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.

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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.

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MDCCXCV.

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.

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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 ſanies, read lymph.
Id. line 21, for it, read them.
251, line 5, from bottom, in the note, dele only.
272, line 5, from bottom, for ungrateful, read ungraceful.

CHAP. 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 "heredi-"tary 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 ne-
" ceſſarily 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

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 hap-"pens 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 ſubſe-"quent 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 surprised 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

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 acci-

dent

dent than the effect of thoſe laws, the uniformity of which, deſerves a better name than "unnatu-"ral 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 theo-

ries

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 veſ-"ſels 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 ſur-"geon 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 ve-
" nereal pus does not act as a ferment. Mr.
" Hunter's opinion muſt therefore be well found-
" ed;" (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 endea-
" voured 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 ten-
" dency 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 difference*, 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 ferment 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* fermentation. 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 evi-
"dent that it irritates the ſolids. A complete ex-
"planation of the phænomena of the diſeaſe,
"therefore, can only be obtained by admitting
"both the theories of fermentation and irrita-
"tion."

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?

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 deſtruc-"tion of parts are offered in vindication of ſo un-"common an aſſertion, that the diſcharge from the "urethra is pus; why has the profeſſor ſo roundly "given this ſecreted fluid the unqualified appella-"tion 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

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 nondum obtinuiſſe.**

To thoſe who wiſh to correct a part of the writings 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 blenorrhagia, from βλεννος (it is uſually written βλεννα), mucus, and ῥεω, 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*, Chaudepiſſe. *Italis* Gonorrhœa

Character in viris. Titillatio, præcipue in urethræ parte anteriore, in lacuna Morgagni ſub frænulo; ſubſequente poſt biduum aut quatriduum, 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 Fœminis, &c.———

Variat ratione ſedis

	Materia fluente
Blennorrhagia balani	ex glande
——— urethralis	ex urethra
——— veſicalis	ex veſica
——— labialis	e labiis vulvæ
——— vaginalis	e vagina
——— uterina	ex utero
——— naſalis	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 veſicæ
——————— 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 *muciſluxus*, 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 va-

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.

nereal

nereal 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 opthalmia 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 infect-"ed 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 ve-"nereal 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 gonorrheal 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 vaginæ,
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.

CHAP.

CHAP. 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-

Torricellian 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 axiomate, forma *inductionis* alia, quam adhuc in usu fuit, excogitanda est; eaque non ad principia tantum (que vocant) probanda et invenienda, sed etiam ad axiomata minora, et media, denique omnia. Inductio

" ing an axiom," ſays he, " a new form of induc-
" tion 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. At *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.

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 na- "ture 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 τϑ οντος ζητησις 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 ſurpriſe 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 for ever 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 fœtus. 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 phænomenon. 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

ſyllo-

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 digeſtione mentis; ab excellentia functionum & remediorum ſequentium non curantur. Nov. Org. Aph. 50.

contented

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

or

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.

* Cauſae rerum naturalium non plures admitti debere, quam quæ et *veræ* ſint & earum phænomenibus ſufficiant. Dicunt enim philoſophi natura nihil fruſtra fit per plura quod fieri poteſt per pauciora. Natura enim ſimplex eſt & rerum cauſis ſuperfluis 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 auctis 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 Lineæ, 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 (which) renders them inattentive to the improvements of other nations, or (is it) 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

their backwardneſs in Phyſiology to be aſcribed to

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.

CHAP.

CHAP. 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 hydrophobiac 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 Dimſdale, 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.

Dimſdale'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 eryſipelatous 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 phænomena 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 intermedi- "ate ſtate. What is named diſpoſition, appears "to me impoſſible; for no part of the body is "diſpoſed to diſeaſe, although all are ſuſcep- "tible 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. 72.

the

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 ſcarlitina, 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 reduceable 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 ſloughs 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 reſem-"ble, 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.

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In this fungating property, it ſhows ſome reſemblance to the ſivvens of Scotland. But in the latter the funguſes riſe after ulceration. In this diſpoſition to form fungus, ſivvens 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 ſivvens does not appear from the accounts I have been able to collect. Mr. Bell,* and Dr. Swediaur,† conſider ſivvens, 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 ſivvens. Mr. Bell tells us too, on the authority of a practitioner, whom he does not undertake to refute, that ſivvens 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 ſivvens, 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.

