

A treatise on the supposed hereditary properties of diseases, containing remarks on the unfounded terrors and ill-judged cautions consequent on such erroneous opinions; with notes, illustrative of the subject, particularly in madness and scrofula / Joseph Adams.

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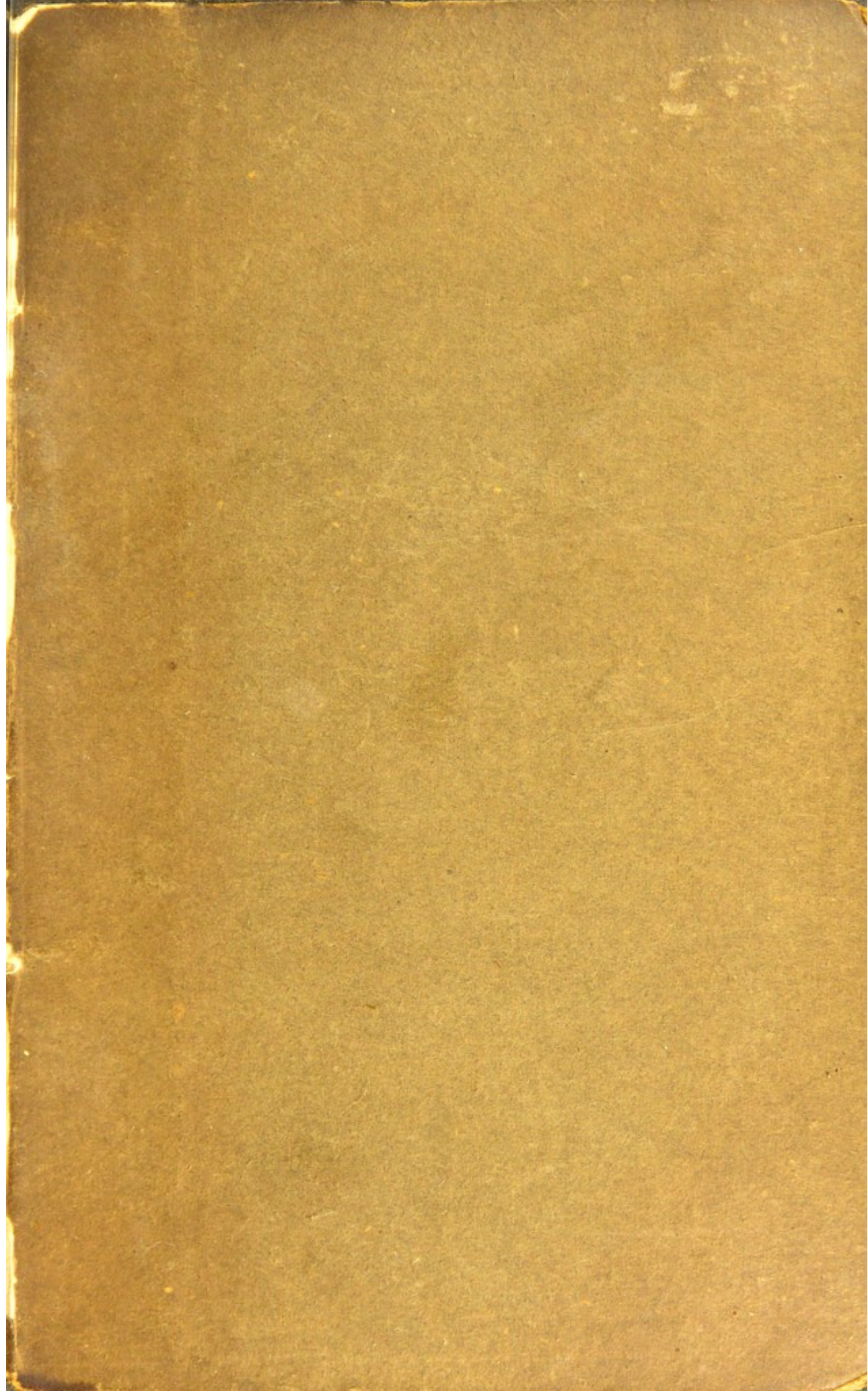
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M Baillie

A
TREATISE
ON THE SUPPOSED
Hereditary Properties of Diseases,
CONTAINING
REMARKS
ON
THE UNFOUNDED TERRORS AND
ILL-JUDGED CAUTIONS
CONSEQUENT ON SUCH ERRONEOUS OPINIONS;
WITH
NOTES,
Illustrative of the Subject,
PARTICULARLY IN MADNESS AND SCROFULA.

Tu ne cede malis sed contra audentior ito
Quam tua te fortuna sinet.

Virgil.

BY **JOSEPH ADAMS, M.D. F.L.S.**
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HONORARY SECRETARY TO THE MEDICAL SOCIETY OF LONDON,
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AND VACCINATION; TO THE NEW FINSBURY OR CENTRAL
DISPENSARY, AND AUTHOR OF OBSERVATIONS
ON MORBID POISONS, &c.

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ERRATA

Hereditary Properties of Diseases

REVISED

THE LONDON SCHOOL OF MEDICINE AND SURGERY

UNIVERSITY OF LONDON

NOTES

OF THE

LECTURES

BY

BY JOSEPH ALGER, M.D., F.R.C.S.

Printed by W. Thorne, Red Lion Court, Fleet Street.

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TO THE
RIGHT HONOURABLE
SIR JOSEPH BANKS,
BARONET,

KNIGHT OF THE MOST NOBLE ORDER OF THE BATH,
MEMBER OF HIS MAJESTY'S MOST HONOURABLE
PRIVY COUNCIL, AND PRESIDENT OF THE
ROYAL SOCIETY OF LONDON.

MY DEAR SIR JOSEPH,

IT cannot be from any wish to inform the world of your Titles, that they are thus enumerated; it is from a real satisfaction at seeing so many Honours so worthily bestowed.

Accept my sincere Thanks for the flattering manner in which you urged the publication of these Sheets; still more, for per-

IV.

mitting your Name thus to precede them ;
and allow me to hope with every Friend
to Science and Humanity, that you may
long continue to preside over that learned
and respectable Body, whose wish must be
the perpetuity of your services as well as of
its own existence.

I have the honour to be, with every senti-
ment of gratitude and esteem,

Your faithful and obedient

Humble Servant,

JOSEPH ADAMS,

Hatton Garden,
May the 21st, 1814.

PREFACE.

TWO great sources of distress, much aggravated by the uncertainty in which they are involved, are the danger of contagion and the apprehension of hereditary diseases. The former has often embittered the enjoyment of all that providence has bestowed upon us, and even stifled the feelings of consanguinity, friendship, and love: the ill effects of the latter have been in proportion to the strength of the moral feelings. The dread of being the cause of misery to posterity, has prevailed over the most laudable attachment to a beloved object; and a sense of duty has imposed celibacy on those who seemed by nature the best constituted for the duties of a parent!

In these, as in many other highly important questions, men seem afraid of enquiring after truth; cautions on cautions are multiplied, to conceal the skeleton in the

closet, or to prevent its escape, till our very fears bring the object constantly before us, not in its real form, but multiplied into every possible shape, and magnified in all.

Mr. Hunter, by instructing us in the means of ascertaining the laws of contagion, and the characters of morbid poisons, has relieved us from much of this uncertainty. I have endeavoured to continue his mode of research, and to elucidate his doctrines, not by a greater accuracy of expression, but by adopting, where it could be done, a more popular language. The opinion of the medical world has been so much in my favour, that I have only to regret the limited field in which I have been enabled to act as an interpreter to such an oracle. There are, however, sufficient documents to prove, that neither time, industry, danger, expence, nor what, with most of us, is greater than all, obloquy, ever arrested me in those professional inquiries.

Connected with them was one, the value of which I accidentally learned. Whilst Sir Joseph Banks did me the honour of perusing

one of my papers, I waited with some impatience to hear his objection to my remarks on the hereditary properties of diseases. As soon as I learned that the pause did not arise from any difference of opinion, I had no difficulty in determining to make a distinct Essay on what had hitherto been only incidentally noticed.

On a discussion so new, some indulgence may be expected in the use of new terms, or rather in assigning to old terms, meanings more strictly appropriate. The work being intended for the general reader, every technical expression is carefully avoided; and in order that the attention may not be distracted from the chain of reasoning, every thing not necessary to illustrate the doctrine is added, in the form of Notes, at the end. The Reader will readily account for, and it is hoped, pardon the unexpected length of one of them.

CONTENTS.

	Page.
Division of the Subject - - - - -	10
Importance of the Distinction between Family and Hereditary Diseases - - - - -	12
Necessary to trace the Degrees of Susceptibility in each	13
Connate Diseases rarely Hereditary - - - - -	14
Dispositions to Disease, when Hereditary, show themselves at certain Ages - - - - -	15
If the Predisposition only is Hereditary, the Disease may be prevented - - - - -	16
Of the supposed Hereditary Properties of Gout and Madness, Consumption and Scrofula - - - - -	17
The Doctrine illustrated - - - - - from 17 to 27	
If Susceptibilities do not appear till the more advanced Climacterics, the Disease may be often prevented -	27
General Conclusions from this Part of the Inquiry -	29
Provisions of Nature for preserving and improving the Race—in Climate—in what we see in a State of Nature—and in the Progress of Refinement - - - - -	31
A particular Provision for an Hereditary Susceptibility, which could not be corrected by either of the above Means	37

INDEX TO THE NOTES.

Danger of ill-directed Cautions - - - - -	45
The Author's Claim to Public Indulgence - - - - -	47
Abstract of M. Portal's Paper - - - - -	59
Definition of Madness attempted - - - - -	71
——— of Scrofula - - - - -	81
Goître and Cretinism, is their Prevalence in certain Districts the Effect of Hereditary Causes? - - - - -	85
Remarks on Elephantiasis and other Cutaneous Diseases, in Answer to Dr. Bateman - - - - -	89
A single Case of supposed Hereditary Deafness - -	124

ERRATUM. P. 46, l. 22, *for* intereos, *read* inter eos.

A

DISSERTATION,

&c. &c.

THOUGH hereditary peculiarities in Man are infinitely more important than in other Animals, yet the little information hitherto collected on the subject, would lead us to suppose they have been less attended to. **MERCATUS**, in the beginning of the 17th century, published among his voluminous works, a short but very judicious Treatise, *De morbis hereditariis*. This has been overlooked by every succeeding Writer who has touched incidentally on the question; and from that time to the present, I can meet with no systematic performance on the subject. A late number of the Memoirs of

B

the French National Institute, contains a long paper by M. PORTAL, "*Sur la nature & traitement de quelques maladies hereditaires ou de famille.*"—This appears to have been written as the sequel of several Prize Essays; we may, therefore, consider it to be the present state of our knowledge, as far as has been published: yet in that Essay, so little is done towards arrangement, that we find the terms *connate* or *congenital*, *hereditary*, and *family*, used almost as synonymous, and even confounded with *connutrite*, or diseases derived from nurses.

We can only account for such apparent backwardness in so important an Inquiry, by the great difficulty in selecting facts on a subject which does not admit of experiment; and by the unwillingness to be informed in those who are most interested, on a matter which proves an alloy to their best feelings. Perhaps, however, it may be found in this, as in most other sources of uneasiness, that a more accurate knowledge is less painful than constant suspense: nor can the

inquiry be unimportant to any family, how free soever they may fancy themselves from any hereditary peculiarity; for it will require no argument to prove that, like the varieties in other animals, all these peculiarities must have originated in the offspring of couples who were free from them.

But facts, however numerous and well authenticated, lose much of their value if injudiciously collected, as none but Naturalists are aware of the peculiar characters by which varieties are more strongly marked. In the following paper, therefore, my wish is,

1st. To propose an arrangement, which, after long application to the subject, I have found most convenient in this early stage of Inquiry.

2dly. To illustrate this arrangement by well authenticated facts, and to show their practical importance by inferences which naturally arise from them.

3dly. To ascertain what provisions are made by Nature to correct any apparent deviations in the human race.

And, lastly, To see how far these provisions may be imitated or improved by Art.

The proposed arrangement consists, first, in keeping always in view the necessary distinction between a *family* and an *hereditary peculiarity of constitution*; and, secondly, in marking the period of life and other circumstances under which such peculiarities, whether family or hereditary, show themselves.

The distinction between a *family* and *hereditary* peculiarity consists in this; that the first is confined to a single generation, to brothers and sisters, the children of the same parents; and the second is traced from generation to generation.

Though these terms are generally used as synonymous, and though some of the peculiarities we shall describe may be applied to either in different families, yet we shall see, as we proceed, the importance of attending to the distinction.

The period of life at which such peculiarities discover themselves is the next

consideration. This will be best illustrated by diseases, because these are most strongly marked; and to these our inquiries into human physiology should be principally directed.

Diseases either appear at birth, in which case they are called *congenital* or *connate*; or they arise afterwards.

The first only can with propriety be called *hereditary* or *family diseases*, all others we should consider as *hereditary* or *family susceptibilities* to certain diseases. The degrees of *susceptibility* are, in one point at least, so strongly marked, as to render it necessary that they should be distinguished by appropriate terms.

If the *family* or *hereditary susceptibility* is such, that the disease, though not existing at birth, is afterwards induced without any external causes, or by causes which can not be distinguished from the functions of the economy, such a state may be called, A DISPOSITION to the disease.

But if the susceptibility, though greater

than is remarked in other families, is so far less than a disposition as always to require the operation of some external cause to induce the disease; this minor susceptibility may be called, A PREDISPOSITION to the disease.

Having thus marked the distinction between hereditary and family peculiarities, and the division of each into the *connate*, the *disposition* and the *predisposition*, in proportion as the susceptibility may be greater or less; I shall offer some general remarks on the different ages at which family and hereditary diseases as they are called, for the most part show themselves.

Connate or *congenital* diseases are more commonly family, than hereditary: some of them being mortal, cannot indeed be transmitted, of which connate hydrocephalus or watery head is one among other instances: other connate peculiarities are more properly organic privations or imperfections, as connate deafness, or connate cataract.

DISPOSITIONS are found in some families

to diseases which are connate in others; *hydrocephalus*, which we have remarked as connate in some families, in others occurs to several brothers and sisters in succession, as they arrive at a certain age. The disposition to blindness and deafness is often hereditary, though the connate privation of those senses is, I believe, always confined to a single generation. When the disposition is hereditary, the children are born with perfect organs; but usually about the age of puberty, a dullness of the sense is discovered, which gradually increases for the remainder of life, or till the entire loss of the faculty.

PREDISPOSITIONS also are found in some families, and *dispositions* in others, to diseases in the same organs, and called by the same name, of which the pulmonary consumption may serve as an illustration. In some families, we see a number of brothers and sisters falling into consumption in succession as they arrive at a certain age. This we may strictly call a *family disposition* to the disease, inasmuch as it is confined to a single

generation, and as we can discover no external cause to excite it. Another kind of consumption, and the most common in cold climates, is hereditary; but only in *predisposition*, always requiring the influence of climate to induce it, and consequently always to be prevented, and often relieved, by avoiding the exciting cause. In this instance, we see a disposition in some families, and a predisposition in others, to diseases different in themselves, though called by one common name. We shall now see, that even in the same disease a difference of susceptibility may be discovered in different families: and this difference it will be found not less necessary to mark.

Gout and *madness* are, by almost universal consent, considered hereditary; yet, if we admit the general implication as to their immediate causes, both these diseases, and particularly the former, should be considered as only hereditary in predisposition. If it were true in all, as it is in most cases, that the habits of the sedentary and wealthy are

necessary to induce the gouty action, there could be no question, that it is only hereditary in predisposition; but in some, the susceptibility to gout is so strong as to require no other stimuli for inducing the action, than such as seem absolutely necessary for the support of ordinary health. In *gout*, therefore, we must admit the two degrees of susceptibility, *disposition* and *predisposition*, nor will it be often difficult to fix their exact limits.

In *madness*, the difficulty may seem greater, but this is only on account of the frequent impossibility of ascertaining the state of the mind previous to such a change, and still more from the ill-judged secrecy with which such events are often obscured.

It may be asked, Why this attempt at discrimination, where the differences seem almost to meet? I shall therefore proceed, as proposed,—To illustrate the arrangement by well authenticated facts; and the necessity of it, by inferences which naturally result from them.

Connate or *congenital diseases*, it has been remarked, are rarely hereditary, which is the more remarkable, because peculiarities of form in less important parts are often hereditary. A striking feature is transmitted from generation to generation; but though *congenital cataract* is frequent among brothers and sisters, I have never known, nor can I discover any record, in which it has occurred in their offspring. Those who are born *deaf*, and consequently *dumb*, rarely marry, which makes it difficult to decide the question in them. I have however known an instance, in which a nobleman of this description married and had a numerous offspring, all perfect in those organs: his grand-daughter married, according to the laws of her country, her own uncle, that is, the son of her deaf grand-father; yet, none of her numerous children have any defect in hearing. It is true, in this instance, the grand-father was the only one of the family who was deaf, so that the defect was neither an *hereditary* nor *family privation*.

