An experimental investigation into the functions of the eighth pair of nerves, or the glosso-pharyngeal, pneumogastric and spinal accessory / by John Reid.

AN

EXPERIMENTAL INVESTIGATION

INTO THE

FUNCTIONS OF THE EIGHTH PAIR OF NERVES,

OR THE

GLOSSO-PHARYNGEAL, PNEUMOGASTRIC, AND SPINAL ACCESSORY.

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In a former Number of this Journal, (No. 134, January 1838, p. 109,) I detailed some experiments upon the three distinct nerves, which in this country are generally denominated the eighth pair, and also stated the conclusions which I believed may be legitimately deduced from them. I was obliged, for want of the requisite data, to defer the consideration of several important points connected with the functions of these nerves, and I have since that time been endeavouring to accumulate sufficient facts to supply some of those deficiencies. And as my remarks upon the functions of the pulmonary branches of the *vagus* were very cursory and incomplete, and as I did not even enter upon the consideration of the functions of the gastric branches of this nerve, I

* A short epitome of this paper was read at the last meeting of the British Scientific Association.

intend, in the present communication, to confine myself chiefly to their examination. Before proceeding, however, with what may be considered as forming the principal object of this communication, I am anxious to make a few additions to and corrections of some of the statements contained in my former paper, which, in the order of arrangement, precede the remarks which I have there made upon the pulmonary branches of the *vagus*.

Glosso-Pharyngeal Nerve.

At p. 123–4 of my former communication, I state, while making some remarks upon the reflex movements of the muscles of the throat and lower part of the face, observed on irritating the trunk of the glosso-pharyngeal, that I had "endeavoured in several of the experiments, by gently pricking, pulling, and pinching the nerve, to produce the usual muscular movements of deglutition," or "of those excited by disagreeable sensations in the fauces and pharynx, but without effect." In an experiment which I performed last summer, a distinct effort of deglutition was made each time the trunk of the glosso-pharyngeal was irritated; and that under circumstances which, notwithstanding my numerous previous failures, induce me to believe that this was not an accidental coincidence, but actually stood in the relation of cause and effect. I shall briefly relate the facts of the experiments as they were witnessed by myself, so that every one may be able to judge how far I am justified in making this inference.

Exp. I.—The glosso-pharyngeal was exposed in a middle-sized terrier. The trunk of the nerve was pinched three times with the forceps, at intervals of two minutes, and each time a distinct movement of deglutition immediately followed. No such movement was observed when the nerve was not irritated. The animal was now deprived of volition by a dose of prussic acid, and though the respiration went on for a short time, no effect followed the pricking of the glosso-pharyngeal, and the irritation even of the trunk of the *par vagum* appeared to have little effect in exciting the respiratory movements.

This last fact ought not, we conceive, to be considered as at variance with the first, for the prussic acid may, by its action upon the *medulla oblongata*, have rendered it considerably more obtuse to impressions conveyed by the glosso-pharyngeal, and the trifling effect which followed excitation of the *par vagum* may favour this view. Besides, it must be remembered, in judging of the functions of the glosso-pharyngeal as an exciter of deglutition, that impressions applied to the extremities of nerves generally act more powerfully than when applied to their trunks.*

* Volkmann has, from experiments upon the glosso-pharyngeal in the frog, ar-

Pneumogastric Nerves.

I have again had ample opportunities of confirming the statement made in my former communication,—drawn from experiments upon dogs, rabbits, cats, and calves,—that the pinching, cutting, and even the stretching of the *vagi* nerves, when exposed in the neck, are, in by far the greater majority of cases, attended by indications of severe suffering. In opposition to the opinion expressed by Dr M. Hall and Mr Broughton, that the *nervus vagus* is not a nerve of sensation, I adduced the authority of Haller, Brunn, Dumas, and Dupuy. If additional evidence be thought necessary, I may also add to those the names of Molinelli, Mayo, Magendie, and Brachet. In the 1st and 5th experiments, upon the *vagus*, related by Molinelli,* it is expressly mentioned that the animals (dogs) gave indications of suffering, in tying these nerves with a ligature. Mr Mayo says, that "asses, cats, and dogs, almost invariably express great pain when this nerve, yet entire, is pinched with the forceps, and after its division equal suffering appears to result from pinching the part connected with the brain."† Magendie, in pointing out to his pupils an experiment where the nerve was stretched and cut without exciting pain, remarked, "In certain cases, on the contrary, the *nervus vagus* appears to possess the most exquisite sensibility; for it is scarcely touched without exciting immediately cries and convulsive motions."‡ Brachet in one experiment irritated the upper end of the cut *vagus*, with the view of subjecting the animal to suffering, and with success.§ I attempted to give an explanation of the source of fallacy which had misled Dr M. Hall and Mr Broughton, in their very limited number of experiments; but I am now convinced that there is another circumstance which is more likely to lead to such errors than the one I mentioned, and that is the very different degrees of sensibility possessed by different animals even of the same species. I have experimented on dogs which have endured the incisions necessary to expose the sheath of the carotid artery without any apparent uneasiness, and they remained quiescent though the *vagus* was violently stretched, pinched, and cut. Magendie, in explaining an experiment upon the *vagi* made before his pupils, adds, "The degree of sensibility in the pneumogastric nerve is variable; the division of this nerve is sometimes followed by acute pain; and sometimes, on the contrary, the animal seems scarce-

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rived at the conclusion which we have from experiments upon the dog,—that this is not a motor nerve.—Vide British and Foreign Medical Review, Vol. vii. p. 237, January 1839.

* De ligatis sectisque nervis octavi paris. In Comment. Bonon. Tom. iii. p. 280, 1755.

† Anatomical and Physiological Commentaries, No. ii. p. 15.

‡ Leçons sur Les Phénomènes Physique de la Vie, Tom. i. p. 208, 1836.

§ Recherches Experimentales sur les Functions du Système Nerveux Ganglionaire, Chap. i. Exper. 25.

ly conscious of the operation."* Such facts ought to make us hesitate before arriving at negative conclusions upon the sensibility of nerves, and forcibly point out a serious error to which limited observations are liable.

In confirmation of an observation made by Dr M. Hall and Mr Broughton, I formerly stated (p. 131,) that I had frequently repeated the experiment of compressing the *vagi* nerves in the neck, and that in some cases powerful respiratory movements were produced. As some additional confirmation of this observation, I may mention, that I remarked distinct respiratory movements apparently excited in three animals by compressing the *vagus* with the forceps, after they had been deprived of volition by a dose of prussic acid. This experiment, however, fails much more frequently than it succeeds.

In a note at page 134, I stated, that it is perhaps not quite correct to say, that the pharyngeal branches of the *par vagum* furnish all the motor nervous filaments of the pharynx and soft palate, and the more especially, as Palletta and Mayo had described, in the human species, a twig passing from the third branch of the fifth to the *circumflexus palati* muscle. With the view of enabling us to decide whether or not the branches of the fifth pair assist in moving the muscles of the soft palate, the following experiment was performed.

Exp. II.—Three dogs were deprived of sensation and volition by small doses of prussic acid, and the skull-caps were sawn off as expeditiously as possible, the root of the fifth pair exposed on the cerebral side of the Casserian ganglion, and irritated by a powerful galvanic battery. Before, however, the galvanic wires were applied to the nerve, the cheeks and the greater part of the temporal and *masseter* muscles were rapidly divided, and the soft palate and *isthmus* of the *fauces* fully exposed. On irritating the nerve no movement of the muscles of the soft palate and *isthmus* of the *fauces* could be detected, though the *elevator* muscular fibres which remained attached to the lower jaw, acted so powerfully, that it required a strong effort to prevent its closure. After this had been repeated for a short time, the pharyngeal branch of the *nervus vagus* was exposed and irritated in two of these animals, both by the forceps and by galvanism, and distinct movements of the soft palate followed. In these experiments, we more than once supposed that the parts about the *isthmus* of the *fauces* moved on irritating the fifth; but were afterwards satisfied that this was mechanical, and dependent upon the convulsive movements of the muscles of the lower jaw; for when this bone

* Oper. cit. Tom. ii. p. 234, 1837.

was kept nearly fixed during the irritation of the nerve, the movements did not recur.

We believe, then, that in the function of deglutition the impressions are conveyed to the *medulla oblongata* along the branches of the glosso-pharyngeal; along the branches of the fifth pair distributed upon the *fauces;* and probably along those branches of the superior-laryngeal distributed upon the pharynx. The motive influence transmitted outwards from the *medulla oblongata* passes, we believe, along the pharyngeal branches of the *vagus;* along the branches of the hypoglossal, distributed to the muscles of the tongue, the thyro-hyoid, sterno-hyoid, and sterno-thyroid muscles; along the motor filaments of the recurrents ramifying upon the larynx; along some of the branches of the fifth supplying the elevator muscles of the lower jaw; along the branches of the *portio dura* ramifying upon the digastric and stylo-hyoid muscles and muscles of the lower part of the face; and probably along some of the branches of the cervical plexus, which unite themselves to the *descendens noni.** An examination of the various muscular actions engaged in deglutition will convince us that the muscles moved by the motor nerves we have enumerated, are employed in the performance of this function.

Laryngeal Branches of Vagus.—All the recent observations which we have had occasion to make on these nerves while performing other experiments, have only served to strengthen the conclusions drawn from our previous observations. At page 149, we stated, in making a few remarks upon the pathology of *laryngismus stridulus*, that "from the experiments we have detailed it is, however, apparent, that severe dyspnœa amounting to suffocation, may arise both from irritation and compression of the inferior laryngeal nerves, or of the trunks of the pneumogastrics." Without pretending to decide whether the disease we have referred to, can in many cases, or only in a few, be explained on Dr Ley's theory, I would merely remark, that in some experiments to which I shall again have occasion to refer, I have witnessed, after the division of the *vagi* in the middle of the neck and the consequent arrestment of the movements of the muscles of the arytenoid cartilages, sudden and violent attacks of dyspnœa, which generally went off in the course of a very few minutes, when they did not terminate in suffocation,—phenomena which have been considered by some as affording an insurmountable objection to the theory of Dr Ley, as it supposes the exciting cause of the disease to be permanent and not occasional. In these animals the respirations

* We do not wish dogmatically to maintain, that the pharyngeal branches of the *vagus* may not include some sensitive filaments; but we believe, on the grounds stated in the former communication, that they are almost, if not entirely, motor.

were performed with ease after section of both *vagi*, as long as they remained at rest, or made moderate exertion; while, as soon as they began to struggle,—in other words, when the inspiratory movements were performed with greater force,—symptoms of suffocation presented themselves, and these in some cases shortly subsided, occasionally rather rapidly, on the animals refraining from the violent exertion. We have also, in some of those animals, designedly brought on a paroxysm of dyspnœa, by causing them to struggle.

I have lately had frequent occasion to experiment upon the inferior laryngeal nerves, and have observed that very few dogs give any indications of suffering when these nerves are irritated or cut. We are not, however, to conclude that they are entirely motor, for it must be remembered that many dogs remain perfectly quiescent, if previously well secured, during the incisions necessary to expose the nerve. Besides, the anatomical distribution of some of the filaments of the nerve, and the fact, that animals do occasionally give indications of suffering on irritating it, are sufficient to prove that it does contain some sensitive filaments.

Œsophageal Branches of Vagus.—Subsequent experiments on rabbits have furnished me with results exactly similar to those I formerly detailed under this head. I have satisfied myself, however, that substances seem to pass pretty freely along the *œsophagus* in most dogs after section of the *vagi*. Upon what this difference depends I have not yet been able to form any probable opinion.

Cardiac Branches of Par Vagum.—Brachet relates an experiment, in which he states, that he tortured an animal (dog) in various ways, after having previously divided the *vagi*; and although it manifested by its struggles and cries the pain which it suffered, yet the heart's action was not quickened, "le cœur est resté impassible, ses mouvemens n'ont pas varié." * This statement is so much at variance with all we know of the physiology of the heart, that we might at once declare that some oversight must have been committed. The movements of the heart are distinctly referrible to the same laws which regulate muscular contractility in other parts of the body, only somewhat modified to adapt it to the performance of its appropriate functions. Like all the other muscles of the body, it is endowed with the property of irritability, which enables it to contract upon the application of a stimulant; and whether we embrace the opinion, that muscular irritability is dependent upon nervous influence, or adopt the much more probable doctrine, that it is a property of the muscular fibre itself, it is sufficiently proved,—and this is ad-

* Opus. cit. Chapitre premier, Exper. 25.

mitted by M. Brachet himself,—that the irritability of the heart is not derived from the *vagus*. The ordinary and habitual excitant to the irritability of the heart is the blood which is constantly flowing into its cavities. When the blood is forced on more rapidly towards the heart, as in exercise, its contractions become proportionally more frequent; and when the current moves on more slowly, as in a state of rest, its frequency becomes proportionally diminished. If the contractions of the heart were not dependent upon the blood, and their number not regulated by the quantity flowing into its cavities, very serious and inevitably fatal disturbances of the heart's action would soon take place. As statements such as those of M. Brachet are, however, more effectually met by facts than by arguments, I proceeded to put them to the test of experiment. These experiments were seven in number, and six of them were made in the following manner. The *vagi* and sympathetics,—and in some cases the recurrents also—were cut in the middle of the neck, and a portion removed. At a longer or shorter period after the operation the pulsations of the heart were reckoned when the animal was lying or standing on the ground, and after it had been caressed for some time to calm its fears. It was then lifted up on the table on which it had been previously tied and operated upon, and, after having been spoken to harshly, the pulsations were again reckoned. After being again caressed for some time the pulsations were counted a third time; and when replaced upon the ground they were reckoned a fourth time. The following results were obtained:—In the first dog, the pulsations of the heart were about 140 before the commencement of the experiment. The animal at this time was apparently somewhat alarmed. Four hours and-a-half after division of the nerves, the pulsations of the heart were about 170 when the animal was standing on the ground, and rose to 200 at least when placed upon the table. After it was replaced on the ground they had again fallen to about 170. After nineteen hours the pulsations were 160 on the ground; they rose again to about 200 when placed on the table,—again fell to about 160 when still on the table, and were not increased by being replaced on the ground. In the second dog the pulsations were 156 on the floor, and about 190 on the table; and in the third dog they rose 20 beats in the minute when placed on the table. In both of these two last experiments the pulsations of the heart soon subsided to their former frequency, and were not increased by replacing the animals on the ground. In the fourth dog the pulsations, twenty-four hours after division of the nerves, were 140 on the floor, and instantly rose to 180 on the table. After waiting until they had again fallen to their former frequency, they were not increased by replacing it on the ground.

In the sixth dog the pulsations were 140, the third day after the section of the nerves, when the animal was on the floor, and were raised to 160 by placing it on the table. In these experiments it was particularly observed, that the animals made no struggles in carrying them to and from the table, and, consequently, the increased excitation of the heart must have arisen from the mental emotion of terror. In the seventh dog this was conjoined with violent struggles. The pulsations eight hours and-a-half after the operation were 130; when placed on the table, and made to struggle, the pulsations, as far as could be made out, were about 220; when he had been subjected to pain, and had struggled more violently, they became so frequent that they could not be accurately reckoned, but were at least 260 in the minute. A large tube had been previously introduced into the *trachea* in this last animal. These experiments are, we conceive, sufficient to prove that, after section of the *vagi*, the pulsations of the heart may not only be quickened by muscular exertion, but also by mental emotions. Though in all probability the *vagi* are the usual channels through which mental emotions affect the heart, yet it appears from these experiments that this may also take place through the medium of the ganglionic system of nerves.

Pulmonary Branches of the Vagus.—At p. 158 I gave the results of several experiments, from which, in opposition to the observations of Magendie, Wilson Philip, and Swan, I concluded "that lesion of one of the pneumogastrics does not necessarily or even generally induce disease of the lung of that side." Since that time I have carefully examined the lungs of two dogs and a cat, killed some time after a portion of one *vagus* had been removed. One dog lived two months, the other nine days, and the cat three weeks. No morbid change could be detected in the lungs. I have now removed a portion of one *vagus* in seventeen animals, which have been allowed to live a longer or shorter period,—from twenty-four hours to six months,—and in none of these could I detect any morbid change in the lungs which I could attribute to the section of the nerve. In an experiment made by Magendie before his pupils, the results were completely at variance with his former expressed opinions. The right lung of a dog, from which a portion of the *vagus* of that side had been removed six months before, was on examination found to be perfectly healthy.* The circumstance of the lung remaining healthy after section of the *vagus* of the same side might, I formerly supposed, be accounted for by the anastomoses between the pulmonary plexuses of the two sides. I now believe, however, that there is another cause in operation which I shall have to discuss at some length when we

* Leçons sur les Phénomènes Physiques de la Vie, Tome i p. 203–4.

come to examine the morbid changes in the lungs induced by lesion of both *vagi*.