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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 phænomena 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 ſivvens, under its medical article. But

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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 funguſes can be deſtroyed by applying eſcarotics as low as the ſound 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 bookſellers 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;

termined; 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 abſceſs, 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 Eſſays.* 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 Eſſay,‡ 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

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 ſore 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 φαγεδαινα 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 obſcænarum partium vitiis* 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.

* This is not correct. See Celsus. *

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

* Lib. v. cap. 26.

actions

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 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 ſurpriſe 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 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 ſanies. 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 *quædam*

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 venera, 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,

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

of the word *vitiatum*; and his deſcription, *primo fere pinque 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 audacter 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 Eſſays,* 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

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 ſloughs, 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.

CHAP. IV.

OF THE FIRST LOCAL ACTIONS INDUCED BY MORBID POISONS.

LET us now keep the caſes above related in view; and if, at the ſame time, we conſider the natural hiſtory of an ulcer, we ſhall be able to form a juſt idea of phagedæna, and the other appearances we have enumerated in morbid poiſons.

WHEN a diviſion of any ſolid part on the ſurface of the body takes place, the moſt ſimple operation of nature is to form a kind of ſcab over it. By this means the whole is defended from the external air; the blood or other interſtitial fluid is abſorbed, and the ſides unite by contracting cloſer to each other, or by the appoſition of new formed ſubſtance; after which, the ſkin recovering its ſmoothneſs, the ſcab falls off. But the injury may be of ſuch a nature, or the parts may be kept ſo long divided, as that the edges muſt die for want of a due circulation. The neceſſary conſequence of this is, that before any other proceſs can commence,

mence, 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 ſore 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 ſore 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 callus, & circum oræ craſſæ livent. Poſt quæ quicquid medicamentorum ingeritur parum proficit, quod fere negligenter curato vulneri ſupervenit.

Cels. 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,

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

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

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 immediate

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.

CHAP.

CHAP. V.

OF THE MODE OF CURE, ORIGINAL AND REMEDIAL, AND OF THE DIFFERENCE 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 reduceable 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 [illegible] is fairly applicable to this pro

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

ceſs. 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 eſcarotic 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 ſeconda-

* 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~~ pustules 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

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 of ~~theſe~~ 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 ſlighter, the ſlough not being caſt off ſo early, the healing proceſs, which in this inſtance would be ſkinning, cannot commence

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 maturated. 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 ſuc-
"ceeded," 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 alter-
"nately do at length make thoſe pits that frequent-
"ly 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.

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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 adheſive inflammation. Hence it is obſerved in confluent caſes, the puſtules of the face do not riſe with the convex figure which marks that complete 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 perceive, 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 inoculated 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 fœtus. 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 fœtal 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 ?

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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.

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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 ſivvens, 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.

ſorption. 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

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 *fœtor*, 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, "increaſ-"ing 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 liv-"id 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 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

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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 head-aches 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 glans, which is neither conſtant nor always neceſſary, we find it producing head-ache, debility, and a total inca-

* Medical Tranſactions, vol. 2, page 325.

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pacity 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.

CHAP.

CHAP. 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 deſecting 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. lxx.

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

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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,

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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 ſlough, the *ſloughing* 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 ſloughing ulcers in the vagina, ſo that if they occur at all, it is at moſt but rarely.

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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 fewer words; where the slough cast off readily the part skinned over, and where that process was interrupted 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 Turner should propose curing the penis by local applications 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 between 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 excdens* 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 the 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 cox-comb, 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 Curtorum Chirurgia.

nomy,

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

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.

CHAP. VII.

OF CARCINOMA AND OTHER LOCAL DISEASES USUALLY CALLED CANCEROUS.

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

by the diet and reſt of an hoſpital. Dr. Akenſide,* 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 ciſts, 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 ciſt 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 ciſts 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 ciſts 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 ciſts 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 ciſts 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 ciſt, 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 opake 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 ciſts, as it ſometimes continues for a conſiderable length of time, and in great quantities. This evacuation of the ciſt or ciſts, 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 mean while 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 abſceſs. How much is it to be regretted, that a phyſician, who has enriched the Pharmacopæa 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 Dran† 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 38 & ſeq. ‡ Id. Page 43.

But

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 recepit, a carcinomate quod non recepit 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 encreaſe 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.

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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 ſound 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 ciſt is formed. Inteſtinal hydatids are frequently found contained in one large ciſt, 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

in a ſingle ciſt, 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 ciſt, 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 ſcirrhus 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

have been conſidered as cancerous, and his own importunate advice in favour of early amputation proves his opinion of the precarioufneſs 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 inciſted 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 ciſt, 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 ciſt, ſo that how great ſoever their multiplication may be, it can only tend to encreaſe that ciſt as long as it remains entire. That in this

* Id. page 70.