But though *congenital blindness* and *deafness* are rarely, if ever hereditary, the *disposition* to these defects is often so ; and in such cases it will be found, that the disease usually shows itself at an early period of life. The family of the Le Comptes is a striking illustration of an hereditary disposition to blindness :* many might be adduced of an hereditary disposition to Cataract, and Mr. Bass's family of Peterborough is, as far as the race has extended, one among many other instances, of an hereditary disposition to deafness. All the Le Comptes saw clearly till about the age of 16 or 18 ; at that age, some of them, without any apparent cause, became dim-sighted, and grew gradually more so, till they became dark : such has been the case for three generations, with a certain number in each race ; meanwhile, such as have escaped that critical age, have retained their sight through life. In the Bass family, the same progress has followed

* See Baltimore Med. and Phys. Reg. 1809.

in defect of hearing, at the same age, and excepting the difference of numbers, with nearly the same consequence.

It has been remarked in another place,* that the hereditary disposition to *elephantiasis* is governed by similar laws. In families the most disposed to that disease, such as have the disposition, show it at an early age; and those of the same family, who escape beyond that age, remain free throughout life. Such has been the case also, with a disposition to acute *hydrocephalus*, in a family of several children. Three were seized with the disease about the age of puberty, and in all, the rapidity was so great, that the most powerful remedies seemed scarcely sufficient to protract the fatal issue. Those who escaped that age, have continued free from the complaint. I knew three brothers attacked with *angina pectoris*, each as he arrived at the age of about eighteen; to all it proved fatal in a few months; such of the children as grew up to manhood escaped the disease,

* See Morbid Poisons,

The above illustrations are sufficient to shew, that when the *susceptibility* to an hereditary or family disease is so great as to amount to a *disposition*, that is, so great, that the disease is induced without any external causes, we can have little hopes of preventing it ; and that if the disease has arisen during the changes about the age of puberty, we are to expect a cure, more from a proper direction of the efforts of nature during that period, than from remedies which may be useful in the same disease, when excited by external causes, or induced at a more advanced age. It is, therefore, of the utmost importance to recollect, that such constitutional dispositions are more commonly confined to brothers and sisters, than hereditary ; and that, whether family or hereditary, they always show themselves at an early period of life. Hence, those of the children who have passed that age without any of the symptoms, may be considered as free from the constitutional disposition. It will also appear by what follows,

that the danger or security of the rising offspring may often be estimated by a similarity of feature or character to those of their brothers or sisters, who have previously fallen into the disease. The mother of the hydrocephalic children before mentioned has been correct in her prognostic, which of her latter children would be seized, and at what age; painful as such a prospect may be, it is more tolerable than the horrors she might have suffered from constant suspense, as she can view most of her children without apprehension, and is in some measure prepared for the only mode however precarious, of preserving the rest.

This remark is still more applicable to that kind of consumption which affects several brothers and sisters about the same age. The parents are often healthy, or at least free from this disposition; but the fate of some of their children, gives an early presentiment concerning others born afterwards of a similar complexion, features, and temper. Meanwhile the young subjects are the

last to see the danger, and when it is suspected, the excess of life, if I may so call it, or the precocity of growth and intellect is such, as to precipitate a most interesting figure and character into a vortex, from which no caution can prove any security. But when the susceptibility is so slight as to amount only to a *predisposition*, we have rarely any means of discovering it till the disease itself approaches; nor is there any age at which we may call the patient secure. As, however, some external cause is always necessary to induce the disease, we may hope to prevent it by avoiding such causes, or to cure it by removing them. Hence, the importance of distinguishing the first described consumption from the scrofulous; the one a family disposition, requiring no external cause to excite the disease, which exists in all climates, and is fatal in all; the other an hereditary predisposition, never excited into action but in certain climates, and the disease often cured by an early removal from them.

In this instance, the different degrees of susceptibility in different families may be thought to be sufficiently explained by the difference in the two diseases; which, though generally confounded, have little more in common, than that they are both seated in the same organ. But many instances occur of well-marked different degrees of susceptibility to the same disease in different families. I shall only mention *gout* and *madness*, for the reasons beforementioned. When *gout* appears at an early age in a temperate subject, when it invades the cottage, or is seen in hospitals, we cannot question that the susceptibility is such as amounts to a *disposition* requiring no external causes to produce it. When the disease follows intemperate or sedentary indulgences as exciting causes, the susceptibility amounts only to a *predisposition*; and though this *predisposition* is often hereditary, yet the *disease* itself will be found in more instances original than hereditary.

This may, at first, appear paradoxical,

but for the truth of it, I appeal to common experience, nor will it be difficult to account for the cause on the principles laid down.

Where the hereditary susceptibility amounts only to a *predisposition*, some external cause is necessary to induce the disease. These external causes are well known. The predisposition may, therefore, exist from generation to generation, without any appearance of the disease among those whose habits are frugal, from necessity or choice. But if one of them should acquire the means, and yield to habits of indulgence, we shall see him the first of the family to bring this predisposition into action. Thus it is, that we so frequently find gout in members of a wealthy corporation, whose ancestry never felt the disease, only because they were never exposed to the exciting cause. From this it follows, that where the predisposition exists, it is of no consequence to the future progeny, whether the disease has been excited or not. For it will be

sometimes found, not only that the disease has been thus excited for the first time in the family, but that the progeny of the same individual will remain free from it. The indulgencies above alluded to, though new to the individual, are familiar to, and consequently less valued by his posterity; and labor, which, from early necessity, was irksome to him, is by them courted as exercise. The inference follows, that in a *gouty disposition* we cannot expect to prevent the disease; the most we can promise, is to protract the periods of intermission, and to moderate or shorten the paroxysms:—In the *predisposition*, on the contrary, the disease may be prevented or cured.

Madness, as well as *gout*, is never *hereditary*, but in *susceptibility*; and those who have paid the greatest attention to the subject, must admit the two degrees of susceptibility. When we perceive, (an event by no means uncommon) several children of the same parents, and sometimes in different branches of the same family, seized with

madness about the age of puberty, we cannot but admit a disposition to the disease ; for though some mental irritation is usually assigned, yet the cause is often so trivial, that we cannot doubt whether the supposed effect has preceded it. Sometimes we find the disease cease, as the changes of the constitution during that period are compleated. If that should not be the case, little can be expected from art. But when the *susceptibility* amounts only to a *predisposition*, requiring the operation of some external cause to produce the disease, there is every reason to hope, that the action of the disease may be for the most part much lessened, if not prevented altogether : for this purpose, the hereditary peculiarity should always be kept in view in the direction of the early studies, in the subsequent employment, and in the discipline, during that early period of life, which admits and requires every judicious retsraint.

These remarks may be for the most part applied to a state of susceptibility hitherto

unnoticed, because not easily included in either of the other divisions. The state to which I refer is induced by pregnancy and child-birth in women, and at the more advanced climacteric in both sexes. Though the actions excited on these occasions arise from the functions of the economy, yet they are not the ordinary functions. In most cases the provisions of Nature are sufficient for preserving the subject during such changes; and on that account they are often too little regarded. In women, not only pregnancy and child-birth, but the critical period of advanced life is strongly marked, and many judicious cautions are to be found in medical writers on this last subject; but it is a great mistake to suppose, that the change in men, about the same age, is always unattended with any disturbance of the constitution. I know a family in which three brothers were each, in succession, attacked with symptoms of Angina pectoris between the age of forty-seven and fifty. The disease was less violent than in the young sub-

jects before mentioned, so that each lived with a continuance of the complaint to a fair old age. Those who escaped to the age of fifty, remained free for the remainder of their lives, and none were attacked earlier. Whether the disease was hereditary, or confined to that generation, cannot be ascertained. The mother was said to be asthmatic, and died suddenly ; but it was before Dr. Heberden had given a name to that species of asthma. All her grand-children, many of whom have survived the critical age, have remained without any symptoms of the disease, and all have with much prudence looked forward to every means of prevention, as the period has approached. It is worth remarking too, that the females of the former generation escaped ; whether by a greater attention to the changes of that age, it is not in my power to determine.

On the whole, may we not draw the following conclusions from this part of our Inquiry, namely, That when the disease is *connate*, we can have no means of forming

any prognostic, nor of superseding it. That when the *susceptibility* is such as amounts to a *disposition*; that is, when the disease is expected at an early age, and without any external cause to induce it, our hopes of prevention must be very feeble, and our hopes of cure must rest principally on the completion of the constitutional changes. But,

That when the susceptibility amounts only to a *predisposition*, or is insufficient to induce the disease without the access of some external cause, it may often be prevented or cured.

That the same attempts at prevention and cure may often succeed when the susceptibility is excited into action by pregnancy, or by changes which take place at critical periods in more advanced life.

That, as we have so little chance of superseding a connate disease, or of preventing one to which there is a constitutional disposition, it will be a most important consideration to ascertain how far such connate diseases, or such dispositions to early disease,

are in other cases confined to a single race, as we have seen in most of the instances adduced; and whether, as in the rest, those of the family who escape the critical age, are free from the disposition.

Such appears to be the present state of our knowledge respecting the diseases themselves. Let us now consider what provision has been made by Nature to correct such hereditary peculiarities, and how far they may be imitated or improved by Art.

We must at once see, that if no provision had been made in the construction of animals to prevent it, hereditary diseases would by degrees have become universal; whereas there is every reason to believe, that they lessen in the human race, as Society improves: and we shall see that so important an end is not left to the uncertainty of human institutions. Throughout all the animated productions with which we are acquainted, there is found a disposition in every variety to return to the original form; and in those animals which are reared for

our use, much industry is required to prevent it. In a state of nature the race of all gregarious animals is probably progressively improving, as far as is consistent with their capacity for improvement. The strongest male becomes the *vir gregis*, and consequently, the father of most of the offspring. In the ruder state of human society, or rather in its earliest formation, something of the same kind may prevail; but in a more advanced stage, sufficient provision is made by the preference which health and intellect will for the most part produce in either sex.

Another provision arises out of climate; which we have seen is, in some cases, the only means of exciting a diseased susceptibility into action. Those constitutions, which are peculiarly susceptible of such diseases as are excited by climate, fall an early sacrifice; hence, the propagation from such sources gradually lessens, and the disease would cease altogether, were it not that parents, free from such susceptibility,

occasionally produce an offspring in whom the susceptibility originates.

Thus we see the natives of warm climates, when removed to colder, are peculiarly liable to scrofula; and it cannot be necessary to add, how much the natives of colder climates suffer under the Tropics, from causes which produce little or no effect on the offspring of the old inhabitants. By these means a race is gradually reared with constitutions best calculated for the climate: a law which, I suspect, has been too much overlooked, in our inquiries after the causes of the more marked varieties in the human species.

The provisions of Nature, however, for restoring the original form, in animals reared for our use, may be very much interrupted by accidental circumstances, and almost superseded by the industry of man. Sir John Sebright informs us, that if a flock of sheep, in which there is any defect, are permitted to breed *in and in*, the defect will

gradually increase among them ; and Colonel Humphries, by selecting for breeding a marked variety, has succeeded in procuring a flock, all of them with deformed bones. If the same causes operate in man, may we not impute to them, many endemic peculiarities found in certain sequestered districts, which have hitherto been imputed to the water, and other localities? And may we not trace a provision against such a deterioration of the race, in that revealed law, by which any sexual intercourse between near relations is forbidden, on pain of death? This prohibition, as far as we can judge, proves sufficient to prevent the too great influence of such an hereditary cause, since the number of maniacs does not increase in proportion to our increased population, and the great exciting causes of madness, namely, increased wealth, and other sources of ambition. Nor is this the only provision we can trace. The worst stages of madness are attended with a total indifference to the sex, not to mention the very general inclination

to suicide, which the utmost vigilance cannot always prevent. Seeing then how little is left in so important a concern to the operation of human institutions, have we not reason to be satisfied with the provisions of Nature, and with the Divine commands? Yet, in the most serious of all hereditary peculiarities, the great susceptibility to madness, celibacy has been recommended as a duty. Before we venture to propose measures contrary to one of the strongest impulses of Nature, and to the first blessing which the Almighty Fiat bestowed on man, it becomes us seriously to weigh the consequences.

Were this opinion universal, it would probably produce its effects only on the most amiable and best disposed, whilst the profligate and unprincipled would indulge themselves, regardless of posterity: It is scarcely necessary to hint at the result. To interdict marriages with the healthy individuals of such families, might do much towards extinguishing that enthusiasm, which,

when well directed, proves the source of those atchievements which aggrandize families, which encrease the glory of nations, and improve the condition of mankind. Nor is this confined to heroes and statesmen, but extends to the effusions of genius, and to the cultivation of the softer virtues. It is neither necessary, nor proper to introduce names, they will occur to every one who has lived long enough to become acquainted with the ramifications of families.

We see the influence of climate sufficient to preserve a race suited to particular regions; and where the irregular action is unconnected with climate, we find a Divine law, with other causes arising from the irregularity itself, all tending to restore the original form.

But should there exist a disease, the disposition to which is excited by climate; should such a disposition become hereditary, and should the disease when excited prove incurable, from such a combination of causes we could expect nothing less, than the gra-

dual extinction of the race ; and should the district be repeopled, the same succession of causes and effects must gradually extinguish the descendants of the new Colonists ; yet, such a disease does exist in the finest and most extensive part of the habitable globe. Human institutions have indeed made some feeble attempt at restraining it, but human endeavours must have proved ineffectual. Happily, the same power which permitted such a cause, has fixed limits to its effects.

The *Elephantiasis* of ARETÆUS is peculiar to warm climates ; the disposition to the disease is hereditary, and the disease itself has, hitherto, proved incurable. I have never been able to learn, that it has attacked emigrants from a colder climate, nor their immediate descendants. A residence therefore of some generations, is probably necessary to induce the disposition. When the diseased disposition is *derived* from *inheritance*, the action always commences before the age of puberty ; and the subject never

arrives at that state; the organs are never evolved, and no other marks of virility appear. When the disease originates with an individual, it usually commences at a more advanced age; but from that time, the organs which distinguish the sexes decay, and become gradually unfit for their original purposes. This fact of a disease, which arrests the progress to virility of every youth, and emasculates every adult whom it attacks, is so surprising, that after having witnessed it myself, I should have been backward in publishing the result of my observations, had not others been present at every examination; and I should have been unwilling to draw inferences from them, had not subsequent Writers confirmed my account.*

Thus is an hereditary disposition to an irregularity of the most formidable nature, which being excited by climate, must have progressively increased in spite of all human

* See Edinubrg Medical Journal, vol. v. p. 500, Note.—Gentleman's Magazine, Vol. 81, July to Dec. 1811, p. 145, second Column; and Dr. Gourlay's History of Madeira.

institutions, arrested as soon as it occurs, by those very actions which form a part of the deviation from the usual progress of Nature.

If what has been stated in this and the other parts of the paper should be confirmed by future observations, the result of the Inquiry will be,

That *connate* diseases or privations are not hereditary.

That *dispositions* to certain diseases are more commonly family than hereditary ; that the diseases arising from them usually show themselves at certain ages ; if early in life, that we have little chance of preventing or curing them ; but that such of the children as escape that age, are as safe as the descendants from other families.

That *hereditary predispositions* to the most prevalent diseases are brought into action, either by climate, which destroys at an early age those who would be the means of transmitting such predispositions to posterity, or by such external causes as may often be prevented.

That whenever an *hereditary* or *family susceptibility* to any disease is suspected, the changes in the constitution induced by gestation, parturition, and the more advanced climacterics, should be particularly attended to.

That if the human race, like other animals, has a constant disposition to restore itself from every irregularity, the Divine law, which forbids any sexual intercourse between near relations, seems sufficient to correct every peculiarity unconnected with climate.

That if an *hereditary disposition* is generated by climate, it must progressively increase from the constant operation of such combined causes. That no remedy, therefore, can be sufficient, but the prevention of propagation as soon as the disposition becomes hereditary; and that such provision is made by the diseased action itself.

That as far as our inquiries into these irregularities have hitherto extended, sufficient provision is made for correcting them

by the influence of climate, by the interdiction of sexual intercourse between near relations, and by the effects which the irregularities themselves induce.

That all interference, therefore, with the dictates of Nature, beyond the expression of revealed will, appears unnecessary.

That to lessen anxiety, as well as from a regard to the moral principle, family peculiarities, instead of being carefully concealed, should be accurately traced and faithfully recorded, with a delicacy suited to the subject, and with a discrimination adapted to the only purpose for which such registers can be useful.

NOTES.

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NOTES.

NOTE 1, Page 6. — "*Cautions on Cautions are multiplied, &c.*"

When men gravely tell us, that we cannot be too cautious, or that it is better to take too much than too little care, we can only remind them of the *male sedula nutrix* of Ovid, and the *operosus nihil agendo* of another Latin Poet. A caution ill-directed is a greater evil than no caution at all, inasmuch as it supersedes inquiry, by interrupting the common order of facts, and lulls us into an ideal security, when we have not advanced a step towards so desirable an end. When the English shut themselves up during a pestilential season in the Mediterranean ports, they conclude that they have secured themselves, because when the season is passed, they meet their friends alive. Would it not be worth inquiring, by what means the higher class of natives have escaped for years, without any such cautions? And also, by what means some have fallen a sacrifice, who have fancied themselves safe, in consequence of their caution? On the subject of quarantine, on which so much time has been spent, and by the strict observance of which, every honest trader is so much injured, whilst every unprincipled smuggler is making his harvest, would it not be reasonable to ask, Why the Dutch escape, who use no quarantines? and how Malta should be afflicted, where the most rigid quarantine has been observ-

ed? I am aware it has been urged, that whenever the plague appears, it has always been found, that some infected person or goods, or some person from an infected port, has been discovered. Let us ask, If the same diligence were used at other times, would not the same discovery be made? In short, is it possible, in a commercial port, to preserve a strict quarantine?

