearful of being interrupted in securing his foundation, the other had already prepared the fluttering genii that were to embellish the arcades: and if perchance the weakness or obliquity of a column should, to a fastidious critic, suggest a doubt on the duration of the future structure; the attention of the spectator was never suffered to dwell upon it, but engrossed entirely by the taste and propriety of the enrichments.

MR. Hunter, when satisfied of the firmness of his ground, was only anxious to be understood. Having no taste or scientific ambition beyond his researches into nature, and aware of the difficulties that attend such investigations, he thought it enough if he could by any means convey the result of his enquiries to his hearers. His manner was frequently ungrateful; his illustrations coarse; his language always unadorned. But Mr. Pott seemed constantly to have in view

*Non satis est pulchra esse poemata dulcia sunt
Et quocunque volent animum auditoris agunt.*

IN the theatre, his style of operating was elegant at the age of seventy. In conversation, he was always brilliant and correct; in composition, tasteful, energetic, and interesting; in the lecture room, eloquent and perspicuous. It was not enough that he seduced the attention by the improvements he introduced: the errors of his predecessors must be glanced at, and that nothing might be wanting, our more remote forefathers* must be introduced, and the names of Celsus, Fallopius, Fabricius ab aqua pendente, Hildanus, and others were brought forward to heighten the antithesis, flatter the vanity of the rising generation, and display the extent of his own erudition.—Nor was the contrast greater in consultation. Mr. Pott, as his biographer remarks, was “prompt in judgment, rapid in decision.”† Mr. Hunter, while inquiring into a case, seemed engaged in an experimental investigation, and never scrupled to acknowledge himself ignorant, where he could not satisfactorily make up his mind. Talents so different were less exposed to rivalry, and if any existed, it was restrained by native good sense, and an attention to propriety on both sides.

I BEG pardon of my reader, for this narrative specimen of “*laudator temporis acti.*” But if he feels

* A common term with him in lecturing.

† See his epitaph in Earle's edition of his works.

with me at the recollection of “those rare men, that
 “taught in those rare days,” he will not think the
 digression long. If he only knows them by their
 works, (and in these who can be ignorant of them?)
 he cannot be displeas'd at being thus personally in-
 troduced to them.—But having turned my recollec-
 tion to that period, when the only business of life
 was to learn, and the means of improvement always
 at hand—to scenes which, like all others, are heigh-
 tened by being view'd in a reflecting medium, and
 which, in proportion as we retreat from them, blend
 with a more enchanting softness their various co-
 lours, and smooth all their little asperities:—having
 thus far indulg'd the transition of ideas, I shall,
 without further apology, conclude the subject with
 a sentence which is always obtruding itself whene-
 ver these characters are contrasted, and which is
 as apposite as any can be, that compares individuals
 to a nation. *Atheniensium res gestæ sicuti ego ex-
 istimo satis amplæ magnificæque fuere; verum ali-
 quanto minores tamen quam memoriæ feruntur. Sed
 quia provenire ibi scriptorum magna ingenia, per
 terrarum orbem Atheniensium facta pro maximis cele-
 brantur. At populo Romano nunquam ea copia fuit;
 quia prudentissimus quisque maxime negotiosus erat:
 ingenium nemo sine corpore exercebat.**—But it
 is time we should attend to the passage that has in-

* Sallust. Bellum Catalinarium.

troduced this digression: on this I shall be very short.

THE first paragraph is Mr. Hunter's, as extracted by Mr. Foot. The second is, Mr. Foot's remarks:† “ Testicles have been known to waste in
“ cases of rupture, probably from the constant
“ pressure of the intestine. Mr. Pott has given
“ cases of this kind.”

“ I JUST remarked that the professor is singularly
“ unfortunate in all his assertions: it gives me great
“ pain to be constantly contradicting him, but I do
“ here flatly; because it must place the one or o-
“ ther of us in a *mean and most disgraceful situa-*
“ *tion.*”

IF the reader will take the trouble of referring to Vol. ii. p. 58, of Earle's edition; or 260 of the quarto edition, published by Mr. Pott himself, in the note he will read the following passage:
“ The caul adhered to the hernial sack in sever-
“ al places, the intestine in none, *the testicle in-*
“ *cluded in its tunica vaginalis was much wasted.*”
In the third vol. page 350, is an account of a cystic hernia, in the operation for the cure of which Mr. Pott says, “ I discovered a testicle which lay

† Observations, Part II. page 145.

“ immediately behind the body, forming the tumour, and was small, flat, and compressed.” In the last case, the testicle was not pressed by the intestines, but still it was compressed by the contents of the hernial sack. On the first I make no remarks, but leave our readers to judge what inference they are to draw from the proposition, as stated by Mr. Foot.—Had I seen it sooner, it might have saved the reader and myself some time that might have been better employed.

THE last writer I shall take notice of is Mr. Bell, of Edinburgh, who, in the year 1793, published “ A Treatise on Gonorrhœa Virulenta and Lues Venerea.” This gentleman’s first object is to prove, that the two diseases originate from different species of contagion. After remarking the different phænomena they exhibit, he observes, that the smallest local affection of lues venerea is apt to produce a general affection of the system which may even be produced by absorption, without any previous ulceration on the part to which it is applied. “ This,” says Mr. Bell, “ is denied by many ; but I have met with various instances of it, and it will be admitted by every practitioner of experience. Now this being established” *—Let the reader

* Vol. 1, page

determine whether it is established. As to what Mr. Bell or I have seen, it amounts to little; *vidi ego*, is no more argument than *ipse dixit*. When a man has formed an opinion of his own, it is not enough to prove it in this manner, by generals; he must inform us of all the circumstances on which his facts rest; and even then, if he is as punctilious as an experimental philosopher ought to be, he will be unwilling to trust to the evidence of his own senses. It is true Mr. Bell raises a number of objections to his theory, which he is at no trouble in answering, because they are such as no one who has a just conception of the disease would offer. They rest on the retropulsion of Dr. Swediaur and the influence-shifting property of another author. We have accounts of some surgeons who have thought it necessary to salivate their patients, because the discharge suddenly ceased, as if something was thrown into the blood. But these opinions can be only the offspring of ignorance. The stopping of the discharge is only a proof that the source of infection no longer remains: in other words, that the secretion, which arose from the irritation of virulent matter, has ceased, which is no more a proof of any thing being thrown into the constitution, than in those diseases, which, to use Mr. Bell's words, are found to proceed from what is termed "a translation of the matter of gonorrhœa to other parts

“ of the body.” Mr. Bell has three or four cases ready. In some, a discharge from the eye, in others, from the nose, were the consequence of the sudden stopping of gonorrhœa. The nose case, I have reason to believe, is peculiar to Edinburgh. It is mentioned by Mr. Bell and Dr. Swediaur as a very common accident; though I do not recollect any other author who has described it.

BUT however common such accidents may be in that part of the world, no one that thinks can ever ascribe them to retropulsion, absorption, translation of matter, or the shifting of influence. Retropulsion and influence-shifting are words without meaning. The only way besides immediate contact, by which matter can be conveyed to the eye or nostril, is by absorption, which may exist just as well while the discharge is considerable, as when it is less, and which must cease when matter is no longer secreted, to be absorbed. It rarely happens that two parts of the body suffer violent inflammation at the same time. Inflammation, therefore, attacking the eye, may supersede that of the urethra; and no loss of substance being endured by the latter, the cessation of the inflammation may cure the disease. But the rapidity with which this gentleman writes, is only excusable from the habit he has been in of compiling. “ We
“ have

“ have also to remark,” says he, “ that in *numberless instances* people have been poxed by the matter of syphilis being by accident applied to a cut or a scratch, as often happens with surgeons in dressing chancres and buboes.” By the matter of syphilis I conceive Mr. Bell means the secondary; or, as they are sometimes called, the constitutional sores; in other words, those which arise from absorption. Now of these *numberless instances* it would have been worthwhile to have referred us to a few, since even the possibility of such an event is a matter at least problematical. As to those accidents which are said to happen *often* from dressing chancres and buboes, I trust the surgeons of Edinburgh will not thank Mr. Bell for this accusation of their awkwardness. “ But,” continues he, “ no one ever heard of a pox being got in this manner from the matter of gonorrhœa.” It is very true that gonorrhœa is not usually dressed by surgeons. “ It has indeed been said, that chancres may be produced by insinuating the matter of gonorrhœa beneath the skin.”—“ It has been said.” Thus, to some writers, experiments made in public hospitals, by men proverbially celebrated throughout Europe for justness in conceiving, and accuracy in conducting their enquiries, are treated with less attention than *numberless instances*

none of which are particularized; * and accidents, which, were they to happen, would only prove what no one doubts.

