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INAUGURAL DISSERTATION

ON THE QUESTION,

HOW FAR ARE

SECRETION AND NUTRITION

DEPENDENT ON

NERVOUS INFLUENCE?

SUBMITTED TO THE

MEDICAL FACULTY

OF THE

University of Edinburgh,

IN CONFORMITY WITH THE RULES FOR GRADUATION,

BY AUTHORITY OF THE

VERY REVEREND PRINCIPAL BAIRD,

AND WITH THE SANCTION OF THE

SENATUS ACADEMICUS.

BY

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OF LEEDS,

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DEGREE OF DOCTOR IN MEDICINE.

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TO

JAMES SYME, Esq. F. R. S. E.

PROFESSOR OF CLINICAL SURGERY IN THE UNIVERSITY OF EDINBURGH,

THE FOLLOWING ESSAY

IS RESPECTFULLY INSCRIBED,

IN ADMIRATION OF HIS PROFESSIONAL EMINENCE, AND

IN GRATEFUL ACKNOWLEDGMENT OF HIS VALUABLE INSTRUCTIONS

AND REPEATED ACTS OF KINDNESS AND ATTENTION,

BY HIS PUPIL AND FRIEND,

THE AUTHOR.

TO

JAMES WILLIAMSON, Esq. M. D.

SENIOR PHYSICIAN OF THE LEEDS GENERAL INFIRMARY, AND LECTURER ON THE PRACTICE OF PHYSIC IN THE SCHOOL OF MEDICINE,

THIS ESSAY

IS, BY KIND PERMISSION, DEDICATED

BY HIS FRIEND AND FORMER PUPIL,

THE AUTHOR,

WHO WOULD THUS EVINCE HIS GRATITUDE FOR THE VALUABLE ASSISTANCE AFFORDED HIM IN THE PROSECUTION OF HIS PROFESSIONAL STUDIES.

FOR

DR MADDEN, SURGEON,

EXTRAORDINARY MEMBER OF THE ROYAL MEDICAL SOCIETY
OF EDINBURGH;

WHOSE KIND COMPANIONSHIP HAS EMINENTLY CONTRIBUTED

TO RENDER THE HOURS OF STUDY AGREEABLE,

AND THOSE OF RELAXATION PROFITABLE;

AND WHOSE FRIENDSHIP WILL EVER ENDEAR THE REMEMBRANCE

OF HIS ACADEMICAL RESIDENCE,

THIS DEDICATION PAGE IS AFFECTIONATELY RESERVED,

WITH THE BEST WISHES FOR HIS FUTURE SUCCESS,

BY THE AUTHOR.

HOW FAR ARE

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In the bodies of all animals in which a circulation is known to be performed, there is found existing a species of apparatus, either forming distinct organs and occupying determinate stations, or more diffusely spread throughout the system, whereby certain fluids are separated from the blood, differing as widely from each other in their appearance, character, and purposes, as from the vital stream whence they are derived. The organs which perform these all-important functions vary greatly in their anatomical constitution, presenting at one part the appearance of a compact and solid gland, whilst at another that only of a simply extended membrane. Yet though differing so much, as they certainly do, in their general appearance, anatomists inform us that their necessary constituents are not very dissimilar. It is not for me to enter on the discussion as to what constitutes a secreting organ, and whether in it it is absolutely necessary that an excretory duct should form a part. We know that similar processes go on in some membranes whose minute glands cannot strictly be said to have proper excretory ducts, and also over the whole surface of the body, as well as in those which possess these tubes. We know likewise that in all these processes blood-vessels, conveying an abundant supply of their proper contents, are essentially required; and it is also ascertained, with a considerable degree of certainty, that in all these cases nerves are distributed to these organs, excepting, perhaps, in some of the very lowest in the scale of animals, where the fact of the existence of a nervous system has not been fully proved, though, I think, with a tolerable degree of reason, inferred. As to the part performed by the blood, physiologists are at once agreed, from it are separated those fluids which it is the function of each organ to afford; but how these changes are brought about, and what share the nerves have in the perfection of the process, constitutes one of the most mysterious and difficult questions to which the enquirer can direct his attention. Notwithstanding its alarming perplexity, it is the one I propose to make the subject of enquiry in the following essay, and without further preface I enter upon the task. I purpose, in considering this subject, to notice, in the first place, the various experiments which have been performed by several physiologists, and which appear to bear upon, and in any way elucidate the subject. I shall also endeavour to ascertain from what source has arisen that diversity of opinion which authors have been led to entertain. Afterwards I shall state the ideas I have myself gathered whilst engaged in the study of the subject; and if possible, in the last place, draw some conclusion on this most interesting point, thus answering, to the best of my ability, and as far as the present advance of physiological research will allow, the question under notice. In pursuing the subject, however, considerable difficulty will have to be encountered, not merely from the intricacy of the subject itself, but likewise from the very loose and imperfect manner in which I find this branch of science has been treated of by those learned individuals who have elucidated, by their enquiries, so many physiological difficulties, and to whose names, as authorities, we are accustomed to look with deference and respect. They will be found in many instances referring to this subject with comparative neglect, or founding their opinions on what those studying it must be compelled, in this age of close research, to pronounce as insufficient data.

The influence, real or supposed, exerted by the par vagum on the secretion of the juices of the stomach, during digestion, has long been referred to as a proof that the process generally depends on the action of the nerves. Most probably, from the ease with which these nerves can be exposed in the neck, during their passage to the stomach, they have come to be a favourite object of experiment. Many and various are the means which have been adopted for destroying or suspending their action, but the conclusions derived, are, I fear, liable to considerable objection. But without anticipating, I shall endeavour to give a brief outline of what has hitherto been done by physiologists on this part of the subject. It is not my intention to introduce all the published experiments relating to this point, as that would be totally inconsistent with the limits of an essay of this kind, but merely a few of the most important, from which an idea may be formed of their value as an argument in considering the question under present notice.

Experiments shewing the effects of section of the par vagum are of very ancient origin; perhaps, however, those performed with a view of ascertaining the changes produced by this operation on the respiratory functions have the highest claim to antiquity. With this part of the enquiry, however, I have not to interfere, but to those authors who have mentioned the effects on digestion, I may, I imagine, with propriety here refer. According to a paper of Messrs Breschet and Milne-Edwards, Baglivi is the first who notices this class of effects of injury of the par vagum. On consulting his work, I find that he refers to Willis,* who makes mention of the injurious effects of ligature of these nerves on the digestive functions. Shortly after him Valsalva † notices similar effects produced on these organs by like operations. Baglivi ‡ states that, on dividing the eighth pair, he found that respiration became difficult, and that whatever was taken for some time after such an operation was vomited. On dissection after death, which usually took place about the twelfth day, the œsophagus was full of undigested food, which had been more recently eaten, which circumstance he attributes to the inability of the animal to swallow perfectly. Brunn § mentions that, in his experiments when the par vagum on both sides was divided, the dog on which the operation was performed would eat nothing but a little grass, which was found in the stomach, after death, unchanged, along with matters having the appearance of true fæces. Haller appears, as far as I have been able to ascertain, to have paid little attention to the influence of the nervous system on secretion. He gives, however, a few experiments on the par vagum, along with their results, which have some connection with the present part of our enquiry.

* Willis de Nervorum usu, Tom. i. cap. 24.

† Valsalva Opera omnia, curante Morgagni.

‡ Baglivi Opera omnia. Antwerp, 1715.

§ Brunn, Experimenta circa Ligaturas Nervorum.

These generally were, that after fruitless attempts to vomit, along with very difficult respiration, the animals perished a few hours after the operation, and on dissection, putrefaction of the contents of the stomach was discovered. He states, that his observations contradict those of Brunn, as to the pain occasioned by the operation, the length of time the animals lived after it, and the conversion of the contents of the stomach into fæces, which the latter imagined he detected.* I cannot here help expressing my regret that this able physiologist did not pay more attention to this enquiry. Blainville found that, on dividing the nerves some secretion did take place, but that the digestion of food was wholly suspended. Dupuy‡ has recorded that when the eighth pair of nerves is divided in the neck in various animals, as sheep, horses, &c. the œsophagus is found to be filled with unaltered food, as also is the stomach, on the contents of which no change appears to take place. He found that when the trachea was artificially opened, so that a free supply of atmospheric air was afforded to the lungs, the animals usually lived for seven or eight days, and at length appeared to die from affection of the digestive organs alone. When, however, this precaution was not taken, they perished in a few hours from congestion of the lungs, the air passage being closed

* Haller, Opera Minora, Vol. i.

† Journal de Medecine, 1816, Vol. xxxvii.

from paralysis of the laryngeal muscles supplied by the recurrent branch of the par vagum.

The experiments and observations of Le Gallois * on the division of this nerve were principally made in regard to the changes produced on the respiration. He, however, appears to believe, that digestion is suspended, and that if death did not take place from other causes, the derangement produced in the exercise of this function would be sufficient to produce that event.

Sir B. Brodie, in endeavouring to investigate this subject experimentally, found, from the speedy death caused by the division of the eighth pair in the neck interrupting the respiratory functions, that it was necessary to avoid, by some means or other, this source of fallacy. He had previously ascertained, that when a dog was poisoned with arsenic, a considerable effusion of fluid was found always in the stomach. He accordingly administered some of the deleterious drug to an animal, and also introduced some into a wound made in the thigh, and afterwards, in both cases, divided the branches of the par vagum upon the œsophagus, just as they pass from it, upon the surface of the stomach. In these cases, the breathing was not in the slightest degree altered from the natural state, and the poison took effect with the usual phenomena, excepting the secretion of the fluid, which was entirely wanting. Hence he appears

* Le Gallois sur la Principe de la Vie.

to infer, that the secreting power of the stomach is interrupted by the section of these nerves, and he observes,* "we cannot venture to deduce from these any positive conclusions respecting the necessity of the nervous influence to the secretions in general, but as forming one link in the chain of an interesting, but difficult physiological investigation,—the circumstances which have been mentioned, may be considered as possessing some value."

Sir B. Brodie and Dr W. Philip turned their united attention to this subject, and the results of their enquiry were published in the Philosophical Transactions. Some of their experiments showed that after division of the nerves, when the ends were left in contact, or even when the cut extremities had considerably retracted within the muscular substance of the neck, digestion was in a great measure completed. But again in others, when they took the precaution to turn back the divided ends of the nerves from each other, little or no perfectly digested food was found on examination after death. The great mass had the appearance of masticated food without any other change, however long the animal was allowed to live.†

Dr Wilson Philip proves that the motions of the alimentary canal, meaning by this the stomach

* Philosophical Transactions, 1814. † Ibid. 1822.

and intestines, are wholly independent of the nervous system. From the experiments of Dr Hastings, conducted under his (Dr Philip's) own inspection, (and published also separately by Dr H. himself,*) he concludes that when the par vagum is divided, the digestion of food is suspended, and that this arises from the non-secretion of the gastric juice. He states that when the division was made below the situation where the recurrent branch is given off, the difficulty of breathing did not supervene so rapidly as when made above. Rabbits were usually made the subject of these operations, two being employed for the purpose, one for comparison, which was always placed in the same situation as the other, excepting as regards the injury of the nerves. The one was killed upon the death of the other, and the respective states of their stomachs compared. Efforts to vomit always occurred sooner or later during the experiments. In those animals which were fed after the operation, it ensued immediately, whilst in those which had eaten previously, the consequence was delayed until sometime afterwards.

