MEMOIRS

ON

RESPIRATION.

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ON

RESPIRATION.

BY

LAZARUS SPALLANZANI.

EDITED,

From the unpublished Manuscripts of the Author,

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PREFACE.

I HAD promised to my friend Spallanzani to give a translation of his *Memoirs on Respiration*, whenever they should be published; and, as the promises of friendship should in no case be violated, I cannot conceive myself absolved by his death from fulfilling an engagement, the intimation of which appeared to afford him much satisfaction. Had, however, this powerful motive been insufficient to induce me to undertake this office, I should, nevertheless, have entered on it, from the desire alone of advancing the knowledge of animal physiology; since I was acquainted with some important discoveries which the professor of Pavia had made on this interesting subject.

I only waited to fulfil this determination, until the relatives or the friends of Spallanzani had printed that part of these Memoirs which had received their last corrections from the hand of their author. But the difficulty of printing a work in Italy, at least without expence to the author or editor, made me dread that this valuable production might never be published. I wrote, therefore, to Nicholas Spallanzani, the brother of my friend, and communicated to him the intention I had conceived, of publishing it in French from the original manuscript. He seized this idea with eagerness; because he knew, from my correspondence with his brother, that I had been made acquainted with the subjects contained in this work:

but a variety of obstacles, which it was impossible either to foresee or prevent, impeded the execution of the design, and would perhaps have caused it to be wholly abandoned, had not Mr. Menu-Wiss, a member of our Society for the Improvement of Arts, actuated by a laudable zeal for the advancement of the arts and sciences, departing for Italy to settle some private affairs, charged himself with the care of transmitting to me this manuscript. I doubt not but every one will join with me in acknowledging the obligation that we owe to him for this important service.

The three following Memoirs are only the commencement of the large work composed by Spallanzani, on the respiration of animals; as may be perceived by the long letter addressed to me by the professor of Pavia, in order to convey some idea of the vast plan he had in view, and to make me acquainted with his principal discoveries. From this letter, as well as from the Memoirs themselves, it is evident that he had finished the greatest part of the experiments which he deemed necessary not only to demonstrate the truth of the doctrines advanced in this work respecting worms, but likewise in relation to all the other classes of animals. It also appeared to me to be his wish, that some intelligent naturalist would take on himself the care of arranging the minutes of his various experiments, observations, and theories, in order to gratify the curiosity of those who should desire a faithful interpretation of nature. I shared this idea with his family, and indicated to them that I would not decline the important and difficult task, although all my leisure was requisite to terminate the different works in which I was then engaged: but I was fully convinced that it would be more beneficial for the true interests of science that I should give to the world the labours of Spallanzani, in preference to my own; and too anxious to enjoy the pure pleasure, which friendship can alone bestow, of adding new laurels to those which will immortalise my illustrious friend, to remain indifferent to the publication of experiments made by him with the design of throwing additional light on the phenomena of respiration.

I have just learned that these valuable manuscripts are to be transmitted to me; and I hope to be able, immediately on their arrival, to arrange and digest them with a view to their speedy publication.

The three Memoirs which I now present to the public bear the stamp of this great observer of nature, and besides furnish an excellent course of experimental logic reduced to example. These Memoirs astonish us by the extent of the knowledge displayed in them, and we conceive that the whole of nature, thus to speak, was continually under his eyes, so that he could apply her general laws to the particular researches in which he was engaged; but they astonish still more particularly, by the immense labour they must have occasioned, by the rigorous accuracy of such a great number of new and remarkable experiments, by the interesting details which are never unnecessarily obtruded on the reader, by the solidity of the direct consequences drawn from these experiments and repeated observations; in short, by his address in interrogating nature. Whether in discovering the point of the object whence proceeded the light that could illuminate it, or in devising and executing experiments which were before deemed impossible, I will not hesitate to assert that Spallanzani is as original and as happy in the present as in any of his former works, which have justly ranked him among the first naturalists of Europe.

Believing that the lively interest which Spallanzani must inspire will be shared with me by all those who pursue the study of natural philosophy, and that a short historical account of his life and writings may be perused with pleasure, I have been induced to compose that which is prefixed to the present volume.

To conclude.—I must observe, that the weights and measures mentioned in these Memoirs are not reduced to those of the Republic, because I was unacquainted with those he employed; but, as I do not despair of ascertaining this circumstance, so essential in a work like the present, I shall not omit hereafter to supply that deficiency.

A SKETCH

OF THE

LIFE AND WRITINGS

OF

SPALLANZANI.

LAZARUS SPALLANZANI was born, the 10th of January, 1729, at Scandiano, in the department of Crostolo, to the north-east of the Appennines, about seven miles from Reggio, and fourteen from Modena. He was the son of John Nicholas Spallanzani, a celebrated advocate, and Lucia Zugliani, a native of Colorni, in the duchy of Parma.

Spallanzani commenced his education in his native country, and confined himself to the study of grammar; the importance of which is not in general sufficiently felt in forming the mind, so as to seize those relations which are adapted to confer on it distinction, or at least happiness.

At the age of fifteen, he went to Reggio, in the duchy of Modena, where he studied the belles-lettres, under the direction of the Jesuits: the Dominicans, attracted by the rapidity of his progress, wished to attach him to their order; but a passion for the acquisition of knowledge led him to visit Bologna, in the university of which city his cousin Laura Bassa, a woman justly celebrated throughout Italy for her genius, her eloquence, and her knowledge of physical and mathematical science, was one of the most illustrious professors. Under the direction of this enlightened guide, he learned to prefer the study of nature to that of her commentators, and to estimate their value by comparing them with the originals they professed to describe: the scholar at once perceived the wisdom of these counsels, and quickly experienced their happy effects. He evinced his gratitude to his instructress, in a Latin dissertation, published in 1765, which was dedicated to Laura Bassa; and in which he recounted the applauses she received at Modena, when entering the hall, where her pupil, on being appointed a professor, defended a thesis, *De lapidibus ab aqua resilientibus*, she opposed it with the graces of an amiable woman, and the wisdom of a profound philosopher.

Spallanzani had not, however, acquired an exclusive taste for philosophy; he perceived, with all great men, that the study of the ancients, and of belles-lettres, was necessary in order to give a precision to our ideas, an accuracy to our expressions, and a connection to our reasoning, without which the happiest thoughts remain altogether unproductive. He studied with great care his vernacular tongue, and perfected himself in a knowledge of the Latin, but he was particularly attached to the Greek and French languages; Homer, Demosthenes, and Saint Basile, were his favourite authors: I have likewise frequently heard him recite many of the verses of Homer and of Virgil, as well as those of Ariosto and of Tasso.

Spallanzani applied to the study of jurisprudence, in conformity to the wishes of a father whom he loved: he was even on the point of being elected doctor of laws, when his countryman Vallisnieri, professor of natural history at Padua, advised him to relinquish this pursuit, promising to obtain the consent of his father, who, being affected with such a proof of obedience to his will, readily consented that his son should henceforth be left at liberty to follow the bent of his own inclinations: from this moment, he devoted himself to the study of mathematics, whilst he continued that of the living and dead languages.

Spallanzani was very soon known, and his country was not slow in rendering homage to his talents. He was chosen, in 1754, professor of logic, mathematics, and Greek, in the university of Reggio, in which situation he continued during six years, dedicating every moment he could spare from the discharge of his public duties to the study of nature. Some discoveries which he made about this period encreased his passion for natural history, which continued to augment with every new success. His observations on the Animalcula Infusoria attracted the attention of Haller and Bonnet; the last of whom pointed out to him that path which he had himself trodden with so much glory, and he announced him to the learned world as the interpreter of nature.

In 1760, Spallanzani was called to the university of Modena. However much his interest was involved in his acceptance of some advantageous offers made to him by the universities of Coimbra, of Parma, and of Cesena, he was nevertheless induced by his patriotism, and his attachment to his family, to dedicate his talents to the immediate service of his own country. Similar motives operated on him some years afterwards to reject proposals made to him by the academy of Petersburgh, and he remained at Modena, until 1768, where he saw a race of celebrated men rising up, who still constitute the glory of Italy. Among these may be reckoned Venturi, professor of natural philosophy at Modena; Belloni, bishop of Carpi; Lucchesini, ambassador from the king of Prussia; and the poet Angelo Mazza, of Parma.

In 1761, Spallanzani addressed three letters to Algarotti, on the celebrated translation of Homer into Italian verse by Salvini, in which he pointed out some very material errors committed by this translator: he charges him in particular with having enfeebled the energy of the Greek language, by too exuberant a diction, and even with sometimes wholly misconceiving the meaning of his author. In order to justify these charges, he enters into the most curious details respecting the etymology of various words, ascertaining their true import, and restoring the original sense of the Greek text; whilst, at the same time, he shows that the Italian language is sufficiently bold and energetic to preserve all the beauties of Homer. With this view, he translated several portions of this great poet, and especially his description of the cestus of Venus. In short, he interested the literati by the comparison, which he instituted, between different passages of the Iliad, and the imitations of them by Virgil and Horace; as we may see in the fourth volume of the works of Algarotti, printed at Venice.

Spallanzani always evinced a taste for travelling; an ardent desire to explore the recesses of nature made him extend the field of his researches, in order to multiply the chances of making new discoveries. In 1762, he made an excursion to the Appennines, where he had the satisfaction of witnessing a new confirmation of the opinion he had adopted respecting the origin of fountains. He observed that they were more or less numerous and abundant, according to the direction of the mountains, their structure, and especially the facility they possessed of converting into water the vapours suspended in the atmosphere, and their disposition to preserve and transmit it through the lateral openings which are constantly below the level of the tops of the adjacent or remote mountains. We find this subject treated in the *Descrizione d'un viaggo montano con osservazioni sull' origine delle fontane, lettere due al Vallisnieri, figlio*, which is inserted in the *Racolta d'opuscoli scientifici*, t. xiv.

During his abode at Modena, Spallanzani published, in 1765, *Saggio di osservazioni microscopiche concernente il systema di Needham e Buffon*, in which he establishes the animality of microscopic animalcula, by the most convincing and ingenious experiments. This work he transmitted to Bonnet, who drew from it the most favourable presages of its author, which he lived to see fully accomplished: from this time, the most intimate friendship subsisted between these two philosophers, which formed the happiness of their lives, and only terminated with their existence.

Spallanzani likewise published, during the same year, an original dissertation *De lapidibus ab aqua resilientibus*, in which he shows by experiments, in opposition to the prevailing opinion on this subject, that the rebounding of a stone, when flung obliquely upon the surface of water, is not to be attributed to the elasticity of this fluid, but to a necessary change in the line of its direction, produced by the cavity which it forms by coming into contact with the water; and he accounts for the repetition of the rebounds, as long as the original projectile force continues, from the smallness of the angle of projection, which is occasioned by each successive cavity. The explanation of this phænomenon must appear the more ingenious, since any former attempts, even supposing them adequate to demonstrate the existence of elasticity in the water, were only calculated to show that it possessed this property in a very trifling degree.

In 1768, Spallanzani prepared the philosophical world to expect some surprising discoveries by the publication of his *Prodromo di un opera da imprimersi sopra le riproduzioni animali*, wherein he gives the outline of a work on animal reproductions, with which he was then engaged. This little piece contains more luminous views than any former work on this important subject, as it not only points out the proper method to be pursued in conducting this obscure investigation, but likewise combines several curious facts; the pre-existence of tadpoles to fecundation in several species of toads and frogs: the reproduction of the head, in land-snails, after it had been amputated, which he communicated to Bonnet in 1766, and which was disputed, notwithstanding the repeated confirmation of this phænomenon by Bonnet, Herissant, Lavoisier, and myself, he afterwards demonstrates anew in the *Memorie della Societa Italiana*.

In short, we learn, from this publication, that the fresh-water worm is reproduced like the polypus; that toads recover their paws; and salamanders not only their paws, but likewise the tail and jaw-bones, after repeated amputations. It is true, Prisciani has very justly observed that the brain of the land-snail is not situated in the part which was amputated, and which he has denominated the head; but the experiment is not the less wonderful, since it exhibits the regeneration of the eyes, the mouth, the tongue, and the teeth.

These facts continue even at present to be regarded with astonishment, although we have had time to be familiarised with them; and we are at a loss whether most to admire the decisive proofs exhibited by Spallanzani of their existence, or the enthusiastic boldness which prompted him to the investigation. Such reflections must always be accompanied with regret, that the project of his large work on this subject had been abandoned; but different circumstances prevented him acquiescing with the solicitations of his friends to fulfil this intention. I rather suspect that he despaired of rendering every part of so intricate a subject sufficiently clear, and that he conceived it prudent still farther to mature his ideas by reflection and more extended experiments, but was prevented from fully pursuing his inclinations by the multiplicity of his other avocations. Accustomed to the greatest accuracy in his researches, he was anxious on every occasion to exhibit nature altogether without disguise, and to remove every part of the veil which might conceal her from the sight.

Having attentively studied the physiology of Haller, Spallanzani, from this circumstance, was induced to investigate the circulation of the blood, in which he observed several remarkable phænomena. An account of his experiments on this subject was published, in 1768, in a small work, *Dell' azione del cuore ne' vasi sanguigni nuove osservazioni*, which was reprinted with three additional dissertations: *De fenomeni della circolazione osservata nel giro universali de vasi*;—*De fenomeni della circolazione languente*;—*De moti del sangue independenti dell' azione del cuore et del pulzare delle arterie**. In this treatise, he establishes the power of the heart on the arteries, and the relative velocity of the blood in the different vessels, by processes which had not been hitherto employed. He evinced much ingenuity by the employment of reflected light, in tracing the course of the blood from the heart to the extremities of the vessels, which he found equally useful in conducting his difficult experiments on cold-blooded animals; and

* A French translation of these memoirs, by professor Tourdes, appeared at Paris in the year 8; and, shortly afterwards, the same work was published in this country, translated into English by Dr. R. Hall, of London.

particularly on salamanders, of which the transparency and the red blood permitted him to trace all the ramifications of the vessels. He afterwards repeated these experiments on the *chick in ovo*, and was thus enabled to observe many facts which had escaped the most celebrated physiologists, and even Haller himself: he likewise confirmed the observation of the latter, that the heart does not entirely empty itself during the systole. He observed, afterwards, that the momentum of the blood depends entirely on the action of the heart: he remarked the causes which retard the circulation; such as the obstacles produced by the gravity of the blood, the changes occasioned in the motion of this fluid by wounds, or ruptures of the vessels; and he proves that there exists a real dilatation in the arteries resulting from the impulse of the blood, which, being retarded by the cavities of the heart, strike laterally against their sides.

The active mind of Spallanzani continually presented to him important subjects for investigation, which he had not himself leisure to pursue, but which he recommended to the attention of his learned friends: among these, he suggested to them some interesting experiments on mules, in a work entitled *Invito a intreprendere sperienze, onde avere muletti n'el popolo degli insetti per tentar de sciogliere il gran problema de la generazione*, 8vo. *Modena*, 1768*.

When the empress Maria Theresa had re-established the university of Pavia on a more extensive plan, she wished to render it at once celebrated by the attainments of its professors: with this view, she empowered count Firmian to invite Spallanzani to give lectures on natural history;—

* No one, it is to be lamented, has availed himself of the plan suggested by Spallanzani, to enter into the investigation of this subject; though its prosecution would certainly tend to elucidate one of those functions with which we are least acquainted, the fecundation of animated beings. T.

a distinction which, though solicited by several learned men, she bestowed on him, in consequence of his great reputation, and which he certainly merited by his success, and the crowd of students who flocked to his lectures. The ideas of great men being more clear, more enlarged, and better connected, render them in a peculiar manner suited to fill the office of instructors. Spallanzani united, to the most extensive knowledge, a cultivated genius; a simple and exact method, built on solid and well-defined principles: his enthusiastic love of truth made him scrutinise, with the most anxious solicitude, the prevailing theories, appreciate their merit, and discover their vulnerable parts: the important art he had acquired, of making nature her own interpreter, gave to his instructions a clearness, which rendered luminous every thing that could be enlightened, which penetrated even sometimes the obscurity it was impossible wholly to dissipate, and which distinguished the proper measures to be pursued in order ultimately to succeed in the discovery of truth. He led, as it were, his students by the hand; he continued his instructions until every difficulty beginning to disappear, afforded them the prospect of a termination to their labours, and thus procured for them the pleasure of a discovery. A simple and lively eloquence animated his discourses; the purity and elegance of his elocution captivated his hearers; in short, he always possessed the means of rendering his lectures instructive: they were composed a year previous to their delivery, and rendered every day more valuable by the new observations and extensive views which reflection continually suggested to his mind. His learned colleagues became his pupils, in order to improve themselves in what they already knew, and to learn what perhaps they might otherwise have never known.

On his arrival at the university of Pavia, Spallanzani took for his text-book "The Contemplations of Nature, by Charles

Bonnet:" he supplied the deficiencies, developed the philosophical ideas, and confirmed, by his own experiments, the theories contained in the work of his friend; conceiving, justly, that a book which had inspired his own mind with a taste for natural history, was the most proper to create the same ardour in the mind of his disciples. With this view, he translated it into Italian, enriching it with notes, and a preface; in which he pointed out those parts of the vegetable and animal economy most deserving the attention of his pupils, as well as the means by which they themselves might succeed in these researches. It was thus that he devoted himself at first to the noble employment of instructing his fellow-countrymen, and that he became the model of those who would wish to become useful instructors.

The friendship that subsisted between Spallanzani and Charles Bonnet, considerably influenced his genius and pursuits. He imitated the critical exactness of the philosopher of Geneva, and adopted the principles of his philosophy; as well as the method employed by him in the investigation of truth. He was principally struck with the enlarged ideas of Bonnet respecting the generation of organised beings; and he explored nature, during a long period, for proofs of the existence of germs previously to fecundation, which he was at last enabled to demonstrate. Spallanzani gloried in being the disciple of Bonnet; he meditated continually on the valuable works published by this great man, and he constantly consulted him in all his undertakings. He published, in 1776, the first two volumes of his *Opusculi di fisica animale e' vegetabile*, which form the exposition of a part of the microscopic observations which he had already published.

The art of observing, if it is of all arts the most difficult, is at the same time the most necessary. It implies the greatest portion of intelligence and mental resources in the observer. Hence, though every man thinks himself more or less skilled in it—because by all men it is, daily, more or less, exercised—yet none, except great men, practise it in a distinguished manner. Genius alone fixes its attention, with any useful effect, upon the objects which may be presented to it: alone it directs the organs of sense, with a certain aim, towards the obscurities which require to be dissipated; alone it watches over them, to prevent their errors, and animates them to the close pursuit of what they have descried: it removes those evils which conceal the object of research; it supports that patience which awaits the moment of observation amidst multiplying obstacles: in a word, it is genius which concentrates the attention upon a particular object, and communicates to it that energy to imagine, that sagacity to discover, that accuracy to perceive, without which, though truth should not altogether escape our view, we can only see a part of it. Nor is this all: when we have read nature with precision, we must interpret her with fidelity; analyse, by means of the understanding, the phenomena anatomised by the senses; attend to the species, while studying the individual; and anticipate general propositions, in considering insulated facts. In this case, prudence and circumspection are not always securities against error, did not an ardent love of truth assay observations with their consequences, and reduce to scoria every thing that is not the truth.

Such was Spallanzani in all his researches; such have we first seen him in his *Opusculi.* Occupied with the grand phenomenon of generation, he examined the opinion of Needham to demonstrate its impossibility. The latter, displeased with the microscopic observations of Spallanzani, which weakened the vegetative force, imagined for the purpose of putting matter in motion and animalising it, challenged the professor of Reggio to re-observe the phenomena which he had published; but Spallanzani proved to him, so as to leave him no reply, by a series of new facts, that, in general, what has been once well observed may always be seen again; but that we never can re-discover what we content ourselves with imagining.

I shall not remark upon the rigid logic, and the amiable politeness, displayed by Spallanzani in his refutation; the art with which he demonstrates to Needham the cause of his error, by shewing him, that infusions of animal and vegetable substances exposed to an intense heat in vessels hermetically sealed produce no moving being; so that the animalcula observed by Needham, in his infusions, were not the products of those substances themselves, but had been conveyed thither by the air which contained their germs. In this excellent work we always learn, that the animalcula of infusion have their germs like other animals; and that there are some of them which, like some eggs and seeds, resist the heat of boiling water. Here he takes occasion to treat of the influence of cold on animals; and he proves that the lethargic torpor of some of them, during the winter, does not at all depend upon its influence on the blood, since a frog deprived of its blood becomes lethargic when it is chilled in ice, and swims as it did before on the restoration of warmth. He shews that odours, various liquors, a vacuum, act on the animalcula as on other animals; and that they are, in like manner, oviparous, vivaparous, and hermaphrodites. While we thus traverse these remote regions of nature, with this illustrious traveller, we always meet with new facts, profound remarks, interesting details, curious anecdotes; in short, the universal history of those beings which are the most numerous of our globe, though scarcely imagined to exist; and though their organisation is, in several respects, very different from that of known animals, whilst they resemble them in all the grand characteristics of animality; such as nutrition, the necessity of a constant supply of air, and the power of reproduction.

The second volume of this work contains a new journey into more unknown regions. They had been already painted with a sublime pencil, but the picture was not drawn after nature. Spallanzani here gives a history of spermatic animalcula, which their eloquent historian always confounds with the animalcula of infusions. Here we admire the modest distrust of this sage observer, almost always struggling against the testimony of his senses and the authority of Buffon; and he seems to admit but with repugnance the results of those observations multiplied and varied in a thousand ways, which lay open the frail supports of the system of organic moleculæ.

Spallanzani then describes the *volvox* and the *tardigradus*, those colossi of the microscopic world, so singular from their figure and organisation, but still more singular from their faculty of resuming life after a suspension of all its functions for several years. It is this phenomenon which he principally considers, in order to ascertain its limits and conditions; to investigate its causes, and to connect them with others of an analogous nature.

I shall not, in this place, speak of the experiments of Spallanzani on the death of animals in air-tight vessels; because he resumed, extended, and elucidated them, by the lights of the new chemistry: but he closes this collection with a history of *moulds*, the seeds of which he shows floating in the atmosphere; and he remarks that these microscopic mushrooms are distinguished from other plants by their tendency to grow in all directions, without being subject to the almost universal law of the perpendicularity of the stems to the ground.

Spallanzani was appointed to superintend the cabinet of natural history of the university of Pavia; but he found himself almost the titulary depositary of a treasure which had no existence: he laid its foundation, and to him it owed its commencement, its augmentation, its arrangement, and its celebrity. He enriched it by repeated journeys by sea and land, in Europe and Asia, across the Appenines, the Alps, and the Krapacks, to the bottom of mines, on the remains of volcanoes, and to the mouths of craters. Supported by curiosity in the midst of perils, he retained the coolness of the philosopher in order to contemplate those wonders, and the eye of the observer to study them. Thus did he every-where distinguish the objects adapted to the perfecting of science by facilitating instruction. Thus did he complete that vast deposit of riches, which all the gold in the world could not have collected, because gold cannot supply the place of the genius and the discernment of the enlightened naturalist.

In 1779, Spallanzani made the tour of Switzerland and the Grisons. He at that time visited Geneva, where he stopped a month with his friends, who admired his conversation as they had his writings. I have seen him enjoy the pleasure of interesting Trembley, Bonnet, and Desaussure: his soul went forth to meet those of these great men. He took a pleasure in unfolding to them his grand conceptions, and he warmed as he reflected on the grand views to which they gave rise. A report of those amiable conversations would contribute to the honour of those who took a part in them, and to the instruction of posterity; but it will always be of importance to know, that men of exalted genius can relish the sweets of friendship, and find their happiness inthe effusions of the heart, as well as in the discovery of the mysteries of nature.

Spallanzani returned to Pavia, and, in 1783, published two new volumes of his *Dissertazioni di fisica animale e' vegetabile*, in which he reveals the secrets of nature as to two very obscure phenomena of the vegetable and animal economy.

Some experiments made by Spallanzani upon digestion, for the purposes of his lectures, induced him to study this obscure process. He repeated Reaumur's experiments on gallinaceous birds; and he observed that, in this case, trituration is an aid, without being the means, of digestion. He found that the gizzard of those animals, which pulverises walnuts and filberts, and even lancets and needles, does not digest the pulverised matter; that it must undergo a new preparation in the stomach, in order to form the alimentary pulp which contains the elements of the blood and of all the humours. He evinces that digestion is effected in the stomach of a multitude of different animals, insects excepted, by the action of a juice which dissolves the aliment; and, to render this demonstration more striking, he had the courage to make experiments on himself which might have proved fatal to him, and the address to complete his proofs by artificial digestions executed on his table in glass vessels, wherein he mixed the aliments with the gastric juice of animals, which he knew how to extract from their stomachs. But this book, so original from the multiplicity of the experiments and observations which it contains, is still more deserving of attention from the philosophic spirit which dictated it.

This subject was one of the most difficult in physiology. The enquirer is compelled to explore his way in the dark; to economise the resources of the animal, in order to avoid the derangement of his operations; and, when he has laboriously completed his experiments, it is farther necessary that he should distinguish the false conclusions which may be sometimes drawn from them, from those of direct observation, which never deceive when they are immediate. In this work Spallanzani forms a truly delightful spectacle—scrupulously analysing facts, in order to discover their causes with certainty; comparing the nature of his experiments, in order to judge of them with accuracy; inventing happy resources, in order to surmount renovated obstacles; seizing upon what is most essential in his observations, in order to reject useless discussions; estimating their solidity by the augmentation or diminution of the supposed causes; conceiving decisive experiments; always drawing rigid conclusions; repelling the most plausible hypotheses; demonstrating the errors of his predecessors; and employing analogy with that philosophic circumspection which inspires confidence in an instrument at once so useful and so dangerous. Spallanzani may be said to have had a peculiar method of discovering the truth. Whilst the greater part of observers scarcely reach her but by a circuitous course, he rushes forward in a straight line, and seizes her so completely as to leave no possibility of her escape.

This work gave offence to John Hunter. I know not the cause of his displeasure; but, in 1786, he published his "Observations on certain Points of the Animal Economy;" in which he discharges some piercing shafts against Spallanzani, who avenged himself by publishing the work in Italian, and addressing to Caldani, in 1788, *Una lettera apologetica in riposta alle osservazioni del signor Giovanni Hunter*; in which he repels, in a tone of moderation, but with an irresistible strength of reasoning, the affected disdain of the English physiologist, and demonstrates his errors so as to leave him no hope of a reply. However this may be, the criticisms upon this work are no longer known: it has thrown into oblivion all the theories invented to explain the phenomena of digestion; and Spallanzani has opened the only path by which our information can be extended relative to the mysteries of nutrition.

The second volume of the *Opusculi* treats of the generation of animals and plants. Spallanzani in this work proves, by experiments no less solid than surprising, the pre-existence of the germs to the act of fecundation. He demonstrates the existence of tadpoles, prior to their fecundation, in five different species of frogs, toads, and salamanders. He showed their amnions and their umbilical cords before copulation. He discovered that the fecundation of tadpoles takes place out of the body of the female as she lays her eggs, which constitute the actual fœtus, and which are fecundated only by the aspersion of the seminal fluid of the male at the moment of exclusion. He relates the success of artificial fecundations practised upon those species of tadpoles, and even upon a quadruped. In a word, he shows the seed existing in the blossom before the emission of its farina; and by a subtle anatomy, of which it is difficult to form an idea, he exhibits, in the flower of the *spartium junceum*, the pod and its seeds, with their lobes and their infant shoots. He traces them in their evolutions before and after their fecundation, and leaves no manner of doubt that the seed and its teguments existed long before the blowing of the buds, and, consequently, long before they could have been fecundated. He repeated his observations on different kinds of plants, and with the same results; and he raised individual plants with female flowers, which yielded productive seeds, though they were rigorously preserved even from the suspicion of any communication with the farina of male flowers. Such is the series of surprising phenomena which Spallanzani added to the known history of nature. Seraphina Volta denied that these experiments had been really made, and he published his memoir among the Transactions of the academy of Mantua. Spallanzani replied to him in the most convincing manner, but with a degree of acrimony which, had that been possible, would have diminished the force of his arguments, in his *Lettera ad un amico di Mantora*, 8 *vo. Pavia*.

I shall leave the mind to repose, for a moment, on those beautiful discoveries, in order to occupy myself more particularly with the great man to whom we owe them.

Spallanzani, according to his custom, took advantage of the academic holidays of 1751 to undertake a journey, of which the augmentation of the cabinet at Pavia was the principal object. He set out, in the month of July, for Marseilles, where he commenced a natural history of the sea, which had already presented a multitude of facts new and curious, with respect to several kinds of the inhabitants of the waves. He also went to Finale and Genoa, whence he passed to Massa and Carara, to observe those marble quarries so celebrated among statuaries. He returned to Spezzia, and carried back to Pavia an immense collection of crustaceous and testaceous fishes, and deposited them in that cabinet, of which his journeys had already made him worthy of being the guardian, from the treasures with which he had enriched it. With the same views and the same success, he visited the coasts of Istria in 1782, and the Appennine mountains in 1785; where he observed those terrible tempests and that singular vapour which have rendered that year so famous in meteorology: the cabinet of Pavia thus found its treasures yearly augmenting, and it daily became more and more the object of admiration to enlightened travellers; but a still greater object of astonishment was the activity and labour with which Spallanzani had collected all its materials. It would scarcely be believed, that it is amidst those monuments of the ardour of this great man, that endeavours are made to find subjects for diminishing his reputation; that some love to suspect errors in his nomenclature, and take delight in exaggerating them; though, even admitting this to be true, there is not one of the articles in that cabinet which does not, to the utter despair of envy, declare—it was Spallanzani who placed me here. The emperor Joseph II. thought very differently when he visited Lombardy. He, at once, courted the conversation of the creator of this celebrated cabinet, and testified his approbation of it by giving him his medal in gold.

The travels of this naturalist still furnished him with discoveries which he sometimes communicated to the public. Of this description is a letter, inserted in the fourth volume of the *Opuscoli scelti* of Milan, in which he points out some new relations between the electrical fluid and that which occasions the shock of the torpedo; he at the same time shews that the magnet has no influence upon this fish. Such also are the *Lettere due relative a diverse produzioni marine et diversi oggetti fossili e montani al signor Carlo Bonnet*;—t. ii. *De memorie della societa Italiana di Verona.* In these letters he discusses the cause of the nocturnal light which is seen on the sea. He attributes it to the phosphorescence of animals swimming on the surface; he investigates the organ of its production, and the duration of its brilliancy; he studies the multiplication of those animals, &c. He also gives a description of several zoophytes, and a curious explanation of the phenomena which they present; and he gives an account of several new animals in the class of tubularia.

The university of Pavia, in 1785, offered Spallanzani the professorship of natural history, vacant by the death of Anthony Vallisnieri, with more considerable emoluments than he had at Pavia; but the archduke doubled his salary, and permitted him to accompany, to Constantinople, chevalier Zuliani, who had just been appointed envoy from the republic of Venice.

He departed from this city on the 21st of August. During his voyage, he made a great number of observations upon the marine productions that he found in these latitudes, as well as on the meteorological occurrences of each day; among which he had the good fortune to observe a waterspout: he has justly remarked that it neither raised nor absorbed the waters of the sea, but was merely a current of air confined in a vaporous tunnel, produced by several' opposite winds rushing violently against each other, and thus communicating to the cloud a kind of whirling motion. This canal appears to be formed by a whirlwind forcibly penetrating into the body of the cloud; which dilates in its course the lower part of it, and, falling on the surface of the sea, dashes back its waters, in the same manner as it sweeps the earth on which it falls when formed over the land.

In the course of his voyage, Spallanzani touched at several different islands of the Archipelago: he landed at Troy, for the purpose of visiting the places celebrated in the verses of Homer; and, in treading that soil so renowned among the ancients, he made several original geological observations.