Effects of Lesion of the par Vagum upon the Respiratory Muscular Movements.—It has now been fully ascertained by numerous experimenters, more especially by those who have investigated the functions of the *vagus* since the time of Legallois, that an animal will continue to breathe for a longer or shorter time, after this nerve has been cut on both sides of the neck, if care be taken to secure the free ingress and egress of air into the lungs. In my former communication I examined, under the section *Laryngeal Nerves*, the effects of lesion of the *vagi* in the middle of the neck upon the respiratory muscular movements of the *larynx*, and it will be here unnecessary to resume the discussion of that point. I there adduced various facts to show that when the *vagi* are injured above the origin of the inferior laryngeal or recurrents, the movements of all the muscles which enlarge or diminish the cavity of the *larynx* are arrested, and the superior aperture of the *larynx* can no longer be dilated during inspiration. If the *larynx* be large, and the animal refrain from any violent effort, an adequate quantity of air may still find its way through the diminished aperture of the *larynx*, and the respirations are at first performed with ease. If, on the other hand, the *larynx* be small, the quantity of air admitted through the interior of the *larynx* may be insufficient to carry on the respiratory process, and the animal may labour under dyspnœa from the moment the nerves are divided until its death. In young animals, in which the *larynx* is naturally small, and even not unfrequently in those full-grown, especially if the respiratory movements become forcible as in struggling,—or, in other words, when the capacity of the chest is suddenly and greatly enlarged,—the air rushes through the diminished aperture of the *larynx* in a narrower stream and with increased force, carrying the arytenoid cartilages mechanically inwards, producing the complete occlusion of the superior aperture of the *larynx*, and thus suffocating the animal.

As it becomes a matter of importance, in enabling us to arrive at some accurate conclusions upon the cause of the morbid changes induced in the lungs by lesion of the *vagi*, to ascertain the immediate effects of this operation upon the respiratory movements, I have of late attended particularly to this point: and have notes of thirty experiments in which the immediate effects of division of the *vagi* were watched upon dogs. In a considerable number of these, the recurrents were also divided. Seventeen of the experiments were performed chiefly for the purpose of investigating the morbid changes in the lungs. In the others I had different objects in view, and some of the animals were therefore allowed to live only a short time after the operation, but sufficient-

ly long to ascertain its immediate effects upon the respiration. I need only state once for all, that in every one of these experiments the nerves were not simply divided, but a considerable portion was also removed. Many of the animals operated on were full-grown, so that no opening was made into the *trachea*. In others which were not full-grown a large tube was introduced into the *trachea* previous to the division of the nerves; and in a few in which (although they were full-grown) violent paroxysms of dyspnœa occurred after the nerves had been divided, an opening was immediately made into the *trachea* and a tube introduced. Such experiments are liable to several sources of fallacy, which must be carefully avoided, if we wish to arrive at accurate results. When an opening is made into the *trachea* under such circumstances, and no tube introduced, the incision in the skin and muscles must be very free, to secure a direct communication between the opening in the *trachea* and the external air, as the animal generally droops his head and the *trachea* is moveable. If a tube be introduced into the opening, care must be taken that it be sufficiently large, and not clogged up by blood or mucus, and that its shape be such as to prevent the orifice being obstructed in the bent position of the neck. Reflecting upon these inconveniences and sources of error, it occurred to us that by selecting aged animals, and after first ascertaining what effect the section of the recurrents had upon the supply of atmospheric air in the particular animal operated on, we might be enabled to judge more accurately of the extent of the influence of the *vagi* upon the other respiratory muscular movements. We believed that by selecting those animals in which the division of the recurrents produces no impediment to the respiration, the subsequent division of the *vagi* would produce no additional diminution of the supply of air through the *larynx*, and would consequently determine in a satisfactory manner the immediate effects of the lesion of the *vagi* upon the respiratory movements under circumstances where the egress and ingress of air to the lungs were quite free. Another difficulty, however, again presented itself, to which I have already alluded. In some of those animals the violent respiratory movements produced by the pain of dividing the *vagi*, or some powerful effort to liberate themselves before they were unbound, produced violent dyspnœa, which in some cases soon went off when the animal became more quiescent; but in others necessitated the opening of the *trachea* and the introduction of a tube. The idea of dividing the right *vagus* below the origin of the inferior laryngeals now occurred to us, but this being a severe and difficult operation was after one trial abandoned. It appeared then that the best plan to follow was to select full-grown animals; to avoid as much as possible any thing likely to produce the struggles of

the animal, after, or during the division of the *vagi;* and if dyspnœa, threatening suffocation, should occur, quickly to introduce a pretty long and large bent tube into the *trachea*, and efficiently secure it. These details may appear uninteresting, and to many may seem unprofitable; but it is obvious that those who may wish to test the accuracy of the facts which I am about to mention, must repeat the experiments in a similar manner, unless it should appear that I have overlooked the agency of some extraneous circumstance which might interfere with the results. It is not so much the frequency of false observations that we have to complain of in medical science, as the errors which arise from making these observations under dissimilar circumstances, and from drawing general conclusions from insufficient data.

Of the thirty animals operated on, I find from my notes that fourteen, or rather less than one-half, had the *trachea* opened. Of these fourteen, the *trachea* was opened, and a tube introduced in nine, previous to the cutting of the *vagi*, as the age of the animal or the dread of the supervention of suffocation in some other subsequent experiment to which the section of the *vagi* was preparatory, made it prudent to proceed in this manner. In the remaining five, the *trachea* was opened subsequently to the commencement of the severe dyspnœa,—in four of these after section of the *vagi*, and in one after section of the recurrents, and before the *vagi* had been exposed. Of these thirty animals, I find that the respiration was easy immediately or soon after the division of the *vagi*, and this continued for a longer or shorter time in twenty-seven; while in the remaining three it is stated to have been difficult and approaching to dyspnœa. The *trachea* had been opened in these three animals, and in one of them, as no sufficient tracheotomy tube was at hand, none was introduced. In the other two, tubes were introduced; but I have every reason to believe that they answered the purpose very imperfectly. Besides, it ought also to be mentioned, that these three experiments were performed before I had matured my plans for providing for the passage of a sufficient supply of air through the *trachea*. I may also state here, that I could adduce several experiments, performed last year, which would seem to show that dyspnœa may occur immediately after section of the *vagi*, even where precautions have been used against it; and which at the time almost induced me to adopt the opinion, that the synchronous movements of the muscular fibres of the bronchial tubes were essentially necessary for healthy respiration. In reflecting upon the somewhat imperfect manner in which these experiments were performed, and finding that they are not confirmed by the twenty-seven experiments where all apparent sources of fallacy were avoided, I have arrived at the conclusion, that when a sufficient quantity of air traverses

the *trachea* after section of both *vagi* and recurrents, the respirations are at first performed with ease. Very important effects, however, upon the respiratory movements manifest themselves instantly after section of the *vagi*. The frequency of the respiratory movements suffers a great diminution, and are at the same time performed more slowly, and generally even from the first in a somewhat heaving manner. I have notes, more or less complete, of the frequency of the respirations taken at different periods after section of the *vagi* in twenty dogs. We found it frequently difficult, and in some cases impossible to reckon the number of respirations accurately in some of these animals previous to the commencement of the experiment, owing to their restlessness, and the gentle manner in which their respiratory muscular movements were performed; and this will explain some deficiencies in the following tables. We were also sometimes not a little perplexed by the varying frequency in the number of the respiratory movements in some of those animals, even within the space of a few minutes, and when no very apparent change had occurred in the circumstances in which they were placed. After the *vagi* had been cut, we experienced much less difficulty in reckoning the number of the respirations, as they were performed in a more slow and heaving manner. In the following table, I shall not arrange the experiments in the order in which they were performed, but classify them in the way which appears best adapted to show the results. The first table is for the purpose of pointing out the difference between the number of respirations before and at various periods after section of the *vagi*, and in the cases which I shall afterwards mention of the recurrents also. I may state that the observations were made in this manner. The respirations were reckoned several times immediately before the operation was performed, —with the exception of Experiment XI. where the animal was repeatedly examined for two days previous,—and then again immediately after the division of the *vagi*, and before the incisions were stitched up. The T in the tables indicates that a tube was introduced into the *trachea*. I have marked at the end of some of the experiments the time the animal lived after division of the nerves, as we shall afterwards have occasion to refer to these.

TABLE I.

Expert.	Before operation.	Immediately after.			
3, T.	15	6–7			
4,	14–16	5–6			
			2 min.	5 min.	15 min.
5,	18–20	7–8	8	7–8	8–9
6,	16–17	6–7			

Expert.	Before operation.	Immediately after.	10 min.	15 do.	25 do.
7,	16–18	8	12	7	7–8
8, T.	24	8			

	10 m.	4 hours.	19 do.
9, T.	24–28	14	12–14 dying.

	2 hours.	8 do.	24 do.	32 do.	48 do.	4 days.	5 do.	6 do.	7 do.	8 do.
10,	16	8	9	9	9	7	8	7	7	7	7	died.

	2 hours.	24 do.	3 days.	4 do.	5 do.	6 do.	7 do.	8 do.
11,	10–16	8	6–7	7	6	6	5	5	4–5	5	dying.

	2 hours.	
12,	20–26	7–8 8–13 died two days after.

	5 h.	6½ do.	11 do.	24 d.	2 do.	3 do.	4 do.	5 do.	6 do.	8 do.	9 do.	12 do.
13,	16	8	7	7	7	8	8–12	7–8	7	7	7	7	7	7 killed.

Table II.

Expert.	Before Operation.	4½ hours After.	8 hours.	10½ h.	24 h.
14, T.	16	7	7	7	died.

	½ hour.	19 h.	28 do.	48 do.	3 d.	5 do.	6 do.	7 do.	8 do.	9 do.	12 do.
15,	16	14	9	12–20	9	10	12	12	10	16	7	died.

	10 m.	2 h.	5 do.	9 do.	24 do.	32 do.	48 do.	52 do.
16,	12	8	8	7	7	10	9	10	dying.

	Soon after.	4 h.	8 do.	23 do.	28 do.	31 do.	34 do.
17, T.	16	20	8	6	8	8	8	died.

	8 h.	23 h.
18, T.	16–18	6	8–10

Table III.

	¾ hour.	4 do.	19 do.	21 do.	24 do.
19,	14	7	7	11	died.

	Soon after.	24 h.	48 do.	3½ d.
20,	7	7	6	died.

	7 h.	15 do.	23 do.	54 do.
21,	12	8	12	died.

	5 h.	24 do.	24 do.	69 do.
22,	8	10–12	10–12	6–7	dying.

In examining the above tables it will be at once apparent in comparing the number of respirations immediately before and immediately after section of the *vagi*, that a very considerable diminution of the number of the respiratory muscular movements followed the division of these nerves, with the exception of Experiments XV. and XVII. It is stated in my notes, and the vary-

ing number of the respirations sufficiently indicate the fact, that the respiration was very easily excited in the animal the subject of Experiment XV. And as this was not remarked until the nerves had been divided, no safe conclusions can be drawn from such an experiment, especially as the respirations were not reckoned sufficiently often before the commencement of the experiment to ascertain their average frequency. The result of Experiment XVII. is so completely at variance with the others, that the circumstances connected with its performance require some explanation. This animal was one of those in which it was necessary to open the *trachea*, and introduce a tube after section of the *vagi*. It had suffered much from dyspnœa before the tube could be introduced, and the respirations were reckoned soon after the insertion of the tube, and before it had recovered from the effects of the dyspnœa. I may also state, that this was the first experiment which I made to ascertain the effects of the division of the *vagi* upon the frequency of the respiratory movements, otherwise I would have paid more attention to this circumstance, and would have again reckoned the respiration immediately after the animal had become calm. This experiment ought, perhaps, to have been rejected from the list, but I was anxious to give all the observations made on this point exactly as they are entered in my notes, so that every one may be able to judge of the soundness of the conclusions which we intend to deduce from them in a subsequent part of this paper. In judging of the effects of the division of the *vagi* upon the frequency of the respiratory movements, it is absolutely necessary to reckon them, for as these movements become more apparent, prolonged, and heaving, we may be readily deceived. From neglecting this, I fell, in my earlier experiments upon the *vagus*, into the error of supposing that the respiratory movements were increased in frequency, especially in rabbits. It is in this manner that we can account for the statement of Dr M. Hall, that after division of the *vagi* "the acts of respiration become much more frequent."* At a shorter or longer period after section of the *vagi*, the respirations become more heaving and prolonged, while the expirations continue to be comparatively short and rapid, and frequently attended by a sound caused by the sudden expulsion of the air.

In some cases the number of respirations was afterwards still farther reduced; while in others it varied at different periods; and in a few it became more frequent shortly before death. At a longer interval after section of the nerves, the inspirations generally become still more heaving and prolonged; the blood is less perfectly arterialized in the lungs, and the arteries circulate blood

* Memoirs on the Nervous System, p. 85, 1837.

gradually approaching to the venous character; the animal becomes more dull and stupid, the evolution of animal heat is diminished, and it dies asphyxiated.

We believe that it is sufficiently evident from these experiments, that the *vagi* are important nerves in transmitting those impressions to the *medulla oblongata* which excite the respiratory muscular movements; and they may appear to favour the opinion of Dr M. Hall, that the continuance of the respiration, after division of the *vagi*, is a voluntary and not an excited act.* In my former communication, (p. 161,) I stated that I had observed respiratory movements continue in animals after they had been deprived of all volition by a small dose of prussic acid. I have since made similar experiments upon animals deprived of volition by alcohol, and by blowing air along the carotids towards the brain. I am also satisfied that, in such experiments, the inferior laryngeal nerves may be also divided without affecting the results. As, however, it has been stated by Dr M. Hall, on the authority of Cruveilhier, (Lancet, February 17, 1838, p. 733,) that after the function of the cerebrum is destroyed, or, in other words, after the animal has been deprived of volition, the respiration ceases instantly on dividing the pneumogastrics near their origin, I thought it necessary to repeat this experiment.

Exp. XXIII.—The *vagi* were first exposed in the upper part of the neck in a puppy six days old. A considerable portion of the *cranium* was removed on both sides of the superior longitudinal sinus, and the hemispheres of the brain were completely destroyed down to the *corpora quadrigemina*. The *vagi* were then cut, and the *trachea* opened, without arresting the respiratory movements. The animal was evidently much exhausted from loss of blood, and the respirations were performed at long intervals even before the *vagi* were divided. After the section of the *vagi* the respiration went on for several minutes, during which the *cerebellum* was also broken up by the forceps.

Exp. XXIV.—The *vagi* were exposed and the hemispheres of the brain removed, as in the preceding experiment. The respirations continued after the division of the *vagi*. A few minutes after division of the nerves, the respirations were reckoned and found to be 4 in the minute. The recurrents were then divided, and the *cerebellum* broken up as cautiously as possible by the forceps. Seven minutes after the respirations had been first reckoned, they were between 3 and 4 in the minute; at the thirteenth minute they were still between 3 and 4. The spinal column was completely severed between the second and third cervical vertebræ fifteen minutes after the respiration had been first reckoned, and

* Oper. cit. p. 87.

the respiratory movements still continued at the rate of 1 in the minute. At the twentieth minute the head was entirely removed from the body, and some cold water was dashed over the face after it had lain quiescent for nearly two minutes, and the muscles of the face moved as in inspiration. In about two minutes more, a similar movement followed without the artificial application of any external excitant. Between the seventh and thirteenth minute after the respiration had been first reckoned, the animal repeatedly sucked the finger introduced into the mouth, and at the fifteenth minute it still did this though feebly.

These two experiments being positive ones are sufficient, independent of others which we could adduce, to prove that the respiratory movements are not immediately arrested after removal of the *cerebrum* and *cerebellum* and division of the *vagi*. We have seen that division of the *vagi* in an animal in which the *encephalon* was uninjured was immediately followed by a diminished frequency of respiration; and it becomes an interesting question to ascertain what is the effect of the lesion of those nerves in an animal deprived of volition by the previous removal of the brain, as such experiments will determine the influence of the *vagi* as exciters of respiration in a more precise manner than those performed upon animals in which the respiratory movements may be readily modified by volition.

Exp. XXV.—The pneumogastrics were exposed and surrounded by a loose ligature in a kitten one day old. The hemispheres of the brain were removed, and attempts were made to break down the *cerebellum* without removing the osseous *tentorium*. Before the commencement of the operation the respirations were nearly 100 in the minute; after removing the hemispheres of the brain they were about 40. On cutting the *vagi* they instantly fell to between 3 and 4 in the minute, and the animal continued to breathe in this manner for an hour, when it was left. During this time it sucked the finger introduced into the mouth.

Another experiment of the same kind was performed on a kitten of the same age, whose respirations were also about 100 in the minute. This animal appeared to be more exhausted from hemorrhage, and only breathed twelve times in a minute after the removal of the hemispheres. After the section of the *vagi* the respirations were only between 2 and 3 in the minute; and it continued to breathe in this manner for a quarter of an hour, when it was left. On examining these two animals after death, it was ascertained that the whole of the *cerebrum* down to the *corpora quadrigemina* had been removed, and that the *cerebellum* was only partially injured. I find it stated in my notes of these two experiments, that, if the animals had been left immediately after section of the *vagi*, we might have gone away with the impression that

they had ceased to breathe, as the respiration appeared quite arrested for about a minute.

Exp. XXVI.—The *vagi* and recurrents were exposed, the hemispheres of the brain removed, and the *cerebellum* injured, in a kitten five days old, as in the previous experiments. The respirations before the commencement of the experiment were 120; after removal of the hemispheres of the brain 40; after section of the *vagi* and recurrents they instantly fell to 4 in the minute; two minutes after this they were between 3 and 4: five minutes after they were 4; and seven minutes after they were still 4. It was now pithed at the lower part of the neck, between the sixth and seventh cervical vertebræ, and the respirations fell to one in the minute. It continued to breathe in this manner for seventeen minutes, when it was left.