This he considers as a powerful argument in his favour. For in the first case, says he, the internal coat of the stomach is not defended from the irritation of the crude food by the proper secretion which would convert the surface of the alimentary mass into chyme; whilst in the latter,

* Journal of Science, Vol. xi.

some gastric juice having been secreted before the power of the nervous influence has been destroyed, a certain time must elapse before the chyme thus formed can be propelled onwards, and the stomach subjected to that unaccustomed irritation which causes the action of vomiting.*

Dr Clarke Abel, in prosecuting the enquiries of the last mentioned author, in regard to the effect of galvanism on secretion conveyed through the nervous system, found, on dividing the par vagum in the neck, that the process of digestion was suspended from the loss of the secretory power of the stomach. †

Broughton, in a number of experiments, performed on different animals, ascertained that when food of various kinds was given both before and after the operation, that first taken was partially digested, whilst the latter had undergone no change. On some of them the operation produced little or no distress for several hours, but that after this time they were greatly affected, and usually perished within the space of twenty-four hours. ‡

Magendie, himself, formerly attributed the derangements of the digestion to the effects produced on the gastric secretion, by the interruption of the

* Wilson Philip's experimental enquiry into the laws of the vital functions.

† Medical and Physical Journal, Vol. xliii.

‡ Journal of Science, Vol. xi or Magendie's Journal, Vol. i.

nervous action ; but it appears from his more recent publication that he now ascribes it to the disordered state of the respiratory function, caused by the operation in question, and therefore that it is only a secondary effect of such lesion. *

Messrs Breschet, Milne-Edwards, and Vavasour have published a number of experiments, in which they confirm the results obtained by Wilson Philip, as to the effects of simple section of the par vagum, as well as of section, when the ends of the divided nerves were turned away from each other. Moreover, to obviate the objection of Magendie, they divided the œsophagus of a guinea pig, just before its passage through the diaphragm, ensuring in this way the section of all the nervous branches ramifying upon it, finding it otherwise impossible to separate all the nerves without this precaution. The animal died after an interval of eight hours, having previously appeared lively and without pain. Neither inflammation nor effusion was found in the abdomen, yet still, however, the food remained unaffected by the action of the stomach. † The above were published in an early number of the Archives Generales de Medecine, but in a subsequent part of that work they attribute the results to paralysis of the muscular fibres of the stomach. The function of the par

* Précis Elementaire de Physiologie, a translation.

† Archives Generales de Medecin, Tome ii.

vagum, is, according to their account, to preside over the motions of that organ. *

Paetsch has given an account of some experiments, in which he cut through the œsophagus, between the diaphragm and stomach, and found that the digestion of food previously taken was entirely suspended, although the animals shewed little or no signs of uneasiness for several hours after the operation. His friend, Augustus Schultze, Professor of Physiology in the University of Friburg, found precisely opposite results in similar experiments, for, according to his account, the process of digestion was, not only not interrupted, but even performed with greater rapidity. †

Messrs Leuret and Lassaigne object strongly to any experiment in which the abdomen or chest is opened, as, from the severity of the operation, no precise results can be obtained. Their first trial was upon a horse, in whose neck they divided these nerves. Digestion was suspended, but this they attribute to the inflammation of the stomach which ensued in this case, as in some future experiments the food was found perfectly digested. Analysis shewed the chyme and chyle formed to be similar to that under ordinary circumstances. A great number of gentlemen witnessed these experiments, and were convinced of the accuracy of the results; among whom was M. Dupuy, who con-

* Archives Generales de Medecine, Tom vii.

† Dissertatio Inauguralis. Gottingæ.

fessed the error of his previously published observations. They conclude that digestion can be completed independently of the par vagum.*

Dr Ware, in some experiments intended to prove the inaccuracy of Dr Wilson Philip's theory of the identity of the nervous influence and galvanism, found that, on dividing the eighth pair, digestion was entirely suspended.†

To conclude the notice of this class of experiments I may briefly mention some which I performed myself during the last summer. They corroborate those which have been already noticed, where the suspension of the digestive functions was the result obtained. After the operation the animals were always allowed to live for a time, sufficiently long for digestion, under ordinary circumstances, to have proceeded some length. Little pain was manifested during this time. Difficulty of breathing was often, though not invariably, considerable, and the effusion of fluid into the bronchi moderate. Other rabbits, for these were the animals made use of, were always kept for comparison, placed under precisely the same circumstances, excepting the division of the nerves, which obviated in a great measure the objection urged by some, that the pain of the operation is at any time sufficient to suspend digestion.‡

* Recherches Physiologiques et Chimiques pour servir à l'Histoire de la Digestion.

† London Medical and Surgical Journal, Vol. i.

‡ Vide Appendix.

M. Brachet, in a great number of experiments, performed on various animals, and related in his late work on the ganglionic system, confirms the above observation in every respect ; but whilst he believes the secretion to be under the immediate power of the nervous system, yet he does not allow this to the par vagum, but ascribes to it, amongst numerous other offices, that of presiding over the motions of the organ.*

In estimating the value of these experiments in regard to the question under consideration, and of the conclusion which may be drawn from them, numerous difficulties obstruct that simplicity of deduction which all positive physiological inferences require. There are three separate functions which the par vagum may discharge in its distribution on the stomach. It may alone be of use in influencing or regulating the secretion of the gastric juice; on it may depend those sensations with which physiologists state the organ to be endowed ; or lastly, its office may be to call into action the peristaltic movements of the viscus. And I find, in the various authors whom I have consulted, these three different theories advocated, in explanation of the suspension of the digestive function, which now, from the numerous experiments recorded in proof of the fact, must, I think, be allowed, by all candid enquirers, to be the con-

* Recherches experimentales sur les fonctions du systeme nerveux ganglionaire.

sequence of division of these nerves. To fix upon the correct one of these three theories constitutes not a slight portion of the difficulties of the subject, for if we agree with those who are inclined to admit the first hypothesis, the question is immediately asked, how it happens that, whilst this secretion is suspended, that of the mucus of the lungs is so greatly increased ? and does it not appear strange, that from the same cause two such decidedly opposite consequences should arise ? We shall be told likewise, by those advocating the third theory mentioned, that for a certain time the secretion does go forwards, as is proved by the existence of a layer of chyme on the surface of the alimentary matter, which, say they, would be continued, did not the paralysis of the organ prevent the application of fresh unchanged food to the secreting surface. As to the first of these objections, although it is stated, by almost all experimenters on the subject, to be the fact that the secretion from the internal mucous surface of the air-passages is much increased, and though not prepared positively to deny it, yet I am inclined to think, from my own observation, that this is more apparent than real. The deception arises, I imagine, from the air breathed being mixed, by the violent respiratory efforts, with the fluid already there, thus giving it a frothy appearance, which might easily be mistaken for augmentation of its quantity. In confirmation of this idea I have found that when only slight efforts were made in respiration, the fluid discovered was considerably less.* To the second of these objections it has, with some degree of plausibility, been answered, that the secretion might have taken place before the operation, and immediately after the reception of the food into the stomach, or, as it has more hypothetically been explained by others, as owing to the action of the nervous power which remained in the nerves below the place of section. The former of these explanations appears to be strengthened by the observation of Dr W. Philip, already mentioned,† regarding the time at which vomiting comes on, when the animals are fed before and after the operation. To those who are inclined to embrace the second theory, the following serious objection will occur. For a perfect sensation, it is requisite that a communication should exist between the part affected and the brain—how does it happen, then, that, when these nerves are divided, simple mechanical or galvanic irritation of the cut extre-

* My friend, Dr John Reid, who is still investigating this subject, informs me, that "he is at present inclined to believe that this *serous frothy* effusion is the *result* of the severe dyspnœa which generally precedes death, and not the *cause* of it, as is usually imagined." He says, "the grounds on which I have adopted this opinion are these, 1*st*, In my experiments the extent of this effusion appeared to be proportionate to the extent of the dyspnœa preceding death; and 2*dly*, This effusion appears to me to differ in no respect from that found in the lungs in all cases of death where severe and protracted dyspnœa have been present."

† Vide pp. 9, 10.

mities causes the function to be properly discharged? The third theory involves another much disputed point in physiology, viz. the dependence or independence of the muscular contractions on the nerves, and accordingly as the one or other of these opinions is held, will the answer given to this part of the enquiry be regulated. Wilson Philip, as has already been stated in this essay, affirms that the motions of the intestines are perfectly independent of the nervous system, and for this opinion he has the authority of the illustrious Haller, and numerous other distinguished physiologists. In addition to these, I may here shortly advert to the opinion of Magendie, who conceives that this effect upon the stomach is produced secondarily, by the derangement taking place in the respiratory organs; for he states, that he found, when the par vagum was divided below the branches sent to the lungs, a perfect digestion took place. Several experiments of very able physiologists, some of which have been already quoted, contradict this result; neither has the opinion elsewhere received any material support. M. Brachet states in regard to it, that, from the numerous branches into which these nerves divide in the chest, M. Magendie must have failed in dividing them all. But it is time that I should leave this part of my subject; and I imagine that little more need be advanced, under this head, to prove the perfect complexity in which it is involved as connected with the stomach. If the question under consideration is capable of being elucidated by any series of experiments, it must be by one, performed on some of the numerous secreting organs of the body, whose functions are less complicated, than those of the viscus which has hitherto occupied our attention.

It has been found by numerous trials, that the kidneys may be exposed by cutting into the abdomen, and yet the animal survive several hours, and, notwithstanding the severity of the operation, it is ascertained, that the shock thus caused to the system is not sufficient to suspend the secretion of urine. That this is the case may easily be proved by emptying the bladder at the time of the operation, and afterwards, when death has taken place, or a sufficient time has been allowed to elapse, examining the state of the organ.

The kidneys are supplied with nerves from the renal plexus, which is composed principally of branches of the sympathetic system, having, however, communication with the cerebro-spinal nerves.

I shall now briefly notice some of the experiments which I have collected from various authors, illustrating the effects of injury of the different parts of the nervous system on the urinary secretion, believing, as I do, that these are, in the present state of our knowledge, better calculated than any others we possess, to clear up, in some measure at least, part of the difficulties by which we are so completely surrounded.