“NOTHING therefore,” says Mr. Bell, “can be admitted from *this argument*; for, in order to avoid fallacy, and give support to the opinion, these experiments would not only require to be conducted with accuracy, but to be numerous, and to be repeated on a variety of patients, under every possible variety of circumstances.” If it were *an argument*, instead of a fact, that is treated in this manner, there might be some apology for Mr. Bell’s scepticism. But he should consider the difference between argument and experiment. A single fact, well ascertained, is of more consequence to a philosophic enquirer, than ten thousand suggestions of *numberless instances*. He ought to know also, that when a fact is established with satisfactory clearness, the repetition of the experiment is not only unnecessary, but a misapplication of that time which should be better employed. But the negative experiment mentioned by himself, proves he admits nothing. While it is allowed that we have no satisfactory test of the difference between virulent and simple gonorrhœa, nothing can be collected from the application of

* Necessè sit ad instantias particulares recurrere.—See page 32, note.

the discharge proving innocent; still less when we consider how frequently it happens that the parts are insensible to the virus: such appears to have been the case in the instances related by himself; for not only did venereal virus fail to produce gonorrhœa when applied to the urethra, but chancre also. "But," says Mr. Bell, "we cannot place much dependence on these or any other experiments that have been made, but must trust to experience and the ordinary course of practice for means to ascertain it."* Cases then that occasionally occur, and the history of which we can rarely trace satisfactorily, are to be considered as stronger evidence than experiments made with accuracy, attended to with caution, and the whole progress of which is immediately within our own cognizance. The former certainly admit of more reasoning upon, because where a fact is not clearly ascertained, we may take the liberty of suggesting a few probable inferences that might follow from probable events. But when we recollect, that what is contested by Mr. Bell was founded on an experiment, the accuracy of which cannot be doubted, and that his attempts only go to reason us out of it, we can only answer him by a maxim of the first philosopher the world has ever known; "If once we permit conjectures, to lessen the validity of inductions drawn from experiments, it will be impossible that science should be progressive."

* Vol. I. page 35.

THE different times at which the diseases showed themselves is next urged, and it is said that gonorrhœal virus was not discovered till forty years after lues venerea. But as the running of the reins is as old as the Mosaic law, and as it is universally admitted, that we have no certain criterion to determine between the different species of gonorrhœa, it is not to be wondered at if the venereal passed unnoticed as long as that period. If it was so long before gonorrhœa was considered a venereal affection, it seems to follow that symptoms of syphilis were first discovered in a person in whom no other source could be detected, and that the theory was consequent to the fact.

IT has been my intention throughout, to make as few practical remarks as possible, and to say as little of the practical observations of others. When a disease is perfectly understood, the mode of treatment will not be difficult, nor will the practitioner be at a loss how to vary his remedies. But it is impossible to pass over the misfortune that attends the fluency of Mr. Bell's pen. The ease of his style is much assisted by the facility with which cases, remedies, cures, and failures of success offer themselves spontaneously to him. One should almost conceive, that, from a habit of compiling, he had gradually made every case he has read his own, as Herschell, from constantly observing the moon,
is

is said to call every new discovery he makes, an addition to his freehold estate. Either this must be the case, or the diseases of Edinburgh, as I observed, are very different from those of London. "The testes," says Mr. Bell, "we find are *apt* to decay."* He then enumerates various ways in which they have decayed, the most common ages at which the disease happens, the constitutions most susceptible of it, and the most usual causes: all this too, with a fluency and ease that would lead the young practitioner to suppose the disease so common that he is to be perpetually expecting it. But if this is the case in the city of Edinburgh, it would at least have been right to have warned his readers that such is not common in any other part of the world. Mr. Hunter, though for several years the consulting surgeon of the whole empire, and in many instances, of Europe, could recount only two cases that he had seen, and a third that was communicated to him. It is however a consolation, that in a town where the disease is so *apt* to occur, Mr. Bell has been lucky enough to discover that a blister has been in one instance equal to checking its progress.

SOME of Mr. Bell's opinions concerning lues venerea are not less novel. "If it were not more
" a matter of curiosity than real utility, many ar-

* Vol. I. page 356.

“guments might be adduced to show that the disease prevailed among the Jews, Greeks, and Romans, and their descendants, long before the discovery of America.”*—And why not in Paradise? If this mode of hurrying through a question is to be adopted, might it not be improved, by hinting that Eve gave it Adam, and that the story of the apple is only allegorical of this event. If my recollection does not betray me, I have met with something of this kind extracted from an ancient writer. Like most of the Cabalists, he was for deriving all the heathen fables from *their* mysteries, and asserts, that the celebrated story of the golden pippin, given by Paris to Venus, originated from this source. That hence the disease was indifferently called *morbus Venereus*, and *morbus Parisiensis*. In succeeding ages, these names were supposed to signify beaux and courtezans, and the golden apple to point out the danger of purchasing the favours of the sex, till a *more correct nosologist*, recollecting that the classical name for the city of Paris was *Lutetia*, thought it necessary to change *Parisiensis* for *Gallicus*, which he could do with the more propriety, as the disease at that time pervaded *omnem Galliam*.—I grant all this is very absurd. But it is less likely to mislead the reader than a suggestion about “many arguments;” because, when the arguments are produced, the

* Vol. 2, page 2.

reader can judge what force they contain. "But," says Mr. Bell, "being a matter of curiosity, rather than of real utility, I shall not enter further upon it at present." 'Tis strange that an author who has devoted two thick octavo volumes to this disease should not be aware that the enquiry is by no means so trifling as he expresses it. I have already shown in the instances of Phagedæna, one important question, that depends upon it; and Mr. Bell is not backward in availing himself of dates among the proofs he alledges of the difference between the chancrous and gonorrhœal contagion.

AMONG the modes by which the disease may be communicated, we are told of its passing, as other hereditary diseases do, from parents to their offspring, a subject that is hereafter explained more at large. It is next said to be frequently given by a diseased child to a nurse.—"An infected nurse can scarcely give suck without communicating the disease to the child.—When this happens, merely by sucking the milk, the disease does not appear at first about the mouth of the child: it proceeds more readily to contaminate the system, than to produce any local effect."—Devoted babes! why should your fate be so peculiarly hard? For in the space of about three pages we are told, "It is somewhat remarkable that the

* Id. Page 5.

"venereal

“ venereal virus should so readily enter the system
 “ when applied to any part of the surface of the
 “ body, when we find, from a variety of facts, that
 “ the absorbents of the stomach and intestines,
 “ do not receive it.”* But peculiarly hard as the
 fate of children may appear, grown people are
 not without their dangers.—The disease we are
 told, may be communicated on parts where the
 cuticle is thick and entire, and without producing
 any local mischief:—by matter left on the lips in
 kissing, in drinking out of a cup recently used by
 an infected person, and in some instances, with-
 out any ulceration being induced.

