On a new membrane in the eye : being the substance of a lecture delivered at Oxford; before the late meeting of the British Association for the Advancement of Science / by George Hunsley Fielding.

ON A

NEW MEMBRANE IN THE EYE;

BEING THE

SUBSTANCE OF A LECTURE

DELIVERED AT OXFORD;

BEFORE THE

LATE MEETING OF THE BRITISH ASSOCIATION

FOR THE

Advancement of Science.

BY

GEORGE HUNSLEY FIELDING,

MEMBER OF THE ROYAL COLLEGE OF SURGEONS IN LONDON; MEDICAL SUPERINTENDANT OF QUARANTINE TO HIS MAJESTY'S CUSTOMS AT HULL; MEMBER OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE; HONORARY CURATOR OF COMPARATIVE ANATOMY IN THE HULL LITERARY AND PHILOSOPHICAL SOCIETY, &c. &c.

PLERIQUE ERRARE MALUNT EAMQUE SENTENTIAM QUAM ADAMAVERUNT PUGNACISSIME DEFENDERE QUAM QUID VERISSIME DICATUR EXUIRERE.—CICERO.

HULL:

PRINTED AND PUBLISHED BY I. WILSON, LOWGATE:

SOLD BY SIMPKIN AND MARSHALL, LONDON; J. PARKER, OXFORD; AND MAY BE HAD OF ALL BOOKSELLERS.

1832.

TO THE REVEREND

WILLIAM BUCKLAND, D.D. F.R.S.,

PROFESSOR OF GEOLOGY AND MINERALOGY IN THE UNIVERSITY OF OXFORD; PRESIDENT OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE;

THIS LECTURE

IS, BY PERMISSION,

DEDICATED, AS A TESTIMONY OF THAT PERSONAL RESPECT AND ESTEEM, WHICH,

(THOUGH IT MUST BE FELT BY ALL THE MEMBERS OF THE ASSOCIATION WHO VISITED OXFORD,—WHO WITNESSED HIS ZEALOUS, HIS UNWEARIED EXERTIONS, AS PRESIDENT, AND WHO EXPERIENCED HIS GREAT KINDNESS AND LIBERALITY)

CAN BE FELT BY NONE MORE DEEPLY THAN BY

HIS MOST OBEDIENT SERVANT,

THE AUTHOR.

TO

GEORGE FIELDING, ESQUIRE,

MEMBER OF THE ROYAL COLLEGE OF SURGEONS, IN EDINBURGH; SENIOR SURGEON TO THE HULL GENERAL INFIRMARY; SURGEON TO THE WORSHIPFUL THE CORPORATION OF THE TRINITY HOUSE, HULL; CONSULTING SURGEON TO THE DISPENSARY AT CAISTOR IN LINCOLNSHIRE; CORRESPONDING MEMBER OF THE MEDICO CHIRURGICAL SOCIETY IN EDINBURGH; VICE PRESIDENT OF THE HULL LITERARY AND PHILOSOPHICAL SOCIETY; MEMBER OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, &c. &c.

THIS LECTURE

IS INSCRIBED,

AS A SMALL TOKEN OF FILIAL ATTACHMENT AND AFFECTION,

BY

THE AUTHOR.

PREFACE.

In presenting this Essay to the Public, the Author must beg to observe, that (with the exception of a few notes) he has deemed it advisable to preserve it as nearly as possible in the state in which it was delivered to the Meeting of the British Association. It may probably, therefore, appear somewhat brief, as the limited time which was allotted to the reading of each Paper (a circumstance arising from the numbers presented) rendered condensation imperative, and must now afford the best and only apology.

The sole object of the Author is to elicit Truth;—he has been led away by no favorite theories—has given way to no imaginative speculations—but has only advanced the results of the most careful and diligent observation and the most rigid experiment; and he therefore hopes for that indulgence and that liberality which a British Public is ever ready to bestow.

Kingston Square, Hull, August 10th, 1832.

AN ADDRESS,

&c.

CULTIVATED and flowery as seems the Garden of Science, and smooth and tempting as its winding paths may appear, there are none now before me who will not be ready to confess, not merely that many of its plots are still sterile and unyielding, but that every individual bed, rich and luxuriant as it may be, is still capable of higher cultivation and improvement. The institution of so noble an Association as the one I have now the honour to address, while it powerfully proves the truth of what I have just advanced, proves also the necessity of an union of efforts, by shewing that the higher we advance in the scale of perfection, the more toilsome and laborious is our way. It is the duty, therefore, of every one to use his best endeavours to forward the progress of Science; and though the light he may be enabled to throw on the path be but a single, a solitary ray, it may serve as a guide to

further improvement, and can never be unacceptable to the real lover of truth.

The discovery I have made, and the observations I am now going to offer to you, if they be correct, must necessarily alter the received laws of vision ; and if these remarks shall finally conduct us but one step further in the elucidation of truth, my object will be answered. Acquainted, and well acquainted, as all before me of course are, with the Anatomy of the Eye, it would be superfluous as well as tedious to enter into any anatomical description of its various parts, and I shall therefore proceed at once to my subject.

If we look through the Cornea into the bottom of a recent Eye, (say of the sheep or ox, as these are the most readily obtained) we shall perceive a bright shining surface, which is more or less brilliant according to the time the Eye has been kept, and the consequent transparency of the coats and humours. It is still better, however, to remove the Cornea, and take out carefully the humours and the Retina; we shall then have a perfect view of the part in question.

By this last process we shall find that this beautiful part is immediately behind and in con-

tact with the Retina, or to speak more anatomically, with the exterior coat of the Retina, now called the Tunica Jacobi, in honour of its discoverer, Dr. JACOB, of Dublin.

I would ask then, what is this brilliant surface which we find placed immediately behind the Retina, and which we can any day see in the dusk, glittering like gold in the Eye of the domestic cat?

If we refer for an answer to any standard anatomical work, we shall find that what is termed the Pigmentum of the Eye, is immediately behind and in contact with the Retina; therefore this bright surface is the Pigment of the Eye.

In proof I need but quote Bell's Anatomy, than which we have not a better treatise in our language—

"The pigmentum nigrum is the black or brown mucous substance, which lies between the choroid coat and the retina." "This matter is in *immediate contact with the medullary pulp of the optic nerve.*" "The *dark colour of the secreted pigment* is in some measure peculiar to those animals which see in the brightest light of day; but is *wanting, or of a bright reflecting green or a silvery whiteness, in such as prowl by night.*"

Another remarkable sentence in the same work tends very much to confirm my opinions.

In describing the Choroid, Sir C. BELL observes —"And it has upon its inner surface a PIGMENT, *which being sometimes firm, might be taken for a* MEMBRANE."*

It may, however, be said, that this bright part is the Tapetum. If so, I should be glad to have it defined what is the Tapetum ?† And this question, simple as it may sound, is not so easy of

* Bell, III. 33.

† "The *internal surface*, (says Sir C. Bell) of the Choroid coat, has long been called the Tapetum, from its villous or fleecy appearance, when seen through the microscope. This surface, in the adult, is of a brown colour; in very young subjects, it is red and bloody; and when minutely injected, it is like scarlet cloth. It is by this vascular surface or Tapetum, that the black Pigment, which is laid under the expanded Retina, in the human Eye, is secreted."

In a note, the same Author observes, he "cannot conceive how the Pigment should be confounded with the Tapetum." "The name of Tapetum or Tapis was first given by the French Academicians, in their account of the dissection of a lioness." "The membrane which is put into the bottom of the Eye, and laid on the Choroides, which we call the Tapetum, was of an isabella colour, intermixed with a greenish blue. It was easily separable from the Choroides, which remained entire, with its ordinary thickness, after that we had taken away the membrane which forms the Tapetum."‡

Finally, he says—"In regard to the Choroid coat, we have to understand that it consists of two laminæ; the outer and that which is next to the Sclerotic, being the proper Choroid; the internal lamina the Tunica Ruyschiana: that on the surface of the Ruyschiana there is a pile or fleece which is called the Tapetum; and lastly, that the *secretion of this inner surface is a Pigment*, which, in the human Eye, has the appropriate name of *Pigmentum nigrum*; but, in many animals, *it is of a silver, golden, or*

‡ Bell's Anatomy, III. 33.

solution. Some regard Tapetum and Pigmentum as synonimous terms; others think the Tapetum

isabella colour; though, in my apprehension, *the colour, in all these varieties, depends still upon a peculiar secreted matter.*"

With all due deference to the high authority of this account, (and more especially as from my respected tutor, some years since,) my opinion is, that so far from rendering the matter clearer, it makes it still more obscure. In page 34, Sir C. Bell observes—"The *internal part*" (of the Choroides,) again, is organized into a *secreting surface*, and is the Tunica Ruyschiana." Therefore, according to this plan, the Choroides has *two internal surfaces, both of which are to secrete the Pigment.* These two different parts (as Sir C. Bell states them to be,) occupy the same place, and perform the same functions. The late Mr. Shaw, (Sir C. Bell's assistant at the Anatomical Theatre,) we shall find advocating the identity of these two with each other, and regarding them as synonimous.

Indeed Sir Charles himself, in what may be called the summing up of his account of the Choroides, (and which is quoted at length above,) seems rather sensible of this difficulty. He makes the Choroides consist of Two Laminæ; the *external*, the proper Choroid; the *internal*, the Ruyschiana; upon the Ruyschiana he places the Tapetum, which he defines as a *pile* or *fleece*, (the strict anatomical meaning of which term I confess I cannot comprehend, for it is evident it is not intended to be regarded as a distinct membrane, nor as a lamina of the Choroid; for then the Choroid would have been stated to consist of *three laminæ,)* and *upon the Tapetum, and secreted by it*, (that is by this pile or fleece, the Tapetum, and not by the Ruyschiana) he places the Pigmentum, which is of various colour in different animals. It is of no importance however, to me, what is meant by the term Tapetum, unless it be synonimous with Pigmentum. I am only sorry that it is not in my power to obtain a perusal of the original account of the dissection of the lioness by the French Academicians, from which Sir Charles Bell has translated a portion (and no doubt it will be the most important part) in his work.

The Tapetum has never, within my recollection, been shewn in the British Anatomical Schools, as a separate membrane of the Eye, nor as a lamina of the Choroides. Had either of these been done, it would long ere this, have been generally known. Of course the Pigmentum nigrum has never been shewn as a membrane, and it is with this last only that I have to do, in its situation behind the Retina.

a new name for the Tunica Ruyschiana; and Sir CHARLES BELL defines it as a "pile or fleece laid upon the Ruyschiana," and upon it, he says, the pigment is laid, to be in contact with the Retina. The celebrated CUVIER, whose loss the scientific world will long and deeply deplore, when speaking of the Tapetum, says—"The bottom of the Ruyschiana is frequently covered with a very slight coat of this *Pigment*, through which we can perceive its colour, which varies remarkably in different species."

Our illustrious countryman, JOHN HUNTER, published an essay "on the colour of the Pigmentum Nigrum of the Eye," in which he treats of all the beautifully varied colours we find in the eyes of different animals, as the true Pigment of the Eye. The remarks of Dr. BOSTOCK, in his beautiful work on Physiology, are also to the same purpose. RICHERAND embraces the same opinion. FYFE, who seems inclined to deny the division of the choroides into Choroid and Ruyschiana, as did HALLER, ZINN, BICHAT,*

* Pour voir la structure de la Choroide, il faut la soumettre pendant quelque temps à la macération. Privée alors de son enduit, et en partie de sa couleur ordinaire, qu'elle doit un peu à cet enduit, mais qui lui est aussi inhérente, comme je l'ai dit, elle devient transparente presque comme une membrane séreuse. *Sa ténuité et son peu d'épaisseur, lorsqu' elle est ainsi réduite à elle même, ne permettent point de lui distinguer* LES DEUX LAMES *dont plusieurs ont parlé, et dont l'existence isolée a déjà été*

and others, observes, "Upon the inner side of the Choroides, there is a mucus termed Pigmentum nigrum." "In graminivorous animals, and in those which go in quest of prey in the night, the *Pigmentum is of a light and shining colour at the bottom of the Eye, and is called Tapetum.*"

Nothing can be more clear and decided than this. The late Mr. SHAW is the only writer, that I know of, who advocated the identity of the Tapetum and Ruyschiana. He says—"The external part is called the *true Choroid*, from its resemblance to the chorion of the fœtus,—the inner part has, in honour of the discoverer, been called *Tunica Ruyschiana.* The variegated colour of the internal surface in some animals

rejetée PAR TOUS LES VRAIS ANATOMISTES. *On n'y trouve qu'un seul feuillet qui se rompt par le moindre effort.*

La nature de cette membrane est entièrement inconnue.

Speaking of the Pigment, Bichat observes—"On peut la soumettre aux divers agens chimiques soit sur le Choroide elle même, soit sur le papier auquel elle donne une teinte solide que l'air n'altère point, comme si m'en suis souvent assuré. J'ai du papier teint ainsi depuis six mois, et qui est comme le premier jour. Fixée sur le papier et soumise aux Acides Sulphurique, Nitrique, Muriatique, &c., à l'Ammoniaque, á l'Alcool, a la dissolution de Potasse Caustique, elle est de même absolument inaltèrable. C'est à cette couleur particuliere que l'on rapporte les usages de la Choroide, destinée probablement à absorber les rayons lumineux qui ne doivent point servir á la vision."—*Bichat Traitè d'Anatomie, tom.* 2, 436.

having some resemblance to the colour of fine tapestry, induced the Parisian dissectors to give it the name of *Tapetum*." It is therefore evident that the term Tapetum is at best ambiguous in its signification; but if we are inclined to abide by the opinion of the majority, this will be decidedly in favour of the Tapetum and Pigmentum being synonimous terms. Now as all Anatomists agree that the Pigmentum is placed immediately in conjunction with the Retina, and as the part to which I wish to draw your attention is situated immediately in conjunction with the Retina, I apprehend that the following will be the only proposition I shall have to prove; *viz.*— That the part situated immediately behind and in connection with the Retina is Membrane, and not Pigment. The first object, therefore, will be to define the nature of the true Pigment of the Eye.

In Bell's Anatomy, we find the following description :—" The Pigmentum Nigrum is the black or deep brown substance which lies between the Choroid coat and Retina. It is of a nature *to be washed away with a little water and a soft pencil.* This brown tint pervades the whole texture of the Choroid. *This matter is in immediate connection with the medullary pulp of*

*the optic nerve.** As I have already quoted, Sir C. BELL states, that the colour varies in different animals. According to the analysis of Dr. YOUNG,

* From the uses ascribed to the Choroid Pigment, it is plain that its situation must be immediately behind the Retina.

"Its use is *apparently* to stifle the rays of light after they have impinged on the sensible surface of the Retina; for we know that blackness is owing to the absorption of the light, as whiteness and colour is the reflection of it from the surface of bodies." And again—"The natural conclusion, therefore is, that the Pigmentum Nigrum subdues the intensity of the impression; while the reflecting colours of the surface, in animals which see in the night, strengthens the effect of the light on the surface of the Retina by repelling it."—*Bell's Anatomy*, III. 36.

—"Destinée *probablement* à absorber les rayons lumineux qui ne doivent point servir á la vision."—*Bichat*, II. 436.

"The use of the Choroid is not so much to afford a covering to the other parts, as to present a dark surface, destined to absorb the luminous rays, when they have produced on the Retina a sufficient impression. If it were not for the Choroid, the light would be reflected; after having impinged on the nervous membrane, its rays would cross, and produce only indistinct sensations."—*Richerand*, 261.

"The Choroid of some animals, more easily separated into two distinct laminæ than that of man, presents at the bottom of the Eye, instead of a darkish, uniformly-diffused coating, a pretty extensive spot of various colours, and in some most beautiful and brilliant. *It is not easy to say what is the use of this coloured spot, known by the name of Tapetum.*"—*Richerand*, 268.

