Observations on morbid poisons, chronic and acute : the first compehending syphilis, yaws, sivvens, elephantiasis, and the anomala confounded with them ; the second the acute contagions, particularly the variolous & vaccine / by Joseph Adams.

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OBSERVATIONS

ON

MORBID POISONS,

CHRONIC AND ACUTE.

THE FIRST

COMPREHENDING SYPHILIS, YAWS, SIVVENS, ELEPHANTIASIS, AND THE ANOMALA CONFOUNDED WITH THEM.

THE SECOND

THE ACUTE CONTAGIONS, PARTICULARLY THE VARIOLOUS & VACCINE.

SECOND EDITION,

ILLUSTRATED WITH COLOURED ENGRAVINGS,

AND FURTHER COMMENTARIES ON THE DOCTRINES OF MR. HUNTER.

By JOSEPH ADAMS, M.D. F.L.S.

PHYSICIAN TO THE SMALL-POX AND INOCULATION HOSPITALS.

London:

PRINTED FOR J. CALLOW, CROWN COURT, PRINCES STREET, SOHO;

By J. & W. Smith, King Street, Seven Dials.

1807.

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DEDICATION.

To JOHN COLLIER, Esq.

MY DEAR SIR,

WHEN the first Edition of MORBID POISONS was addressed to you, the Author, and the Nature of the Work, were equally unknown to the Public. Under these disadvantages the sale was so much more rapid than could have been expected, that a new edition was only delayed by my absence from England.

That absence has afforded me opportunities of seeing Diseases peculiar to, or modified by, warmer climates; and my return to England introduced me to the charge of a Hospital, for the reception of the most formidable Contagion with which we are acquainted.

I am prepared to hear the disappointment of yourself and Mr. Hunter's other admirers. A work from one of his Pupils, favoured with so many opportunities, and with so much assistance from others, cannot equal the expectations that may be formed of it. As, however, ten years have been spent in improving these advantages; as you expressed some approbation of the first imperfect attempt; above all, as it has pleased Providence to protract your life, with the full enjoyment of it, till the completion of the Work in its present form, I cannot refuse myself this opportunity of again assuring you with how much gratitude and respect,

I shall for ever remain,

Your faithful friend and servant,

JOSEPH ADAMS.

Berners Street, Nov. 1806.

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ERRATUM.

Page 174, 7 lines from the bottom, after practitioner, add " that."

PRELIMINARY REMARKS.

ON THE IMPORTANCE OF PRECISE LANGUAGE IN MEDICINE, ON THE ADVANTAGES ALREADY DERIVED FROM THE ACCURACY OF MR. HUNTER, AND ON THE PROGRESSIVE IMPROVEMENT WE HAVE REASON TO EXPECT.

IT was observed by the late Mr. Hunter, that no person has a correct idea in his own mind, unless he can communicate his meaning to another. Such a position came with particular force from one who always felt a difficulty in explaining himself in popular language, and was even accused by some of being ignorant of his own meaning. If by this suggestion is meant that he was not always ready with an answer to every question that was put to him, this his warmest advocates will allow, and those who knew best the value of his answers will be the readiest at explaining his backwardness. The correctness with which he habitually thought, often obliged him to express himself in a manner different from others, because he could not use those terms in a general sense to which he had applied a more precise meaning. This produced a necessity to alter the form of a question before he could frame his answer, or to begin with other matter, the connection with which would not occur to every impatient enquirer.

It is not at all extraordinary that an original mode of thinking should produce a mode of expression different from that in common use. Accordingly we find the same remark has been applied to other men of original genius. Brindley, to whom the country is so

much indebted for the improvement of our inland navigation, it is said, never could explain himself in the common technical language. Pemberton, the publisher of a View of Sir Isaac Newton's Philosophy, gives the following account of his intercourse with that great man, the pride of our nation, and the glory of the age in which he flourished.

" I found," says he, " that he perfectly understood his own writings, contrary to what I had frequently heard in discourse from several persons. This opinion might arise, perhaps, from his not being always ready at speaking on these subjects, when it might be expected he should. But as to this, it may be observed, great geniuses are frequently liable to be absent, not only in relation to common life, but with regard to some of the parts of science they are best informed of. Inventors seem to treasure up in their minds what they have found out, after another manner than those do the same things who have not this inventive faculty. The former, when they have occasion to produce their knowledge, are in some measure obliged immediately to investigate part of what they want. For this they are not equally fit at all times: so it has often happened that such as retain things chiefly by a very strong memory, have appeared off hand more expert than the discoverers themselves."

This, we have no reason to doubt, is a very correct description of that luminary; nor is it possible for those who were acquainted with Mr. Hunter, to read it without reflecting on *his* manner. " In relation to common life," indeed, Mr. Hunter could show the same attention as other men, provided the conversation led to any thing useful; but when it was altogether desultory, he seemed at first to feel irksome, and rather thoughtful than absent, till an opportunity occurred of giving a new turn by some lively sally, pertinent enquiry, or apposite reflexion.

On physiological subjects he seemed indeed to "treasure up in his mind what he had found out, after another manner than those do the same things who have not this inventive faculty." The search after what really exists being his sole object, and cautious of using any terms which were not perfectly appropriate, he was constantly engaged in correcting or confirming his opinions by the acquisition of new facts: and his mode of expressing those opinions showed that "when he had occasion to produce his knowledge, he was in some measure obliged to investigate part of what he wanted." Of this we have a remarkable instance in the difficulty which attended his deposition before men competent to engage in philosophical enquiries, accustomed to the intricacies of oral examinations, and engaged in so solemn a concern as the life or death of an individual.

In the trial of Capt. Donellan, for the murder of Sir Theodosius Boughton, the other medical gentlemen, who were examined, had no difficulty in making themselves understood: their language was plain and decisive. But Mr. Hunter, who had seen more, was proportionally more backward in making up, and consequently in expressing, his mind. He first thought it right to distinguish between such questions as were strictly medical, and such as applied to those subjects which came within every one's observation. In replying to the first, the extent of his researches taught him that all the appearances, described in the deceased, might have happened from other causes than poison. That in all cases of sudden death of persons previously in health, the appearances observed in the Baronet were usual. This explanation was immediately connected with his profession and the particular objects of his researches. But when asked whether, from the concomitant events stated by those who were present at the death, he should be inclined to suppose that it was occasioned by poison, he conceived, after the information he had given, that such a question was more readily answered by

those who were best acquainted with the parties, and were present at the time.

Another question was then proposed, Whether, if a father died apoplectic, such a disease ought not to be considered as constitutional? It is now well enough understood, that if a man dies apoplectic, his constitution must have been pre-disposed to such a disease, which constitutional pre-disposition may, like any other, descend to his son. But when this led to an enquiry, Whether the disease was hereditary or not? The answer was, that there was no such thing as an hereditary disease, though a disposition to a disease might be hereditary. Every one is now aware of the propriety of these distinctions, because the father might have a constitutional pre-disposition to apoplexy, yet by great caution on his part the disease itself might not have occurred, or he might have died first by some other complaint: yet still the same disposition might descend to his son. Thus the father having died apoplectic, was not necessary to give the son a pre-disposition to the disease.

This cautious mode of assertion, however, to men accustomed only to a written law or recorded decisions, was sufficient to invalidate the whole of Mr. Hunter's deposition, and the jury were advised to attend to such of the faculty as had no doubts.

But it is not only those who are unaccustomed to physiological enquiries, that find it easy to set aside Mr. Hunter's opinions. The last edition of this work produced many such instances from those who ought to have known better. I shall not now repeat them, not only because none of the writers, then noticed, have thought proper to defend themselves, but because it is easy to perceive that Mr. Hunter's opinions are every day gaining ground, and even his language coming into more general use. There is, however, one writer, who has published a second edition, in which he has again produced a sentence which need only be transcribed as before. "I have at present," says this author, "a person who has been long liable to

piles, who some time ago was attacked 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 these are *obviously* in existence at this moment upon the same parts, and as instances of such combination of diseases are occurring daily," &c.

We are told, that when Sir Richard Blackmore requested Sydenham to recommend him the books proper for a medical student, none were proposed but Don Quixote. To my contracted intellect, there is nothing in the exploits which the hero of La Mancha undertook more extraordinary than in detecting the "*obvious* combination" above related. If this mode of writing, for it cannot be called reasoning, is deemed clear, it is not surprising that Mr. Hunter is often thought obscure.

A similar instance just occurs to me of the facility of writing clearly, as some people may call it, in another author, who has amused himself and his readers with Mr. Hunter's awkward mode of expressing himself. I had begun to set this gentleman right in his misconceptions, till some of his errors seemed so obvious, that it was difficult to believe he had any wish to be better informed, or to inform his juvenile readers. I shall, therefore, content myself with contrasting a single passage as an illustration.

In his Treatise on the Blood, Mr. Hunter remarks, that in the earlier period of science, men are naturally too much struck with the more common appearances of the object, the nature of which they are investigating. Finding the blood of the animals, on which they made their experiments, warmer than the atmosphere, and perceiving coagulation to follow exposure, it was natural enough to connect coagulation with cooling, as they found a somewhat similar process in jelly. "This term," he adds, "has been applied to the coagulation of the blood, but *I think* improperly, for I should only call that jelly which became solid by cold, and fluid again by heat.

Coagulation is totally different, for it is a new species of combination. The freezing of blood may be called congelation."

When Mr. Hunter made this apology for the incorrect associations of antiquity, he little expected it possible that in our own times a teacher could fall into such an error. On this account he contents himself with a note, containing the few hints above transcribed. It must be admitted, that his mode of expresssion has no claim to elegance; but it is intelligible and modest, not only as he shows an unwillingness at altering the language of others, but because, when he feels himself obliged to do so, he only reminds his readers of those distinctions, the necessity of which he conceives need not be enforced. Though such caution and modesty is not inconsistent with an agreeable style: yet smoothly flowing periods are much more readily formed, when such incumbrances are dispensed with.

" But what harmony," says another writer, " he can find betwixt the occasional, voluntary, regulated contractions of the living solid, and this sudden, irretrievable, inorganic coagulation of the blood, I cannot conceive. Does not jelly coagulate; and what is it but a part of the blood? Does not glew congeal, dissolve, and congeal again, yet what is it but an animal jelly? Does the blood itself ever congeal till it is out of the body, or extravasated in aneurismal sacs? When it is out of the body it coagulates; when it coagulates it is dead. Coagulation is so far from resembling the contractions of the living body, that it is the marked character of dead animal matter, which you melt and coagulate again and again. Shall we then define life, by saying coagulation is the mark of the vital principle? If so, we give the mark of its death as the proof of its living power."

I have selected this extract as a specimen, to show how easily we are led astray when inattentive to words. After being taught better by an examination of the very passage I have referred to, we

find this author confounding together coagulation, consolidation under atmospherical temperature [or jellying] and congelation [or freezing], which is the more remarkable, because the freezing of blood was among the experiments he must have read a few minutes before, as contrasted with coagulation. It is equally surprising, too, that he should have overlooked Mr. Hunter's proof that the blood does coagulate [not indeed congeal] within the body, and even within the vessels, without any aneurism. It is true, it coagulates when out of the body, but there is reason to believe not when it is dead; for if an animal is killed so instantaneously that no contraction of the muscles follows, that is, that the body never stiffens, the blood likewise never coagulates.

This coagulation of blood in the living vessels, reminds me of a passage in another writer, whom, though very different from the former, I am inclined to bring forward in illustration of the subject. Dr. Beddoes, in his edition of Brown's Elements, has the following note:

" Mr. Hunter, who deserves so much praise for ascertaining facts, has been led astray in some of his attempts to establish principles by a different, but a very curious species of delusion. In treating of that obscure subject, for instance, the coagulation of the blood, he observes, that it sometimes takes place very quickly, as in mortification; but then 'it is to answer some good purpose, and arises from *necessity*, which appears to act as a stimulus in disposing the blood to coagulate.' He adds, that by 'actions taking place from necessity, effects are meant which arise from some unusual or unnatural change going on in the parts, and become a stimulus to action. The stimuli from this cause may vary exceedingly among themselves; but as we are unable to investigate them, I have included them under this general term, *stimulus of necessity*.'— On the Blood, p. 24.—It may be laid down as a rule in logic, that general terms ought never to be employed, unless we can substitute

particular terms, expressive of appearances, in their place. Mr. Hunter confesses his ignorance of those changes, which he comprehends under the phrase, stimulus of necessity. It is manifest, therefore, that it refers to nothing cognisable by sense; and his position amounts simply to this, *the blood coagulates because it must coagulate*. This is not the only occasion on which this ingenious anatomist has been betrayed into the mysticism of occult causes; and it would probably create some surprise in an ancient poet to find allegorical beings, like *necessity* and *death*, figuring in a modern work among the principles of physiology. It is easy to excuse Mr. Hunter for mistaking nominal for real essences; but the example deserves notice, as it so clearly shews the extensive utility of the philosophy of words."

The liveliness of Dr. Beddoes's genius has prevented his attention to a chain of reasoning, which, to a less philosophic mind, might be considered as dull. But Dr. Beddoes might easily have seen that Mr. Hunter uses the term, *stimulus of necessity*, in a sense which obliged him to adopt some phrase, in order to spare the perpetual recurrence of a complete sentence. Besides those customary actions by which life is preserved, certain accidents occur, the effect of which can only be remedied by new actions. In these cases the new actions are *necessary*; and as we find them usually following that necessity, we have a right to say that they are excited by it—in other words, that such a state of the animal requires such a change of action, and that such a change takes place. If these actions are found uniformly to follow each other when the animal is preserved, we must suppose that the injury done to the animal is the stimulus to such actions; and to me there appears no term so appropriate as the *stimulus of necessity*. In this manner we see a variety of new actions set up under different circumstances of disease or accident in the whole body, or in particular parts. But the most striking instance of all is the rapidity with which blood

coagulates in parts in which it always remains fluid in a state of health. Thus in mortification, coagulation in the vessels themselves is absolutely necessary, because, by a similar law of necessity, the parts in contact with the mortification are stimulated to absorption, in order to separate the dead from the living part. But if this separation should take place, however partially, before the mouths of the arteries are closed, the consequence would be, that the animal must bleed to death. The blood therefore coagulates, not from any necessity imposed on itself, but from that necessity which induces a new action for the preservation of the animal, in consequence of a new state of the parts.

If Dr. Beddoes expected Mr. Hunter to explain those powers by which the blood coagulates without the mixture of a chemical process, without a change of temperature, without even a change in its local situation; and why at other times it resists, or has not the power of, coagulation, even in a change of temperature or local situation—he expects an explanation of the powers of life. But no reasonable philosopher ever attempts more than to discover those laws which may be detected by tracing the regular series of every action, and the uniformity of every appearance that is cognisable to our senses.

No one will suspect me of any partiality for Mr. Hunter's phraseology, because, whenever more appropriate expressions occur, no one is more ready to adopt them. This passage from Dr. Beddoes has been selected, not out of any regard to Mr. Hunter's language, but because it affords an opportunity of connecting the subject of coagulation with a disquisition, addressed to one who possesses candour and good sense enough to judge fairly of the question.

It is still less necessary to apologise to another gentleman, whose labours I have found so useful in the progress of this work, if

I remark how much a slight, though very judicious, deviation from Mr. Hunter's language, loses its effect by not being defined.

In some of the valuable cases, which Mr. Abernethy's Surgical Observations afford, we meet with the term *morbific* poison. It is well known that Mr. Hunter divided animal poisons into the natural and the diseased. The first [which I should rather call the original] are those which form a part of an animal when in health, and though deliterious to another, do not communicate a power of affecting others. Of this kind is the poison of the scorpion, the adder, and other venomous animals. The morbid poisons, on the contrary, never exist but under disease, and have a power of exciting a similar disease by the secretion of matter, which will have the same properties. Now we shall find that a number of cases, communicated by Mr. Abernethy, cannot be proved to come strictly within the description of either, and that there is every reason to believe they arise from contagion; that is, he found diseases in the penis, which, from every attendant circumstance, appeared to arise from coition, yet he never describes similar ones in the vagina. In these instances the secretion of the vagina appears to be *morbific*;* but till we are better acquainted with the whole history of such cases, we are not authorised to say that it is *morbid*.

In the former edition of this work, I took notice of certain complaints of this kind, which occurred on the penis, but which had never been described in the vagina.† That Mr. Abernethy has the same circumstance in view, appears by some passages, which I shall now produce.

" The frequent cases of such disorders, which I have recently met with," says he, " has suggested the idea that they are increasing of late; nor is it improbable, since they are, like syphilis, propagated by promiscuous intercourse from secretions or sores, not so readily curable by mercury as those that are venereal, and *some of*

* Surgical Observations, 1804, page 127.

† Morbid Poisons, page 144, first edition.

which are not, from their nature, so prohibitory of that intercourse."—Page 142 and 143.

In page 168, the subject is further illustrated by a very valuable communication. A newly married gentleman, faithful to his wife, who was in perfect health, was yet infected with a disease, which there cannot be a doubt arose from mutual intercourse, yet did not prove venereal. In this instance the secretion of the vagina appears to have proved *morbific* to the husband, though we have no right to say that it was *morbid*, because the lady was in health. This subject will be hereafter considered at large; my only intention, in this place, is to give Mr. Abernethy credit for the propriety of his distinctions, and to express a wish that he had done justice to himself by marking them more pointedly.

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 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 a 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. What can be so simple as to trace the diagram in a geometrical problem? What so easy as to follow a demonstration uninterrupted by any external objects, consisting only of lines and

circles, the different sections and points of contact in which we have before us? Yet this we are apt to consider among the higher branches of erudition, because it cannot be accomplished without a power in the mind to retain and combine, and the constant application of that power. How many ages did men rest satisfied with what they fancied a solution of mercury rising in the 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. Yet this *fuga vacui*, though a mere hypothesis, 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 have exhibited 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 as arising from no other cause than the force of their mutual attraction, which by the rapidity with which it takes place, 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 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 to learn the cause of gravitation, and in the last what the matter of fire is, and why it does not exhibit similar phænomena 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 show its agreement with those laws when applied to the same bodies, under different circumstances.

Sir Francis Bacon, the father of modern philosophy, in his *Novum Organum Scienciarum*, first explained the causes which 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 affect 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 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 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

* 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.

few facts, and inferring from them without attending to all the objections, is inconclusive. * In constituting an axiom," says he, " a new form of induction must be thought of, from what has hitherto been in use; one that will apply to the principal and all the intermediate axioms. The induction that will be truly serviceable in demonstrating the arts and sciences, ought to analyze nature by rejecting and excluding as far as is necessary, and afterwards forming conclusions on what may be ascertained." In the succeeding book he proceeds to what he calls the art of interpreting nature; and, after expressing the insufficiency of the human powers to arrive at the primary causes of things, observes; † " For although 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, finding out, and explaining of it, is the foundation of science and experiment." " These laws and their

* 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 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 est invenienda, verum etiam ad notiones terminandas, utendum est. Atque in hac certe inductione spes maxima sita est.—Nov. Org. Lib. 1. Aphor. 105.

† Licet enim in natura 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, eam autem legem ejusque paragraphos, formarum nomine intelligimus presertim cum hoc vocabulum invaluerit & familiariter occurrat.—Nov. Org. Lib. 2. Aph. 2.

divisions," he adds, " are usually expressed by the name of FORMS."

It is hardly possible to conceive a more concise, yet perspicuous distinction than this between true theory, which, I believe, the late Sir Joshua Reynolds defined to be " the knowledge of what nature is," * and that fondness for reasoning which teaches us for every effect to assign a cause, without the labour of ascertaining 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 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. Still, however, he points out his mode of induction, as that in which only we can expect success.

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 at in one who wrote in

* Probably from the το ον or τα οντος ζητησις of Plato.

† See the conclusion of the quotation in the note of page 14.

‡ Sterne's sarcastic allusions to *radical heat* and *radical moisture*, seem pointed at this little work of Bacon.

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 more has not 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 must 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, excepting the discoverer himself, should have made that change a subject 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 might be found in the blood, the new theory became a subject 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, to show where this change of the food commences, if not in the blood. Dr. Fordyce's definition of fermentation* will indeed solve, as far as words can, this and almost any other difficulty, but without bringing us forwarder in our enquiries. "Fermentation," says he,

* Elements of the Practice of Physic.

" is the conversion of one compound into another, by a new arrangement, or manner of combination of its elements. The fermentation [of digestion] is peculiar to the organs of digestion, and has never been produced by any artificial means."* Here 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; that the foundation of science is to trace that LAW by which simple, individual ACTIONS take place in individual bodies; and that when that law is discovered, we shall have no difficulty in accounting for any variety of FORMS which sometimes present themselves.

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 orignally formed in the

* In the Essay on Digestion, published some years afterwards, 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.

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. 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 may 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 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,

* Denique sub inguine in plena ætate ponuntur, situ mutato, vi forte sanguinis irruentis.—Prim. Lin. 807.

hernia congenita, instead of a rare, must be almost an universal disease. 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 descend 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 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 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 as long as any objection remained. 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 explain those FORMS which sometimes occur, or to foresee the consequences that must follow the interruption of the proper series of actions; and by these means to account for the orifice remaining

open, and the descent of the intestine, which sometimes, though rarely, happen.

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

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

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

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 the directing of the testes in their passage through the rings, 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.

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's, and because I have the printed authority of those names 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 often supported by reasoning on mere mechanical principles. 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 lan-

* 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.

† Primæ Lineæ, DCCCCXXI. 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.—Poetry is a most dangerous accomplishment for a philosopher!

guage 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 the rest? I am not ignorant, it may be urged, that 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 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 their backwardness in physiology to be ascribed to 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 to trace pure individual *actions*, and the *law* by which they are governed? Thus much I have thought it right to premise, lest my attachment to Mr. Hunter's mode of reasoning should seem

the offspring of blind partiality. It is, however, with much pleasure I find that the opinions of that great man are insensibly gaining ground. With his opinions his language necessarily makes its way. This will save me the unpleasant task of exposing the insufficiency of many undefined expressions which are daily falling into disuse.

If, then, Mr. Hunter, ungifted by any powers of elocution, unaided by the advantages of early education, has taught us not only how to explore nature, but to avoid those modes of expression which, whilst they are admitted without enquiry by others, may lead ourselves astray, and impede or relax our investigations—let us at least be cautious how we quit the path he has marked out. But let us remember that Mr. Hunter has only begun what can never be completed: he has taught us how to direct our enquiries, but we must think, we must enquire, we must reason ourselves. If Hunter, like Bacon, has in any instance deviated from his own plan, we shall find the most ready way to set them both right is by returning to that plan. Though in physiology, as in geometry, nothing should be granted that will admit of being proved, yet such is the activity of the human mind, that it is hardly possible to restrain at all times its eagerness in forming conclusions on subjects which admit of experiment.

The knowledge of the absorbents is a discovery of the moderns. As by the previous knowledge of the circulation of the blood, we became familiar with a circulating fluid, there was little difficulty in admitting the circulation of a less dense substance. But still the mind could form no conception of the absorption of a solid body into tubes which always convey a fluid. Thus ulceration was considered as a solution into pus, and we have seen how Haller explains the diminution of the thymus gland. Some, indeed, have insisted that these solid bodies must be dissolved before they are absorbed. But this is an assertion without proof. Hitherto we can

only say that the parts disappear, and that the only means we are acquainted with by which they can be removed is absorption. What are fluids but parts more divided than solids? May not bone be taken away in such small portions and be so diffused in lymph, as to be undistinguishable from the fluid in which it circulates. These enquiries, like the communication between the extremities of arteries and veins, we can never expect to satisfy, but must be contented with ascertaining such facts as are cognisable by our senses.

Considering that the figure of a bone, before its complete growth, is in many respects similar to what we find it afterwards, it was a matter of very reasonable enquiry, whether this form is preserved by a gradual enlargement of every part, or whether the elongation of the bone is entirely from its two ends. In the latter case it must be evident that an insensibly new modelling of the bone must be continually going on, because depressions, processes, or spines, at a certain distance from each other, become by degrees further removed in proportion as the bone is lengthened. Mr. Hunter reduced this to experiment, by placing two small pieces of lead in the tibia of a growing pig, exactly marking the distance between them. After the animal had grown sufficiently to show a considerable increase in the length of the bone, the two pieces of lead were found at the same distance as before. Hence it followed that the elongation of the bone had been at the two ends; consequently, that to preserve the relative differences of every prominence and depression in the bone, it must have been continually remodelled, which could only be done by some parts being absorbed as others were increased. The following is the manner in which this doctrine is represented by two different writers:

"One could never," says the first, "have ventured to suppose the extravagant conclusion to which this idea, once entertained, has led Mr. Hunter. He proceeds to consider the many kinds of solids the lymphatics have to carry away, and the variety of mouths in

different animals suited to the great variety of substances they have to work upon, and then draws the conclusion, or leaves his reader to do so; that not only the mouths of the absorbents are calculated to absorb fluids; not only do they carry away solids; but each vessel, according to the hardness and toughness of the material upon which it has to operate, has a mouth adapted to the work. Here we do not see the genius of Hunter, but a poverty of imagination."*

Let us contrast this with the modesty of one, who, by additional experiments, has carried the enquiry further than Mr. Hunter.

Mr. Gibson, in his valuable paper† on the Sutures in the Skull, shows the use of them in forming various points from which that important covering may be gradually enlarged from the extremities of each individual bone. He proves from Mr. Hunter's experiments, and from his own observations, the manner in which the cylindrical bones are elongated, the probability that the same takes place in the flat bones, and that such a process would be inadequate to the increased size of the brain, if the skull were not divided horizontally during its growth into distinct parts. But whilst he gives Mr. Hunter full credit for his discovery, it is pleasing to see how entirely he waives the merit of one improvement he himself has made in it: viz. that this elongation of the cylindrical bone is only to the point of contact with the epiphysis, which must of course have of an œconomy of its own, regulating its growth, formation, and future induration.

* If on this and on other occasions, a long transcript should be objected to, as the means of *making up a book*, the author's only apology is, that he does not think himself justified in objecting to any opinion without producing the words, and if necessary, the whole paragraph, with which they are connected.

† Memoirs of the Manchester Society.

In this instance, it is true, there is nothing that contradicts Mr. Hunter's general theory, nor the facts on which he founded it; for the modelling process, which he requires of the absorbents, must be carried on. But still there is an oversight of Mr. Hunter corrected. The elongation of the bone is not from its head, but from the part in contact with the epiphysis. A less ingenuous mind, therefore, might have triumphed in having seen what was overlooked by so accurate an experimenter, or at least have claimed the merit of completing what Mr. Hunter had left unfinished.—Here we see a pleasing instance of the advantages derived from a well regulated mind—from that harmony which pervades every part of the conduct, whilst the object is to gain and to communicate information. It will, indeed, be difficult to find two better specimens of correct reasoning, and of the proper feelings of an ingenuous heart, than in the two papers furnished by Mr. Gibson, in the last number of the Manchester Memoirs.

It is not to be wondered if Mr. Hunter, who discovered so much, and whose mind was always occupied in different parts of physiology, should leave some of his enquiries unfinished. His experiment was equal to all his purposes, but it certainly wanted that neat finish which Mr. Gibson has given it. This is common with the greatest genius. The object of their enquiry being ascertained, minuter points, provided they are not contradictory to the grand axiom, are passed over, till others, actuated by similar motives, but exploring with different views, extend the various branches of the question, and thus confirm the whole.

I shall endeavour to keep Mr. Gibson in view, whilst I give two other instances, in which Mr. Hunter appears to me to have left a subject unfinished, but which is to be completed by pursuing the enquiry in the manner he has begun. The first is selected, as a proof of the goodness of his heart, which would not suffer him to omit the discussion of a question, on which it is evident he had

thought deeply, and which he must have been aware would excite the ribaldry of the unfeeling.

There is not, in the whole series of pathology, a passage more truly philosophical than his treatise on impotence; nor a passage which speaks more the feelings of a benevolent mind. No one at all accustomed to practice, but must have met with instances of distress from the apprehension of such a state, in men, whose only difficulties arise from that very apprehension. I cannot so well express this as Mr. Hunter has done.

" The will, and reasoning faculty," says he, " have nothing to do with this power; they are only employed in the act, so far as voluntary parts are made use of; and if they ever interfere, which they sometimes do, it often produces another state of mind which destroys that which is proper for the performance of the act; it produces a desire, a wish, a hope, which are all only diffidence and uncertainty, and create in the mind the idea of a possibility of the want of success, which destroys the proper state of mind, or necessary confidence.

" There is perhaps no act in which a man feels himself more interested, or which he is more anxious to perform well: his pride being engaged in some degree, which, if within certain bounds, would produce a degree of perfection in an act depending upon the will, or an act in voluntary parts; but when it produces a state of mind contrary to that state, on which the perfection of the act depends, a failure must be the consequence.

" The body is not only rendered incapable of performing this act, by the mind being under the above influence, but also by the mind being perfectly confident of its power, but conscious of an impropriety in performing it; this, in many cases, produces a state of mind which shall take away all power. The state of a man's mind, respecting his sister, takes away all power. A conscientious

man has been known to lose his powers on finding the woman, he was going to be connected with, unexpectedly a virgin.

" Shedding tears arises entirely from the state of the mind, although not so much a compound action as the act in question; for none are so weak in body that they cannot shed tears; it is not so much a compound action of the mind and strength of body, joined, as the other act is; yet if we are afraid of shedding tears, or are desirous of doing it, and that anxiety is kept up through the whole of an affecting scene, we certainly shall not shed tears, or at least not so freely as would have happened from our natural feelings.

" From this account of the necessity of having the mind independent, respecting the act, we must see that it may very often happen that the state of mind will be such as not to allow the animal to exert its natural powers; and every failure increases the evil. We must also see from the state of the case, that this act must be often interrupted; and the true cause of this interruption not being known, it will be laid to the charge of the body, or want of powers. As these cases do not arise from real inability, they are to be carefully distinguished from such as do; and perhaps the only way to distinguish them is, to examine into the state of mind respecting this act. So trifling often is the circumstance which shall produce this inability, depending on the mind, that the very desire to please shall have that effect, as in making the woman the sole object to be gratified."

Mr. Hunter then produces several cases in illustration, all of which are well worth perusing. But my principal object is to show that he did not extend his scope of reasoning as far as it would have led him, and that to make the chain complete, we must continue it in the manner marked out by himself.

He very justly shows the absurdity of supposing that an indelicate practice, which does not alter the structure of a part, can

destroy its powers. But after what he has so accurately shown are the proper stimuli for these organs, it follows that if these, like any other parts, are habituated to an unnatural stimulus, they will for a time be more obedient to that than to their natural stimulus. Nay, this unnatural stimulus may for a time become necessary to bring the parts into action. Thus the intestines, accustomed to frequent purgatives, will be insensible to the stimulus of the natural secretions. In both cases the remedy is a dereliction of the unnatural stimulus, and a determination to submit patiently, and to attend to those habits by which the parts will recover their natural functions.

This simple addition of reasoning, which follows as an inference from his own, would have rendered his chapter still more valuable, and prevented a part of that coarse obloquy, by which, on subjects of this kind, those who have the least regard to decency can always make themselves popular.

One of Mr. Hunter's most striking discoveries is the digestion of the stomach after death. Dr. Stevens and others had, by various experiments, shown that the secretion of that organ is the great instrument of digestion; that the fauces in animals with teeth, and the gizzards in granivorous birds, served no other purpose than to divide the food in such a manner, that as much as possible might come into contact with the gastric juice. At last it became a matter of astonishment that the stomach, which digested every thing else, should not be itself digested by its own secretion. Mr. Hunter at length perceived, that under certain circumstances of death, this would actually happen. But for this purpose he saw that the animal must be in health immediately before death, otherwise neither the quality nor quantity of the secretion could be equal to the purpose. He was confirmed in this by the instances in which he saw the stomach digested. Both were men who had died by a violent death; both had been previously in sufficient health to eat a hearty

meal. The fair inference from these was, that when men die of disease, the appetite usually ceases, and probably the secretion of the gastric juice also. In consequence of this, Mr. Hunter seems to have conceived, that by killing some animals in a previous state of high health, the digestion of the stomach might follow; but I believe he never succeeded satisfactorily excepting in fishes.

Spalanzani, with a patience characteristic of the man, multiplied these experiments to a degree that is even tedious to peruse. He saw the same consequences in fish, but never could produce it, excepting in a partial degree, in other animals. He seems convinced, however, that it might be done, for he saw that digestion was continued after death, and proved that there was nothing indigestible in the stomach itself, by finding that the stomach of one animal could be digested by another.

I fancied, perhaps without reason, that among some people a suscipion might remain as to the accuracy of Mr. Hunter's observation. This induced a greater attention to the subject, and by comparing Mr. Hunter's other observations with those on digestion, it appeared to me that he had overlooked one important distinction, which he has accurately made in his paper on recovering persons apparently drowned, that is, a distinction between what he calls absolute universal death, and such a cessation, as can never be restored, of all the actions by which life is supported. It may perhaps assist us, if we use for these two states the more common distinct terms — expiration and death. When a man ceases to breathe, we may truly say he has expired; but we cannot call him dead, nor does a medical man pronounce him so, till his limbs have stiffened, and afterwards have lost their rigidity, or till putrefaction commences.

This distinction need not be urged, because, besides the heat which, after a certain time, follows expiration, the rigidity of the body, and the translation of the blood even to the extreme branches

of the veins, can only be effected by the contraction of the muscles and of the arteries. That this contraction of the muscles is from remaining life, has never been disputed.

But Mr. Hunter has taught us to distinguish this mode of death from another so instantaneous, that neither subsequent heat nor stiffening take place: that under such circumstances the muscles cannot be made to contract, and the blood never coagulates, whether suffered to remain in the vessels or removed out of it.* In this manner he accounts for the contradictory histories we hear of some who are recovered from an immersion in water much longer than has proved fatal to others. For if the person has been greatly fatigued before he expired, he may be in the situation of animals, who, when chased to death, will drop down dead; in which case the body never stiffens, or stiffens instantaneously, and putrefaction soon ensues. It may even happen, that during the struggles for saving himself, the deceased may be so violently agitated, as to produce a similar effect; for no one is ignorant how much the body suffers by strong mental irritation, which in some instances has produced immediate death.

By this time the reader will anticipate my conclusion; he must, however, allow me to proceed. That to produce a secretion of healthy gastric juice the animal must be in health, cannot be doubted; but it must also be recollected, that for the stomach to be digested it must be dead. Now, an animal may be killed when in high health, yet he may expire before death has taken place in the constituent parts. This we find almost universally the case in slaughtered animals, as the blood usually coagulates, and the muscles become rigid. In this case, though the stomach may contain healthy gastric juice, yet the whole will be exhausted in digesting the contents, before the stomach itself is in a situation to be digested. But when men are destroyed in this manner, the agitation

* See his Paper on recovering Persons apparently drowned.

of the mind will sometimes produce immediate death at the moment of expiration. Under these circumstances, if, by a previous meal, the glands of the stomach have been stimulated to secrete, we may expect to find the organ itself digested, because it is in immediate contact with the digesting menstruum, a part of which it must contain within its own substance not yet effused into the cavity.

Hence it follows, that if we wish to see such an effect produced, we must contrive to kill a healthy animal in such a manner, that death shall instantly take place in the stomach. For if the animal is not in health, the gastric juice may not be secreted: and if the stomach is not dead, the gastric juice can have no more effect on it than before the animal expired.

This very easily accounts for our meeting with the case so seldom in the human subject. It, however, has been met with, as may be seen by those who take the trouble of examining the descriptions of dissections in Morgagni and others. But no one before Mr. Hunter was aware of the cause. The destruction of the inner coat of the stomach is by no means uncommon, and many otherwise accurate writers have imputed certain symptoms, during life, to an event which took place after death.

In other animals, if we wish to produce the full effect, we must select such as are in health, and kill them in such a manner as to produce instantaneous death. This is always a matter of uncertainty. In fish the difficulty is less, because they not only expire in health, like the other animals killed for our purposes, but because every part becomes dead very soon, as is proved by the suddenness with which putrefaction takes place. Add to this, their stomachs, excepting in those that have gizzards, are extremely thin, and they swallow their food whole and alive. The most striking instance I ever met with was in a john dory. On opening this animal, part of the liver, the intestines, stomach, &c. were found

loose in the cavity, with part of a fish, half digested, and a living worm. The contents were kept by themselves, and the worm killed. On inspecting them a few hours after, the worm was more than half digested.

But we are not to expect this effect uniformly to follow the death of fish. Some die slower after expiration than others, as is well known by the advantage of keeping them before they are dressed for the table, whilst others will not admit of any delay. The season of the year and time of day, make a great difference in these as well as in land animals.

Among the modes of producing instantaneous death, Mr. Hunter mentions a blow on the stomach. I made several attempts to kill dogs in this way. But whether it arose from the protection of their thick elastic skins, or from whatever cause, I never could succeed. In one instance, however, after a second blow, the animal expired. This was about nine in the morning, in the summer, in the island of Madeira. The dog was immediately put in woollen cloth, and laid on a broad stone sill against a south wall, both of which had been previously heated by the sun. Seven hours afterwards the body was examined. No stiffening had taken place: the blood did not coagulate: a hole was found in the great end of the stomach, and on examining the inside, a considerable part of the substance was found consumed. By morning the putrefaction was universal, and intolerable even to those accustomed to such experiments. This was, however, the only instance in which I succeeded; but it was the only instance in which I had been able to kill the animal in such a manner, that neither stiffening of the body nor coagulation of the blood followed. I, therefore, resolved to leave the experiment till my return to England should procure me the convenience of an electrical machine, powerful enough to kill a dog. But here I was not more fortunate. Mr. Cuthbertson's most powerful battery did not succeed till after

three or four shocks. Hence the death being more gradual, the stomach and other parts retained their life. On the following morning the animal was stiffened, and, as might be expected, the stomach was found entire.

However, the enquiry and conversation to which this experiment gave rise, convinced me, that in proportion to the suddenness with which an animal suffers such violence as to produce a cessation of all the actions by which life is supported, is the probability that death in every part will take place. Thus in Portugal and other places, where bullocks are killed by a wound in the spinal marrow, the animal is instantly afterwards cut up, and dressed at any hour of the same day; yet if the butcher has been successful in his aim, the flesh is never tough, and will not keep long. On the contrary, the mutton which is killed after the English custom, requires keeping to make it tender, because a contraction of the muscles follows the slow mode of dying which bleeding produces.

It is well known that the usual mode of killing rabbits is by a blow on the neck, which, if successfully inflicted, produces immediate death. This is proved, because the animal may be dressed immediately, without the necessity of being kept till the muscles become relaxed. Consequently, in these cases, stiffening does not take place. Mr. Astley Cooper and Mr. Carlisle both assured me, that in the course of their experiments on these animals, they had sometimes found the stomach so much digested, that the contents were loose in the abdomen, and in some instances part of the liver was digested also. Whoever wishes to repeat these experiments, must recollect how much depends on the suddenness with which the animal is killed, and on his previous state. It is, therefore, not to be wondered, if we do not always succeed; but it will be found to succeed in proportion as the muscles remain relaxed, or if they stiffen, to the suddenness with which stiffening and subsequent relaxation take place, and also to

the want of coagulation in the blood, or the suddenness with which it is coagulated. The experiment, on all these accounts, will, for the most part, succeed best in summer, when actions of every kind follow each other with the greatest rapidity: indeed, I expected less from our experiment with the dog, because it did not suit Mr. Cuthbertson to undertake it before evening.

However, from the hint those gentleman had given me, it appeared that the experiment might be made with very little inconvenience on rabbits. Between nine and ten in the morning, I waited whilst the poulterer killed a rabbit, by dislocating its neck. It was immediately brought to Mr. Brookes' (that gentleman continuing his dissections during the summer), and exposed to the sun in his south room. About three o'clock of the same day, Mr. Brookes procured another rabbit, which he killed by a wound in the spine. In the evening the rabbit, first killed, was found without any stiffening—the other had stiffened.* The following morning both were opened: The stomach of the rabbit, first killed, was so much digested, that its whole contents were in the cavity of the abdomen. The blood was fluid, excepting that in the right ventricle was a small piece of clot, which Mr. Brookes compared to what he had seen in menstrual blood which had been confined. It was not larger than a small horse bean, without any appearance of fibrine, so loose in its texture as to preserve no form, and to lessen its bulk as it was moved. In the other rabbit, which had stiffened, the stomach was entire, and the right ventricle contained so firm a coagulum, as to preserve the

* Yet immediately after the wound in the spine, the animal ceased to breathe or struggle. This shows that, however successful such a wound may be, it does not always produce absolute universal death. The animal was not in health when killed, and no pains were taken to preserve his warmth after the wound was inflicted. Probably, if the experiment is often repeated, other concurrent causes may be discovered; but the facts before us are sufficient for my purpose, as will appear by subsequent inferences, which cannot be disputed.

exact shape of the cavity and its communicating vessels. When taken out, it was moved without staining the board on which it was placed.

From the result of these experiments we may draw the following inferences:

1st. That nothing but life preserves the stomach from being digested by its own secretion.

2dly. That as the stomach is digested by its own secretion in an herbivorous or granivorous animal, the gastric juice is probably the same in all animals, how various soever their food may be.

3dly. That if the stiffening of the body after expiration, is the effect of life remaining in the muscles, there is reason to impute coagulation to the same property in the blood, as both take place under the same circumstances of the animal, and both may be prevented by the same means.

If these inferences are just, they furnish answers to some suggestions which have been offered since Mr. Hunter's experiments.

"*It may seem superfluous,*" says an author, "to observe, that the gastric juice has no power of acting upon the coats of the stomach during life. Whether this be owing to the property in the living fibre of resistance to the action of the fluid, or that there is a secretion bedewing the surface, which prevents the action, it is not easy to say; but more probably it is owing to the resistance to its action inherent in a living part."

"The gastric juice," says the same author, "acts on those substances which are nutricious to the animal, and which are peculiarly adapted to its habits. It has, consequently, some variety of properties in different animals. The secreting powers of the stomach seem so far to accommodate themselves to the food received into it, that the property of the gastric fluid is altered according to the nature of the food."

" It seems to be a peculiarity in the human stomach, that it has a greater capacity for digesting a variety of animal and vegetable bodies.—In other creatures, a sudden change of food is rejected, and the powers of the stomach are found incapable of acting duly on the aliment, though time so far accommodates the gastric fluid to the ingesta, that the animalization becomes perfect. Mr. Hunter speaks of the power of cattle eating and digesting their secundines. I have known a cow die from this, the membranes being coiled up within the bowels," ——

That the membranes were found coiled in the bowels of a cow, which died soon after parturition, is a very inconclusive argument to prove, that, whilst obeying the common instinct of nature, she should have been the cause of her own death. In the neighbourhood of Berkley, it was always the custom, among the farmers, to remove the secundines as soon as possible, to prevent their being eaten by the cows, till Dr. Jenner taught them better. Whilst we were going over a farm, in which the cow-pox had made its appearance, we discovered the remains of a secundine, the cow having consumed as much as she chose. The farmer remarked, that he had always left them to themselves, since the Doctor has assured him there was no danger, and that since that time he rarely had sick cattle after calving.

It may not be altogether useless to show how exactly Harvey, the illustrious discoverer of the circulation, reasoned like Mr. Hunter, concerning the proofs of vitality in the blood. We have seen how the celerity of the blood's motion in the vessels, and its other actions both within and without the vessels, depend on the necessities of the animal, or on those causes which produce a corresponding action in the solids: and though, from the different objects of the two experimentors, the proofs brought by each are somewhat varied, yet the conclusion is not only the same, but almost in the same words.

" Quinimo ex variis ipsius motu in celeritate aut tarditate, vehementia aut debilitate et cetera, eam et irritantis injuriam et foventis commodum persentiscere manifestum est. Ideoque concludimus sanguinem per se vivere et nutriri, nulloque modo ab aliqua corporis parte vel priore vel prestantiore dependere."*

We have so lately gone over the same ground with Mr. Hunter, that it is unnecessary to remind the reader of the manner in which he leads us to the same conclusions. But it is very troublesome to follow the well directed experiments of these laborious and honest enquirers: a much easier method is to seize an analogy between an animal with his circulating blood, and a plant growing in water.

" Plants, it is said, have active and irritative fibres. By the most curious actions they drink in water; water alone they can convert, by the most simple mechanism, into the most delicate perfumes, into delicious fruits, and the most terrible poisons. ' There stands,' says Blumenbach, ' a hyacinth before me; generations of these flowers, of which this is the last, have grown there successively, touching the surface merely by a little water. But shall Mr. Hunter persuade me that this water is alive? vel hyacinthi me monent.' "

If the Professor had submitted to be advised by his hyacinths, he would have found that, besides water, they have occasion for light, air, and heat. This would have led him to reflect, that the water, absorbed by a plant, has no more claim to life than the water taken in by an animal with its food; that though a plant can live by water, light, air, and heat, yet that this water does not circulate but as part of a secreted fluid in the vessels of the plant; that it is incapable of coagulating with various degrees of celerity, according to the exigencies of, or according to any other action going on in the plant. In short, that there is no analogy between water absorbed by a plant, and blood formed by the powers of an animal.

* Harvei Opera, page 396, Editio Collegii Med. Lond. 1766.

If it is thought unnecessary to combat arguments which refute themselves, it is hoped that the reader will only consider what is here produced as the means of illustrating some of Mr. Hunter's doctrines. These doctrines are, for the most part, so simple when known, that it is by some thought *superfluous* even *to observe* them, and probably for that reason the inferences which flow from them are overlooked. By contrasting induction from a series of discoveries with the rapid manner in which a difficulty may be passed over, the mind of the reader is somewhat arrested: is forced to form its own combinations, and better prepared for a mode of reasoning on pathological subjects, which is slowly gaining ground, since Mr. HUNTER first taught us its necessity.

OBSERVATIONS

ON

MORBID POISONS.

CHAPTER I.

HOPES and fears are said to constitute more of our happiness and misery than realities. It is at least certain that few pleasures are equal to those which we anticipate, and few distresses to continual apprehension or suspense. Among the ancients the dread of poison was the continual alloy of successful ambition, and the object of medicine was rather to discover antidotes than remedies. Mithridates seems to have accumulated all the antidotes known in his own times, and by his compound found himself secure against the effect of substances always considered as poisonous. This gave a celebrity to his composition, that long outlived himself, and to his name, which is scarcely yet extinguished. But when Mithridates wished to die, and swallowed all the poisons without using his antidote, he still lived. One might have expected such an event to have lessened the terror of his poisons, and the credit of his antidote; but pre-conceived error was not so easily overturned. It was supposed that by constantly applying to his antidote, he had rendered himself no longer susceptible to the effects of the poisons; and this error has been continued by succeeding historians almost to our own times.

Improvement in natural knowledge has relieved us from many of these terrors; but so indolent are we in enquiring after truth, that the only inference some of us draw is, to suppose the ancients were acquainted with poisons of which we are ignorant. One should willingly believe that any art of destruction had fallen into disuse: but there is no reason to consider these among the *artes perditæ*. Though Homer describes Ulysses as having learned the art of poisoning arrows, it does not appear that the poet found himself justified in describing the effects of them during the Trojan war. However, the vague tradition remained, and probably it will hereafter be found, that our terrors of similar weapons among ill-taught barbarians, are no better founded. Since musquetry has been invented, it has happened from reduced health, by a tainted atmosphere, or insufficient food, or both, that every wound has proved gangrenous or ill conditioned. This, at one time, produced the suspicion of poisoned bullets. Probably the same cause produced the same suspicion of arrows; nor is there any thing bold in suspecting that many accounts of poisoned springs have no better foundation.

But if the ancients were haunted with these terrors of poison, they were free from many of those apprehensions concerning contagion, which have detracted so much from the peaceable enjoyments of the moderns. There are many reasons for this.

Those infectious disorders which arise from a mere tainted atmosphere, in consequence of the too close accumulation of diseased subjects, were probably more frequent than in our own days. They were, however, less attended to. The different classes of mankind were more separated, and the destruction of thousands of slaves, or the plebeian race, was a matter little felt by the higher rank*. In the plague described by Homer, it does not appear that any of the

* Conficerunt me infirmitates meorum mortes etiam, et quidem juvenum———non ignoro alios hujus modi casus nihil amplius vocare quam damnum, eoque sibi magnos et sapientes videri.—PLIN. JUN. lib. viii. Epist. XVI.

chiefs suffered; but the lessened number in the ranks at last became an object of consequence, and the hero who first thought this calamity worth notice, is throughout the poem described as particularly attentive to his Myrmidons. In the siege of Syracuse, the Carthagenian generals, having no advantage above the privates, all perished; but considerable as the mortality was among the Romans, all the generals escaped. For the most part in war, and still more in peaceable times, famine was the forerunner of pestilence, and the rich generally escaped both. The disease ceased with the cause which produced it, and the individuals who fell were scarcely known, excepting by their nearest connections.

But by some causes which have not hitherto been explained, diseases have been introduced, which spread in a manner that defies all caution. No prudence can protect us against a contagion which may be conveyed by so many sources as the small-pox, and the venereal poison finds its way to the retreats of the most wealthy libertine. Men, whose lives are varied with a perpetual series of enjoyments, or who promise themselves such a succession, feel proportionably apprehensive of whatever may shorten or interrupt such a career. Hence imaginary evils are multiplied, and the anxiety to enjoy without danger, proves a source of greater miseries than we dread.

When a plentiful table, luxurious baths, and a cautious seclusion were no security against a disease which might be introduced with a napkin, all cutaneous complaints became more than ever an object of suspicion without discrimination, and almost without examination. Under the general denomination of lepers, thousands, whose only crime was poverty, were condemned to imprisonment or exile. If we may believe Matthew Paris, the number of lazarettos erected after the holy wars, amounted to 21,000; and we are told that in the metropolis of a neighbouring kingdom, the façade of such a

building exhibited a gibbet for such as dare either enter or escape without permission.

These terrors might gradually have subsided: eruptive fevers produce their crisis, and death or recovery follow. Chronic eruptions were less frequent among the wealthy, and when they occurred were overlooked, like their vices. Among the poor, who depended more for support on uncertain contributions than on manual labour, a prison was at least a security from want. But when a disease arose, which spread in proportion as the means of indulgence were extended—for which, at first, no remedy was known, and which, when apparently subdued by a remedy, would afterwards show itself in a distant part, and even when driven from the skin, would appear in the bones—all reasoning seemed at defiance. The horrors of so uncertain an infection were scarcely less dreadful than those of religious superstition; and quacks as well as regular practitioners were sometimes as industrious in availing themselves of these, as monks and the secular priests of the terrors of eternal damnation.

CHAPTER II.

As our knowledge of the productions of nature has relieved us from many terrors concerning poisons, so a more correct attention to the laws of disease has somewhat lessened our apprehensions of contagion. There is therefore reason to hope, that in proportion as this knowledge increases, the happiness of mankind will be less alloyed; and if such is really the case, there cannot be a subject more deserving the attention of a physician, than an enquiry into Morbid Poisons.

In these enquiries we shall have frequent occasion to use terms, in the application of which men are not universally agreed. Let us therefore endeavour to fix meanings as precise as possible to words very often considered as synonimous. Whether we shall be supported by etymology or authority, will, I trust, be less considered than whether we adhere to our definitions. The words I refer to are *endemic*, *epidemic*, *contagious*, and *infectious*. The first might be extended to all the diseases which are found only in certain districts and climates, but it is not intended to include those which attack only certain constitutions or under certain circumstances of living. Among these are the goitre of Switzerland, leprosy of Syria, Barbadoes leg, scrophula of cold and uncertain climates, and other complaints, which, though only known in certain regions, are even in them confined to the smaller part of the inhabitants. But the agues of damp and colder, the remittents of warmer climates, and other acute diseases, are so general in their effects, that only few constitutions escape them. These may therefore be called endemic. They prevail also at certain seasons only. Whether they are infectious it is not our business to enquire; but in their origin they are unconnected with the morbid secretions of the human body. The liver complaint of

the East Indies has, I believe, never been considered as infectious; the same opinion of the yellow fever is gradually gaining ground; and even the respectable testimony of Dr. Cleghorn has been insufficient to induce us to rank intermittent fevers among this class of diseases. They are, however, all of them confined to the inhabitants of particular countries, and may therefore, when they rage, be strictly called endemic.

Epidemic is a term which should be only applied to such diseases as are confined to no particular country or district, but originating in some unknown properties of the atmosphere, extend according to the progress of the wind. Of this kind the most remarkable is the influenza, and probably the autumnal dysentery. These in their origin being no way connected with diseased secretions, do not come within the description of morbid poisons.

Mr. HUNTER, who first employed that term, confined it to such poisons as arise from disease, and excite a secretion capable of reproducing the same disease. He divided them into simple and compound. It will be more consistent with my plan previously to divide them into contagious and infectious.

By *contagious* I would understand those diseases with the origin of which we are now unacquainted, but which at present can only be propagated by contact with a person, or matter from a person under a similar disease. Of this kind are the itch, venereal disease, the yaws, small-pox, measles, and many others.

Infectious diseases, on the contrary, may be traced in their origin, and do not require for their production matter similar to their effects, but may at any time be generated by crouding together the sick or wounded of any description. Of this kind are the hospital, prison, or ship fever, camp dysentery, and some peculiarly malignant ulcers. Though these diseases, when formed, may produce their like in others, yet we can always trace their origin in causes different from their effects.

Contagious diseases, which it is now our business to consider, may be divided into chronic and acute; of the former are the itch, the venereal, and several others. These are, for the most part, incurable by the unassisted powers of the constitution. The acute, of which are the small-pox and many other exanthemata, produce a critical fever, which ceases with the disease. The chronic may attack a person as often as he is exposed to the exciting cause, the acute, for the most part, leave the constitution no longer susceptible of their irritation.

Though I have found it necessary to make this division, yet, like all other pathological arrangements, it may be liable to some objections.

Infectious and contagious diseases may both be said to be epidemic at certain times; that is, there are certain constitutions of the air more favourable to their *extension* than others, yet they differ from true epidemic diseases, in as much as the state of the air never can *produce* them without the presence of certain diseased animal secretions.

Some infectious diseases are said to become contagious, but they differ from the true contagious, because they can originate in animal secretions different from those by which they are afterwards propagated, as we have already illustrated in the difference between hospital fever and small-pox.

Hydrophobia seems an exception to all these divisions, containing the terrors of all united. It is at least doubtful whether it may not be produced by the secretion of an animal that exhibits no previous symptom of the disease. It is certain that it can afterwards be propagated by contact, that it produces a critical fever, under which the constitution is unable to support itself, and against which we have no remedy.

There are certain circumstances common to most morbid poisons. — The first is that all persons, if susceptible of their

impression, are more so in proportion as they are unaccustomed to it. It is now pretty generally admitted, that a new-born infant, if not infected in utero, is insensible to the contagious or infectious properties of the atmosphere which his mother breathed during utero gestation, and in which he is born: he would probably continue so always, if never removed from it. Physicians and nurses escape the infection of an atmosphere which is deleterious to others, till it acquires properties to which even they are unaccustomed. Most contagious diseases affect the same person constitutionally only once through life. This is well known in the small-pox, and probably the same happens in syphilis; for though the local effect may be produced an indefinite number of times, we have no cases on record in which the constitution has been infected from more than one source.

2dly, Mr. Hunter, who, as I before remarked, first saw the necessity of a distinction between the original and morbid animal poisons, observed three states of the body which are necessary for the existence of the latter. Susceptibility, Disposition,* and Action.

Susceptibility is either of a part or of the whole constitution, and depends on the state of either of them at the time it is exposed to the influence of a morbid poison.

If the susceptibility exists when the poison is applied, the DISPOSITION must take place: and after a certain period, various according to the laws of the poison, the action will follow.

That susceptibility is necessary we know, because we find that some people cannot be affected by a poison, to the influence of which the rest of mankind are subject. We find also that some constitutions, or even some parts, are insensible to a poison at one

* Disposition must be carefully distinguished from *pre-disposition*. Pre-disposition has no reference to morbid poisons, but implies an original, usually an hereditary formation in the constitution, which renders it liable to fall into certain diseases peculiar to certain climates, or excited by certain causes, as scrophula or madness.

time, the influence of which they feel at another time. It also appears that at certain times the *part* to which a poison is applied, will be found susceptible thereof, yet that the *constitution* will resist it. This, in most cases, happens in the venereal and also in the small-pox, if the constitution has already gone through the disease. It will even sometimes, though rarely happen, to people who have not had small-pox.*

That such a state of the part and of the constitution does exist, and from causes beyond our present knowledge, is now well established, and, I trust, it will presently appear to be a matter of more consequence than a mere distinction of words. Inoculators, who are much accustomed to such observations, can very early satisfy themselves whether the part or constitution, or either, is susceptible of small-pox, or cow-pox, when a puncture is made and matter applied.

1. If neither the part nor the constitution is susceptible, the puncture heals like the effect of any common injury.

2. If the part only is susceptible, inflammation takes place *early*, and suppuration *soon* follows, with little or no fever.†

3. If the constitution, as well as the part, is susceptible, the local action takes place *more slowly*, as if interrupted by the constitutional *disposition*. As the local action advances, the suppuration is preceded by the constitutional action, and the disease becomes general.

It is of the greatest importance to attend to these three states of the constitution, since, for want of such attention, many lives have been rendered wretched, as well as many constitutions ruined

* See Med. Transactions, Vol. III. Page 385.

† Had this been well understood at the time, Mr. Dawson would not have been deceived, as he candidly acknowledges himself, in the case he gives of the inoculation of two children. The attentive reader will perceive the whole progress was quicker in the first than in the second inoculation. See Medical Transactions, ut supra.

by the apprehension from or ill treatment of venereal cases, and others have been lulled into a false security respecting their susceptibility to the variolous poisons. This will be further explained when those diseases come before us. But it was necessary, at this place, to mark these distinctions, because even if the *susceptibility* exists, and the *disposition* takes place, though the *action* must follow, yet it may be suspended for a while, if the constitution is engaged in another action. For,

4thly, Two actions cannot be carried on at the same time in the same part, or in the same constitution.

Though this law was entirely overlooked till Mr. Hunter's time, yet it is now as well ascertained as any other in pathology. It is worth remarking, that in all the epidemics described by Sydenham, in which small-pox and measles raged at the same season, he gives no hint of their ever appearing at the same time in the same person. Dierembroek, indeed, mentions a solitary instance in which the two diseases took place at the same time. His son remarks on the passage, that he never met with a similar instance more than twice in his own practice. Dierembroek's account is somewhat confused. It should be remarked, too, that measles was at that period not so distinctly marked as in later times, and that with the small-pox and other exanthemata, an universal efflorescence, which may be mistaken for measles, is not uncommon. We have, however, two cases given us by that close observer, Dr. Russell,* which, from the reputation of the author, deserve particular notice. The series and order of symptoms are traced with such accuracy, as not only to place the fact beyond a doubt, but to enable us to make remarks without conjectures, or with only such as every reader will see are admissible.

We shall first observe, that when in the ill-built and crouded city of Aleppo, small-pox and measles were at the same time

* Medical and Chirurgical Transactions, Vol. II. Page 90.

epidemic for three months together, only two such cases occurred. In both these cases the measles were of a favourable kind. It is now established, that if measles do not produce their full action, they sometimes occur a second time.* It would be a curious enquiry, if there were any chance that it could be satisfied, to learn whether, on a future epidemic, these children took the measles again. However, those who know how few laws in pathology are without any exception, will only be surprised that this should so rarely occur.

Dr. Lettsom † relates the history of a family, consisting of the parents, eight children, and three servants, among whom scarlatina and measles appeared about the same time. Some had measles first, others scarlatina. All had the latter, and as many as had not gone through them before, had measles. In consequence of this succession, adds the Doctor, the diseases continued in the family for two months, which probably might have terminated in as many weeks; "but no person had the two diseases at the same time, so far, at least, as could be ascertained by the symptoms."

The industrious De Haën, wishing to discover a common original cause for all the exanthemata, remarks, that small-pox and measles usually become epidemic about the same time. Referring to his notes, he observes, that in the year 1752, scarce a family but was visited by both diseases at the same time, yet each individual had the two diseases in succession—videas successive iisdem in ædibus occupare infantes quorum alii morbillis, variolis alii laborere demum incipiant.—Rat. Med. Vol. I. Page 102.

Dr. Winterbottom, in a general vaccination, describes the retardation of that process, by the occurrence of measles, as an event too common to excite any particular attention.‡ The same retardation was remarked by Dr. King, from the same cause.§ The

* See Dr. Willan's Diseases of London, page 207.

† Mem. of Med. Society, Vol. IV. Page 288.

‡ Med. Trans. Vol. XIV. Page 25. § Id. Vol. XIII. Page 167.

same occurred to Mr. Wachsell, at a general inoculation in Walthamstow.

Dr. Willan observes, that it is generally found the small-pox, measles, scarlet fever, and hooping cough, become epidemic about the same time, and continue their progress, though not with equal violence.* Yet of the three former he gives no instance in which two of them appeared at the same in the same person. It is evident this was not from any inadvertency, because we find the same accurate writer, on another occasion, observing, that hooping cough and small-pox had occasionally occurred in the same person at the same time. On this I would remark, that after the febrile paroxysm of hooping cough has subsided, the disease loses its specific character, and if the lungs have materially suffered, the cough may be exasperated by the variolous paroxysm. But Dr. Willan, in another place, † asserts, that in some instances hooping cough commenced during the small-pox eruption. If this fact had been furnished by a less accurate observer, I should have objected, that whenever hooping cough is epidemic, we have usually other severe coughs at the same time, which, in children, are not easily distinguishable from hooping cough. But, admitting the accuracy of the statement, it only proves, as Dr. Willan observes, that the law, though very general, is not without any exception. We have also his own authority, that in some instances the hooping cough was instantly superceded by small-pox, and after the decline of the latter, returned with the same violence as before. Mr. Oakes ‡ relates the case of a child whom he was under the necessity of inoculating with small-pox whilst under the hooping cough. The consequence was, that as soon as the eruption appeared, the cough ceased, and never returned. The same has frequently happened after vaccination, and, I have reason to believe, permanently, as the cough has not returned, at

* Diseases of London, page 105. † Page 38 and 39.

‡ Medical Journal, Vol. VIII. Page 426.

least, ten days after the process of vaccination has been completed. This is now so generally understood, that many mothers have brought their children to the hospital for vaccination, under an expectation of curing them of hooping cough, and I do not recollect that any of them have been disappointed. However, I would never recommend it till the acute symptoms of the cough are passed, for, as till that time the full action of the disease is not over, it is reasonable to expect its return when the process of vaccination is completed.

To those who are fond of tracing the operations of nature under disease, it will be curious to mark the exact regularity of the succession of these morbid poisons when they occur in the same subject. Mr. Hunter* found the variolous insertion in his patient interrupted on the fourth day after the puncture was made, till which time it had proceeded regularly. On the day following, the morbillous fever commenced, and on the fourth day after that, the measles appeared. Four days afterwards, the measles began to disappear. During these eight days, the variolous insertion had made no progress; but on the following day it recommenced its process, and in five days afterwards the variolous fever commenced. Here the constitutional *disposition* was interrupted on the fourth day. On its recommencement, five days more were necessary before the *action* could take place, nine days being the medium between the insertion of small-pox and the commencement of the variolous fever.

Mr. Cruickshanks's case† appears to have been interrupted *ab initio:* for he found his patient with all the symptoms of morbillous fever on the ninth day after inoculation. The punctures of the arms, therefore, continued invisible till the constitution began to recover from the measles, after which time the punctures inflamed,

* Introduction to Treatise on the Venereal Disease, and also to the Treatise on the Blood, &c.

† Treatise on the Absorbent Vessels, page 126.

and required their full period of eight days for the appearance of small-pox.

In a general vaccination at Walthamstow, two children were seized with the morbillous eruption on the eighth day after vaccination. In these the areola from that virus was suspended for four days. In a third, on the tenth day; and here the interruption to the process was only three days.

But a most elegant experiment, in illustration of this subject, is contained in Dr. Willan's last number of Cutaneous Diseases.— "I inoculated," says Dr. Willan, "about the same time, three children with the fluid contained in these [lymphatic or miliary vesicles in measles], but no effect was produced by the inoculation. A similar trial, at the inoculation hospital, proved more successful. Richard Brooks, aged 18, was inoculated by Mr. Waschsel, with fluid from the miliary vesicles in measles, and with vaccine virus, January 6, 1800. On the 10th, there was some redness and elevation in both the inoculated places. January 15th, the redness round the part, where the lymph of the measles was inserted, had disappeared, while the vaccine pock was vivid. January 18th, the vaccine disease was over. January 22d, he had a severe cough, sneezing, and watery eyes, with cold shiverings and fainting. January 28th, the measles appeared; his eyes were inflamed and the lids swollen. January 29th, the efflorescence was diffused all over the surface of the body; frequent cough and violent fever. February 1st, efflorescence disappeared; cough and fever much abated. From that time he gradually recovered, and was dismissed in health on the 12th of February."

This interesting passage, besides containing an account of the successful inoculation of measles, affords also a striking illustration of the protraction of a disease, after the disposition to it had taken place, and its regular return to complete all its periods, as soon as the cause which interrupted them ceased. The subject was, at the

same time, susceptible of the two contagions; and as long as the diseased actions were local, both went on at the same time in different parts. But as soon as the constitutional disposition commenced in one [the vaccine], the local action from the other was suspended. When the vaccine disposition and action were completed, the rubeolous disposition commenced. Four days afterwards, the constitutional symptoms first showed themselves, with cough, sneezing, and watery eyes.* In six days more the eruptive symptoms began, and were completed on the following day. On the fourth day afterwards, the efflorescence disappeared, and the other symptoms abated, making, in the whole, about twenty-seven days, a fair allowance for the two diseases.

It is to be hoped that a very correct attention to the processes of variolation and vaccination will enable us to ascertain the exact analogy which exists between two morbid poisons evidently distinct, yet not entirely separated by those laws which we have traced in others. The correspondence in the laws of each—the peculiarities in which they differ—the manner in which they interrupt each others action, and the manner in which they may be made to act in concert, are all equally interesting, and are the daily subjects of experiment and observation. At present, the following may be considered as established facts:

Each, when inserted, requires about the same time to produce its local effects.

A subject, that has regularly passed through either, is insensible to the future constitutional effects of the variolous poison.

Each will produce secondary eruptions, having the same property of infecting as the primary; but secondary eruptions appear more than ninety-nine times in a hundred in small-pox, and not

* Dr. Willan considers the common period, after infection by effluvia before these symptoms appear, to be from six to ten days. The anticipation, in this case, by inoculation is nearly analogous to the mean difference between inoculated and casual small-pox.

once in three hundred, if the skin is otherwise entire, in the cow-pox.

In the small-pox, secondary pustules appear whilst the primary is advancing, and maturate two or three days after them. In the cow-pox, when secondary vesicles appear, it is not till the primary has begun to scab.

The small-pox may infect by the effluvia, the cow-pox can only be communicated by the secretion from the local infection.

When these poisons are inserted at the same time in the same part of the same subject, by mixing the secretion of each, only one will produce its effect.

When inserted separately in different parts of the same subject, each will produce its local effect, and at the same time.

If cow-pox is inserted at the same time that the subject is exposed to a variolated atmosphere, the former will supercede the effects of the latter. If inserted four days later, the effect will be less certain, but as far as can be ascertained, not more uncertain than from the variolous insertion.

It has been said, that no combination of them will ever produce a hybrid disease. I am not fond of adopting a term which may produce further altercation for want of a precise definition; but that the diseases may be confounded, that is, that the laws peculiar to one may occasionally influence the other, will appear by the following registers:

REGISTER, 1st.

August 14, 1805, William Croft was inoculated, with several others, from a subject who had casual small-pox. Croft had diarrhœa three days after he was inoculated, a circumstance in children often favourable for the future disease.

On the 3d day the insertion appeared elevated.

6th, a vesicle.

8th, the vesicle spread.

10th, has a vaccine appearance, with fever.

13th, one hundred and fifty pustules appeared, which passed regularly through their stages somewhat shortened, as often happens in inoculation.

Rogers was inoculated, 26th August, from Croft, in two places. Only one took effect, which was perfectly vaccine in all its stages. The child had been previously ill, so that it was difficult to ascertain whether any or what degree of constitutional disorder was produced by the inoculation.

Mary Ann Dobins, having been previously inoculated from Croft without effect, was,

September 2d, inoculated from Rogers.

The arm proved vaccine in all its stages.

On the same day were inoculated from Rogers—

I. Richard Jude. His arm was vaccine in every stage.

On the 13th day, as the arm was drying, appeared 150 variolous pustules.

II. Eleanor Watts. Arm vaccine.

Pustules appeared on the 11th day.

On the 13th five hundred were counted; all maturated, but dried early.

III. Elizabeth Gray. Her arm regularly vaccine to the 8th day.

On the 10th, appeared stationary, in consequence of which inoculation was repeated from Edward Christian's arm, who had been inoculated twelve days.