The cautions relative to hereditary diseases, if less numerous, have not been better directed. On those moral cautions, which have been repeated by some of the best intentioned writers, I shall offer no remarks in this place. But, as I know only one instance in which Legislature has interfered, and as it is impossible that any future act can be more pointed or *cautious*, I shall take this opportunity of transcribing the passage, leaving it to the decision of others, whether any, or what effect has been produced by it, towards exterminating such diseases from the families of our sister kingdom.

* “ Morbo comitali, amentia, mania aut simili
 “ tabe, quæ facile in prolem transfunditur, laborantes,
 “ intereos ingenti facta indagine inventos, ne genus
 “ fœda contagione ab iis qui ex illis prognati forent læ-
 “ deretur, castraverunt, mulieres hujusmodi morborum
 “ quavis tabe leprave infectas procul a virorum consor-
 “ tio ablegaverunt. Quod si harum aliqua concepisse
 “ inveniebatur, simul cum fœtu nondum edito defor-
 “ diebatur viva.—Voraces, manducones supra quam
 “ erat humanum, helluonesque et perpetuæ ebrietati in-

* Scotorum Historiæ a prima Gentis Origine, cum aliarum et rerum et gentium illustratione non vulgari, Libri XIX. Hectore Boethio Deidonano Auctore. Parisiis, 1574, lib. i. p. 12.

“dulgentes aut addictos, ne tam fœda monstra in
 “patriæ dedecus superessent flumine mergentes, prius
 “quantum libuit et cibi et potus vorare ac ingurgitare
 “eis præbentes, miti supplicio exterminarunt.”

How severe soever these laws may seem, they may be easily justified by saying, that *one cannot be too cautious*; and perhaps, future ages may be of opinion, that they are not more cruel than the destruction of a year's harvest of the staple commodity of an island, the crowding pestilential subjects into a Lazaretto, and confining those who are in health by a *cordon* within the sphere of a pestilential atmosphere. It may be thought unreasonable to compare the institutions of the present day with the laws of a less enlightened period, but we shall see, that these cautions for the preservation of a sound progeny, were mixed with others, founded on sentiments which would do honor to any nation or age.

“In bellis autem gerendis aut privatis item inimiciis, nihil dolo, nihilque fraude agebant. Aperto Marte decernere ac vincere generosum ducentes. Turpe vero arbitrabantur inimicitias blandiloquio occultantes, per dolum postea nec opinantibus insultare, censentes id esse imbecillium, nec suis viribus confidentium. Simpliciter omnes synceritatemque ex æquo semper colebant.”*

NOTE 2, Page 7. — “Neither time, industry,
 expence, nor obloquy.”

To improve what Mr. Hunter has done must appear a bold undertaking. It may therefore be pro-

* Id. loco citato.

per to inform the general reader, that in the opinion of most medical men, the writings of that justly celebrated physiologist are often obscure, and that he concludes his *TREATISE* with some "remarks on diseases resembling Lues Venerea," acknowledging, that what he has "said, should be rather considered as hints for others to prosecute the Inquiry, than as a complete account of the subject."

My first attempt was to execute an obligation imposed on me by the last conversation I had with that great man. This was to defend him from a host of writers, who had opposed, I might almost say abused, without understanding him. To do this, it was only necessary to explain his meaning; a task very much facilitated, by pointing out the mistakes of his enemies. This naturally led me to the attempt of "prosecuting the Inquiry which he left for others." In doing this, all the information that could be collected, by conversation among the most experienced practitioners, by the access they allowed me to their respective hospitals and libraries, as well as from correspondence, was brought into aid. The fruits of my conversations enabled me, among other things, to announce the cow-pox to the Public two years before the ingenious discoverer. Every reader attached to physiological researches, will easily conceive, with how much reluctance I restrained myself from pursuing this inquiry, especially as it strongly confirmed one of Mr. Hunter's conjectural theories. But Mr. Cline, from whom I received my information, assured me, that Dr. Jenner was actively employed in some important experiments. It would, therefore, have been great injustice to have interfered, till the result of his observations were made public.

Various difficulties occurred in prosecuting the other inquiries. Conscious, therefore, how imperfect the work still remained, I concluded by inviting the communications of all my brethren, and particularly of those whose residence allowed them a full opportunity of investigating certain local diseases. My own industry, however, did not relax. Nothing that could be personally examined was afterward omitted. The leprosy of Madeira was supposed so highly contagious, that the physician who had the charge of the unhappy objects, had never ventured into the Lazaretto destined for their reception. In this place, I spent a considerable part of several days. Finding the insect, which was, by some, supposed to be the cause of the Itch, very common in the island, I determined to put the controversy on this subject at rest, by inserting two of the *acari* into my own hand. Not perfectly satisfied with any history of the African Yaws that could be met with, nor with the correspondence I had instituted with some West Indian practitioners, it was my resolution to repair to those islands, had not a case occurred to me in Madeira.

My researches into books were not confined to medical writers. Every publication that came in my way, particularly those accounts of voyages which mention the diseases of the countries, were carefully examined in those passages. By comparing them, I was convinced, that a disease said to have been carried from Europe to Otaheite, and to have depopulated those Islands, had never been known in any of them; and the subsequent report of Mr. Wilson,

Surgeon in his Majesty's Navy, who arrived under a conviction that the disease existed there in all its forms, has proved the truth of my suspicion.

On my return to England, to prepare my Second Edition, I was disappointed in finding that none of the northern practitioners had furnished me with any information on the subject of a morbid poison, known only in that kingdom, and hitherto imperfectly described. The only remedy was, to repair to Scotland, and spend as much time in the district, as might be necessary for a fair investigation of the disease: and this I made no scruple to undertake.

Furnished with such materials, and with some claim to maturer judgment by lapse of years and by greater experience, my Second Edition appeared on a larger scale, with an attempt to comprehend diseases which, though not strictly Morbid Poisons, are generally considered as contagious. Among these are the yellow fever and plague, neither of which appeared to me contagious. My opinions were principally founded on the facts and reasonings of those who, after a practical knowledge, held a different opinion. To engage, if possible, the public attention, a smaller tract was published, under the title of an "Inquiry into the Laws of different Epidemic Diseases." This was circulated with much industry and some expence, in order that the subject might be more carefully examined by all travellers, who are certainly as capable as medical men of forming an opinion concerning matters of fact. The contagious property of the yellow fever had been long disputed by those who had the most ample means of information, and the controversy was in such hands as must

gradually produce conviction. Such was not the case with the plague. As soon, therefore, as that disease was known to visit Malta, I expressed a wish to Sir JOSEPH BANKS, to be entrusted by Government with such a commission as would have enabled me to put my opinions to the proof, without danger to any one but myself, and such others as might chuse to volunteer with me. In this undertaking, we should have exposed ourselves to the effluvia and contact of all the subjects submitted to our charge, not in the manner Mr. White had done, the whole history of whose proceedings served to confirm the doctrine contained in the "Inquiry."

It is unnecessary to say more than, that in all the delays of office, and the reference to different boards, we had the learned President with us. The correspondence which passed on the occasion is preserved, and may be resumed, if ever the question meets with that fair investigation, to which, by its importance, it is justly entitled.

What has been said will be considered as a vindication against any charge of remissness in duty, not as a claim to any superior merit. No man scruples to plead his industry, especially if those whom he addresses, have no other means of knowing its extent. Industry, with some perspicuity of description, is all that can be inferred from the above account. It is hoped also, that the introduction of the word "obloquy" will not lead to the suspicion of petulance. Few men have better reason to be satisfied than the author. To be destined to an occupation which is followed with delight, and without being prevented from pur-

suing the more intricate inquiries of his favourite science, are enough of themselves to excite gratitude; but for a physician to be distinguished by the College, and to be answered by the Arbiter of Science in this kingdom, that he should feel flattered in seeing a physiological work which he had perused, dedicated to him, would prove a balm against discontent, if any cause for it existed.

There will, however, arise incidents so congenial to the wishes of the best of men, that any thing less than enthusiasm may excite surprize, which the more designing will readily direct in their own way, as long as it serves their purpose. Perhaps too, when it is feared that the public mind can only be reconciled by acclamation to what is really good, or when it is found necessary to meet invective by invective, the cool reasoner ought to be thankful, if he is only overlooked. Let me conclude, and in some measure, explain the cause of this digression, by subjoining a correspondence, which will show the uniformity of my opinion of Vaccination, and how generally the same is now admitted.

“ Copy of a printed Letter from Dr. Hervey, Registrar of the Royal College of Physicians, to Dr. Adams, Physician to the Small-Pox Hospital.

“ SIR,

*“ HIS Majesty has been graciously pleased,
“ in compliance with an address from the honourable
“ House of Commons, to direct his Royal College of
“ Physicians of London to inquire into the present*

“state of Vaccination in the United Kingdoms, to
 “report their observations and opinions upon that
 “practice, upon the evidence adduced in its support,
 “and upon the causes which have hitherto retarded
 “its general adoption.

“The College are now engaged in the investiga-
 “tion of these several propositions, and request you
 “to communicate to them the result of your experi-
 “ence and inquiries on the subject, that they may be
 “thereby assisted in making their report as perfect as
 “possible.

“I am, SIR,

“Your most obedient servant,

“JAMES HERVEY,

Registrar.

“By order of the Royal College of }
 “Physicians, Oct. 23, 1806.” }

“*Copy of a Letter from Dr. Joseph Adams to Dr.*
 “*Hervey, of the College of Physicians.*”

“November 17, 1806.

“SIR,

“I have been honoured with your circular,
 “expressing the wish of the Royal College of Physi-
 “cians to comply with his Majesty’s gracious com-
 “mand, relative to Cow-pox.

“The College are pleased to expect a communica-
 “tion on the three following points :

“1st, My own experience in Vaccination.

“2dly, The result of my inquiries.

“3dly, My opinion of the causes which have hi-
 “therto prevented its general adoption.

“ 1st, My own experience fully confirms all that
 “ *Dr. Jenner* promised in his Inquiry into the Causes
 “ and Effects of Cow-pox.

“ 2dly, I have made no digest of my inquiries, ex-
 “ cepting as they lead to experiment, which are con-
 “ sequently included in the former answer.

“ 3dly, Besides the prudent backwardness of most,
 “ in admitting novelties into practice, without ample
 “ proof of their utility, the causes which have pre-
 “ vented the general adoption of Vaccination, appear
 “ to me to have been principally the mistaken zeal of
 “ its friends. It could not be expected, that men
 “ who value themselves on their talents at investiga-
 “ tion, and feel conscious of their scrupulous adher-
 “ ence to truth, could patiently submit to be uncan-
 “ didly treated for a scepticism induced by events,
 “ however accidental. When their accuracy was
 “ questioned, whilst they disregarded the assertions
 “ of their accusers, they became diligent in collecting
 “ collateral evidence, and when their reasoning was
 “ ridiculed, instead of expressing only their doubts,
 “ they became parties in their own defence.

“ Another inconvenience has arisen from a too
 “ great forwardness at answering objections before
 “ they were sufficiently matured ; hence, when Variola
 “ appeared after Vaccination,* the event was either
 “ denied, or explained by so many minute causes as
 “ were sufficient to frighten the ignorant, disgust the

* In the year 1796, I gave it as my opinion, that small-pox had
 occurred twice to the same person; and in 1805, was the first to
 urge this argument in defence of Vaccination.

“ candid, and induce the prudent to avoid an experi-
 “ ment, the result of which was not sufficiently un-
 “ derstood.

“ A practice at one time represented as so simple,
 “ that the clergy and females were invited to under-
 “ take it, became at once so mysterious, that only a
 “ chosen few were said to *understand* Vaccination;
 “ every untoward event was imputed to ignorance be-
 “ tween the true and the spurious pustule, to taking
 “ matter at a too late period, and to other causes still
 “ less satisfactory.

“ Had these uncertainties really existed, they
 “ would have been sufficient objections against a
 “ practice, the object of which is to secure the sub-
 “ ject from a formidable disease, and from which he
 “ might be secured by another, certainly less desirable
 “ but well ascertained operation. But the truth is,
 “ that Vaccination is as simple as it was at first an-
 “ nounced; that the true character of its vesicle is
 “ more certain than the local effect of any other mor-
 “ bid poison; that it is impossible to confound it with
 “ a pustule of any kind; and that every difficulty
 “ might have been avoided, by requiring a correct
 “ register of the progress from the period of insertion
 “ to cicatrization, or for the most part of perfect
 “ scabbing.

“ I am, Sir,

“ Your obedient,

“ Humble Servant,

“ JOSEPH ADAMS.

“ To Dr. James Hervey,

“ &c. &c.”

The following is an Extract from the First Edition of my "Answers to the Objections against Vaccination," published for the Benefit of the Small-pox Hospital in 1805.

"However, if we were to admit, that some instances have occurred of small-pox after cow-pox, I shall show that this is really no objection to the practice. There are three ways in which this may happen. 1st. By an imperfect vaccination.

"2dly. By the constitution being under the influence of some other disease at the time of vaccination.*

"And lastly, By the person being liable to the small-pox twice.

"This last seems very strange to those who suppose, that there are rules which admit of no exceptions. But it is well-known, that some people never take the small-pox at all; and it is equally certain, that some few, happily very few, have it twice. I know some say, that this was never thought of till the cow-pock made its appearance; but the following quotation is from a book published before vaccination was practised.

"It is a law with most morbid poisons, that a constitution which has once gone through the action excited by them, is no longer susceptible of it. This is the case, with very few exceptions, in the small-pox; I say with very few exceptions, because some cases to the contrary, have been so well authenticated, that I am not sceptical enough to

* "See Observations on Morbid Poisons, published by Johnson, 8vo, 1795."

“doubt that the susceptibility may be so strong in
 “some particular constitutions, as to *admit of the dis-*
 “*ease a second time, after a certain period.*”

After this, follow cases published by the College of Physicians, of Small-pox by Inoculation, twice in the same person; and one from the Memoirs of the Medical Society, of the casual Small-pox twice severely in the same person, and in the second instance fatal.

Of the work from which the above is extracted, near ten thousand copies have been circulated among that class, who at one time seemed averse to Vaccination. This may be an apology for the stile in which it is written.

NOTE 3, Page 7. — “*Whilst Sir Joseph Banks, &c.*”

The little concerning hereditary diseases, which was contained in my paper on Elephantiasis, was derived from Mr. Hunter's Lectures, and may be found in his Evidence on the Trial of Donellan, for the murder of Sir Theodosius Boughton. Not aware that the former had never been printed, and that the more interesting events of the latter would easily eclipse a pathological remark, which, from its novelty, the learned judge acknowledged he did not clearly understand, I could not help feeling surprised at the manner in which the attention of the learned President seemed arrested, whilst perusing this part of the paper. The subject afterwards came frequently before us; and for most of the illustrations derived from the inferior animals, I am indebted to his conversations, and his reference to his own library.

NOTE 4, Page 8.—“ *Every technical expression is avoided.*”

Technical language should always be avoided, and is never necessary in medicine, excepting when parts are described anatomically, for which there are no English words, or the functions of which it would be necessary that the reader should know before he can understand the question. If these terms continue to be technical, it is not the fault of physicians, but of the public. It ought to be part of every gentleman's education to learn the structure of his own body. On all other occasions, such words are only a cover for ignorance or indolence. If a man cannot explain his terms, it is because he has no precise meaning. If he understands his subject, his wish to explain himself will always induce him to adopt language the most simple, because he will find it the most intelligible. It is very true, that this will be less pleasing to those who can be satisfied with such words as *scrofula*, *hereditary taint*, *eradication of disease from the constitution*, and many others, which, as the writer is too cautious to explain them, do not oblige the reader to form any combinations, nor consequently present any difficulties in his way.

NOTE 5, Page 9.—“ *No systematic performance on the subject.*”

Stahl is quoted by most authors who have noticed hereditary diseases, but the great variety of his works, or of the different editions, makes it difficult at all times to refer to the passages cited. Haller, in his *Elementa*, Vol. 7, Article *Similitudo Parentum*, gives us no reference but to page 497. The fact quoted,

I have found in Stahl's *Theoria Medica Vera*, published at Halle in 1737, page 377. In the Collection of Dissertations, published by Stahl in 1707, there are several passages which refer to hereditary diseases, and an Inaugural Dissertation, "*De Hereditaria Dispositione ad varios Affectus*," by Burchart. The title of this seemed to promise some information, but no systematic arrangement of the subject is to be collected from any part of the volume. If Stahl himself has written any regular Treatise on hereditary diseases, I have not been able to meet with it.