Naveau, in his inaugural dissertation, gives the following as the result of some of his experiments: Having previously ascertained that rhubarb administered to dogs might be easily detected in the urine, he divided the nerves going to the kidneys, and emptied the bladder. He gave the animal rhubarb, and in the space of eight hours the bladder was found filled with a purple-coloured urine, which was otherwise much changed in character. Urea was considerably less in quantity, and albumen much increased. Lint dipped in the fluid was tinged red by the colouring matter of the blood which was contained in it, but not the slightest trace of rhubarb could be detected. After this the secretion continued gradually to lose its characteristic properties, until at length it contained nothing but colouring matter of the blood suspended in a clear and thin watery fluid. This experiment was repeated by him several times with precisely similar results. He also states that division of the par vagum slightly affects this secretion, for although when rhubarb is administered it cannot be detected in the contents of the bladder, yet the natural constituents of the fluid remain the same. When the spinal marrow was divided at the first dorsal vertebra, the secretion continued, and rhubarb given was speedily detected in it, but the characters of the fluid were slightly changed. When the spinal chord was destroyed from the place of section, above-mentioned, to the sacrum, little change was observed, but when the destruction was carried further up into the medulla oblongata, respiration instantly ceased, and, although artificially carried on for thirteen minutes, no secretion was formed. The brain was next gradually removed, and here likewise the secretion was not affected. Immediately, however, on the medulla oblongata being injured, respiration ceased, and although this function was again artificially maintained, the same result as before mentioned occurred.*

Brodie informs us that, on removing the brain, the secretion of urine was suspended, although artificial respiration was maintained. In repeating the experiment he found the same result, together with a considerable reduction of the animal heat. He concludes, that, when the influence of the brain is cut off, the secretion is arrested, as also the formation of animal heat, notwithstanding that the usual changes in the appearance of the blood are effected by respiration.†

Westrumb relates three experiments which appear to have been carefully performed. He divided the spinal marrow at the first vertebra, and maintained artificial respiration, by which means the temperature of the body was kept at the na-

* Dissertatio Inaug. circa urinæ secretionem, Halæ, 1818.

† Croonnian Lecture, Philosophical Transactions, 1812.

tural standard. He injected into the stomach in the first two cases ferrocyanate of potassa, and into the third rhubarb, neither of which could be detected in the urine, although evidence of their presence in another part of the body was yielded by the proper tests. *

Having found numerous references to the opinions and experiments of Krimer, a German physiologist, on this subject, I feel sorry that I have not been able to obtain more than a brief notice of the conclusions at which he arrives. † They appear, however, so exactly similar to those already given from the thesis of Naveau, that I shall omit their repetition. Drescher likewise, in his inaugural dissertation, having referred to the observations of Krimer, confirms them in every respect.‡ Brachet states that division of the par vagum does not prevent the secretion of urine from being formed, though he adds there was some difference in the quantity and colour of the fluid of two dogs in which he divided this nerve at the same time. After division of the spinal chord in the neck, M. Brachet informs us, that the secretion proceeded, and when he combined this operation with the one last mentioned, the animal lived forty-seven minutes, during which time nearly an ounce of urine was secreted. In several separate instances, he divided all the nervous branches going to the kidneys,

* Journal Complementaire, Vol. xvi. † Ibid. Vol. xxv.
‡ Dissertatio Inauguralis de Systemate Uropoëtico.

but still found that a fluid was separated from the blood, of a rose colour, and having a distinctly urinous smell. Again having proceeded as in the last case, he in addition introduced a tube into the renal artery, and then divided the vessel completely upon it, by which means the circulation was perfectly maintained, whilst the continuity of nervous communication was as perfectly destroyed. The animal in this case lived four hours, during which time three ounces of a red liquid had passed into the bladder, which in all respects resembled blood. No wound in the kidney could be detected to account for this. He proved by another set of experiments, that this mode of operating did not suspend the circulation through the organ, for on making incisions into it, in various parts, perfect jets of arterial blood were yielded from the divided surfaces.*

I have thus endeavoured to collect the results of the principal experiments, which have hitherto been used as arguments on the subject under consideration. I have already stated my belief that those first detailed, and which have hitherto been principally dwelt on by physiologists, are too complicated in their results, to allow a positive conclusion to be derived from them. Although the others which are afterwards mentioned, are perhaps better calculated for this end, yet I would not have

* Recherches experimentales sur les fonctions du systeme nerveux ganglionaire, par M. Brachet.

it understood that I entertain the belief, that, by any facts hitherto recorded, a positive decision can be arrived at. In bringing this essay to its conclusion, I will first state some of the arguments by which it is determined that secretion is, in any degree, under the influence of the nervous system, and next examine the extent to which this influence is exerted.

I have only to mention the effects of violent mental affections on the various secretions,—as of grief on that of the tears, of fear on that of the perspiration and urine, of the stimulus of savoury odours on the flow of saliva, and lastly, of the increased production of milk in the female breast on the sight of her infant,—to convince the most sceptical that the different processes, by which these fluids are produced, are liable to suffer changes from the action of the nervous system. If these will not suffice, I refer to the results of some of those experiments first mentioned in this essay, which, though I have already deemed insufficient to determine the more difficult part of our enquiry, furnish, I imagine, in themselves abundant evidence that my conclusions in this particular are not incorrect. But it is almost superfluous to proceed further in arguing this part of the question, as I scarcely anticipate that any material objection can be raised against the view I have here taken. Those authors who are quite decided in their disbelief of the necessity of the nervous power in secretion, are perfectly willing to admit the exertion of an occasional influence. Thus Dr Alison, in a very able paper published in one of the numbers of the Journal of Science, says, when referring to some previously performed experiments of Mr Brodie, "when Mr Brodie concludes from these experiments that the suppression of the secretion was to be attributed to the division of the nerves, and that the secretions of the stomach and intestines are very much under the control of the nervous system, his inferences must command general assent."* "Functional secretion," says Mayo, "is to a remarkable degree influenced through the nervous system." † Bostock too, confesses, that "it is sufficiently obvious that the organs of secretion in the higher orders of animals are very much under the influence of the nerves, and in many cases materially affected by them." ‡

But do these secretions depend on some influence communicated by the nerves to the elaborating organs? can they, or can they not, take place without this transmitted agency? The principal objections which have been raised to the possession of this power by any portion of the nervous system will now fall to be considered. The first I shall notice is one to which great importance has been attached by its promulgators; and I must

* Journal of Science, Vol. ix. p. 113.

† Outlines of Physiology, 3d edition, p. 93.

‡ Elementary System of Physiology, Vol. ii. p. 426.

allow, that, if all were perfectly agreed as to its correctness as an ascertained fact, the question might be considered as decided. It is stated that in a considerable number of the lower animals, and in the entire vegetable kingdom, no traces of a nervous system have hitherto been discovered by the most minute anatomical investigations. Thus Bostock declares that, if a good instance of secretion, performed in animals where a nervous system has not hitherto been detected, can be furnished, the controversy is at once decided. If then this distinguished physiologist had followed out this line of argument, used in the former, and still adopted in the later edition of his work, and had cited an instance of this kind, his choice might probably have fallen upon some one of those animals in which a nervous system, truly of a more simple character, has lately been discovered. For since the time when this argument was first advanced, nerves have been distinctly detected in some classes of animals where their existence was not previously suspected, which circumstance proves at once that the controversy considered in this manner is any thing but decided. To this, however, it may be replied, that many still exist to which the remark of Bostock strictly applies; but as we have seen patient and successful investigation overcoming in a great measure this powerful barrier, and, arguing from the uniformity invariably observed by nature in all her works, we may infer, I think, with considerable propriety, that future well-directed research will establish the existence of nerves in those which remain. Then, in regard to vegetables, independently of the fact of their belonging to a different natural kingdom, in which all the vital processes are performed with greater simplicity, and in which many belonging to animals are altogether deficient, we may state that the analogy is not strictly perfect, and, therefore, although admissible as a corroborative, can scarcely be used as a decisive argument. I have much pleasure in being able to add the authority of Roget in support of the above argument, who, after observing that the organic affinities which produce secretion, and all those unknown causes which effect the nutrition, developement, and growth of each part, are placed under the control of the nervous power, goes on to remark, that " as the functions of plants are sufficiently simple to admit of being conducted without the aid of muscular power, still less do they require the assistance of nervous energy." * I make no use of the statement which has, I am aware, been ventured by some vegetable physiologists, that nerves do actually exist in this kingdom, as by M. Dutrochet and M. Brachet, the latter of whom declares his opinion that they belong to the ganglionic system, for the fact of

* Bridgewater Treatise, Vol. ii. p. 357.

such existence cannot, in the present state of our knowledge, be positively asserted. Another argument made use of on this side of the question, is the occasional production of fœtuses, in which a portion or portions of the nervous system have been found deficient, and in which the different parts of the body had attained a full size, inferring justly from this fact, that the various secretions have been duly performed. The instance of this species of malformation most frequently quoted, is that published some years ago by Mr Lawrence.* In this case, the child lived a few days after birth, and amongst others, the secretion of urine was duly performed. Besides this, numerous others have been from time to time recorded, of a similar nature, but in these as in that detailed by Mr Lawrence, more or less distinct traces of a nervous system have generally existed.

Positive experiments are likewise confidently appealed to in support of this doctrine, as, for instance, that of Bichat on one of the testicles of a dog, the nerves going to which he is said to have divided. Inflammation terminating in suppuration succeeded to the operation, which of course, as he observes, obscured the result of the experiment; but he insinuates that the very fact of the formation of matter taking place, would lead him to conclude that the nerves are not necessary to the seminal secretion, as this, the formation of matter, is an

* Medico-Chirurgical Transations, Vol. v.

analogous process. But it may be inquired, did M. Bichat divide those numerous branches of nerves which closely accompany the spermatic artery, and which, if he did not, might suffice for the performance of the analogous function of which he speaks ? * Mr Mayo, too, formerly published an experiment, in which he is stated to have divided the nerves of the kidney, after which the natural secretion proceeded. † This statement is, I imagine, sufficiently answered by the very numerous and satisfactory experiments already detailed, ‡ which, from the apparent accuracy with which they were performed, must be deemed more worthy of attention than the single one in question, in the performance of which some fallacy must have existed. The probability is, that Mr Mayo, seeing some fluid collected in the bladder, deemed this a sufficient proof that the secretion had been duly performed ; whereas, had he carefully examined the nature of the contents, he would have found them resembling in character that described in the thesis of M. Naveau.

Again, the state of patients when labouring under paralysis has been adduced as a further argument, for it is said that, under these circumstances, secretion is continually performed, and that inflamma-

* Anatomie Generale, Vol. iv. p. 604.

† Outlines of Physiology.

‡ Of Naveau, Brachet, &c. p. 19. et seq.

tion and suppuration may occur, the latter of which, as we have before stated, is termed an analogous process. Instances are also recorded where, in complete paralysis of the lower half of the body, erection and ejaculation of the semen have occurred; and Bichat, when arguing this point, relates the case of an individual in this state having contracted a gonorrhœa. But what do these instances prove? Certainly nothing for the purpose they are intended. To do this it must be allowed that all nervous power was suspended; but the very fact of the venereal appetite existing at all, and influencing the generative organs, denies that this could have been the case.