"Between the Choroid and the Retina there is a thin stratum of a black viscid substance, termed Pigmentum Nigrum, probably a secretion of the vessels of the Choroid. Its use has been *supposed* to be to absorb the superfluous rays of light, that might otherwise oppress the sight or render objects indistinct. This is illustrated by those animals which are devoid of Pigmentum Nigrum, as the Albino. In these cases the organ is unable to bear the strong light of day without uneasiness, while at the same time

the Pigment consists of mucus, combined with carbonaceous matter, on which its colour depends. BICHAT has subjected the Choroid Pig-

it can discern objects distinctly by a very small quantity of light. *Hence we find that those animals which seize their prey by night, or whose habits lead them to spend their time principally in darkness, are either without this substance or have it of a lighter colour.*"—*Bostock's Physiology.*

"The Choroid Coat, with its dark paint, serves to *suffocate the rays of light* which pass through the Retina, thereby allowing a distinct image to be formed upon the bottom of the Eye, and preventing the rays from being *reflected* so as to form a *second image*. In those animals in which this coat, or its paint, is of a bright colour, it acts as *a mirror to reflect light* and make the impression stronger."—*Fyfe*, II. 62.

Here we have the Choroid Pigment, serving two entirely opposite purposes, viz. *absorption* and *reflection* of light. Now, if by *reflecting* light it must necessarily form a *second image*, and if this be urged as an objection to reflection in the first instance, why does it not equally apply in the second instance?

It has often been remarked by dissectors, that the Retina is never found stained by the Pigment after death. Yet they are placed in apposition, according to anatomists; and under these circumstances, we should be led to conclude a stain would be unavoidable. The intervention of the membrane I am describing, will, of course, at once account for this fact. I am aware it may be said that Jacob's membrane will prevent this;—and indeed Jacob's membrane is the most convenient membrane imaginable, and has been liberally used by certain Philosophers at Hull, as a Jacob's ladder out of every difficulty. In fact, if there be such a part as I am describing, and which we find in every Eye, (though some boldly deny the evidence of their own senses) it *must* be Jacob's membrane. A transparent colourless substance, and a semiopaque brilliantly-coloured one, present exactly the same appearance, and must be the same thing! —and therefore black and white are identical. If it be not Jacob's membrane, Jacob's membrane is just as good as it, and therefore there can be no occasion for such a membrane. The world has been said to be supported on the back of a tortoise; but what supports the tortoise?

ment to various chemical processes, and states the following curious facts respecting it, which are of importance to my theory :—

"It may be exposed to the action of various chemical agents, either on the membrane itself or upon paper, to which it gives a permanent tint, on which the air has no effect. I have paper touched with it six months since, which is the same as it was the first day. Fixed on paper, and subjected to the action of Acids, Sulphuric, Nitric, Muriatic, &c., to Ammonia, to the solution of Caustic Potash, it remains absolutely unchangeable."

With regard to the use of the Pigmentum, all Anatomists *seem to agree* (for many express themselves far from confidently) that it is intended to stifle the illuminating rays, after they have impinged on the Retina, and consequently to prevent the reflection of light, which they affirm would cause imperfect vision. The reason of their hesitation is evident; because they find a great number of different animals in which, from the colour of the Pigment, there must evidently

The Tunica Jacobi prevents the Tunica Nervosa from being stained; but what prevents the Tunica Jacobi from being stained?

The celebrated Zinn denied the possibility of separating the Retina into two layers: "Alteramque," says he, "ab altera integram detrahi, ultra hominum artem positum esse videtur." Yet now this thin and extremely delicate membrane, is acknowledged to consist of *three* layers. The exquisitely delicate expansion of the nerve is included between two as exquisitely delicate layers of membrane, and the three together are not the ten thousandth part of an inch in thickness.

be reflection instead of absorption of the rays. They observe, however, that these animals generally see better in an obscure light than a bright one, and therefore tell us that reflection of the rays assists their vision in this obscure light. The cause of this contradiction is left unexplained. —According to Dr. Bostock, M. Desmoulins affirms that he has found animals possessing this bright-coloured Pigment seeing perfectly well in the full light of day. The names of the animals, however, are not mentioned.*

I shall now in the second place, proceed to shew a few experiments to prove my proposition.

Take an ox or sheep's Eye, (for with these only have I conducted all my experiments) and making a section of it, parallel to the Cornea but a little below it, carefully remove the Retina and Vitreous Humour; you may then for convenience again divide the remainder into two parts, so as best to present the coloured portion for examina-

* As instances open to all observers, I would name the ox, the sheep, the deer, and the horse: but all these animals, though probably seeing well, certainly see worse than man in bright glare, and it is well known see better than him in obscure light. Any one accustomed to riding by night and day, as medical men are, will know this with regard to the horse. In the horse, the Membrana Versicolor is a fine blue.

tion. Suppose the Eye of an ox thus prepared, we shall find a pretty extensive spot of a bright blue colour, frequently intermixed with yellow and green.

1st—We shall find the Retina is not stained by any of these colours.

2ndly—Take a piece of white paper and apply it to the green, or yellow, or blue surface, you will find it *affords no stain.*

3rdly—Wash it with a camel's hair pencil and water, you will *not remove the colour.*

4thly—Its appearance is bright and polished, like a varnished surface.

5thly—By careful dissection you may (under water) separate it in layers from the Ruyschiana. In the sheep I have several times succeeded in shewing three laminæ. It is spread over the whole internal surface of the Ruyschiana, but varies much in thickness, and consequently in the number of its laminæ. It is thickest where the brilliant colours are found, and thinnest in the circumference where the dark colour of the Ruyschiana is visible through it. A very re-

markable circumstance, as regards it, is, that the extent of the coloured spot varies very much in different animals of the same species;—I have sometimes found it occupying nearly three-fourths of the concavity of the globe, and at others scarcely one-fourth. Sometimes the nature of the colour will vary; thus, for instance, I have twice seen this membrane of a bright yellow in the Eye of the sheep, whereas it is generally of a blueish green. But whatever its colour may be, it still presents a bright and polished appearance, and is essentially performing the same office, though varying in its degree of perfection as a reflector, and causing some difference as to the gradation of light best suited for the perfect vision of the animal.

6thly—The true Pigment, possessing its usual appearance and attributes, will invariably be found in the same Eye wherein we see these bright colours, but it is *behind this membrane*, and most plentiful on the posterior or external surface of the Choroid, in connection with the Sclerotica. Thus in the ox, where this membrane is of a fine blue, (not unfrequently mixed with yellow and green) we find the true Pigment very plentiful, and of an uniform deep brown; in the cat, the membrane is a bright yellow, the

Pigment a rich black ; in the fox, it is much the same ; in the deer, it is a very pale blue, the Pigment a pale brown ; in the dog it is greenish blue, the Pigment brown. The statement, therefore, I before quoted, viz.—" *That the dark coloured choroid paint is wanting, or that it is of a bright reflecting green, or silvery whiteness, in such animals as prowl by night,*" is proved to be incorrect by the instances of the cat and the fox. I have never yet found an Eye in which the true Pigment was wanting, excepting in the Albino animal, under which term I include all white animals with red eyes.

7thly—I will now carefully detach a small portion of this substance, and place it between two pieces of glass. It will be found to present a hard and well defined outline; and on putting the glasses in closer approximation to each other, and then suddenly relaxing them, you will perceive that the substance *expands and contracts. This proves that it possesses the property of elasticity.* Again, view this portion by reflected light, you will observe its usual colour ; but look at it by transmitted light, and it will present a totally different one. Neither of these properties belong to any known Pigment;—as regards the last peculiarity, this membrane evidently follows Sir Isaac New-

TON's laws respecting the colours of thin plates. For instance, a small portion of this membrane, of a pale blue colour by reflected light, presents a yellowish red by transmitted light.

8thly—Having succeeded in obtaining a small piece of the substance, I placed it between two pieces of thin glass, and subjected it to examination with a fine achroamatic Amician microscope, by CHEVALIER.* When viewed as an opaque object, the portion was pale blue; and when as a transparent, yellowish red. With a power of 800 to the diameter, *not only were blood-vessels plainly discernible, but even the globules in those vessels!* By still further increasing the magnifying power to the highest degree of which the instrument was capable, the globules were increased to the size of a very small pin's head. I fancied also that I could trace nervous filaments, but could not quite satisfy myself on that head. This was proof undeniable of its being a membrane.

9thly.—When the Choroid has been so finely injected as to resemble a piece of red cloth, this

* For the use of this splendid instrument, and for his kind assistance in the experiment, I am obliged to M. Gregoire de Lanquetot.

membrane will be found unaltered in appearance, and where the colours exist they will be as beautiful as ever.

Immerse the Eye in spirit for a few days, the colour will disappear, and a whitish filmy semi-opaque membrane will be found covering the Choroid. Re-immerse the preparation in water for a few hours, and the colour will be restored, and the membrane will finally become nearly transparent, for the red injection will be distinctly seen through it. I found this most beautifully exemplified in a preparation I had had for some months by me, and which was minutely injected with size and vermillion.

10thly—Another curious circumstance, as regards this membrane is, that, like the feathers of the humming-bird, it presents different colours according to the angle in which you view it. Take a section of a beast's Eye, in which the colours are vivid, place it in a strong light, and, fixing your eye steadily on one point, walk round the table, you will easily verify this statement.

11thly—We have seen that, according to the experiments of BICHAT, no known chemical agent

has any effect in altering the colour or appearance of the true Choroid Pigment. I shall now proceed to shew a curious experiment, by which the brilliant colours of this membrane may be made to appear and to disappear at pleasure. Take a portion of a beast's Eye, where the colours are bright, and dip it into any dilute acid, (Nitric, Muriatic, or Sulphuric) you will perceive the colours begin to fade and get brown (like the tint of a faded leaf); plunge it into cold water, and on removing it you will find the colours have entirely disappeared, and a black surface is presented to the view; immerse it again in the acid, and on withdrawing it you will find the brilliant colours restored; dip it again in the water, they will vanish, and in the acid they will be restored, as if by the aid of magic! This you may repeat, with the same effect, as often as you please. A solution of Ammonia will produce the same effect.

To explain this phenomenon: I have already stated my belief that the colours of this membrane depend upon the thickness and disposition of the numerous and exquisitely delicate plates of which it is composed; and therefore I conclude that the disappearance and re-production of the colours, by the action of the chemical

agents, is effected by their causing alternate heat and cold, and consequently exciting alternate expansion and contraction in these delicate laminæ.

Lastly—I must offer a few remarks respecting this part of the Human Eye ;—and here I must confess that my opportunities for investigation have been necessarily very limited. I cannot say that I have ever seen a *coloured surface* presented, yet I think it highly probable this may exist in some instances, though not generally. That a membrane of this kind *must* exist, will, I think, be shewn when I come, in the next division of the subject, to treat of its utility in the formation of the image. In the specimens of the Human Eye that I have seen, (after carefully extracting the Retina and Vitreous Humour) I found this membrane bright, polished, and semi-transparent,—the brown colour of the Pigment of the Ruyschiana being visible distinctly through it. In fact it appeared like a fine coat of varnish upon a brown ground. By immersion in Sulphuric Æther it became rather more opaque and somewhat milky in its appearance, and was sufficiently firm to be raised and separated from the Ruyschiana.

Having thus shewn that the surface placed

behind and in connection with the Retina does not possess the usual attributes of a Pigment; that its colours are not the result of any secreted matter; that it consists of an indefinite number of layers, separable from the Ruyschiana; that it possesses *elasticity*, and above all *circulation*, we are, I think, warranted in coming to the conclusion that it is MEMBRANE *and not* PIGMENT.

To this membrane I have given the name of MEMBRANA VERSICOLOR, as in some degree descriptive of its appearance and properties.

It now only remains, in conclusion, to offer some brief observations on its use, and on the necessity that such a membrane should exist.

It is generally assumed and believed, that the image of any object presented to the Eye is pictured upon the Retina,—and by it the sensation producing vision is transmitted along the Optic Nerve to the brain. With this theory I differ most decidedly, and trust to lay before you satisfactory reasons for so doing.

The Retina, during life, is almost universally acknowledged to be *transparent;* and those who do not accord with this opinion call it *semi-transparent.* I believe it to be *perfectly transparent.*

I have seen great numbers of Retinæ, at various periods after death, and always found a degree of opacity in them which, I have no doubt, depends upon the absence of vitality. I once, however, had an opportunity of examining the Eye of a horse (which had dropped down dead suddenly) while all the humours, &c. were quite warm. Here the *Retina was quite transparent*, and possessed internally a very light pinkish-coloured hue, produced, no doubt, by the presence of fluid blood in the Tunica Vasculosa.

If we look at the Eye of the cat, in the dusk, it appears quite luminous,—and this effect can only be produced by the reflection of the rays of light from the Membrana Versicolor, which in this animal is of a bright golden colour. Now if the Retina (which intervenes between the beholder and this bright membrane) were semi-transparent, it would be impossible to have this effect, because the Retina would act like ground glass before a lamp, or gauze before a looking-glass.

Of course, in all philosophical investigation, we are not justified in ascribing to matter properties which are not known and cannot be proved to exist. It is acknowledged that neither

light nor heat produce any sensible effect until they are obstructed in their course,—and, consequently, none will be produced by their passing through a transparent medium. Therefore if the Retina be transparent, no image can be imprinted on it, because the rays of light must pass through it.*

It is acknowledged that there is no susceptibility of vision at the point of insertion of the Optic Nerve, and hence the theory of MARRIOTTE that the Choroides is the true seat of vision.

I have myself seen, in a recent Eye, the light pass through a full quarter of an inch of the Optic Nerve ; and so great was the transparency even of this thickness of nerve, that the light was

* The division of the Optic Nerve causes blindness. The same effect is produced by dividing the Trigeminus. Both, therefore, are essential to the performance of vision. The latter is a Ganglionic Nerve, the former is not. Reasoning, therefore, *a priori*, we should expect to find the Optic the Motor Nerve, the Trigeminus the Nerve of Sensation. As far as we know at present, neither of these conclusions would bear the test of experiment. We find, on the contrary, that the Iris, which is so largely supplied with Nerves from the Lenticular Ganglion, is *insensible to mechanical stimuli*. It appears to be only sensible to light. Yet the other branches of the same Nerve, confer the most exquisite sensibility upon other parts of the face,—nay, even of the eye itself.

The Retina, as we should expect to find, is insensible to stimuli. Still we cannot, at present, avoid believing it to be the true Nerve of Sensation for the production of vision.

not red but straw-coloured when seen after its transmission. How much easier then must it be for light to pass through an expansion of the same nerve, of such extreme tenuity as the Retina?

If any one were to tell us to project an image upon fine transparent glass with a double convex lens, or to heat water with a powerful lens alone, we should think it sufficiently ridiculous; but allow us to place some charcoal at the bottom of the water, in the focus of the lens, and we can make the water boil; or to put a foil behind the glass, and we shall have an image.

So, therefore, it is impossible that the transparent Retina can receive and retain the image of any object. Indeed the experiment may be tried on the Retina itself, after death. I have repeatedly seen that when this membrane has become only partially opaque (as it does soon after death) that if you project an image upon it, that image will be exceedingly faint and nebulous; and allowing the same to take place, *cæteris paribus*, in the living Eye, that image would be totally unfit for the purposes of distinct vision.

But the Retina is transparent, and conse-

quently cannot receive an image; and immediately behind the Retina and in contact with it, we are told, we have the Pigment of the Choroid, the use of which is to stifle and absorb the rays of light after they have passed through the Retina ;—therefore, according to our present system, the image projected by the Crystalline Lens upon the Retina will pass through that membrane and be finally stifled and absorbed by the Pigment of the Choroid, which is absurd.

But even allowing that the Retina is not transparent but semi-transparent, it has been already shewn, that it can but receive an exceedingly faint and nebulous image, which would be totally unfit for distinct vision.