† Philoſ. 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 ciſt and the ſurface of the ſkin. Hence if the ciſt 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 eſcarotics. 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 ſpongeous ſubſtance, anſwering the purpoſe of a placenta. After this ſtatement may I not be allowed to aſk,

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

2dly, 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.~~ lymph.

It has been aſſumed, perhaps a little too generally, that blows occaſion cancer. But the inſignificancy 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~~ them. 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. 70.

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 eſcarotic 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

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

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 ciſts, 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. xlvii. 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 ſcirrhous 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 ſub-"ſtance, interſected by ſtrong membranous divi-"ſions." 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 233.

"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 nature. Life is ſhort, and our art much too extenſive for the limitation of our capacities. Objects of diſtreſs 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 ſufficiently 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 enquiries we find ourſelves overtaken with new paſſions, 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 "ſilky 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

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 inciſted hydatids were thin, and ſome of Watſon's were filled with bloody lymph.

tiply

tiply 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 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.

THE reader who has had the patience to travel with me thus far, need not be informed of the labour with which my materials have been collected. Moſt of the diſeaſes deſcribed in the former ſheets have been hitherto little noticed; or confounded 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 ſymptoms enumerated which never appeared ſince more authentic documents have been produced; either becauſe men were credulous enough to believe

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

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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 reaſoning 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.

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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 aſperſion is 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 eryſepelatous inflammation of the "internal membrane of the urethra.*" Now, if the inflammation were eryſapelatous, 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 eryſapelatous 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

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 neceſſity 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 abſorbed 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 happen 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 ſynonimous? This is the leſs excuſable, becauſe

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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 hav-"ing frequently obſerved in ſtone-horſes a running "of this kind;" and alſo "in dogs more frequently."

* Swediaur, 27. † Id. 37.

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 *micturitio*, 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 ſalacious curs and ſtone horſes, ſhow, that though Dr.

* Bracken's Farriery, vol. 1.

† Syphilis, page 5.

Swediaur

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

" 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 acknowledged; but I cannot help arreſting the reader to aſk if he knows *quid vult aut num quid*, this " diffuſing itſelf like a fermentation," theſe " degrees of acrimony or volatility," or whether either 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 chemical 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 produce a certain effect on the conſtitution, very different 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 aukwardneſs of his ſtyle. From the habit of reading little, and in ſome meaſure from

* See page 29.

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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 ben 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 taſk 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

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in conſequence be in the veſiculæ ſeminales, 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 celluloſam urethræ ſubſtantiam ſe diffundat et eam expandat.

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

† Hunter's Treatiſe, page 30.

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 Hu-"man 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,

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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 ſurpriſing 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 inſtan-
"ces, where, after mercurial ſalivations, the ſymp-
"toms 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 Luiſini 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 Luiſinus'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 reappearance 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 *luem 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 *luem eo tempore dominantem tollat.* Such being the caſe with the *predominant* 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

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 ſymp- " toms, which continuing through a double ſol- " ſtice, 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

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 extermi-"nated, 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 underſtands, and the metaphor, whether it leads us aſtray or not, at leaſt ſuperſedes 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 patient is free from ſyphilis, and concludes with obſerving, 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, immediately 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 ſentences 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 *poiſon*, 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-appearance of ſymptoms after they have been ſubdued; 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 *certainty*, is certainly very agreeable to thoſe who are not fond of cloſe reaſoning. Such may fancy they can form an idea of virus circulating and remaining active for any indefinite time. As to the *extirpation* of the diſeaſe, it is only another 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 admitting 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 divine 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 mixture of myſtery in this buſineſs inſcrutable to profane readers.

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

It may be ſaid, that as it is well known mercury 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

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

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 not 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

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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 continuiug 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 operatioh 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

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

" paroxyſms. 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 paroxyſm, 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 re- " turn. Having therefore acquired from obſerva- " tion ſome general ideas of the different propen- " ſities to relapſe, in different ſituations and in dif- " ferent 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 pe- " riod 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 ſuperſedes 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

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 paroxſym. 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.