The progress of the formation of the chick in ovo, has yielded also a very formidable objection against the nervous theory of secretion. It is stated that the secretory processes "go on in the chick before any vestige of brain or spinal marrow can be traced, as also in the early part of the existence of the human fœtus, when the brain and nerves appear incapable of performing their functions."* I, however, apprehend that this argument will, in a great measure, be deprived of its weight, when it is remembered that, if allowed, it must likewise be admitted in regard to the formation of the various parts of the animal from the blood; for it is ascertained that the blood-vessels are distinct-

* Alison, in Journal of Science, Vol. ix.

ly formed before any of the fluid which afterward circulates in them can be accurately observed.

But I have arrived at that part of my essay, where I think it requisite to sum up the arguments, which appear to conduce to the establishment of the nervous theory of secretion, as it has been denominated by Bostock.

Bordeu affirms that the number of nerves usually distributed to glands are more than sufficient to ensure the life of the part; neither, says he, are these organs very sensitive, nor are they endowed with motion, yet, allowing that some are required for each of these purposes, several remain to answer some other end, " and are they not employed in secretion ?"* Though this manner of stating his argument may be considered as very ingenious, rather than strictly correct, yet I am inclined to attach some weight to the anatomical distribution of the nerves in favour of this theory. It is generally allowed by physiologists, that the secretions are performed by the minute arterial, and not by the venous capillaries. Now, anatomists inform us that arteries and excretory tubes, to their minutest ramifications, are abundantly supplied with nervous branches, particularly of the sympathetic system, whilst they do not appear to be so connected with the veins.† What, then, can be the use

* Recherches Anatomiques sur les Glandes, p. 341.

† Lobstein de Nervi sympathetici humani fabrica, usu et morbis, p. 103.

of this abundant distribution? They cannot serve the purpose of assisting in the circulation, else we might infer that they would be found upon the veins: and Dr Allen Thomson tells us, in an able paper on the circulation, lately published, "that although the circulation in the small vessels is obviously liable to be modified by the state of the nerves in their neighbourhood, or perhaps by affections of the nervous system in general, there is no reason to consider the capillary circulation as more immediately dependent on the nervous influence than is the action of the heart."* The known effects of opium, in suspending the secreting functions generally over the body, will serve likewise in support of this doctrine, more especially when the method in which it acts upon the system is remembered. It is through the nerves alone that this very powerful drug produces its effects; and we find that, whilst the economy is labouring under its action, secretion generally is either arrested, or at any rate materially diminished. Lobstein relates the case of a friend of his own whose hair turned white in a few days, from the great mental anxiety occasioned by the burning of his house; and numerous other instances are to be found on record of similar occurrences. Now the peculiar colouring matter of the hair is allowed to be the result of a secretion taking place in the

* Cyclopædia of Anatomy and Physiology, Article Circulation.

glandular apparatus, seated at the roots, and the manner in which this change was produced can only have been through the medium of the nervous system, and must therefore be regarded as a corroborative proof of the nervous theory. But by far the most cogent arguments, on this side of the question, are those derivable from the very important experiments already detailed, as performed by Naveau, Krimer, Brachet, and others. The first mentioned of these gentlemen, it will be remembered, found that substances introduced into the stomach, as rhubarb, which in the natural state may be easily detected in the urine by well established tests, soon after its administration, could no longer be so on division of the nerves going to the kidney; moreover, that the secretion gradually ceased to manifest its usual properties, until at length nothing but the serous part of the blood, with a little of the colouring matter, was separated; the result merely of a mechanical process of transudation. The result in these cases is the effect of an apparent and sufficient cause, and to me appears inexplicable on any other grounds than those to which it is here referred. The experiments of M. Brachet seem to confirm the above in every respect, and even go further in proof of the point in question; but these will require confirmation by other experimenters before they can be implicitly trusted. If, however, the whole of this series be found to be correct, and I see no reason to doubt the accuracy of the results of the former part of it, we need, I apprehend, no further proof of the decided part taken by the nerves in the process of secretion. I have hitherto purposely avoided all reference to that theory which has been advocated by Dr Wilson Philip, of the identity of the nervous influence and galvanism, which, though one of an unusually interesting nature, could not have been introduced here, without increasing the thesis to an unwarrantable length. Moreover, the two questions, viz. that which forms the subject of this paper, and the one to which allusion has just been made, are, I consider, perfectly distinct, at any rate in the state in which the latter at present exists. If, however, that identity should in process of time be satisfactorily proved, then these experiments, in which secretion, already suspended by the destruction of the nervous communications, was found to be re-established by galvanism transmitted along the divided nerves of the organ, will come to be powerful arguments on this side of the question. As, however, I have not been able to find sufficient data for the establishment of this fact, and for the reasons above-mentioned, I consider myself justified in having omitted a more lengthened detail of them.

It now remains for me to state the opinions which I have been led to entertain by the study of this subject; but the previous observations contained in the essay render it almost unnecessary to perform this part of my duty. The evident inclination which I have to adopt the theory of the dependence of the secretion on the nervous influence must have been easily detected. But I feel myself compelled to admit that the subject still requires a considerable share of close investigation, and it has always been allowed to be one of the most mysterious and difficult of elucidation in the whole range of physiological science. The arguments, too, on the opposite side, must, in common candour, be allowed to be most powerful. Yet, nevertheless, I regard the theory which I am thus inclined to adopt as one progressively gaining strength, which it will continue, with the general advance of physiology, to do until established on a firmer basis than that on which it at present exists. In support of this idea we have the recent discoveries of nervous systems in the lower classes of animals, which it must be remembered takes away at every step a strong hold of the opposing theory.

We have likewise the recent, and, if correct, very important experiments of Brachet, which, I imagine, only require confirmatory repetition almost to decide the controversy.

The observations of this physiologist, from some cause or other, which I am at a loss to explain, are viewed with general distrust in this country. This may arise from the results of almost all his experiments, too exactly supporting, what appear to have been his pre-conceived notions on the subject; but whatever may be its cause, as the results which he has published are, as will have been already concluded, of a very important nature, as connected with our present subject, I cannot help coinciding with the hope expressed in a late excellent review of M. Brachet's work, that "some of our British physiologists will be induced to turn their attention to this department of physiology, with a view of settling, as far as experiment can settle, the doubtful questions connected with it."*

But is not this the proper place for enquiring to what part of the nervous system should this power of superintending, as it were, the secretory functions be ascribed? I believe that this question might be easily avoided, on the ground that it is not strictly included in my present enquiry, the object of that being to ascertain whether such a power does exist, and not to point out its particular location. Thus, however, I am not anxious to effect my escape, but am willing to admit that at present it cannot be answered satisfactorily. Various situations appear to have been indicated for the residence of this influence, as the different observations which have been already noticed in the foregoing pages, and which need not here again be enumerated, will at once suggest. Present appearances, however, favour the idea, which has long been entertained by many, that through the

* Edinburgh Medical and Surgical Journal, Vol. xxxvi.

ganglionic system this power has its means of operation. When, however, the first of these questions shall have been answered, then the second will open out a most inviting field for interesting enquiry.

I have now only, in conclusion, briefly to notice the second part of my subject, and to explain the reasons why its consideration has not formed a more prominent feature in the pages of this essay. Its omission has not been one of accident but rather of design; for regarding, as I do, the process by which the solid textures of the animal machine, when worn out by the continued performance of their various functions, are constantly renewed, one, as essentially belonging to the order of secretion, as that of the urine or saliva, I was inclined to believe that its introduction might, in some measure, complicate the subject, and thus tend to obviate that degree of clearness which it has been my object to attain. I need scarcely adduce proofs of the similarity which I have just mentioned; they are performed by the same system of vessels, and are, as far as is yet known, regulated by the same laws. In this view I am supported by numerous physiologists; thus Haller regarded nutrition, and characterized it as "omnium simplissima secretio;" and in this his authority has been deservedly respected.

Few experiments have hitherto been made respecting the direct influence of the nervous power on this function. Those of Dupuy, Veterinary Professor in the University of Alfort, appear to have been the principal, in which he extirpated the superior cervical ganglia from the neck of a horse. He found that after this operation the animal might live many days, and even weeks, and that the principal effects were obstruction of the pupil, redness of the conjunctiva, general wasting of the body, and an eruption of a mangy nature, affecting the entire surface. Hence he concludes, that these nerves exercise considerable influence over the nutritive functions.* The effects of the different diseases of the nervous system on nutrition, as in paralysis, one of the consequences of which is wasting of the parts affected, have long been noticed, and adduced by some as arguments on this question, but they have on the other hand been explained by "the total inactivity of the parts affected."† Some observations which have been made, among others, I believe, by Magendie, on the eye, in cases in which the part of the fifth pair of nerves supplying that organ has been either divided, or its trunk diseased, have been likewise adduced, in which the first symptom was drying of the conjunctival surface, and subsequent rupture of the cornea from ulceration, terminating in discharge of the contents of the eye-ball. These facts, however, are

* Journal de Medecine, Vol. xxxvii. p. 340.

† Alison's Outlines.

far from sufficiently numerous or authentic to serve as decisive arguments, and, indeed, those last related appear rather to belong to the former part of the essay, and might perhaps have been more properly introduced there, as another proof of the suspension of secretion on injury of the nerves supplying the secreting part. Difficulties of a very perplexing nature present themselves to the performance of any series of experiments for elucidating this question, and it must be rather the analogy, if the similarity for which we have just been contending be admitted, on which arguments must at present, and I imagine likewise in future, be founded, establishing at the same moment, when the merits of the question shall come to be more fully understood, the dependence or independence of the two functions of secretion and nutrition on the action of the nervous influence.

APPENDIX.

Experiment I.

July 13, 1836. $1\frac{1}{4}$ P. M.—Two rabbits, previously kept fasting for twenty-four hours, were fed for half an hour on vegetables, and at a quarter to two the par vagum was divided on each side above the recurrent, and about an inch in length removed. Immediately after the operation the breathing appeared difficult, but soon became more placid, though still evidently affected. In about an hour the respiration again laborious, and attended with a slight noise, which increased up to four o'clock. In the second rabbit, the nerves were exposed, and lifted from their places, but not divided. It appeared quite unaffected by the operation. At 8 P. M. Noise attending inspiration of the first somewhat increased. The second is quite lively. The heart's action in both unaffected from the first. Six hours and a half after the operation both killed and immediately examined.

The stomach of the second, was not fully distended with the masticated food of a dark-green colour; a portion towards the lesser curvature was semifluid, of which some had passed into the duodenum.

In the first the stomach was more distended, and the contents externally of a darker hue, and throughout of one degree of consistence, resembling more nearly the natural state of the vegetable. The entire mass was dry. The duodenum and œsophagus empty.

The lungs in neither case were congested, but if at all, it rather applied to the second than the first. Neither trachea nor bronchi contained any appreciable quantity of fluid.

The bladder in the first much distended. On careful examination the nerves were found distinctly divided.

Experiment II.

July 16.—Two rabbits, previously starved for twelve or sixteen hours, were subjected at 8 P. M. to the same operation, after being fed as before. The one, whose nerves were divided, suffered from difficulty of breathing, which continued for half an hour, when they were left for the night. No attempts to vomit were observed. Next morning it was found dead.

On examination the stomach was distended, but not fully. The contents were little changed throughout, but on the surface was a layer of lymph-like matter. The œsophagus was partially filled.