SUCH, and a few others, are the modes in which
 this disease maybe communicated, and which, though
 not very frequent, the author has seen various in-
 stances of.—Now for the description of the disease
 itself. “ When lues venerea is not interrupted in
 “ its progress by the use of mercury or other
 “ remedies, the following is the order in which
 “ the symptoms commonly appear, viz. chancres,
 “ buboes, ulcers, and inflammation in the throat,
 “ ulcers in the mouth and nose, eruptions or blot-
 “ ches on the surface of the body, ulcers in dif-
 “ ferent parts, nodes and swellings in the periof-
 “ teum, bones, and tendons, excrescences about
 “ the anus, swellings of the testes, loss of hair from

* Id. p 8.

“ all parts of the body, blindness, loss of hearing,
 “ and other anomalous symptoms.”*—Such is the
 disease that was known to the Jews, Greeks, and
 Romans, who were ignorant of the medicinal ef-
 fects of mercury; and yet Horace did not die of
 the pox! But, supposing that by some art, now
 unknown, he contrived to escape; is it not re-
 markable, that, in his second satire, a principal
 part of which is taken up in directing his friend
 in the choice of his amours, no caution should
 occur relative to this train of symptoms?

THE following is Mr. Bell's description of the
 formation of chancres. “ The first effect, which
 “ usually results from the application of the matter
 “ of lues venerea to any part of the surface of the
 “ body, is a slight degree of inflammation. The
 “ part becomes itchy, red, and, in some degree
 “ painful; and nature endeavouring to wash away
 “ the irritating cause, a quantity of serum is thrown
 “ out under the cuticle, in the form of a small boil
 “ or pimple. This soon bursts, and leaves a sore of
 “ a corresponding size, foul and sloughy at the bot-
 “ tom, with hard retorted edges.”† The first part
 of this history of chancre I have already shown is
 confirmed by every correct writer.‡ The probable

* Id. page 12. † Id. page 13.

‡ Except that most of them speak of pus, instead of serum.
 cause

cause of the pustule or vesicle was not suggested, I believe, before Mr. Hunter. But while Mr. Bell is so ready to adopt it, he is not aware of the consequence it leads to. "It is an invariable effect," says Mr. Hunter, "that when any part of an animal is irritated to a certain degree, it inflames and forms matter, the intention of which is to remove the irritating cause. This process is easily effected when it is on a surface whose nature is to secrete; but when on a surface whose nature is not to secrete, it then becomes more difficult, for another process must be set up, which is ulceration."* Though I have chosen to give Mr. Hunter's words, yet, if the cause ascribed by either him or Mr. Bell is admitted, the inference follows, as stated by the former; and Mr. Bell will find, that, without being aware of it, he has given a reason why the same species of contagion may produce two different effects. Mr. Hunter proceeds, "This, like most other inflammations that terminate in ulcer, begins first with an itching in the part; if it is the glans that is inflamed, generally a small pimple appears, full of matter, without much hardness, or seeming inflammation."—"The itching is gradually changed to pain," &c. Hitherto the two descriptions only differ in Mr. Hunter's being so minute as to render it tedious to every reader that is not aware of the great importance of

* Treatise, 216.

accuracy in every pathological minutia. I cannot be thought to accuse Mr. Bell of plagiarism by thus showing the similarity of language of the two, when it is considered, that I have traced the description of chancre beyond Mr. Hunter; and as it is to be concluded each delineated what he saw, we are to expect their accounts to be similar. But the concluding part of Mr. Bell's description, is all his own,—“foul and sloughy at the bottom, with hard retorted edges.” On this I shall only remark, that the sloughy bottom is not the true character, but only an accidental circumstance of chancre, and that the retorted edge is what I have never discovered, nor found described by any one. That there is a hard, thickened, and somewhat elevated edge is certain. This does not however appear on the bursting of the pimple, but is gradually formed afterwards. If retorted means that kind of edge which usually attends cancer, and arises from the fungus below the skin, reflecting the latter back on the sound part, I can only say, that none of the London hospitals, to which I have had access, exhibit such chancres, that I have never seen them in private, and that no one describes them but himself.

“It has been remarked,” says Mr. Bell, “that old venereal (secondary) ulcers do not produce buboes; and this has given rise to an opinion, that the matter produced by these sores is not of

“ a venereal nature ; that is, that it would not con-
 “ taminatè the system were it carried into the blood
 “ by the absorbents. Mr. Hunter was, I believe,
 “ the first who advanced this *opinion*.” * If indeed
 Mr. Hunter had given nothing more than *opinion*,
 and supported that opinion only by reasoning on
 buboes, Mr. Bell’s answer might be satisfactory ;
 but how much like *verberat iēlibus auras* is all the
 apparent weight of argument produced by the lat-
 ter, when compared with those experiments which
 were publicly repeated, and with such uniform ef-
 fect. † When Mr. Bell gives us the result of ex-
 periments made with the same publicity and accu-
 racy, it will be time enough to consider on which
 side the argument preponderates.

THESE secondary ulcers are said to be, for the
 most part, preceded by blotches or small pimples,
 sometimes by only a slight degree of itchiness.
 “ But,” continues Mr. Bell, “ in whatever manner
 “ a venereal ulcer may form, the appearances
 “ which it afterwards exhibits are nearly the same.
 “ A considerable destruction of parts soon takes
 “ place ; more quickly indeed than usually hap-
 “ pens from any other cause except mortification.
 “ Instead of proceeding gradually from the surface
 “ downwards, as other ulcers do, a certain portion

* Id. page 33.

† Appendix No. II. See also page 104.

“ of the soft parts between the affected spot of the
 “ skin and bone beneath seem to be contaminated
 “ at once ; for almost as soon as the skin becomes
 “ evidently ulcerated, the corresponding parts be-
 “ neath not only appear to be diseased, but are
 “ soon thrown so entirely out, that scarcely any
 “ thing but the periosteum is left to cover the
 “ bone.”——

SUCH is Mr. Bell's description of the secondary venereal ulcer: let us see how far it agrees with other authors. Sydenham speaks of phagedænic ulcers, but describes none except in the throat, and his translator and commentator acknowledges that he has failed in giving so exact a description of this as of other diseases.* Boerhaave describes a case of phagedænic ulcers, of which, however, he says, *neque in profundum penetrasse*: and that this is different from the disease *quod nunc pervulgatum Europæos affligit*.† Van Swieten, taking Boerhaave as his guide, and occasionally glancing at Astruc, says, “ *Miasma venereum in sanguinem jam receptum & dein ad cutem depositum produxisse maculas*** & post plures squamas successivas in ulcus venereum mutabantur quod copiosum tabum ex flavo—viridescentem quotidie eructabat.*”——Afterwards—“ *Lenta tamen hæc est inflammatio & per iodum suam tardo gradu percurrit non enim*

* Swan, page 310. † Prefatio ad Luifin.

“subita adeo sequitur suppuratio ut in aliis morbis
 “inflammatoriis; nec reliqui inflammationis ex-
 “tus; sed simul adest acrimonia, quæ partes cor-
 “poris quas occupat ne ossibus quidem exceptis
 “*lenta* exesione consumit & destruit.”* Astruc
 so entirely passes over this rapidity described by
 Mr. Bell, that he speaks of ulceration as a very
 late process, and always superficial.† Sauvage
 speaks only of *pustulæ crustosæ*.—Sagar uses the
 same expression.—Cullen says, “Cutis, presertim
 “ad marginem capillitii, papulæ corymbosæ, in
 “crustas & in ulcera, crustosa abeuntes.” The
 three last I have extracted from Cullen’s Nosology,
 whose superior accuracy is to be remarked in dis-
 tinguishing the ulcer covered by a crust of har-
 dened pus from *pustula crustosa*, as described by
 the other two.