12th day, the arm first inoculated retains its vaccine appearance, though somewhat jagged with elevations round the vesicle. She had fever the day before, and pustules first appeared on the body.

13th, the arm retains its circumscription, but is yellow. The fever considerable all night.

14th, the first inoculation dry; the second contains a yellow crystalline lymph, with areola. Has upwards of sixty small circumscribed pustules.

15th, arm drying, pustules suppurating.

19th, pustules drying.

22d, scabbed.

IV. Thomas Dyson. His arm was perfectly vaccine in all its stages.

10th day, a few pustules appeared; had been sick on the 9th evening.

12th day, the arm drying.

From Dobins seven were inoculated; of these

Five had no eruption; the arms were vaccine in all the stages, and in the appearance of the scab.

One had a perfectly vaccine appearance on the arm, areola, and brown scab, with one hundred variolous pustules, which appeared on the 12th day, and began to dry on the 16th; but the desiccation was not completed till the 29th, when the appearance was horny.

The other had a vaccine arm, somewhat irregular, with fever, but no pustules.

From the last were inoculated four. Of these two had vaccine arms, perfect in all their stages, and without pustules.

One had the vaccine vesicle regular, excepting that the edges sloped in such a manner, that the base was broader than the apex. The top was, however, flat, and the whole appearance such as occasionally occurs in the genuine vaccine.

The other had small pustules, which dried, as well as the place of insertion, by the 15th.

Elizabeth Gray, we have observed, has pustules. Two were inoculated from her arm, and two from her pustules.

The two from the arm had the legitimate vaccine appearance.

One from the pustules had fever, with general efflorescence.

The other had all the symptoms of vaccination, with the areola; but the contents of the vesicle became yellow before it dried.

It is unnecessary, in this place, to pursue this register any further. Suffice it to say, that the enemies to vaccination, about this time, excited so great a clamour, that every mother was suspicious lest her child should be clandestinely inoculated with the cow-pox; and even those who saw matter taken from secondary pustules, and applied to the arm, were scarcely satisfied, unless their own children had unequivocal symptoms of small-pox. Reflecting, therefore, that an event of this kind must either occur again, or be unsatisfactory from being unsupported, we contented ourselves with the record preserved in the register, waiting till it should be explained by subsequent occurrences.

REGISTER, 2d.

On the 21st November, 1805, Thomas Froome, aged 15 years, in service in London, was seized with natural small-pox. His employer inconsiderately sent him to his family, consisting of his parents and seven other children, neither of whom had gone through the disease. As they lived only a few miles from town, the father applied to the hospital for assistance. His son was received into the house, and the father advised to be vaccinated, with all his family, in order to supercede the variolous infection. When every argument proved ineffectual, the whole family was admitted into the inoculating house, and inoculated from Thomas. His eruptions were of the opake white sort, a distinction which has not been made either by Sydenham or Frend, but which, if there is a kind more favourable than others for inoculation, has hitherto claimed the preference. The character of the pustule was preserved in the whole family, the insertions proving more circumscribed than usual,

and the whole process being attended with very little fever and few eruptions.

From one of these a boy was inoculated, who had an hundred pustules, which retained the same character. He had but little fever, and the inoculated part was circumscribed.

From him a subject was inoculated with three insertions, and at the same time vaccinated with two insertions in the other arm. All the insertions preserved their character in all their stages, the variolous being circumscribed as above : he was feverish for two days, and had five white secondary variolous pustules.

From the above one person was inoculated from the vaccine insertion and another from the pustules in the face. The arm of the first pursued the regular process till the 16th day, when the areola was arrested, and one hundred white circumscribed pustules appeared.

In the other, who was inoculated from the pustules in the face, the insertion was vaccine in all its stages. The vesicle much larger than common, and dried of the dark brown colour. A few papillary eruptions appeared, but never maturated.

It is unnecessary to continue any further account of this register. The above cases are sufficient to show such an analogy as might be expected between two poisons, one of which is sufficient to render the constitution no longer susceptible of the other. I shall only remark, that in all the cases of both registers, the secondary pustules were variolous, having the exact character of the white sort above described. The secondary cow-pox eruption, when they appear, are totally different from the primary, and also from any kind of small-pox pustules, excepting the crystalline. Their resemblance to these made me very sanguine in my expectations, that by inoculation they would assume the vaccine character. But in this I have been constantly disappointed.

The invaluable Jennerian discovery has introduced us to a morbid poison, the properties of which are different from all others that we are acquainted with in superseding the constitutional susceptibility to another. As such a law as this is unknown in any other two morbid poisons, we might suspect the analogy between these two would be closer than between any other two. We might even expect that the characters of the two might be altered by applying both at the same time, and also that the phænomena of one might imitate the phænomena of the other in such a manner, as to render the distinction between the two often doubtful. It is, therefore, rather a matter of surprise, that the distinction should be so regularly preserved, and the laws which separate other morbid poisons so rarely infringed.

The following laws, then, are to be admitted with as few exceptions as any others that are received in pathology. To render these as plain as possible, I shall offer them in the form of aphorisms and add a few commentaries to illustrate them still further.

1st. All persons are susceptible of the impression from a morbid poison, in proportion as they are unaccustomed to it.

This is proved in a variety of instances. Many children, born in London, live for several years without receiving the small-pox. In the same neighbourhood a person arrives from the country, and without any apparent intercourse with an infected person, is attacked by the disease. A want of due attention to this circumstance has produced, I suspect, some of that scepticism relative to the infectious effiuvia of the small-pox, which appears even in the candid and accurate Dr. Heygarth. It is not to be wondered if the small-pox infection was propagated in America from such apparently slight causes, when we reflect, that, during the war, twenty-nine practitioners in medicine were inoculated in a few weeks. This is enough to show how little accustomed the Americans were to the small-pox effluvia, and, consequently, how much greater the susceptibility to it must

have been in them than in the inhabitants of large towns in England, where the disease is seldom absent for many years.

It is well known that voluptuaries, who remain in a state of celibacy, without much restraint on their indulgences, are rarely infected with the venereal disease after a certain time.

2dly, That *susceptibility* and *disposition* are necessary in a constitution or part, before the *action* excited by a morbid poison can take place.

We find some persons are never susceptible of the small-pox. Others have for years resisted every means of infection, and yet afterwards proved susceptible. Even if the part or constitution, or both, are susceptible, the disease does not immediately follow the application of the poison either by effluvia or in the form of matter. It requires a certain time, during which the constitution seems *disposing* itself for the future action; for if any other disease interferes, the action of the disease from the infection is protracted; the new disease goes through its usual progress, after which the constitution recommences the process of disposing itself for the first disease, and the action follows after the usual period required for that purpose. As this state of the constitution, from the period of receiving the infection to the commencement of the disease, is often the subject of very important enquiries—as it is a state in which we can neither prevent nor cure the disease which is to follow, but of which, in some morbid poisons, we can protract the subsequent action, and in others we find it occasionally protracted by incidents which we could not prevent, but ought always to be prepared for—on all these accounts Mr. Hunter denominated this state of the constitution *the disposition for the future disease.* I hope this explanation will be intelligible, if it is not satisfactory. Though I have thought it necessary to offer it, because Mr. Hunter's writings cannot be understood without it, yet, as several candid and ingenions men have objected to the term, I shall avoid using it as much as possible.

3dly, That after the constitutional disposition has taken place from a local diseased action, the destruction of that local action will not prevent the future appearance of the constitutional disease.

This is a subject of the highest importance to be well understood and attended to. But as it can only be ascertained by inference from a variety of facts, which cannot here be anticipated, we must reserve the further explanation of this aphorism till we come to the proofs on which it is founded.

4thly, That no two actions from two different morbid poisons can be carried on at the same time in the same part, or in the same constitution.

After the numerous illustrations already offered, this aphorism scarcely requires any comment. It will also follow from this and the third aphorism, that,

5thly, If a constitutional disposition to one morbid poison exists, whilst the action of another is going on in the constitution, we ought to expect the action of the first to appear after the action of the second is completed or has ceased.

The above illustrations have confirmed this aphorism.

6thly, Though nothing can prevent an *action* from following after a *disposition* has taken place, yet a *disposition* may be *prevented* by preventing a susceptibility in the constitution or part.

7thly, The susceptibility may be prevented by rendering the constitution familiar with the morbid poison, or, as long as the constitution is exposed to it, by keeping up a constitutional action previously excited by another morbid poison, or any other cause.

CHAPTER III.

CONTAINING INFERENCES FROM WHAT HAS BEEN RECORDED BY OTHER WRITERS, WHICH MAY ILLUSTRATE THE DOCTRINE OF MORBID POISONS.

FROM our being able so well to ascertain the immediate causes of such diseases as are to be traced from morbid poisons, and from an accurate observation of their progress under different combinations of events, the laws peculiar to some of them are likely to be ascertained with as much precision as pathology can admit. It is to be regretted, that the same cannot be said of some others, which, without doubt, when well understood, will be found to observe the same regularity as all the other operations of nature. Even in the venereal disease, which has existed for three centuries, and for which a specific remedy has been known almost as long; till our own days, no writer has undertaken to trace the series and order of the poison and remedy, so as to detect the laws by which each of them is governed. It is still more 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 occular 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 was usually condemned at first sight. Such has been the facility with which this *insidious disease,* as it was often called, was 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 was said to have its crisis and termination, though its periods, from being longer, might appear more irregular. By some * too it has been described as never inducing suppuration. 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 resemble, being knobbed as they are." If the cure goes on well, the funguses scab over, and by degrees, the scab falling off, discovers the sound skin underneath. Such is the general description of the disease; its other peculiarities will be remarked in their order.

In its fungating property, it was supposed to show some resemblance to the sivvens of Scotland. But in the latter the funguses are described as rising after ulceration. In this disposition to form fungus, sivvens differs from the accounts we have of the Canadian disease, and both of them from the venereal in the phagedenic nature of the ulcers. The Canadian disease, too, is described as sometimes cured by the unassisted powers of the constitution, which never happens with the venereal. 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,

* Edin. Med. Ess. Vol. VI. Page 312.

† Bell's Treatise on the Lues Venerea, Vol. II. Page 242.

‡ Swediaur on the Venereal, Page 175.

whom he does not undertake to refute, that sivvens yields more readily to corrosive sublimate, than to mercury in any other form. This is contrary to the now established practice in the venereal disease.

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

* Phys. Essays, Vol. III. Page 154.

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 endeavours will entitle him to. The Encyclopædia Britannica disappointed me much, in making no mention of sivvens, under its medical article. But as the letter S is nearly in order, we may expect to be gratified in this particular, in a work which challenges a sort of national reputation.

In both these applications, which are here copied from the former edition of this work, I was disappointed, however, I have reason to hope that the communications which have since been published, and my own observations on the spot, will enable me to give a tolerably accurate account of the disease, which shall be related in its order. Concerning the Canadian my enquiries have been less successful, as, I believe, there are no printed account of it, excepting such as have been extracted from very confused manuscripts.

Though all these and many other morbid poisons 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 it arrives 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, the progress 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 pus confined under the cuticle of the palms of the hand and soles of the feet. In this case we find a large sub-cuticular collection, which breaks the cellular membrane, 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 effects by passing through the pores of the cuticle, and ulcerating the skin beneath it. Yet, even 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. 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 discovered in the part, or in the person with whom the infected has been in contact. The antients, who were ignorant of the venereal poison, 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 reflect, that these ulcers are seldom, if ever, met with in the *vagina*. It is evident, however, that the ancients were sensible of the great liability to ulceration in the mouth and genitals. Pliny * has enumerated more remedies for ulcers in these than any other parts. 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 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. These he considers as curable 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 to excite apprehensions for the orifice of the urethra. Celsus was so well aware of this, that whenever the case happens, he advises instant circumcision, and a probe to be introduced, lest the prepuce should adhere to the glans, and 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

* Histor. Natural, passim. † Lib. VI. Chap. 18. ‡ Vol. I. Art. 19.

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 will, I think, be admitted from a deficiency in their character, which will hereafter be taken notice of, 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 account for the disease ceasing. In the second no mercury was given till a 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 having appeared afterwards on the scalp and tibia. The cure of these likewise without mercury is a further proof that the source was not venereal, and that other poisons may by inoculation produce effects which may be mistaken for 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

* Syph. Page 248.

† Hunter on the Venereal, Page 385.

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*. 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.*—The worst kind of erysipelatous inflammation is for this reason among his species of cancer.

But what I particularly wish to remark here is his description and division of phagedæna into two species. The first is the common phagedæna, for the cure of which he advises the actual cautery. The other he describes as beginning with a blackness or slough, and if not prevented, spreading to the bladder, in which stage no assistance can be given. If this is seated on the glans near the urethra, he advises the same remedy, with proper care to preserve the orifice of the urethra, but if the disease has penetrated deep, that the knife should be used.

The first species is by no means uncommon. The second volume of the London Medical Transactions 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 which proved fatal 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 can-

* Lib. V. Cap. 26.

cerous nature, occasioned 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 previous scirrhus or any surrounding callosity?

The other species of phagedæna is, I believe, described only by Celsus.† Of the accuracy, however, 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 it 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

* Med. Trans. Vol. II. Page 337.

† Since this was published Mr. Platt (Enquiry into the Efficacy of Oxygen) has produced another case. Mr. Abernethy has also favoured the public with a case of alternate sloughing and ulceration. See Surgical Observations, Page 129. I shall hereafter have occasion to mention this performance.

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 encreased. 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. This opinion was not 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 that 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 those changes common in an irritable habit. In a short time all these unfavourable symptoms disappeared, the wound assuming 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 aspect, 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 appearance about the skin at the edges of the wound. As the fever went off, all that part of the surface was cast off, 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 some other assistance. 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 was of opinion 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, it was impossible not to suspect that the disease was venereal, though Mr. Cline expressed his surprise 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 was now thought 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 more.

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 the constitution began to show the effects of the mercury, by the smell of the 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.

These symptoms were so different from what were to be expected in syphilis under the influence of mercury, that it was agreed to leave the decision of the case to the single test of the bones, which, if they should recover without more mercury, would prove that they were not likely to have been tainted with that virus. At all events, the remedy 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, was not objected 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, 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. The patient's 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 the physician he had consulted 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 the patient 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.

Such was the state of this gentleman's case when the first edition of this work was published. Since that time he came again under my care, and the following is the sequel of this very interesting history, as I afterwards collected from the patient, from the medical gentlemen who had attended him, and from my own observations.

For the last mentioned symptoms he was put under a sixth mercurial course, which had so completely affected his intellects, that, though he had no lineal or presumptive heir, yet his nearer relations thought it necessary to apply for a statute of lunacy. This they had no difficulty in obtaining, and he remained at a house in Hoxton till he gradually recovered his reason. In this state he again applied to me, about six months after we had parted. I found him with an ulcer on his shin, on the toes of each foot, on the forehead, and on each arm. He informed me, that a scale had exfoliated from his shin, and a joint of one of his toes had cast off. His health was, however, mending, his sores assuming a more

favourable appearance, and his reason having returned, he expressed great anxiety to resume the management of his affairs. In the mean while only common applications were used for his local complaints, and occasionally such remedies as his constitution required. By a legal process he recovered his liberty and the management of his concerns, when, partly from despondence, partly from habit and example, he indulged in a diet inconsistent with his irritable situation. This was not likely to lessen his despondence, and its temporary remedy was always at hand.

Retiring for better air to a neighbouring village, he consulted a surgeon of deservedly high reputation, whose assistance he had experienced in a former illness. This gentleman considered the disease venereal; but hearing my opinion was different, wished for the assistance of a surgeon, whose opinion was not less desirable, on account of his well known abilities, than his large opportunities of seeing such cases. The result of the meeting between these two gentlemen, was a confirmation that the disease was venereal, but that in the present state of the patient's health, it would be imprudent to give mercury.

In this posture things remained, as long as it was possible to keep him in patience, without the exhibition of mercury. At length, after repeated solicitations to begin his course, and being as often assured it was improper, he determined to judge for himself, and applied to an apothecary for some slight mercurial preparation. He received some pills, which he was assured were such as an infant might take without injury (probably a preparation of calomel.) After taking them a very few days, all his ulcers dried, his health recovered, but his reason again failed. He assumed the habit of a naval officer, sold to a public office the benefit of an insurance he had on his own life, and with this ready cash supported a kind of artificial existence without eating, during three days of the most extravagant dissipation. On the fourth morning he expired, as he attempted to rise from his bed.

This case, in its first stage, is exactly described by Celsus, as the phagedæna, distinguished by *quædam 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 the case above related was the effect of a morbid poison introduced from the broken skin at the lower part of the prepuce, can hardly be doubted; and that it was not venereal, is to me equally certain. 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 shown 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 might appear more probable, 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 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 suspicious, but the blotches ulcerated much earlier, and the progress of the ulcers was much more rapid than in the venereal disease.

The nodes appeared almost as soon as the skin was affected, 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 syphilis. Those spongy granulations are equally foreign to the true character of venereal ulcers. Lastly, as Mr. Hunter has observed on another occasion, there was an ambiguity in all the stages of the disease, its appearance, and the effect of the remedy.

I shall not now insist on one argument drawn from Mr. Hunter's observations on the laws of the venereal virus, namely, that when the disease is once cured in the skin or bones, it never returns to those parts from the same stock of infection. If the disease in other respects resembled lues venerea, this circumstance would only prove the fallacy of Mr. Hunter's theory. But I will venture to say, that, excepting that it was 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 on the penis described by Celsus, there is no mention of a thickened edge and base 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. Asruc,* whose principal failing is an attempt to admit too many anomalous symptoms on the authority

* Astruc, Vol. I. Page 407.—Barrowby's Translation.

of others, is however very particular in describing the callous edge and base, and though his theory on the cause of their formation cannot now be admitted, yet it serves very well to illustrate the progress of the ulcer till it acquires its characteristic hardness. 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 the ulcers he describes were much more common in his time than at present. But the truth is, that since the knowledge of the venereal poison 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 of the word *vitiatum*; and his description, *primo fere pingue est,* would lead us to suppose he had an imperfect idea that the bone was in some way contaminated. This opinion is strengthened by the great anxiety he shows, that the whole of the diseased part should be removed, either by the hot iron producing exfoliation, or by rasping or scraping. *Qui radit hæc 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

* Swan's Translation, Page 308. † Lib. viii. Cap. 2.

useless, and that nothing short of cutting away the part can be efficacious. It is now well known, that this severe treatment is not always necessary. Our knowledge of the venereal disease 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 cases which could never be relieved but by the removal of all the vitiated part, it is not surprising that 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 his 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 them.—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, as far as he confines himself to the supposed antiquity of the disease. I would also particularly recommend the perusal of Mr. Becket's defence of his paper, which may serve as another instance of the necessity of accurately ascertaining a disease. For though we read much about leprosy and the venereal disease, the author never thinks of defining either.

I could relate other instances of ulcers of this kind, but my wish is to confine myself to such cases 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 breast 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. Yet this ulcer was supposed to be the most virulent, as no ladies were infected by the woman after that ulcer was healed. The author remarks too 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, 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. The disease was considered as venereal, by the village practitioner, and in consequence she had been salivated by a mer-

* Fifth Edition, Page 297.

curial friction. The complaint 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 case, he determined it 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 one 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 veneral. 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. Many of these 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

* See Treatise on Veneral, Page 386, and seq.

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 rapidity never known in 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, and the constitution never showed any great mercurial irritation.

An accurate examination of all the Tooth cases will convince the attentive reader, that 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 was venereal. In each 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 degree, 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 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 32.

* Medical Transactions, Vol. III. Page 325. Hunter ut supra.

CHAPTER IV.

THE SAME SUBJECT FURTHER ILLUSTRATED.

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 many of them yield to mercury, either in their primary or secondary stages, how should the world have been so alarmed at the appearance of the venereal, which at most could be only an addition to those calamities it had always witnessed? I could answer, that the venereal, as far as we can ascertain, is 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 produce only gonorrhœa, the discharge from which is infectious, but this is probably the case in all other morbid poisons.

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 little known in Europe when the venereal poison 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 or phagedænic ulcers in the vagina, so that if they occur at all, it is at most but rarely.*

This last circumstance naturally leads us to enquire into the origin of a poison which cannot be traced in more than one sex, and which, as far as we know, may be given by the other when in apparent health. This is still more probable in the tooth cases. The subjects selected to supply the teeth were all carefully examined and most of them of an age when there was not only a probability of their freedom from contamination, but when the only proof remained that they had never been exposed to it. In all these cases, we may remark, there was a previous breach of the external covering in all the infected. 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 that part. In the case I have so frequently referred to, coition was continued while the skin of the prepuce was broken. 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 unecessary 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, that 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. Morgan, his late dissector, and more than one of his predecessors in Windmill-street, is well known.

* Mr. Blair has lately shown me a gangrenous sore in a woman at the Lock, which resisted mercury in every form and degree, and proved infectious. This is the only one Mr. B. has ever met with.

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. "Several 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 endeavours 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 re-

* Since the first edition was published, Dr. Heygarth has favoured the public with three other very valuable cases, all which were attended with fever. These with my remarks may be seen, Medical Journal, Vol. III. Page 201. and Vol. V. Page 498.

† Syphilis, Page 248.

maining slough, a fungus rose, which we shall presently find the law of every 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 to mercurial salivation. All these circumstances, particularly those of the primary disease, amount to a conviction that the case, 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. That neither the cases on the genitals or teeth are exactly similar to those which have been received by contact with putrid animal matter is certain. Those of Mr. Abernethy are very similar to the little ulcers we sometimes find on the glans, and particularly about the corona, which spread rapidly and superficially, and for the most part heal so readily, that we know not how to impute their cure to the mercury exhibited. Such is probably the *ulcer exedens* of Celsus. When the custom of putting the thumb over the orifice after bleeding was more general, this kind of ulceration or festering, as it was called, was much more common. I well recollect one in St. Bartholomew's, which spread so considerably, that such of us as were present readily made up our minds the patient had been bled with a lancet stained with venereal pus. 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* suscipions, and removed Mr. Crane's doubts, who, to

* 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.

the surprise 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. What has been said is, however, sufficient to point out the differences between well-known morbid poisons and others, which, though palpably distinct, have been confounded with them.

In attempting this, the principal caution is to mark every local appearance with accuracy, so as never to confound diseases which uniformly exhibit a different character. This want of accuracy has, on former occasions, been the cause of very serious evils. When the venereal disease 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, its series and order.

There is one peculiarity in the action induced by all these poisons, which we can only know by its effects, and which effects vary with each respective poison. I mean the properties of the secretion. By what means, or by what stimulus, the action of a part was first so altered, as to secrete matter which will stimulate another part in another constitution, to set up a similar action, we shall, I fear, in vain endeavour to enquire. At present we can ascertain this law with much regularity in those morbid poisons which have long been the subjects of our investigation. But if we have traced diseases in the mouth and other parts, which could not be discovered in the subjects from whom they were derived, it will not, I think, be doubted, that the apparently healthy secretions of one animal may, when applied under certain circumstances, prove poisonous to another: or that a part of one animal may prove a stimulus for an action in another, different from any that can be discovered in the first.

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 œconomy. 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.‡

* De Curtorum Chirurgia. † Alexander Benedictus.

‡ See Gentleman's Magazine for November, 1794.

Thus far we have only traced the poisonous effects of matter applied from one animal to another of the same class, under the influence of no specific disease, and also where a diseased action exists, but have learned nothing to teach us how diseases, which at present never appear to originate with the sufferers, and the contaminating effects of which are now so regular, 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. From the accounts given us we may conclude, that the same symptoms have not uniformly appeared as those which have followed his bite. 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 tumor 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 im-

* Though this description of cow-pox is incorrect, excepting in its consequence on the human, I have preserved it as an historical register of my imperfect knowledge of this disease, when the first edition was published. There was then no printed account of the cow-pox. Mr. Cline, knowing the object of my enquiries, acquainted me with what he had heard from Dr. Jenner, and by his correspondence procured me further information. What has since been explained by the discoverer, serves to illustrate my theory still more, since it has been shown that the disease in the cow may originate in a horse, and shows different phænomena in three different classes of animals.

possible to ascertain. It would be easy to suggest many arguments that might favour such an opinion, derived from the countries from which some of them 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.

CHAPTER V.

OF THE FIRST LOCAL ACTIONS INDUCED BY MORBID POISONS.

In tracing the first local actions induced by morbid poisons, we must keep the cases above related in view, and attend to the process of nature under other local stimuli. These we shall find varied according to the nature of the part and of the injury received. There are few injuries which do not prove a stimulus for an altered action, by which the part or constitution endeavours to relieve itself. If a stimulus, however apparently trifling, is applied to a secreting surface, the consequence is an increased secretion. Thus, if a pebble is retained in the mouth, an increased secretion of saliva follows. As the mouth becomes accustomed to the extraneous body, it ceases to become a stimulus, and only the customary secretion is induced. This is well known in the first application of a false tooth. But if the substance applied is of a more pungent nature, as in the instance of tobacco, the stimulating property continues from the impression the parts continue to feel, and the secretion continues also.

If a substance, to which the skin is unaccustomed, be applied, that substance proves at first a stimulus, and a disagreable irritation, often an itching, is produced, till the skin becomes accustomed to the sensation. If, however, the substance applied be of such a nature that the part cannot so readily accommodate itself to, as the fetters of criminals, the consequence is the ulceration of the part. After this the action varies according to the constitution or state of health of the patient; or the manner in which the extraneous body presses.

Sometimes the ulceration continues to extend in surface and depth, at others a fungus is thrown up, to serve as a further protection for the parts below, and sometimes the part heals, and a thickened or horny cuticle is formed, which proves an effectual preservative against the existing inconvenience.

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, that the edges must die for want of a due circulation. The necessary consequence of this is, that before any other process can commence, the dead part must be separated, which can only be effected by the absorption of the sound part with which it is in contact. 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,

* See Pott on Fistula Lachrymalis.

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. How much more complicated is such a process, than to admit that the lost substance, both of the slough 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 precede healthy granulation. If the same treatment be 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 for a great length of time. This is another resource of the œconomy to provide against the effects of injury or disease, when, from want of power, or from frequent interruption, the healing process is at last given up. In the last 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 any extraneous body is present, which cannot be dislodged, as often happens with a piece of dead nail, or as occurs with a pea in an issue, or with the continued application of fetters; instead of true granulations, we shall have a soft, spongy, 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.

Any one of these processes, or the interruption of them, 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 increased secretion as the effect of any stimulus applied to a secreting surface, and ulceration as the means of separating a dead from a living part; and also as produced by pressure and other stimuli. Suppuration, which is only an internal ulceration, is the means used by nature to bring any substance to the surface. In the case of morbid poison, increased secretion or ulceration arises from the stimulus of that poison, and continues as long as the parts are sensible to the stimulus. In this, as in all the

* 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. v. Cap. 26. Sect. 31.

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.

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 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 variety of consequences happens in morbid poison, according to the nature of the stimulus in each. 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 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 the three last cases as soon as slough commenced, the first ulceration ceased, and when the slough cast off, skinning immediately followed. Under such circumstances it would not be 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, were it not for the manner in which the parts heal, as will be hereafter explained. Certain it is, however, 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, though after a longer period, in yaws. But if the

* Philosophical Transactions, Vol. LXXII. and Introduction to Treatise on the Blood, &c.

† See pages 30 and 47. ‡ See page 30. § See page 42. ‖ See page 44.

constitution remains susceptible of the virus, and the slough is 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. Of this kind was the primary ulcer of the long case already related,* and also a case related by Mr. Abernethy.† 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 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 quality of the pus is altered. If this last should be within the power of the economy, the attempt will be continued; and the only danger will be, lest, before the pus is altered, so much mischief should be done as the constitu-

* See Page 32.

† Diseases resembling Syphilis, Page 129. See also Platt on the Efficacy of Oxygen.

tion must sink under. It is not, however, always necessary that the ulceration should be continued by the presence of the 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 case we may expect to see ulceration extending in one direction and a healing process in another. This will be best illustrated when we come to consider mercury as a remedy.

But if 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 as the powers of the constitution are unable to resist, or if no healthy actions 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 by being frequently interrupted.‡ And 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 from the second; hence the edge and base will be absorbed, and a new one formed. Of this kind are the cancerous and venereal ulcers. 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

* 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.

† See Page 63. ‡ See Pages 55, and 56.

parts of the cancer. Hence the ulcer by degrees assumes a rough, irregular appearance. In the venereal the progress is more regular, the callous edge and base being formed with more regularity; for the same reason they are as regularly ulcerated, and new ones formed. But though the progress of ulceration is quicker, and consequently more regular than in cancer, it is much retarded by the thickened edge and base 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:

Encreased and altered secretion on secreting surfaces, without loss of substance.

———————————— on non-secreting surfaces, with loss of substance, viz.

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 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 Sivvens and some anomalous Phagedænæ.
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.

CHAPTER VI.

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.

When the disease has attacked a secreting surface, and only altered and encreased the secretion without any destruction of parts, the cure consists in restoring the secretion to its natural quality and quantity. If the poison is of such a nature, that the parts may, by degrees, lose their susceptibility of it, the cure will gradually take place as the parts become accustomed to the impression. But if the constitution is for ever susceptible of the poison, we may suspect, that when a diseased action commences from such a cause, it will continue for ever, or at least till some greater stimulus supersedes it, by those laws which we have traced when considering that no two actions can exist at the same time in the same part and in the same constitution.

Thus we find the salivation in small-pox, and also the discharge from the eyes and urethra, when among the symptoms of that disease, cease as the pustules dry. We well know that as the constitution loses its susceptibility to that virus, all the actions it excites cease of themselves. We have also sufficient proofs that virulent gonorrhœa will cease spontaneously. But we have no means of ascertaining

whether these are venereal. As the constitution has no powers of altering the other actions excited by that virus, and as it for ever retains its susceptibility of it; we might be led to conclude, that when a diseased secretion is once set up from such a cause it would continue for ever, or till superseded by an action excited by a more powerful stimulus. I am aware this opinion is different from what is generally held, and from what has been sanctioned by very great authority. But all the arguments hitherto brought to prove the contrary, seem to me only to show that most gonorrhœæ will cease spontaneously, and that even the venereal gonorrhœa may be more readily cured than chancre. This by no means proves that the mode of cure must be always different; it shows, indeed, that an altered secretion being a more simple form of the disease than ulceration with callous edge and base, may be superseded by a stimulus less powerful and of shorter continuance; and as no loss of substance has been sustained, the parts will more readily return to their healthy actions.

When loss of substance has followed the irritation from a morbid poison, we shall find the curative process governed by laws, which, if not peculiar to these cases, are at least not to be traced with the same uniformity after any other diseased actions. I need not repeat that what was described in the former chapter, as the process of digestion in common sores, whether it be the separation of a dead part, or the recommencement of action in 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 new skin, the surface of the wound is contracted; 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

* Treatise on the Blood, &c.

must refer all those who wish for information in this most important branch of physiology. He has also remarked, that in all ulcers, or abscesses, a process takes place to prevent the effusion of matter, that is, a thin lamina of coagulating lymph, which lines the surface, and connecting every part of the cavity, is one species of his adhesive inflammation. My business is only to trace the deviation from these laws, when the disease arises from the stimulus of a morbid poison. In these, though at first sight there appear varieties; yet 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,* but after morbid poisons she seems prodigal 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.

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

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 provision of nature in the cure of ulcers from such causes. 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.

It may be urged that in the genitals, this peculiarity may arise 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, 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.

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

Such is the restorative process after the loss of substance from a morbid poison, when the actions are all rapid and uninterrupted. But it will hereafter be shown, that if the process of skinning commences before all the slough is cast off in small-pox, and in other morbid poisons, inducing slough, or before the callous edge and base are absorbed in the venereal, a fungus will rise up, which becomes the basis of future granulations. Thus, the probability of the restoration of lost parts by granulation in common ulceration, is in proportion to its rapidity; and after morbid poisons, in proportion to its slowness.

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 their secondary or constitutional ulcers. This is always the case in syphilis. It was the case in the sloughing phagedæna; in the case related by Mr. French, and may be easily traced in other morbid poisons of whose secondary 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 is infectious like that of the primary ones. Mr. Hunter has reduced this to experiment in the syphylis, and shown that the poison in the course of the circulation loses its contagious property. I have had two opportunities of confirming this experiment, but as nothing of the kind can be 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 and constitutional ulcers or pustules equally contagious. 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 is no longer infectious, we might expect ulceration would be proportionally 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 discolouration 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 horny 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 disease 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 virulent; 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 virulent pus. In some of these we see the disease a second time in the skin, 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, mentioned above,† was of this kind. In this disease were three stages. The first, on the part infected, which resisted the use of mercury, and might be said to heal spontaneously, and *without granulation*.

* Treatise on the Venereal, pages 389 and 393. † See page 32.

The second were blotches, soon becoming phagedænic ulcers, which, though not *prevented* by a full course of mercury unsuccessfully given to cure the first, when they appeared were very early relieved by that remedy.

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 relieve the second, and also the third.

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

But excepting in one poison, which we shall hereafter consider by itself, we have not a sufficient number of facts to ground any other reasoning upon, but 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 progress of a poison, 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

* 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 often relieved, but never cured, by the same remedy.

† It is not improbable that yaws is sometimes, if not always, inoculated either by immediate contact, or by the intervention of flies, who carry the contagious matter from a diseased subject to the broken skin of another. Hence we find the disease so much more common among Negroes, whose skin is naked and often broken, than among the Whites,

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 higher 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 influence of the poison being over, the pustule acquires the property of 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.

who are better protected, but often enough exposed to inhale the effluvia of the disease, could it be communicated by these means.

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 if 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, 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 kind of pustule, remarked by Sydenham of the regular small-pox, which appeared for six months of a period he traces with his usual exactness. "The pustules of the face are succeeded," he says, "by a scurf or branny scales, and those sometimes by pits or pock marks; for when the pustules fall off, the skin looks smooth; but these scales coming on and falling off alternately, do at length make those pits that frequently continue visible long after the recovery of the patient in this disease, though the distinct small-pox very rarely leaves any marks behind it."* This phænomenon, so accurately traced by Sydenham, arises from the bottom of the foveolus being skinned over, while its surface is crude, and while the fungus of the sides is covered only by the thin skin mentioned in page 56.† This interruption to the skinning over of the sides produces a perpetual attempt at accomplishing it, and hence arises the succession of branny scales (probably cuticular processes) which could not pass unnoticed by one who suffered nothing

* Swan's Sydenham, page 98.

† See also note in page 63.

to escape him. It very commonly occurs after inoculation, where the eruption 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 that disease.

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 the chancre, that 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 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.*—The

* 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

symptomatic fever therefore runs proportionably high, 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 should be confined to a smaller spot, we should have little to apprehend from the subsequent 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 disease 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 coming 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. I shall not undertake to determine the reason why, in the casual way, the disease appears first in the face and hands. But if, as is probable, it arises from re-action after the previous chill commencing in those parts, then we may conceive that every part of the surface of the fœtus being similarly circumstanced, the primary eruptions will extend over the whole body. Accordingly we find, in all the accounts we have of children born with the small-pox on them, that the matura-

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.

tion has been equally forward in every part of the body. Dr. Pearson tells me the state of maturation appeared similar in every part of the fœtus he examined. Such was the state of the 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 during 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

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

‡ 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?

inconsiderable, 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 yaws the inflammation is still less, and the suppuration still slower; hence the slough is separated so slowly, that the fungus forms the character of the disease.

Whenever a part which was the seat of a poison throws up this fungus, the influence of the poison has ceased, and consequently the attempt of separating the diseased from the sound part by ulceration; the parts are therefore only prevented from skinning by a slough acting as an extraneous body, or by the sores still retaining an unhealthy disposition independent of the poison. 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.

In the small-pox then, and probably in 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.

But in those morbid poisons, whose secondary local actions are different from their primary, the law appears to be skinning, without granulation in the primary, and with granulation in the secondary ulcers.

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

MERCURY.

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

In 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 being then 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 that 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 super-

seded 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 ulcer, whose callous edge and basis we see disappear as the mercury produces its new action, and often skinning instantly follow.

When funguses arise where a morbid poison has produced a loss of substance by ulceration, 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 from commencing. This being the only object, we need not excite that irritation which is necessary in the venereal poison, the influence of which 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, will sometimes prove more immediately efficacious. In this way it may be safe 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, 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. 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 exciting a new stimulus, 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 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, a method which, in cases of inveterate chancre, is known to be unequal to the object in view.

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 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 ulcer before it has arrived at such a height as to affect the sound substance; and the cuticle, not partaking of the disease, will immediately shoot over and line the now healthy surface. 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 always 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 base, 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 base, than where these are present.

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

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 cure an ulcer, arising from a morbid poison, which has no callous edge. In some of these cases we have every reason to believe, that the powers of the constitution are equal to the cure of the ulcer. All, therefore, that is required of mercury is to interrupt the action excited by the poison. But even supposing that the ulcer is the effect of a morbid poison, over which the constitution has of itself no power, still 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 the inveterate chancre it is not enough to excite a new action, the whole callous edge must be absorbed before we destroy the seat of the venereal action.

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 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 32). 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 secondary ulcers are unattended with slough, we may possibly succeed in 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

* Hunter's Essay, Page 385.

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 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 the insertion of the tooth; the second, or blotches, more than a month later, and became ulcerated painful sores, "increasing daily." But what is most to our present purpose is, how much more readily the secondary ulcers yielded to mercury than the primary ones. During the exhibition of one or two pills a day, containing two grains of calomel in each, and before fourteen of them were taken, which produced no other apparent mercurial irritation, than upon the bowels, "the ulceration of the mouth and cheek *did not spread*, was *less painful*, and of a *milder appearance;* while such of the blotches as had ulcerated, *healed apace*."†

The tooth-case related by Dr. Lettsom, in the Memoirs of the Medical Society,‡ was evidently the sloughing phagdæna. The primary sypmtoms are so accurately described, as to need no comment. They commenced about six weeks after the tooth was inserted. Dr. Lettsom saw them fourteen days after, when "the ulceration had an irregular, jagged, loose appearance, with livid sphacelated interstices." For the succeeding week, the ulceration continues nearly stationary, and no mention is made of sphacelus. The two following days the ulcerations "have not increased, but rather diminished.

* Medical Transactions, Vol. III. Page 325.

† 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.

‡ Vol. I. Page 330.

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 favourable 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 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 on the venereal poison, and to state those facts concerning the disease which have been admitted by every writer of reputation before his time.

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 may prove equal to a permanent cure.

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 been relieved by 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 ori-

* See page 67, and the first note in the same.

† 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

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

begun till the lips of the wound had commenced suppuration. This was always Mr. Crane's pactice, and though in consequence of that his patients were sometimes a very long incumbrance on the hospital; he never was puzzled with a phagedænic buboe.

ances, will fancy that the ulcer is beginning 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 something worse than before. This subject might be pursued much farther if it were necessary; but we have said enough to show the danger of the indiscriminate use of mercury in local diseases: and the subject is better understood than it formerly was.

There is only one situation of such an ulcer in which mercury can be serviceable. When the lips are pretty universally thick, when very little appearance of phagedæna can be traced, and the constitution has well recovered from the mercurial course: the local application of mercurial ointment, or the internal use of mercury, 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 be found constantly to exas-

perate the disease, or to increase the irritability of the constitution, it will be in vain to trust any longer to this remedy. This subject has been somewhat better understood, since the celebrated controversy concerning acids has taught us, that diseases exasperated by mercury will yield to new remedies.

The influence of mercury 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 expossed to view. 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 susceptible of the irritation of the poison, though contrary to its original law. 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 a 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. There is no doubt, however, that it will produce nodes on a sound bone. It is not uncommon to see nodes rise whilst a person is under the influence of mercury. Mr. Cline furnished me with one instance of this kind which occurred whilst a chancre was healing. I have seen two other cases, though less striking, which convinced me of the fact. I have no doubt that they often occur: and are considered, as they were in the instances which came under my own knowledge, as the effects of a peculiarly virulent syphilis.

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 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, when 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 restorative powers which often follow convalescence from acute diseases. These remarks are certainly foreign to our present purpose. I have, however, introduced them because of the error some few practitioners have fallen into, of considering many diseases venereal, merely from their giving way to mercury.

Such is the operation of mercury on the constitution as a remedy. The constitutional diseases it produces are not less remarkable. Besides its well-known determination to the salival glands, which is neither constant nor always necessary, we find it producing head-ach, debility, and a total incapacity for application of any kind. If the mercurial irritation has been regularly excited, these symptoms gradually subside, after the cause is discontinued. But should the disease for which it was excited be such as will not yield to mercury, or from its improper exhibition should the constitution be perpetually harrassed by it, the consequences are often much more serious. Local diseases spread in the manner already described, and a train of constitutional symptoms occur, which, for want of a better name, we call nervous, and which too often end in epilepsy, gutta serena, mania,† or fatuity. From these

* Medical Transactions, Vol. II. Page 325.

† Since the former edition was published, it is with much pleasure I find Mr. Haslam has enumerated this among the causes of madness.

considerations it can surely require no arguments to prove that mercury is a remedy not to be given without a precise object in view, nor without an accurate attention to its effect on the constitution, and on the disease during its exhibition.

In anomalous morbid poisons, we have shown the difficulties with which it is attended. When we come to consider those which are better known, and found invariably to yield to mercury, we shall be better able to establish a certain mode of practice, which will at least assist us in conducting our process where we have fewer facts to rest upon. I shall therefore, at present, conclude this subject, by an illustration of the effects of mercury with those which follow the frequent use of spirituous liquors.

The effects of each are stimulating, and each, when carried to excess, produces feverish symptoms, which gradually subside, and leave the constitution, after a time, little, if at all, the worse for it. But the frequent repetition of these stimuli produces each its specific effect, which at length becomes permanent.

If either mercury or ardent spirits be given in such a manner as perpetually to excite powers of action different from what ordinarily exist, yet without that fever which follows a larger exhibition, the effects are slower, though not less certain, nor less deleterious. For some time after beginning the use of each, a larger portion is required to produce the same effect, and even after the constitution has become injured by the use of either, it seems to require, or to be relieved by, the same temporary stimulus. But ultimately the irritability of the frame is so great, that the exhibition of the smallest quantity is sufficient to produce the highest effect. This state is the forerunner of death, or some incurable local disease.

CHAPTER VII.

OF THE VENEREAL DISEASE.

HAVING thus far pursued our enquiries into the laws common to all morbid poisons, we are better prepared to attend to the particular phænomena of each. Among these the venereal comes first in order, because from the frequency with which it occurs, and its slow progress, we have an opportunity of marking every appearance with peculiar accuracy.

I shall leave to others the task of enquiring into the origin of this morbid poison: but it seems strange that any man, who takes the trouble to reason, or who has the courage to address the public, should venture on so bold an assertion, as that the disease was known in the flourishing states of Greece or Rome. The ancient physicians, being ignorant of the medical powers of mercury, must have been infinitely more familiar with every form of the disease than ourselves. Yet till near the close of the fifteenth century, we have no description of local appearances that can be mistaken for venereal, and during the following century, the industrious Astruc enumerates more than one hundred writers on the subject. If other proofs are required, let us mark the difference between the licentious poets of those days and our own. Can a reader of common sense suppose, that Horace, Juvenal, Perseus, or Ovid could have been silent on a subject so perpetually occurring in the satirical writings of Pope and Swift. The former can scarcely lose sight of it even in his Imitations of Horace, though conscious that he is not authorized to introduce it. The question is certainly important, and those who attempt to

prove what is disbelieved by the most industrious enquirers, should not expect their readers will be satisfied with a bare assertion. If I had met with any thing more that had not been satisfactorily answered, I should think it right to enquire into the validity of such proofs.

Another question, taken up at different times, is, Whether the matter of chancre and gonorrhœa is the same; or rather, whether each possesses similar properties of contamination? It is difficult to set about answering the objections made to the positive side of this question, when Mr. Hunter (who had no theory to support on the subject, and who, we shall presently see, is ready to admit, on insufficient grounds, one strong argument against the opinion he holds) assures us, that he has seen all the symptoms of lues venerea originating from gonorrhœa only—that he had even produced venereal chancres by inoculating the matter of gonorrhœa—and that he afterwards repeated these experiments in a manner in which he could not be deceived.*

It is evident, however, that if the only difference consists in the properties of the surface to which the stimulus is applied, we ought to expect the same consequences in other morbid poisons, according to the nature of the surface infected by them. I shall presently have occasion to show a most striking analogy in another morbid poison, which resembles the venereal in all its laws, though not in the character of its local appearance. This property is not, however, confined to morbid poisons of this description. It has been before remarked, that in small-pox we have pustules on the skin, and increased secretion in the fauces: and that both these cease as the constitution loses its susceptibility of the poison. To ascertain whether the contaminating property of the secretion was the same, the following experiments were made by Mr. Wachsel in the Small-pox Hospital.

* Hunter on the Venereal Disease, page 293, & seq.

REGISTER, 3d.

November 30, 1805, J. D. was inoculated, in the right arm, with saliva from a patient under confluent small-pox. On the same day he was inoculated, in the left arm, with matter taken from inoculated pustules.

On the 4th day, the puncture in the right arm was slightly red, the left arm more elevated.

On the 6th, vesicles on each.

8th, vesicles spreading—head-ach the preceding night.

10th, vesicles much enlarged in each—pain in axilla.

11th, lumbago—vesicles circumscribed.

12th, efflorescence in each arm—pain in axilla continues.

14th, efflorescence continues.

16th, the arms drying—has three secondary pustules.

19th, scabbed.

24th, discharged well.

December 5, five persons, of different ages, were inoculated in the right arm with saliva, and in the left arm with small-pox matter. Of these,

In one both inoculations succeeded.

In one only the inoculation from the saliva succeeded.

In three only the inoculation from the matter succeeded.

These experiments are sufficient to show the virulent property of the secretion from the fauces. Whether gonorrhœa, when it occurs under small-pox, would produce similar effects, it would neither be proper, necessary, or satisfactory to try. There might be some doubt whether the discharge had existed before the invasion of small-pox. The experiment could not prove more than we learn

from inoculating the saliva, nor can we with certainty ascertain that there are no pustules concealed, as phymosis is a constant attendant on variolous gonorrhœa.

I am aware it has been said, that the experiment made by Mr. Hunter, has since been repeated with a different result. Can we wonder at this, when we consider from how many causes gonorrhœa may arise, and how impossible it is to distinguish the venereal from any other.

Another objection started by those who dispute the identity of gonorrhœal and chancrous virus is, that the two diseases may be cured differently. This, as well as the question concerning chancres in the urethra, will be considered before we come to the treatment of gonorrhœa.

The last argument I shall notice is, that gonorrhœa was not described as a symptom till nearly half a century after the other symptoms of the venereal disease were known. This argument, if it proves any thing, makes entirely for the side we have supported. To answer it, however, obliges me, contrary to my intention, to take a short view of the opinions of contemporary writers on the first introduction of this morbid poison.

At this time the importance of the remedy being scarcely known, the primary symptoms must have remained uncured whilst the secondary were breaking out. Such a concurrence would be sufficient to teach the practitioner that both arose from the same cause. The recollection of symptoms also, which were known before the existence of this disease, must have been fresh in the memory of most, and though some, as in later times, might be disposed to impute every thing strange and many things common to the new disease, yet the facts, to which their opponents appealed, must have been sufficient to check that indolence or boldness which was afterwards almost uncontrouled.

At length the success of mercury, in the hands of men unable to judge and unaccustomed to reason, produced an universal confusion in prognostic, diagnostic, and method of cure. It was not, therefore, without some appearance of reason that the regular practitioners disclaimed against a mineral which had been considered as poisonous by Discorides, Galen, and Theophrastus. Yet still the wiser part of the profession could not be ignorant of cures performed by mercury which had resisted all other remedies. Joannes de Vigo therefore adopts the practice, but with an ingratitude not at all uncommon, after dating the disease at the close of the fifteenth century, he derives his practice from Theodoric and Arnoldus, who wrote about the close of the thirteenth. The arguments and authorities of John, would probably have been as little attended to as those of his teachers, the barbers, had it not been found that his practice procured him riches and honors.

The remedy having now acquired the sanction of regular practitioners, all that mass of error, unfounded reasoning, and uncertainty, soon followed, which has not hitherto been swept away. The quacks grew bolder and the regular practitioners more confounded. The first symptoms being cured before the secondary had appeared, their recurrence afterwards was imputed to the imperfect cure of the first, without any inference being drawn from the genitals remaining sound. If, after the cure of secondary symptoms, the bones were affected, no one seemed to express any astonishment that still the skin and genitals remained sound; and as these events happened at uncertain periods, from the uncertain exhibition of the remedy, the disease was now supposed to be constrained by no laws, and the most fortunate guesser acquired the reputation of the ablest physician. It was soon forgotten not only that other ulcers would attack the genitals, but also that mercury would cure other cutaneous diseases. Whatever appeared on the former or yielded to the

latter confirmed the wishes of the suspicious, the guesses of the ignorant and impudent, and added to the wealth of the unprincipled.

In this state of things we find Hutton, a soldier and a well educated gentleman, publishing his own case, by which it appears that after nine years illness, and eleven salivations, he was cured by guaiacum. Strange, that to so enlightened a character, it should not occur, that so powerful a remedy as mercury might have been a sufficient cause of all his sufferings, and that the dereliction of it might have proved his cure.

The disease now became more uncertain, in proportion as few thought of distinguishing it from the effects produced by the remedies, and still fewer of doubting that whatever yielded to mercury must be venereal. But as some symptoms in the same person were exasperated by mercury, the comparative merits of the mineral and guaiacum became a subject of early controversy.

Fracastorius, who wrote nearly fifty years after the first appearance of the disease, takes some pains to enquire into the descriptions of the writers about the fifteenth century. Among these he finds the node, *Callus qui interdum exulcerabatur interdum integer usque ad mortem perseverabat.* Had he proceeded a few years lower he might have quoted from John de Vigo — *exostosis quemadmodum in spinæ ventositate accedit;* and from P. Maynardus warts and excrescencies of all kinds on the genitals.

Fracastorius fancied the disease changed. The truth is, the primary symptoms being earlier attended to, secondary symptoms were less frequent and milder. But though pains in the bones were less frequent they were more severe, perhaps because many of them arose from mercury, and were afterwards exasperated by the repetition of the same remedy. The bubo was now marked by other writers, and probably it was soon forgotten that Celsus had described inguinal tumours, when suppurated, as difficult to be healed. Fracastorius lived long enough to add universal baldness and loss of the

teeth to the catalogue of symptoms. But Fracastorius was a poet, a most dangerous accomplishment for a physician, and even contemporary writers were disposed to impute some of these symptoms to the remedy, which at last *he tells us*, every one was convinced arose from the disease.

After this Fallopius adds, *tinnitus aurium*, which he boasts of as his own discovery. It is true he admits that this tingling in the ears arose from other causes, but adds *Sed etiam fit a Gallico.* It is hardly probable that so common a complaint should never occur in those who had shown venereal symptoms, and still more in those who had been debilitated by the frequent exhibition of mercury. It is certain that this, and other chronic complaints of the head, are often cured by mercury where no venereal cause can be suspected.

This author is, however, among the first who observed gonorrhœa as a symptom of this new disease, if it could be called such, half a century after its appearance. Lastly, Christallines are discovered as a new symptom. These, it is evident, can only be considered as an epidemic erysipelas, prevalent at a particular season, and though the same may have occasionally occurred since, especially in fleets and armies, they have not been thought of sufficient consequence to be noticed, or have been cured by the common remedies.

It is astonishing that the industrious Astruc, who so ably maintained his opinion against later writers concerning the antiquity of the disease, should so tamely follow men of remoter date in their accounts of these successive changes. It is not less curious to see the illustration with which he introduces these *periods of the disease*, as he calls them, comparing them with the revolutions of cities and empires; as if the laws of nature were as fluctuating as the institutions of man.

Fortunately many of these symptoms, not being very common, have fallen into oblivion; excepting when systematic writers have wished to swell the enumeration, and to lead their readers to believe

that, by such enumeration, they evince their own accuracy. Some of them seem, indeed, to have no other object but to interest, surprize, or keep us awake. I should not, therefore, have considered this historical enquiry necessary, had it not been introduced in support of the opinion before mentioned. If, however, venereal gonorrhœa was unnoticed till fifty years after the other forms of the disease were described, what does this prove but that contagious gonorrhœa was so common, as to be disregarded as a symptom of the new complaint? Can there be a doubt from the caution given by Moses that gonorrhœa was considered as contagious in his days? During the classical age, we find, inconveniencies of the urinary passages were imputed to incontinence,* and the police of several states, before the siege of Naples, made laws for preserving the health of such as would content themselves with public stews, instead of disturbing the peace of families.

This is enough to lessen our surprise that gonorrhœa should be unnoticed for some time after the appearance of the venereal disease. But so far is it from proving the two contagions are different, that the fairest inference we can draw is in favour of their identity. For if by this time the disease began to be so far understood, that secondary symptoms were found the consequence of primary ones in the genitals, it is most probable that the first suspicion of venereal gonorrhœa arose from the occurrence of such secondary appearances where no other primary symptoms could be traced. I mean not to produce this in support of an argument which rests on the basis of experiment, but as an answer to what has been urged on the contrary side.

* If the following passage is intended for any thing more than a pun, it very much strengthens Mr. Hume's opinion concerning the frequent occurrence of stricture in the East Indies. Dicat [Epicurus] δυσουρικα και δυσεντερικα παθη sibi molesta esse quorum alterum morbum edacitatis alterum etitam turpioris intemperantiæ.—Ciceronis Epistolæ ad familiares. Lib. 7. Epist. 26.

Another opinion, which at one time divided the medical world, is, Whether gonorrhœa is the effect of ulcers in the urethra? Time and better information has much changed the nature of this controversy. It was once supposed that pus could not exist without a breach of the solids, and that it might be always distinguished from mucus. This opinion, which is at least as old as Hippocrates, stood its ground almost till our own days, when it was discovered that a substance, having all the properties of pus, might be secreted without any apparent ulceration. Mr. Hunter proved that gonorrhœa might exist without any breach of the solids. In consequence of this, the name of the discharge was altered, and what was before called pus without any dispute, now became mucus. One author thought it right to alter the name of the disease in consequence of this supposed alteration. Happily for the credit of our art, no subsequent writer has chosen to adopt his nomenclature, however they might be disposed to admit his theory. This gentleman likewise assures us, that whenever secondary symptoms occur from *blenorrhagia*, they are always the consequence of a previous ulcer, because matter cannot be absorbed from the surface of the urethra, but may from an ulcer.

I should not have thought the enquiry worth notice, had it not been lately introduced by a writer, whose practical remarks are certainly entitled to attention. Mr. Whateley has, in some measure, interwoven his opinion concerning ulcers in the urethra with his method of treatment. That gentleman finds, indeed, sufficient authorities to show, that ulcers, or the supposed effects of them, have been discovered sometimes in dissections of the urethra. But the former are descriptions of chronic indolent ulcers, very different from the true venereal, and the latter more properly belong to stricture than any other cause. We are, however, obliged to this gentleman for several authorities, showing the advantages of administering mercury in some species of gonorrhœa. It is right to

add, that in most of these cases the internal use of mercurial salts was found sufficient, though it is now generally admitted by the best practitioners, that in curing chancre, a different plan is much more advisable; and it is worthy of remark too, that in most of the cures enumerated by Mr. Whately, it was admitted, that, though the more violent symptoms of gonorrhœa yielded to the mercurial course, still the discharge continued, in some instances, in the form of gleet, and in others of well formed pus.

But nothing of this kind proves that gonorrhœa is ever the effect of chancres in the urethra. No one will dispute that they are seen at the orifice, the only part of the canal with which the virus comes in contact. Here, therefore, we should expect them, and here we see the ravages they commit. But have we the same evidence of chancres higher up, or do the same causes operate to produce them? We know that every symptom of gonorrhœa may exist without an ulcer, and that the property of chancres is to increase in all directions till the exhibition of mercury. As, therefore, we see their effects at the mouth of the urethra, and as the exhibition of mercury, in the manner it is given for chancres, has been long discontinued in gonorrhœa, it follows, that if such ulcers ever exist in the inferior part of the internal surface of that membrane, the consequence must be a perforation through the external skin, which, according to the laws of morbid poisons, would heal without granulation, and leave a perpetual memorial of the event. If, therefore, chancres were known, however rarely, to exist in the urethra beyond our sight, instead of any controversy on the subject, surgeons would be constantly apprehensive of such an event, and never fail, particularly if they met with any suspicious feel along the external surface, to administer mercury with a rapidity proportionate to their apprehensions: but none of the writers who maintain this opinion, ever express such an apprehension, or mention other ill

consequences of delaying the use of mercury, excepting the continuance of the painful symptoms.

That these painful symptoms may be relieved by mercury, may be admitted, without any suspicions that they arise from chancres. They arise from a virulent substance stimulating a part, which continues to secrete the same substance. As therefore was shown, when we explained the general principles of morbid poisons, the same effects will be produced, till the parts become so far accustomed to this substance, as to be less sensible to its stimulus, or till that action is altered by which the substance is secreted.

Mr. Whately, in his Remarks on the Treatment of Gonorrhœa Virulenta, divides the disease into three different species. The first is attended with ulcer in the urethra, or a considerable induration at the lips of its orifice, &c.

The second without any appearance of ulcer. Afterwards he observes—" In all gonorrhœas of the second species, we have reason to conclude, that the poison has penetrated and excoriated the inner membrane of the urethra for *some depth*, though it may not have produced actual ulcers in it." It is far from my wish to undervalue Mr. Whately's accuracy: his practical remarks I esteem very highly; but it is extremely difficult to conceive what can be meant by *excoriating to some depth*, and particularly the *inner membrane* of the urethra. If we admit this expression to Mr. Whately's first species of gonorrhœa [by ulceration] it will require, as was before observed, very little depth to penetrate even through the external skin, in which case the existence of ulcer can be no longer doubtful.

Though all Mr. Whately's arguments appear to me insufficient to prove that chancres are ever seated high up in the urethra, yet we have seen no reason to question the accuracy with which he has collected his facts. I trust, however, we have shown, that those facts are insufficient to admit the inferences he has drawn from

them. One only remark I have reserved for this place, because it is connected with the manner of cure, and also with the question concerning the identity of chancrous and gonorrhœal virus. It is asked by the advocates of the contrary opinion, If the two diseases arise from the same cause, why are they not cured by the same means? At the same time Mr. Whately produces several authorities to show that mercury has been found useful, if not necessary, in the cure of many gonorrhœas. In answer to both I shall only repeat a remark made in the former edition of this work, namely, that Mr. Hunter seemed to have conceded something to general opinion in admitting that mercury has no effect on gonorrhœa: that it does not always stop the discharge was allowed; but as it has the power of inducing an alteration in other secretions, it was urged that we were not authorized to say, that after a salivation the discharge, if it continues, is still virulent.

I shall now remark further, 1st, That we have, for the most part, no means for ascertaining a venereal from any other gonorrhœa.

2dly, That in the true venereal gonorrhœa we have the same difficulty, as long as the discharge continues, in ascertaining whether it retains its virulence or not.

3dly, That though many gonorrhœas are cured without mercury, yet we have no proof that such were venereal, or if they were, that they were not cured in a manner similar to chancres.

1st, The only means we have of ascertaining whether a gonorrhœa is venereal or not, is by the appearance of secondary symptoms, which we can impute to no other cause; or by the matter producing chancres in another person. Both these facts have confirmed the identity of the poison, but we have not sufficient proof that the gonorrhœas, which produced these effects, were not cured by such means as we might expect from the fair analogy between the

two forms of the disease; as we shall see when we come to consider the third proposition.

2dly, Though it is notorious that many gleets are not virulent, yet it is equally certain, and Mr. Hunter has proved, that some of them are, and also that the discharge may continue virulent without any marks of inflammation, and even without any suspicion of, or inconvenience to, the patient, for a length of time to which we can fix no limits.

Before we enter on the third proposition, I shall offer a few extracts from Mr. Hunter, to which I must otherwise refer the reader.

"From the idea," says that gentleman, "which I have endeavoured to give of the venereal disease, namely, that in whatever form it appears, it always arises from the same cause, it might be supposed that, since we have a specific for some forms of the disease, this specific would be a certain cure for every one, and therefore, that it must be no difficult task to cure the disease, when in the form of inflammation and suppuration upon the secreting surfaces of any of the ducts or outlets of the body; but from experience we find gonorrhœa the most variable in its symptoms while under a cure, and the most uncertain with respect to its cure of any of the forms of this disease: many cases terminating in a week, while others continue for months under the same treatment."*

Nothing can be more just than every part of the above paragraph; and the following are inferences we may draw from it, namely, that gonorrhœa being the most simple form of the disease, we might expect it to be cured by the same means that cure every other form, and that it would be unnecessary to use those means to the same extent. But we find gonorrhœas, which will cease of themselves, or may be cured by remedies which have no effect on the

* Treatise, page 69.

other forms of the venereal disease. It is therefore at least doubtful, whether such gonorrhœas are venereal, especially as we know that an increased and altered secretion from the urethra will happen from a variety of causes.

" I have observed," continues Mr. Hunter, " that this form of the disease is not capable of being continued beyond a certain time in any constitution."* That the inflammation will not continue is what we might expect, as the parts become accustomed to the action, but as we proceed in tracing Mr. Hunter's remarks, we shall find this is no proof that the action of secreting virulent matter has ceased. " The distinction," says he, " between a gonorrhœa and a gleet is not yet ascertained, for the inflammation subsiding, the pain going off, and the matter altering, are no proofs that the poison is destroyed. It is no more necessary that there should be a continuance of inflammation to produce a specific poison, than that there should be a continuance of inflammation to produce a gleet."† For the proof of this, he refers us to two cases, which show that inflammation is not necessary to the existence of the venereal poison; and that inflammation may exist after the matter has ceased to be venereal. " I have known," continues he, " cases where the inflammation and discharge have continued for twelve months, and with considerable violence. In the mean time a free intercourse with women has not communicated the disease. However, this is not an absolute proof that there was no virus in the discharge."‡

If then, there is this great uncertainty in the nature of gonorrhœas, if there are some which are not virulent, and others, which, though virulent, are not venereal, our only enquiry must be, whether there are any which require a similar mode of treatment with chancre, though in a slighter degree.

In the preliminary remarks, we have seen that the action from morbid poisons either subsides of itself, or, if the constitution has

* Ibidem. † Page 94. ‡ Page 95.

no power over the poison, that the action is continued till superseded by a more powerful stimulus. Now, we have already seen that mercury has often been serviceable in the cure of gonorrhœa; and if in less quantity than is necessary to cure a chancre, this is perfectly consistent with the different forms of the disease; all that is required being to alter the action in one instance, whilst in the other it is necessary to excite such an irritation, and to keep it up so long as to produce an absorption of the callous edge and base of an ulcer. For the same reason a mere local irritation may be sufficient to cure the first, which would be inadequate to the last.

"Irritating injections," says Mr. Hunter, "of whatever kind, act upon the same principle, that is, by producing an irritation of another kind, which ought to be greater than the venereal, by which means the venereal is destroyed and lost, and the disease cured, although the pain and discharge may still be kept up by the injection."* I am aware it may be said, that gleets, which there is great probability originate in venereal gonorrhœa, will cure themselves, and also that Mr. Hunter mentions a case in which gonorrhœa was cured without mercury, yet secondary symptoms followed. But we should recollect, that so simple an action as mere increased and altered secretion, may be superseded by some constitutional irritation, the importance of which the patient would not be aware of, and therefore not think of mentioning. Of this Mr. Hunter is well aware. In treating of gleets, he observes—"This disease, [when become habitual] has not, however, always the disposition to continue, for it often appears to stop of itself.——It is most probable," he adds, "that this arises from some accidental change in the constitution, not at all depending on the disease itself."†

In his chapter on the mode of curing gleets, he mentions several instances in which it had been effected by different irritations to the part, to the constitution, and to a different part by sympathy.

* Page 77. † Page 100.

This mode of curing, by very slight means, the action from a morbid poison, the impression of which is grown familiar to the part, is perfectly consistent with our previous remarks,* and confirms our third proposition, "That, though many gonorrhœas are cured without mercury, yet we have no proof that such were venereal; or if they were, that they were not cured in a manner *similar* to chancres, that is, by a stimulus more powerful than the poison."

On the whole, then, we are entitled to conclude, that, though there is nothing absolutely inconsistent with the laws of this morbid poison in admitting, with Mr. Hunter, that gonorrhœa may cease spontaneously, as the same parts become accustomed to the impression, yet that we have not sufficient facts to prove it; on the contrary, that it is at least undetermined whether the action of venereal gonorrhœa, when once begun, will not, like that of chancre, continue till superseded by a more violent stimulus: and if a slighter irritation should be sufficient to supersede gonorrhœa than chancre, this is only what we should expect from the different effect of the poison on different parts.

I have dwelt thus much on the laws of this form of the disease, because the discovery of such laws in all diseases appears to me the great business of pathology. To describe with accuracy is the first object. But the symptoms of one disease will be perpetually mistaken for another, or the young practitioner will be alarmed with novel appearances, or often too decided in his prognostic, until he learns to trace the disease and remedies by certain laws. Lest, however, I should be accused of unnecessary prolixity, let me relate a case, which, as it is not in some particulars solitary with me, has, I doubt not, occurred to others.

A young gentleman, who had been very much what is called upon the town, consulted me on account of his wife's throat.

* Turn to Page 76 of this work.

Upon examination, the appearances were so very striking, that I could not help suspecting a venereal origin: but in a private conversation with the gentleman, he assured me he had been married three years, since which time he never had any venereal symptoms. That when he first became a husband, he had a trifling colourless gleet, which he was assured was not virulent, and which left him soon after. His wife also asserted, that she never had seen any other symptoms but in her throat. Under these circumstances I was disposed to leave the case to explain itself further. The throat, however, grew worse, its character became more confirmed, and the voice showed that the bones of the nose were in danger. My enquiries now became more urgent and solemn; both parties appeared anxious to give the most minute information, and even to court every enquiry, whether examined alone or together. I now learned that the lady, being about sixteen when she married, had suffered considerably *primo congressu*, and from that time had been subject to a discharge *per vaginam*. The only inference I could draw was, that the gentleman's gleet had continued virulent, and was become so habitual, that the inflammation excited by another cause had superseded the former action; that the lady was infected with virulent gonorrhœa, and continued so from that time; that the gentleman, from long habit, was not susceptible of, though perpetually exposed to, the virus; and, lastly, that some accidental circumstance, at a period which could not be ascertained, had produced a susceptibility in the lady's throat to be infected by the matter absorbed from the vagina.—The remedy was instantly applied by friction, and the success was as regular and permanent as in any undisputed case.

It is very probable that an event of this kind has given rise to a vulgar error and a brutal crime. In the inflammatory stage of gonorrhœa all the painful symptoms must be dreadfully exasperated by such an attempt, and in the chronic stage this, of all others,

must be the most uncertain means of exciting inflammation. But it is not for *brutes* to reason. The importance of the case arises from the effect it may have in cautioning *men* to avoid connections as long as a discharge of any kind remains, and in affording one other explanation of a question, sometimes involving the honour of the sex, or the tranquillity of the *irrupta copula*.

CHAPTER VIII.

ON THE TREATMENT OF GONORRHŒA.

THE cure of gonorrhœa has been much anticipated in the former chapter, the nature of the disease being necessarily illustrated by the manner in which the symptoms gradually subside, or yield to remedies. When this is well understood, it will not be difficult for a practitioner to guide himself according to the nature of the symptoms, and his previous knowledge of the patient's constitution. The general indication is to abate inflammation where it is considerable, and as early as possible to apply such means as may alter the action of the parts. The first must be accomplished in the manner all other inflammations are subdued.

The specific action of the part, which we have seen may continue after all symptoms of inflammation have ceased, must be altered by exciting some new action. If the discharge gradually ceases as the inflammation subsides, we need give ourselves or patient but little trouble. This is so often the case, that many injections are calculated for no other purpose than to produce a sedative effect. But many of those remedies, which are called sedatives, are among the catalogue of astringents. Of this class are most of the metallic salts; and these, when used in large proportions, have the property of stimulants, as is proved by the smarting they occasion. Most practitioners have their favourite injections, which is enough to convince us, that most gonorrhœas are of a description that will cure themselves, or be easily cured. However, the plan of exhibiting stimulating injections has been gradually

gaining ground since its first introduction, and what is a most important consideration, is found least likely to be followed with gleet. No practice, indeed, seems more rational, in a disease which is nearly, if not altogether superficial, than to attempt the cure by exciting a different action on the part, and keeping it up, for a certain time, by superficial applications. "Irritating injections, of whatever kind," says Mr. Hunter, "I suspect, in this disease, act upon the same principle, that is, by producing an irritation of another kind, which ought to be greater than the venereal, by which means the venereal is destroyed and lost, and the disease is cured, although the pain and discharge may still be kept up by the injection. Those effects, however, will soon go off, when the injection is laid aside, because they arise only from its irritating qualities. In this way bougies, as well as many injections, may be supposed to perform a cure; and although they increase the symptoms for a time, they can never increase the disease itself, any more than the same injection which produces the same symptoms, if applied to the urethra of a sound man, can communicate the disease."