It is evident, therefore, that some other part of the Eye must be appropriated for the reception of the Image.*

I shall now proceed to shew that the membrane I have been describing, is the one intended

* It has been said that the formation of an image is not essential to vision. With this, however, I cannot agree, were there no other reason than the fact *that an accurate image is always depicted in the Eye*, and the consequent presumption implied in supposing that any structure or function of the body is designed in vain.

for this purpose. It presents a bright polished surface of various reflecting power in different animals; having the least reflecting power in animals intended to see well and clearly in a bright light, and the most reflecting power in such as are intended to see well in very obscure light. I term it therefore a Reflecting Membrane, but in so doing it must not be supposed that I intend to compare it to silvered glass, or to a well polished speculum. Different bodies possess very various reflecting powers, and I conceive the membrane in question (especially in the Human Eye,) to have only a low reflecting power. The best substances to which I can compare it, in this point, are polished woods, pearl, and certain sea shells. All these will receive a well defined image from a double convex lens, with but little dispersion of the rays from the surface. I shall now project an image of this candle upon the blue membrane in the sheep's Eye, and you will observe that it receives a perfect image, with little or no dispersion. If on the contrary, I throw the image on the same part (which is of a bright yellow) in a cat's Eye, there will be, *with the same degree of light*, a nebulous image formed from the excessive dispersion of the rays from the surface,—but with a less powerful light we shall have a perfect image.

Hence the inability of these animals to see well in a bright light is from the high power of the reflecting membrane.* How then is vision performed? On this point I rather incline to the theory of De la Hire. I believe that the image of the object is projected through the transparent Retina upon the Reflecting Membrane, which is placed behind and in contact with the Retina. Upon this sensible surface† the stimulus of the rays produces certain undulations, (in the part where the image is depicted); these undulations are communicated to the adjacent (I might almost say *connected)* membrane, the Retina, in which similar undulations are excited, which are propa-

* This intolerance of bright light will be evident to any one who will take the trouble to observe the eye of the cat in full day-light, and especially in glare from the sun. He will find the pupil closed to a mere line, to exclude the admission of too much light,—and the Iris (itself a reflector) actually throwing off the rays from its surface, and yet the animal seeing very imperfectly. See the same animal in the dusk, and we shall find the Iris dilated to the utmost extent, the Reflector glittering in the bottom of the eye like burnished gold, and vision perfectly performed.

† All the Ciliary Nerves which, as we have already seen, are derived from the fifth pair, (the true nerves of sensation of the face) pass upon the outer surface of the Choroid coat, or, piercing it, run along its inner surface to the Iris. In this transit, as Lizars observes, it is more than probable they give off branches. If branches be given to the Choroid, they will doubtless also be distributed to the Membrana Versicolor, and hence its sensibility to the stimulus of light. My experiments have also proved that it is sensible to chemical stimuli.

gated in a series of vibrations along the Optic Nerve to the Sensorium.

As the time allotted me, on this occasion, is necessarily very brief, I shall conclude with a few observations respecting the effects of the various colours of the Membrana Versicolor.

The greater the density and the lighter the colour of the Reflector, the greater will be its reflecting power ; and the less the density and the darker the colour of the Reflector, the less will be its reflecting power.

The more or less intense the rays of light are which fall upon the Reflector, the greater or less will be the possibility of the dispersion of rays from its surface ; and the more perfect the Reflector the less light will be required to produce a perfect image from a double convex lens, and vice versa. Consequently the more perfect the reflecting power of the Membrana Versicolor is, the better it is adapted for vision in obscure light, and the worse for bright light ; and the less perfect its reflecting power, the better it is adapted for vision in full light and the worse for obscure light.

Thus, for instance, in the cat, very little light suffices for the purposes of vision,—and here we have a bright yellow Reflector of considerable power.

In man, on the contrary, who sees best in a strong light, the Reflector is dark coloured, and of the lowest reflecting power. The image is perfectly formed in the day time ; but in obscure light, when the cat sees in perfection, objects are scarcely visible to the Human Eye.

Having now, I trust, satisfactorily shewn that the part behind and in connection with the Retina is Membrane, and not Pigment, and, moreover, demonstrated the necessity that such a membrane should exist, I shall terminate this paper by requesting you will accept my thanks for your kind attention to it.

I. WILSON, PRINTER, LOWGATE, HULL.

A DESCRIPTION

OF THE

SKELETON

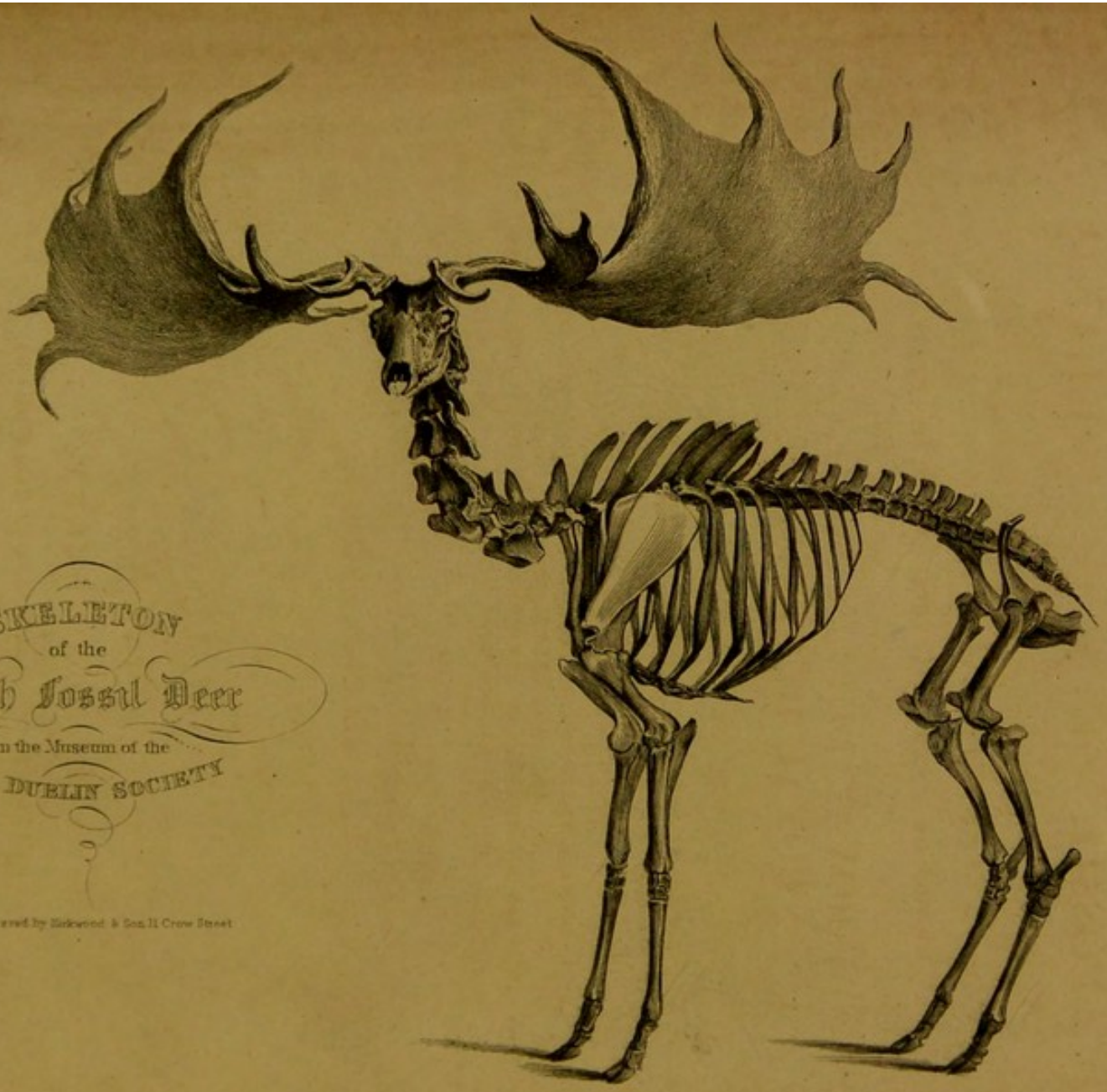
OF THE

FOSSIL DEER OF IRELAND,

&c. &c.

Since the publication of the first Edition of this Memoir, a translation of it into French has appeared in the Annales des Sciences Naturelles, for August 1826; PROFESSOR JAMESON *of Edinburgh has also added it, in the form of an Appendix, to the fifth Edition of his translation of* CUVIER'S *Theory of the Earth.*

PLATE I.



SKELETON
of the
Irish Fossil Deer
in the Museum of the
ROYAL DUBLIN SOCIETY

Drawn & Engraved by Sirkwood & Son 11 Crow Street

Dr Grange
with the Author's
compliments

A DESCRIPTION

OF THE

SKELETON

OF THE

FOSSIL DEER OF IRELAND,

CERVUS MEGACEROS;

DRAWN UP AT THE INSTANCE OF THE COMMITTEE OF
NATURAL PHILOSOPHY OF THE

ROYAL DUBLIN SOCIETY.

By JOHN HART, M.R.I.A.,

MEMBER OF THE ROYAL COLLEGE OF SURGEONS IN IRELAND,
AND OF THE ROYAL DUBLIN SOCIETY,
LECTURER ON HUMAN AND COMPARATIVE ANATOMY AT THE MEDICO-
CHIRURGICAL SCHOOL, PARK-STREET, ETC. ETC.

SECOND EDITION,

WITH AN APPENDIX.

DUBLIN:

R. GRAISBERRY,

PRINTER TO THE ROYAL DUBLIN SOCIETY.

1830.

DESCRIPTION,

&c. &c.

THERE are few sciences for the advancement of which more has been effected within so short a space of time than Comparative Anatomy; and when it is considered that it affords to those devoted to the study of the healing art means of acquiring more correct ideas of the laws of life, by enlarging their views of the animal economy, there are few which can exert a more direct influence over the welfare of society.

It was not expected, however, that a more extended cultivation of this science would have led to a new train of highly interesting speculations regarding the changes which the surface of our globe may have undergone; speculations resting on the degree of certainty with which it is possible to ascertain that the fossil remains of particular genera are found exclusively in particular strata of the earth.

This proposition of drawing geological inferences from anatomical facts originated with the Baron Cuvier, who enjoyed opportunities enabling him to cultivate Comparative Anatomy to a greater extent than, perhaps, any other individual living. Some idea of the nature of this great man's researches may

be formed when he avers that, from the examination of what might be considered a mere fragment of a bone, he could determine the genus of the animal to which it had belonged: nay, he has in some instances ventured to delineate what from analogical inferences he considered to have been the external forms of animals, of whose existence, even at some period far remote, we have no other kind of evidence save that derived from the occurrence of their less perishable parts in a fossil state.

From a combination of anatomical and zoological deductions, we may refer the various organic remains which occur in strata of alluvial formation to two divisions, one including those of animals belonging to species still in existence, the other comprising those of species which have long since become altogether extinct. Of this latter division there are none more deserving of our attention, or more calculated to give rise to feelings of surprise and admiration, than those bones and antlers of enormous size belonging to an animal of the deer tribe, which are almost daily dug out of the bogs and marl pits of Ireland.

So frequently do these remains occur in most parts of this island, that there are very few of our peasantry who are not, either from personal observation or report, acquainted with them by the familiar name of the horns of "the old deer:" indeed in some parts of the country they have been found so often, that far from being regarded as objects of any extraordinary interest, they have been either thrown aside as useless lumber, or applied to the very commonest of economical uses.*

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* In a Report which I made to the Committee of Natural Philosophy of the Royal Dublin Society, and which was printed in their Proceedings

I have made diligent but fruitless search for an account of the particular time when any of these remains were first discovered. As they generally occur in marl, it is most likely that they did not begin to attract attention until the advanced state of agriculture had created an increased demand for that mineral as a manure. We can very easily imagine the astonishment which the appearance of antlers so large, and of such strange form, must have excited in the minds of those who discovered them for the first time, and how readily they obtained a place in the hall of some adjoining mansion, where they were deposited as an ornament of great curiosity, from the contrast which they formed with the antlers of the species of deer known at present. In this way we may account for the preservation of so many specimens as are found in the possession of the gentry in different parts of this country.

The other bones of the animal, although of a large size, yet to common observers appeared to be on a scale so far beneath that of the antlers, that little or no attention was paid to them. This circumstance would appear extraordinary were it not considered that formerly there were in this country very few persons who had applied themselves to the study of Comparative Anatomy, except in a very superficial and general

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of July 8, 1824, I alluded to an instance of a pair of these antlers having been used as a field gate near Tipperary. Since that I have learned that a pair had been in use for a similar purpose near Newcastle, County of Wicklow, until they were decomposed by the action of the weather; there is also a specimen in Charlemont House, the town-residence of the Earl of Charlemont, which is said to have been used for some time as a temporary bridge across a rivulet in the County of Tyrone.

way. Hence they were not competent to form a correct estimate of the size of the animal of which these bones formed a part, much less of the points in which it resembled, or differed from, other animals in its organization.

It is gratifying however to observe, that the value of this science is now more duly appreciated amongst us, and the study of it beginning to receive the attention which its high importance, in several points of view, demands.

Comparative Anatomy can neither be studied with advantage or taught with success, without the assistance of the means of illustration which a well selected museum affords, where the various modifications of structure exhibited by corresponding organs in different animals, are brought together as it were within the compass of a single view.

Three such collections now exist in this city: that of Dr. Macartney at the University, the Museum of the Royal College of Surgeons, planned and commenced by the late Mr. John Shekleton and the collection at the Medical School in Park-street: in addition to these a private collection has been commenced by Dr. Graves, the King's Professor of the Institutes of Medicine.

With the facilities thus afforded, separate courses of lectures on Comparative Anatomy are now annually delivered both at the College of Surgeons and at the Park-street School; and the manner in which they are received shows that this interesting science is beginning to attract its due share of attention.

The interest excited by these remarkable animal remains increased in proportion as the particular branch of science alluded to was more zealously culti-

vated; and a complete skeleton of this stupendous animal had become a desideratum of first rate importance with the scientific world. The first tolerably perfect specimen was found in the Isle of Man, and presented to the Museum of the University of Edinburgh by the Duke of Athol a few years since. About the same time a considerable number of the bones of another, found in the county Down, were forwarded to the Museum of Trinity College by the Bishop of Dromore: these, at the request of Doctor Stokes, the learned Professor of Natural History to the University, I connected in the summer of 1823, by placing the several parts as nearly in their natural relations to each other as it was possible, considering that most of the vertebræ, and many other bones of importance were deficient. The object I proposed was rather by shewing what could be done, to encourage visiters to the Museum to contribute whatever bones opportunity might place at their disposal, than to claim the credit of having given a correct representation of the form of the skeleton from such imperfect materials.

Thus much has been done towards accomplishing this very desirable object, when the following communication was made to the Royal Dublin Society, which I here transcribe from their Proceedings of April 22d, 1824.

"The following letter from Lord Viscount Northland to the Hon. and Rev. John Pomeroy, V. P., together with the extract of a letter from the Archdeacon of Limerick, were read.

"My dear Sir,

"May I beg the favour of you to lay the enclosed Paper before the Royal Dublin Society at their first meeting.

"I am, &c.

"NORTHLAND."

"*Hon. and Rev. John Pomeroy, V.P.R.D.S.*
Dublin, April 9th, 1824."

"Extract of a letter from the Rev. William Wray Maunsell, Archdeacon of Limerick, to Lord Viscount Northland, dated Limerick, April 7th, 1824.