THE following is Mr. Hunter’s account of the
 disease on the skin:—“The appearance of the
 “parts themselves next begins to alter, forming a
 “copper-coloured, dry, inelastic cuticle, called a
 “scurf; this is thrown off, and new ones are form-
 “ed. These appearances spread to the breadth
 “of a sixpence or shilling, but seldom broader, at
 “least for a considerable time, every succeeding

* Vol. V. sect. 1444.

† Book IV. Chap. III. sect. 2. Barrowby’s translation,
 vol. II. pages 20, and 22.

“scurf

“ scurf becoming thicker and thicker, till at last
 “ it becomes a common scab; and the disposition
 “ for the formation of matter takes place in the
 “ cutis under the scab, so that at last it turns out a
 “ true ulcer, in which state it commonly spreads,
 “ although but *slowly*.”*

I SHOULD not have troubled the reader with all these enumerations, but that Mr. Bell's account of rapid ulceration surpris'd me so much, as to induce me to compare it with the writings of all the best authors; and as I conceive the reader would wish to do the same, he may not object to a reference to them. It is impossible not to be struck with the difference, and as Mr. Bell could only have collected his description from his own observation, it is entitled to the more attention. Another peculiarity Mr. Bell remarks, which I do not recollect to have met with in any other author. “ If the mercury is laid aside before the virus is e-
 “ radicated—if the sores have healed, and again
 “ break out, although they will not go to such a
 “ depth as they did in the first process of ulcera-
 “ tion, they will spread further, and become more
 “ numerous. Although these new formed ulcers
 “ seldom penetrate to such a depth as those to
 “ which they succeeded, they assume every other
 “ appearance of the true venereal ulcer; they are

* Treatise, page 320.

“foul, and somewhat spongy—nor does mercury act
 “with such certainty upon them. The same either
 “continue obstinately to resist *the effect* of this re-
 “medy, or, if these heal, others break out, and
 “thus spread over a very considerable extent of
 “surface, giving all the appearance of the pha-
 “gedænic or depascent ulcer of different au-
 “thors.”* The whole of this account is different
 from what we see of the disease in this part of the
 world. Neither the spongy appearance, nor the
 phagedænic ulcer, nor one fore healing under a
 mercurial process, while another breaks out, are
 consequent on too slight a mercurial course. I
 should therefore think Mr. Bell has been describ-
 ing either syvens, or some morbid poison different
 from the venereal, as found in the London hospi-
 tals. What very much strengthens this last opini-
 on is, a previous passage in his account of chancre,
 by which it appears, that of seven cases of phage-
 dænic ulcers of the glans penis, he traced four from
 one, and three from another individual source of
 infection.† I have already shown this primary ul-
 cer

* See page 103, 4, and 5.

† “WHEN chancres are properly treated from their com-
 mencement, they commonly assume a healing appearance
 “in the course of a few days; but in some instances, owing
 “to neglect, in others, to some peculiarity of constitution,
 “and perhaps occasionally *to the matter of infection having*
 “*been particularly virulent*, instead of becoming clean, and

“ of

cer was well known to Celsus, and his description of the first division of *ignis sacer* will be found not less applicable to the secondary symptoms, as described by Mr. Bell. Celsus speaks of a “reddish appearance, or a mixture of red and white, the irritation of the part being increased by very small

“ of a red, healthy complexion, and which they always do
 “ before a cure takes place, they become daily more foul,
 “ and at the same time more extensive, and if their progress
 “ be not stopped by a judicious external treatment, combin-
 “ ed with a proper course of mercury, they proceed to form
 “ sores of a very considerable magnitude. The danger from
 “ these, when seated in the preputium, is inconsiderable; but
 “ in the glans, this variety of chancre is apt to go to such a
 “ depth as to prove very alarming. The danger is some-
 “ times great from the hæmorrhages with which they are ac-
 “ companied, and we are often astonished at the rapid progress
 “ of the sores. In some instances they extend so quickly, as
 “ to destroy a great part of the penis in the course of a few days.

“ This rapid progress, which chancres in some instances
 “ make, is, for the most part, supposed to depend upon some
 “ peculiarity in the constitution of the patient; for in gene-
 “ ral chancres remain circumscribed, and nearly stationary for
 “ a great part of their duration. But I have reason to think,
 “ that in some instances it proceeds *from the nature of the mat-
 “ ter by which they are produced*: I conclude that it is so from
 “ chancres of this description, being much more frequent at
 “ particular times than at others, and from observing them at
 “ the same time in *different people receiving the infection from
 “ the same woman*. About two years ago I met with more
 “ instances of this phagedænic chancre in the space of three
 “ or four months, than I had seen for several years before,

“ and

“small pustules. These usually contain matter,
 “and are often attended with heat and redness.
 “A fresh one will spread while that which first
 “appeared heals;”* [this is common to the secondary
 symptoms of the venereal and most other
 morbid poisons in their early state] “sometimes
 “ulceration spreads from the breaking of the puf-
 “tules, and a discharge follows, of a complexion
 “between pus and sanies. The disease attacks
 “principally the breast, the sides, prominent parts,
 “and particularly the soles of the feet.”† In
 his account of the cure, he considers the disease as

“and in four of them the infection was traced to the same
 “woman: the chancres in all of them appeared early, and
 “made such rapid progress, that very troublesome hæmorrhages
 “occurred from them in the space of three or four
 “days from their first appearance; and in a small town to
 “which I was lately called for an alarming hæmorrhagy,
 “produced by an ulcer of this kind, the surgeon in attend-
 “ance informed me, that in the space of a few weeks he had
 “met with three instances of the same nature, and in which
 “*the infection was also traced to the same woman.*” Page 21, &c.

* “But although eruptions of this kind disappear from
 “time to time, no advantage is gained by this: they either
 “proceed from one part to another, or they attack some
 “parts with more violence.

“In some cases these red, or rather mottled blotches, in-
 “stead of proceeding to the state of ulcer in the manner I
 “have just described, are all covered over with an infinite
 “number of very small pimples, each of which contains
 “matter.” Page 87 and 88.

§ Lib. v. cap. 28. sect. 4. page 320. Ed. Alm.

less dangerous than most spreading ulcers, but among the most difficult to be relieved by art, or any remedy but a short fever supervening. In the manner of treating the disease locally, he speaks of such ulcers as *mediocriter*, and such as *vehementius serpunt*. This description comes nearer Mr. Bell's account of secondary venereal ulcers, than that gentleman's does to any other author's description of syphilis. If, therefore, the ulcers he describes are really venereal, he is certainly justifiable in tracing the disease to the ancient Romans, and their descendants.

HAVING stated this question with all the impartiality I am master of, the decision must be left to others, as well as Mr. Bell's *unanswerable proof*, that Mr. Hunter's opinion of the impossibility of two diseases existing at the same time in the same place is fallacious. "I have
 " at present," says he, "a person who had long
 " been liable to piles, who some time ago was at-
 " tacked with condylomatous excrescences about
 " the anus, from a venereal taint; to these succeeded
 " a common abscess from inflammation, and, last
 " of all, the parts have become cancerous. As all
 " of these are *obviously* in existence at this moment,
 " upon the same parts, and as instances of other
 " combinations of local diseases are occurring daily,
 " it is with surprise and astonishment that I find
 " much labour and ingenuity employed, to prove
 " that

“ that this connection of diseases never takes place,
 “ and in a work too, which does the highest credit
 “ to the author.”*

“ No part of the treatment of lues,” says Mr.
 Bell, “ gives more distress to patients, or more per-
 “ plexity to practitioners, than the management of
 “ this ulcerated state of buboes ; which, besides be-
 “ ing accompanied with much pain, is often one of
 “ the most obstinate symptoms of the disease. Instead
 “ of yielding to the use of mercury, sores of this
 “ description often become more obstinate after
 “ mercury has been employed ; fever is apt to take
 “ place ; and where the constitution is not very
 “ robust, even sometimes end in the death of the
 “ patient.”†

SOME writers would have doubted whether
 such sores as do not yield to mercury, but become
 worse after its use, should be called venereal. But
 Mr. Bell proceeds, “ The great degree of invete-
 “ racy in these sores may originate from diffe-
 “ rent causes.” If, by *inveteracy* is here meant,
 that they are long in healing ; perhaps it may be
 accounted for by the very remedy that is used.
 And, if ulcerations of the inguinal glands are to be
 considered as venereal, whether they yield to
 mercury or not ; no wonder Mr. Bell should

* Page 100.