A similar experiment was performed on another kitten of the same age. The respirations were few and irregular after the removal of the cerebral hemispheres. The *vagi* were then cut, and it continued to breathe about once in the minute for half an hour, when it was left. On dissecting these two animals after death, it was found that a considerable part of the *cerebellum* had escaped injury in the former; while in the latter, nothing except the *medulla oblongata* had been left within the cranium.

While these experiments illustrate the great importance of the pneumogastrics as exciters of respiration, they also prove that the impressions made on the filaments of the pneumogastrics distributed in the air-cells of the lungs are not the sole excitants of respiration; and that there are other nerves which can transmit impressions to the *medulla oblongata* capable of exciting the involuntary respiratory movements.

I formerly suggested (p. 162) that the continuance of the respiration after section of the *vagi* might depend upon the transmission of impressions along the sympathetic branches distributed in the lungs, backwards to the spinal chord. Several facts related in the above experiments upon the effects of the division of the *vagi* after removal of the brain seem to prove that all the impressions conveyed to the *medulla oblongata* capable of exciting the involuntary muscular movements under such circumstances, are not transmitted through the sympathetic system. I need only refer to the gasping of the animal after decapitation—a fact formerly observed by Legallois,—and to the continuance of the respiration after the pithing of an animal in the cervical region, from which the brain and *cerebellum* have previously been removed, and the *vagi* divided high in the neck. I may also state, that I have seen the respiratory muscles twice called into simultaneous movement after all the thoracic viscera had been rapidly removed in an animal deprived of sensation and volition by a dose

of prussic acid. The facility with which the respiratory movements are excited by impressions made upon the sensiferous filaments of the fifth pair,—well illustrated by the effects of dashing cold water on the face,—will at once suggest this nerve as a likely channel through which these impressions may be conveyed. The effects of certain impressions upon the surface of the skin generally, and the details of two very interesting and important cases of the resuscitation of new-born children by the contact of the cool atmosphere with the surface of the body, given by Dr M. Hall, on the authority of Dr Henning * and by Dr Wagner, † point out the filaments of the spinal nerves distributed on the skin as also probably engaged in transmitting those impressions which excite respiratory movements. It would be difficult to devise experiments likely to afford such decisive results as would enable us to ascertain the relative share which these different nerves have in carrying on the respiration after section of the *vagi*. The results of experiments XXIII. and XXV., where the respiratory movements suffered another very apparent diminution on pithing the animal in the cervical region, would seem to show that the nerves distributed about the face and upper part of the neck do not certainly furnish the only channels through which the impressions which excite respiration are conveyed to the *medulla oblongata* after division of the *vagi*. We must, however, be cautious in drawing conclusions from negative experiments so few in number. With the view of throwing further light upon this question, the following experiment, which is similar to one detailed by Mr Cruickshank, ‡ was performed.

Exp. XXVII.—The *vagi* and sympathetics were divided low in the neck in a powerful bull-bitch at 8 o'clock A. M. A large tube was introduced into the trachea before the division of the *vagi*. The respirations before the operation were from 16 to 18 in a minute. After the operation it breathed freely through the tube and walked about. At 4 P. M. the respirations were 6 in the minute, when it was lying at rest. Next morning at 7 o'clock the respirations were reckoned repeatedly, and were found to be from 6 to 7 when lying, and from 8 to 10 when standing. It was now pithed at the lower part of the neck, and it continued to breathe by the diaphragm alone, and appeared uneasy during the short time it was allowed to live. The respirations were reckoned between two or three minutes after the spinal chord had been cut across, and were 6 in the minute. For twenty minutes after this they were reckoned at short intervals, and were still 6 in the minute. Both femoral arteries were now opened, and the blood,

* Memoirs on the Nervous System, p. 88.
† British and Foreign Medical Review, Vol. v. p. 582.
‡ Medical Facts and Observations, Vol. vii. Exper. vii. p. 147.

though of arterial hue, was darker than usual, and flowed out with diminished impetus. No satisfactory conclusions can be drawn from this experiment, as the frequency of the respiration was probably influenced by volition and other causes. As far as a single experiment of this kind can go, however, it appears to show that the frequency of the respiratory movements was somewhat diminished by the pithing of the animal, for I am convinced that the respirations would have been more frequent had the animal been as much excited before it was pithed as it appeared to be afterwards. In Mr Cruickshank's experiment the animal died sixteen hours after the operation. The number of respirations varied from 5 to 15 in a minute. Their frequency before the operation is not stated.

The diminution of the frequency of the respiratory movements consequent upon division of the *vagi* has been observed by other experimenters. A. G. F. Emmert concluded, but apparently more upon theoretical grounds, than from any direct observations made in the two experiments which he had at that time performed on rabbits, that after lesion of the *vagi* the respirations became less frequent and prolonged. * Mayer reckoned the number of respirations, both before and at various periods after section of the nerves, in five experiments, and gives the results in the following table.

	Before operation.	After operation.
Ass,	17	8
Dog,	48	8
1st rabbit,	80	40
2d do.	100	48
3d do.	80	28†

The late Mr Broughton mentions, that in a horse in which the *vagi* were divided, " the respirations became slow, 12 in a minute."‡ In another horse, " the respirations fell to five in the minute."§ At what period after the operation this diminution in the frequency of the respirations was observed, and what was the number of the respirations previous to the experiment, we are not informed. Sir Astley Cooper has given the results of two experiments upon rabbits, which well illustrate the effects of section of the *vagi* upon the frequency of the respiratory movements. In one rabbit the respirations were 132 in the minute before the commencement of the experiment. One hour after division of the nerves they were 48; and after eleven hours and a-half they were only 30. In the second experiment the respirations were 135

* Archiv. für Physiologie, von Reil und Autenrieth. Neunter Band, 1809. S. 417.

† Tiedemann's Zeitschrift für Physiologie. Zweiter Band, 1826, S. 77.

‡ Quarterly Journal of Literature, Science, &c. Vol. x. p 205.

§ Oper. cit. p. 307.

before the division of the nerves. One hour after, they were 48; six hours after, they were 36; and after sixteen hours they were 28.* The experiments we have related prove that this diminished frequency of the respiration instantly follows the division of the nerves, and cannot depend upon any blunting of the sensation from the circulation of dark blood in the arteries of the brain; though this no doubt may, after a while, probably assist in diminishing them still farther. We shall have occasion to examine the probable influence of the diminished frequency of the respirations in producing the morbid changes which are observed in the lungs, when we come to discuss that part of our inquiry.

Are the Pulmonary Branches of the *Vagus* both Motor and Sensitive Nerves? We have made various attempts to obtain some satisfactory evidence that the muscular fibres of the bronchial tubes are moved through the influence of the *vagi*, but we must confess that these as yet have all failed. I have frequently performed the following experiment upon animals immediately after death. An opening was made into the *trachea*, into which the broad end of a bent tube of the form of a blowpipe was inserted and there secured, while the narrow end was introduced below the surface of water in a shallow vessel. The nerves were then immediately exposed, and irritated both by the scalpel and by galvanism; and we anticipated, that, if the muscular fibres of the *bronchi* were thrown into contraction, a part of the air would be forced along the tube, and rise through the water in the form of bubbles. No such appearance, however, presented itself, though the muscular fibres of the œsophagus moved at each application of the excitant. We do not, however, consider this a very delicate method of testing this point, from some circumstances which we observed in performing these experiments. In one or two cases in which I believed I heard the natural respiratory murmur after section of the *vagi*, I could detect no change in the respiratory sound when the lower end of the cut nerve was pinched by the forceps during inspiration. The restlessness and tremblings of the animal, generally, however, interfere with the accuracy of such observations. We certainly do not consider these negative experiments as by any means sufficient to entitle us to conclude that some of the pulmonary branches of the *vagus* are not motor nerves—the more especially as the well-known symptoms of what is called spasmodic asthma, and certain phenomena observed by Laennec on applying the stethoscope over the chest,† render it exceedingly probable that muscular movements occur in the bronchial tubes; and if such movements be influenced by

* Guy's Hospital Reports, No. 3, September 1836, p. 409.

† Treatise on the Diseases of the Chest, &c. p. 418, 1829, Forbes' Translation.

nerves, it is much more likely that these should belong to the *vagus*, than to the sympathetic system. M. Brachet has related some experiments, which, if correct, prove in a most satisfactory manner that part of the pulmonary branches of the *vagus* are motor.* We have not repeated those experiments; but, from an examination of the details given by M. Brachet, we cannot help suspecting that some error must have been committed in their performance, which had been overlooked.

From the distribution of the *vagus* upon the bronchial tubes we might conclude *à priori*, that it must be concerned in conveying those impressions to the central organs of the nervous system which excite sensations. It does not, however, necessarily follow, that division of the *vagi* should annihilate all the sensations referred to the lungs. I formerly stated, that I was convinced, from frequently repeated experiments, that section of the *vagus* on both sides neither arrests the transmission of those impressions to the *medulla oblongata* which excite the involuntary respiratory movements; nor does it annihilate the sense of anxiety arising from the deficiency of fresh air in the lungs. I have since that time had frequent opportunities of verifying this statement, and I am confident that animals will evince great uneasiness—to judge from the struggles of the animal—after section of these nerves, when the access of air to the lungs is prevented. It is possible, however, that this *besoin de respirer* may be diminished by section of the *vagi*, though it certainly is not annihilated. In order to perform such experiments properly, it is necessary that the access of air to the lungs be fully and suddenly prevented. Such experiments as those performed by Brachet† and Mr Grainger, ‡ where the animal was placed under a bell-glass in a limited quantity of air, are liable to an obvious source of fallacy. When carbonic acid is accumulated slowly in the air inspired even in a healthy individual, a torpor gradually creeps over the nervous system, and the sense of anxiety at the chest is never very urgent, and frequently not sufficient to rouse a person from his sleep. The melancholy fate of many an unfortunate individual could be easily adduced in confirmation of this. I have seen rabbits confined under a bell-glass until their breathing became quick and heaving, without their evincing any marked uneasiness, and without making efforts to escape. The other method of experimenting followed by Brachet, viz. of cutting the *vagi* in a puppy three days old, and then placing the nose under water,§ is equally liable to error. From the various experiments we have made, we are led to believe that section of the *vagi* in an animal so

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* Opus cit. p. 299, Experiment 125–6. † Ibid. Experiment 37, p. 134.
‡ On the Spinal Chord. § Opus cit. Experiment 34.

young is followed by the instant occlusion of the superior aperture of the *larynx*, so that the struggles and uneasiness arising from the occlusion of the *larynx* may have ceased before the nose of the animal was placed under water. At all events, we found on repeating this experiment on kittens, that plunging the nose in water was a very useless proceeding, for violent struggles and evident uneasiness instantly followed the division of the *vagi*.

In our last communication, (p. 162,) we alluded to the experiments made by Brachet, from which he concludes that all the sensations occasioned by foreign bodies, &c. in the air-passages are dependent upon the integrity of the pneumogastrics. We stated that we had never yet been able to induce the severe paroxysms of coughing described by Brachet, by any mode of irritating the inner surface of the *trachea* which we have adopted. To show that others have also sometimes observed great insensibility of the mucous membrane of the *trachea*, even when the *vagi* were entire and uninjured, I have only to refer to the works of Haller.* He there relates several experiments upon different species of quadrupeds,—two cats, a she-goat, a rabbit, a lamb, a he-goat, and a sheep—in which the *trachea* was opened, and various irritating substances, such as oil of vitriol, butter of antimony, and fumes of sulphur, were introduced into the air-passages, without exciting cough. Some of the animals gave indications of suffering, and breathed forcibly. The sheep was supposed to be phthisical. Every practical surgeon is aware of the little uneasiness produced by the introduction of a canula into an opening in the *trachea*. The violent paroxysms of coughing consequent upon the introduction of foreign bodies into the *trachea* from the mouth seem principally dependent upon their passage through the *larynx*. I have suggested that Brachet in some of his experiments may have overlooked a source of fallacy which would very seriously interfere with the results,—and that is, the facility with which some of the irritants used might reach the interior of the *larynx*; although it is to be remembered that, in the experiments which he details to illustrate the annihilation of the sensibility of the inner surface of the *trachea* and bronchial tubes after division of the *vagi*, the nerves were cut in the middle of the neck. It is nevertheless quite possible, that this may affect the results in some experiments more than in others, and it is evident that, if the head be depressed when a quantity of fluid is thrown into the *trachea*, or if the fumes from a muriatic acid bottle be used as an irritant (as by Brachet in some of his experiments,) the chances are, that a part of these will reach the interior of the *larynx* and excite violent efforts to cough.† In

* Opera Minora, Tom. i. p. 402, Laus. 1762; or Sur la Nature Sensible et Irritable, Tome i. p. 394, Laus. 1756.

† Müller refers to the experiments of Krimer, as corroboratory of those by Bra-

the last communication we illustrated at some length (p. 144,) the great difference in the acuteness of sensibility between the mucous surface of the *larynx*, and that portion of the air-passage placed below it; and the same thing was remarked by Mr Key in an operation on an individual of the human species for the extraction of a foreign body from the *trachea*.* I have performed several experiments with the view of satisfying myself regarding the effects of the division of the *vagi* upon the sensibility of the mucous membrane; and though I have seen good reason to believe that it is much blunted, I am by no means satisfied that it is entirely annihilated. I must also state that it is very difficult to procure a decisive experiment on this point. The animal frequently becomes restless and uneasy after section of the nerves, if care be not taken to secure a sufficient supply of air to the lungs; and it may make forcible expirations not unlike a cough when the mode of irritating the inner surface of the *trachea* is such as to interfere materially with the passage of the air along it. This, however, in all probability, may arise from the sense of anxiety occasioned by the impediment to the respiration. Another source of error arises from the great facility with which the air-passages tolerate stimulants, which at first excite great uneasiness, as was well illustrated in the practice followed by Dessault.† I shall now detail a few of the experiments which I made, to illustrate these sources of fallacy I have mentioned, and also show that the sensibility of the mucous membrane of the *trachea* and *bronchi* is at least blunted after division of the *vagi*.

Exp. XXVIII.—The pneumogastrics were exposed without disturbing them in a large terrier dog. A small opening was then made into the *trachea*, and a quantity of water was thrown down towards the lungs (the head being kept erect) without exciting cough. A small quantity of alcohol was then injected downwards, and this was followed by efforts to cough. After these had ceased, it was thought advisable to witness their repetition. A fresh quantity of alcohol was therefore injected downwards, but the animal remained perfectly quiescent. This was again repeated with the same results. It was now apparent that it was useless to proceed farther, so that, instead of dividing the *vagi*, we injected half an ounce of air (by measure) along the carotid artery towards the brain. This was followed by strong convulsions of the limbs and trunk, succeeded by coma, which continued for at least four hours,—the time it was

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chet on this subject. (Baly's Translation, Vol. i. p. 353.) As I have not yet been able to procure the treatise by Krimer in which these experiments are detailed, I am ignorant of the manner in which they were conducted.

* Lancet, 1828–9.

† Oeuvres Chirurgicales par Bichat, Tome ii. p. 266, 1801.

last seen alive. It occasionally howled and moved its limbs rapidly.*

Exp. XXIX.—The *vagi* were exposed in the middle of the neck, but not disturbed, in an ordinary sized terrier. A small opening was then made into the *trachea*, and some cold water injected downwards, the head being kept erect. This was followed by slight efforts at coughing. The nerves were then divided, and the animal became restless and uneasy. Both water and alcohol were injected downwards after division of the *vagi*, without exciting cough as long as the head was kept erect, while it occurred when the head was placed in a depending position.

The alcohol injected was at first in small quantites at a time, and after short intervals. The whole quantity injected was two drachms, and this produced some symptoms of narcotism.

Exp. XXX.—The *vagi* were exposed in the middle of the neck, but not disturbed, in a very lean and large mongrel dog, lately recovered from the distemper. A very small opening was made into the *trachea*, and a quantity of cold water thrown downwards without inducing cough. A small quantity of alcohol was then injected, and three or four distinct coughs followed. The *vagi* were then divided, and this was followed by no impediment to the respiration. Alcohol to the extent of half an ounce in all was thrown in divided quantities down the *trachea*, without exciting any efforts to cough. The animal was at last evidently affected by the alcohol, for when let loose it staggered in walking.