NOTE 6, Page 9.—"*Overlooked by every succeeding writer, who has incidentally touched on the subject.*"

M. Portal, like his predecessors, refers us to Hippocrates, Galen, Pliny, Fernelius, Ingrassias, Bailon, Lazarus Riverius, Mead, Boerhaave, Morgagni, Stahl, Lieutaud, Haller, Zeller, Van Swieten, Sennertus, Etmuller, Hoffman, and other great physicians. All the above, whom I have had opportunity to examine, only introduce the question as connected with different diseases, and most of them refer to the same authorities. Mercatus, who alone wrote a distinct Treatise on the subject, is overlooked by all. In a note, M. Portal mentions a work of great merit, by M. Forestier, *De morbis aut noxis puerorum in vitiatis depravatisque parentibus*. I regret much not being able to meet with this, and that M. Portal never quotes a passage from it. It is surprising, that Mercatus is never quoted by the industrious Haller, in his article "*Similitudo Parentum*." M. Portal's communication is very valuable, on account of the number of facts and references which it con-

tains ; and also, on account of the extreme candour which pervades the whole. But it is deficient in that cautious rejection of undefined terms, which distinguishes the true disciples of the Hunterian School.

He begins with attempting to trace the causes of hereditary diseases, to diseases *contracted by the parents before marriage*, or to events which happen to the mother during pregnancy, urging, after Ferrius, that the older people are when they have children, the more likely they are to have acquired imbecility or disease, and to transmit the same to their children, from whom they may become hereditary ; he concludes this part, by ascribing some of these effects to wet-nurses, admitting, at the same time, the impossibility of a satisfactory explanation, and urging, with much propriety, that it cannot be necessary always, to know the cause before we admit the effect.

Several paragraphs follow, on the external peculiarities of form, which are hereditary in different families ; this leads to some remarks on organic affections, particularly such as are connected with an unnatural form of the heart, discovered after death. Mention is made of some other hereditary, and one family complaint ; which last, we are informed, occurred to a brother and four sisters, all at about the same age. After enumerating a few other facts, M. Portal asks, “ Do these hereditary diseases arise from various causes, or are they to be ascribed, in most, to one alone ? ” This leads him to some remarks on tumours in different parts of the body, and to rickets, with other diseases of the bones ; from the frequency of which, and their connection with *scrofula*, he sug-

gests the probability, that all, or most hereditary diseases, may be ascribed to that source.

In the instances of epilepsy, mania, and apoplexy, it is admitted, that one and the same cause cannot, with propriety, be suspected of producing effects so various. We are, however, soon led again to scrofula, which is kept in view as a cause throughout the remaining part of the paper.

At length, the author conceives it right to ask, What is the nature of the scrofulous taint which occasions so many and various hereditary or family diseases? After a variety of surmises, he concludes, by deriving the whole from the *lues venerea*, and conceives, by the application of proper remedies, that the venereal taint may be destroyed, and the hereditary or family disease with it.

I have given this abstract of M. Portal's Treatise, as the only one since Mercatus's, which has come to my hand. It is contained in the last Vol. of the French National Institute, and a Translation of it in the 21st Vol. of the Medical and Physical Journal. Several parts of it will come under consideration in the subsequent notes; I shall, therefore, say but little in this place.

The first chain of causes assigned, are diseases contracted by the parents before marriage; children produced by parents at an advanced period of life; and events which occur to women during pregnancy.

The first and second of these will be considered in their place. On the last, I shall here offer a few words.

At one time, the apprehension from such a cause was carried to an absurd length; and perhaps, it may

be so at present, by one class of people. But I am not ashamed of saying, that, in my opinion, the question has been too generally consigned to ridicule by medical men. There is, however, no reason to believe, that any such effects are transmitted beyond the individual in *utero*. I shall, therefore, offer only one remark on this occasion, viz. that women who are habitual drunkards, generally produce immature or idiot children. Let it not be understood by this, that such is often the cause of those unhappy events. Female ebriety, especially at the age of conception, is very rare; nor does it often happen, that there are many idiot children from the same parents. Even when all the offspring are in this unfortunate state, it is by no means a proof, that the failings of the mother has been in any degree the cause.

A poor woman has three idiot children, to whose comfort she is not less attentive, than many are to their quadruped pets. By those who are unacquainted with her, it is often asked, Why she does not send them to the parish work-house, as it must be a matter of indifference to such objects, who may have the charge of them?—Her answer is, “It is painful enough for me to be constantly watching them, therefore I cannot expect it of a stranger!” Can such a mother be suspected of causing such a calamity, by such a failing? and would not such a failing unfit her for so praise-worthy a charge?

NOTE 7, Page 13.—“*The first only can with propriety be called hereditary or family diseases.*”

This involves a most important question, namely, the transmission of *contagious* diseases to posterity.

That a child may be born with the small-pox, was first shown by Mr. Hunter, in a paper published in the Philosophical Transactions. The belief, that the venereal disease may be transmitted to posterity, was at one time almost universal, and is still maintained by some writers, in most formidable language. Whoever considers the intimate connection between the mother and child, must at once admit the probability that a disease may be conveyed from one to the other. But, in these instances, we ought rather to speak of communication by contagion, than transmission by inheritance; and in all such cases, the various morbid poisons will be governed, in the time of their appearance, by their customary laws. Such we find the case with every history of connate small-pox. The appearance on the child has been at, or very nearly, the same period, after receiving the infection, as we might have calculated in the ordinary way.

I am aware, that in syphilis, no limits are admitted by some to the period at which that disease may appear on the offspring of a person once infected. It has even been asserted, that the father having been cured any number of years before marriage, is no security. These opinions are gradually dying away, since Mr. Hunter taught us the true pathology of this disease. It is, however, still maintained, by those who have not made themselves masters of Mr. Hunter's doctrine, and consequently, very generally on the continent, where it cannot be expected that he should be so well understood. Having, in another place, very fully discussed this subject, I shall only remark here, that in a populous and wealthy island, in which I was for many years consulting physician,

cases out of number of this kind were brought to me, and the result in all served to confirm me in Mr. Hunter's doctrine.

Dr. Ferguson has some very ingenious remarks on this subject, at the conclusion of his paper in the last volume of the *Medico-Chirurgical Transactions*. After this just tribute to his merits, I trust the author will not be offended, if I regret that he did not apply Mr. Hunter's mode of reasoning to the cases in the preceding part of his valuable communication.

Diseases from nurses are of two kinds. It sometimes happens, that the milk of a healthy nurse, even of the mother, will so far disagree with a child, as to induce a cutaneous disease, which is readily cured by a change of nurse. But, more commonly, a child, even apparently healthy, will produce ulceration on the nipples of its nurse. At other times, a cutaneous disease about the nurse, will be communicated to the child, and assume, on some occasions, a form different from that on the nurse. All this will be obvious to the senses. But the stories of permanent future injury to the distant parts of the body, by what is taken into the stomach, to say the best of them, have not hitherto furnished such an uniformity of facts, as can be reduced to pathological reasoning.

The stomach is a very sensitive organ, and soon shows us the effect of any thing injurious received into it. If the skin, as is often the case, is made to sympathize with it, such an effect only continues as long as the cause remains.

NOTE

NOTE 8, Page 17.—“ *It may be asked, Why this attempt at discrimination, where the differences seem almost to meet.*”

The want of accurate distinctions is the great impediment to the progress of knowledge; and in disease, is not only the source of erroneous practice, but of unfounded terror or dangerous security. By confounding hereditary with family diseases, we excite an unnecessary apprehension in the rising generation; I shall quote the following passage from M. Portal, to show the manner in which he has confused this part of the subject. “ To these hereditary diseases, may we not add cancer and cataract, with deafness and dumbness from the birth? Morgagni saw three sisters who were dumb from their infancy. Other authors have mentioned similar instances; and many such have come under our own observation.” In the notes to this passage, we are referred to Morgagni, Woolhouse, and other authorities mentioned also by Haller. In turning to all these, it will be found, that the disease was not derived from the parents, nor is there any authority that it was transmitted to the offspring. Woolhouse expressly says, that the parents were free from the complaint; Morgagni makes the same remark. Haller seems aware, that these diseases were not necessarily hereditary, but he confuses the subject greatly, by confounding connate diseases or privations with peculiarities of form or structure, and even with hereditary dispositions and artificial mutilations. Hence, after remarking, with much propriety, “ *Cæci certe fere bene videntes pueros generant,*” he adds, “ *Cum matre sana leprosus sanos generat.*”

"Catella cui lien excisus erat peperit catellas liene
"præditos, &c."

Mr. Stevenson, in his valuable "Treatise on Cataract," speaks of hereditary disposition as among the causes. But such is his accuracy in all the cases given as his own, that it is easy to see, the congenital cataracts were all confined to a single generation, as that gentleman has since explained to me. Let me add, that some few histories of this kind, which I have been able to trace, confirm the above law.

I shall conclude this part of the subject, by transcribing the result of a correspondence I have been favoured with, through the kindness and liberality of the officers of one of the most interesting charities with which this metropolis abounds. "Of 148 scholars upon the foundation of this Institution, one is of a family where there are five deaf and dumb, (himself included); one, where there are 4; eleven, where there are 3; and nineteen, where there are 2. Of the scholars, 57 are girls, and the rest boys; none of them of deaf and dumb parents." At this time there are 72 candidates; in thirteen of these, the privation extends to brothers and sisters; but as no mention is made of a similar misfortune in the parents, we may presume, it does not exist in any of them, since the mention of such an event might have been urged as a higher claim to charity.

The gentleman who superintends the manufactories, and who consequently has the best opportunity of tracing the subsequent history of his scholars, informs me, that some of them are married and have children, all of whom are perfect in the organ of hear-

ing. One instance has occurred, in which both parents were born deaf, yet their children hear.*

The uniformity of these events will, I trust, be sufficient to reduce the whole into a law; but I cannot help remarking, that we should be extremely cautious, in drawing too close analogies between man and the less complicated animals. I have been told, from an authority which, though not strictly philosophical, I cannot question, that there is a race of white cats, called the Persian breed, in which deafness is hereditary, though not universally so. How far it may depend on their transplantation to this climate, is a question which will occur hereafter.

NOTE 9, Page 18. — “*Connate or congenital diseases are rarely hereditary, which is the more remarkable, because peculiarities of form in less important parts are often hereditary.*”

M. Portal, among many other instances of the transmission of family features, mentions the following, which has since been confirmed by Dr. Pritchard in his valuable “*Researches into the Physical History of Man.*” “*Dr. Gregory, one of our old pupils, now the distinguished Professor of the Theory and Practice of Medicine at Edinburgh, related to his numerous students, in order to convince them of the resemblance between children and parents, both with respect to external and internal structure, that having been once called to a distant part of Scotland, to visit a rich nobleman, he discovered in the configu-*

* Correspondence with Dr. Wilson and Mr. Sandwich. See also the printed List of Candidates for admission into the Asylum for Deaf and Dumb.

ration of his nose, an exact resemblance to that of the Grand Chancellor of Scotland, in the reign of Charles I. as represented in his portraits. On taking a walk through the village after dinner, the Doctor recognized the same nose in several individuals, among the country people; and the nobleman's steward, who accompanied him, informed him that all the persons he had seen were descended from the bastards of the Grand Chancellor. How many similar examples might we not adduce, if we were to pay attention to the subject?"

These family likenesses are matters of such notoriety, that they need no confirmation, but peculiarities bordering on monstrosity, in unimportant parts, come nearer to disease, or organic privation; yet the former, we shall see, are hereditary, though the latter are not. Not to mention what has been said by Aristotle and Pliny on this subject, we have later authorities for the hereditary transmission of supernumerary fingers and toes. Dr. Pritchard has collected several well ascertained facts on the subject. Haller quotes from Stahl the hereditary transmission of a web between the toes. I know myself an instance of a gentleman, who inherits a similar membrane from his father. Sometime ago, an Irish mendicant scholar introduced himself with a Latin letter, describing a deformity in his hand, which he showed me, and said was an exact resemblance of his father's. I admit these transmissions are uncommon, and I believe, rarely extend, like the mere peculiarities of features, through many generations; still, however, they ought to be distinguished from connate diseases or priva-

tions, which always cease with those in whom they first appear.

NOTE 10, Page 21.—“ *When the hereditary or family susceptibility is such, that the disease is induced without any external cause, we can have little hopes of preventing it; and if the disease has arisen during the changes about the age of puberty, we are to expect a cure more from a proper direction of the effects of Nature during that period, than from remedies which may be useful in the same disease, when excited by external causes, or induced at a more advanced age.*”

We are too apt to confound, in our prognostic, diseases which occur before puberty, with those which are excited by the changes induced in the constitution about that age. The late Dr. Heberden, in the invaluable legacy he has left us, very justly remarks, “ It has been an old observation among physicians, that epilepsies beginning in childhood, often terminate about the age of puberty, which has by no means been verified by any experience which has fallen in my way.” Nothing can be more just than this remark. The error seems to me to have arisen from confounding epilepsies which commenced in infancy, with those which commence about the age of puberty. The former remain unabated, and frequently increase by the changes which take place in the constitution; the latter, on the contrary, usually cease as the change is completed. The same observations are applicable to many other diseases, indeed, to most of those anomalous complaints which occur at that critical age; and which, though they sometimes appear very formidable, for the most part, cease

within the space of a twelvemonth. We shall hereafter have occasion to take notice of diseases arising at the more advanced climacterics.

NOTE 11, Page 22.—“ *This remark is still more applicable to that kind of consumption, &c.* ”

This passage at once shows the impropriety of expecting benefit from the same remedies in all diseases of the same organs, and accounts for the contradictory reports of Dr. Beddoes, who, though he at one time, it must be admitted, trusted to solitary cases and fallacious amendments, was always candid enough to acknowledge his disappointments. These recantations were, however, often attended with the announcing of a new remedy, which was, in its turn, abandoned. It is true, to his persevering diligence and readiness of communication, we are indebted for a spirit of inquiry among us; nor can we fail to regret, that he was so soon cut off after he had begun with such earnestness to correct his theories, by a due attention to practical morbid anatomy. On this occasion too, I cannot help remarking the impropriety of sending consumptive patients of every description to warm countries, or of undervaluing the efficacy of such change of climate, because it is not universally successful. The consumption I have described at page 21, is indigenous in every climate, and fatal in all; and, as far as I have been able to judge, runs through its courses with greater rapidity, in proportion to the mildness and purity of the climate. See a paper in the Med. and Phys. Journal, Vol. v. page 307.—1801.

NOTE 12, Page 25.—“ *Where the predisposition exists, it is of no consequence to the future progeny, whether the disease has been excited or not.*”

If the reader is willing to admit so fair an inference from facts which are daily passing before him, and from what I have stated relative to gout, he will see the impropriety of that passage in Fernelius, so often quoted by subsequent writers, “ *Senes et validitudinarii imbecilles filios vitiosa constitutione gignunt.*” What a gloomy prospect of life would such an opinion afford us! what moral restraints would it not impose! how much beyond what can be expected in the human constitution, and how unnecessary! Because weak children are sometimes the offspring of aged parents, we are apt hastily to forget how often the same event occurs to parents in the vigour of life. The instances are innumerable, in which the younger branch of a family has revived its splendor, which had been decaying for a succession of ages. The late Mr. Pitt was the youngest son, born when his illustrious father was 51 years of age. I think I have heard, that the great and venerable Pratt was the son of a much more aged parent.

NOTE 13, Page 26.—“ *Madness as well as gout, is never hereditary but in susceptibility; and those who have paid the greatest attention to the subject, must admit the two degrees of susceptibility.*”

Cases of very early insanity are recorded, but they appear to me, rather to come under the description of mental imbecility. This leads to one of the most difficult questions in physics; and the more so, as it is

scarcely possible to divest it of metaphysics ; I mean a definition of madness. The great difficulty, however, in this, as in every other pathological inquiry, seems to arise from our attempting too much. The shades of madness are so various, that few of us can be said to be at all times free from it ; and of this we are so sensible, that we perpetually accuse others, and even ourselves, of acting under the impression of madness. We must also admit, that there are few madmen who do not show a soundness of intellect on some occasions. That madness consists in reasoning well on false premises, is a definition sanctioned by high authority ; but it seems to me, that in order to apply this doctrine, we must be previously acquainted with the character and external circumstances of the man. Fox and Pitt both reasoned well, and on the same premises, yet we accuse neither of madness, though each drew a different inference. If interest should be suspected to have warped either, the same cannot be thought of Clarke and Leibnitz.

If I were to venture a definition of Madness, I should call it a reverie from which a person cannot be recovered. It will then be asked, What is a reverie ? To this, the general answer is, A waking dream ! What then is a dream ? If I must give an answer, I should say, That state of the body and mind, in which imagination passes for reality, the senses being at that time so torpid, as to require an unusually strong stimulus to produce any impression. The mind, during sleep, seems to have no power of arrangement or combination ; but from its activity, recalls certain impressions, in proportion as the torpid state of the animal is less complete. In a reverie, the mind is so entirely engaged in a single subject, that external objects

exposed to the organs of the senses, produce no impression whatever; or if any, those impressions are immediately associated with the subject in which the mind is engaged. In either of these conditions, a strong stimulus applied to the senses, brings the person to the clear perception of all the objects around him; the dream is found to have been a fallacy, and the train of thoughts during the reverie being now disturbed, external objects produce their full impression.