"In a communication from your Lordship, you intimated your approval of the fossil remains of the nondescript deer, which were found at Rathcannon, being presented to the Dublin Society; and I have, I confess, been induced to defer carrying this object into effect, which accords entirely with my views, in the hope that by discovering another perfect head and antlers I should be able to retain the one I have in my possession for myself; but as our operations in that part of the property in which these wonders were found, have ceased, I must, I believe, act the patriot, and request that your Lordship will intimate to your brother, Mr. George Knox, who is Vice-President, that I shall be prepared any time after the 22d of this month to deliver to any one they may be disposed to authorize to receive them, the only skeleton of this extraordinary animal which I believe is to be found in the empire. It will, however, I think be expedient, that some person skilled in anatomy should be selected, as there is such a mass of the component parts of different deer, as would puzzle any but an adept in this science to make a proper selection from. I be-

lieve there are a few of the vertebræ of the back wanting, but there is every other bone, even to the bones of the tongue, the knee, the fetlock, and coffin bones of the foot, and all on a great scale; the marrow is also perfect in some of the bones, and though changed into spermaceti, blazes like a candle. I have also a skull of a dog of a large kind, (at least of a carnivorous animal,) which I found lying close to some of the remains, and which I will transmit with the bones of his old acquaintance.

"Should the Society, upon viewing the specimens, wish to receive an account of the circumstances under which they were discovered, I shall have great pleasure in transmitting them a detailed statement, as I was present when they were all raised."

This very handsome and liberal offer was eagerly embraced by the Society, who did me the honour to appoint me on that occasion to proceed to Limerick, for the purpose of making the proposed selection of materials for a skeleton, and on my return confided to my care the task of conducting the necessary process of articulating it, preparatory to its being deposited in their Museum of Natural History, in which it now stands.

The statement of the circumstances under which the bones were found, with their geological position, alluded to in the concluding paragraph of the extract above quoted, has been lately laid before the Society, in a letter from Archdeacon Maunsell to the Right Hon. George Knox, V. P. as follows:

"The following letter from Archdeacon Maunsell to the Right Hon. G. Knox, V. P., being read:

"*Middleton Lodge, March*, 8, 1825.

"My dear Sir,

"I deferred replying to your letter of the 1st, as it was my intention to proceed to Limerick in a few days, and I was anxious to look over notes I had taken, and which I left there, of the circumstances connected with the discovery of the fossil remains which the Royal Dublin Society have received. As I have, however, been obliged to postpone my departure for several days, I can no longer defer offering my best thanks for the kind manner in which you have received the conjectures which I formed upon a subject to which my attention was directed, by having fortunately been present before the bones were disturbed from the situation in which they had lain during a period which I apprehend it would not be easy to define. I am sensible that any consideration which may have been attached to my observations should be attributed to the interest which the subject itself is calculated to excite, rather than to any ability of mine to do it justice. The opinion which I took the liberty of communicating to you was formed after some consideration, and although I had not the most remote idea of its being worthy of any attention, I can have no objection to your making any use of it which you may conceive expedient. There is, I conceive, much interesting material for speculation, resulting from the discovery of these fossil remains, and the first that naturally occurs is the manner in which the animals were destroyed, and the bones so singularly preserved. I stated, in the hasty sketch which I gave you of my theory upon this point, that I apprehended they must have been

destroyed by some overwhelming deluge, that they were probably drowned upon the hills where they had taken refuge, as the waters rose, and that, as they subsided, they were drawn from thence into the valley in which they were found; that the agitation of the waters had occasioned such a dispersion of the bones, when the ligaments dissolved, as would account for their having been scattered in the way in which they were found, and that the deposit of shell marl, with which I supposed the water to have been turbid, had so completely protected them from atmospheric influence as to prevent their subsequent decomposition. To enable you to form some estimate of the reasonableness of this supposition, it is necessary that I should endeavour to explain the situation, &c. of the valley and the adjoining hills. The valley in which the remains were found contains about twenty plantation acres, and the soil consists of a stratum of peat about a foot thick, immediately under this a stratum of shell-marl, varying from $1\frac{1}{2}$ to $2\frac{1}{2}$ feet in thickness; in this many of the shells retain their original colour and figure, and are not marine; under the marl there is a bed of light blue clay: through this one of my workmen drove an iron rod, in several places, twelve feet deep, without meeting opposition. Most of the bones and heads, eight in number, were found in the marl; many of them, however, appeared to rest on the clay, and to be merely covered by the marl. The remains were disposed in such a manner as to prevent the possibility of ascertaining the exact component parts of each skeleton; in some places portions were found removed many yards from others, and in no instance were two bones found lying close to each other. Their position was also singular; in one place

two heads were found, with the antlers entwined in each other, and immediately under them a large blade bone; in another, a very large head was discovered, and although a most diligent search was made, no part of the skeleton found; within some hundred yards, in another, the jaw-bones were found, and not the head. The concluson which, I conceive, may fairly be deduced from such a position of the various parts of the animals is, that there must have been some powerful agent employed in dispersing them after their death; and as I consider it impossible that their own gravity could have been sufficient to sink them through the various strata, I conceive these must have originated subsequently to the dispersion of the bones. I also think, that if they had been exposed for some time to atmospheric influence they never could have been preserved in their present extraordinary perfection.

"The hills immediately adjoining this valley are composed of limestone, with a covering of rich mould of various degrees of thickness. One of them, whose base is about thirty acres, rises directly from the edge of the valley, with sides very precipitous, and in one place perfectly perpendicular, of naked limestone. In every part of this hill the superficies comprises as much stone as mould; on the side nearly opposite, the hill is equally high, but the sides not so steep, and the covering of mould thicker; on the other sides the ground only rises in some degree (twenty or thirty feet perhaps) and consists of a thin mould, and immediately under *very hard* limestone gravel. Indeed, except where limestone forms the substratum, this is the character of all the soil in the vicinity except the Corkasses, which are evidently alluvial. I am fully

aware, that assuming the destruction of the animals to have been occasioned by a flood, they would naturally have retreated from the water to the hills, and that, as they probably met their fate there, their remains should have been discovered on the summit of the hills, and not in the valley, particularly as one of them is perfectly flat on the top, which contains six or seven acres. I apprehend that the remains of many of them were deposited on the tops of the hills, but as they have *now* only a slight covering of mould, not sufficient to cover a small dog, they were formerly perfectly bare; and as they were thus devoid of the means of protecting the remains from the atmosphere, whatever was left there soon became decomposed, and resolved into portions of the mould, which is now to be found on the hills. This remark I conceive also to be applicable to the soil with the substratum of limestone gravel, which affords quite as little material for preserving the bones as the hills do.

" It is material that I should observe that of eight heads, which we found, none were without antlers; the variety in character also was such as to induce me to imagine, that possibly the females were not devoid of these appendages: unfortunately, however, from the difficulty of raising them, being saturated with water, and as soft as wet brown paper, only three were at all perfect. Having now disposed of these antediluvians, a question naturally arises how it happens that the fossil remains of no other animals were found, when the same fate probably overwhelmed every existing creature? Could deer have been the only living beings at that period? Was Ireland part of a great continent when this catastrophe occurred, and were these unfortunates the first emigrants to

our isle from that great centre, from whence the Globe was supplied with occupants, and did they perish before other animals less influenced by enterprise, and less endowed with physical strength, could have followed their example? These problems I confess myself unable to solve, and shall not presume to obtrude my many reveries upon this and other points, which have originated in the discovery of a few bones, upon those who I know are so much better competent to form a sound opinion. I shall, I hope, be able to send the antlers, which are very fine, on the 15th of this month.

"If you have any desire to make use of this letter, I can only say I can have no objection. I remain, dear Sir, with feelings of great respect,

"Yours most truly,

"WILLIAM W. MAUNSELL."

Although it is obvious that the interests of science can be more effectually promoted by the co-operation of its cultivators, regardless of artificial distinctions originating in difference of nation, or party, still it must be admitted that such distinctions are not altogether unattended with benefit, so far as they are made a source of honourable emulation.

With this qualification we may well be excused in the present instance, for feeling a degree of national pride that our native Institution for the encouragement of the Arts and Sciences, should have been the first public body in Europe to obtain a perfect specimen of the skeleton of this, one of the most remarkable animals which ever existed; and which, although not exclusively indigenous to, yet seems to

have had its favourite haunts in our fertile plains and valleys."*

This magnificent skeleton is perfect in every single bone of the frame-work which contributes to form a part of its general outline: the spine, the chest, the pelvis, and the extremities, are all complete in this respect; and when surmounted by the head and beautifully expanded antlers, which extend out to a distance of nearly six feet on either side, forms a splendid display of the reliques of the former grandeur of the animal kingdom, and carries back the imagination to a period when whole herds of this noble animal wandered at large over the face of the country.

To proceed with a description of the several parts of this specimen in detail, I shall commence with the antlers, which give the animal its chief characteristic feature.

The antlers.—That the description of these may be the more intelligible, I will first explain the terms which I mean to apply to their several parts. Each antler consists of the socket or root, the burr or coronary circle, the beam or shaft, the palm and the branches.

The socket or root is the part of the antler which grows out of the frontal bone, and which is never shed; it is smooth, of a brown colour, an inch and half in length, and eleven inches three quarters in circumference; in the animal's life-time it was covered by the skin. The coronary or bead-like circle, or burr, is a

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* Portions of these remains have been found in Yorkshire, and by Parkinson on the coast of Essex. They have been found in the Isle of Man, different parts of Germany, in the forest of Bondi near Paris, and, according to Cuvier, in many parts of Lombardy near the Po.

ring of small, hard, whitish prominences, resembling a string of pearls, which encircles the junction of the socket with the part of the antler which falls annually from the heads of all deer.

The beam or shaft extends outwards with a curvature whose concavity looks downwards, and backwards. This part is nearly cylindrical at its root, and its length equals about one-fourth of that of the whole antler; its outer end is spread out and flattened on its upper sarface, and is continous with the

Palm, which expands outwards in a fan-like form, the outer extremity of which measures two feet ten inches across, being its broadest part. Where the beam joins the palm the antler undergoes a kind of twist, the effect of which on the palm is, to place its edges above and below, and its surfaces anterior and posterior; the anterior surface is convex, and looks outwards; the posterior is concave, and its surface looks towards that of the opposite palm. Such is the position of the antlers, when the head is so placed that the zyomatic arch is parallel to the horizon, as it would be were the animal in the act of progression, or standing in an easy posture.

The branches are the long pointed processes which project from the antlers, two of which grow from the beam anteriorly; the first comes off immediately from the root, and is directed downwards, overhanging the orbit; this is called the brow antler, which in this specimen is divided into two points at its extremity.*

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* I have seen this antler divided into three points in two specimens, one at the Earl of Besborough's, county Kilkenny, (which measured eight feet four inches between the tips,) the other in the hall of the Museum of Trinity College: it is single in the greater number of specimens, as in those which Cuvier describes.

The other branch, which comes off from the beam, the sur-antler: in this specimen it consists of a broad plate or palm, concave on its upper surface, horizontal in its direction, and forked into two points anteriorly, an appearance which I have not observed in any other specimen of upwards of forty which I have seen, nor do I find it marked in any of the plates of those bones extant.

There is one long branch given off posteriorly from the junction of the beam with the palm: it runs directly backwards parallel to the corresponding one of the opposite antler. The inferior edge of the palm beyond this runs outwards and backwards: it is obtuse and thick, and its length is two feet six inches. From the anterior and external borders of each palm there come off six long pointed branches. None of these are designated by any particular name. The number of the branches of both sides, taken together, is twenty-two.

The antlers are of a lightish colour, resembling that of the marl in which they were found; they are rough, and marked with several arborescent grooves where the ramifications of the arteries by which they had been nourished during their growing state were lodged. The antlers of this specimen, with the head attached, weighed eighty-seven pounds avoirdupois. The distance between their extreme tips in a right line is nine feet two inches.

Head.—The forehead is marked by a raised ridge extended between the roots of the antlers; anterior to this, between the orbits and the root of the nose, the skull is flat; there is a depression on each side in front of the root of the antler and over the orbit capable of lodging the last joint of the thumb, at the bot-

tom of which is the superciliary hole, large enough to give passage to an artery proportioned to the size of the antlers. Inferior to the orbit we have the lachrymatory fossa, and the opening left by the deficiency of bone common to all deer, and remarkable for being smaller in this than in any other species.

Below the orbits the skull grows suddenly narrower, and the upper parts of the nasal bones become contracted by a depression on either side, at the lower part of which is the infra orbitar hole. The opening of the nares is oval, being five inches long by three broad, the greatest breadth being in the centre. From the roots of the antlers to the occipital spine measures three inches and an half; the occiput descends at a right angle with this, being three inches deep to the foramen magnum: the greatest breadth of the occiput is eight inches. The temporal fossæ approach to within two inches of each other behind the horns.

Teeth.—The inscisors were not found, having dropped out; there is no mark of canine teeth, the molares are not much worn down, and are twenty-four in number. The dentition stands thus: inscisors $\frac{0}{8}$, canini $\frac{0}{0}$, molares $\frac{6}{6} - \frac{6}{6}$.

The skeleton measures, from the end of the nose to the tip of the tail, ten feet ten inches. The spine consists of twenty-six vertebræ, viz. seven cervical, thirteen dorsal, and six lumbar. The size of the cervical vertebræ greatly exceeds that of the other classes, and the spines of the dorsal rise to a foot in height. The necessity of these bones being so marked is obvious, considering the strong cervical ligament, and powerful muscles, required for supporting and moving a head, which, at a moderate calculation, must have

sustained a weight of three quarters of a hundred of solid bony matter.

The extremities are in proportion to the different parts of the trunk, and present a conformation favourable to a combination of great strength and fleetness.

It is not the least remarkable circumstance connected with these bones, that they are in such a high state of preservation as to present all the lines and impressions of the parts which had been attached to them in the recent state. Indeed if we examine them as compared with the bones of an animal from which all the softer parts have been separated by maceration, the only perceptible differences in their physical properties are, that they are a little heavier, a degree harder, that their surface is brown, and that they all, with the exception of the antlers, present a polished appearance, which is owing to the periosteum having been preserved, and still remaining to cover them, as was first discovered when they were chemically examined.

The existence of fat or adipocire in the shaft of one of the bones mentioned by Archdeacon Maunsell, and which I saw in his possession, is a thing for which it is extremely difficult to account, as it occurred but in one solitary instance, and it did not appear that this bone was at all differently circumstanced from the rest. Those which I had an opportunity of examining, by boring holes in them, were hollow, and contained, for the most part, only a small quantity of black animal earth.

I requested Doctor William Stokes to make an analysis of a small fragment of a rib, which he found to contain the following constituents :

Animal matter, ...	42.87
Phosphates with some Fluates, ...	43.45
Carb. Lime, ...	9.14
Oxides, ...	1.02
Silica, ...	1.14
Water and loss, ...	2.38
	100.00

With a view to ascertain the state of the animal matter, I had a portion of a rib submitted to chemical examination by Dr. Apjohn, at the laboratory of the New Medico-Chirurgical School, Park-street, of the result of which he gave me the subjoined statement:

"I regret that time did not permit my making a more particular examination of the Moose Deer-bone, which was left by you at my laboratory. Knowing that you were in possession of a tolerably correct analysis of its earthy materials, my attention was directed to its animal constituents, which, as the following experiments establish, were found in a state of perfect preservation.

"The bone was subjected for two days to the action of dilute Muriatic Acid. When examined at the end of this period, it had become as flexible as a recent bone submitted to the action of the same solvent. The periosteum was in some parts puffed out by Carbonic Acid Gas, disengaged from the bone, and appeared to be in a state of perfect soundness.

"To a portion of the solution of the bone in the Muriatic Acid some infusion of galls was added, which caused a copious precipitate of a dun colour. This

proved to be Tannate of Gelatine, mixed with a small portion of the Tannate and Gallate of Iron.

"The cartilage and gelatine therefore, so far from being destroyed, had not been perceptibly altered by time.

"Your's very sincerely,

"JAMES APJOHN."

Such result I had expected, and I ventured to predict it in my report already alluded to.*

Until Baron Cuvier published his account of these remains,† they were generally believed to have belonged to the same species as the moose deer or elk of North America, an opinion which appears to have been first advanced by Dr. Thomas Molyneux in 1697,‡ and which depends principally on the exaggerated description of that animal given by Josselyn in his account of two voyages to New England, published in 1674, in which he states that it is sometimes twelve feet high, with antlers of two fathoms wide! This statement was the more readily believed by the learned Doctor, as it tended to confirm him in a favourite theory which he seems to have entertained, that Ireland had once been joined to the New Continent.