After this, Mr. Hunter gives a number of cautions concerning cases in which such injections should not be used. They chiefly amount to this, that where the inflammation is already high, it may be dangerous to raise it still higher, or where the neighbouring parts sympathize, as we cannot ascertain the extent of such sympathy, it may be dangerous to excite any new irritation. These observations certainly are very just, and if Mr. Hunter has afterwards carried his cautions further than necessary, we should recollect, that, since he wrote, the practice has been more matured, and in some measure reduced to rule. Doubtless there are constitutions or urethræ so irritable, that it may be impossible to use a solution strong enough to answer our purpose. In these cases we must vary our remedy according to circumstances so numerous and so uncertain, that unless the practitioner has courage and science enough to

judge for himself, it will be endless to propose plans suited to every individual case. But for general practice there cannot be a more convenient injection than the different proportions of this solution as recommended by Mr. Whately.

It is most likely that the internal use of mercury, though given only to preserve the constitution from the danger of contamination, may at the same time be efficacious for the local disease. If we may believe Turner, the quacks of his day cured gonorrhœa by the exhibition of crude mercury, triturated into pills, and probably the same forms are continued to this time. In this manner we shall presently see a disease, in many respects analogous, is always cured. At present we are hardly competent to judge what effect mercurial courses of any kind would have on gonorrhœa, as the practice is so rarely tried without the use of injections.

It can scarcely be necessary to repeat any thing from the former edition of this work, on the danger of *drying up the discharge—forcing the poison into the blood*—producing a *retrocedent or retropulsed gonorrhœa—shifting the influence of the poison*, and a number of other expressions, which have been used without any meaning. As long as the discharge continues, and retains its virulence, there is certainly danger lest it should be absorbed, and produce disease in other parts of the constitution. Nor are we yet sufficiently acquainted with the properties of the poison, to ascertain when the disease is virulent, and when it is merely an increased secretion. But as soon as the discharge ceases, the danger of absorption and consequent contamination must cease with it.

Though the general symptoms of the disease, with their treatment, may, for the most part, be reduced to this simplicity, yet a few remarks on some which are less constant may not be useless. The most troublesome of all the more frequent attendants is chordee. This seems to arise from a thickening of the urethra through inflammation, in consequence of which it becomes less dilatable when

the penis is erect. The only remedy found useful for this, besides the common means of subduing inflammation, is opium. If the pain in erection arises from the urethra only, it will more readily subside; but if the whole of the lower part of the corpus spongiosum is distended with lymph, and adhesions have taken place between the cells, the process will be more slow.

Whenever the prepuce is much distended, phymosis follows, which must be treated like the other symptoms of inflammation. Care should be taken to keep the parts elevated as much as possible in a line with the abdomen, that no extravasted fluid may further distend the prepuce. If the case is at all obstinate, but most of all if we have not had an opportunity of seeing the parts beneath, it will be advisable to exhibit mercury as if we had a chancre to cure, which is most commonly the case. At the same time the parts should be diligently washed by means of a syringe. The solution of hydrargyrus muriatus in lime water, is found most efficacious.

The testicles should always be suspended. If they swell, rest and an horizontal position are absolutely necessary. Should the gonorrhœa cease at the same time, this is a matter of no importance, and is only to be considered as one of the modes of cure by sympathy, though by no means the most desirable. In this, as in all other cases where the running stops suddenly, from the accession of any other local or constitutional complaint, it is uncertain whether it will return as the new complaint ceases. Mr. Hunter says, he has seen it cease altogether—he has seen it return—and he has seen it exasperated by the new complaint, after which both have gone off together.—I have thought it has depended much on the length of time that the gonorrhœa has existed. If long enough to render the disease chronic, that it has usually ceased as the testicle swelled, without returning as it subsided; but if in the earlier stage, that the running, though lessened, does not entirely cease, and returns as the testicle recovers.

When buboes occur from gonorrhœa, they must be treated according to their character, or the constitution of the patient. But as we cannot ascertain whether the gland is affected by mere sympathy, (as is often the case in other local complaints) or by the absorption of the venereal poison, it is always advisable to rub mercurial ointment into the thigh below. When the complaint proves venereal, it must be treated in the same manner as will be proposed for those buboes which are the effect of chancre.

CHAPTER IX.

OF CHANCRES.

WE are now to consider the consequence of the irritation produced by the venereal poison, when lodged on a part more disposed to ulceration, than to mere increased secretion. That such is the case with the glans penis, cannot be doubted, when we consider the frequency of ulceration on that part, and even that a mere increased secretion is sufficient in many instances to produce such an effect. The prepuce is not a secreting surface much beyond the corona. If, therefore, matter lodges long enough on its surface to produce irritation, or if, during the actus coitûs, any abrasion of the cuticle should take place, ulceration is the only consequence which can follow.

The part most liable to such an effect is the *frenum* and its attachment, not only because, as Mr. Hunter observes, matter is more easily lodged on these parts, but because excoriation, or even rupture, is a common consequence of the means by which the disease is caught. Next to the frenum and its attachments, the parts most liable to chancres are the glans and corona; next to these, the part of the prepuce which comes in contact with the glans when in its relaxed state; and next, the exterior part of the prepuce. Chancres occur also on the scrotum itself, but more rarely. I have once met with them on the skin of the pubes.

In whatever part they occur, their first appearance is often uncertain, depending on the constitution, or the state of it at the time the irritation commences. It is seldom we see the first vesicle

which is formed: more commonly, especially if the disease is on the glans, this vesicle is broken, and discovers an excoriation, and oftentimes a slough. In either of these states, it is not easy to ascertain the real character of the disease, and all prudent, perhaps I might say, all honest men, suspend their opinion and their treatment beyond what may be necessary for the immediate symptoms, till the part shows a disposition to heal, or discovers its true character.

Nothing can be more certain than the true character of a venereal chancre, and nothing cured with more certainty. The difficulties and intricacies attending this form of the disease, have arisen from indolence, ignorance, or artifice. Those who have not industry to attend to the discrimination of ulcers on these parts, or who have not had opportunities of distinguishing them, may be mistaken: but unfortunately, there is a class of men too often applied to in these cases, whose only object is the advantage they can make of their patient, and who indiscriminately condemn whatever ulcers are found in these parts. This is the more disgraceful, not only because in the early stage it is often impossible to ascertain what such ulcers may be, but because the use of mercury will exasperate some, and induce a degree of uncertainty on all whose character has not shown itself before the use of the remedy.

The time after the application of virulent matter, till the appearance of chancre, is somewhat uncertain. The common period is from a week to fourteen days: perhaps ten days may serve as a general medium. But before the actual appearance of even a vesicle, the patient, if he is in that situation of life which gives him leisure to attend to every little inconvenience, is sensible of occasional pains, which become more frequent as the period arrives. If the disease, as is most frequently the case, is situated at the bottom of the frenum, there is always some uncertainty in its first appearance, because there has usually been a rupture of the frenum,

which, of itself, would produce a ragged, and sometimes a sloughy sore. In this case, we can only judge at first by the nature of the pain, which in chancre every intelligent patient describes as a *hard* kind of pain. This is different from common soreness; the sensation of something, which will not yield or accommodate itself, being superadded. In a very short time, however, a regular coarse granulated surface shows itself, of different shades, from white to a greyish ash, or dusky brown colour. By candle light it appears whiter, especially if covered with pus. In a little time an excavation discovers itself, which is regularly deeper from the sides to the centre. There is a peculiar hardness arising from a general thickening under the whole surface. This is more remarkable at the edges, because it is discoverable by the eye, and on touching the part, the hard feel will be imputed to the edges only, unless the examination be very accurate.

If the chancre is situated on the corona, the first view we have of it usually presents a mere excoriation, with an increased secretion.

The same kind of pain, however, usually attends it, and in a little time the hard, coarse granulated surface shows itself, and the future progress, if not interrupted by remedies, nor any accidental increase of inflammation, is the same as was described before.

Chancres on the glans are very similar, excepting that if we see them early and frequent enough, we find in the beginning sometimes an attempt of the thin cuticle to shoot over them, and form a kind of imperfect cavity, or the whole assumes the form of a pimple. But in a very little time the true character appears, as in the other instances.

If the seat of the disease be the prepuce, particularly the exterior part, the process is still slower before the disease assumes its true character. At first we have a vesicle; as soon as this is broken, not only the cuticle attempts to cover the part, but the ulcer itself

attempts to contract and bring its sides close, like a common granulating ulcer. Hence, if the disease is near the margin of the prepuce, we have so frequently phymosis from this contraction, and the consequent inflammation with which it is attended. But if only the common progress of the disease takes place, in a short time a scab is formed from the pus and cuticular processes. This is soon broken, either by friction, or by the increase of pus from below. For a time an attempt is made at forming a similar defence, till the surface grows so large, that nothing remains but to retard as much as possible the progress of the ulcer by that hard edge and base, which we have before described as constituting the character of the true chancre.

Such is the progress of primary venereal ulcers, according to the different parts in which they are situated, when uninterrupted by any constitutional peculiarity, or accidental occurrence, affecting the general health. If seated on the glans, there is rarely any irregularity, excepting a superficial sloughing, arising from the novelty of the stimulus in an irritable constitution, or from any subsequent intemperance, or other cause which may affect the general health. But if in the prepuce, many inconveniences occur. The contraction of the sides will produce a difficulty in the denudation of the glans; and if to this be superadded the inconvenience of high inflammation, from any of the above causes, the loose cellular texture becomes turgid with coagulated lymph. If the prepuce is long, and the penis suffered to remain in its pendulous state, the tumifaction may become so considerable, as to prevent all chance of our seeing, for a time, the progress of the ulcer.

When a chancre arrives at such a state, that the parts become accustomed to its irritation, it acquires still more the property of the chronic ulcer, or the *vetustas* of Celsus,* and is little affected by

* See Page 56, Note.

any change which may happen to the health. But the matter secreted, and the progress of ulceration, remain for ever the same. Those who have seen the immense excavations which have been formed in such unhappy victims, whose mode of life will not permit their seclusion during the time necessary for a cure, will not doubt the above position. In these cases, however, there is one little variety worth notice, and which ought to have been regarded before. If the chancre is situated on the prolabium, it is broad and shelving, as was before remarked. If higher up in the vagina, the ulceration will sometimes be narrower and almost perpendicular, or even with the sides excavated. The same difference takes place, in a certain degree, between the glans and the prepuce.

Such is the true character of the primary venereal ulcer, a character liable to as few varieties as any law in pathology, and which, under all its varieties, or under whatever interruption it may meet with, can always be distinguished, if we have patience to wait till we can relieve any accidental effects which may have produced an uncertainty in its appearance. I must beg my young reader to be very attentive to it in all the opportunities he has of examining ulcers on the genitals, for if he conceives that all must be venereal, and treats them all as such, how long soever his life may be, or how large soever his opportunities of knowing better, he will go out of the world without distinguishing probably the first disease he is called to cure.

CHAPTER X.

OF THE TREATMENT AND CURE OF CHANCRES.

BEFORE we begin to treat a chancre, our first business should be, as has just been urged, to be sure that such is the disease. Without this preliminary we shall always be at a loss, should any awkward circumstance occur; and with such a previous knowledge, we shall seldom meet with any difficulties, as long as our patient obeys our instructions. When we have ascertained the disease, we have no difficulty about the remedy, nor, for the most part, about the manner of using it. The mercurial salts, however convenient the use of them may be, should never be administered where we can prevail on our patient to rub in the ointment. There certainly are constitutions so susceptible of the effects of mercury, that calomel, continued long enough, is sufficient for the cure of a chancre. But this is rarely the case in the northern climates. A more common effect is to induce a kind of hectic irritation, which will prevent the spreading of the ulcer, and sometimes produce the appearance of amendment, but rarely a cure. Add to this, we are always in a state of uncertainty concerning the stomach and bowels of the patient.

In mercurial frictions we have much fewer difficulties. In common cases there is no necessity for confining the patient, if he is well protected by flannel; and the constitution recovers much quicker and more permanently from the irritation excited in this manner than any other.

As Mr. Hunter's theory of the manner in which mercury cures the disease, is now pretty generally adopted, I shall not stop to take notice of the conjectures concerning chemical properties or antidotic virtues. Every accurate observer will find, if his patient uses mercury for an ulcer such as I have described, that, in proportion as the constitution becomes affected, the ulcer will become cleaner, that is, more florid. If the chancre has been a recent one, when it looks perfectly clean, and the constitution shows evident marks of mercurial irritation, the process should be desisted from, and the part suffered to heal, or treated according to circumstances, as will hereafter be detailed.

I have already remarked, that the mercurial salts should always be avoided. When continued for a length of time, they are not only attended with the inconvenience before mentioned, but often produce a weakness of stomach, which continues for years, if not for life, and this in proportion to the activity of the preparation. This does not, as far as my observation has extended, follow from the use of the crude mineral, triturated with any conserve, or exhibited in any other form. Pills of this description may be taken during the mercurial friction, or if the latter is not submitted to, they are the only manner in which mercury should be exhibited by the mouth. In irritable habits, it is often advisable to add a small portion of opium. To secure them still further from affecting the naked coat of the stomach or intestines, the patient should be advised to take the pills as soon after a meal as possible, avoiding every thing sour, particularly at the dessert. The dose will of course be small at first, and may be gradually increased, if the bowels are not affected.

During this process, we should always keep in mind the object we have in view, which is to excite an irritation greater than the venereal, and to keep it up till the callous edge and base of an ulcer are absorbed. That this is the mode in which mercury cures the

disease, is now pretty generally admitted, and is proved by every analogy which we have traced in the manner in which morbid poisons yield to each other, and to various other irritations. In common cases, therefore, nothing can be more absurd than attempting to support the constitution under a mercurial course. The constitution must be reduced, it must feel the mercurial irritation, or the disease will not get well, how great soever the quantity used may be. I am well persuaded that the exhibition of bark, during the mercurial process, is oftentimes worse than useless. I have seen it protract the disease, and have seen the complexion both of a chancre and bubo mend, as soon as the bark was omitted.

The object, then, being to produce such an irritation as will supersede the disease, we must be guided by two rules in the period for which we should continue the process—The appearance of the chancre, and the degree of irritation produced—If the chancre is recent, it will soon become clean and heal. If at this time the mercurial irritation is excited, however slightly, we should carefully examine the ulcer, and if we find nothing hard about it, we may safely discontinue our remedy, as its effect will continue and somewhat increase afterwards. But our examination should be very satisfactory before we desist; because, in the progress of cure, a small chancre returns to its original state, which we described as occurring before its true character is formed; that is, the edge or part of it becoming clean, the cuticle will make an effort to cover the whole. If, in this case, we perceive a hollowness in any part of the new cuticle, and a hardness or inequality in the surface below, we may conclude that the whole chancre is not absorbed, and unless the mercurial irritation is already very violent, it will not be safe to desist, and trust to the chance of its keeping up long enough, without more of the remedy. If the chancre is more inveterate, little or no change will be perceived, till the mercurial irritation is very apparent. But though we must continue the remedy till the

irritation is very considerable, yet we may always allow for its increasing a few days after we cease, and remaining stationary for some time afterwards. If the chancre grows cleaner, we should not be uneasy if it does heal, or even if it should spread a little. This is merely the effect of the absorption of the callous edge and base, and perhaps of a mercurial ulcer; in either case the whole will heal as the mercurial irritation ceases.

In conducting a mercurial process, it is of great importance to have a previous knowledge of our patient's constitution, particularly as to the manner in which he bears mercury. Where there is no particular objection, we should excite the irritation as quick as possible. For this purpose the patient should not only rub once or twice in twenty-four hours, and take the remedy internally, but avoid as much as possible wiping himself after his frictions, that the absorption may continue. He should sleep in drawers, and if he rubs only once a day, it should be early in the morning, remaining an hour or two afterwards in bed. If he has never used mercury before, or is particularly susceptible of its effects, we should begin our process with greater care, but never cease till we have produced a sufficient irritation, or till we perceive an amendment in the chancre, or both. Though salivation is the most usual proof of mercurial irritation, it is by no means universally such. Some people begin to spit because they expect to do so. Others, on the slightest increase of the secretion, encourage it to a most absurd degree. We should always examine the mouth with care, to ascertain the first mercurial appearance, which is the starting of the gums before the angles of the teeth. From the time we discover this, we should not fail, if possible, to see our patient every day, and by carefully marking the progress of the disease, and of the remedy, to regulate our process accordingly.

There are constitutions in which we have all the symptoms of mercurial fever without any affection of the mouth. The breath

will be tainted, but in no way different from what happens in common feverish complaints. I have thought the perspiration has had the peculiar smell attending those who are under the influence of mercury, but this may arise only from the ointment about them. The head-ach is usually intense, and the incapacity to apply extends to the most trivial objects. Under these circumstances, the cure takes place with the same regularity as under the severest salivation. I have never met with a case in which mercury produced no effect, excepting after a long and ill-conducted course, as will be hereafter shown.

As the constitution becomes affected, the chancre becomes cleaner and smoother, and, if nothing interrupts it, heals with great rapidity, for the reasons we have before assigned. It might, therefore, be supposed, that this form of the disease might be cured by local applications only; but as the diseased action is extended as far as the callous edge and base, and probably the disposition still further, nothing less than a caustic can remove them. The depth to which we must causticate must depend on the date of the chancre. Probably, in a recent case, it may be unnecessary to go beyond the callous part. But in inveterate chancre, it will be impossible to say how far the disease may have extended. As all this is involved in some obscurity, it is always safer to attack the disease by the constitution, especially as we are certain, that whilst we are so doing, the constitution cannot be contaminated; which is not the case whilst we are repeatedly exciting absorption for the separation of sloughs, occasioned by our caustic.

During the mercurial friction, I have thought that by covering the chancres with dry calomel, they have healed more readily, and been less liable to fungate. This is particularly the case, if the disease is seated in any part of the glans, in which case, if no interruption happens from the above cause, skinning immediately follows the absorption of the diseased part. If the chancre has been on the

side of the frenum, a hole will sometimes be made to the other side. In these cases, as the part heals, we see the perforation beautifully lined with thin cuticle, without the least attempt at filling up the cavity. The same happens if a perforation is made a little higher up into the urethra. The part is lined in the same manner, and the perforation remains for ever. I am aware this last has been imputed by some to the peculiarity of the part, rather than of the disease: but we should recollect, that after abscesses in perineo, or in any other part of the urethra, if the disease is cured, the perforations are filled up, and the canal is preserved.

Whenever the frenum has been the seat of chancre, the patient should be particularly cautioned not to indulge in venereal commerce for some time after recovery, as the general consequence is a rupture of the part. But as it is easier to give advice on these subjects, than for our patients to attend to it, perhaps the better way is, as Mr. Hunter proposes, to divide the whole frenum at once.

If the chancre has been on any part of the prepuce, unless it has been very extensive, the mode of cure is by a contraction of the sides of the ulcer without granulation, so as to heal by a kind of pursing up of the part. The texture of the prepuce is very favourable for this process, but the consequences of it are often extremely inconvenient: for if the ulcer has been seated in any part near the duplicature, the future denudation of the glans is sometimes rendered extremely difficult, and this on those occasions when it is most wanted. The patient should, therefore, be instructed to draw the skin back very often during the cure, and if even then the contraction should be considerable, it will be better to touch the part with caustic, so as to reduce it to a common sore, in which case it will be more manageable.

If phymosis has been induced so as to conceal the ulcers, it is hardly necessary to remind the reader, that the penis should be kept

in a perpendicular posture parallel to the pubes, to prevent as much as possible the extravasted lymph from subsiding, and increasing the intumescence. In this state, though we cannot watch the progress of the ulcers, we may be certain that when the constitution shows strong symptoms of mercurial irritation, they are losing their venereal character. The irritation must, therefore, be kept up a longer or shorter time, in proportion as the chancres have been recent or of longer standing. In the mean while the glans should be frequently washed by means of a syringe, with the solution of hydrargyrus muriatus in aqua calcis, till the absorption of the extravasated juices permits the denudation of the glans, and the treatment of the ulcers in the common way.

If chancres, after their venereal character is destroyed, show but little disposition to heal, especially if they exhibit a roughness without the hardness peculiar to their original character, it will be found very useful to cover them frequently, in the course of the day, with calomel. If this has been neglected in the beginning, or if, in spite of it, the roughness should increase, so as to exhibit the appearance of a spongy sore, it will be necessary to use caustic, which, after one or two applications, will destroy the life of this spongy substance, after which the parts will be skinned over with the customary rapidity.

Sometimes this fungus becomes so hard, and rises so high, as to assume the appearance of warts. Though these are crude, rough, and hard, yet they never can be mistaken for chancres, because, instead of a loss of substance, there is a real increase: the new part rising higher than the old ulcer, or even than the original surface. In this case the caustic must be applied with more vigour.

All these effects arise partly from the peculiarity of the parts, and partly from the stimulus of the poison. The parts themselves are subject to form warts without any previous chancre; but if,

after the venereal action is destroyed, any impediment should occur to the skinning, from the irregular absorption of the previous callosity, or any other cause, according to the laws of all morbid poisons, a fungus will rise, which will continue till it is destroyed, or till the character of the ulcer is so far altered, as to acquire the properties of a common sore, in which case the fungus will scab, and granulate under the scab.

Mr. Hunter was very well aware of these peculiarities in ulcers arising from morbid poisons. In his description of the manner in which several of those ulcers healed, which arose from the transplantation of teeth, or on the female nipples, he particularly remarks, that in the first instance the gums remained shorter, and in the other the lost substance on the nipple was never restored. In his remarks on the local treatment of venereal ulcers, he observes: "Chancres, after having their venereal taint corrected, often become stationary; when they become stationary only, they can be often cured by slightly touching them with lunar caustic. They seem to require that the *surface* which has been contaminated, or the *new flesh* which grows upon that surface, should be destroyed or altered before it can cicatrize; and it is surprising how quickly they will heal after being touched: and probably once or twice may be sufficient."

Here we find him first speaking of the *surface* only which has been contaminated: and afterwards of the necessity of destroying the *new flesh* before cicatrizing can take place: as if he cautiously avoided the expression *granulation*. In a subsequent passage we find him still more cautious; avoiding the mention of those processes which are always expected in the cure of common ulcers. "When," says he, "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." Such, it is evident, he considered as the kindest or best

provision of nature in the cure of chancres, that is, by skinning without the process of granulation.*

As the mercurial irritation ceases, and even before, there appear sometimes small ulcers in different parts of the glans, and even of the prepuce. If these happen without the re-appearance of the original chancre, we may be certain they are never venereal. They occur much more commonly if the disease has been attacked early, which induces me to believe, that had not the mercurial course been entered upon so early, these parts would have been the seats of chancres: that is, that the venereal disposition has commenced there, but has not come into action; that in consequence the disposition to ulcer having been formed, the parts could not return to their healthy action without ulceration, though that ulceration is never venereal. This is the more probable, because the occurrence of a second or third chancre, before the mercurial course is commenced, is a frequent event; and in people who are attentive to their feelings, these new chancres are always preceded by pain in the part.

* I make a distinction between skinning and cicatrization, because in the former, after the granulating process is completed, the granulating eminences contract, and by that means lessen the size of the sore, and the necessary quantity of new skin. Every one is aware how extremely tedious this last part of the healing process in most common sores is, and how much it is retarded in proportion as the sore approaches to a circular figure, by which the contraction of the granulations is more difficult than in a longitudinal sore, in which the edges can approximate more easily. Mr. Hunter imputes this to the new skin being formed from the granulations, and not by a mere elongation of the cuticular fibres at the edge of the sore, which is confirmed from what we sometimes observe in large surfaces, restored by granulation, after scalds or burns. In these cases, the progress is quick, compared to other sores, and we frequently see skinning commence in little islands, if I may use the expression, at a distance from the edges. This readily accounts for the slow progress of cicatrization in other cases. A process is to take place on a granulating surface altogether new, and at which, from the long habit of carrying on a different action, the parts will be less ready. After morbid poisons, on the contrary, the cuticle seems to extend from the edges and to cover the whole surface, before any granulations commence. Mr. Hunter was so much struck with the greater facility with which ulcers from morbid poisons heal, that I recollect, though I cannot now turn to, the passage where he proposed to a patient, whose common sores were usually very troublesome, to touch them in future with venereal matter.

But that these ulcers are not venereal, is as certain as any established law in pathology. I have never known them fail to heal without mercury, nor to assume the chancrous character. If they do not heal soon, they are apt to degenerate into soft red warts, and should be touched with caustic: but mercury will have no effect on them. Mr. Hunter takes notice of these ulcers,* and seems to consider them as arising from the cause I have assigned: but his language is somewhat obscure: perhaps the reader may say the same of mine.

When an ulcer appears on the part which was the seat of the chancre, the question is not easily decided. We should first ascertain whether the chancre really healed, and continued sound, without having skinned over before the callosity was all absorbed. If the mercurial course has been conducted in the manner I have described, there will be little difficulty in ascertaining this, as the mercurial irritation will not have subsided till some time after we desist from rubbing, and consequently not till there is scarcely a probability but that all venereal action must be effectually superseded. But if the course has been conducted differently, either by mercurial salts, or by resting for a short time, as soon as the mouth becomes sore, and beginning again, the question will be more complicated. In the last case, we should wait till the true character of the disease shows itself, and if it should prove venereal, by the hard surface and pain, we should begin our operation with the regularity I have before recommended: and, I need not add, with the conviction of the impropriety of stopping *re infecta*. It may, however, be useful to caution the young practitioner, that if this hard edge and basis is at all above the common surface, and not skinned over, he is to consider it as a hard fungus, or soft wart, whatever colour it may assume, and trust to a few touches of lunar caustic.

* See Page 242. 247 second edition.

Thus far we have confined ourselves to the chancre, the primary venereal ulcer, the only kind of ulcer on these parts undescribed by Celsus; the ulcer, that is, the progressively increasing sore, with a callous edge and base.

CHAPTER XI.

OF BUBOES.

IT has been already remarked, that bubo arising from gonorrhœa, is, for the most part, sympathetic, and requires for its cure only the common means of allaying inflammation. It was, at the same time, admitted, that an affection of the inguinal glands might arise from the absorption of venereal matter from the urethra, in which case the disease must be treated like the venereal bubo, which we are now about to consider.

If the bubo is the only symptom when our patient is first introduced to us, it becomes an important object to learn the exact history of its first appearance, of every attendant circumstance, and of every remedy which has been applied. If the tumor is all that has appeared, to say the least, the chances are that it is not venereal. Such a disease was well known to Celsus, and even in the milder climate of Italy was often found extremely troublesome. It is contrary to general analogy, that the glands should ever be affected by any contagion, without the appearance of primary local action, if the law of the poison is to produce a primary local action where it is first applied: I know of no instance on record, in which the constitution has been affected by a bubo, without a previous chancre, or gonorrhœa. Lastly, as I am aware that many honest and ingenious men maintain a different opinion, I shall only add, that I have never seen reason to repent the not having treated such buboes as venereal.

If a bubo has been the consequence of an ulcer on the penis, which healed spontaneously, we may be certain that it is not venereal. It may be the effect of a morbid poison, as probably many of Celsus's were: it may be assisted by, and may even heal under, the use of mercury: but this will be no proof of its venereal origin.

If the bubo has not appeared till after the chancre has healed, it will often prove venereal, and must be treated as such.

If the first view we have of the bubo is in its open state, the same enquiries and the same cautions are necessary as before. The first and most important of all these is, whether mercury has been used for it, to what degree of constitutional irritation, and with what effect on the part. The knowledge of all this is absolutely requisite before we commence our treatment, and an attention to it is equally important during the whole progress of cure. This subject will, therefore, make a necessary part of the succeeding chapter.

CHAPTER XII.

OF THE TREATMENT OF BUBOES.

If a bubo appears during the open state of a chancre, or does not yield to common remedies after arising from gonorrhœa, there is the fairest reason to believe it the effect of venereal matter absorbed from the open ulcer or gonorrhœa. In these cases our business is to push our mercurial process as quickly as possible, that the action of the parts may be altered before matter is formed, or before the skin is broken, if that should be ultimately necessary. It is possible we may gain something by directing our process through the glands, that is, by rubbing into the limb below; but if any interruption occurs from the skin being fretted, or from any other cause, we should avail ourselves of every possible means to saturate the constitution.

Under these circumstances we have two means of judging of the progress of our mercurial course—the state of the mouth, with the degree of general irritation, and also the appearance of the chancre. If these are such as were described in the last chapter, we have every reason to believe that the action of the bubo is no longer venereal; in which case, if the skin has not hitherto been broken, nor matter formed, the tumor will usually subside, and the part gradually, though slowly, return to its natural figure. Even if matter has been previously formed, it will sometimes be absorbed, which is perhaps of less consequence than is suspected: for if we open the skin, we do not prevent absorption: on the contrary, we render it necessary, as ulceration must, in a certain degree, take

place, which is only a process of absorption. And whether a smaller or larger portion of venereal matter is absorbed, can be of no consequence, since we find in this, and in all other contagions, that when the constitution is susceptible of the impression, the smallest quantity applied is sufficient to produce the disease.

If, however, the fluctuation is evident, the tumor increasing, and the pain considerable, the probability will be, that the matter will advance to the skin. If a blush appears on the integuments, not caused by any external friction, the consequence is certain, and all prospect of preserving the skin must from that moment be given up. The question will now be, Whether the tumor shall be left to burst spontaneously, or be opened by art? When the pain and uneasiness in walking are not too great, the disease may be left to its usual course. But if the matter is deep seated, and the pain from the pressure of the fascia, particularly in walking, is irksome, the patient may be considerably relieved by making an outlet for the matter. Even if the abscess proves particularly indolent, it is often advisable to open it, because the retention of the matter under the skin only occasions an enlargement of the cavity, and the inflammation produced by the artificial opening is often serviceable.

Every attempt of this kind should, however, be avoided, till the mercurial irritation begins to subside, and if possible, till the health has almost recovered from it. It is fortunately not often necessary to open a bubo sooner, because high inflammation seldom, if ever, exists during the mercurial irritation, and because at this time the patient seldom engages in those exertions which would increase the inconvenience from the tumor.

If, however, after the mercurial irritation has somewhat subsided, it should be thought right, from the causes above mentioned, to open the skin, the most advisable means will be by the application of kali purum, which may be allowed to dissolve under the inspection of the practitioner, and to destroy as much substance,

and in such a direction, as he shall think proper. Not only the terror, but the pain from this caustic, is less than from the knife; besides which, the surface will never be so entirely crude as after a cut; a most important consideration, as long as the least danger of mercurial phagedæna exists.

Thus far we have traced the local treatment of buboes from their first state, and as long as the skin remains entire. Their treatment constitutionally will be well understood by attending to the rules given for ascertaining their nature. If the tumor is the only symptom that has appeared, the reader will perceive that I should not advise the use of mercury. That such buboes have often been cured under such treatment, it is not my intention to doubt; but as far as my experience extends, they may be cured without that process. I should, therefore, only resort to it as in other chronic sores, which are frequently roused to healthy action by such a stimulus, but which stimulus should never be wantonly applied.

If the tumor may be traced from an ulcer on the penis, which has healed spontaneously, it is evident that the cure of such ulcer, whether arising from a morbid poison or not, was within the power of the constitution. We have, therefore, every reason to suppose that the bubo may be cured in the same manner.

Sometimes a chancre will be healed, after which the bubo will first appear, or begin to come forward. In this case, either the venereal action was, in this part, only suspended during the mercurial course, or, which is most probable, the gland had been contaminated, so as to give the venereal disposition; but the action not having taken place before the mercurial irritation began, was consequently suspended till that irritation ceased. This last appears to have been Mr. Hunter's opinion.

"It would appear," says he,* "in some cases, that it is some

* Treatise, page 260.

time after the absorption of venereal matter, before it produces its effects upon the glands; in some it has been six days at least. This could only be known by the chancres being healed six days before the bubo began to appear; and in such cases it is more than probable that the matter had been absorbed a much longer time before; for the last matter of a chancre most probably is not venereal: and indeed it is natural to suppose, that the matter may be as long before it produces an action on the parts, when applied in this way, as it is, either in the urethra, or in forming a chancre, which I have shown to be sometimes six or seven weeks."

Such buboes should be treated in every stage in the same manner as those which occur during gonorrhœa or open chancre, that is, the mercurial course should be conducted in the same manner, and with the same vigour and perseverance, as was proposed for the cure of chancres. Without this, we never can ascertain that we have subdued the venereal action, nor, consequently, how we are to direct our future practice.

Hitherto all our cautions have been, if possible, to prevent the suppuration of a bubo, or to alter its action before suppuration takes place. We are now to consider what should be our practice, if the bubo has burst during the mercurial process, or if the importunities of the patient, or the confidence of the surgeon, has induced him to open it at such a time. The greatest attention to the constitution, and the frequent as well as strictest examination of the bubo now become necessary, lest we should induce a mercurial phagedæna, to avoid which it must, by this time, be understood, that all our cautions relative to the exhibition of mercury have been hitherto directed. In the treatment of open buboes, this is much more to be apprehended than in any other form of the disease, because in this we have an open sore, only a part of which is venereal. If, therefore, we begin our course whilst the edges of the wound are crude from a recent incision, or even from the first casting off of

a slough after the application of a caustic, we shall be in great danger of inducing the mercurial phagedæna on these surfaces, and this probably before the constitution is so far saturated with the remedy, as to supersede the venereal action in the other part of the wound. The same caution is absolutely necessary whenever, during the mercurial course, we see a crude part on the sore looking cleaner and redder than a granulating surface, and without those white edges which always mark a healing sore. Above all, we should be particularly alarmed if the external edges of the whole space spread, with a red fiery appearance, without the whiteness above described. In any of the above cases, but most of all in the last, we should apprehend a mercurial phagedæna, which is a much worse disease than a venereal bubo.

If these appearances gain upon us, even though the other parts of the sore retain their appearance, or become sloughy or otherwise ill conditioned, the mercurial course should be suspended altogether. If the constitution requires it, bark may be given, and such topical applications should be used as are found most agreeable to the patient's feelings. Of these, a poultice made of boiled poppy heads is often the most soothing, as well as a fomentation of the decoction. Internally, if the pain is considerable, cicuta or opium may be used with much advantage. If, in the mean time, the complexion of the sore in other parts becomes still more venereal, we must submit to it, till the phagedænic appearances cease, and not recommence our course as long as we see any parts of a fiery or brownish red gaining ground upon us, either in the form of fistulous sores, or a spreading ulcer. Should this fistulous appearance become stationary, or increase, it will be worse than useless to open the sinus. Wherever the knife goes, whilst this state of the parts and constitution remains, we shall have some kind of ulcer, either spreading or chronic, and always attended with pain.

When this appearance has changed to that of a suppurating

ulcer, it will be necessary to examine its whole surface with great attention. It is not so easy to ascertain the true character of an open venereal bubo as of a chancre. The dissimilarity of parts, of which the bubo consists, and the probability that one part may be affected by the disease, whilst another is not, always produces an uncertainty. If a sore continues to spread after the entire cessation of the phagedænic appearances above described, we should first allow time to see whether there is any disposition to heal in that part only, which has undergone the phagedænic process; if so, there is great reason to believe that the action on the remaining part is venereal. If the whole becomes indolent, almost stationary, and with little pain, it is more probable that the character of the sore is what we usually call scrofulous; for we should recollect, that whatever may have been the original character of the disease, it may have assumed a new form after the mercurial irritation.

It is very surprising that the swelling, or consequent unkindly suppuration of these glands, should so often induce us to treat them as venereal, without sufficient collateral proof. If it should be asked, What the proof of venereal buboes is? I should not be backward in admitting, that I know no decisive character by which they can at once be distinguished from common scrofulous glands in the same parts. But with a little patience they may always be ascertained. In the present instance, if the disease was originally venereal, we shall find the part about the circumference cicatrize, whilst the centre, if it retains its character, will continue painful and ill conditioned.

The true venereal open bubo, which has never been treated with mercury, is chiefly distinguished by an uncomfortable aching pain, which occurs only at particular times, and is different from the scrofulous, in which the patient rarely complains of much uneasiness, excepting when he uses exercise, and then only of soreness. The matter is usually more yellow, (but this is not a mark to be depended

upon) and some part of the bottom sloughy, but not always the whole surface. When such a sore spreads under these circumstances, we shall seldom do wrong in treating it as venereal. But whatever the attendant state of health may be, always begin your course as you must end; that is, begin with mercurial frictions. Even if under hectic, arising from a venereal cause, your patient will find almost immediate relief in his general health. At this time he should be cautioned to expect, that the course must be continued till his health suffers from it, if he expects a permanent cure. This will render him more patient, and when he finds, as he certainly will, that the sore mends as his health suffers, the irksomeness of your visits will be very much lessened.

When this course is completed without any interruption from partial phagedæna, that is, when the mercurial irritation is excited and kept up, till the whole surface acquires a cleaner appearance, and shows a disposition to granulate or contract, or even continues stationary, though clean, we may be certain that no venereal disposition remains in the bubo.

With a strict attention to these rules, though we may not always find the sore heal as readily as we can wish, yet we shall see our way, and, for the most part, our patient will be satisfied with our explanation of his case. But even with all this care, phagedæna will sometimes occur, nor can we always controul it without much difficulty, by laying aside the remedy, and attending to every other caution. I once saw a gentleman, who, after his bubo broke (which there was no reason to doubt was venereal) was seized with scarlet fever: from this time the edges became phagedænic, and fistulæ formed in several directions. It was near four months before these subsided so completely, as to admit the repetition of mercury. During this time the other parts of the bubo had gained some ground: but the subsequent exhibition of mercury was unattended with any ill effects, and the whole readily healed afterwards. All the stories we hear of venereal buboes which will not yield to mercury, or will be exas-

perated by it, as they are related by several authors, have no other origin than what I have now described, that is, they have either never been venereal, or their venereal character has been partly or completely superseded by the mercurial course; but the practitioner having nothing in his head but the first disease, has redoubled his industry in the application of his remedy, and consequently increased the distresses of his patient.

All these cautions should be kept in view, when the first sight we have of our patient is with an open bubo. After an attentive examination, we must be close and accurate in our enquiries, and as much as possible leave the sufferer to relate his own story. Our patients are not always aware of the importance attending a correct answer to our questions. Without any bad intention, they will frequently contradict themselves, especially if hurried or interrupted in their narrations. Our first business is to learn the symptoms about the genitals, to which we may impute the tumor; next the manner in which it has been treated, particularly whether a mercurial irritation has been excited high, and kept up long enough after the appearance of the bubo, to supersede its venereal action: if this is not the case, and the whole surface is suppurating, though painful, we may suspect that the origin was venereal; and if the patient's history should strengthen such a suspicion, there can be no doubt how we should proceed. In women it will sometimes be difficult to ascertain every fact, because venereal gonorrhœa will often be mistaken for leucorrhœa, a circumstance we must always keep in view in our examinations of the sex.

But in men it is rarely that we are consulted for this form of the disease in the first instance, till after they have grown weary of some other person. In this case we shall most commonly find the bubo in a truly phagedænic state, with sinuses, irregular and crude edges, intense pain, and every other symptom of ill conditioned ulcer. Under such circumstances, neither the importunity of our patient,

the irksomeness of daily seeing a sore for the most part stationary, nor even the certainty that one part of it retains its venereal action, should induce us to recommence mercury till the whole surface becomes suppurating: after which we shall rarely be at a loss how to manage it. If the edges of the wound are white, and within that you see a granulating surface covered with pus, where lately you saw the fiery or brownish red mercurial phagedæna, you may be certain a healing disposition is begun, which nothing but the remains of a venereal action in another part will interrupt. I should have remarked, that you will have little to apprehend concerning the venereal action gaining ground, whilst the mercurial phagedæna exists in another part of the sore. The phagedænic state of the sore arises from a state of the constitution, which, though not sufficient entirely to supersede the venereal action, will, I believe, always, in a great degree, suspend it, as was before remarked of the irritation excited by the exhibition of mercurial salts for chancres. But as soon as the constitution is recovered from the mercurial hectic in either case, the former action will recommence, and we shall be interrupted in our healing process by the venereal ulcer gradually gaining upon us. We may now with safety commence our mercurial course, which we should do with vigour, and without expecting to cure the disease till the patient feels all the symptoms of reduced health, and that irritation which is peculiar to the proper administration of this important remedy.

To conclude—The inguinal glands will swell from any of those causes which produce the same effects in the glands of the axilla and other parts; and these causes are more frequent in the groin than in the axilla, because the lower extremities are more frequently inflamed, and the genitals are exposed to a variety of morbid poisons.

If, therefore, the genitals are free from disease, or have suffered no local complaint, but what has been spontaneously healed, we

have reason to expect the bubo may subside by the common remedies which allay inflammation.

Even if we believe the origin of the disease to be venereal, we should endeavour to alter its action before, and, if possible, to prevent, suppuration.

Whilst a bubo is open, mercury should never be exhibited as long as any part of the surface is crude.

If, during the exhibition of mercury, any part of the circumference looks clean and red, or of a brownish red, without white edges, we should be cautious concerning the continuance of our remedy; and if the ulceration continues to gain upon us, particularly if the pain increases and sinuses are formed, we may be certain our disease is mercurial, and should lay aside our remedy till suppuration recommences in every part of the sore.

If, after a regular course of mercury, in which the constitutional irritation has been considerable—which has produced a healing disposition, or a clean surface throughout the whole sore—if, after this, the bubo should be troublesome, it is no longer venereal, and must be treated according to the constitution of the patient, or other temporary circumstances.

If the mercurial phagedæna should not subside readily, in addition to the remedies before suggested, those acids, which were at one time so much extolled in the cure of syphilis, seem, by pretty general consent, well worth attention.—Mr. King, of Bristol, informs me, that he has been very successful in applying Mr. Baynton's plaister bandages. The disease is certainly very troublesome, and the practitioner will often be glad to avail himself of every means that have, under different circumstances, been found useful.

CHAPTER XIII.

ON SECONDARY SYMPTOMS.

INTRODUCTORY REMARKS.

WHATEVER appearances may occur on the genitals after the courses of mercury, conducted as above described, they are never venereal, unless the patient has again exposed himself to the infection: and whatever ulcers we find on the genitals, before the application of mercury, which have not, after a short time, the character above given of chancres, are certainly not venereal. They, however, are often the effect of morbid poisons, and where their progress is dangerous, it is always advisable to attempt arresting it by mercury. If the success of that remedy depends, as is now pretty universally admitted, on the excitement of a new action more powerful, but not permanent, like the venereal, it is consistent with every analogy to expect advantage from it in other morbid poisons. But this must depend on the comparative forces of the two actions, which it is impossible we should estimate, till we have a greater number of cases, and these traced with greater accuracy than most of those which have hitherto been related. And though it may be justifiable to treat many ulcers on these parts in this manner, and is certainly our duty, where the probability of a morbid poison is strengthened by the unusual rapidity of the ulceration, and its similarity to those which have been described by other writers, as yielding to that remedy, yet we should always prepare ourselves for a contingency which we cannot prevent. If the ulcer does not yield to mercury,

it will probably be exasperated by it, as the constitution will be rendered more irritable; or the progress of the ulcer may be only suspended by the most violent mercurial irritation, and may resume its activity as soon as that irritation has subsided. All this must be exceedingly embarrasing, but it will be less so, if, in the beginning, we have exactly ascertained the disease as far as our present knowledge permits; that is, if we have ascertained that it is not venereal, and that it is probably the effects of some new or hitherto ill-described morbid poison. In these cases we can only act according to our own or the experience of others, accurately marking every appearance, and the result of every remedy, for our own improvement, and for the benefit of future sufferers and practitioners. But as we shall be obliged to resume this subject hereafter, we may content ourselves at present with its application to the primary character of that disease, which by this time, I hope, is well ascertained.

Though after the process, directed in the manner above described, for the cure of chancre and bubo, we can assure our patient that he is free from all remains of the disease in those parts, yet we are not only entirely ignorant whether his constitution has been affected by the absorption of matter whilst the primary symptoms existed, but, even if we know with certainty that it is affected, it is out of our power to prevent the future appearance of the disease in the skin, throat, or bones. All continuance of the mercurial course, therefore, with a view of preventing secondary symptoms, is not only absurd, but injurious, inasmuch as the health may be permanently injured by the longer application of that remedy; and if we should find it necessary to have recourse to it on a future occasion, the difficulty of affecting the constitution by a regular course is greatly increased. I am aware there may be people who will object to such a position, and I should be extremely glad to meet with such objections as carry even so much plausibility with them,

as to enable an impartial enquirer to answer them. However, as this mode of considering the appearance of secondary symptoms, without the re-appearance of the primary symptoms, was never thought of till our own times, I shall first show that it is the only means we have of accounting for facts which no one controverts; next, that it was tacitly admitted by all well-received writers before Mr. Hunter; and, lastly, I shall take notice of some of the objections that have been made to his doctrine since I undertook to defend it in my former edition.

It will not be disputed, that after the disease is cured in the genitals and groin, it will sometimes appear in the skin. It is not less certain, that after it is cured in the skin, it may appear in the bones. It is also certain, that when it does appear in the skin, it is cured by the same means, and still more readily, than the parts first diseased; for we know that if the parts first affected are not cured before the skin shows the disease, that the latter gives way to the remedy much sooner than the former. The question will therefore be, When did the skin become affected? To this we can make only one rational answer, namely, that the absorption from the bubo or chancre, before they were cured, contaminated the skin, though the effect of that contamination did not appear till after those primary symptoms were cured.

If, then, the skin is more easily cured than the primary affection, as the evidence of our senses proves, when they both show the disease at the same time, how happens it that, after a course of mercury sufficient to cure permanently the primary symptoms, the skin should ever show the disease? Yet that this is really the case daily experience proves, and no one has the hardiness to doubt.

That Hoffman was sensible of the difficulty of preventing the re-appearance of the disease, is evident from many parts of his works; and also that he found it permanently curable when it did re-appear, by those remedies which would not prevent it.

" We could," says he, " produce numerous instances, where, after mercurial salivations, the symptoms have abated for a season, but after a while returned with greater violence, because the taint was not discharged; but part remaining behind, gradually prevailed and acquired fresh force: but by nearly the same course of mercurials, and drying decoctions, together with a proper use of warm bathing, the cure has been completed, and the virulent matter discharged from its innermost seat of the nervous parts."* We shall hereafter 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.

But no writer before or since his time has spoken with such honest simplicity as Boerhaave. With a dignity worthy of his character as a physician, a man, and a Christian, we hear him freely acknowledge, that the most skilful physicians were unable to find any means of securing those parts which are liable to secondary symptoms, and that himself had been thirty-six years attempting it to no purpose. His words are very striking.

" Enimvero in tractando hoc malorum fœdissimo talia quandoque accidere notantur, qualia annosa pridem observatio alias non viderat; prorsus ut vel exercitatissimus quisque veteranus artifex

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

novitius quandoque in hac disciplina tiro evadat, faterique cogatur invitus, ignorari sibi modum morbi, nesciri bona ad hunc *præsidia*. Neque minus frequens est hac in ægritudine cernere leve quoddam, atque animadversione fere vix dignum habitum, incidens, ex quo tamen brevi postea horrendum, late serpendo, monstrum nascitur, & indomitum. Vah quoties in faucium, linguæ, palati, narium, ulcusculis vilissimis, vel prudentissimi cæterum Medici errores suos doluere! fateor, ultra triginta jam & sex annos versor in hoc morbo curando, neque toto hoc tempore mihi defuit occasio, neque sane omisi ea oblata sedulo uti, ut discerem penitus indolem heterocliti hujus mali."*

By the use of the word *præsidia*, it is evident he was not so much at a loss in curing the disease, as in *securing* his patient from a return of it in the palate, nose, and neighbouring parts. This induced him to look for remedies among the early writers, as if it were possible that any useful remedy for so serious a disease could have fallen into disuse. By perusing these books, however, with an industry characteristic of the man, he found that Sir Ulric Hutton had been cured by guaiacum, after several ineffectual salivations. Whoever carefully peruses Hutton, will find his whole complaint, when he applied to guaiacum, arose from his frequent and severe courses of mercury. Boerhaave, however, conceived he had revived a remedy, by which the poison could be *expelled from the bones*, which he supposed mercury could not accomplish.

It is easy to see how Boerhaave was led into this error. In those cases in which secondary symptoms appeared on the skin or throat, he had used violent and repeated salivations, conceiving that for want of such a process, these secondary symptoms had appeared. But in spite of all his endeavours, he found it sometimes occurred again; not indeed in the parts in which he had cured it,

* Præfacio ad Aphrodisiacum.

but in the bones. Hence he supposed that the whole adipose cellular membrane, as well as the cancellous part of the bones, was infected, and that though mercury would cure it in all these parts, yet that the poison could not be expelled from the bones without other means. Finding Hutton was relieved by guaiacum after repeated salivations, he supposed that this remedy had the power of expelling the virus, but that it was first necessary all the fat in the body should be dissolved by mercury.

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 his 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 not only permanently cured all the venereal blotches and ulcers that appeared in the skin and throat, but that he could prevent the disease from returning to those parts. Still, however, the bones were not secure. 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 unsatisfactory his reasoning may be, it is a strong proof of the closeness of his observations, and of his desire to be serviceable in the profession to which he had devoted himself.

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 this re-appearance of the disease, his theory is not more satisfactory than Boerhaave's. He first proves that 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 sometimes by degrees decrease and disappear, and sometimes keep such an even tenor, as constantly 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 quality, and the manner of its generation, as to permit a renewal of the poison, but such a renewal as is constantly one and the same, without 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 by the same means as

the blood departs from its natural disposition, the before latent poison will presently gain ground, both in quantity 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 always wishes to appear in the character of a divine and moral philosopher, as well as a physician, acknowledges the great difficulty of ascertaining when a person is cured. " Nor," says he, " can any man have any other security than the absence of all the symptoms, which continuing through a double solstice, it is to be hoped he will hear no more thereof." He concludes, therefore, with advising men to chastity, in the words of a *royal apothegmatist*, as the only certain preventive of a disease, the cure of which is so uncertain.

Chapman gets rid of the question like too many other writers, by ascribing the secondary symptoms to the neglect or ill treatment of the primary, 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,

* Barrowby's Translation, Vol. I. Page 159.

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 concludes his remarks on this subject, with acknowledging his incapacity to determine when the lues is *radically exterminated.* "To know whether the lues is radically exterminated, is a nice point of practical judgment: 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, I advance nothing but what we see daily confirmed. 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 discovers the presence of iron, there would be nothing necessary but to administer that remedy the moment we think the patient had taken mercury enough. I have made some experiments, he adds, on this subject; but the number of facts are not yet sufficient to enable me to form a conclusion." †

When we hear of *radical* extermination, I wish not to be accused of an 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 met with 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

* Chapman's Treatise on the Venereal Disease, page 212, & seq.

† Swediaur, page 169.

syphilis, and concludes with observing, that he has made some experiments on the subject. Now, what is meant by experiments here? If he had acknowledged, that in spite of the closest observation on 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 satisfy ourselves, 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 unequivocal sign that mercury has exerted its action on the venereal virus. If venereal ulcers, which 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

* Id. page 167.

consequence of the first error, viz. ascribing that to the action of mercury on the *poison*, which ought to be ascribed to the action excited in the *constitution*. But all my present business is to show that though Dr. Swediaur conceives it is possible, by a long-continued use of mercury, to *eradicate* the disease, as he calls it, or to prevent the re-appearance of symptoms; yet he acknowledges all his observations on the subject have hitherto proved unsatisfactory.

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 indifference. But it may be fairly traced from the acknowledgment of every writer, and every candid practitioner will admit, that the disease does sometimes appear in the skin and in the bones, 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 remedy. 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, and other juices, 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 contaminated 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 remedy, is no certain security against the secondary symptoms of the disease.

Secondly, That 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, That 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 the primary.*

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 the appearance of the disease in a part, which it readily cures when the diseased action has shown 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 fre-

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

quently enough to be accurately traced. The yaws is a disease arising from a morbid poison, the symptoms of which gradually subside; but mercury produces a considerable effect on it. If given in any stage it will often suspend the disease; and if its exhibition is delayed till the disease has produced its full effect, it will facilitate and hasten its 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 arrived at a certain stage. This is analogous, in many respects, to what is well known of the venereal disease. The secondary symptoms never appear while the constitution is under the mercurial irritation. They are, however, only suspended if the parts are contaminated; for 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 shows itself by a regular fit, we have

* 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, without 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 of health is soon followed by a paroxysm of regular fever."

Jackson on the Fevers of Jamaica, p. 134.

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

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

* The bark is to be continued during the time the subsequent paroxysm should have continued, and then is to 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."—Fordyce's Practice of Physic. See also Dr. Jackson.

yet I must also observe, that its virtues do not extend farther than to a temporary suspension of the paroxysms. That bark does not *eliminiate* 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 attended to, that some periods are more remarkable 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 circumstances of the patient, and attend to the nature and degree of the prevailing epidemic, we may be often enabled to form a tolerable conjecture with regard to the most probable period of return. Having therefore acquired from observation some general ideas of the different propensities to relapse, in different situations and in different subjects, I usually began to give the bark in quantity, and to use other precautions, on the fifth day after the suppression of the paroxysm, in cases where there were the strongest suspicions of a speedy return; while this was delayed till the eleventh, nineteenth, or twenty-seventh, in others, in proportion to the different degrees of healthiness.

This practice was continued for the space of three days, or till the suspicious period was past."*

The fact, that bark does not eliminate, or, as we might as well say, exterminate the cause of the disease, is proved with much perspicuity by this accurate observer. But it 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 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 paroxysm. 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 we shall trace in the secondary symptoms of syphilis.

If in yaws the circumstances are not exactly similar to the secondary appearance of 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 ad-

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

† We must except sivvens, as will be seen hereafter.

mitted, that mercury never cures but when it excites a peculiar action in the constitution. This action, the testimony of our senses 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 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 action from that which has been the subject of experiment, and therefore, as in the instance of the bark with some species of intermittents, or of 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, is it not justifiable to call the then state of the parts a *disposition to take on the diseased action?*

Having thus far, I hope, cleared the ground, I shall proceed to state Mr. Hunter's theory upon the disease. A theory which, for reasons I cannot pretend to determine, has been overlooked by many, and misunderstood by others, who seem to have attended to it. For this reason perhaps it may appear in a more striking light, if we attend to the arguments which have been brought against it by his antagonists. It has been said, that "if mercury, prior to the action, will not remove the disposition, what reason have we to give mercury internally, during a clap, bubo, or chancres?"* Without entering into the question, whether these latter symptoms can be cured without the internal use of mercury or mercurial frictions, I shall only answer in Mr. Hunter's words, "Mercury hinders a disposition from forming, or in other words, prevents contamination."

* Swediaur, p. 309, third edit.

It is 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. This is not peculiar to the power of mercury over the venereal disease. During the prevalence of any epidemic, or of the ague, in humid situations, prudent people often escape the disease by the frequent use of bark or other means. 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. It is true, this prophylactic property can never be reduced to certainty, because we never can know whether those who escape would have taken the disease without such precaution: but this argument is equally applicable to the secondary symptoms of lues: we can, therefore, only reason on the probability in all as far as inferences may be drawn from facts.

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.

A gentleman, who, when earlier in life, wrote too rapidly to do justice either to his own abilities or the arguments of others, ran more boldly into this error than most of Mr. Hunter's opponents. "It is evident," said he, "that mercury prevents the morbid effects of virus which is in the habit, but not come into action. Indeed, if mercury had not this power, it would be of little use. For if every particle of virus began to produce disease before the infection could be expelled, no man, once infected, could ever be cured. The constitution of the strongest would be ruined, and his life terminated, before all the particles of virus absorbed from a chancre or bubo could successively come into action and be destroyed, by the same number of courses of mercury."

In stating this proposition, either the theory of Mr. Hunter, or the writer's own, should have been adhered to, or the conclusion must be unsatisfactory. There is nothing in Mr. Hunter's book like the opinions here fastened upon him: nothing about "*particles* of virus coming into action:" or even "*particles* of virus beginning to produce disease." When the venereal matter has got into the circulation, Mr. Hunter* conceives, to use his own language, that it acts on all parts of the body with equal force. But the parts that receive the disposition, and afterwards come into action, he divides into two orders: First, the throat, fauces, and skin: Secondly, the bones. 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 formed, 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 in this stage no mercury is exhibited, the blotches will increase in number, and ulcerate, but if the first are cured by mercury, no fresh ones will appear. 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 mer-

* See Treatise, page 307, & seq.

cury is exhibited so as to cure the first, no others will ever show the disease from that stock of infection. The error, therefore, arises from *particles of virus* being mistaken 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 more closely, by answering some further objections which have been made to it. But as I trust every difficulty is now cleared, as far as this stage of the argument requires, I shall conclude with refreshing my reader's memory by a general recapitulation of the doctrine.

First, The matter which produces chancre may produce gonorrhœa only.

Secondly, 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 disease.

Thirdly, 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, 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, When the action has begun in an order of parts, it may be cured, and will not return in the part or that order of parts, from the same stock of infection. But,

Sixthly, The diseased action may 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, 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, 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, which periods have not varied more than in other morbid poisons.

Ninthly, Whatever doubtful appearances may arise on the skin, throat, or bones, during the mercurial irritation under which chancres or buboes are giving way, they are certainly not venereal: and even if such secondary symptoms appear after that mercurial irritation has ceased, but earlier than the period specified in the preceding proposition, they are to be taken with great caution.

Tenthly, If no secondary symptoms appear for three months after the mercurial irritation has ceased, and the constitution has not in the mean time been occupied by any other disease, we have, for the most part, no reason to apprehend any thing in the skin or throat from that stock of infection.*

Lastly, If solitary instances occur, in which the periods above stated are anticipated or protracted, they are not more numerous

* The bones must be more uncertain, for the reasons before mentioned, and because many of our patients of the poorer class, under this disease, are extremely inattentive to every symptom which does not show itself, or confine them. The general medium is, however, about twice the period of the soft parts.

nor the deviations greater, than what occur in inoculation for small-pox, considering the comparative difference in the periods of each.

Such is the theory formed by Mr. Hunter, which is consistent not only with the history of the disease as given by every faithful writer, but with those facts which every honest practitioner, who has seen enough of the disease, must admit have occurred in his own practice.

I shall now take the liberty of starting an objection or two, not against the theory, because more than twenty years' experience, have satisfied 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, that mercury will not cure virulent gonorrhœa, has been already considered.

My next objection is to the number Mr. Hunter 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 the late 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 salivations, 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 mercury could arrest it, took off a part of each. Where ulceration exists, absorption must always be going on, as the only means of

producing 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 less likely they should ever be so afterwards from the same stock. It is possible indeed, cold, or some other exciting cause that 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.

The last objection I shall make is to a position, which seems to me, more than any, a concession to the general opinion prevailing when Mr. Hunter wrote, namely, that the cure of the disease depends partly on the quantity of mercury used, and partly on the irritation excited. If the disease is superseded by an irritation greater than its own, it must be of no consequence by how little or how much mercury that irritation has been excited, and so I have always found it. I well remember a gentleman, of a highly irritable constitution, who was so soon affected by mercury, that by rubbing in a single drachm, a salivation and fever followed to such a degree, as to make his friends fearful for his life. On this account he was, after the first time, backward in beginning his course, apprehending the consequences. His chancres, however, always healed readily from this small quantity of mercury, nor did he ever show any secondary symptoms. This gentleman afterwards expired under the too great operation of an emetic.

CHAPTER XIV.

HISTORY AND TREATMENT OF SECONDARY SYMPTOMS.

THESE preliminary observations seemed necessary, before we enter on the symptoms and cure of what is called syphilis, or the secondary stage of the venereal disease. Had the discoveries been my own, I might have urged them with less certainty, and have dwelt upon them with less minuteness. But as they are the result of the long experience of Mr. Hunter; as they are the only means by which we can explain those difficulties acknowledged by the ablest medical writers; as they have been confirmed by my own experience; as the ground work is pretty generally admitted by the best practitioners; and as, to use the author's expression in the last conversation we had together, the inattention to these leading facts has been the cause of all the uncertainties of so common a disease, I cannot help wishing such as may not be perfectly satisfied that they understand the preceding chapter, would peruse it again. If I knew how to make it plainer, I would attempt it; but as no better means occurs to me of explaining what appears perfectly simple, and what I verily believe has been overlooked from that very simplicity, I shall proceed to the practical part of the subject, reminding my reader, that if he does not take with him those conclusions, which have been the result of fifty years close attention, he will be perpetually at a loss, not only in ascertaining the disease, but in the application of his remedy.

Whenever we have cured a patient of the first local symptoms of the disease, should he enquire whether we can now ascertain that he

is entirely safe, the proper answer will be—That as long as he keeps out of the way of mischief, we can assure him from all danger of those parts which we have cured [that is, the genitals, if such has been the seat of the primary symptoms] but that we can no more pretend to ascertain whether the disease will appear in his throat, skin, or bones, than after inoculating a person for small-pox, we can ascertain whether he will have an eruption beyond the insertion in the arm. That, however, should the disease appear in any other part, we shall be able to cure permanently all the soft parts with much more ease than we did his chancre ; and should even the bones prove affected, if he gives us timely notice, we shall be able to cure them with equal certainty, though we may require more time. That though the present uncertainty of the re-appearance of the disease will remain for a few months from the time his chancre was cured, yet that the chances in his favour are very great, the occurrence of secondary symptoms being very rare, in proportion to the number infected with the primary. I have never known a patient who was not perfectly satisfied with this language, and have more than once had reason to be pleased at having used it.

If we see the whole progress of the disease, as we generally do when we possess the full confidence of our patient, we shall usually find the first warning of secondary symptoms very much resemble those of common hectic. They are perhaps more similar to the common intermittent, excepting that the sweating is inconsiderable, sometimes scarcely perceived: the head-ach is often intense, and even before the patient is aware of the cause, he feels much greater languor and dejection than his pulse and other signs of disorder would seem to produce. If the disease is to appear on the skin, these symptoms are much greater than when it occurs in the throat. In a few days the skin becomes generally of a mottled red. This is not, indeed, always the case, and when it is so, this appearance is not the real disease, but an affection of the skin, which often pre-

cedes small-pox and other eruptions. As this redness gradually subsides, the true disease appears, and usually the fever for a time subsides as the local symptoms show themselves. The first appearance is sometimes copper spots, pretty regular in their form; but more commonly before these we have small flat elevations of the cuticle; under these is contained a greyish pus. If the cuticle is elevated, we perceive under it a reddish or copper coloured spot, of the same form as the elevation. If the cuticle is left to itself, it will, in a few days, dry and disappear, leaving a similar spot. If the skin is very generally covered with this eruption, each spot or elevation will be small, and rarely increase in size, but gradually in number. If the spots are solitary, they will increase in size: by degrees the cuticle will be lost in the scab formed by the pus, which is so thick and so sparingly secreted as to dry upon the surface, and gradually enlarge the scab, till in some instances it will project considerably beyond the surface. In parts subject to friction about the thighs and nates, the pus being disturbed as soon as secreted, a superficial ulcer is formed. If the disease is under the nail, the latter shares the same fate as the cuticle, the structure of each being similar.

During this whole process the degree of fever is very uncertain, and seems to depend much on the irritability of the subject. The poor, particularly men, often pay no regard but to their local inconveniencies, so that the disease is considerably advanced before we see it: but women, whose constitutions are more irritable, and those men whose situation in life is such, as to make them impatient under every thing which prevents the full enjoyment of their ease or indulgences, usually complain much of want of appetite, shivering fits, succeeded by heat and sometimes sweat, continual head-ach, and great depression of spirits.

When these, more particularly the local complaints, appear about the time that we might expect secondary symptoms, we can

have no doubt of the reality of the disease, and it behoves us instantly to apprise our patient of his situation, and also, whether we have explained ourselves before or not, to let him understand that all his present symptoms arise from the previous chancre of which we cured him, without being able to foresee or prevent the present consequences, which will, however, be soon remedied. There will then be no difficulty in composing his mind, after which it will be desirable to begin the course in the same manner as was described for the primary symptoms. If the situation of the patient renders this particularly irksome, the triturated mercury in pills, or rather bolus's, should be given as before proposed. The patient will find relief in his general health long before the mercury seems to have produced any effect on his constitution: the local symptoms, if they have not been of long standing, will also give way, though not so soon. It will still be advisable to continue the remedy, so as to keep the constitution or mouth, or both, for some time affected.

If, at the time these symptoms appear on the skin, ulceration should be formed in the throat, we shall have every reason to believe that both arise from the same cause; and if so, we may be certain they will be cured by the same process. But let us not on that account be inattentive to the most minute particulars in the appearance of the ulcer, in its progress and in the manner in which it yields to mercury. These are the cases to teach the young practitioner in particular, but the oldest will not lose time in marking the true character of an ulcer, which is so often submitted to his consideration. The character, as Mr. Hunter remarks, is very well defined; but it has certain varieties, and other ulcers will sometimes assume an appearance very similar: on this account no opportunity should ever be neglected of tracing such ulcers in the throat, whose syphilitic origin cannot be doubted, nor indeed any others which may lead to a comparison.

The venereal ulcer in the throat is always what may be termed a foul ulcer: though its edges are defined, its surface is always ragged and uneven, of a complexion which can never be mistaken for a clean or healthy sore, that is, for a sore disposed to heal. The pus is of various complexions, from the ash-coloured to the dusky brown. From the nature of the parts a scab cannot be formed, so that the ulcerous appearance is never interrupted. Its progress is more rapid than on the skin, as every action of inflammation, ulceration, or healing, is always more rapid in these very sanguiferous parts. It is rarely attended with pain.

Every other appearance, whether of mere inflammation, of suppuration after abscess, of spots, of coagulated lymph, which are called sloughs, and the separation of which produces a temporary excavation, or of superficial circumscribed redness, like excoriation, is certainly not venereal. But it does not follow that every ragged ulcer is such. I have seen more than one of this description, which has healed whilst I have been making up my mind whether I should salivate my patient. The only distinction I know between these and true venereal ulcers is, that the former are usually attended with more pain, the edge is also, for the most part, less defined, and the surface itself is more irregular; the fever, too, if any attends, is not such as we have described in syphilis. But the venereal ulcer is not always entirely free from pain, and there is generally some irregularity in its surface; the fever, too, we have remarked, is often slight. Happily this intricacy does not often occur, but often enough to teach us not to value ourselves on a hasty decision, when a little delay will be unattended with danger, and perhaps save our patient a tiresome and unnecessary process. By watching the ulcer attentively, we shall be able to observe whether it continues to spread regularly, though slowly, still retaining its character, and not healing in any part. If this should continue for a few days, we shall have no reason to doubt its syphilitic character; but if the pro-

gress is slow, there can be no harm in a further delay, the only inconvenience attending which will be the importunity of your patient. If, as is sometimes the case from the nature of the part, and the irritability of the constitution, the progress of the ulcer should be quicker, the character in all other respects well defined, and the history of the case leading to a similar conclusion, we may, by using every possible means of introducing mercury, easily accelerate our course. This will rarely be very difficult, because the same irritability of constitution which produces an ulceration more rapid than usual, is, for the most part, attended with a quicker susceptibility of the mercurial irritation.

The same directions are applicable, whether the ulcer is seated on the tonsils or uvula, or palatum molle, or any of the neighbouring parts, excepting the tongue; in which case the progress is slower, the edges consequently thicker, from the structure of the part, and the pain and inconvenience greater from the same causes, and also from its particular offices.

If we see the case early, it will not be necessary to keep up the irritation very long, nor often to excite it very high. But if these secondary symptoms have been of long continuance, the irritation must be excited as high, and kept up as long, as in the inveterate primary ulcer or chancre.

Mr. Hunter, we have observed, divided the parts, liable to secondary symptoms, into two orders; the first comprehending the skin and throat, with the soft parts about the mouth and nose; the second the fascia, periosteum and bones. The first we have already considered: and from the time that they have been cured in the manner above described, we may assure our patient that whatever appearances may occur, not only in the throat, but in any of these parts, are not venereal. If the skin has been long affected, it will frequently show spots something like those which arose from syphilis, and our patient will consequently be alarmed; but we need only

remind him that he has no hectic, and a few days will convince him that these spots are not permanent, like the former, but perpetually vanishing as often as they re-appear. These returns only occur when the eruptions have been small, numerous, and long continued.

But though we may venture to assure our patient that he will see no more of the disease in these parts, yet we cannot, with the same certainty, promise what may happen in the second order of parts. These are slower in all their actions; so that, though they may have been contaminated at the same time as the first order, they will not show the disease till several weeks after them. This uncertainty will therefore be greater, if our patient has applied to us, and been relieved as soon as the disease appeared in the first order of parts. On this account it may be thought better to leave the disease to take its course in the skin and throat, long enough to determine the state of the bones. But this would certainly be very improper, not only because it is probable the bones may not be contaminated, but because the situation of the patient is in the mean while very irksome, his disease in the skin or throat gaining ground upon him; and whenever we begin the cure, the mercurial course must be severe and long kept up, according to the length of time the symptoms have continued. If we have not seen him till the disease in the first and second orders of parts has become apparent, we must continue our course long, keeping up the irritation in proportion to that slow action which we have remarked in the bones, and which is not less slow in arriving at a cure, than in taking on the disease.