We may anticipate the interest with which the voyage of Spallanzani to Constantinople will be received, from some memoirs which have already appeared respecting the torpedo, on different marine productions, and on the island of Cythera, where he discovered a mountain composed of different species of fossils, among which there were several human bones. I am fully aware that this fact has not only been called in question, but represented as a gross error in this distinguished naturalist. I shall be cautious, however, either in affirming or denying this fact; but when I see a celebrated anatomist, a man who has made so many important discoveries, who was never reproached with one physical error, and who always displayed such uncommon circumspection in all his researches, I can scarcely believe he could be so grossly deceived; particularly when we reflect that he possessed a profound knowledge of the bones of men and animals, and that he would not risk an opinion he had not sufficiently examined, respecting an object that could add nothing to his reputation, and which could throw no light on the geological subject, with which he was only recently occupied. In short, when we are informed that he could distinguish finger-bones, fragments of the radius and of the tibia, and that a physician of the island assured him he had seen amongst them a portion of the inferior jaw-bone and a human scull, I ought at least to suspend my opinion until the facts related by Spallanzani are either invalidated or confirmed by new observations.

Spallanzani arrived in Constanstinople on the 11th of October, where he remained eleven months: his situation in this abode of ignorance and superstition would have proved extremely disagreeable, but for the study of nature and the company of his friend Zuliani. The physical and moral condition of this country attracted his attention: he traversed the borders of the two seas, ascended the neighbouring mountains, visited the isles of Calki and Principi; in the former of which he discovered a mine of copper, and in the latter a mine of iron, of the existence of which the Turks were completely ignorant. He revisited Europe, laden with the spoils of the east, collected from the three kingdoms of nature in these regions, after having conferred an essential benefit on a people incapable of appreciating his merit, or rather of imagining that he could have done so. He departed on the 16th of August, 1786.

A voyage by sea was in every respect the most certain and commodious, but Spallanzani despised the dangers and inconveniences of the great roads: when he hoped to meet with new instruction, he courageously braved all the perils of these desert regions without police and without security. Arrived at Bucharest, he was detained by the celebrated and unfortunate Mauroceni, hospodar of Wallachia: this prince, who was a lover of the sciences, received him with the greatest distinction; he presented him with several of the curiosities of his country; and, at his departure, furnished him with horses, and ordered an escort of thirty soldiers to accompany him to the boundary of his dominions. Spallanzani passed through Hermandstadt, in Transylvania, and arrived at Vienna on the 7th of September, after having visited the numerous mines of Transylvania, Hungary, and Germany, which were situated in the vicinity of his route.

Spallanzani remained five days in the capital of Austria: he had two long audiences of the emperor Joseph the Second, was received in the most distinguished manner by the great lords of the court, and visited by men of letters. On his return to Pavia, the students, in great numbers, met him without the city; and, crowding around him, testified, by repeated plaudits, their joy at his return. He accompanied them to his former lecture-room, where he was surrounded by a great number of persons of every description, who evinced the pleasure they had experienced from his instructions, by their anxiety to have them resumed: they forced him into the chair where he was accustomed to lecture. Spallanzani, affected by this scene, painted in glowing colours his gratitude and his attachment; the vows, the cries, the clapping of hands, prevented him from being heard, and obliged him to conclude. He was attended, during the whole of this year, by five hundred students.

Spallanzani had acquired sufficient glory to become an object for the shafts of Envy; but his discoveries were too recent, too original, and too solid, to be disputed; she was, therefore, not only obliged to conceal her hatred, but witness in silence the encreasing reputation of this great man. She found, however, the moment to prove she had not forgotten him: he was charged with want of fidelity in the administration of the cabinet of Pavia, which he had formed; but the arrows thus levelled at his reputation only furnished an occasion of rendering his merit more conspicuous before their tribunals.—But I forbear.—Spallanzani had the magnanimity to forget an event which lacerated his heart: the greatest part of his enemies recanted their error, abjured their hatred, and despaired not of regaining his friendship.

He published some letters, in different journals; one of them respecting a pretended animal asserted to have been seen alive, which proved to be only the trachea of a bird. He likewise entered into a correspondence with Thouvenel, which was inserted in Brugnatelli's Journal, respecting the divining-rod of Pennet; of the power of which he seems to have entertained a momentary belief, although he afterwards discarded it as a chimera.

The cabinet of Pavia was always a favourite object with Spallanzani; who, amidst the numerous curiosities he had deposited in it, thought only of those which were still wanting. Struck with observing some specimens of volcanic substances, which were without arrangement, without interest, and wholly useless for the purpose of instruction, although Italy was the theatre where volcanic fires had for a long series of ages displayed their desolating influence, he took a resolution worthy of his talents, his courage, and his zeal; he wished to instruct his pupils, his countrymen, and himself, respecting these important phenomena so little known, he was anxious to collect the documents of their history on the spot where they are always beheld with terror, and where they have been almost uselessly the subject of philosophical observation; he prepared himself for this great enterprise, by attentively studying every thing that had been observed or written on this copious subject.

He set out for Naples, in the summer of 1788, and had scarcely arrived in that city when a great eruption of Vesuvius occurred; and seemed, as it were, to invite him to observe its phenomena: he ascended this mountain still trembling, considered its crater, examined the yet fluid lava, and endeavoured to ascertain its heat; he examined those of the Solfatara, the mephites of the Grotto del Cane, the lake of Agnano, Misena, and the isles of Ischia and Procida; he embarked for the isles of Lipari, investigated the nature of their volcanic substances with the accuracy of a naturalist who anatomises a butterfly, and the intrepidity of a soldier who braves with coolness the most imminent dangers.

It was then that, like William Deluc, he had the boldness to walk over the smoking sulphureous crust, which covers the centre of the burning mountain of Vulcano. He passed thence into Sicily, where he ascended mount Ætna and walked along the side of its immense crater: but his curiosity was not yet satisfied, he longed to collect around him all the remarkable productions which that island contains: he attentively examined the stones which compose its mountains; he discovered some new species of marine animals; and by traversing, in a small boat, the strait of Messina, ascertained the cause of the violent agitation of the sea between Scylla and Charybdis, rendered celebrated by so many shipwrecks, and by having furnished a theme for the most sublime of poets. It was thus that, at the age of sixty, Spallanzani collected those materials which compose his Travels into the two Sicilies; and in which he compares what he himself observed with the descriptions given by Homer, Pindar, Virgil, Diodorus Siculus, and Strabo, of these renowned regions.

We are presented, in the Travels of Spallanzani, with a new volcanology: he therein teaches us to estimate the intensity of the heat in volcanic fires; to take a view of the causes by which they are produced; to determine, by means of an analysis of different lavas, the particular gas which, like a powerful lever, raises from the bowels of the earth to the summit of mount Ætna those torrents of fluid matters which it disgorges: he likewise points out the mode of ascertaining the nature of pumice-stones, which he himself fully demonstrated after his return to Pavia. I must here stop, in order to notice the fires of Barigazzo and various other places, some of which are entirely subterranean; but which, he renders probable, proceed from, and are supported by, the agency of carbonated hydrogen gas. Neither should I forget to mention that he rendered these fires useful in the manufacture of lime, and that they are still employed with advantage for this purpose. He was much astonished to learn, some years after the publication of his Travels, that Kœmpfer, in his *Amœnitates exoticæ*, describes similar fires, which he saw at Baku in Persia, and which were appropriated to the same purpose. Spallanzani terminates this valuable work by some interesting researches respecting swallows: he points out their amiable habitudes, the rapidity of their flight, the use which might be made of them as aërial messengers; he shows that their migrations are determined by the temperature of the air, and the production of those insects which require a warm atmosphere. He likewise examines whether they become torpid during winter, and proves that these birds are not rendered lethargic by a much greater degree of artificial cold than prevails in our climate. He makes known to us a species, or variety, of owl hitherto ill described. In short, he enters into an investigation respecting the nature of eels and the mode of their propagation, and closely pursues this intricate subject, until he approximates towards a complete solution of the difficulty with which it is attended. We shall pass over a small number of observations, on different subjects, which the academic avocations of the learned professor forced him to relinquish to the pursuit of others. Spallanzani attended, with the most lively interest, to the progress of the new chemistry, and was not slow in adopting it: this system was the offspring of a mind which, like his own, relied altogether on facts and observation. The solidity of its principles, the precision of its methods, the clearness of its explanations, and the generality of its results, very soon engaged him to embrace it in preference to the old system, which consisted merely of detached facts, without any concatenation; and he anticipated, with satisfaction, the triumphs it would quickly obtain. I recollect that, when Gottling published his experiments on the combustion of phosphorus in azot, I communicated to Spallanzani an account of their results, and my doubts of their accuracy: surprised at this unexpected attack on one of his favourite opinions, he resolved to try the temper of the weapons which had been levelled against them; and he published, in the year 5, his *Chemico esame degli esperimenti del signor Gottling proffessore a Jena*, which he dedicated to me, and in which he completely invalidates the conclusions of this chemist, by overthrowing the facts on which they were founded. He clearly established that the brilliancy of phosphoric light is always in proportion to the quantity of oxygen gas mixed with the azot; that agitation and change of vessel do not constantly revive the flame of phosphorus; that, after the second combustion, the phosphorus cannot be rekindled in the same atmosphere, even by agitation or changing the recipients; that the agitation of the recipients may bring the phosphorus more completely than before into contact with some of the oxygenous gas; that, on introducing a small quantity of oxygen gas into the recipient, the inflammation of the phosphorus immediately recommences; that the combustion of phosphorus disengages the caloric from the oxygen gas, and forms with it a new acid; that azotic and hydrogen gases dispose the oxygen more readily to combine with the phosphorus in certain temperatures, without however operating as the immediate cause of its combustion; that the sun only disengages the oxygen from the phosphorus when the experiment is made in water, in which case the oxygen gas is furnished by the decomposition of that fluid; and that rotten wood, and other phosphorescent substances, exhibit similar results as phosphorus.

In the prosecution of his laborious researches, Spallanzani frequently discovered facts which were before deemed impossible: in the year 2 of the republic, a circumstance of this kind occurred, which he published in his *Lettere sopra il sos petto d'un nuovo senso nei Pissistrelli*, in which we are informed that bats, although deprived of sight, nevertheless fly with the same precision, avoiding the most minute objects in their way, and fixing with facility on a proper resting-place when inclined to repose, as when not destitute of that organ. These extraordinary experiments, confirmed by several naturalists, inclined him to suppose that these animals might be furnished with a new sense, since he had demonstrated that the other senses did not supply to them the loss of sight; but I know that some anatomical experiments, undertaken by professor Jurine, inclined him to acquiesce in the opinion, that in such cases the faculty of hearing in the bat actually supplied that of sight.

Spallanzani terminated his literary career by the publication of a *Lettera sulla pioggia de' sassi avenuta in Toscana nel* 16 *Giugno del* 1794; in which, after some hesitation, he concludes, that these stones had either been thrown from the surface of the earth by a whirlwind, or some violent subterraneous eruption; and he quotes a variety of facts, in confirmation of this opinion. He addressed a letter on this subject to the celebrated chemist Giobert, *Sopra le piante chiuse ne' vasi dentro l'aqua e l'aria, exposte a l'immediata lume solare e a l'ombra.* It was, however, unfortunate for this department of science that death prevented him from prosecuting his discoveries on the subject: it should however be mentioned, that after having communicated to me the whole of his experiments, I became the student in my turn, and pointed out to him the cause of the anomalies he had observed with respect to those I had made on the same subject, which seemed to make a considerable impression on his mind; but, as my researches were not finished before the commencement of autumn, his death, which happened on the following winter, prevented him from repeating these experiments; a particular account of which will be found in my *Physiologie Vegetale*, t. iii.

These justly celebrated works, presented to the public by Spallanzani himself, do not, however, comprise the whole of his valuable labours. He was long occupied in researches respecting the phenomena of respiration in different species of animals, and had already finished the following Memoirs: these would have been quickly succeeded by others, for which he had collected a great variety of important observations; but of which I shall not here speak, since he himself has given an analysis of this immense work, in a letter addressed to me, which is prefixed to the present publication. He likewise left a numerous collection of new experiments, and observations on the reproduction of animals, on sponges, and a great variety of other interesting phenomena. He had nearly completed an account of his Travels in Constantinople, as well as in Switzerland, and had amassed sufficient materials for a new history of the sea. This great man drew instruction from every occurrence; and his thoughts—as rapid as his sensations were lively, and as just as his perceptions were faithful—penetrated at once into the depths of science.

What idea must we at present entertain of this natural philosopher? There are men respecting whom the unanimous judgment of their cotemporaries is confirmed by posterity, and whom the shafts of envy only furnish with a new passport to glory; as rust seems to prefer shining substances, but can make no impression on gold, so envy loses her poison when attacking the reputation of great men: the stamp of solid genius upon their minds is a seal for immortality, which sets malice at defiance and braves the ravages of time.

What a superlative idea must we form of the merit of Spallanzani, should it be estimated by the number and variety of his works! A literary man may fill volume after volume without possessing a single original idea: but the philosopher who delineates from nature, who discovers her secrets, pursues his way in the midst of darkness, illuminated only by his own genius; compelled to explore his route, to fix its precise situation, to free it of difficulties; he alone may be considered as the sole creator of the discoveries he has made, notwithstanding the efforts of the senses, of authority, and of nature, to lead him astray; and he might for ever fruitlessly wander in search of truth, if his mind were not habituated to studies calculated to rouse the imagination, and strengthen the judgment.

If the merit of Spallanzani be appreciated by the labours in which he was engaged, they will be found of the most important and difficult nature;—the generation of animals and of plants, the circulation of the blood, digestion, respiration, the reproduction of the different parts in animals, spermatic animalcula, animalcula infusoria, mineralogy, volcanoes, combustion, the description of several unknown animals, as well as a great variety of crustacea and testacea, and the solution of several physical and chemical problems; all these in their turn occupied his attention. The history of a particular object may be accomplished in the space of several years, without the possession of extraordinary talents; but to discuss such a number of intricate subjects in a manner so original, to remove so many obscurities, and discover so many truths, is indicative of uncommon acquirements and great energy of character. The works of eminent men are so numerous and valuable, because every moment of their lives is marked by great ideas, and because the clearness of their conceptions enables them to elucidate every part of any subject with which they are occupied.

If we are to estimate the merit of Spallanzani by his method, it is uniformly the most ingenious, the most simple, and the most accurate: the nature and solidity of his explanations evince that he could never rest satisfied with any remaining doubt. His discoveries have already stood the test of the most accurate scrutiny, and set at defiance the penetrating glance of envy. The same vast conceptions are observable in all his works: they exhibit a happy display of great ideas built on the universal principles of natural history; and, as this connection is always rendered evident, we are led to believe, if I may be allowed the expression, that he was in possession of the chart of the universe, from which he detached some portions to place under the eyes of the less enlightened part of mankind.

To conclude. If we are to form an opinion of Spallanzani from the style of his works—which is another characteristic of genius when it is chaste, polished, luminous, and harmonious—his cotemporaries have ranked his productions among those of their most eloquent writers. But I have already endeavoured to display the merit of this illustrious man, by recounting the labours which occupied the greatest part of his life, and the exalted opinion entertained of them by his learned cotemporaries. Such, at least, was that of Haller*, of Trembley, and of Bonnet: he was studied not only by the professors of Pavia, but by all the celebrated men of Italy and of Europe, with most of whom he maintained a correspondence. France, Germany, and England, appropriated to themselves his works by translations. He was admitted as a member of the academies and learned societies of London, Madrid, Stockholm, Upsal, Gottingen, Holland, Lyons, Bo-

* Haller dedicated to him the 4to volume of his large physiology—*Lazaro Spallanzani: summo naturæ in minimis indagatori, ob ejus in veri finibus extendendis merita D. Hallerus.*

logna, Milan, Sienna, Turin, Padua, Mantua, Geneva, Berlin, and the Italian society. He was a corresponding member of the academy of sciences of Paris and Montpellier. He was the correspondent of Frederic the Great; and received, from the hand of that monarch himself, his diploma as member of the academy of Berlin. Citizen Salicetti, commissary from the directory of the French republic to the army of Italy, was authorised to solicit his acceptance of the professorship of natural history at Paris, which he declined on account of his advanced age.

Spallanzani was about the middle size; his gait was lofty and firm; his countenance dark and pensive. He had a high forehead, lively black eyes, a brown complexion, and a robust frame. He had never experienced, during his whole life, but one fever; and that was caught in coming out from the mines of Schemnitz, in very cold weather. In the third year of the republic, he was attacked with a slight retention of urine and some symptoms of the gout; which, however, did not in the least suspend his literary labours.

Spallanzani had appropriated certain hours of every day for the purpose of study, at which periods he preferred the most perfect solitude; but, at others, he was fond of active exercises: he loved hunting and fishing, at which last amusement he was extremely dexterous; he likewise played well at foot-ball, and at chess; his conversation was full of energetic expressions, original ideas, and happy applications.

A retentive and well-stored memory made him embrace at once every thing that was known respecting any subject he was inclined to investigate, whilst a correct judgment enabled him to explain it with precision. His ardour in the pursuit of truth equalled his patience of research: his bold and expansive ideas overcame every difficulty he had to encounter, but he was extremely circumspect in forming his opinions;

he seized instantly the whole of a question, and distinguished at once all its bearings and relations, as well as the analogy it had to others: ardently attached to Truth, he followed whithersoever she led, and proclaimed her without ceasing. It should seem that Nature bestows on her votaries a simplicity and greatness of character which always form the most striking feature of moral perfection.

Spallanzani was universally esteemed: he displayed the most mild virtues in every situation of life: he was a sincere and constant friend, and particularly amiable in the midst of his family; where, however, too many individuals drop the mask that veils their imperfections from public view. He was adored by his relations, whose delight he was: he never quitted them without regret, and always returned to them with eagerness: he had inspired them with similar tastes to his own;—his brother Nicholas, a doctor of laws, assisted him in his experiments, and accompanied him on his return to Pavia. His sister Marianne was a distinguished naturalist: she was perfectly acquainted with her brother's cabinet of natural history, knew the properties of all the specimens contained in it, and was capable of reasoning upon them. Her mind was modelled upon that of her illustrious brother, whom it was a pleasure to her to study and imitate. Spallanzani superintended the education of his nephews with the most anxious solicitude, the eldest of whom he had the satisfaction to see elected honorary professor of medicine in the university of Padua.

Spallanzani on the 3d of February, 1799, experienced symptoms of iscuria, with which he had formerly been attacked. He passed a very restless night, and in the morning lost the use of his senses, which he never recovered but at very short intervals. His intimate friends, professor Tourdes, a French physician, and the celebrated Scarpa, did every thing that genius or knowledge could devise to save the life of their friend; but he died in a few days, notwithstanding all their efforts. He perceived, with firmness, his end approaching, and endeavoured, by his piety and his faith, to edify those who surrounded him. His death penetrated his family with the most lively grief, called forth the tears of his friends, filled his disciples with profound affliction, and excited the regret of a nation proud of having given him birth. At Milan, his colleague, Gregory Fontana, made an eloquent motion in the council of Youngers; in which he proposed that a monument to the memory of Spallanzani should be erected, by the side of those of Frisi, of Beccaria, and of Verri, who had adorned the Cisalpine republic by their genius and their learning; and his brother, Nicholas Spallanzani, caused one to be raised in the church of Scandiano, the place of his nativity.

LETTER

FROM

SPALLANZANI

TO

CITIZEN SENEBIER

ON

RESPIRATION.

You already know that, for a long time past, I have been chiefly occupied with researches into the nature of respiration in man, as well as in other animals. I have communicated to you the motives which induced me to enter on this investigation, and the plan I originally adopted in my examination of this function, in different animals, beginning with the lowest class, and proceeding to the highest or that which which comprehends the mammalia. Before finally terminating these labours, which are already considerably advanced, I wish to obtain your opinion respecting what has been already done; and, in particular, to inform you of a phenomenon, the result of which will doubtless occasion some surprise.

Wishing to enter with you on the subject of respiration, I should describe the experiments which were made on living animals: I shall here, however, pursue an opposite course, and begin with the animals deprived of life, or at least of respiration, being anxious to exhibit the effects they would produce independent of this vital function: not but that living animals were the first object of my enquiries; but, in proportion as I observed the chemical changes produced in the air in which they breathed, I was anxious to ascertain what effect would be produced on it after their death.

It cannot be doubted but it will greatly contribute to the advancement of physical science, to strike out a new path of enquiry, or to extend that which has been already trodden by other naturalists, setting out from the point at which they stopped. The small experience which I flatter myself I have acquired on this subject, has convinced me, that it is frequently most advantageous to pursue unbeaten tracks, or even those which no person has hitherto ventured to explore. Such is the course I have pursued in my present inquiry.

I shall not here give you the specific names of those animals that were the subject of my examination; but confine myself, for the present, to the general results, reserving for the work itself a minute detail of each individual experiment.

I have only farther to observe, that, in order to ascertain the chemical changes produced in the air, I employed the eudiometer of the celebrated Giobert, which I found the most convenient, and best calculated for my chemico-physiological researches. But to proceed.

I enclosed, in a given quantity of atmospheric air, different species of worms: the result of which was, that those provided with respiratory organs, as well as those which were destitute of them, absorbed the whole quantity of oxygen contained in the air; at least, none could be afterwards detected by the phosphorus of Kunckel. I observed that the skin, in these animals, supplied the place of lungs; and I wished to ascertain if it would preserve this property after their death. With this intention, I placed them in air-tight vessels, under the same circumstances as in the former experiments, and found that, in this case, the oxygen was likewise wholly absorbed.

Notwithstanding these animals began to give evident signs of putrefaction, by the fœtid odour which they exhaled, the change of colour, and the flaccidity of the parts, I again enclosed them in a quantity of air, when the fermentation continued to encrease without destroying their absorbent power. Having repeated this experiment several times, I ascertained, from the analysis of the enclosed air, that the oxygen had been uniformly and completely destroyed by these substances, from the beginning of the putrefactive fermentation, until they were reduced to a state of total decomposition.

The combined action of water and heat, in decomposing animal substances, is well known, and may be easily perceived by the intestine fermentation which is produced. I had recourse to this mode likewise, in order to ascertain whether the faculty of absorbing oxygen was encreased or diminished; when I perceived it remained uninjured, although the fermentation had continued until the organisation of the worms was wholly destroyed. These experiments were made on the different species of animals composing the order of testacea, and the result was always the same.

The singularity of these phenomena made me conjecture there might be some ambiguity in this absorption of oxygen, the possibility of which I had previously suspected.

In each of these experiments not only the usual proportion between the oxygen and the azotic gas was destroyed, but a certain quantity of carbonic acid gas never failed to be produced. Could this gas be the result of the oxygen having entered into combination with the carbon of the animal fibre? But, then, it was evident that the animals could not absorb the base of the oxygen gas which they consumed. This reasoning acquired additional strength from the following circumstance: when the animals were confined in pure oxygen gas, in place of common air, the consumption of this gas was more considerable in proportion to the carbonic acid gas produced.

This observation, however, did not appear to me decisive, since it was possible that the unusual production of carbonic acid gas might proceed from a greater portion of carbonic acid being disengaged from the animal fibre, by means of the additional quantity of oxygen, which is well known to possess a highly stimulant quality.

It is true the encrease of carbonic acid gas produced by the animals confined in pure oxygen gas was not uniform, since they had several times consumed $\frac{50}{100}$ of this gas, whilst there was only five or six hundredths of carbonic acid gas found in their atmosphere. At the same time, in making this experiment in common air, when its oxygen was wholly destroyed, there was seldom above two or three hundredths of carbonic acid gas discovered in the remaining air.

In order to reconcile these apparent contradictions, I had recourse to an expedient which I conceived should be decisive: the dead animals were placed in a medium entirely deprived of oxygen gas; because, if no carbonic acid gas was produced in this situation, it would afford an irrefragable proof that the production of this gas depended on the oxygen of the atmosphere, or, in other words, that it was the effect of a combination of this principle with the carbonaceous matter emitted from the animal; or, otherwise, I should have obtained this gas nearly as when the animals were shut up in common air; and, in this case, it would have demonstrated, that its presence did not depend on the oxygen of the atmosphere, and, consequently, that it proceeded directly from the body of these animals in a gaseous form.

I confined, then, different species of worms, newly killed, in pure azotic gas, drawn from the fibrous parts, well washed from the recent blood, by means of the nitric acid, according to the method of the celebrated Berthollet; but in these experiments the carbonic acid gas was evident, as well as when the animals were confined in pure hydrogen: even more than once the quantity of carbonic acid gas, produced in these mephitic gases, was far more considerable than when the animals were shut up in atmospheric air; from which I was compelled to conclude, that the carbonic acid produced under these circumstances is not derived from the oxygen of the atmosphere; and, consequently, that the oxygen gas, destroyed by the presence of these dead animals, has its base absorbed by the animals themselves.

Having remarked that many animals of this class could live several hours in these mephitic gases, I availed myself of this circumstance to confine, in hydrogen and azotic gas, some of those which are provided with proper respiratory organs; whilst, at the same time, I shut up other individuals of the same species in atmospheric air; the result of which was that, in both cases, I obtained nearly an equal quantity of carbonic acid gas. There must then also have occurred in these animals an absorption of oxygen, and the appearance of the carbonic acid gas was a product either of the carbonic acid gas, or of carbonic acid the base of which had proceeded from these animals.

But you may perhaps enquire, if the

worms alone continue after their death, or during the progress of their decomposition, to absorb the oxygen of the atmosphere? Impressed with the importance of this question, I endeavoured to ascertain if this circumstance likewise occurred among the superior classes of animals: with which view I submitted to experiment several of those insects which preserve always the same form, as well as others that pass through different metamorphoses. These experiments were made under similar circumstances as the former; but after having killed the insects, and attended to every stage of their decomposition, I always obtained a complete absorption of the oxygen, when I allowed the putrid substances to remain some time enclosed in common air, only this process was far more slowly carried on than by the living insects, in which case it took place with astonishing rapidity.

You will be surprised when I inform you, that a larvæ, which weighed only a few grains, absorbed, in a given time, nearly as much oxygen as an amphibious animal infinitely larger, and that this great absorption is uniformly repeated, according to the multiplicity of air vesicles distributed throughout the system of these living beings.

I next extended my researches to salt and fresh water fishes, which, after being killed, were enclosed in common air: their size enabled me, likewise, to repeat these experiments on the different internal parts; such as the stomach, intestines, liver, heart, ovaries, &c., after they had been separated from the body of the animal; all of which, like insects and worms, completely absorbed the oxygen of the common air.

One of the principal objects I had in view was to ascertain the proportion of oxygen absorbed from the atmosphere by the living and dead animals. Water is the natural element of fishes, but that which is allowed to stagnate in a vase, although exposed to the atmosphere, very quickly becomes tainted, and unfit to support the life of the animals confined in it. From the disagreeable sensations experienced in such a situation, they ascend, in order to respire at the surface of the water, and perish in a very short time. I have seen several different species die sooner in stagnant water than when exposed dry to the open air.

Proceeding in this manner, much useful instruction may be obtained, respecting the chemical changes produced in the air which is in contact with the water. I should, however, have been very inaccurate, had I trusted to that method alone; but I likewise placed the vessels containing the fishes in a stream of running water, by which means the water in the vessels was continually renewed, and in this manner I was enabled to ascertain the different proportions with greater precision.

The amphibia after death displayed the same phenomena as the worms, insects, and fishes; but when alive, they presented other subjects of enquiry. Having observed that some of them survived the destruction of their lungs several days, I was enabled to submit them in this state to examination, and thus to ascertain with precision the different quantity of oxygen absorbed by the lungs and the skin. I was likewise enabled to institute a comparison between the quantity of oxygen absorbed by these mutilated animals, and by those which had not undergone this operation.

You will see, in my work, what a small quantity of oxygen is absorbed by the lungs, in proportion to that taken in by the skin; although it is generally believed that in this, as well as in the two higher classes of animals, the destruction of the oxygen contained in the atmosphere is attributable to that organ alone: some species of amphibia, from which I cut out the lungs, lived even longer in free air than those which were not deprived of this organ when confined in mephitic gas, or air wholly destitute of oxygen. I discovered, farther, that some of them died much sooner when their skin was slightly covered by a varnish with spirits of wine; the reason of which is evident, as, by means of this varnish, they are not only prevented from absorbing the oxygen, but rendered incapable of exhaling the carbonic acid, the expulsion of which seems equally essential to the continuance of their existence; whilst, on the contrary, if the experiment be made in mephitic gases, the carbonic acid is always found in a gaseous state.

I have been able, however, to determine the precise quantity of oxygen absorbed by the skin, without cutting out the lungs from these amphibia, by confining their bodies in recipients in such a manner that, while the head alone had any communication with the external air, they could breathe without pain or difficulty. In this way, I ascertained that the absorption which takes place after the death of the animals, is only a continuation of that which was carried on during their life.

Hitherto I have only spoken of four classes of animals with cold blood; it remains for me, therefore, to make a few observations respecting birds and mammalia: these, being more nearly related to man, ought particularly to arrest our attention. In my examination of different birds, I not only found that, both during life and in a dead state, they absorbed oxygen; but that the brain, muscles, and other internal organs, and, in short, the skin, possessed the same property. I enclosed the body of living birds in the same manner I had done the amphibia, allowing their heads to remain without the vessels so that they might freely breathe the external air; by which means I ascertained the proportion of absorption carried on by means of the skin.

The experiments made on the mammalia, which belong to the order of quadrupeds, were attended with similar results as those on birds; but I likewise obtained others extremely important, in the course of my examination of that singular species of quadrupeds which the cold renders torpid, or which sleep during winter.

Having remarked that the phenomena of respiration varied in these animals, according to the different degrees of atmospheric temperature to which they were exposed, and that similar phenomena were observable in the circulation, from the intimate connection subsisting between these two functions, I wished to enter into a minute investigation of this subject. In order, however, to multiply my observations, and render them more consistent, I reared in my own house, during several years, the five species of this class which are found in Italy. I had, besides, another object in view, that of studying the habits and natural history of this class of animals, which had not hitherto been sufficiently elucidated; and by thus having them under my own eye, during different seasons of the year, as well as observing them in their natural retreats, I have been enabled to accomplish my wishes in their utmost extent.

As the circumstance was somewhat curious, you may perhaps recollect my marmot, which became so extremely torpid during the rigorous winter of 1795. I kept this animal, at that period, four hours in carbonic acid gas, the thermometer marking 12°; yet it continued to live in this gas, which is so very deleterious that a bird and a rat which I exposed to its influence at the same time perished instantaneously. It appears, then, that a total suspension of respiration had taken place during the whole of that period. The same experiment was repeated on a bat equally lethargic, with a similar result.

In pursuing still farther my experiments, I preferred these flying quadrupeds to the marmot, afraid that this animal might sink under repeated trials; and I had only two, which I wished to reserve for other experiments; whilst, on the contrary, I possessed a great number of bats.

I first wished to ascertain if, when respiration was suspended in these animals, there would be any production of carbonic acid from the skin; for which purpose I substituted azotic for carbonic acid gas. I then placed in this gas two bats, the thermometer standing at 9°, and allowed them to remain in it about two hours; after which I gradually removed them into a warmer medium, when they exhibited evident signs of life; but I could discover no carbonic acid gas in the azotic gas, from which I was led to conclude that the temperature was too low for the exhalation of this gas. I repeated these experiments at different temperatures successively raised to $3\frac{1}{2}$°, when $\frac{5}{100}$ of carbonic acid gas were produced, although the torpidity of the animals was equally great.

In this state of things I repeated the experiments under similar circumstances, only removing the bats into another vessel filled with atmospheric air; when I found not only the production of $5\frac{1}{2}$ hundredths of carbonic acid gas, but the destruction of $\frac{6}{100}$ of oxygen gas. Although these two small quadrupeds were enclosed in common air, their profound torpor prevented them altogether from respiring; nor could that swelling and sinking in their sides be perceived, which are occasioned by the inflation and colapse of the lungs during respiration; neither did these phenomena occur in the open air: from all which it is evident, that the partial consumption of oxygen gas was in consequence of its absorption by the skin.

The result of the whole is, that this chemical power, of absorbing the vital part of the atmosphere, possessed by these warm-blooded animals after death, is the same as that which they display during life, and which continues to act until their bodies are wholly decomposed.

Besides, the complete suspension of respiration experienced by these animals, when exposed to a violent degree of cold, becomes insupportable, and ultimately produces death, as I have shown in my experiments; so that the torpidity to which some small quadrupeds, and in general the amphibia, are subjected when in their natural retreats, is usually accompanied with feeble respiration, as will be demonstrated in another part of this work.