In madness, the organs of the senses still retain their capacity for impression, but no stimulus, however powerful, is sufficient to lessen the illusion under which the mind labours.

After all, it may be difficult to distinguish such a state of mind from misguided enthusiasm; if then, we require a definition which will include every shade, there seems no objection to the proposition offered above, That madness is a reverie from which a person cannot be recovered.

NOTE 14, Page 27.—“*For this purpose, the hereditary peculiarity should always be kept in view, in the direction of the early studies, in the subsequent employment, &c.*”

This attention should not be confined to those in whom an hereditary susceptibility is suspected. For, as it has been frequently hinted, wherever disease exists, there must have been susceptibility to that disease, whether it existed in the parents or not: the same regard, therefore, is necessary in the management of all young subjects, in whom we see strong

marks of character at an early period of life. The dangerous age with such is somewhat beyond that of puberty, when they first find themselves exposed to the busy world; and from the attention they receive, at that interesting age, feel as if the eyes of all their acquaintance were directed towards them. In proportion to the delicacy of their feelings, and often to the strictness of their education, they become more sensibly alive to every impression. Perhaps this may be entering on the subject of education, which I shall leave to others. Those, however, who have paid the best attention to it, must have remarked, that though one general system may be sufficient, as there is a general similarity in the human character; yet the plan should be varied, wherever we see any striking peculiarities in the progress of intellect, or the impulse of passion. The variety of character is so great, that it would be impossible to prescribe rules for all; but in our endeavours to repress forwardness, or give courage to timidity, we shall gain no permanent advantage, without a strict adherence to truth.

It will be a vain attempt to undervalue the attainments of the early genius, or to over-rate the proficiency of the dull: each will be sensible to his own standard; and the only mode of checking the one, or encouraging the other, must be to remind each, that there is the same variety in the period at which the mind expands, as in that in which the growth of the body increases. Emulation, so much talked of, excepting where we can measure the capacities of the individuals, or have reason to suspect indolence in either, should rather be repressed than encouraged, as it is

more frequently the parent of bad passions than of amiable affections.

But it is the nearer approaches to perfect manhood that we have most to apprehend. The character, as well as the constitution, is then assuming a more permanent form, and must be watched, in both sexes, with a degree of delicacy which cannot be defined, as it depends so much on the variety of character and the influence of early impressions.

NOTE 15, Page 28.—“ *The more advanced climacterics in both sexes.*”

Diseases excited during the changes about the age of puberty are, for the most part, temporary. I have known even hereditary madness arise from this cause, and cease as the change was completed, without returning for a long series of years; probably, the whole of life. But in the succeeding climacteric, the completion of manhood, the access of disease is usually attended with more permanent consequences. Madness, the most incurable, and with the fewest lucid intervals, sometimes originates at that age. Mr. Haslam has some very ingenious remarks on this subject.*

I have not sufficient experience, to say whether madness occurring at this age, if attended to as soon as suspected, could be cured. But, by the success I have met with in the early stages of epilepsy, under similar circumstances, I cannot help again urging the importance of watching the slightest alteration of character at that critical age.

* Treatise on Madness, p. 64 and 208.

NOTE 16, Page 29.—“ *Before Dr. Heberden had given a name to that species of Asthma.*”

Angina Pectoris, for which I know no better English name than spasmodic asthma, is the disease which afflicted Mr. Hunter the last twenty years of his life; and, as he himself remarks, arises from a variety of causes. It was first distinguished from other forms of asthma, in a paper published by Dr. Heberden in the 22d vol. of the Medical Transactions, in the year 1772. The author speaks of it as not extremely rare, though he does not recollect the mention of it by any other medical writer. I suspect, however, that it was noticed by the artless, though expressive term, “rising of the lights;” a term still retained in the Bills of Mortality, though no numbers have lately been affixed to it. Is it not extraordinary, that in the present state of London, so little attention is paid to this important branch of police? In the “Inquiry” before alluded to, were inserted some proposals for reforming these registers. With this work was circulated a specimen of our weekly Bills in their present state, and the following address:

“ Without correct Registers, every attempt at preserving the public or individuals from EPIDEMIC DISEASES must be formed on insufficient data. It has even been suspected, that the means adopted have, in some places, proved worse than unequal to the end; that subjects in previous health, sent from ship-board to Lazarettos, have perished by despondency and unwholesome air.

“ A sense of duty, strengthened by the suggestions of a character distinguished for science, urbanity, and political knowledge, has induced the

“ author to collect such particulars as come within the
 “ province of a physician, concerning diseases usually
 “ considered contagious. These INQUIRIES have
 “ been prosecuted among documents, which, however
 “ uncertain, are all that the utmost industry could
 “ procure. They are sufficient to show, that without
 “ more authentic records and a general co-operation,
 “ every future, like every preceding alarm, must be-
 “ come a subject of terror and controversy instead of
 “ well-directed caution.

“ A weekly bill of mortality for London is enclosed
 “ as a specimen. It differs from the others only in
 “ the numbers annexed to each article. They all a-
 “ gree, among other absurdities, in distinguishing
 “ scarlet fever from scarlatina : and though the mor-
 “ tality from that disease has been for some years past
 “ so frequent and so notorious, no number is ever af-
 “ fixed to either article. Such are the records fur-
 “ nished by the largest and wealthiest city in Europe !

“ The ‘ Inquiry’ is submitted to the consideration
 “ of those who have the means of acquiring political
 “ knowledge on these subjects. If, on perusal, more
 “ *medical* information should be expected, the author
 “ will endeavour to improve every hint with which he
 “ is honoured. At the same time, it cannot be neces-
 “ sary to express his wish, that Gentlemen of suffi-
 “ cient leisure, and some influence, would turn their
 “ attention to securing such registers as may form the
 “ basis of future, more extended, and more accurate
 “ Investigations.”

These were presented to many distinguished pub-
 lic characters, some of them in office and others at
 leisure ; many were waited on personally, but the re-

gisters remain as before, excepting that scarlatina and scarlet fever are now both omitted, which, if there should be no other documents, may lead future ages to suppose, that scarlet fever was suspended for a certain number of years in this city. Mr. Rose has since introduced his Register bill, which I doubt not may be very useful for statistical purposes, and perhaps, may gradually lead to a discrimination of the various diseases. Should this ever be accomplished, it would prove, by extending over the whole kingdom, an important source of improvement to our medical knowledge. But to return to angina pectoris, as a climacteric disease.

Mr. Hunter was 45 years old when he was first attacked; Dr. Heberden remarks, that most of the cases he saw, were above 50 years of age. The case which makes the first article in the 3d vol. of Medical Transactions, is related by the patient himself. He informs Dr. Heberden, that he is 52 years of age, and as far as he can recollect, had been troubled with the complaint five or six years; probably, some years might have elapsed, before Dr. Heberden was consulted in the other cases. If so, it will appear that the age mentioned in the text is the most frequent for the first attacks of angina pectoris. In Mr. Hunter, the first attack was preceded by acute symptoms, and I have suspected the same in other cases which have fallen under my own observation. Though most people pass this critical period of life without any alarming symptoms in the constitution, yet it ought always to be attended to, and most of all, where there is reason to suspect any family or hereditary predisposition. Sometimes, the change is ushered in by a tedi-

ous hectic, which will continue, with few intermissions, for more than a twelvemonth, during which the friends of the patient will feel serious apprehensions for the event. On a sudden, symptoms of recovery appear, and advance with such rapidity, that the person is congratulated by his friends on having taken a new lease of his life, an expression which, though figurative, is almost admissible, on account of the increased health and vigour which often attend the completion of this change in the constitution.

NOTE 17, Page 33.—“ *By these means a race is gradually reared with constitutions best calculated for the climate: a law which, I suspect, has been too much overlooked, in our inquiries after the causes of the more marked varieties in the human species.*”

The last vol. of the Philosophical Transactions contains a very full and ingenious dissertation on this subject, by Dr. Wright. It cannot be necessary, if the reader is satisfied with the attention that has been paid to the work before him, to hint, that though Dr. Wright's paper appeared earlier than the present publication; yet, that this part of the question had not been overlooked by the author. It is well known, that the influence of the sun is not sufficient to produce a permanent change in the complexion of the human integuments; yet, it must be admitted, that that the shining black skin is best calculated to repel radiant heat, and the woolly hair to protect the contents of the skull from a vertical sun. There is not, however, a sufficient uniformity in these peculiarities, according to heat or equatorial situation; but in all the inhabitants of warm climates, we find a capacity to

generate cold, greater than in the natives of colder regions or their immediate descendants; nor does this arise, as once suspected, from mere evaporation by perspiration, for the skin of the native of the South is dryer than that of the North under the same temperature.

I have somewhere met with a remark on the capacity of the Greenlanders to generate heat. The author took his observation from the suffocating heat he experienced in one of their chapels. These properties may be imputed to habit, but I very much suspect, that they arise more from peculiarities of constitution, increased and perpetuated by the intermarriage of couples who, through this constitutional peculiarity, were enabled to survive those, that from the want of it became victims to the climate. In a conversation I had with Sir J. Banks, he reminded me, that in their well known tour towards the South Pole, all were able to resist the intense cold, excepting two negroes, the only black men of their party. We find in England, individuals, and even families, impatient of cold, and others equally affected by heat. This variety we might expect, from the inconstancy of our climate, and our artificial mode of living; by which, provision is made against each. But disease, where the cause is constantly present, is not so easily escaped, as the inconvenience of temporary sensation. It has been thought, that habit might be sufficient to enable the constitution to withstand change of climate; an experiment was tried, by removing a regiment to the West Indies who had passed a summer in Madeira, but the mortality was as great as in troops removed immediately from Great Britain.

We are not to suppose, that the inhabitants of the South are entirely free from those fevers so destructive to emigrants from the North. The negroes in the West Indies, the natives of Africa and of the East Indies, feel the effects of periodical miasmata, as some inhabitants of colder regions suffer by scrofula; but neither of them in any degree proportionate to what is felt by emigrants from one climate to another. Having mentioned the word Scrofula, after professing to avoid all technical expressions, I might perhaps excuse myself, by saying, that the word is sufficiently vernacularized; but unless it were as well understood, this would scarcely be a sufficient apology. The learned may require an etymology; this would be extremely difficult, in a word, the orthography of which is not well ascertained, some spelling it with the f, and others with the ph. The former derive the term from *Scrofa*, the Latin word for a sow that has frequently littered. Even here there is a doubt, concerning the application of the etymology, some ascribing it to the frequency with which pigs are affected, and others to the multiplication of certain tumors, similar to a large litter of pigs. Most authors say, that it is a disease of the glands of a particular species; yet, parts are often said to be affected by it, in which none of these glands are discovered, as the bones, the lungs and membranes of the eyes and nose. It is said to be hereditary, but is only so in predisposition, always requiring some exciting cause. These causes are all of them such as produce a disposition to local disease, and lessen the restorative powers of the constitution or of the diseased part. The most gene-

ral of these is cold. Hence it has been remarked, that among the inhabitants of the colder parts of our island, there are few families who are not scrofulous; but this is only saying, that the exciting cause is constant among them, that therefore no individual of any family can entirely escape, who has the slightest predisposition to it. The truth is, that the inhabitants of warm climates are much more predisposed to scrofula, as is proved by the effect produced on them when exposed to the cause. Negroes in cold climates rarely escape; and the children sent by their parents from the South for education, often suffer the effects of a disease never before seen by the family.

Cold, however, is not the only exciting cause. Poor diet, bad clothing, and all the other attendants of poverty, or privations from any other cause in the most favourable climate, will produce a similar effect, but less aggravated than when increased by the continual operation of cold. The late Dr. Heberden, whom I so often mention, and always with pleasure, speaks of the unwholesomeness of diet or situation as an exciting cause. The inhabitants of Rheims are said to have been very much afflicted with scrofula, when they used no other water but from their wells; and to have been relieved by the introduction of the water of a neighbouring river. Is it not highly probable, that the meliorated condition of the inhabitants which enabled them to afford the expence of such aqueducts, might also, by proving the means of better diet, clothing, and lodging, have contributed to their exemption from scrofula? Every community, as it is poor, is subject to cutaneous and other local diseases, which are ill-conditioned and inveterate, in proportion

as the means of relief are less in their power. It is universally admitted in Great Britain, that the inhabitants are more free from these complaints, as they are become richer. But the uncertainty of our climate will always expose those who have a strong susceptibility, because we never can be sufficiently prepared for changes of temperature, like the inhabitants of more settled, though colder regions.

Scrofula is very well described by the common expression of bad flesh to heal, so that any one may become scrofulous by a low state of health, induced from any cause; but some constitutions, in every state of health, shew a less aptitude for healing wounds than others. If, in these subjects, inflammation is excited in any part, there will be always danger of consequent abscess, and the healing process will be slow. In young subjects, the glands are the most liable to inflammation; probably, on account of their increased action at the period of growth, in order to model the form to its various changes. Hence, the glands of the neck, by their exposure to cold, are the most liable to become scrofulous. Though the consequent scars, from their unsightliness, are usually considered as the strongest proofs of a scrofulous constitution; yet, as the causes which induced the inflammation of those glands have ceased, there is no reason to apprehend any subsequent symptoms, without the access of some external cause. White swellings, and a variety of other complaints, are very properly enumerated as scrofulous, yet they more frequently appear in those who have escaped the early symptoms. The terror of scrofula is much increased by its sometimes affecting the lungs; but the most frequent

form of consumption, we have already seen, is neither hereditary, nor connected with scrofula. In the same manner, the delicate skin and high complexion are often described as marks of a scrofulous constitution; but the fallacy of this is proved, by the frequency with which the disease attacks emigrants from the southern regions. The high complexion is, in this country, the usual attendant on the family consumption, and marks an irritability very easily affected by external causes; it differs, however, from scrofula, inasmuch as the ulcers rarely become indolent.

On the whole, then, I should define scrofula to be that constitution, in which local disease is excited by the slightest causes, and in which the restorative power is the most feeble: admitting that such a constitutional predisposition, like every other, is often hereditary; but that the disease itself may, for the most part, be prevented or cured, by avoiding the exciting cause.

NOTE 18, Page 33 and 34.—“*Sir John Seabright and Colonel Humphreys.*”

In the conclusions drawn from the philosophical experiments of these gentlemen, we may remark, that the progress of Nature is interrupted in both, though most in those of Colonel Humphreys: in both, the animals were prevented from ranging at large. There is a prevailing opinion, that the families in many animals degenerate by breeding *in and in*, even when the males are selected by art. I am not sufficiently acquainted with comparative physiology, to offer any opinion on this subject. From the inquiries I have made of those who have attended to

the breed of dogs, such would appear to be the case in that race. I have even heard it asserted, that the females, when in heat, show a preference in the choice of their mates; but many and accurate experiments are necessary to ascertain these questions.

A very attentive practical shepherd, with whom I had frequent opportunities of conversing, informed me, that though he selected the best rams, yet he could not always depend on the issue. Some, after a succession of good lambs for two or three years, would, without any apparent changes in themselves, become the parents of an inferior offspring, insomuch that he thought it necessary to part with them immediately, in order to prevent the deterioration of his flock. It is right that this subject should be well understood, and I sincerely hope we shall, by degrees, receive satisfactory information from philosophical and practical breeders. But after all, we must be extremely cautious, how we apply to one race of animals, the laws we have detected in another. We should also recollect, that in a state of Nature, the law of the strongest must prove a constant check against the deterioration of the race in the less complicated animals; and that, under the protection of man, they will be artificially coupled, whilst the progress of refinement in himself will necessarily tend, by a variety of means, to the same end.

NOTE 19, Page 34. — “ *Many endemic peculiarities found in certain sequestered districts, which have been imputed to the water and other localities.*”

The prevalence of certain diseases in particular districts, has always been an object of inquiry. Hip-

pocrates ascribes many of them to the water and air of the places. This opinion has prevailed ever since his time. The improvements of modern chemistry, by a more accurate analysis of these waters, have taught us the insufficiency of many of these opinions; but we remain as much as ever in doubt, as to the real causes. The prevalence of calculous complaints in some provinces of France, was a matter of inquiry among the learned societies in that kingdom; and at different times, imputed to the wine, the beer, and the water. A comparison between the different districts evinced the fallacy of all these conclusions. That calculous, as well as most other complaints, are hereditary, I shall offer one satisfactory proof. Mr. Ferreira, an ingenious surgeon from Sicily, did me the honor to attend a course of my lectures. After having attentively listened to my remarks on hereditary diseases, he asked me, Why I had not included calculus among them? This led to a conversation, during which, I discovered the great advantage he possessed, in tracing these hereditary properties in the human race, from the great caution with which the Sicilian nobility preserve the purity of their blood, by continual intermarriages, each in their own family. This is carried so far, that one family in which elephantiasis is hereditary, disdains an alliance with any other, on account of their own elevated rank.

I cannot help suspecting, that it will hereafter be found, that goître and cretinism are endemial in certain districts, from no other cause than this hereditary property. M. Gautier, who has written a long account of goître, assures us, that it is not hereditary; yet, he afterwards adds, "the first circumstance no-

ticed, is a weak constitution derived from parents, which may contribute to give a predisposition to the disease." A general opinion prevails, that goître is principally confined to mountainous situations; yet no one pretends to say, that every mountainous situation produces goître.