If the soft parts have been cured early, we cannot consider our patient as safe respecting the bones, till a medium of at least six weeks after our last mercurial irritation has ceased. Of this we should apprise him, and also how easily he will be relieved if ever such symptoms should occur. Nor should we be hasty in deter-

mining that every pain in suspicious bones is venereal; mercury alone will induce pains and even exostosis in these parts.

If, however, the disease occurs in the second order of parts after being cured in the first, we shall not have much difficulty in ascertaining it, because it will usually be ushered in by the same kind of fever as was the forerunner of the complaint in the first order. When pains in the bones arise from mercury, though the patient is always out of health, he has not that regular hectic which, for a few days, seems almost to counterfeit the intermittent fever. If, therefore, symptoms of hectic occur from three to six weeks, after the mercurial irritation, which cured the disease in the first order of parts, has subsided — if this fever is attended with pains much exasperated towards night—if these pains will not give way to common remedies, we may soon expect a thickening, or enlargement of the fascia, periosteum, or bone.

When the bones only are contaminated after the cure of a chancre, without the disease showing itself in the skin or throat, we shall have more difficulty in ascertaining the complaint. We must in this case be guided by the period, in which, from the use of mercury, the primary symptoms were cured, and by the hectic. If we have seen the progress of the disease from the original chancre or gonorrhœa, we shall have less trouble: but if the first we see of our patient is with pains in his head or shin bones, or even with a node, every enquiry will be necessary that may instruct us in forming our diagnosis. If an irregular mode of administering mercury has rendered every thing uncertain, we have no other remedy but in the delay of a few days, during which time every means should be used to restore the general health. We shall also, in the repetition of our visits, learn with more certainty all the previous circumstances in the history of the complaint and its treatment, which, with its present progress uninterrupted by mercury, will soon enable us to make up our mind.

If we should ascertain that the disease is venereal, the mercurial course should be commenced by friction; and as by this time we are pretty well acquainted with our patient's constitution, it is unnecessary to give any other direction than that the course should be pursued till the constitution becomes highly affected, and that such affection should be kept up for several days. This is absolutely necessary in all bone cases, for the reasons before mentioned.

After we have discontinued our course, as the patient recovers from its effects, he will sometimes complain of rheumatic pains. These are frequent in irritable constitutions after courses of mercury; but if those courses have been conducted in an irregular manner, such pains may be always expected. The patient will naturally consider them as the remains of his old complaint. Fortunately we shall have but little difficulty in determining whether he is right or not. If our course has been stopt too early, the node or pain will return on the same place as before, and we shall be convinced, that, whilst we have been anxious to save our patient from the effects of mercury, we have suffered him to desist before we have accomplished our object: all the former symptoms will return, and we have no resource but to return to our remedy, and to persevere *ut aliquid agamus*. But if the parts, for which we conducted our salivation, remain free from pain, we may depend upon it that the new pains are not venereal. They will frequently be at the joints, when the first pain was in the centre of the bone. This will be a further, but not a necessary confirmation: and however tedious the case may prove—however importunate our patient may be to recommence mercury—and however troublesome his friends or his friend's friends, it is our duty to be firm, as mercury, in any form, will, after a time, only exasperate his disease.

There are a few other complaints, which I shall now take notice of, as they have been considered among the secondary symptoms of this disease. These are warts about the genitals, and various

complaints about the anus. The first are never venereal, though, as was before observed, they may sometimes be the effect of a chancre, from the manner in which it heals. Warts, of every description, are much more common in the vagina, probably from the difficulty an ulcer of any kind may have in healing, if near the orifice, and often exposed to the urine. This interruption to their healing will produce various effects, but the most common is a soft wart, very red, and ready to part with a few drops of blood on a very slight pressure. The remedy is to touch them with caustic, and to defend them as much as possible from every interruption to their healing afterwards. Hard warts on the penis, or at the orifice of the vagina, should be examined, to see whether their basis is small. Whatever remedies are applied should be to this part, the prominent part being unorganized.

Rhagades, fici and condylomata were described long before venereal complaints were heard of. The first are, I believe, never the effect of lues. They are common, in warm climates, in several parts of the body, particularly about the fingers' ends in such constitutions as rarely perspire. I believe they never arise from any other cause, though the old authors used to ascribe them to leprosy, as the moderns do to lues; probably for the same reason, because some writer discovered them in a leprous, and another in a syphilitic subject. Before the introduction of syphilis, lepra was the common source to which cutaneous diseases were ascribed. Of late years, syphilis has more generally usurped its place. This made Professor Pitcairn, who lived at a period before leprosy was quite exploded, or before it was thought useless to consult the older writers, observe, that, since the introduction of the Neapolitan disease, leprosy was no more mentioned.*

* Lepra autem ante famam morbi Neapolitani hydrargyro cessit, nomenque nunc amisit. A. Pitcairnii de ingressu morbi, &c.

Mr. Becket, in his paper on the antiquity of the venereal disease, brings this passage as

There are a number of soft excrescences about the anus, to which various names have been given. They arise sometimes in consequence of a discharge from the rectum, stimulating the neighbouring parts to ulceration. If such ulcers are prevented from healing by the discharge continuing, or by the friction of the parts, they must either ulcerate deeper and wider, or the cuticle will send out processes to defend them. These, on account of the pressure they receive, grow in various shapes, from which they have acquired their names.

They will arise from a venereal origin in two ways. If a secondary ulcer is seated in these parts, that ulcer having no power of healing itself, will take the character, above described, from the nature of the parts. Sometimes, also, the matter of gonorrhœa, by falling from the vagina along the perinæum, will produce ulceration, and the same consequences follow. In either of these cases the remedy which cures the first disease, will cure these local complaints, or if they afterwards remain, they will no longer retain their syphilitic property, and may readily be cured by topical remedies.

To conclude this part of our subject—Where we have seen the whole progress of the case, and treated it in the manner above described, our difficulties will be inconsiderable. But it too often happens that, either from the inattention of the patient, his awkward manner of relating the history of his disease, or from some erroneous opinions of his first medical adviser, the case will be involved in much obscurity. This happens more commonly in the bones, where we have nothing to direct us but the patient's account, which, strange as it may seem, will be sometimes wilfully erroneous. Some patients are so determined to be poxed, or so much afraid of any

a proof that it was known, under the name of leprosy, long before the date usually assigned. But Pitcairn's observation serves only to prove the hastiness of writers, and the indolence of their copiers; and the turn of that writer's expression renders it very probable that such was his intention.

remaining spark of virus, as the figurative writers style it, that they are determined to be salivated at all events; and as most men are acquainted with the usual seat of the disease, when it attacks these parts they will assert that they have violent pains in the shin bones, the head, and arms, which are greatly exasperated by night. I knew a man of this description, who was in St. Bartholomew's for half a year, on account of 'sciatica and other rheumatic pains, and left the house without being much relieved. On enquiring all the particulars of his long case, he confessed that, having early in life imprudently caught the disease, he had never conceived himself free from it. That he had frequently taken mercurial pills; that, during the winter, he had been subject to rheumatic pains; and at last, being determined to go through a mercurial course, he had applied for admission to a hospital, the medical gentlemen of which he had deceived, by assuring them that he had intolerable pains in all his bones, particularly about his shins, his nose, and cranium: that he had been salivated in consequence to his heart's content, and was now convinced that his case was not venereal; but from that time he had been unable to move his right thigh, and was in constant pain about the hip joint.

Such, and many more, are the difficulties we have to struggle with in forming our diagnostic of this stage of the disease. For the whole of them, there are a few rules which we should always keep in view. If the patient has been using mercury in a manner reduceable to no regular conclusion, either from his own inattention, or from the mistaken notion of the practitioner, that it is better to introduce the remedy cautiously, and to stop as soon as he perceives any mercurial irritation excited, particularly if he has thought it right to give bark in order to check an irritation, without exciting which his patient never will be cured. In these cases we have no means but by laying aside either one or the other remedy, to wait patiently till we can ascertain the certain character of the disease, and then attack it accordingly.

As I am well aware that many, who have not traced the disease with any other guide than the accidental effect of those cases which have come before them, and the manner in which they have been treated—that many such have fancied they have discovered something like reasoning in the figurative language applied by some authors to what they term an insidious disease—in its retropulsion, retrocedence, and influence—shifting property—in the eradicating, exterminating, and antidotic properties of mercury—on these accounts some examination may be expected of what has been done by others since the former edition of this work: the object of which was to explain and defend Mr. Hunter's doctrine to such as had misunderstood or had opposed it. The frequency with which the work has been quoted, convinces me it was not overlooked. But no observations have come from the gentlemen from whom I took the liberty to differ, though most of them have since addressed the public. This part of the controversy must, therefore, be left to the decision of our mutual readers. Before we attend to what has been since written on the subject by others, let me offer what I have been able to collect of two other morbid poisons, which may illustrate still better the laws of the venereal, and place in a stronger point of view the necessity of that close discrimination which has been so often enforced.

CHAPTER XV.

OF SIBBENS OR SIVVENS.

(IMPROPERLY CALLED YAWS BY THE PEASANTRY IN THE WEST OF SCOTLAND.)

THE first account of this disease was published by Dr. Gilchrist. In the year 1771, it appeared in the third volume of Essays and Observations, Physical and Literary, by a Society in Edinburgh. It was previously read in 1765, and at that time printed in a sheet, for the benefit of the class of people to whom it was principally useful. On this account more pains are taken in enforcing the necessity of avoiding the disease, than in the description of it, the latter being probably well known among those to whom the paper was addressed. The description, however, is well attended to; and if the author had confined himself to those cases which came under his own immediate observation, there is no reason to doubt that the disease might have been ascertained with great accuracy. But Dr. Gilchrist, wishing probably to instruct his readers in every possible variety, has given all the accounts he could collect in addition to what he saw himself.

From his paper we may learn, that there were three forms of the disease. The first—inflammation of the uvula, tonsils, and neighbouring parts, with sometimes a " superficial ulceration appearing either raw, or covered with a white slough." By this language we may understand high inflammation, with apparent excoriation, or a crust of coagulated lymph adhering to the part. Most probably,

when the latter was cast off, the surface below exhibited that raw or excoriated appearance, not improperly called superficial ulceration. The same appearance is described through the whole roof of the mouth. " Frequently," continues the same writer, " there was a thrush, with white specks or sloughs upon the roof of the mouth and inside of the cheeks and lips, which commonly showed itself at the corners of the mouth, in a small rising of the skin, of a pearl or whey colour, where likewise a very small excrescence, or *fleshy sprouting*, like a rasp, often appeared, which turns into a scab, and is a pretty sure sign of the disease, although there be no sore throat."

This is a very exact description of the most common appearance of the throat and neighbouring parts. Dr. Trotter, of New Galloway, a gentleman to whom I was introduced on account of his great success in this particular disease, very aptly described these appearances as resembling a piece of toasted cheese. Every medical reader will be aware that these symptoms must be attended with an increased and somewhat altered secretion. This state of the disease I shall, therefore, consider as analogous to the gonorrhœal state of the venereal, only arising from a different morbid poison.

Another form of the disease is that of ulceration. " The uvula," continues Dr. Gilchrist, " has been destroyed by it. Children on the breast, seized with it, perished of hunger, not being able to suck or swallow." This form of the disease is analogous to the primary venereal ulcer or chancre, only the character of the ulcer is different, as we shall hereafter remark. Thus we see, that when the primary symptoms attack a secreting surface, or a surface with a thin cuticle, like the lips and inside of the mouth, or throat, the consequence is either an increased secretion or ulceration, exciting in both cases a secretion, which is capable of stimulating a part in another subject to a similar action. If this secretion is applied to a non-secreting surface, it must either produce no

effect, or excite ulceration. Such seems to have been the case with children, in whom the skin is more tender, in every part of the body exposed to come in contact with the poisonous secretion. In some the consequence is described to have been boils, producing ulcers in the arms, shoulders, face, legs, and feet, penetrating as far as the muscles, leaving them quite bare, and seeming to eat into them. All these, I suspect, were primary symptoms.

The following appears to me the author's description of secondary symptoms, in some patients, in whom the primary symptoms not having been more than an increased and altered secretion, were overlooked or easily removed. "When it affected the skin only, penetrating no deeper, or very little, it appeared," says Dr. Gilchrist, "in various shapes. The whole surface of the body has appeared mottled or flecked, of a dusky copper colour, or dirty red, as the discolourings of the skin in this disease commonly are."—"A cluster of small pustules come out, the skin grows dry and peels off, leaving a new tender skin beneath, and this will happen a great many times. Scabby eruptions were often met with in the scalp, forehead, inside of the thighs, and parts contiguous; where frequently small hardnesses, just within the skin, caused very troublesome itching. Inflammation, soreness, and excrescences about the fundament, seemed very frequent."

Dr. Gilchrist afterwards describes the tubercle resembling the raspberry, from which the disease derives its name,* but acknowledges he has never seen it in his own neighbourhood.—Many other appearances in this description are omitted, because they are such as are frequently found where no such disease exists, and which, as we have seen before in leprosy and the venereal, were probably at that time confounded with a disease which all were dreading, and consequently suspecting.

* "Sivven, in the Highlands, being a common name for a wild rasp."

Two years after this paper was read, and four years before the volume, in which it was printed, appeared, Dr. Adam Freer published his Inaugural Thesis de Syphilitide Venerea, in an appendix to which he gives some account of sivvens. He introduces it with a whimsical opinion concerning its origin. Producing Dr. Mead's paper, from the Philosophical Transactions, to prove that the itch is owing to an insect of the *acarus* tribe, and the authority of Hauptman, Langius, and others, to show that syphilis is also suspected to arise from a similar cause, the author suggests the probability that coition of the male syphilitic insect with the female itch insect, may have produced an hybrid race of animals the cause of sivvens. If one could suppose Dr. Freer in earnest, one might express some surprise that, as both the supposed parents of this disease were so well known in most parts of the world, they should never have met to cross the breed, till some workmen, who arrived at Galloway to build a bridge, brought with them syphilis, and met with the itch. I shall hereafter show, that how common soever the itch may have been in Galloway, the *acarus sabiei*, if it exists at all, is unknown, though very familiar in other places, where syphilis is no stranger, and where sivvens is unknown. However, we are obliged to this gentleman and his coadjutor, Dr. Hope, for the diligence with which they collected their intelligence from various parts. Imperfect as their description of the disease is, we may collect from it the following facts. That it was frequently spread by smoaking with the same pipe—that it is more easily cured by mercury than syphilis—and that some women have been permanently cured, without the use of any remedy, during the alteration that their constitutions have undergone in gestation or parturition.

Mr. Hill, of Dumfries, whose other writings have, I think, been too much overlooked, is the last writer I shall quote. This gentleman maintains, that the disease is the same as common syphilis, and has no difficulty in proving it in his own way. Instead

of offering a true diagnostic of each disease, and comparing them together, he contents himself with collecting certain symptoms, described by different authors as occurring in syphilis, and showing that similar appearances are described by others in sivvens. But even in this uncertain mode of reasoning, it is impossible to admit his position.

I shall select one instance, as the history of it has already been brought before the reader. " Dr. Barry, of Cork," he says, " gives an exact description of sivvens, under the name of the malignant lues venerea, communicated from ulcers in the mouth of a woman," &c.—I trust we have already seen that this disease was not venereal. It has a better claim to sivvens. But though we can determine what the disease was not, the account is not sufficiently minute to ascertain precisely what it was. The uniformly quick progress in the mothers, and fatality to infants, of which the author speaks, are much like sivvens: the case of the lady, too, who was exposed to the infection, yet escaped by being seized with confluent smallpox, is very analogous to the account of those women who were cured of sivvens by gestation and parturition. The appearance of secondary symptoms, also, about the pudenda, is such as is described in sivvens, though not in syphilis. But when the author speaks of some, in whom the disease returned after his severe salivations, we remain at a loss to know, whether those salivations were commenced before any secondary symptoms occurred, which makes it impossible precisely to ascertain the disease.

The only description of the disease, given by Mr. Hill, is in the mouth and throat. " When," says he, " the infection is not immediately received by the mouth, the sore throat is the consequence of an universal taint in the blood; in which case, the first redness and hoarseness are not easily distinguished from a slight stuffing or common cold; but when these symptoms are the effect of cold, they are either soon discussed, or bring on an inflammation, with a quick

pulse, which is not the case with sivvens; for in healthy or cold constitutions, it sometimes continues for weeks, nay months, without any great change."

"But," continues he, "when the infection is communicated by a foul pipe or spoon, the angles of the mouth, the lips, gums, &c. are first affected. The first appearance of an ulcer on the lip, exactly resembles a bit of fine white soft velvet pasted on the skin. But after it has eaten in for some time, it has the appearance of a piece of the red skin cut out, and a white velvet patch put in its place. These ulcers spread broader than deep, and the whiteness always continues, more or less, till the cure be completed, being sometimes as bright as the whitest paper, but more commonly yellowish."

Comparing this account with Dr. Gilchrist's and others', I should suspect that the only difference, in these two forms, depends on situation and a higher degree of inflammation. I should not thus venture to differ from one who had such repeated opportunities of seeing and curing the disease; but it must be admitted, that Mr. Hill does not venture to say that the general inflammation, he describes first, is subsequent to any other local affection; only that it is the consequence of an universal taint in the blood. It is, therefore, probably only that mild disease, described by Dr. Gilchrist, as inflammation of the uvula; and Mr. Hill's other form, the same as described by Dr. Gilchrist under the name of thrush, when in the mouth, and when at the corners of the mouth, "a small rising of the skin, of a pearl or whey colour."

From the above accounts, it appears that the primary local action of sivvens is either phagédæna, with all the rapidity that attends that ulcer, or else increased and altered secretion, with inflammation, sometimes so high, as to produce an effusion of coagulated lymph, forming a white speck, usually called a slough. The description of secondary symptoms, by Dr. Gilchrist, was very useful

for the purpose he intended, but not sufficiently minute for accurate discrimination.

Wishing for further information, as a means of assisting my enquiries into Morbid Poisons, Dr. Crawford did me the favour to apply to Dr. Paterson, of Air, whose answers are contained in a volume published by Dr. Beddoes, under the title of "Contributions to Physical and Medical Knowledge, chiefly from the West of England." Though all the proposed queries are answered in the order they were stated, yet, for want of completely understanding the precise objects of each, probably from some obscurity in my mode of expressing them, the result was not perfectly satisfactory.

Under these circumstances, I was induced to make a journey, that nothing might be wanted, as far as my own exertion could go, in offering an exact description of the disease. Before I begin my account, it becomes me to thank my brethren in London, and in many of the principal towns north-west, for their kindness in furnishing me with letters of introduction to all the medical gentlemen in Dumfries, Air, and the other counties to the south-west of Scotland, in which the disease is most known, as well as to Glasgow and Edinburgh; though an event, which could not be controuled, prevented my availing myself of the latter. But above all, I feel it a duty to acknowledge the obligations I owe to the medical gentlemen in Dumfries, and from thence to New Galloway, who, far from feeling jealous at my undertaking to enquire after a disease well understood by themselves, were ready to give me every information, and to listen patiently to all the prolixity of my enquiries and suggestions. I cannot omit mentioning, by name, Dr. Gilchrist, the son of the gentleman who first favoured the world with the account of the disease, and Dr. Maxwell, both residing at Dumfries. Besides the hospitalities I received from both, to the former I am indebted for introductory letters to Mr. Halliday and Mr. Milligam, surgeons at Castle Douglas; and to the latter for devoting much of his

time in conveying me to those gentlemen and to various other parts of the country, and enabling me to receive the attentions of the neighbouring families, particularly of his own relations.—Last of all, let me not omit this opportunity of expressing my gratitude to the Rev. Mr. Gillispie, of New Galloway, and his amiable family, who indulged me with the hospitality of his house during my stay in that part of the country, and furnished me with the means of making every necessary excursion, in which his son was my guide and companion. If I omit the names of Sir W. Douglas, Rev. Mr. Thompson, Rev. Dr. Burnside, and others, it is not that I have forgotten their attentions, but that I begin to recollect, how interesting soever these events may be to me, the reader can only wish to know the result of my enquiries. Imperfect as it may be, I have at least the satisfaction to find, that it is not contradictory to the remarks of the earliest and best informed writers, as well as the practitioners of the present day.

The first patient, to whom I was introduced, had lost the whole of her uvula and part of the tonsils—there are numerous foveolæ about what remains—her voice is affected, but not so as to excite a suspicion that any of the bones about the nose are injured. The loss of substance about the lips is very trifling, but the edges are covered with an opaque white cuticle, apparently newly formed. The complaint has remained with her now for nearly seven years, during which time she has frequently taken mercury, but never to any great excitement. She had at one time complaints about the anus, but these have disappeared long since, and never returned afterwards. At this time she is taking mercury, and her mouth is somewhat affected by it. She complains of some soreness in her throat, but as there is no appearance of ulceration, that soreness may be the effect of mercury. Every part seems healing without granulations.

This woman had evidently primary ulcers in her throat, and

probably only inflammation and increased secretion on the lips. When a sore of any kind attacks the lips, particularly the angles of the mouth, it is well known how tedious these parts are in healing, from being so perpetually interrupted by their necessary functions.

The next patient was a young woman, the daughter of a respectable farmer. She had fortunately the primary and secondary symptoms upon her, when I was introduced to her. She had been taking mercury for five days. The uvula and tonsils were suffused with a viscid mucus, aud in some parts covered with the white appearance before mentioned. The soreness extends from the edge of the lips along the inside of the cheek and side of the tongue; but if there be any loss of substance, it is only at the uvula and tonsils, which appear rather wasted than ulcered. The mucus is so glutinous as to keep the uvula atached to one of the tonsils. The secondary symptoms, she says, are mended. These are small elevations above the cuticle; she showed some on her legs and arms, which *seemed* pustular, but were quite dry. She says the skin breaks and discharges a little. She had fever on their first appearance, but it subsided soon after she began mercury.

A few days afterwards I made a second visit, and found her mouth sore, and all the symptoms mended. None of the pustules had broken, but all appeared paler, though still somewhat elevated. The throat cleaner—the loss of substance, if any, did not affect the uniform appearance of the parts. In a few days she recovered, having never been confined from her work, which at that time was getting in the harvest.

The third and fourth cases were a gardener and his wife. These had no other symptoms than increased viscid secretion, with inflammation and pain. They were soon relieved by mercury. Dr. Maxwell exhibits this remedy by making the patient inhale the fumes from a heated iron, through the tube of a gun barrel, which produces a surprising effect, with little trouble or waste of the mineral.

The first case I saw having exhibited the entire loss of the uvula, and the latter nothing that could with certainty be called an ulcer, made me exceedingly at a loss to account for the loss of substance. I could almost have fancied that the uvula and neighbouring parts were absorbed by a slow process, without ulceration. But the next case undeceived me in this particular. This was a young woman employed in the harvest, who admitted that she was accustomed to smoke with the peasantry, and with the same pipe. She had no symptoms but in her throat, and had taken no medicines for these. The sides of the cheeks, lips, and angles of the mouth, were all unaffected, but the uvula highly inflamed, and, to appearance, the upper part nearly detached. The part, however, was so smooth, as to give no appearance of ulceration; she was feverish. She began the fumigation immediately, and her mouth became sore in three days. The swelling and inflammation had then greatly subsided, which showed the throat more plainly. It now appeared that there was a hole through the velum pendulum palati, which was, however, perfectly clean, like the true phagedæna, with only a small quantity of mucus or pus attached to one part of the hole.

As this case was tolerably recent, it was evident that the ulceration must have been rapid, and, consequently, that the loss of substance could not be so gradual as I had suspected in the rest. By this it appeared that the former cases, excepting the first, were all mere cases of increased and altered secretion, or analogous to the venereal gonorrhœa, and that the first and this last described were primary ulcers.

The last case was of all others the most satisfactory. A poor girl had been ill about twelve months. Her first symptoms were mere sore throat, and treated as such with little or no advantage. At length the secondary symptoms appearing, with some fever, were at first suspected to be small-pox, till the slow progress of the pustules evinced the contrary, and a surgeon being consulted, at

once discovered the disease. From her situation she had not been able to persevere so regularly in her remedies as to produce a complete cure. Her throat exhibited only a dry scaly appearance in the left arch; whether it had ulcerated I cannot determine, only that the parts appeared to me as if diminished. She had several very hard dark coloured scabs in different parts of her body, cicatrices in other parts, and in others pustular appearances, like those described in the second case. On visiting her afterwards, some of the cicatrices were breaking out afresh.—She has all the symptoms of low fever, and an indolent superficial sinus appears on her neck. Whether these last symptoms were the effect of the disease, or of poverty, cannot easily be ascertained. On enquiry, she recollected complaints she had about the pudenda and anus, which had healed under the mercury she had taken, and which, she assured us, remained well.

From all the above accounts, it is evident that sivvens is different from the venereal disease, though approaching nearer to it than any other morbid poison with which we are acquainted.

The venereal gonorrhœa differs from the throat inflamed by sivvens, in the appearance of the discharge, and in the greater disposition sivvens shows to excite the effusion of coagulated lymph.

The ulceration differs,—the venereal being attended with callous edge and base, and sivvens consisting only of the clean phagedænic ulcer.

Secondary local symptoms differ,—the venereal retaining longer its copper appearance, and afterwards becoming more elevated, retaining more the colour of the skin, and the scab, when formed, being more scaly.

In sivvens, the *appearance* is very early pustular, though I never could detect pus under the cuticle. I should, therefore, conceive the pus still less in quantity than in syphilis. It is probably thinner, that is, more truly lymphatic, as it hardens into an irregular dark brown crusty or stony scab. There is nearly the same difference

between this and the venereal scab, as between the cow-pox and small-pox scabs.

Lastly, it is now universally admitted, that sivvens never attacks the bones but by spreading from the soft parts, and that it yields earlier to mercury than syphilis.

But though we are confirmed in the difference of these diseases, by the improved experience of all the practitioners from Dumfries to Air inclusive, and, I believe, even as far as Glasgow, yet I am far from asserting, that sivvens never appears among us as a primary symptom in the genitals. As it yields readily to mercury, it may have been confounded with the chancre. Nor is it easy to say, that none of the ulcers, described by Celsus, were the effects of sivvens. It is true, he was unacquainted with the medicinal properties of mercury; but the local remedies he proposes were of a stimulating kind, well calculated to excite a new action on the part; and if these were insufficient, he advises, as we have seen, amputation.

I need hardly repeat, that such a disease as this could not be so generally propagated by the genitals as the venereal. The ulceration is much too rapid, and attended with too much inflammation, to admit of coition; and we have seen that the mere altered secretion is attended, for the most part, with such violent inflammation, as to occasion an effusion of coagulating lymph. But there probably are stages of the latter, in which coition may take place, and those consequences may have followed, which have been remarked by Becket and others, which were once called leprosy, and have since been confounded with the venereal disease.

If it should be urged that the ancients describe no such disease in the throat or mouth, I have already observed, how frequently ulceration about the mouth must have occured, from the number of remedies proposed by Pliny; and it is worthy of remark, that the more accurate Celsus* recommends those stimulating remedies we

* Lib. 6. Cap. 18. Sect. 2. *de colis morbis.* "Eadem autem compositio tonsillis, uvæ tadenti, oris nariumque ulceribus accommodata est."

before took notice of, not only for such complaints about the penis, but also for an increased discharge from the uvula and tonsils, and for ulcers of the mouth and nose. The complaint in these parts, however, could not be so common, where spoon victuals were not so universal as in the northern part of this island, and at a time when smoaking tobacco was unknown. I may further remark, that so lately as the time of Erasmus, the custom of drinking out of the same cup was considered as more prevalent in England than any where else. To this observation Astruc, or his translator, adds a note,* which, however severe it may seem, at least shows some improvement among us in these days. But how just soever these censures may have been, the custom of using a single pipe for a whole family, and almost for a whole village, is, I believe, peculiar to some districts in Scotland. If Cromwell's soldiers introduced the smoaking of tobacco among the inhabitants, and if they afterward continued the practice, indiscriminately using the same pipes, it is not improbable that the origin of the distemper may be rightly dated, even if the soldiers were free from the complaint.

In Celsus's description of one kind of *ignis sacer*, we may, I think, suspect the probability of the secondary symptoms of a morbid poison. The remedies he proposes very much confirm this opinion. In the most obstinate cases, he advises cutting out the part below the ulcer, and further remarks, that an ephemeral fever is the most powerful remedy.†

* Where there is nothing but "here's to ye," and "here's to you again."—See Baroughby's Translation, Vol. II. Page 266.

† *Remedium fortissimum.* Other copies have *fortuitum*, which is equally to our purpose. We have seen that one of Dr. Barry's patients was saved from the effects of the morbid poison, he describes, by confluent small-pox; and that several women were relieved from sivvens by gestation or parturition. Such an irritation as the latter may be sufficient to supersede the effects of a morbid poison, which yields to so slight an exhibition of mercury. The following is the history of the last case I have described of sivvens, as well as I could collect it, after my departure.

Among the passages selected from different authors, to prove the antiquity of the venereal disease, are a few so pointed, that Astruc has thought it right to take notice of them. The first is from Salicet, of Placenza, who wrote in the year 1270, and has a chapter *de pustulis albis vel rubris, et de milio et scissuris quæ fiunt in virga vel circa preputium propter coitum cum fœtida muliere aut cum meretrice, aut ab alia causa.*—In this passage it is impossible not to take notice of the expressions, *pustulæ albæ vel rubræ et de milio*; and

Oct. 29th, she was inoculated for the cow-pox, never having had the small-pox. The same day she went to Dumfries.

Nov. 2d, I saw her at Dumfries, and the insertion had evidently succeeded.

3d, being the sixth day after inoculation, the redness and vesicular appearance had increased.

Nov. 4th, seventh day after insertion, it became stationary, or went back.

On that day I left Dumfries for Clifton.

From some interruption to my correspondence, which I cannot comprehend, I have been unable to collect the subsequent accounts from any medical gentleman who had the care of her, excepting Mr. Halliday, who vaccinated her, and saw her on her return, after some weeks stay at Dumfries. "I then observed," says Mr. Halliday, "a cicatrix upon that part of the arm where the vaccine virus had been inserted. It was of the usual size, and surrounded by a brownish coloured disk. It is proper to remark, that a number of cicatrices, of a similar size and appearance, were visible in various parts of the body and extremities, which were evidently the effect of secondary blotches of sivvens.

"So far as I can learn, the vaccine disease, after proceeding in the usual course for some days, became stationary for about five or six, and then advanced and terminated in the ordinary manner. As it advanced, the symptoms of sivvens gradually declined, and for some time entirely disappeared, without any medicine having been used. As the disease, however, returned in about six weeks, it was found necessary to remove it ultimately by mercury."

The reader, whilst he feels grateful to Mr. Halliday for this communication, will sympathize with the author of Morbid Poisons, that no more minute history of this interesting case could be procured. From the above, the fidelity of which cannot be questioned, we may learn, that so slight a complaint as vaccination, was sufficient to procure a suspension for six weeks of a disease, which, for twelve months before, had kept possession of the skin and the constitution. Had the latter been more recent, or vaccination a more violent disease, the relief might have been permanent.

also, that though among the modes of catching this disease, intercourse with improper women is considered, yet no suspicion is expressed that these women had the same complaints.

The next writer is Landfranc, of Milan, in 1290, who, describing various kinds of ulcers to which the *virga* is liable, speaks of one which may arise—*ex commissione cum fœda muliere, quæ cum ægro talem habente morbum de novo coierat.*

In this passage it is still more remarkable, that, though the author is fully aware of contagion as a cause, yet he more than tacitly admits that the disease is not known in the vagina.

In 1300, Bernard Gordon describes similar complaints, and refers, among the various causes, to intercourse with women—*cujus matrix est immunda plena sanie aut virulentia, &c.*—Here we see the disease traced indeed to the matrix, but no suspicion of the vagina.

In 1400, Valescus, of Tarenta, says—*Pustulæ virga fiunt si quis cöeat cum fœmina habente ulcus in matrice.*

But the most remarkable of all is De Argellata, of Bologna, who has a chapter *de pustulis quæ adveniunt virgæ propter conversationem cum fœda muliere, quæ albæ sunt vel rubeæ.* Here we see again the red and white appearance, so particularly noticed in sibbens, described in the penis, supposed to be derived from the vagina, but without any hint that the latter is affected in a similar manner. The proposed remedies are what are called detergent and styptic lotions, which are said to be useless, unless applied with some violence, *nisi corruptione illa remota & loco absterso.*

There is no reason to believe these complaints were uncommon, because they occur so rarely in the writings of the ancients. Celsus observes, that the more respectable professors of the art among his countrymen were too delicate to write on such subjects; but that he conceives it proper to instruct the world in the treatment of those parts—*quæ invitissimus quisque alteri ostendit.* Mr. Becket gives, from a MS. in Oxford, the cases of two public characters, who died

putrefactione membrorum suorum et corporis, causata per frequentationem mulierum. Probably the causes of their death would have been for ever unknown but for this MS. What is very remarkable, neither the writer of it, nor the sufferers themselves, though acknowledging that their disease arose *per copulam carnalem cum* MULIERIBUS, express the least suspicion that they contracted it from a woman affected like themselves, but only by a too frequent repetition of their indulgences. As the MS. was written by the chancellor of the university, who was not a physician, we cannot exactly ascertain what is meant by *putrifactione, &c.* It is plain, however, that the disease was not sufficiently acute to kill by mere mortification of the parts first infected: and as the use of mercury was still unknown, sivvens, unless treated as directed by Celsus, would have been sufficient to produce all the effects described.

The last authority I shall quote is a paper mentioned by Dr. Beddoes, in the Contributions above referred to. "J. G. Klein *Tranquebarra*—Dani Spec. inaug. de morbi venerei curatione in India Orientali. Hafniæ, 1795." In this paper the author speaks of the venereal disease as having been known, under the name of Moecha Wiadi, for time immemorial in the East, and cured by mercury. I have not seen the paper, but as Dr. Beddoes expresses a wish that the symptoms should be more satisfactorily described, till that is done we can only remark, that, for more than a thousand years past, the Asiatics have been acquainted with a disease in the genitals, which yielded to mercury.

The inference I shall draw from these facts is—ulcers in the penis were known to Celsus which required a very stimulating treatment, and others which were incurable, and for which he advises amptutation.

Similar complaints were described before the venereal disease was known, in countries where the medicinal effects of mercury were unknown; and though stimulating remedies were locally used,

probably in some cases with success, yet there is no doubt that the disease was frequently fatal.

In the East Indies, such a disease has been long known, for which mercury was a specific.

None of the accounts which we have been able to examine, describe similar complaints in the vagina, nor do the writers of those days seem to suspect them.

It was shown, in the early part of this work, that the secretions of one person, apparently healthy, or very slightly altered, may prove deleterious when applied to another; and by Dr. Barry's, and some other accounts, it further appears, that a disease, originating from such a cause, is capable by contagion of exciting a similar disease in another person, which may be universally propagated—that such a disease has proved quicker in its progress than the venereal, and has yielded to mercury.

May we not from hence conclude, that mankind must always have been liable to such complaints in parts which come in mutual contact; and if, before the appearance of syphilis, their contagious nature was not suspected, was it not because such diseases were less common, because they appeared in the ulcerated form in the genitals of *men* only, and because, from the violence of the symptoms in these parts, they could not often be propagated?—Hence, if any such disease should find its way to a part, the secretion from which may be conveyed to another person by a pipe of porous clay, or any such means, its spreading would be in proportion as such a custom prevailed. And even should the symptoms be so palpable, that no one would receive a pipe from an *infected* person, yet from what has been observed of the origin of such a disease, it may be perpetually revived, as long as such a custom remains unchanged.

From all this, there is at least a probability that sivvens may be among the *morbi in cole* mentioned by Celsus; that it may have been noticed by different writers among the complaints of the *virga*;

that since the introduction of the venereal disease, it may have been confounded with it, when met with on the genitals; and lastly, that it may be among those morbid poisons which are sometimes excited by applying to one person the secretions of another apparently healthy, or not labouring under a similar disease. It remains for those gentlemen, who have constant opportunities of watching the disease, to confirm or rectify these suggestions.

Though it is possible to form some comparison between the secondary symptoms of sivvens and a wild unripe raspberry, I very much doubt whether the disease has not derived its name from some other complaint in the Highlands, in which the similarity is greater; or perhaps in that part of the world the word *sibbens* may be applied to any eruption with scabs. I am the more inclined to one of these opinions, because neither of the writers who have had the largest opportunities of seeing the disease, nor the present practitioners whom I consulted, insist upon this appearance. So little is the name regarded in Dumfrieshire among the poorer class, with whom one might expect the ancient language to be most familiar, that the only term they are now acquainted with is *yaws*; a word they must have derived from such sailors as have returned from the West Indies.

But our present business is to fix the character and mode of cure of sivvens. In the former, I trust, we have so far succeeded, as to show the manner in which it differs from syphilis. The mode of cure only differs in yielding more readily to the same remedy, which is admitted by all who have the best opportunities of judging. A general opinion prevails that the disease is now but little known. This is true of the larger towns. But it is very much because the village practitioners, being better educated, and their number multiplying with the increased wealth of the country, it is no longer necessary to seek relief at a greater distance.

Dr. Swediaur, and, after him, Mr. Bell, mention a disease in

Canada, in many respects similar to sivvens. Dr. Swediaur tells us his account is taken from the papers of Governor Hamilton, on whose representation six surgeons were sent from England, to give gratuitous assistance to the poor sufferers.

It is probable many of the symptoms described were unconnected with the disease, not only because no morbid poison can assume such a variety of forms, but because such appearances often occur from other causes. Among the rest, we meet with mortification of the feet and other joints, which, in the north of Canada, would be more properly suspected to arise from frost bitten. This inaccuracy is not to be wondered at, as the account was not taken from the papers of a medical man. The only professional name quoted is Dr. Bowman.

Ten years ago, when the first edition of Morbid Poisons was printed, I made every enquiry after Mr. Bowman (as he is called by Mr. Bell), but his name was unknown to the booksellers and medical literati. On the present occasion my enquiries have been equally unsuccessful among all those mercantile gentlemen who have connections in Canada, among those public officers concerned for the colonies, and particularly for Canada, and also such medical gentlemen as would be likely to be consulted, before a mission of six surgeons should have been sent from England.

If such an event really happened so lately as 1786; and if, as Dr. Swediaur observes, in the year 1785 five thousand eight hundred persons in Canada were reported infected, besides those who concealed their complaints, it is scarcely possible that all these enquiries should have been unsuccessful. Indeed, it is evident that there must be some inaccuracy in Dr. Swediaur's account, as no gentleman of the name of Hamilton has ever been governor of Canada. However, there is sufficient internal evidence that some disease, very similar to sivvens, has appeared at the Bay of St. Paul in Canada, and probably from similar causes.

The friendship of Dr. Pitcairn has favoured me with a MS. and with the following history. About twenty-five or thirty years ago it was put into his hands, with other papers of natural history, by the Hon. John Cochrane, who had resided several years as a merchant in Canada, and was, I believe, one of the council of the colonial government. It was said to be written by a Mr. Beaumont, a French surgeon, who had been sent from Quebec by Governor Haldiman to investigate the nature of a distemper in the Bay of St. Paul. Dr. Pitcairn copied the MS. which contains the description of a disease in many respects similar to Dr. Swediaur's account, and also to sivvens. In one respect they all agree, that the contagion is often conveyed by smoaking with the same pipe.

There is, therefore, reason to believe, that such a disease has existed in the Bay of St. Paul in Canada, though probably the account has been greatly exaggerated. Nor is it impossible, when we perceive that Dr. Swediaur writes Hamilton for Haldiman, and Bowman for Beaumont, that the surgeons may have been sent from Quebec instead of London. Here I shall, however, dismiss the subject, having only introduced it to show the difficulty that attends an enquiry into the validity of an event, which appears to be supported by official documents, and which must be highly interesting to all, and we should suppose have been submitted to the consideration of those medical men who reside in the metropolis.

CHAPTER XVI.

OF YAWS.

HAVING had an opportunity of tracing this disease in a white subject, from the previous fever to the beginning eruption, and through its whole progress to cicatrization; having at first watched the symptoms with much anxiety, in order to discover the disease, and afterwards, as well to ascertain its precise laws, as to relieve the sufferings of my patient; and having submitted my remarks to the London Medical Society, I shall begin with transcribing a paper, which they were pleased to distinguish among their prize essays.

Read JUNE 30, 1800.

IN the latter end of September, 1798, a young Danish nobleman, in the naval service of his king, was left at Madeira, his health not permitting him to continue his voyage to the West Indies. He complained of a slight, but tolerably regular intermittent, attended, during the paroxysm, with a spasmodic contraction of the fauces. For this he found some relief in bark, the power of opening his mouth returning as the fever went off. This relief was only temporary; nor were other tonics attended with better success. He took a grain of calomel daily for ten days, during which his mouth became slightly affected, and his fever completely left him. It, however, returned as his gums recovered, and recourse was had to sal ammoniac, myrrh, and chamomile. These seemed to succeed after five days use, but the remission of fever was attended with an universal pimply eruption over the face, and inflammation of the throat.

In the space of two days the eruption was universal, excepting on his hands and feet. The throat became ulcered, and the glans penis was covered with several small, but not painful ulcers. For several days the ulcers spread on both these places, and the pustules continued to increase: his fever returned, and the throat and pustules of the face were attended with considerable pain. The latter had arrived, in less than a week, to the size of small-pox pustules; and the interstitial part of the skin being redder than usual, gave the whole very much the appearance of that disease. The figure of the pustule was indeed somewhat flatter, or rather more horizontal, the edges rising perpendicular from the skin, and the surface being plain. There wanted also that appearance of indentation which very soon discovers itself in small-pox, and frequently in the chicken-pox also. The throat had precisely the appearance of a venereal sore throat, but was more painful.

The continual increase of the pustules after those of the small-pox should have scabbed, precluded all doubts concerning that or any similar morbid poison. In order, however, to satisfy myself still further, I had removed the pellicle from one of the pustules, and, instead of the slough peculiar to the small-pox, found only a rough sloughy surface attached to the subjacent cellular membrane.

By the application of *aq. phageden.* the ulcers on the glans penis soon became stationary, put on a better appearance, and healed. It was therefore evident they were not venereal, and as there was every reason to believe the pustules only differed from these ulcers in being covered by a thicker cuticle, it was highly probable that the cure of them also was within the power of the constitution. It seemed therefore prudent to watch the disease, without attempting to interrupt it.

The eruptions continued to increase in number and size to such a degree, that the soreness, abstracted from the pain which was confined to those in the face, rendered life scarcely tolerable. Be-

fore the end of the month, my patient counted, beside a number of smaller ones, fifty-six large sores, some of which being of an oval form, were not less than from two to three inches in their largest diameter. He was beside this very much reduced in flesh; for though his appetite was throughout the whole better than could be expected, yet the state of his throat prevented his swallowing even liquids without very great pain. From this time all the pustules began to scab, and no new ones appeared. If, therefore, the disease was yaws, which I had long suspected, the present seemed the time at which, by the concurrence of most authors, mercury may be exhibited to advantage. The corrosive sublimate, though given in very small doses, was more than the stomach would bear: it was therefore given up almost as soon as tried; and, as most of the sores showed a disposition to scab, no other preparation of mercury was exhibited. The fever continued all this while with as much violence as before, and the state of the throat preventing the exhibition of bark by the mouth, it was given by clyster with an opiate. Though at first it did not lessen the fever, yet the general irritability of the constitution was much abated, and by degrees the fever subsided.

The following had been the appearance and progress of the pustules: The cuticle shrunk and hardened into a scab, with the pus underneath it. In this manner it remained for a few days, when a suppuration commenced at the edges of, and under the scab, from which matter issued, and either raised part of the scab, or was diffused over it, or attached to its circumference, and hardened upon it. This was repeated an indefinite number of times, and each time attended with a return of fever. If any hairs were in the neighbourhood of a pustule, they were so incrusted with pus as to make them appear white. The accumulation and hardening of matter over the scab, gave some of them the appearance of being studded with tubercles: in others, the accumulation was more regular, so

as to exhibit a horny appearance: the colour was, for the most part, of a light brown; but where blood was mixed with the pus, the complexion was redder. Wherever the inflammation ran high, the pus was thinner, and never hardened on the part. This happened only to a few pustules of the face.

In about two months from the first appearance of the eruption, the fever returned with as much violence as before. The eruption, particularly about the face, continued dry, and, in a few days, a slight separation of the edges of some of the scabs showed a sound skin underneath. I had therefore no doubt that the return of fever was symptomatic of a new action taking place, namely, that of desquamation and the formation of new skin, as the former fevers had been symptomatic of eruption and suppuration. This disposition to skin was not universal, some of the pustules still continuing to extend in the manner before described. They were however few, unattended with pain; and the new suppuration, instead of extending round the whole scab, was confined to an inconsiderable portion of the edge. On removing any of the scabs, a fungus, covered in some instances by a thin cuticle, was found shooting out of the foveolus, that had been the seat of the slough.

The ulcer on the throat had made such ravages, that I was fearful the whole uvula would be destroyed. The left side was entirely lost, and what remained, instead of hanging in its natural form, was drawn by its muscular fibres to the arch formed by the right tonsil. The voice was as much affected as when the uvula is lost by a venereal ulcer. As the other symptoms abated, however, the throat recovered nearly its original figure, and the voice its natural tone.

Though the violence of the fever had now subsided, yet it returned occasionally, and was constantly attended with a new suppuration in some of the pustules, or rather at a small point in

the edges of some of them. The paroxysms lasted, at intervals, for three or four days; during which, however, the appetite continued; and, at the remissions, was that of a convalescent.

At the end of six months from the first symptoms of fever, and four from the eruption, most of the pustules, particularly those on the legs, and many about the body, had scaled off, the throat was nearly well, and this succession of symptoms was to be ascribed to the resources of the constitution, which was only supported, without any attempt at altering its actions.

I was unacquainted with yaws but by description, and my patient had been ten months absent from the West Indies before he felt any indisposition. During his stay there, all he recollected that might have exposed him to the disease was, that being once with a physician, who was visiting the sick negroes of a plantation, he was desired to keep at a distance from one of them. It was therefore fortunate, that enough of the pustules still remained in a state of partial suppuration, to satisfy Dr. Wright, who at this time touched at our island in his return to the West Indies, that the disease was truly yaws. As it was of the most violent species, the pustules being large as well as numerous, it may be right to distinguish between such symptoms as are peculiar to this state of the disease, and such as are common to all. If the fever was greater than usual, this variety is not greater than the secondary stage of the venereal disease, which is often attended with fever, counterfeiting the intermittent. This sometimes goes off as the eruption appears, and sometimes the eruption is preceded by no fever. The spasm on the fauces must be considered as an accidental circumstance. The disease does not usually attack the throat, but instances of it are mentioned. I have before remarked, that its appearance no way differed from the venereal ulcer on that part. The surrounding inflammation was indeed more considerable, continued longer, and

the pain was much greater. The following I consider as the genuine character of the disease, by which it may be distinguished from all other morbid poisons.

I have already remarked the figure of the pustules in their early stage. If at this time the cuticle is taken off, you find under it a rough whitish surface, consisting partly of slough, and partly of living animal matter. This I conjecture, because, on the closest observation, the pus appeared formed over the surface, and not at the edges only. Where the cuticle is left, the matter usually pushes out at the extremities, but so slowly as only to harden, upon it. Suppuration is not constantly going on in any of the pustules, excepting where the inflammation is very high, as was the case in the face. In other parts, some pustules appear to suppurate at one time, and some at another, but commonly several at the same time. Hence the drying and hardening of a scab is no criterion that ulceration has ceased, and that the part will skin. Even the same individual pustule would heal in one part while it spreads in another.

From the time, however, that the scabs begin to dry, may be dated the second stage of the disease; and if, at this time, you break off the scab, you find a red fungus shooting up under it.

There were no pits left, after healing, in any part but the face. In these the inflammation was so violent, and the matter so thin, that, instead of hardening under or upon the cuticle, it proved more than any superficial dressing would absorb. Hence, instead of a scab, we had an open sore, and instead of a fungus shooting up, the part skinned over, without any previous granulation. This is analogous to what I have observed in small-pox, and all other morbid poisons.

To give the character, then, in as few words as possible:—If in the early stage of the pustule you remove the *cuticle*, you are to expect a ragged but moist slough. In a later stage, if you remove

the *scab*, you will find a fungus, varying in shape, size, and colour, according to the period of the yaw. Where the inflammation is very high, you will neither have scab nor fungus; but when suppuration ceases, the part will skin over, and leave a pit.

Authors have generally remarked, that all the hairs near the yaws turn white. This was not permanent in my patient. He had, indeed, no hairs near any of the pustules that granulated, excepting at the eye brows, which recovered their colour on being washed. On the face, where pits remained, the hair was not renewed for more than two months. The few that then grew were of the natural colour. This patient had the fair complexion of the natives of a northern latitude. It is probable that, in darker people, the hair may turn white, as we know it sometimes does after cicatrices from common wounds in the head, and as frequently happens in the black hair of other animals.

Having thus described the character of the disease, I shall offer a history of it, founded on observation, and leading to practice.

1st. The violence of the disease must be in proportion to the susceptibility of the constitution for it.

2d. When the susceptibility is great, it is likely to be proportionably permanent, and, as long as it continues, the matter of each pustule will infect those parts it comes in contact with. Hence the spreading of the same individual pustule or scab.

3d. Nothing will destroy the susceptibility of a part, or the whole constitution, but its full action. Hence,

4thly, Though the action may be suspended for a time, by exciting a different action, yet the susceptibility still remaining, the action will return as soon as that which superseded it ceases.

To illustrate this theory the better, I shall contrast yaws with the only two morbid poisons to which it bears any analogy, without exactly resembling either.

The venereal is a poison of which the constitution is for ever

susceptible, and which it has no power of curing in itself: consequently, the disease is kept up for ever, or till a more powerful stimulus is applied; after which, on a fresh application of the contagion, the constitution is found as susceptible as before.

The small-pox is a poison of which the constitution is no longer susceptible, after having gone through a certain fever and eruption, occasioned by the application of its poison. Consequently, from this time, all the pustules heal without spreading, and the constitution is found to have lost its susceptibility on a fresh exposure to the contagion.

Of yaws, on the contrary, the constitution remains susceptible after the eruption and fever are completed. Hence, as in the venereal disease, the action continues. But this susceptibility only continues for a time, uncertain according to the difference of constitution, or state of it at the time. When the susceptibility ceases, the parts heal as in the small-pox, though more slowly, from the slow progress of all the other actions. When healed, the constitution has for ever lost its susceptibility of the disease.

In one point they all agree, namely, that they may be suspended for a time by another more powerful stimulus, but will show themselves as soon as the effect of that stimulus ceases.—When it happens that the constitution is infected by absorption from a local venereal ulcer, the consequent disease will never show itself while the mercurial irritation which cured the chancre continues; but when that irritation ceases, the disease will appear in the skin, fauces, or bones.

In the small-pox, inoculation has taught us, that, after the contagion is received by a constitution susceptible of its impression, the disease may be for a time superseded by some other irritation, most commonly an erysipelatous fever, or the measles. As soon, however, as they cease, the small-pox resumes its action, and continues its course.

In yaws, the progress of which is particularly slow, it appears that, even after the disease has proceeded to suppuration, it may be superseded for a time by mercury. But if that remedy has been applied, before the full action of the disease, namely, scabbing has taken place, whenever the mercurial irritation ceases, the yaws resumes its action, which it continues as long as the constitutional susceptibility remains.

To conclude, the constitution is always susceptible of the venereal poison; so that the disease will spread till superseded by a more violent irritation; and return on a fresh application of the poison. The small-pox will cure itself as soon as the suppurative fever is over; and, with the disease, the susceptibility for it is extinguished for ever. In yaws the suppuration, whether attended with fever or not, does not immediately relieve the constitution from its susceptibility to the disease; nor is there any remedy yet known that will cure it. But this susceptibility ceases by degrees, after which the parts heal, and the susceptibility never returns.

Though only the venereal is absolutely incurable, excepting by a remedy, yet all three may be arrested, at certain stages, without being cured.

That such is the nature of yaws, appears from every authority I have been able to find. The anonymous and modest author of a paper in the Edinburgh Medical Essays, Dr. Hillary, and Sauvage, all agree, that if mercury is given before all the yaws are scabbed over, the best that can happen is a return of the disease when the mercurial irritation ceases. The two first mentioned writers (the only two practical ones) never seem to have left the disease to its natural cure, and differ much in their opinion of such a practice. Hillary, with his usual hastiness, conceives it would always kill the patient; but the other has the modesty to believe it would probably get well of itself, though he never had the courage to try.

Such was the case with the Count, to whom mercury might be said never to have been exhibited after the eruption appeared, or to have been exhibited so slightly, as to produce no effect. When the scabs began to fall off, they continued to do so, several every day; but, on the arms, some remained, which it was found necessary to rub with caustic. Even these would sometimes ulcerate afresh. Perhaps they were not sufficiently cauterized. They were at last left to themselves, and healed spontaneously.

I cannot conclude this paper, perhaps already too long, without an inquiry whether the leprosy of the Jews was yaws or not. In these remarks I shall confine myself to the English translation, as much more pointed in characterising the disease, though there is nothing in the Septuagint that contradicts it.

" When," says the divine lawgiver*," " a man shall have a rising, a scab, or a bright spot;"—this constitutes the early stage, when the pimple is just forming, or the cuticle broken off, so as to discover the slough underneath, splendid from the exudation of a small quantity of matter, or when the matter begins to harden into a scab.—In the second stage, viz. when the loss of the cuticle and contraction of the slough has showed a depression, so that the " plague is deeper than the skin," and the matter had encrusted the " hair, so as to turn it white," the disease was considered as beyond a doubt, and the subject pronounced unclean. But if no loss of substance appeared, and the spot was not lower than the sound part, nor the hair altered, the subject was to be shut up for a week, at the end of which, if the disease was stationary, seven days more were required. If the disease now became a dry scab, of a darker complexion, without having spread, it was to be considered a common scab, and the subject re-admitted into the camp. But if the scab had increased between the first and second examination, the subject was to be again secluded for a future examination, and if it

* Levit. chap. 13.

then appeared that the scab continued to spread, he was to be pronounced unclean.

In the next stage, the excrescences being all white, and the hair also, the priest was directed to look for the red fungus underneath, and, if he found "quick raw flesh in the rising," he was to consider the disease an old [or confirmed] leprosy, and exclude the patient accordingly.

Lastly, he was to determine when the patient might be considered as no longer infectious, and re-admitted into the camp. For this purpose every part of the body was to be examined, and if all the cicatrices were covered with a new cuticle, which, in this case, will be opake and "white," the subject was to be declared clean; but if any of the fungus flesh remained uncovered, or covered only with a thin transparent cuticle (in both which cases the appearances will be that of "raw flesh"), the priest was directed to seclude the subject, till, on a future examination, every part appeared white, and then, after the necessary ablutions, he was to be re-admitted into the camp.* The rest of the passage relates to an old cicatrix, or an open sore of any kind, becoming the seat of a leprous pustule.

The above description exactly resembles yaws, and no other disease now known, in every material point; in the vesicle, or pustule; in the depression from the loss of the cuticle, and the contraction of the sloughy part beneath; in the subsequent fungus; in the disposition of the scab to spread; in the changing the hair to white; in the infectious property of the disease; in the slowness of its progress; and, lastly, in the constitution being equal to its cure. In most of these it differs from elephantiasis† or the Arabian

* Dr. Mead supposes, that this last passage, ver. 12 and 13. relates to a different disease: but there is no foundation for such an opinion; and whoever carefully compares Dr. Mead with the text, will be convinced of the fallacy of the Doctor's arguments.

† A description of this disease will follow in the progress of the work.

leprosy (a disease well known in Madeira), and, in many particulars, from the *lepra grecorum*.

It is most probable, by the prudent measures pursued during the long march through the wilderness, that the yaws was altogether extirpated from the Jews before their arrival in Palestine. For though leprosy is spoken of in other passages, it is evidently a different disease. The cases of Moses and Miriam are not to our purpose, as they were both antecedent to this event. The case of Naaman (2 Kings, chap. v.) was evidently different. As his disease will presently come before us, at present I shall only remark, that it was incurable by any human means, or by the powers of the constitution. That the universal whiteness, which was considered as a symptom of recovery from the former disease, appears in his to characterize its worst stage. For, after the malediction of the prophet, his servant "went out a leper, as white as snow." Thirdly, his disease does not appear to have been contagious; else Naaman would not have spoken of the King his master as "leaning on his hand" (by which he must at least have been near his person) when performing his devotions, or "worshipping in the house of Rimmon." Lastly, there is no intimation that Naaman's wife, or any of his family, were infected, or that his servant was apprehensive of being so.

It is, however, probable, that the two diseases, having the same name, might be confounded, and, when one of them had disappeared, the custom of seclusion might be continued for the other. But this will be better understood when we attend to Elephantiasis.

My constant visits, sometimes repeated two or three times a day, in the above case, gave me an opportunity of examining every appearance with peculiar exactness, and the particular bent of my

enquiries probably would induce me to improve these opportunities as much as possible. I was, however, much disappointed in examining the only works on the subject, to which my situation in Madeira gave access, to find so many particulars unnoticed. None of the writers I could refer to, make mention of fever. This made me doubt whether such an occurrence in my patient, was not the effect of a greater irritability of constitution than might exist in the negro slave, whose only concern must be to render his situation as tolerable as possible. Dr. Moseley, however, in his Treatise on Sugar, admits two kinds of yaws: the Common, and the Running Yaws. The former, he says, is without fever—this leads us to presume that the latter is attended with fever. Probably the difference between the two depends on their comparative severity, and perhaps even with the running yaws, the common may exist in some parts of the body, as we find in small-pox, that the eruption shall be confluent in the face, and distinct in other parts. It has been already remarked, that in my patient, where the pustules were attended with little inflammation, the matter was so little in quantity and so thick, as to harden on the scab, but in the face, where the inflammation was higher, the discharge was greater and thinner.

Dr. Dancer, in his " Medical Assistant," observes, that yaws has been compared with the small-pox and other exanthemata; but that there seems little analogy between them. " The small-pox," says he, " and exanthemata are produced through the medium of the air, as well as by inoculation—small-pox and exanthemata are preceded by fever, which is not the case with yaws—small-pox and exanthemata have a determined course of only a few days, while the yaws may, by different modes of treatment, be protracted or cut short. The resemblance, then, appears fanciful, and leads to no practice."—It would be very desirable to ascertain with certainty that yaws can never be communicated by the air. However, Dr. Dancer seems almost to admit fever, when he says, " Yaws

seldom make their appearance without previous indisposition—pains in the bones, languor, and dry skin." That yaws has its course as well as small-pox, is universally allowed, though that course is much longer, and consequently liable to more interruption. But it cannot be admitted, that this, or any other fact, leads to no practical inferences. Had it been universally known that yaws runs through a certain course, the surgeon, mentioned by Mr. Hunter, would not have attempted to shorten it, by an *immediate* exhibition of mercury, as soon as he conceived that he had inoculated himself. Two army surgeons, to whom I communicated the result of my case, and who had both treated the disease in the West Indies, were under a similar error. One of them conceived he had been unable to cure his patient, because he had not an opportunity of beginning his mercurial course early enough, and the other acknowledged he had killed his patient, a private in a black regiment, by attempting to cure him with mercury. I need only add, that Dr. Dancer admits mercury is productive of more mischief than good, when given early in the complaint. This is enough to show the necessity of watching the *course* of yaws.

It is true Dr. Cullen places frambœsia among the *impetigines*. But we should recollect, that the illustrious Professor, in a note, tells his reader, he had never seen that disease nor elephantiasis. How much it is to be wished that we should all be as careful to distinguish between what we have seen, and what we describe from report.—The next writer I shall mention corrects this error, and explains the reason why the disease has hitherto been so imperfectly described.

Dr. Ludford, a native of Jamaica, has made yaws the subject of his inaugural dissertation. Before he gives his definition, he remarks—" Celeberrimus Cullenus nuper defunctus—frambœsiam ad ordinem, impetigines retulit. Hoc tamen m nus recte factum videtur. Ad *exanthemata* enim potius pertinere videtur. Quia *variolæ*

instar, suam accessionem, statum, declinationem habet, a febricula aliqua, nunc levi, nunc graviore, incipit."

This writer afterwards adds, that the master yaw always leaves a cicatrix. Such was the case with the yaws in the face of my patient, the inflammation of which was the greatest. He also remarks, in contradiction to Hillary and some others, that the hairs do not become white. I suspect the error of his predecessors has arisen from the cause mentioned in the account of the case under my care. The particular situation of Dr. Ludford, in the island of Jamaica, has enabled him to explain to us a circumstance, which accounts very much for the incorrectness with which this disease has been hitherto described.

" It is to be observed," says he, " that this complaint among the blacks is rarely known to the physician, who has the charge of the estate, till some acute disease, such as fever, peripneumony, or dysentery attacks them. And even under these circumstances the physician is in the greatest danger: for if he happens to be taken with the disease, his life and future prospects are at stake. He must seclude himself for many months from all intercourse, and it is well if he recovers at last: hence we see why yaws is ill understood and unskilfully treated: hence we must make allowances for the errors of so many authors, who, in their writings, have depended on the faith of others, not their own observations."

This author concludes with some remarks on the method of cure. In this and other passages he dwells much on the danger of the early exhibition of mercury.

The last writer I shall notice is Dr. Winterbottom, who, in his Account of the Diseases of Africa, gives a very full history of Yaws, enriched with a number of references to various authors. He describes fever, with pains about the joints, languor, and debility, as the first symptoms. " These," he adds, " are succeeded by a degree of pyrexia, sometimes attended with rigor, though, in

other instances, the fever is slight, and scarcely noticed." He is of opinion, that the disease can never be communicated by miasmata floating in the air; but, like other authors, omits to mention any distinction by which we can ascertain the part in which the poison has been introduced. He observes, indeed, that " where there is an ulcer or slight wound in any part, the pustules either appear there first, or are more copious there than elsewhere;" but the same happens in small-pox when taken by effluvia, or even under inoculation, though at a distance from the place of insertion. This author is of opinion, that the patient fares best when nothing more is done for him than an attention to his general health.

Such is the account of this disease, as far as I can collect from others, and from my own observation. If I have not taken notice of every writer, it is because those I have omitted contain nothing satisfactory, but what is to be met with in those above mentioned; or because I conceive enough has been said to enable any person, who has never seen the complaint, to detect it without difficulty. This is by far the most important consideration in the disease. The mode of treatment seems to depend more on avoiding to do harm than in expediting the cure. The disease, though extremely uncomfortable for the time, appears to owe all its more lasting terrors to the too early use of mercury, which was probably first exhibited before the laws of the complaint were attended to, but is now confined to that stage when it is approaching to a spontaneous cure. There remain a few very important considerations, which, I trust, in the present state of our art, will be no longer left as matters of doubt.

Dr. Moseley says, the disease is now inoculated with success. If so, it is to be lamented that we have hitherto no account of the period, after insertion, when the previous fever commences, when the local action shows itself, and when the general eruption follows, or whether they both appear together? Whether there is any difference

in the progress of the primary and secondary pustules, and what are the advantages derived from inoculation. There is a passage in Dr. Ludford's valuable dissertation, which relates to the subject, but not with that minuteness which we may hereafter expect from the same pen. He observes—" The virus of yaws seems to act slowly on the body, since the person affected, for some time, perceives no change of the wound, nor of the ulcer, nor of the part affected, excepting the difficulty of keeping the surface clean. After some weeks, the neighbouring parts are inflamed, and the ragged margins of the ulcer feel painful." Page 20.

Valuable as is this account, it is deficient in not precisely stating the effect of artificial inoculation (if such is practised) without which it will be difficult to ascertain the precise period at which the infection was received.* We, however, learn, that the progress is slow. Another most interesting enquiry remains to be perfected, and, as far as we may judge, it cannot be in better hands than the ingenious author of the Thesis, who is himself well aware of the importance of the investigation. " Those," says he, " who are under yaws, are liable to the other exanthemata, such as measles and small-pox. The latter may be induced by exposure or inoculation, which last is better attempted when yaws is in the decline, for then the small-pox will either entirely take away yaws, or at least will check it for some time; nor will the fungus's continue long, even if they should happen to appear again on the surface." All this is perfectly analogous to what has been traced in other morbid poisons. It is probable that the irritation from small-pox and

* It is absolutely necessary to ascertain all these periods, before we determine on the truth of the accusation brought against slave factors and surgeons on board slave ships. They are said, if the disease appears on a slave during the voyage, to repel it by a mixture of iron dust, gunpowder, and lime juice. I cannot help doubting the power of these ingredients to produce such an effect; especially to induce " a glossy smoothness in the skin, where the yaws commonly break out."—Grainger, page 72.

measles, being greater than that from yaws, may interrupt the latter at any time. But the laws of that poison, requiring a certain course to be pursued, if the new irritation is induced before that course is completed, the disease must return as that new irritation ceases. If, on the contrary, that irritation has not been induced till the course of yaws is completed, and nothing remains of it but an habitual ulceration, the new irritation will not only supersede the old action, but, by breaking the habit, very much expedite the cure.

Dr. Dancer, in confirmation of the above, gives the following quotation from Dr. Nembhard:—"During the universal prevalence of the small-pox in this island, in the year 1784, it was remarked, that several negroes, afflicted with yaws, who had the yawy pustules on the surface of the body, and *had been a considerable time* under all the afflicting circumstances of the disease, were inoculated promiscuously among many other negroes. The result was, that upon the decline of the small-pox, and drying away of the pustules, the yaws also gradually disappeared; as if both might be considered in the light of one congenial disease." Dr. Dancer adds, the "advantages of ascertaining the above fact, would be a discovery little short of that of preventing the small-pox by the cow-pox." But we should keep in mind the difference between superseding the effects of a morbid poison, which has gone through its course, and destroying the constitutional susceptibility, without passing through the disease.

After what has been said, it is scarcely necessary to add any thing on the subject of cure. In a disease which must go through a certain course, the practitioner can only be guided by symptoms, which must vary according to constitution and circumstances. But by the concurrent testimony of all, he must avoid mercury, at least till the decline of the disease. If I were to give an opinion, it would be, to omit it altogether, unless the patient should be con-

valescent in general health, whilst some of the ulcers remain indolent. In this, as in other similar cases, mercury may be useful. But if this indolence is attended with want of health, it would at least seem desirable to try bark or the chalybeates, or other tonics, before we have recourse to a remedy, which, in a warm climate, always induces debility, if continued for any length of time. On this subject, however, those who have the largest opportunities must inform us. But the most important consideration of all, is an accurate description of the disease, that we may not, in its early stage, fall into the error of exhibiting a remedy, the calamitous consequences of which are insisted upon by every writer.

CHAPTER XVII.

OF ANOMALOUS MORBID POISONS,

SHOWING THE NECESSITY OF ASCERTAINING THEIR TRUE CHARACTER, BEFORE WE COMMENCE OUR TREATMENT, WITH REMARKS ON SOME PUBLICATIONS WHICH HAVE APPEARED SINCE THE FIRST EDITION OF THIS WORK.

WE have now, I trust, traced with tolerable accuracy three morbid poisons, a perfect understanding of which may serve to illustrate each other. The first and second have many laws in common. Each, when applied to a secreting surface, may induce either ulceration, or a mere increased and altered secretion. Each produces secondary symptoms different from the primary, and both are curable by mercury. But the venereal poison has a power of producing the same kind of ulceration in whatever part it is applied. Chancres, whether at the orifice of the urethra, the glans penis, the corona, the prepuce, or the external skin beyond it in men, or in women, whether in the vagina or labia pudendi, all exhibit the same character. Sivvens, on the contrary, does not appear to produce its characteristic phagedæna, excepting on spongy and highly sanguiferous parts, such as the uvula and tonsils; for those ulcers at the angles of the lips, attended with the white skin or slough, as it is called, appear to be only superficial inflammation, excited so high as to produce an effusion of coagulated lymph. If this is really the case, it will explain the reason why ulcers from this, or some similar poison, may have appeared on the glans penis, without ever having

been detected in the vagina, and hence why they were not suspected to arise from contagion, till the venereal poison exhibited similar ulcers in both sexes. This and every other difference between poisons which yield to the same remedy, though highly worthy the attention of the physician, is less important in a practical point of view, than the accurate discrimination of such as are found to be exasperated by the injudicious use of a remedy to which the others yield.