Several worms, and amongst them the greatest part of the testacea, many insects which systematic writers rank among the crustacea, besides the immense tribe of fishes, whose natural element is water, sometimes become unable to live in it. Does the faculty which they possess of absorbing oxygen, when exposed to the air, support them in this fluid, because it contains a small portion of oxygen gas? I felt myself inclined to acquiesce in this opinion; but, in order fully to ascertain its truth, I appealed to direct experiment.

With this view, I plunged different species of these dead animals separately into tubes, containing water in which a given quantity of common air had been made to ascend. The oxygen gas of the water being in contact with that of the air, it appeared to me obvious, that if the former was absorbed, the latter, or at least that part of it contained in the surface of the water in the tube, would ascend into the vacuum and restore the equilibrium that had been destroyed. This, in fact, proved to be the case; for whenever I repeated this experiment, which I did a great number of times, on several individuals of these three classes of animals, I found that the air on the surface of the water was deprived of its oxygen gas.

I cannot here pass over another observation which I made: if, instead of these aquatic animals, I placed under the water, at a given depth, land animals, or some part of their bodies, I observed the same diminution of oxygen gas in the superincumbent air; which proves that, although water be not their proper element, yet they preserve, even in this fluid, the faculty of absorbing vital air.

Hitherto I have only considered the

skin; and shewn, in the six classes of animals that were the subject of my experiments, that this integument possessed the power of absorbing vital air from the atmosphere, not only during life but even after death. I shall only stop a moment, to notice the power bestowed by nature on other parts which, although apparently subordinate, are nevertheless essential to the economy of these animals; such as the shells of the different testacea, which you know are comprehended in the family of worms.

Observing that these animals, after death, decomposed the common air, by absorbing the oxygen contained in it, I supposed their shells might concur in this operation, particularly when I reflected that Herissant had demonstrated them to be organised bodies, and that they formed a whole with the animals which inhabit them: by enclosing the shells in common air, the result confirmed the truth of my conjecture, and evinced that this absorbent faculty equally belonged to the land and water testacea. I have been able to estimate the proportion of oxygen absorbed by the animals alone, as well as that taken in by the shells when separated from them; only I remarked that the absorption was carried on by the shells much more slowly than by the animals themselves.

In the course of these experiments, analogy suggested the idea of a body which, although of a different kind, nevertheless appeared to merit attention. The shells of the testacea are formed of two different substances, calcareous earth and animal matter. The shells of the eggs of birds being formed in a similar manner, is it not therefore possible, or rather probable, that this absorbent power resides wholly in these external membranes? which I found to be the case in all the eggs I subjected to experiment. And as I have shewn that a portion of the vital air absorbed by the shells of the testacea passes into the animals contained in them, it may probably assist in their preservation. I conceive I possess proofs sufficiently strong to evince, that the oxygen gas likewise enters into the egg, in order to vivify and assist in the evolution of the germ contained in it.

Before terminating these considerations respecting the shells of testacea and of eggs, I must endeavour to elucidate a circumstance essential to their nature. The constituent principles of these shells are, as we have already seen, an organic substance and calcareous earth. Is the vital part of the atmosphere, however, absorbed by these two substances in conjunction or by one of them alone? In order to solve this question, it was necessary to submit them separately to the test of experiment. With this intention, I employed the carbonate of lime; as, from the similarity of its nature to that of the calcareous carbonate of shells, I might safely infer that, if the former did not possess this absorbent faculty, neither could the latter; and that, consequently, the absorption of the oxygen gas must be effected by the animal, not by the calcareous matter of the shells, which I found to be precisely the case: because the most pure calcareous carbonate, the transparent calcareous spar, produced not the slightest alteration in atmospheric air, although immersed in it for a considerable length of time.

I had a farther confirmation of this circumstance, from having accidentally found some shells belonging to those species of snails known to naturalists under the names of *Pomatia* and *Nemoralis*, which, from the changes they had undergone, appeared to have been long deserted by these animals; but the calcareous matter was readily detected by the agency of acids and of heat, so as to leave no doubt respecting their nature. I observed, however, that their power of absorbing oxygen was greatly diminished, and that in proportion to the degree of disorganisation they had experienced. It must hence be admitted, that the organisation of the shells of the testacea, a description of which has been given in this work, is the cause of this absorption, independent of the calcareous matter; or, at least, without this organisation the shells could not produce that effect. In like manner, when these shells were preserved, even a great number of years, without becoming sensibly decomposed, I remarked, with astonishment, that, notwithstanding the length of time, their faculty of absorbing oxygen remained unimpaired.

Such, my learned friend, are the principal subjects of which I wished to give you some idea in a few words. For, although the chief object of my experiments for several years has been the function of respiration in many different species of animals, I wished to confine myself, in this letter, to a rapid view of the subject. My intention was to shew you that living animals consume or absorb oxygen gas wholly independent of the lungs, and that they retain this power after death. You have seen this evinced in cold-blooded animals, such as worms, insects, fishes, and the amphibia, as well as in those with warm blood, birds and mammalia. This absorption of the vital air, contained in a given portion of atmospheric air, was complete; at least none could afterwards be detected by the phosphorus of Kunckel.

In the course of my experiments on the mammalia, I discovered that different parts of their bodies, as the muscles, tendons, bones, brain, fat, and blood, possessed the power of absorbing oxygen gas in different proportions; the bile alone formed an exception to this rule: but the blood, which I had supposed might evince a greater susceptibility than the other parts of attracting oxygen, from what had been written on its power of decomposing the air, did not appear to possess this property in any super-eminent degree. Both the venous and arterial blood in cold and warm-blooded animals afforded similar results.

At the beginning of this letter, I expressed some doubts whether to attribute the consumption of oxygen gas, by the worms confined in common air, to the absorption of its base, or rather to its combination with the carbon disengaged from these animals: but my hesitation was quickly dissipated by the presence of vital air, even when the worms were confined in azotic or hydrogen gas. The same circumstance occurred in my experiments on the other five classes of animals; from which, as well as from the fact in the experiment on oxygen gas, I am led to conclude that its base is absorbed by the skin and the other parts of these animals.

But you may perhaps enquire if, in the multiplicity of my experiments, I observed any chemical change produced in the azotic gas contained in the atmosphere? In answer to this question, I may observe that, according to the variety of animals employed in these experiments, it either remained wholly undiminished, or at least only suffered a very trifling diminution in comparison with that of the oxygen gas, although the proportion of the latter in atmospheric air be but as one to four of the former.

I infer, then, that this absorbent animal power is particularly intended to attract and appropriate the oxygen, and that it has a direct relation to the temperature of the atmosphere. Hence it may almost be laid down as a general rule, that the absorption of oxygen by the animal is directly as the heat of the surrounding air, which perfectly agrees with the observations made on phosphorus.

To conclude these results, the proofs of which I have reserved for the work itself, you have yourself witnessed the immense quantity of oxygen gas abstracted from the common air by these animals: the consumption is unquestionably very considerable by the lungs, or those organs which supply their place; but it is incalculably greater by the external surface of the body, not only during the life but after the death of the animals, and even when putrefaction is considerably advanced. Reflecting, then, on the vast number of land

and aquatic animals which inhabit every quarter of our globe, we might be led to suppose that this immense destruction of the vital part cf our atmosphere would occasion the total extinction of animated life. From eudiometric observations, however, we learn that the quantity of oxygen in the atmosphere remains always the same; from which we must necessarily conclude, that nature furnishes some means to compensate for this destruction of oxygen gas, in a similar manner as an exact balance is maintained between the death of animals and vegetables and their reproduction.

But how is this equilibrium preserved? In your works, and those of Ingenhouz, which do honour to their authors by the originality of the views displayed in them, and form a brilliant epoch in natural philosophy, it is demonstrated that vegetables exposed to the sun give out to the atmosphere a prodigious quantity of oxygen gas, which may unquestionably supply that which is consumed by the breathing of man and animals. But after having found that this absorption of vital air is incalculably greater by the external surface of the body in living animals, and that it is carried on even after their death, I can scarcely suppose that such an immense loss can be wholly compensated by that which is daily thrown out by vegetables; more particularly when it is considered that the number of animals greatly exceeds that of plants, and that the absorption of oxygen gas is constantly going on, not only during their life but even after death; whilst vegetables, on the contrary, only diffuse their beneficial influence through the atmosphere when covered with foliage, at certain determined seasons of the year. These considerations almost led me to think there must be some other constant source of vital air; and, as nothing in nature can be annihilated, I supposed that animals themselves must be endowed with some power of again giving out the oxygen gas they had previously absorbed from the atmosphere.

But this is not the proper time to enter into the region of conjecture on so important a subject; I shall discuss it in my work on the respiration of man and other animals. I have the satisfaction at present to inform you that the composition of four Memoirs are finished; which, with the others, will form the first part of my researches, and which will be published in a very short time*.

I receive with the most lively gratitude your offer of translating into French this new production, as it cannot fail to extend its circulation among those who are unacquainted with the Italian language. In performing a similar service to my other works, you have at the same time enriched them by important introductions, well calculated to elucidate and render of greater utility the different subjects contained in them;—but I stop, &c.

* Hitherto I have only received the manuscript of the three following Memoirs.

MEMOIRS

ON

RESPIRATION.

INTRODUCTION.

RESPIRATION is a function so essential to life, and serves so many important purposes in the animal economy, that it has in all ages, but still more particularly within these last few years, very deservedly attracted the attention of naturalists: the discoveries, however, of modern chemistry have thrown greater light on this important subject in a short time than all the vague conjectures and theories of former ages. I mean not, however, to affirm that every difficulty has been completely overcome: at present, even some celebrated philosophers differ in opinion respecting several circumstances connected with this important function, although they had carefully attended to it in man, as well as in a few other animals. Their labours certainly merit the greatest attention; yet I will venture to affirm that such discordant opinions might have been easily reconciled, had they extended their researches to a greater number of the different classes of animals, and especially to those which form the last link in the chain of animated beings. I am even of opinion that it is with these, as being more simple in their nature, they ought to have begun their experiments, and proceeded as it were step by step, from genus to genus, and from class to class, until they arrived at those animals possessing a more complicated structure, in order ultimately to succeed in attaining a perfect knowledge of the mechanism of respiration in man.

It must be admitted as a maxim by all thinking men, that it is proper in their investigations to begin with the study of the most simple objects, in order to attain an accurate knowledge of those which are the most complicated and obscure. Thus, on the subject of respiration, it may readily be perceived that new, valuable, and diverſified knowledge may be obtained, by extending our views towards different genera of animals, even when the organisation of their respiratory organs is not extremely dissimilar.

Attracted by the beauty and importance of this subject, and animated by the hope that my researches into this branch of animal physiology would not prove altogether uninstructive, I determined to make it the object of my immediate examination; and I will not conceal that it has chiefly occupied my time during a period of three years. I had been formerly employed in the study of other branches of physiology; what I published at different times on the reproduction of animals, on the circulation of the blood, on the generation and digestion of man and animals, is sufficiently known to embolden me to present these Memoirs to the public, on a subject so analogous to my former labours.

I propose, then, to examine these four principal points.—

1. The respiration in the six classes of animals, beginning with the lowest and gradually ascending to the highest, which comprehends the mammalia, and consequently man himself.

2. The respiration in those animals rendered torpid by cold. It is well known that the air is deteriorated by respiration; but how is this deterioration produced by the agency of those animals which do not respire, or are destitute of respiratory organs, of which there are a very great number?

3. I have been led, in the course of these investigations, to endeavour to discover if the skin of those animals, which are not possessed of respiratory organs, produce the same alteration in the surrounding air.

4. Finally, having observed that the air

in which the animals were closely confined became vitiated, I conceived it necessary to ascertain if their dead bodies produced the same effects.

But these ideas are too confined to exhibit merely the first outlines of my labours, whatever they may be, upon this physiological subject. I am anxious, therefore, to state them more at large; and this I shall endeavour to do as far as the limits of a simple Introduction will permit.

In the class of worms, every one knows that there are different kinds of land snails, of which some have shells and others have none. The latter are called naked snails. It is certain that these animals have lungs, or, at least, organs of an analogous description. These beings were the first which attracted my curiosity, and led me to examine whether they altered the elements of the air in the process of respiration, and to discover what was the nature of such alteration.

I made a series of similar experiments and observations on aquatic snails.

Worms have some strong points of relation to insects. The stigmata of the latter are the lungs of the former; but these stigmata are lungs more multiplied than those of worms: besides, the lungs of the insects which are subject to transformations merit, in this respect, the most particular and profound reflections. A caterpillar, a chrysalis, a butterfly, may, as has been proved, be the same animal under different teguments; but it is nevertheless true that these three states occasion successive changes, which are remarkable in the number and position of the trachea.

Besides, several of these insects live for some time in one of these states, in the midst of water; whilst, in another state, they become inhabitants of our atmosphere. In this case, we have to enquire whether the air diffused throughout the water loses the oxygen gas which forms a part of it, so as to support the life of those animals during their abode in this fluid. Is the quantity they absorb of it during their aquatic life less than during their terrestrial existence?

Ascending from insects to fishes, though the latter seem to have a higher rank in the scale of animal perfection, it may, nevertheless, be asserted that they are inferior as to the organs of respiration, which consist of a series of vascular cartilages, called *gills*. How, then, is respiration carried on in this class of animals? Will it be said that the water, impregnated with air, gives out its oxygen gas to those respiratory organs of fishes? Or will it, more truly, be said, that the oxygen resulting from the decomposition of the water is attracted by those organs?

The class of amphibia, such as serpents and oviparous quadrupeds, succeeds that of fishes. The lungs of these animals approach much nearer to those of man, viviparous quadrupeds, and birds, than the lungs of worms, insects, and fishes. This double class live not only in the air, but they can also live, for some time, under water; so that they present a two-fold series of different experiments to be made. It would first be necessary to observe the changes which the respiration of these animals effects in the air, when they live on land; being careful, however, in the next place, to examine the changes which this fluid undergoes when confined in their lungs, and when they are compelled to remain under water; because, though these animals sometimes expel from their lungs, during their immersion in the water, the air which they had inspired, they nevertheless then frequently retain it in that organ; and it is not uncommon, at that time, to find the lungs of the animals inflated after their death, though it may have been occasioned from preventing the renewal of the air by respiration.

Birds and quadrupeds, which are the most perfect of animals as to the organisation of their bodies, are also the most perfect as to that of their lungs. They are remarkable for the frequency of their action which is very great, and their indispensable necessity to the life of those animals, which ceases when their respiration has been interrupted for some time; whereas the respiration of the amphibious class, which is generally slow, may be suspended several hours without proving fatal.

It is principally on the animals of these two important classes, that chemistry has exercised itself with particular industry, in order to elucidate the mechanism of respiration; but, notwithstanding the discovery of some leading facts in this enquiry, it has been impossible to avoid the uncertainty of hypothesis in their explanation.

The common air which we inspire contains 0.27 of oxygen gas, and 0.73 of azotic gas, not taking into the account the very small quantity of carbonic acid gas which is mixed with it; but in expiration the oxygen gas is very much diminish-ed, and, with the oxygen and azotic gas remaining in the lungs, we find inter-mixed a considerable quantity of carbonic acid gas.

The air respired has been received into the cavity of the lungs, and thence ex-pelled.

How, then, during this operation, has the air undergone these two changes?

It has been shewn that, in the inspiration of the atmospheric air, its oxygen remains partly absorbed by the blood which circu-lates in the lungs, and partly combined with the carbon given out by the blood; whence it results, that, in expiration, the mass of air is diminished, and, at the same time, impregnated with the carbonic acid gas, whilst the azot remains in its original state. It was afterwards supposed that the atmospheric oxygen is not entirely absorbed by the blood; but, rather, that a part, by combination with the carbon of the lungs, forms carbonic acid gas; and that the other part, by its union with the hydrogen of the lungs, produces water.

Lastly, it has been asserted that, in respiration, a part of the atmospheric azot is absorbed by the lungs, and enters into and incorporates itself with the substance of the animals.

Such are the principal theories formed relative to the mechanism of respiration by great chemists, who have generalised them as to the whole animal system; though, in reality, they are founded only on some examples drawn from a small number of animals.

As to myself, I shall here adduce the experiments which I have made on the whole of the six classes of the animal kingdom. I shall, in the first instance, compare them with each other relative to the mechanism of respiration; and I shall then enquire whether it operates in the same manner in all living beings, or whether it varies with the varieties of those different beings.

2dly, There is a great difference between the animals which preserve life when exposed to a low temperature, and those which under the same circumstances lose their locomotive power, become lethargic, and remain in that state during the whole winter. The two classes of animals which are, beyond comparison, more numerous than all the others, namely worms and insects, are subject to this lethargy. The class of serpents also exhibits this phenomenon, as does that of oviparous quadrupeds, those at least of the cold and temperate climates; and we find several viviparous quadrupeds in the same situation. It appears, therefore, that the number of lethargic animals is much more considerable than that of the animals in which the functions of life are not suspended during any period of their existence.

In this innumerable class, the oviparous quadrupeds, serpents, and several viviparous quadrupeds, have real lungs, and manifestly respire; but, as respiration depends on the energies of life, it is clear that the impairing of the latter must, in the same degree, weaken respiration. Hence it was extremely curious to observe the gradation of the symptoms manifested by the animals who thus become lethargic; and, above all, it was very important to ascertain whether respiration entirely ceased in the animals which had fallen into a complete state of lethargy. But I could not treat this vast subject without attentively observing the modifications induced upon the natural habits, &c. of the animals which resemble each other in this respect; in the same manner as it was impossible for me to draw any conclusions from these modifications, without having observed the habits of these animals in vigorous life; so that it was indispensably necessary for me to study them in the twofold condition of active life and apparent death.

I shall not, at present, speak of the worms and insects, because it is necessary that I should advert to them at some length in the course of this treatise; but shall proceed to the consideration of serpents, oviparous quadrupeds, and such viviparous quadrupeds as are lethargic.

Every one knows that, at the approach of winter, these animals have the foresight to seek out for themselves retreats capable of sheltering them against the attacks of their enemies, and the injuries of a rigorous season, when they are deprived of the use of their members by the immobility proceeding from the lethargy into which they have fallen. Hollow trunks of trees, the cracks of old walls, the chinks of stones spread over the surface of the ground; small subterraneous cavities found by chance, or formed by these animals; the slippery mire of marshes, rice-grounds; the light and penetrable soil of cultivated fields; the interior parts of the embankments of rivers; the bottoms of hedges, the close stems and sharp thorns of which present situations inaccessible to the enemies of these animals; together with the most remote depths of the forests—such are the asylums which promise security to this tribe of beings during their sleep in the winter months.

It was to such places that I frequently proceeded, under the guidance of viper-catchers and fishers. I caused the snow to be removed; the ice to be broken; the bosom of the earth, often covered with a strongly frozen crust, to be laid open: and by these means I was enabled to discover the burrows which contained their lethargic tenants; and, according to the common phrase, to surprise nature in the act.

It was easy to perceive the profound sleep in which these animals were, from the compact situation of their bodies, the careless position of their members, their eyes remaining shut, and their insensibility to every mode of irritation. The degree of internal heat of these animals, measured with accurate thermometers, became, in this case, an important object of observation. It was requisite to ascertain whether the action of the lungs, the motion of the heart, and the circulation of the blood, were merely weakened, or whether these functions were totally suspended. It was necessary to observe whether there was any chink, any opening, which might afford a passage for air into the retreats of these sleeping animals, so as to sustain life; or whether every mode of communication was shut. Such were the nice researches which I rapidly executed at those fortunate moments.

Having noted down, on the spot, the low temperature to which these profoundly sleeping animals were exposed, I made them be carried home, where I artificially subjected them to the same degree of lethargy which they might have experienced when I found them. I made different experiments on them, relative to respiration; but as, notwithstanding my utmost efforts, I could not succeed, during the winter, in drawing out of their sleeping places all the animals that I required, and in turning to advantage the multitude of ideas derived from those which I had obtained, I procured a great number of them in the beginning of winter; and, during several years in succession, I collected all the species of quadrupeds belonging to the lethargic class. We have five of them in Italy;—the porcupine, the bat, the muscadine rat, the dormouse, and the marmot. I do not include moles; because, though Linnæus and some nomenclators place them amongst the animals which sleep in winter, it is, nevertheless, certain that, during that season, they are very wakeful, and throw up the earth then as well as in summer. This incontestible truth is known to every gardener, and to every labourer in the fields.

Amidst other important remarks which I was enabled to make, I learned, with a degree of pleasure mingled with surprise, that, among the multitude of animals benumbed by the cold, there are different species in which, during the whole of spring, the two vital functions which appear the most energetic remain suspended;—namely, respiration, and the motion of the heart with the circulation of the blood. Accordingly I was less liable to be astonished when I saw different kinds of these animals, which were in a state of complete torpor, continuing to live as well in the most deleterious mephitic gases as in common air—such, in particular, were the bat, the muscadine rat, and the marmot.

In the enumeration of living lethargic animals which will form the subjects of my experiments, I have not mentioned birds and fishes. With regard to birds, I have already shewn, in another work, that they do not fall into this state, when exposed to an equally intense cold, though such had been the supposition with respect to a species of swallow*. As to the second

* Travels in Sicily.

class, as it had not, in my opinion, been shewn that they experience a real lethargy, I undertook, upon this subject, a series of experiments, which I shall state in treating of these animals.

In a former work I have proved, that the proximate and immediate cause of the torpor of the animals is not, as the illustrious Buffon had conceived, the refrigeration of the blood occasioned by a low temperature; since there are animals deprived of all their blood, which become torpid in the same manner as the others: it therefore rather appears to me, that this torpor is produced by the augmented rigidity of the muscular fibre, and, consequently, by the diminution of the irritability *.

In order still farther to ascertain the solidity of my opinion, it appeared to me that a re-examination of this subject would not be foreign to the present work.

After treating of fishes and crabs, and

* Opuscules on Vegetable Physics, vol. I.

having spoken generally of lethargic animals, without distinguishing the cold-blooded from the warm-blooded, though their differences may be very essential, then, instead of placing all these animals, as Buffon has done, in the class of cold-blooded animals, I have been the first to shew that those viviparous quadrupeds called cold-blooded—such as marmots, muscadine rats, hedge-hogs, &c.—are actually warm-blooded animals.

Hitherto we have been considering such animals as have lungs, or analogous organs. All of them, indeed, seem to be divided into different branches, from the mammiferous to the vermicular; not but that in the latter there is a number of species, partly aquatic and partly terrestrial, in which the most subtle anatomy has not been able to discover even a vestige of respiratory organs.

But as we shall shew that all the breathing animals require the oxygen gas of the atmosphere for the purpose of sustaining life, and as this oxygen gas is exhausted in the process of respiration, what must we think of non-respirant animals? This was another enquiry which deserved to be made; and, in making it, I learned that this order of animals is subject to the same laws, and that the organ of the skin performs to a certain degree the functions of the lungs.

Having terminated this enquiry, my mind, occupied with these matters, soon found out a new and not less important subject of examination. Is the decomposition of oxygen gas in the respirant animals exclusively performed by means of the lungs, or is it assisted by the exterior state of the body? I at once perceived that, in order to decide this question, it would be proper to confine the animals in receivers filled with common air, so as to interrupt the action of the lungs, without extinguishing life; or that the orifice of the lungs should be above the mouth of the receiver, whilst their bodies were within it, all communication being prevented between the external air and that of the receiver.

I could not make use of those kinds of insects which have their stigmata or the orifices of their lungs on both sides of their bodies, because I was aware of their dying the moment their stigmata were besmeared with oil, or any other greasy substance. I therefore had recourse to those which breathe at the extremity of the tail, and which Reaumur called *rat-tailed vermes.* This class, though they live in the water, keep the point of their tails above the surface, in order to enjoy the benefit of the atmospheric air. By tying their tails tight with a fine silk thread, I deprived them of this benefit, and, by this means, ascertained that the external surface of their bodies destroyed a part of the oxygen of the atmospheric air; although this destruction was more considerable when the action of the respiratory organs remained unimpeded. I was thus enabled to compare, with precision, the proportion of the oxygen gas destroyed by repiration, and that destroyed by the cutaneous membrane.

By the employment of similar means, but accommodated to the nature of fishes, it was not difficult to suspend the respiration of some of these animals without injuring their gills; and I was able to ascertain that this class likewise consume oxygen, not only by their gills but also by the surface of their bodies.

As serpents respire by the mouth, it was easy to confine their bodies in long tubes filled with air, leaving their heads freely exposed; their skin, however, although covered with scales, likewise absorbed oxygen gas, but in a much smaller quantity than that which was taken in by the lungs during respiration.

Oviparous quadrupeds were well calculated for the purposes I had in view. With some address, it was easy completely to cut out their lungs, yet they nevertheless continued to live for a longer or shorter period. This circumstance enabled me to make a great number of experiments, from all of which I learned, with astonishment, that the large lungs of these amphibia, which are continually receiving and expelling air during the alternate motions of respiration, can scarcely however, contrary to the generally-received opinion, be appealed to in this respect, since the consumption of oxygen gas by the lungs is extremely small in comparison with that which is absorbed by the external surface of their bodies.

This absorption of oxygen gas by the external surface of the body of these animals, is so necessary to those which have been deprived of their lungs, that they perish much sooner when placed in any mephitic gas than in the common air. In like manner, they live a shorter time in atmospheric air than in pure oxygen gas, as I have frequently observed in those insects in which the action of the lungs had been interrupted, as well as in those species of fishes which can live some time in the air after the function of respiration has been suspended. The skin of those oviparous quadrupeds, which I have already mentioned, is not the only organ by which the oxygen gas is absorbed; the denuded muscles possess the same property, as I have witnessed in those animals the skin of which had been taken off, and which lived some time after this operation.

In fine, I have repeated these experiments upon the different classes of birds and quadrupeds with similar results: they absorbed more or less oxygen gas, when, by various methods, I forced them to remain enclosed in air-tight vessels in such a manner that, whilst their heads remained without the vessels, their bodies were wholly immersed in the air contained in them, without having any communication with the atmospheric air which they respired.

I have hitherto only spoken of the consumption of the oxygen gas contained in the atmosphere, by the external surface of the bodies, both of respirant and non-respirant animals; I shall only farther observe, that, whether they remained during a longer or shorter time in a given quantity of atmospheric air, it was wholly deprived of its oxygen gas: but is this destruction of the vital part of the atmosphere produced by the absorption of the base of the oxygen gas by the animal body, or only by a carbonaceous principle escaping from the skin of the animal which combines with this oxygen gas? This question, which I conceived it necessary to investigate with the greatest attention, naturally presented itself to my mind, because in each of those experiments I always found more or less of carbonic acid gas. The numerous facts which I shall relate incline me, however, to believe with confidence, that the base of the vital air is actually attracted and absorbed by the skin of animals.

In the following experiments, in place of the expression *destruction of oxygen gas*, I shall employ that of *absorption*, as being less general, and more conformable to truth.

ON

THE RESPIRATION

OF SOME

TERRESTRIAL TESTACEA

AND SNAILS WITHOUT SHELLS.

MEMOIR FIRST.

CHAP. I.

Helix nemoralis. LINN.

Common garden snail.

§ I.

THERE are doubtless few individuals in Italy, or in Europe, who do not remember the rigorous winter of 1795; but, in confining myself to speak of that which was felt in Lombardy and at Pavia, I may observe that, during the twenty-nine years I had been an inhabitant of this city, I never experienced a winter equally long and severe. For, independently of the prodigious quantity of snow which fell in the course of that year, and which covered the ground from the 25th December, 1794, to the beginning of March, the cold was so intense in the latter end of January and the commencement of February, as to make the mercury in the thermometer descend during the night, and sometimes even in the morning, to 8, 9, 10, and even to 11, 12, and 12¾. These observations were made with very sensible thermometers exposed towards th nort h.

§ II.

This rigorous season being favourable for my purpose, I was induced to enter upon a series of experiments on the different animals which become torpid by cold; I therefore subjected to examination not only those having the pulmonary organs of a vesicular structure, but likewise others in which the lungs are membraneous, in order to observe under these circumstances the various phenomena of respiration, the change induced in the air inspired, as well as in the circulation, the consideration of which cannot be separated from that of respiration, on account of the intimate connection subsisting between these two functions, so essential in the animal economy: but a learned memoir by the celebrated Vauquelin, inserted in the twelfth volume of *Annales de Chimie*, published at Paris, under the title *d'Observations chimiques et physiologiques sur la respiration des insectes et des vers*, which fell into my hand, induced me to extend my observations to some of those animals that are wholly destitute of proper lungs. The examination of some individuals comprehended under these two genera by this illustrious chemist, and on which he had made several experiments, convinced him, first, that the oxygen gas of the atmosphere is equally necessary to the support of life in these animals, as in the other classes provided with proper lungs: secondly, that, among these, several snails, whether with or without shells, answer, in his opinion, the purpose of excellent eudiometers, since they possess the power of separating, with the greatest accuracy, the oxygen from the azotic gas; and, consequently, of determining the constituent principles of atmospheric air.

§ III.

As these two beautiful and interesting discoveries were well calculated to rouse my curiosity, I became anxious to ascertain if they would be verified in other kindred species. I was not able, during this extremely cold season, to find the *Limax*, Linn., or the naked snail; but I happened to discover some of those species provided with shells: at first, I met with *Helix nemoralis*, Linn., which multiply abundantly in the fields and gardens around Pavia. This species was perfectly familiar to me, from having made some observations of a different kind upon them during the preceding winters, as well as from having amputated the heads of several of them, every spring, in order to shew my students, when treating of animal reproductions in my public lectures, that their heads were regenerated after repeated decollation.

As I proposed at the commencement of the present work to join to my chemical researches observations on the natural history and physiology of the animals I examined, in order to render it more varied and complete, it is necessary that I should first convey to my readers an idea of the nature and habits of this worm.

§ IV.

The shell, or envelope, of this animal, is nearly of a globular form; its diameter, in the greatest number of individuals, is usually fifteen lines; its surface is smooth; its colour blackish, but striped with small whitish belts; the lips are dark, and the external surface of the shell is furrowed by five spires or volutions.

§ V.

The worm which inhabits these shells is of a cinereous brown, and, in common with all the kindred species, is hermaphrodite: it is oviparous; and, during the fine season, lives on the surface of the earth, where it is nourished by vegetables; but at the approach of winter it buries itself under the ground, where it remains until the commencement of spring.

In October this animal enters the earth, and shuts the aperture of its shell with a very thin membranaceo-calcareous operculum, white in the outside and yellow within. It remains immoveably fixed in this situation until the beginning of April, at which time it awakes from this kind of torpor; and, animated by a mild atmosphere, seeks the light as much from a sense of hunger as from a desire to propagate its species.

§ VI.

During winter these snails are constantly found from one to four inches under the surface of the earth; at which time the operculum is closely shut, and when they retire from their hole the opening is usually turned downwards. It is commonly around the bottom of walls that are covered with tiles projecting outwards that these animals conceal themselves, as in this situation they are completely sheltered from the rain; but they may also be found in places that produce abundance of vegetables, or where there are many stumps and roots of trees.

§ VII.

Some naturalists are of opinion that these snails remain at a greater or less depth in the earth, according to the higher or lower temperature of the season. I believe this to be the case with frogs, toads, and salamanders, but my opinion is different with respect to snails: in order, however, to ascertain this fact, I waited until the first hoar-frosts of autumn had fallen, and the snails were concealed in the bosom of the earth and had closed their shells; I then placed, perpendicularly in the ground, a rod pointed at the extremity, in such a manner as slightly to touch the body of the shell, and I allowed it to remain fixed in this situation. I afterwards attended to the encrease and diminution of the temperature of the air, in order to visit the snails I had thus marked; but I could never perceive the slightest variation in the position of the shells. I repeated this experiment on several individuals of this species during two years successively, and always with similar results.

§ VIII.