† Page 107.

find the constitution may be so often infected without a primary ulcer. I shall not enter into the dispute with this gentleman, whether the swelled testis is ever a symptom of the constitutional disease, or whether alopecia more commonly attends it than the exhibition of mercury. As he states his facts with so much precision and exactness, I can only conclude, as on a former occasion, that the disease is different in London and Edinburgh. I should make the same remarks of the various causes of blindness from lues venerea, which are so accurately described and so well distinguished into gutta serena, cataract, affections of the aqueous and vitreous humours, and opacities of the cornea. On these I can say nothing, having never seen any of them. The particular affections of the eye-lids, Mr. Bell observes, have never been described by any author before himself; he has certainly the merit of the discovery. Many of the anomalous symptoms too have been first discovered by himself, particularly that atrophy, or wasting of the body, which is *one of the most frequent symptoms of the advanced stage of this [Edinburgh] disease,* and from which few or none recover!

IN the next section* "Of the venereal virus" Mr. Bell undertakes to enquire upon "What part does

* Section iii. cap. 4. page 156. Vol II.

" the

“ the matter chiefly act in producing lues venerea ?
“ When received into the system, can the poison
“ remain there without producing the disease ; and
“ is it possible for a person, with the virus in a la-
“ tent state, and without any mark of disease, to
“ communicate the infection to others ?” He thinks
it “ evident that every disease, produced by conta-
“ gion, acts first upon the fluids ; and next, that the
“ solids are ultimately affected in a secondary way
“ only.” Among the few arguments to prove this,
which the nature of his work will admit, or the
danger of a contrary opinion require, we are intro-
duced to the mode in which the fluids are infected.
The fallacy of the old opinion of fermentation is
glanced at as a thing we cannot easily conceive, to
go on in circulating fluids. “ But although,” conti-
nues Mr. Bell, “ fermentation in the full accepta-
“ tion of the word, may not occur from this cause, I
“ think it highly probable, that the matter of all con-
“ tagious diseases, and *more especially the matter of*
“ *lues venerea*, has a power of assimilating to its own
“ nature a certain portion, and ultimately, perhaps,
“ the whole fluids in the body.” When I know what
this assimilation means, or when I am convinced that
all the fluids partake of the venereal, or any other
contagion, I will then enquire whether the matter
of all contagious diseases, and particularly that of
lues venerea, has this assimilating power. At pre-
sent I shall not undertake to question whether, as

Mr.

Mr. Bell *reasons*, this power may be quick or slow, according to circumstances;—how large the accumulation must be before it is capable of exciting a morbid irritability;—in what manner the solids are affected from the fluids, so as to produce the disease, &c. &c.

MR. Hunter's assertion, that the blood of a pocky person has no infectious property, as wounds and scratches that draw blood do not produce chancres, is readily answered. "I have at this time," says Mr. Bell, "a gentleman under my care, whose case affords an instance of this, where the bites of leeches, applied to the scrotum on account of inflammation in one of the testes, the consequence of gonorrhœa, degenerated into venereal sores. This, I informed him, would not have happened from gonorrhœa alone; and, upon enquiry, he acknowledged that six months ago he laboured under chancres and bubo, for which he suspected he had not used enough of mercury, and that for a week past he had felt uneasiness in his throat, where I found, on inspection, that an ulcer had already formed on one of the tonsils. In the course of business I have met with a variety of similar cases, and every practitioner must have done so."—"Mr. Hunter was resolved, however," continues Mr. Bell, "not to rest satisfied with asserting, that the
" blood

“ blood of a venereal patient could not communi-
 “ cate infection. In the same page, *viz.* 292, he
 “ even says that the matter produced by such ul-
 “ cers as occur in lues venerea is not venereal,
 “ and will not produce the disease.” Mr. Bell
 might have added after “ in the same page, *viz.*
 “ 292, he says,” “ in the next, *viz.* 293, he proves.”
 But as it is extremely inconvenient to argue a-
 gainst facts, Mr. Bell chuses to answer Mr. Hun-
 ter’s reasoning, and to omit his experiments, to
 which the former is only introductory.* That
 the disease may be communicated by a person in
 whom it is latent Mr. Bell proves, by telling us
 he knows from a variety of facts, both father and
 mother, in situations such as this, have communi-
 cated the disease to the fœtus. “ Where this hap-
 “ pened from the mother being infected, the mat-
 “ ter of contagion must pass from the mother to
 “ the child along with the blood; but where it
 “ takes place from the father, as is most commonly
 “ the case, there is no other mode in which it can
 “ be conveyed but by the semen.”—Certainly!

OF the remedies used in lues venerea, I shall
 only take notice of Mr. Bell’s own opinions, and of
 his answer to Mr. Hunter’s; the latter occur first.
 “ The symptoms of lues venerea being consider-

* SEE the passage referred to in Mr. Hunter, or Appen-
 dix, No. II.

“ ed by some as the effect of a peculiar irrita-
 “ tion, excited by the presence of the poison in
 “ the system, they have also ventured to say, that
 “ mercury cures the venereal irritation, as they
 “ term it, by producing an irritation of a different
 “ kind.”

* * * * *

“ LOCAL pains may for a time be suspended, or
 “ they may even be removed altogether, by irri-
 “ tation of a different kind being excited, either
 “ in the contiguous or some more distant parts of
 “ the body; but we know of no constitutional dis-
 “ ease that has been cured by such means. This
 “ affords, therefore, much cause to suppose, that
 “ the opinion is ill founded, when applied to
 “ the action of mercury in the cure of lues
 “ venerea; for when no such effect is found to
 “ take place in other diseases, we cannot, with-
 “ out the strongest proofs were given of it, be led
 “ to imagine that any thing so very unusual should
 “ take place here. Mr. Hunter indeed gives
 “ as the chief reason which he suggests in support of
 “ his opinion, that ‘ the disease can, in many cases,
 “ be cured by raising a violent stimulus of another
 “ kind.’* “ I must say, however, that I never
 “ met with an instance of this, nor have I been
 “ able to hear of any who have done so. Neither

“ * VIDE Mr. Hunter's Treatise on the Venereal Disease,
 “ page 365.”

“ has

“ has Mr. Hunter, in treating of the method of
 “ cure, mentioned any article for the purpose of
 “ curing the disease in this manner.”*

THE short quotation from Mr. Hunter, contained in this extract, is correct, except in punctuation. In the original it ends with a semicolon instead of a period, after which follows—“ and perhaps if we could raise such a constitutional irritation without danger, as we often can in local cases, we might cure the venereal disease in the same manner; and in one quarter of the usual time.”† By this it follows, that the irritation referred to by Mr. Hunter, is the local one which cures gonorrhœa, (see Treatise page 77) and that he acknowledges lues venerea is not to be cured in

* Vol. 2, chap. 4, sect. 4.