Whenever we wish to ascertain the operations of nature in those phenomena, the laws of which have not hitherto been detected, we are instructed to keep two objects in view. The first is to trace and record, with the utmost minuteness, every action as it appears in the object of our research, with the series and order by which it is preceded or followed through every possible variety. The second is to observe in what respect these actions, with the attendant series and order, differ from or agree with the actions of other phenomena, the progress and nature of which have been established. In pathology this is so difficult, that it is rarely undertaken. Sydenham in the small-pox and gout, and Hunter in the venereal disease, have left us the best, if not the only types. But such is our ingratitude, that the labours of these great men have been too much neglected, and more pains have been taken to refute their doctrines, than to study their discoveries and consequent practice. Sydenham, by his frequent querulous apostrophes, shows us how little he was understood in his own life time, and how often misrepresented. After his death, due honours were paid to his name; but a practice, the direct reverse of his own, was established in small-pox: and in the treatment of gout, those varieties in its form were not attended to, which his accuracy traced and provided for. Mr. Hunter, during life and after death, received that tribute to which his anatomical discoveries entitled him; nor could he be refused the credit of those physiological improvements which were the necessary result of his discoveries.

But the original structure of parts is uniform and permanent, the alteration from disease is various, and evanescent. To exhibit the first, therefore, when the discovery is made, is the business of every anatomical school, and to be acquainted with it requires little more than the regular attendance of the scholar: whilst a knowledge of the progress of disease demands not only the most patient attention to a series of events and a consequent chain of reasoning, but an application of these to what is constantly passing before us. Hence the slow progress of our art, and hence the difficulty with which a practice, founded on accurate observation, and supported by close reasoning, becomes current.

But what the patient industry of a SYDENHAM could never accomplish, was readily atchieved by the lucrative boldness of a few successful inoculators; and a temporary zeal for new remedies, too furious to allow leisure for ascertaining the disease for which they were proposed, may perhaps accomplish more than all the labours of a HUNTER.

Had this simple proposition been attended to, "Either these acids will cure the venereal disease, or the disease will cure itself," we might by this time have learned, with some certainty, what ulcers on the genitals will heal spontaneously, what will yield to the exhibition of acids, and what require mercury for their cure. But J. Hunter was no more! and this calm, this patient enquiry, this silent investigation, without engaging in party violence, which warps the understanding, and almost teaches us to deceive ourselves, was too much to expect a second time in a century.

The controversy has, however, produced important consequences. The opposers of the new remedies could not but discover that many diseases, they had before treated as venereal, were curable without mercury; and this would teach them to enquire, whether such appearances would not yield to the common efforts of the constitution. Hence they would be convinced of what they had long

been taught, that it was time to form some discrimination before a powerful remedy is applied. The advocates for the new remedies also discovered at last, that, though they could cure some diseases by acids, yet others were too obstinate to yield to any thing but mercury. Hence some of them conceived that the venereal disease might assume such a variety of forms, as in some instances to require mercury, in others to yield to milder remedies, and in others to heal spontaneously.

The question might have been much narrowed, had those who held the latter opinion described the different *forms of the same disease*, and the remedies applicable to each, in which case it would not have been worth disputing, whether they were different diseases or not. It was not less incumbent in the others to explain what they meant by a venereal ulcer or chancre, or at least to fix the exact description of that complaint which would yield to mercury only: for want of this, should hereafter any thing remain of this controversy, posterity may be at a loss to determine what diseases were called venereal at the close of the eighteenth and beginning of the nineteenth century.

Of the numerous writers in favour of acids, for the cure of venereal complaints, I shall only select Dr. Currie, because, though his cases are imperfectly described, they are less so than any others.* The two first in which he succeeded he calls open buboes and ulcerations of the glans penis. Nothing is more common among sailors, and such was the occupation of the paients, than ulceration of the glans and inside of the prepuce, from mere want of cleanliness, in a long voyage, during which they are obliged to live chaste. Buboes will arise from these as well as every other inflammation of

* See Appendix to Dr. Currie's Medical Reports; and lest the reader should suspect the death of that eminent man has had any influence on my mode of expressing myself, I must refer him to Medical and Physical Journal, Vol. VIII. Page 419.

the genitals. Whether such was the condition of these men, we can only judge from the history, as their ulcers are not described.

Of the next patient we are told, " That the disease had been in the habit upwards of a year; the surface was covered with venereal eruptions; the throat *had been* affected, and the glands of the neck, on each side, had been indurated, and were in a state of ulceration. The patient had undergone a course of mercury in the hospital, but after pushing it as far as her constitution would bear, she had been discharged a few weeks before, with little or no alteration in her symptoms. The last six weeks she had been in the country, on a milk diet, and her health was somewhat recruited." By the use of the acids some amendment was produced, which continued for a month. " The eruptions on the skin diminished, the nocturnal pains in her head and limbs went off, and the ulceration in her neck assumed a healing appearance; her general health also improved rapidly." From this time she derived no further benefit, and the Doctor thought proper to salivate. Here again the relief was temporary, after which her health declined, and the local symptoms were exasperated. Again the acids were used with the same success, and after them mercury. At last she was cured by arsenic.—This case hardly requires a comment; the primary symptoms and the throat, probably the only venereal symptoms, never returned after they were cured by mercury. The neck was scrofulous without any doubt, as the venereal disease never affects the glands but by absorption from primary ulcers. It is impossible to say what the eruption might have been; it is, however, worth observing, that we have no account of its return, with the other symptoms, after the mercurial irritation which succeeded the first use of acids.

The case of Elkins is not less remarkable. Four years before the Doctor saw him, he was affected with the usual symptoms of lues, for which he underwent a course of mercury, and was supposed to be cured. These we may suppose were primary symptoms.

About nine months afterwards, the disease appeared in his throat, accompanied with obstinate pains in his head. By this account it is probable that the throat was not attended to, till the disease attacked the second order of parts, viz. the bones. He was again, we are told, salivated with similar good effects. From that time, according to the laws established by Mr. Hunter, the patient was to expect no more of the disease. However, " twelve months afterwards, having been for some time attacked with pains, supposed to be rheumatism [probably the effect of mercury] the disease appeared again, and resisted the long continued and repeated use of mercury."— " Three months afterwards he was admitted with thickening of the pericranium, severe pains at night in the bones of the head, arms, and leg, and a large increasing node on the right tibia." Of all these symptoms, which might have remained for a time after mercury had superseded the venereal action, or might have been occasioned by mercury alone, he was cured by the nitrous acid.

As Dr. Currie regrets that he is obliged to answer repeated enquiries, before his experience was such as to enable him to speak with confidence on the subject, this is a sufficient apology for an account which, though imperfect, is more minute than many others compiled for the public eye. The history is very correct, and if the descriptions are not sufficiently minute, they are as much so as the generality of writers have thought necessary. They are, indeed, all, excepting the first, deficient, in leaving us in the dark respecting primary symptoms. But this is not Dr. Currie's fault. It is reasonable to collect from the whole, that the sailors never had venereal ulcers. That when the Doctor saw the female, she had been cured by mercury of the ulcer in the throat, which did not return, and that such symptoms, as had been cured by mercury, did not return, nor any new ones in the same parts. All this is perfectly consistent with what should be expected, according to that series and order of the disease and its cure which Mr. Hunter has

pointed out. The same may be said of Elkins's case, as has been just observed.

At length the subject of acids was taken up by men who had the largest opportunities of conducting such experiments. Mr. Pearson had already appeared before the public, to correct an erroneous practice, which had its advocates for a time.* On both occasions he proved himself equally industrious, and his coadjutor, Mr. Blair, joined in the last laudable enquiry. The latter, who first appeared before the public, gave a variety of cases from his own and the observations of others, to prove, that the acids were not equal to the cure of syphilis. Nothing could be more certain than the position he wished to establish; but it is impossible not to remark, that many of the supposed successful, as well as unsuccessful cases, are indiscriminately called venereal; that is, the primary symptoms are said to be chancres, and the secondary are called syphilitic. It is thought enough to give them this name, without describing any of the appearances. It is the want of this discrimination, which he neither favours his readers with, nor requires of others, that has produced much of the embarrassments between the different advocates for these remedies, and protracted a controversy which must otherwise have been soon decided. For if any one of the inveterate cases, described as cured by acids, was really venereal, it is absurd to suppose that the same remedy would not have been invariably successful, as we find in the exhibition of mercury, without a single exception.

Mr. Pearson's "Observations on the Effects of various Articles of the Materia Medica in the Cure of Lues Venerea," is a valuable addition to our registers on this subject. In this the author takes a view of all the remedies which have been exhibited instead of mercury, and shows the inefficacy of them, or at least that none of

* Observations on the Effects of Opium in the Venereal Disease. Medical Communications, Vol. II. Page 56.

them can be depended on like mercury. But it is impossible not to wish that Mr. Pearson, whose opportunities were still more extensive than Mr. Blair's, had done more towards ascertaining the true character of the disease, or at least that he had written with less ambiguity on certain points which it was necessary to ascertain, in order duly to appreciate the value of the various remedies he brings forward. In the paper before referred to [Medical Communications, Vol. II. Page 56] the same omissions is not less conspicuous. Some of the cases, which were cured by opium, were called chancres, yet they are not described, and the author's subsequent reasoning renders it doubtful whether he afterwards considered them as venereal. I have taken notice, in a former edition, of a similar inattention in the "Practical Treatise on Cancer." In this we are told of "*venereal* ulcers, which continue to spread during a mercurial course." These should not be called venereal, or at least they should be described, instead of being named, that the reader may judge of their nature.

In his last, and, in other respects, truly valuable work, it is particularly to be wished Mr. Pearson had been more attentive to the importance of accurate description. It is painful, where we meet with so much to commend, to have to lament an omission, which so much lessens the value of the whole. It will be enough for me to make a few remarks on the *general conclusions*, to show how unsatisfactory they must prove to an inquisitive reader.

These conclusions are, "1. The guaiacum, sarsaparilla, mezereum, walnuts, opium, and Peruvian bark, have often removed some of the primary and secondary symptoms of lues venerea, and have alleviated others. They are likewise each of them capable of removing certain sequelæ of lues venerea, where the farther administration of mercury would prove injurious. Yet, no satisfactory series of evidence can be adduced, demonstrating that any, or all of these vegetables, given singly, or combined, are competent to the *eradicating* of lues venerea from the animal body."

When we see the uniformity of nature in the venereal disease, and in its cure under the use of mercury, it is impossible not to be surprised at this *unsatisfactory series of evidence*. My meaning will be best explained by the succeeding paragraphs.

" 2. It must be conceded," continues Mr. Pearson, " that certain indubitable symptoms of syphilis have disappeared, during a course of the vegetable remedies; but the same symptoms have generally recurred, even at the very time when the patient was taking largely of the medicines which had produced this temporary benefit. Even where the patient has remained apparently well during five or six weeks, the disease has nevertheless always returned; and, what is worthy of particular attention, the same symptoms precisely have recurred, which had been seemingly cured during the administration of the medicines alluded to. This fact may be considered as a proof, that venereal symptoms are not cured by them in any proper sense; because local appearances admit of a perfect cure by a mode of administering mercury, which shall nevertheless be insufficient to secure the constitution."

In this sentence we are relieved from every possible doubt; we are told, that " *precisely* the same symptoms have recurred, which had been seemingly cured during the use of the above remedies." When local symptoms, of the same character, appear on the same places, we may be sure that the disease has not met with that cure which it derives from mercury: " because," as this author observes, " local appearances admit of a perfect cure by a mode of administering mercury, which shall nevertheless be insufficient to secure the constitution." By this sentence we clearly understand, that local symptoms are not cured by these remedies; and also, that they may be perfectly cured by mercury, yet that constitutional symptoms may afterwards appear.

3dly, We are told, " The muriated barytes, and two of the mineral acids, when given to venereal patients, have the power of

suspending, for a limited time, the progress of the disease, and of removing many secondary symptoms; but they are not equal to *the subduing of the virus*, and freeing the constitution entirely from the effects of that destructive malady. They may likewise be employed with great advantage in those phagedenic ulcers of the genitals, and of the groin, which may be classed among the sequelæ of syphilis."

Thus it appears that the above remedies have the power of " suspending the progress of the disease, and of removing many secondary symptoms." By this apposition are we to suppose that they suspend primary and remove secondary symptoms." This would be, in many respects, analogous to what we might expect, as in this and all other morbid poisons, we find secondary symptoms in the soft parts more easily cured than primary. But the next sentence involves the whole in obscurity.

4thly, Mr. Pearson adds, " the nitric and nitrous acids have removed both the primary and secondary symptoms of syphilis; and, in some instances, it seems, that the former have not recurred, nor have secondary symptoms appeared, at the period when they commonly shew themselves, when the cure has been imperfect. But, as far as my own experience extends, and that of many respectable friends, who are connected with large hospitals, a permanent cure has never been accomplished by these acids, where secondary symptoms have been present."

Thus, though secondary symptoms are more easily cured than primary, and though these acids will cure both primary and secondary symptoms, yet no instance can be produced, in which a " permanent cure has been accomplished, where secondary symptoms have been present." I confess myself unable to understand the difference between a cure and a permanent cure. If these remedies have produced a cure of primary symptoms, so that they never have returned, this is as much as we can promise of mercury. If it has

done less than this, the disease has only been suspended. But what is most remarkable, the cure was not permanent "where secondary symptoms were present." To derive any advantage from this position, it is absolutely necessary that we should be instructed what these secondary symptoms were, and in what form the disease afterwards appeared. If in the same form as that which was supposed to be cured, the remedy was certainly insufficient; but if the secondary symptoms, which were present and cured, were in the soft parts, and the subsequent appearance of the disease was in the bones, this likewise is as much as we can promise from mercury. After this, the ingenious author observes:

"The same acids, when exhibited with the utmost care and attention to many patients labouring under the primary symptoms of the venereal disease, and where they have agreed perfectly well with the stomach, have been nevertheless, found inadequate to the cure of those symptoms. Indeed the failures which have occurred, both in my own practice and that of many of my surgical friends, have been so numerous, that I do not think it eligible to rely on the nitrous acid, in the treatment of any one form of the lues venerea."

Here again we meet with what we might expect, that remedies which excite so slight an irritation in the constitution as these acids, have been found inadequate to the cure of the *primary symptoms* of a disease, which we have seen is not subdued without exciting a more violent action than is required for superseding the irritation of some other morbid poisons. This would at least lead us to doubt, whether the more successful cases were truly venereal; a doubt which we are authorised to entertain, till the appearances are accurately described.

If an instance could be produced, in which primary symptoms or chancres had been permanently cured by acids, those remedies would, in the opinion of Mr. Hunter and his disciples, stand on as good a foundation as mercury, even if secondary symptoms occurred

afterwards; and it is not so easy to understand what Mr. Pearson can mean, if he does not admit as much when he says, local appearances admit of a perfect cure, by a mode of administering mercury, which will not secure the constitution. May not the advocates for the new remedies ask: What is the mode of administering mercury, which *will* secure the constitution? I grant the object of the book is to show the effect of other remedies. But in doing this, is it not absolutely necessary to make a comparison between them and a well known specific? Should we not, therefore, have expected of Mr. Pearson, from his large opportunities and talents, 1st, an accurate definition of the disease; 2dly, of the powers of mercury in curing it; 3dly, of the effect of other remedies in the same disease; and, lastly, a luminous comparison to illustrate the whole?

Whilst this controversy concerning acids was in its vigour, Dr. Clutterbuck did me the honour to take notice of Mr. Hunter's opinions, in a letter* addressed to the author of Morbid Poisons, at that time in Madeira. This letter was answered in the Medical and Physical Journal.† I shall not, however, pass over it in this place, not only because this very able writer is the only one who has offered any thing against my former defence of Mr. Hunter, but because, in answering his objections, Mr. Hunter's opinions may be further illustrated.

Dr. Clutterbuck, in accounting for the appearance of the disease in the skin, after it has been permanently cured in the parts first affected, observes, that "Though the occasional re-appearance of the symptoms gives a colour of probability to the notion, that mercury does not remove latent contamination, it is yet not conclusive; for the fact may be otherwise accounted for.

"In the first place," continues he, "it is not one of frequent occurrence. From my own observation I should say, and the opinion

* Published by Boosey, 1799. † Vol. VIII. Page 412.

is not singular, that when mercury is carefully employed, and its action kept up for a considerable length of time, the secondary symptoms will very rarely appear. In the greater number of instances of the secondary disease, which have occurred to my observation, I have been able to ascertain, that mercury had been employed in a superficial manner, either from carelessness in the patient, or from a pre-conceived theory in the mind of the practitioner. 'The venereal action,' Mr. Hunter observes, (p. 241), 'is just as soon destroyed in a large chancre as in a small one; for every part of the chancre being equally affected by the mercury, is equally easily cured.' On this principle I have known practitioners push the mercury vigorously, till a considerable affection of the mouth was produced, and then suddenly discontinue it: under this treatment the chancre will often heal. Sometimes too, it has happened unexpectedly, that the mouth has been so violently affected by a very small quantity of the remedy, that a further use of it became for some time impracticable; and the chancre having healed before the salivation had declined, it has not been judged requisite to recur to the use of mercury. In each of these cases, I have observed the secondary symptoms to succeed. On such occasions, therefore, the recurrence of the disease may be explained, without supposing a want of power in mercury to remove the latent contamination."

My reader will recollect I have already admitted, that secondary symptoms are by no means frequent; and so contrary is the "superficial or careless manner of administering mercury" to my "preconceived theory," that I do not consider the primary symptoms can be cured without a careful exhibition of the remedy, in such a manner as to produce a considerable constitutional irritation. This makes it the more surprising to me, that in any of the instances alluded to by Dr. Clutterbuck, the chancres should have remained healed. This he accounts for in the succeeding passage.

" Does mercury," proceeds Dr. Clutterbuck, " act equally in the system? or is salivation a test of the whole having taken on a new action? This is a question of considerable importance in explaining the phenomena of the disease, and the facts respecting its cure.

" I have many times seen a violent affection of the mouth take place suddenly, from a small quantity of mercury,* without a corresponding change being produced on the symptoms of the disease. When mercury runs off by stool, as it is called, or produces any other violent and partial action in the system, the disease generally remains stationary till the remedy be cautiously repeated. Again, it is very common to see both chancres and secondary ulcers under the use of mercury, healing in one direction and spreading in another, so that they come at last to occupy an entirely new seat: and when various symptoms exist together, some are found to give way, at the time that others go on increasing. These circumstances prove the unequal action of mercury in the system, and enable us to understand, why the symptoms recur at all: why they return repeatedly in the same parts, as I hope to shew hereafter; and why some symptoms now and then resist its influence altogether for a considerable length of time, although the system appears to be fully impregnated with the remedy."

The first part of this extract is not immediately to the point, because Dr. Clutterbuck admits that the primary symptoms remained unchanged, which I have already shown † may happen under some circumstances of salivation. But having never seen chancres or secondary ulcers heal in one part and spread in another, I cannot reason upon them. Of the bubo, under such circumstances, enough

* " Three or four grains of calomel have often produced salivation in women; and when this has been the case I have never observed the symptoms of the disease to be materially relieved, till a more gentle and general mercurial action was induced."

† See Page 119.

has already been said, and the cause, I trust, satisfactorily accounted for.

" The readiness to be affected by mercury," continues Dr. Clutterbuck, " depends probably on the state of the irritability at the time. Both the sensibility and irritability of parts are much augmented, when inflamed or ulcerated, and, when in this state, they are more open to the impression of stimuli. Venereal tumors and ulcers are thus more susceptible of the mercurial irritation, than parts whose sensibility and irritability remain unchanged, which is the case for some time after contamination. A reason may hence be afforded, why the same mercurial course, which is sufficient for the removal of the apparent action, may yet be inadequate to overcome the latent degree of the disease which Mr. Hunter terms *disposition*.

" Mr. H. remarks, p. 335, and it is, no doubt, true, that mercury acts readily on diseased parts, whilst it affects little those which are sound. The less, therefore, parts are removed from the healthy state, as in the early stages of contamination, the greater will be the difficulty of impressing them with the mercurial irritation, and thus the preventive powers of mercury will be less apparent than its curative, though the former cannot hence be denied to exist."

If Dr. Clutterbuck admits that the same course of mercury may be sufficient to cure the apparent action, and yet inadequate to overcome the latent degree of disease which Mr. Hunter calls the *disposition*, what are we disputing about? Surely no one will suspect me of so blind a devotion, as to quarrel whether a state of the constitution shall be called a " disposition to," or " under the latent degree of, a disease." The only question will therefore be, Whether any mode has hitherto been ascertained of conducting a mercurial course, so as with certainty to prevent secondary symptoms? Dr. Clutterbuck conceives such symptoms are more numerous, since

mercury has been more sparingly exhibited. But it has been shown that none of the writers, before Mr. Hunter's time, pretended to know when their patients were secure; besides which, it is universally admitted, that the disease has lost many of its terrors, which can only be accounted for by an improved practice.

Dr. Clutterbuck is certainly not aware how directly his theory leads to the practice he objects to in Mr. Hunter; for if, which he does not seem to dispute, we have no means of ascertaining whether a part is contaminated as long as its "apparent action remains unchanged"—if in this state, should it be contaminated, it requires more mercury to overcome its "latent degree of disease," than to cure it when the disease has become apparent; surely no one would persevere in the use of an important remedy to cure a disease which he is not sure exists; which, if it does exist, he has no means of ascertaining that he has cured; and which, should it afterwards become apparent, he can cure with ease.

But it is impossible not to remark, that, whilst this gentleman conceives the occurrence of secondary symptoms may be imputed to the want of a sufficient exhibition of mercury, he, at the same time, supposes that some diseases, which are cured spontaneously, or without any mercury, are still venereal. Thus all the cases, described by Mr. Hunter as the effect of transplanted teeth, one of which was cured by a few grains of calomel, which produced considerable purging,* and others without the exhibition of mercury in any form, are said to be venereal. The case related by me,† the primary symptoms of which were exasperated by mercury, and in which secondary symptoms appeared after four violent salivations, is also said to be venereal; and here the recurrence of the symptoms are imputed to "abstaining from mercury in the first instance." In

* See the last note quoted from Dr. Clutterbuck.

† Letter, page 58.—See also page 32 of this work.

answer to my remark, that the primary ulcer was in every respect different from the true venereal character, and exactly resembling the *nigrities serpens* of Celsus, in alternately sloughing and assuming a phagedænic character—it is admitted " that the phagedænic appearance is not observed in ordinary chancres; but the same thing is said to occur in small-pox, the pustules of which, instead of drying, spread into large and foul ulcers."—When the latter disease, it may be answered, assumes this form, it is no longer small-pox. The matter secreted will not inoculate the disease, and the cure is not subject to the same laws as have been long ascertained in small-pox. But if ulcers, whether sloughing or phagedænic, or with other appearances — whether healing spontaneously — refusing to yield to any remedy but mercury, or being exasperated by it, and yielding to no remedy at all—are to be called venereal, we can no longer dispute the opinion of those who suppose the disease to have existed from the remotest time, as many such ulcers were certainly described before the date usually assigned to syphilis. Dr. Clutterbuck is indeed of opinion, that the ulcers, described by Celsus as *cancer in cole*, " present nothing but what is commonly observed in phagedænic, or, to use common language, ill-conditioned spreading ulcers, such as may be seen every day." They are probably more common than would be expected by the number of cases on record, as is well known to men of extensive practice, or who converse much with the medical world, but, I believe, not so common as to authorize Dr. Clutterbuck's expression.

The last writer I shall notice is Mr. Abernethy. This gentleman, in a miscellaneous work, [Surgical Observations, &c. 8vo. Longman, 1804], has two very valuable chapters on diseases resembling syphilis. In the first of these he gives a description, somewhat indeed too general, of the venereal ulcer or chancre, and also of some of the primary local ulcers, which, from the parts in which they first appeared, and from the consequent secondary symptoms, might have been suspected to be venereal. There is not, I

am sure, a medical reader, who will not regret with me, that this ingenious writer did not describe some of his cases with more prolixity. However, enough is said to satisfy most of us that they were not venereal, and probably there would have been fewer doubts on the subject, if the author had not proposed them himself.

"If, then," says Mr. Abernethy, "the occurrence of such cases be frequent, and the necessity of discriminating them from those of syphilis be of great importance, we may solicitously enquire by what circumstances we are to distinguish between diseases so similar in appearance, but so different in their nature. Mr. Hunter seemed to wish the prosecution of this subject, probably from the expectation that some characters appropriate to these diseases might be detected: I have not, however, been able to discover any; the fictitious disease in appearance so exactly resembles syphilis, that no observation, however acute, seems to be capable of deciding upon its nature. Although the ulcers in these ambiguous cases generally spread more extensively along the surface of the part which they affect, yet this does not constantly happen, as is shewn in the case related at page 118.* In this case, however, the induration which surrounded the chancre occurred suddenly, and went away as rapidly. The history, therefore, of the two diseases was very dissimilar. It must also be remarked, that true venereal spots and ulcers sometimes assume the appearance of other diseases, and do not possess their ordinary characteristics." P. 134.

If, in this paragraph, by venereal ulcers are meant chancres, the truth is, that this form of the disease does possess the same character, varying in appearance and mode of cure less than perhaps any other disease with which we are acquainted. But it is not a little remarkable, that Mr. Abernethy should be so wavering in his conclusions, after having assumed a proper decision during the treatment of this very case, page 118. In relating his

* This case we shall presently consider.

first case, (page 111), his words are, " The wound fretted into a sore about the size of a sixpence, which the patient showed me, and which *I affirmed* had not the thickened edge and base, and other characters of a venereal chancre."—In the secondary symptoms, Mr. Abernethy was " *more* inclined to believe the disease was not syphilitic, from the sudden and simultaneous occurrence of the node and sore throat, &c." Surely there was no difficulty in distinguishing this case from syphilis.

" Another patient," he observes, " had a very irritable sore on the prepuce, covered with a slough." This having been thrown off, was succeeded by another slough. — " The sore continued to slough, and extend nearly round the prepuce." Page 129.—All this is so different from the character of a venereal sore, as described by Mr. Abernethy himself, that there could be no danger of mistaking it. I could almost have hoped, while Mr. Abernethy was lamenting with Mr. Hunter the want of some appropriate characters by which to detect these diseases, that he had indulged me in calling the last-mentioned case by the term I have ventured to adopt, of sloughing phagedæna, or *nigrities serpens* of Celsus.* Though the other cases are not quite so well marked, there was enough in most of them for Mr. Abernethy to distinguish their primary symptoms from the true chancre. This difference, of course, rendered him confident in the treatment of the secondary symptoms, which he very properly conceived would not require mercury, as the primary had healed without it.

In some secondary cases, the primary symptoms of which he

* See page 58, and also the case, page 32, & seq. The reader, who has not seen the edition of 1795, should be told, that this case and the term, as well as the subsequent passages referred to concerning anomalous morbid poisons, and the actions excited by mercury, are transcribed from that edition. This is mentioned to show, that how much soever my reasoning may accord with Mr. Abernethy's cases, it could not have been built upon them, but upon my own observation. It is to be hoped, therefore, that our coincidence will illustrate and confirm the theory.

had not seen, but in his enquiries after which he could discover nothing like chancres or primary venereal ulcers, he was more at a loss, and proposes a mode of practice worthy of that circumspection which we should expect from the experience he had derived from his other cases.

" Since, then," says he, " our senses fail us in our endeavours to discriminate between these two diseases, and since the most important circumstance is to distinguish whether the disease be venereal or not, we may enquire whether there are any circumstances in the progress of these different diseases, which will serve us in distinguishing one from the other. It appears to me that there are; and these cases are published not merely to shew the frequency of such occurrences, and the necessity for discrimination, but to engage a more general attention to the means by which such distinction may be made. A very simple fact has enabled me in most cases to distinguish between the two diseases; yet, simple as it is, if it be generally true, it is very important; and if it were universally true, it would be of the highest consequence. The fact alluded to is, that the constitutional symptoms of the venereal disease are generally progressive, and never disappear unless medicine be employed. It may be added too, they are as generally relieved under an adequate effect of mercury on the constitution. An attention to such plain and simple circumstances has been of great use in directing the medical treatment which I have pursued, and I am induced to solicit the public attention to them, that others may determine the value of such remarks."

In this passage it appears to be Mr. Abernethy's intention to speak only of those secondary symptoms, for which he had been consulted, without having seen the primary ones. But unfortunately he has not been sufficiently explicit on this subject. This renders both his account and proposed practice extremely obscure, and in a great measure justifies the manner in which he has been treated by the different journalists.

"It is," says a writer in one of these periodical publications, "an outrage to probability, and opposed by every analogy in the animal economy, that two poisons, of essentially different natures, should produce effects so similar to one another as to be undistinguishable. An unprejudiced person would require the strongest proofs to induce him to believe such a thing possible. He would require proof that no possible access could have been had to a source of venereal poison. But in all the cases of the sort which have been published, such access was not only possible, but probable. The only thing, therefore, which is to prove their dissimilarity, is their getting well without mercury. This argument would be decisive, were it unequivocally shown that venereal affections, in no stage, get well of themselves."

Another Journalist remarks—"It is greatly to be wished, that in calling the attention of practitioners to these diseases resembling syphilis, characters should be pointed out by which they should be known and distinguished from the venereal disease. But here the resemblance is so perfect, that every reader must have pronounced them syphilitic, had he not been informed by Mr. Abernethy that they recovered without the use of mercury."

"*Chancres* of *various appearances*," continues this writer, "followed by buboes, and successively by copper coloured blotches, coronæ veneris, and nodes of the bones, are the symptoms of the disease *now* submitted to our consideration as resembling syphilis." This is by no means the first time they are submitted to the public view. Mr. Hunter, twenty years ago, remarked the same. But he was too cautious to call *ulcers*, of various descriptions, *chancres*, merely because they appeared in these parts.

"These remarks," continues the reviewer, "we would wish to be understood as applying only to diseases strictly resembling syphilis, for *we are assured* that there are primary sores affecting the genitals, which are not syphilitic, which may be often characterised

and distinguished from the venereal chancre. Such are the *ulcerating phagedænic sores and those which spread by sloughing*; yet even here we cannot pronounce dogmatically. And our confidence in our diagnosis will be much lessened, if Mr. Abernethy's observation be justified, that " the venereal poison can be conveyed into the constitution, and contaminate it through the medium of a sore, the general actions of which are probably not venereal."

In answer to the first of these critiques, the remark may be repeated, that Mr. Abernethy, whenever he describes primary symptoms, does show a marked difference between them and the true chancre. The same reply is still more applicable to the second criticism, in which it is boldly asserted, that the " *chancres*, &c. resembled syphilis, and *only* seemed to differ in getting well spontaneously; to which perhaps may be added some anomalies in their secondary symptoms." Yet the truth is, Mr. Abernethy describes a marked difference in the primary ulcers. The difficulty he met with was in distinguishing the secondary symptoms. We cannot indeed wonder if these writers, who certainly show no disposition to be uncandid, should have felt themselves confused, when Mr. Abernethy has taken so little pains to derive or to explain to others the practical advantages derivable from his cases: for he seems not to be aware that it is necessary not only to decide whether they were venereal or not, but so far to ascertain their characters as to determine whenever they may occur hereafter, whether they are curable by mercury or not; for he afterwards adds:

" I have stated this rule as general, but not universal; for I could myself relate cases of diseases, in which, from the great abatement, and even disappearance of symptoms, I have concluded the disease was not syphilitic; yet, from the duration of the disorder, or from the subsequent aggravation of its symptoms, the patient has desired, and I have recommended the use of mercury, and the disease has been treated as venereal without its real nature being ascertained." Page 138.

How much we must regret that the appearance which these diseases assumed are not described. That there are morbid poisons which may be communicated in the same manner as the venereal, which retain their own precise characters different from that disease, and which will still yield to mercury, is well ascertained. The primary ulcers of sivvens we have seen are not only different from the venereal, but uniformly preserve that difference, yet are only curable by mercury. Others, which occur occasionally, have probably been only remarked by more accurate observers. By others they have been confounded with the venereal, and having given way early to mercury, the surgeon has congratulated himself and his patient on his rapid success. All this uncertainty is remarked by Mr. Abernethy himself, and very properly introduced by a caution against the early use of mercury.—These are his words:

" In recommending delay it cannot, I suppose, be thought that I would advise any one to wait till an ulcer destroyed the velum pendulum palati, or did material injury to any important part. There are cases where the progress of the disease obliges a surgeon to use mercury, even though he may be suspicious that it is not syphilitic. The effect of exciting a mercurial affection of the constitution in diseases resembling syphilis is, as far as my observation enables me to determine, very various. It sometimes cures them very suddenly and very differently from the gradual amendment which it produces in truly venereal diseases. Sometimes, however, these diseases yield more slowly to its operation, and are cured permanently. Sometimes the diseases recur in the same parts after a severe course of mercury; sometimes mercury merely checks the disease, and can scarcely be said to cure it; in which case it seems important to support the strength of the constitution, and to keep up that mercurial effect which controuls the disease, and can be borne without material derangement of the constitution for a great

length of time. Sometimes also the use of mercury aggravates these diseases." Page 141.

Nothing can be more just than these remarks, and nothing, we should conceive, could impress the writer with a stronger sense of the absolute necessity of marking, as far as possible, the characters of different diseases on the same part, some of which are healed, some unaltered, some healed for a time, and some exasperated, by a remedy which cures by inducing disease, and a frequent recourse to which Mr. Abernethy found attended with such serious consequences, as to authorize a free administration of it, in order to prevent its repetitions.

" I believe," says he, " that I am myself more likely to err in recommending the too free than the too sparing administration of mercury in diseases of this nature. Any surgeon, who has observed the ruinous consequences of repeated mercurial courses in some constitutions, would probably err in the same manner; and his dislike to disorder the constitution by mercury, would probably lead him even to use it more freely than might be absolutely necessary: this he would do in cases clearly syphilitic, in order to prevent the possibility of the recurrence of disease, and a repetition of a mercurial course. In doubtful cases, which are cured by exciting the mercurial action in the constitution, he would adopt a similar mode of treatment, in order to suppress the disease for so long a time as to make it less likely to recur; or if any subsequent disease should take place, to render it highly probable that this was not venereal, since it had broken out after such a course of mercury as must be considered to be adequate to the cure of almost any disease of that nature."

When we have established the laws of a disease, of the effects of our remedy on it, and on the constitution, then will be the time for us to decide how we shall direct our practice. Till these are accomplished all practice is merely experiment, which in the human

body, it is unnecessary to add, must be conducted with peculiar caution. But in the paragraph above alluded to, we are left at a loss to know, whether by " recurrence of the disease," is meant the appearance of the disease in the same part, or of secondary symptoms. A still more important omission is, that no provision is made for " doubtful cases" which appear cured, but whose cure may be no longer permanent than whilst the mercurial irritation continues. This is the more to be regretted, because it appears by our former quotation, that " sometimes the disease recurs in the same part after a severe course of mercury; sometimes mercury only checks the disease." All this shows the necessity of ascertaining, as far as possible, the nature of a disease, before we apply so powerful a remedy, and would more than reconcile us to that delay which is proposed at the beginning of Mr. Abernethy's paper. At the same time we must be still more at a loss to reconcile the following paragraph, either with that cautious delay or bold decision, which seems recommended in the preceding extracts.

" If then," says Mr. Abernethy, " as seems to be the opinion of Mr. Hunter, the venereal actions in a chancre may be sometimes so modified by the diseased propensities of the constitution, or part, as to form an ulcer scarcely cognizable as a venereal one; and if in some rare cases the poison may infect the constitution, and produce a sore, the general actions of which are not syphilitic, it follows, as a general rule of conduct in practice, that surgeons are not to confide in their powers of discrimination, but in all cases of ulcers, arising from impure intercourse, to act as if the sore was venereal, to give sufficient mercury slightly to affect the constitution, to guard against the consequences of absorption, and, by local and other general means, to cure as quickly as possible the local disease, and thus remove the source of contamination, and the necessity for the continuance of medicine." P. 157.

Mr. Abernethy, after this, dwells much on the uncertain appearance of chancres, by which, I suppose, he intends primary venereal ulcers. He shows also the various appearances of other ulcers in these parts, and frequently expresses a wish that a more accurate discrimination could be formed. As, on this occasion, he does me the honor to introduce me, in company with Celsus and Mr. Hunter, I cannot but be proud of transcribing the following passage:

"With respect to sores that are not venereal, the difficulties of investigation are greatly multiplied. If a description cannot be given of venereal sores, it seems almost absurd to say any thing of those multiform sores produced by infectious matter, the qualities of which may be probably variously modified, and the effects of which appear equally liable to modification from peculiarities of constitution. Yet in this intricate subject there are certain facts which can be distinctly observed, and deserve attention. Some of these sores spread by ulceration, and some by sloughing, of which instances are related in the first section of this paper. Even Celsus has described several species of sores which, as Dr. Adams has observed, we are acquainted with in the present day. I have never seen that phagedænic ulcer, which suddenly sloughs, affect the constitution; neither do I believe that surgeons in general have remarked it; those who regard all these sores as venereal, attribute the absence of secondary symptoms to the chancre having been removed by the sloughing of the surrounding parts. Yet in the case related by Mr. French, in Mr. Hunter's Treatise on the Venereal Disease, secondary symptoms did occur from a sore of this kind, and got well without mercury. It may therefore, perhaps, be doubted, whether this disease be not an aggravated form of the sore which sloughs more slowly, and from which the constitution is much more frequently affected. As I consider any observations that I have made on these sores to be incomplete, and therefore not

to be depended upon, and Dr. Adams having restricted the term Phagedæna to one kind of destructive sore, I feel more inclined to leave it as a generic term for all these sores, and to divide them into species according to their peculiar characters. Then we may describe them as ulcerating phagedænic sores, and sores which spread by sloughing. Again, the ulcerating or sloughing process may extend not in all, but in particular directions, and the sloughs may take place from the edges or from the whole surface. As Dr. Adams has treated these subjects at large, I refer the reader to his book; but I will take upon me to describe one species of sore which frequently occurs, and is generally treated as venereal, but which I am convinced is not so." Page 162.

The description of the sores follow, which seem to me of that character which I have called red soft warts, and have advised, like Mr. Abernethy, to treat with caustic applications. As to the paragraph above transcribed, I shall only remark, that I have not found this difficulty in distinguishing the true chancre or primary venereal ulcer. It is not to be supposed that there are no instances in which this, like other local complaints, may be altered by some accidental constitutional ailment. But it more rarely happens in this than in any other morbid poison; and whenever it does occur, the accidental symptoms subside, and the chancre shows its true character. Mr. Abernethy has before remarked, that "Mr. Hunter has most excellently described the clearly marked venereal chancre."* Other ulcers on these parts, whether arising from a morbid poison or not, can scarcely fail to fall under one of the descriptions of Celsus. As

* In the next page, Mr. Abernethy says, "Mr. Hunter, probably influenced by this belief, speaks briefly on the subject of chancres, and observes, that the sore induced by venereal virus, will partake of the prevalent disposition, or disease, existing in the constitution or part." Mr. Hunter's description of chancre is more minute than is to be found in any other writer. Still, however, it is brief, because the uniformity of its character is greater than in any other ulcer, and this character will always show itself before it is necessary to treat the disease as venereal.

that accurate physician wrote many centuries before the venereal disease was known, we may fairly imply that what is met with in him could not be venereal; and this conclusion is further confirmed, because, though he describes phymosis, paraphymosis—ulcers, dry and purulent, spreading superficially and deep—sanious—so corrosive that the glans is entirely lost under the prepuce—warts also, which he advises to be destroyed by medicine or fire—though he distinguishes all these, as curable, from others, under the names of *cancer in cole* and *phagedæna in cole*, which admit of no remedy but by the destruction of the parts—though even in these he is particular in marking every variety with which he was acquainted, carefully teaching us to distinguish the ulcerating phagedænic sore from the sore which spreads by sloughing, yet we find no mention of that species of ulcer which Mr. Hunter, and every accurate writer, consider as the true venereal character. Is not this a presumptive argument that every other kind of ulcer was well known by him?—that, therefore, they are not venereal—and is not his example sufficient to teach the possibility of classing them? It has been urged, indeed, that Celsus makes no mention of any infectious property. This has been already accounted for; and it is not less certain that he considered the diseases as in some measure peculiar to the parts, and some of them as absolutely incurable. No doubt he would have said the same of the ulcer with a callous edge and base, had he been acquainted with it. Ignorant of the use of mercury, he could have proposed no other remedy than the destruction of the part. By mercury we have learned to induce a new action on the whole constitution, and consequently on the part; and by such a process to supersede the effect of every morbid poison whose irritation is less than we can excite, or whose laws do not require a certain course before the cure can commence.

Though I have made thus free with Mr. Abernethy's papers, I consider his cases as most valuable records, and his remarks as

highly important. His want of decision, and the very errors which appear in his reasoning, prove that he had no particular system to support, and stamp a greater credit on his facts, if that were necessary. It becomes such of us, therefore, as are uninterrupted by the operative part of surgery, to avail ourselves of these materials, which will be much more satisfactory to ourselves, and probably to our readers, than a fabric composed of our own.

The only cases of primary symptoms, which seemed to have been at all doubtful, were those sores on the penis, which, after a time, acquire a considerable hardness, resembling what Mr. A. calls indurated chancres. These are of two kinds. The first are what I have called soft warts. Some have an even granulated appearance; in others the surface is more irregular, from some parts giving way. This irregularity is generally attended with an increased discharge, and followed by a spontaneous cure. They are very common both on the penis and in the vagina, and are easily cured by being well rubbed with any kind of caustic. As they are always above the healthy surface, Mr. Abernethy has very properly distinguished them by that mark. Chancres often assume this form, after their venereal character is destroyed; and probably other morbid poisons, whose cure is within the power of the constitution, may do the same, as soon as the influence of the poison has ceased.

The other kind of indurated sore may be traced from the same causes, or from any other which, by interrupting the healing process, induces the *vetustas* or thickened lips. We may account for Celsus passing it over in his description of the diseases of this part, because he had already described it among the forms which all ulcers will sometimes assume.* Mr. Abernethy's history of two of these cases

* See pages 55 and 56 of this work, with the note in 56. There is indeed this difference in the *vetustas* in these parts. The most usual place for these kind of ulcers is the lower extremities, where the interruption to the healing process is greater, and the circulation more languid. In the penis, the first cause only can exist.

forms a fine illustration of Celsus's description, and of the commentary I have ventured to offer upon it. The first case, [p. 118], did not resemble a venereal chancre, till it had *continued a fortnight, during which it could scarcely be said to be better or worse*, when it suddenly became indurated, and the surrounding parts inflamed. This ulcer, which, by its subsequent history, appears to have been the effect of a morbid poison, was cured without mercury, and also secondary symptoms which succeeded it.

The second case [p. 135, note] is contained in a note to an extract we have already made. This sore, Mr. Abernethy says, so exactly resembled an indurated chancre, that he did not scruple to advise confinement, with a mercurial friction. Finding no amendment, a more close examination was thought necessary, as to the "local treatment of the sore, which it was found the patient was constantly irritating by various stimulating applications."—The case seems to have been originally venereal, as the patient had kept his mouth slightly affected by mercury for some time before Mr. Abernethy was consulted.

These two are the only kinds of primary sore which seem to have impressed Mr. Abernethy with any doubts. They may be distinguished from the chancre by Mr. Hunter's character of that ulcer, which Mr. Abernethy has expressed in very few words—"A sore of a somewhat circular form, *excavated*, without granulations, with matter adhering to the surface, and with a thickened edge and *base*." Such a description never can accord with one which rises above the surface; nor with one which is stationary; nor which is indurated only in its circumference. All the other primary sores come within those enumerated already from Celsus.

Respecting the treatment of all such primary sores, as was before remarked, our first business is to ascertain whether the symptoms we observe are imputable to some peculiarity of the constitution at the time. If this is the case, the disease will soon show

itself. But if, after a reasonable delay, we see nothing like the ulcer with a hard edge and base, our next anxiety will, of course, be engaged in marking the progress of the disease, whether rapid or slow. If the latter, it is certainly our duty, in a case the nature of which we cannot ascertain, to use only common applications, and not hastily refer to a remedy, which will give a new and uncertain character to, and may perhaps exasperate, the disease. As long as the disease is stationary, or its progress is slow, we shall never have occasion to repent this delay.

If the disease shows itself by rapid ulceration, without a hard edge and base, we cannot leave it long without a remedy, and mercury is, of all others, the most likely to alter its action. But as we have here no hard edge and base to be absorbed, we should expect a very early amendment from mercury, as we find in sivvens. If, on the contrary, we find the disease increase under that remedy, we can expect nothing from our perseverance but to satisfy the mind of the patient, or to meet the suggestions of those who may be afterwards consulted. Though the case related above at page 32, from the Medical Transactions, does not seem to have been exasperated by mercury, yet we may recollect that it gave rise to the report of " six others, all which had become of a cancerous nature, from the two free and imprudent use of that remedy. In five of them the ulcers had spread, notwithstanding the use of every external and internal remedy which could be thought of, till they put an end to life; and in two of them had separated the skin, muscles, and peritoneum from their attachment to the os pubis, and laid the contents of the abdomen quite exposed to view before death."—[Medical Transactions, Vol. II. page 342.]—I am much afraid these events are more common than our mere records would teach us to expect. When they occur, delicacy to the patient prevents our knowledge of them on his part, and the surgeon feels little disposed to record a case, for the treatment of which he has nothing to suggest.

We must next remark, that some of these phagedænic ulcers end in slough, which produces a spontaneous cure. If these are attacked with mercury during their phagedænic state, the natural cure by slough seems protracted, and the ulceration becomes more rapid.* Mr. Abernethy's language, in one of the passages above quoted, confirms my opinion, that this disease is not uncommon; he adds, that he never knew secondary symptoms follow such cases "This, he says, is attributed by some to the chancre being removed by the sloughing of the surrounding parts." But the first symptom is a phagedænic ulcer, and not chancre; besides which, the constitution might have been contaminated before the slough took place. The inference which Mr. Abernethy would wish to draw is, that, excepting in the solitary instance related by Mr. French, in Mr. Hunter's Treatise, we have no case on record, or which has come within his own, or the knowledge of those among whom he has enquired, in which this morbid poison has been attended with secondary symptoms. Might it not in this case, then, have happened from the administration of mercury in the early stage of a disease, which ends in a spontaneous cure, as we find sometimes happens in yaws from a similar practice. At all events, it is certain that there are phagedænic ulcers peculiar to these parts, which not only spread with alarming rapidity, but are exasperated by mercury; and that when mercury is laid aside, some of these recover their proper character. Some slough and heal; others continue to extend by repeated and regular sloughings, till the diseased action ceases spontaneously; and others continue their phagedænic property with less, or with the same rapidity, as during the use of mercury. For these last, when we are convinced of the inefficacy of mercury, it will be right to try the acids and other remedies, constitutional and local; but if these and every other means prove ineffectual, nothing seems to remain but the practice proposed by Celsus, of causticating the ulcerated surface, or cutting beyond the diseased part.

* See page 79.

Though this, in a young subject, may not always be so serious in its consequences* as he may apprehend, yet, without doubt, nothing less than the preservation of life, with the probability of preserving a part of the organ, can justify such an operation. On this account we are called upon, by every obligation, to assist in ascertaining, as early as possible, every character of ulcers, which sometimes destroy life by their rapidity, and sometimes by the remedy which is used for their cure. Among the other enquiries, one of the first should be, whether the skin of the penis was at all broken either before or during the period when the disease was probably contracted. I have already remarked, that many of these anomalous cases have occurred by the application of the secretions of another person to a part, the skin of which was broken.† Without doubt this will expose a part more to the venereal contagion; but wherever the appearance of the sore is different from what we expect from that poison, and particularly where the intercourse has been such as is far removed from suspicion, this enquiry becomes still more important.

The next and most careful examination should be, whether the patient has fever with these primary sores. This is not only of consequence, inasmuch as it is contrary to the general character of the chancre, and is so frequent an attendant on the anomalous morbid poisons, but because it should have a considerable influence on our practice. Whenever we find fever, we should be particularly careful to avoid mercury as long as the local symptoms will admit. The cases of phagedæna, succeeded by slough and spontaneous cure,

* See Edinburgh Medical Essays abridged, Vol. II. page 130. The case of amputation of the penis concludes thus:—"What we took for a fungus forms now a well formed and proportioned glans penis."

† See page 46.—Mr. Abernethy also remarks, that, "though these sores must be imputed to some specific contagion, yet that men, who have naturally an irritable state of the prepuce, are most liable to them." Page 167.

have all been described with fever, when the accounts have been accurately stated.* The same has happened with several other cases, whose continued treatment with mercury has proved calamitous or ineffectual, or which have spontaneously subsided when that remedy has been early laid aside. It is not less certain that some others of this description have readily yielded to mercury, when its use has been deferred till the disease has run a certain course, or till the constitution has lost its high susceptibility of its impression, so as to be fitter to receive that of mercury.†

Mr. Abernethy remarks, that the irritable state of the constitution was in some of his cases so great, as to counteract the operation of mercury.‡ In another place he observes, that some of these diseases, if healed suddenly by art, are very apt to return; but if more slowly, or after the disease has exhausted itself, that the cure will be permanent.§ All this serves as a fair illustration of what was remarked in the passages before referred to, when we were speaking of actions excited by mercury.

I shall conclude what may be said on this subject of primary anomalous symptoms with the following aphorisms:

Whenever we see a sore in these parts without pain, and scarcely distinguishable from mere excoriations, we should content ourselves with the most simple applications, and without any internal remedies.

* See page 47. † See page 77 and 78.

‡ Surgical Observations, page 152. Dr. Jackson has made the same remark of some fevers, in which he tried the effect of mercury. In the worst cases he found the constitution could not be affected by it; and seems to suspect that in other cases, where salivation was produced, the fever had subsided before the constitution felt the effects of the remedy.—Remarks on the Medical Departments, &c. page 327.

§ Surgical Observations, page 144. Mr. Abernethy's observations in this passage, are directed to cases of sore throat, which probably were considered as secondary symptoms, but the rule is not less applicable.

If the sore heals firmly for some days, or if it continues stationary, or spreads only superficially without pain, we may be satisfied it is not venereal, or that it has not yet acquired a venereal character.

If attended with pain, we may suspect a morbid poison of some kind.

If the inflammation is considerable, and the disposition to ulcerate rapid, or slough should have commenced, we shall probably have fever at the same time. In all these cases we must attend only to the general and local symptoms, as in ordinary cases, by allaying the inflammation and fever.

If the fever and ulceration both continue, our prognosis must be unfavourable; but the longer we delay the use of mercury, the greater will be the probability of success from it.

If the disease is not soon relieved by mercury, we have reason to fear it will be exasperated by it; and if we find this the case, we must refer to those remedies which have been before suggested.

If slough should have commenced, and its extent appear to be considerable, the probability is, that as soon as it is cast off, the part will skin over without granulation.

If granulations follow the rapid separation of a slough, we must consider the case as common mortification.

But it may have arisen from inflammation, excited by the presence of a morbid poison; we must, therefore, carefully examine whether any ulcer remains where the slough has not taken place, and watch its progress, so as to ascertain its character.

If the slough is superficial, and the part from which it is separated looks particularly clean, that is, retains the crude surface of separation, neither skinning nor granulating, we may expect a succession of sloughs along the surface; and in the early stage of such a disease we shall gain nothing by mercury in any form.

If, as the inflammation subsides, or after the slough is separated, we find, instead of healing, a hard and somewhat painful ulcer, without any restoration of parts, we may be certain of a chancre.—In these cases the sloughs will generally be small, almost circular, and about the size of a beginning chancre.

If the ulceration should be slow, and without the character of chancre, the fever somewhat abating, we have every reason to believe the disease will cease spontaneously, or that as soon as the constitution is become familiar with it, that it will yield to mercury.

This, in addition to what has been said of the soft wart and the thick lipped ulcer, will, I trust, be sufficient for every practical direction in the treatment of primary anomalous symptoms.

But whilst there is this certainty in distinguishing primary, it must be admitted that secondary symptoms are by no means so readily ascertained. It is but justice to add, that these were the only cases in which Mr. Abernethy was undecided; and we cannot be too grateful for the number of instances he has produced, and the descriptions he has given, imperfect as some of them may be.

The difficulty of distinguishing secondary symptoms, seems to arise from our associating so much the idea of copper spots in the skin, with the secondary symptoms of syphilis. We should recollect that all eruptions, whose inflammation is slight, and whose progress is not rapid, must begin in this manner. Even in the exanthemata, if we watch carefully, we shall discover something of this kind as the eruption commences. The appearance arises only from the transparency of the cuticle, which exposes the slightly inflamed cutis underneath. In those morbid poisons, which have not a certain course to run, the progress is so slow, that this copper spot is more circumscribed and permanent. It is of different complexions, according to the degree of inflammation, which in syphilis is so trifling, as at first only to produce a slight separation

of the cuticle, which continues to be formed again for some time before ulceration, or even scabbing, takes place. The same happens in many other morbid poisons; nor ought we to be more surprised, if we cannot distinguish these eruptions at first, than at our incapacity in their early stages to distinguish small-pox from chicken-pox. The reader will recollect, that the early state of secondary symptoms in sivvens, as I have transcribed them from Dr. Gilchrist, are the same as might be given of syphilis. In all the cases described by Mr. Hunter, in which the disease appeared on the skin, whether they healed spontaneously, or yielded to mercury, the copper spot is the first marked secondary symptom. Whenever we see this appearance, we may be certain the subsequent disease will not be so rapid as to produce any danger from a delay, during which we are to watch the progress of the eruption. The numbers enumerated by Mr. Abernethy, which recovered without mercury, must satisfy all those who are of opinion that syphilis is only curable by that remedy. Such also as agree in the opinion, that the venereal disease is of late origin, will be struck with the similarity between some of Mr. Abernethy's cases and Celsus's account of *Ignis sacer*; and it is not a little remarkable, that one of the patients was cured by the *remedium fortissimum* or *fortuitum* of Celsus, that is, by a severe febrile complaint, during the continuance of which all the symptoms disappeared, without returning.*

If there is this uncertainty as to the appearance of secondary symptoms, it is fortunate that the danger of delay is proportionally small. It is also worth observing, that when these secondary symptoms are more ready in running into ulceration, they are, for the most part, more readily cured by mercury, as if the disease had completed its necessary course, and as if the same original irrita-

* Surgical Observations, page 117.

bility of constitution, which produced this early ulceration, was attended with the same susceptibility to the impression of mercury. This renders the treatment of secondary symptoms on the skin much more simple.

But when secondary symptoms appear on the bones, the question is more complicated. We must learn whether their history agrees with that series before described in syphilis, and we must carefully observe the progress of the disease itself. If the disposition to suppuration should be rapid, we have every reason to believe that the case is not venereal; and if the symptoms are not very urgent, we should attend more to the constitution than the sore, but most of all to the history. If these sores have appeared after others which were cured in the bones, under the administration of mercury, we may be sure they are not venereal. If they have relapsed, after a long, continued, and powerful salivation, and after remaining apparently sound for some time, we shall have reason to form the same conclusion; and if this salivation has been first conducted or repeated under our own observation, however successful it may seem in relieving the part for a time, or permanently, yet without preventing the re-appearance of the disease in the same order of parts, that is, in some other part of the same or of another bone—under all these circumstances we shall not, I think, scruple to avoid the repetition of a remedy, the necessity for and the efficacy of which has been found so uncertain, and the danger of the continual repetition of which we have so much reason to dread. If the bone is bare, we should scrape it; we may have recourse to other remedies, but at least avoid mercury as long as possible. Not only the acids, but the arsenical solution, may be tried, and every other means of restoring the constitution attended to. If, after a time, the sores become chronic, it is possible they may still yield to a very slight exhibition of mercury; but whenever we make up our mind to begin such a course, it should always be with frictions, so

that we may continue it in a satisfactory manner, or relinquish, if we find the disease exasperated, or even if the change we have induced should authorize us to believe that the slight stimulus we have excited has been sufficient to induce a healing process, now that the disease is probably within the power of the constitution.

These cautions might be considered as superfluous, if all that has been said, concerning the improper exhibition of mercury, were mere fables. But I need not remind many practitioners, that the number of cases recorded are few, compared with those which actually occur in practice. Some may think it enough to reason on facts familiar to themselves; but as the use of a practical book is greater in proportion as the opportunities of the reader are fewer, I shall not dismiss this part of the subject without again reverting to the serious consequences of the improper or too frequent exhibition of this important remedy.

Though Mr. Haslam is the only writer on Madness that I recollect, who has enumerated mercury among its causes, yet the fact is well known. A gentleman, in full practice, in London, whom I met in consultation on a case which had resisted repeated mercurial courses, observing the strong inclination of the patient to begin afresh, from the temporary relief he had usually found, remarked, that he would certainly go on till he arrived at Bedlam, as if that were no uncommon event. If I have selected only one instance on my own authority, it is because this case was so well known to such a number of gentlemen, one of whom I have named. It is not, indeed, so easy to prove what the consequences of delay might have been in those cases which are not yet reduced to any certain laws. But as we see that many cases, equally suspicious, have done well without that remedy, or when it has been applied later, it is at least probable that the same might have happened in such as we have found exasperated by a contrary treatment.

The yaws furnish us with a satisfactory proof of one morbid

poison of this description; and it is hardly credible, though every writer on the subject, since the publication of the Edinburgh Medical Essays, has repeated the same caution against the early use of mercury in this disease, yet that it is not entirely laid aside. We are to suppose that those Essays must be in every library; if so, they are certainly ill attended to, which will, I trust, excuse my transcribing the substance of the ingenious author's inductions.

"The venereal disease and the yaws," says he, "as far as I have described the latter, are distinct distempers; but the symptoms of the yaws ill cured, coincide so exactly with those of an inveterate pox, that in most cases it will be difficult, if not impossible, to distinguish them. The symptoms are violent pains in the limbs, in some attended with nodes and exostoses, in others with ulcers, which render the bones carious."—After this the author proceeds to give a few cases, the most remarkable of which was a young woman, who caught the yaws of her husband. As soon as it was discovered, she was immediately salivated, and the salivation kept up for seven weeks, during four of which she was unable to speak. After this she seemed perfectly well, recovered her strength, and went to England. Finding herself ill, and growing worse in spite of every remedy, she returned to Jamaica, and was under the author's care. She had now ulcers on her arms and legs, which not giving way to any dressing or alterative remedies, she was again salivated; but even during the process all the symptoms were exasperated. Having dragged on a miserable existence for between six and seven years, distressed with pain and carious bones, she died.

"When I came to this island," continues the author, "it was the practice here, as soon as the yaws appeared, to give twenty-five drops of a solution of two drachms of corrosive sublimate, in eight ounces of strong rum, in the morning, drinking warm water after every puke, and they would vomit and spit all the forenoon. This dose they repeat every morning, increasing the quantity five drops

every dose. In a few days they were seemingly well; but most who had been treated after this manner, either broke out again, or in time complained of gnawing pains in their bones, or were subject to ulcers in several parts of their bodies. The disease, at its second appearance, was longer of coming to an height, and required a longer course of mercury to clear the skin; and sometimes, after all, they would relapse a third and fourth time. Of these patients who were affected with ulcers, I have succeeded with some by salivation, and long courses of the æthiops, with the decoction of the woods in lime-water; many I have been foiled in, and never been able to cure, but left them, I think, rather worse than I found them: nor can I pretend to better success in those who have complained of pains in their bones, which have generally ended in nodes, exostoses, and caries. The bones of the arms and legs frequently break without any external violence.

"A negro man, after having had the yaws, complained of pains in his limbs, and had been useless in the plantation for nigh twenty years, most of his bones being full of nodes, or exostoses, and caries. His os humeri broke in the middle, without any external accident. I reduced and dressed it as a common fracture. About six weeks after, when the callus ought to have been grown strong, I found the ends of the bones move easily one upon another; and upon a gentle extension of his arm, they were a full inch distant from one another. In about twelve months more, the os humeri was consumed entirely within an inch of the scapula, and about the same distance from the elbow. Soon after this he died tabid."

As this writer seems to have been the first to reform the practice respecting the treatment of yaws, probably that very circumstance induced him to withhold his name from a paper, in which he was obliged to arraign the conduct of his brethren. For a knowledge of his name and description, we are obliged to Dr. Ludford, before mentioned, who, in his inaugural Dissertation, calls him the

first British writer on the disease. *Joannes Home olim nosocomii navalis Jamaicensis chirurgus, nunc vero vulneratorum et ægrotantium præfectus regio diplomate constitutus.* As these dissertations are frequently not circulated but among the author's friends, I am glad of this opportunity of adding a small share of publicity to so deserving a name.

I shall conclude this chapter with a few cases and miscellaneous remarks, which, though connected with the subject, could not be so easily introduced in any other part.

There are, in every complicated case, certain embarrassments, which will make it difficult to learn all those minutiæ, a knowledge of which is absolutely necessary, in order to ascertain a disease. This is particularly the case in syphilis, because our diagnostic often depends as much on the series and order of past events, as on the symptoms submitted to our examination. Sometimes it happens that a surgeon, fancying the appearance of secondary symptoms in his patient disgraceful to himself, will be very backward in admitting them, especially if he has a pre-conceived notion that they can be infallibly prevented by a particular mode of conducting the mercurial course. At other times the patient, either anxious to begin a new course, from the apprehension of the poison *taking deep root* in his constitution, or tender of his surgeon's reputation, will under-rate the course of mercury which he has gone through. In these last instances a practitioner who is consulted, and has not witnessed the previous process, may hastily conclude that the ulcer on the penis, which yielded to mercury before the constitution had felt the effects of the remedy, could not be venereal, and consequently that the secondary symptoms, how much soever they may resemble the syphilis, cannot in reality be such. I shall illustrate both these circumstances by cases, which will not only afford some practical remarks, but also furnish hints to practitioners, under events which may sometimes embarrass them.

Sen. —— of a strong mind and constitution, but of an irritable temper, had contracted a chancre, of which he was cured in the usual way. About two months afterwards, he became feverish and languid, in a manner much resembling a slight intermittent. After this a discoloration, and consequent superficial ulceration, appeared in a few places about the breast. His fever was relieved for a short time, but recurred again in the same manner, with slight sore throat. Like most men under similar circumstances, he suspected all these symptoms to arise from a venereal source. His surgeon, however, having conducted the mercurial course with the greatest attention, and repeated it as soon as the effect of the first was over, assured him he was in no danger. But the symptoms continuing, he was obliged to give way to his patient's importunity in occasionally allowing him to take a few calomel pills. These always seemed to afford relief; but they never were persevered in, and all the complaints soon returned. In this manner things remained for several months, till at length the patient and surgeon became mutually tired of each other, and I was consulted.

At this time the ulcers were partly scabbed, very superficial, and though the appearance was suspicious, yet ulcers of that complexion are not uncommon from a mere low state of health, or poorness of blood, as it is usually called.

It seemed, therefore, prudent to wait, in order to ascertain whether the symptoms increased when the effect of the mercury might be supposed to have ceased. A very short time sufficed to show the true venereal character of the ulcers on the breast, and also of one in the throat. A regular mercurial friction was now begun, and continued till a powerful salivation was produced, and kept up for some time. All the symptoms ceased. The remedy was discontinued, the salivation gradually subsided, and the patient was cautioned not to be surprised if the bones should hereafter be affected.

In about two months afterwards he sent to me, complaining of the same feverish symptoms as before, and violent pains in the cranium. At first it seemed doubtful whether these symptoms might be the effect of his mercurial course, aided by his imagination, in consequence of having been told, that there was as yet no certainty whether his bones were affected or not. But in a few days his voice showed that his nose was affected, and the pains in his other bones became stationary in the most suspicious parts. The mercurial friction was therefore begun in the usual form, and persevered in to a considerable extent, on account of the slow action which takes place always in bones. At length, however, having continued the frictions about five days after the salivation, and the mercurial fever had been raised pretty high, the course was discontinued, from the presumption that the irritation would not only continue for some time, but increase, as proved to be the case.

At this time I was seized with a fever, which not only prevented my attendance, but for a time precluded any intercourse between us. I learned, however, that he recovered his health as the mercurial irritation ceased, but was soon afterwards seized with rheumatic pains. As these grew more violent, he was prevailed on to apply for relief; but the recollection of my former attention induced him to acquaint me with the step he had taken. I endeavoured to learn his exact situation from his relation, whom politeness induced him to send to me on the occasion. If in this I was unsuccessful, the error was partly my own, in not recollecting that none but a medical man could be aware of the importance of minute exactness in every part of the account. To my enquiries, whether the pains were seated exactly in the same parts of the same bones, as before the last salivation, I was answered in the affirmative. Every other enquiry was, of course, less pointed, but the accounts I received were such as induced me not to interfere with his present treatment, at least till we could meet. Unfortunately this was not till it was

too late. His physician, finding he had in two relapses, as he conceived them, been relieved by mercury, supposed that mercury must still be the remedy, and that the only caution necessary was to avoid the mercurial salts, and depend on frictions alone. This plan, as usual, produced a temporary relief; but even before the salivation was excited the pains returned, and never left him during or after the remainder of the course. He had, besides this, a purulent discharge from his nose, evidently the effect of an exfoliation, or the separation of a carious part of the bone. This, of course, continued after the mercurial course was at an end; and the patient, or his friends, not being aware that such a process was necessary towards the restoration of the bone, became dissatisfied with the last physician.

Unfortunately the gentleman next consulted was not so much averse to the mercurial salts. He exhibited the corrosive sublimate after the manner proposed by Van Swieten, promising to effect a cure in twenty days. Before that period arrived, the patient's stomach was so much weakened, that he could neither retain the medicine nor any thing else. However, the process of exfoliation happened to be completed during the time, and the remedy had the credit of curing the nose; but the sickness increasing every day, the physician and patient became mutually tired of each other.

In this situation I found my unfortunate friend on my recovery, which had proved very tedious. His pains greater than ever, constant sickness, and occasionally diarrhœa, which threatened to destroy him. On a more accurate enquiry from himself, I now found that his rheumatic pains, which had affected him after he had been first salivated for the disease in his bones, were seated in parts quite different from the former. Many were in the joints, and none in the places of which he had previously complained. They were likewise unaccompanied with fever, excepting what the pains might

occasion; that is, without that kind of hectic which has been described as attendant on this stage of the disease.

By the repeated use of mercury, and most of all by the continued use of the sublimate, his digestion was so weakened, that all which could be done for him was to try the effects of tonics and change of air. The remedies were varied without end, and without advantage, for six months, during the greater part of which he remained in bed. At the end of that period his diarrhœa returned with such violence, as to yield to nothing, till he expired.

The next case proved more fortunate. A gentleman, who had been cured of a chancre by a physician, for whom he had a strong personal friendship, found, about two months afterwards, a soreness in his throat. Though he wished for my opinion on this occasion, he felt extremely anxious that no blame should be attached to his friend. He, therefore, gave me to understand, that he had been cured of a chancre about the time above specified, but that he had used very little mercury, much less, indeed, than he had been advised. As the physician was present, and did not contradict the account, it appeared of course doubtful whether a true chancre could be so easily removed. At all events I was persuaded that the throat, if venereal, might be readily cured, whenever we chose to set about it. It was, therefore, agreed, that the patient should try, for a few days, the effect of sarsaparilla, till we should see what the progress of the ulcer would be. It continued to increase, with all the true syphilitic character, and the patient had the usually attendant hectic. Though there was now no reason to doubt the nature of the case, yet as a venereal sore throat has nothing formidable in it to me, I had no objection to continuing, for a few days, a plan which seemed agreeable to the patient, and as it appeared to reflect less on his friend. But in the mean time a medical gentleman offered his gratuitous assistance, and having no doubts on the subject, urged the immediate commencement of mercury, at least

in small doses of the salts, if I were unwilling to do more. As my only objection was to doing so little, I begged, whenever we began mercury, which I had no objection to do at any time, that it might be regularly by frictions. This was acceded to, and my patient was found so ready at his new employment, and so well prepared for the penance he was to go through, as soon taught me that he had by no means been hasty in relinquishing his former application of the remedy. The disease regularly gave way as the mercurial irritation advanced.

This patient might have been saved all the inconveniences he suffered, from the time I was consulted till he began the use of mercury, had he explained the history of his case with accuracy, or could I have convinced him of the importance of that accuracy, and that no blame would remain with his friend, should his throat prove venereal.

I have given these two cases, to show that it is often necessary, when our patients are sufficiently intelligent, to make them acquainted with the object of our enquiries. With some we are obliged to gain our information by the best means we can, and if we have not an opportunity of seeing the practitioner who had the charge of them, we are under the necessity of judging from imperfect accounts.

There is another set of patients, who, as I before remarked, are so persuaded that their case is venereal, that they never can believe themselves cured till they are salivated. That process sometimes proves a cure to many who, to use the hospital phrase, have the *disease in the head*. I shall not interfere with the mode of treating these patients, the question not being altogether medical.