Visiting one day the subterranean dwelling of my snails, I observed a fact which seemed to me worthy of attention. Some time in the month of February, 1793, when the wind blew from the south, I removed the earth which covered these animals; the rods which I had planted precisely marked their situation in the ground. In the open air the thermometer in the shade stood at 8°, but at that depth under the earth where the snails lay buried it indicated only 6°. I carried a few of them with me, and placed them on a window in the evening, without entertaining the most distant idea that they would leave their shells, more especially as a little after sun-set a very cold north wind began to blow, which caused the thermometer to descend in a very few hours to 1°: about three o'clock in the morning, however, going accidentally to the place where I had left the snails, I was much surprised to see them nearly out of their shells, and crawling on the surface of the window: on the following morning I found them attached to the sides of a neighbouring wall. I became afterwards convinced that these animals do not become lethargic at 1°, or above zero, of Reaumur's thermometer; as even when enclosed in their shells, they can be made to leave them and crawl about, during the continuance of this temperature, by cautiously breaking a small portion of the spires near the apex of the shell, and slightly pricking the animal by means of the point of a straw introduced through the opening.

§ IX.

Why, then, do these animals remain in

their shells, buried in the earth, when the temperature of the atmosphere is from 6 to 8°, and only leave their retreat when it rises to 10 or 12°, at the commencement of spring; whilst, on the contrary, if they be removed from their subterranean habitation and transported to the open air, they seem to disdain remaining in their little cells although exposed to a much lower temperature? It should seem that the motion they experience during their removal may produce in their system an excitement, and thus impart to it that degree of vigour and energy which had been suspended during their long repose. Or rather we should attribute this effect to the lively impression of the air, which, penetrating the subtle operculum by which they are then covered, stimulates them to resume their activity. Be this, however, as it may, it is certain that, should they make their appearance on the earth before their usual time, they would be wholly destitute of nourishment, as the vegetables on which they feed do not spring up at such an early period of the season; and they appear, at least, only to leave their winter retreats when the food necessary to their existence may be found in abundance. I have observed this to be a constant law of nature among all the animals which sleep during winter*. At least, if we perceive some species of this class in Lombardy running upon the ground, or flying through the air, during some mild days towards the end of winter, it is only a few small lizards or bats, which subsist at that time on the insects they can find, as I have frequently had occasion to remark, by finding the prey they had swallowed remaining in their stomachs when I have killed and opened them during this period of the season.

* This appearance of death in these species of animals, during winter, should be termed *lethargy*, or *torpor*, rather than true sleep, and it is by these terms I have characterised it in the sequel of this work. I have sometimes, however, employed that of *sleep*, although less proper, because it is frequently used for this purpose by a great number of naturalists.

§ X.

I had made these observations on snails at different times before the rigorous winter of 1795; at which period I was induced, by the reasons already mentioned, to undertake some experiments relative to the function of respiration in these animals. On the 3d of February, of this year, I caused the snow which had fallen in the course of January, and now lay thirteen inches deep, to be removed from the corner of a garden in Pavia, where I had found these snails during former winters. Since the first fall of snow in December (§ 1) it had greatly increased; but, as the ground at that time was not frozen, I did not suppose it would since have become affected by the frost, because a great quantity of snow acts as a powerful means of preventing the cold from penetrating into the earth which it covers; vegetables even continue to thrive beneath the snow, whilst they would otherwise suffer during very severe winters;

but in this year the cold was so extremely intense that the earth, notwithstanding the snow, had contracted the hardness of stone, and I was obliged to cause it be broken by means of a pick-axe ; it formed a frozen crust from four to eight inches thick, according to the difference of exposure, and in this icy crust were the snails enclosed.

This fact afforded me an additional proof that these snails do not descend farther into the earth when the cold becomes greater, as some naturalists have supposed (§ VII).

An examination of those snails, contained in the icy crust, convinced me that they were all frozen in a greater or less degree ; but they could easily be recalled to life by gentle warmth, and the heat of a chamber with a stove, or of a hot-house, completely re-animated them. It was with these snails, collected during several days, that I began the experiments already mentioned.

§ XI.

Whether these snails possess respiratory organs? was the first question I conceived it necessary to ascertain, in order to fulfil the intentions I had proposed to myself; and the following are the results of the repeated experiments I made on this subject.

If we observe attentively the aperture of the shell when the snail is within it, we may readily perceive, in that part which is somewhat to the left, that the animal alternately opens and shuts a round hole, the diameter of which, during its greatest dilatation, is about a line and a half; and, when viewed by a strong light, we discover that this opening is extended even to the shell, which we distinctly perceive on the opposite side. This hole—which opens and dilates, contracts and shuts, at the will of the animal, without any regard to fixed periods—did not escape the accuracy of Swammerdam, who conceived it was intended as a passage for the air: on the opening of this aperture a slight noise may be distinctly heard, occasioned by the exit of that fluid; but the investigation in which he was engaged rendered it unnecessary for him to extend his researches on this subject.

Wishing still more accurately to ascertain the termination of this aperture, I carefully removed the first volution of the shell; by which means, without in the smallest degree injuring the animal, I obtained a complete knowledge of the mechanism of that organ.

After this operation, it is easy to perceive that this hole leads to a membraneous bladder situated in the back of the snail, which appears to supply the place of lungs, since it not only becomes inflated by the air it receives through the hole, but since that air, after being retained some time, is again expelled by the animal, as may be observed from the slight noise that accompanies its expulsion, as well as from the effect produced by it on the flame of a small candle exposed to the opening.

Notwithstanding the breaking of the shell, the animal continued to inspire and expire the air as usual: at each inspiration the lungs were inflated for some time, but became colapsed and wrinkled on the succeeding expirations; in snails, however, the inspirations and expirations do not succeed so rapidly as in warm-blooded animals. If we blow through a small tube introduced into the hole, which I shall hereafter describe, the lungs become so much extended as to contain an air-bubble of a pretty large size.

Through the pulmonary membrane the motion of the heart can be readily observed; and, on cutting this membrane lengthwise, we perceive that this organ is so placed as to be continually surrounded by the air that is respired by the lungs.

§ XII.

Besides the other proofs I shall shortly offer in confirmation of the opinion, that this air is necessary to the existence of these snails, it may be rendered manifest by this circumstance alone, that they perish at the end of some days in the exhausted receiver of an air-pump, and even when confined under water; in which last case I dare venture to affirm their death is in consequence of suffocation.

When I kept them under water in a vase, that I afterwards reversed on a plate filled with the same fluid, they betrayed evident symptoms of uneasiness: at first, they pushed out of the shell their head, the four antennæ or tentacula, the neck, and afterwards every part of the body they could draw after them. They attached themselves to the sides of the glass, or they rose up, descended, and forcibly endeavoured to escape. When the temperature was low, they usually died at the end of three or four days; but in a much shorter space of time, when it was somewhat higher.

§ XIII.

The weight and bulk of these snails become considerably augmented by the great quantity of water which they imbibe during the preceding process. Having weighed one of them before immersion, I found its weight to be 358 grains; but, on again weighing it when taken out of the water, it had encreased 252 grains. So slowly do these snails throw off the fluid they have thus absorbed, that sometimes twelve or even fifteen hours elapsed after their removal from the water before they were reduced to their original weight.

§ XIV.

From so great an absorption of water, we are led to suppose, that these animals must be provided with a great number of absorbent vessels, or at least pores, which open on the surface of their bodies when they are plunged into the water. That such a quantity of water is hurtful, and even fatal to this genus of worms, cannot be doubted; but it seems probable, in this case, that the abstraction of the air produces a kind of dropsy. A certain quantity of water, however, is agreeable, or rather necessary, to these animals; on which account nature seems to have given to their bodies a texture well suited to imbibe moisture; at least, we observe them seek out with as much avidity a humid atmosphere, as they avoid, with terror, one that is dry. They delight to run about during the night, and to crawl on those plants that are covered with dew, which they feed upon in preference to those of the most tender and succulent kinds; and in summer, during a great fall of rain, these snails, although for several months they have remained attached to the roots of trees, stones, or the foundation of walls, and other bodies not exposed to the rays of the sun, will, nevertheless, burst the operculum by which they are enclosed, and leave their shells even during the continuance of the rain. Under similar circumstances, I made the following observations: first, that a single repast was frequently sufficient to restore to their bodies that bulk of which it had been deprived by their long abstinence, so that it completely filled the shell, which before appeared as if almost empty; secondly, that, when they are in a place sheltered from rain, the humidity alone with which the atmosphere is loaded in the morning will induce them to go out, so that these worms may be considered as a kind of rude hygrometers.

§ XV.

Before entering on the principal part of my subject, I shall briefly relate the observations of Vauquelin. The *helix pomatia* of LINN., or the vineyard snail, was the subject of the experiments of this celebrated chemist. He confined a single individual of this species in a recipient containing twelve cubic inches of atmospheric air, in which he allowed it to remain four days; at the expiration of which, the oxygen gas was so completely decomposed or absorbed, that phosphorus would not burn in the azotic gas which remained, but this residue contained a portion of carbonic acid gas. It is observed by this illustrious man, at the conclusion of his memoir, that the worm in question, from its completely depriving common air of its vital principle, would answer the purpose of a very exact eudiometer.

§ XVI.

I observed that the phosphorus of Kunckel was employed, by the French chemist, to ascertain whether any oxygen gas remained in the air wherein his snail had been confined. This mode is certainly one of the most simple, and I had recourse to it in making the experiments I am about to relate, with the eudiometer of the celebrated Giobert; because I found this instrument, by its enabling me to institute accurate comparisons, the best adapted to my purpose. The description of this eudiometer has been already given to the public by Giobert, and afterwards by myself, in a small work*, which I trust will supersede the necessity of again repeating it in this place.

* *Esame chemico degli esperimenti del signor Gottling sopra la luce del fosforo osservata nell' aria commune ed in diversi fluidi aëriformi permanenti.*

I shall only, in general, observe, that this instrument is composed of a small cylindrical tube of white glass, the internal diameter of which is half an inch, and its length about eighteen inches; it is bent so as to form nearly a right angle at two-thirds of its height, hermetically closed in the superior, and open in the inferior part; it is divided into one hundred equal parts, from the upper to within two or three inches of the aperture in the lower extremity of the tube. When it is filled with water, and plunged into the same fluid, we can force into it even one hundred degrees of air, or any permanently-elastic fluid we wish to examine: we then apply the flame of a candle to the hermetically-sealed extremity, where the phosphorus is placed; by its combustion the base of the oxygen gas confined within it is absorbed, and in this case the water, into which the tube had been plunged, is even raised to about twenty hundredths, when atmospheric air was made the subject of experiment; from which it results that this air contains 0.20 of oxygen gas and 0.80 of azotic gas, although I have proved in a Memoir already mentioned that the vital portion of the atmosphere is not wholly destroyed by the phosphorus ; but as we know precisely the very small quantity of oxygen gas which remains, it cannot vitiate the experiments, more particularly as those conducted by this instrument are extremely proper to be compared with each other.

§ XVII.

Such is the mode I employed from the commencement of my experiments. I placed one of my snails on mercury, in a pneumatic apparatus, covering it with a small tube of glass hermetically sealed at the top ; and I afterwards introduced a bent syphon, in order to extract the air from the tube, until the mercury was elevated to about the height of an inch : I then replaced the small tube on the shelf of the apparatus, covered with a thick coating of this metal. The volume of air in which the animal was confined I found to be seven cubic inches, and I allowed it to remain six days in this situation, during which time the temperature was from seven to eight degrees, and the mercury rose to two lines and a half.

We must beware that the oxygen gas be not absorbed, in combining with the mercury, or in oxydating its surface; at least a pellicle has been observed, which has been supposed to proceed from the oxydation of this metal.

§ XVIII.

I next wished to ascertain the precise quantity of carbonic acid gas produced by this animal, in the same manner as that generated by all the animals of which I shall have occasion to mention in this work. I could not obtain this knowledge by filling the eudiometer with water previously to the introduction of the air, because this gas would have been more or less absorbed by the water, on which account I had recourse to the adoption of mercury in the whole of these experiments; but I only employed this metal when certain it was perfectly dry, and consequently that the carbonic acid gas mixed with it remained unchanged*.

After having filled the eudiometer with mercury, and introduced into it a portion of the air which had been changed by the snail, so as to occupy a hundred parts, I afterwards washed it in lime-water, and re-introduced it into the eudiometer, on which the mercury rose to 11°, consequently this snail had produced eleven degrees of carbonic acid gas†. The flame of a candle, externally applied to the tube, occasioned a rapid combustion of the phosphorus, accompanied with a transient smoke; and, when every thing was again brought back

* In order to convince myself that the carbonic acid gas was not changed by the mercury, I allowed eleven inches of it to remain upon this metal during fifteen days, without perceiving any sensible diminution in its volume, or the least alteration in its qualities.

† In this and the following experiments, this gas precipitated the lime, and formed with it calcareous carbonate.

to the former temperature, the mercury had risen to $11\frac{1}{3}°$. The phosphorus had then absorbed $\frac{1}{3}$ of a degree of oxygen, from which it follows that the destruction of this gas by the snail had not been complete.

§ XIX.

The circumstances detailed in § XVII and § XVIII relate to the effect produced on the air by a single snail. Having subjected three of these animals to a similar experiment, I ascertained that they died more quickly, surviving only three days and seven hours; that they produced $8\frac{1}{4}$ degrees of carbonic acid gas; and that, after having lighted the phosphorus, and allowed the apparatus to cool, the mercury ascended $\frac{1}{3}$ of a degree in the eudiometer—consequently these snails had not absorbed all the oxygen gas contained in the atmospheric air.

§ XX.

Being anxious to ascertain if it were possible to obtain a complete absorption of the oxygen gas by means of these snails, I endeavoured to bring it to the proof, by confining a greater or less number of them in common air, at different temperatures.

When the air enclosed in the eudiometer is entirely deprived of its oxygen gas, if we kindle the phosphorus it is observed to dissolve and boil up, whilst the air by which it is surrounded always remains very transparent; but, on the contrary, is obscured for some moments, and becomes whitish, if it be mixed with the smallest quantity of oxygen gas, which may then be discovered by these appearances. This phenomenon is always more or less observable in the atmospheric air wherein these snails are confined.

The burning of phosphorus did not indeed occasion a very sensible elevation of the mercury in the tube; but the nitrous gas evidently shewed that a portion of oxygen gas was contained in the atmospheric air wherein these snails had respired. Having forced into the eudiometer of Fontana a given quantity of this air, and mixed with it an equal proportion of nitrous gas, the total volume of 200 parts was reduced to 194, 195, and at the highest to 196. Hence the quantity of oxygen gas was indicated by the diminution of the degrees in the eudiometer 6, 5, 4.

§ XXI.

I have remarked, in § XVII, the temperature that the snail, confined in seven cubic inches of atmospheric air, experienced during six days. This remark is not unimportant, since I have observed a very considerable difference of time in the death of these snails, according to the difference of temperature to which they were exposed, although the volume of the air wherein they were confined was the same.

After a very great number of different experiments, I found, that it may be established as a law, that the death of these snails is more rapid in proportion to the mildness of the temperature in which they are kept during their confinement, and that they die so much more slowly as it is reduced: in the first case the oxygen gas disappears sooner than in the second. The same law prevails among the animals that become torpid during winter; respecting which I shall have occasion to offer several proofs in the course of the present work.

The reason of this difference is evident: these animals belong to the cold-blooded class; for if some of them appear to form an exception to this rule, they seem from the effects produced on them by cold to be naturally included under it. We observe that they are enfeebled by too low a temperature; that they become benumbed and inanimate: we observe, also, that they are roused by a mild atmosphere, which restores their powers and augments their energies. In the first case, their organs are much less agile and susceptible of action than in the second, on which account the consumption of the oxygen gas is carried on more slowly, and consequently these snails will die later; since it appears demonstrated, by the preceding experiments, that oxygen gas is necessary to their vitality.

§ XXII.

By gradually reducing the temperature, I reached that point when the air, in which the snails were confined, underwent no longer the change of which I have spoken. The principal subject which I propose to consider in this Memoir, in treating of animals which become torpid during winter, is that of fixing the degree of cold which renders them lethargic, and that by which they are destroyed, in order to examine the changes and alterations observable, under these circumstances, in the respiration, in the action of the heart, and in the circulation.

I have already observed that the coldness of the air did not render the snails torpid with which I was occupied (§ VIII); that it was necessary to reduce it to the freezing point, and that even they did not die although it was still more intense: it was not until the mercury in the thermometer was at 2 that they became frozen in a chamber, in my house at Pavia, adjoining to one heated by a stove which I myself occupied during winter, and in which the thermometer marked this degree during several nights: in this situation the snails became frozen, which occasioned their death, although during the day the thermometer was frequently at 0: at this last degree of cold the snails only became torpid; and when I confined them, in this state, under a recipient, they produced no alteration whatever in the air with which it was filled. I always found the usual proportion of 0.20° of oxygen to 0.80° of azotic gas undiminished; but when I made the same experiment in the chamber wherein the thermometer indicated about $8\frac{1}{2}°$, the decomposition of the air became evident, and in some hours the oxygen gas had completely disappeared.

§ XXIII.

But it is natural to enquire what phenomenon is exhibited by the heart during this state of torpor and inaction of the organs, considered in relation to the decomposition of atmospheric air?

I have already observed that, in proportion as the temperature is lowered, the pulsations of the heart become less frequent; at the freezing point they are still more retarded, and when the thermometer is at — 1, they altogether cease; at least, for a full quarter of an hour at a time, in this situation, I have viewed attentively the heart of these animals, and observed it remain perfectly at rest. I likewise remarked this immobility from the stagnation of the whitish fluid which in these snails supplies the place of blood; since, while the circulation of this fluid continues to be carried on through their venous and arterial vessels, the trunks of which open into the heart, its motion is evidently produced by the systole and diastole of that organ.

By thus observing the reciprocal action of the solids on the fluids altogether discontinued, we comprehend much better how these small animal machines cease to absorb oxygen from the air; they have no farther occasion for this absorption in order to preserve the vitality of the system.

§ XXIV.

I have little hesitation, therefore, in believing that, during winter, when these snails are buried under ground, their respiratory organs, or those which supply their place, as well as the others which concur in the circulation of the humours, remain in a state of absolute rest; for although the temperature which they experience, in their retreats, be somewhat above the freezing point, as I have proved, they nevertheless remain in a torpid state; which indeed should be the case, as the operculum, by which the aperture of their shell is covered, remains perfectly shut, and consequently precludes all communication with the external air. My second reason is founded on this circumstance:—These snails, selected of an equal size, after having taken food, were enclosed in their shells by the operculum formed of their slime; I raised up this opercule in one of these individuals, and confined it under a recipient containing eleven ounces of atmospheric air; I placed, likewise, another of them in a recipient containing the same quantity of air, and in similar circumstances, with this difference, that I suffered its operculum to remain entire. The thermometer indicated at the time 15°. At the expiration of thirty-two hours the air wherein the snail had remained with its shell open lost $\frac{1}{5}$ of its oxygen gas, whilst that in which the other was placed with its shell entire preserved 10°; in the atmosphere of the first 7° of carbonic acid gas were generated, and in that of the second $2\frac{1}{2}$°.

Although this operculum be formed of two transparent and very subtle pellicles, we nevertheless observe that they are penetrable by the air, and that they serve the purpose of a very fine sieve, for the decomposition of the greatest part of the oxygen gas contained in the atmosphere; the confirmation of which I have witnessed in several similar experiments.

The result of all these experiments is, that the snails which are shut up during winter, by this operculum covering the aperture of their shells, and which are then in a state of torpor, are only reduced to this situation from a suspension of the vital functions; and that it is to this suspension we must attribute the atmosphere with which they are surrounded remaining unchanged, since the air can readily penetrate through the pores of the earth, and the subtle coverings by which they are enveloped.

§ XXV.

But my principal object was to ascertain if the snail *Helix nemoralis* consumed all the oxygen gas of the common air; and consequently, if it could be usefully employed as an eudiometer. I have already clearly shewn that it cannot answer this purpose, since I always found some portion of oxygen gas in the air wherein these snails were confined; and even though this gas should be wholly decomposed, it appears to me that they would still be found but imperfect eudiometers, since it is likewise necessary that the azotic gas, another component part of the atmosphere, should be unchanged. It was, then, of importance chiefly to direct my attention to this point; but neither the method of Vauquelin, nor that hitherto employed by myself, was sufficiently satisfactory.

By means of the first we could ascertain the precise quantity of oxygen gas, as well as the presence of carbonic acid gas, but not the quantity produced of the latter. By my method, which consisted in filling the eudiometer with a portion of the air in which the snails had died and analysing it, I readily attained a knowledge of the precise quantity of the oxygen and carbonic acid gas, but not of the azotic gas; at least in those cases, which are not unusual, where the carbonic acid gas is trifling in comparison with the oxygen gas which had disappeared.

I was still ignorant then whether the azotic gas had augmented, diminished, or remained the same; which circumstance I have at least demonstrated by the following example.

I filled the eudiometer with a portion of this air, from which a living animal had absorbed all, or nearly all, its oxygen gas, so that the azotic gas and a small portion of carbonic acid gas alone remained, which last I removed before introducing the air into the eudiometer, in order to simplify the experiment. It is then evident that the eudiometer contained neither more nor less than 100° of azotic gas, whether the proportion of this gas had been augmented, diminished, or remained the same.

We can affirm so much respecting it, when the oxygen gas had been only partly destroyed by the animal, as may be readily perceived.

§ XXVI.

I come now to detail the new method to which I had recourse, for the purpose of ascertaining the proportion of the azotic to that of the oxygen and carbonic acid gas.

I filled with air an eudiometer that

had been previously filled with mercury, and afterwards made that air flow into a cylindrical tube, containing likewise this metallic fluid, which I reversed so that the hermetically-sealed extremity was uppermost, and the other rested in the mercury; I then caused one or several snails, or some other small living animal, ascend into the tube; and, to prevent them coming into immediate contact with the mercury, I placed under them a small plate of glass on which they rested during the experiment. The glass plates I employed in my experiments were always somewhat less than the calibre of the tube, as in this way the animals were separated from the mercury, which might have proved hurtful to some of them. As soon as I perceived the animal was dead, I poured out the air from the eudiometer, and observed the height to which the mercury ascended. If we suppose that it rose to the twelfth degree, and that the burning of the phosphorus did not occasion any farther elevation: if we suppose, besides, that, after washing this residue of the air in lime-water, and returning it into the eudiometer, the mercury ascended to the twentieth degree: it is then evident that the animal must have consumed all the oxygen gas; that it will have produced eight degrees of carbonic acid gas, and left the original quantity of azotic gas.

§ XXVII.

If we suppose, secondly, that, after having introduced the air into the eudiometer, the elevation of the mercury was at 10°; that the burning of the phosphorus produced no change in its height; and that, after withdrawing the carbonic acid gas, the mercury ascended to 16°;—it must follow that the oxygen gas has been consumed by the animal; and that to the 80° have been superadded 4° of azotic gas.

If we suppose, lastly, that the elevation of the mercury was at 15° and at 25°, after washing the air in lime-water, and withdrawing the carbonic acid gas; and that it ascended no higher on burning the phosphorus; it must readily be perceived that the animal, after having produced 10° of carbonic acid, will have completely destroyed the oxygen gas, as well as 5° of the azotic gas.

In this manner we may always ascertain whether the azotic gas has encreased, diminished, or remained stationary, not only when the whole of the oxygen gas has been destroyed, but even when the destruction of this gas has not been complete.

§ XXVIII.

The quantity of atmospheric air contained in the eudiometer was four ounces and one-eighth of an ounce; it was transferred into the tube where I confined the snails, and afterwards returned into the eudiometer. The experiment was made first with a single snail, and I repeated it for the most part in a mild temperature, as by this means the results were more quickly obtained. I allowed the snails to remain in a state of confinement until they ceased to exhibit any signs of vitality; and the following are the results of four experiments conducted with this view*.

	Oxygen gas destroyed.	Carbonic acid gas produced.	Azotic gas destroyed.
1 snail,	20	7	5
2 ——	16	5	3
3 ——	18	6	4
4 ——	20	8	8

I repeated the above experiment on four pair of snails, and the following are the results which I obtained in half the time required to cause the death of the former snails.

* Having remarked that the snails, on issuing from their shells, allowed the air to escape, which again surrounded them on their re-entrance into these habitations (§ XI); it was incumbent on me to attend to this circumstance, in order to render my experiments sufficiently accurate. Before, therefore, introducing the snails into the tubes, it was necessary that they should be shut up in their shells; and on withdrawing them, in order to analyse the air, I forced them to re-enter these abodes, if they had again left them, or, at least, I pressed them under the mercury, so as to force into the tubes the small portion of air which these animals might contain.

	Oxygen gas destroyed.	Carbonic acid gas produced.	Azotic gas destroyed.
1 pair of snails,	20	3	2
2 ————	$19\frac{1}{3}$	4	6
3 ————	20	8	6
4 ————	17	3	5

From these eight experiments, we learn—First, that the oxygen gas is constantly destroyed by the snails. Secondly, that there is likewise constantly a production of carbonic acid gas. Thirdly, that there is always a greater or less destruction of azotic gas.

We discover, farther, that if the oxygen and azotic gases be destroyed, or absorbed, by the snails kept in confined air, the destruction of the first is much greater than that of the second, as appears from the facts that I have related.

Lastly, in these experiments, as in all the others I have made on snails confined in the common air, the internal surface of the tubes in which they were shut up appeared always somewhat humid.

§ XXIX.

I have already mentioned that the snails were left in a state of confinement until all signs of life had disappeared; and it was not until I had observed them remain some time altogether without motion, although shaken by means of a brass wire which I passed under the mercury between the glass plate and the tube, that I withdrew them from that situation. Some of these snails appeared to me really to be dead: but others of them recovered, by slow degrees, both sense and motion; they abandoned their shells, and crawled from one place to another, from which it should seem they had only experienced a high degree of asphyxia. I remarked, however, that two snails, affected in this manner, had wholly consumed the oxygen gas of the air, since the mercury ascended in the eudiometer to 20°.

I was not then surprised that these snails recovered at the end of some time, because I had seen others of them able to remain in the mephitic gases—as, for example, the hydrogen, nitrous, and carbonic acid gas—for as long a time, without being deprived of vitality.

From some of the snails surviving the destruction of the oxygen gas of the atmosphere, I was convinced that these worms, which had consumed 20° of this gas, were preserved by the 5° or 6° which still remained.

§ XXX.

The experiments related in § XXVIII and § XXIX being compared with those detailed at the beginning of § XXV, clearly demonstrate that this species of snail cannot answer the purpose of an eudiometer, because it does not absorb the whole of the oxygen gas, and because it consumes a portion of the azotic gas of the common air.

§ XXXI.

If these snails decompose the impure oxygen gas, or that which, in atmospheric air, is mixed with the azotic gas, it might be readily imagined that they would produce the same effect on pure oxygen gas, and even probably consume a greater quantity of it in a given time. It was necessary, however, to bring this opinion to the test of experiment, by which I was convinced that I had not been deceived in my conjectures.

The following are the results of my experiments on four snails; two of which remained thirty-six hours in $4\frac{1}{8}$ inches of atmospheric air, and the two others, during the same length of time, in an equal bulk of oxygen gas, drawn from the red oxyd of mercury, by means of the nitrous acid, in order to obtain it extremely pure. I have always employed this in the whole of my experiments.

The two snails confined in the common air consumed 20° of oxygen and 4° of azotic gas, and produced 6° of carbonic acid.

The two snails enclosed in the oxygen

gas consumed 38° of this gas, and produced 14° of carbonic acid gas.

We thus learn, however, that the snails decompose a greater quantity of oxygen gas when it is pure than when it is mixed with azotic gas; but that they also produce a greater quantity of carbonic acid. This phenomenon is altogether natural: it necessarily results from the composition of the carbonic acid gas, which is formed of seventy parts of oxygen and twenty-eight of carbon combined with caloric.

§ XXXII.

I might have terminated my researches, especially those I had devised in order to discover the changes effected in the common air by the presence of living snails: it seemed to me useless to endeavour to ascertain the modifications produced on the oxygen gas by the dead animals, since the power of their respiratory organs was at least greatly diminished. As, however, I was always in the habit of varying and extending my experiments to every part of any subject with which I was occupied, however distant it might appear from my original views; and as I have always found this mode of interrogating nature productive of discoveries; I conceived it proper, in the present instance, to extend my enquiries somewhat farther.

After examining all the modifications produced in the air wherein living snails were confined, I next endeavoured to ascertain the changes it underwent from the presence of the dead animals: and I may venture to affirm that this new enquiry opened a road that conducted me to several concealed truths; as, by applying it gradually to the more perfect animals, and even to man himself, I have been able to demonstrate that the influence of the oxygen gas of the atmosphere is extended over every living being, much beyond what has been hitherto supposed.

§ XXXIII.

Although the snails are very tenacious

of life, I deprived them of it almost instantaneously, by keeping them some moments immersed in boiling water. Having killed two of them by this means, I placed them instantly in these tubes, which contained $4\frac{1}{8}$ inches of atmospheric air, where they were allowed to remain twenty-four hours. As the vital principle was extinguished in these animals, and as the putrid fermentation had commenced from the mildness of the temperature in which they were placed, I supposed that, animal substances being composed of oxygen, carbon, and azot, I should discover by their decomposition the formation of azotic gas, carbonic acid gas, and water, by the analysis of the air contained in the eudiometer; this I doubtless found to be the case, but I likewise discovered a sensible diminution of the oxygen gas, as will appear from the following proportions:

Oxygen gas consumed, -	9
Carbonic acid gas produced, -	6
Azotic gas produced, - -	5

§ XXXIV.

This experiment made me enter on others of a similar kind, with this difference, that I kept the dead snails successively longer confined, in order to ascertain if the diminution of the oxygen gas was in proportion to the length of their confinement, which I found to be really the case, as at the expiration of fifty-two hours this gas was wholly consumed. The azotic and carbonic acid gas were likewise proportionally augmented.

§ XXXV.

An important question here presents itself, Whether the dead or living snails consume, in a given time and under similar circumstances, the greatest quantity of oxygen gas; or whether the quantity of this gas destroyed in these two cases be equal?

In order to resolve these enquiries, I undertook several experiments, which were repeated and varied in every possible manner to answer this intention: I believe it unnecessary, however, here to detail them; it will be sufficient for me to state, that the consumption of the oxygen gas is much greater by the living than the dead animal. Thus, for example, a living snail consumed the whole oxygen present in the air, that is 0.20; whilst 0.04, or 0.05, or at most 0.06, were only destroyed by the dead animal.

Other causes may, however, be assigned for this than the cessation of respiration occasioned by death; but, on the other hand, if, notwithstanding this cessation, the snails continue to consume, although slowly, the oxygen gas, it is evident that this consumption does not depend on the lungs, but on some other parts of the body; and if this consumption is carried on by the snails after being deprived of vitality, we see no reason why it may not be performed in the same manner by the living snails. I am likewise of opinion that, besides the oxygen gas consumed by the lungs, there is also a portion of it destroyed by that part of the body which is pushed out of the shell at the will of the animal; and, although the quantity thus destroyed is small in comparison with that taken in by the lungs, it is nevertheless continued after death, and extends not only to the consumption of the oxygen gas of the atmospheric air in close vessels (§ XXIV), but likewise to the pure oxygen gas in which these animals were sometimes confined.

§ XXXVI.

To demonstrate this, it is only necessary to relate the following observation: I enclosed nine snails which I had killed, by means of boiling water, in fifteen inches of pure oxygen gas; they were left in this situation twelve days, or as long as the mercury continued to ascend in the tube; on the thirteenth day the gas was scarcely reduced to one inch; a chemical analysis convinced me that about $\frac{1}{3}$ of this inch consisted of carbonic acid gas, and $\frac{2}{3}$ of azotic gas. These nine snails had then destroyed, without the use of their respiratory organs, fifteen inches of oxygen gas;

the temperature was between 14° and 16°, and the snails emitted a very fœtid smell at the conclusion of the experiment.

§ XXXVII.

In reflecting on this neglected phenomenon of the absorption of oxygen gas by the dead snails, an idea occurred to me which appeared at first rather extravagant; but I was tempted to realise it, although without much hope of success.

Herissant had demonstrated, by experiments as conclusive as they were ingenious, that the shells of the testacea are organised in the same manner as the animals themselves; and that they are formed of two substances,—the one earthy, and the other animal, or parenchymatous; and that the last, by means of ligaments, unite with the snail, and form with it one body. He also justly believed that these shells are nourished by means of this mechanism, and that the fluids circulate through them as in the animals which they enclose. If the shells of snails be organised like themselves, and if the snails consume the oxygen gas, may we not conjecture that this absorption is also produced by the shells? In that case it would appear that all the oxygen gas destroyed by these worms in close vessels is not effected by their soft parts, but in some measure by their hard coverings or shells.