But the assertions of Josselyn regarding the size of the American moose have not been confirmed by

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* A gentleman told me of a bonfire which was made of a heap of these bones in a village in the county of Antrim, in celebration of the battle of Waterloo, and the bones were observed to give as good a blaze as the bones of horses, which are usually employed on such occasions.

† Vide Annales du Museum d'Histoire Naturelle, Tom. XII. et Ossemens Fossiles, Tom. IVeme.

‡ Philosophical Transactions, Vol. XIX.

the testimony of later travellers, from whose observations it is now clearly ascertained that the only large species of deer inhabiting the northern parts of America are the wapiti or Canadian stag, *(Cervus Canadensis,)* the rein-deer, *(C. Tarandus,)* and the moose or elk, *(C. Alces.)*

The peculiar branching of the brow antlers of the rein-deer, and the rounded antlers of the wapiti,* are characters sufficient to prevent us confounding either of these animals with the fossil species.

The palmate form of the antlers of the elk gave greater probability to the opinion of its specific identity with the fossil animal.

A little attention, however, to a few circumstances, will shew a most marked difference between them.

First, as to size, the difference is very remarkable, it not being uncommon to find the fossil antlers ten feet between the extreme tips,† while those of the largest elk's antlers never exceed four feet. A pair in the Museum of the Royal Dublin Society measure three feet seven inches; the largest pair seen by Pennant in the house of the Hudson's Bay Company measured thirty-four inches.‡

The antler of the elk has two palms; a greater and a lesser, the latter of which grows forward from that part of the front of the beam where the principal palm

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* A fine pair of this species, male and female, were exhibited by Mr. Bullock in this city a few summers ago. They did not answer to any description of Pennant or of Dr. Shaw, but had the characters of C. Canadensis as given by Cuvier.

† Dr. Percy, Bishop of Dromore, describes a pair which measured fourteen feet by the skull. Archælogia Britt. Vol. VII.

‡ Pennant's Zoology, vol. I.

begins to expand, called brow antler by Cuvier, but it corresponds in situation rather to the sur-antler, there being, properly speaking, no brow antler attached to the root of the beam. The elk has no posterior branch similar to that of the fossil animal, nor does its beam take a similar arched direction, but runs more directly outwards.

Cuvier remarks, that the palm of the fossil antler increases in breadth as it extends outwardly, while that of the elk is broadest next the beam.

The palm of the elk's antler is directed more backwards, while the fossil one extends more in the lateral direction. The antlers of the elk are shorter and more numerous than those of the fossil deer.

As the antlers of the fossil animal exceed in size those of the elk, so on the contrary does the skull of the latter exceed in size that of the former; the largest heads of the fossil species not exceeding one foot nine inches in length, while the head of the elk is frequently two feet. The fossil head is broader in proportion; its length being to its breadth as two to one; in the elk they are as three to one, according to Parkinson.* The breadth of the skull between the roots of the antlers is but four inches in the fossil skulls; in that of the elk in the Society's Museum it is $6\frac{1}{2}$ inches.

Cuvier thinks it probable that the females of the fossil species had antlers,† an opinion to which I am very much disposed to subscribe, from having observed that these parts present differences in size and strength, which appear not to be dependent on diffe-

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* Organic remains, vol. III. † Ossemens Fossiles, tom. IV.

rences of age; for instance, the teeth of the specimen in Trinity College are much more worn down, and the sutures of the skull are more effaced than in the specimen described in this paper; yet the antlers of the latter are much more concave, and more expanded than those of the former; and on comparing a single antler of each of these specimens together, that belonging to the Society exceeds the other by nearly a sixth in the length, and little less than a third in the breadth; it is not therefore unlikely that the animal whose antlers were larger and more curved was a male. Something similar to this is observed in the rein-deer, both sexes of which have antlers, but with this difference, that they are smaller and less branched in the female.

Hence we find that this animal possessed characters of its own sufficient to prove it of a species as distinct from the moose or elk as this latter species is from the rein deer or any other; therefore, it is improper to retain the name of elk or moose deer any longer; perhaps it might be better called the *Cervus megaceros*, a name merely expressive of the great size of its antlers.

That this animal must have shed its head furniture periodically, is proved by the occasional occurrence of detached antlers having the smooth convex surface below the burr, similar to what is observed on the cast antlers of all deer. Specimens of this are to be seen in the Museum of Trinity College, and I possess one myself, of which I have had a drawing made. As every other species of deer shed their antlers annually, there is no reason for supposing that that process occurred at longer intervals in this.

It is a popular opinion with the Indians that the elk is subject to epilepsy, with which he is frequently

seized when pursued, and thus rendered an easy prey to the hunters. Many naturalists affect to believe this account, without, however, assigning any sufficient reason. But if it be considered, that during the growth of the antlers there must be a great increased determination of blood to these parts, which are supplied by the frontal artery, a branch from the internal carotid, it is quite conformable to well established pathological principles to suppose, that after the antlers are perfected, and have ceased to receive any more blood, that fluid may be determined to those internal branches of the carotid which supply the brain, and establish a predisposition to such derangements of its circulation as would produce epilepsy, or even apoplexy: if such an effect were produced in consequence of the size of the antlers in the elk, it is not unlikely that it prevailed in a greater degree in the fossil animal whose antlers were so much larger.

What could have been the use of these immense antlers? It is quite evident that they would prevent the animal making any progress through a country thickly wooded with large trees, and that the long, tapering, pointed antlers were totally unfit for lopping off the branches of trees, a use to which the elk sometimes applies his antlers,* and for which they seem well calculated, by having their branches short and strong, and set along the edge of the palm, somewhat resembling the teeth of a saw in their arrangement. It would rather appear then, that they were given the animal as wea-

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* The elk, when pursued in the forests of North America, breaks off branches of trees as thick as a man's thigh.—Josselyn.

pons for its protection, a purpose for which they seem to have been admirably designed; for their lateral expansion is such, that should occasion require the animal to use them in his defence, their extreme tips would easily reach beyond the remotest parts of his body; and if we consider the powerful muscles for moving the head, whose attachments occupied the extensive surfaces of the cervical vertebræ, with the length of the lever afforded by the antlers themselves, we can easily conceive that he could wield them with a force and velocity which would deal destruction to any enemy having the hardihood to venture within their range.

It is not unlikely that the possessor of those formidable antlers was exposed to aggressions of some carnivorous animals of ferocious habits; and such we know to have abounded in Ireland, as the wolf, and the celebrated Irish wolf dog. Nor would it be surprising if limestone caves should be discovered in this country containing the remains of beasts of prey and their victims, similar to the hyæna's dens of Kirkdale, and other places, respecting which such interesting researches have been lately laid before the public by Professor Buckland, of the University of Oxford.

The absence of all record, or even tradition respecting this animal,* naturally leads one to inquire whether man inhabited this country during its existence? I think there is presumptive evidence in the affirmative of this question afforded by the following

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* It is evidently not the animal mentioned by J. Cæsar, under the name of Alces, vide Comment. de Bello Gallico VI. Caput X. Nor is it the Alces of Pliny.

circumstances: a head of this animal described by Professor Goldfuss of Bonn, was discovered in Germany in the same drain with several urns and stone hatchets; and in the 7th volume of the Archæologia Britannica, is a letter of the Countess of Moira, giving an account of a human body found in gravel, under eleven feet of peat, soaked in the bog water:— it was in good preservation, and completely clothed in antique garments of hair, which her ladyship thinks might have been that of our fossil animal. But more conclusive evidence on this question is derived from the appearance exhibited by a rib, presented by Archdeacon Maunsell to the Royal Dublin Society, in which I discovered an oval opening near its lower edge, the long diameter of which is parallel to the length of the rib: its margin is depressed on the outer, and raised on the inner surface, round which there is an irregular effusion of callus. This opening appears evidently to have been produced by a sharp pointed instrument, which did not penetrate so deep as to cause the animal's death, but which probably remained fixed in the opening for some length of time afterward; in fact such an effect as would be produced by the head of an arrow remaining in a wound after the shaft was broken off.*

It is not improbable, therefore, that the chace of this gigantic animal once supplied the inhabitants of this country with food and clothing.

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* I am well aware of the occasional existence of holes in the ribs, a few instances of which I have seen in the human subject: but they differ essentially in character from the opening here described, as they occupy the centre of the rib, mostly at its sternal extremity, and have their margins equally depressed on both surfaces.

As to the causes which led to the extinction fo this animal, whether it was suddenly destroyed by the deluge, or by some other great catastrophe of nature, or whether it was ultimately exterminated by the continued and successful persecution of its pursuers, as has nearly been the case with the red deer within the recollection of many of the present generation, I profess myself unable to form any decided opinion, owing to the limited number of facts as yet collected on the subject. On some future occasion I may, perhaps, be induced to revert to so interesting a topic, should I have opportunities of discovering any thing worthy of communication.

The following Table exhibits a comparative view of the measurements of different parts of the skeletons of the Cervus Megaceros in the Museum of the Royal Dublin Society, and in the University of Edinburgh, with some parts of the Moose:

	R. D. Soc.		U. of Edin.		Moose.	
	Ft.	In.	Ft.	In.	Ft.	In.
Length of the head, ...	1	8½	1	8¼	——	
Breadth of the skull between the orbits, ...	0	10½	0	9	——	
Breadth of the skull at the occiput, ...	0	8	——		——	
Diameter of the orbit, ...	0	2⅜	0	2½	——	
Distance between infra orbitar holes across the skull,	0	7	——		——	
Length of alveolar processes of the upper jaw, ...	0	6	0	6	——	
Length of lower jaw, ...	1	5½	1	3½		
Diameter of foramen magnum,	0	2	——		——	
ANTLERS.						
Distance between the extreme tips, measured by the skull,	11	10	——		——	
Ditto, in a straight line across,	9	2	6	8	3	7
Length of each antler, ...	5	9	5	1	——	
Greatest breadth of the palm,	2	10	——		——	
Length of the beam, ...	1	9	——		0	6½
Ditto of brow antler, ...	0	8¾	——		——	
Ditto of sur-antler, ...	1	4	——		——	
Circumference of the beam at the root of the brow antler,	1	0¾	——		0	7½

	R. D. Soc. Ft.	In.	U. of Edin. Ft.	In.	Moose. Ft.	In.
BODY.						
Length of spine, ...	10	10	9	8	——	——
Ditto of sternum, ...	2	4	——	——	——	——
Height to the upper extremity of the dorsal spines, ...	6	6	——	——	——	——
Ditto to the highest point of the tip of the antler, ...	10	4	——	——	——	——
EXTREMITIES.						
Greatest length of the scapula,	1	$6\frac{1}{2}$	——	——	——	——
Ditto breadth at the base,	3	$10\frac{3}{4}$	——	——	——	——
Ditto depth of its spine, ...	0	$2\frac{3}{4}$	——	——	——	——
Length of the humerus, ...	1	4	1	$3\frac{1}{2}$	——	——
Ditto of ulna and radius, ...	1	8	1	6	——	——
Ditto of carpus, ...	0	$2\frac{3}{4}$	0	2	——	——
Circumference of do.	0	$9\frac{1}{2}$	——	——	——	——
Length of metacarpus, ...	1	$0\frac{1}{2}$	1	$0\frac{1}{2}$	——	——
Length of phalanges, ...	0	7	0	$6\frac{1}{2}$	——	——
From anterior superior spine of one ileum to that of the other, ...	1	$4\frac{1}{2}$	1	$6\frac{1}{2}$	——	——
From anterior superior spine to the tuber ischii, ...	1	8	1	$9\frac{1}{2}$	——	——
Greatest diameter of foramen ovale, ...	0	4	0	3	——	——
Least do. of do.	0	$2\frac{3}{4}$	0	$2\frac{1}{4}$	——	——
Length of the femur, ...	1	$6\frac{1}{2}$	1	$5\frac{1}{2}$	——	——
Ditto of tibia, ...	1	6	1	6	——	——
Length of the tarsus, including the os calcis, ...	0	8	——	——	——	——
Ditto of the metatarsus, ...	1	$1\frac{3}{4}$	1	$1\frac{3}{4}$	——	——

APPENDIX.

SINCE the publication of the former edition of the foregoing paper, I have anxiously endeavoured to collect such additional facts relating to the geological position of our native fossil deer, as might tend to throw some light on the question touching the cause of its extinction.

While engaged in this inquiry, numerous additional instances of the remains of this animal having been found imbedded in marl, were communicated to me from various quarters. To enter into any thing like a detail of these would be tedious as well as superfluous, inasmuch as the result merely serves to confirm what is already sufficiently well established, that marl is the depository in which these remains usually occur. To this general observation, I have succeeded in discovering the existence of one well authenticated exception, in a skeleton of the Irish fossil deer, found in a locality in which its occurrence has not heretofore been noticed in this country.

The particulars concerning the discovery of this skeleton were first mentioned to me by Dr. Gason, of Enniskerry, and conceiving that they are of sufficient interest to claim the attention of the Society, I beg leave to submit the following account of them:

In the Autumn of 1828, while some workmen

were employed in making preparations for planting the southern aspect of a hill of loam sand close to Enniskerry, they dug up several bones belonging to the fossil deer, *C. megaceros*, which lay buried in the loam at a depth of three or four feet below the surface, and at an elevation of about forty feet above the level of the bed of the river which runs at the base of this hill. As the persons into whose hands these remains fell were not aware that any importance could be attached to their discovery, the occurrence attracted no particular notice at the time, in consequence of which the greater part of the bones were lost, or variously dispersed, when the above circumstances became known to the Rev. Robert Magee, who, after some search, recovered a few bones and a fragment of an antler. This latter he presented to the Royal Dublin Society, in whose Museum it is deposited. It consists of the root and part of the beam of the antler of the right side; its length is eleven inches, and its circumference at the base ten inches; a portion only of the brow antler remains, and is much worn, apparently by attrition.

The bones found in this place were not in that high state of preservation, for which the bones of this animal are so remarkable when found in marl: they had less specific gravity, were friable and powdery on the surface, and their projections or processes were generally worn off.

Not being able to ascertain whether duplicates of any particular bone occurred in this instance, I have no means of determining whether these remains had belonged to one or to several individuals.

The hill in which these bones were found is situated on the north bank of the river of Enniskerry,

opposite the village; its height is about sixty or seventy feet above the river: it is one of a series of heaps of diluvial gravel dispersed through an extensive valley, lying between primitive mountains.

This gravel is composed principally of disintegrated granite intermixed with clay, and contains rounded pieces of secondary limestone of various sizes, which are occasionally met with in such quantity that it is profitable to collect and burn them. Through most of the valleys separating these gravel hills, small streams or rivulets run over beds which often contain marl; such is particularly the case with respect to the river of Enniskerry, from the bed of which marl containing a large proportion of carbonate of lime is sometimes raised as manure.

The presence of these bones in the gravel, would seem to warrant the inference, that the destruction of the animal to which they belonged was owing to the same cause which conveyed those large heaps of sand and gravel to the situation they at present occupy; and that this was the work of a vast inundation or deluge, by which the surface of this country was once submerged, appears to be sufficiently evident from the very striking resemblance which these gravel hills bear on a great scale, to the smaller heaps of sand and gravel left in the beds of mountain rivers after floods.