Exp. XXXI.—The *vagi* were exposed without being disturbed in a young middle-sized mongrel. Water injected into a small opening in the *trachea* excited pretty urgent cough, and it appeared uneasy and coughed frequently when alcohol was injected. Section of the *vagi* was followed by struggles and symptoms of suffocation. It recovered from this apparently to a considerable extent; and some alcohol was then injected into the *trachea* without exciting uneasiness or cough. A drop of muriatic acid was also placed in the *trachea* without exciting uneasiness. Some alcohol was now injected towards the *larynx*, and this was followed by uneasiness and cough. The animal was now killed by a dose of prussic acid. In none of these three last experiments was any cough excited by irritation of the inner surface of the *trachea* and *bronchi* after division of the *vagi*, but I could adduce two expe-

* I have repeated this experiment of blowing air along one of the carotids or vertebrals towards the brain, in animals which had not been previously subjected to the influence of any narcotic, and always found that it produced convulsions and coma, lasting for some time before death. The quantity of air injected was from half an ounce to an ounce, and even more. The mode of death in such cases is very different from what I have observed when air is injected *rapidly* and in *considerable* quantity into a vein leading directly to the heart, for it then kills by mechanically arresting the movements of the right side of the heart.

riments where a large tube was first introduced into the *trachea* before the division of the *vagi*, in which uneasiness and forcible expirations, which I could not distinguish from an effort to cough, were excited, when irritating substances were injected into the *trachea*. The details of Experiment XXVIII. are very instructive. If we had proceeded to divide the *vagi* after the first injection of the alcohol without repeating it, we might have attributed the different results which followed, entirely to the section of the nerves. We cannot assert that the same thing may not have occurred in one or more of the three other experiments. It is worthy of remark, that in four dogs which lived beyond the fourth day after section of the *vagi*—in three of which the recurrents were also divided—frequent cough, or at least forcible expirations, which I could not distinguish from a cough, were observed. The animal the subject of Experiment XIV. coughed so incessantly during the three last days of its life, that I could not reckon the respirations until after many trials, and even then imperfectly. Whether this was dependent upon any irritation of the upper part of the cut nerves, or upon any irritation of the mucous surface of the lungs, or upon any other cause, I cannot pretend to determine. I may, however, mention, that the examination of the cut ends of the nerves after the death of these animals did not disclose any marks of high inflammation. In an experiment by Valsalva, where the *vagi* were cut in a young dog which lived to the eighteenth day, the animal is described as coughing on the third and some of the subsequent days.*

Morbid Changes in the Lungs.—I have removed a portion of both *vagi* in seventeen dogs, for the purpose of examining the morbid changes in the lungs consequent upon section of these nerves. In ten of these the recurrents were also divided. The time which twelve of them lived after division of the nerves is given in the tables illustrating the diminution of the frequency of the respirations after this operation. I may also add here, the period of time which the other five lived. In Experiment XXXII. the animal lived thirty-one hours; in Experiment XXXIII. it lived thirty hours and a-half; in Experiment XXXIV. it was killed after ninety-six hours; in Experiment XXXV. it lived fifty-five hours; and in Experiment XXXVI. it lived twenty-one hours. In all of these last experiments except the XXXVth, a tube was introduced into the *trachea*. We find on examin-

* Valsalvae Opera cum Epistolis Anatomicis J. B. Morgagni, Epistol. Anatom. xiii. 36.—Supposing it to be proved that excitants applied to the mucous membrane of the lungs can excite sensation after section of the *vagi*, it would not necessarily follow that the impressions which give rise to these sensations are conveyed through the grey filaments proper to the ganglionic system, since it appears (Müller's Physiology, p. 664–672,) that a certain number of the white filaments of the cerebro-spinal system accompany all the branches of the ganglionic system.

ing the period of time which these seventeen animals lived, that four died before the completion of twenty-four hours, and in three of these a tube had been introduced into the *trachea;* three died between the twenty-fourth and forty-eighth hour—all of which had tubes introduced into the *trachea;* four died between the forty-eighth and seventy-second hour, none of which had tubes introduced into the *trachea;* one died between the seventy-second and eighty-sixth hour; in this animal no opening was made into the *trachea;* one was killed after four days and eight hours, and, though an aged dog, had an opening made into the *trachea;* two died after the completion of the eighth day; one died on the twelfth day; and one was killed after the completion of the twelfth day. In the last four experiments the animals breathed freely without opening the *trachea.* To these seventeen experiments I could also add several others, where the animals were killed at longer or shorter periods after division of the nerves, and to some of which I shall have to refer. The young dogs subjected to these experiments generally died earlier than the more aged, as may be inferred from the circumstance, that the greater number of those which lived longest did not require the introduction of a tube into the *trachea.* The lungs were in a state unfit for the healthy performance of their functions in fifteen out of the seventeen animals experimented upon. One of the two animals in which the lungs were found healthy (Experiment X.) died after the completion of the eighth day, apparently from inanition; the other (Experiment XIII.) was killed after the twelfth day, when it was apparently in perfect health. More extensive experience has enabled me to make some material alterations upon the few observations I made on these points in my last communication, (p. 163.) The most common morbid changes found in the lungs of those animals was a congested state of the blood-vessels of the lungs, and the effusion of frothy serum into the air-cells and bronchial tubes. In eight out of the seventeen these appearances were strongly marked. In some portions of the lungs the quantity of blood was so great as to render them dense. The degree of congestion varied in different parts of the same lung, but it was generally greatest at the most depending portions. This condensation was not unfrequently greater than what could be accounted for by mere congestion of blood in the vessels, and probably arose from the escape of the solid parts of the blood into the tissue of the lung. The frothy serum had frequently a more or less deep tinge of red. Some of those animals were opened immediately after death, which in three cases was hastened by a dose of prussic acid, given when the animal had become cold and stupid. In Experiments XVI. XX. XXII. and XXXII. though different parts of the lungs were

much loaded with blood, the quantity of frothy serum found in the bronchial tubes was but trifling, and certainly not sufficient to impede the respirations to any great extent. In Experiment XVI. the animal lived fifty-two hours; and it is stated in my notes of the dissection, that there was a minute quantity of frothy serum in the larger bronchial tubes, and a small quantity of serum could be pressed from the minute bronchial tubes. A great portion of both lungs was so dense as not to crepitate when cut, was of a dark colour, and sank in water, though the cut surfaces were smooth, and did not present the granulated appearance of the second stage of ordinary pneumonia. In Experiment XX. the animal died between the seventy-sixth and eighty-third hour. Without giving the details of the dissection as they are written out in my notes, I may merely mention that part of the left lung was in a state of gangrene, and that some portions of the right lung were dark-coloured and dense, but floated in water. In Experiment XXII. the animal was killed after sixty-nine hours, and after it had become feeble. The lungs were dense, and there was only a small quantity of frothy serum in the bronchial tubes. In Experiment XXXII. the animal lived thirty-one hours. The chest was opened immediately after death, in fact, as soon as it had ceased to breathe. The lungs were dense and loaded with blood, but there was very little frothy serum in the larger and smaller bronchial tubes. In Experiment XI. the animal received a dose of prussic acid on the eighth day, after it was evident, from the state of its breathing, that it could not survive, and the chest was immediately opened. A great part of the left lung was in a state of pneumonia, in some places approaching the third stage, and many of the smaller bronchial tubes were full of a puriform matter. The right lung was dark-coloured and rather dense in some parts, but these when cut out floated in water. There was no appreciable quantity of frothy serum in the bronchial tubes of right lung. In Experiment XV. the animal died on the twelfth day. The left lung was voluminous, of a deep dark-colour, and dense. When cut into, little blood could be squeezed out, and the cut surfaces were not granular. The right lung was somewhat congested with blood at a few parts. The *trachea* contained some, and the left bronchial tubes much red mucus; while the bronchial tubes of the right lung were empty. In Experiment XXXIV. the animal was killed after four days and eight hours. The right lung was nearly healthy; while the left lung was almost entirely in the second and third stages of pneumonia, and there was very little serum in the bronchial tubes. Of the eight experiments in which the frothy serum was effused in great quantity into the bronchial tubes, the left lung was partly gangrenous in one, (Experiment XXI.) In other two experiments, XXXV. and

XXXVI. part of the lungs was in the second and third stages of pneumonia, with effusion of puriform matter and much frothy serum into the bronchial tubes. In the seventeen experiments, therefore, five presented distinct traces of pneumonia, and in two it had run on to gangrene. In one of these the gangrenous portion was surrounded by circumscribed abscesses. In these experiments, I need scarcely again remark, great care was taken to secure a free passage to the air along the *trachea*, and this was apparently accomplished, except in Experiments XXXIII. and XXXVI. where the tracheotomy tubes employed were too small. One of the most important points to ascertain, in an investigation of this kind, is the first departure from the healthy state; to decide whether the effusion of the frothy reddish serum, by interfering with the usual changes of blood in the lungs, *causes* the congested state of the pulmonary blood-vessels and the laboured respiration; or whether this effusion is the *effect* of a previously congested state of the blood-vessels. If it be made out that the effusion of serum is consequent upon the congested state of the blood-vessels, we have next to inquire, what is the probable cause of this congested state of the pulmonary blood-vessels? In examining, with a reference to this question, the results of the experiments which we have made, one of the most important circumstances which presents itself is the fact, that the effusion of frothy serum into the bronchial tubes, in quantities sufficient to impede materially the respiration, is not a necessary consequence of division of the *vagi*, even when the lungs were found loaded with blood and when the respiration before death was very laboured. And this naturally leads us to doubt whether the frothy serum is the cause of the laboured respiration and the congested state of the pulmonary blood-vessels in those cases where it is present, though there can be no doubt that, when once it is effused, it must powerfully tend to increase the difficulty of the respiration and the impeded circulation through the lungs. Another important circumstance in enabling us to decide this interesting point is derived from the fact, of which I have satisfied myself after much careful examination, that this frothy fluid is not mucus, though it is occasionally mixed with it, but is the frothy serum so frequently found in cases where the circulation through the lungs has been impeded for some time before death. To investigate this point still farther, we killed several dogs at longer and shorter periods after section of the *vagi*, and we were more and more confirmed in the opinion, that the congestion of the blood-vessels is the first departure from the healthy state of the lungs, and that the effusion of frothy serum is a subsequent effect. Perhaps one of the most illustrative experiments I can select is the following.

Exp. XXXVII.—The recurrents and *vagi* were cut across and a portion removed in a middle-sized bull-dog at 2 o'clock P. M. At 7 P. M. his breathing was easy, and the respirations were 8 in the minute. Next day at 2 P. M. the respirations varied from 10 to 12. On the following day the respirations were 6 to 7, easy but heaving. On the third day at 11 A. M. the respirations were still between 6 and 7 in the minute, very heaving and prolonged, and the animal was evidently becoming very feeble. The femoral artery was exposed and a small branch opened, when the blood escaped in a feeble stream, and was almost as dark-coloured as venous blood. A fatal dose of prussic acid was now given, and the chest immediately laid open, care being taken to avoid the large vessels at the root of the neck. The large pulmonary vessels were now opened, and the blood both in the arteries and veins was dark and fluid. The lungs were not emphysematous, and contained much blood in their vessels. The left lung was of a deep dark-colour at the summit of the anterior lobe, and this when cut into was found quite dense, without granules, and scarcely any blood could be pressed from it. The bronchial tubes of this part contained some puriform mucus. A similar but smaller dense portion was situated about the middle of the same lung. Slices from these two portions sank in water. The right lung contained much blood; some parts of it were denser than others, but none of it sank in water. The mucous membrane of the bronchial tubes was moist and lined by a thin layer of mucus. A small quantity of frothy serum was found in some of the bronchial tubes. If the congested state of the blood-vessels precede, as we believe, the effusion of the frothy serum, we have next to inquire, what is the cause of the retardation of the blood, and congestion of the blood-vessels in the lungs? This, we were formerly inclined to believe, might depend upon paralysis of the muscular fibres of the bronchial tubes; but, being unable to obtain any satisfactory evidence of this, we again watched the phenomena more narrowly, and now believe that all the morbid changes observed in the lungs can be traced to the diminished frequency of the respiratory muscular movements. We have already dwelt at some length upon the influence of the *vagi* as exciters of respiration; and we have shown that when these nerves are tied or divided, the number of the respiratory muscular movements is instantly considerably lowered—generally more than one-half. Now, it is an established fact, that the flow of blood through the lungs is dependent upon the continuance of the respiratory process, and the great diminution in the activity of the respiratory muscular movements must be followed by a retardation and congestion of the blood in the lungs. This congestion of blood, as is well known, is generally followed by effusion of serum, and also predisposes

the organs so circumstanced to various morbid changes, chiefly of an inflammatory kind. In the lungs this congestion is not only followed by the escape of the serum from the vessels, but also of the more solid materials, rendering the tissue dense. The effused serum is mixed up with the air moving along the bronchial tubes during inspiration and expiration, and it thus becomes frothy. A little blood also exudes from the congested mucous membrane, giving the serum a reddish tinge. Such, we believe, is the explanation of the morbid changes observed in the lungs after lesion of the *vagi*. If artificial respiration, which is a very imperfect substitute for the natural process, be carried on for some time in healthy lungs, a similar effusion takes place. Legallois, who must have been perfectly familiar with the appearance of this effused fluid, after stating that its formation was accelerated by artificial respiration in animals after section of the *vagi*, and after decapitation, is obliged to admit that a similar fluid is very frequently formed in animals not subjected to either operation when artificial respiration is carried on. "Car très-souvent il s'en forme un semblable dans les animaux entiers qu'on insufle." * We would not wish peremptorily to deny that the effects of the diminished frequency of the respiration may not be aided by paralysis of the muscular fibres of the bronchial tubes; we only state that after the most anxious endeavour to obtain some distinct evidence of this, we have been unsuccessful. Some may be inclined to believe that the *par vagum* may exert some favourable influence upon the capillary circulation of the lungs. The thing is possible, but we know of no well ascertained facts which could be adduced even as an analogical argument in favour of such an opinion.

I have not unfrequently observed in the human species, where the respiration has been impeded for some time before death, a condition of the lungs similar to that observed in the lower animals after division of the *vagi*. In several of the fever patients whom I have had lately occasion to inspect at the Royal Infirmary, the lungs in the posterior and middle parts were dark-coloured and gorged with blood and serum. When cut into, the substance of the lungs in some cases appeared at different parts denser than what could be accounted for by mere congestion of the blood-vessels, and a comparatively small quantity of blood could be squeezed from the cut surfaces, though the blood in other parts of the body was fluid; but sections of the lung generally, though not always, floated in water, and presented none of the granular appearance. In some cases the bronchial tubes contained a considerable quantity of frothy serum; in others very little. In all probability these morbid appearances are occasionally dependent upon the disturbed respiration consequent upon de-

* Experiences sur le Principe de la Vie, p. 242, 1812.

rangement of the central organs of the nervous system. I lately saw a gentleman labouring under fever, whose respirations for a short time were only 8 in the minute, though the lungs at the time were unaffected, but they fortunately soon rose to 16 in the minute, and he ultimately recovered. Dr Alison has suggested to me that these morbid changes in the lungs are sometimes owing to another cause. He believes that, in cases of fever, where the heart's action is feeble, the bronchiæ often somewhat obstructed, and the blood altered, the right side of the heart is unable to propel the blood through the lungs; it consequently goes on accumulating in their depending parts, and the same results follow as when the respiratory movements are diminished in frequency. In confirmation of this view he stated that he has seen these morbid changes occur in the lungs without any preceding diminution of the respiration. When we remember that the pulmonic circulation is dependent upon two distinct causes varying in efficacy—the contractions of the right side of the heart, and the chemical changes going on at the lungs—both of which are necessary for the proper propulsion of the blood through the lungs to the left side of the heart, we can easily understand how a diminution in the activity of the respiratory muscular movements, and the impaired contractility of the right side of the heart, should produce the same effect, viz. congestion of blood in the lungs and effusion of frothy serum.* I lately examined the bodies of two patients who died in the Royal Infirmary, which afforded a very important confirmation of the views which we have adopted regarding the cause of the morbid changes in the lungs after lesion of the *vagi*.

Case I. James Hallam, a sailor, aged 38, was admitted into the Royal Infirmary, on the 30th November last, with a slight attack of *bronchitis*, from which he rapidly recovered under the usual remedies. On the 6th December it was reported that he had no complaint. On the 11th it was remarked that he had been drowsy and almost constantly asleep since the day preceding, but he was easily roused, answered questions, and made no complaint. The pupils were contracted and little sensible to light. The respirations were only 5 in the minute and heaving. Some appearance of *anasarca*. Urine spec. grav. 1009, coagulable by heat and nitric acid; pulse of moderate strength. On the 12th the respirations were still only about 5 in the minute, heaving and prolonged, and exactly similar to what are observed in dogs after section of the *vagi*. On the 13th the respirations varied from 5 to 10, and were still heaving and prolonged. On the

* The fluid state of the blood in fever will naturally aid in producing this congested state of the pulmonary vessels. Vide Magendie's Leçons sur les Phénomènes Physiques de la Vie, Tom. iv. 1838. As far as my observation goes the coagulability of the blood is not materially affected after section of the *vagi*.

morning of the 14th the respiration remained of the same character. He had passed no urine since yesterday, and three ounces were drawn off by the catheter. He died at noon in slight convulsions.

Sectio cadaveris, 16*th*.—The arachnoid membrane appeared somewhat dry, and the convolutions were perhaps rather flattened. The vessels on the surface of the brain were well filled with blood, and a considerable number of red points presented themselves on slicing the brain. The substance of the brain was not firmer than natural, and there was little fluid in the ventricles. The heart was healthy, and the right side was filled with coagulated blood. There was a little reddish serum in the right side of chest. Lungs emphysematous along their anterior margins. The bronchial tubes were full of frothy serum, and the whole of the posterior and middle portions of both lungs were of a dark-red colour, and were full of blood and serum. Some parts of the lungs were so dense as not to crepitate when cut, but they presented none of the granulated appearance. The liver was natural, with the exception of the presence of an old cicatrix. The kidneys were of a yellowish colour externally, and their capsule was firmly adherent. The cortical structure of the kidney was nowhere apparent, and its place was occupied by a yellowish substance, which also encroached in some places upon the tubular structure. The kidneys were of their usual size, and firmer than natural. The trunks of the *vagi* were examined at their origin, at the middle and lower part of the neck, and within the thorax, and no morbid appearances observed.