In order to ascertain this point, I placed the shells of two snails in the vessels, filled with common air, which I had formerly employed. Before transferring this air into the eudiometer, I had begun to suspect that the oxygen gas had undergone some change, from the elevation of the mercury in the tube. I had at least remarked, in my daily experiments, that this elevation was almost a certain sign of some alteration; accordingly, at the termination of sixty-six hours, having analysed the air, the result was

Oxygen gas consumed, -	$9\frac{1}{2}$
Carbonic acid gas produced, -	3
Azotic gas consumed, - -	0

§ XXXVIII.

Several different experiments, made in the same manner on other shells of these snails, fully convinced me of this truth, and even demonstrated that, when I left the shells a longer time in the enclosed air, they completely consumed the oxygen gas. Farther if, instead of placing these shells in an atmosphere of common air, we enclosed them in pure oxygen gas, the consumption of this gas in a given time was much greater.

I have besides observed, in comparing the destruction of the oxygen gas by the shells with that produced by the living snails deprived of these coverings, in equal time, that in the last case the consumption was much more considerable.

It is necessary, however, to observe, that the quantity of carbonic acid gas produced by the shells was extremely small, although the oxygen gas had wholly disappeared.

§ XXXIX.

The property possessed by these shells, of destroying the oxygen gas, is not only evident when they are recently separated from the animal, but also when they have been detached from it a considerable time, and even then it did not appear to me that they had lost much of this absorbent power. I shall, however, relate a fact which produced in my mind a considerable degree of astonishment.

In a corner of one of my chambers, where I kept the subjects of my experiments, I had several shells of the *Helix pomatia* of LINN., either the vineyard snail or the common garden snail, which had been uninhabited for eighteen months. Having observed that several other snails' shells, of which I shall elsewhere speak, that had been empty for several months, destroyed the oxygen gas, I had the curiosity to ascertain whether they would produce any effect on the common air: with this view, I therefore enclosed six of them in four inches of atmospheric air, after they had been washed in water and again dried. At the termination of six days, I examined the air, and found it deprived of its oxygen gas, of which I fully satisfied myself by the combustion of phosphorus.

This unexpected result induced me to repeat and verify these experiments. I employed three tubes, each of which contained four inches of air, and six shells, in order to observe the progressive diminution of the oxygen gas. At the end of two days, the air in one of the tubes contained only 12½ hundred parts of oxygen gas; at the end of four days, the air of the other tube gave only 6 hundred parts; and, at the end of six days, the air in the third tube was totally divested of it, and the phosphorus burnt in it only a few minutes.

It was by mere accident that I possessed these shells, which had been eighteen months deprived of their inhabitants; but, from this experiment, I concluded, without any hesitation, that the shells of land snails, though for several years untenanted, would continue to absorb oxygen gas.

§ XL.

The shells to which I have hitherto alluded, though pretty old, were in very good preservation; and, on comparing them in all respects with other recent shells, I found that they still retained their natural colours, thickness, solidity, and weight. It should hence seem that their organisation had not been impaired. I conceived, however, that the only property of organisation which they retained was that of destroying oxygen gas; and yet, unless I was mistaken, it would follow, that shells more or less disorganised must have lost this power in proportion to the degree of their disorganisation. It was easy to convince myself of this fact.

In the garden where I had been accustomed to procure my snails, there were some shells the inmates of which had been long dead, and which exhibited very perceptible marks of decay. Their colours, instead of being blackish, had become whitish; the transverse stripes were scarcely discernible; they were become lighter, and crumbled between my fingers; their organic texture was, therefore, altogether, or in a very great measure, destroyed. Having put two of these shells into two tubes filled with atmospheric air, I could not perceive, after the lapse of fifteen days, that they had produced the slightest change in the oxygen gas; whilst, in the same space of time, this gas had entirely disappeared in consequence of being kept in contact with two similar shells which I had deprived of their inhabitants.

§ XLI.

The discovery of this truth led me to conjecture another, with almost an assurance that it would be realised by the test of experiment. I conceived that the successive diminution of the atmospheric oxygen gas would be in proportion to the degree of disorganisation of the shells.

I therefore procured a great number of empty shells, which were collected in the fields about Pavia, and proceeded to put my conjecture to the proof. I selected such as were in different stages of decay, and enclosed them in separate tubes with the same quantity of air. I cannot go so far as to state that, in these experiments, the oxygen gas diminished in proportion to the apparent decay of the shells. There were some of them which, though a little more decayed than others in their organic texture, absorbed a few degrees more of oxygen than such as appeared to be less decayed.

It is, nevertheless, certain, that, when the shells were very little decayed, they absorbed almost the whole of the oxygen gas; that they absorbed only one-half, when in a proportionate stage of decay; and that, when the decay was very considerable, the oxygen gas suffered little or no diminution.

§ XLII.

The existence of the organic texture of

snail-shells has, hitherto, been supported only by the analogical reasoning of Herissant, in regard to the shells of other testacea (§ XXXVII), and the preceding experiments appeared to confirm it. I thought it necessary, however, to give a direct demonstration of the fact, by availing myself of the no less simple than ingenious process of this penetrating anatomist—that of decomposing the shells by means of the diluted nitrous acid. After this decomposition, the earthy part of their texture remains entire, and the truly animal part becomes palpable.

§ XLIII.

I accordingly put some small pieces of these shells into the diluted nitrous acid, so as that the effervescence produced by the extrication of the carbonic acid was inconsiderable, and the basis of the latter free, having been disengaged from its combination with the carbonate of lime, and dissolved by the caloric. In this state only very small gaseous bubbles were formed, almost all of which proceeded from the convex surface of the shell. A few issued from the concave surface, when the nitrous acid was not so much diluted; but in that case it was impossible to accomplish the object of my experiment, on account of the great number of the bubbles of the carbonic acid, and the impetuosity of their escape, which destroyed the consistence of the shell and reduced it into small fragments.

§ XLIV.

Having taken the shells out of the nitrous acid, at the expiration of four-and twenty hours, and washed them in water, I found that they were really composed of a pure earthy substance, joined to a membrane of a fine, uniform, and transparent texture, which covered the external surface. This membrane came off entire; and, on applying it to the flame of a candle, it afforded the characteristic signs of animal matter, by its smell of burnt horn, and its reduction into a black and carbonaceous pulverulent substance.

Notwithstanding the removal of this membrane, the shells preserved their hardness and cohesion, only they were become smaller from the loss of the portion of calcareous earth dissolved in the nitrous acid. But their colours were more lively than when they were covered with the membranes; a proof that the colours did not adhere to these membranes, but that they were imprinted on the calcareous earth.

Having returned the remains of these shells into the acid, the solution of the earth continued, and at length I found nothing left, except some membraneous shreds; which clearly proved that the exterior membrane which I had separated was not the only one, but that the earthy matter was incorporated in a still finer membraneous tissue, incapable, from its extreme fineness, of resisting the little bubbles which escaped from the manifestly organised substance of the shell.

§ XLV.

We may now form the conclusion, that

the absorption of the oxygen gas by the shells was produced by their own organisation, without any assistance from the calcareous earth; since I have already shewn in shells more or less disorganised, but still retaining a considerable portion of that earth, that the absorption of the oxygen diminished as the disorganisation encreased (§ XLIII): but I obtained a more evident proof of this fact by enclosing, for four-and-twenty hours, a piece of carbonate of lime in a tube, containing five cubic inches of common air; at the end of that time, the air had not undergone the smallest alteration.

§ XLVI.

The consequences which may be deduced from these experiments are:—

1. That the *Helix nemoralis* has respiratory organs.

2. That these snails destroy nearly the whole oxygen gas of the atmospheric air, and that it is indispensable to their existence.

3. That they also destroy the azot, but in a smaller degree.

4. That these worms cannot serve as an eudiometer, because a perfect eudiometer should exhibit the entire destruction of the oxygen, and the complete preservation of the azot.

5. That the milder the temperature, the more accelerated is the destruction of the oxygen gas, and also the death of these worms.

6. When the temperature sinks to —1, the absorption of the oxygen gas ceases; but at the same instant also the pulsation of the heart and the circulation of the fluids are suspended.

7. It is probable that this suspension of the action of the heart and the fluids continues in these snails during the winter.

8. As the destruction of the oxygen gas takes place in dead as well as living snails, it necessarily results, that the lungs are not the sole cause of the absorption, but that

there are other organs in the animal which produce the same effect.

9. The shells themselves of these snails destroy the oxygen gas, and continue to do so long after they are deprived of their inhabitants.

10. The shells lose the property of destroying the oxygen gas in proportion to their disorganisation.

11. The carbonic acid gas produced is in proportion to the destruction of the oxygen gas.

12. There is a more or less sensible degree of humidity in the vessels in which the snails are confined.

§ XLVII.

In describing the chemical alterations in the atmospheric oxygen gas, produced by the confinement of snails in common air, I have employed the general expressions of the *destruction*, the *loss*, or the *disappearance*, of this gas; because, in the different analyses which I have made, it has actually been found to have suffered a diminution. It is necessary to define this idea.

This phenomenon has the greatest analogy to that of respiration. A quadruped, a bird, confined in common air, destroy the oxygen gas, and at the same instant produce carbonic acid gas. Snails perform precisely the same operation. The similarity of the effects naturally suggests a similarity in the causes; so that the chemical theory of modern chemists, with regard to respiration, strictly applies to the circumstances above stated.

§ XLVIII.

In the exercise of this astonishing function of the animal economy, a part of the oxygen of the air enters into combination with the hydrogen of the blood, and forms water, which exhales during expiration. Another part seems, at the very same instant, to unite with the pulmonary blood, which hence assumes its lively red colour. A third part unites with the carbon of the blood, and produces the carbonic acid; or, rather, according to the opinion of a celebrated naturalist, the carbonic acid pre-exists in the blood, and escapes in the gaseous form, in consequence of being precipitated or disengaged by the oxygen gas.

§ XLIX.

The very numerous traces of humidity on some points of the inside of the vessels under which the snails were placed, certainly furnished a proof of the combination of some parts of the oxygen with the hydrogen of these animals. This humidity, however, though small in quantity, is not entirely the product of this combination, but must in part be referred to that aqueous viscosity with which their bodies are always covered. The presence of the carbonic acid gas cannot be acounted for, but by admitting the combination of the oxygen with the carbon of the snails; or, as appears to me more probable, and as I shall shew in the sequel, by admitting that the carbonic acid gas issued completely formed from the bodies of these animals.

The most convincing proof that a portion of the oxygen unites with the blood, on being received into the lungs of living respiratory animals, is derived from the experiment which shews that blood brought into contact with the oxygen gas assumes a lively red colour.

The blood of snails is not exactly similar to that of warm-blooded animals. It cannot, however, be denied that a part of this oxygen is absorbed by their bodies, and the argument drawn from this consequence affords an incontrovertible proof of it.

It sometimes happens that these worms on being released from their confinement, whether in atmospheric air or in some mephitic gas, exhibit all the appearances of certain death, though they are not really dead, but gradually recover their former vigour in the open air. This effect is produced by the renewal of the air, or, to speak more philosophically, by the action of the atmospheric oxygen on the bodies of these animals: a circumstance which implies the existence of the oxygen in a fixed or combined state with the animal fibre; particularly after the demonstration given by Humboldt of the cause of irritability, which he attributes to the intimate combination of the oxygen with the fibres of the animal organs. The shells, therefore, must necessarily absorb a considerable quantity of this vital principle; and as the quantity of carbonic acid gas produced by the snails is small in comparison of the oxygen gas which they destroy, and as the humidity which they otherwise produce on the sides of the vessels is extremely inconsiderable, we are compelled to conclude that this destruction results in a great measure from the absorption of the oxygen by the shells. Upon the whole, it is impossible to suppose that the snails do not absorb a great quantity of this principle.

§ L.

It appears, therefore, that the soft and fleshy bodies of these snails, united to the hard incrustation which covers and protects them, and the apparatus of their respiratory organs, occasion those chemical alterations in the air which, in other animals, are produced by the lungs, and that they continue to produce them even after the respiratory organs cease to act, and the snails are deprived of life.

Such is the sense in which I wish the terms *destruction*, *loss*, *disappearance*, to be understood; and though I also employ that of *absorption*, it has precisely the same signification. The idea which it conveys is certainly not perfectly accurate; but, for the sake of greater simplicity of expression, since it accords with appearances, I will not deny myself the advantage of using it.

CHAP. II.

Helix Lusitanica—Helix Itala—Limax agrestis, ater, albus, flavus maximus. LINN.

Snail of Portugal—Snail of Italy—Naked, black, white, yellow, very large, snails.

§ LI.

THE snail of Portugal is somewhat larger than the *nemoralis*, or common garden snail; the colour of the shell inclines to the white or yellowish; it has five volutions; the apertures are nearly circular; the lips are tinged with a slight shade of flesh-colour, and are open as far as the umbilicus.

The organs of respiration in these snails are perfectly similar to those of the *nemoralis*. Their flesh is more tender, and of a more agreeable flavour; and hence the snail of Portugal is in greater request than the other kinds, and fetches a higher price. At Pavia and Milan they are on sale in the shops, during the whole of the winter, and quantities of them are brought in barrels from some parts of Switzerland and the Valais. The snails procured from those quarters are supposed to be more delicate and better flavoured than those which are produced in the gardens and fields of Pavia.

These snails conceal themselves under ground, in autumn, sooner than the *nemoralis*, and issue from it sooner in the spring. It should, therefore, seem, that they are more sensible to the impressions of cold; and of this they furnish a proof, on being preserved within doors. They do not leave their shells at the temperature which induces the *nemoralis* to issue from theirs; and while the latter become lethargic from cold and continue frozen at —2 (§ XXII), the snails of Portugal become lethargic at the same degree, and die when the thermometer sinks to zero.

§ LII.

As soon as these snails have buried themselves in the earth, at their usual depth of from one and a half to four inches, they form out of their viscous humour a membranaceo-calcareous opercle, with which they shut themselves up during the winter, and which they break only at the return of fine weather in the spring.

The thickness and consistence of the opercles of this class are greater than those of any other kind of land snails with which I am acquainted. They are so closely attached to the edges of the internal surface of the aperture of the shell, and adhere to it so strongly, that they should seem wholly to exclude the external air. If this were a fact, the snail could not enjoy a renovation of the air from the middle of September to the beginning of April, and it would remain only in that small portion of vitiated air which is interposed between the opercle and the fleshy substance.

It was an important point in the progress of my enquiry to elucidate this phenomenon, and to ascertain whether the aperture was completely shut by the opercle. To attain this object, I employed the following process. I made a small round hole in the centre of the opercle, into which I dextrously introduced the extremity of a small glass tube drawn to a point. I attached it to the outside, and fixed it in a perpendicular position by means of sealing-wax, so as to prevent the admission of the external air. Having then blown into the external extremity of the tube, I could not perceive that any air escaped either through the opercle or the shell. I might hence, I thought, infer, with some degree of confidence, that the opercle of these snails intercepted all communication between the external and internal air. I was still anxious, however, to possess a more rigorous and decisive proof of the fact, and that I obtained in the following manner.

§ LIII.

I repeated, on a second snail, the simple process of the glass tube; but that which I used, in this case, was thirty inches long. Having inserted it as above, and fixed it with sealing-wax, I turned the lower extremity upwards, and poured in mercury so as to fill up the small cavity between the opercle and the animal and the tube itself; then, applying my thumb to the top, in order to prevent the mercury from running out, I inverted it, perpendicularly, into a vessel filled with mercury: that in the tube immediately fell about three inches, leaving about 28 inches in suspension, which was nearly the height at the time in an adjacent barometer. It is evident that, had the smallest portion of the external air penetrated the shell, the mercury would have gradually sunk in the tube; but, during a period of four-and-twenty hours, I found that the only variations to which the mercury was subject were those which proceeded from the varying weight of the atmosphere, and that they were perfectly similar to those in the adjacent barometer; so that of this little tube, terminated at its upper extremity by a snail, I had constructed a perfect barometer.

It is, therefore, incontestable that the opercle of this snail supplies the process of hermetically sealing. Having several times repeated this experiment for other purposes, I observed that on piercing the opercle, or perforating the shell, with the finest steel point, the mercury in the tube instantaneously sunk into the vessel. This circumstance furnished another equally decisive proof that the atmospheric air cannot penetrate into the shell when shut by means of the opercle.

All the snails of this species are not equally adapted to the purposes of this experiment. It is previously necessary to inspect the shell and its operculum in the most careful manner, and to reject such snails as appear to have the slightest chink or hole, because it is evident that it would give access to the external air;—a fact which I have had occasion to remark in several experiments of this nature.

§ LIV.

This curious observation leads us to conjecture that these snails, while they continue shut up by their opercles, have no transpiration. But have we any instance of a living being which can exist without transpiration for six months, the space of time during which these worms remain hermetically sealed under ground? This was another curious object of experimental enquiry.

I made the experiment on six snails which appeared to me perfectly sound both in their shells and opercles, so that I had reason to think the external air could not penetrate them. I weighed them at the commencement of winter, and some time after repeated the operation, in order to ascertain whether they had undergone any sensible diminution of weight. They were uniformly kept in a temperature which varied only from two to six degrees. The weight of each of the six snails, as noted down on the 10th of December, was:

The 1st,	-	-	-	309	grains.
2d,	-	-	-	304	
3d,	-	-	-	416	
4th,	-	-	-	411	
5th,	-	-	-	380	
6th,	-	-	-	391	

The same as re-weighed on the 10th of February:

1st,	-	-	-	303	grains.
2d,	-	-	-	300	
3d,	-	-	-	409	
4th,	-	-	-	405	
5th,	-	-	-	375	
6th,	-	-	-	387	

Each of the six snails, it appears, sustained some loss of weight between the 10th December and the 10th of February.

The 1st	lost	6 grains
2d,	-	4
3d,	-	7
4th,	-	6
5th,	-	5
6th,	-	4

I weighed them, for the third and last time, on the 8th of April, when I found they had sustained a still further diminution, which, added to the former, made their total loss of weight—

For the 1st,	-	-	14 grains
2d,	-	-	13
3d,	-	-	11
4th,	-	-	12
5th,	-	-	10
6th,	-	-	11

Such was the sum total of the loss of weight in each of the six snails, in the space of four months, during the time of their confinement under their opercula.

After I had weighed them, I assured myself, by means of the process already described (§ LIII), that the external air had no possibility of communication with the interior part of the shell. How then is their loss of weight to be accounted for? Every one must have remarked that this loss is extremely small when compared with their original weight; and it is very probable that it is sustained by the shell only; or, to explain myself more fully, that it is occasioned by the emanations from the testaceous substance: at least, there is not a single existing substance which will not, after a certain interval, be found to have lost some portion of its bulk and weight, in consequence of the small particles which are gradually detached from it. It is, however, very possible that the animal itself may lose somewhat, and that some of the most subtle of its component particles may escape through the pores of the shell, though they are impervious to the atmosphere. There are certainly some fluids, such as water, that penetrate into pores to which air has no access.

I do not, therefore, reject the second cause of diminution, because it appeared to me that the snails which I opened on the 10th of April did not so completely occupy their shells as they did at the commencement of the experiment.

§ LV.

Another enquiry, much more important than all those which I have completed, and which was more particularly connected with my views, was to examine whether the portion of air so long stagnant in the shells had undergone any decomposition during its confinement.

I facilitated the means of resolving this problem, by making an abundant collection of snails during the winter. I had only to pierce their opercles in the pneumatico-mercurial apparatus, and to fill my eudiometer with the air issuing from the inner part of the shell. I made this experiment at three different periods.

The first was at the commencement of December, that is two months after the snails had hermetically sealed their shells. The result was, that the internal air was of equal purity with common air. I found in it scarcely any carbonic acid gas, twenty parts of oxygen gas, and eighty of azotic gas.

I repeated the experiment on the 15th of

February, and with the same result. The internal air had, by this time, remained stagnant in the bodies of the snails for four months and a half, without suffering the slightest decomposition.

It may, therefore, be inferred, that, in the torpid or inactive state of these snails, the disengagement of their hydrogen and carbonic acid had been suspended, and that they had absorbed none of the oxygen contained in the small portion of air confined in their shells.

This experiment must necessarily be conducted with the greatest attention. It not unfrequently happens that, on bursting the opercula, some of the snails, as may be perceived by their smell, are found to be already dead and in a state of putrefaction. In this case, on analysing the air, there would be found a considerable quantity of carbonic acid gas, and a diminution of the oxygen gas; and one or two of these putrid snails would lead to an error in using the eudiometer. In order to obviate this error, the air of each snail ought not to be transferred from the mercurial tube into the eudiometer, until it has been already ascertained that the animal is still alive. On thus examining the state of the snails, which are to be submitted to the eudiometric process, and retaining only the air contained in living snails, the result of the experiment admits of no exception.

§ LVI.

The date of my last experiment was the 3d of April, the period when the snails have not yet burst their opercula; but when, preparatory to their coming out, they begin to soften them with their slime, in order to separate them from the edges of the shell.

At the two former periods, the temperature was between 4 and 7 degrees; at the latter it had risen to 11°.

The result of this experiment was, that the snails had partly decomposed the confined air.

On this occasion, I removed the opercula of a great number of these snails; and, filling three eudiometers with the enclosed air, I found that it had undergone the fol lowing chemical changes.

Air of the 1st eudiometer.

Oxygen gas destroyed.	Carbonic acid gas produced.	Azotic gas destroyed.
9 degrees	$4\frac{1}{3}$	0
2d eudiometer.		
$7\frac{1}{2}$ ——	5	0
3d eudiometer.		
8 ——	6	0

The augmented warmth of the spring had re-animated these snails; in which case the encreased motion of their circulating fluids favours the exhalation of the hydrogen and carbonic acid from the surface of their bodies, and at the same time renovates the chemical power which attracts the oxygen; so that the latter must be diminished in proportion to the production of the carbonic acid gas. This decomposition I also conceive to be the principal cause which compels the snails to leave their prisons, where they feel the same uneasiness as in a tube filled with common air which has begun to be decomposed. Nevertheless, the hunger they must experience after so long a fast must be regarded as a very strong additional stimulus to make them leave their shells.

§ LVII.

The latter observation, joined to that which I have stated in § LIII, led me to imagine that I could have ascertained the precise degrees of the alterations produced by the fleshy substance of these animals, without depriving them of their shells; and even that I might ascertain the proportion of the alterations experienced by these animals during the same period, both in their bodies and their shells: for, as the animals themselves decomposed the air, it was easy to suppose that their shells would also produce this effect.

In the same section (LIII) I have described a kind of barometer made by the insertion of a tube into the opercle of the shell. This contrivance suggested to me the thought of filling the small tube with mercury, and of pouring into it one measure of the common air of my eudiometer, so that this air should be in contact with the enclosed animal.

Having already perceived that the snails produced a chemical alteration in the small portion of air confined within their opercula (§ LV and § LVI), I had not the least doubt that the same change would take place in the air of the little tube, which I could have easily remarked because the mercury in the tube was higher than that of the vessel wherein it was plunged; but, at the expiration of three hours, the snail fell down from the upper extremity of the tube, in consequence of the operculum having separated from the shell.

This accident, so unfavourable to my views, happened thrice in succession, and doubtless from this reason, that the animals were occupied in ridding themselves of the obstacle, which their opercula presented to their efforts to escape from the extreme pain they must have felt, and to go in quest of that aliment which they so much needed.

The rest of my snails having by this time removed their opercula, I was forced to postpone my intended experiment until the following winter; this delay, however, proved fortunate, as I was then able to vary the experiment at pleasure.

§ LVIII.

The temperature of the month of December, which, in my laboratory, was between 3½ and 6 degrees of the thermometer, was extremely favourable to these experiments, because it was necessary that the snails should continue fixed to the small tubes. Almost all of them adhered accordingly, and neither did they disappoint my expectation, nor was I mistaken in the opinion which I had first formed with respect to the air confined in the small tubes. I conceived that it had sustained no alteration, because it remained at the same height, and an analysis of the air contained in some of the tubes proved that I was right.

I then saw that it was necessary to subject the snails to a milder temperature, such as that of a stove which raised the heat of the apartment to about 9 degrees. Very few of the snails detached themselves from the glass tube, even at this temperature; but I could already perceive, from the elevation of the mercury, that the enclosed air was in the course of alteration. An experiment made on the contents of two of the tubes clearly shewed that the column of air diminished, and consequently that the oxygen gas began to be destroyed and the carbonic acid gas to be produced.

§ LIX.

Finding that the success of the experiment was so easily evinced, whilst I was preparing several tubes with snails attached to them by the operculum, as in the preceding instances, I took an equal number of other tubes, of a larger size, having their upper extremities hermetically sealed by means of a blow-pipe, and, after pouring into them a measure of air, in the same manner as into the other tubes, I placed under each of them a snail with its opercle entire; and, in order that all the circumstances might be as nearly similar as possible, I selected such as were of an equal size. This latter point of the comparison enabled me to ascertain the proportional decomposition of the air produced by the shells, and by the animals themselves.

Among a very great number of experiments made with this view, I shall only relate six; that is, three on the enclosed air when the snails were placed at the top of the tubes, and three when they were placed at their lower extremity.

Analysis of the air contained in the tubes when the snails were placed at their upper extremity:

	Oxygen gas destroyed.	Carbonic acid gas produced.	Azotic gas destroyed.
1 snail,	12°	5°	3°
2 ——	11	$4\frac{1}{2}$	2
3 ——	13	6	4

Analysis of the air in the tubes under which the snails were placed:

	Oxygen gas destroyed.	Carbonic acid gas produced.	Azotic gas destroyed.
1 snail,	18°	9°	5°
2 ——	20	6	6
3 ——	20	7	4½

§ LX.

From these experiments, two important consequences are deducible; first, that these worms, like their shells, destroy the oxygen and azotic gas, but a less quantity of the latter than the former, and that they generate carbonic acid; second, that the consumption of the oxygen and azotic gas, and the production of the carbonic acid gas, are greater, in a given time, from the action exerted on the air by these worms alone, than by that of their shells. This observation perfectly agrees with that which I have made upon the common garden snails, after being taken out of their shells, and upon the shells by themselves (§ XXXIX). This observation is altogether natural, and perfectly conformable to what is known of the animal economy; at least, we may readily suppose that these worms throw off from their bodies a greater quantity of hydrogen and carbonic acid than from their shells, and that more of the oxygen is absorbed by the animal bodies, than by the shells which cover them.

§ LXI.

The snails, which furnished the results that I have just related, remained alive at the conclusion of the experiment. It is very remarkable that two of them had consumed the 20° of oxygen gas, which, as being the usual proportion contained in atmospheric air, was all that these animals could destroy.

I afterwards discovered, by some new experiments, that the shells destroyed the whole of the oxygen gas, if they were enclosed a longer time than the worms in the same air.

§ LXII.

It cannot be doubted, that these snails, after having lost their opercula, decompose the common air at a certain temperature, and that no decomposition takes place when these opercula are entire, as I have shewn in the experiment made with the small tube, at least while the thermometer stood at the eighth degree (§ LVIII). I may then observe, that to the tenth degree the destruction of the oxygen gas was evident, as well as the production of the carbonic acid gas, by the snails the opercula of which were wanting, and that in both cases they became greater at a higher temperature; only the destruction of the azotic gas did not always correspond with that in the two preceding cases, since I have more than once found the whole eighty degrees remaining. On the contrary, in descending from $5\frac{1}{2}°$ of the thermometer even to 0, the common air underwent no change whatever, only the pulsations of the heart in these snails became slower and less frequent, and wholly ceased at the 1° of the thermometer; consequently, the circulation was then suspended.

These observations agree with those which I delivered at § XXII and § XXIII, when I spoke of the common garden snail.

§ LXIII.

The experiments made on those snails convinced me that they consumed the oxygen gas after the extinction of vitality (§ XXXIII); from which it might be presumed that the Portugal snail would produce the same effect, but it was necessary to submit this conjecture to the test of experiment. With this intention I killed them, as I had done the common garden snail, by means of boiling water; but, in making these experiments on dead snails, I did not omit to extend them to others which were living, in order to ascertain the difference of the results, which I am here about to detail.

In a given time, the living snails destroyed more of the oxygen gas than the animals which had been killed; although, by protracting the confinement of the latter, they at last wholly consumed the oxygen gas.

§ LXIV.

If, instead of confining these dead snails in an atmosphere of common air, I plunged them into pure oxygen gas, in a given time the destruction of this gas was much more considerable; but the consequences resulting from this fact appear to me so very important, that I conceive it necessary to enter into the detail of them.

I enclosed six snails in eight inches of oxygen gas during seven days: at the end of this period I examined the air, which was reduced to two inches, but it did not contain the smallest quantity of oxygen gas, consequently the eight inches of oxygen gas were wholly consumed. This result furnished by the Portugal snail perfectly agrees with that exhibited by the common garden snail, when it was confined in pure oxygen gas: nine of them enclosed in fifteen inches of this gas wholly consumed it in thirteen days, whilst during this period they produced $\frac{2}{3}$ of azotic gas and $\frac{1}{11}$ of carbonic acid gas (§ XXXVI).

§ LXV.

In the two cases in which so great a quantity of oxygen gas was absorbed by the dead animals, there is no foundation for believing that a part of its base was combined with the hydrogen of these animals, in order to form water; it is at least very doubtful whether the slight humidity, which in some cases appears on the internal sides of the vessels containing the animals, be the production of this combination, it appears to me more probable that it proceeded from the evaporation of the aqüeous part of the bodies of the snails themselves.

Shall I then affirm that this oxygen is combined with the carbon of the snails, and that it produced the carbonic acid gas which appeared in these two experiments? But this hypothesis cannot be supported, when we compare the inconsiderable production of carbonic acid gas with the very great quantity of azotic gas obtained: in the first experiment, however, instead of fifteen inches of oxygen gas which had disappeared, there was $\frac{1}{5}$ of an inch of carbonic acid gas; and, in the second, $\frac{1}{2}$ inch, although eight inches of oxygen gas had been consumed. Besides, the carbonic acid gas which appears during respiration, as well as in these experiments, seems rather to be the product of carbonic acid pre-existing in the mass of animal humours, according to the opinion of Lavoisier, which I shall shortly confirm by additional proofs.

We must then conclude that, notwithstanding the great quantity of this oxygen gas, its base has been absorbed by these worms. If this absorption take place in the dead body, we have greater reason to suppose that it will be carried on during the life of the animal, which I have proved to be the case; thus, for example, it has been shewn that, if two dead snails consume 0.07 of oxygen gas in six hours, two living snails will destroy 0.10, and even more, during the same period of time.

§ LXVI.

The motive which induced the illustrious French chemist to adopt the opinion that the carbonic acid gas, found in the air in consequence of respiration, should be regarded as a product of carbonic acid preexisting in the animal, was founded on the observation he had made in the course of some experiments on digestion, in which he observed much carbonic acid gas disengaged from the alimentary mass, even at the moment when the chyle began to be formed; he justly concluded that if this acid passed not into the blood, he did not see of what use it could be in the animal economy.

I repeated the experiments of Lavoisier on aliments more or less digested, by different animals, which I took out of their stomach in this state, but I always found this carbonic acid present in great abundance: I believe I am also able to furnish a direct proof that this acid passes from the stomach into the blood and the other animal fluids; but that it is afterwards given out during respiration, and by the external surface of the body.

According to this hypothesis it is evident that animals after a full meal should furnish a much greater quantity of carbonic acid gas than when fasting, or enfeebled by long abstinence.

Several experiments which I undertook with this view, on a great number of warm-blooded animals, demonstrate this truth, as will appear in the sequel. At present I shall confine myself to the common garden snail, and to those of Portugal which remain buried, during winter, in the ground; and which, on leaving their shells in the spring, devour their food in abundance, and with great avidity. This circumstance was extremely favourable to my view, as it enabled me more fully to ascertain the validity of my conclusions: the following was the mode by which I conducted these experiments.

§ LXVII.