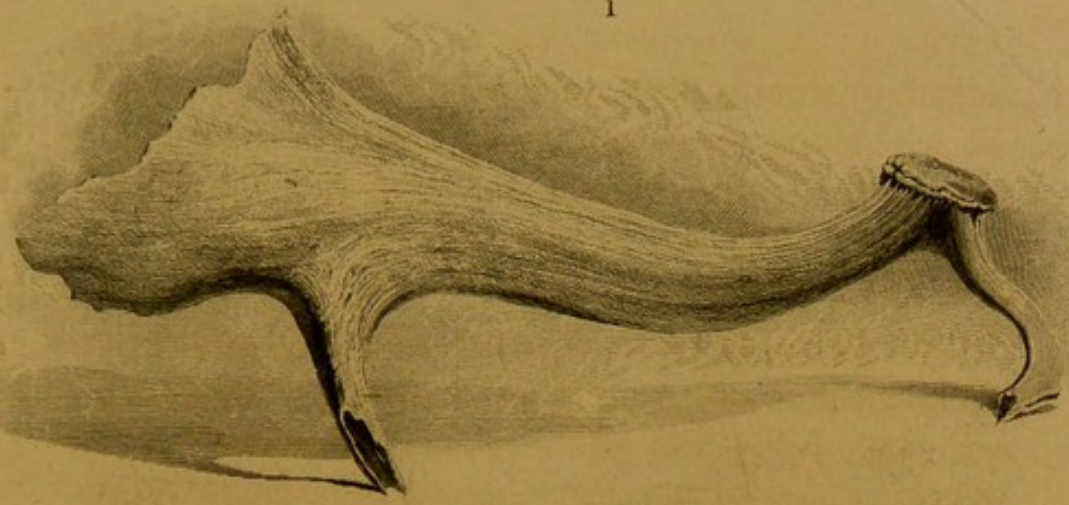
The bodies of animals overtaken and drowned by this inundation, after remaining for a short time under water, would naturally run into a state of putrefaction; and having become inflated by the gaseous fluids disengaged in their interior during that process, they would rise and float on the surface until the soft parts were completely decomposed, when the bones

having their connecting media destroyed, would descend by their own gravity: and should the surface on which they came to rest at the bottom consist of a soft material, they would sink into this to a greater or lesser depth.

It was thus, in all probability, that the bones of the fossil deer came to be deposited in their usual position in the marl, at a time coeval with, or immediately subsequent to the formation of that substance:—while the bones found in the sand would seem to owe their position there to the circumstance of the animal they belonged to happening to have been overwhelmed by the enormous masses of gravel and clay which the water rolled before it in the violence of its first irruption.

PLATE II

1



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3

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EXPLANATION OF THE PLATES.

PLATE I.

A view of the entire skeleton, as it stands in the Museum of the Royal Dublin Society.

PLATE II.

Fig. 1.

The portion of cast antler mentioned in page 26, having the smooth convex surface at its root.

Fig. 2.

External surface of the perforated rib described in page 29.

Fig. 3.

Internal surface of ditto.

Fig. 4.

The portion of antler mentioned in the Appendix.

EXPLANATION OF THE PLATES.

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Fig. 2.

External surface of the perforated rib described in page 30.

Fig. 3.

Internal surface of ditto.

Fig. 4.

The portion of antler mentioned in the Appendix.

ANOTHER CASE

OF

AMPUTATION OF THE THIGH

AT ITS UPPER FOURTH,

IN WHICH

ACUPRESSURE

WAS SUCCESSFULLY EMPLOYED;

WITH REMARKS.

By P. D. HANDYSIDE, M.D., F.R.S.E.,

FORMERLY SENIOR ORDINARY SURGEON TO THE ROYAL INFIRMARY, EDINBURGH.

(*Read before the Edinburgh Medico-Chirurgical Society.*)

REPRINTED FROM THE EDINBURGH MEDICAL JOURNAL, FEBRUARY 1862.

ANOTHER CASE

OF

ACUPRESSURE IN AMPUTATION.

A. A. M., aged forty-one years, of strumous habit, temperate, a native of Edinburgh, received, in 1839, while at Leeds, and leading a wandering life, a severe burn along the right thigh, knee, and leg, from a mass of live cinders that fell on him while he, being exhausted, lay asleep before the fire. He was taken to the Leeds Infirmary, where he remained for four months and a half; at the end of which time there existed an extensive and obstinate ulceration, for which he was transferred by the parochial authorities to Edinburgh, and remained under treatment in the Royal Infirmary for five weeks. He then left the house with the burnt surface partially cicatrized. Thereupon the ulceration opened anew; and, in July 1840, he returned to the Infirmary, and came under my care; when, in addition to treatment for an ophthalmic affection, he had his limb attended to, which, he states, cicatrized before he left the house. A. A. M. afterwards went to Sheffield, where, for an eruptive complaint, he took eighteen blue pills, by which he was profusely salivated. So powerfully was his system affected by this medicine, that mercurial tremours, attended with paralysis of the bladder, supervened. Subsequently the ulceration broke out afresh. After this he wandered to London, and lay in St Bartholomew's under treatment for a spreading ulceration of the old cicatrized surface. Thence he was returned to Scotland, as formerly; and, in April last, during fourteen days, lay again in the Infirmary here, but without improving in his condition; and before leaving that institution, although his life was in extreme peril from an excessive discharge of matter, he refused to submit to amputation of the limb.

When asked in May 1861, by Dr M'Cowan, to visit A. A. M., I found him emaciated, debilitated, and labouring under exhausting irritative fever, with extremely feeble intermitting pulse of about 160 per minute. But as he was apparently free from organic disease—an opinion in which I was confirmed by Professor Simpson—and as the sufferer was anxious for me to remove his limb, I placed him under preparatory treatment for the operation. During this time the greatest obstacle encountered in the way of progress, notwithstanding the large and lofty apartment occupied singly by the patient, was the putrid emanations that arose from the profuse discharge, mixed with blood, issuing from the ulcerated surfaces, as well as from abscesses in the popliteal cavity, which it had become necessary to open freely. A plentiful use, however, of Condy's patent fluid proved of the greatest service in deodorizing the vitiated air of the room and lobby.

Accordingly, A. A. M. rallied somewhat from a state of extreme prostration, and now presented the symptoms of pure hectic only.

His pulse had fallen to about 130, though it was still feeble and intermitting.

On *June 22d, at noon*, I removed the limb in the presence, and favoured with the assistance, of Professor Simpson, Drs M'Cowan, Oliver of Prestonpans, Alexander Simpson, and Mr Edwards. The method of amputation by rectangular flaps, as advocated by Mr Teale of Leeds, I partially followed, adopting certain of the modifications recommended by Mr Spence.[1] In order to avoid the marginal cicatrices of the burn, I had to remove the limb at its upper third, and had to select an antero-internal very short flap, including not more than a third of the circumference of the thigh, and a postero-external flap about three times the length, and at its base twice the breadth of the other. The former flap was made by transfixing the limb, grazing the inner margin of the bone, as in the ordinary double-flap amputation, and carrying the knife in the gentlest curve downwards and inwards; while the latter, or long flap, was made by dividing in a curve first the skin only, and after retraction of it to the extent of more than an inch, I then directed the knife's point upwards to the bone, revolving it round which—after Alanson's mode[2]—I divided the remaining two-thirds of the fleshy parts of the thigh, forming them into a hollow cone, so as to avoid redundance of muscle. Before applying the saw, both flaps were farther retracted, and the bone was divided not far below the *trochanters*.

The principal vessels thus lay on the internal flap. The means used to arrest the bleeding were seven short well-tempered needles, (four of No. 4, women's size, and three of No. 4, men's); to each of which was attached, for its subsequent withdrawal, fine iron wire, according to the method introduced into practice by Professor Simpson.[3] The unimportant vessels were submitted, as usual, to torsion.

Twelve points of wire suture, deeply inserted, served next to retain the flaps in contact. At this stage of the operation, the effects of the chloroform wearing off, the artery of the sciatic nerve—which, just before the flaps were brought together, had been shortened about two inches—bled freely; and to avoid the removal of the stitches, a needle 5 inches long, flattened towards its point, was—at Dr Simpson's suggestion—brought down between this nerve and the *os femoris* through the skin of the region of the hip, and made to emerge at 5½ inches peripheral distance from its point of entrance. The effect of this acupressure was instantaneously seen in the bleeding being arrested.

The patient bore the operation well, having been supported with wine at intervals. Very little blood was lost. On removal to his bed, the stump was supported by a pillow and a sling, wetted lint

[1] Edinburgh Medical Journal, Nov. 1859, Case III.

[2] Practical Observations on Amputation. By Edward Alanson, Surgeon to the Liverpool Infirmary. Second edition, London, 1782, p. 53.

[3] See references in my former paper on this subject in the Edinburgh Medical Journal, vol. vi., p. 510.

was applied over its extremity, and an opiate with whisky was given. At 2 P.M. he suffered from thirst, exhaustion, and irritability, but was cheerful. The opiate in whisky was then repeated. At 8 P.M. his pulse, still intermitting and feeble, had however fallen to about 120. Another opiate in whisky was then given.

June 23*d*. The patient is feverish and depressed, and complains of great pain in the back; pulse less intermittent. Has had tea and toast. The long needle was removed from under the sciatic nerve *at the twenty-fifth hour* after the operation, without trace of bleeding. The pain in the back becoming more severe towards night, a strengthening plaster was at the patient's own request applied, and the opiate in whisky repeated.

June 24*th* (*forty-ninth hour*). Pain in the back relieved; pulse only slightly intermittent; and patient looks better. I withdrew five of the short needles from their corresponding smaller arteries, by simple traction of their connecting wires. On endeavouring likewise to remove the needles from the *femoral* and *profunda*, I found that I had inadvertently allowed their connecting wires to become mutually entangled, so that no farther traction was made at that time. No trace of bleeding followed these manipulations. A. A. M. has partaken of light food, but is dyspeptic. Towards evening the margins of the flaps had assumed an unhealthy aspect, and I therefore removed some of the stitches, and applied the tincture of myrrh. At this time I recollected that, in burying the point of the needle that occluded the *femoral* artery, I had made its point emerge for a moment through the skin. Making firm pressure, therefore, around that point of skin, I now succeeded in causing the needle's point again to protrude; on seizing which with the forceps, I extracted the needle entire, and with the scissors liberated it from its connecting wire. In a day or two after, I withdrew the needle from the *profunda*, by firm traction of the two entangled wires. In neither case did any bleeding follow, except what was evidently traceable to scratching of the granulations by the entangled bit of wire. The patient had at bedtime his opiate and whisky as usual.

25*th*. The stump yielded a slight discharge of pus. A lotion of zinc was now used; and wine was given occasionally, together with an opiate in whisky and water, after the forenoon and evening dressings. 26*th*. The patient is weak, with a returning severe pain in his back. For this at his own request a fresh strengthening plaster was prescribed. Wine is to be given thrice a-day, and animal diet. 28*th*. He takes his food well; his pulse is about 90; and he feels easier. 29*th*. Discharge lessening. Has his steak and wine, besides his opiate and whisky and water twice a-day as usual, as his irritability is great. Pulse to-day is 140. 30*th*. Less discharge. Pulse 120. *July* 1*st*. The last of the stitches was this day removed. The purulent discharge is healthy. The tongue is dry. The pulse is limping and 125. Had ℥iij of wine during the dressing of the stump. He complains of cough, for which a liniment

and mixture were prescribed. 2*d*. Less cough. Pulse 110. 3*d*. Pulse 108. Continues the opiate and spirits twice daily after the dressings. 4*th*. Is much better. To have quinine and sulphuric acid. Pulse 120. 5*th*. Pulse 115. 6*th*. Pulse 110. Takes his food well, and asks for porter instead of wine. 7*th*. Pulse 115. Takes his tonic and cough-mixture thrice, and his opiate twice, daily. 8*th*. Pulse 117. 9*th*. Pulse 115. 10*th*. Pulse 112. 11*th*. Sleeps well. Has full diet, for which his appetite is good. Pulse 115. 20*th*. The face of the stump is contracting. Discontinue the quinine and acid, and use the sesquinitrate of iron externally and internally. Continue the opiate at bedtime only.

From this date the health of the patient and the appearance of his stump greatly improved. His pulse varied from 80 to 70, and gradually became less intermittent. His appetite continued good, his bowels regular, and his cough left him. *Aug*. 20*th*. On visiting A. A. M., I found his pulse 72, and regular. His stump and entire body are become stout. During the entire progress of this case I received very efficient assistance from the Messrs Millar, Black, and Vartan, pupils, who furnished me with notes of the case. *Oct*. 31*st*. To-day I made a cast of the stump.[1] *Jan*. 8, 1862. A. A. M. is robust and well. His pulse remains at 72, and is natural.

Remarks.—1*st*, From the condition of the skin, this amputation could not have been performed lower down the limb.

2*d*, The patient was indebted for his recovery from the shock mainly to the large doses of opium, and to the free use of stimulants. He was largely benefited, too, by the internal as well as the external use of the sesquinitrate of iron. He has regained sound health, and possesses an excellent stump.

3*d*, From its contributing favourably towards our surgical statistics, I feel warranted in asking the attention of the profession to this narrative. The results of 300 cases of amputation at Guy's Hospital, as recorded by Mr Bryant in vol. xlii. of the Transactions of the Royal Medico-Chirurgical Society, p. 70, show, under the head of *Pathological Amputations* (or amputations from necessity) *through the thigh*, a fatal result in 18 out of 100 cases, or 18 per cent., or 1 case in 5·5. Farther, by reference to Mr Bryant's tables, it will be seen that out of 39 cases of *pathological amputations through the leg*, 3 only were fatal, or 7·7 per cent., or 1 case in 13. And, as a whole, of these amputations through the *thigh* and *leg* taken together, 15 per cent. proved fatal.

4*th*, Recovery in the foregoing case, that of a man above forty, of very feeble habit, much shattered by long years' purulent discharge, and deficiency of food and clothing, and so weak that a mere puff of wind seemed enough to blow him over, may perhaps be connected with the small loss of blood sustained during the operation. This seemed to be due, in part, to the greater expedition with which I was enabled to close the bleeding vessels *by*

[1] Now in the Museum of the Royal College of Surgeons, Edinburgh.

acupressure; a process easily performed by the operator alone, without the aid of an assistant.

5th, Experience now enables me to express a decided preference for *short* over *long* needles.[1] By the use of the former, I have found it as easy as by the use of the latter to command the flow of blood from the large as well as from the smaller arteries.[2] Transfixion of the skin is thus avoided—a material point in dealing with a texture weak in its action, and farther weakened by contact with the knife. The accidental entanglement in my hands, unperceived, of two of the wire threads in the foregoing case, shows, however, that some care in manipulation is necessary. No blame can be attached to acupressure, as such, if it be only properly applied.

6th, Another testimony is here afforded to the success of acupressure in closing summarily, within a few hours, the large vessels divided in amputations of the extremities. So far as I am aware, no instance of failure in closing an artery, by the use of the long or the short needle, has yet occurred; nor has secondary hæmorrhage taken place under this mode of procedure.

This observation, if correct, contrasts favourably with our current surgical practice. Indeed, the liability to secondary hæmorrhage that attends the use of the ligature on a wounded artery is well expressed by Professor Syme[3] as follows:—"If a ligature has been applied, the ulceration by which it is separated, if too rapid or extensive, may cause a bleeding from three days to as many weeks after the infliction of the injury." After another remark, he continues thus:—"It has been proposed to obviate the immediately

[1] The application of short instead of long needles in acupressure is described by Dr Simpson as follows, in the Medical Times for April 21, 1860:—"Arteries as large even as the femoral, exposed in operations and wounds, can be effectually and easily secured by common short sewing-needles introduced from *the surface of the wound*. The needle is dipped down into the soft tissue on one side of the artery which it is desired to close, then raised up and bridged over the artery itself, and subsequently pressed downwards and onwards into the soft tissue beyond. A slender iron or silk thread passed through the eye of the needle, and left out between the lips of the wound, serves as a simple means of withdrawing the needle itself as soon as the artery is sufficiently occluded."