Dr. Barton, Professor of Materia Medica, &c. in Pennsylvania, enumerates the various causes assigned for this disease. These are principally impregnation of the waters with calcareous and other matters, exposure to cold and drinking cold water, insects inhabiting the water, unwholesome food, and a residence in confined vallies. Dr. Barton easily shews the insufficiency of such causes, and concludes, with imputing the disease to those exhalations of marsh miasmata which induce the intermittent fever. But surely, there is the same want of uniformity between cause and effect in this as in all the other instances. Though the Professor has no view to any hereditary properties, yet he mentions a family, consisting of father, mother, and four or five children, all afflicted with the goître; and in his description of the places, in which the disease is found, seems to speak of it in one part, as confined to the Indian families, and in another, in which it is wholly restricted to the Canadians; meaning, I presume, the Colonists. In that part of Kent which borders on Sussex, and also in the adjoining parts of the latter county, the swelled throat is extremely common, and rarely confined to a single person in the same family. Whether there could be traced any alliance in these different families, it would be very difficult to ascertain, as the disease is principally confined to the females, who change their names

on marriage, and also to the lower class, whose family connections are not so easily traced. I admit there are difficulties in the way, if, as is said by some, the inferior animals are sometimes affected; I have not, however, heard that such is the case in Derbyshire, or any other part of England. At all events, we must admit, that if the disease is excited by any local causes, the predisposition to such excitement is hereditary.

I have never been able to learn, that ideotcy is associated with the swelled throat, any where but in the Alps; and some authors, who have given us an account of the diseases of those parts, assure us, that the union of the two is by no means universal: we cannot suppose that one cause should produce two such effects, and it is well ascertained, that ideotcy is hereditary in all its stages. Of this, Haller gives a striking instance from his own knowledge, "*Ex duabus patriciis sororibus ob divitias maritos nactis cum tamen fatuis essent proximæ, novimus in nobilissimas gentes nunc a seculo retro ejus morbi manasse seminia ut etiam in quarta generatione quintave omnium posterorum aliqui fatui supersint.*"* — May we not then impute its general prevalence in that secluded spot, to the accidental settlement of a family in which it was hereditary to produce ideots, and to the frequent intermarriages of their descendants, which was very likely to happen, where poverty and the wildness of the country would prevent migration from, or colonization among them? It is true, that no person who has made observation

* Haller, l. c.

on the spot, as far as my reading has extended, has taken this view of the question; but most of them visited those places under previous impressions, and all of them speak of numbers in the same family afflicted with these diseases. I have attempted a correspondence with an eminent surgeon in that part of Italy, but the unsettled state of the continent has hitherto prevented me from receiving any information.

NOTE 21, Page 38.—“ *A disease which arrests the progress to virility of every youth, and emasculates every adult.*”

The reader will recollect the importance which, in the Dissertation, is attached to the above fact. But this is not all; in another place it was remarked, that contrary to the testimony of two thousand years, I had ventured a prognosis concerning the Lazars, which examination afterwards confirmed. It will not, therefore, be thought unreasonable, if I claim some attention in resolving a doubt which may seem to implicate either my own veracity, or the accuracy of four gentlemen who visited the Lazaretto with me, the fidelity of two artists, who drew the full length portraits of two naked Lazars, and the testimony of two subsequent witnesses. A passage has been shown me in Dr. Bateman's Synopsis, which a friend assured me, on its first perusal, left an impression on his mind unfavorable to my accuracy. His partiality induced him to examine several times over before he could feel himself at all reconciled. This is an indulgence that cannot be expected from the general read-

er. As Dr. Bateman acknowledges he has never seen Elephantiasis, I shall pass over his description, and insert only the following paragraphs.

“Aretæus and the ancients in general consider Elephantiasis as an universal *cancer* of the body, and speak of it with terror: they depict its hideous and loathsome character, its contagious qualities, and its unyielding and fatal tendency, in strong metaphorical language, which, indeed, tends to throw some doubt on the fidelity of their description. The very appropriation of the name is poetical; and Aretæus has absurdly enough prefixed to his description of the disease an account of the elephant, in order to point out the analogy between the formidable power of the beast, and of the disease. It is probable, that his terrors led him to adopt the popular opinion respecting the malady, without the correction of personal observation: for, although his account has been copied by subsequent writers,* and the same popular opinions have been constantly entertained, there is much reason to believe, that some of the prominent features of his portrait are incorrectly drawn.

“Notwithstanding the care with which the separation and seclusion of lepers have been enforced, in compliance with the ancient opinion, there is great reason to believe, that Elephantiasis is *not contagious*.

* “It is impossible to read the description of this disease, (as said to occur at Barbadoes) by the learned Dr. Hillary, without a conviction that that respectable physician had in his mind the history detailed by the eloquent Greek (Aretæus), and not the phenomena of the disease, as he had himself seen it. See his *Obs. on the Air and Dis. of the Island of Barbadoes*, p. 322, second edit.”

M. Vidal, long ago, controverted that opinion, having never observed an instance of its communication from a leprous man to his wife, or vice versâ,* altho' cohabiting for a long series of years. Dr. T. Heberden daily observed many examples of the same fact in Madeira, and affirms, that he never heard of any one who contracted the distemper by contact of a leper. And Dr. Adams has more recently given his testimony to the same truth, remarking, that none of the nurses in the Lazar-house at Funchall have shown any symptoms of the disease; and that individual Lazars have remained for years at home, without infecting any part of their family.†

"With respect to the *libido inextinguibilis*, which is said to be one of the characteristics of Elephantiasis, the evidence is not so satisfactory. Its existence, however, is affirmed by most of the modern writers, with the exception of Dr. Adams. M. M. Vidal and Joannis mention it among the symptoms of the dis-

* "See his Recherches et Obs. sur la Lepre de Martigues, in the Mem. de la Soc. Roy. de Med. tom. i. p. 169.—Dr. Joannis, a physician at Aix, who investigated the disease in the Lazar-house at Martigues, in 1755, also asserts the rarity of its communication between married persons. See Lond. Med. Obs. and Inquiries, vol. i. p. 204.—Indeed, several able physicians, two centuries before, though bending under the authority of ancient opinion, yet acknowledged their astonishment at the daily commerce of lazars with the healthy, without any communication of the disease. See Fernel, de Morb. Occult. lib. i. c. 12. Forest. Obs. Chirurg. lib. iv. obs. 7: also the Works of Fabricius, Plater, &c. Fernel, indeed, admits, that he never saw an instance which proved the existence of contagion."

† "See his Obs. on Morbid Poisons, 2d edit. c. 13,"

ease at Martigues;* Dr. Bancroft, senior, states its occurrence in the Elephantiasis of South America;† and Professor Niebuhr attests, that it appears in the Dsjuddam of Bagdat.‡ But Dr. Adams observed, on the contrary, in the island of Madeira, an actual wasting of the generative organs in the men, who had been seized with the malady subsequent to the age of puberty, and a want of the usual evolution of them, in those who had been attacked previous to that period. Is the Elephantiasis in Madeira now less virulent than that of former times? has it undergone some change in its character? or, is the ancient account of the disease incorrect?"

Dr. Bateman's Synopsis, p. 298.

It is painful to be so often under the necessity of reminding medical writers of that logical precision which is gradually gaining ground in the other sciences. To the casual reader, it may seem as if the substance of the last paragraph was collected into the three queries which form its concluding sentence. Yet, on a re-perusal, it will be seen, that virulence, *virus* or contagion; change of character in the disease, or incorrect account of *ancients*, all relate to the preceding para-

* "M. Vidal particularizes the case of Arnaud, a sailor, who had been afflicted with the tubercular Elephantiasis six months, when he died of putrid fever. "Il n'avoit cessé, presque jusqu'à sa mort, de ressentir les ardeurs d'un assez violent Satyriasis."

† "Lepers are notorious for their salacity and longevity." Nat. Hist. of Guiana, p. 385.

‡ "Loc. cit. The story related by Niebuhr, of a lazar gratifying this propensity by infecting a woman by means of linen sent out of the Lazar-house, and thus obtaining her admission, appears, however, to be entitled to little credit."

graphs. I shall, therefore, leave them for the present, to consider the evidence concerning that symptom, which "is affirmed by most *modern* writers, with the exception of Dr. Adams." The names opposed to mine are, M. M. Vidal and Joannis, Dr. Bancroft, senior, and Professor Niebuhr.

M. Vidal and Dr. Joannis, though they describe the Leprosy of the same town, differ exceedingly in their history. The former speaks of tubercles in the face as the first symptom; the latter assures us, that the *leaven* of the disease first appears by a swelling in the calves of the legs. The words of the former only are quoted by Dr. Bateman, who describes the disease as the *tubercular* leprosy. My readers will determine the probability of such a symptom as that mentioned by Vidal, in the last stage of putrid fever! Not less remarkable is that related by Joannis, whose patient, twenty-four hours before his death, *plus quam semel cum uxore coivit*. Dr. Bancroft, by the brevity of his language, seems only to have given the popular report, which, from the nature of his work, was as much as was required. For the value of Niebuhr's evidence, see the note quoted from the "Synopsis."

The result of the whole is, if we even admit the very questionable symptom mentioned by Vidal and Joannis, it is by no means certain that they ever saw the true Elephantiasis. The accounts of both are confused and contradictory. Dr. Bancroft does not mention any personal observation; and Niebuhr is not allowed to be a credible witness. It must be admitted, that the above are only a few of the many names which might be produced in proof of the salacity of lepers. How then, it may be asked, has such an opinion re-

mained uncontradicted from the days of Aretæus till the second edition of "Adams on Morbid Poisons?" And what greater credit is due to the latter than to such a host of witnesses? The first question is readily answered in the too high respect we are apt to pay to names deservedly great, which is well illustrated by Dr. Bateman in the first of the three quoted paragraphs, and in the note concerning the learned Dr. Hillary. The second I should feel more difficulty in answering, were it not that Dr. Bateman has assisted me, by the manner in which he has thought my name worthy of a place in the New Cyclopædia.* "The "Elephantiasis," says he, "was *accurately examined* by "Dr. Adams, in the Lazaretto, near Funchall.

In the Second Edition of "Morbid Poisons," to which Dr. Bateman here refers, it will be found, that the examination was made in the presence of four Gentlemen, three of them public functionaries of the island; and that the result was laid before the Governor, by his order. From a proper respect to the sex, even in this degraded state, and that the report might be duly authenticated, a midwife (the most experienced of her order) was procured, to assist in the examination of the women. The drawings annexed to the descriptions show that the males were in a state of

* See article *Elephantiasis*, which, Dr. Bateman informs us, was written by himself. (Synopsis, p. 233, Note) The copious extracts in the Cyclopædia from my "Observations on Morbid Poisons" are, in many other respects, incorrect. In this place, I shall only notice a great error in the number of subjects contained in the Lazaretto. Dr. Bateman says ten. The numbers mentioned in the "Observations" are ten women and eighteen men, all of whom were *accurately examined*; and also some who were not in the Lazaretto.

perfect nudity. When enough were examined in this manner for every satisfactory information, the inspection of the rest was less irksome to the patient, but not less minute in whatever relates to the peculiar marks of the disease. The description begins with the hairy scalp, and is continued seriatim to the toes. Yet, after having perused all this, and admitted the *accuracy* of the examination, Dr. Bateman concludes his account with observing, that an *accurate* history of the Elephantiasis which occurs in hot climates, must be still deemed among the *desiderata* of *pathologists*.—*Synopsis*, p. 304.

It has been well remarked by Sir Isaac Newton, that, excepting in mathematics, “to demand any thing beyond well ascertained facts and fair inductions, is to reduce all philosophy to mere scepticism, and to exclude all prospect of making any proficiency in natural knowledge.” “Hence, he concludes, whatever is thus collected ought to be received, notwithstanding any conjectural hypothesis to the contrary, till such times as it shall be contradicted or limited by further observations on Nature.”*

Where then are the further observations which contradict or limit the facts contained in my second edition? Dr. Bateman offers none from himself or any subsequent writers. The only two documents that have come to my knowledge, are Dr. Christie, in his account of vaccination at Ceylon, and Dr. Gourlay’s *History of the Island of Madeira*. The first of these refers his reader to “Adams on Morbid Poisons, for a good account of Elephantiasis;” and

* Pemberton’s Introduction to Sir Isaac Newton’s Philosophy.

Dr. Gourlay notices those peculiar changes of the organs described by me, in contradiction to all former writers, as "a leading character of the disease."

It may be asked, by those who have not read the "Synopsis," or who do not take the trouble of referring to the passages quoted, Why so much time should be spent in explaining a thing so clear? But it may be questioned, whether it will appear so from a mere perusal of the Synopsis, for between the ambiguous set of queries at the conclusion of the paragraphs quoted above, page 92, and the pathological *desideratum* of "an accurate history of the disease," there is interposed a long discussion concerning the Lazaretto's of the middle-ages; the leprosy of the Jews, and Dr. Winterbottom's mistake, who, Dr. Bateman informs us, instead of the Elephantiasis, has described the *Leuce* of the Greeks; the *Barras*, and not the *juzam*, of the Arabians; some learned notes are subjoined on Hippocrates, Celsus, and Lucretius; Avicenna, Aëtius, Paul Aginet, Actnarius, and others, concerning the Alguada (Alphos) and white Barras—Barras with the epithet black of Avicenna.—The disease described by Alzaharavius, which, when arising from putrid phlegm, and commencing with Barras, or with white Bohak, (Alphos of the Greeks) becomes juzam in its advanced stage. The last of the notes tells us, that if Dr. Winterbottom was *correctly* informed, (by the bye, after the remark in the text, his report must be very doubtful) the *Foolas*, on the coast of Africa, employ the Arabian terms in an *inverted* sense. 1. The *damadyang*. 2. The *didyam*, (sometimes written sghidam, dsjuddam and juzam) and, 3. The *barras*. But if I understand the text, the whole is erroneous in

Dr. Winterbottom, which very well prepares the reader for the conclusion of the paragraph, "Accurate Histories of the Elephantiasis, Leuce, and other modifications of the formidable cutaneous diseases that occur in hot climates, and especially where agriculture and the arts of civilization are imperfectly advanced, must be deemed still among the *desiderata* of the pathologist."

We may now return to the three queries which were before passed over as misplaced; but which, forming the conclusion of a paragraph relating to Dr. Adams, ought not to be left unnoticed. 1st. "Is the Elephantiasis of Madeira now less virulent than that of former times? 2dly. Has it undergone some change in its character? or, 3dly. Is the ancient account of the disease incorrect?"

Two of these questions are answered, not indeed without some reserve, in the quotation from the Synopsis. In the first of the three paragraphs, great suspicion is thrown on the accuracy of the ancients. In the second, we are told, "there is great reason to believe that Elephantiasis is *not contagious*. The other question, concerning *change of character*, is involved in much difficulty, because we know no means of describing a disease but by its character. The following paragraph, transcribed from another part of the Synopsis, still further shows the intricacy in which the subject is involved.

"Surely then," says Dr. Bateman, "the imperfection of the knowledge of the ancients, respecting the nature of these eruptive fevers, affords no just inference against their existence; while, on the contrary,

the brief, but repeated notices which they have transmitted to us, of eruptions resembling nothing that we are now acquainted with, except the contagious maladies in question, lead to the fair and legitimate conclusion, that *the diseases of mankind*, like their physical and moral constitution, *have not undergone any great and unaccountable change*; and that the eruptive fevers have prevailed from the earliest ages.*

A wish to preserve accuracy in medical reasoning, induces me much more than any personal concern, to mark the haste with which the preceding paragraph has been published. The object is to prove, that the small-pox and other "eruptive fevers have prevailed from the earliest ages." The proofs are—That no inference to the contrary is to be drawn from the imperfect knowledge of the ancients respecting such fevers; and that frequent notices of eruptions, resembling nothing we are now acquainted with but such contagions, lead to the conclusion—of what? "That the diseases of mankind, like their physical and moral constitutions, have not undergone any great or unaccountable change;" that is, ancient ignorance does not disprove, and imperfect description leads us to conclude, what no one will attempt to dispute. But because diseases do not undergo any unaccountable change, does it follow, "that the eruptive fevers have prevailed from the earliest ages." Is there no difference between a *change* which we cannot account for in the *same* dis-

* Synopsis, p. 68. See also p. 66 et seq. of the same work, for the author's opinion of the inaccuracy of the Arabians, and of the learned Sinnertus, and the intelligent Dutch Professor Diemerbroek, and others.

ease, and the introduction of a new disease? Have we not witnessed in the cow-pox, a sufficient cause for the introduction of a new contagion? Lastly, Would the ancient Greeks, with all their inaccuracy, have overlooked what has been noticed by the Arabian and succeeding writers, namely, that these eruptive fevers attack the same person only once during life?*

The reader may be tired of this attempt at close reasoning; still, a regard to my own reputation and the interest of humanity, obliges me to detain him on a most serious part of this Inquiry. The difficulty of fixing the exact bounds of contagion, and the important interests in which the question is involved, are equally great. Yet we are told, there is great reason to believe, Elephantiasis is *not contagious*; and the authorities brought in support of this opinion are, M. Vidal, Dr. Heberden, and Dr. Adams.