On the 4th of April I raised up the operculum in eight Portugal snails which I had preserved throughout the winter: I placed them on a piece of paper, with the tender leaves of young lettuce which appeared to give them great pleasure, if I may judge from the quantity they devoured; and I afterwards placed them all under four small tubes filled with atmospheric air; that is, two underneath each tube: and at the same time I made a similar experiment upon eight of the same kind of snails, which were also shut up by their opercula, and had eaten nothing. Upon analysing the air contained in these eight tubes, at the end of thirty hours, I obtained the following results—

Snails which had been fed.

	Oxygen gas destroyed.	Carbonic acid gas produced.	Azotic gas destroyed.
1 pair of snails,	20	11	4
2 ————	20	9	
3 ————	20	10	3
4 ————	20	13	2

Snails which had not been fed.

1 pair of snails,	20	4	2
2 ————	20	8	5
3 ————	20	7	3
4 ————	20	9	3

In comparing the carbonic acid gas generated in these experiments, we observe that, in a single case, the quantity of this gas produced by the snails which had been recently fed, was equal to that generated by two of those animals which had undergone a long abstinence; but that, in all the other cases, the quantity of carbonic acid gas produced by the former, exceeded that which was generated by the latter: notwithstanding this difference, however, they both destroyed the oxygen gas.

§ LXVIII.

A long-continued drought, during summer, produces on these snails the same effects as the cold of winter. It sometimes happens that, in several places of Lombardy, not a single drop of rain falls during the whole course of July and August, as I observed in 1797. During these great droughts, usually accompanied with considerable heat, the snails conceal themselves in places sheltered from the sun, where they remain enclosed in their shells by means of the membranous opercula, and attached to some bodies to which they remain fixed during the long continuance of this scorching and dry weather. As in this situation they remain wholly without food, they become as lean, and even more so than during winter, on which account I conceived it a favourable opportunity to enter upon a second experiment. With this intention I enclosed several of these meagre animals, as well as some others which had been carefully fed in the common air, in the same accurate manner I have described in § LXVII.

Without entering into a detailed account of the results, I shall only observe that from them I learned, that among the snails which had been well fed, four-fifths furnished a greater quantity of carbonic acid gas than the others which had long been deprived of food.

It follows, from this circumstance, that the animals had taken in a great quantity of carbonic acid gas with their food; but it likewise appears proved, that this gas passes into the blood and the circulating fluids, since it is discoverable in the air wherein these animals have breathed, and since the quantity which they produce is greater in proportion as they have been more recently and more abundantly fed.

But if it happen, though seldom, that a snail, which has been just fed, should only produce an equal or somewhat greater quantity of carbonic acid than another which has fasted a long time, I believe this apparent exception must proceed from the food not having answered the purpose of nutrition, on account of some indisposition consequent on the feebleness produced by so long an abstinence. This conjecture is not altogether gratuitous, since I have witnessed several snails perish after a long fast, although they had taken food a short time before their death.

§ LXIX.

I shall terminate my experiments on the snails of Portugal, by a single observation respecting the organisation of their shells, which is nearly the same with that of the common garden snail.

When submitted to the test of the nitrous acid, we also discover in these shells a calcareous earthy matter, and a membrane adhering to their external convex surface: there is likewise another internal membrane, but I have not been able certainly to discover whether it be united with the earthy part, or rather attached to the concave and opposite surface of the shell. These two membranes possess all the characters of an animal substance.

Before leaving this subject, I must examine the calcareous operculum. Its inferior surface is of a pale yellowish colour; and the superior white, when freed from the earth with which it is always covered, since, in order to observe the operculum in its natural situation, it is necessary to dig the snails out of the ground.

The operculum is somewhat hard, and about the third of a line in thickness; in breaking it falls into small pieces, nearly like the shells of hens' eggs: it is somewhat convex externally, and concave within; on detaching a membrane, with which this cavity is lined, we perceive the calcareous substance.

All these circumstances may be ascertained without the aid of the nitrous acid; but when we employ this acid diluted with water, according to the method of Herissant, an effervescence immediately takes place, in the same manner as from the shells, and which proceeds from the same cause; the calcareous matter being dissolved, we observe a second membrane finer and much more subtle than the other.

These opercula, like the shells, are therefore composed of two substances;—the one calcareous, the other membranous. It did not occur to me to examine whether these membranes occasioned any decomposition of oxygen gas, but analogy induced me to believe this would be the case.

This operculum is not the only one by which the aperture of the shells is closed; on removing it, a second is discovered, somewhat more interior, which is very thin and transparent; it is not calcareous, but wholly composed of extremely thin membranous laminæ, which may however be readily separated.

This operculum shuts the opening of the shells, but does not intercept the passage of the air: in order, however, to give greater precision to the experiments, it should be broken, after piercing the calcareous operculum, when we wish a free admission given to the external air.

The snails only form this calcareous opercle when they bury themselves at the approach of winter: the one which they fabricate in the other seasons is membranous: it is, however, necessary to mention, that the calcareous matter intended for that purpose is only generated in the body of the animal at the commencement of winter, when it is necessary, in order to protect them from the influence of the season, and from external injuries.

§ LXX.

In the gardens, and all those places covered with the plants sought after by the common garden snail and that of Portugal, we frequently find a third species of a yellow colour with brown stripes, and the lips of the hole black.

I know not why Swammerdam called it *lacche*; it appears to me to have all the characters of the *itala* of LINN., by which name I shall designate it. During winter, like all the others, it is found buried in the earth, shut up by its membranous opercle, and in a state of torpor from which it recovers at the commencement of spring; at this season it is too well known to the inhabitants of the country, by the ravages it commits on the young fruit-trees, and especially the vines, by devouring their tender shoots.

These snails have thus several points of resemblance, either external or internal, with the two snails which I have just now mentioned; but they have, however, very essential differences which have been overlooked by the celebrated Dutch naturalist. Thus, for example, the head of this snail is not regenerated after amputation, as I have had occasion to remark a great number of times, like that of the two species of which I have spoken: although this Italian snail be very common, it does not, like the other species, furnish food to man.

§ LXXI.

I repeated on this species all the experiments I had made on the two others, both in a living and dead state, either in atmospheric air, oxygen gas, or azotic gas, as well on the entire snails as on those deprived of their shells, and on the shells alone; but as the results were in every respect, under all these points of view, perfectly analogous to those furnished by the two other species of snails, and which I have already related, it would be superfluous, as well as tedious, to enter into a detail of them.

§ LXXII.

I shall rather take a general view of the experiments made on these three species of worms, in relation to the oxygen, carbonic acid, and azotic gas. The oxygen gas of the atmosphere is always absorbed by these snails, when they remain some time in the common air: it is evident that in this case more or less carbonic acid gas is produced, but it is not equally clear that azotic gas is destroyed. It is indeed true, that in the experiments formerly related this destruction of azotic gas was almost constantly manifested; but this circumstance only indicates that, among the immense number of observations and experiments contained in my journal, those only have been related wherein this effect was evident; I ought, however, likewise to mention that they are in all respects the most numerous. Not but that some of them exhibited a contrary result, and displayed a production of azotic gas, even at three, four, and five degrees, although the snails were not deprived of vitality.

I have observed this singular phenomenon under two circumstances. The first, after an abundant meal, devoured with much appetite; the second, when these snails were ready to perish. If I am not deceived, this two-fold phenomenon may be explained in the following manner: The azot being one of those principles introduced into the animal system by means of food, the snails, after a full meal, must have taken in so much of it that it is exhaled in a gaseous form. In the other case, when animal life is nearly extinguished, this small animal machine, approaching towards a state of decomposition, it is supposable that the azotic gas begins to escape; at least, we know, when the decomposition is complete, this gas is produced in great abundance.

§ LXXIII.

The mountains of the mean height of the Appennines, the hills, valleys, woods, and humid meadows, in their vicinity, afford retreats to several genera of snails specifically different from those I have just now described. By my own researches, and those of my friends, I have been able to procure several individuals of these species, which I submitted to some experiments relative to this subject, and from which I always obtained a destruction of the oxygen gas, and a greater or less production of carbonic acid gas. After having made these experiments on the snails, I always repeated them on the shells, by which the same destruction of oxygen gas was produced, with this difference, that a less quantity of it was absorbed by the shells than by the snails themselves.

§ LXXIV.

The power possessed by these organised matters, of absorbing that principle intended for the preservation of the animal, recalled my attention to the eggs of birds, the shells of which have the greatest analogy with those of snails, and in general with all those of the testacea. They are alike formed of a calcareous earth, more or less dry, and united to an organic tissue. I formed a conjecture which appeared to me not far distant from the truth ;—I thought that the shells of the eggs of birds absorbed the oxygen gas of the common air, in the same manner as the shells of snails, as I had the satisfaction of observing; but, before relating the results of this new series of experiments, it is necessary I should make a single observation on the structure of these shells.

It was long since observed that they are full of little holes which form the extremities of very minute vessels, going out from the membrane with which the shell is internally covered, and opening at their surface by small orifices, where they form a very fine and transparent net-work.

§ LXXV.

I employed in this case the same solvent as in the analysis of snail-shells, by putting some very small pieces of egg-shells into the nitrous acid diluted with water, so that the effervescence was so extremely slight as not to injure in the smallest degree the organic tissue, which would have been the case had the liquid been used somewhat stronger. The small pieces of shells were taken from the obtuse part of the egg, because in that point it has no inferior membrane; I know besides that it forms in this part a lenticular cavity, and that the membrane which concurs in its formation does not here come into contact with the shell, which is left naked.

It was not without astonishment that I observed, at the beginning of the solution of the calcareous earth, a membrane in this obtuse part, which had not been before visible. Fearing to be deceived, or to have committed some error, I repeated the experiment on a shell recently separated from the egg, since in this state I could detach the membrane from it with very great facility; I put some of these fragments into the nitrous acid, and at the end of some hours I again saw this new membrane, which I completely detached from each of the small pieces of the shell. I likewise perceived that under the usual membrane there was another which from my observations appeared a little transparent, but finer and more delicate than the other.

Besides that, on the convex surface of the shell, I found, not only this fine and diaphanous net-work, which had already been observed, but I likewise succeeded in raising up, by means of forceps, a more subtle and transparent membrane than any thing I could have possibly conceived : on viewing it with a lens, it appeared filamentous and full of extremely small holes. After having raised up these two internal and two external membranes, the small pieces of the shell, on exposure to a strong light, appeared full of holes.

These fragments deprived of their membranes, and changed by solution in the acid, became still more subtle, although they preserved their hardness so as to crackle between the teeth ; when, however, they were allowed to remain in the nitrous acid, the calcareous substance was at length wholly destroyed, leaving a new and very subtle membrane, which was doubtless concealed and covered by this substance.

By this chemical analysis I ascertained, that besides the external and internal membranes which are vascular, there are three others, one of which is situated behind the internal, and another accompanies the external vascular membrane, whilst the third is concealed in the middle of the calcareous part of the shell.

§ LXXVI.

The first experiment which I undertook, in order to ascertain whether the egg-shells of birds would absorb the oxygen gas, was by enclosing a hen's egg in three inches of atmospheric air.

I have elsewhere remarked, that the elevation of the mercury in the tube was a certain indication that some destruction of the enclosed air had taken place. I viewed, therefore, from time to time, the tube containing the egg, in order to ascertain if the mercury ascended above the line which at first separated it from the air; and, at the end of some hours, its elevation was such as led me to suppose that a considerable change had taken place in the air. Was this alteration to be attributed to the diminution of the oxygen gas, the azotic gas, or both? Of this I was ignorant; but, at the termination of four days, I transferred the remaining air into the eudiometer; by which means I found that the azotic gas remained undiminished, that eighteen degrees of oxygen gas had been consumed, and six degrees and a half of carbonic acid gas produced.

§ LXXVII.

This experiment was not, however, sufficiently decisive; it was still necessary to ascertain whether this destruction of the oxygen gas was produced by the shell alone, or whether the egg concurred in abstracting the most subtle part of the air, through the minute pores with which the shells are furnished. I endeavoured to remove this uncertainty, by making an experiment on the shell alone, the result of which was that a small diminution took place: at the end of an equal space of time, and under the same degree of heat, thirteen degrees of oxygen gas were consumed, six degrees and three quarters of carbonic acid produced, and the azotic gas remained undiminished.

§ LXXVIII.

A doubt, however, still remained on my mind, respecting the greater consumption of oxygen gas by the shell when not separated from the internal substance of the egg than by the shell alone. Might not this great consumption be effected entirely by the shell, or rather might not the membrane which covers its internal sides concur in producing this diminution, as in the preceding experiments I left it adhering to the sides of the shell? In order to resolve this question with sufficient exactness, and determine the proportions of this consumption by the entire egg, by the shell with its membrane, and by the shell deprived of it; I placed an entire egg in one tube, the shell of an egg with its membrane adhering to it in another, and in a third the shell without its membrane. By this comparison I was enabled to form a certain judgment respecting this subject; at the end of five days the entire egg had consumed eighteen degrees of oxygen gas, the shell united to its membrane fifteen degrees and a half, and the shell deprived of its membrane thirteen degrees.

It should appear, then, that although the greatest consumption of oxygen gas was by the shell alone, a small part had, however, been destroyed by the membrane and the other fluids.

Two experiments, that I have neglected to relate, confirmed by their results those which I have detailed.

§ LXXIX.

It was no longer doubtful, that the eggs of other birds might produce the same effects on the oxygen gas of the common air; at least this property, which I first discovered in the shells of two species of snails, I have likewise found in all the others I had an opportunity of examining. The experiments on five or six different species of eggs gave the same results as those which had been exhibited by hens' eggs; and as these shells, as well as those of snails, do not putrify, I am inclined to believe that the consumption of oxygen gas from the air proceeds either wholly, or in a great measure, from the absorption of the largest portion of its base, by the shells themselves.

§ LXXX.

I shall conclude this memoir by a few observations on some snails not furnished with shells, which are termed slugs, and by the inhabitants of Tuscany naked snails. The remarks of Vauquelin on the *limax flavus*, of LINN., called by him the large yellow meadow slug, induced me to bestow some attention on them.

I subjected two of these slugs to experiment, by enclosing one of them in common air, and the other in sulphurated hydrogen gas. Both the two perished, but in a very different space of time: the second exhibited no signs of life at the expiration of half an hour: the first destroyed wholly the oxygen gas, since the introduction of phosphorus produced in the remaining air no diminution whatever.

§ LXXXI.

Several individuals, of the *limax agrestis* of LINN., became afterwards the subject of my investigation. These snails not only inhabit the fields, gardens, and meadows, according to the observation of this illustrious botanist, but they likewise multiply in our houses, in cellars, in ground-floor chambers (especially when they are damp), in timber-yards, and other similar places.

They usually go out during the night; but they also go abroad at every period of the day. Their path is marked by a thin stratum of glutinous matter, which on becoming dry assumes a brilliant silver colour.

These slugs pass the winter in subterranean holes, the most obscure corners in low chambers, and similar retreats; some of them even conceal themselves under the ground, like the snails of which I have spoken.

These slugs, having no shell to defend them against external accidents, draw themselves up into a kind of ball, and remain immoveable, in this contracted position, during the cold of winter, and in this season they are found with greater difficulty than the snails: I succeeded, however, in finding some of them, but they were always drawn up in this manner, motionless and in a lethargic state: this torpor commences when the thermometer indicates about the second degree above zero.

§ LXXXII.

This species of snail has been so well described by the elegant pens of Swammerdam and Rhedi, as to render any further description on my part altogether unnecessary. I shall, therefore, confine myself to remark, with these two anatomists, that a hole is observable in the right side of the neck, at the edge of their hood, and that this hole is the canal which gives a free passage to the external air. In the largest of these slugs, which exceed three inches in length, and one and a half in circumference, I have observed the diameter of this hole to be one line, when fully dilated. This opening is for the most part either of a circular or an oval form: it is opened and shut at the will of the animal, but at irregular intervals; sometimes it remains open during several minutes, and at others only for a few seconds. When viewed in its most dilated state, we observe that it serves as an opening to a small canal that sinks under the region of the neck, where it forms a kind of purse or bladder, which, according to Rhedi, is the pulmonary organ of this worm.

§ LXXXIII.

When looking attentively at this hole, I have seen the smallest animalcula turn rapidly round about and conceal themselves within it, which recalled to my mind a curious insect, termed by Reaumur the *insect of snails*. In itself there is nothing remarkable, it is confounded with a very great number of other small insects, and on the whole well deserves to have a name given to it by those who are skilled in nomenclature. The place which it selects for its dwelling, and other circumstances that

I shall relate, induced this illustrious naturalist to give a description of it.

They usually inhabit the intestines of snails, but do not remain in them constantly, like a great number of worms which are found in the intestines of other animals: they come out of the anus, and run with great rapidity on the surface of the body of the snail, without at any time entering into its shell. Reaumur is of opinion, however, that they do not voluntarily leave the intestines of the snail, but are pushed out during the exclusion of the excrements; they then collect together until the snail opens its anus, when they instantly re-enter the cavity of the intestines: these insects are so extremely minute that several of them can enter at once by this passage.

§ LXXXIV.

For several years the observations of this celebrated naturalist made me, on a variety of occasions regard with admiration this astonishing scene. I believed it confined to snails, until, on observing the slugs, and viewing attentively this hole which communicates with the external air, I witnessed the same phenomenon (§ LXXXIII).

I perceived not only these animalcula surrounding the hole, but observed some of them enter, and others come out of it; and as, according to the opinion of Rhedi, the intestinal canal opens a little way within this hole, I conjectured that this ingress and egress might be to and from the intestines; so that, as the slugs do not exclude their excrements by this hole, the entrance and exit of these animalcula were voluntary during its dilatation, but when closed they then wandered upon the body of the slug with astonishing rapidity: I have counted fifteen on some of their bodies at the same time. If they had not been white, and running on a dark ground, which is the colour of these slugs, it would have been very difficult to distinguish them with the naked eye; they are mere atoms, or points of matter: but I shall not detail the microscopic observations I made upon them, because Reaumur has described them with much care and accuracy.

§ LXXXV.

But to return to my subject.—I have observed, with the celebrated physician Rhedi, that this species of small purse, or bladder, which opens in the right side of the neck of the slug, serves as its pulmonary organ, although it must be admitted that it has little resemblance to lungs. In order to throw some light on this difficult subject, it was necessary to submit it to experiment.

With this view, I forced two slugs to remain immersed in a vase filled with water, which I placed with its opening downwards on a plate full of the same fluid. They at first shewed evident signs of uneasiness; they manifested the most violent motions, ascended the sides of the vase, again descended, and endeavoured to escape; but, not succeeding in the attempt, they sunk on the bottom of the plate whereon the vase rested, and there perished.

This experiment was not, however, altogether decisive, since similar results had been obtained from snails not furnished with proper respiratory organs, and since the same thing is observed in other animals which nature has not even endowed with any hollow viscus to admit the external air.

§ LXXXVI.

The immersion of other two slugs in hydrogen gas appeared to me a less equivocal experiment; but I also wished to perform it on the common garden snail, in order to obtain an object of comparison that might prove useful. The slugs were deprived of life by remaining three hours in this gas, whilst the snails continued alive in it during the space of eighteen hours. This experiment, which does not differ essentially from that of the celebrated Vauquelin, proves that slugs die much sooner than snails, in a medium wholly destitute of oxygen gas.

§ LXXXVII.

It appears, then, that the death of the slugs happens more rapidly, in proportion to the diminution of the oxygen gas, than that of the snails. It was easy to ascertain this circumstance, by enclosing each of them separately in common air.

I placed, then, a common garden and a Portugal snail singly in a small tube full of air: I had, likewise, several snails, and small tubes full of air with slugs disposed in such a manner that all the circumstances were in every respect similar. The greatest part of the slugs perished some hours sooner than the snails. On afterwards submitting the air in the tubes to a chemical analysis, it appeared that in those of the snails the whole of the oxygen gas had been absorbed, which was not the case in the tubes with the slugs; in several of which I discovered three, four, and even five, degrees of oxygen gas: it appears, then, that the slugs absorb less air than the snails.

If in an equal space of time I examined the air in which a snail was confined, and that which surrounded a slug, the absorption in both cases was nearly equal. The cause of this difference must be that the slugs, by dying, sooner ceased to absorb the oxygen, which the snails still continued to absorb, in consequence of surviving some time longer.

The acceleration of the death of the slugs, in a medium destitute of oxygen gas, such as azotic gas, or in which the oxygen gas is constantly absorbed, as atmospheric air, affords, however, a very plausible proof of the existence of lungs, or at least of an analogous organ in these animals.

§ LXXXVIII.

I observed, in the foregoing paragraph, that several slugs immersed in common air ceased to live in it, although several degrees of oxygen gas remained in it unabsorbed. I must likewise remark that this observation was not general, because I have seen more than once that the air wherein a slug had recently died, poured into an eudiometer, after having been freed from the carbonic acid gas, ascended, however, above 20°; from which it follows, that all the oxygen gas had been absorbed. The word *all* ought here to be understood in a limited sense; that is to say, that the snails absorbed all the oxygen which could be absorbed by the phosphorus, although this absorption was not rigorously complete. Besides this air, which appeared, by the eudiometer of Giobert, to be wholly deprived of its oxygen when submitted to the test of nitrous gas, suffered a considerable diminution of bulk, from which it follows that it still contained oxygen gas.

Respecting the azotic gas contained in the common air, I shall only observe that, in these experiments, none of it had been absorbed, but, on the contrary, some degrees of it had been produced: I should suspect even that it had appeared after the death of the slugs, because it is very difficult to ascertain in close vessels the precise moment when life becomes extinct.

§ LXXXIX.

When dead slugs are enclosed in common air all the oxygen is absorbed, but it requires a much longer time to produce this effect than when they are capable of making exertions; and if the experiment be made on living slugs, a small quantity of carbonic acid gas is produced, but very little or no azotic destroyed, whilst in those on dead slugs the production of these two gases is very considerable.

§ xc.

I was anxious to ascertain what effect would be produced on pure oxygen gas in which some living and dead slugs were enclosed: but these experiments are connected with similar ones made in common air. I took for that purpose four small recipients: in the first I enclosed a living slug in common air, and in a second another living slug in oxygen gas; the third contained a dead slug in common air, and the fourth another dead slug in oxygen gas. I took the greatest care that all the circumstances under which these experiments were made should be exactly alike, and at the end of twenty-eight hours I obtained the following results:

	Oxygen gas absorbed.	Carbonic acid gas produced.	Azotic gas produced.
Living slug enclosed in common air,	18	6	0
Dead slug enclosed in common air,	13	11	14
Living slug enclosed in oxygen gas,	42	14	0
Dead slug enclosed in oxygen gas,	27	15	10

§ XCI.

I several times repeated the foregoing experiments upon four other species of slugs,—the *limax ater*, *albus*, *flavus*, and *maximus*, of LINN.,—and they all exhibited the same phenomena.

1st, They are furnished, like the terrestrial slug, with that small purse, or canal, discovered by Rhedi; but it may be questioned whether it serves for the purpose of respiration.

2d, They absorb all the oxygen gas from the atmosphere in which they are confined, and which is essential to their existence.

3d, Their death does not put a stop to the absorption of oxygen.

§ XCII.

I shall conclude these researches, by replying to a question that may be put to me. The oxygen gas having been decomposed by the animal, the caloric ought to remain free; consequently, by communicating with the blood, it should produce and maintain animal heat. This heat, more or less above the temperature of the atmosphere, is peculiar to those beings which have true lungs, such as man, birds, and the quadrupeds that we usually term warm-blooded animals. The same degree of heat is not observable in that other order of living beings characterised by the appellation of cold-blooded animals. This temperature, which is very little if at all superior to that of the air by which these animals are surrounded, must unquestionably proceed from the slowness with which they decompose oxygen gas. Such are snails and slugs: but when these animals are confined in close vessels, and absorb the oxygen, as we see by the elevation of the mercury, does there not occur an evolution of caloric, that may be rendered evident to the senses, or which may at least be detected by the thermometer? This question, which will probably be addressed to me by the philosophical reader, occurred to myself, and I endeavoured to find the means of replying to it: with this intention I provided myself with a very sensible thermometer, which I placed in the vessels containing these animals, and which enabled me to make the following observations.

On insolating either a snail or a slug in common air, the thermometer remained stationary; but I observed the mercury ascend to $\frac{1}{10}$, $\frac{1}{7}$, and even $\frac{1}{5}$ of a degree, when several individuals were placed together in the same vase for an equal space of time. When I enclosed a single individual in oxygen gas, the mercury in the thermometer sometimes rose $\frac{1}{12}$ of a degree, and even more; but it ascended still more, when a greater number of those animals were put into the same vessel: in this case it sometimes ascended $\frac{1}{3}$ of a degree. Another thermometer, placed in the neighbourhood of the tubes, secured me from any error in my observations. The successive elevation of the mercury in the vases, and consequently the decomposition of oxygen gas, and the evolution of caloric, was in proportion to the number of animals. This elevation was also more rapid in oxygen gas than in common air, because in the first case the absorption was much more considerable in an equal space of time.

§ XCIII.

The conclusions deducible from the preceding experiments are:

1st, The snails of Portugal are not furnished with real organs of respiration.

2d, The common air enclosed within their shell, by means of the operculum, has no communication with the external air during winter.

3d, During this interval of time, the enclosed air is not decomposed by the snails; but they experience a great diminution of weight.

4th, The decomposition of the air begins when the snails are ready to break their opercula.

5th, These snails, like their shells, absorb the oxygen gas from the air with a small portion of azotic gas.

6th, The absorption of oxygen, that of azot, and the production of carbonic acid gas, are more rapid when the animals are naked than when they are covered with their shells.

7th, These animals after death absorb the oxygen of the air more slowly than during life; but the absorption is much greater in oxygen gas than in common air.

8th, There is reason to suppose that the carbonic acid gas, which always more or less appears in the experiments on snails, is rather the product of this acid pre-existing in these animals than that of the combination of the oxygen with their carbon.

9th, Land snails, of different species, exhibited results altogether analogous to those above related.

10th, Oxygen gas is absorbed by the egg-shells of birds, in the same manner as by the shells of snails.

11th, Slugs possess an organ analogous to that of lungs.

12th, They perish more rapidly in hydrogen gas than either the common garden snail or that of Portugal.

13th, These slugs, during life, do not always absorb the whole oxygen of the air; but, in this partial or complete absorption of oxygen, they leave the azotic gas undiminished.

14th, Dead slugs absorb the oxygen gas from the air more slowly than before the extinction of vitality; and this absorption is then slower in common air than in oxygen gas.

15th, The *black*, *white*, *yellow*, and *large black slug*, although not possessed of respiratory organs, absorb, both during life and after death, the oxygen gas, without which they could not support existence.

16th, Both snails and slugs, in decomposing the oxygen gas, produce a sufficient evolution of caloric to be indicated by the thermometer.

ON

THE RESPIRATION

OF

AQUATIC TESTACEA.

MEMOIR SECOND.

CHAP. I.

Helix vivipara. LINN.

Viviparous snail.

§ I.

TERRESTRIAL animals, which exist always in an aëriform medium, furnished the subjects of the preceding experiments; but in this order, even among those which are furnished with calcareous shells, a much greater number inhabit rivers, ponds, lakes, and salt water. I have shewn that land testacea absorb the oxygen contained in the atmosphere, and that they perish when deprived of it: but do the aquatic testacea absorb, in the same manner, the oxygen diffused in the water; and is this absorption as necessary to their existence as it is to that of the land testacea? If respiration, however different its mode of action in different species of animals, be notwithstanding a function essential to all living beings, we cannot doubt that nature has subjected to the same law the inhabitants of the water: it must, however, be acknowledged that we have no direct proofs on this subject, and that our ideas are altogether built on those of analogy.

This subject is nevertheless agreeable and interesting in itself, and also of great importance in the animal economy; such were the motives and the views which induced me to enter on the investigation of aquatic snails, after having examined land snails in the same manner. The marshes of Pavia furnished me with three fresh-water crustacea.

§ II.

The first was the viviparous snail, *Helix* *vivipara* of LINN. Several reasons operated on me to examine this animal. It does not lay eggs, but brings forth its young. I remarked on this occasion the following circumstance, which is peculiar to this species of snail, and which I never observed in other animals placed, like the testacea, among the lowest order of living beings. All these viviparous or oviparous animals have a certain season when they produce their young or lay their eggs. To some of them nature seems to have appointed the spring for this operation, to others summer or autumn, and to a few even winter; but those which multiply twice a-year are not very common. I am certain, however, that these snails, of which I have spoken, give birth to their young during every season of the year. I have kept them for several years in my house, in large vases filled with water, covered at the bottom with muddy earth by which they are nourished, and I observed young snails upon the mud in winter as well as in the three other seasons, although they multiplied in far greater abundance during the spring.

An attention to the anatomy of these animals corroborates this observation. If we cautiously divide the body of one of them, we discover that its uterus is a canal which opens externally, and contains a greater or less number of fœtuses or small snails, as well formed as the mothers that nourished them in their matrix. Some snails contain fifteen small ones, others twenty-five, and I have reckoned thirty-five in a third; in short, in one of them I even found seventy. The largest of them are usually in the lowest region of the uterus, as being the most mature, and most prepared for exclusion.

If we remove the young from the uterus into a glass filled with water, they at first fall to the bottom, because they are specifically heavier than this fluid; but it was not without surprise that I afterwards perceived them leave their shells with their antennæ pointed on their head, crawl on the bottom of the glass, climb up its sides, and ascend to the surface of the water. These snails had then arrived at maturity, and consequently were at the point of exclusion. I was furnished with an irrefragable proof of this, in observing some of them, taken from the uterus by a kind of Cæsarean operation, evolved in the vessels wherein I had placed them.

§ III.

But this investigation presented another interesting phenomenon. On viewing attentively the most elevated portion of the uterus, we observe the fœtuses become gradually less, and in fact they are transparent eggs, in which we can distinguish the fœtuses themselves with the greatest clearness. They were partly without the shell, which was already formed, and swimming in a fluid that at this period served them for nourishment. But these eggs, of a globular form, and lined with a subtle membrane under the shell, were of different sizes: in the smallest, when viewed through a microscope, we perceived already the small animal like a point of organised matter, but the shell was not yet visible.

I discovered, then, that these small snails, which the large ones brought forth, proceeded from an egg hatched in the uterus of the mother.

This observation is valuable, since it shews that an animal termed viviparous, because it brings forth its young, should at first be called oviparous, since this progeny originally proceeds from an egg which is hatched in the matrix.

As anatomical researches have likewise discovered other animals similar to these under the same point of view, analogy would induce us to believe, that those which are termed viviparous, generally considered, derive, in like manner, their origin from an egg.

§ IV.

In order not only to gratify my own curiosity, but excite that of my students, I opened, in the course of my public lectures, a great number of these snails; and I can affirm that they all contained young ones in the uterus, which made me suspect that this species, like a variety of land snails, participate of both sexes. But if this opinion be just, are they what is strictly termed hermaphrodites, that is, can they procreate their species without copulation, like the fresh-water polypus and several other worms? Or, rather, are they less strictly hermaphrodites; and must we suppose that the concurrence of two individuals is necessary to fecundation, as is the case with land snails? We observe, however, the two sexes united in each individual of these last animals, and in their intercourse they reciprocally fecundate each other. We look, however, in vain for these two sexes in the viviparous snail: must we then suppose that these snails are true hermaphrodites?

If this be the case, copulation cannot be essential to their fecundation, and each individual must possess within itself the faculty of reproducing its species. In the constant attention I have given to these snails, with a view of studying their habits, when they were in clear and shallow water, I have always been solicitous, especially during spring and summer, to observe if they actually copulated, as we frequently witness in land snails: but I was never able to discover them in this situation; from which circumstance alone I am not, however, warranted to affirm they are really hermaphrodites, because it was very possible the act of copulation might be performed during my absence.