A more recent method of exerting acupressure on an artery is described by Dr Simpson in the Transactions of the Medico-Chirurgical Society of Edinburgh, as follows:—"In the last case of amputation at which he had been present he had adopted a new procedure. He introduced a small needle, threaded with iron-wire, behind the artery, and then throwing the noose of a duplicature of another iron thread over the point of the needle, he carried this double thread across the mouth or site of the bleeding vessel, gave it one twist below the eye-end of the needle, and thus compressed the artery easily and speedily to any required extent. It was a kind of 'temporary ligature,' to use the language of surgeons, removable in a minute, hour, or day, by drawing out the needle by traction at the thread passed through its eye."—See Edinburgh Monthly Journal, vol. vii. p. 788.

[2] See a comparative observation by me on this point in the Transactions of the Medico-Chirurgical Society of Edinburgh, published in the Edinburgh Medical Journal, vol. vi. p. 571.

[3] See the paragraph ("Secondary Hæmorrhage") in the second section ("Wounds of Arteries") of the seventh chapter of his Principles of Surgery, second edition, p. 81.

fatal effect of excessive hæmorrhage by transfusing the blood of another individual into the veins of the patient."

7th, The cases of amputation generally, in which acupressure has been employed, range, I am informed, from thirty to forty in number; of which I hope soon to be able to present a summary that may prepare the way perhaps for a cautious induction. These include at least six cases of amputation through the thigh.[1]

8th, It is interesting to observe, *first*, how very slight is the amount of acupressure that is necessary[2] in order to close a bleeding vessel; *secondly*, for how short a time pressure need be maintained[3] in order to produce that effect; and, *thirdly*, how acupressure has not been productive hitherto of any unpleasant symptom, or even of any appreciable effect on the adjacent veins or nerves.

In applying acupressure is there no risk of producing injurious pressure on adjoining parts?—is a question that has been asked. In the foregoing narrative it was stated that the artery of the sciatic nerve had been directly compressed; now here was sufficient compression to arrest hæmorrhage from the centre probably of the nerve, and yet no injurious pressure was exerted upon the delicate nerve tissue. The patient has remained indeed to this day free from any such symptom. But it has been suggested that the great pain in the back that was felt on the day after the operation was due probably to compression of the great sciatic nerve by the long needle employed to control its central artery. It seems clear, however, that this lumbar pain experienced by the patient after the operation depended simply on cold and exposure on the operating table (or, as he himself expressed it, "to cold and the hard bed on which he lay on his back"); for, while removal of the long needle did not lessen the pain he endured, which became more severe towards night, and was relieved once and again by the use of a common strengthening plaster, applied at his own suggestion (a remedy which, while resident at Sunderland, he had had recourse to for cold), it gradually disappeared from his feeble and exhausted habit under a freer use of wine, an animal diet, and a tonic draught.

[1] See a notice of five of these cases in the Journal of Pract. Med. for Nov. 1860, English edition, p. 481.

[2] See Professor Simpson's remarks on this subject in the Edin. Med. Journal for Jan. 1860, p. 645; the Dubl. Hosp. Gaz. for Jan. 2, 1860, p. 7; and the Med. Times and Gaz. for Feb. 11, 1860, p. 137.

[3] Illustrative of this remark is the following extract from Malgaigne's Man. de Méd. Operat., 6th edit., Paris, 1853, p. 41:—"*Procédé de Koch.*—Après toutes les amputations, l'opérateur ramène le lambeau sur la plaie, et le maintient par les bandelettes agglutinatives. Une compresse longuette est fixée sur le trajet de l'artère, à l'aide d'une simple bande. On donne ou moignon une position un peu élevée, et un aide exerce avec la main sur le moignon une douce pression continuee pendant une ou deux heures, et même aussi longtemps qu'on y ressent des pulsations considérables. Quand elles ont cessé, et que l'appareil est teint en rouge par la lymphe qui a suinté, *Koch* assure que tout danger d'hémorrhagie consécutive a disparu, pourvu que le malade reste tranquille. De semblables promesses n'étaient pas de nature à séduire beaucoup les chirurgiens, et ce procédé est justement abandonné."

EDINBURGH, 11 HOPE STREET.
15*th February* 1862.

ON THE

TREATMENT

OF

SCARLET FEVER AND MEASLES,

WITH SESQUI-CARBONATE OF AMMONIA.

BY

THOMAS J. GRAHAM, M.D.,

FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS, EDINBURGH.

"I have always thought it a greater happiness to discover a certain method of curing even the slightest disease, than to accumulate the largest fortune; and whoever compasses the former, I esteem not only happier, but also better and wiser than the latter. As it is the part of a wicked man to destroy his fellow-creatures, so it is the duty of a good man to preserve them, and instruct others how to save them from death, even after his own decease."—*Sydenham's Works*, vol. ii. p. 384.

THE SECOND EDITION.

LONDON:

PUBLISHED BY SIMPKIN, MARSHALL, & CO.

PRICE ONE SHILLING.

1861.

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In the Report of a meeting of the *Epidemiological Society*, on March 4, 1861, which appeared in a recent number of the *Lancet* (6th April, 1861) it is stated that an eminent Metropolitan physician, Dr. Babington, then said, "Few diseases were so little amenable to treatment as scarlet fever in a severe form."

This is undoubtedly true under the usual means employed; but it is an important question, are those means the best? I think not. Blood-letting, bark and acids, emetics, purgatives, diaphoretics, calomel and antimony, iron, chlorine, cold affusion, and fumigation, are not to be depended on. They have been extensively used, and still continue to be trusted to, often without the least good effect. No wonder, then, that scarlet fever in a severe form, still continues to be so little amenable to treatment, and that the consequences of even slight cases are sometimes so disastrous and fatal.

Nevertheless, I believe there is abundant evidence before the profession to prove that in the *sesqui-carbonate of ammonia* a Beneficent Providence has provided a remedy of *specific powers* in scarlet fever and measles; but that it is not employed and trusted to as it merits. It has always been ranged by medical writers in the class of *neurotics*, *stimulants*, or nerve-medicines, and this error appears to be a principal cause of the prevalent want of confidence in its use in these and some other diseases. In internal use, dissolved in water, it does not, in an ordinary dose, accelerate the pulse, or produce any of the usual sensible effects of stimulants, but it has the property of a *restorative;* it gradually restores the strength without stimulating the system. It is difficult to understand what some writers mean with respect to the action of nerve-medicines. Perhaps the general notion is that ammonia affects the body through its action on the nerves, but of this we have not the shadow of a proof; the fact is, that ammonia is carried directly into the blood, improves its quality, and beneficially affects the nerves through the

circulation, of which we have clear and abundant proof. It is a diffusible sustaining saline alterative, and antiseptic; but from the crude notions usually advanced by writers on Materia Medica, professional men have contracted the opinion that it is merely a stimulant, although a valuable one, and therefore not to be depended on in formidable cases of these disorders. This is assuredly an erroneous opinion, from which many have severely suffered. Dr. Chapman, who was at the head of the profession in America, has an important remark in point. "In one respect," he says, "this medicine differs from every article of the class to which it is attached, and, it would seem, from all other medicines. The peculiarity to which I allude is this, that the excitement it raises approaches more nearly to that of healthy action."* No doubt it does, because it does not properly belong to the class of stimulants, but to that of restorative blood medicines, which counteract a septic tendency in the organic fluids.

The opposite opinion has led to its being so often combined with other medicines, as if it were not worthy to be trusted alone. We have a notable proof of this in Dr. Copland's *Medical Dictionary*. The learned author of that very valuable work says (vol. iii. p. 700) "The sesqui-carbonate of ammonia was strongly recommended by Peart, and is certainly often most beneficial when combined, as above advised, according to the peculiarities of individual cases." Now we believe that it is always most beneficial when given alone, and that the combination with other medicines is never requisite, but is often hurtful. It is hurtful by interfering with the full specific powers of the remedy, from which it diverts the attention of the physician, while it impairs his confidence in the persevering employment of it, when anything untoward occurs during its administration in a combined form. Then, entirely in the dark as to which article in the combination is at fault, he alters it, and perhaps prescribes something less suitable, it may be discards the ammonia altogether; and thus is never in a position to test fully the efficiency of this incomparable remedy. This was the experience both of Dr. Peart and Mr. Wilkinson, and is that of Dr. Witt.

Sesqui-carbonate of ammonia does not act on the nerves but on the blood, and is one of the mildest and yet most powerful and salutary *blood medicines* we possess. If it is not natural to the blood, yet it is a *vital antidote*, possessing the specific property in the two diseases now under our consideration, of counteracting and destroying the morbid material by which they are induced. Instead of being temporary in its action, as it has been too much the fashion to regard it, it is permanent, effecting in a marked manner lasting relief and cure, partly by counteracting the putrescence of the fluids, and partly by expelling the offending matter. It does not directly exalt nervous force, but it is most evident that it supplies vital force, and thus increases nervous energy.

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* Eberle's *Materia Medica*, vol. ii. p. 164.

Hence its great use in syphilis, a blood disease for which it was at one time highly and justly extolled. In many cases of that complaint it is certainly a far more suitable and efficient remedy than mercury. It is a powerful blood solvent.* It invigorates the capillary vessels, accelerates the movement of the blood in them, and effectually controls the strong tendency in scarlet fever and diphtheria to fibrinous deposition. It aids materially in the change of venous into arterial blood. Hence its singular efficacy, in combination with squills, in the chronic bronchitis of old people; and when combined with tartar emetic, in acute bronchitis and pneumonia at all ages. Both in acute and chronic bronchitis, when we see the signs of venous blood mingling in the general circulation, the complexion being dusky, and the lips bluish, ammonia is a valuable remedy in combination with emetic tartar. The latter medicine checks and reduces the inflammation present, and the ammonia sustains the strength and promotes expectoration, by invigorating the capillary circulation and assisting in arterializing the blood.

With the erroneous views generally entertained respecting the operation of ammonia by writers on Materia Medica, no wonder that we should meet with such remarks as the following. Dr. A. T. Thomson says, *Elem. of M. Medica, vol. i. p.* 402, "It is not easy to explain the manner in which ammonia operates when it is taken into the stomach for warding off the effects of the bites of poisonous snakes, unless we admit that the virus introduced into the wound acts as an immediate and direct sedative to the nervous system, and that the ammonia, by sustaining the nervous energy, enables the system to withstand the influence of the poison until it expends its power."

Here is a fair specimen of the neuro-pathology—a fruitful source of so many errors in practical medicine. The virus of snakes does not act at all on the nerves. Its sedative operation on the nervous system is merely imaginary, and so is that of ammonia directly sustaining the nervous energy. The certain facts are that the poison of serpents instantly attacks the blood, to the extent of producing a visible fermentation in it when microscopically examined, and that ammonia, by destroying the morbific power of the virus there, proves a specific remedy. It evidently neutralizes the virus carried into the blood, and thus circulated throughout the system. This is perfectly intelligible; and ammonia operates in the same way in scarlet fever and measles, diseases in which the whole blood of the sufferer is struck with a virulent poison, which ammonia neutralizes. Its well-ascertained power over the potent poison of venomous serpents, spreading as it does with such fearful rapidity through the circulation, is a striking proof of its remarkable antiseptic and life-giving qualities.

That "the poison of scarlet fever primarily acts on the great

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* Richardson on the Blood, p. 300 and 322-7.

nervous centres, deranges their functions, and produces fever," * is a gratuitous assumption, clearly contrary to the fact; to use a homely phrase, it is putting the cart before the horse. This erroneous *neuro-pathology* is probably another cause of the unsound and unstable views frequently entertained in the profession, with regard both to the nature and treatment of scarlet fever and measles. They are diseases in which the blood is so primarily and specially affected, that its vital properties are greatly impaired. It is to this vital fluid that the practitioner must look both first and last, if he would treat them scientifically and successfully, for the circulating blood is the medium through which the poison acts upon the solids. What Dr. Stevens says in his admirable *Treatise on the Blood*, in reference to remittent and typhus fever, is perfectly true in the fevers I now treat of (p. 294). "The blood is the first part of the system which feels the effect of the remote cause; and when the blood is poisoned, the impression which is ultimately made on the nervous system is merely the effect of the diseased state of the vital current, which, as I have said, receives the shock, and communicates the disease to the whole system. Now, if the cause of the disease produces its effects entirely through the medium of the blood, we shall then be more likely to effect a cure by the use of those agents which enter the circulation, and, like the remote cause of fever, act not only on the blood itself, but on every organ, and every solid of the living body, not by any direct impression on the nervous system, but entirely through the medium of their nutritive fluid."

It cannot be too frequently insisted upon that the blood lives and is nourished through itself, and in no degree depends on any other part of the body; which cannot be said of the nerves, tissues, or any of the vessels, for they are all strictly dependent on the blood, both for their proper life, and for the due performance of their several functions. The blood is the natural source of life and health to the whole body;—it is the first seat of disorder in almost all complaints, hence to a deteriorated or poisoned condition of the blood, we are justified in referring the great majority of cases both of derangement of function, and disorganisation of structure.† Therefore when the natural composition and vital properties of this all-important fluid are much altered, we at once recognise a clear and sufficient cause for the textural changes in the kidneys, brain, and other organs, which so frequently depend upon, or follow, scarlet fever and measles, when not treated with ammonia. Happily, professional men are now becoming awake to the pre-eminent

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* R. Williams's *Elem. of Practical Med.* vol. i. p. 121. Dr. R. Williams was one of the most thoughtful and discerning of modern physicians, and yet if the reader will turn to his *Elements of Medicine*, vol. i. p. 159, he will find there a striking and lamentable proof how entirely and unquestionably erroneous the opinion of the most sagacious may sometimes be.

† Jones and Sieveking's *Pathological Anatomy*, p. 57.

importance of directing special attention to the blood in the treatment of disease, and consequently to the superior value and efficacy of the neutral and alkaline salts, whereby our therapeutics have been vastly improved, and our success considerably increased.

But the chief appeal must be to experience. Does experience justify the opinion that few diseases are so little amenable to correct treatment as scarlet fever in a severe form? I think not. On the contrary, it proves that this fever in its severest form is more amenable to treatment by ammonia, than almost any other severe malady is by any means usually employed. Acute rheumatism is not so controllable by alkalies, or any other known remedy. Ague is not so controllable by bark. Syphilis of an aggravated character is not so controllable by mercury. I have no desire to extol this remedy above its value, and have no temptation to do it; but it is generally estimated so far below its great merits, as to render necessary a caution not to underrate them; and, in the hope that the profession may be induced to give it a fair trial in the diseases now considered, it is desirable not to shrink from a plain declaration of its specific power as verified by ample experience.

Dr. Peart, a physician in considerable practice at Gainsborough, was one of the first discoverers of the value of sesqui-carbonate of ammonia in scarlet fever and putrid sore throat. He dissolved two drachms of the carbonate of ammonia in five ounces of water, and directed the patient to take half a table spoonful, or two teaspoonfuls every two, three, or four hours, according to the urgency of the symptoms. If the difficulty of swallowing abate, and the patient wish for it, a little cold water may be added to each dose. Cold water, or toast and water, may be drunk at pleasure. The above remedy was given in every form, and in every stage of scarlatina. "Some," he says, "were glowing with universal efflorescence; in some, the extremities were swelled; in others, fœtid ulcers appeared; in most, the throat was swelled and inflamed, often ulcerated; and respiration almost prevented; but, in the most alarming cases, a scorching fever, and raging delirium, rendered the patient's situation horribly distressing: yet, in all these variations of the disease, the volatile alkali was my specific, which I administered to between two and three hundred patients successively and successfully." "The immediate effects of the remedy are stated to be a diminution of heat, fever, and delirium, and a disposition to sleep." It is hardly necessary to mention that during the exhibition of this remedy, the bowels should be kept in proper order; and that if, at any time, there should be any accumulation, two grains of hydrarg. chlorid. and ten of pulv. jalap comp. should be given; gargles, likewise, should be employed.