Case II. Peter Ballantyne, aged 40, a tailor, was admitted into the Royal Infirmary on the 14th December, with cough and expectoration tinged with blood. He also complained of palpitation, which was much increased by exertion. He stated that he had been subject to headache and vomiting, and his friends mentioned that he had been lately languid and indulged in long sleeps. When admitted the respirations were 14 in the minute. Urine scanty, specific gravity 1011, coagulable by heat and nitric acid. The pupils were contracted, and he appeared to have a tendency to drowsiness. After his admission the cough and expectoration diminished, but the strong action of the heart continued, and the urine retained its former characters. On the 18th the cough was slight; his respirations had fallen to five in the minute; pulse 96, small; countenance exsanguine; was very drowsy, but answered questions. On the 19th, he gradually verged into a state of coma. The respirations varied from 5 to 8 in the minute. The pupils were immovable and contracted. Pulse 84, rather feeble, but extremities warm. He had passed no urine for several

hours. There was no œdema. No paralysis was observed during life. He died at 6 o'clock P. M.

Sectio Cadaveris, 21*st*. — The surface of the brain was unusually pale ; few red points presented themselves on slicing the brain, and the vessels appeared to be principally filled with serum. There were about two drachms of serum in the lateral ventricles, and half an ounce at the base of the brain. Two small clots of blood were found in the right *corpus striatum*, without surrounding softening. In the central portion of the anterior part of the *tuber annulare* there was a small cyst lined with a yellow membrane ; and immediately posterior to this there was a small clot of blood about the size of a large pin's head. The large blood-vessels of the brain were dilated and thickened in their coats, but without calcareous deposit. The trachea and bronchial tubes contained a considerable quantity of frothy serum. The posterior and middle parts of the lungs were gorged with blood and frothy serum, and a considerable portion was so dense as not to crepitate when cut ; but it did not present the granulated appearance, and floated in water. There was a mass of old tubercles in the apices of both lungs. The left ventricle of the heart was considerably thickened in its walls without diminution of its cavity. The valves were quite sufficient to fulfil their functions, and the thoracic aorta retained its elasticity. The kidneys were rather smaller than usual, and had numerous yellowish spots scattered through the cortical portion. The *vagi* were examined at their origin, at the lower part of the neck, and within the thorax, and no morbid appearance could be detected.

We were prevented by the friends in this, as in the former case, from examining the *vagi* through their whole extent ; but this is perhaps a matter of little importance, as it was sufficiently evident from the symptoms that the drowsiness and slow respiration in both arose from cerebral derangement. In the first case, this in all probability arose from the accumulation of urea in the blood, exerting a deleterious influence upon the brain—a not unfrequent concomitant of kidney disease.* It may be a matter of doubt whether the cerebral derangement in the second case arose from the same cause, or from the effusion of blood. The symptoms certainly presented themselves in a more gradual manner than what we should expect in cases of apoplexy, and more resembled the coma which occurs in kidney disease. I may state that Dr Christison has seen cases of coma in kidney disease where no œdema was present.

We have embraced every opportunity of examining the effects of lesion of the *vagi* upon the secretion of mucus from the inner

* Vide Christison on Granular Degeneration of the Kidneys, Edinburgh, 1838, p. 92.

surface of the bronchial tubes. We have already stated that we believe the frothy fluid so frequently found in the bronchial tubes is not mucus and the result of a morbid secretion, as Wilson Philip* and others have imagined, but that it is chiefly, and in some cases almost entirely, composed of serum, and is consequent upon the congested state of the blood-vessels—an effect so frequently observed in other parts of the body. In killing animals at very various periods after section of the *vagi*, we have never found the inner membrane of the bronchi unusually dry, but always covered with the usual quantity of protecting mucus, except in those cases where inflammation was present. And we can explain how the application of galvanism in the experiments of Dr W. Philip was so efficacious in preventing this effusion, without being obliged to admit that the secretion of the mucous membrane is deranged; for, since the galvanism was applied so as to keep up a constant spasmodic action of the limbs, the frequent contractions of the respiratory muscles produced by this excitation would prevent the diminished frequency of the respiratory movements. The explanation given by Brachet† of the source of this frothy fluid—viz. that it is the usual mucous secretion of the lining membrane of the air-passages which has accumulated there, because this membrane has lost its sensibility, and the muscular fibres of the bronchi have lost their contractility,—is so palpably objectionable as to require no remarks.

An excellent illustration of the numerous difficulties with which the physiologist has to contend, from the impossibility of insulating any individual organ from its mutual actions and reactions with others, when he wishes to examine the order and dependence of its phenomena, is furnished by the experimental history of the *par vagum*. The two first experimenters, Rufus of Ephesus,‡ and Galen,§ attended only to the loss of voice. A new direction was then given to this investigation by the suggestion of Piccolhomini‖ that it affected the heart's action, and numerous experiments were afterwards adduced in support of this opinion. Some described its effects as instantly fatal. F. Schrœder stated, that, when both nerves are tied, the animal dies instantly "animal vitam protinus amittit."¶ Bohnius, in mentioning an experiment of this kind, indulges in a highly figurative expression, which appears to be still a favourite with some physiologists, and describes the animal dying as if struck with a thunderbolt, "fulmine quasi tactum."** Varingnon relates, that when he

* Inquiry into the Vital Functions, Chapter XII.
† Oper. cit. p. 160.
‡ Vide Morgagni de Sedibus et Causis Morborum, Epist. XIX. Art. 23.
§ De Locis Affectis, Lib. I. Cap. 6.
‖ Anatomicæ Prælectiones Arch. Piccolhomini, Romæ, 1586, p. 272.
¶ Addit. ad Vesling. Synt. c. 10, n. 7, as quoted by Morgagni.
** Circul. Anatom. Progym. vi. p. 104, Lips. 1686.

had tied these nerves in a cat, the animal "était mort dans l'instant sans aucun mouvement d'aucune partie de son corps." * Some experimenters, however, such as Willis, Vieussens, and many others, found that the operation was not always instantly fatal, and that the animal might live twenty-four hours, and even several days: and though puzzled to account for this on their favourite theory—that a constant supply of animal spirits flowed along the vagus to the heart—yet they succeeded in accommodating the facts to this explanation, by supposing that a diminished supply of the vital spirits continued to reach the heart through the more circuitous and less favourable channel of anastomoses.† Little progress was made in this inquiry for a long series of years, and the prevailing doctrine appears to have been, that section of the nerves, some way or another, affected the heart's action. The experiments of Dupuytren ‡ are chiefly important, as they directed the attention of physiologists in an especial manner to the derangement of the lungs. No doubt the derangement of the respiratory function had been previously remarked by Valsalva, Morgagni, Molinelli, Haller, Brunn, Cruickshank, and some others. Valsalva states, however, that nothing morbid was found in the chest in a dog which he dissected after death from this operation. § Morgagni found the lungs congested with blood. ‖ Mr Cruickshank, in Experiment II. where the animal died on the seventh day after division of the second *vagus*, found every organ healthy except the lungs, and these he describes to have been of a red-brown colour, so dense as to sink in water, and containing frothy fluid with some puriform matter in the bronchial tubes. In Experiment III. he found the lungs loaded with blood.¶ Dupuytren concluded from his experiments,—and in this Hallé and Pinel, the reporters on the memoir, concurred,—that animals in which the *vagi* are cut die of asphyxia, because the atmospheric air, although continuing to penetrate freely into the lungs, can no longer combine with the blood, since this combination cannot take place except under the agency of the nervous system. This is also the opinion which Sir Astley Cooper is disposed to adopt in the account of his experiments on the *vagus*, given in Guy's Hospital Reports, already referred to.** Soon after the publication of Dupuytren's memoir, it was shown by the experiments of Dumas and

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* Histoire de l'Acad. Roy. p. 28, 1706.

† Willis Cerebri anatome, p. 324; Vieussens Neurographia Univer. p. 179, Ludg. 1785.

‡ Biblioth. Med. Tom. xvii. p. 1. An abridgement of the memoir is given in the Journal Medec. de Corvisart. Tom. xiv. p. 45.

§ Valsalvæ Opera, &c. Epist. Anatom. xiii. p. 512.

‖ Oper, cit. Epistol. Anat. 29.

¶ Medical Facts and Observations, Vol. vii.

** No. III. p. 470.

De Blainville in France, and of Emmert in Germany, that arterial blood may continue to circulate in the arteries after section of the *vagi*. Dumas proved that the arterial colour could be restored by artificial inflation of the lungs after the blood in the arteries had become venous.* Emmert satisfied himself that, after the trachea had been compressed in rabbits and cats in which the *vagi* had been divided, the arterial hue of the blood could be restored by removing the pressure, and allowing the air again to enter the lungs.† It was likewise proved by Sir B. Brodie in this country, that the usual chemical changes take place at the lungs in a decapitated animal when artificial respiration is kept up.‡ Provençal in France also showed that an animal in which the *vagi* have been divided continues to deteriorate the air by the formation of carbonic acid gas, though not to the same extent as before the operation. § The experiments of Le Gallois ‖ at last came, and satisfactorily explained all the anomalies connected with the occasionally rapid death of the animal, and the sudden arrestment of the chemical changes at the lungs, which had so long puzzled physiologists. He pointed out by decisive experiments, that when the *vagi* are divided, more especially in young animals, the superior aperture of the larynx frequently becomes closed, and the animal is suffocated. He also fully described the congested state of the blood-vessels of the lungs and the effusion of frothy fluid. Legallois, however, seems to have imagined that the effused frothy fluid was the cause of all the dyspnœa, and that this effusion in its turn was dependent upon a kind of paralysis in the lungs. The same morbid appearances in the lungs pointed out by Legallois have been frequently observed and described by more recent experimenters, among whom we may mention Dr Wilson Philip,¶ Dr Holland,** and M. Brachet.†† In examining the morbid appearances in the lungs, we have observed no facts in confirmation of the statement of Mr Swan, that section of the vagi produces emphysema of the lungs.‡‡ We have undoubtedly seen portions of the lungs emphysematous in a few cases, but we had no reason to believe that this was the necessary result of the section of the nerves. That the lungs should collapse imperfectly when gorged with blood and frothy serum, is certainly no proof that they are emphysematous. Neither have we seen any thing confirmatory of the

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* Journal General de Médecine, Tome xxxiii. p. 353.

† Archiv. für Physiologie von den Professoren Reil und Autenrieth. Neunter Band. S. 380, 1809. und Eilfter Band S. 117, 1812.

‡ Philosophical Transactions, 1811.

§ Bulletin des Sciences Medec. Tome v. p. 361.

‖ Sur le Principe de la Vie, 1812

¶ Oper. cit. ** An Experimental Inquiry, &c. Edinburgh, 1829.

†† Oper. cit. ‡‡ Essay on the connection of the Heart and Arteries, &c. 1829.

opinion of Mayer, * that if the animal live more than forty-eight hours, the arteries and veins of the lungs, even to their minute ramifications, as well as the cavities of the heart, are filled with white, firm, and compact coagula, consisting of the fibrine and albumen of the blood; while if the animal die sooner, the coagula are soft and dark-coloured. It is these coagula, according to Mayer, which cause the arrestment of the movements of the heart and produce death. He believes that when the influx of the nervous influence is arrested by division of the vagi, the fluidity of the blood ceases, and it separates itself into its constituent parts, as when drawn from the body. Müller has been equally unsuccessful in finding these fibrinous clots in the pulmonary vessels. †

The explanation of the fact, that lesion of one *vagus* does not necessarily nor even generally induce disease of the lung of that side, may, I believe, be satisfactorily obtained, when we consider that this is not likely to diminish the number of respirations so much as the lesion of both *vagi*. We have in several dogs cut the *vagus* of one side, and after having observed the changes produced, we then divided the *vagus* of the opposite side. Some of the results will be found in the following table.

	Respirations before operation.		One vagus divided.		Both vagi divided.
Exp. 1st,	14 to 16	-	11 to 12	-	5 to 6
— 2d,	15	-	13	-	6 to 7
— 3d,	16 to 17	-	9 to 10	-	6 to 7
— 4th,	16 to 18	-	12	-	8

In other two experiments the frequency of the respirations was not sensibly affected by section of one *vagus*, though the animals were kept for eight days. In one of these the other *vagus* was cut, and the frequency of the respirations suffered a very marked change, falling from 20 to 8 in the minute.

In examining the effects of lesion of the *vagi* upon the lungs, it is particularly worthy of remark, that these morbid changes in the lungs do not necessarily follow the division of both *vagi* and recurrents. In a dog, Experiment XIII. which was killed in presence of Dr Alison, twelve days after the section of the *vagi* and recurrents, the lungs were found perfectly healthy, though the respirations before death were still slow and heaving. This animal had recovered from the effects of the operation on the digestive system, and was rapidly gaining flesh and strength. In dissecting the nerves after death, the cut ends of the left *vagus* were one inch and one line apart; and the distance between the cut ends of the right was one inch and two lines. In another dog, Experiment X., which

* Tiedemann's Zeitschrift für Physiologie. Zweiter Band, S. 74–5, 1826.
† Elements of Physiology, Vol. i. p. 358. Baly's translation, 1838.

died apparently from inanition eight days after division of the *vagi* and recurrents, the lungs were also found healthy. The nerves in both of these animals were carefully examined after death, and no anormal arrangement of them could be observed. Previous to the performance of Experiment XIII. I fully believed in the correctness of the opinion expressed by Magendie* and by Müller, † that the simultaneous division of both *vagi* is always fatal within a few days. I have, however, no doubt in my own mind, and this was the opinion of all who saw this animal after the tenth day, that he had fairly recovered from the operation. About the sixth and seventh days after the operation, he was so weak as to stagger occasionally in walking, while at the time of his death he was so lively and active as to leap over places between three and four feet high, for his amusement. I afterwards regretted that I did not permit him to live longer, but I then had hopes that two others would survive the operation. In this I was, however, disappointed. I find that M. Sédillot, in detailing an experiment, (to which I shall have occasion again to refer,) where the animal lived two months and a-half after section of both *vagi* with loss of substance, states, that there was no induration of the lungs, and that they contained little blood. In examining the details of the very numerous experiments which have been made upon the effects of the division of both *vagi*, we shall find that by far the greater majority of animals die before the completion of the third day. In the seventeen experiments we have so frequently referred to, eleven died before the completion of the third day, and seven of the eleven before the completion of the second day. Valsalva remarks that animals in which the *vagi* are simply divided, live longer than those in which the nerves are tied.‡ In one of Petit's experiments the animal lived seven days.§ One dog experimented on by Baglivi died on the twelfth day; and another on the seventh day.‖ Morgagni details, in his additions to the works of Valsalva, the results of two experiments on dogs—one of which lived to the tenth, and the other to the eighteenth day.¶ The eighteenth day is apparently the longest period to which he had seen an animal live after section of the *vagi*, as he refers to it with this view when speaking of the duration of life after this operation. None of the animals experimented on by Baglivi and Morgagni ever recovered from the operation, but gradually became weaker. Besides, we are led to believe, that the nerves were simply cut across without loss of substance. M. Dupuy, in his experiments, found that horses lived

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* Milligan's translation, p. 398, 1831. † Baly's translation, Vol. i. p. 356.
‡ Oper. cit. Epist. Anatom. xiii. 28 and 38.
§ Memoires de l'Acad. Roy. A. 1727, Exper. 2, p. 6.
‖ Opera omnia, p. 676–7, Exper. 7 and 8.
¶ Oper. cit. Epist. Anat. xiii. 36–7, Tom. i.

to the fifth, sixth, and seventh day, when care was taken to admit a sufficient quantity of air into the *trachea*.* De Blainville, in giving the results of his experiments upon pigeons, informs us, that these animals died on the sixth or seventh day.† Sédillot states that, in one experiment, a dog lived twenty-one days; and that another dog lived two months and a-half after the operation, though more than an inch of the nerve was removed on both sides‡ Arnemann has detailed an experiment at considerable length, where the animal appears to have completely recovered from the effects of the division of both *vagi* with loss of substance.§ A portion measuring rather more than four lines was removed from the right, and one of eight lines from the left *vagus*. The animal at first suffered very severely from the operation, and it is described as gasping for breath. After a week it began to improve gradually, though the breathing was still difficult. After a month and a-half it had wonderfully improved, ate voraciously, but did not become fat. From the description given by Arnemann of the condition of the nerves in this animal when dissected after death, and from an engraving of the right *vagus*, (Tab. III. Fig. XVIII.) from which a smaller portion was removed than from the left, it appears that there was no regeneration of the removed portions of the nerves. The preparation of the right *vagus* was in the possession of Blumenbach. Notwithstanding the minuteness of the details of this case, and the apparent care with which the experiment was performed, and the nerves examined after death, we find that some authors express great suspicion of its accuracy.‖ It would appear that Arnemann had seen several dogs recover completely after division of both *vagi*, and that the animals were afterwards employed in other experiments upon the regeneration of some of the large nerves of the limbs; but I must confess that the statement is somewhat startling.¶ M. Brachet states in a note, that M. Fouacade had in 1820 presented to the Académie de Médecine, a dog in which four lines had been removed from the left

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* Journal de Medecine, Chirurgie, &c. Tome xxxvii. p. 356. December 1816.

† Nouv. Bullet. de la Soc. Philom. Tome i. An. 2, p. 226.

‡ Thèse du Nerf, Pneumogastric, &c. Exper. 1 and 6, Paris, 1829. M. Sédillot also states, (p. 23,) that M. Begin kept a dog alive for a month after division of both *vagi*. I find that M. Chaumet states (Essai sur la Physiologie de l'Estomac, p. 17, Paris, 1828,) that this animal operated on by Begin lived thirty-three days, and that no very obvious change was observed in the digestive process. M. Chaumet farther adds, that, in his own experiments, a dog lived fourteen days, and digested.