In the former part of the work, I mentioned that nodes would sometimes occur, whilst chancres are giving way to mercurial frictions. When this is the case, there can be no doubt that they arise from the remedy only. But when we first see them at a later period,

the question is more uncertain. I believe the mercurial salts are more apt to produce this effect in warm climates; if so, it may account for the reputation the acids acquired in the cure of this supposed stage of syphilis. In Madeira such cases may be said to be common. A gentleman consulted me, soon after my arrival, for nodes in both his shins, the pain of which was intolerable, and for which he had been taking mercurial salts, and rubbing volatile liniment, mixed with some mercurial preparation, for several months. There was no difficulty in distinguishing this case, because the nodes disappeared in one part, and rose in another. Though he laid aside all mercurial remedies, he was full two years before his nodes entirely disappeared; nor was he ever afterwards, for any length of time, free from rheumatic pains. Another gentleman had permanent nodes, which arose from taking mercurial salts during a gonorrhœa. When I saw him, he had been four years afflicted, occasionally taking pills when the pain was greatest. I advised him to lay aside mercury, and by attention to his general health he recovered somewhat, though his situation was delicate. At this time he went to Lisbon on business, where he was persuaded to undergo a salivation, under which he died.

In colder climates, tumors on the bones are not uncommon, from a low state of health, which produces a state of the constitution usually called scrofulous. The same causes will sometimes produce vibices, as they are called, in sea scurvy, which may be confounded with the copper spots of syphilis.* Both are easily distinguished. The nodes are usually much softer, and without pain. The spots are less circumscribed, disappear, or spread without loss of the cuticle, and sometimes occur in different parts at different times. On these occasions the gums should always be examined, and if they confirm our suspicion of scurvy, the vegetable

* See Hunter's Treatise, page 380, bottom paragraph.

unfirmented acids will, with more certainty, prove efficacious. I have seen them succeed where the gums have not shown the disease. If the season or situation is less favorable for procuring lemons or other fresh fruits, the decoction of malt will often prove efficacious. The nodes are of little consequence: if unattended with pain, as is usually the case when soft, they require only attention to the general health. When hard and painful, they are much more untractable than venereal tumors. But these cases make no part of our enquiries, excepting the necessity of distinguishing them from the effects of Morbid Poisons.

ADVERTISEMENT.

THE following Paper was composed by the Author, during his residence in the island of Madeira. The intention was to offer it as a supplement to Dr. Thos. Hebberden's communication, already published in the Transactions of the London College. In order to procure the most exact description, application was made to the Governor of the island, that there might be no impediment to the examinations; and to give due authenticity to some peculiarities in the disease, not less extraordinary than contradictory to all former accounts, the names of the various gentlemen are inserted who attended officially or from choice. For the same reason the form in which the paper was drawn up is preserved.

In this form it was read before the College, and ordered for publication in their next volume of Transactions; but that volume being delayed longer than was expected, the Author was permitted to withdraw his Paper, and print it in the present work.

It may be said, that the disease, not being contagious, does not come within the description of a morbid poison. This is probably true. But in this country, where it is little known, it has been often confounded with other complaints. A correct description is also the more necessary of a disease which so rarely appears: for that it does sometimes appear is not less certain. Dr. Baillie has introduced me to a case, and informs me of another which occurred to him several years ago. Mr. Pearson also tells me he has met with a case.

It was thought the Paper might be rendered still more useful, and the disease further illustrated, by comparing it with two others, which have some symptoms in common, and which have been confused by the application of the same terms.

CHAPTER XVIII.

OF LEPROSY,

COMPRISING A DESCRIPTION OF THE ARABIAN LEPROSY, OR ELEPHANTIASIS OF THE ANCIENTS, THE ELEPHANTIASIS OF THE MODERNS, OR THE BARBADOES LEG, AND THE LEPRA GRECORUM.

Memorial presented to His Excellency Don JOSE DA CAMERA E LEME, *concerning the Inhabitants of the Lazaretto near Funchall. By* JOSEPH ADAMS, *M.D. Extra Licentiate of the Royal College of Physicians in London, and Physician in the Island of Madeira. With a further Account of that Institution.*

[Read before the College of Physicians, in May, 1806.]

THE unfortunate objects of the lazaretto, near Funchall, cannot but attract the notice of every stranger: a disease entirely unknown in the colder regions, must, in a particular manner, excite the attention of an English physician, were it only from observing that of some of the men, whose scalp is well covered with hair, the chin is perfectly smooth. I had also remarked, that this want of beard is in some attended with a delicacy of voice, not indeed such as we perceive in eunuchs, but rather like that of boys. Still a doubt remained whether this might arise from any change in the figure of the throat, which in most is considerably deformed by irregular prominencies. The voices of some are also very nasal, from similar causes.

To satisfy myself whether this want of beard arose from any imperfection in the organs of generation, I obtained permission of his excellency, Sen. Don Jose da Camera, &c. &c. &c. our governor, to make whatever examination I thought necessary, and his order to lay the result before his excellency.

In the beginning of March, 1803, Major Patrone, inspector of artillery, and interpreter general of the island; Mr. Banger, an English merchant; the chaplain of the hospital; and the inspecting officer appointed by the *Camera* for the year, did me the favour of attending me to the lazaretto. I also procured a midwife, to assist me in the examination of the females.

The first person we examined was a youth named *Gonsalves*,* about twenty-three years of age. His face is strongly marked with the disease, with which he has been several years afflicted, though he has resided only two years in the house. His voice is nasal, his *uvula* either lost or concealed by the tubercles about his palate. Has no hair on his chin or pubes; his testicles can scarcely be felt; his scrotum, and all the organs, very much resemble those of a boy of six or seven years old, excepting that the prepuce is somewhat elongated. With all this he is chearful, being free from pain; and when asked whether the state of his throat made it difficult for him to swallow, answered, that his only difficulty was in procuring enough to swallow. His father died of the disease.

The second, who passed under examination, was said to be one of fifteen children, born of both healthy parents, and grand parents. Of these children, four died young, and, by *his* account, of the disease, the rest are all healthy. The chin and genitals in this youth are as in the former subject, though he is considerably advanced beyond the age of puberty.

A third boy is more than twenty years old, and the son of a leprous woman. He has no beard, nor hair on the pubes. On the first examination it seemed that if he had any testicles, they were

* See Plate I.

so small as not to be distinguishable from the corrugation of the scrotum. But at a subsequent visit, when the season was more advanced, it was found that this uncertainty arose from contraction by cold, as the testicles were now very perceptible, though very small, and hanging at the bottom of a narrow scrotum.

Such, with very little exception, may be said to be the description of all the young males, excepting *Caëtano de Aguillar*. This youth was born with cataracts in both eyes, calls himself eighteen years old, and has been two years afflicted. His face shows no appearance of disease, but he has no beard, which, at his age and in this climate, is scarcely natural. He has also no hair on his eyebrows; yet this cannot be considered as a mark of the disease, as will hereafter appear. His voice and genitals are natural, pubes somewhat hairy, but his scrotum is quite free from hair. He has tumors about the upper part of the thighs, which will hereafter be noticed. His legs are scaly, swoln, and have small tubercles.

If this youth is correct in the account of his disease, it did not commence till his sixteenth year, a period much later than that of puberty in this country. We may therefore, I think, safely decide, that all such boys as are attacked with the disease, before the age of puberty, never acquire the distinguishing marks of that change in the constitution; on the contrary, that the testicles, for the most part, diminish, and, as far as can be collected from their conversation, that they retain the simplicity of infancy in whatever relates to the sexes.

The first of the men, whom we examined, was to appearance about fifty. His beard was natural, as well as the hair on the pubes and scrotum. His testicles also had a natural healthy feel, but the penis is tuberculated. This, and the last mentioned subject, are the only two in whom the testicles were found perfect.

Manoel de Nascimento has been a porter in the house for fifteen years. His voice is natural, his chin and genitals hairy, as is indeed

the case with his whole body. His testicles are soft, thready, and much diminished. His scrotum is strongly marked with tubercles, and the centre is the most dependent part: his legs are beset with elevations, most of which are much broader, smoother, and more regular, than those on the faces of the other lazars, and particularly so than those in their legs.

Joao Bernardo has been deemed a lazar about three years, and appears about fifty years old. His face is sufficiently marked with the disease; his ears and nose tuberculated. His voice deep and masculine; his chin and genitals hairy; but one of his testicles is soft, the other has lost its figure, and feels thready.

Francisco Fernandez is married. His wife * is also a lazar, and an inhabitant of the house. They have had a child since the commencement of his illness; but he assures us that at present he has no desire for women. His beard has disappeared in his upper lip and in part of the chin. He says also, that the hair about the scrotum lessens. On a subsequent examination, it was found that his testicles, particularly the right, were much wasted.

Manoel Fernandez is thirty-six years old; has been ill thirteen years; says he never had a beard; has but little hair on the pubes. His testicles are much wasted; has only one eye.

The above cases are sufficient to show, what was afterwards proved by an accurate general examination, that when the disease attacks a male subject before the age of puberty, he never acquires that state; and that such as are affected later in life, gradually lose the power of procreation, as far as can be judged by the changes which take place in their organs.

The proofs of a defective organization in the women are scarcely less striking, as far as those organs can be examined. The labia pudendi and mons veneris have disappeared; nothing appears but a fissure, without any projection. In most a few light curled hairs

* See Plate III.

are scattered over, which do not at all conceal the parts. The breast have generally disappeared; but this may be the effect of age and meagre diet: indeed, the defect is not universal. But in all the nipple is smooth, having entirely lost its porosity; seems flattened and much wasted; can never be serviceable for suckling; and little or no areola can be discovered. They are also free from hair under the arm pits. Though hair in this part is not a constant attendant on puberty in whites, yet it is very general, and among blacks so constant, that slave merchants use no other test to ascertain the ages of females. I was informed by the nurses, that of the ten women now in the house, only three menstruate, though more are within that period of life. Of these three, the evacuation in one rather resembles a hæmorrhage: in the other two it is natural.

Chronic diseases, which seem to arise spontaneously, and which never yield to the unassisted powers of the constitution, must have their cause in climate, predisposition in the patient's constitution, peculiarity of diet, or probably in all. If the first and third causes only exist, we may hope for relief in change of climate and mode of living; but if the disease never occurs, excepting where there is an original predisposition, the cure can only be permanent as long as the patient is removed from the exciting causes. As the predisposition is born with the patient, the inference follows, that he must have derived it from his parents: we are, therefore, apt hastily to conclude, that his offspring must be infected also; and because this sometimes occur, we fix no limits to our extravagant terrors concerning what are called hereditary diseases. We do not consider, that unless the disease can be traced back to our first parent, and thus involve us all, it must have originated in one whose ancestors, from the beginning of the world, were free from it. Still less do we take notice, that when the disease has appeared in a family, it often ceases with the individual who is the first subject of it, and can rarely be traced beyond his immediate offspring.

It is not my intention to dispute that the predisposition to leprosy is hereditary. The number of subjects in the lazar house, from whose parents the disease may be traced, is sufficient to establish this fact. It is, however, a most important remark, that all those subjects, who appear to derive the disease from their parents, have been attacked at an early age; and as far as we could learn, in those in whom the disease has shown itself at a later period, it has originated with themselves. It is not less notorious, that the disease has gradually disappeared among the wealthier families, since they have adopted a more generous diet, and since the increased mildness of the government has relieved them from anxieties, as destructive to health as poverty or want. The disease, though at present nearly confined to those whose diet is the poorest and most precarious, is yet, in a few instances, found to visit such whose situation is above want: and many, in the most abject state of poverty, escape it. Hence it follows, that no external cause will produce it, unless the constitution is *predisposed* to it. Now a predisposition must be derived from the parents, whether they are diseased or not.

To render this proposition more obvious, we need only remark, that the lazar house affords instances of more than one child born of the same parents, yet whose parents and grand parents have been free from the disease. From whence could this general predisposition in the children originate but in the parents? This is not peculiar to leprosy. The English consumption will prove fatal to a numerous offspring, whose parents will survive them to an advanced age.—To produce a still more familiar instance. How frequently do we find a striking resemblance between the brothers and sisters of a family, yet none of them shall show any similitude to either parent, and this when the chastity of the mother has been above all suspicion?

From all this it follows, 1st. That no causes, we are acquainted with, will produce the disease, excepting in constitutions predisposed to it.

2dly, That though the predisposition is more to be apprehended in those who are born of leprous parents, yet it is not a necessary consequence of such an event, and is often found in those who are born of healthy parents.

3dly, If, as has been asserted, the disease is much less frequent among the wealthy families, it is probable that where a predisposition exists, it may be prevented from coming into action by climate, or even by a generous diet, free from excess; but this can never be ascertained, because we cannot be certain that a predisposition exists, till the disease shows itself.

Lastly, That, as far as facts have hitherto directed us, when the predisposition is derived from a leprous parent, the action of the disease takes place so early in the offspring, as to preclude the possibility of his perpetuating the race. If this can be well ascertained, there will be every reason to trust that the children of such lazars as escape the disease till the age of puberty, will continue free from it during life.

Having thus, as far as it can be, settled the question concerning the hereditary nature of the disease, our next enquiry should be, how far it is infectious; but as this more immediately belongs to the mode of treatment, I shall first proceed to mark the particular signs by which the disease may be ascertained.

Among the unequivocal marks, we may consider tubercles about the face, particularly on the external ear, *alæ nasi*, eye brows, or forehead. These tubercles, till an advanced stage of the disease, are not only smooth, but have, for the most part, a higher complexion than the natural skin, approaching nearer to the sanguineous hue, appearing as if semi-transparent, splendid as if the surface were smeared with oil, and, on a closer examination, sometimes

exhibiting small blood vessels ramifying on their surface. This appearance is not equally marked in all; but the following may serve as their general history and form.

At first they rise only a little above the skin, have the natural colour of that membrane, or are even paler. The circumscription is irregular, seldom circular, but beset with lateral projections, which, however, are not angular. The colour and elevation of the tubercles will, in most instances, remain stationary for a considerable time; as they become redder, or in people of a fairer complexion, more transparent, they acquire the splendour before mentioned. Commonly the centre becomes more elevated, and so on towards the edges, so as to render the tubercle somewhat rougher. They still, however, retain their splendour, till they crack in one part, in consequence of which the whole is suffused with a white furfuraceous substance. As the tubercles increase in number and size, the face becomes proportionally more distorted, the forehead loses its smoothness, the eye brows become prominent, and the hair of them is entirely lost, or discoverable only between the tubercles. The eye lids are thickened with very small tubercles, so that when the eye is open, its figure is almost circular; frequently nothing beyond the iris is visible; the lashes, for the most part, remain. The nose spreads considerably, the cheeks are much enlarged, and the chin partakes of the disease. If the number of tubercles is considerable, the beard is lost, or is only visible in the interstices. It has been before remarked, that if the disease commences before the age of puberty, the beard never appears. The lips are frequently tuberculated, and in consequence shortened, so as to expose the teeth. This, with the roundness of the eye, has, I suspect, produced the term *Rictus Satyrorum*, and, probably the epithet Satyriasis, as Hillary says the figure of the brows and nose gave rise to the term Leontiasis.

The inside of the mouth is often so irregular, that it is very difficult to describe it. The roof is sometimes universally thickened.

The uvula disappears, though I have never been able to discover an ulcer: it is, however, probable that ulcers exist, as the *os vomer* is sometimes lost, so that the nose becomes flat, whilst its external tumors have rendered it patulous. Under these circumstances the voice is much altered, but I have not known an instance of impeded deglutition.

The arms sometimes are tuberculated, but the tubercles are never very prominent, and soon become furfuraceous. This is probably from the friction of the cloaths, because about the wrist, particularly, this furfuraceous crust is accumulated to a considerable thickness.

The body usually escapes better than the limbs, but is not always entirely free. The genitals have been already described.

In the upper and anterior part of the thigh, nearly in contact with the lower part of the scrotum, there is, in almost every case, a firm (to appearance) glandular swelling, moveable and prominent, or concealed, according as the patient is fat or lean, or in proportion to the progress of the disease. It is remarkable that none of the women are without it. In most of the men these tumors are particularly prominent, extending gradually upwards. In some there are also inguinal buboes. In every case the swellings are indolent, never giving pain, nor becoming discoloured, nor shewing any disposition to suppurate.

The anterior surface of the thigh, almost as low as the knee, is, for the most part, tolerably free from the disease; but in most cases the legs are either covered with the furfuraceous substance above described, or with foul ulcers, rarely proceeding much below the surface. These ran so much into each other, that the tubercular appearance could not be always distinguished; insomuch, that I should have doubted whether they were really leprous, had not two cases put the matter beyond question. The first was a young woman, who, excepting the loss of her nose, shewed no signs of the

disease above the thighs. Here she had the two glandular swellings in front, and the posterior part, as well as her whole legs, were covered with a superficial foul ulcer, without any characteristic tubercular distinction. This woman's pudenda and nipples were such as I have before described, and she had no hair in the *axilla*. She is the one, who was before remarked to menstruate copiously; she is also the only one who shows any feminine fulness in the breast; and the only female we could discover in the house, who was afflicted before the age of puberty. Major Patrone, who first made this remark, enquired with some care if another instance could be found in the island, but without success.

The other case is a boy, whose legs are covered with small tubercles, some of which were furfuraceous, others scabbed, and a few retained their smoothness. This boy was attacked before the age of puberty, showed no signs of virility, and had the glandular tumors before mentioned.

From these two cases I should not scruple to consider such running superficial ulcers as the effect of this leprosy. The only description I can give of them is, that the ulcers, though they show no disposition to heal, are never painful, excepting after long continued exercise; that they are never deep; and that in many parts, the skin seems to cover them in loose wrinkles, as if extending over a surface wet with pus. Hence it is probable that there is no other difference between the furfuraceous surface and these ulcers, excepting a greater disposition in some parts to form matter, or a less capacity to preserve themselves under disease, which is well known to be the case with the lower extremities. Furfuraceous or scaly appearances occur in other diseases, as we shall hereafter have occasion to show more at large.

Thus far we have traced the disease to the extremities, I mean to the hands and feet. The fingers are sometimes so contracted, as to lose all power of motion: the feet are so affected with foul ulcers,

that the toes drop off, one after another. Sometimes as a joint is cast off, a firm cicatrix is formed; but more commonly the different phalanges are lost in succession. There is, however, so little uniformity in these symptoms, as to render it doubtful whether they should be considered as the necessary consequence of the disease, even in its worst state. First, because they are by no means a frequent occurrence; and next, because they happen from other causes, in countries where this species of leprosy is unknown. The contraction of the fingers occurs in old people, from the mere habit of being kept too long and too often in the same posture, in consequence of which the tendons shorten, so as never to recover themselves: and when we recollect how very liable the lower extremities are to ulcerate, and the great difficulty of healing parts so far from the source of circulation, we cannot wonder at the mischief which follows incurable ulcers in the toes. It is well known that these parts consist of little more than skin, tendon, and bones, the cartilaginous joints and capsular ligaments. These having less red blood circulating through them, have consequently less power of restoration. Hence when part of a tendon is diseased, the whole will often mortify. The same will sometimes happen to capsular ligaments, the periosteum, and cartilages. The consequence must be a separation of the different joints, and their loss in succession. The same will happen in sea scurvy, and still more frequently from frost, under long exposure in cold climates. It should also be remarked, that in all these cases, the disease is connected with poverty, a meagre diet, and dejection of mind from untoward circumstances.

There is another symptom not very common, and of course not to be considered as a necessary character of the disease. I mean the loss of sight. Two of the men have each lost an eye, and in one the second eye is considerably injured: in the other a tubercle is rising from the conjunctiva of the blind eye. In both these men the disease is so similar, as to leave no doubt that it arises from the same

cause. In both the figure of the globe remains; but in one the lens, in the other the lens, with all the humours, seems hardened into a brownish substance, which has destroyed all distinction of colour in iris or pupil.

Having now completed the description of the disease, I shall point out those phenomena which characterize it with most certainty, and afterwards take notice of the peculiar marks by which it may be distinguished from two others, with which it has sometimes been confounded.

When the disease arrives at such a state, as to distort the lip, nose, eye brow, or lobes of the ear, it will require little more than common observation to detect it. But if these are so little affected as to render it doubtful, we should always examine the upper anterior part of the thigh, towards the genitals, for that tumor, which I have called glandular, but the exact nature of which cannot be ascertained without dissection. In the mean time it may be easily distinguished from those buboes in the groin, which are formed by venereal or any other causes, producing inflammation in the genitals or lower extremities. These are usually in the hollow of the groin, hard, and immoveable till they begin to lessen, as inflammation subsides. On the contrary, the tumor from leprosy is never very hard, always more or less moveable, without pain or discoloration of the integuments, usually much larger than the common bubo, and lower in its situation. There is, indeed, some variety in the height, size, and mobility, but never sufficient to confound it with the inguinal bubo. It should be further remarked, that in a few instances I have found the inguinal bubo and femoral tumor in the same subject. In two subjects also, in whom the disease was, in other respects, well marked, I have discovered only a general thickening of the integuments over the part where the femoral tumor is usually found. In one of these was a similar, though smaller, tumor in the hand. In young subjects, particularly boys, before the age of

puberty, if we see any marks of approach towards that state, we ought to suspend our judgment, till we discover signs of the disease the most unequivocal and certain.

The two diseases, with which that of the lazar house has been confounded, are the leprosy of the Greeks, and the elephantiasis of the moderns, or, as it is usually termed in England, the Barbadoes leg. This confusion has been much increased, because the disease of the lazar house has been by some authors called leprosy, and by others elephantiasis, or even in the same work has been called by both names. It is true, that the most accurate among the ancient writers use no other term than elephantiasis: but that this confusion has existed among the moderns, will appear by quotations from two well received authors, whose object was to characterize the diseases with more acccuracy than their predecessors. Dr. Mead* remarks, that "the Syrian leprosy," [which before he calls elephantiasis], "did not differ in nature, but degree, from the leprosy of the Greeks:" and Vogel, after describing the *elephantiasis* of the ancients, adds, as a division *Elephantia:* "Eadem duntaxat in pede valde tumido et duro." Thus we see Mead considers leprosy and elephantiasis as the same disease, without hinting at the enlarged limb; and Vogel considers the elephantiasis of the ancients and the moderns as the same disease, only attacking different parts. It may, therefore, be right to observe, that when I use the single term elephantiasis, my wish is to confine it to the modern disease.

Thus we have,

1. The Arabian or Syrian leprosy, or elephantiasis of the ancients; the disease already described.

2. The leprosy of the Greeks.

3. The elephantiasis, or elephantiasis of the moderns, called also the Barbadoes leg.

* Medica Sacra.

Let us now attend to a short description of the two latter diseases, remarking such appearances as are most likely to be confounded with the Arabian leprosy, and also those which characterize each with the greatest certainty.

The leprosy of the Greeks is known in most parts of the world. It is unattended with any particular distortion of the features, and commonly begins on the limbs. It is known by certain elevations of the skin, covered with white scales. But the elevations are never considerable as in the Arabian leprosy, though their surface is usually much greater. In the Arabian leprosy, too, the summit of the tubercle is, for the most part, as large as the base, and sometimes even broader; in the leprosy of the Greeks, the base is usually the largest. But a sufficiently striking difference is,* that in the Grecian leprosy the white scales appear on the elevations as soon as the disease commences, whereas in the Arabian the whiteness never appears but in an advanced stage. In the Grecian, as the disease advances, the scales disappear, which may be considered as a mark of returning health, before the skin can recover its colour and proper complexion. In the Arabian, after the whiteness commences, it never ceases, unless an open sore is formed, which rarely occurs, excepting in the lower extremities.

The causes of this difference will be easily understood, if we consider the nature of these scales. Mr. Hunter, whose accuracy is universally admitted, considered them as cuticular processes, arising from an attempt in the cuticle to extend itself over an unsound surface. Thus, in the copper spots of lues venerea, the cuticle, not partaking of the disease, continues to grow, but not being supported by the parts below, the new processes fall off, and the same necessity existing to cover the parts, the attempt is renewed. Hence a scaly appearance till the specific remedy restores the healthy action of the parts; after which the new cuticle is supported, and the parts

* Those who wish for a longer account of the varieties of lepra Grecorum, may consult Dr. Falconer, in Mem. Med. Society, or Dr. Willan. The Bath hospital affords a most desirable clinical view of this disease.

recover their original smoothness. The same happens in lepra Grecorum in a still greater degree, because the progress of the disease is quicker. It also ceases spontaneously, as the disease is only temporary. But in the Arabian leprosy, the progress of which, as of most incurable diseases, is slow, the cuticle for a time remains firm; and when the scaliness appears, its progress is slower, though permanent, as was before observed.

The last difference I shall remark between these two diseases, is of the femoral tumor, so generally found in the Arabian leprosy.

The elephantiasis of the moderns, or, as it is often called in England,* the Barbadoes leg, is a disease which never would have been confounded with the former, had it not, from the causes above mentioned, acquired the name originally given to the Arabian leprosy.

It consists of a swelling of the leg and foot, sufficient to bury the toes, excepting at the extremities. The cuticle, at the same time, grows thick, rough, and scaly; but this scaliness is quite different from that of leprosy. It is, in fact, only an enlargement of the natural scales of which the cuticle is composed: for if the sound cuticle is examined with a common lens, it will exhibit nearly the same appearance, excepting that in elephantiasis the complexion is darker. The parts about the ancle, which are compressed at each motion of the limb, are formed into deeper plaits, over which the skin extends in such a manner as to cover part of the foot. The foot is in consequence shortened, and being thickened at the same time, little more than the toes appear, which being also thickened, appear shorter.

Such an exact resemblance to the limb of an elephant, could not fail to suggest the application of a name already familiar in nosology. And as in the Arabian leprosy, the leg and foot are

* My account of this disease is taken principally from Dr. Caddel's ingenious inaugural Dissertation, "De Morbo Glandulari."

sometimes swoln, it was easy, after the same term had been applied to each, to consider them as different appearances of the same disease; yet nothing can be more distinct than the two, under every stage, and in every part of their history.

The elephantiasis of the ancients, or, as we are now to call it, the leprosy of the Arabians, is a chronic disease, unattended with fever, and appears on most parts of the body in distinct tubercles. The elephantiasis of the moderns is confined to the extremities, most commonly one of the legs, and is found to arise from an inflammation of the lymphatic glands, and often of the vessels themselves, as may be traced by a painful increase of the gland, and sometimes by a red line in the direction of the vessels. The disease always commences with a shivering and consequent heat, during which lymph is extravasated into the cellular membrane. Before this lymph is entirely absorbed, another paroxysm supervenes, and then a fresh extravasation, till by degrees the disease becomes habitual, and the limb, or the scrotum, is rendered permanently more tumid, in proportion to the frequency and violence of the paroxysms. The skin is thickened, as in all occasions of extension, to support itself in proportion as it is stretched, and hence the enlargement of the scales, and all the other phenomena.

To conclude this part of the subject, it may be enough to remark generally:

1st. That Lepra Grecorum is distinguishable by the scales being coeval with, and as general as the disease—whilst in Lepra Arabum they never appear but in an advanced stage, and then only partially.

2dly. That Elephantiasis of the moderns, or the Barbadoes Leg, is distinguishable by the enlarged glands being always in the groin—by the pain and increase they suffer with each returning paroxysm of fever—by the sudden swelling of the

limb at the same time—by the thickening of the skin, without any loose furfuraceous scales, and by the tubercles, if there are any, being horny and rough, instead of the semi-transparent smoothness of those in the Arabian Leprosy.

These marks, or some of them, will always be sufficient to distinguish the disease, though there may be cases which require a diligent attention to their history. Lepra Grecorum, when the elevations become less scaly, may approach nearer to the tubercles of the Arabian leprosy; and, as in all chronic ulcerations of the lower limbs, more extravasated lymph is sometimes thrown out than is absorbed, so in the Arabian leprosy we shall often find an enlarged leg and foot, but never with that thickened cuticle, or free from furfuraceous scales, as in the elephantiasis of the moderns.

Having said all that seems necessary to ascertain the disease, and to distinguish it from those to which it bears the nearest resemblance, it might be expected that I should add something concerning the cure. But how can we expect to cure a disease, which originates in the constitution, and which the constitution is unable to cure? Could we find a remedy, how should we secure the patient from a return of the disease? As the complaint seems confined to certain climates and modes of living, we might expect relief from change of climate and a more generous diet. It is, however, certain, that these unhappy sufferers are extremely sensible of cold, even in the mild winters of this island.* It is, therefore, difficult to say, whether such a change would be attended with advantage. A generous diet, however, certainly protracts life, and renders it more tolerable. As all other remedies have hitherto proved ineffectual, and as I shall now have no opportunity of seeing the result of any new ones, I shall conclude with taking notice of such particulars as may arrest the progress of the disease, render life as comfortable as possible, and relieve the community from the constant sight of such

* Madeira.

objects of misery. For this purpose our first enquiry should be, Is the disease infectious?

I am aware that as early as the Levitical law, we have authority that leprosy is infectious, and that lepers should be separated. To prove that this disease was not the Arabian leprosy,* we should remark, that the separation was temporary, and continued only till the disease was cured; which appears to have been effected by the efforts of the constitution, for no remedies are proposed; after which the subject was to undergo certain ablutions, and be again admitted into the community. Far different from this was the leprosy of Naaman, the Syrian: for his disease there was no relief, till the prayers of the prophet produced a miracle in his favour. It is not less evident, that his disease, though incurable, was not highly infectious: for Naaman assured the prophet of that God, to whom he ascribed his recovery, and whose power he acknowledged, that it was the custom of the king, his master, to lean on his hand, during his devotions in the house of Rimmon. 2 Kings, ch. v. 18.

Medical writers assure us, however, that the disease is infectious; but why should we attend to uncertain authorities, when we have evidence before us? Let us then, for a moment, reflect what would be the consequence, if a disease, which is often known to originate with the sufferer, and which is hereditary, should be also infectious. Must not the union of so many causes depopulate any country, however extensive, or render all the inhabitants such objects, as effectually to deter strangers from any intercourse with them? If we add to this, that such as are attacked before the age of puberty, never arrive at that state, and that others are soon rendered incapable of continuing the species, we shall see that a people, among whom such a disease is once introduced, must, if it were infectious, without any peculiar mortality attached to it, diminish in numbers, till their gradual extinction. But the Fountain of Good

* See page 205.

has not thus abandoned his own image, the work of his own hands!

It appears, by authentic records, that as the population of the island has increased, the inhabitants of the lazaretto have diminished; insomuch, that, since the commencement of the present century, there have been fewer in that house than in the same period for fifty years before. To this we may add, that none of the nurses have shown any symptoms of the disease; that it is well known many of the patients are to be met with in the town and other parts of the island; and that individual lazars have remained for years at home, without infecting any part of their family.

It may be urged, that if the disease is not infectious, still it may be right to seclude from society such as may propagate a diseased race. I am aware, too, it has been said, that the subjects of this disease have a peculiar propensity to venereal excesses; and as this opinion is supported by strong authority, it deserves particular attention.

The most respectable, if not the most ancient writer, who gives a satisfactory description of the disease, is Aretæus: and I cannot help remarking, on this occasion, the danger of applying whimsical names to diseases, and also of trusting too implicitly to great authorities. Though Aretæus derives the term elephantiasis from the disease being more insuperable than any other, as the elephant is the most powerful of animals, and from some appearances unconnected with the swoln limb, yet we have seen the confusion that has arisen by the application of that name to another disease, from the mere form of the leg.

The same writer remarks, that the disease has been called Lion, from the roughness and projection of the eye brows; and also satyriasis, from the redness of the cheeks, *atque inexplebilem impudentemque coëundi libidinem.**

* Ἡ δε σατυριησιν τωντε μηλων τω ιχυθηματος, και τησ των συνησιων θερμης ασχετω τε και αναισχυντω.

It is much to be regretted, that the high authority of Aretæus has been sufficient to induce most of his successors to follow him, without examining for themselves. It is, however, but justice to remark, that this otherwise most accurate writer seems anxious to inform his readers, that his account of the disease could only be collected from others, or from a distant view: for after a correct, (excepting, in some few respects, exaggerated) description, he adds: "From such objects who would not fly, though it were from his father or his child, as there must be apprehension lest the disease should be infectious."* After this confession, we cannot suppose the writer could be well acquainted with the habits of a people, with whom he was afraid of conversing.

Celsus considers the disease as scarcely known in Italy.† He, therefore, contents himself with a general description from the Asiatic Greeks; but is too cautious to say any thing concerning the habits or propensities of the sufferers.

From among the moderns, I shall select such as had opportunities of tracing the disease themselves. Hillary, in his "Diseases of Barbadoes," is tolerably correct in his description, the greater part of which, however, it is plain that he has taken from Aretæus, or his copyers. It is, therefore, not to be wondered, as he insists much on the infectious nature of the disease, if his apprehensions prevented a close examination, or if, on the authority of others, he should assert, that "All who labour under this complaint, have strong and frequent dispositions to venery."

The anonymous author of *Charta Critica sobre o Methodo curativo dos Medicos Funchalenses*, has a long paper on this disease, in which he scruples not to insist on its contagious nature, and also on the

* Τοιούσδε μὲν ἐόντας τίς κε ἂν φύγοι, ἢ τίς κε ἂν ἐκτραπείη, κἢν υἱὸς, ἢ πατὴρ ἔῃ· κἢν κασιγνήτοσ τύχῃ. δέοσ καὶ ἀμφὶ μεταδόσιοσ τε κακοῦ.

† Ignotus pene in Italia, frequentissimus in quibusdam regionibus, is morbus est quam ἐλεφαντίασιν vocant.

lascivious disposition of the diseased. But this learned author, like most of the physicians of those days, is more anxious to prove by authority, than by his own knowledge of facts.

Dr. Thomas Hebberden, who practised with so much reputation in this island, and is better known among the natives by the name of Don Thomas (in a paper in the Medical Transactions of the London College) offers many convincing proofs that the disease is not infectious. He is silent on the subject of any disposition to venery in the patients.

Is there not too much reason to believe that this latter opinion is a mere error, arising from a fancied similarity to probably a fabulous animal? Has it ever been urged that women have been ravished by lazars? No. But it has been said these men show much anxiety for the society of women, and are distressed at being deprived of it. Do not those, on whom the church imposes the vow of chastity, domesticate themselves with their female relations? and do not the most austere find relief, from their severer studies, in innocent conversation with women? Lastly, shall we make no allowance for that universal propensity in mankind to seek with greater earnestness whatever is forcibly withheld?

Though all men expect relief, under habitual distress, from the society of women, shall we confound such a wish with another, which is always more powerful in proportion as health, youth, and vigour abound? Men, who have every other wish gratified, will be always seeking new enjoyments; but do we ever see them more salacious in proportion as they are ill-fed, ill-clothed, and ill-lodged *(sine Cerere & Libero friget Venus?)* Above all, have we not seen that when the disease attacks boys, it prevents their ever arriving at a state capable of such indulgencies; and even in a more advanced age, though the parts are slowly affected, they still are rendered useless long before the period at which men in health lose the power of procreation.

It is truly melancholy to reflect on the obloquy into which the miserable appearance of these unfortunates has thrown them. M. Sauvages, one of the first who attempted a methodical arrangement of diseases, not contented with *Veneris appetitum ardentem*, continues, *His adde mores dolosos, malos, &c. &c.* and all this on the authority of other writers, probably as ignorant as himself.

Unhappy mortals! Is it not enough to be the common dread and outcast of society, to be tormented with a disease which requires all their attention to keep themselves clean, and to contemplate all this misery without hopes of relief? Can it be suspected that well-intentioned people should be so deceived, as to add to such a weight of misery by separating husband from wife; denying the last solace and assistance of that helpmate with whom God has united them, and ordained that they should never after be separated?

It is, however, but justice to add, that the improvement of modern manners has connived at a law evidently too rigorous. We must also admit, that, when from any infirmity, one part of the community is to be supported by the industry of the rest, a compact seems implied, that those who are thus relieved should submit to some privations in return. Whenever, therefore, the disease seizes a man of such an age, that he may have contracted friendships or alliances which would make seclusion painful; if he is incapable of his customary labour, let him receive 100 reis per day, on condition that he is never seen begging; and if the sight of him is disgusting, that he shall not appear at public spectacles. As his infirmities increase, let his pension be doubled or quadrupled.

The remainder of the paper consists of a plan for bettering the condition of these unfortunate objects, and some well-deserved compliments to the Governor of the island.

List of the Subjects in the Lazaretto, from the Time a Register has been kept, and probably from the Time of its first Institution.

Years.	Men.	Women.	Years.	Men.	Women.
1702	11	0	1762	21	13
1708	10	0	1767	27	18
1746	29	20	1769	19	17
1751	24	18	1770	20	21
1752	29	16	1778	19	21
1753	26	18	1779	19	22
1755	22	16	1786	21	15
1756	24	16	1795	21	16
1757	23	18	1796	20	18
1758	25	13	1797	20	14
1759	17	10	1799	15	12
1760	20	11	1802	18	9
1761	18	11	1803	18	10

SUPPLEMENT TO THE ABOVE PAPER.

THE following account was given me by a woman, who had been three years in the lazaretto, and afterwards discharged, her disease proving not to be leprosy.

Formerly the allowance for each inhabitant of the lazaretto was 60 reis per day: at present they are allowed 100 reis, or about 6*d*.

Two days in the week (Wednesday and Saturday) a lazar carries a box to beg. A servant is with him. Whatever is collected is divided among the men in one week, and among the women in the succeeding week.

This woman washed in the same bowls, and messed with the female lazars, and has continued free from the complaint.

The men, who have wives, are visited by them, and occasionally eat with them, but are constantly within sight of the whole ward.

The men and women form little messes together, at which the women preside, taking care of the linen and food of the men. They

are, however, always, when together, in sight of the whole ward, and part at night.

The greatest decorum always prevails, though the lazars are by no means disposed to melancholy when kindly treated. On the anniversary of their tutelar saint, Lazarus, they have always provided for them a festival dinner.

On the whole, this woman found her situation so easy, that she offered her prayers to the saint, whose picture is over the altar of the chapel, that she might be slightly affected, in order to procure for herself a certain maintenance.

She says, the lazars themselves look for a sign of the disease, in the thickening of the ears, and the loss of hair in the eye brows. In this I suspect some error, as these are by no means constant symptoms. It is true, when the lobes of the ear are thickened, the disease cannot admit of a doubt; but in some it makes great advances before that symptom occurs.

Though the lazars rarely arrive at old age, they, for the most part, seem to die of other complaints. I recollect, at the period of an influenza, these people suffered earlier and more severely than any others.

Though, by the above account from the woman, and also from the several circumstances mentioned in the paper, it is certain that the disease cannot be highly infectious, yet it is right to remark some circumstances, which I shall leave others to determine, whether they should be considered as merely coincident.

The porter of the house is become a lazar since his residence in the lazaretto: but he is the only servant, since its establishment, who has shown the disease.

Besides the married couple now in the house, the accounts I collected mention two other women, whose husbands were lazars; but in these cases we must recollect the same mode of life, local residence, and probably some consanguinity.

It appears not only that the Greek writers were unacquainted with the swelled leg and foot, which afterwards acquired the name of elephantiasis among the Arabs, but that the Latins were ignorant both of this and of the elephantiasis of the Greeks. By an attention to these particulars, we shall readily discover the cause of that confusion of names above mentioned.

When the medical school was transferred from Greece to Arabia, where the swelled leg and foot was a frequent disease, and the elephant often in sight, it was impossible not to be struck with the similarity. I pretend not to determine whether the Arabs had previously distinguished this swelling by a name which had reference to that animal; but it is certain that the term elephantiasis, was by them applied to a disease no where noticed by the Greek physicians, from whom the word was adopted.

When the sciences found their way back to the West, and the Salernian school was established, it is well known that the Arabic writers were at first the principal sources of information: and as the Arabians had not only appropriated the term elephantiasis to a disease of their own, but had given the term leprosy to the elephantiasis of the Greeks, a confusion must have arisen in the translations through the different languages. However, the Latins soon found it necessary to distinguish between a disease which was easily cured, viz. the leprosy of the Greeks, and one which the old writers had denominated a cancer of the whole body; that is, a disease gradually increasing in whatever part it commenced, and without a remedy. Hence the distinction between *lepra Grecorum* and *lepra Arabum*, which last they perceived must be the elephantiasis of the Greeks.

Though they had thus discovered the difference between the Greek and Arabian leprosy, they had less inducement to enquire after the Greek and Arabian elephantiasis, because they were equally ignorant of both, or saw them so seldom, as to confound them with the more intractable state of other diseases, with which they were better ac-

quainted. At length the appearance of syphilis produced a more accurate research whether this disease could be discovered in the writings of the fathers of physic who, about this time, began to be studied in their native tongues. It was then found that the Arabian writers confined the term elephantiasis to a disease in which the leg and foot are much swelled, and which was unnoticed by the Greeks.

To pursue the subject through the whole controversy, would be more ostentatious than useful. Those who wish to follow it beyond the quotations produced by Lorry, may consult Sebastian Aquilianus, Nicol. Leonicenus, and several others, preserved in the Luisinian Collection. To do justice to the industry of these writers, we should recollect, that the works of Aretæus were not recovered from the rubbish under which they were concealed for near half a century after that time. The Italians, therefore, could only discover the meaning of the Asiatic Greeks from the writings of Galen and his commentators, whose descriptions are very obscure, and scattered in different parts of his works.

The dispute concerning the leprosy of the Jews, seems very much to have arisen from a want of remarking some peculiarities attending the different diseases, called by a term which was in general use. It is evident that the disease, which we noticed when speaking of yaws, was curable probably by the common powers of the constitution; for after a time the patient was re-admitted into the camp, on a satisfactory examination by the priests. It is most probable that, by a cautious attention to seclusion, the yaws was exterminated from the Israelites after their migration from Egypt. For though the term leprosy often occurs afterwards, yet it evidently refers to an incurable disease, probably the Syrian or Arabian leprosy. We cannot suppose that miracles would have been required to restore those who were afflicted with a curable disease: the disease of Naaman must have been considered as incurable, or the king of Syria, his master, would not have sent a messenger to the king of Israel, in behalf of his servant.

When the messenger arrived, we find the king of Israel was so much distressed at being expected " to recover a man of the leprosy," that he could only suspect a pretence for a quarrel, till the prophet relieved him, by assuring him of the extent of his miraculous power.

But though the disease was different, it is not to be wondered if the same ceremonies were required for one of the same name, especially when we consider the aspect of the afflicted. Thus we find, when Naaman* arrived before the door of the prophet, he received only a message to bathe three times in the river Jordan. This want of attention, so contrary to Eastern hospitality, struck him with astonishment and disgust. But the cause is easily traced in the strict observance of the law, by which Elisha would have no conference with a leper till he was clean. After this he would touch none of the things about him, as they had not undergone any purification, and even dismissed Naaman with a short answer to his offers, entreaties, and enquiries—" And he said unto him, Go in peace."

When he afterwards discovered that his servant, Gehazi, had been guilty of receiving apparel, and even of prevarication, he accuses him of avarice, and assures him that the consequence will be an incurable disease, which may descend to his posterity. But by a miraculous power, the servant was instantly inflicted, in consequence of which he was probably immediately secluded, and all hopes of posterity cut off for ever. Such, we are told, was the case of king Uzziah,† who, after the leprosy was discovered upon him, passed the remainder of his life in a lone house, and a successor was appointed.

By this it is evident that seclusion, at these times, was not with any prospect of a future return into society. But it seems as if the poorer class was only forbidden to enter the towns, and they probably

* 2 Kings, chap. v. † 2 Chron. chap. xxvi.

begged alms at a distance from passengers. Four lepers,* we find, were refused admittance into the city of Samaria, even during the siege. Regardless of life on such terms, they determined to take the chance of being relieved by the enemy.

Among the miracles of our Saviour, the cleansing of lepers very often occurs, and this is one of the powers with which he particularly invests his disciples. The instance, in his own administration, which is related with circumstances the most minute, is contained in Luke xvii. v. 11. & seq. At *his entrance* into a certain village, we are told, ten lepers met him, *and stood afar off raising their voices*, whilst they begged his mercy. Our Lord immediately directed them to comply with the ordinances of their religion, in shewing themselves to the priest as they were cleansed.

* 2 Kings, chap. vii.

CHAPTER XIX.

ACCOUNT OF THE ACARUS SIRO (EXULCERANS OF LINNÆUS) BY SOME CONSIDERED AS THE ITCH INSECT.

In a Letter to the Right Hon. Sir JOSEPH BANKS, *K.B. P.R.S. &c. &c. &c.*

[Read before the Royal Society, April, 1805.]

DEAR SIR,

THE late Mr. Hunter, in his lectures and conversations, always acknowledged that he could never discover the itch insect, and went so far as to suspect that the opinion concerning its existence was derived from a pre-conceived theory, and supported by credulity.

These doubts, indeed, were not confined to Mr. Hunter; but the influence of great names, and the difficulty of disputing a fact supported by credible eye witnesses, has at last, in some measure, silenced the controversy, but by no means removed the doubts. Some writers have thought the difficulty might be compromised, by supposing that the animalcule is not necessary to form the disease, but that the vesicles prove a convenient nidus for its ovum. Dr. Willan seems to favour this opinion; but this will be better understood when, in the progress of his work, he arrives at the disease in question.

The first account I have met with of this insect, as the cause of the itch, is in the Philosophical Transactions, in a paper by Dr. Mead, containing the substance of a letter from Dr. Bonomo, of Rome.

You will perceive by the inclosed drawing, as well as by the insect inclosed within the lamina of talk, that Bonomo was tolerably exact in his description. The drawing is by Mr. Ratteker, a travelling professor to his Royal Highness the Prince Royal of Denmark, who assured me he had never seen any thing of the kind in his own country. From these circumstances, I should have suspected that the disease was peculiar to warm climates, had I not learned, from satisfactory authority, that it is well known in Ireland, where it is called the flesh worm. In Madeira, the name given to it is ouçāo or ouçam, the last syllable being pronounced like the French nasal terminations. When the disease reaches the head, so as to produce a general scabbiness, it is called zagra. Probably both the words may be of Moorish origin. The people here are very ready at distinguishing it from the scald head, for which their vernacular name is tenia. I shall trouble you with only a few other remarks, before I relate the experiments made by my friend, Mr. Banger, and myself, of colonizing the insects on ourselves.

Dr. Bonomo informs us, that by enquiring of an itchy patient at what part he felt the greatest sensation of itching, he was able to discover the insect in most of the watery pustules. He admits, indeed, that he could not find them in all. With all my perseverance, till properly instructed, I never could discover more than a single one, nor was this in a vesicle. At the same time an old woman, who had consulted me for her niece, found no difficulty in detecting several, but these were not in the vesicles. In vain did I endeavour to trace the appearances by which she was directed, and even submit patiently to her instructions. My friend Banger, whose patience in every investigation of natural history infinitely exceeds whatever I have met with, even in professed naturalists, was, for the most part, as unsuccessful, or if he succeeded oftener, his success was not in any degree proportionate to his unremitting application.

Dr. Bonomo further remarks the great difficulty he met with in detecting the egg, which, however, he at last discovered, and has described. Without suspecting the good intention of this writer, you will readily admit the uncertain discrimination of the egg of an insect, described by Degeer as about the size of a nit, but which, on placing it under a miscroscope, by the side of a nit, did not appear more than a fourth part of its bulk. For myself, I never could discover what could satisfactorily be called an egg, and in only one or two instances an animalcule, so much smaller than the others, as to give any satisfactory idea that it was younger, or had recently escaped from an egg. Dr. Bonomo had, indeed, the advantage of seeing several of the infected subjects naked at the same time, and in a bath. This might have furnished more expert hands in detecting the insects, or the warmth of the place might have facilitated their evolution; for it is past a doubt that the ouçöes are often torpid, even during the period of our mild winters.

The author admits he could never distinguish the sexes. In this we agree; but there is one peculiarity, which, as he does not remark, probably he never witnessed. This is a power of leaping with a force not less than a flea. Such was the case with one whilst I was examining it under a convex lens. Whether these leaps were repeated we could not ascertain, on account of the minuteness of the object and its grey complexion; but whilst we were expressing our surprise, the old woman, our tutoress, appeared unconcerned, and assured us that the event we had witnessed was by no means uncommon. I have collected the same account from several other people.

I shall now proceed to trace the progress of these insects in myself and Mr. Banger.

In July, 1801, I procured two ouçöes from the young woman, whose aunt consulted me for the cure of her neice. The old woman, without spcetacles, which she always used when working with her

needle, but not without much diligence and nicety, extracted them from the girl by means of a pin, and placed them between the fingers of my left hand, the skin of which, and I believe of my whole body, was entire. They remained apparently without any disposition to move, as long as I watched them; but on examination two hours after, nothing was to be discovered but a small desquamation of the cuticle.

For more than three weeks little or no inconvenience was felt. From that time began frequent itching in different parts of my body and arms, but no eruption could be discovered. In less than a fortnight afterwards, my arms and belly were covered with a general efflorescence, but few vesicles appeared. I applied to my old woman, who readily drew two ouçöes from my arm, but not from the vesicles; indeed on this, and on all other occasions, I could not help remarking, that, though I could not discover what the marks were by which she was directed, yet she constantly passed over the vesicles, without suspecting them to be the nidus of the insect. At length I perceived it was a small slightly discoloured elevation of the cuticle, which appeared as if soon likely to become vesicular; that the woman always attacked, but not always with success. However, she constantly answered to our enquiries, that where the bladder was formed, the ouçaõ had left the spot.

No remedies being used, nor any alteration in diet, or my usual habits, the weather also being warmer than common, even for this season of the year, the disease continued to spread rapidly, so that by the end of August, my whole body, arms, and thighs were covered with the efflorescence. As, however, the vesicles were few, I was willing to believe the eruption might be the prickly heat. On this subject I applied to my old woman, who confirmed my suspicions; but there is reason to fear I was not sufficiently cautious in leaving my teacher to form her own opinion before she had discovered mine. In the mean time my health suffered exceedingly, not only

from the inconvenience produced by the itching, but about noon a quotidian fever began, with a slight shivering, and was succeeded with head-ach, dry heat, great thirst, loss of appetite, and considerable exacerbation of the itching. The consequent perspiration was not greater than what the season might have produced.

So little was I prepared to expect such effects to arise from such causes, or so determined to ascertain the cause, that to allay the itching, I used an ointment of pomatum and saccharum saturni, from which some relief seemed to follow. It was, however, for a short time, and the paroxysm of fever being as regular, though much slighter than in common ague, I had recourse to the bark. If this produced relief, it was very temporary.

By the middle of October the efflorescence was universal over my abdomen, and very general over my arms, breast, and thighs. My hands were only slightly affected, but sufficiently to be detected by the natives. The character was, indeed, here more strongly marked, for the white shining cuticular elevations were such as I should not scrupled in England to call the itch.

About this time one of my family became slightly infected with the eruption, but her fever was very considerable. It seemed, therefore, reasonable to attribute all my own symptoms to ouçöes, or at least it was time to try whether by ridding the skin of them, the other symptoms would cease. The ointment I had invariably found successful in all other cases was composed of a dram of white præcipitate of mercury and an ounce of soft pomatum. The effect of this on us both was almost incredible. In three days time the itching nearly ceased, and the fever entirely. However, it was found necessary to have recourse to the ointment occasionally for near a month afterwards: little cuticular elevations and some vesicles arose at different times during that period; and when they arose, were constantly attended with symptoms of fever. From that time we felt no further occasion for any remedies.

In the mean while my friend, Banger, had begun a similar experiment on himself. He borrowed a single oucöen of me, and its multiplication soon showed itself. The first part in which he discovered either itching or eruption, were the inside of his left arm, near the axilla, and that part of the chest which comes in contact with it. This he imputed to his habit of sleeping with his right hand in that position. At first he used no means of cure but warm bathings. He next tried dry frictions of brimstone over such parts only as showed the eruption, or gave the sensation of itching, and gained some assistance from them, but was not cured without a sulphur ointment; and the efficacy of this was slower than I have usually found from the white precipitate ointment.

Such is the general progress of the disease, but there are varieties of which nothing less than demonstrative evidence would have convinced me. A patient (an European) applied to me on account of a spreading inflammation, attended with large vesications, collections of serum, in some places of pus, with intolerable itching; sometimes intense pain and smart fever. All these symptoms were much exasperated at a certain period every day. I treated it like any other inflammatory complaint, with evacuants and poultices to the part. The latter afforded some relief, but my patient grew extremely impatient from the fever and frequent violent pains, which deprived him of sleep. This induced me to examine the part with more care, and to convince myself, that, how great soever the pain might be, the mischief extended only immediately under the cuticle. In the mean time the female servant, who assisted with the poultices, pronounced the disease ouçöes; and to convince him of the truth of her assertion, extracted two from the edges of the sore, which he saw crawling on his nail. This appearance of the disease so entirely local, and the part affected with such violence, was so different from any thing I had met with before, that no evidence, less than the above, would have satisfied me. The pain,

indeed, was less surprising, when we consider the disease was immediately on the rete mucosum. Subsequent experience taught me that these symptoms are by no means uncommon. The disease yielded instantly to the usual topical remedy.

These circumstances were enough to convince me that itch and ouçöes are two distinct diseases. Setting aside any presumption from the accuracy of Mr. Hunter, this probability is confirmed by vulgar opinion, which, after the history above given, is entitled to some respect. The servants of this country are much offended if suspected of having *(sarna)* the itch, but think little of ouçöes. The nurses and mothers, who are sufficiently at leisure to attend to cleanliness, search the skins of their children for ouçöes as regularly as the hair for lice, and by extracting them, early prevent the spreading of the disease. My friend, Mr. Banger, brought a child to his house, much covered with the disease, that he might learn to extract the insect himself. He was so diligent in the lessons he received daily from an old domestic, that in about six weeks the child afforded no more ouçöes, though all remedies were carefully avoided. When I have been inoculating a child, I have heard the parents very often endeavour to quiet it, by assurances that I was only drawing out an ouçao: so general is the custom among people of all conditions.

In searching for the insect, in which I was regularly instructed by the old lady before mentioned, the bladder is always passed over, if a red, and as it appears by the microscope, a somewhat knotty line is discovered to issue from it: at the end of this line, which is about a quarter of an inch long, is found a reddish elevation, to appearance dry and firm. Under this, parts of the ouçao are sometimes discoverable with a good glass; but whether such is the case or not, this is the only place in which the ouçao is expected, and if not found here, the search is abandoned.

2dly. Another presumptive proof of the difference between itch and ouçöes may be collected from the fever. This was unquestionable in myself, in the person I infected, in my friend, Mr. Banger, in the patient whose case I have detailed, and in many other subjects, whose symptoms I have watched since my own sufferings; yet I am aware some caution is necessary in admitting such a distinction. For it is certain that some people are very little affected by this symptom even at an early stage, still more are by habit insensible to it, or the constitution grows so familiar to the irritation, that no fever is excited. It is, however, not less certain, that all delicate, and especially young subjects, are constantly meagre and out of health whilst ouçöes are upon them; and I have restored many from this state to high health merely by ridding them of these insects, though they were never considered as the cause of the general indisposition. The fever, to those who are at leisure to watch its symptoms, is very remarkable. My friend, Mr. Banger, was so well acquainted with it, that during his voyage to Lisbon, he was sensible by his feelings that he had an ouçaõ some where under his skin; and when he had leisure, after his landing, to search, he readily extracted it, and cured himself.

The third proof I shall offer of the difference between these two diseases, is the greater facility with which the patient is relieved of ouçöes. I have not mêt with an instance in which the disease did not easily yield to the white precipitate. It is also readily cured by sulphur, and even when used only internally. Though these are specifics against the itch, yet the cure is much more tedious, nor will white precipitate always succeed. It is also said to be useless to give sulphur internally. But this may, perhaps, be only a presumption since the publication of Dr. Mead's paper, at the conclusion of which he endeavours to show that internal remedies must be useless.

There is yet another peculiarity attending the cure of ouçöes. Though the relief from precipitate is experienced so early, yet if that remedy is not occasionally resorted to for near a month, after all the symptoms seem to cease, the disease constantly returns. It is most probable, that during that time young insects are evolved from their eggs, or that they arrive at such a stage of existence as to propagate. This is still more probable, because the disease will sometimes subside of itself during the winter, mild as that season is in this climate, and return in spring. Even if cured late in the autumn, it rarely happens that it does not return in the spring.

But an attentive observer will want no other proof of the difference between ouçöes and itch, than the difference in the appearance of the skin. It cannot be questioned that to distinguish them about the hands, in an early stage, would require the accuracy of those eyes which are accustomed to search for these insects; and in the more advanced stages, those who are unaccustomed to such an irritation, will confound all distinctions by scratching. But whoever will take the trouble of examining the common itch on such as have been long accustomed to it, will always find a great variety in the form and size of the vesications. In the neighbourhood of the joints, or in parts less subject to the friction of cloaths, the bladders are sometimes very large, and their size varies very much in other parts. Under ouçöes, on the contrary, all the vesications are exactly uniform, and constantly attended with the red line, terminated in the manner described. It is to be remarked, that this uniformity is only in the vesications. The general irritation of the skin, without considering the great disposition to scratch, produces a variety in the efflorescence and little papular eminences, but none in the vesicles.

The above paper was written in Madeira, which afforded me access to no other accounts than could be collected from the authors

alluded to. Since my arrival in London, Dr. Shaw, of the British Museum, has shown me the *theatrum insectorum* of Moufet, and Mr. Dyer, of Bristol, directed me to a paper in the London Med cal Journal for 1788.

As the latter contains an abstract of whatever had appeared on the subject, with the remarks of Dr. Simmons, the ingenious editor of the work, I shall pursue the enquiry in the order in which he has arranged it.

The first cited author is Moufet, whose description of the manner of searching for the insect is extremely correct. He confounds, indeed, the different *acari* or *syrones*, which is not surprising at a time when the miscroscope was so little known in England. He observes, that these insects are called *seuren* by the Germans. This induced Dr. Wichman, the author of the work alluded to by Dr. Simmons, to examine the German entomologists, before that period, but he could find nothing on the subject. Had he looked into the dictionaries of his own, or any other modern language, he would have been more successful. In a work, however, by Dr. Hauptman, published twenty years after, on a different subject, he unexpectedly met with an account of these *çyrones*, called by the Germans *reitleisen:* a drawing is given, but the insects are confounded with the common *acari* of cheese.

Part of Bonomo's letter is next inserted, by which it appears that Mead has omitted the introductory and by far most important part. For in this Bonomo tells us, that he was first indebted to his dictionary for his knowledge that such an insect existed, and afterwards to his friend, Hyacyntho Cestonio, who assured him that the nurses and galley slaves extracted the insect from their children and each other *(a minutioribus tuberculis, vel, ut vocitant, immaturis.)* But Bonomo says, that *he* took them out of the vesicles when they were wet. This renders the whole account of his own experiments suspicious; and as he is the only person who has seen the egg, its

discovery must be still a desideratum. Mead must have seen the incongruity of the different parts of the letter, and therefore very prudently published only half. However, to these communications we are indebted for a more accurate engraving of the insect.

Several subsequent writers, medical and microscopical, are afterwards introduced, who either content themselves with Mead's account, or are altogether silent on the subject of this insect. Among the latter, are Reaumur, Swammerdam, and Leuenhoek. At length Linnæus, in his *exanthemata viva*, admits the *acarus scabiei*. But whoever peruses carefully that work, will see that Linnæus appears to admit many more insects in the different *exanthemata*, than he pretends to have seen. In his *Systema Naturæ*, he considers the *acarus scabiei* as nothing more than the *acarus farinæ*, which finds its way to the chafed skin of infants, by the flour with which nurses sprinkle them. He even supposes that these acari multiply in this situation, and infect other children. After this he describes another species of *acarus*, which he calls *acarus exulcerans*. This is evidently the *acarus scabiei* of Bonomo, Mead, and Degeer. To account for this, Dr. Simmons remarks, that Linnæus admits two kinds of itch, the common itch and *scabies ferina* (the *fera* or αγρια of Celsus.) But it should be recollected, that when Linnæus speaks of scabies, we are not authorized to say that he has the itch in view. In his Nosology, *scabies* is an ORDER, of which psora makes a genus. Scabies seems, therefore, a loose term in his other works. Probably he had never seen the true *acarus scabiei*, and contrived this means of reconciling the different accounts of others.

In this manner his editor, Gmelin, seems to consider the question. Under the article *acarus siro*, he gives the words of Linnæus—*Inter sirones farinæ scabiei, &c. vix reperi aliam differenciam quam a loco petitas.* He then adds—*Utrum causa an potius symptoma mali?* And after *acarus exulcerans*, he adds, with the same scepticism, *An satis distinctus ab acaro scabiei?*

I shall pass over the other authors introduced in this paper, to make room for the modest Fabricius. This writer was a clergyman, and not only pursued the true mode of acquiring physical knowledge, but did justice to his teachers. In his account of this insect (Faun. Groenland.) he says: Habitat in vesicula scabiei Groenlandorum, qui illum acu eximere scientes, mihi MIRANTI, ut vivum animal incedentem ostenderunt.—En Groenlandos Entomologos!

By this time the reader will be convinced, that to find the names of this insect in different languages, he must look to the dictionaries; and to learn how to find the insect itself, he must consult nurses, or those who are ready to learn the habits of every class. In Great Britain he may be disappointed in both. Our modern dictionaries have omitted *wheal worm*; and if the insect exists among us, the art of finding it is, I believe, lost.

Littleton's Latin and English Dictionary, published in 1715, has wheal worm — *acarus*. But Morell, in his last edition of Ainsworth, leaves out wheal worm altogether from the English part. *Acarus* is not allowed a place in his Latin dictionary; but in his second index, or glossary, containing the *sweepings* of some inferior writers, *acar*, or *acarus*, is found translated hand worm, or mite. Perhaps *acarus* deserved no better fate. But this is not the case with wheal worm, and when we recollect that Bonomo first took his hint from a dictionary, I hope future lexicographers will consider that philosophy is more concerned in their labours than they may be aware of.

I shall not pretend to say, whether the editors of the Della Crusca Dictionary, of whom Bonomo gained his information, considered the insect as the cause of the itch: if so, that opinion was probably derived from some philosophers; for in an Italian and English Dictionary, published in the year 1611, the word *pedicello* is translated—*a little worm, breeding between the skin and the flesh*, without any reference to the itch. I before remarked, that though Dr.

Wichman could not find any thing about *scur* among the German entomologists, he might have been more successful in the dictionary writers. In a German and English Dictionary, published at Leipsic, in 1770, the English of which speaks a more ancient date, *seur* is translated hand worm, and *reitlaus* the hand worm, or the vesicle it occasions, neither having any reference to *itsch*, the itch. So in all the French dictionaries *çiron* is used to express either the insect or the vesicle, but has no reference to *gale*, the itch. The Spanish dictionaries have *atador* and *arador*; the older copies *arradon* and also *gusgarillo*, which they derive from the Arabic *gus*, a worm; but neither make any reference to *sarna*, the itch. The Portugueze have been already noticed, and I have been able to learn, among the colonists, the common meaning of the words, and of the ideas annexed to each. The same may be said of the Irish flesh worms. My friend, Mr. Pitcairn, being stationed as army inspector at Athlone, took some trouble to enquire among the medical people concerning these insects; but all his endeavours were fruitless, till he directed his attention to the clergyman of the parish, by whom he was informed, that the difference between flesh worms and itch consists in one being an animated disease, and the other inanimate. His domestics, and the cottagers in the neighbourhood, told him the difference was, that the one disease was cured by extracting the worm, and the other required sulphur. Probably the Greenland entomologists and galley slaves of Rome would have said the same.

I have had an opportunity of enquiring of an old French woman. Her answer was, that *çiron* meant *un bouton*, a pimple or pustule,* and also an insect which occasioned the pustule, but had nothing to do with *la gale*; that she had seen *le çiron* move and leap like a flea.

Having said so much of dictionary writers and others, it is time I should do justice to physicians. Sauvage, in his 4to. edition, 1768, thinks so little of any analogy between itch and syrones,

* My friend, Dr. Robert Jackson, informs me, that the Arabic *çirré* means a pustule. It is, therefore, probably the origin of *çiron*, *seur*, and perhaps *ouçāo*, and the Latin *syro*.

that with him *scabies* is a genus of impetigines, the fifth order of his tenth class, and Psydrachia a *sironibus* is included in the second order of his first class. He adds—*Nomen sironis tribuitur et papulæ & insecto quod hanc excitat.*

If the reader should feel himself tired of all this, he will at least allow that it has not been collected without some industry, and it will, I trust, serve to convince him, that in every enquiry the sources of knowledge are numerous, and should be all explored. As the two diseases, here contrasted, are cured by the same remedies, it may be thought of less consequence. But as it is impossible to say how much we may be led astray by a single mistake, so it is impossible to say what new lights we may acquire by accurately ascertaining a single fact. Bonomo first introduced this uncertainty into England and Germany, by asserting that he had found the insect in the vesicle; Mead increased it by suppressing the beginning of Bonomo's letter; and we have seen how extensive it has proved. In the course of the above enquiry, I find the Italians have the word *setole*, which literally signifies a bristle, or horse hair, but is used to express a small worm, which sometimes is found in the inner palpebræ, and produces intolerable pain. May not some very obstinate opthalmiæ arise from such causes? The ung^{t}. citrinum, now ung^{t}. hydrargyri muriatum, is a well known remedy for tedious inflammations of the inner eye lids. The paper before alluded to, by Moufet, has a passage, which I cannot help inserting at full length.*

* Syrones intra occulos generari posse quibusdam impossibile videtur sed id fieri vidimus, et ex Epistola D. le Jeune regii Chirurgi, ad Jacob. Guillemæum olim factum esse accepimus; cujus hæc sunt verba. Scias inquit quod in membrana conjunctiva (sive albo occuli ut vulgo loquunter) syrones quidam majusculi, vagis hic illic reptatiunculis ac morsibus tantum excitant pruritum, ut abstinere quin frices haud possis? Ego hoc in casu remediis usus sum a majoribus contra phthiriasin præscriptis, sed sine ulla utilitate. Tunc amici ægrotam ad fæminam miserunt, quæ coram acu argentea adeo dextre et nullo cum dolore syrones extraxit, ut mihi miraculo esset! Et sane nisi propriis luminibus gradientes bestiolas vidissem, syrones ibidem ortos fuisse nunquam credidissem.—Moufet, Theatr. Insect. Lib. 2. Cap. xxiii.

Sir Joseph Banks, whose true love of science is only equalled by his uniform urbanity, has favoured me with the following letter, in consequence of a conversation we once had on the subject.

MY DEAR DOCTOR, *Soho Square, July* 17, 1806.

I CAN have no possible objection to your stating the case you mention to the public, if you think it likely to be of service. The particulars are as follows:

Several persons belonging to the Endeavour brig, while at Otaheite for the purpose of the observations on the transit of Venus, in 1769, were troubled with a severe and tormenting itching round the extremities of the eye lids; one of these, while sitting in the tent, was told by an Otaheite woman that he had lice in his eyes, which she could cure.

He readily submitted to her operation, which was performed with two minute splinters of bamboo, with which she extracted abundance of very minute lice from between the liliæ, scarce visible without a lens, but the motion of which, when laid on our thumb nails, we could distinctly observe.

I am, my dear Doctor,

Very faithfully your's,

To Dr. Adams. JOSEPH BANKS.

Sauvage seems to refer to such an insect when he says, that the hordeolum of Linnæus is the *çiron des pauperes.*

I have somewhere read that in the Egyptian expedition, those [I believe among the French] who had the itch escaped the plague. The old writers speak much of the security derived from the itch under many contagions, and still more of the danger of hastily

curing that disease. I can easily conceive that the fever, which the wheal worm occasions, may render the constitution insensible to other diseased impressions. It will be recollected, that this security may be uncertain, depending on the comparative force of the two impressions; and also that the first impression may become, by degrees, so familiarised to the constitution as to lose its force, or be superseded by one more powerful. This may account for the scepticism of the moderns on this subject. It is probable that the danger of curing these eruptions may have been magnified by the consequences which have followed such a process during a reigning epidemic; and that the safety of curing them at other times may have induced us to think too light of an alarm which may not have been raised without some foundation. I cannot, however, help thinking, when we so frequently find ourselves mistaken in our expectations from the theories we have been taught, that it would become us to be cautious before we smile at the simplicity of our predecessors, who had little more than facts to guide them.

Dr. Gillispie, whose work, by the general tenour in which it is written, seems to be composed of notes taken on the spot, makes the following observation on the fresh breaking out of the epidemic fever on board the ships moored in *Trois Islet Bay*.—" Those affected with tenesmus, or white flux, with ulcers or ITCH, were not liable to be attacked with the disease; but the stoppage of the discharges, and the consequent plethoric state of the system, rendered such persons very subject to a severe disease."* Whether the first disease, in these cases, was removed by art, or gave place to the access of a more violent stimulus, does not exactly appear. But in a subsequent passage we are informed—" Whilst the seamen and soldiers at Martinique suffered so much from a malignant epidemic, about 800 prisoners of war, confined in transport ships in the sickly careenage, preserved their health in an extraordinary degree. A

* Observations on the Diseases of the Fleet, page 145. Cuthell, 1800.

slight catarrhal remittent fever, only attended with danger when a diarrhœa occurred, whether spontaneous, or occasioned by purgatives, and a large moist species of ITCH, were almost the only diseases which affected them."