Mr. Wilkinson, of New Broad Street, in his day a practitioner of extensive practice and great integrity, says: *(On Cutaneous Diseases, p. 13 et seq.)*—"In the year 1803, I attended several cases of the scarlatina maligna with Dr. Willan, and the late Dr. Hamilton. It is well known that the disease raged most

fatally during that period, and we lost four of our patients out of five in one family. Never were men more puzzled to know what remedies to adopt: all which Dr. Willan has recommended in his publication were employed. Emetics, purgatives, calomel, and antimony; many diaphoretics; opium, wine, and acids; bark, blisters, decoct. contray. with oxymel of squills; application of cold water, gargles of different descriptions, fumigations, etc.: all without the least good effect; all without making the least sensible impression upon the disease in any of its stages.

"One fine girl, about eleven years of age, in high health and spirits in the morning, was attacked, an hour after, by the disease, and destroyed in thirty-six hours.

"About this time, Dr. Peart published his *Practical Information on the Malignant Scarlet Fever and Sore Throat*, in which he describes the wonderful effects of the subcarbonate of ammonia, and considers it to be endowed with a specific power over that disease. Like other practitioners, he was continually lamenting the loss of his patients by that dreadful malady; till, by his own suggestion, he employed the subcarbonate of ammonia, in the manner he describes; and from that moment, he did not lose one patient out of nearly three hundred.

"When I read this account, I immediately enquired after the character of Dr. Peart, and finding, that he was most respectable, both in talent and probity, and engaged in very considerable practice, I had no reason to doubt the truth of his statement, and therefore immediately adopted his remedy; and consonant with my own principle, that an effectual remedy for one genus will, with proper management, cure all the genera of the same order, I administered it in all the following diseases, erysipelas, rubeola, scarlatina, urticaria, roseola, and erythema, with all their varieties; and I am happy to be able to declare that, from that moment to the present, a space of seventeen years, I have, not only never lost a patient in the above diseases, but have never had a case of the kind that has even appeared dangerous, or that has even given me a moment's anxiety.

"I will take the liberty to state, that I depend not upon its diuretic, nor its diaphoretic qualities, but believe, that it possesses the power of increasing the strength of the arterial action, at the same time that it diminishes its frequency; that it supports the *vis vitæ* without increasing the heat or irritability of the system; and by such means counteracts the tendency in the scarlatina anginosa and maligna, to ulceration and sloughing, and all the other evils which sometimes attend this dreadful disease. But, to effect such purposes, it must be given as Dr. Peart has described, in a state as strongly stimulating as it can be swallowed, and not made into a mere diluent by a quart of white-wine whey.

"I hope I shall not be thought to have expatiated too much upon the virtues of a remedy which I have found so efficacious in my own practice; but, I have seen so many cases of scarlatina and

rubeola treated by others with the common antiphlogistic remedies, which have been so lingering, and have left such ill effects in the system, that I feel it my duty to urge the employment of the subcarbonate of ammonia as extensively as possible. I am well acquainted with the success in scarlatina attending the affusion of cold water; but, at the same time, that the employment of it is frequently objectionable to the friends of the patient; in my own experience, the general effects of it are not equal to those produced by the ammonia. Nor do I find, that the affusion of cold water is yet employed in rubeola or erysipelas, whereas the subcarbonate of ammonia may be given with almost equal success in all the exanthemata attended by erysipelatous inflammation."

Dr. Witt, of Spring Gardens, has recently published a pamphlet, in which he states that he has "from long and varied experience found ammonia to be a sure remedy for the cure of scarlet fever and measles." He was a pupil and private friend of the late Mr. Wilkinson, and writes with equal confidence of the efficacy of this valuable medicine; adding (p. 7), "My own medical friends, to whom this practice was before unknown, have shown the greatest readiness, after due explanation had been given, to afford it a fair trial; and I can add, that they all, without a single exception, have since expressed their conviction of its surprising efficacy." At the end of his pamphlet, Dr. Witt has inserted an appendix of cases, some of which are strikingly instructive and convincing. His third case, p. 21, is so instructive that I venture to call the reader's particular attention to it. "I was summoned during the past year to attend a young lady, supposed to be labouring under inflammation of the bowels. There certainly were several of the symptoms of that disorder; but, instead of bleeding, I saw reasons for pursuing a treatment which was chiefly of a cordial character; and in three days she was relieved for the time from pain, and left her bed, apparently well. In two days more, however, she was seized with dispnœa to a fearful extent, which lasted for eighteen hours; from this she was also relieved without bleeding, and all seemed right again: but, soon after, a restlessness and disturbance to a still more alarming extent succeeded: for three days and nights she had no sleep; her distortions of countenance were so great that, from having been remarkable for her beauty before, she ceased to be at all so; while her naturally gentle and amiable disposition changed to one so exacting and imperious, as severely to try the patience of all about her. Although she did not lose her consciousness, it seemed as if the case would end in mania. On the ninth day from my first having seen her, I discovered on her chest the faintest possible pinkish blush, when the conviction flashed upon me that it was a case of scarlet fever. I at once commenced the administration of the ammonia, and gave it in unusual doses; for although medicines and treatment of a lowering kind had been avoided from the first, still I considered that her constitution required the greatest possible support in contending with this dis-

order. A solution containing seven grains to each two tablespoonfuls of water was therefore prepared, which quantity she took every hour for the space of twenty-four hours. The eruption then came fully out, exhibiting as perfect a specimen of the fever as could be witnessed. The same dose was next day repeated every other hour for the same space of time, when the eruption was declining fast. All cerebral disturbance ceased on the eruption becoming general; she slept profoundly; perspiration came on, and her pulse became less frequent. The health of the patient afterwards improved rather than deteriorated, as the health is so apt to do after an attack of scarlatina when subjected to the ordinary treatment. That the case was unquestionably one of scarlatina appeared from the fact that, during the time of the peeling of the skin, a child entered the patient's room and caught that disease.

"This case is instructive, as shewing how, when nature's efforts to throw out the poison on the skin had failed, the peritoneum was the membrane first affected, and next the membrane lining the lungs; the latter would seem to have been so from the extreme difficulty of breathing, which indeed was more distressing than I ever remember to have seen, except in patients near death. The third irregular effort fixed the poison on a still more dangerous site, the membranes of the brain. Now, if upon either of these three occasions, violent purgatives or the lancet had been resorted to, no one, I think, will doubt that the powers of nature would have been exhausted; the eruption would not have been thrown out; and death would to a certainty have ensued."

His last paragraph is this: "Every case of measles that I have known, when treated with ammonia, has followed as nearly as possible the course above described; and I may add, that, for the space of thirty years, I cannot call to mind a single case so treated in which I have found the results at all less favourable."

The eruption is well known to be a critical deposition on the surface, by which the animal economy endeavours to relieve itself from some internal morbific irritation. The appearance of the rash is essential to the perfect and safe resolution of the disease, and a remedy like ammonia, which at the same time moderates the precursory and attendant fever, softens the dry and burning skin, and sustains the vital and nervous power, is of eminent service in insuring the regular appearance and character of the eruption. Indeed, it is found to be far more efficacious in this than the warm bath, hot friction, warm drink, or anything else, and for this reason, that it is at once carried into the circulation, and in a striking manner improves the quality of the blood, and expels the offending matter from the system. Hence these diseases when treated with ammonia are not found complicated with visceral inflammation, oppressive internal congestions, or other irregular and dangerous symptoms, as they are when subjected to other kinds of treatment.

In scarlet fever and measles the natural secretion from the skin is checked, and the kidneys are very liable to assume a congested

state, which causes the accumulation of excrementitious and poisonous matter in the blood, and tends to the aggravation of the disorder. It must, therefore, always be a principal indication to eliminate this matter, and ammonia, from its known influence as a diaphoretic and mild diuretic, by its evident salutary action on the skin and kidneys, sets free these grand emunctories through which the morbid matter is expelled.

That very enlightened and skilful physician Dr. Graves, from not being fully aware of the efficacy of the volatile alkali in scarlet fever, was for a long period sorely puzzled and disappointed in its treatment, and after trying for years measures of all sorts, he concludes by saying, that those which he at length found more successful than any other in the malignant forms of scarlet fever were—"Wine and carbonate of ammonia freely given with camphor mixture."—*Graves's Clinical Medicine*, p. 541. The camphor mixture I believe to have been useless, except as a vehicle.

Albert Bodenius, a medical practitioner at Bretten, in Germany, has thoroughly investigated our present subject, and says, that having been led to the use of the volatile alkali, "he found to his unspeakable satisfaction and joy every gratifying result that he had been led to anticipate from the carbonate of ammonia." "It does not only cure the scarlet fever, but leaves no illness afterwards, when this remedy is employed in the right way." "He thinks it his duty to express his full conviction that the carbonate of ammonia is as certain a remedy in scarlet fever, as vaccination is a protection against the small-pox." *

"In other countries it has found advocates. Dr. Strahl of Philadelphia has made public his faith in it, grounded on his experience, as a real specific for the cure of scarlatina; and Dr. Reicken, of Brussels, has written at some length on the surprising effects which he has found it to have over certain epidemics, more especially over scarlatina." †

My practice in these maladies has not been so extensive as that of the professional men previously named, but all that I know confirms the truth of their statements. I believe, with Mr. Wilkinson, that this admirable remedy increases the strength of the arterial action, at the same time that it diminishes its frequency; that it sustains the vital powers without increasing the heat or irritability of the system; and by such operation counteracts the tendency in malignant scarlet fever to ulceration and sloughing, and all the other evils which sometimes attend this dreadful disease. Mr. Wilkinson's pamphlet on the efficacy of the volatile alkali in scarlet fever, was published in 1822, and yet in 1839 we find Dr. R. Williams, physician to St. Thomas's Hospital, publishing (*Elem.*

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* Discussions and experiences respecting the specific power of carbonate of ammonia in scarlet fever. By A. W. Bodenius, Heidelberg, published in German, 1842.

† Dr. Witt on Scarlatina, p. 13.

of Practical Medicine, vol. i. p. 157) the following appalling facts, which we may adduce in proof of that round of ignorance, error and prejudice in which we seem always, more or less, to live. "A man was received into St. Thomas's Hospital with the eruption out upon him, and attention was alone paid to the state of his bowels. In a few hours the right lower extremity mortified from the heel to the hip, and it is unnecessary to state that he died.—A child, about four years of age, was seized with scarlet fever. The disease was not interfered with, but permitted to run its course without the aid of medicine. On the decline of the eruption, a discharge took place from the nostrils, the inflamed lip became gangrenous, and the child died in consequence."

How often, alas! has this terrible fever carried mourning and woe, not only among the poor, but into the fairest and best of families, sweeping off three or four out of five or six children, sometimes even more; it is therefore desirable to spread to the utmost the knowledge of the efficacy of this medicine, and to call upon the profession to extend to it a greater measure of confidence, and a fairer trial, than it has yet received from them. The preventible mortality in this country amounts to 90,000 lives a year; the deaths by scarlatina and measles alone during 1856, in England and Wales, were 21,280; these are not, as some imagine, evil things in the course of nature which we cannot reasonably hope to remove, or greatly to lessen. A beneficent Providence has provided means for their reduction, but our ignorance, indolence and inattention, or our prejudices and prepossessions, sadly interfere with their right use and application; and the mortality just noticed is one of the fearful consequences which necessarily flow from the neglect of the beneficent laws of God in nature, and from inattention to well-authenticated facts.

No acids or acid drinks should on any account be allowed while taking the ammonia, but the patient may drink at pleasure of thin barley water, toast and water, or water. The usual dose for children of ten years old is about five grains in a table-spoonful of water, sweetened with a little syrup, and repeated every three or four hours; but in the severer cases it may be given every hour or two, until a remission of the worst symptoms is visible; and in the case of adults, it may be increased to ten or even twelve grains at a dose in extreme cases.

The mildness of the fever being no security against very troublesome, and even fatal, *sequelæ*, the administration of ammonia should never be neglected in these diseases, how mild soever they may be in the commencement. There seems to be always a septic tendency induced in the whole system, by a depraved and poisoned state of the circulating fluids, against the possible consequences of which we ought to guard by the use of the volatile alkali. And I beg leave in the strongest manner to press this on the attention of the junior members of the profession, for however efficacious ammonia may be in holding the fibrine of the blood in solution,

and in promoting free elimination of the poison from the skin, when given early, it cannot be reasonably expected always to produce these salutary effects, and save from a harassing or even fatal termination, when commenced only at a late period of the malady. I am the more induced to urge this recommendation from its not being unusual for eminent authorities to advise ammonia to be given, "whenever the pulse presents the characters of frequency and softness combined," intimating thereby, if I understand them aright, that it is applicable only in that particular state of the pulse. (Dr. West's *Lectures on the Diseases of Infancy and Childhood*, p. 732). Dr. Barlow says, (*Practice of Medicine*, pp. 201, 684), "Of medicines, strictly so called, ammonia is the best of stimulants," and, "if the pulse be very compressible, ammonia may be added" to other medicines. Now I am reluctant to differ from physicians so judicious and accomplished; but here is the old erroneous notion of a stimulant attached to ammonia, and so calculated to mislead that I am constrained to dissent. The volatile alkali does not act as a stimulant, but as a sustaining saline alterative, antiseptic and diaphoretic, equally applicable and requisite when the pulse is not soft and compressible, as when it is. Its effects are so uniform on the blood and skin, that it always determines to the surface, and therefore is calculated to render a hard and frequent pulse soft and slow. By thus setting free the emunctories of the skin and kidneys, it wards off attacks of pneumonia in measles, and typhoid symptoms in scarlet fever, and frequently even cures them when they have supervened under other treatment.

Some professional men seem to think that sometimes, from the state of the throat, ammonia cannot be taken. I cannot share in this opinion; but if such a thing should temporarily occur, probably the next best medicine is the liquor ammoniæ acetatis. This is a valuable remedy in the diseases now treated of, since it operates in the same way as the volatile alkali, producing a free diaphoresis, and promoting the solvent power of the blood. It may be given in the usual dose, in a little water, every three hours. But it is not worthy of the same confidence as the sesqui-carbonate of ammonia, and I only mention it in consideration of the hesitation which some may feel at once to commit themselves to the employment of ammonia.

The state of the throat always demands attention, but it is a secondary consideration,* and I believe never to be so effectually and agreeably relieved by gargles or washes, as by the nitrous acid fumigation. The refreshing antiseptic vapour detached from nitrate of potash by means of sulphuric acid, and circulated through the room, presently clears the patient's throat without any annoy-

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* I am glad to see that Dr. Richardson (*Clinical Essays*, vol. i, p. 110) is much of the same opinion with respect to attention to the throat.

ance to the sufferer, and at the same time removes the fœtor both of the breath and perspiration.

The patient should be put into the best ventilated apartment at command, where an abundant supply of pure air can be secured, *free from all draughts, and the possibility of chills.*

Diphtheria is so frequently associated with scarlet fever as to justify the inference that its *materies morbi* is some modification of the poison of scarlatina. If so, will not ammonia be found one of the best remedies in diphtheria? Besides, the sudden and unexpected death which so often takes place in diphtheria and scarlet fever seems generally to be the consequence of fibrinous clots in the heart—a deposition of fibrine in the right side of the heart, arresting the circulation; and ammonia being one of the most powerful blood solvents known, its early administration in diphtheria may be expected to be followed by the same gratifying results that we witness in scarlet fever. The tendency to fibrinous exudation in the throat and elsewhere is the most prominent manifestation of this terrible malady, and clearly points to ammonia as a principal remedy. An instructive letter on the subject of fibrinous deposits in the heart in diphtheria, from Mr. C. R. Thompson, of Westerham, may be read in the *Medical Times*, 7 Jan. 1860, and a valuable paper thereon from Dr. Milner Barry was inserted in the *British Medical Journal* for July, 1858. But it is in the first volume of Dr. Richardson's excellent *Clinical Essays* that the most highly instructive information will be found on the pathology, as well as on the history and treatment of scarlet fever in general. It appears to me to contain also such enlightened views on other matters of great interest and importance to the pathologist and practical physician, as render it a volume of uncommon value.

Epsom, Surrey,
December, 1861.

J. Davy & Sons, Printers, 137, Long Acre.