ſhows

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

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 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 for-"gotten, 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 coagu-"lated, 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 ef-
"fects of virus which is in the habit but not come
"into action. Indeed, if mercury had not this
"power it would be of litle uſe. For if every par-
"ticle of virus began to produce diſeaſe before
"the infection could be expelled, no man once in-
"fected could ever be cured. The conſtitution
"of the ſtrongeſt would be ruined, and his life ter-
"minated before all the particles of virus abſorbed
"from a chancre or bubo could ſucceſſively come
"into action and be deſtroyed, by the ſame num-
"ber 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 be-
"ginning 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

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 ſivvens, 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 cauſticated, 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 *oſſa 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 authoriſe us to ſuppoſe ſo many would ſhow the ſecondary ſymptoms as Mr. Hunter ſtates.

Mr.

Mr. Jeſſe 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 irrita-
"tions upon ſecreting ſurfaces endeavour to pro-
"duce 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, fau-
"ces, and eyes,† &c."—"Any little innocent ex-
"traneous ſubſtance faſtening on theſe mucous
"ſurfaces, will excite from them an additional diſ-
"charge." "When any thing obnoxious ſtimu-
"lates 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

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 & 13.

" 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 proving 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

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 gonorr-" hœa 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

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 queſtion as ſtated by Mr. Foot, it amounts to this, The altered ſecretion reſembles pus in ſome appearances, mucus in none. What then ſhall it be called? The anſwer is, as we find in the Obſervations, "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 myſelf, 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 Johnſon 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.

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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 ſur-"rounded 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 Buboes.

John Hunter—"A knowledge of the ab-"ſorbing 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

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 quo- "tations 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 unintel- "ligible." Nothing can be fairer than ſuch a propoſal; let me therefore tranſcribe his account of the cauſe of bubo, as tranſlated by Barrowby.

* Left. page 441.

CAUSES

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. 358.

THOSE who are ignorant of chemiſtry and "the "additional acquirements which have been attain-"ed 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 dyſury, 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 ſallies, 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

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 Diſcover-"ed 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 endea-"voured 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

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: affligе- "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 properties 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 venereal, by their apparent morbid vitiations, but alſo every part of the conſtitution is venereally vitiated."† 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 indiſcriminately to every part, if it has any action,

* Treatiſe, page 307. † Lectures, page 465.

act

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 viti- "ated." 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 parti- "cular power in the animal machine, nor is the "nature of the poiſon ſuch, as will fall more rea- "dily 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 vitia- "tion 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 conſidered 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

* Left. 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 diſputed 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 himſelf, 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 mercury 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.— "Caſes," 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 pro- "duced 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 irri- "tated, is capable of ſhifting its influence, of pro- "ducing 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

" 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

" 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 gonorrhœa 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 opinion in every point of view. Mr. Hunter only contends for the local action being cured by a fever. The caſe alſo ſhows preciſely that two diſ-

* Obſervations, part I. p. 8.

eaſed

eaſed 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

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 con-"ceive the virus was abſorbed before the gonorr-"hœa quite ſtopped, (for it could not be after-"wards) and, when abſorbed, gave a diſpoſition "to certain parts of the body, to take on the diſ-"eaſe, but which diſeaſed action did not take place "while the conſtitution was engaged with the va-"riolous." 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

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 per-chance 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 ungraceful; his illuſtrations coarſe; his language always unadorned. But Mr. Pott ſeemed conſtantly to have in view

Non ſatis eſt pulchra eſſe poëmata dulcia ſunto
Et quocunque volent animum auditoris agunto.

In

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 rivalſhip, 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 diſpleaſed at being thus perſonally introduced to them.—But having turned my recollection 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 heightened by being viewed in a reflecting medium, and which, in proportion as we retreat from them, blend with a more enchanting ſoftneſs their various colours, and ſmooth all their little aſperities:—having thus far indulged the tranſition of ideas, I ſhall, without further apology, conclude the ſubject with a ſentence which is always obtruding itſelf whenever theſe characters are contraſted, and which is as appoſite as any can be, that compares individuals to a nation. *Athenienſium res geſtæ ſicuti ego exiſtimo ſatis amplæ magnificæque fuere; verum aliquanto minores tamen quam memoriæ feruntur. Sed quia provenêre ibi ſcriptorum magna ingenia, per terrarum orbem Athenienſium facta pro maximis celebrantur. 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 introduced

* 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 tu-
" mour, 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 Viru-
" lenta 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 ad-
" mitted by every practitioner of experience.
" Now this being eſtabliſhed"*—Let the reader

* Vol. 1, page

determine

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 tranſla-" tion 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 *num-*
" *berleſs inſtances* people have been poxed by the
" matter of ſyphilis being by accident applied to
" a cut or a ſcratch, as often happens with ſur-
" geons 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 worth while 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 aukwardneſs. " 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

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

the

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 experi- "ments that have been made, but muſt truſt to ex- "perience 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 per- "mit conjectures, to leſſen the validity of induc- "tions drawn from experiments, it will be impoſ- "ſible that ſcience ſhould be progreſſive."