" The retropulsion of this eruption, or the sudden desiccation of ulcers of the legs, were attended with dangerous attacks of fever and dysentery among some of the prisoners. Hence the administration of sulphur was found less safe than that of decoction of the woods, of sharp pointed dock root, or an infusion of that of the perena brava.†

In whatever view we may be disposed to examine the above theory, the facts are highly important. As the subject must come before us again, in our remarks on acute contagions, I shall dismiss it for the present, but without any apology, how weary soever the reader may find himself. If he should be disposed to remind me that what we collect with diligence, we are apt to value in proportion to the labour it has cost us, I can only answer, that we are now going to engage in enquiries of a magnitude equal to any others in this life—enquiries concerning diseases, the fatality of which is so general, that to prevent them seems scarcely a greater discovery than to cure them—diseases which, when they occur, will outrun our deliberation—diseases, from which scarcely a family of distinction does not lament the disappointment of their fondest expectations—diseases, by which the poor are deprived of their principal support, the country of its great sources of industry, and the world of its most distinguished ornaments—yet of the nature and mode of treating these diseases we are still disputing!

After this shall we refuse to receive knowledge from any source? Shall we forget that to the barbers we owe the bold use of mercury—to the Jesuits of the Peruvian bark, which they learned of the Indians—that an African showed us the value of quassia—that a

* Id. page 162.

Greek slave taught a woman the art of inoculation, the blessings of which were for a time almost lost, by our fancied improvements and ill-directed cautions? Lastly, shall we contrast all this with the manner in which a Jenner has availed himself of the neglected traditions of Cow-herds and Dairy-maids! Let such an example teach us, that whilst the empiric is always ready with his remedies, the physician's higher department is to ascertain diseases. It is this only that renders his knowledge progressive, that enables him to improve the facts he every where meets with, and to use with safety and advantage what would be otherwise misapplied or neglected. Such was the business of Sydenham's life, such the lesson he has bequeathed us—

Si morbi cujus libet historiam diligenter perspectam haberem par malo remedium nunquam non scirem adferre.

PART II.

OF MORBID POISONS ATTENDED WITH CRITICAL FEVER.

CHAPTER I.

ON THE VARIOUS CAUSES OF FEVER.

THE importance of the subject we are now engaged in is unfortunately attended with proportionate difficulties. The actions we are to consider are more complicated, not so cognisable to our senses, and rarely admit of experiment. Yet we are called upon to act with more decision. " Le retardement dans l'administration des remedes est une occasion perdue que ne se retrouve jamais: les erreurs du malade, du medicin ou de la nature y coutent souvent la vie."*

Under these circumstances it may be permitted to premise some general remarks on fevers, the application of which may not immediately appear, but which will facilitate the progress of our future enquiries.

For some time we seem to have too generally reasoned as if we considered fever, when neither symptomatic of local affection, nor attended with increased strength or action of the arterial system, as always connected with infection. The general term of typhus seems not only to have superseded in words the *febris lente nervosa* of Huxham, but almost to have consigned the disease to oblivion. Such language may be too unqualified, but it cannot be a loss of time to say a few words on the various causes which induce that change in the animal œconomy denominated Fever.

We have already seen the variety of causes which, by the impeding or prevention of those local actions by which a part is preserved, render a new series of actions necessary, either to restore

* Gilbert, quoted by Valentine, in " La Fievre Jaune de Amerique."

the original and healthy action of such a part, or if it can no longer be preserved, to separate it from the surrounding parts. We have traced the new series of actions which takes place: and seen that on secreting surfaces the secretion is increased, altered, or ceases for a time, according to the nature or violence of the injury. That in parts unaccustomed to secrete, secretion of pus, a substance unknown to a healthy body, takes place to restore health. Let us now see how far the causes, to which we may ascribe fevers and the phenomena with which they are attended, will admit of a similar mode of reasoning.

The causes which impede health are so numerous, and for the most part so well known to each individual, that most readers will feel reminded of some peculiarities in their own. There are, indeed, constitutions so firm, as rarely to be ruffled by those causes which, to others, prove the source of disease — constitutions in which occasional intemperance is relieved by sleep, or almost without it, intense fatigue is scarcely felt after a temporary relaxation, and long application produces no other effect than the necessity of a short repose. But these constitutions are few, and are not exempted from other causes of disease. Even in these such irregularities, committed in a climate where every action is more easily excited, often prove fatal. But these causes of constitutional disease, the most obvious by their frequency in such climates, and the uniformity with which they are followed by their effects, are few, compared to those which often occur and pass almost unnoticed in the more temperate regions.

Few expressions are more common or more just, than that a person has been worried into a fever—that he was seized with fever from over fatigue, from anxiety, from sudden fear, or sudden distress of mind. Any cause operating suddenly on the constitution, so as to impede its necessary actions, must produce death, or a new train of action for the temporary support of life, till the healthy

actions can be restored. If such a cause is slighter, but permanent, the effect will be modified accordingly. The constitution, perpetually harassed, as in large ulcers, which it vainly attempts to heal, will excite actions which it is unable to support, and which actions, whilst they last, will to the ignorant, and to the patient himself, be considered as marks of health, till the impossibility of supporting the debility consequent upon them, undeceive both. Of this kind is the true hectic, from whatever cause. The constant attendance of this fever on suppurating sores, which the constitution in vain attempts to heal, for a long time impressed the idea that it was occasioned by the absorption of matter, and it is hardly credible that to this hour we sometimes hear this language repeated. Happily the practice, founded upon it, is no longer pursued. But it seems strange that when we are constantly wishing for the absorption of abscesses, to save our patients the inconveniencies we know must follow an open ulcer, we should apprehend danger from the absorption of matter similar to the contents of such abscesses.

The constant marks of hectic fever are increased heat towards night, succeeded by perspiration. All the other symptoms vary, according to the cause of the disease. If it arises from suppurated lungs, extreme coldness is often complained of during the morning, attended with a purple appearance about the lips and cheeks, and increased extenuation of every part of the body that is exposed. This probably arises from the loss of a considerable part of the lungs, and the imperfect action of the remainder. The constitution takes the alarm, and at that period of the day which, even in health, is attended with some increased action, the lungs and the whole vascular system are excited to furnish the blood with those properties necessary for restoring the standard heat, and for the support of life. The difficulty of accomplishing this is greater, in proportion as the atmosphere is cold or impure; and consequently whenever the attempt is made, the symptoms of fever are more striking. At

length the constitution sinks under the attempt at its own relief; the healthy actions of other important organs are interrupted; the fever, and consequently perspiration, no longer occur; the coldness is less considerable, or may be relieved; the patient feels himself better, and dies!

Such a state, though perpetually exciting our sympathy, is enviable, compared to many others equally fatal, but in which the doctor is deceived instead of the patient. We are apt, in order to heighten the colouring of an object which needs no such aid, to remark, that consumption is usually the lot of the young and the innocent. Let me remark, that other slow and incurable diseases are the lot of those whom, from the principles of our religion, and from our regard to the order of society, we ought to consider as the best members of both. In times of general distress, hope serves the sufferers, of whom we hear the least, as long as she affords them only a glimmering prospect of herself. When that ceases, the mental distress is not indeed furious in such characters, but such as will no longer admit the customary actions by which the body is preserved. Suicide rarely follows, but a fever, the cause of which being always present, only acts with increasing force at the usual diurnal period. This period, however, at first, is found the least distressing of the whole revolution. The mind is then resigned to a situation, against which it can no longer struggle. Sleep, which, during the night, was chased by the constant occurrence of dreary images, now succeeds to a half delirium of the senses, in which few objects appear in their true light—the patient has a short respite, and is accused of spoiling his night's rest, by an indulgence which the night never would have afforded him.

Happily these calamities now rarely occur on a great scale. Commerce, and that encouragement which industry and prudence derive from labour, and the uninterrupted circulation of the necessaries of life, have produced a greater equality in their distribution.

But we have lived to witness such times; and the recollection of every one will furnish him with instances of those who, too submissive to complain loudly, too proud to steal, and too grateful to be importunate, sunk under the weight of their distresses. It may afford some consolation to such, the enjoyment of whose affluence is only lessened by recollecting the distresses of others, to know, that such objects rarely die of hunger. Whilst the mind can be cheared by hope, the mere actions which support life may be maintained with less than could be believed: but when, from the causes above mentioned, a new train of actions must be set up, if these causes continue, such a change of actions can never lead to the restoration of health, but kindly to shorten the period of misery and existence. But this is only where, from constitution or habit, the mind is taught to consider submission as a virtue, or when the decreased vigour of the body supersedes the natural bent of the mind, or when, from the nature of the case, all resistance would be ineffectual. Thus we find the peaceable Indians submitting patiently to die from want, rather than violate their religion, or resist their rulers. Nor is this to be imputed to cowardice altogether. These same men form the strength of our Indian battalions, and show a degree of courage we could scarcely expect from their food, and the little attachment they must feel for their employers. A more striking instance has occurred in the northermost part of our own island, the scene of the proud submission of feudal heroism, and of some remains of the self denials required by ascetic religion. Whilst the inhabitants of this commercial metropolis were only restrained by violence and subscriptions, partly voluntary and partly forced—and whilst these means were insufficient in other parts of England—the same men, who could formerly intimidate government, engaged in so laudable a cause as the diffusion of religious liberty, submitted quietly to die rather than violate that order,

swerve from that submission, or infringe those rules which are so much talked of, but so rarely practised by others.

Thanks to an improved state of society these national calamities are rare, which is the more consolatory, as the remedy is not within the power of those who have the nearest view of the evil. Let us then all be cautious how we contribute to others within our own scope. Let us remember, that these are only among the impressions which impede the common actions of life, and which must, if continued, produce local or constitutional disease. Above all, let us reflect that these causes will be proportionate to the susceptibility of the subject to whom they are applied. The different periods of life render us susceptible to different impressions, which are varied according to the nature of our education or rank in life, permanent or transitory from the same causes, but most of all from the original dispositions with which we are born. The child is distressed if less praised than his equals; the youth if he feels conscious that his progress is slower; but most of all the man just rising to settled life, if his prospects are clouded, his character suspected, or his inclinations opposed in such a manner, that he is forced to forego what he conceives absolutely necessary to his future happiness. Under any of these circumstances the apprehension of disappointing his friends—that contracted view he has of life, which induces him to suppose that the eyes of all are fixed upon him, and the keen sensibility peculiar to his age, produce a conflict that has often proved fatal to the most ingenuous minds. From having seen this period so frequently fatal to young physicians, it is often suspected to arise from contagion. This may sometimes be the case; but those who converse with the commercial world on the subject, will find that the event is by no means uncommon in men of very different pursuits at that particular age.

In the other sex these causes operate still more powerfully, in proportion as the constitutional irritability is greater, the view of

external objects more contracted, and the necessity of preserving an unsullied reputation the more imposing. But in both it is much to be wished we could recollect, that the best are the principal sufferers. In both there are characters who would die at the suspicion of an act, which others would be proud of concealing, and almost indifferent to if discovered. A severe expression, to which a sense of duty prevented a retort, an imputation to which pride prevented a reply, have been considered as proofs of tame submission or guilt, and the repetition or want of explanation have proved fatal to the best of minds.

"'Tis the brain of the victim that tempers the dart!"

We ought further to remark, that the various changes the constitution undergoes at different periods, though for the most part so gradual as to be but little observed, are sometimes attended with those difficulties which will end in fever. This is more noticed in females, but I doubt whether the change in men is not often more striking. At what is called the more advanced climacteric, a long continued hectic ends in a continued, though slow, fever, the issue of which depends much on external circumstances, and from which, if the patient recovers, he is afterwards congratulated by his friends on a renovation of health and youth.

I have mentioned these among the causes of fever, which are continually occurring, some of which are, from their nature, too often unknown by the physician, and too little attended to by mankind at large. When some of these causes occur in a circle, whose habitation is rarely enlivened by the full rays of the sun—when it occurs at a season during which, those who have the happiness of a blazing hearth, are distressed at an open crevice, physicians well know, and have often deplored, the consequence. In large towns infectious air is the most common cause of fever among the poor,

and the most alarming to the rich, as it is impossible to say by what means they may be exposed to it. But whilst we are thus cautious, let us not increase the exercise of that selfishness, which, where life is concerned, is too apt to predominate. Let us reflect, that, as these causes have been more accurately explored, they have been found less formidable, and that prudent courage consists in ascertaining a danger, pusillanimity in flying from it. The love of life, inseparably connected with its possession, has sometimes unnecessarily superseded all those charities, which to man can only render life valuable: or the severity of state policy has confined innocent victims to a charnel-house, from which they might have escaped with security to themselves, and without danger to their persecutors.

We are then to consider, as the causes of fever, those impressions on the body or mind, which prevent the necessary train of actions by which life is maintained. These may be,

1st. The common events of life.

2d. When such events are general, the numbers affected will be so great, as to produce an infectious atmosphere, which will prove a new cause of fever.

3dly. A state of the air in its cause, unconnected with living animals, but equally affecting all who come within its influence, as intermittents from marsh miasma, influenza, or any other endemic or epidemic disease.

4thly. Contagions, the stimulus of which excite a secretion similar to that which caused the disease.

We shall proceed to consider the laws of each.

CHAPTER II.

OF THE MANNER IN WHICH DIFFERENT FEVERS SUPERSEDE EACH OTHER. OF INFECTIOUS ATMOSPHERE, ENDEMIC EFFLUVIA, AND PESTILENTIAL CONSTITUTIONS OF THE AIR.

WHEN those regular actions, by which life is maintained, can be no longer preserved, a new series of actions takes place, which, from the increased heat with which it is usually attended, is called fever. Strictly speaking, this change may be said to be present when the actions of life are so far altered, that hunger not only ceases, but the body grows insensible to the want of food; when the standard of heat, and the actions of the heart and arteries, are different from the ordinary state; and when all external objects are perceived with some uncertainty, though sometimes with an impression partaking of the same that existed during health.

Such a state as this can only remain for a time. It has its periods, like all other revolutions in the constitution; and if, during its existence, no organ necessary to life has been injured, or if the original cause ceases to exist, or the constitution is rendered insensible to its impression, convalescence follows the completion of its course. But if an important organ has been injured during the febrile action, or if the cause still remains, and the effect of its impression also, convalescence is impeded, or relapses occur, and sometimes with a rapidity not distinguishable from the primary disease. Such may be considered as laws applicable to all fevers. Let us next attend to the manner in which they influence each other.

We have seen how, by the operation of common events, fever is induced; how it becomes more general, in proportion as the

cause is more general; and how, under certain circumstances, it may give rise to a new cause, the effects of which will be extensive, in proportion to the circumstances from which it originates. But if fever, arising from such causes, and taking place under such circumstances, is sufficient to produce an infectious atmosphere, it can require no argument to show that fever, arising from any other cause, under the same circumstances, that is, the crowding of a number of sick together, must produce a similar effect; and this will be the case, whether the first cause should be the ordinary events above described, or the endemic ague from marshes, or an epidemic, as influenza or dysentery, or the contagion, as of small-pox or any other such disease. There is so little new in these remarks, that, instead of bringing arguments or authorities to prove them, perhaps most of my readers will expect an apology for being reminded of them. I must, however, take the liberty of pursuing this beaten path into divisions, which render it more obscure, and some of which have been overlooked by the earlier writers.

In the former part of this work, we have seen that one local stimulus may supersede another, and that a constitutional may supersede a local stimulus. We have seen that such an effect depends partly on the comparative force of the two stimuli, and on the susceptibility of the constitution to the impression of each: and that this susceptibility is much influenced by the novelty of an impression, so that a stimulus, which at first could not affect an impression which has already taken place, may afterwards supersede the same impression when it has lost the force of novelty.* All this reasoning may be transferred to constitutional diseases. Such effects have been traced by the most accurate writers, though the succession of events has not always been marked with that accuracy which is necessary for the establishment of a law in pathology.

* See pages 77, 78, 79, and 248.

In these temperate regions, and in that comparatively easy state in which the majority exist even in this overgrown metropolis, diseases assume a milder aspect. But if we trace them in countries nearer the sun, in camps, or in the writings of Sydenham, during whose practice London was little better than a garrison, and in some respects worse, we find some one disease constantly prevailing, and succeeding another in proportion to the force of each: sometimes determined by the seasons, varying with them, alternating with infection, and superseding or giving way to different contagions. Under such circumstances it would not be easy to make a distinction between endemics, epidemics, infections, and contagions. All will appear to owe their origin to one common cause, all will seem to commence in a similar manner, and such will be the transition from one to the other, that it will be difficult to fix the exact limits of each.

Sydenham, as Dr. Willan observes, was certainly not aware of the distinctions even between the origin of endemics, produced by marsh miasma, and contagions, which can only be produced by the diseased secretions of living animals. This is less surprising, when we consider that he never found the town free from some reigning disease, and that, whenever the common endemics were less powerful, the contagions seem to have raged with uncontrouled fury.*

The intermittent and continued fevers of 1661, he describes as beginning with the same symptoms, and as if originating in similar causes, the intermittent constitution ceasing with the autumn, when the continued fever was uninterrupted in its progress.

In another place the same diligent observer remarks, that when the small-pox is mild or regular, it commences about the vernal

* He is, however, disposed to look for the specific contagion of the plague as necessary, in addition to a peculiar constitution of the air, in order to render that disease epidemic; but even this concession he makes to facts, which he could not trace in the sphere of his own practice.

equinox, but when it appears sooner, viz. in the month of January, it seizes whole families, sparing none, unless they have already had it. By these, and every other passage, it is evident that, during the winter, London was at that time never free from an infectious fever, which was superseded during the spring of 1661, and three succeeding years, as is afterwards explained, by the vernal intermittent: as that abated, a short interval was sufficiently free from the infectious fever and endemic, to admit the contagion of small-pox, which " appeared a little, but disappeared upon the coming in of the autumnal endemics, viz. the continued fever and quartans which then raged." " In this manner," continues the author, " did these diseases succeed each other, during the constitution of these years."

But if either, from the mildness of the winter-infectious fever, or on account of the greater susceptibility of the human constitution to the variolous contagion from having been long free from its impression, the latter should prove so powerful as to commence its impression in January, instead of being retarded by the above cause to the short period of the summer—under such circumstances its force will be uncontrouled, " seizing whole families, and sparing none who had not already gone through it."

Considering the events above related, and witnessing them, as Sydenham did, he may at least stand as well excused in confounding the sources of endemics, infection, and contagion, as the no less cautious Cleghorn, who boldly asserts, that " these tertian fevers have as good a right to be called infections as the small-pox, measles, or any other disease."—" For although," continues he, " there certainly is a peculiar disposition in the air to affect numbers in the same way, yet those who are much conversant with the sick are most liable to catch the distemper;"* that is, by the infected air they breathe they become diseased, and that disease is superseded by the more powerful impression produced by the endemic miasma.

* Diseases of Minorca, page 121.

Thus the only difference in the error, between these justly celebrated writers, consists in one considering all these diseases as endemics, and the other as contagions. Had Sydenham practised in a remote part of the country, where the introduction of small-pox contagion can always be traced, or had Cleghorn treated intermittents only among a scattered peasantry, instead of a camp or military hospital, probably if neither of them had been confused by the infectious atmosphere, which the seat of their practice produced, both would have avoided their error, and posterity have derived still greater advantages from their labours.

On this account it will be absolutely necessary, as far as possible, to ascertain the laws of an atmosphere, rendered infectious by accumulating the sick, and also the changes produced in the human body by the application of such an atmosphere. In order to distinguish this, which we have called *infection*, from epidemics, endemics, and contagion, we are to consider that, though contrary to the two first, the cause is to be looked for in the human secretions, yet for its production the accumulation of numbers of sick, labouring under any complaint, is sufficient. Thus, in its origin, it differs from contagion, which can only be excited by its own particular disease. The next question will be, Whether, when once excited, it can be communicated like contagion; that is, whether the secretions of an individual, labouring under the disease excited by the infectious atmosphere, can infect another person in a similar manner; or whether the presence of such an atmosphere, or the accumulation of numbers, will be necessary to produce such an effect?

In this enquiry we must recollect, that the actions excited in the human body by this atmosphere, are not always similar, though hitherto we have not succeeded in discovering any difference in the properties of the cause which should lead to such a difference in the effect. The mildest form in which we see disease thus excited, is a general vesicular appearance about the hands and fingers, which is

contagious in a degree, in some measure dependant on the situation of the subject to whom it is communicated. If to one of the children of poverty, among whom the cause has originated, the effect is pretty similar. If to one in better condition, a few solitary vesicles will appear, and soon disappear spontaneously. But as long as the cause continues among the former, the effect will remain, varying in different subjects, according to partial exposure and other accidental circumstances: sometimes assuming the appearance of *favus*, as described by Dr. Willan, or a scabbiness about the face and scalp, as mentioned by Dr. Jenner. The latter gives the general name of herpes, or herpetic blotches, to the whole; and perhaps if we call it *herpes pauperum*, we shall find this as descriptive a term as any appearance so general will admit. I suspect that the moist itch, mentioned by Dr. Gillispie,* is of this kind: for it is past a doubt, that a constitution engaged in such an action, may be insensible to infections and contagions. This has been well remarked by Dr. Jenner,† in his vaccine experiments, and I can add my own most ample testimony to the same.‡ Those who have had the largest opportunities, and improved them the best, describe a variety of other diseased forms which the infectious atmosphere induces. Among these are diarrhœa, dysentery, inflamed eyes,§ sore legs, and gangrenous ulcers. These, under an aggravation of the cause, are sometimes suddenly superseded by the worst form of infectious‖ fever.

In considering how far the subjects, thus affected, become contagious, we must then admit that such secretions, as can be retained in a substantial form, have the power of contagions. Not, however,

* See above, page 308.

† Med. and Phys. Journal, Vol. XII. Page 97, & alibi. ‡ Id. Page 194.

§ This has no reference to the Egyptian opthalmia, the nature and cause of which the author has had no opportunity of learning.

‖ See Dr. Jackson's Outlines, Page 42.

with the uniformity of those well marked contagions, which may be justly called Morbid Poisons. The forms themselves are uncertain, and their effects vary not so much with the constitution of the persons exposed, as with other external circumstances. Thus all the varieties in small-pox are, as far as we can judge, independent of the degree of impregnation in the atmosphere, or concentration of contagion. Of the infectious atmosphere, on the contrary, we shall find these degrees inducing a corresponding affection in the persons exposed: in small-pox, whether we apply the contagious matter in a substantial form, or by effluvia, we produce the same disease, though in a milder degree. From infectious atmosphere the whole disease often varies with the form in which it is applied. And though we admit these secretions, in a substantial form, may excite similar actions in another person, yet the enquiry still remains, how far the insensible secretion or effluvia, from a person under the infectious fever, is contagious.

It is established beyond a doubt, that the infectious atmosphere may be so attached to many things long retained in it, as to infect those who are exposed to such things, when removed from the source of the atmosphere. But if these substances, generally called *fomites*, are imbued with the atmosphere from the *crowded sick*, the property thus acquired does not prove, that the effluvia from an *individual*, infected by such fomes, would produce the same effect. To ascertain this, we must refer to certain facts, their series and order: and as the question will not admit of common experiment, on account of the subjects affected by it, we must trace with minute diligence such records as are well authenticated, and by the frequent recurrence of uniform consequences, form our decisions, till they are contradicted by subsequent events.

The first consideration, in these cases, is the place in which an individual is infected. If two friends are in Newgate, and one of them under a fever; should the other be seized with the same, whilst

attending upon the first, no one will pretend to ascertain whether he was infected by such an attendance, or from the same cause that infected his friend. We are next to recollect, that the effect produced is in proportion to the degree with which the atmosphere is impregnated, or, as it is usually called, in proportion as the infection is concentrated, joined to the susceptibility of the persons exposed. Let us suppose, for instance, that the air of a prison is at first pure. The despondency and little ailments unattended to, under the general distress, vitiate this air so gradually, that the inhabitants are familiarized to it, and though none may be in high health, yet actual fever may not be perceived. A stranger is introduced from a pure air, and seized with fever, from his greater susceptibility. The air now becomes more vitiated from this greater accession of cause, and though the patient may recover, yet his recovery will not restore the air to the state in which he found it. The next person, therefore, that is admitted, is seized with greater violence, in proportion as the cause is greater, though his susceptibility may have been the same. The air is further vitiated as the same events are multiplied; at last the jailors are infected, though accustomed to a certain degree of infection; and strangers are seized with a suddenness and violence proportioned to the concentration of the cause, to their degree of exposure, and of their own susceptibility.

In 1586, happened what is called a black assize at Exeter, the circumstances attending it are so remarkable, that it seems difficult in these times to conceive how the cause should have been so mistaken. A number of Portugueze sailors, taken prisoners at sea, were put into the common jail, where they remained some time before they all fell sick. The sickness soon spread through the jail, so that when the sessions arrived, the prisoners were many of them brought in hand-barrows to be tried. The mortality in the court was dreadful. But what I wish to remark is, that because the Por-

tugueze, from being new to the prison atmosphere, were first infected, and by their numbers and consequent disease, no doubt, contributed further to vitiate the air of the prison, they are considered by Baker as having introduced an infection, which did not appear among them till they had been some days confined, without change of cloaths, in a "*deep pit and stinking dungeon.*"

Of the black assize at Oxford, it is particularly said, that the disease and mortality, though affecting near 300 people, did not spread beyond those who were immediately exposed to its influence.

In the black assize at Taunton, not only the court were infected, but many in the town; which was easily accounted for, because the infected prisoners were brought from Ilchester jail, and, it is unnecessary to add, the probability that the whole populace flocked to see them on their arrival.

The last black sessions in the Old Bailey was held on the 11th of May, 1750. On the 13th, died Alderman Lambert; on the 14th, R. Cox, under sheriff; on the 17th, Baron Clark; on the 19th, Sir T. Abney, Justice of Common Pleas, T. Otway, barrister, W. Baird, ditto, W. Sharplop, and four others; on the 20th, the Mayor and eight of the Middlesex jury. All these individuals were seated in the course of an atmosphere likely to pass from the prisoners to an open window. The rest of the court escaped.

At the assizes at Oxford it is evident, by the history, that no one was infected by those who were in court, for their sickness was by some imputed to causes which could only influence such as were present. In London, too, there is reason to believe that no persons were affected in the families of the deceased; because the list of deaths in the Gentleman's Magazine, which is very precise in marking all who died from this event, gives no account of any but such as were in the court; nor do the subsequent remarks on the calamity hint at any further extension of the infection. But we are not on that account to determine that such a fomes is, under every circum-

stance, innocuous beyond its immediate source, since we find, in two years after, a fever from the same jail was communicated to the families of the infected.

By a paper, read before the Royal Society, and published in the Gentleman's Magazine for February, 1753, it appears, that in consequence of the dreadful event at the Old Bailey, above related, Dr. Hales' tubes and windmill were constructed for purifying the air of Newgate. Five carpenters, engaged in this business, were affected with jail fever. Of these, two were inhabitants of close rooms: the wife of one was infected: of the other, the wife, two children, and his sister-in-law. His wife's mother, who did not reside in the house, but came to nurse, died of the fever; and the man himself, being obliged to labour during his convalescent state, died at the end of several weeks. The other three carpenters, being in well aired rooms, do not appear to have communicated any infection. Two of them were removed to the hospital, but there is no reason to believe any infection was spread in that house, as the reporters, who seem to have taken great pains to trace the progress of the infection, are silent on that subject. This valuable history, which does so much honour to all the parties concerned in collecting it, would probably have been lost, but for the laudable industry of those gentlemen: and a family would have been exposed, without any means of relief, to all the miseries attendant on the death of a husband and father, who perished in the public service.

By comparing these two events of the sessions-house and the prison, it appears that persons affected by the infection of jails, are not in themselves infectious, but that they render a close apartment infectious to such as before lived in the same without any apparent inconvenience. It may be doubted whether, in these instances, the families were infected by the fomes contained in the cloaths of the husband, or even by infection previously existing in a close neighbourhood. But the first is contradicted by the different periods at

which the several persons became sick, and the second by the alarm the neighbourhood expressed at the introduction of an infectious fever among them.

Though there is nothing altogether new in these remarks, yet I have thought it right to bring forward such striking facts relative to this particular subject of infection, because, whether it is introduced into a camp or hospital, or produced from the numbers of sick accumulated together, it must, in a certain degree, influence the character of every other complaint, whether endemic or contagious, and, as we have before seen, produce a degree of uncertainty as to the contagious property of each.

The difficulty of exactly ascertaining the limits of these diseases, of tracing their origin, and even appropriating the symptoms to each, will be much increased, if, during this complication of causes, an epidemic should supervene. The force of this last, as we have observed of all other stimuli, will be great in proportion as the human constitution has been long unaccustomed to its impression. In crowded towns the constitution is almost familiarised to air in a certain degree vitiated, and in the neighbourhood of marshes to the endemic miasma. But the epidemic catarrh, or dysentery, is sometimes so novel a stimulus, as to supersede for a time every other impression. It proves more universal than the contagions, insomuch as the constitutional susceptibility returns with the cause: has often been more destructive in its consequences, inasmuch as its universality injures the state of the atmosphere, whilst the body, already debilitated, is less able to resist infection thus produced. If such an epidemic should invade a town previously influenced by, or from circumstances disposed to, any general disease, the consequence must be, that the new impression will not only seize those who are not already affected by the former reigning complaint, but in others supersede it, or aggravate the disposition to any other disease, so as to bring it into action. Hence new trains of symptoms will arise.

In some the progress of the previous disease will be suddenly arrested; in others convalescence will be impeded; others will complete the periods of the first disease, which will instantly be succeeded by this new impression; and all who are obliged to breathe such an atmosphere will remain valetudinary, till some conflict of the elements induces a change wherever its influence is admitted. See how well all this was remarked by SYDENHAM, in his description of the "epidemic constitution of 1675."

He describes the epidemic catarrh invading the town, at a time when it was already under the influence of infectious fever.* This infection was at first mild enough to admit the intrusion of measles and small-pox, the former of which prevailed from spring to the summer solstice, when the latter was the prevailing distemper through the autumn, and as long as the "uncommon warmth of the winter" remained; "but when the cold weather commenced it abated, and gave place to the fever."

"This fever, which had continued," we are told, "the whole year, made great devastations in the beginning of July, 1675." In this manner it preserved its character, slightly influenced by bowel complaints as the autumn approached, till the arrival of the epidemic catarrh. "But it is of moment," adds our author, "to observe, that the stationary fever of this constitution usually succeeded these coughs, and hence became more epidemic, and likewise varied some of its symptoms."

All this is well explained, when in a subsequent passage he enters into a detail of the invasion and symptoms of the epidemic catarrh.†

"In 1675, the season having continued unusually warm, like summer, till towards the end of October, and being suddenly succeeded by cold and moist weather, a cough became more frequent

* Swan's Sydenham, page 188, 2d edit. † Id. page 209, 210.

than I remember to have known it at any other time; for it scarce suffered any one to escape, of whatever age or constitution he were, and seized whole families at once. Nor was it remarkable only for the numbers it attacked, but also on account of the danger that attended it. For as the constitution, both now and during the preceding autumn, eminently tended to produce the epidemic fever above described, *and as there was now no other epidemic existing*, which by its opposition might, in some measure, lessen its violence, the cough made way for, and readily changed into the fever. In the mean while, as the cough assisted the constitution in producing the fever, so the fever on this account attacked the lungs and pleura, just as it had affected the head even the week preceding this cough; which sudden alteration of the symptoms occasioned some, for want of sufficient attention, to esteem this fever an essential pleurisy or peripneumony, though it remained the same as it had been during this constitution.

" For it began now, as it always did, with a pain in the head, back, and some of the limbs; which were the symptoms of every fever of this constitution, except only that the febrile matter, when it was copiously deposited in the lungs and pleura, through the violence of the cough, occasioned such symptoms as belong to those parts. But nevertheless, as far as I could observe, the fever was the very same with that which prevailed to the day when this cough first appeared; and this likewise the remedies to which it readily yielded plainly shewed. And though the pungent pain of the side, the difficulty of breathing, the colour of the blood that was taken away, and the rest of the symptoms that are usual in a pleurisy, seemed to intimate that it was an essential pleurisy, yet this disease required no other method of cure than that which agreed with the fever of this constitution, and did no ways admit of that which was proper in the true pleurisy, as will hereafter appear. Add to this, that when a pleurisy is the original disease, it usually arises betwixt

spring and summer; whereas the distemper we now treat of, begun at a very different time, and is only to be reckoned a symptom of the fever which was peculiar to the current year, and the effect of the accidental cough."

Such was the progress of the influenza invading a crowded town, and with an atmosphere already infectious. Whoever carefully examines the reports concerning influenza, contained in the last volume of the Medical Society, will be struck with the different effects produced, according to the state and population of the towns it invaded. In many of the more crowded it is described as inducing typhus; in others as exasperating other diseases, and rendering them fatal. Sir John Pringle also has left us a lively description of the different effects produced by epidemic catarrh, according to the circumstances of those who were exposed to it.

"In the beginning of February, the troops moved from their winter-quarters, and marching into Germany, were cantoned in the county of Juliers and at Aix-la-Chapelle. Only part of the cavalry was left at Brussels, and the sick and weak, to the amount of 600, being collected from all the garrisons, were put into the general hospital at Ghent. The weather being favourable, the troops entered Germany in good condition.

"Soon after the influenza (a short fever, attended with violent catarrh) passed through great part of Europe, was sensibly felt at Brussels, though but little in the cantonments, otherwise than that many, who, in the preceding autumn, had been seized with agues, then relapsed."*

These epidemic diseases, whether catarrhs or dysentery, were much more fatal, when a more unsettled state of things obliged our ancestors to crowd within towns defended by walls and ditches. Our own history affords many presumptive proofs and some well ascertained instances of this. The plague of 1479, began in Sep-

* Diseases of the Army.

tember, and lasted till the November following. That of 1543, was preceded by a distemper among cattle. The first is rather a period of the commencement of a catarrhal or dysenteric epidemic than of the plague, and the latter is a frequent attendant on human epidemics. In the year 1732, a cold, attended with fever, raged so violently in the city, particularly among the aged, that in one week the bills of mortality were quadrupled. It was computed not one in six escaped.* Had Sydenham given us the history of this complaint, we should probably have found that the infectious fever, or, as he calls it, the epidemic fever of the winter, had a considerable share in the mortality; and probably had the same events occurred before the great fire, this might have been enumerated among the plague years. Sinnertus, when describing the epidemic catarrh of 1580,† observes several symptoms in common with the plague, and afterwards pursuing the enquiry into the histories of malignant angina, pleurisy, and peripneumony, he shows that by the two last he means, as well as Sydenham, the common infectious fever, with a constitution of the air which induces catarrhal or pulmonary symptoms.

In the accounts we receive from antiquity, it is not easy to ascertain what disease is exactly meant by a pestilence. When fever, producing great mortality, affects only a single town, we may impute the whole to the common infectious atmosphere, exasperated by some general distress or accidental epidemic. But when such a complaint travels over a considerable part of the globe, the cause must be in the atmosphere itself, though the effect may be greatly exasperated by other contingent circumstances. It appears as if the celebrated plague of Athens, if not induced, was at least much hastened by the invasion of an epidemic catarrh. The suddenly increased commerce and consequent population of that city, par-

* Maitland.

† De Catarrho cum Tussi Epidemia Maligna, Vol. II. Page 208.

ticularly near the port, had so outrun the necessary accomodations, that the aqueducts were not prepared when all the inhabitants of the country required the protection of the walls. At this time an influenza prevailed over the southern part of the known world, and seems to have produced or augmented a similar calamity in Ethiopia, Egypt, and the more populous towns under the dominion of Persia. Thucidydes describes the first symptoms as those of common catarrh or pleurisy, which yielded to no remedy. It is remarked, that the previous period had been particularly healthy, that is, no other reigning disease existed at the time in the town, so that the epidemic was uninterrupted in its influence: even the few who were affected with common complaints, soon felt them changed to the new disease.

In a short time the symptoms, which at first were principally confined to the fauces, chest, and stomach, showed themselves in every part of the body. Under the general infectious state of the atmosphere, putrid fevers and dysenteric fevers prevailed: by degrees the tragedy becomes so complicated, that only the more striking characters can be observed; but among these the true pestilence is easily distinguished. The disease, with various degrees of violence, is described as remaining with the Athenians for four years, following them in their fleet and encampment, as well as attached to the town. It is, however, probable, that after a time nothing but the infectious fever existed, so general, and with symptoms so violent, as to pass for a milder form of those various diseases which were before confounded together.

Thus have we pursued the subject of infectious atmosphere till it has insensibly led us to the plague, a disease which by some has been considered as only a higher degree of what is called putrid fever, or, as I have preferred calling it, of fever from infectious atmosphere. But there must be an uniform difference in some of the

symptoms, for though we may expect that such a disease would soon produce an infectious atmosphere, and thus that both fevers would appear at the same time, yet this is what we have seen to be the case in many epidemics, whose symptoms cannot be so mistaken. The mortality at the last black assizes was in some of the sufferers not less sudden than often happens at the worst periods of the plague; in others the symptoms were slower, yet of the number of victims, none exhibited any pestilential tokens.

Before I proceed on this subject, I shall offer a few remarks on all I have been able to collect concerning one cause of fever, which, though generally confounded with others, seems to me peculiar to itself. The cause to which I refer is the effluvia from the corruption of dead animal and vegetable matter, principally the filth of unpaved or ill-paved towns, where the level is badly preserved, and the police inattentive to cleanliness. This has sometimes been confounded with the infectious atmosphere formed by the accumulation of sick. Dr. R. Jackson, in my opinion, with more propriety, classes it with the miasma from marshes. But as neither the cause nor the effect are exactly similar either to this, or the fever from infectious atmosphere, it may not be amiss to remark those particulars in which they all differ.

That the summer fevers of the large towns on the continent of America is not the common ague, however exasperated by circumstances, cannot, I think, be questioned. The country about some of those towns is too well acquainted with ague to mistake its symptoms, or to suspect its contagious properties. If the inundated soil in the neighbourhood of rivers could be the cause, what relief could the citizens find from abandoning their residence? Does not the disease also commence at a period between the vernal and autumnal intermittents?—Still less can it be the infectious fever from the crowding of unhealthy or distressed subjects. The Americans, of all other people, are the least obnoxious to such causes, and the

season of the year is of all others the least likely to increase them. If, then, it is neither marsh miasma, nor the infection from an accumulation of sickness and distress in the winter season; if it is connected every where with a want of cleanliness in the street and a burning sun; and if it requires an universal calm, or at least is found to prevail in those spots which a vertical sun may reach, whilst the breezes may be excluded; if it ceases as these causes cease, it must follow that the cause is different from infectious atmosphere, and probably the effect also.

It must be unnecessary to remark, that this disease, being well known to men who have abilities equal to their opportunities of investigation, all that has been said on the subject of America, can be with no other intention than, if possible, to illustrate those limits to which we may confine contagion. This enquiry is, for the most part, of less importance in epidemics, because no caution can secure us from being exposed to the influence of an epidemic catarrh, or dysentery. In endemics it is of great consequence, because it will teach us rather to leave the place than to confine the sick, who, when removed from the source of the disease, may be no longer dangerous: and such has happily proved the almost universal practice in the American towns, amidst all the discordant opinions relative to the contagious properties of their fever.

But, uniform as the success of such a plan may be in the American towns, we are not to expect it can be so undisputed in every other part of the world, or among men whose modes of life are different. For if the endemic prevails in old crowded towns or in a camp, and in an unfavourable season, and has continued long enough to produce an infectious atmosphere, that atmosphere will remain attached to the cloaths and persons of the fugitives, and may infect those who receive them. Even the atmosphere, which produced the endemic disease, may, in some instances, be conveyed in a similar manner; but as it cannot be increased, its effects, in this

new situation, must be very partial and transitory. The progress of the infection, on the contrary, must depend on the manner in which the sick are disposed, on the condition of those who afford them hospitality, on the season, and on the prospects which the posture of affairs holds out. If, as in America, they are received in an open country, during the summer season, and among those who, from experience, have no apprehension of contagion reaching themselves, where the want of provisions is unknown, and at a period of profound peace, the happiest consequences must follow; and the mutual congratulations of the convalescents and their friends, will diffuse those sentiments of reciprocal regard, from obligations conferred and received, which will convert a scene of terror and suspicion into a display of all the amiable passions.

But if this endemic prevails under all the unfavourable circumstances of precarious existence, an infectious atmosphere must soon be the consequence; and if the condition of the neighbouring villages and towns scarcely differs, excepting in the absence of the endemic, the fugitives, whilst they fly from the *endemic*, will not only bring with them the *infection*, but probably meet with a slighter degree of it in the towns. The same will soon be communicated to the villages, and increased in all by the apprehension of calamities, exaggerated by hourly reports.

In speaking in this manner of yellow fever, it is hardly necessary to remark, that we must confine ourselves to the fever known by that name in the towns of the American continent. Of this we have had sufficient particulars, historical and descriptive, to form some conclusive opinions on data admitted by all parties. Not so of the fever under the same name in the islands, in our armies serving in those latitudes, nor in our fleets. Many of the writers of the first have omitted to give accurate descriptions of the site of their practice, of the seasons, of the previous exposure of their patients to the elements, of their customary habits, and of those

immediate causes which may have contributed to accelerate actions in many instances leading rapidly to fatality. We know, indeed, that most of the towns are built in unhealthy spots, on the leeward of the islands, for the advantage of a vicinity to ports; and we know that in these places fevers rage with the greatest violence. Whether sewers are constructed, or proper slopes preserved, or whether the offal of markets and slaughter-houses, are left for the evaporation of the sun, in these places so quick, as to prevent inconvenience to passengers—on these and on many other subjects, we are not sufficiently informed; and if we were, the complicated nature of the subject might render further knowledge necessary. In our fleets and armies new difficulties occur: we know how much every fever partakes of the season and climate, and have seen, even in this temperate region, the difficulties our most accurate writers have met with in discriminating causes whenever the effects have been rapid and confused. Can we suppose, then, that the infectious fever of crowded sick will not assume a character from the climate of the torrid zone? And can we wonder, as long as we suffer ourselves to be governed by names, if a yellow fever in camps, in garrisons, or in fleets, is alternately infectious and endemic? Dr. Jackson, who was well aware of these intricacies, admits the number of symptoms each have in common. Even the more violent forms of the infectious fever in Europe, he found bore a strong resemblance to those in St. Domingo: but with his usual accuracy he warns his readers to attend to the causes of each, even should the symptoms be similar, and the early treatment also.*

Whether the fever of the American towns be really a contagion or not, does not seem to me a matter so much of difference in opinion as in words. For argument's sake we will admit that it is a contagion brought from abroad, yet all admit it can only be introduced at certain times; and in a short time all will admit that it can

* Outlines, Chap. VII.

only be introduced under certain circumstances. The first is so perfectly understood, that quarantine is only required during the summer months; and wherever the towns have been regularly paved, furnished with plenty of water, and the police properly enforced, the fever has never been introduced. Such as are seized in the town, have never infected others in the country; so that those who maintain the contagious quality of the fever, allow that a certain state of the air is necessary for its admission when imported. Now, if this state of the air is such as can be prevented, common prudence would induce us to use such a precaution, rather than run the hazard of those surreptitious intrusions, which, whenever the fever has appeared, have always been discovered, and which probably always exist. I am aware the same has been said of the true plague, and that the absence of buboes in one instance, and of the yellow colour in the other, seem to be the only difference between these two diseases: nay, that these very marks are by no means deemed essential to constitute either: that both commence about the same time of year; are peculiar to towns neglected in similar points; that both cease under the same circumstances; and as they cease, that they leave no fomites in the cloaths or dwelling of the infected. Let us then see whether the contagious property of the plague can be maintained on any better foundation than the yellow fever of the American towns.

In this enquiry we have the same difficulties to encounter as in the former; but happily we have the same assistance. We have the authority of men, whose fidelity is unquestioned, and whose opportunities have been equal to ascertaining all they describe. To these I shall confine myself, not from any disposition to doubt the good intentions of others, but because the evidence of the first is as much as I venture to advance on a subject so important.

Mr. Howard, on his departure to visit the various lazarettos in Europe, was furnished with the following well selected questions by Drs. Aikin and J. Jebb:

1. Is the infection of the plague communicated by the touch?

To this six different physicians give answers with different reservations; the seventh, a priest, asserts, that from eighteen years observation, he has found the plague communicated by contact.

2. Does the plague ever arise spontaneously?

To this two physicians of Marseilles, and one of Venice, answer in the negative — one from Malta, and one from Leghorn, with doubts--one from Triest in the affirmative. A Jew physician, of Smyrna, pleads the ancient authorities, and a prior common observation, that the disease is always imported.

3. To what distance is the air round the patient infected?

To this, as may be expected, the answers are various and uncertain.

4. What are the seasons in which the plague appears; and what the interval between the infection and the disease?

The Marseilles physicians say all seasons; one of them adds, less at the two solstices; the other most in the greatest heats of summer.

The physician at Leghorn says the same.

The Maltese—warm and moist weather contribute to the production of *all* contagious diseases.

Of Triest—the spring.

The Jew—from spring to midsummer.

The prior—from April to July: great heats and cold diminish, and copious dews extinguish it.

Those who speak decidedly, assert, that from twenty-four hours to three days is the medium from the reception of the infection to the appearance of the disease.

5. What are the first symptoms?

All agree that these vary, and that buboes are not absolutely essential to the character of the disease.

6. Are there two fevers called the plague, one of which may be communicated from the air without contact, the other by contact only?

The Venetian physician says there are two.

The physician of Triest says they are both the same.

The Jew of Smyrna—there is only one, yet some get the disease nobody knows how.

The prior—only one, but different in malignity.

The 7th and 8th questions relate to the treatment and prevention.

The 9th to the probability of relapse.

I shall now make a few remarks on the facts offered us by Dr. P. Russell, passing over all that he selects from others, and the candid manner in which he treats all those who hold an opinion contrary to his own.

The first and second cases, related in Dr. Russell's invaluable Journal, are of two porters, who are supposed to have been infected by attending upon some merchants from Damascus, where the plague then existed. These men both died; but of four Armenians, who by turns attended them, "no one caught the infection."—The most that can be inferred from this case is, that the fomes excited the disease, but that the persons in whom it was excited gave no proofs that they were contagious.

The third case was the son of a Jew banker. The patient lay in a small but clean chamber, with a low ceiling, and was well attended by three women, besides his mother, who were constantly sitting on or near the bed. "None of them caught the infection." Yet these women had searched for eruptions or buboes. Eleven days after the death of this patient, his brother, who does not appear by the account to have visited the deceased, was seized, and died in three days. Immediately after which the women left the house. The grave-digger was taken ill on his return from the

funeral, and died. Five more of his family successively met the same fate.—The fair inference from this account is, that the two young bankers, having business in various parts of the town, were each of them exposed, in some of the obscurer and more dirty quarters, to the pestilential atmosphere, but that neither of their persons proved contagious. It is generally admitted, that a corpse is not contagious: the probability therefore is, that the grave-digger lived in a pestilential part of the town, which proved fatal to himself and all his family.

Cases IV. and V. are a mother and daughter, *both taken ill at the same time.* "Their nurse was not infected."

VII. A Jewish rabbi. "Of several females, who closely attended this patient, all escaped, his wife only excepted. She was taken ill eight days after her husband's death."

VIII. An Arab, whose wife suspected that he caught the disease in a *keisaria*. This place Dr. Russell went to see, and found it inhabited by bricklayer's labourers, and that numbers had died there. IX. and X. were in the keisaria; a mother and son ill on the same bed.

XII. A boy, "most affectionately attended by his mother. Two other women, with a girl of seven or eight years old, were often in the chamber, and during the two or three first days of his sickness, he had been visited by more than a dozen women; but none of them," adds the Docter, "so far as I could learn, were infected."

XIII. and XIV. A Jew and his wife, and a child, died in the same "ruinous old house, several steps lower than the level of the street." The rest of the inhabitants escaped, contrary to Dr. Russell's expectation, on account of their miserable dwellings, which, he adds, "from the want of ventilation, the sordid beds, and neglect of cleanliness, I always considered as dangerous receptacles of contagion." It should be remarked, that the Doctor gives an account

of three who died, and by the history it is probable that he might be unacquainted with the subsequent events, for, after his first visit, he says, " I directed proper drinks, and they were to send for their medicines; but I heard no more of them till after they were buried."

XV. A delicate youth, constantly attended and nursed by his grandmother, one or two old women, a man, and an Armenian boy, all of whom escaped the infection; a circumstance which the Doctor thought the more remarkable, " considering the unusual number of eruptions."

XVI. A Jewish girl died; nursed by one woman and two men. These, and the whole family, consisting of twelve or fourteen people, had been often in the chamber, yet all escaped.

XVII. An Armenian youth, cook to an English gentleman. He lived, not with his master, but in a house with seven others, six of whom died (probably in a pestilential part of the town.)

XVIII. and XIX. who died, being the whole family, excepting one old man: two others had died before.

XXI. A Christian youth, nursed by his sister, who escaped.

XXIV. A merchant, attended by two or three people, who all escaped, as well as his family.

XXVII. A young lady; the family all escaped.

XL. Was a woman, delivered at five months: the child had buboes when born.

XLI. A lady, with a child at her breast, who escaped.

LI. A young man, an *Armenian*. Several ill in the house.

LXXI. A Christian woman; her husband seized a month after.

LXXVII. to LXXX. called a distressed family: the father, the son, his wife, and daughter, were seized at various times.

LXXXIII. to LXXXVII. Members of one family, seized in

succession. The first is said to be a girl, who continued going about her household affairs till she was taken ill.

LXXXVIII. A youth, who had lost two brothers.

XC. A Circassian lady, in the house of a Turk merchant, in which seventeen, out of twenty-two, had died.

XCIV. and XCV. A youth and his sister, both seized at the same time.

XCIX. and C. A boy, and the slave, about his own age, five days after.

These are all the cases in which the Doctor mentions whether the family was infected or not: the presumption is, that he had not heard any other particulars than such as he mentions. It should be remarked, that after the Case XXVIII. he was shut up with the Europeans, consequently could only collect the cases of those patients who consulted him from his window, or who sent their friends for that purpose.

The history contains an account of twelve families, in which the disease did not extend to the attendants, though, in four of these houses, two persons were affected. Eight houses, in which the disease extended. Some of these were at a period that one should expect infection from the sick, but most were not. However, there must always be an uncertainty in this, excepting that when two are seized at the same time, the contagion must have been from one common cause, and not from each other.

The houses, in which the diseases extended, were principally among the poor, and therefore in situations in which the plague was likely to be endemic. This appears more probable, from the presumptive difference between some of those families in which the disease spread, and others in which the attendants all escaped.—We have already taken notice of the porters and of the banker's two sons. The fourth and fifth Cases are described as both occurring at the same time. We have no account that the Arab's wife, who sus-

pected her husband caught the disease in a keisaria, was infected; but Dr. Russell found the keisaria full of pestilential subjects.

The merchant (Case XXIV.) whose family escaped, caught the disease at a khane, at a distance from his house, where another person died. These remarks might be extended further. But such as wish to pursue the enquiry will consult the work; and I feel more fearful of giving the reader a wrong impression, than anxious to convince him. For this reason I have left unnoticed all those writers who oppose the contagious property of the plague. Having, however, taken a review of the Doctor's facts, let us follow him through some general observations, which lead to practical inferences.

Dr. Russell remarks, that contagion, *per fomitem*, is much more extensive than in any other way. Even if this should be ascertained, it would only prove that *pestilential* air may be absorbed by certain substances, and produce disease, as we find the case with *infectious* atmosphere. But it does not necessarily follow, that the diseased are contagious in either case. It seems admitted, indeed, by the same author, that the occurrence of fomes and pestilential atmosphere, are both necessary to produce the plague.

"We ought," says he, "not more to dread the vicinity of the infection, than the vicinity of the pestilential constitution of the air, which has been observed to pass progressively from one region to another."

"The approach," continues he, "is slow, silent, and imperceptible; no human barrier can be opposed to it; but if it do not meet with latent seeds of contagion to animate or invigorate, it will pass on, perhaps harmlessly, to other regions. The prevention of a concurrence so destructive to mankind, is the grand object of quarantine."*

* Russell on the Plague, 434.

By this passage we are to understand, that a pestilential atmosphere must meet with a fomes, or subject already diseased, in order to render it deliterious. We shall also see that these may be innocuous, excepting a pestilential atmosphere is present.

" In the mean time," says Dr. Russell (page 26), " so far is certain, that although infected persons came from the mountains to the three towns just mentioned, and some of them died in the families where they lodged, yet the distemper, by such means, was not propagated, as if divested of that contagious property in the plains, which it seemed to retain undiminished in the mountains."

Thus it appears that a fomes of itself is an insufficient cause for the production of the disease. Is the addition of putrid effluvia sufficient? This is improbable, because we have no reason to suppose that the towns, which are only occasionally visited by the plague, are any cleaner when the fomes does not induce the disease.

Yet it is impossible to impute the exemption, which London has enjoyed since the fire, to any thing but greater cleanliness: and this cleanliness consists principally in the destruction of our ditch, which surrounded the wall, and the preservation of a proper level in our paved streets. It appears by Stow, that Wal*brooke*, Sher*bourne*, Lang*bourne*, Old*bourne*, &c. were truly brook-, till perpetual encroachments, and the quantity of filth, from increased population, by degrees reduced them to sewers. Maitland informs us, that in some places the currents were so much interrupted, or the level so badly preserved, as to form pools. After the great fire, the level of the streets was pitched in such a manner, that there might be a free current to the river. The ditch was covered or filled up, excepting the Fleet, which at this time has sufficient back water to form a stream, such as its name imports, and which still turns a mill between Saffron Hill and Turnmill Street. But though the above causes were always present before the fire, and though London was constantly the seat of infectious fever, yet the true

plague was only an occasional visitor, or, at most, only sporadic, for years together.

Dr. Hebberden very justly argues, that " the correspondence in the dates of our last great plagues, with those of Amsterdam, affords a strong presumption, either that one of these cities must have received the infection from the other, or that both of them received it *from one common source*." The only question is, What is that common source? If diseased subjects are at one time contagious, or if a fomes at one time induces the disease, and at other times both prove innocuous, we are at least certain such causes are insufficient of themselves. Is it certain that they are at all instrumental? If " the pestilential *constitution* of the air is slow and progressive from one region to another," the wind, which wafts a ship from a pestilential city, is likely to be pestilential: and if the plague arises at the place of destination, about the time of the arrival of the vessel, the question will be, Whether such an event is to be ascribed to the pestilential constitution meeting with a fomes, or with a dirty town, during a temperate season?

It is true, that the last plague is suspected by writers of that date to have been brought from Amsterdam. But Hodges, whose authority is often quoted, opens his historical account by saying, that " at the close of the year 1664, three persons died in Westminster; and that some of the neighbours, under apprehension of contagion, removing into London, brought the pestilential taint with them." Mead says, that " the disease began in St. Giles's in the Fields,"* for which he quotes the bills of mortality.

All this shows great uncertainty as to the fomes, but none as to a peculiar constitution of the air, or the bad state of the town.

* Dr. Hebberden remarks, " And there it would, probably, break out again, should we be visited with such a calamity." Can we wonder at the improvidence we see in the poor, when such a consideration, from such authority, is unattended to, and when this dangerous spot is in the neighbourhood of increasing opulence?

The former was distinguished by a calm of several weeks, and the latter may be implied by the absence of that calamity since the improvement of our city. Is there not, therefore, a presumption that the *common* cause, to which Dr. Hebberden refers, must be a similar constitution of the atmosphere, incumbent on towns similarly circumstanced?

Of all that has been told us concerning this dreadful visitation, little satisfactory is to be collected, excepting from Dr. Russell, and his opinion is decidedly that the plague is contagious. Yet we have seen that he does not consider it so under all circumstances. This, as Dr. Rush observes of the yellow fever, is different from what we see in small-pox. It may be urged, that besides the difference of symptoms between the yellow fever and plague, there is a most important difference in the historical part, namely, that the yellow fever is never carried by fugitives into the country, whereas the plague is described as spreading devastation among the villages. But we cannot now ascertain the difference between the present state of America and of our villages, when the plague was said to have spread among them. I have already remarked, the probable situation of the villages in America, which might exempt them from infectious atmosphere, which, it is admitted by most, is often at these times mistaken for pestilential. But Dr. Russell informs us, that the villages in Syria and Cyprus are at this time the proper seat of the plague, "resembling the keisarias within the city, which are inhabited by the lower class of people, in which the contagion spreads with great fury."*

From all this we may collect, that a certain constitution of the air, and a certain state of a town, appear necessary for the introduction of the plague; 2dly, that it is not yet satisfactorily proved, that these are not sufficient to excite the disease without a fomes; and, 3dly, that where the two first causes are present, it is probable

* Of the Plague, page 61.

a fomes, or diseased subject, even if not necessary, may sometimes prove an additional or immediate cause, when the other two would not have been sufficiently powerful; or a fomes may induce the disease in an individual, which individual may become contagious in a certain atmosphere. In this last respect the disease may resemble the fever from infectious atmosphere.

It is hoped, that what has been offered on this important subject, will not be deemed presumptuous in one who has had no opportunity of seeing the disease. It should be remembered, when facts are stated with the attention Dr. Russell has shown, that it is evidently the wish of such a writer to lead others to reason, independently of opinions he may offer. It may be added, that I have availed myself of no arguments which have been brought by those who think differently from Dr. Russell; and lastly, that the practical inference must in many respects be similar, which ever conclusion we may adopt. Both require a strict attention to cleanliness in large towns: in addition to this, both enforce a more than common caution against the introduction of fomes, when, by the calamity of any great commercial city in the neighbourhood, we have reason to apprehend the progress of a pestilential atmosphere.—When the plague is known, or suspected, to be in a city, the practice must be directed according to such facts as are well ascertained, and the inference deducible from them.

If it is found that some of the crew of a vessel are seized with suspicious symptoms as soon as they arrive, and if the voyage has been of any length, before we hastily pronounce that they have imported the plague, let us learn the state of the town, and of the houses, near the landing place, and judge of the probability of men, fresh from a voyage, being affected by an atmosphere, to which the residents were before insensible. Let us carefully enquire whether any inhabitants of the other parts of the town, who have had occasion to resort thither, have suffered in a similar manner; and if so,

whether they have proved contagious to their families. If we find that some have been so affected, without injury to such as had the care of them, our business will not be to shut up individual houses inhabited by individual sick, but to forbid all access to the dangerous district. To quarter all the inhabitants of the nearest airy village on some other healthy village, and require the inhabitants of the pestilential district instantly to relinquish it, and occupy the evacuated village, bringing with them nothing but their sick. As these calamities usually happen in a temperate season, an encampment should be erected, that no house may be over crowded, and a police, formed during times of less alarm, should be entrusted to such members of the district as had the most influence among the inhabitants, before they relinquished their former residence.

A very few days will be sufficient to show whether the disease is contagious in this new situation, and future measures must be conducted accordingly. But by beginning in this manner, we shall avoid leading to certain sacrifice all who repair from healthy parts of the town to the relief of the sick. Such as are seized in consequence of these visits, will not be hastily deserted by their friends, and abandoned to those, whose wretched existence renders them indifferent to life, and who will probably be procured from parts in which their cloaths are likely to become a fomes of plague or infectious atmosphere: lastly, infectious atmosphere will not be produced by accumulating the sick in a pest-house.

If the progress should prove such as is related of the last dreadful calamity, the only resource must be to quit a town, every part of which is pestilential. There is, however, reason to hope that this arose from every part being, at that time, equally crowded and filthy; but even admitting this, the constitution of the air must very much have contributed to the sudden universality of the disease. Still the event may teach us to how little purpose the sick were imprisoned, and a forty day's quarantine imposed on their

inmates. For in the month of July was issued the dreadful ordinance, that a red cross should be affixed to every such house, and an inscription of LORD HAVE MERCY UPON US. Notwithstanding this, and though the people deserted the town, "as if London had quite gone out of itself, and like the hurry of a sudden conflagration, yet," continues Hodges, "in the months of August and September the contagion changed its former slow and languid pace." From the same author we learn, that such was the progress of the disease among the "common people, that it was by some called the poor's plague;" by which it is evident, that, though it appeared in every part of the town, yet the ill ventilated and dirty passages were its principal residence, and such we know were formerly intermixed with the best of our streets. We are apt to suppose, by the near coincidence of the fire, that it was the instrument of staying the plague. But this was not the case. For though, in the month of September, 1665, Maitland's expression is, "Death rode triumphant," and part of October proved nearly as fatal; yet during November and December the town was re-peopled with as much rapidity as it had been deserted; "and many went into the beds, where persons had died, even before they were cold, or cleansed from the stench of the disease." Admitting that this account, by Hodges, may be highly charged, enough of it must be true to show, that no precautions were taken at all adequate to what had been enforced. Yet we find the distemper gradually ceasing, and such is described as the usual progress of the disease in those countries which it still visits. In these respects it seems to bear an exact analogy with the fevers of the American continent.

But neither this, nor many other particulars, in which plague seems to resemble yellow fever, is sufficient to authorize the considering them as the same. Most fevers resemble each other, excepting in certain laws, which form the respective characters of each. All vary, according to climate, season, peculiarity of constitution, and some state of the

air, with which we are at present unacquainted. We must, therefore, discriminate each by certain diagnostics, which are found in the majority of cases. Now, it is universally admitted, that glandular swellings are found in the majority of plague cases, perhaps in all where the patient lives long enough; we are not, therefore, authorized to confound such a disease with one which never induces glandular affections, excepting in the parotids; a symptom by no means confined to these two fevers. It seems also as if there was a difference in the cause. Though both are regulated by a certain temperature, and are confined to ill-managed towns, yet these two causes seem sufficient to induce yellow fever, whereas the addition of a pestilential constitution of the air seems necessary to induce the true plague. The frequent visits of yellow fever in individual towns in America make this probable, and also the annual return of what is called the plague in Constantinople and some other towns, whilst Aleppo, which is described as well watered and clean, excepting in the keisarias and some other parts, enjoys long exemptions.

If what we have said is admitted, it will follow, that the fever from infectious atmosphere is to be ascribed to the confined effluvia from living bodies—the yellow fever to effluvia of putrid animal or vegetable matter, exposed to a hot sun—and the plague to the latter, joined to an epidemic constitution of the air. That in all a fomes may excite the disease, yet that we are not certain whether a diseased subject is contagious, unless in a certain atmosphere. Lastly, If a pestilential atmosphere may sometimes be of itself sufficient to excite fever, it is still probable that its effects would be greatest in crowded cities, as we have seen is the case with influenza.

As these enquiries are now submitted to a board, in all respects competent to so important a subject, what is here written will only

be considered as an auxiliary, which it becomes every good citizen to offer. For the same reason I shall make no apology for a few other suggestions, with which I shall conclude this chapter.

When we reflect on the contrariety of opinions concerning the contagious property of these diseases, and concerning the manner of treating some of them, we cannot but recollect the invaluable observation of Sydenham, on the various modes of treating small-pox. It is impossible to think cooly on the devastation which every campaign, however bloodless, produces in our armies, and many seasons in our colonies, without admitting, that all our present means are greatly insufficient for opposing so dreadful an enemy. Whether such is the unalterable lot of things, I pretend not to determine; but if there is any thing in medicine, let it at least have all those advantages which we freely offer to other sciences. Let us not attempt, in the confines of the arctic circle, or in the bosom of peace and luxury, to teach the exercise of a practical art between the tropics, in fleets, or in armies. Surely our transatlantic possessions might support establishments and professors, who might teach their disciples some certain diagnostic, by which they might vary their treatment, according to the varieties of seasons and constitutions of the air; some rules to distinguish endemics from contagions; spontaneous cures from the result of remedies; and the causes which render the latter so frequently inert* as well as ineffectual. We have surely veteran army and navy physicians, who may practically teach the treatment of diseases in our naval and military hospitals; and though no institutions of this kind may be adequate to the purpose, they will at least establish some uniform treatment under certain well defined symptoms, some well founded prognostic, or such clearly ascertained facts, as may lead to these desirable purposes. The continent of America, by the

* See page 248, second paragraph, and note.

establishment of medical schools, has furnished writers, whose very differences have brought to light many important facts, the utility of which is proved by the greater health of their increasing towns. In the possessions of the East India Company, it has been found necessary that men should begin the study of their profession, when they conceived it was completed at home. Can we doubt that every medical staff, how well soever instructed, would gladly receive the assistance of local practitioners, from whom they might learn what they are to expect from climate, from season, from local aspect, and other particulars, tending to the comfort and restoration of our brave defenders!

Nor ought our long exemption from the plague to lull us into too great a security. The plague of 1665 being the latest, is that to which we are apt to confine our attention; and certainly our documents concerning it are more authentic than of any that occurred at a more remote period. But there is no reason to doubt that the visitation of 1348 was much more dreadful, inasmuch as it destroyed above a third more, when the population of the town must have been less. Nor is there any reason to doubt that this pestilential atmosphere pervaded the whole of the East from China, in which it is computed to have destroyed three millions of inhabitants. It is true, that M. de Guines imputes the progress of the disease from Constantinople, westward, over every part of the European continent, to the distribution of merchandize, Constantinople being at that time the grand emporium. But the same author informs us, that in many places the disease began upon women and children; in others upon cattle.

We have consuls, factories, and physicians at most, if not all, the pestilential cities. How desirable it would be, if the only omission of Dr. Russell should be filled up by these gentlemen, viz. Whether the disease is endemic only in certain parts of the town; and whether a person, who has contracted the disease in these parts, is

found to be contagious in places where it is only sporadic? In this part of the enquiry we should govern our reasoning by what we see of the intermittent fever. Though this is now, for the most part, confined to marshes, yet it has been occasionally sporadic in the town: that is, persons have shown the disease, without having visited those spots in which it is endemic: but they have never proved contagious. May not the same occur to individual females in convents, and in private houses, who have been seized during a pestilential season, but who have not communicated the disease to their attendants? These, and every other fact which may be ascertained, it certainly becomes us to learn whilst the enemy is at a distance. To form plans of escape or resistance is of little avail, unless we learn his mode of attack. The invaluable assistance Dr. Russell has afforded, shows us how much may be done, and we are all convinced how much ought to de done. But it is time we should turn our attention to a subject, which being always before us, we can investigate without foreign assistance.

CHAPTER III.

OF ACUTE CONTAGIONS, WITH OUTLINES OF THE SYMPTOMS AND MODE OF TREATING ACUTE DISEASES IN GENERAL.

WHATEVER doubts we might entertain concerning the contagious properties of the diseases enumerated in the last Chapter, there can be none concerning some which we are now to consider: and this general uniformity in opinion as to the latter, and the qualifications under which the former are supposed to be contagious, are at least sufficient to justify the division we have made.

It has been remarked by Fracastorius and some others, that the ancients were unacquainted with contagious diseases. This is not strictly true in the sense to which that elegant writer would extend it. Though few but a poet would pay much attention to his story of the infectious coat, yet that the intercourse with the sick was dangerous, seems to have been so well known to the philosophic historians of the classical age, that probably the physicians thought the remark unnecessary. I have already shown one reason why the infectious atmosphere was in common life less attended to.* In military expeditions it was of more consequence, and it is worth remarking, that in proportion as a nation became more martial, all the means of avoiding the causes of such diseases became a part of their discipline. Who can possibly forget the lively description of a Hebrew camp, given by an enemy, during the military period of that once warlike nation,† or of Leonidas' troops, when waiting the

* See Chapter I.

† Numbers xxiv. 5. How goodly are thy tents, O Jacob, and thy tabernacles, O Israel! As the vallies are they spread forth, as gardens by the river side, as the trees of the ling-alloes which the Lord hath planted, and as cedar trees beside the water.

assault from the largest army the world had ever seen—" some exercising themselves, and others combing their hair."* Among the Roman historians, " *corpus curare*," is an expression which perpetually occurs in describing the progress of armies. Add to this, their cloathing was more simple, and the want of linen rendered frequent bathing an indispensible custom. When the severity of the service obliged them to dispense with these habits, we have seen that they suffered like modern armies. In the passage already quoted from Livy, we find an accurate account of the progress of an endemic in producing fever from infectious atmosphere.† Thucydides was not less aware of disease communicated by the sick, when he remarks, that the physicians died most as they visited most.‡ The ancient physicians being well acquainted with endemics and epidemics, could not be ignorant of the infectious atmosphere. But if Sydenham confounded the contagions with them, can we be surprised if they were not well characterised before his time?

Whatever doubts we may entertain concerning the ignorance of profane writers, whoever peruses the Levitical law, and the early part of the Jewish history, will find proof that the divine Legislator made ample provision against the danger of contagion. This not only appears by many laws against any communication with those afflicted with diseases deemed unclean, but also by the extreme caution with which all intercourse was prevented with other nations. Throughout the whole of their march they avoided some by circuitous routes, and all whom it was thought necessary to attack,

* Herodot.