Why are not MM. Vidal and Joannis, on this occasion coupled, as in the succeeding paragraph? If either of them is omitted, should it not be the former, who pleads that the account was extorted from him, and the almost intire extinction of the disease when he began his enquiries. M. Joannis tells us, that the *leprous infection* is different from that of the pox, and that it is not so easily communicated. Dr. Heberden is more cautious in his conclusions than he has credit for.† In my own defence, I must again refer to the

* See Observations on Morbid Poisons, p. 360; and Inquiry into the Laws of Epidemics, p. 22.

† His words are, "It is certainly not so contagious as is generally imagined."—Medical Transactions, vol. i. p. 23. In the "Synopsis," no reference is made in the notes to this passage.

Cyclopædia. Here we are told, "Dr. Adams is *disposed* to deny altogether the contagious nature of the disease;" but the paragraph concludes with a just caution. "It must not be omitted, however, that the wife of the married Lazar just mentioned, was also diseased, and that Dr. Adams heard of two other couple in a similar condition; and also, that the porter of the house had become a Lazar, since his residence in the Lazaretto."

That the reader may be acquainted with both sides of the question, let him turn to the Cyclopædia. Besides the justice there done to my caution, in relating four possible cases of contagion, he will find the article commence with Dr. Bateman's definition of Elephantiasis, "a loathsome *contagious* disease;" and afterwards, "*contagion* is another source to which Elephantiasis is attributed. *It is not to be doubted*," adds the author, "that contact with the diseased will excite Elephantiasis, as in other chronic diseases, where *a morbid poison is generated*." Avicenna's opinion is quoted, that "it may be communicated by effluvia, as in acute diseases; but this, it is added, seems *questionable*." "The disease seems to have prevailed throughout Christendom, more particularly in the South, but not confined to the warmer countries, as it was prevalent in England." Mathew Paris's authority is quoted without any reserve, "that 19,000 Lazarettos were erected for this disease only in Europe." Professor Niebuhr's story of the infected linen is related with as little reserve; and we are told, "the disease imported into the West India Islands from Africa appears to have been one of the punishments which that

abominable traffic the slave trade inflicted on the inhabitants."

It will naturally be asked, What should have given rise to this change of opinion? Has any new light been thrown on the subject, since the publication of this article in the *Cyclopædia*? The suspicious testimony of Aretæus and his copyers, including Dr. Hillary, must have been well known at that time, as Dr. Adams's work, which is spoken of so favourably, and from which such copious extracts are made, contains the same opinion, and almost in the same words.* The change of sentiment, concerning the 19,000 Lazarettos in Christendom may indeed have occurred since, as the 'Inquiry into the Laws of Epidemics' was not then published. The only difference between the passages contained in that work and in the Synopsis is, that the language of the latter is the most decided.†

The question whether Elephantiasis is contagious, is too important to be hastily passed over. Careful as we should be, not to excite unnecessary alarm, which must render the condition of the unhappy objects still more deplorable, we are scarcely warranted in asserting, that there is no contagion in any kind of intercourse with Lazars.

It appears probable, that in this country at least,

* Compare *Observations on Morbid Poisons*, p. 284, with Dr. Bateman's *Synopsis*, p. 296, note. In the *Cyclopædia*, Hillary's account is called a transcript of Aretæus, but without any mention of inaccuracy in either.

† See the first note at the end of "Inquiry into the Laws of Epidemics," in which the subject of Leprosy, and particularly of these Establishments, is considered much at large.

little is to be apprehended; and if so, less inconvenience may happen from the security expressed in the Synopsis, than from the terrors contained in the Cyclopædia. It must be admitted too, that about the time that article appeared, a very general opinion prevailed, that contagions might be exterminated. As a proof, it was urged, that Elephantiasis, under the name of Leprosy, had been exterminated from England by the prudent caution of our ancestors; and some apprehension was expressed lest it should be re-imported. During this period, a Lady, native of Ceylon, arrived in London, with the disease on her. In the same vessel came a very experienced Army Physician, who being consulted on the case, entertained several doubts, whether he should denominate it *Leuce* or Elephantiasis; but in either case, expressed his alarm, lest this dreadful contagion should be spread through the British Islands. With the best intentions in the world, therefore, he proposed that a guard should be immediately set over the patient, her husband and child; and that all three should be sent back to India by the first opportunity. The husband was a most respectable character, the confidential servant of a nobleman at the head of one of the Governments in India, who not wishing to be deprived of a faithful Secretary, desired the advice of Dr. Baillie before any other measures were taken. That gentleman having seen my description of Elephantiasis in manuscript, had no difficulty in ascertaining the case, but very politely referred it to me. The nobleman, with the humanity that does credit to his rank, honoured me with a call, full of anxiety, to know the event; his secretary attended him, who was said to be already infected. On a

careful examination, however, only a trifling efflorescence could be found on any part of his skin, not at all characteristic of the disease. The poor Lady was dreadfully afflicted; she afterwards retired to Bath, and died in one of the Lodging-houses, without being injurious to any one.

I have given this little history, to show how important it is, as far as possible, to ascertain the laws of contagion. Yet, by some passages in the *Cyclopædia*, particularly some quotations, which stand uncontradicted, one might apprehend every thing. By the *Synopsis*, we are divested of all caution, and a quotation from Vidal would lead us to conclude, that there is no danger even in the sexual intercourse with lepers. Surely, this passage ought to have been contrasted with the account of the three couples mentioned in the *Cyclopædia*.

Five cases of Elephantiasis have fallen under my notice since my return to London. It is probable, that many others may have appeared, not only in the metropolis, but in other sea-port towns. The description of the disease is, therefore, a matter of importance. To those who are in possession of the last edition of the *Cyclopædia*, nothing need be added to what the writer calls "the latest account of Elephantiasis occurring in Europe, and the history transcribed of its "minute examination." In the *Synopsis*, the author, after professing never to have seen the disease himself, adds, "I should have omitted the subject altogether, had it not appeared to me, that some comment on the mistakes of translators and their followers, as well as on the history of the disease in general, might contribute to

put the matter in a clearer light than it now stands." It cannot be necessary to remark, that the attempt to make an accurate account clearer, may tend to obscure it. Such is certainly the case. Besides the number of notes already transcribed from the Synopsis, many more are affixed, and in such a manner, that the reader is left in the dark, whether the quotations are to be considered as containing faithful descriptions or not. The history of the disease in the text is also erroneous, in the omission of symptoms which form its strongest characters, and in the addition of others which are not to be met with. All this is now readily decided, without any reference to a warm climate. The case of an adult has already appeared at St. Bartholomew's Hospital, and another of a boy is to be seen there at this time. Mr. Ashburner, the house-surgeon, recognized the disease at first sight, and the examination of every part of the body confirmed his suspicion, as well as the faithfulness of the account given in the "Observations on Morbid Poisons."

In the passage which immediately follows in the Synopsis, as part of the same subject, the name of Dr. Adams might, with as much propriety as in the former, have been opposed to most of the moderns. "By the surgeons of the present day, we are told, the appellation of Elephantiasis is appropriated to an enormously tumid condition of the leg," &c.* This appropriation of the word is not confined to surgeons. It is so general among medical men of all descriptions, that in the paper before-mentioned, as submitted to the

* See Synopsis, p. 304, with the learned Notes annexed.

London College of Physicians, I did not scruple to enter minutely into the confused application of the terms *Lepra* and *Elephantiasis*. I might offer an epitome of this paper ; but if my reader feels, like me, nothing less than minute descriptions of local diseases will satisfy him. To hurry over twenty or more at a sitting, is an entertainment which may be often repeated, because they will be forgotten as soon as perused. But the slightest inaccuracy will, to an enquiring mind, render the whole perusal unsatisfactory, and even painful, whenever a doubtful case presents itself. We shall then gain little by learning the opinions of Haly Abbas, Avenzoar, or Alsaharavius, with many others, whether their language is quoted or their works referred to. That the reader, however, may not be at a loss, if he wishes to examine the original authors on Leprosy, and the various senses in which the above terms have been used, I shall insert a short paragraph.

“ 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. Leonicens, and several others, preserved in the Luisinian Collection. To do justice to 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." *Observations on Morbid Poisons*, p. 290, 2d. edit.

The succeeding article in the Synopsis is Yaws. This, the author says, has "been imperfectly investigated by European practitioners;" and afterwards, in a *Note*, that "it is not easy to discover the precise character of this eruption from the varying language of authors." I have been more fortunate. By visiting my patients daily for several months, and sometimes two or three times a day, and taking frequent opportunities of dissecting the pustules† or breaking the scabs, in order to view the bottom in different stages of the disease, it was easy to reconcile the accounts given by different authors. Though they can scarcely be called incorrect, yet none of them are sufficiently minute; some describing appearances and symptoms omitted by others, yet without contradicting each other. Every appearance is most minutely detailed in the Levitical description of Leprosy. This part of the controversy is, in the Synopsis, confined to a few symptoms, chiefly contained in *notes*. The following comprehends most of the arguments.

"The anonymous writer in the *Edin. Med. Essays*, and after him Dr. Hillary, and others, have deemed the Frambœsia to be the Hebrew leprosy, described by Moses. (*Leviticus*, chap. xiii.) In some respects, and especially in the appearance of what is called

† See *Observations on Morbid Poisons*, p. 207. It is remarkable that we have no instruction for such a mode of ascertaining the character of a morbid poison, but in the Levitical Law; if we except Mr. Hunter's account of *Connate Small-pox*, published in the *Philosophical Transactions*.

'raw flesh,' in the leprous spots, together with *whiteness of the hair*, the description of the Leprosy of the Jews is applicable to the Yaws. But the leprosy is described by the great legislator as beginning in several varieties of form, in only one of which this rising of 'raw flesh' is mentioned: and the two circumstances, which all these varieties exhibit in common, were a depression of the skin and whiteness of the hair. Now this change in the colour of the hair is common to the Frambœsia, and to the Leuce, as stated; and it is conjoined, in the latter, with cutaneous depression. It seems pretty obvious, indeed, that the term Leprosy was used, in the Scriptures, to denote several diseases of the skin, against which the law of exclusion was enforced, and others, to which it did not apply. An instance of the latter occurs in Gehazi, whom we find still in the employment of Elisha, and even conversing with the king, after the leprosy had been inflicted upon him, "and his seed for ever." (2 Kings, chap. v. and vi. and chap. viii. ver. 4.)

Synopsis, p. 311, Note.

It might have been presumed, that the anonymous author and *others*, comparing what passed under their own eye with the accurate description of a disease, might be able to judge of the identity of the two. My own opinion has been given more at large than would be consistent with the present undertaking. I shall, therefore, only notice the objections made in the Synopsis. What the author speaks of as "beginning in several ways, or appearing in a variety of forms, relates to the different stages of the same dis-

ease. To the end of verse 8 (Levit. xiii.) the Divine Legislator gives directions for examining the pimple, pustule, or scab. In verses 9, 10, 11, he describes the old [the confirmed] Leprosy, in which the fungus or raw flesh, renders the disease no longer doubtful. From the 12th to 17th verse inclusive, we are taught the marks of returning health, which are similar to those described by the anonymous author and others, as the termination of Yaws, excepting that more caution is required in the examination by the priest, before the person was allowed to reside in the camp. What the *Leuce* was, I pretend not to determine; but if *cutaneous depression* means a loss of substance below the skin, the same is implied as the early appearance of Yaws by the anonymous author, by Dr. Winterbottom, and I believe by every other writer; for how can there be a slough or sordes without a loss of substance, or, as described by Moses, a plague deeper than the skin of the flesh? In *Leuce*, I cannot find that the hair was changed white, though the surface was covered with white hairs like down, *In ea pili sunt albi lanugini similes*. In the Synopsis, it is said silky. Whichever it might be, the change, if it was such, was more than merely that of colour.—Lastly, “*Leuce* was of an incurable nature,” which is different from the Levitical leprosy.—See Synopsis, p. 30 and 31, note; p. 55, note; p. 269, note; p. 299, note; p. 311, note; and probably some other notes.—By referring, however, to the above, the reader will determine, whether he can reconcile the *Leuce* of Hippocrates with the *Leuce* of Celsus, or either of them with the Hebrew Leprosy.

At the close of the last quoted note, it is said, “we still find Gehazi in the service of the Prophet,

and even conversing with the King, after the Leprosy had been inflicted upon him and his seed for ever."

(2 Kings, chap. v. and vi. and chap. viii. verse 4.)

If, during this interview with the king, Elisha was not on a journey, I can at least find no proof that Gehazi continued in his service.

This history, as it respects morbid poisons, at one time engaged much of my attention ; and if what I now presume to offer, is not new, it may not have met the eye of those, whose studies, like the author's, have been for many years purely professional.

Writers have remarked, that white people rarely catch the Yaws, because their skins are entire and their bodies covered. The Mosaic account of the Egyptian bondage informs us, that the Isrealites were striped by their masters, and doubtless badly covered, Egypt, from its close population and settled climate, has always been famous for its cutaneous and contagious diseases. No wonder, therefore, that the Divine law-giver should very early direct his attention to their extermination ; and if, as is generally believed, Yaws can only be communicated by inoculation in some form, we may readily conceive that this, and many other similar diseases, were exterminated by those wise institutions ; and the accurate description of them shows the antiquity of the writings ascribed to Moses. I am aware it has been said by some, that the Jews were only that part of the Egyptians which were separated from the rest, on account of the Leprosy ; and even Dr. Mead seems to admit, that they are to this day particularly subject to cutaneous ulcers ; but nothing can be more unfounded. The Jews, though numerous and poor, are much more free

from such diseases, than the nations among whom they sojourn. This was long ago remarked by the two Buxtorfs, who, though no way friendly to their general character, seem reluctantly to admit "*Lepra revera non tam frequens est apud eos quam apud Christianos,*"* which is very reasonably imputed to their attention, as far as possible, to the Mosaic instructions. That they are of one family can hardly be questioned, from the correspondence of their features; and what could be more preposterous, than to suppose, that an army of invalids could have accomplished so long a march, and under such difficulties.

It has often occurred to me, that the instructions they received, to avoid every nation, if possible, and to destroy those they were obliged to encounter, arose not only from the necessity of preserving the purity of their race, but to prevent the introduction of contagious diseases; and this seems confirmed by the description of those among the conquered Midianites, who were to be spared.

If by these means, the Jews became free from contagions, perpetuated by contact only; still they could expect no exemption from Elephantiasis, a disease excited by climate. It is not to be wondered, however, if the law of exclusion was continued, and gradually moulded to the condition of the sick. Accordingly, we find King Uzziah, when the Leprosy was discovered upon him, confined to a lone house, not "during all the days of his Leprosy," but as long as he lived. The poorer class, the most numerous and the most liable to the disease, seem only to have been for-

* *Synogoga Judaica*, cap. XLV.

bidden to enter the towns. Thus we read, "at the *gate* of Samaria there were four lepers;" and in the 17th of St. Luke, *at the entrance of a village*, there met Jesus ten lepers, *which stood afar off and lifted up their voices*. Such was probably the manner in which Gehazi conversed with the King. That this disease was different from the Leprosy mentioned in Leviticus, and for which a *temporary* exclusion only was required, is evident, inasmuch as it was incurable.—Am I a God, says the King of Israel, that this man expects me to heal a leper! [2 Kings, chap. v.] and the cleansing of lepers was admitted among the most extraordinary of our Saviour's miracles.

If Gehazi's disease was really Elephantiasis, it explains at once the import of the prophet's words. We must recollect, that though they seem to imply a curse; yet this may be imputed to the manner in which the future tense is formed in the English language. In the Septuagint, the two verses taken together, produce a very strong and pointed effect. "And you were now grasping at (*τινι λαβεις*) silver and garments, and olive yards and vineyards, and sheep and oxen, and men servants and maid servants, and the leprosy of Naaman, *κολληθήσεται*, shall or will cleave unto thee and unto thy seed for ever." At such times as these, are you thinking of the aggrandizement of yourself and family? recollect the troubled state of your country, and that you have an incurable and hereditary disease upon you! Whether the words of inspiration were really directed to that peculiarity in the disease, which would preclude the possibility of any posterity (*τω σπέρματι σου*) it is not necessary for us to enquire.

Before I take leave of the Synopsis, I cannot help congratulating the writer, as well as the public, on the temporary injunction concerning its sale. I doubt not, it will be productive of a new work, more worthy of the author. The present, besides evident marks of haste, shows an attempt at more than can be accomplished. To shorten the accurate description of any eruptive disease, is to render it unsatisfactory, and consequently useless; besides, that the labours of men, who have devoted a life to any branch of a profession, should be touched with extreme caution. Their text should be preserved, and whatever elucidation or correction may be thought necessary, should be offered in commentaries. Some arrangement is required in every undertaking; and as Dr. Willan proposed one, it was necessary to pursue it, in offering an abstract of his proposed classification. But I should rather wish to see an original work from Dr. Bateman, on a plan purely his own, in which he might refer to Dr. Willan in common with others. I could wish also to see him less shy of modern authors, and much more so of the Greeks, Latins, and Arabians. Dr. Cullen remarks in his Preface, that as he could make nothing of *Synonyma veterum*, he has omitted them. The venerable Heberden speaks of them in much the same manner; and we should, least of all, have expected the revival of them by one who conceives, not only that the Greek and Roman writers were incapable of describing small-pox, but that even the Arabians, tho' not ignorant of the disease, confounded it with others, to which it bears no resemblance; an error which was continued to the close of the 17th century. If Dr. Bateman would favour us with a distinct Essay on the

probable application of ancient terms, or recommend it to a junior, or an *emeritus*, as an exercise or recreation, some entertainment, if not use, might be made of such a work, and the young reader need not be perpetually surprised or discouraged, by meeting with hard words, or being referred to authors he is not likely to find in his library.