* Vol. I. page 35.

THE

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

" guments might be adduced to ſhow that the diſ-
" eaſe prevailed among the Jews, Greeks, and Ro-
" mans, and their deſcendants, long before the diſ-
" covery of America."*—And why not in Para-
diſe? 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

reader can judge what force they contain. "But," ſays Mr. Bell, "being a matter of curio-"ſity, 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 commu-"nicating 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, without any ulceration being induced.

Such, and a few others, are the modes in which this diſeaſe may be communicated, and which, though not very frequent, the author has ſeen various inſtances 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 blotches on the ſurface of the body, ulcers in different parts, nodes and ſwellings in the perioſteum, bones, and tendons, excreſcences about " the anus, ſwellings of the teſtes, loſs of hair from

* Id. p 8.

" all

"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 effects 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 remarkable, 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

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 bot-"tom, 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-
" taminate 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 ictibus auras* is all the apparent weight of argument produced by the latter, when compared with thoſe experiments which were publicly repeated, and with ſuch uniform effect.† When Mr. Bell gives us the reſult of experiments made with the ſame publicity and accuracy, 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

" 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 venere- " um mutabantur quod copioſum tabum ex flavo— " virideſcentem quotidie eructabat."——Afterwards—" *Lenta* tamen hæc eſt inflammatio & pe- " riodum ſuam *tardo* gradu percurrit non enim

* Swan, page 310. † Prefatio ad Luiſin.

" ſubita adeo ſequitur ſuppuratio ut in aliis morbis " inflammatoriis; nec reliqui inflammationis exi- " 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 diſtinguiſhing the ulcer covered by a cruſt of hardened 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 ſurpriſed 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 ſore 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 deſcribing either ſivvens, or ſome morbid poiſon different from the venereal, as found in the London hoſpitals. What very much ſtrengthens this laſt opinion is, a previous paſſage in his account of chancre, by which it appears, that of ſeven caſes of phagedænic ulcers of the glans penis, he traced four from one, and three from another individual ſource of infection.† I have already ſhown this primary ulcer

* 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, combined 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 ſometimes great from the hæmorrhages with which they are accompanied, 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 general 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 matter 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 puſ- " 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 hæmorr- " hages 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

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

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 introduced 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," continues 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 preſent 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 ſcro-"tum on account of inflammation in one of the "teſtes, the conſequence of gonorrhœa, degene-"rated into venereal ſores. This, I informed him, "would not have happened from gonorrhœa a-"lone; 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 againſt facts, Mr. Bell chuſes to anſwer Mr. Hunter'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 communicated 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 Appendix, No. II.

" ed

" 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 ſacer*. 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 conſti- "tutions in which none of the uſual effects of mer- "cury 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 al- "moſt 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 ſivvens, are not 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 opera- "tion 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 equal- "ly 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 ope- "ration of mercury in the cure of ſyphilis is analo- "gous to this; but the one I think is equally in-

"com-

" comprehenſible with the other, and that mercu-
" ry 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 addi-

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 tri-* "*turated mercury is rendered inert, and will not* "*produce the diſeaſe.* The fact is recorded by the "beſt authority, the late Doctor Cullen, who hav- "ing 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 ve- 'nereal chancre, and mixing it with a quantity of 'Plenk's gummy ſolution of mercury, he applied 'this mixture to a ſound perſon, but could not 'find that it produced chancre or any other ſyphi- 'litic ſymptom.'* This fact of itſelf might be "judged to be *deciſive*, but more complete con- "viction 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 con- " cluſion; 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 Plenk, it may ſtill be doubtful, " notwithſtanding," to uſe Mr. Bell's expreſſion on another occaſion " of all that Plenk, the inven- " tor, 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

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 be-"ginning 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 *ſeve-*"*ral* healthy children." How numerous might theſe prolific families have been, had their ſurgeon been poſſeſſed of a "magnetic teſt," or "any di-"vine 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

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

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 γαρ γέρας ἐστι θανόντων.

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

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 ſizy: the pain left him ſuddenly again, but never returned. A mortification however enſued, and a deep ſlough 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, ſloughed away ſo as to expoſe the glans, from which two ſmall ſloughs 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 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 ſore 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.