† Lib. xxv. Cap. 26. Et primo, temporis ac loci vitio, & ægri erant & moriebantur; postea curatio ipsa et contactus ægrorum vulgabat morbos.

‡ The old scholiasts, (vid. Stephani. edit. Frankofurti, 1594, p. 129) on this passage observe, that those physicians, who were aware of the pestilence, did not undertake the cure διὰ μιλαδοῖικον. Whether this is the just interpretation of the passage, I pretend not to determine, nor whether there is any other authority for the use of μιλαδοῖικον in this sense, excepting in a writer of so late a date as Aretæus.

they were enjoined to exterminate. Some of the directions, on this occasion, are so pointed, as to leave no doubt that the object was to avoid every possibility of contagion.* This and many other circumstances, would confirm the general opinion, that our contagions are derived from Egypt and the neighbouring kingdoms. If it is really true that they originate with other animals, the manner in which the Arabians are domesticated with their cattle, may account for some of them.

If I leave this part of the subject thus imperfect, it is for want of sufficient information, and not from any wish to undervalue its importance. Whenever these facts are satisfactorily ascertained, they will lead to others, which will assist our enquiries in prevention—the most important part of medicine. As we have physicians now in all parts of the world, I trust from some we shall derive accurate information on a question, which will admit of experiments. But before we quit the subject of the probable antiquity of contagions, it is at least justifiable to remark, that we can collect no accounts from the early writers of any disease which would attack a person only once during life. Thucidydes observes of the plague which he witnessed, that such as recovered, were not afterwards fatally affected by it: and that this appeared so surprizing to some of the convalescents, that they flattered themselves with an exemption from all disease for ever after: a presumptive proof that they had no conception of those exanthemata, whose laws, in this respect, could not be overlooked. I pretend not to say that this is an universal law with all the contagions which affect the whole constitution; but as they produce their effect in every known state of the atmosphere, and seem to be more certain in proportion as the element is in other respects pure; if this provision had not been made against some that we are acquainted with, life would scarcely be worth ac-

* See Numbers, chap. xxxi.

ceptance on so precarious a tenor, and subject to such constant anxiety.

I shall divide acute contagious diseases into two classes. The first comprehends the exanthemata, the second all that cannot be included in that division. Of the first I have selected small-pox, as the most striking, as one which I have had the largest opportunities of investigating, and as containing, besides some peculiarities of its own, every thing formidable which is to be met with in the rest.

In enquiring into the laws of this disease, we meet with nothing before Sydenham that deserves our notice, nor any thing since, in a practical view, till the introduction of inoculation. Mr. Hunter has assisted us in the physiological part. I shall, therefore, confine myself to the observations of these two authors, and add a few remarks of my own, in order to illustrate and confirm what has been compleated by them.

Sydenham's description of symptoms, and even his treatment, are so full and so correct, as to leave nothing to add. As his pages must live for ever, it might seem unnecessary to say any thing of a disease described by such a master: but unfortunately a theory he has interwoven, obscures his description, and often contradicts his practice. I shall, therefore, offer an epitome from him, to show the manner in which his practical rules are confirmed by subsequent dissections, whilst his own advocates drew contrary inferences from his mistaken theory.

After describing the previous symptoms of the *distinct* small-pox, according to the age of the patient, he shows, that in proportion as such symptoms are mild, and the eruption protracted, we may expect the subsequent disease to be favourable—that these symptoms subside as the eruption appears. " About the eighth day," continues he, " from the beginning of the disease, which is a time I always take particular notice of, the space between the pus-

tules, that appeared before of a pale white, begin to grow red, and swell in proportion to the number of pustules, with a throbbing of these parts, the increase of which occasions the swelling of the whole countenance."—" The pustules, which were before smooth and red, now grow whitish and rough, which is a sign of beginning suppuration. Meanwhile the eruptions in the other parts of the body are approaching to suppuration; but in their progress they grow smoother and whiter, whilst those on the face become yellower and rougher."

On the eleventh day, all the symptoms abate: by the fourteenth, the scabs fall off, succeeded in the face by branny scales, which sometimes leave pits.

The confluent small-pox is described as beginning with similar symptoms, only more violent, in consequence of which the eruption is earlier by a day at least. The eruptive fever is often attended with violent local pains in the head and various parts of the trunk, sometimes affecting peculiar organs, so as to interrupt their functions. The eruption does not, as in the distinct kind, immediately relieve the fever, which remains for some days. " The pustules, in their progress, do not rise so high as in the distinct kind, but running together, appear at first like a red bladder, covering the whole face, and making it swell sooner than the distinct sort; and at last they appear like a thin white pellicle, closely adhering to the face, and rising little higher than the surface of the skin.

" After the eighth day, this pellicle grows every day gradually rougher, as appears by the touch, and inclines to a brown, and not to a yellow colour, as in the distinct kind. The roughness and colour of the skin daily increase, till at length the pellicle falls off in large scales: but when the disease has been very severe, it usually sticks to some parts of the face till after the twentieth day. The more violent the small-pox proves, the nearer the eruptions, as they ripen, incline to a dark brown colour, and the longer they are in

falling off, if left to themselves; whereas contrariwise the less they run together, the yellower they grow, and the sooner they scale off. When this pellicle, or scab, which covers the face, first falls off, it leaves no roughness behind, but it is immediately succeeded by branny scales, of a very corrosive nature, which not only make larger pits than the distinct kind generally do, but also much disfigure the face with unseemly scars. And in the confluent kind, if the disease has been very violent, the skin of the shoulders and back sometimes scales off, leaving these parts bare."

Sydenham next proceeds to describe the pustules in the hands and feet as largest, afterwards notices the salivation, and then points out the prognosis. This last I shall select, to show how exactly (when divested of his unfortunate theory) it accords with a discovery of Mr. Hunter's, and with dissections which have been observed by others. In doing this, I shall be obliged to explain still further some of Mr. Hunter's discoveries in the process of inflammation.

It has been long known, that all inflammation is attended, not only with increased action, but with an increased number of blood vessels, or of vessels which, though before not pervious to blood, now become so. Inflammation, though the effect of an unnatural stimulus, may still be healthy, that is, it may be conducted in such a manner, as to preserve the life of an animal, and, if possible, the functions of the part inflamed. For this purpose its progress must vary, according to the nature and offices of the part. If the inflammation is immediately under the integuments in the adipose cellular membrane, the first healthy provision is to unite the cells firmly, in order to prevent the extension of the inflammation. If any part has suffered so much violence, that it cannot return to its original state, a new action is set up, namely, the formation of pus. As this is formed, the absorption of the parts in contact follows, to allow a cavity for containing the pus, and the previous adhesions prevent

its diffusion. If the pus increases, the cavity must be increased, and new adhesions formed, so as to make a proper capsule for the accumulated pus. But if the inflammation is still more violent, or if the powers of life, from any cause, are unequal to the support of that increased action which has been excited, the animal must die in attempting to maintain an action, to which his powers are unequal, or the part itself must die, that the attempt may cease, and the animal may be preserved. In this case the blood coagulates in the vessels themselves, and mortification follows. After this a new process is set up, namely, the separation of the dead part, which is accomplished in the manner before described.* We have before seen, that the peculiar property of some morbid poisons is to produce death in a part, whether the inflammation be violent or not. Of this kind is the small-pox, every individual pustule of which is found with a slough at the bottom, which may be removed with ease, after time has been allowed for its separation by suppuration.

The progress of the small-pox is, therefore, to form a number of sloughs under the skin, and the danger depends on the number formed, and the violence which the constitution suffers from the first shock of that stimulus which excites it into this process.

Every action, either of the body or mind, affects the constitution the more in proportion to its suddenness; and this suddenness will depend on the novelty of the shock, and the susceptibility of the subject. Every inflammation will be completed with a rapidity proportioned to its commencement; for though, by various means, we may be enabled to retard it, yet those means can only be efficacious in part, and must be proportioned to the actions which they are intended to oppose.

Let us apply all this to the prognosis and practical remarks of our two authors. If the stimulus of the small-pox virus is moderate,

* See pages 54 and 55.

its local action follows by adhesive inflammation and slough: after which the parts and constitution have sustained the first shock, and the subsequent process of suppuration, to separate the slough, is accomplished with so much ease, that the constitution is rarely sensible of any general inconvenience. But the face, having sustained the first shock, the actions on that part began with the greatest rapidity, and continue so throughout their whole progress: in consequence of which the pus has a higher tinge, and the progress of skinning beginning as soon as the slough begins to separate this irregularity, produces an inequality in the surface of the pustule. On the contrary, the actions being slower in other parts, the pustules acquire the property of common sloughs, and granulation follows suppuration for the restoration of the lost part. "Hence," as our author observes, "the surface of the pustules on the face are yellower and rougher, and, as they cast off, sometimes leave pits, whilst those on the other parts are whiter and smoother," [without leaving pits.]

But notwithstanding all these favourable appearances, the constitution may be brought into such violent action during the progress of the eruptive fever, that mischief may have taken place by inflammation in an important organ. This can only be suspected by the violence of pain in the region of such organ, which pain must be the effect of inflammation whilst the constitution was engaged in such a process. The effect of inflammation will depend on the nature of the part, and the state of the constitution. If the pain is very violent about the abdomen, we may expect exhesions of the peritonium with the intestines, and with each other; if in the head, effusion of lymph; if in the liver, or other solid viscera, a suspension, for a time, of their proper functions. If the mischief extends no further, though convalescence will be retarded, yet it will gradually follow. But if the inflammation has extended so far as to give the disposition for suppuration or mortification, a fallacious

pause will ensue, during which the physician, if he has not witnessed the early progress of the disease, may be deceived, and on the eighth day, when suppuration begins on the surface, the same action may commence in an important organ, and the patient be lost with a suddenness, which nothing but dissection can explain. Happily these cases are very rare.

In the confluent small-pox, the actions about the face, where the shock is first felt, are so violent, that before the necessary adhesions can be completed, sloughing takes place, and extends, as might be expected, without any regularity as to depth or extent of surface. The same rapidity continuing, the separation of the slough is earlier, absorption rapidly following, with little or no suppuration: " the pustules in the face do not rise so high [therefore], but running together, appear like a red bladder, making the face to swell sooner than in the distinct sort; and at last they appear like a thin white pellicle, closely adhering to the face, and rising little higher than the surface of the skin." For the same reason the " pellicle begins drying on the eighth day, instead of the eleventh."

During this whole process the first fever continues, though somewhat abated, till the eighth day, when a fallacious pause ensues till the eleventh, at which time the process of skinning produces a new action in the constitution, and in the commencement of this new conflict the patient often dies.

In this description it may be remarked, that we have attended to no part of the surface, excepting the pustules of the face. These, as our author observes, are alone of importance, inasmuch as on them depends our prognosis. However sudden or violent the first shock may have proved, it has prepared the constitution for the stimulus. The eruptions, therefore, which follow on the body and extremities, assume a regular progress of adhesions: and slough, suppuration, and granulation are so gradual, as to affect the constitution no otherwise than by their local irksomeness.

Sydenham remarks, that the dangerous day, in the distinct, is the eighth; in the confluent the eleventh. This has led into a common error, that the progress is slower in the confluent than in the distinct; and this error has been confirmed by the scaling being later in the confluent than in the distinct. But whoever carefully attends to Sydenham, or to the progress of the disease, will find, that all the actions are quicker in the confluent. When the distinct proves fatal, it is from the causes and at the time before-mentioned. The confluent is sometimes fatal from the same causes, and then the catastrophe occurs earlier. But without any organic mischief, the eleventh day is always dangerous in the confluent. So rapid are all the actions, that the eruptive fever does not cease, before the inflammation, which ends in slough, has produced a new constitutional action, or, as Sydenham expresses it, "the fever does not cease, as in the distinct, on the appearance of the eruption." It continues, without interruption, till the slough is formed, and suppuration commenced for its separation. Violent inflammation then ceases, and the constitution recovers its tranquillity. But when action has commenced with such force, we are not to expect the same gradual progress, which we remarked in the distinct small-pox. On the contrary, as soon as the slough is separated, which will be with a rapidity proportioned to its formation, a new action commences, namely, skinning. Hence a new fever in a subject already exhausted.

Thus we see, what is usually called the turn in the distinct small-pox, is only the commencement of suppuration to detach the sloughs; a process which, in the confluent, so rapidly follows the first inflammation, as to be confounded with it. The skinning, which scarcely excites alarm in the distinct, is the dangerous period, or what is called the turn in the confluent. As all the actions have been violent, the lymph effused in the confluent is much firmer or more adhesive: it, therefore, remains longer on the surface, and is

even attached to the new cuticle, interrupting its regular formation. Hence, whilst the softer scabs, formed by the pus in the distinct, readily fall off, the lymph adheres firmly in the confluent, and the cuticle shoots in every direction in which it can find room; so that on the face scales are repeatedly formed, and the neck may sometimes be pealed more than once, without pain to the patient.

Hitherto we have only attended to the disease in the skin, slightly remarking those effects which the first fever may produce in important organs. There are, however, other symptoms attending the progress, which are traced with equal accuracy by Sydenham. Before or with the eruption sometimes appear an universal efflorescence, threatening an erysipelatous eruption, or measles. This has been already remarked as no uncommon attendant on other morbid poisons, when they affect the skin. The inflammation also, attending the first eruption, will extend to the fauces, and produce an increase and alteration of their natural secretion. Besides the delirium, violent pains, and interruption to the due action of the organs, there are sometimes other and fatal appearances, which occur during every epidemic small-pox; but these, as they do not entirely depend on the events of the disease, we shall enumerate among the anomalous symptoms, which we are now about to consider.

It is to be observed, that all the symptoms above enumerated, dreadful as many, and loathsome as all of them are, must be considered as the necessary progress by which the constitution passes through certain actions, excited by the application of small-pox virus. All the danger in these cases depends on the progress, which, when the constitution, from its own susceptibility, or the mode of application, is highly affected, will be proportionally rapid. But if, from any previous disease, from habitual insufficient diet, or from constantly breathing an infectious atmosphere, the patient should be in so low a state of health, that the constitution feels unequal to all the actions above described, the only chance for the patient is,

that the eruption may be inconsiderable; for if high action is excited, it is evident that such action not only cannot be supported, but must be instantly succeeded by proportional weakness, and, instead of the effusion of coagulated lymph, to form adhesions, we shall find little more than thin serum, sometimes mixed with red particles of blood. Should high inflammation be attempted mortification must follow; not with the regularity we have described in the former instance as the law of the disease, varying only according to the degree of inflammation, but extending with or almost superseding the necessary actions excited by the poison. This was soon practically discovered by the early inoculators, so that, though they were, for the most part, indiscriminate in their inoculations, and made no objection to a state of convalescence after acute diseases, yet experience convinced them, that during or after an intermitting fever, was a dangerous period even for introducing the disease in that form.*

Sydenham gives a description, worthy of himself, of this form of the disease, when it alternated for three seasons with dysentery, the bilious colic, intermittent and continued fevers, and when probably it often attacked subjects much reduced by one or more of them.—The season, and many other circumstances, evidently point out that his continued fever was the winter fever of infectious atmosphere. Such subjects as were attacked with small-pox after these complaints, were very likely to fall into the symptoms above-mentioned, but more accurately detailed by himself.† It is not less certain, that there are periods, or, as he more properly denominates them, constitutions of the air, in which the human constitution is more apt to fall into certain modes of action, the causes of which are unknown, but the effects very sensibly perceived, even by persons in apparent health. These constitutions of the air

* Sir George Baker's Enquiry, page 8.

† Anomalous or irregular Small-pox of the Years 1670, 1671, 1672.

are found to influence the exanthemata in common with other diseases.

Thus have we gone through, in an imperfect manner, a history, which Mead tells us was first reduced to order by Sydenham, which Boerhaave says every medical student should read over thirteen times, and which every succeeding writer has copied with an exactness proportionate to the accuracy with which he has described the disease. But most unfortunately, Sydenham thought it necessary to add a theory, which, though he accommodated it to his practice, seemed directly contradictory to it. And, as we have before remarked, how much more suitable a weak theory is to the indolence of the human mind than an accurate demonstration, so we may the less wonder if all the valuable, but to common minds tedious, descriptions of Sydenham were forgotten, whilst his mistaken opinions were readily retained. That sagacious observer, after tracing the various phænomena attending the different stages of the disease, divided them into the concoction, the separation, and the expulsion of the morbific particles, which last he determined must be by the skin. Aware, however, of the conflict of the constitution (which he calls "ebullition") under these processes, he soon discovered that the slower they took place, the greater was the probability of the patient's escape. He therefore advises, in the genuine confluent small-pox, bleeding and other evacuations, with a cool temperature. But finding some cases, in which even this plan was less successful, he remarks, that, though it is dangerous by cordials and the hot regimen to "raise the ebullition" too high, so contrariwise the danger is equal in bleeding and other evacuants, "lest the due secretion of the separable particles should be prevented." To one who was constantly alive to every minute event, such language might be admissible, as it points out the necessity, that every practitioner should judge of symptoms as they occur; and that even with every advantage of treatment, a degree of uncer-

tainty would attend all confluent cases. But this mode of requiring the practitioner to use a discretionary power is less popular, because few of us have sufficient confidence in ourselves, and unfortunately Sydenham found " not only the vulgar are apt to attribute the patient's death to the over officiousness of the physcian, but even the professors of the same art gladly take occasion to defame their brother, and haranguing before partial judges,"—Though happily the liberality of the age has relieved us from such grossness of behaviour, yet if we consider the interesting spectacle before us, it is scarcely possible for a medical man entirely to divest himself of sentiments, which would induce him gladly to escape the responsibility of " regulating this ebullition."

Accordingly, as it was admitted by our author, that the expulsion is by the skin, the popular doctrine soon became current, that it was *improper to move the humours from the circumference to the centre, as nature seems to affect the contrary in this disease.* Morton, after an introduction of Sydenham's name with *Clariss. Doctissimusque,* laments his fatal mistake in this instance.* As long as the practice, founded on this notion, was only supported by *reasoning,* which was all that Morton offered, there was less danger lest it should become universal. But when Mead assured his readers, that *he himself* had " *frequently* seen subjects, in which the lungs, brain, liver, and intestines were thick beset with pustules;" and that " the reason why the confluent small-pox is more dangerous than the distinct is, because the matter, though more abundant in the former, is not pushed out in a proper manner."—After this, it was in vain to think of any thing but diverting the disease as much as possible to the surface. All Mead's urgent prescriptions, therefore, in favour of bleeding and other evacuations, were soon forgotten, and his charge of confining the patient to bed, during the first days of the dis-

* De Variolarum Apparatu, Vol. II. Page 87.

temper, was enforced with tenfold severity and cruelty. Such was the practice to our own times, and in the memory of some of us. Such it had continued for more than a century, and even impeded the invaluable discovery of inoculation, till the Suttons, with a becoming boldness, united the advantages of that operation with the practice of Sydenham.

Having thus far followed our English Hippocrates, as he is often called in this disease, and shown, that, however unsubstantial his theory might be, yet that his practice was founded on observation, and is confirmed by appearances on dissection, I shall proceed to some general remarks on the treatment of all fevers, as far as my observation will assist me, supported by men, to agree with whom gives me a confidence I might otherwise have wanted.

To the medical reader it is unnecessary to remark, that there is, for the most part, something similar in the beginning of most fevers. Some of them are, however, soon distinguished by particular symptoms. In the small-pox, the sore throat, if the progress is violent, generally is an early symptom; in the measles, inflamed eyes and scarlatina soon discovers its true character on the skin. Though all these are subject to varieties, yet by accurate attention we may soon discover the path we are to pursue. If the eyes are red, the pain of the head intense, and this pain extends to within the cervical vertebræ and spine—if the pain in the abdomen is violent, particularly with tenderness, and without the well-known symptoms of griping, the free use of the lancet is absolutely demanded, and, to use the language of Dr. Jackson, not measured by ounces, but by the effect on the patient. For this purpose he should be placed horizontally during the whole operation, that we may not be deceived by that temporary faintness which often attends the erect posture. If after this, and a free evacuation of the bowels, the urgent symptoms should be relieved, we may rest; but if

the threatening symptoms increase, we must repeat the operation before their full action has continued long enough to do lasting mischief. In internal inflammations, it is hardly necessary to remark the uncertainty of the pulse. If an important organ is inflamed, the powers of the whole system seem to suffer; and it is well known that the pulse rises as the vessels are emptied.

After the more violent symptoms have subsided altogether, we must not be less attentive in watching the first appearance of their return, which we must meet with the same courage. If such remedies have been omitted too long, they are still the only means we can propose against such symptoms. If remissions should be well marked, the judicious use of the bark may prevent a return; but if the febrile paroxysm ends with partial perspiration, confined to the upper part of the body, the case will be protracted to a tedious length, even if the patient recovers at last.

If, after these active measures, only coma, oppression, or anxiety remain, without pain, blisters are often sufficient to relieve them, or the warm bath usually produces instant relief, with the most grateful sensations. Whenever the animal heat is excited above its standard, the affusion of cold water is not less agreeable. In the more advanced stage, nothing is more refreshing than the evaporation produced by the sprinkling of cold water on the patient. Dr. Jackson found further advantage from gestation in the open air, by which the atmosphere was constantly changed, and the patient proportionally refreshed. At this period, if the disease has been violent in the beginning, or long protracted, it is unnecessary to add, that our patient must be supported with the same attention as we conducted our early treatment; but if any important organ has materially suffered, all our hopes of restoration will be disappointed. The patient will die exhausted indeed, but his weakness may perhaps be the effect of mischief, which might have been prevented.

An ingenious and equally candid writer has given us an account of the manner in which, in this country, he treated fever from infectious atmosphere, invading troops in previous high health.* The author, though sensible of the origin of the disease, chuses to call it, from its symptoms, a " remittent fever of this climate," and shows, that, instead of the common means proposed for typhus, bleeding was the only certain remedy; in some instances to the amount of sixty, in one to eighty ounces, and in most to thirty at a single venesection. By this bold but judicious practice, " in five cases in six, the patient became convalescent within the first week of the disease, with strength rarely impaired, and appetite greater than it was thought prudent to indulge."

These were cases which, experience had shown, demanded a more than ordinary promptitude in lessening the quantity of blood. Others will require a treatment very opposite, and the whole business of the practitioner will be to support his patient. But where local symptoms are urgent in subjects not previously reduced, it will rarely happen that bleeding will prove injurious in the early stage of acute diseases. In this manner fevers were arrested in their beginning by Sydenham, who considered the symptoms more than the names of diseases; who carefully watched the general standard of health as he found it affected by seasons, from causes he pretended not to account for, but the effects of which he saw.— In this manner Russell found it prudent to meet the early attacks of plague. In this manner Dr. Jackson gave an example, which has been followed by Dr. Sutton in England—by Dr. Pincard and others in the West Indies. In this manner Dr. Rush has gradually acquired more and more of the confidence of the faculty and the rest of his fellow citizens. In this manner the cautious Pringle, the modest Cleghorn, conducted their practice. Even intermittent

* See Dr. Sutton's Practical Account, &c. 8vo. 1806, Robinsons.

fevers, if attended with violent symptoms in the beginning, were found to require the free use of the lancet: dysentery was arrested in the same manner; and the only regret expressed concerning continued fevers was, when the remedy was not used with sufficient boldness in the beginning. In this manner fevers were once treated, perhaps too indiscriminately; for it must be admitted, that, excepting in a few instances, our forefathers were more anxious than their successors for the sanction of authority. As a general disposition to inflammation often first shows itself in the more sanguiferous parts, and this inconvenience is principally felt in the throat, bleeding in inflamed throat was at one time as general as in pleurisy. At length the loss of some individuals in a noble family, under scarlatina, alarmed the public, and ulcerated sore throat became synonymous with putrid sore throat. To this, by an easy transfer, was added the term putrid fever, and afterwards TYPHUS was re-echoed from the professor's chair to the nursery. The slow nervous fever, as we have observed, was now confounded with the fever from infectious atmosphere, and as it was found that the latter was produced among the crowded poor, and that such subjects would ill bear the lancet, the objection to its use was more than ever enforced: as if a cause were no way varied in its effect, whether applied to the vigorous and perhaps plethoric subject, or to the enfeebled, dispirited, and ill-fed.

It is not to be questioned, that fever from infectious atmosphere is usually attended with symptoms of debility, or that, when such fevers are most common, there is often a constitution of the air, such as we have before mentioned, in which the human frame seems in a state of reduced strength. This, though by no means universal, should be always attended to, as well as the patient's previous mode of life. When fever occurs among the poor, its form is for the most part mild, excepting in those periods of distress which render them indifferent to life, and inattentive to all the

means of preserving it. During the dreary period of a precarious existence by soup shops and voluntary contributions, Dr. Willan might, with much propriety, caution his readers, " that a wakeful and active delirium, a quick and agitated pulse, with every symptom of violent irritation," should not be mistaken for coma or phrenitis—that " drawing much blood, even with cupping glasses," is dangerous—and that " whoever is bled largely from the arm, is precipitated into certain death."* Nothing can be more just than these observations; and if this truly sagacious physician has not been equally decided in teaching his readers the cases in which the patient will be suffered to die if the lancet is spared, yet he refers us to an authority, which contains almost every thing that can be said on the subject.†

* Diseases of London, page 284.

† " Having formerly (says Dr. W. p. 57.) noticed the impropriety of applying the same mode of practice to every case of a disease, and of prescribing from its name instead of considering the state of the constitution affected with it, I cannot perhaps better conclude the present article, than by referring to the general directions with regard to blood letting, dictated with equal simplicity and precision by the elegant physician of the Augustian age.

" Celsus, Lib. ii. Cap. 10. De Sanguinis Detractione per Venas." I shall transcribe a few lines which are most to our purpose.

" Ergo vehemens febris, ubi rubet corpus, plenæque venæ tument, sanguinis detractionem requirit: item viscerum morbi, nervorumque resolutio, & rigor, & distentio: quicquid denique fauces difficultate spiritus strangulat; quicquid subito supprimit vocem; *quisquis intolerabilis dolor est;* & quacumque est de causa ruptum aliquid intus atque collisum est: item malus corporis habitus; *omnesque acuti morbi, qui modo, ut supra dixi, non infirmitate, sed onere nocent.*

" Fieri tamen potest, ut morbus quidem id desideret, corpus autem vix pati posse videatur: sed, si nullum tamen appareat aliud auxilium, periturusque sit, qui laborat, nisi temeraria quoque via fuerit adjutus, in hoc statu boni medici est ostendere, quam nulla spes sine sanguinis detractione sit; faterique, quantus in hac ipsa re metus sit: & tum demum, si exigatur, sanguinem mittere. De quo dubitare in ejusmodi re non oportet. satius est enim anceps auxilium experiri, quam nullum."

The first sentence is subject to some restriction in this country: the red colour of the skin, and apparent fulness of the superficial vessels, without any marks of local mischief, do not indicate the necessity of bleeding, unless the epidemic of the season has been found sud-

In the above, we have traced the symptoms and mode of treatment in the two extremes of strong action and great debility. Happily the greater part of fevers, as we see them in common life, follow a course which produces a favourable, though sometimes a tedious issue; and to this we may impute the steadiness with which each party pursues its own practice. But, besides these three forms of disease, there is a fourth, in which, from the state of the body when the cause is applied, or from the concentration of the cause itself, all the actions of life seem arrested, and without the common efforts for relieving itself, dissolution seems approaching. In these cases, it will be in vain to depend solely on lowering the system by bleeding, or on supporting it by cordials. The whole series of actions must be altered, or the patient irrecoverably lost. Mercury has been proposed as a mode of cure, by exciting new actions. It is admitted by its advocates, that when it fails to produce its customary irritation, it proves inefficacious against the disease. This induced Dr. Jackson to suspect, that in the apparently successful cases, the remedy produced its effect as the disease subsided, and not that the fever subsided from the irritation excited by the remedy. If mercury be indiscriminately used in all cases, such an inference seems reasonable; for we must admit, that the fatality or success will be various under every mode of practice. But where we find symptoms threatening consequences which must otherwise end fatally—where dissections have taught us that these symptoms augur, not as in less urgent cases, mere effusion, or even suppuration in the brain—not mere inflammations in the intestines, but congestions which threaten apoplexy and impeded circulation, which the arterial system can scarcely overcome, and gangrenes, of which we have no warning by violent pains—in such cases the vigorous

denly to fall on some important organ. The last sentence of this question is in every mouth, on too many occasions, and men are too apt to forget that Celsus only proposes *remedium anceps* in cases which must otherwise prove fatal.

practice of Dr. Jackson seems loudly indicated. Copious bleeding, emetics, and cathartics, warm bathing, alternated with cold effusions, blistering, clysters, in short, every means which may instantly, and by any means arrest a progress which must otherwise end fatally.

Without entering into the controversy concerning the claim to reviving the practice of cold affusion, it is much to be wished we could ascertain its effects, so as to direct us in its practical uses. These are differently estimated by the two writers, whose priority is disputed. Dr. Currie considers its principal, if not its only use, as a means of lessening the heat excited. Dr. Jackson conceives, by exciting new actions, it supersedes the former ones; and as both observe that it cuts short the progress of the fever, the opinion of the latter physician stands well supported. At all events the practice is important; but in those urgent cases, in which Dr. Jackson used it most, his attendant remedies seem indispensible.

The conclusion of the whole is, that many fevers will cure themselves, in spite of every mode of treatment. That threatening symptoms must be treated as they indicate, according to the discretion of the practitioner, and that there is reason to hope, in some instances, the whole progress of fever may be superseded.

All these remarks, excepting the last, are applicable to the exanthemata. It seems an universal law, that all morbid poisons will produce certain actions before the irritation excited by them ceases, whether spontaneously or by remedies. In small-pox these are so serious, that we can promise safety at no period of the disease; and if the worst symptoms appear in a very early stage, the case must, till we discover new laws in medicine, prove fatal: for even supposing all the internal organs to escape, the bare injury done to the skin may be sufficient to produce death, as we find the case with extensive burns or scalds.

All fevers, if violent, and attacking persons in high health, sometimes produce mortification in the intestines, and, if long protracted, mortification will sometimes occur in superficial parts. In some instances, from the violence of the shock, or from the low state of the patient's health, or from a peculiarity of constitution, or even from a constitution of the air, which we cannot explain, mortification will take place, or the powers by which life is maintained will sink so early, that the patient shall be irrecoverably lost before we are sufficiently aware of his danger.

In these cases, the secretion of the mouth and fauces becomes blacker and more viscid, in proportion as the disease is protracted, and until some progress is made in the restoration of the healthy action of these parts, our prognosis must be unfavourable. The patient is now said to have a putrid fever, or fever with putrid symptoms. But all the fevers we have enumerated may assume these forms. Thus we have the putrid small-pox, the putrid measles, the putrid as well as the ulcerated sore throat, and the scarlet fever. The same epidemic has also its putrid and inflammatory character, and the fever from infectious atmosphere will be influenced by similar causes.

In the treatment of diseases, we are therefore to attend to symptoms. To prevent them, we must enquire after their origin, become acquainted with all their laws, and distinguish each by a name which can never be misapplied.

Hitherto we have confined ourselves to the exanthematous contagions. In these, as an altered state of certain parts is obvious to our senses, we may be less surprized at those concealed fomites, by which the diseases are sometimes excited. But the hooping cough is attended with difficulties in the mode in which it subsides and returns, that I pretend not to account for. In many respects it seems to partake of the endemics, aided by a peculiar constitution of the air, and the advantages derived from change of

situation, would seem to strengthen such a suspicion: but I have not satisfied myself on the manner either in which this disease arises, or seems communicated. It is, however, an agreeable reflection, that we have at last arrived at a rational mode of treating it according to symptoms, instead of the empirical mode of considering only its name. The infant frame is no longer teazed with a perpetual repetition of emetics, but the inflammatory symptoms of the early stage are properly attended to, and that period being thus shortened, the constitution is more readily restored from the debility induced.

Last of all, let me be indulged in a few words on a disease, which it seems disgraceful that we have not already exterminated. The hydrophobiac poison is neither conveyed by the atmosphere, nor is there any fomes besides the diseased subject. When it appears in the human, it is never communicated, and there is reason to believe it never arises spontaneously in other animals. It has been unknown in most of the smaller islands, and prudent sportsmen have preserved their kennels from the contagion.* In this, surely it would be justifiable to push the *auxilium anceps* of Celsus much further than has hitherto been attempted, and not only the constant fatality of the disease, but the symptoms of a case of inflammation in the stomach, as described by Dr. Innes, would sanction such an attempt.†

* Chirurgical Transactions, Vol. I. Page 294.

† See Edin. Med. Essays, Abr. Vol. II. Page 361. This patient, under all the symptoms of hydrophobia, recovered, after losing a hundred and sixteen ounces of blood.

CHAPTER IV.

OF PREVENTION.

WHEN we consider how formidable these fevers sometimes are, and that the danger is often in proportion to the vigour of the patient, and consequently to the value of life to his friends and the community, it becomes a most important consideration, if possible, to prevent the access of such diseases. There is reason to believe that this may be accomplished in such as affect the constitution more than once during life, and happily the means are those which contribute most to the good order of society, and the well-being of every class.

That the infectious atmosphere is produced from the crowding together of individuals is certain; nor does it seem doubtful that it is only to be communicated by the diseased under similar circumstances. But this law, though it may exempt the wealthy community, will not protect the individual from the danger of fomites, which may obtrude themselves from quarters of which he can never be aware. Let the wealthy of both sexes reflect on the situation of those to whom they are beholden for every article of dress, some of which come fresh from the chambers of the work people to their own persons. Nor is it possible, by means that human ingenuity can suggest, to be prepared against a danger which may meet them in an open street, as they descend from their carriages. Equally impossible will it be to trace the cause of these calamities by their effects. In the mansions of the wealthy, they often cease with the person first attacked, and the symptoms are insufficient to ascertain the cause.

It is a pleasing consideration, that improved sentiments have taught us to look a little further than ourselves, which did not seem to occur to so great a man as Lord Chancellor Bacon, in the passage quoted by Sir J. Pringle. "It were wisdom therefore," says his Lordship, "that in such cases the jail were aired before they [the prisoners] are brought forth." We have now advanced a step further, and found that it is worth while to *prevent* the first production of this air, by rendering the situation of the prisoners as comfortable as can be admitted. Two important considerations still remain. The first is to avoid erecting such edifices in the form of inclosed quadrangles; the next, that no new prisoner should be mixed with the rest, without a previous quarantine. We should reflect, that the man who is fearful of being apprehended, seeks obscurity, and is often consigned to the haunts of poverty. Such are the sources of infectious atmosphere, which we have seen cannot be introduced in any form with impunity into a crowded habitation.

These considerations lead us beyond the confines of the prison, and teach us, that in proportion as the condition of the inferior class is meliorated, in proportion as their relish for the true comforts of life is improved, the wealthy are less exposed to such dangers. In a national view, this is still more important. On those sudden emergencies, when armies or fleets must be instantly equipped, all the sources of infectious atmosphere are ransacked, the new levies are crowded, and in proportion as these are mixed with the old regiments or crews, the strength of the whole is lessened as the numbers are increased.*

It is scarcely necessary to say any thing on the means of preventing endemics. These are well understood, and, excepting in a few parts of this immense town, are well attended to. It is to be hoped that these few places will be improved like the rest, especially

* See Dr. R. Jackson on this subject.

as we cannot well question that epidemics are much influenced by the causes of endemics. Not only have we escaped the plague as our streets are cleaner, but epidemic dysenteries and catarrhs have lost much of their terror. Dr. Hebberden has traced the gradual decline of dysentery. Of the epidemic catarrh in 1732, it is said that not one in six escaped, and that the bills of mortality were quadrupled in one week.* In 1762, an epidemic catarrh occurred, which was severe, compared with that of 1782, yet in the last the weekly bills appear to have been more than doubled. This increase was principally by fever, a proof that it was chiefly confined to the poor, as the practice of the physicians, to whom we are indebted for the report, does not seem to confirm such a fatality.† The influenza of 1803 has been already noticed.

Thus it appears possible not only to prevent the extension of fever from infectious atmosphere, but even that the causes of such atmosphere may be prevented—that the causes of endemics may be prevented—and that in proportion as these two causes are prevented, the effects of epidemics will be lessened. If, then, so much may be done towards the prevention of diseases, which arise from causes produced without even the necessity of a fomes, one might suspect that such as can only be induced by a fomes, or a diseased subject, might be exterminated without trouble. This would appear still more easy, inasmuch as they are found to attack the same person only once during life. But it unfortunately appears, that in proportion as the first mentioned diseases have declined, the contagions have gained ground. For though the improved mode of treating the small-pox, and the improved condition of the inhabitants of the metropolis, must have lessened the fatality of the disease, yet more people are found annually to die of it. Hence it follows, that the number of people affected, even in the casual way, must

* Maitland. † Medical Transactions, Vol. III. Page 54.

have been considerably more increased than the accounts of deaths would mark. The same may be said of measles in a still greater degree, and I suspect of scarlatina more than all. The increase in the small-pox is, by Dr. Hebberden's calculation, as five to four in a thousand, and that of measles of ten to five in a thousand, comparing the first with the last thirty years of the century. If scarlet fever was in the early part confounded with measles, this will make the increase still greater. Scarlatina has, for some years past, been very improperly included in the annual bills with *fevers of all sorts*. Notwithstanding this, the annual number of deaths by fever is lessened one thousand. These two circumstances prove a still further addition to the contagious, and a diminution of fever from infectious atmosphere.

As these subjects are at last beginning to attract the public attention, it is of the utmost importance that such enquiries should be properly directed, lest in the eagerness for accomplishing the ends, we should mistake the means. Our first busines is, therefore, exactly to ascertain the means by which these diseases are continued. And first, we are to reflect, that the contagions not only are excited by fomites, but that every diseased subject becomes a fomes, not merely in a confined air, but perhaps more so as the atmosphere is purer; for it is certain that a contagious subject is much less to be apprehended in a crowded city, than in a scattered village. Thus, whilst infectious atmosphere, town endemics, and epidemics are in their extent, formidable in proportion as the air is less pure, the contagions, on the contrary, seem to increase in activity with the purity of that element. This was most remarkably exemplified in the school at Ackworth. The cleanliness and love of order, peculiar to the managers of that seminary, are well known. Whilst the influenza was prevalent all over the neighbouring country, and pervaded the whole kingdom, the complaint was unknown

among the scholars and officers.* Yet no endeavours they could use were sufficient to prevent the spreading of scarlatina;† nor could the source, from which they derived the contagion, ever be discovered.

It is unnecessary to urge, how often this happens in small-pox and measles, or that an individual exemption at one time is not a security in future. A lady, the daughter of a respectable gentleman, a commissioner of the peace, lived with her parents in the populous eastern part of the town, and was one of the very few who remain in the metropolis, without catching the small-pox, till the marriageable age. After this, during the summer, her husband's residence was in the neighbouring country. At one of the seasons of their retreat, the small-pox was in the village: the lady caught it, and died. Here it is probable that the habit of breathing a purer air rendered her more susceptible of contagion. But the instances of those who have resided for years in the metropolis, without being affected, and afterwards have caught the disease, without being able to say how, are so numerous, as to convince us of the impossibility of fixing the exact bounds of contagion.

We learn from Dr. Willan, that scarlatina is not mentioned by medical writers before Sydenham; and it must be admitted, that the disease, described by that writer, rather resembles a mild measles than the ulcerous throat, or scarlatina anginosa, of Cotton, Fothergill, or Huxham. In the worst cases, blistering was thought a sufficient remedy, and the sore throat is not among the symptoms. Morton, who first mentions the throat, considers the disease the same as the measles. The first accounts we meet with of scarlet fever, in the bills of mortality, are at the close of the seventeenth century; since which it has been as uncertain in its fatality as the measles. But within our own memory it has excited much more alarm with

* Mem. of Med. Society, Vol. VI. Page 350.

† Willan on Cutaneous Diseases.

the public than the small-pox. At present it is too much overlooked. Doubtless it is much more common in young subjects than adults; but it is much more fatal when it occurs among the latter, and has often proved so, after parturition, to mother and child. As diseases, truly contagious, occur only once during life, and as it is found so difficult to avoid them, particularly the small-pox and measles, parents have, in many instances, given up every attempt at escaping them, and suffered their children to pass through them, that they may be relieved from one source of that solicitude, which must always attend guardianship. In the small-pox (the most formidable of all), the seasons so strikingly pointed out a favourable or unfavourable sort, that it is not to be wondered if children were voluntarily exposed to that contagion when it appeared in its mildest form. This was in some degree the custom in our memory, before inoculation had acquired the reputation, which it retained till the discovery of vaccination.

It must seem very remarkable, that in proportion as we are secured from the small-pox, our anxiety concerning its existence should be increased. Before the introduction of inoculation, it was submitted to as a necessary evil attendant on the metropolis; and from the time that inoculation became an established practice, it was apprehended by some prudent people, that the country would be deserted as the terror of that disease ceased. A very respectable and well informed clergyman used frequently to exclaim, that the country would be ruined by three improvements—inoculation, turnpike roads, and the militia. The first would depopulate the villages; the second would raise the price of every commodity, by the facility of conveyance to London; and the last would destroy the morals of the single, and lessen the independent spirit of the married, who would leave their families on the parish during their absence from home. This gentleman lived to find his apprehensions were greater than the events justified. He probably reasoned, however, from

what he saw in his own village, and fancied he could discover the ends of projects, which were only beginning.

So little idea was entertained of the possibility of exterminating small-pox, that the noble lady, who introduced inoculation, was only apprehensive of its success from the opposition of the faculty to the loss of a lucrative branch of their profession. And so it has certainly proved to the physicians; for there are men in the highest practice in London, who have rarely had occasion to visit a small-pox patient. This was not the case in the year 1751. At that time Miss Joanna Cumberland, in the bloom of youth and beauty, arrived in London, was seized with the small-pox, and died. Inoculation had then made so little progress, that scarcely more than fifty had been inoculated in a year at the hospital, and all these were in the house. But the danger was not confined to the metropolis. The same memoirs inform us, that Mr. Cumberland's father raised two companies, in the year 1745. When these repaired to the siege of Carlisle, Mr. C. adds, the "small-pox made such cruel ravage among our young peasantry, that, though they had been cheaply raised, the distresses of their families brought a considerable and lasting charge on my father."

When Baron Dimsdale had established his inoculation house in the country, Drs. Watkinson, Lettsom, and some other well-intentioned physicians, erected a dispensary for inoculating the poor in the metropolis. The Baron exclaimed against the practice, urging, that it tended to spread the disease. This was certainly true; but the Baron should have enquired, how many persons remained in London, for a given time, without catching the small-pox; and if the increased mortality from the disease was confined to the poor, and arose from inoculation among the rich, a new question should have been agitated: Whether the children of the poor should be exposed, by the practice of the rich, to a danger which their means did not enable them to escape? To dismiss this subject in a few

words, we need only add, that to remain susceptible of small-pox contagion, is to be cut off from many of the advantages of social life, besides existing under continual apprehensions. In the public estimation, it is to be considered as a life less valuable, and requires a proportionally large premium of insurance. The only argument of any weight against permitting small-pox inoculation is, that the number of deaths, by the disease, has increased since the introduction of the practice. Dr. Hebberden remarks, that out of one thousand deaths, the number by small-pox, during the first thirty years of the last century, before inoculation could have any effect upon them, was seventy-four. During an equal number of years, at the end of the century, they amounted to ninety-five. This is an incontestable proof that more have died of small-pox since the introduction of inoculation: and if this greater comparative mortality were confined to that disease, the argument would be incontrovertable. But we shall find the same increase in all other diseases, unconnected with abject poverty. The great mortality of children, it was remarked by Adam Smith, is confined to the poor, who have less time to attend them. Between the years 1728, when the ages were first marked, and the year 1738, the number of deaths, under two years, amounted, one year with another, to 10,000; in the next decade to 9000; in the following to 7800; and between 1790 and 1800, to little more than 6000.* Here is a diminution of no less than 4000 annually. By the same author it appears, that the average diminution of deaths by fevers, from the beginning to the end of the century, is from 3000 to 2000; of dysentery from 1100 to 135, and lastly to 20. The first proves how many more children

* Hebberden's Commentaries, page 33. The judicious author remarks, that part of this decrease may be owing to an act of parliament, by which parish children are directed, three weeks after birth, to be nursed in the country till six years. But should this be the case, the diminution of burials is the same; and consequently the average comparison with small-pox the same.

are reared to the age at which they may be likely to receive the small-pox; the second and third how much fewer die from that accumulation of distress which poverty induces. At the same time as all must die, we find a proportional increase of deaths from those causes, from which, after escaping the dangers of infancy, easier circumstances will not secure us. Of these the contagions should be the most considerable, as their power seems increased by the causes which lessen the force of others. But consumption, as might be expected, is most considerable of all. Constitutions, subject to pulmonary diseases, are preserved in the exact proportion that they are attended to. Among the poor, it is well known, by the dissecting rooms, that it is a common cause of death in infancy. In a better condition, the fabric is preserved with care, till the common casualties of busy life bring the constitutional disposition into action: among the more elevated ranks, extreme caution preserves life still longer; perhaps the disease may never advance to fatality; but in either case, the death of this class rarely swells the London bills of mortality. If, however, death takes place in infancy, the cause is seldom suspected.

The following is Dr. Hebberden's statement of the average of ten years, at the beginning, middle, and conclusion of the last century:

	Beginning.	Middle.	End.
Colic, Flux, Gripes, &c.	1000	135	20
Fever	3000	3000	2000
Consumption	3000	4000	5000
Small-pox	1600	2000	2000

By this it appears, that whilst the increase of deaths by small-pox has been as four to five, that from consumption has been from three to five, without any allowance for the numbers who leave the metropolis to die under the last mentioned disease. As we have the

evidence of Sydenham, that when small-pox became epidemic early in the season, *none* escaped, we cannot doubt that the mortality from this disease would have increased considerably more, from the greater exemption of other causes, if inoculation had not been introduced; and this is confirmed by what we see of the other contagions. In this calculation I have taken no notice of the infinite numbers susceptible of small-pox, who now venture to the metropolis, but who formerly would not have found employment in any family.

It may be thought, that a work like the present, would be very deficient if no notice were taken of a subject, which has lately much attracted the public notice, namely, the EXTERMINATION OF SMALL-POX. To me I confess the subject, however important, seems at present less so than ever, for small-pox may be said to be virtually exterminated. Every friend to vaccination may secure himself and his children. Those whose minds are not completely made up on the subject may inoculate, which, according to our register for the last eighteen months, affords a security in the proportion of 6000 to 2. Those who refuse both, must do it from motives which no laws can controul. But if these motives are religious, they ought to be respected; if they arise from inattention to offspring, or even from imbecility of mind, in either case such children, or such grown people as are unable to take care of themselves, are entitled to public protection.—As I shall not be accused of any wish to lessen the terrors of the disease, my remarks may be confined to what has been said of the facility with which it may be exter minated.

1st. It is said that small-pox has been kept out of various districts for numbers of years, and when introduced, its source has always been discovered.

2dly. Inoculation tends to increase the extension of the disease.

3dly. Since the discovery of vaccination, districts, which were formerly visited by small-pox, have been free from it.

The first argument will not extend to the metropolis; and

The second has been answered as far as relates to the metropolis.

The third only shows, that, since vaccination has been discovered, not only districts, but individuals, may secure themselves whenever they think proper.

" Not only," says a pamphlet now before me, " is small-pox universal in its depredations, but its approach is so insidious, as to render it *almost* impossible to be avoided. It has been ascertained, that the small-pox has been imported more than a hundred times, in seven years, into the Channel, and twenty times, in about six months, in the year 1800, by the Channel fleet alone." If this were not too absurd to believe, it would only prove the absolute impossibility of preventing the importation of such a disease by any quarantine law. It is most likely that fresh men, coming on shore, have caught the disease at the lodging houses in the ports, all of which probably abound with more than one contagion.

The same pamphlet observes, " the grave itself destroys not the contagious principles of small-pox, of which many well authenticated proofs could be given."—I am contented with one, as related by Dr. Jenner. Whilst a boy was attending to a grave-digger in a country church-yard, he was cautioned to be careful, as the ground then opened contained the body of a person, who died of small-pox ten years ago. The boy laughed heartily at the conceit; however, at the usual period, he paid for his temerity, by receiving the disease, which, from every enquiry, he could have derived from no other source.

From all this it appears, that the means of exterminating small-pox are, first, by an universal vaccination; next, by an universal quarantine; next, by shutting up all burying grounds, burning the

furniture of all suspected houses, painting, and white-washing the houses themselves.

When this is accomplished, we may have the good fortune to reduce ourselves to the situation of the inhabitants of New Spain, who, the same pamphlet informs us, lost half their number when the disease was first introduced among them. In Quito alone, one hundred thousand were destroyed. Greenland was almost depopulated in 1733. In the Isle of France, five thousand four hundred died in six weeks; and the Esquimeaux quitted their country for five years, after which, on their return, they found the skeletons of five hundred persons, who had fallen victims to the disease. To avoid these calamities, after the extermination of the disease in London, it will be advisable to keep up a succession of contagious cows, so that, should the small-pox contagion be introduced, our posterity may be secured. This is, however, looking a great way, for no one can be secure in London, without vaccination, till all the sources above mentioned are destroyed. As, in spite of every attention, all this must be attended with some uncertainty, and as vaccination offers us a complete security, I could wish the exterminating project to be first tried on some other contagion, which, should our success not prove permanent, is less formidable, and against which we have no means of securing ourselves.

Inoculation is not found to lessen the violence of the measles, and vaccination is no security against this or scarlatina. Either of these diseases is formidable enough, but I would recommend the experiment to be tried on the latter as the least so. So much has been said on the means of accomplishing this desirable object,* that I shall content myself with a few remarks, to show that the public mind does not seem sufficiently alive to the danger of this disease, under circumstances the most interesting.

* See Dr. Blackburn on Scarlatina, and Dr. Willan's last number of Cutaneous Diseases. The above disease has been so accurately traced by these two gentlemen, and measles by the last, that I have thought it unnecessary to say any thing on either, excepting as connected with the subject of contagion.

There can be no necessary connection between puerperal fever and scarlatina; but if they occur about the same time, the natural consequence must be increased inflammation about the peritonæum, which is the principal danger attending parturition. After the authorities, to which I refer,* and the events, which will, I am persuaded, occur to other practitioners as well as myself, in proof of such coincidence, the probable cause of it is well worth enquiry. Let us then recollect, that though a lying-in nurse is procured, who is conscienciously scrupulous in refusing to attend any families but for that purpose: yet that her residence may be in a neighbourhood, of whose situation she may not always be informed. Nor is this by any means the greatest danger. At these times, by the genial impulse of nature, every female is attentive to herself, and still more to her future offspring. Besides the baby linen, certain paraphernalia are to be procured. These are worked with a degree of delicacy, which requires more time than the wealthy can bestow; they are brought home with unsullied nicety, but the making, washing, and getting up have probably been in some confined apartment. When they are first produced for use, the fire of the lady's chamber is the most convenient for immediately airing them, before they meet her own or her infant's person.

That this, or some other such cause, produces the coincidence of these diseases, and the melancholy consequences which many of us have witnessed, cannot be doubted; and to this I suspect we may impute, that whilst the mortality from child-bed seems rather increased by the London bills, the lying-in hospitals afford registers so much more favourable.†

From these considerations, it is much to be wished that the exterminating project should be first tried on a disease, from which we have no means of protecting ourselves by inoculation, and in attempting to

* See Dr. Willan's Diseases of London, page 322.

† See Dr. Hebberden's Commentaries, page 38.

exterminate which, should we fail, we shall not be left in a worse situation than at present. Whilst we are looking forward to so desirable an object, we ought not to be inattentive to every means of lessening the fatality of this and measles. That there are seasons in which both are more favourable than at others, cannot be doubted. Scarlatina is, by general consent, distinguished by the terms scarlet fever and putrid sore throat. Whenever the disease assumes a mild form at any public institution, notice should be given of it; and if the accounts are similar at the Foundling, at Christ's Hospital, the Asylum, and Mary-le-bone Workhouse, or as many as may have received the contagion, it may be left to the discretion of parents, whether they chuse to take this opportunity of exposing their children, at a time when they can attend to them, and have no reason to suspect any thing in their constitution or habits which should unfit them for going through the disease; or whether they prefer that they should take the chance of receiving it at a distance from home, and perhaps immediately after violent exercise; or even in advanced life, under circumstances still more unfavourable. The same may be said of measles; for notwithstanding the errors which have been committed in confounding these two diseases, I am still satisfied that there is a putrid measles, and that it is more prevalent at some seasons than at others. I cannot easily reconcile myself to the opinion, highly as I respect the authority by which it is sanctioned, that the putrid measles, described by Dr. Watson, was scarlatina. Dr. Cotton's Account of Scarlatina appeared in 1749. Dr. Fothergill's third edition of "Account of the Sore Throat with Ulcers," is dated 1751, and Huxham's "Dissertation on the Malignant, Ulcerous Sore Throat," 1757. Dr. Watson's paper was read before a learned society in 1769, on events which occurred in 1763 and 1768. The writer had been, for many years, engaged in that branch of the profession, which would afford him the largest opportunities of seeing such complaints.

The distinction, therefore, between the two diseases must have occurred often to him. But the most striking circumstance is, that out of 180 children who were attacked, and of whom many had gangrenes in different parts, no one is described with a sore throat of any description. It is very certain that putrid diseases, of all kinds, are much less frequent among us, and though more is to be apprehended in scarlatina, from the law of the disease being to induce slough, yet in those seasons in which measles are fatal, the mouth shows all those symptoms we have described among the marks of putrefaction. But enough has been said to show, that there are seasons of greater and less fatality in both these diseases, and that until we have succeeded in exterminating them altogether, it may be desirable to lessen their terrors, as has been accomplished in small-pox.

As this most dreadful of any known Morbid Poison, may not only be rendered mild, but almost superseded, I shall conclude the work with some remarks on the advantages we derive from inoculation and from vaccination, which last seems to me only a perfection of the former.

It has been already remarked, that the advantage of inoculation arises from the first shock of the disease being confined to an inconsiderable space. That this is really the case is incontestibly proved: for if, from the backwardness of the inoculated part, or the forwardness of any other part, particularly the face, the first shock should be more general, the patient derives no other advantage from inoculation, than what arises from the caution with which he has been, in other respects, introduced to the disease. Though the first improvers of the practice were not sensible of this important fact, yet experience taught them to regulate their practice in a manner exactly conformable to it. Instead of large incisions, or the insertion of extraneous substances with the contagion, by which inflammation was excited, which might for a time supersede the wished-for

irritation, their incision was made as simple as possible. Not only recent matter was chosen, but the crude lymph was preferred, because it was found to be more active. When it became purulent, it was found more uncertain, because at that time suppuration had taken place to dislodge the slough, and probably on some occasions the pus might not even be mixed with the contagion lymph thrown out when the slough was first formed.

In proportion as the inoculated part showed early marks of the contagion, was their prognosis favourable. If it proved backward, every means were used to excite irritation. It is surprising, after this, that some of the most experienced writers should assert, that the suppuration at the arm and at the eruptions kept pace together. That this is sometimes the case cannot be doubted. But under the most favourable circumstances the arm will begin to dry before suppuration commences at the pustules, and to scab before the pustules begin to dry.

But should the most unfavourable event happen, and the eruption on the face show the same maturity as in the inoculated part, still the patient has been introduced to the disease under many advantages. He has neither been surprised, till the moment of the fever, under ebriety or high excitement from juvenile exercises, or before he has become convalescent from other diseases, and has with all a certain confidence in the operation, which tranquillizes his mind.

The success of their practice led them into another error. Finding that the early progress of the inoculated part was a security, they hastily conceived that the art could not be further improved, and that it was of no consequence with what matter they inoculated. This, however, does not appear to be the case, for, besides the register* already given, we could produce two others, in which we

* See page 16.

were interrupted by a similar suspicion of the parents; and at this time we have continued a kind of small-pox, which I am now going to describe, with scarcely any variety, for full three months. It has not, indeed, proved so frequently unattended with pustules, but this may arise partly from the necessity we have found of inoculating from pustular cases, to remove a suspicion which is still entertained, that cow-pox has been substituted instead of small-pox.

I should conceive it lost time to offer even a summary of the arguments adduced to prove that cow-pox is a security against small-pox. There is, in my opinion, no medical fact that stands on a securer foundation. The very exceptions, which have been made, are so few, as to establish the law. In all enquiries we are to consider the credibility of witnesses. On the side of cow-pox they are not only the most numerous, but include those names to which the public looks up with the greatest confidence. On the other side, there are certainly respectable names, but their number is comparatively small. The publications, however, on the subject are so numerous, and have been so lately digested by Dr. Willan, that I shall leave the mere question of evidence, and content myself with offering a few proofs that the cow-pox and small-pox are the same Morbid Poison. These proofs shall rest on those laws which were established in the early part of this work.

The varieties of small-pox have been marked by different writers. There is one, however, which is unnoticed by all, not excepting Sydenham. Probably he had only seen it so casually as not to think it deserving of particular notice. This variety I wish to call the *pearl* sort, because the nearer the pustule arrives to the figure and colour of a pearl, the more perfect is the character of the disease.

Dr. Jenner, in his first "Enquiry into the Nature of Cow-pox," mentions a small-pox, which for some time spread through the county

of Gloucester so mild, that the lower orders of people lost all their terrors of it, and the usual intercourse was maintained, as if no such contagion existed. No other description is given than that it never became confluent, and that it was, in a given number of subjects, as mild as if they had been all inoculated.*

The most striking distinction in the disease, to which I refer, is, that, contrary to what Sydenham observes both of the distinct and confluent, the pustules in the face remain white, like those on the body, till they scab. In the best marked cases the pustules are never very large, but particularly round. As they increase, the upper surface extends over the base, and as they dry, the scab becomes nearly globular, that is, the whole is distinguishable above the skin, without concealing more of the sphere than what would happen were such a figure actually placed upon the surface. If the pustule is dissected, the slough at the bottom is found particularly thin. Hence, though the inflammation is inconsiderable, yet the lymph, being less diluted with pus, the scab is much harder, as well as more regularly formed, than from the common distinct small-pox.

This small-pox is not only thus regular in its appearance, but is uniformly mild, and is never attended with what Sydenham calls the secondary fever, that is, the symptomatic fever from skinning. Thus, then, there may be a variety in small-pox, which proves permanent under inoculation.

The next thing I would remark is, that small-pox and cow-pox, contrary to the law of all morbid poisons, which are different in their nature, will proceed together in the same person, without the smallest interruption of each other's course. If inserted nearly at the same time, in the same person, each proceeds in the same

* "Enquiry," &c. First Part. I cannot now tell in what Journal or Transactions I have read an account of a small-pox so generally mild, that after a time no one took the trouble to inoculate, but exposed themselves without fear. A.

course as if they were in two different subjects:—if inserted nearly in the same spot, the two form one common areola, but the vesications are distinct, and each preserves its own character, till that of small-pox becomes purulent from suppuration for the separation of the slough. If secondary pustules follow from the small-pox, and they should continue coming out till the cow-pox has completed its progress, its vesicle, like any other inflamed part, will become the seat of a small-pox pustule, or the whole vesicle will become purulent, contrary to its legitimate character. In the first case, you may take small-pox matter from the pustule, which, by the adhesive inflammation, will remain distinct from, though seated in part of the vaccine vesicle; and from the other parts of the vesicle you may take vaccine matter, and each will perpetuate its respective morbid poison. If the whole vesicle becomes purulent, it is a variolous pustule, and will inoculate small-pox.

It was remarked by Dr. Woodville, that if a person is inoculated with small-pox to day, and three or four days after is re-inoculated with the same morbid poison, though the last insertion may remain a smaller pustule than the first, yet both inoculations will arrive at their height at the same time. The same takes place in cow-pox; and also, if a person is inoculated to day with cow-pox, and three or four days after with small-pox, or to day with small-pox, and three or four days after with cow-pox, the two insertions, though the last may remain smaller than the first, will maturate and scab at the same time.

By these facts it appears, first, that a marked kind of small-pox may be perpetuated. If, therefore, the cow-pox is a marked kind of small-pox, there can be no reason why it should not have been perpetuated with its true character; and that the cow-pox is such, appears, secondly, by its not being interrupted by, and not interrupting the progress of small-pox, and by both retaining their respective laws and characters at the same time, whether inoculated separately in different subjects, or in the same: or if each has been

inoculated in the same subject at different times, the consequence is similar to the inoculation of either one, at different times.

These experiments have been repeated so often, as to leave no question concerning the law. The same experiments have been repeatedly tried with small-pox and varicella, with small-pox and measles, and also with cow-pox and each of the others, yet those interruptions have always followed, which have been remarked in the early part of the work.

As, therefore, a marked variety of small-pox is capable of preserving its distinct character under inoculation, there seems no reason why the cow-pox should not be among such varieties; and as any of the known varieties will destroy the susceptibility to the disease in all other forms, so there is no reason why cow-pox, if among the varieties, should not do the same; and there is the more reason to expect this, because, contrary to any other morbid poisons, the action of small-pox and cow-pox are maintained at the same time in different parts of the same constitution, subject respectively to similar laws, whether only one or both of them are applied in any variety of forms.

It may be said that small-pox is an eruptive disease, whilst cow-pox, though affecting the constitution, is only confined in its local action to a single part. But small-pox is sometimes, we have seen, equally confined in its local action, and principally in those cases in which its appearance most resembles cow-pox. It is not less certain that cow-pox, on some occasions, produces secondary eruptions. Besides the cases I have seen myself, the Rev. Mr. Holt* gives an account of a full eruption of vesicles, which had the same properties of contagion as the inoculated part. The Rev. Mr. Fermor* saw a few scattered in different parts. Dr. Woodville first remarked that they appear at the time the inoculated part has formed a scab, which was not the case with those which, in his earlier experi-

* Med. and Physic. Journal.

ments, proved to be variolous. This appearance after the disease is considered as complete, as well as the few instances in which they occur may be the reason why they are so rarely seen. Dr. Jenner describes them as occurring sometimes in persons vaccinated whilst under herpes, in which case they occupy the places before covered with perpetic blotches. He informs me, likewise, that he has inoculated from a secondary pustule on the knee. In the Small-pox Hospital we have had several such opportunities: but they are certainly rare, compared with the numbers vaccinated. However, the last week has furnished us with a secondary pustule in the neck, the fluid from which is now producing the legitimate character in the arms of two different subjects.

The laws, above mentioned, might be unsatisfactory to prove the security of cow-pox, if they were not confirmed by irresistible facts: but the two together place the result with me beyond a doubt. That small-pox has occurred after cow-pox cannot be questioned, nor that it has occurred after small-pox. That fatal cases may have occurred, and others in which herpetic diseases may have followed vaccination, is what I am not disposed to doubt. But it is strange that the strong recommendations of some of its votaries, who have more zeal than wisdom, should have made the sober part of us forget, that both inoculation and vaccination are only submitted to, in order to avoid greater evils. We know that men have been poisoned by being sucked by leeches. Are we, on that account, to refuse their use? We know that suppurations in the joints have followed inoculation for small-pox: but how much more frequent has it occurred under the casual disease. Cow-pox has never been accused of more than a few accidental herpetic eruptions, which very rarely occur, and which, when left to themselves, have, for the most part, subsided spontaneously.

Respecting the laws of the two, they are certainly more permanently different than between any other varieties of small-pox.

But this is all we can urge. The cow-pox has never proved contagious by effluvia. The cow-pox is unattended with a slough, which never fails to attend every variolous insertion. But though the cow-pox does not induce slough, the action excited by it is not merely the elevation of the cuticle into a bladder: it extends to the cellular substance, without destroying its texture. Though the fat seems absorbed, yet the cells remain entire with their former communication, till the lymph reaches a certain defined disk; after this, if the fluid increases, it stretches the cuticle, so that the base is the narrowest part of the whole. In this state, if punctured, the exit of the lymph is very slow, on account of the various little cells communicating with each other in which it is contained. If suffered to remain long unpunctured, the cells will sometimes break, or be absorbed, and the cavity will be less interrupted. Secondary vesicles are entirely under the cuticle, and cannot be distinguished by the eye from the chrystallines of small-pox in their early stage. This, and the time of their appearance, prove to a certainty that they cannot be the effect of re-inoculation by the fingers of the patients. The insertions from cow-pox, though subject to some varieties, are more regular in their character than any morbid poison with which we are acquainted, whose progress is so rapid. The insertions from small-pox, though all attended with a slough, are extremely various in their appearance, and sometimes for the first eight days, or longer, cannot easily be distinguished by the eye from the cow-pox. But the ingenious discoverer has marked a permanent difference between the two, which has been too much unnoticed, namely, that in cow-pox the contents of the elevation are always limpid till the scab is formed: the small-pox, on the contrary, always becomes purulent before it dries. This difference arises from the slough in the latter, which renders suppuration necessary for dislodging the dead part. The cicatrix is a general, but not a certain criterion. In the small-pox it is either smooth or corrugated, depending on the manner in which the slough

has cast off. In the cow-pox it is either beset with smooth indentations, or consists of one or two smooth depressions, depending on the progress of the vesicle. If the cellular substance remains unbroken, the surface will remain marked, as the fat has been absorbed to admit the fluid into the cells. If the inflammation has induced more violence, so that the cells themselves are broken or absorbed, the surface will be more even.

As to every other mark of perfect or imperfect vaccination, nothing has been added since the discoverer published his "Enquiry." There is no mode of judging of vaccination but by the progress of the vesicle, by its contents, its scab, and cicatrix: and though the last may prove a confirmation when regular, it is not to be considered, when irregular, as a sufficient proof of want of security. The vast numbers we have tested in every possible way at the hospital, prove to a conviction, that whatever objection may be made by the enemies of vaccination, or whatever doubts and anxieties may be nursed by its over zealous friends, the practice must ultimately bear down all opposition, and the arguments against it will be forgotten, or only recollected, like the early pamphlets against variolous inoculation.

APPENDIX.

No. I.

CONSISTED, in the last edition, of two letters, the first from Mr. Brown, of Spitalfields, shewing that when the glans penis, or prepuce, slough from common inflammation, they granulate like any other part—the second from Mr. Wadd, of Basinghall Street, giving two instances in which the same parts skinned, without granulations, after a considerable loss of substance by morbid poisons.

No. II.

ON THE SMALL-POX SLOUGH.

THE facility with which the slough is discovered at the bottom of every pustule, would induce me to wonder that it has not been universally noticed since Mr. Hunter first pointed it out, were it not that at one time I found it difficult not to see the slough, for that is never absent, but to remove it entire, and without injuring its figure. Experience has now taught me, that this arose from attempting it before a sufficient separation had taken place. Since that time we have had no difficulty in removing it in its exact circular figure, though in the pearl small-pox its texture is so thin, that it seems always surprising it should not break as it is taken up by the forceps.

I have since seen that Mr. Cruickshank considers this substance as part of a fourth membrane, composing the common integument. It is not for me to dispute the accuracy of a professed anatomist; and unfortunately, as long as he lived, after publishing his "Remarks on insensible Perspiration," we were too far removed to converse together on the subject. However, as in his explanation of the plate, he calls the circle I have described a *slough*, surrounded by an elegant radiated vascular circumference, as depicted in the plates, it may be thought a matter of no consequence, whether it consists of a lamina of the integuments, or cellular substance, or coagulum thrown out into the latter. I shall only remark, that the difference of its thickness, depending on the degree of inflammation, would not lead me to consider it as part of a membrane, which is not likely to vary so much in different subjects, and sometimes in different parts of the same subject, as this slough does. This, however, being a subject of experiment, I shall leave to be decided by those who have the best opportunities, especially as the practical inferences will be the same, whatever the slough may consist of.

The only cases, in which the slough cannot be traced with the same certainty, are those irregular vesicular small-pox which have appeared after cow-pox, and on some other occasions. Their figure is generally somewhat eliptic, and though the pustules preserve a regularity in the manner in which they succeed each other in different parts of the body, yet the whole process is completed two or three days sooner than the mildest of the regular distinct small-pox. Nothing but the progressive hardness of the pustules would mark the disease in some of these cases, and that would have been insufficient if the insertion from them had not produced the true small-pox.

THE END.

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EXPLANATION OF THE PLATES.

THE figure of Gonsalves is contained on two plates, in order to show the features on a larger scale.

The plate marked 2, describes a youth in his eighteenth year, without any marks of puberty, though the disease has made so little progress.

N. B. The marks on his body are not characteristic of the diseases, but from *acari syrones.*

The figures on each side delineate the upper and lower surface of the *acarus syro,* magnified by a solar microscope.

The 4th plate, marked plate 3, is the figure of the married woman referred to in the account.

The red patches on the face in this figure, and on the thighs of the last, arise from scratches, and are not truly characteristic of the disease.

Direction for the Binder.

Place the two plates, which are not numbered, first—The plate marked 2 will then be the third in order, and that marked 3 the fourth.

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