The lungs were covered with black patches, and frothy mucus in the bronchi and trachea in considerable quantity. The mucous membrane of the latter was highly injected. The nerves had been perfectly divided. The second rabbit was not killed, as it would not have afforded a fair comparison.

Experiment III.

25th July, $7\frac{1}{2}$ A. M.—A portion of the eighth pair from each side removed as before without the rabbit being previously fed. Breathing much disordered, and refused food. 8. 30′. Still refuses food; several ineffectual attempts to vomit. 9. A little dandelion leaf eaten, when held close to its mouth. Breathing afterwards more thick, and attended by louder noise. 9. 40′. Loud noise still continues, and refuses food. 10. Respirations regular, but with much noise. 1. P. M. Breathing still with much noise, rather quick. Does not move about. 7. Noise during respiration increased. 7. 15′. Killed by a blow on the head. Contrary to expectation stomach found full of apparently digested food. Lungs natural, containing little mucus. Œsophagus empty.

Experiment IV.

25th July, 8. A. M.—A rabbit fed after fasting, and nerves divided, without any portion being removed. The cut ends were left, as much as possible, in contact. Respiration much disturbed, accompanied by a gurgling sound. 9. 30′. Respiration laborious with little noise. 9. 50′. Respirations irregular, without noise. 10. 50′. Appears lively, but breathing irregular. 1. P. M. More lively than either of the others operated on at the same time. Ate some dandelion, which had been accidentally left in its way. 3. P. M. Symptoms little altered, but if at all is more quiet. 4. P. M. No change, has again taken food. 7. 40′. Killed. Stomach quite distended; contents of black colour, unaltered in consistence, but that recently taken quite green. Duodenum empty. Œsophagus filled with green recently swallowed food. Lungs natural in appearance, containing little or no mucus. Trachea empty.

EXPERIMENT V.

25th July, 8. 15′. A. M.—In another rabbit previously allowed to feed freely, the nerves were divided, and portions removed, as in Experiments 1. and 2. Respiration increased in frequency, and accompanied by a slight purring noise. 9. 50′. Respiration quick, and very laborious, but with less noise, chiefly abdominal; one or two ineffectual attempts to vomit. 10. 50′. Much the same as at last report; one or two attempts to vomit. 1. P. M. Remains quiet. Breathing more difficult, but with less noise, still attempting fruitlessly to vomit. 4. Breathing if at all changed more difficult. 8. P. M. Killed.

Stomach found distended. More appearance, however, of digestion at the pyloric extremity. Central portions of the mass quite unaltered.

Duodenum and œsophagus empty. A little mucus in the trachea and bronchia, as also in the lungs.

Experiment VI.

25th July, 8. 20′. a. m. The nerves having been divided as in the last experiment, the trachea was opened, and a quill tube was introduced and secured. The respiration was, however, more difficult than in the two other rabbits. 9. a. m. Stands with its neck extended, apparently to facilitate the breathing, which is very difficult. The tube removed with some relief. 9. 30′. Breathing more easy. 9. 55′. Breathing again exceedingly laborious, with slight noise. 10. 50′. Vomited a slight quantity of mucus mixed with blood. 1. p. m. Breathing laborious ; has again vomited some clear fluid. 3. Breathing continues laborious. 4. Difficulty of respiration increasing. 6. 30′. Died suddenly, having, from accounts given, appeared previously very lively.

Examined at 7. p. m. Stomach much distended, contents covered with a film of grayish white matter. Interior of the mass unaltered. Œsophagus empty. Lungs dark-coloured, much congested. Air-passages filled with a frothy mucus. Duodenum contains a turbid fluid, having the appearance of bile and mucus.

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9

OBSERVATIONS

UPON

A "REPORT

BY THE SELECT COMMITTEE ON

SALMON FISHERIES, SCOTLAND:

TOGETHER WITH THE

MINUTES OF EVIDENCE, APPENDIX, AND INDEX."

30TH JUNE 1836.

BY

ROBERT KNOX, F.R.S.E.

EDINBURGH:
ADAM & CHARLES BLACK, NORTH BRIDGE.

1837.

To WILLIAM MURRAY, Esq. of Henderland.

SIR,

The following "Observations" were suggested by a conversation I had the honour to have with you and with your brother the Lord Advocate for Scotland. In that conversation the Lord Advocate and yourself expressed a desire to have the Salmon question tried upon its own merits, without a reference to what may be advantageous or disadvantageous for river proprietors or sea-side fisheries, or as to how far they are injurious to each other; and, as a preliminary step, that the Report of the Second Committee of the House of Commons, then just published, should be examined. I have since examined, with all the care in my power, that Second Report, and I flatter myself, that the following observations will satisfy any unprejudiced person that the question ought to be tried in the way the Lord Advocate suggested, with a view not merely to put an end to a most fertile source of litigation, but likewise to protect the public, independent altogether of the advancement of Natural Science, which, in respect to the Herring and Salmon, has never yet received from the Legislature the slightest consideration.

It is scarcely necessary that I should remind you, that our conversation upon the occasion I have alluded to mainly hinged upon my remarkable discovery of the food of the Vendace. By the kindness of your factor, Provost Thomson of Lochmaben, I enjoyed every facility for research, and left no link, I trust, deficient in the chain of an investigation entered upon with much anxiety, inasmuch as I foresaw great results to Natural Science. The Castle Loch of Lochmaben, which ornaments your property, furnished me the means of deciding the long agitated question, *the food of the vendace ;* the food of a fish which refused every bait of the angler ; whose habits in this respect were even more mysterious than those of the salmon and herring. Through this discovery did my brother and self go directly up to the real food of the Herring whilst in the deep seas, and proved what heretofore could not have been credited by any one, but which was yet perfectly in accordance with the grand provisions of Nature, that countless millions of fishes, of admirable quality as food for man, subsist on an animal so small as to be altogether invisible to the naked eye, and which, but for the aid of the microscope, must have remained for ever unknown to mankind.

With many thanks for your great liberality in forwarding the cause of Natural Science,

I have the honour to remain,

Sir,

Your very obedient servant,

R. KNOX.

NEWINGTON,
1*st August* 1837.

OBSERVATIONS, &c.

The difficult questions connected with the Scottish Salmon Fisheries,—the conflicting interests of upper and lower heritors,—the evident destruction of river Fisheries threatened by the extension of sea-coast fisheries,—and the development and discovery of new modes of capture of this much-prized fish; modes bearing almost exclusively on coast fisheries and discoveries, which could not be foreseen and therefore could not be provided for, by any Legislature:—these had slowly accumulated for a series of years, until something evidently required to be done for the relief and satisfaction of all parties. A Commission was accordingly appointed by the House of Commons in 1825, with power to send for persons, papers, and records. They sat during more than three years, and examined all classes of the community, scientific and otherwise, likely in the smallest degree to afford data for legislation. The landed proprietor, the tacksman, the practical or (as was to be expected) working fisherman, the London salesman, even the amateur naturalist was not forgotten, and if no professed naturalist was brought before that Committee, it cannot, we hope, be ascribed to the cause that no such person exists. The Committee failed altogether in obtaining any correct scientific knowledge of the habits of the fish; but, obliged to do something, an act was passed, known usually by the name of Home Drummond's Act, that gentleman having mainly contributed to effect its passage through the House, and to secure the co-operation of at least a majority of the parties. But as the act was merely a *compromise*, ten years had not elapsed when it became once more necessary to re-open, as it were, the whole commission; to appoint another committee with powers similar to those of the former one, but ostensibly more limited as to its inquiries, as may be gathered from the following preamble;—" Ordered, That a Select Committee be appointed, to consider the state of the Salmon Fisheries in Scotland, in as far as relates to the altering the close times in different districts in that part of the United Kingdom,—the laws for the observance of the Saturday's slap or opening in all cruives, engines, machines, or devices of whatever description used in salmon-fishing,—the construction and regulation of cruives, the regulation of mill-leads or courses, and the removal of dams and obstructions in all rivers, streams, or waters, and to report," &c. That the Committee did not adhere to their instructions, is not to be wondered at; *the natural history of the salmon had not been ascertained by the former one.* Now, so long as this remains doubtful, just so long is it impracticable to legislate upon any certain principles, whether upon the matter of *close-time* or *proprietorship*, or, indeed, upon any other point whatever, connected with the Fisheries.

Before examining the labours of the former Committee, and the voluminous and conflicting evidence submitted to them, we shall first analyze the evidence submitted to, and the Report thereon by the present one; the single heading of " proper period of close-time for salmon-fisheries in Scotland" would have sufficed as a heading to all we purpose saying on the matter, had the Committee adhered to their instructions; but we shall presently find, that they did not do so, and this compels us to follow them in their digressions, which were neither few nor brief.

1. The Committee having met, agreed at once to extend their inquiries, and to *include within its range the following important subject-matter*;—" To inquire into the increase or decrease in the numbers or weight of salmon-grilse and sea-trout taken in the several rivers and sea coasts of Scotland since the act 9th Geo. IV, c. 39, came into operation." This necessarily opened a wide field for inquiry,—ripping

up, as it were, old sores that had never healed well,—we mean the local, though extremely important, question of the gradual growth and vast preponderance of sea-fishing by stake and bag-net over *river-fishing*, however practised, in respect not merely of superior quality of the fish taken, but likewise the regularity of supply, its far greater abundance, and the far more *economical* mode in which it is managed.

Is the question of legislating for the salmon fisheries a national question or a particular one? We consider it a particular question, and would suggest as follows :—

1*st*, When a river belongs to one proprietor, it is the height of folly to pretend to advise him how to fish his river; to legislate to him about close or open cruive-dykes, legal or illegal engines, Saturday's slap, or Sunday's fishings. Let him do what he pleases with his own, nobody can better know how to study his own interests. The spendthrift of an entailed estate hurrying on to ruin might be disposed to destroy the fishings of that river of which he is a life-renter,—but those are partial evils. Let the heir-at-law look to it.

2dly, Let the heritors or proprietors of each Scottish river meet and decide by a commission, jury, or otherwise, what best suits the river which belongs to them; let them legislate for it, or submit to the Legislature regulations for it; let all modes of capture be legal to which they give their consent.

It is amusing to watch the movements of the Committee in their efforts to devise means to protect the spawning fish and fry, in other words, to prevent the total destruction of the fisheries, which could be effected in a few years by allowing the lower orders of peasantry on the upper streams of rivers to have their way. *First*, An act is passed declaring it poaching to kill salmon during a certain time called close-time, but as this is the only period when the fish appear in the upper streams, the heritors or proprietors of the banks of these upper streams having no interest whatever in the preservation of the salmon, very naturally neglect the act, or set themselves in direct opposition to it.