§ Versuche über die Regeneration der Nerven. hundert und zehnter versuch. S. 99, Göttin. 1787.

‖ Emmert. Oper. cit. Eilfter Band S. 420. Lund, Physiologische Resultate der vivisectionen neuerer Zeit. S. 241.

¶ Ich habe *mehreren* starken hunden *beide Vagos* und einen Intercostal-nerven zu gleicher Zeit durchschnitten. Nach einen *Monate* durchsnitt ich eben diesen Thieren mehrere grosse nerven beider *Vorderbeine* und in der *Folge* nach beide *Ischiadische Nerven* von *allen diesen Thieren* habe ich kein einziges verloren, Oper. cit. S. 193.

vagus, and the same operation had been repeated on the right a few days after.* On this case M. Brachet remarks that, supposing the nerves to have been accurately divided, the free anastomoses between the laryngeal nerves at the *larynx* may have sufficed to transmit the nervous influence. In our experiment the recurrents were also divided.

Gastric Branches of the Vagus.

Are the gastric branches of the vagus partly nerves of motion? In my former communication (p. 150) it was stated that I had repeatedly seen muscular contractions of the œsophagus induced by irritating the trunk of the nervus vagus in the neck, extend also over the cardiac extremity of the stomach. In the stomach they were evidently slower, more prolonged, and vermicular than in the œsophagus, and they extended somewhat slowly from the cardiac orifice over a greater or less extent of the left portion of the stomach. Irritation of the vagus frequently fails to produce these movements in the stomach, even where the muscular fibres of the œsophagus are thrown into vigorous contractions. These muscular movements of the stomach from irritation of the vagus, have been frequently observed by Tiedemann and Gmelin.† They have also been inferred by Breschet and Milne-Edwards from the effects upon the digestive process of galvanizing the lower ends of the cut *vagi* in the neck of a living animal.‡ Mr Mayo, however, could not perceive any muscular movements of the stomach upon irritating this nerve;§ and the same thing is very strongly stated by Müller.‖ I have again carefully and repeatedly performed this experiment, and I am confident that though it frequently fails, yet it often succeeds. These muscular movements are, as I have already stated, different in their character from those of the œsophagus, and resemble more those of the intestines. I have been unable to satisfy myself whether the muscular movements seen on irritating the vagus are dependent upon any direct excitation of the muscular fibres of the stomach through the filaments of the vagus distributed upon it; or whether they are dependent upon the movements of the muscular fibres of the lower part of the œsophagus being propagated onwards in the same manner as we find that contractions excited in the muscular fibres of the intestinal tube are propagated along it to a greater or less extent, and apparently without the aid of nerves. The supposition,

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* Oper. cit. p. 167. This experiment is also referred to by M. Sédillot, Oper. cit. p. 23.

† Recherches Experimentales Physiol. et Chem. sur la Digestion, &c. traduites par Jourdan, p. 374. ‡ Archiv. Gen. de Med. Tome vii.

§ Anatomical and Physiological Commentaries, No. ii. p. 15.

‖ Elements of Physiology, translated by Baly, 549.

that the movements of the stomach resulting from irritation of the vagus are merely propagated onwards from the œsophagus, is favoured by the circumstances, that the stomach frequently remains quiescent when the œsophagus is thrown into vigorous contractions; and that when these movements of the stomach do occur, they commence at the cardiac orifice, and extend themselves in a vermicular manner over the left extremity of that organ. I have likewise seen similar movements of the stomach produced by throwing the lower part of the œsophagus into contraction by pinching it with the forceps; but it is, however, quite possible that some of the gastric branches of the vagus were also included between the blades of the forceps. With the view of elucidating this point, I performed the following experiments upon a dog and three rabbits. In these animals the vagi were exposed in the neck, but without injuring them, from two to five hours after a full meal, and the stomach was then exposed by cutting through the parietes of the abdomen. I intended to have cut the *vagi* after watching the movements of the stomach, and to have observed the effects of the section upon those movements. The muscular movements of the stomach were, however, in all of these animals so indistinctly marked, that I could obtain no satisfactory results; and, as the experiment was a cruel one, I did not persevere in it. That muscular movements may occur in the stomach after section of the *vagi*, sufficient to propel the chyme onwards into the *duodenum* is, we conceive, sufficiently proved by the experiments which we shall immediately have to examine in detail, where the lacteals were found full of chyle after section of both pneumogastrics in the neck. It is the opinion of Breschet and Milne-Edwards, * and of Brachet,† that lesion of the vagi interferes with the proper performance of the digestive functions, by arresting the movements of the muscular fibres of the stomach; in consequence of which the aliments always preserve the same relation, and cannot be transformed into chyme except at the surface, or that part in contact with the walls of the stomach. Tiedemann and Gmelin also believe that the movements of the stomach principally depend upon the influence of the vagi. ‡ That the gastric branches of the vagus are not partly motor we will not deny, but of this we have not been able to obtain any decided evidence. It, however, cannot be to the extent maintained by the authors we have mentioned, from facts already stated.

Effects of Lesion of the Vagi upon the Functions of Digestion.—That lesion of the *vagi* is generally followed by vomiting (in those animals susceptible of it,) loathing of food, and arrestment of the digestive process, has been incontrovertibly

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* Oper. cit. Tome vii. p. 197. † Oper. cit p. 204–5.

‡ Oper. cit. p. 374.

proved by numerous experimenters. That perfect digestion may occasionally take place after division of the vagi in the neck, even when the cut ends of the nerves are kept apart from each other, we are also fully convinced. In four of the seventeen animals experimented on for the purpose of examining the morbid changes in the lungs, we obtained sufficient evidence of the restoration of the digestive process. The most satisfactory of these was the subject of Experiment XIII. to which we have already more than once referred. The animal operated on was a pretty large, full-grown, and lean bull terrier. Both recurrents and vagi were divided, and a portion of each removed. Immediately after the operation the respirations were much diminished in frequency, and continued so until its death, but were quite easy, unless when the animal was made to struggle. The nerves were cut at half-past eight o'clock, A. M. on the 15th June, and at three P. M. of the same day he ate some bread and milk, which he soon after vomited. He appeared to suffer no uneasiness, but was dull and languid. On the afternoon of the 16th, he was prowling about, apparently for food, and ate voraciously when he obtained it; but it was soon after vomited unchanged, except that it was mixed with fluid. He also drank freely. He appeared pretty lively. On the 17th, he still ate and vomited the food unchanged. On the 18th, vomited some animal food unchanged, which had been retained for at least seven hours. On the 19th, vomited the food taken in the morning, and again swallowed it. 20th, No vomiting to-day and food retained. 21st, Ate voraciously and apparently with relish, but food is vomited, and again partly swallowed. In the evening appeared duller than he had been for the two previous days. On the 22d was less lively, and appeared weak,—having a considerable difficulty, when lying, in getting upon his legs, and occasionally staggering when walking. Almost constant cough. No vomiting to-day, except of some bread and milk about a minute after it was taken, which he again immediately ate up and retained. 23d, Less feeble than yesterday; still some vomiting, and the animal food vomited was partly in a pulpy state but without fœtor. 24th, Less appetite to-day, and some vomiting of animal food in a state of pulp. Is, however, regaining strength, and can walk, and even run without staggering. 25th, ate voraciously to-day. Still occasional vomiting of half-digested food. Is rapidly gaining strength, and has less cough. 26th, Has eaten largely of animal food; no vomiting. 27th, This morning was very lively and active, and prowled about for food and water when liberated from the room in which he was confined; and leaped over a place between three and four feet high, apparently for his amusement. Barks, but very feebly. At half-past 11 A. M. he was fed with three-quarters of a pound of sweet

butter, a small part of which he immediately vomited, and again swallowed. After this he made no efforts to vomit. About half-past 3 P. M. he was killed by a dose of prussic acid, in the presence of Drs Alison and Duncan and Mr Spence, and the abdomen was immediately laid open. After the effects of the prussic acid began to manifest themselves, he vomited some of the butter in a fluid state. *Sectio.* The mesentery presented a beautiful display of innumerable lacteals filled with a white milky fluid; the thoracic duct was also full of chyle. Part of the butter, altered in its appearance, was found in the stomach and small intestines. The mucous membrane of the stomach presented a blush of red in the splenic extremity, and was of its natural thickness and consistence. The nerves were then carefully exposed in the neck, and it was ascertained by measurement, that between the cut ends of the left vagus, when the neck was not stretched, the distance was one inch and one line; and between the cut ends of the right vagus it was one inch and two lines. The distance between the cut ends of one recurrent was one inch, and between those of the nerve of the opposite side it was somewhat less. The cut ends of the *vagi* formed little bulgings, and there was no regeneration (as was to be expected) of the nerves. The lungs crepitated everywhere, and contained very little blood. Some adhesive mucus was found in several of the larger and smaller bronchial tubes. In one portion of the lung, which was about an inch in length and half an inch in breadth, the bronchial tubes appeared full of this adhesive mucus. The sympathetic was dissected in the chest, and, compared with a similar dissection in two dogs, was nearly of the same size. The nerve of the left side appeared somewhat larger than that of the opposite side, and also larger than the corresponding nerves in the other two dogs, but this was not to any great extent. A portion of the mesentery, in which the chyle has been retained in the lacteals, and also the neck of the animal, showing the distance between the cut ends of the nerves, have been preserved.

We shall give some short details of the other three experiments. One of these was performed upon a full-grown middle-sized terrier bitch, and has been already referred to under Experiment X. Both *vagi* and recurrents were divided, and a portion removed. The frequency of the respirations was diminished to one-half immediately after the division of the *vagi*, but they were performed with ease. The nerves were divided on the 22d of June, at 8 o'clock A. M. At 10 A. M. she appeared very dull and languid; and at 4 P. M., when solicited to take food, refused to eat. On the 23d, took some bread and milk, which were soon after vomited. 24th, Took some bread and milk at half-past 10 A. M., which had not been vomited at 4 P. M. Part of it was vomited in the evening, but was scarcely changed. 25th, Took

some animal food at 11 A. M. Part of it was vomited unchanged between 3 and 7 o'clock P. M.; the rest was retained. 27th, Vomited part of the animal food taken yesterday, which was softened on the surface without fœtor. 28th, Is more lively, looks about her when she expects food, and eats with apparent relish. Part of the animal food (liver) taken yesterday had been vomited, and was partially digested,—some of it being reduced to a brownish fluid; other portions were reduced to a pulp on the surface. All of it was without fœtor; and the altered portion permanently reddened litmus paper placed for a short time in contact with it. On the 29th and 30th, and the 1st of July, had very frequent vomiting of food, which she again swallowed only to vomit the greater part of it again. It was evident, however, that more food was swallowed than what was rejected by vomiting. Was obviously becoming weaker, and was very lean. On the 2d July was found dead at 8 A. M. The central parts of the body were still warm, and the abdomen was immediately laid open. *Sectio.*—A few lacteals were observed on the mesentery, some of which were punctured, and a thick white fluid flowed out. The thoracic duct was then exposed, and found full of chyle, and a ligature was placed around it at the upper part of the chest. Another ligature was also placed around it at the lower part of the chest, and the duct punctured below it, when a thick white fluid flowed out. The nerves were then exposed in the neck; and it was ascertained that the distance between the cut ends of the *vagus* on one side was one inch two lines, and on the other side seven-eighths of an inch. The distance between the cut ends of the recurrents was at least one inch. The parts were left in this condition until they had been examined by Drs Alison, Knox, Craigie, Handyside, Duncan, and Mr Fergusson. The mucous membrane of the lungs was pale, and covered by a thin layer of mucus. There was no effusion into the air-cells, and the lungs were everywhere spongy, and contained very little blood. The stomach contained a considerable mass of hair, but no food; and its mucous surface was quite healthy. No softness or redness of cut ends of nerves. The preparation of the neck and the thorax, showing the distance between the cut ends of the nerves, and the thoracic duct full of chyle, is preserved in the University Museum. The subject of another experiment was an old and large pointer, already referred to under Experiment XI. The *vagi* were cut on the 27th June, at half-past 8 A. M. On the 28th he was dull and languid; had no apparent uneasiness; refused to eat, and had a little vomiting. On the 29th, took a little food, which was soon after vomited. On the 30th, was very dull, and vomited the small quantity of food taken. 2d July. Took a quantity of milk, which some time after was vomited in a coagulated state. 3d, Appetite deficient,

and the food taken was in great part again vomited. On the 4th the appetite had considerably improved; but a great part of the food taken was vomited after a longer or shorter time. On the 5th, still vomited a great part of the food taken, and was evidently becoming more feeble. At 3 P. M. the state of his breathing showed that he was evidently beyond recovery; and he was poisoned by a dose of prussic acid. After the acid had begun to produce its effects, he vomited some half-digested bread and milk. *Sectio* —The abdomen was immediately laid open in the presence of Dr Patterson of Leith, and we were shortly after joined by Dr Knox. Several lacteals were seen upon the mesentery full of chyle. Some of these were punctured, and a white thick fluid flowed out. The thoracic duct was now exposed, and found full of chyle. The pneumogastrics were then dissected in the presence of these gentlemen; and it was ascertained by measurement, that the distance between the cut ends of the nerves was seven-eighths of an inch on one side, and one inch and one-eighth on the other, when the neck was in a semi-bent position. The stomach was quite healthy, and slightly red on the inner surface, as is usual when digestion is proceeding. The lungs, particularly the left, were considerably diseased, as we have already described under the head of morbid changes in the lungs. The fourth and last experiment was performed upon a full-grown and large mongrel, (betwen a pointer and terrier,) the subject of Experiment XV. The *vagi* and recurrents were divided on the 19th June, at 3 P. M. The respiration was easy after the section of the nerves. On the 21st, vomited some liver taken the day before, which presented no decided change. Is more lively. At 7 P. M., ate some rabbit's flesh with apparent relish, but vomited it soon after. On the 22d, at 4 P. M., vomited unchanged some rabbit's flesh taken at 8 A. M. Is still languid and dull. On the 22d, coughed pretty frequently, and vomited all, or nearly all, the food taken. 24th, At 10 A. M,, walked across the room to reach some bread and milk, of which he ate a considerable portion. Is generally dull and listless. At 4 P. M. he had vomited part of the bread and milk. 25th, Is more lively; still coughs. Ate some liver at 10 A. M., which was not vomited at 3 P. M. On returning at 6 P. M. I found part of it had been vomited, and this was decidedly converted into a pulp on the surface. 26th, Took a considerable quantity of animal food (sheep intestine) with apparent relish. Some of it was vomited after eight hours, and was partly soft and easily lacerated, and partly reduced to a pulp, without emitting any disagreeable odour. 27th, Still vomits part of his food in various stages of digestion. 29th, Vomited some liver in a pulpy state, not putrid, and reddening permanently litmus paper. Very frequent cough, and discharge of mucus from the nostrils. 30th,

Eats little, and vomits that little. In the evening refused food, and was found dead next morning. *Sectio.*—The distance between the cut ends of one *vagus* was one inch and two-eighths, and between those of the opposite side one inch. The distance between the cut ends of the recurrents was one inch and-a-half. The left lung was very considerably diseased, as we have already described.

We believe that no one who examines the details of the first of the four experiments we have here related, can for a moment doubt that the digestive process was fully re-established before the death of the animal. Though the contents of the lacteals were not chemically examined, every one conversant with the recent history of physiology knows that late experiments have fully proved that the lacteals absorb none of the ingesta in any great quantity, except they have been previously converted into chyme in the stomach. Besides, as the animal was enclosed in a room where nothing vomited could escape detection, I am confident that considerable quantities of food which he had taken into his stomach, during the last few days of his life, had disappeared from the digestive tube. Above all, the animal was undoubtedly gaining rapidly in flesh and in strength; and what is digestion but the name given to those actions by which the ingesta are converted into materials fit for ministering to the nutrition of the body? In the second experiment, besides the evidence afforded by the vomiting of animal food partially digested, and capable of permanently reddening litmus paper, we discovered the presence of chyle in the lacteals after death. I am also equally confident that a considerable quantity of food had been retained in the stomach, notwithstanding the frequency of the vomiting; and this had disappeared from the digestive tube. In the third experiment the milk was vomited in a coagulated state, and chyle was also found in the lacteals and thoracic duct. In the fourth experiment we had also what I cannot regard but as unequivocal evidence of digestion, viz. the vomiting of half-digested food permanently reddening litmus paper, and the disappearance of a considerable quantity of food from the digestive tube. In the experiment of Arnemann, to which we formerly referred, digestion must have been re-established, since the animal lived 165 days. In the experiment mentioned by Sédillot, digestion must at least have been partially established, otherwise the animal could not have lived two months and a half. It is of importance to remark, that we only obtained distinct evidence of digestion in the four animals which lived beyond the fifth day, and that even in these the digestive process was at first completely arrested and only gradually improved. In the other thirteen experiments the animals either refused to take food; or the food when taken was either vomited or remained in the

stomach unchanged. We believe, then, that we are justified in concluding, that a deleterious influence is propagated downwards to the stomach by lesion of the *vagi*, yet that, if the animal live for a certain time, the digestive process may be re-established. Leuret and Lassaigne have detailed an experiment where digestion proceeded immediately after section of both *vagi* with loss of substance. They removed from three to four inches of each pneumogastric in a horse in perfect health, after fasting for four days. *Tracheotomy* was performed. After the operation the animal eat with appetite, and a ligature was then tied around the *œsophagus*. The animal was killed eight hours after eating. The stomach contained about the half of what it had eaten; the rest had passed into the small intestines. That which remained in the stomach was chymified, as analysis proved. The lacteals on the mesentery were distended with chyle, which was found by analysis to consist of the usual ingredients. The thoracic ducts (in the horse there are two) were filled with chyle. M. Dupuy was present, and renounced his previously published opinions, "J'en conviens a-t-il dit je m'etais trompé." * They also state that the same experiment was repeated with the same results. † Paetsch, in an inaugural dissertation, ‡ transcribes part of a letter from Augustus Schultze, then Professor of Physiology at Fryburgh, in which it is stated that he fed two dogs with milk, and in one of these he cut the *vagi* on the *œsophagus*. § Both were killed an hour and a half after. In the dog operated upon, the milk was digested, and the small intestines contained chyme, and the lacteals were full of chyle. The digestion had not advanced so far in the dog not operated upon. He states that he satisfied himself by subsequent dissection that all the filaments of the nerves had been cut. From this single comparative experiment he rashly concludes that section of the *vagi* does not retard, but, on the other hand, accelerates the digestion by the irritation of the intestines. He further states, that he repeated this experiment with the same results, only the digestion had not proceeded so far.