† It is impossible not to remark the coincidence between this reasoning and Celsus's observation on the cure of other morbid poisons. We have found all other morbid poisons that yield to mercury are cured by a slighter irritation than the venereal. Celsus tells us that a short fever is the most powerful remedy for *ignis facer*. Might we not hence infer, that a constitutional irritation without mercury might be raised so high and kept up so long as even so supersede the venereal action? The history of the supposed venereal disease, communicated in the Edinburgh Medical Essays before alluded to, contains a case in which the disease from the poison appears to have been superseded by the small-pox.

this

this way. But the question is much narrowed if one of Mr. Bell's premises is admitted. "From time to time," says he, "we meet with constitutions in which none of the usual effects of mercury are produced, and yet even in these it does not fail of curing the disease. In all such cases too, the cure is accomplished with more ease than where mercury operates in the usual way: and it is worthy of remark, that though all the more acrid preparations of mercury will cure the disease, yet their stimulating properties are found to be so hurtful and unnecessary, that almost by the universal consent of practitioners they are now generally laid aside."

THE first part of these remarks again convince me, that Mr. Bell has been describing some other disease besides the venereal. Diseased actions arising from other morbid poisons, we have found, where they yield to mercury, are always relieved with less quantity than the venereal requires, or with no apparent constitutional irritation. But in London the cure of this disease is not accomplished with more ease in constitutions in which the usual effects of mercury are never produced, than where mercury operates in the usual way: for though, as I before observed, the determination to the salivary glands is not always necessary, yet the inveterate chancre is never cured without other

symptoms of mercurial irritation. It is admitted too by the best practitioners, that the mercurial salts, which have been observed to be most successful in the cure of syvens, are not to be depended on in the venereal. Nor is this on account of their stimulating properties, for excepting on the stomach and bowels, they are found less stimulating than the pure mineral, continued for a sufficient time. But where facts appear so different, all reasoning is nugatory: the decision must be left to the observations which have occurred to practitioners.

OF all the opinions," says Mr. Bell, "that have yet been advanced concerning the operation of mercury in the cure of lues venerea, that which supposes it to act as an antidote, is liable to the least difficulty."

* * * * *

WRITERS of this class deny the antidotal power of every medicine, merely from their not being able to account for the manner in which they act. They do not consider that many of the most common operations of chemistry are equally inexplicable. Thus no person can say why, or in what manner the properties of an alkali should instantly be destroyed by an acid brought into contact with it. I do not say that the operation of mercury in the cure of syphilis is analogous to this; but the one I think is equally in-

not

" com-

“comprehensible with the other, and that mercury acts in this disease consistently with the idea which we affix to an antidote is at least highly probable. What I conceive to be the property of an antidote is, that when it comes in contact with the poison, for which it is appropriated, it either renders it altogether inert, or so far changes its nature, that no deleterious effects are produced by it.”

IF this reference to an acid and an alkali is intended as an illustration, Mr. Bell might as well have taken Sir John Cutler's worsted stockings, which by frequent darnings were converted into silk. But he does not “say that the operation of mercury in the cure of syphilis is analogous to this.” What then is it introduced for?—To show that one is equally incomprehensible with the other. For this purpose then the meat-roasting quality of a jack, taken from the same author, would have been equally to the purpose: for though we know that a jack has a certain meat-roasting, as mercury has an antidotal, quality, we are as perfectly ignorant of the cause of gravitation by which the former is produced as of the manner in which mercury comes into contact with the poison, so as to render it inert. Perhaps Dr. Watts's examples of mere words, which have no ideas belonging to them, may be hereafter enriched by this *antidotal quality of mercury* in addition

tion to the *ustorious and cleaving powers of a convex glass and wedge, or the sonorous and indicating forms and qualities of a clock.*

BUT it must be acknowledged, Mr. Bell has produced an experiment, not indeed of his own, nor of the writer from whom he has quoted it without thinking it worth while to examine the source. "We know by experiment," says he, "that the matter of lues venerea, on being mixed with triturated mercury is rendered inert, and will not produce the disease." The fact is recorded by the best authority, the late Doctor Cullen, who having supported more ably perhaps than any other person, an opposite opinion, we are not to suppose that he would have given room to what militated so strongly against it, *if he had not been convinced of the fact being well founded.* His words are, 'A physician took a quantity of matter from a venereal chancre, and mixing it with a quantity of Plenck's gummy solution of mercury, he applied this mixture to a sound person, but could not find that it produced chancre or any other syphilitic symptom.* This fact of itself might be judged to be *decisive*, but more complete conviction is obtained of its being so from every

* VIDE a treatise on the Materia Medica by the late Doctor William Cullen, vol. II. page 448."

" view

“ view that we can take of the chief phenomena
 “ observed to occur from the operation of mercu-
 “ ry in the cure of this disease.”

PERHAPS this extract might have been shortened, by giving a few lines that follow the quotation contained in it from Doctor Cullen. These are the words. “ This may seem to afford some conclusion ; but as I am not acquainted with the circumstances of the experiment, nor have any account of its being repeated with attention, I cannot admit of any conclusion from it.” Such are the reflections of the writer from whom Mr. Bell borrows his *decisive fact!* of which it can only be said, that though the person on whose authority he quotes, thought it amounted to nothing, it will do for Mr. Bell. If that gentleman took it on the authority of Plenck, it may still be doubtful, “ notwithstanding,” to use Mr. Bell’s expression on another occasion “ of all that Plenck, the inventor, has said to the contrary.”*—The antidotal question, as defined by Mr. Bell, must rest on the following points.

Do we know of any disease that is cured by a remedy, which coming in contact with the matter producing such disease renders it inert ?

* Bell, vol. II. page 233.

SECONDLY, Do we know of any constitutional disease that is cured by exciting an irritation different from that disease?

THIRDLY, Have we any proof that mercury produces any particular effect on open sores not venereal, so as to alter their action?

THE first being an antidotal question, must be left to Mr. Bell, for I confess myself entirely incompetent even to reason upon it.

THAT constitutional diseases are often cured by irritations excited different from the former, we are not, I think, in want of proofs. The ague is cured by a variety of stimulating remedies, and many chronic diseases yield to gout or to acute fevers of different kinds. The irritation excited by mercury is found the most powerful remedy for the liver complaint of the East-Indies, and gout, which is a remedy for so many chronic diseases, has often yielded to violent mental irritations.

ALL I have to urge on the property of mercury, to produce an alteration on sores of every description, is contained in the account previously given of the actions excited by mercury. See Page 118.

I SHALL

I SHALL conclude my remarks on Mr. Bell, with his account of the disease in infants.* This the author thinks it necessary to be the more particular upon, because Mr. Hunter has, *unguardedly*, fallen into the error, that the disease cannot be communicated to the fœtus in utero. It is hardly credible what numbers besides Mr. Bell have *unguardedly* fallen into this among many other errors concerning Mr. Hunter's opinion. It has even been said, so determined was Mr. Hunter to support his theory, that he would not admit the small-pox could be communicated in that way. This has been asserted by one writer, and both Mr. Lynn and Mr. Turnbull, in the account published by them of children born with the small-pox, seem to hint at something of the kind. I have heard it indeed asserted by many, but upon what authority I have always been at a loss to determine. Mr. Hunter, in the Philosophical Transactions, has given the most decisive proof of the fact of any that has hitherto been offered. He has shown that the pustules of a fœtus had the true variolous character, that of slough at the bottom of each, as far as he examined—a peculiarity that has been so entirely overlooked by every other writer, that nothing but his wish to ascertain the fact could have induced him to record it. Unless my edition of his Treatise is different from others, the reader

* Vol. II. Chap. IV, Sect. IV.

will find, in page 291, two ways in which Mr. Hunter conceives the fœtus in utero may be infected with syphilis: one is by absorption of matter deposited in the vagina; and which, without producing any local effects on the mother, may be conveyed pure to the fœtus; the other by absorption from the mother's chancres. These he considers as unlikely, but possible, means of infection. It is true this mode of accounting for the disease in infants will not extend to those *remarkable* cases related by Mr. Bell; and such as he conceives other practitioners must have met with. In one instance, Mr. Bell was consulted for a child seven or eight days old, covered with a *venereal rash*. The parents had been married nearly three years. The mother does not appear ever to have been infected, nor the father later than six months before his marriage. Their first born, for whom Mr. Bell unfortunately was not consulted, had died of the disease. The virus was extirpated from this couple, if the woman ever had any, by salivating the husband, as was the case in some other instances! Is it not a little remarkable, if the virulent semen did not contaminate the women, that they should have escaped being infected by their pocky fœtuses?