2dly, The Committee next endeavour to coax them into good humour by allowing them to angle during close-time for fourteen days or more, that is, to have the pleasure but not the profit; and thus they hope to induce the proprietor of the upper streams to quarrel with his tenantry, to watch them, to prosecute as he would do for the unlawful killing of grouse, deer, or partridge; truly an excellent device, but, notwithstanding its ingenuity, we predict it to be a failure. Upon the whole, the idea of establishing a bait of fourteen days' angling in close-time as a kind of private *bon bouche*, a sort of stolen sugar-lump to secure the upper heritors in the interests of the lower, is a lamentable kind of legislating. We vastly prefer the mode adopted in the north—*buy up the rivers, and remove the peasantry* to the sea-side, or wherever they may choose to go. There is one thing which appears to me extraordinary as not having occurred to practical men—That if the upper heritors are to be consulted at all—if it be thought worth while bribing them for the sake of their protection to the spawning fish and fry, why not offer them *something substantial*, such, for example, as *the produce* or *value of the produce* of ten or twelve days' fishing of the first mile of the river from the mouth upwards; this would surely be better than licensing them to kill foul salmon for their amusement in September and October.

The matters inquired into by the Committee may be reduced to the following heads:—1. The regulation of the close-time, and whether it *should vary for almost every river*, or be general, and the same throughout the kingdom, that is, whether or not the varying principle should be adopted as the basis of legislation. A careful examination of the evidence has led us to conclude, that there exist no data for the determination of this question, and that in respect to river fishing, there exists a circumstance which must render it extremely difficult of decision. The circumstance we allude to is the state of the rivers as to floods; but, barring this, which must produce puzzling anomalies, we apprehend that the only way to decide the question would be the evidence of an impartial person as to the exact condition of the fish taken in such a river, say from 1st August to 1st October, the usual latitude proposed for the commencement of the close-time. None, we think, can doubt the necessity of closing all or most rivers after the 1st October. If river fisheries were done away with, as they ought to be, the facility with which *close-time* could be determined and ordered throughout the kingdom becomes manifest. Nay, it is just possible that no such regulation would be required; and here arises another great question in the natural history of the salmon which has never been solved by proper inquiry—Are foul salmon ever taken in the sea? and, if so, under what circumstances and in what numbers? To decide an extremely important question like this, an extensive series of experiments should be instituted round the coasts, by means of bag and stake nets, fished uninterruptedly winter and summer for three or five years under the superintendence of unprejudiced persons. We lean to the opinion that foul fish can never be taken in the sea in any great numbers, and that consequently there should be no close-time whatever, and all restrictions in this matter might be done away with on this opinion being proved. The salmon remaining in the sea during winter are probably barren fish, and in the highest condition as food for man. By barren fish we mean those in whom for that season at least the milt and roe had not become developed. If this important measure were found impracticable, then the next best measure would be a journal of the state of the fish (kept by an officer appointed for the purpose), taken on each day from the 1st August to the period when by their advance towards spawning they become not only poor, but even unwholesome food, and when a continuance of the fishery would lead to destroy it root and branch by a capture of the breeding fish. An average from such journals might be taken after five years, or a person appointed annually to fix the day, by actual inspection, of the produce of the stake and bag net fisheries nearest to the river mouth; even the London salesmen could decide this point annually without much difficulty.

Again, we repeat that there is no evidence in this Report to satisfy any one "when the close-time should commence and when it should cease," keeping in view the interests of all parties. Above all, there is no evidence to shew that close-time should vary throughout the kingdom.

That this will appear a strong statement to some, after all the labours, as well of the present as of former committees, we have no doubt; but it is a statement made after the most careful and deliberate examination of every passage calculated to bear on the point. The statement, that in many rivers, clean salmon may be captured on all days of the year, is below all notice. The question for the nation, is not whether a few clean salmon can be taken during the *natural close-time* of rivers or on the shores, (if a close-time were then required, which we doubt,) for the pampered and over-satiated appetite of the London Alderman, or Grand Proprietaire, or whether the national interests are to be consulted. The occasional presence of a few clean salmon in rivers, at times when thousands foul may easily be proved to be also present, even by the few scanty facts naturalists and fishermen have been pleased to give us, by no means warrants the fishing these rivers.

The Committee is in error, as we shall afterwards shew, in saying that there existed, at the period of their deliberation, one uniform season of close time or fence months; there were always three at least for Scotland, unless the Tweed and Solway and their tributaries be reckoned purely English rivers, these latter having always had their own fence months distinct from the Tay and others.

We cannot believe that any practical man, open to conviction, who has watched the fishings of the Tweed, Tay, &c., can have any doubt that all fishings above the entrance of a river into the sea, or into an estuary, should be abolished, were it only in order to put a stop to the enormous expense connected with it. The farce of fishing the Tweed above Berwick Bridge, is a most expensive farce, but still it is one; it is almost inconceivable that any one can be found silly enough to pay for it. Why not let the entire river fishings to one company or to one individual, who, of course, would fish it only at one point, (the Bar,) and thus save an incredible labour and expense. I rather think that the same remarks might be applied to the Tay, &c. Indeed, during the progress of the former enquiry, members of the committee and others who read the evidence, must have been forcibly struck with the present truly absurd mode of fishing the Tweed, Tay, &c., by net and coble, throughout miles of river, and, of course, at an enormous expense, whilst it was evident to all who had deeply studied the question, and combined that study with the habits of the fish, in so far as they were known, that fishing for salmon in rivers above the tideway, ought to be put an entire stop to, with the exception, perhaps, of one single active fishing party on the bar of the river, and during the height of the season.* But one thing appears evident, that no effectual or satisfactory legislative measure can be made out, until the whole question be tried on its scientific and practical points. In 1825, bag-nets were little if at all used, now they are most extensively; an act against fixed engines could scarcely be applied to them. We could even imagine it possible to withdraw them, and re-set them so frequently as to give them more the character of a moveable than a fixed engine.

It must always be remembered, that Mr Home Drummond's act was after all but a partial one, it did not include the Tweed and Solway.† These rivers had separate acts, and a distinct close-time, and hence arose the singular fact

* This seems to have been the opinion of Mr Oliphant, M.P., and a member of the Committee.

† Have any steps been taken to ascertain the natural close-time of any Scotch river? Any journals of observation been kept by persons worthy of credit? Any scientific data collected? Every one knows that there are no such data, no such journals in existence. The Committee of the House of Commons took the evidence of a naturalist with respect to the natural history of the salmon; of a naturalist who had never seen the spawning of the salmon; had never visited the spawning beds; had never examined the ova; had never observed their growth; had never experimented nor dissected at all; this was the kind of evidence submitted to the Committee of the House and to the public.

and occurrence pressed on Mr H. Drummond himself by Mr Wallace, at a dinner given to 600 gentlemen at Stirling, *after close-time in all Scotch rivers*, except the Tweed and Solway; the table was covered with good fresh salmon. Now, where did these salmon come from? We have no doubt Mr Drummond would say, from the Tweed or Solway. No such thing; they came simply and very naturally from the nearest rivers. Thus, in point of fact, there never was one close-time for all Scotland, and Mr Drummond's act had no effect whatever in stopping poaching on the most extensive scale. In truth the law has been set at defiance by the Duchess of Sutherland; her rivers are opened and closed when she pleases, and impassable cruive-dykes are built in due season across most of them. Now all these doings are against the act. Year after year, all fishings above the mouths of rivers, near which there are also sea-side fishings, have become of less and less value; the expense of the coble-net fishing is great; the management of the Tweed fishings, for example, in 1830, was most extraordinary. At that time, now seven years ago, after the most deliberate examination of the fishings, we declared the fishing of rivers by net and coble, as the greatest delusion and imposture we had ever seen. We further recommended, that the mode of fishing for salmon throughout the kingdom should be entirely altered; all river fishings above the tideway (or any proper part to be determined by practical men,) put an end to in all rivers, and the salmon fished for, at or near their feeding grounds, by stake and bag-nets, &c.

If river fishings must still be maintained "as a nursery for seamen," in despite of the existing evidence, then come again all the difficult questions of close-time, proprietorship, protection of fry and of spawned and spawning fish, &c. &c.

In every river of any consequence, the "lowest stream where salmon spawn ought to be ascertained," and all fishing above it strictly prohibited. This most important point has never been made out, it is not known positively in respect to any Scotch river. It is easy to find book naturalists who will make assertions on these points, but of what real value are such opinions?

The *principle of a varying close-time* means simply an act for each river. *Thus the Legislature returns to the point from which it started.* If the close-time be late in commencing, you destroy the spawning fish. If it terminates too early, you destroy the kelts or spawned fish. Of these two evils the former is decidedly the greater. In fact, it would be advisable to close all river fishings on the 14th August; but why not prohibit river fishings above the tide-way altogether. We contend that this is quite a practical measure, and that it would produce incalculable benefit to all concerned.*

What is the latest period of spawning? Mr Hogarth says that a great number of fish in the Tay have not spawned on the 15th December. Mr Hogarth's theory as to the cause of the good condition of these fish which enter the rivers early in spring may be found at page 48 of the Report. We have ventured elsewhere another theory; they are certainly both theories, but ours is founded on practical examination and analogy. It is curious to trace the gradual correction of the theory of the *distant migration* of the salmon. Every year brings him during his feeding and fattening season nearer to the coasts. The same has happened in respect to the herring. All these erroneous notions about the distant migrations of the salmon and of the herring originated in naturalists not knowing the *food of these fishes and their feeding ground.*

Are there early rivers and late rivers? This is a scien-

* The consideration of a close-time for any of the salmon rivers in Britain or Ireland, involves at least two questions in Natural History which ought to be previously settled: 1*st*, What is the average period of the run of the spawning fish? 2*d*, When do clean fish begin running in sufficient numbers to make it worth while opening the rives? If the evidence offered the former Committee of the House of Commons be worth any thing, it proved, that, from the extreme north of Scotland to the River of Cork, the *spawning fish* run all *at one time*; when clean fish begin *to ascend* in spring in any numbers could not be made out.

tific question. Can the early rivers be fished without injury to the salmon fisheries generally, by the taking of unspawned fish? This is a practical question, and these are quite independent of each other. They are most important, and must not only be viewed separately, but handled separately. To what extent does the taking kelts or spawned fish early in the season injure the fisheries? This also seems a purely practical question, but still it requires, previous to coming to a decision respecting it, the settlement of a scientific question, viz. How many, on an average, of salmon that have once spawned, return to the same or other rivers?

Although the Committees of the House of Commons were both altogether unequal to the investigation of the questions submitted to them, which I hope may be said without disparagement, they still could not but discover, from the extraordinary conflicting testimony brought out, that the *food of the salmon* and his feeding ground had not been made out. We imagined that we decided both these points in 1832. The discovery of the food of the vendace of Lochmaben preceded that of the salmon and herring; and, with regard to the two latter, the field of inquiry is so extensive, and the expense attending minute investigations concerning them so very great, that many points require still to be investigated. Whilst all admit our discoveries respecting the *vendace* (a fish closely allied as a natural order to the herring and salmon, that is, the *clupeadæ* and *salmonidæ*), and their great results upon certain geological and ichthyological questions, some yet dispute how far such discoveries apply to the salmon and to the herring! Thus Mr Yarrell, Sir William Jardine, and other active amateur naturalists, admit fully and readily our discoveries in respect to the food of the vendace, but they think that the salmon lives upon a grosser food than we have stated. Sir William says that salmon are caught in the sea on the north coast with hooks baited with sand-eels or small fish; it is just possible, but in the mean time we doubt the accuracy of the statement; and it appears to us to rest on no positive evidence. We have ourselves taken salmon at Kelso with a thing called a salmon-fly, and with lobworm and other bait, but then we never for a moment imagined that they live upon these. We are aware that herring-fry forms a good bait for the common river trout, although the trout may never have had access to the sea, and certainly never saw a herring or its fry.