Having determined to make a decisive experiment, I took, with this intention, several snails from the uterus, and placed them in small holes filled with water, so that there was only a single individual contained in each hole. I dug six of them in a place near Pavia, well supplied with water by means of subterranean springs, and which, after having been once formed, would remain full of this fluid during the whole year. In the spring the holes were filled to three-fourths of their depth, and I placed in each of them a single small snail immediately on being taken from the uterus. On emptying the holes, at the end of three months, in each of them I found the snail I had placed in it, but somewhat encreased in bulk. I refilled the holes with water, and again placed in them the same snails: I repeated my visits, and in the following year the four which only now remained had encreased to double their original size. I conjectured that the others had perished, because it was not supposable they would have left their natural element. In the second year there was a proportional encrease of bulk; and at the commencement of the third I was enabled, by the two which only then remained, to solve the question which at first induced me to enter on this experiment.

In the bottom of one of the two holes, which contained the remaining animals, I found three, and in the other four, small snails; but, on breaking the shell of the parent animals, I discovered in the uterus the young in all their different stages of growth, as I had formerly observed in the others, and even eggs still smaller than any which had previously fallen under my observation.

Since, then, each of these animals had been constantly kept separated from all others, it affords a demonstrative proof that copulation is not necessary to the multiplication of this species, and consequently that they are true hermaphrodites; and, so far as I know, this property had not hitherto been discovered in the other species of aquatic or land snails.

§ V.

To conclude; I shall relate another peculiarity of this snail, discovered by Swammerdam, and which I myself had the good fortune to verify. This accurate anatomist, who has written so ably on the viviparous snail, but who knew not the mode in which they propagate their species, was astonished to discover in their bodies a prodigious number of crystaline and stony globules lodged under the collar, in the jaws, the antennæ, the uterus, and other parts of the body. I shall present the reader with the description of them in the words of the Dutch naturalist.

"When we open the protuberance that appears on cutting into the collar of this snail, we perceive that it is composed of a mass of small, equal, transparent, crystaline globules of a stony nature, which make a slight noise under the edge of the instrument. The antennæ, the upper jaws, and several other parts of the body of this animal, are of the same nature, and grate under the teeth like grains of sand: this matter produces a strong effervescence with sulphuric acid. In the antennæ these crystaline globules are so compacted as to press on one another.

"The canal of the uterus is composed of similar grains, as numerous and compacted as those of the antennæ, of the skin, and of the tortuous protuberance already mentioned."

Swammerdam appears to have been greatly surprised when he adds—"It is truly very astonishing to see all these hard and stony parts, which are moveable and flexible; nor is it less wonderful to observe the power possessed by the animal, of contracting, extending, developing, and drawing them back into its body, by the agency of muscles and tendons inserted into these parts; and how the nerves, the veins, and the arteries, are distributed throughout these bodies."

§ VI.

Although a long time has elapsed since this discovery was published in the *Biblia Naturæ* of Swammerdam, printed in 1737, no person, so far as I know, has paid the least attention to this phenomenon, or any author taken the smallest notice of it. The astonishing singularity of this extraordinary phenomenon has perhaps operated with naturalists to neglect its investigation, and it has been wholly forgotten, from an idea of its impossibility. Doubts are easily started with respect to certain discoveries which have a paradoxical appearance, and they are at once rejected when not confirmed by others, because they do not coincide with what may have been already known. Of this I had a proof in regard to the reproduction of the heads of snails and the legs and tails of salamanders; the artificial fecundations practised upon several animals, and particularly on certain quadrupeds; and the blindness of bats, who fly with as much precision when deprived of their eyes as when in possession of them: and perhaps, even at present, all these discoveries would be regarded as doubtful, or altogether denied, had they not been repeated in every part of Europe by naturalists the most worthy of credit. In order, however, to repeat and verify the observations of Swammerdam, nothing more was necessary than good eyes, good lenses, and a certain dexterity in performing the minuter anatomical operations.

§ VII.

I killed a few of these snails, for the purpose of studying them with greater facility; and, in the first instance, I placed the antennæ under a lens. This kind may be considered as having only two, the other pair being so short as scarcely to be visible.

At first I saw only the animal substance, that is, the skin, the fleshy fibres, and the membranes; but, on opening these parts with a very short pointed instrument, and gently extending them with a small forceps, I soon perceived some small crystaline points, which I knew to be those of Swammerdam, hard, and resisting the impression of steel: their number was very inconsiderable.

I removed these substances from their original situation, and fixed them on the object-glass of the microscope. The greater part had an orbicular, elongated figure, and in size resembled grains of sand.

I let fall on them a drop of nitrous acid, which produced an immediate effervescence and solution; and hence I concluded that they were particles of carbonat of lime.

To this examination succeeded that of the upper jaws, and afterwards that of the matrix; and the result was, that I remarked the same number of globules soluble in the nitric acid as Swammerdam. I may further observe, that, on taking off the antennæ, the upper jaw, and the matrix, and putting the remaining part of the snails denuded of their shells into the nitric acid, a brisk effervescence took place in the same manner. From this circumstance I was led to suspect that there were still some calcareous particles concealed in the other parts of the bodies of these snails. I thus had the pleasure of ascertaining the accuracy of Swammerdam.

The prodigious number of small bubbles which rose from the acid during the solution of these stony particles, induced me to think that they were produced by the carbonic acid gas. This conjecture I was anxious to verify by means of lime-water; and, that the result might be more decisive, I placed four of these snails, deprived of their shells, in the nitrous acid, and caused the little bubbles to pass into a small tube filled with lime-water. The latter became instantly turbid, and precipitated the lime in the form of calcareous carbonat.

§ VIII.

But, it may be asked, is this phenomenon peculiar to this species of snails? That I cannot take upon me to state; I can only assert that I never remarked it in the land snails, of which I have treated in the first part of this work. They never occasioned a similar effervescence when I plunged them into the nitric and sulphuric acids; though it is, nevertheless, true, that a few bubbles made their appearance, but unaccompanied with noise or effervescence, both of which circumstances were uniformly manifested by the viviparous snails. Besides, the bubbles given out by the land snails, when plunged into water, consisted merely of common air.

§ IX.

The view of this phenomenon excites a desire to know how the parts of an animal, loaded with so prodigious a number of hard and stony granulations, can preserve their great flexibility. I shall, therefore, observe, that these granulations are situated so as never to form a connected whole, separating one part of the snail from the others, but are so disposed at regular distances, as not to impede the functions of the animal.

§ X.

But how has so large an assemblage of small calcareous granulations been formed in the bodies of this species of snails, and whence have they originated? The answer to these questions does not appear to me very difficult. It is certain that this matter is essentially the same as that which forms the inorganic part of the shells, with this difference only, that it is purer, and more like the crystalised carbonat of lime; for calcareous spar is always purer than the carbonat which has no determinate form.

We all know that the calcareous matter which enters into the formation of shells, is previously secreted in the body of the animal. If this matter be not secreted in too great a quantity, the whole will go to the formation of the shell; but, on the contrary supposition, the superabundant part will remain in the body of the animal, deposited in those parts where it may be productive of the least injury. The generation of pearls throws considerable light on this hypothesis. It is well known that this beautiful natural production is found not only in a state of adhesion to the shells of certain *testacea*, but also that it is sometimes met with in their fleshy substance. In the Thracian Bosphorus, near Terrapia and Constantinople, I procured several shells, the tenants of which often contained small pearls. The production of these pearls, the size of which I generally found to diminish in proportion to their encrease in number, and indeed that of the largest and most beautiful pearls, is sufficiently ascertained to be the effect of a redundancy of the liquor destined to the formation of the shell; in which case, from particular determining circumstances, the liquor is sometimes deposited in the interior of the animal. The same account may be given of the formation of the crystaline globules of the snail, since they are essentially of the same nature with pearls, both being carbonats of lime.

§ XI.

The waters of several rivers, and a number of ponds, lakes, and ditches, are almost entirely peopled with the viviparous snails; so that they may be said to be scattered over every part of Europe.

The figure of the shell is somewhat conical; that of the aperture nearly orbicular. The aperture, during the retreat of the animal, is perfectly shut by means of its opercle.

The operculum differs very essentially from those of other shells included under the same genus. It is neither membranous nor calcareous, but of a substance resembling that of horn, or rather that of nails, of which, on being burnt, it exhibits both the elasticity and the smell. It is always strongly attached to the body of the living animal, which can, at pleasure, protrude it, when it leaves the shell in quest of food; and, in like manner, retract it, when it wishes to shut itself in: in this case, it serves as a defence against external accidents.

At the approach of winter, or the first hoar-frosts of autumn, these snails quit the shallows, and retreat to the deepest parts of the waters, where they remain immoveable during the winter. At the commencement of spring, they issue from these retreats, and wander at first with a slow pace, in search of their food, in the shallowest parts of the water, but they never leave these to inhabit the dry land.

§ XII.

Swammerdam says, that this snail swims shell downwards in the water, by elongating, or in other words enlarging, the lower part of its body, called by this and every other naturalist *the foot* of the snail. This part, when the snail is swimming, is uppermost; and, in this respect, the Dutch anatomist compares the viviparous snail to another of the aquatic species, which he denominates the *common* snail, and which swims in the same manner.

This mode of swimming on the surface of the water being directly connected with the process of respiration, it was incumbent on me to attend to it on the spot. This snail was already familiar to me when I was professor of philosophy at Modena. It abounded in the well-known fountains in the environs of that city; and, in my preface to the Italian translation of "The Contemplation of Nature," I mention it as an animal which could reproduce its head after being amputated, in the same manner as several land snails in which I had remarked that phenomenon. From that time I was occupied in making observations on this snail: not, however, with any view to the subject of respiration, to which grand problem I had then no thoughts of turning my attention; but I was curious to become acquainted with its natural habits, and particularly its method of supporting itself and swimming in the water.

I always observed these snails slowly creeping along the muddy bottom, without having once seen them swim; while the common snails were uniformly at the surface of the water. During my residence at Pavia, not a summer passed, for the period of eight-and-twenty years, without my taking out of those stagnant waters a number of viviparous snails, in order to make the experiments stated in § IV. I always employed a small net for the purpose of dragging them from the bottom, and never in any instance did I see them swim. Hence never having, amidst so prodigious a number, seen any in the act of swimming, as Swammerdam alleges, I am compelled to differ from him in opinion. Besides, the description Swammerdam gives of the manner in which the common snail performs this operation, proves that the viviparous snail cannot swim in the mode stated by him. The former, as Swammerdam observes, has in its collar a tube which serves for the purpose of inspiring and expiring the air, when it is raised above the water. The cavity of its body being thus filled with air, it becomes lighter than water, and must of necessity swim upon the surface. But the viviparous snail, as we learn from its dissection by the same author, is not similarly organised; and though the collar of this snail has several apertures for the admission of air, yet it is not received in such abundance as to render the animal specifically lighter than water; and, consequently, if the one of these snails, on issuing from its shell, and inhaling the air, swims at the surface, the other must instantly sink to the bottom.

§ XIII.

The respiratory organ is, according to Swammerdam, composed of certain appendages, five of which appear from under the collar of the animal without dissecting it; the others become visible only after dissection, and are found to adhere longitudinally to the rectum. These appendages are membranous, and Swammerdam supposes them to be gills, because their structure and position are analogous to those of different aquatic animals: but, as he gives no direct proof of this assertion, I wished to put it to the test, by attending to the effects produced in the immediate vicinity of real gills. There is observed in those parts a more or less periodical and uniform motion of the water, flowing towards, and then repelled by, the gills. This is the process which takes place in fishes; and, to avail myself of the instance of animals placed in the order of snails, an analogous motion is remarked in two kinds of muscles, of which I shall speak in the following chapter.

§ XIV.

I had, therefore, to enquire whether I could discover the same phenomenon in these snails. It is evident that, to accomplish this object, it was necessary to place one of them in such a situation that it should leave the shell, and thus afford an opportunity of observing it with facility. This I could do at pleasure. Nothing more was required than to let fall to the bottom of a tube a newly-caught snail, which instantly issued from the shell and began to creep along the glass with its foot completely extended. It remained attached to the glass, by means of the viscous humour which it applied to it. Its progress was so slow, that its passage from one place to another could scarcely be discerned, except by marking a particular point in its course, which it gradually approached, and at length covered with its foot. I found that they required eighteen minutes to pass through the space of twelve inches: I shall, therefore, spontaneously call this snail the bradipode, or the tardigrade, of its genus. The excessive slowness of this animal prevented the water from exhibiting the slightest agitation, a circumstance which gave me a more complete opportunity of remarking, whether there was any slight periodical motion about its body; but I observed the water to be in the most perfect repose near the gills and every other part of the body.

§ XV.

The course of these animals, when confined in tubes filled with water, was from the bottom to the top, as they endeavoured to escape by the upper aperture; but, on reaching as far as the surface of the water, they make a stop and remain immoveable, with some inconsiderable parts, however, of their bodies out of the water, and their anterior parts protruded from the shell. This was a fresh position, well calculated for attending to the action of the supposed gills (§ XIII); but even in this situation the surrounding water never manifested the slightest motion, though I observed it with a lens, and the tubes were placed in the strong light of a solar ray. To convince myself still further of the solidity of my observations, I had put into the water some small pieces of a red substance, in order that their agitation, if any took place, might render apparent the motion of the water, which might otherwise remain imperceptible: but this expedient was employed in vain, and hence I was confirmed in the opinion that these appendages were not gills; for I could not assign that term to parts which do not perform its functions.

Besides, the most dextrous anatomists, in dissecting the minutest parts of animals, may be easily deceived as to their use; because, those parts not being yet sufficiently known, they are forced to conjecture the purposes for which they are employed. Thus Rhedi, in describing the internal parts of domestic snails, gives the name of spermatic vessel to the same part which Swammerdam calls the matrix: and who can tell, though they were anatomists certainly of the first rank, whether they were not both mistaken?

§ XVI.

I was, therefore, compelled to exclude the gills of these viviparous snails, and every other organ capable of performing the same functions by attracting and repelling the water; but, as it is an almost common opinion, that at each inspiration (if I may use such an expression) of the water, the gills absorb a portion of oxygen contained in it, must we thence suppose that this supply is unnecessary to the support of life in these animals? To resolve this difficulty, I put several of these snails, for some time, into water, upon which there was a certain quantity of common air.

I conceived that if there was any absorption of the oxygen in the water, the oxygen of the air would descend to supply its place, in consequence of their communication with each other, and because I imagined that a certain equilibrium must exist between them; and, consequently, the oxygen of the air must have diminished in proportion to the quantity absorbed of that in the water.

With this view I confined six snails in a tube half filled with water. A small stone attached to a string on which the six snails were tied prevented them from coming in contact with the air at the surface. The volume of the water, and consequently that of the air, was about eleven cubic inches. I left them in this situation seventy-two hours. The temperature was between three and four degrees. While they remained in the water, they were more or less out of their shells; and they were all in life when I took them out of the vessel.

The proportions of the azotic and oxygen gas remained the same in this confined air as in the open atmosphere. Hence I was led to infer that the snails had not absorbed any oxygen.

From the low temperature, however, which prevailed during this experiment, it could by no means be considered as decisive, since we have seen that this degree of cold rendered the land snails incapable of a similar absorption, although it always took place when the temperature was somewhat higher.

§ XVII.

Towards the middle of the following March, the thermometer standing at 11°, I repeated the foregoing experiment in the same manner with six viviparous snails, which were left in the tube during eighty hours: in this case the diminution of the oxygen gas, in contact with the water, was evident, but it only amounted to three degrees, or 0.03. I terminated this examination, by making the same experiment in August, when the thermometer indicated 21°, but in the course of eighty hours the consumption of oxygen gas amounted even to 0.05°. The snails continued alive after each of these three experiments.

I had, however, a doubt, founded on the supposition that the oxygen gas of the atmospheric air was absorbed very slowly by the water, which I had demonstrated with respect to pure oxygen gas in contact with this fluid*; but I knew this conjecture to be insufficient, at least for my purpose, since at the temperature of 21 and 22°, the air that remained on the surface of the water, during eighty hours, in a similar tube wherein no snails were confined, had lost about 0.01 of its oxygen gas, whilst that in which the snails were immersed, as usual, lost 0.05. This destruction proceeded then from the snails; and we could only explain it by supposing that the quantity of oxygen gas destroyed had been absorbed by the water, in the case of its absorption by these testacea.

But as these animals are destitute of gills,

* *Chemico esame degli esperimenti del signor Gottling, Modena*, 1796.

and without analogous respiratory organs, we must conclude that their skin is destined to fulfil this office.

It is hence evident, that although water be the natural element of these snails, they nevertheless, like the testacea, require oxygen gas from the atmosphere: notwithstanding this proof, however, as well as that I am about to relate, I fear we cannot acquit ourselves of error, in neglecting a precaution which apparently is altogether useless.

The snails of this species, which inhabit the stagnant water of small marshes and of some ditches, have their shells nearly covered with small green microscopic plants, and which it is almost impossible to detach from them. Those which remain at the bottom of large rivers have, on the contrary, their shells smooth and polished: we remark only some transverse and blackish belts, which in those snails that inhabit stagnant waters are covered with this vegetable in great abundance. It was possible that this matter might produce some chemical alteration, either on the air or the water, according to the circumstances under which the snails had been confined in them. On which account I thought of a new experiment with the snails of the Ticino, the shells of which are perfectly free from every kind of extraneous substance.

§ XVIII.

It follows then, from the preceding experiment, that these snails cannot live in a medium altogether deprived of oxygen gas, which is corroborated by the following fact. The water with which the half of the tube was filled in my new experiment had been as much as possible deprived of its air by boiling, and by means of the air-pump; the other half of the tube was full of azotic gas. I placed in this tube six Ticino snails; and, at the same time, enclosed in another six other snails; but in this last case I employed spring water and atmospheric air. In the last tube the animals lived about seven days, and the consumption of oxygen gas was 0.06, whilst in the first all the snails perished at the end of three days.

§ XIX.

Although these snails be inhabitants of the water, it appears, however, that they voluntarily expose some part of their bodies to the external air: when confined in vases, they also endeavour to attain the surface of the water, even by means of pushing out some part of their bodies, and they remain a long time in this position (§ XV). It is true the same habit does not prevail amongst them in a natural state; but, the vase being a kind of prison, they endeavour to attain their liberty, and for this purpose seek the surface of the water which is in contact with the air: it is, however, equally true, that although these snails do not leave the marshes, or large rivers, which they inhabit, they repair to their borders; where, from the shallowness of the water, their bodies are partly exposed to the air, from the influence of which they appear to derive great satisfaction. It would then appear that a constant immersion in water would prove hurtful to these animals, from not furnishing an adequate supply of oxygen, and that they seek the air in order to absorb a greater quantity of it. Is it not, however, also true, that, when these animals are partly out of the water, they consume a greater quantity of this vital principle than when wholly immersed in this fluid? This circumstance, which appears extremely probable, I wished however to establish on the solid foundation of experiment.

§ XX.

I put then into a tube, half filled with water and the other half with atmospheric air, but inverted on water, six viviparous snails, which I left at liberty, in order that they might gain the summit of the vase; and as comparisons, in the long course of my experiments, were necessary in order to form more accurate conclusions, I repeated this experiment at the same time with six of the same species of animals, retained at the bottom of the vase by means of weights. The first six ascended to the line which separated the water from the air, and, as usual, kept a small part of their body above the water: in this experiment, however, we observed them descend from the surface to the bottom of the water. These two tubes remained in this state during five days, and at the commencement of the sixth I analysed the air contained in them: the twelve snails were alive during the course of this experiment.

The air in that tube wherein the snails were confined underneath the water had lost $5\frac{1}{2}$ hundredths of oxygen gas, and that in the other where the animals were suffered to ascend to the surface of the water had lost $\frac{9}{100}$, and I found in this air $\frac{3}{100}$ of carbonic acid gas.

It is then evident that this superabundant consumption of oxygen gas proceeded from the great absorption of its base, doubtless by that portion of the body of the animals which came into contact with the air itself: in this last case the carbonic acid gas was present, because it had not been wholly absorbed by the water, as happened in the other tube upon the supposition that this gas had really been generated.

§ XXI.

It was scarcely possible any longer to doubt, that the absorption of oxygen gas from the atmosphere was greater, and perhaps complete, when the snails remained in the air: it was proper, then, to demonstrate the truth of this opinion, with which intention I confined some of these animals upon mercury in five inches of atmospheric air.

This experiment was conducted exactly in the same manner as the former, only the snails were kept in a state of confinement seven days. During this space of time all the oxygen gas disappeared, and seven degrees of carbonic acid gas had been produced.

This species of snail absorbs, then, not only the oxygen of the water, but also that contained in the air, although it be in a great quantity.

The three snails first submitted to examination were not only living when I withdrew them from the tube, but also three others that I put into it afterwards, although they had been for some time deprived of all oxygen gas. It is, however, true, that these snails will certainly perish if they be left several hours in this atmosphere, as I have frequently witnessed. This evinces the necessity they have for oxygen gas, in order to live longer out of the water, and it is doubtless in this way that they live a long time after being taken out of this fluid. I have preserved them more than a month in a basket placed near a humid wall: in this situation they remained immoveable, and the opening of their shells closed.

§ XXII.

It may be perhaps asked what purpose this oxygen of atmospheric air can answer in the economy of animals destined to live in water, and which soon perish when removed from this element. The anxiety evinced by nature for the preservation of the species will furnish a reply to this enquiry. This species of testacea frequently live in shallow waters, and are consequently exposed to a further diminution of their depth.

If these animals, like some other aquatic insects, could change their abode by means of feet or wings, they might readily discover more abundant streams; but the extreme slowness of their motions in the water, and their total inability to move on dry ground, preclude the possibility of their attaining a new habitation; they would perish, therefore, if the absorption of the oxygen in the air did not preserve them some time in existence. It is not unusual for pools or streams which have become dry to be again supplied with water, either by rain, by artificial means, or by the overflowing of some adjacent rivulet. In this manner, these snails frequently continue to live some time, although this may not always be the case when they are deprived of this vital principle: but that circumstance is not peculiar to snails alone, several other species of animals which rank higher in the scale of existence, and even some of the mammalia, owe their lives to the vital air of the atmosphere, when they are accidentally exposed on dry land.

§ XXIII.

I know not whether the shells of these snails absorb atmospheric oxygen, and whether it contribute to the preservation of their existence; or if it be incorporated with, and become a part of, the animal substance. It is at least certain that these shells when separated from the animal, and enclosed in common air, possess this absorbent quality. In this respect, as well as in their organisation, they resemble those of land snails. By means of the nitrous acid, we discover a somewhat dense membrane adhering to the convex part of the shell, of a bright chesnut colour, and composed of fine subtle fibres stretching towards one side: when the entire snail is put into this acid it retains its form; but the effervescence shews the solution of the calcareous carbonat, although the membrane preserves the perfect configuration of the snail with all its circumvolutions.

§ XXIV.

When the fœtuses of these snails are taken from the uterus and enclosed in common air, they deprive it, like the parent animal, of its oxygen gas; only, a greater number of them must be confined at once, in order to compensate for the smallness of their size.

§ XXV.

These snails continue after death to absorb oxygen with two remarkable differences: first, this absorption is then carried on much more slowly; secondly, a production of azotic gas takes place, which does not occur during the life of the animal.

§ XXVI.

The following consequences are deducible from these experiments:

1st, That the viviparous snails produce their young in every season of the year.

2d, That these fœtuses proceed from an egg in the uterus.

3d, That the viviparous animals have at first been probably oviparous.

4th, That these snails are, in the strictest sense of the term, hermaphrodites.

5th, That a prodigious number of stony crystaline grains are disposed throughout the bodies of these snails.

6th, That these snails are neither provided with gills, nor any other respiratory organ.

7th, That these snails, immersed in water, absorb its oxygen by means of their skin.

8th, That they absorb all the oxygen of

the common air, when confined in it, without producing any encrease or diminution of azotic gas.

9th, That the same effect is produced by the fœtuses taken from the uterus.

10th, That the shells of these snails possess the same property of absorbing the oxygen from the air.

11th, And lastly, that these snails continue to absorb this gas after death.

CHAP. II.

Duck muscle—*Mytilus anatinus*. LINN.
Swan muscle—*Mytilus cygneus*. LINN.

§ XXVII.

THE first muscle, termed *anatinus*, serves in some countries as the food of ducks, on which account I have called it the duck muscle, and the other the swan muscle, although I know not certainly if swans feed on this last animal.

The natural habits of these two species have a considerable resemblance. They both frequent the bottom of stagnant or slow-running waters, without attempting to ascend to the surface; at the approach of spring they seek out deeper water, and bury themselves in the sand. The locomotive power of these animals is extremely languid, and the mechanism of those parts of the body employed in transporting themselves from place to place is the same in both; it consists in extending a part of the body through the opening in their two-valved shell, which forms a kind of tongue.

Both species are nearly of the same size; the largest are about seven inches in length, and three and a half inches in breadth; only the shell of the swan muscle is much thicker than that of the duck muscle.

Such are the observations I have made in the marshes of Pavia, and particularly in those places, watered from the Po, termed by the fishermen *Pomorto*.

§ XXVIII.

These two species of testacea respire the water, which perhaps supersedes the necessity of their ascending to its surface to receive the influence of the atmosphere; as the viviparous snail, on the contrary, probably seeks the air from being destitute of organs fitted for the respiration of water.

In order to ascertain the reality of this important phenomenon, it was necessary to place on a plain, as the bottom of a vase, two of these muscles, and pour water over them to about the height of an inch. When the water became settled, they immediately opened a little the least obtuse part of the shell, through which the side of their bodies appeared: after the lapse of ten, fifteen, twenty minutes, and even more, during which period not the slightest motion was perceivable either in the water or amongst the muscles, they suddenly opened the two valves, and threw out with considerable force a small quantity of water: they afterwards shut them for a few minutes, and in this manner continued alternately to repeat these operations.

It is evident that nearly as much water enters into the shell each time the valves are opened, as is thrown out by their contraction; a continual periodical alteration in the water which enters, and in that which is thrown out, then takes place, which may be termed the respiration of water: and as two orders of gills are very evident on the surface of the body of these animals, as first observed by Swammerdam, we must suppose they are destined to absorb the oxygen of the water during the few minutes it remains in the interior of the shell.

§ XXIX.

In order to corroborate this opinion, I enclosed some muscles in an inverted tube, partly filled with air and partly with water, as I had formerly done with the viviparous snails; only it was unnecessary in this case to retain them at the bottom of the tube by means of weights, as they are naturally obliged to live under water.

I first directed my attention to the duck muscle, two of which I placed in a tube half filled with air and half with water, the temperature was from about 15° to 16½°. In this situation I observed the constant respiration of water, of which I have already spoken, as also the opening of the shell through which a portion of the animal was observable: this part has the appearance of a fin, which is protruded at every motion of the water, and again drawn back when this fluid remains at rest.

These two muscles remained alive at the end of seven days, and on the eighth the common air had lost 0.07 of oxygen gas; whence it follows that the base of this quantity of oxygen gas had been consumed by the animals, and that it must have previously been absorbed by the water, from the great affinity that subsists between it and this fluid.

§ XXX.

I next prepared two tubes, by filling one of them with pure oxygen gas, and the other with azotic gas; in each of these tubes I placed two muscles: in this experiment the water was deprived of its air, in the same manner as that I employed for the viviparous snails (§ XVIII). At the end of nine days the muscles were alive in the tube containing oxygen gas, which was diminished 0.08; but they perished at the end of three days in that with azotic gas.

On comparing these three experiments, we observe that the duck muscles not only absorb the oxygen contained in the water, but likewise as much more of this gas as is in contact with that fluid; from which it appears that oxygen gas is necessary to their existence.

§ XXXI.

These muscles die sooner when deprived of water than the viviparous snail. Before undertaking this investigation, it was first necessary to ascertain if, when their shells are shut—which frequently happens when they are out of the water—they contained any portion of air; because, during the experiment, it might escape, and, mixing with the common air, impart to it, more or less, the vitiated qualities it had contracted from the animal body: but, after having opened several of these shells under water, I never could observe a single bubble of air; but, on the contrary, a small portion of water always escaped, if I opened them beneath the receiver of an air-pump, or under mercury. I could then, without fear of error, undertake the experiment I had devised, and of which I shall only relate the result. One of these muscles absorbed nearly triple the quantity of oxygen when confined in air that it did when placed under water: in this manner all the oxygen gas of the air might be absorbed.

§ XXXII.

I made the same experiment with dead muscles, and found that in this state they produced a similar absorption, as well as the shells by themselves; with this difference, that the absorption was carried on more slowly.

It is interesting to remark, that a very large valve of one of these muscles, broken into pieces, remained several days in the air; and, after having absorbed from it all the oxygen, continued, twice successively, to absorb a fresh supply of it, after having been thrice subjected to this proof.

§ XXXIII.

At the same time I prosecuted these enquiries on the duck muscle, I made an equal number of similar experiments on swan muscles; but, although some differences certainly occurred in the results, they were too unimportant to affect the general conclusion, or to render it necessary for me to enter into a detail of them.

§ XXXIV.

In the account I have given of the observations I myself made on these two species of muscles, I have only spoken of the consumption of oxygen gas, and ascertained the precise quantity of it which is absorbed by these animals in water or in air; but, in order to avoid unnecessary repetitions, I have hitherto been silent respecting the azotic gas. I shall now, once for all, observe, that when the animals were alive, whatever quantity of azotic gas was present it always remained unchanged; we always found the 0.80 of this gas. These living crustacea possess, then, an affinity with the oxygen, but not with the azotic gas of atmospheric air. Notwithstanding, however, that this affinity with oxygen be preserved by these animals after the extinction of vitality, they nevertheless in this state augment the quantity of azotic gas, in consequence of the commencement of the putrid fermentation.

§ XXXV.

I shall only make a single observation respecting three sea shells, as I had but an opportunity of examining them in a very cursory manner. Being at Venice, in September, 1795, I observed the phosphorescent nature of the *Sepia officinalis*, the cuttle-fish, which I described in my *Chemico esame degli esperimenti del Sig. Gottling*. I expected to have made one or two experiments on the oyster, *Ostrea edulis et Jacobœa*, and on the *Mytilus edulis* of LINN., the common muscle. These three testacea are sold by the fishermen of this city, and are very common in the Adriatic Sea. They are dead when brought to market; but, as it was of importance to me to procure them alive, I went out to sea with the fishermen; and scarcely were they caught before I placed them in large vases filled with sea water, in order to subject them to the same experiments at Venice which I had formerly practised on fresh-water testacea.

§ XXXVI.

I was not able to ascertain if these testacea are furnished with respiratory organs, having no means of examining their internal structure, or even of observing any periodical motions produced in the water, if such organs really existed ; but, although I was not able to observe them with sufficient accuracy in such a short time, I will not affirm that they are destitute of such organs : I can, however, pledge myself with the greatest certainty for the truth of the following results, because I observed them with much accuracy, and even verified them on these three species.

1st, That they absorb the oxygen from sea water, as we observe by the diminution of the oxygen gas contained in the air, which is in contact with that fluid.

2d, That they absorb all the oxygen from the common air, when enclosed in this medium.

3d, That when azotic gas is made to rest on the water, they perish sooner than if atmospheric air be employed.

4th, That the shells separated from these animals, and enclosed in air, likewise possess the property of absorbing oxygen.

§ XXXVII.

Do these shells preserve this absorbent quality when placed under water? I obtained this absorption of oxygen with the shell of the viviparous snail (Memoir II. § XXIII), and with that of the duck muscle (§ XXXII).

We may then apply the above question to a more important enquiry, respecting the absorption of common air; since, in short, the natural element of these snails, and of some muscles, is fresh and salt water.

It is true, we observe some absorption of this principle by these animals, although immersed in water, as I have demonstrated; but there still remains a doubt whether this effect be partly produced by the shells, or whether it be wholly attributable to the living animals. In order to remove this doubt, it was necessary to make experiments on the shells alone; and I prosecuted this investigation at Pavia, where I had a sufficient number of fresh-water testacea always at my command, which was not the case with the salt-water testacea, since the idea only occurred to me after my departure.

I took, then, the resolution of employing with this view the shells of two species of oysters, *edulis* and *jacobæa*, as well as those of the common muscle, which I had in my cabinet. I placed the shells of these six testacea in six inverted tubes filled with water to a given height, and the remainder with common air. They remained nine days in this situation, under a somewhat high temperature: as I had perceived some elevation in the water contained in these tubes, I analysed the air, and discovered that a sensible diminution of oxygen gas had taken place in each of the six tubes: this diminution was, indeed, but inconsiderable, never amounting to more than $3\frac{1}{2}$ hundredths. I had at the same time enclosed in a separate tube an equal quantity of air and water, in order to ascertain what quantity of oxygen might be absorbed by the water, in the course of nine days, but it never exceeded 0.01. Hence it follows that the absorption of oxygen by these testacea is partly produced by their shells.