Mr. Bell has also met with a number of instances of abortions from this disease on the sixth or seventh month, but the latter is the most frequent

quent period. In a considerable number of instances, where abortion had happened on these or the eighth month, a well conducted course of mercury proved effectual. The difference of these periods seems to depend entirely upon the quantity of virus in the parents habit: for if the remedy has only rendered a part of it inert, the period of abortion has been protracted to the ninth month! “In two instances,” says Mr. Bell, “it [mercury] did not answer so completely as in the others, but in both there was sufficient evidence of too little mercury having been employed; for the children in both instances were kept till the beginning of the ninth month, which in one was two months, and in the other six weeks later than had been the case before, and both the parents having in each of these instances been made to take mercury again, and in greater quantities than before, no abortion has since taken place, and each family has within these *few* years had *several* healthy children.” How numerous might these prolific families have been, had their surgeon been possessed of a “magnetic test,” or “any divine intuition!”

THUS far have I proceeded with an undertaking, which it is to be feared will impress the reader with no very favorable opinion of my temper. When I look over the printed sheets, I feel an apology necessary.

necessary, and at times a strong inclination to decline publishing. Without personal resentment to any individual, I have betrayed myself into a severity of language I little expected to have used; and with only a wish to vindicate a name that can derive no lustre from me, have unwarily issued something like a general challenge. When to this consideration is added the reflection how little prepared I must be for such an undertaking, and how very incorrect many parts of the preceding sheets will be found; it becomes me at least to offer every apology for them. Those who are acquainted with the unprofitable fatigues of the most laborious branch of a laborious profession in an over-grown town, will only wonder that a practitioner should venture to write at all. When in addition to this, are pleaded a constitution very unequal to the common fatigues of business, a scanty library, and the want of those opportunities which the practice in public institutions afford, some indulgence may at least be craved.—But it may be asked, Why then publish? As I do not plead request of friends, let me relate the circumstances that gave rise to this attempt.

IMPRESSED with a warm sense of gratitude to that name which has occurred in almost every page, and weary of hearing it so often abused, whilst his opinions were so little understood: I had frequently

ly felt a wish to see how far they might be explained by contrasting them with his opponents, and clothing them in a language different from his own. Little aware of the magnitude of the undertaking, I made the proposal to Mr. Hunter, who not only received it with readiness, but acquainted me with an author I had not before heard of. Flattered as I felt by the conversation this gave rise to; it was impossible not to be more interested in one to which it suddenly reverted. This was no other than that organic affection which in three days afterwards deprived physiology of the further labours of one who seemed born to make researches into the laws and operations of animal life. If the hopes of presenting this tribute to him were at an end, there was at least added an opportunity of indulging a warmth of expression which before would have been less becoming.—When that heart ceased to beat, the irregular actions of which never fail to produce a corresponding irregularity of temper—when that voice became silent which so often seemed the interpreter of nature, and when that patronage no longer existed which might have rendered this feeble tribute suspicious—it was then that what before was only a proposal seemed to impose the obligation of a promise.

DISTANT, however, as the space between us in professional rank must for ever remain, a publication

tion like the present, soon after the death of Mr. Hunter, might have been suspected of some view to interest. With these doubts I acquainted Dr. Baillie, having had an opportunity of conversing with him soon after the loss he had sustained in so near a relation. Whether we perfectly understood each other I cannot at present determine; but he seemed to express a wish that I should at least postpone my design. A few months afterwards a similar conversation occurred with Mr. Hume, which was closed by that gentleman's expressing a wish that I would publish.

It may still be asked, if it was thought necessary to answer Mr. Hunter's opponents, why all this apparently extraneous matter? Which way is the language of medicine, as used by authors who have never written expressly against Mr. Hunter, connected with the present object? What has Bacon and the progress of philosophic improvement to do with a controversy confined to a small circle? It is hardly necessary to answer that no undertaking can be limited but by the abilities of the performer.

Nil actum reputans si quid superesset agendum.

If I could have found a shorter way of accomplishing my object, I would certainly have pursued it, and it is not without several different attempts, and expunging much that I had waded through, of the
darker

Is it not Leonard Home?

darker ages of medicine, that the work has assumed its present imperfect form. To trace Mr. Hunter's opinions in one disease, it was necessary to explain some of his discoveries that are applicable to all. To do this it was often necessary to use his language. To convince the reader of this, it was necessary to show the insufficiency of the language adopted by some of the best received medical authors; and this will, I hope, be accepted as an apology for bringing together names that would not otherwise have been met with in the same production. It was also necessary to produce some standard by which language and reasoning should be regulated in physiological enquiries. To me there appeared no readier way than by recurring to the fountain head. In perusing Bacon, it was impossible not to be struck with the similarity of the path marked out by that philosopher and the one pursued by Mr. Hunter. In proving any axiom, we find him constantly recurring to certain facts, their series and order, overlooking no objection that can be started, and even encumbering himself with such as might have been overlooked by others. From a similarity of sentiment he has necessarily fallen into a similarity of language. Thus we hear him constantly describing the pure individual ACTIONS of those bodies which are the objects of his researches, in order to detect the LAWS that govern them. When these are ascertained the different

FORMS under which they appear, are easily understood and accounted for: and science, thus built upon experiment, may be truly said to be progressive.

HAVING thus made my apology for appearing before the public, it may be allowed me to plead, that what advantages my situation as a London practitioner have afforded, have not been neglected. This I am the more ready to do, as it gives me an opportunity of acknowledging obligations I have no means of returning. If my stock of books is small, I have not failed to avail myself of the public libraries I could have access to, or of those private collections, the owners of some of which have already been mentioned. If I do not particularize the rest, it is because I have acquainted every practitioner that has fallen in my way with the objects of my enquiry, and have availed myself of the conversation, cases, and collections of each. Among the rest I must not omit the kindness of Dr. Pitcairn, to whose liberality and industry I am indebted for several particulars in the case of sloughing phagedæna, the progress of which has furnished me with an illustration of many other morbid poisons. To Dr. Pitcairn's I should add another name, but it might lead to too close a description of the patient himself. To Mr. Cline I am indebted for several hints on the phagedænic effect
of

of mercury, and also for an introduction to such wards and cases in St. Thomas's as he thought most likely to forward my enquires, and in such a manner as enabled me at any time to renew my visits. To Dr. Woodville I owe the same advantages in the Small Pox and Inoculating Hospitals, which were seconded by the most polite attention of Messrs. Wachsell and Uppom, Apothecaries to those institutions. To the medical gentlemen of the Middlesex I owe the opportunity of visiting their cancer ward, after an introduction by Mr. Howard. Nor can I omit Mr. Fearon, who unreservedly opened to me his cases, his observations, and his preparations: I must regret, that he did not direct me to Justamond's cases of cancer till this work was nearly printed off. Not on account of any improvement that author has suggested in the manner of treating the disease; but because among his cases there is one which scabbed over for a length of time without any operation. This is easily understood, if my opinion of the disease is well founded. Under this division are also some cases of phagedæna of the penis evidently distinct from venereal. About the same time, Dr. Willan put into my hand Mr. Pearson's Practical Observations on Cancerous Complaints. That gentleman's extensive reading would have led me to some similar cases of cancer from Wiseman; he gives also one of his own. There is also a description of what he

he calls venereal ulcers, that continue to spread during a mercurial course. This description is so very similar to that of Celsus, in a passage before referred to by Mr. Pearson, that I can only feel surprized at his calling the disease venereal.—But this is returning to a subject already discussed.