The following brief statement of facts may serve to remove some difficulties in the way of a complete settlement of these great practical questions, viz. 1*st*, What is the usual food of the herring, and what part of the ocean is his usual feeding ground? 2*dly*, What is the usual food of the true salmon and grilse (supposing the latter to be distinct from the former) whilst in the sea, and where is their usual feeding ground? To answer these questions properly requires me to state the opinions of others; from my not having done so formerly, that is, in my Memoirs presented to the Royal Society of Edinburgh, the most extraordinary misstatements have been indulged in respecting my views by persons not only altogether ignorant of the subject, but by their education and habits incompetent to arrive at any rational conclusion respecting them.

From the earliest times, nearly all practical and experienced fishermen, and most naturalists of reputation, have candidly admitted, that the food of the herring and of the salmon was *altogether unknown*, and likewise that it was not known into what part of the ocean they retired, when, quitting our coasts, they disappear for a season; leaving these coasts and rivers generally in a state of great exhaustion and meagreness, and returning renovated, strong, and in the highest order as food for man. I find that I must first establish the fact that this was, and still is, nearly the universal opinion of practical fishermen and of experienced naturalists; for, singular to say, when I on a former occasion made this statement, it was denied by those whose very denial of a well known fact in the natural history of these fishes shewed their utter ignorance of the whole question. Instances were cited by those persons of herring and salmon having been examined in whose stomachs a very palpable food had been detected, viz. the ova of other fishes, and even the young of their own and of analogous species! That occasionally, though very rarely, in the stomachs of a few herrings haunting the shores, and always in bad condition, there may be found small fishes, and even the fry of their own species; and that this is true also of the salmon, to a still more limited extent, may be admitted, and is a fact indeed which we had thought was known to all the world. It is quite unnecessary for any one to state that such or such a naturalist found some young fishes in the stomachs of grown herring or salmon, because any one who has been in the habit of opening numbers of these must have observed the fact long before the discovery of such naturalists. *But this is not the question.* The question is, What inference do these persons mean to draw from this circumstance, which had been known from the most remote times? Do they mean to say that these small fishes or fry constitute the usual food of the herring and of the salmon? If this is their opinion, why not candidly say so at once? Being quite aware that the herring and salmon, especially if out of order, were occasionally known to take a common bait, such as any small fish, &c., I felt it necessary to proceed with the inquiry in a different manner than heretofore. It required little reflection to foresee, that although a thousand salmon and ten thousand herring were shewn to have nothing in their stomachs cognisable by the eye (unaided by the microscope, in the hands of a person scientifically and liberally educated), yet the circumstance of one or two being *occasionally* found in a different state would at all times form a *sufficient ground* with such persons to deny the whole of my inferences. I took care to select, as the first object of my inquiry, a fish *in whose stomach no naturalist or other person had ever detected any food*, a fish which had never been known to take a bait; that fish was the vendace of Lochmaben, whose food I discovered in 1832. The inquiry into the salmon, herring, and still later the char, followed in succession, and having got a key to the whole mystery by discovering the food of the vendace, which served as a " light to our path," difficulties disappeared which must otherwise have proved insurmountable, had the examination and discovery of the food of the vendace not preceded that of the herring, salmon, &c. The fact of the vendace, a fish of considerable size, and very numerous, not only thriving and subsisting entirely, but becoming the most delicious of all fishes, *on microscopic food only*, opened up a vast field of inquiry, both in regard to the existing and to the extinct or fossil race of animals. It is even quite possible that the fresh waters abounding with microscopic entomostraca might be nutritious to man himself. But to return, 1*st*, I never yet found a practical fisherman who would offer even a conjecture as to what might be the food of the herring or of the salmon whilst in the sea; but, on being pushed to an answer, some would say, that perhaps the herring lived by suction (for of the food of the salmon they usually declare their utter ignorance), to which term, however, they do not attach any very precise meaning. Others seemed to think that they might live on air and water, and many again going up to first causes, which the ignorant generally do, did not see any necessity for herrings requiring any food. Herrings, say they, were a gift of Providence to man, and as their numbers are miraculous, so is their food.* That

* It is curious to remark that the illiterate fishermen came nearer to the truth than the half-educated scientific man. The fishermen being ignorant of the use of the microscope, simply asserted a fact, viz. that no food was to be made out with the naked eye. The half educated man on the contrary affirmed that, although he could not see the food it ought to be present in a palpable shape, and of a size proportioned to the fish that lived upon it; he then set about inventing an hypothesis to explain what he could not understand.

The less these persons know of the matter, the bolder uniformly are their assertions; and as they care not one farthing about scientific truths, they fearlessly affirm whatever seems best calculated to support their hypothesis. An instance occurred a few years ago, when a very respectable person asserted in a newspaper, that herrings live on fry and small fish, although he was perfectly incompetent by habits and education to engage in any scientific inquiry, and never had, and could not examine the question in any shape. It has been long remarked of these persons generally, that "what they imagine to be true, that they believe to be true; and what they believe to be true, that they will swear to be true."

these are the almost universal opinions, could be proved by the most extended evidence. Since, however, many of these practical men know extremely well that the herring and salmon could not live on air and water, whether singly or combined, and that nobody doubted the food of haddocks, cod, skate, &c., they devised various hypotheses to meet the excessively difficult question, viz. How it happened that the stomachs of the salmon and herring seemed always empty, more especially if the fish themselves were in good condition, for it is a notorious fact, known even to the cook, that if any putrescent *debris* be found in the stomachs or intestines, the fish as food is not good. Of these *hypotheses* I shall mention only two by which the rest may be judged of. In compliment to the great name of Sir Humphrey Davy, we shall give his theory the preference over his more practical followers. Sir Humphrey having no idea that a fish of the magnitude of the salmon, or indeed perhaps that any fish, could live, fatten, and thrive exceedingly on a food whose nature was discoverable only with the microscope (the ova of the echinodermata, and probably also of most crustacea), invented a hypothesis to explain the emptiness of the salmon's stomach; like the vulgar he went up directly to a final cause, and boldly conjectured that the salmon, foreseeing his length of journey in ascending the freshwater streams, took special care to avoid using any food for some time, in order to lighten himself as much as possible. The other hypothesis to which I have alluded was a more ingenious one. We owe it to a Mr Fraser, a practical fisherman, and the tacksman of Dalnacarloch. He finding, as all had done before him, that the stomach and intestines of salmon seemed uniformly empty, but at the same time fully persuaded, and having his mind altogether preoccupied with the idea that the salmon, from his bulk and richness, must eat voraciously of some large palpable kind of food, threw out the very bold conjecture, that the digestive powers of the salmon were so extraordinary, that they digested their food, which he supposed to be other small fishes, almost in an instant of time. His words are: "The digestive powers of the salmon's stomach are like a consuming fire." It never occurred to him that in sea-side fisheries the stomach of the salmon ought even then to be found full pretty often if his views were right. I trust it cannot be required by any rational person that I should seriously refute either of these hypotheses. No animal living, not even the shark, possesses such digestive powers as to consume in a few minutes the bones, tendons, and other less digestible parts of their prey.

The difficulty, however, as we have seen, was not confined to practical men. The most experienced naturalists and anatomists have uniformly avoided the question of the food of the herring and salmon, or candidly admitted their ignorance. The following quotations ought to put this question at rest.

Salmo Salar, Linne.—Habitat in oceano, &c. Præter pisces, vermibus insectisque aquaticis victitans." P. 1365.

Salmo Trutta, Linne.—Habitat alternatim in mare Europæo et fluviis, &c. Præter pisces, vermibus insectisque victitans. P. 1367.

Salmo Sylveticus.—Habitat in Europa, Sibiriæ aliarumque terrarum mari Caspio finitimarum rivis torrentibusque alpinis, &c. Præter pisciculis, vermibus etiam testaceis et insectis aquaticis victitans, et ne propriæ quidem speciei parcens. P. 1365.

Salmo Eperlanus.—Habitat in Europæ lacuum fundo arenoso, in mari, vere parturiens, magnis gregibus in fluvios ascendens, vitæ minus tenax, vermibus testaceis potissimum victitans. P. 1375.

Salmo Laveretus, Guinied, Lavaret.—Habitat in maris Europæ septentrionalis profundis, harengo, cujus ovis inhiat, &c. P. 1377.

Salmo Thymallus, Linne. (*the Grayling.*)—Habitat in Europæ fluviis, &c., testaceis, coleopteris, piscibus minoribus, ovis potissimum favioris, et Salaris victitans. P. 1380.

Salmo Maræna.— Aut vere testacea quæ siturus. P. 1381.

Salmo Marænula.—Gregarius in Germaniæ, &c.; vermibus et insectis victitans. P. 1382.

Salmo Wartmanni.—Habitat in lacus Aeronici profundis; et vermibus, insectis spongiæ quædem speciei victitans. P. 1382.

Clupea Harengus, Linne.—Habitat in maris Europæi Septentrionalis et Atlantici profundis, vere, &c.

NOTE.—No mention is made of the food of the Herring by this the greatest of naturalists.

Clupea Harengus, Linne.

Clupea Alosa; Shad, Linné.—Vermibus insectisque victitans.

Clupea Thrissa.—Habitat in mari Jamaicam, Indiam alluente; crustaceis minoribus, testaceis, piscium ovis, victitans. P. 1405.

Des Saumons, Cuvier.—Ils sont d'un naturel vorace.

Le Honting ou Hanten, des Belges. Salmo Oxyrhincus, Cuvier.—De la mer du Nord de la Baltique, où il pursuit les bandes de Harengs; on le prend dans le Lac de Harlem. P. 307.

Des Clupes, Clupea, Les Harengs, Cuvier.

NOTE.—No mention is made of the food of any of the species.

The Rev. Dr Walker, late Professor in the University of Edinburgh, published an Essay on the Natural, Commercial, and Economical History of the Herring. Extracts from this essay were published by the Highland Society, and from these extracts I take the following observations in proof of my statements:—"The food of the herrings, and especially that on which they fatten, is very little known. I have examined their stomach at the different seasons of the year, without finding in it any sort of palpable aliment." In the same volume of the Transactions of the Highland Society, Mr John Mackenzie has published some remarks respecting the herring and its fisheries. He states them to be the result of personal observation and experience, but this is hardly reconcileable with the following passage:—"In regard to the food of the herrings, it has been frequently observed that the small fry seek their nutrition out of the marine algæ, or from some matter adhering to them; that herrings will swallow a small clear unbaited hook such as is used for catching haddocks, when tied on a fine line; a device which has been successfully adopted when the herring fishery is carried on in deep water, in order to discover the arrival of the shoal. It seems certain, therefore, that the herrings take these hooks for such animalculæ as they at least sometimes feed upon."*

"Another article of their food is an oozy substance at the bottom of the sea," &c. &c. "But no man can say with

* Practical fishermen state, that they can at all times, and in most situations, take a few herrings by means of half-a-dozen hooks tied on a horizontal piece of stick, which again is suspended to a long line. This is simply let down and quickly drawn up again, and the herrings are found transfixed at various parts of the body, but seldom or never by the mouth.

certainty, of what variety of articles the food of herring consists, nor how, for instance, it is produced in the Frith of Forth in such abundance as to nourish the myriads of these fishes that subsist therein, from their arrival in October till their departure in spring."