I was much pleased to see the attention paid in the Synopsis to names and distinctions among the vulgar, p. 67, and 204, notes. This is also remarked in the text, under the article Itch, in which the vulgar names are neatly Latinized. On this occasion, I wish the author had availed himself of information from the same sources from which he derived his names. Few of the Irish labourers who apply for relief at the Carey Street Dispensary, would be found ignorant of the *flesh-worm*, of the difference between the disease it excites, and the *itch*; and even of the different manner in which each may be cured. The experiments made in Madeira, and confirmed in Ireland,* have lately been repeated at Paris; but whether the vulgar were consulted, does not appear.† In the Synopsis, we are referred to Abinzoar of the 12th, and others of the 15th and 16th Centuries. Without questioning the accuracy of either, let me remark, that Ambrose Paré, about the last date, gives a very accurate account of these insects, and says, the patient may be

* See Observations on Morbid Poisons, 4to. edit. p. 292.

† See the 3d Number of the Medical Repository. Without a sight of the Original Paper, and consulting those who are intimately acquainted with the most vernacular French, it would not be fair to offer any other conjecture.

cured by extracting them; after which he advises stavesacre, aloes, or salt, to be rubbed on the parts.* Savaages† and Plenck‡ both advise extracting the insect. Can this be the disease in which Swammerdam, Canton, Baker, Heberden, and Hunter, never could discover an insect with the best microscopes?

It must be admitted, that since philosophers interfered in this question, it has been involved in much obscurity. Bonomo took half his lesson from the galley slaves; and of his crude materials Dr. Mead published a mutilated account in the Philosophical Transactions. From that time, philosophers and physicians have been much puzzled; some having seen the insect, which never could be detected by others, who searched for it with the best microscopes, in the most marked cases of Itch. Savaages, who distinguishes the two diseases in his octavo edition, confounds them in his later quarto edition. Dr. Willan, who was full of candour, was aware of these difficulties, and thought he could reconcile them, by finding that Sinnertus, Mercurialis, and some other writers of their date, had described *pruritus* or *prurigo* as the frequent forerunner of Scabies. His words are, when persons affected with the above disease neglect washing, "the eruption grows inveterate; and at length, changing its form, often terminates in the Itch. Pustules arise among the papulæ, some filled with lymph, others with pus. The *acarus scabiei* begins to breed in the furrows of the skin, and the disease becomes

* Oeuvre de Ambrose Paré, 1746, edit. 12th, livre xxvi. chap. 6, page 476.

† Nosol. Method. vol. ii, p. 23, edit. 8vo, 1763.

‡ Plenck de Morbis Cutaneus, p. 119, edit. 2, date 1783, Viennæ.

contagious."* This was published before I had given him the history of the disease, and shown him the insect, of which he procured me a drawing by the late Dr. Shaw. Had my friend Willan lived to complete his work, I have no doubt that he would have corrected this passage, and done justice to the sources of his information, as he never failed to do on other occasions.

In the Synopsis, there are two attempts at reconciling the difficulty. The first is, by approving Savaages arrangement, in making a separate species of *S. Vermicularis*; the other is, by "supposing that the breeding of these acari in a scabious skin, is a rare and casual circumstance, like the individual instance of a minute pulex in Prurigo, observed by Dr. Willan; and that the contagious property of Scabies exists in the fluid, and not in the transference of insects."

Does the expression "generated in some species of Scabies only," mean that Scabies is necessary for the generation of such an insect, or that there is a species of itch occasioned by the *acarus gyro*? The latter seems the meaning of Savaages; but, were it as Dr. Bateman afterwards expresses himself, "a casual circumstance," it could not be "like the individual instance of pulex mentioned by Dr. Willan;"† We have then only to enquire, whether they will breed under the cuticle of a skin free from Scabies? and whether the disease may be cured by extracting them? Both these facts have been experimentally ascertained. The conclusion is, that an eruptive disease, independent of the Itch, is excited by an insect

* Cutan. Diseases, p. 75.

† Id. p. 85.

called in Ireland the flesh worm ; in France, the çyron ; in Portugal, ouçãõ ; once well known in England by the name of wheal worm, and still sometimes occurring. That the disease which it excites, from some resemblance, from situation, and from yielding to the same remedies, may be mistaken for itch ; but that the insect has never been discovered when searched for in the most marked cases of true itch.

In the Synopsis, we have four varieties of Scabies. These four consist of the true itch only, and another disease, which I have called the *herpes pauperum*.* This is the Scabies Cachectica of the Synopsis, a term which, if contagion were not attached to the *Genus*, would be unexceptionable. It has been noticed by Sir John Pringle, by Dr. Gillespie, and by many other writers on camp and prison diseases. It occurs in confined nurseries, if change of air is not introduced, but without extending to other parts of the house. I have seen it in two or three misguided youths, who have returned to their parents, after a temporary absence, in the lowest order of society ; in these instances, it has never been communicated to any of the family.† I have called the disease Herpes, from its disposition to spread along the skin. This etymology of the word is ad-

* See Observations on Morbid Poisons, p. 325 and 326.

† See also Epidemics, chap. 1. From the last mentioned circumstances, I was led to suppose, that the disease is only infectious in that kind of air in which it is generated.

If the reader is unacquainted with these distinctions, and has not leisure or inclination to refer to the arguments by which they are enforced, I must request him to give me credit for them. One illustration, however, occurs from this passage in the Synopsis. "The most ordinary cause of scabies, we are told, p. 196, is contagion."

mitted in the Synopsis (from *επιπνυσε* *serpere*). Yet, in the same work, "the appellation is limited to a vesicular disease, which, in most of its forms, passes through a regular course. Such is particularly the case with one "*local variety*," which, we are told, p. 233, "was not noticed by Dr. Willan." It is incidentally mentioned under a different name in Observations on Morbid Poisons, 2d edit. p. 94, and is most minutely described by Mr. Royston, in the Medical and Physical Journal, vol. xxiii. p. 446.

These remarks on names, somewhat abruptly introduced, may serve to show how careful we should be in changing commonly received terms. Whether we use psora or scabies for Itch, may seem of little consequence, as it is not certain that those from whom we derive the words were acquainted with the disease to which we now apply them. But whenever a name is changed, some regard should be paid to etymology, as the great use of nosology is, that we

—"It seems, however, to originate in crowded, close, and uncleanly houses."—"When the contagion has been introduced, however, into families where every attention to cleanliness is enforced, it frequently spreads to all the individuals, children and adults," &c. All this is correct, if the genus *Scabies* is to include diseases arising from different causes, and different in their character, and even different in their mode of cure. But, in my opinion, it would be more correct to say, *Itch and Herpes pauperum* are both common in close and uncleanly houses—the former must be introduced into, but the latter may be generated in, such places, and both are communicable in such places. The former, when introduced into a lean family, may be communicated before the parties are aware of it; and will only yield to certain local remedies. The latter is never generated in such families, nor communicable when introduced; and is curable without the local remedies necessary in the former.

should all be acquainted with each other's language. If we expect more than this, there will be great danger of misleading ourselves. The first person who proposed an arrangement of diseases, similar to what is made of plants, was Sydenham. He, however, rather looked to it with a wish than an expectation of its accomplishment, and seems not aware that the arrangements of Botanists were, in many instances, artificial. It is certain, that he lived to lament the application of practice to names, declaring, in the most advanced period of his practice, that "the invention of the term, or opinion of malignity had been far more destructive to mankind than the invention of gunpowder." I leave it to the decision of those who have had most experience, in the comparative effects of disease and gunpowder, how far the introduction of the term Typhus may be liable to a similar charge. We may at least remark, that the very fever which drew this expression from Sydenham, is, by Dr. Cullen, included among the *Synonyma* of Typhus.* As the illustrious Professor refers to every original author, it would be unreasonable to accuse *him*, if such sources of information are not studied by his readers; and if he made his nosology a text book for his lectures, we cannot doubt that his hearers were often apprized, that the same mode of practice could not be applicable to fevers arising from so many causes, assuming so many forms, appearing in such different climates, and under habits of life so different as to comprehend near fifty synonyms. Still, we may lament the influence of a term, in a work which, from

* *Febris nova anni, 1685, Syden. Schedul Monitoria.*

the just celebrity of the writer, and its connection with the "First Lines," may hereafter become the text-book of less enlightened Lecturers, as it is already of Physical Compendiums.

Diseases of the skin being more immediately the objects of our senses, may be thought more easily reducible to orders and genera. Let us try this in Itch and Syphilis; because, in these we are most frequently required to give a decided opinion. Of the first, if enough has not been said, to show the difficulty which attends such an attempt, we may add from the Synopsis, that "from its affinity with three orders of eruptive appearances, pustules, vesicles, and papulæ, it *almost* bids defiance to an artificial classification."—The above orders, comprehending only fifteen genera with their species and varieties, may be less alarming to a modest enquirer. But the eruptions of Syphilis are said to "bid defiance to arrangement, according to external character;" that, "in fact, they possess no common or exclusive marks, by which their nature and origin are indicated:" and that "there is no order of cutaneous appearances, and scarcely any genus or species of chronic eruption *already described*, [and this is the last] which these secondary symptoms of Syphilis do not occasionally imitate."—"Nevertheless," continues the author, "there is in many, a difference which a practised eye will recognize between the ordinary diseases of the skin and the syphilitic eruptions to which the same *generic appellation* might be given; this is often observable in the shade of colour, in the situation occupied by the eruption, in the mode of its distribution, and in the general complexion of the patient. Hence, to a per-

son conversant with those ordinary diseases, a degree of anomaly, in these respects, will immediately excite a suspicion, which will lead him to investigate the history of the progress of such an eruption, and of its concomitant symptoms. And it will frequently happen, that the most experienced observer can only arrive at a satisfactory conclusion, by comparing the cutaneous appearances with these concurring symptoms, and with the previous history of the disease." Fortunate are those tyros and practitioners who are within the reach of consulting a person conversant with with those ordinary diseases, as the discriminating marks between half a score such and syphilis, can afford little assistance to the common reader.

This Note may seem extended to an unreasonable length, and no longer connected with hereditary diseases. But it is most intimately connected with whatever relates to medicine as a science. For if two not uncommon diseases, the progress of which is slow enough to admit the most accurate observation, are reduceable to no laws, or to none that can be described, it is evident that our present mode of investigation must be defective, or that we must give up every prospect of progressive improvement. We are told, from very high authority, that the faculty of tracing actions, so as to discover a law, makes the whole of philosophy.* In medicine this must be more difficult, and more as well as closer observations must be necessary, than in any other branch of natural knowledge; because diseases are modified by constitutions, and their progress interrupted by remedies. Hence, it is not easy to bring men to a general agreement; yet, some progress has been made towards such an event. In

* Nov. Organ.

the Small-pox, Sydenham has taught us to look for certain laws, which were never established till his time.* In gout, he has not been less successful; and the instructions he has given in tracing fevers, have become almost oracular. Fothergill has been scarcely less successful in Scarlatina. Though in the treatment of these diseases, improvements may be expected, and changes necessary under different circumstances, yet the phenomena of the diseases themselves, as marked by those writers, are now so familiar to us all, as to admit of no dispute. If this can be accomplished in fevers, shall we despair of arriving at the same certainty in diseases, whose progress is slower? It must be admitted, that for so desirable an event, we require the same talent at observation, the same persevering diligence, with the same integrity as existed in the man who, after establishing such laws, should declare that there are small-pox cases, which the errors of a nurse could not render fatal; and others, which the skill of no physician could cure. When, therefore, we possess the fruits of such mens' labours, let us learn how to value them.

Having made these general remarks, I shall trace very shortly the progress of Mr. Hunter's inquiries, in a disease which is said "to possess no common or exclusive marks."

For a long succession of ages, the term Leprosy was generally applied to all cutaneous complaints, which had no other appropriate title. About the close of the 15th Century, a new morbid poison was in-

* See Mead's Discourse, chap. 2.

introduced, which, from the various forms in which it appeared, excited universal consternation. It was at length found, that in all its forms, it would yield to mercury, the free use of which had been at that time, only recently introduced into Europe. Hence, other local diseases, not well ascertained, were soon confounded with Syphilis, merely because they yielded to the same remedy; and we find Professor Pitcairne very archly remarking, that nothing more was heard of Leprosy, “*Lepra ante famam morbi Neapolitani Hydrargyro cessit, nunc nomen amisit.*” Opera, p. 315, edit. Lugd. Bat. 1737. So convenient a disease, however, could not long be confined to such limits. It now appeared in every shape, and in some that would not yield to mercury, or were even exasperated by it. Lest it should seem strange, that the same disease should produce such different effects under the same remedy, a most convenient term was introduced, to stifle all enquiry. For who could pretend to fix bounds to a *Proteiform* Disease? Hence, we find Lancisi tracing an aneurism to that source, for which he is quoted by some respectable names. — At length, the diligence of Astruc introduced something like order; but in endeavouring to reconcile the various accounts, he supposes that diseases, like empires, undergo changes; as if the laws of Nature were fluctuating, like human institutions. After him Howard, and some other writers of our own country, did much towards improving the practical treatment. Still, however, the disease retained its poetical name, so that every one felt at liberty to include under it whatever he pleased.

Whilst Mr. Hunter was engaged in reducing this

chaos to order, but before he thought his labours sufficiently mature for the public, we were told, that the Island of Otaheite was almost depopulated by the importation of that morbid poison. Such was the candour of this author, that though he found it difficult to reconcile such an event with his opinions, he never ventured to dispute it. All this while, it should be remarked, that no accurate description was given of the South Sea Disease, as it was thought enough to give it a name. At length, the surgeon who attended the expedition under Peyrouse, confining his letters to his own department, could not fail of being somewhat particular in his description; and it is but justice to his countrymen to add, that their accounts of diseases are generally more minute and accurate than ours. Still the same error remained, for if he did not consider the appearances he described, as the effect of that morbid poison, he never expresses a contrary opinion; and seems, in many places, tacitly to admit it. This is less to be wondered at, in one who could only read Mr. Hunter in a translation or a foreign tongue. From an accurate examination, however, of all the cases described by the French Surgeon, I had no scruple in suspecting, that none of them were venereal. This led me to examine all the accounts of those voyages, and confirmed me in the belief, that Syphilis had never been known in Otaheite.

Without any knowledge of my opinion, and even without any intercourse between us, Mr. Wilson, a Surgeon in the Royal Navy, landed at that island, full of anxiety for the health of his crew, who were to complete a long voyage after their departure. His

uneasiness was much increased by the dismal tales of the Missionaries. This induced him to be particularly attentive to the condition of the natives, as well as of those intrusted to his care; and the result of his most minute enquiries was, that the venereal disease had never been known in Otaheite. After this, shall we say that Syphilitic eruptions possess no common or exclusive mark? or discourage the student by shades of colour, and a general complexion, which cannot be described? But I have said enough to show that medicine is a science; and that, like other sciences, it may be progressive; though it is not as yet sufficiently advanced, to admit of artificial arrangement, without danger to the student.

NOTE to Page 39.—“*That connate diseases or privations are not hereditary.*”

The extensive correspondence these Inquiries have led to, have furnished me with several other instances of fathers born deaf, whose children hear perfectly. I have also been informed of a single instance of deaf offspring from a deaf mother. It is remarkable, that this is the only female I have been able to learn born deaf, who is married and become a mother. It may be proper to mention, that her husband was as near in consanguinity as the canonical law will admit.

After carefully tracing the history of deafness in this family, I have learned that it was truly hereditary, having occurred in three generations out of four; though in the last only in a single individual. Such is the present result of my Inquiry, which, from the distance of time and place, cannot be perfectly satisfactory, without any imputation on the accuracy of those to whose kind attention I am so much obliged.

I have many reasons for believing, that some cases of supposed connate deafness are not really such, but commencing so early, that the subjects can have no recollection of sounds. My principal reason for this opinion arose from hearing, that Mr. Stevenson had succeeded in relieving some of those unfortunate sufferers. Highly as I valued the abilities of that gentleman, my doubts on so extraordinary an event induced me to make a personal application to him, when he informed me, that he had three subjects born deaf and dumb then under his care, in a progressive state of improvement, to each of whom he politely offered to introduce me. Now it is difficult to conceive connate deafness, without such an organic imperfection as would admit of no relief. These remarks can only be made in the hopes of being favoured with further Communications, which, from any quarter, will be thankfully received. If, however, such imperfections are really connate and hereditary, it very much lessens the terror of them, that they are not absolute privations, but in some instances admit of a cure.

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