Magendie states, that in experiments in which the *vagi* were cut upon the *œsophagus*, so as to avoid the disturbance of the respiration consequent upon section of the *vagi* in the neck, he found that the digestion was not arrested. Brachet objects to these experiments, that no one can be certain that all the filaments of the *œsophageal plexus* are divided, unless the *œsophagus* be

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* Recherches Physiologiques et Chimiques pour servir à l'Histoire de la Digestion, p. 133–4. Paris, 1825.

† Oper. Cit., p. 135.

‡ Dissertatio Inauguralis Physiologica sistens Experimenta quædam de Nervi Vagi in Digestionem Vi atque Potentia, p. 30. Gottingae, 1822.

§ It is not expressly mentioned whether the œsophagus was at the same time divided or not, but we are led to infer that it was.

at the same time cut across ; and besides, Magendie does not state that he satisfied himself of the division of all the filaments by dissection after death. Brachet affirms, that, on repeating this experiment, and taking the precaution of cutting the *œsophagus* across, he found that perfect digestion was prevented. * Paetsch in some experiments, cut the *œsophagus* across where it perforates the *diaphragm*, and found that the digestion of milk previously introduced into the stomach, was arrested. † That digestion should be arrested for a time after the serious injuries inflicted in performing such experiments, is nothing more than what we should expect ; and the more especially when we remember the instructive experiments of Brachet, where he found that the mere incisions necessary for laying bare the *vagi* in the neck, though the nerves themselves were left uninjured, were sufficient, in some cases, to arrest the digestion of food previously taken into the stomach. Many experimenters, among whom we may enumerate Haller, ‡ Brunn, § De Blainville, ‖ Dumas, ¶ Dupuy, ** Legallois, †† Macdonald, ‡‡ Wilson Philip, §§ and Dr Hastings, ‖‖ have never observed evidence of digestion after lesion of the *vagi* ; but such negative experiments cannot be considered at variance with the positive experiments we have stated ; they only show, what every physiologist who has experimented much on this subject must, I think, be obliged to confess, that the digestive process is generally arrested after section of the *vagi* for the short time the animal usually lives ; but they can never overthrow the results derived from positive experiments, provided these have been accurately performed, and are free from all sources of fallacy. On looking over my notes, I find, that seven of the seventeen experiments were performed before we obtained any evidence of digestion, and that the four experiments in which this was observed followed each other almost in succession ; and this may

* Oper. cit., p. 213–14–15.

† Oper. cit. Sir B. Brodie cut the *vagi* on the *œsophagus* in one experiment, and the digestion was arrested.

‡ Opera Minora, Tom. i. p. 359–60. Experimenta 132–5–6. Haller states that he found the food in the stomach after section of the *vagi* putrid and converted into fæces.

§ De Ligaturis Nervorum, Ludwig, Tom. ii. Scrip. Nerv. Min. Sel., p. 286–7. Experiments 2, 3, and 6.

‖ Propositions extraites d'un Essai sur la respiration, Paris, 1808.

¶ Oper. cit., p. 354.

** Oper. cit. Dupuy believed that animals appeared to die after section of the *vagi* from suspension of digestion. Müller (Physiology, p. 358) represents Dupuy's opinion on this question as very different from this ; but we have only to refer to p. 365 of the Journal containing the memoir of Dupuy, in proof of the correctness of what we have stated above.

†† Oper. cit. p. 214.

‡‡ Dissertatio Experimenta quædam de Ciborum Concoctione Complectens, Edin. 1818.

§§ Oper. cit.

‖‖ Quarterly Journal of Science, &c. p. 40. Vol.xi.

so far explain how some experimenters may be more successful than others, when the number of their experiments is limited. It would appear that digestion is less impeded when the *vagi* are simply divided and their cut ends left in contact, than when they are separated from each other, or when a portion is removed. This interesting fact was elicited by the controversy between Dr Wilson Philip and Mr Broughton, * and appears to be so far confirmed by the experiments of Breschet, Milne Edwards, and Vavasseur. † In our experiments upon dogs, frequent vomiting invariably followed, for a certain time at least, the section of the nerves when food was taken into the stomach. This varied in degree in individual cases. In Experiment XIII., (the first of the four experiments we have detailed above) the vomiting had almost entirely disappeared. In Experiment X. (the second experiment detailed above) the vomiting was very frequent; and we have seen this animal vomit, and again eat up the food vomited, several times within a few minutes. The death of this animal was, we believe, to be attributed to the frequency of the vomiting, for we had evidence that digestion was proceeding at the time of death, and the lungs were quite sound. The vomiting after section of the *vagi* is, we believe, generally occasioned by the presence of food in the stomach exciting the sensation of nausea, and not from any irritation of the upper ends of the cut nerves. For I have repeatedly observed dogs which made no efforts to vomit when the stomach was empty, seized with vomiting immediately or soon after food was taken into the stomach. So readily is vomiting excited in dogs by substances taken into the stomach after section of the *vagi*, that, in injecting various substances, such as prussic acid, laudanum, alcohol, &c., for purposes to which we shall afterwards have occasion to refer, I found it necessary to tie the *œsophagus* with a ligature, to prevent their rejection from the stomach. It has nevertheless been stated that large doses of emetics do not produce their usual effects when injected into the stomach after section of the *vagi*. ‡

Effects of Lesion of the Vagi upon the Sensations of Hunger and Satiety.—Though these sensations are referred to the stomach as their seat, yet it is evident from well-established facts, that the stomach is merely the situation of the impressions made on the expanded extremities of the nerves by which these sensations are excited, and that the sensations themselves are actually connected with the encephalic portion of the nervous system. Are those impressions which excite the sensations of hunger and satiety made upon the expanded gastric filaments of the *vagi*, or

* Quarterly Journal of Science, &c. Vol. xi. p. 320, and Vol. xii. p. 17.
† Arch. Gen. de Med. Tome ii. p. 483.
‡ Brachet, Oper. Cit. p. 187.

upon the filaments of the ganglionic system, or upon both? If they depend entirely upon impressions transmitted to the *medulla oblongata* through the *vagi*, it is obvious, that, after section of the *vagi* and recurrents in the neck, these sensations could no longer be felt; for it is one of the conditions absolutely necessary for the excitation of any particular sensation, that there be integrity of the nerve upon which it is dependent, throughout its whole course between the organ upon which the impression was made, and the central organs of the nervous system. If the sensations of hunger and satiety depend upon impressions made upon the gastric branches of the *vagi*, it is obvious that, when these nerves are divided in the neck, the stomach may be in the most proper condition for receiving impressions, and the *medulla oblongata*, from which the nerve arises, may be in a fit state for the excitation of the sensation; yet it would be all to no purpose as long as the communication through the nerves between these two organs is interrupted. Brachet relates two experiments * to prove that the sensations of hunger and satiety are arrested by section of the *vagi*. From these he believes it is apparent that an animal, after section of the *vagi*, only eats to gratify the sense of taste, and that it continues to eat until the œsophagus and stomach are so much distended that it can swallow no more. The numerous experiments we have made do not by any means confirm these conclusions of Brachet. Though rabbits generally refuse to eat after section of the *vagi*, unless the food be placed under their nose, as in the experiments upon a dog and two Guinea pigs related by Brachet, yet I have seen one of these animals, immediately after the operation, make instantly for food, when this was thrown down in a corner of the room several feet distant from where it was at the time. Few of the dogs we operated on took food for the first twenty-four hours; and after all the coaxing we could practise, this was always in sparing quantities. Of the four dogs which lived beyond the fifth day, three cocked their ears, and looked out for food when we entered the room after they had fasted for several hours; and the quantity of food they took was variable at different times. In fact, we could not observe anything which would lead us to believe that they had lost the sensations of hunger and satiety. Of course it is quite possible that both these sensations might have been impaired by this operation; and it must be exceedingly difficult to ascertain how much an animal, in partaking of food, may be influenced by gratification of the sense of taste, and how much by the sensation of hunger. The mere retention of food in the œsophagus (the only proof adduced by Brachet) is no sufficient evidence

* Oper. cit. Experiment 52–3.

that the food accumulates there from the over distension of the stomach caused by the loss of the sense of satiety; for in the rabbit, horse, sheep, and also occasionally in the dog, * this may arise from paralysis of the œsophagus.

We believe, then, that no sufficient evidence has been advanced to prove that section of the *vagi* annihilates the sensation of hunger and satiety; while, on the contrary, there are strong grounds for believing in their occasional continuance. And in all experiments of this kind, we must carefully separate the accidental from the necessary consequences of the injury of an organ. There are certain peculiarities in the excitation of the sensation of hunger, which are worthy of being kept in remembrance in judging of the effects of lesion of the *vagi* upon this sensation. If the sensation of hunger depend upon certain impressions being made upon the expanded filaments of the gastric nerves, we are naturally led to inquire into the causes of those impressions. In the case of the eye, for example, we find that, in the healthy state of the organ, we can always observe some excitation from the rays of light or other causes, by which the impressions are produced upon the optic nerve, which give rise to the sensation of sight. In the same manner, in the healthy state of the ear or the other external senses, we are familiar with the excitants by which the impressions made on the nerves belonging to those organs of sense, are usually produced. Can we then also point out any physical or chemical change acting upon the expanded filaments of the gastric nerves, which may be supposed to produce the impressions which excite the sensation of hunger? Many physical and chemical explanations of the origin of the sensation of hunger have doubtless been propounded, but they are all most unsatisfactory. It has been, as Magendie observes, by turns attributed to the providence of the vital principle, to friction of the sides of the stomach against each other, to the dragging of the liver upon the diaphragm, to the action of the bile upon the stomach, to acrimony and acidity of the gastric juice, to fatigue of the contracted fibres of the stomach, to compression of the nerves of this viscus, &c. It would be mere waste of time to enter into any details to prove that each and all of the causes here adduced as the excitant of this sensation can have no such effect. We shall merely state, that some of these supposed causes cannot be present to excite this sensation; for example, there is no bile in the stomach in the natural state of that organ; and it is perfectly ascertained, more especially by the late researches of Dr Beaumont, that no gastric juice is secreted, or at least contained in the stomach, during the empty condition of the organ. This sensation,

* Vide an experiment of Baglivi, formerly referred to.

then, is independent, as far as we know, of any physical condition of the stomach itself, or of the presence of any substance in its interior, but arises from certain organic changes in the constitution generally, connected with the necessity for additional supplies of fresh materials from without; but in what particular manner these conditions affect the expanded extremities of the gastric nerves so as to excite the sensation of hunger, is at present a perfect mystery. It may be supposed that, since various substances taken into the stomach allay the sensation of hunger, though they cannot pass into the body to form a part of the nutritious juices,—and since various physical methods have been also found successful in producing the same effect, which cannot alter the conditions of the circulating fluid,—we have sufficient proof that this sensation is entirely connected with some particular state of the filaments of the gastric nerves, and not upon causes developed in the constitution, as we have here supposed. It is, for example, well known, that travellers find relief from the pangs of hunger by swallowing pebbles, or by compressing the abdominal organs by a tight girdle; and that some of the South American tribes swallow balls made of a kind of aluminous clay, when pressed by hunger. Even in an ordinary meal we are perfectly convinced that the sensation of hunger is completely allayed, and *that*, likewise, to the production of satiety, before the food has been in the slightest degree acted upon by the gastric juice. If this were not the case, this feeling of satiety would come too late to admonish us that enough has been taken into the stomach. All these, however, only succeed for a limited time in allaying this sensation; and if fresh chyle be not added to the blood, it will return, and become gradually aggravated in spite of all other attempts to relieve it. That these different methods should succeed for a time does not prove that this sensation is not excited by some organic cause developed in the constitution; for it is perfectly possible that, when the sensation is not in any great intensity, the particular condition of the filaments of the gastric nerves, induced by the organic causes developed in the constitution, whatever these may be, should be removed for the time by the physical effects of the several methods employed for allaying hunger. And this is borne out by the fact, that those succeed for a limited time only. Besides, it is also proved that the reception of food into the stomach, when not followed by the chymification and subsequent chylification of the food, does not even succeed in allaying the sensation of hunger after a time; as is illustrated by the fact, that, in cases of disease of the pylorus, where assimilation does not take place to any great extent, the patient may feel very hungry with a full stomach. The importance of having this sensation allayed, when the wants of the system exciting it are not yet very urgent, by

the physical circumstances which generally lead to the removal of those wants, even before the means for supplying them are actually furnished; and the advantage of the insufficiency of all these methods to remove this sensation, when the wants of the system become very importunate, until the means themselves are actually applied to their removal, need only to be mentioned to be fully perceived. For it is obvious, that, unless the sensation when not urgent was removed in the manner in which it is, it could no longer serve as a valuable guide in admonishing us to finish our repasts; while, on the other hand, when the supply of nutritious juices fairly begins to fail in the system, by refusing to be contented with any thing less than a fresh supply of these, it forces the individual to look out for more nutritious materials.

Effects of Lesion of the Vagi upon the Secretions poured out on the inner surface of the Stomach.—We have, when discussing the effects of lesion of the *vagi* upon digestion, already detailed sufficient facts to show, that removal of a portion of both *vagi* and recurrents does not always arrest the digestive process, and consequently does not necessarily prevent the secretion of the gastric juice. In two of our experiments we ascertained that the half-digested food permanently reddened litmus-paper; and we consider the presence of chyle in the lacteals and thoracic duct, as observed by Leuret and Lassaigne, and in three of our experiments, as furnishing decisive evidence of the secretion of gastric juice. In experiments by Dieckhof and Müller upon the effects of section of the *vagi* in geese, they found the fluid secreted in the stomach to be acid, but less so than in the sound animal. It is further stated, that they never found the acidity absent.* Mayer also found the chyme acid in rabbits after section of the *vagi*. Breschet, M. Edwards, and Vavasseur, Dr Holland,† and Brachet, maintain that in their experiments the gastric juice was secreted, as the food in the stomach was more or less altered;‡ but many of these experiments are liable to certain objections, particularly where the animals were fed before the nerves were divided, for it is then impossible to decide how much gastric juice was secreted before the operation was completed. In the experiments of Breschet and Milne-Edwards, in which they employed mechanical irritation of the lower ends of the nerves, the digestion was too far advanced to be explained by any supposition of this kind; for they state that, in one experiment, the alimentary matter was in a great degree reduced to pulp, the lacteals were full of chyle, and the digestion

* Müller's Physiology, Vol. i. p. 594. Translated by Baly.

† Oper. cit Chapter x.

‡ The experiments of Mr Broughton (Quarterly Journal of Science, &c. Vol. x. p. 292,) are inconclusive, as the nerves were simply divided and the cut ends left in contact.

was as far advanced as when they employed the galvanic pile.* The positive proofs which we have here adduced of the secretion of the gastric juice cannot be invalidated by the negative results obtained by Tiedemann and Gmelin,† in an experiment upon a dog where the substances vomited after section of the *vagi* were found to be alkaline. As we believe that we have furnished adequate proof that secretion of the gastric juice may occur even when no mechanical or chemical irritation is applied to the lower extremities of the nerves, we have not thought it necessary to examine the supposed efficacy of galvanism, in supplying the want of the usual nervous influence transmitted along these nerves. We shall merely remark, that though it appears from the experiments of Wilson Philip, Dr Clarke Abel,‡ and Breschet, Vavasseur, and Milne Edwards,§ that galvanism transmitted along the cut *vagi* nerves to the stomach very much facilitates the digestive process, yet we are led to believe, from the experiments of Brachet, that it sometimes fails to do so, and Müller asserts that, in several experiments performed by himself and Dr Dieckhof upon rabbits, the application of the galvanism in the mode recommended by Dr Wilson Philip had no effect in promoting digestion.‖

Secretion of Mucus.—We have examined the stomachs of many animals at various periods after section of the *vagi*, and we have never seen any thing which would lead us to suppose that the usual mucous secretions are there arrested; on the other hand, we have every reason to believe that they were poured out in the usual quantity, and presented their usual physical properties. When the stomach was empty it was generally found contracted upon itself. The inner surface never presented the inflamed appearance described by Gendrin as the consequence of division of the *vagi*.¶ Sir B. Brodie relates four experiments, in which animals were poisoned with arsenic after section of the *vagi*, where the usual watery and mucous secretions did not take place from the inner surface of the stomach and intestines, though the mucous membrane was inflamed.** In three of these experiments the *vagi* were cut in the neck, and, in the fourth, they were divided on the cardiac orifice of the stomach, to avoid the effects of the operation upon the breathing. In three, the animals were poisoned by introducing ten grains of white oxide of arsenic into a wound in the thigh; in the fourth, arsenious acid was dissolved and injected into the stomach. Sir B. thinks that the obvious conclusion from these experiments is, "that this secretion is pre-

* Oper. cit. Tome vii. p. 195–6. † Oper. cit.
‡ Medical and Physical Journal, Vol. xliii. p. 385.
§ Oper. cit. Tome ii. ‖ Physiology, &c. p. 550.
¶ Histoire Anatomique des Inflammations, Tome i. p. 584. 1826.
** Phil. Transact. 1812, p. 102.

vented in consequence of the nervous influence having been interrupted by the division of the nerves of the eighth pair." * As it appeared improbable that section of the *vagi* should arrest the secretion of the mucus of the stomach and intestines, when it fails to suspend the secretion of the gastric juice, we were anxious to satisfy ourselves on this point. With this view we performed five comparative experiments, selecting two dogs as nearly as possible of equal size for each experiment. In four of these experiments the white oxide of arsenic was introduced into a wound in the thigh, in the quantity employed by Sir B. Brodie, and in the fifth it was introduced into the stomach. In each experiment the nerves were left untouched in one of the animals, while in the other both were divided and a portion removed, before the arsenic was introduced into the system.