In plain terms he *never saw the food*, and knew nothing of it.*

On the food of the salmon, Dr Walker remarks:—" On examining the stomach, little is found in it except slime, or some half digested and some half entire insects. These appear to be the food of the salmon while in the fresh water," &c. &c. " It is probable that they receive in the sea a more copious food, and of a different kind, but the precise nature of this is unknown."

Mr John Mackenzie, in the same volume of the Highland Society Transactions, says, in regard to the food of the salmon, that " what salmon live upon while in the sea it is not possible to discover."

The opinion of a Mr James Morrison (p. 392), is that salmon occasionally live on herrings. " Some have asserted," &c. It is needless to say that this opinion is directly contradicted by all the evidence.

Mr Drummond, who had also written some practical remarks on the herring, observes of the food of the salmon, " A remarkable thing in the habits of the salmon is, that the longer they remain in fresh water, the more

* In the third volume of the Prize Essays of the Highland Society of Scotland, is a paper by Archibald Drummond, Esq. This gentleman evidently writes from much personal knowledge and experience. " On what they (herring) live we can only form a conjecture. I have seen their stomachs opened at all seasons of the year in which they appear here (   ), but never found any thing in them excepting some slimy matter." " A friend of mine, when at Loch Lhynne, near Fort William, this season, upon the arrival of the boats cut up many herrings, but never discovered any thing in their stomachs; the fishermen, however, assured him that they frequently got in the *foul or spent fish*, several of their own fry, sand-eels, &c. It has been asserted that to the northward of Shetland they feed and fatten on a species of Medusa. I cannot help combating this opinion, as I never took any of these blubbers in my hand without having it disagreeably blistered, and naturally conclude that they cannot be a delicate morsel for the herring."

voracious they become; in the spring, when foul fish, they will eagerly take every kind of bait. Yet voracious as they are, and desirous as they appear for all this variety of food, they still exhibit the singular phenomenon, that when their stomachs are opened, you will never find the appearance of food of any kind."

It was to explain away this difficulty, the usual empty state of the stomach of the salmon, and the absence of the *debris* of such substantial and palpable food as they imagined the salmon *must* live on, that the hypotheses of Sir H. Davy, and of Mr Fraser of Dalnacarloch were devised. But, in fact, the stomach and intestines of the salmon are not usually empty, being always more or less filled with the ova of the echinodermata, and of other shell-fish, than which kind of food nothing can be imagined more nutritious.

Mr Smith of Deanston thinks, that "salmon feed on animalcula." He has forgot to state to what species these animalcula belong, but he thinks these animalcula may find food in common sewers! *

This is an exact state of the knowledge respecting the food of these fishes, previous to our researches.

Mr Rennie, late Professor of Zoology in King's College, London, has the following statement in an edition of White's Selbourne, published by him so late as 1834. His authority has always been reckoned a good one in Natural History. "The food of the salmon and of the herring is perfectly unknown, nothing having been ever found in their stomachs and intestines but a little yellowish liquor."

In the minutes of evidence taken before the Parliamentary Committee which sat in 1825, the opinions of the witnesses are extremely conflicting. The greater number declare the food of the salmon to be unknown; some conjecture that they live on *worms*, and other *insects* got in the sea! and several have mistaken the *tape-worm*, which almost always infests the cœca and intestines of the salmon, for their food! On these points the evidence is below the notice of any scientific person, but proves my original posi-

* Parliamentary Report, p. 273.

tion, that heretofore the food of these valuable fishes was altogether unknown.

From the preceding quotation, it is evident that these distinguished men knew nothing of the food of the salmon or herring. The names of twenty compilers, and copyists of their writings, may be given to prove that they also were naturally enough unacquainted with the subject.

Having thus shewn that the best naturalists, anatomists, and practical fishermen, were, and still are, totally ignorant of the nature of the food of these two remarkable species of fish, and that many have had the candour to state so, whilst others were at least ingenious enough to maintain a profound silence on the point, I trust it cannot be necessary to reply any further to those persons who merely repeat the hackneyed facts, that herring (out of condition) occasionally eat the young of their own species, and that salmon, whilst in fresh water, will take a bait, such as worms, flies, &c. *These circumstances have been known from all times*, but what I think we have a right to ask of the persons who seem to attach importance to these insulated facts, is, to take the next step, and say at once that, in their opinion, small fishes, insects, &c., constitute the natural food of the two species. *But they have not yet said so*, and knowing what reply practical fishermen, who open thousands of these fishes annually, would make to such nonsense, I do not think, however much inclined, that they will venture to err so greatly against common sense.

I shall next state the steps taken by me in the prosecution of these researches. For reasons already stated, the vendace of Lochmaben was selected as the first in the series, in preference to the herring or char, both of which species were known occasionally to take common bait. We were sure, in respect to the vendace, of two points, *First, No one had ever thrown out even a conjecture as to its food, and it did not take any bait of the angler ; Secondly*, The locality was better suited for inquiry ; inhabitant of a small fresh water lake, we were certain of getting more readily at its habits ; the habitat of the herring, on the other hand, is the ocean, a field boundless, if I may so say, in extent, and beyond our grasp. The food of the vendace was quickly found by us to be the *microscopic entomostraca* with which the waters of the lake abound.*

In the autumn of the same year (1832) in which we discovered the food of the vendace, we had an opportunity of examining considerable numbers of very fine herrings, taken off the Isle of May, and particularly in those first examined by us, we found abundance of marine entomostraca, of extreme minuteness.

In the latter end of July 1834 fresh herrings were brought to the Edinburgh market in considerable numbers, and of superior quality. The best, as an article of food, averaged about 9½ inches in length, and in the greater number of these we found abundance of entomostraca still of extreme minuteness. We sent a short account of the condition of these herring, and drawings of the entomostraca, to the most liberal of the weekly journals, but they thought proper not to publish this communication; on the ground, perhaps, of not being of sufficient general interest, and also of its reflecting too strongly on the recklessness with which amateur anglers and traders in herrings will persist in publishing the most incredible nonsense on matters respecting which they are profoundly ignorant. The least informed practical fisherman or salesman of good sense is generally superior, in point of sound knowledge derived from positive observation, to these half-informed persons who see every thing, in fact, through a veil of prejudice.

In the month of August 1835, during a short residence in Glasgow, we observed that fresh herrings of very superior quality were exposed to sale and brought readily a penny each. The average size of these herrings was 9½ or 10 inches, and their organs of generation were scarcely, if at all, upon the increase. The stomachs of most of them were quite full of a rich orange-coloured granular substance; we at first thought this substance entirely composed of the ova of testaceous marine animals, but subsequent careful examination con-

* Muller says, that marine microscopic shell-fish (Entomostraca) abound so much in some Norwegian Bays, as to tinge the wsters of a purple colour.

vinced us that they were entomostraca of extreme delicacy. These herrings were brought to Glasgow by the steamers up the Clyde from Lochfine, and particularly the Gareloch. A curious kind of imposture was attempted on the Glasgow folk by the introduction of herring taken in the Firth of Forth, and brought up the Union Canal; they were at least 1½ inch larger, but of so inferior a quality as to be instantly detected.

We feel confident from intuitive evidence (independently altogether of the most direct analogy) that marine microscopic entomostraca did also form the food of vast numbers of those fishes now fossilized in the limestone and other formations; indeed microscopic entomostraca seem to have so abounded in former ages previous to the destruction of the then existing species of fishes, that I have little doubt of their forming a great proportion of the food of many of these animals. Then was the remark strictly true, that all nature teemed with life.

In March 1835 we read a paper to the Royal Society of Edinburgh on the food of the char, (*Salmo Alpinus*, Linné), and produced specimens, shewing in the most conclusive manner, that the proper food of these fishes was microscopic entomostraca, and that if, by any accident, the nature of the lake in which char are found be changed, as by draining, &c., so as to destroy the entomostraca, the char dies out.

In conclusion, we are desirous that some public body interested in the prosperity of the fisheries would cause a minute and searching inquiry, with a view to the deciding how far our opinions on these great questions are correct or otherwise; we have no farther object in this than a wish to extend the boundaries of natural science, and to prevent, if possible, whatever merit may be due to our discoveries and labours from being appropriated by others.

The following brief summary may serve to render our views clearer to those unaccustomed to scientific research.

1*st*, That any fish so large as the Vendace of Lochmaben or the Herring, should be supported exclusively, or even mainly, upon a species of shell-fish (*entomostraca* of naturalists) so small as to be invisible to the naked eye, was a fact never hinted at previous to our discovery of the natural food of these fishes.

2dly, The same minute animals seem to have served as food to vast numbers of those fishes whose remains now abound in a fossilized state in the limestone formations.

3dly, The Herring, when disordered by the spawning condition, as well before as after the deposition of the milt and roe, will, *like all other animals, including even the human race*, take to other food than what is natural to them; is is then that occasionally its own fry, or that of other fishes, or very small fishes, may be found in *a few* of their stomachs; at these times, and under these conditions, the herring is quite insipid, and unfit to serve as food to man.

4thly, The remarks just made in No. 3, apply equally to the Salmon, whose *natural feeding ground* is in the *sea* only: there he obtains that remarkably rich food, the ova or eggs of the *asteria*, and no doubt of many crustaceous animals, and even perhaps of fishes, but chiefly, so far as our researches go, the *sea food* of the real salomon is the eggs of the asteria and of the crustacea. When he enters rivers in a spawning condition, he deteriorates constantly, takes little food (if any), and becomes wholly useless to man as an article of diet. His habits in this state are like those of other cold-blooded animals; and after spawning, he takes to whatever food he finds within his reach.

5th, There are many both herrings and salmon which are barren at least for a season. These do not deteriorate nearly so fast as those which become foul by spawning. This accords with well known physiological laws. The Journals of Observations kept by us do not yet afford data to determine the average numbers of those barren fish, but we believe it to be considerable.

6th, The petition lately submitted by the West Country Herring Fishers against the Caithness or East Country ones, viz. that they the Northern and Eastern Fishers were destroying the fisheries of both coasts, by catching the herrings upon their spawning ground, contains both truths and errors: truths in respect to certain facts, but deep errors in regard to the conclusions drawn from these facts. We propose returning to the subject-matter of this remarkable "petition" shortly.

EDINBURGH, 22*d July* 1837.

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