THUS, whatever may be the imperfections of the work, I wish not to avail myself of the excuse of a hasty performance: what I have offered has been the effect of the closest application (as far as professional interruptions would admit) aided by the advantages of residing in a large city among liberal practitioners. If, after all, my labours should be thought unworthy of notice, I can only add, that I have fulfilled what appeared a sacred engagement, and made the only return in my power for obligations that never can be repaid:

ὁ γὰρ γέρας ἐστὶ θάνατος.

APPEN-

APPENDIX.

No. I.

THE following cases are inserted in proof of the difference between the manner in which parts heal after a loss of substance from a morbid poison and any other cause. In the first instance the glans and prepuce both granulated, the former so as to restore the lost substance, the latter, as far as could be expected, considering the form and situation of the prepuce.

*Copy of a Letter from Mr. Browne, of Spitalfields,
Surgeon.*

DEAR SIR,

I HAVE sent you the following case, because I think it tends to prove the truth of the observations I have heard you make, concerning the different manner in which parts are healed after a loss of substance by morbid poison, or by other means.

E. F. a man about thirty-seven years of age, married, and having several children, the youngest

Y

not

not six months old, complained in November last of pains in his legs, attended with inflammatory redness. This yielded to saturnine washes and mild sudorifics.—The latter end of November he was seized with a violent pain in his penis, which lasted six or seven hours, and then went off suddenly. The next morning the pain returned to an excruciating degree, and lasted nine hours. The penis was turgid and grew livid: he was bled, fomentations and poultices were applied to the part. The blood was very fizy: the pain left him suddenly again, but never returned. A mortification however ensued, and a deep flough separated from the penis. There was a great loss of substance from the whole circumference of the penis, for more than half its length, but more particularly from the upper and under part of it. The prepuce, which was originally of a natural length, floughed away so as to expose the glans, from which two small floughs had separated. This sore, however, soon put on an healthy appearance, without the use of a grain of mercury, externally or internally, and granulated as fairly as ever I saw a sore in my life. The loss of substance was entirely filled up, and two sores, somewhat smaller than a silver penny, which were upon the glans from the mortification, granulated also, and filled up completely. The man is now perfectly well, and from the present appearance of the parts, you would not suspect there had been
been

been a loss of substance, excepting at the extremity of the prepuce, which remains considerably shorter.

THE case was new and curious to me, and if it strengthens your opinions concerning the manner in which nature performs her work, I shall receive a pleasure in having communicated it to you.

I remain respectfully,

Your sincere friend,

E. BROWNE.

Case communicated by Mr. Wadd, of Basinghall-street, Surgeon.

A GENTLEMAN of a good constitution, but much addicted to sensual indulgences, contracted a chancre on the glans penis. By the use of mercurial friction and pills, his mouth became sore, but the chancre, instead of healing, shewed a smooth spongy surface, a little elevated above the surrounding cuticle. Whilst I was watching its progress, and making up my mind whether to touch the part with lunar caustic, or leave the process to nature, the patient became weary of the restraint put upon his pleasures. Three days after a debauch with women and wine, I found him somewhat feverish, the penis swelled to an enormous size, a phymosis formed, and a beginning sphacelus in the upper

part of the prepuce. Fearful lest there should be any remaining taint in the old chancre, and recollecting that his constitution was not very soon affected by mercury, I returned to the frictions with the bark, carefully at the same time watching the progress of the disease and the remedy.

IN a few days a considerable portion of the upper part of the prepuce and corpus cavernosum sloughed off, deep enough to leave the urethra bare; and what was left of the glans, for the chancre had made great ravage, protruded through the opening. The remaining part of the penis continued much above its natural size, which showed the cavity on a larger scale. The observations I have heard you make, relative to the healing of parts after loss of substance from a morbid poison, prepared me to watch the progress of it in this case. In a few days the whole of the fore looked clean and healthy, and the cavity on the glans occasioned by the chancre speedily skinned over without even an incipient granulation; the corpus cavernosum granulated with a rapidity that might be expected from so sanguiferous a part, and where the inflammation had run through its stages so quickly. When filled up it contracted, but as is usually the case, the formation of the cicatrix was a little tedious. A hole was left in that part of the prepuce which

which should cover the glans, perhaps from the inner surface having been in contact with the chancre.

THIS gentleman has since communicated to me a case under his care, in which rapid ulcerations of the glans and prepuce were succeeded by a sloughing of each, leaving only a narrow portion of the latter of its full length. After sloughing the parts looked clean, and as if prepared to granulate; but the whole was skinned over without a beginning granulation.

APPENDIX.

No. II.

*Copy of a Letter from Dr. Clarke, Queen-street,
Golden Square.*

DEAR SIR,

As you desired to see the notes, which I took at the time, of a case which happened in St. George's Hospital, and which is referred to by Mr. Hunter in his work on the venereal disease, I very readily comply with your request. From particular circumstances I paid great attention to the case, and marked down from time to time every thing which was done in the way of experiment, so that I think you
may

may depend upon the statement which I transmit to you.—In 1782, Caroline Dare, the wife of a soldier, was received into the hospital on account of some ulcers, principally in her legs. She had contracted the venereal disease from her husband, about the end of the year 1781. About Christmas last she had a bubo, which suppurated and was healed. Some time after this, blotches came out all over her body, some of which ulcerated. She has at this time several ulcers under her arms, and upon the breasts near the nipples.

SHE bore twins since Christmas, (having only advanced eight months in her pregnancy) both of which died. They were born, as she reports, with blotches upon them.

ANOTHER child, now in the hospital with her, and which she has habituated to suck her, though nearly three years of age, is also covered with blotches.—Mr. Hunter made these questions:

How did the child receive the infection?

DID it take place from the secondary ulcer on the nipple?

To ascertain whether the ulcers were infectious, that is, whether the matter of them would have the specific effects of venereal matter, Mr. Hunter ordered her to be inoculated with some of her own matter,

matter, and with some primary venereal matter from another person, to see whether or not chancres would be produced equally from both. This was done Sept. 18, 1782.

SEPT. 19.—The puncture where she was inoculated with some matter taken from an ulcer on her knee, seemed to be inflamed a little; the other was not inflamed at all.

SEPT. 21.—Both the punctures have suppurated, and have the appearance of small-pox pustules.

THESE both became sores for some time, but she was forbidden to take any mercury.

OCT. 22.—The ulcer formed from the inoculation with the matter taken from her knee is healed perfectly, but the ulcer from the other venereal matter remains, and spreads very fast, and is extremely painful. She was therefore ordered to be salivated, and perfectly recovered from all her complaints.

ON the 21st of September, Mr. Hunter directed that the child should be inoculated with some matter from one of its own ulcers, and some matter from a common sore, which was done Sept. 22.—They both inflamed to a trifling degree, but did not go into suppuration.

FROM

FROM the result of these experiments made on the woman and the child, Mr. Hunter concluded, that the matter from a primary venereal ulcer is alone capable of infecting, whilst that from a secondary ulcer may produce an inflammation, and even an ulcer; but both these are capable of being cured without mercury, and therefore have not the specific nature of venereal sores.

IN another instance Mr. Hunter directed a patient to be inoculated with some matter from a large venereal ulcer in the throat of a man. A slight inflammation was produced in this case, but it healed without employing any mercury.

SINCE I extracted the passages above from my notes, I have looked into Mr. Hunter's book again, and find that he has related the circumstance so nearly in the same manner, that I might have saved you the trouble of reading this, which only serves to prove, from the observations of another person, the accuracy of Mr. John Hunter, which, however, cannot receive any additional strength from my testimony.

I am,

Dear Sir,

Yours, &c.

JOHN CLARKE,

Queen-street, Golden-square,

Oct. 16, 1794.

THE END.