Exp. XXXVIII.—Two powerful and large dogs. The *vagi* and one of the recurrents were divided in one of the animals, and ten grains of white oxide of arsenic moistened with water were introduced into a long incision in the thigh, and the wound was then stitched up. The same quantity of arsenic was at the same time introduced into the thigh of the other dog, the *vagi* being left entire. The dog in which the *vagi* had been divided appeared to be little affected three hours after the inoculation of the poison. Six hours after, it had vomiting of frothy mucus, and was very dull. Eleven hours after, it was found lying extended in a state of stupor, with frequent motions of the limbs. It was found dead next morning. The dog in which the *vagi* were left entire was very slightly affected after six hours. After eleven hours it was very dull, but was observant of surrounding objects. Twenty-three hours after, it was very feeble and languid, but still lay with the head erect. Other ten grains of arsenic were introduced into an incision made into the opposite thigh twenty-eight hours after the first inoculation of the arsenic. It was still alive, but very feeble and stupid. It was not seen for twelve hours after this time, when it was found dead and cold.

Sectiones. Dog in which the vagi had been cut.—A quantity of greenish fluid flowed from the mouth, before we proceeded to open the chest. The stomach and a considerable portion of the small intestines were removed after being enclosed in ligatures, and then slit open. *Stomach.*—The stomach contained a considerable quantity of greenish fluid, similar to that which flowed from the mouth. The organ was somewhat distended, presented few *rugæ* on its inner surface, which was slightly tinged green. A thin layer of mucus could be wiped off in several parts. *Intestines.*—A thick layer of brownish mucus covered the whole

* Oper. cit. p. 105.

of the inner surface of a great part of the small intestines. This mucus was in great abundance at the upper part, and at some places flowed from the intestine when held up, or even when one of the edges was somewhat raised. The mucous membrane itself was of a uniform red colour. The blood in the heart was coagulated.

Dog in which the vagi were not divided. Stomach. This organ also contained a greenish fluid (apparently tinged with bile,) certainly not more abundant than in the other dog, but there were more flakes of mucus floating in it. Mucous surface corrugated and red in some parts. *Intestines.* A thick layer of yellowish mucus covered the inner surface of a great portion of the small intestines, and the quantity did not differ decidedly from that observed in the former dog, when the two were compared together. The mucous membrane, however, in this animal was decidedly redder and more thickened, and there were several patches of elevated glands of a bluish colour in the upper part of the small intestines, similar to those found in the ileum in the human species, but more rounded. Some decolorized clots of blood in the *venæ cavæ*.

Both animals had been fasting when the experiment was commenced, and were allowed neither solids nor fluids up to the time of death. The same precaution was adopted in the other four experiments.

Exp. XXXIX.—The previous experiment was repeated upon two small dogs. Three and a-half hours after the introduction of the poison, neither animal seemed to be much affected, but the one in which the nerves had been cut was vomiting a good deal of frothy white mucus, and eight hours after the commencement of the experiment it was found dead. The animal in which the nerves were left untouched was standing at the end of eight hours, but evidently dull, and was vomiting frothy mucus. In four hours more (or twelve hours from the commencement of the experiment) it was found dead.

Sectiones. Dog in which the vagi had been cut. Stomach.—The stomach was contracted, and contained some yellowish fluid with flakes of mucus floating in it; the mucous surface was corrugated and slightly reddened, and there was very little mucus adhering to it; no softening of the mucous membrane. *Intestines.* A considerable portion of the small intestines was removed and slit open. A large quantity of thick yellow mucus lay on the inner surface, and could be partly made to flow out on holding up the edge of the intestine. The mucous membrane itself, particularly at the upper part, was thickened, reddened, and softened, and presented several elevated patches of glands. Much grumous blood in heart and large veins.

Dog in which the vagi were not divided. Stomach.—This

organ was contracted, contained some greenish fluid, and its mucous membrane was more red than in the other dog. *Intestines.*—A very considerable quantity of reddish-brown thick mucus lined the inner surface of the upper portion of the small intestines, certainly presenting no decided difference in quantity from that in the former dog. The mucous membrane was reddened and softened, and the glands were elevated, of a blue colour, and surrounded by a red margin.

The blood was thick and grumous.

Exp. XL.—Two stout dogs. The previous experiment was repeated. The dog whose *vagi* had been cut, was found, twenty-three hours after the introduction of the arsenic, in a state of deep coma, and the respirations few and heaving. When visited the previous evening, it had vomiting and diarrhœa. It died twenty-five hours after the commencement of the experiment. The other animal was dull and languid thirteen hours after the inoculation of the poison, but had not vomited. Twenty-four hours after, it was more dull and languid, had some diarrhœa, but no vomiting. Other ten grains of arsenic were introduced into the same thigh, and near the former incision. At the forty-eighth hour was very languid and feeble; diarrhœa continued, but without vomiting. Other ten grains of arsenic were introduced into the opposite thigh. At the fifty-second hour it was still without coma, though very feeble and languid. Its death was hastened by a blow on the head.

Sectiones. Dog in which the vagi had been divided.—Stomach. The stomach contained a small quantity of a brownish fluid; the mucous surface was corrugated, and of a dull-red colour throughout; only a small quantity of mucus in the stomach. *Intestines.* The whole extent of the small intestines contained much brown fluid mucus, which ran out on slitting them open. Numerous small red patches presented themselves on the mucous surface, particularly at the upper part; while in the intervals between these the membrane was of its natural appearance. Blood thick and grumous, with small soft coagula.

Other dog. Stomach. About twice the quantity of fluid found in the stomach of the former dog was contained in this. The mucous surface was thrown into numerous *rugæ*, was of a deep-red colour, and presented many dark elevated spots evidently from effused blood; some of these were of a greenish colour, somewhat like sloughs. *Intestines.*—The mucus found in the small intestines was not decidedly greater in quantity than in the former, but the mucous membrane was everywhere of a very bright red colour, evidently thickened and inflamed, though the glands were not apparent. The stomach and intestines of a dog killed twenty-four hours after division of the *vagi* were compared with those of the two animals employed in the experiments we have just

detailed, and presented a marked difference in the quantity and physical appearances of the mucus found in the intestines.

We need not detail the other two comparative experiments, as they did not differ in any essential point from those we have related. In Experiment XLI. two drachms of white oxide of arsenic were introduced into the stomach, and the œsophagus tied. The dog in which the *vagi* were divided died in convulsions seven hours and thirty minutes after the commencement of the experiment. The dog in which the *vagi* were left entire was seen alive, though feeble, at the end of eight hours, and was found dead two hours after this. The stomach was more inflamed in the latter than in the former, but there was evidently a greater quantity of effusion in the interior of the small intestines in the former than in the latter, which in both consisted of a whey-coloured fluid with flakes of mucus floating in it. In Experiment XLII. the arsenic was introduced into a wound in the thigh. The dog in which the *vagi* were cut was seen alive at the end of thirteen hours, and was found dead and cold at the end of twenty-four hours. At this time the dog in which the *vagi* were left entire was still alive, but was evidently very feeble, and had diarrhœa. The stomach in both of these animals was of a red colour, and contained some fluid. Much thick brown mucus flowed from the whole tract of the small intestines in the animal in which the *vagi* were not divided, and a thin greyish mucus from the other. The mucous surface of the small intestines was inflamed, especially in the upper part, but considerably more so in the animal in which the *vagi* were left entire, than in the other. In three of these comparative experiments the stomach and intestines were slit open in presence of Dr Alison. The stomach and intestines of the two animals the subject of each experiment, were always examined at the same time, and in this manner were accurately compared with each other. Though the quantity of watery and mucous effusions from the inner surface of the digestive canal was apparently nearly the same in the two animals of the same experiment, and, in fact, presented no very marked difference in any of the ten animals killed in this manner, yet in all the experiments except in Experiment XXXIX. the mucous membrane of the stomach and intestines was more inflamed in the animals in which the *vagi* were not divided, than in those in which they had been previously cut. This, we believe, can be sufficiently accounted for by the circumstance, that the animals in which the *vagi* were divided died generally several hours later than those in which they were left entire.

We cannot pretend to account for the discrepancy between the results obtained in the experiments detailed above and those related by Sir B. Brodie. It is possible, however, that Sir B. had not taken the precaution of securing a free ingress of air into

the lungs in the three experiments in which the nerves were cut in the neck, for it is stated that in the first experiment, the breathing of the animal was labouring, and it died after three hours and a-half. In the third experiment the animal only lived three hours. In our experiments one animal in which the *vagi* had been divided died after the eighth hour, and the others lived considerably longer. It must, however, be mentioned, that in the second experiment related by Sir B. the animal lived nine hours. Even in this case the death might have been hastened by the effects of the section of the nerves upon the muscular movements of the larynx. Whatever be the cause of this discrepancy, it is obvious that the negative evidence obtained by Sir B. Brodie cannot affect the value of the positive experiments we have detailed; and it necessarily follows that the conclusion deduced from it by Sir B. is not universally or even generally applicable; whilst we believe we are on the other hand justified in concluding, that lesion of the *vagi* does not necessarily prevent the usual watery and mucous secretions from the inner surface of the stomach and intestines. From all the experiments we have performed upon the effects of lesion of the *vagi* upon the different secretions poured out upon the inner surfaces of the stomach, we have arrived at the conclusion, that, if the agency of the nervous influence be at all necessary for the elimination of these from the blood, this may be affected by the ganglionic system after a portion of both *vagi* has been removed in the neck. We have also, we believe, adduced sufficient evidence to set aside all the arguments which have been drawn from the effects of lesion of the *vagi* nerves upon the functions of the stomach, by those who maintain that secretion depends upon the agency of nervous influence. Though the experiments we have stated cannot, of course, be adduced in proof of the opinion, that secretion is not necessarily dependent upon nervous influence, since other nervous filaments are distributed in the stomach,—yet they nevertheless completely invalidate some of the arguments which have been strongly insisted on by the supporters of the opposite doctrine.

The varied experiments upon the *vagi* ought to be viewed in two different aspects; for while they show that integrity of those nerves is not a condition absolutely necessary for the performance of secretion in the stomach, they yet prove that the secretions usually poured into the interior of that organ may be modified, in a most important manner, by causes acting through these nerves. We have here two perfectly separate and distinct propositions, which have sometimes not been clearly distinguished from each other. The difference between them may be illustrated in a very simple manner. The movements of a horse are independent of the rider on his back,—in other words, the rider does not furnish

the conditions necessary for the movements of the horse,—but every one knows how much these movements may be influenced by the hand and heel of the rider. Though it would be out of place to enter here at any length upon the theory of secretion, yet we cannot avoid remarking, that the important influence which lesion of the *vagi* exerts upon the secretion of the gastric juice, must prevent us from attaching much importance to the negative experiments of Naveau,* Krimer,† Brachet,‡ and Müller,§ upon the effects of lesion of the renal plexus of nerves upon the secretion of urine from the kidney. When we extend our investigation into the manner in which the function of secretion is performed to the whole range of the vegetable and animal kingdoms,‖ we can, I think, have little difficulty in arriving at the conclusion, that the nerves exercise over secretion "not an uniform and essential, but an occasional and controlling influence."¶

Effects of Lesion of the Vagi upon the rapidity of Absorption from the inner surface of the Stomach.—It has been stated by Dupuy** and Brachet,†† that after the *vagi* are divided, the most active poisons, introduced in much larger doses than usual, produce their effects more slowly, and with less energy. On the other hand, Müller asserts, that in thirty experiments on Mammalia, which M. Wernscheidt performed under his directions, "not the least difference could be perceived in the action of narcotic poisons introduced into the stomach, whether the *nervus vagus* had been divided on both sides or not, provided the animals were of the same species and size."‡‡ I have made several comparative experiments,—the details of which it will be unnecessary to give,—in which I injected prussic acid, alcohol, and laudanum into the stomach, taking care, of course, that the two animals selected for each comparative experiment should be as nearly as possible of the same strength, that equal quantities of the same poisons should be injected into the stomachs of both, and that the œsophagus should be tied by a ligature to prevent the rejection of the contents of the stomach by vomiting. In these experiments I could perceive no decided difference between the rapidity of the action of the poison in the animals in which the *vagi* had been cut, and those in which they were left entire. On the other hand, I have seen animals in which the *vagi* were uninjured recover from doses of narcotic poisons, which proved fatal to other

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* Dissertatio Inauguralis sistens Experimenta quædam circa Urinæ Secretionem, p. 16, 17, 18. Halae, 1818.

† Vide Lund's Physiologische Resultate der vivisectionen neurer Zeit. S. 205.

‡ Oper. cit. Chapitre v. § Physiology translated by Baly, Vol. i. p. 471.

‖ Carpenter's Principles of General and Comparative Physiology, Chap. xi. 1839.

¶ Alison in Vol. ix. of Quarterly Journal of Science, &c. p. 124.

** Oper. cit. p. 366. †† Oper. cit. p. 186.

‡‡ Physiology, p. 246.

animals of equal or even superior strength, in which the nerves had been previously divided. And this is nothing more than what we should expect, when we remember that the effects of the diminished frequency of the respiration are conjoined to those of the narcotic poison.

Cause of Death after Section of the Vagi.—From what we have already said it is obvious, that though the animals in which the *vagi* have been divided generally die from morbid changes in the lungs, yet they occcasionally die from inanition produced by the derangement of the digestive organs. Experiment X. is an example of the latter mode of death. We are of course here supposing that means have been taken to insure the access of a sufficient quantity of air into the lungs; otherwise the animal may die, suffocated from the arrestment of the movements of the muscles attached to the arytenoid cartilages.

From the numerous experiments we have made upon the *nervi vagi* and their branches, we have arrived at the following conclusions.

Pharyngeal Branches.—The pharyngeal branches of the *vagi* are entirely, or almost entirely, motor, and move the muscles of the pharynx and soft palate.

Laryngeal Branches.—The superior laryngeal are almost entirely nerves of sensation, supplying the mucous surface of the larynx, and part of the pharynx, with sensitive filaments. The few motor filaments which they contain are distributed in and move the crico-thyroid muscles. The inferior laryngeal nerves are ramified in and regulate the movements of all the muscles attached to the arytenoid cartilages, viz. the *crico-arytenoidei postici* and *laterales*, the *thyro-arytenoidei*, and *arytenoidei*. The inferior laryngeals also furnish the sensitive filaments to the upper part of the *trachea*, a few to the mucous surface of the pharynx, and still fewer to the larynx. When any irritation is applied to the mucous membrane of the larynx in the healthy state, this does not excite the contraction of the muscles which approximate the arytenoid cartilages, by acting directly upon them through the mucous membrane; but this contraction takes place indirectly, and by a reflex action, in the performance of which the superior laryngeals act as the sensitive or afferent nerves, and the inferior laryngeals as the motor or efferent nerves. It is also probable that those branches of the inferior laryngeal distributed in the muscular fibres of the *trachea* are motor.

Œsophageal Branches.—The œsophageal filaments of the *vagus* are partly afferent and partly efferent nerves. In some animals, as in the rabbit, the section of the *vagi* in the neck is followed by arrestment of the passage of the food along the œsophagus

not from destroying the contractility of the muscular fibres of the œsophagus, but by breaking the continuity of the nervous circle necessary for the accomplishment of all reflex actions.

Cardiac Branches.—Though the movements of the heart may be materially influenced by causes acting through the *vagus*, yet mental emotions and injuries of the central organs of the nervous system affect the heart's action, after the *vagi* and recurrents are divided in the neck.

Pulmonary Branches.—The pulmonary branches of the *vagus* furnish the principal channel by which those impressions made at the lungs, capable of exciting the respiratory muscular movements, are transmitted to the *medulla oblongata*. It also appears that they transmit the impressions which excite the sensations of coughing, &c.; but we are inclined to believe that this may likewise be effected to a certain extent by the ganglionic system. It is probable also that the pulmonary branches contain motor filaments, though we have not been able to obtain any decided evidence of this.

Gastric Branches.—Lesion of the gastric branches does not necessarily arrest the muscular movements of the stomach, or the usual secretions poured out from its inner surface; though each and all of these, but particularly the secretions, may be modified to a very important extent by causes acting through the nervous system.

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