§ XXXVIII.

The following are the results of these experiments.

1st, The duck and swan muscles are both furnished with gills.

2d, They absorb oxygen from the water, and perish when deprived of this gas.

3d, When enclosed in air they absorb all its oxygen, but without producing any change on the azotic gas.

4th, This absorbent power continues after death.

5th, We remark analogous phenomena in the oysters termed *edulis*, *jacobæa*, and in the muscle *edulis*.

6th, These five bivalved shells, and that of the viviparous snail enclosed in air, absorbed from it all the oxygen, besides some portion of that originally contained in the water, when they were immersed in that fluid.

ON

RESPIRATION.

MEMOIR THIRD.

Reflections and additional Experiments on the Crustacea already examined, and on some other Animals of different Orders.

§ I.

RESEARCHES into the respiration of different animals comprehended in the class of worms, have been the subject of the two preceding memoirs. In the first I examined some snails provided with shells, and others that were naked: they were both land snails, and consequently continually surrounded with air; they possess real respiratory organs, adapted to absorb oxygen from the atmosphere, and they cease to exist when deprived of it. In this respect these cold-blooded animals resemble those with warm blood, only the last consume oxygen more rapidly, though in a less quantity than the former. If, for example, a small quadruped or a bird consume in fifteen minutes 0.14 of oxygen gas, it requires a great number of hours for a snail to effect the same consumption; but the snails will at last completely absorb the whole of this gas, whilst the quadruped or bird leaves at its death a portion of it unconsumed.

When I enter on the consideration of respiration in warm-blooded animals, I will shew in what manner quadrupeds or birds seldom consume more than 0.19 of the oxygen gas present in the air, and sometimes but 0.17, 0.16, or even 0.15. We cannot, however, affirm, that they might not consume more, were not their lives abridged by the production of carbonic acid gas; but I can assure the reader that even when this gas is neutralised, by means of a caustic alkali, as it is generated, these animals nevertheless perish in the same manner, leaving always unconsumed some hundredths of oxygen gas.

The rapidity of this absorption is produced, in these snails, by the frequent action of the lungs; so that they inspire and expire air every instant, and continually absorb from it, during this process, some portion of its oxygen, and this action is incalculably slower and less frequent in snails. Besides, they are more tenacious of life, and resist more powerfully, than warm-blooded animals, those causes which tend to destroy it, as we see evinced in the reproduction of their heads after amputation. These snails continue to live after the oxygen has been wholly destroyed, or at least when it has been very much diminished, whilst in a similar situation warm-blooded animals instantly perish. The same reason enables these snails to live a short time in an atmosphere from which they have absorbed all the oxygen they could absorb, as well as in hydrogen or azotic gas. I have said *that they have absorbed all the oxygen they could absorb*, because these animals do not possess the property of wholly absorbing it; but they leave about one half-hundredth part, so as to raise the mercury in the eudiometer to 0.20. I made this observation in order to give the greater accuracy to my former researches.

§ II.

The perfect agreement of the air respired by these animals is remarkable when tried by the phosphorus of Kunckel, which usually absorbs only 0.20 of oxygen gas; the cause is the same, because towards the end of the experiment the azotic gas becomes so greatly encreased as to dilute the oxygen gas to such a degree that it can no longer be attracted by these two bodies.

Another similarity subsists between our animals and phosphorus which merits attention. It is generally believed that this combustible appropriates the oxygen, and is enflamed by it at every temperature;

but I have offered the strongest proofs in my *Chemico esame* to show that it only begins to burn at about the sixteenth degree of the thermometer, and that at a lower temperature it cannot unite with oxygen. The same phenomenon proportionally takes place, at the freezing point, in our snails, and, at a degree lower, in the snails of Portugal.

§ III.

When the snails no longer continue to absorb the oxygen, the action of the lungs and heart, and of the whole vascular system, immediately ceases. In order to observe these and their relative phenomena, nothing more is necessary than to remove a part of the largest volute of the shell, which can be easily done without injury to the animal, as this volute is not attached to the fleshy substance. The lungs, the air-hole, the dilatation and collapse of that organ, the pulsations of the heart, and the circulation of the fluids in several of the vessels, become immediately discernible; and this facility of observation, without injury to the snail, is so much the more valuable, as, to the best of my knowledge, there is no other animal possessing lungs which can exhibit, in a naked state, this threefold phenomenon at the same instant of time, and, I may add, in the same place, and without the aid of dissection.

If a snail, prepared in the manner now described, be made, during the winter, to pass from an artificially-heated temperature to one which becomes gradually colder, the dilatation and collapse of the lungs become more rare, the pulsations of the heart less frequent and slower, and the motions of that whitish and semi-transparent fluid, which in them supplies the place of blood, gradually diminish in their vessels. In short, if they are carried to a spot where the thermometer stands at zero, the lungs will, in a very short time, be seen to have lost their elasticity; the air will no longer enter by the orifice appropriated to its admission; and a very few moments after, the heart will cease to beat, and the fluids to circulate. If the snail be kept for several hours or several days successively in this low temperature, all its organs will continue in a state of repose. On the contrary, if the animal be replaced in a milder temperature, their motions will be immediately renewed. The first movement is remarked in the lungs, which dilate and contract by slow degrees: the vibrations of the heart next make their appearance, and, after them, the circulation of the fluids.

§ IV.

The cessation of motion in these organs is not the mere result of cold, nor its renewal the simple effect of heat; since the same phenomena occur, in a warm temperature, on transporting the snail from common atmospheric air into any mephitic gas. I shall here state my observations on two snails deprived of their largest volute, and examined in azotic gas confined in a very transparent tube, the temperature being at twenty degrees.

During the space of eleven minutes, the appearances were the same as in common air. The orifice of the lungs opened and shut for the reception and expulsion of the azotic gas, as was evinced by the dilatation and collapse of that organ; the pulsation of the heart was frequent, and the circulation of the fluids uninterrupted: but at the expiration of the eleven minutes, the orifice remained open, the lungs continued flaccid, the heart lost its natural elasticity, and the circulation ceased. The two snails, which before creeped upon the sides of the tube, became instantly motionless: they were still alive, however, as appeared from their contracting themselves on being touched. They remained five hours in this state without being deprived of life. Before I restored them to the common air, I introduced into the tube another very small glass tube, covering it, in order to prevent the entrance of the mercury during its introduction, with a piece of paper, which I afterwards caused to fall off; to the lower extremity of the tube I fitted a bladder filled with common air. I then made the top of the smaller tube communicate with the pulmonary orifice of the snail, and began to introduce the air gently into the lungs, so as to force them to dilate. I repeated the operation several times, alternately applying and withdrawing the tube. This experiment was not without some effect. I had the pleasure of seeing the lungs dilate and contract, and the heart beat; but these motions speedily ceased, though they recommenced on the introduction of a fresh quantity of air, by means of the pulmonary orifice.

§ V.

I substituted azotic gas for the common air in the bladder, and, having introduced it into the lungs of the other snail, the heart of which remained immoveable during the preceding experiment, I inflated them at intervals in the same manner as I had done with the atmospheric air; but this viscus did not, in this case, exhibit a single instance of spontaneous motion. The *animation* of the lungs and the heart must, therefore, have been the effect of the oxygen gas. This deduction was confirmed by the removal of the two snails into the open air; for, very soon after, the heart and the circulation resumed their original activity, and the two snails, on shaking off their torpor, protruded themselves to their full length from their shells, and crawled along the surface on which they were placed. The connexion of oxygen gas with vitality is hence an evident fact. It appears that the living functions depend principally on the action of the heart, and that this action proceeds from the immediate operation of the oxygen gas; inasmuch as, when the gas no longer acts, the pulsations cease.

§ VI.

The elegant experiments of Lavoisier, and the pneumatic chemists, have revealed the secret of respiration. Its principal phenomena have been explained by the discovery of the oxygen gas and its properties. It has been demonstrated, that this gas is decomposed in the pulmonary vessels, and that vitality ceases the instant this decomposition terminates. By this principle we have been able to account for several circumstances that accompany this important animal function, which the ancients could not at all explain, or explained but very imperfectly. Nevertheless, the observations which I have made on an animal which may appear insignificant in the eyes of unreflecting persons, throw, I will venture to say, very considerable light on the theory of the illustrious French chemist.

They prove that the irritability, in consequence of which the heart contracts itself during the exclusion of the blood which passes through it, essentially depends on the combination of the oxygen with this hollow muscle, and that when there is no supply of oxygen the pulsations cease; though in a multitude of animals the oxygen gas of the atmosphere does not come into immediate contact with the heart, but merely with the lungs. The observations which I have stated convince me that this oxygen gas, by insinuating itself into the pulmonary blood, obtains access to the heart, and, by entering into combination with that viscus, maintains its muscular action in a state of excitement and life.

What I here state perfectly corresponds with the interesting and luminous experiments of Humboldt, which I slightly touched upon in my first memoir (§ XLVIII). In a memoir read before the National Institute, he mentions his having discovered, that the oxy-muriatic acid is a powerful stimulant of the animal irritability. Among the different facts which he offers in support of this truth, I shall only give one, which is perfectly suitable to my purpose. In a course of experiments on the phenomena of animal electricity, he submitted the heart of a frog that had ceased to beat to the oxy-muriatic acid. Its irritability was so far exhausted, that none of the mechanical stimulants could revive it. For the sake of comparison, he at first plunged the heart into the ordinary muriatic acid, without being able to perceive any motion; but, on being put into the oxy-muriatic acid, the pulsations recommenced and became stronger: they continued for several minutes, and, after ceasing, were re-excited by a new immersion in this acid.

§ VII.

But, if animal life be so intimately connected with oxygen, that a deficiency of the latter puts a stop to the former, why does the action of the heart cease? and how do snails live without it during winter? At least, during that season, the snail of Portugal does not decompose the quantity of common air which remains hermetically sealed within the opercle. When I first discovered this phenomenon, in the winter season, I found it to be in opposition to what I had observed in milder temperatures. I, however, reflected for some time upon this subject, in order to determine whether the difference of the seasons could explain why the snails required oxygen to the support of vitality in one season, and could dispense with it in another. I shall state the reasons which occurred to me; and if I cannot altogether untie the knot, I shall, at least, somewhat slacken it.

During the period when the snails feed, which is for several months of the year, there is introduced into their bodies, along with their aliments, by means of digestion, a portion of azot and carbonic acid, which passes into the mass of fluids, and by them is conveyed to the skin and exhaled; otherwise, by accumulating in the animal, they would extinguish life. In order, therefore, that these two elements should escape from the body in a determinate proportion, the circulation of the fluids is necessary, and consequently the action of the heart. If, then, this action cease, from a want of oxygen, the circulation will also cease; the hydrogen, azot, and carbonic acid, will no longer be expelled, and the animal will perish. But, if the season becomes cold, they cease to eat, and become torpid. On ceasing to eat, these two principles, the azot and carbonic acid, no longer enter into their system, and, consequently, it is no longer necessary to expel them. In a state of torpor, the suppression of the action of the heart, and the process of respiration, will not injure the snails, which may continue to live without oxygen; but their life will certainly be in that inferior degree which they exhibit during their lethargic period.

§ VIII.

It is not, however, my opinion that, even in this state of inanition, the fluids are perfectly at rest; for then the animals would be completely dead, at least we can form no other idea of actual death. I should, therefore, rather conceive that some degree of motion, however invisible, is continued in these animals; and that it is occasioned by a remaining irritability in the muscular fibres, which thesnails, on being pricked, manifest by a slight contraction, notwithstanding their profound lethargy.

§ IX.

Is this wonderful phenomenon of the temporary suspension of the action of the lungs, of the heart, and of the mass of fluids, exclusively exhibited by this class of animals; or does it extend to the other classes which sleep during winter—such as frogs, salamanders, serpents, and different quadrupeds, possessing a heart with membranous and vesicular lungs? In the sequel of this work I shall treat more particularly of all the circumstances relative to this phenomenon in different animals: at present, however, I shall anticipate so much of the subject as relates to the torpor of marmots, on account of the very great analogy which it has to my observations on the *testacea*.

I mentioned, at the commencement of this book, that the severe winter of 1795 afforded me a fortunate opportunity of remarking the changes sustained by the lethargic tribes in their animal economy. Among these, I made frequent experiments on two marmots which were half asleep when I received them in the month of January, and which afterwards sunk into the most profound repose. On their awakening, the signs of respiration are extremely manifest from the dilatation and contraction of their sides, in the same manner as appears in cats and other quadrupeds. When the marmots begin to sleep this sign diminishes, and in their lethargic state it altogether ceases. During that period the marmot has its eyes shut: it is no longer stretched at full length, but remains immoveable in a curved position, with the snout almost touching the lower part of the belly. It is cold to the touch; its mouth is shut close, and its teeth seem riveted together. It may be rolled between the hands, tossed about, and irritated in every shape, without exhibiting the slightest indication of life; in a word, it exhibits a true image of death.

Both my marmots were in this state. I took advantage of a very severe frost to subject them to the following experiments.

§ x.

I confined one of them, in the first instance, in common air under a glass bell, plunged into the mercury of the pneumato-chemical apparatus, which was exposed for several hours, near an open window, in the night-time, to a temperature of — 12°. It is universally known, that if a cat or dog be confined in a recipient filled with common air, they attempt to escape; then respiration becomes painful, and soon ceases altogether. The marmot, on the contrary, remained as motionless as a stone. I left it three hours and a half in this situation, and during that time the mercury had not changed its level. Before I removed the mercury, I took a portion of the air in the recipient, and subjected it to a chemical analysis. I found that it had not undergone the least alteration; and that it was, in every respect, similar to the air of the chamber in which the experiment had been made.

From this experiment it was impossible to form any other conclusion, than that this quadruped had not respired during its confinement under the recipient. But, on removing the marmot to a less severe temperature (that of zero, for example, in which I left it), it manifested some very faint appearance of inspiration and expiration, which I could perceive by a very slight elevation and depression taking place at regular intervals in its sides. The torpor, though less intense, was not, however, discontinued.

§ XI.

The cold continuing in all its severity, permitted me to make some new experiments on my quadruped. Having plunged it into its original torpor, by exposing it, during the succeeding night, to all the rigour of the season, in the open air, the thermometer being at $-12\frac{1}{2}°$, I placed it in the recipient filled with carbonic acid gas. I put in along with it a rat *(mus rattus)*, which died almost instantly. The marmot remained four hours in this deleterious gas, without shewing the smallest sign of motion. Having then removed it into a higher temperature, I perceived the renewed respiration from the motion in the sides, and was thence convinced that it had not perished in the carbonic acid gas.

§ XII.

I submitted it to a third and last experiment, apparently less severe than the two former, but which, notwithstanding, proved fatal. I have observed, that the temperature of the freezing point did not render it entirely torpid; and that, though extremely feeble, its respiration was even in that case still perceptible. I placed it, for the second time, in an atmosphere of carbonic acid gas, at the temperature of zero. I now remarked two circumstances: the one, that the slight lateral elevation and depression which took place periodically had entirely ceased; the other, that the body of the marmot was extended in a straight line, instead of being disposed, as I have stated it to have formerly been, in a curved position. This alteration made me fear that it had perished. To my great regret, my fears proved well-founded; since, on removing it into the common air, at a temperature proper for re-awakening it, it was not restored to life.

§ XIII.

I shall, at present, make no remarks upon this phenomenon; I shall treat of it in another place. Upon this occasion, I shall only observe, that the properties of this warm-blooded animal are precisely the same with those of the cold-blooded testacea. The respiration of the former was suspended during its profound torpor, and it required no supply of oxygen gas, until it began to emerge from that state. Snails in the same manner, when lethargic, lose the power of attracting oxygen gas, and nature has so ordered it that it is then no longer necessary to them; but on their resuscitation, they infallibly perish, if they are deprived of this gas.

§ XIV.

During the absorption of all the oxygen, the snails usually absorbed some azotic gas. I will frankly confess that, when I entered on these researches, I was prepossessed with an opposite opinion, founded on the interesting experiments of the celebrated Jurine, of Geneva, an account of which is given in a memoir which obtained the prize from the Society of Medicine at Paris, in which he proved that the air expired by man augmented the quantity of azotic gas; but the fact was directly contrary, and in the naked snails no augmentation or diminution of this gas took place. A moment's reflection taught me not to be astonished at a diversity, which may readily be explained from the different organisation and wants of the variety of beings which form the chain of animal existence. It is also evident from all those I have examined, in the progress of my researches, that the mechanism of the respiratory organs is not the same throughout the animal creation; but that nature, in order to attain the same end, has recourse to different means according to the diversity of animals, but of which a knowledge can only be obtained by experiment and observation.

§ XV.

But we have seen that snails continued to absorb oxygen after the extinction of vitality, only in a less quantity than during life.

This observation proves that this absorption is partly produced by the heart and lungs, and partly by the body of the animal. If this effect occurred after their death, it affords no reason for supposing that the same effect was not produced during life. I conjectured, however, that, with the extinction of vitality, the snails would gradually lose their affinity with oxygen. I was not, however, perfectly certain if this was really the case, but I deemed it of sufficient importance to be decided by experiment.

§ XVI.

Some common garden snails, and others of Portugal, were, with this view, placed separately in several small glass vessels filled with common air. They perished in this situation at the end of thirty hours, after having consumed all the oxygen. I proposed to examine the air in contact with these animals, and to renew it at the termination of every thirty hours; but I removed their shells, in order to observe them with greater accuracy. The thermometer indicated from 13° to 16° during this time, but the putrid fermentation was not much accelerated.

Thirty hours after the death of these snails, they began to diffuse a very disagreeable odour; but their organisation remained entire. On analysing the air, I discovered that all the oxygen gas had disappeared: I do not speak of the carbonic acid, or azotic gas, because they had no relation to my present enquiry.

At the termination of sixty hours it was evident, from the odour and the change of colour in the fibre, as well as from the greater softness of the parts, that the putrefactive process had advanced; but the oxygen gas had in this case likewise disappeared.

In ninety hours the snails were reduced to a state of complete putrefaction, more especially the most tender parts, and the air was entirely deprived of its oxygen.

At the end of a hundred and twenty hours the fœtid odour was less disagreeable, the tender parts were reduced to a jelly, and those which were muscular and hard, had become perfectly soft; the oxygen gas, however, had entirely disappeared.

§ XVII.

Circumstances were greatly changed at the conclusion of a hundred and fifty hours; the absorption was only 0.11, and the phosphorus absorbed the other 0.09 parts: but the mass of the body of these snails was very much diminished from the loss of their component principles, and especially that of the azot which had escaped under a gaseous form. We can then affirm that snails continue, after death, to possess the property of absorbing oxygen, from the commencement to the termination of the putrefactive fermentation; or, in other words, until the animal is reduced to a state of complete decomposition.

It is, however, proper to observe, that atmospheric oxygen had been five times absorbed by these animals—once during their life, and four times after their death—without their having attained that point of saturation which prevented them from imbibing any additional quantity. This faculty of absorbing oxygen after death is also possessed by the other species of snails, without even excepting slugs, as I have verified by similar experiments.

§ XVIII.

The powerful agency of heat and water in decomposing animal bodies is well known, as we see by their diminution in boiling; will the faculty of absorbing oxygen be destroyed in snails by subjecting them to this process? Conceiving this to be an enquiry of some importance, I boiled some snails in water until their soft parts were almost wholly decomposed, after which I enclosed them in air under close vessels, and by this means obtained the following result. I very soon perceived that boiling had not diminished their power of absorbing oxygen, either at the commencement or in the progress of the putrefactive fermentation.

§ XIX.

In giving an account of the destruction of oxygen gas in the whole of these experiments, I have always affirmed that its base was attracted by the animal fibre, by means of a chemical power, even after the extinction of vitality, and that its base is combined with the fibre, although it be reduced to a state of complete decomposition. This doctrine is supported by the strong proofs I have given in my two Memoirs, in which it is demonstrated that the carbonic acid is not the product of the combination of atmospheric oxygen with the animal carbon, since facts clearly prove that the carbonic acid previously existed in the animal, and that it escaped from it under a gaseous form. It appears, then, that the destruction of oxygen is not the cause of the appearance of carbonic acid.

In the prosecution of these new experiments, it occurred to me to make another which might be more decisive; with this intention I enclosed these testacea in a medium wholly deprived of oxygen gas: in this case, if no carbonic acid gas were produced, it would furnish an incontestible proof that its production depended on the combination of the oxygen contained in the air with the carbon of the animal; or if carbonic acid were really present, nearly in the same manner as when these testacea were confined in common air, it would then, on the contrary, demonstrate that the formation of this gas was wholly independent of the oxygen of the air, and, consequently, that it was evolved from the animal body.

My first plan was to enclose these snails in a tube filled with water, and inverted over mercury; the water employed in this experiment was boiled, and subjected to the action of the air-pump, in order to ascertain if any permanently-elastic fluid appeared in the tube. After the death of these animals, they began to give out air, and at the termination of seven days I had collected an inch of it, which was sufficient to fill one of my eudiometers. By a chemical analysis of this air, it was found to contain 0.43 of carbonic acid gas, and that the remainder was a mixture of hydrogen and azotic gas. The above quantity of carbonic acid gas having been obtained, independently of atmospheric oxygen, it is evident that it must have proceeded from the carbonic acid evolved by these two testacea. The common garden snails were the subjects of these experiments; but I conducted, at the same time, other similar experiments on some of the Portugal snails, and I found in the aëriform fluid 0.46 of carbonic acid gas.

§ XX.

Not entirely satisfied with these two experiments, by which I had ascertained the production of carbonic acid, I made a third, in order to demonstrate this circumstance more readily and with still greater certainty, by placing these testacea in azotic and hydrogen gas. It was only necessary that these gases should be perfectly pure: I obtained the first from the fibrous parts, well washed from the recent blood, and treated with the nitrous acid in the hydro-pneumatic apparatus, according to the method of the celebrated Berthollet.

I confined five Portugal snails in five tubes filled with this gas, during forty hours; and, when I withdrew them, they were not only dead, but had begun to exhale a fœtid odour. The five measures of azotic gas had remained, and filled an eudiometer. After pouring them into it, and marking the space which they occupied, I washed them with lime-water. The first gave 0.08 of carbonic acid gas; the second, 0.09; the third, 0.06; the fourth, 0.105; and the last, 0.08.

It was then evident, that this carbonic acid gas proceeded from the carbonic acid of the snails.

§ XXI.

I was uncertain if this acid was the product of the living or dead snails, or whether it was generated by them during both these two states. As snails can live several hours in azotic gas, it was not only easy to obviate this doubt, but to throw considerable light on the subject, by making experiments at the same time on living and dead snails, in separate tubes filled with azotic gas and common air.

I took, with this view, equal measures of these two aëriform fluids, and put four snails into the azotic gas, and four into the common air: not only were these animals of the same species, Portugal snails, but I selected them, as nearly as possible, of the same size and strength, and placed them exactly in the same situation. On removing them from the tubes, at the end of twelve hours, I found the animals still alive; I examined the two aëriform fluids, and was astonished to discover that the quantity of carbonic acid gas was greater in the azotic gas than in the common air.

Common air.	Carbonic acid produced.	Azotic gas.	Carbonic acid produced.
1st tube, - -	0.07	1st tube, - -	0.08
2d ——, - -	0.05	2d ——, - -	0.10
3d ——, - -	0.04	3d ——, - -	0.05
4th ——, - -	0.06	4th ——, - -	0.07$\frac{1}{3}$

§ XXII.

In these three cases wherein azotic gas was employed, the snails furnished more carbonic acid gas than those enclosed in common air; and, in the fourth, the quantity was equal in these two fluids: as the differences were not considerable, I thought, perhaps, they might proceed from the snails themselves, more especially as in the azotic gas three of them had furnished a greater quantity of this acid than the others. If, however, this difference had been accidental, we could discover it by several times repeating the same experiments, which I did on seven snails placed in common air, and seven other enclosed in azotic gas. Without here entering into a detail of these experiments, I shall only observe that, although the carbonic acid gas was produced in a less quantity in two cases, in azotic gas than in common air, it was, however, more abundant in five others, which sufficiently establishes the constant occurrence of the fact.

§ XXIII.

I next entered on the experiments I proposed to make upon dead snails; and, after killing them, by means of boiling water, I enclosed the same number in common air and azotic gas as I had done of the living animals. The carbonic acid gas was more abundant in the two fluids, in proportion to the greater length of time that these testacea were suffered to remain in the tubes; but here the superiority of the quantity of carbonic acid gas in the azotic gas over the quantity of this gas in the common air was not doubtful.

§ XXIV.

Having likewise proposed to make these experiments in hydrogen gas, I procured it by means of iron and phosphoric acid diluted with water; and, in order to obtain it of greater purity, I employed the method described in my *Chemico esame*. Several living and dead snails were placed separately in this gas, an equal number as in the common air. I examined the two fluids in which the living snails were enclosed before death had taken place, and in both the two carbonic acid gas had been produced; but with the same difference as in the former experiments: its quantity was most frequently greater in the hydrogen gas than in the common air. The same difference occurred in the experiments with the dead snails, so that the production of carbonic acid gas in hydrogen gas was nearly the same as in azotic gas.

I enquire not here why the quantity of carbonic acid gas was greater in azotic and hydrogen gas than in common air. I shall only conclude, from these experiments, that it is clearly proved that the carbonic acid gas produced by the living and dead snails in common air, resulted not from atmospheric oxygen, since an equal and even a greater quantity of it was obtained in azotic and hydrogen gas; consequently, in the oxygen gas destroyed by the presence of these animals, its base alone is absorbed by them either during life or after death.

§ XXV.

But does this power of absorbing oxygen, which is retained by the animal fibre even after death, and during the progress of decomposition, alone appertain to these testacea, or does it extend over the whole animal kingdom? The motives which induced me to enter on the subject of respiration in various classes of living being, did not permit me to overlook so interesting a point of view, on which I will particularise the details as they present themselves. At present I shall only make a single observation, in order to satisfy the curiosity of my readers; that I have discovered this faculty in the class of insects, fishes, oviparous quadrupeds, serpents, birds, viviparous quadrupeds, and man himself.

Animals, generally considered, when they cease to respire, although decomposition be commenced, continue to absorb oxygen, at least to a certain period; so that the lungs consume only a part of the oxygen of the atmosphere, whilst the remainder is absorbed by the external surface of the animal body.

§ XXVI.

It has been demonstrated, in the course of these experiments, that not only the fleshy and soft parts, but also those which are hard and calcareous, as the shells of snails, absorbed atmospheric oxygen.—These shells even retained this absorbent power after they were dead, or had been a long time separated from the animal, as I have proved in my first Memoir, by the shell of the vineyard snail, *Helix pomatia*, LINN., after being separated eighteen months from its animal. I shall relate a new observation, still more singular, but equally true.

Having learned that the shells of viviparous snails destroyed, when kept under water, the oxygen gas diffused in that fluid, I wished next to ascertain what effect would follow from subjecting the shell of a vineyard snail to a similar experiment. With this view, I took four of these shells, and kept them, during seventeen days, at the bottom of an inverted tube, containing five inches of water, and above two of common air. From the nature of the snail, I conceived that, at the expiration of this period, the enclosed air would not have undergone any chemical change: but in this opinion I was mistaken, since azotic gas only remained in the tube. These shells possess, therefore, the power of absorbing the oxygen of the water, which is replaced by that of the air, and which is, in that case, either wholly or partially absorbed.

§ XXVII.

What is the use of this absorption of oxygen by the shells of land snails? Does it bestow on them solidity, and by uniting with them become an essential part of their substance? or, rather, does it penetrate through their substance into the internal parts of these animals, and concur towards the preservation of their life? Since this absorbent power continues after their separation from the snails, the incorporation of this principle with the matter composing the shells is incontrovertible; but I scarcely hesitate to believe that, when joined to their proper inhabitants, a portion of this principle passes into the animal, by the intervention of the tendinous muscles which connect it with the shell.

This opinion is built on the following fact: the shells of Portugal snails, enclosed in common air, absorb from it a greater quantity of oxygen than they do when the animals are shut up in them, by means of their calcareous opercula, which preclude all communication with the external air.

§ XXVIII.

If, from the analogy which subsists between shells and the animals that inhabit them, a similar organisation be deducible, I have demonstrated that the former, as well as the latter, equally absorb oxygen; and, if my conclusion be well founded, the same analogy must lead us to suppose that it may be extended, in like manner, to egg-shells; a circumstance which has been since fully confirmed. If, moreover, it be shewn that a part of the oxygen, absorbed by the shells of snails, passes into the body of these animals, and probably concurs to the maintenance of life, a similar absorption of oxygen into the interior part of the egg must be admitted to take place by means of their shells, as in both cases the cause is the same, although it be more obvious in the latter, from our more readily perceiving the necessity for a supply of this principle.

The irritability of the heart, excited by the blood, produces, as is well known, the contraction of that organ; but it depends essentially on the combination of oxygen with this viscus. This process is carried on, in birds, through the agency of the lungs, in whatever manner the action of this organ may commence, and although they be enclosed within the egg. During the period, therefore, these animals remain confined within the egg, the oxygen must find its way into the small heart of the fœtus, through the very subtle pores of the shell. Whilst the process of incubation is going forward, the most essential use of the shell is to convey oxygen to the inert fœtus, by which means it becomes animated, has its parts evolved, and gradually arrives at a state of maturity; and when, on quitting the shell, it ceases in this way to receive its usual supply of this principle, it immediately receives it by means of the lungs, which the oxygen gas had put in motion, and which begin to respire; the embryo then only ceases to live when the action of the lungs is discontinued.

§ XXIX.

Another effect produced on the embryo, by the oxygen which penetrates through the egg-shell, is that of giving a red colour to the blood. It is sufficiently established by the observations of every chemist and physiologist, that the blood acquires, by means of its oxygenation, a red and brilliant colour; but, so far as I know, their researches have never been directed to the origin of this colour. Physiologists have justly observed that at first the blood is yellow, and that it afterwards acquires a brown colour, which gradually becomes deeper until it assumes a reddish hue. In my work, entitled *Des phénomènes de la circulation observée dans le cercle universel des vaisseaux*, in detailing the results of the experiments contained in the first dissertation, I examined this physiological point; and it appears to me that this variety of colours was altogether an optical illusion, as, in reality, I only found the red colour, which encreased, however, gradually in intensity. Returning to this subject, I am inclined to illustrate it by the proofs drawn from my experiments; and as this work, printed at Modena in 1773, is now become scarce, I shall here transcribe that portion of it connected with my present subject, in order to explain the red colour of the blood by the pneumatic doctrine.

§ XXX.

It is now generally admitted as a fact, that the blood in animals is at first yellow, that it afterwards assumes a rust colour, and in the end becomes red: *Primævum sanguinis colorem flavum esse, qui per varios rubiginosi coloris gradus in ruborem confirmatur*, as Haller has asserted in his physiology, supported by observations made by him on the *chick in ovo*; and which was before maintained by other celebrated physicians and anatomists, among whom are Senac and Malpighi.

§ XXXI.

Being engaged in similar experiments, it was natural for me to attend to this part of the subject, uninfluenced by the opinion of those celebrated men, in order that I might observe only nature herself. This is at least the mode I have always pursued, when it was possible, with respect to the most universally received opinions, however respectable the quarter whence they proceeded; I have always myself examined the facts on which they were built.

§ XXXII.

Respecting the *chick in ovo*, I can affirm that I have myself likewise observed, at the commencement of incubation, the same shades in the blood; that is, I remarked the yellow, the rust colour, and the red: but I never observed them succeed each other; they always appeared at the same time. After forty hours' incubation, I perceived the blood yellowish in the small vessels, of a rust colour as they encreased in diameter, and in the heart itself of a shade betwixt this and red (Exper. 115). Anterior to that period, the circulation was not observable, and the blood appeared perfectly colourless. At the end of the second day, although all these colours were still perceivable, that of the yolk was least evident (Exper. 116).

§ XXXIII.

The simultaneous appearance of these three colours somewhat embarrassed me, as I could not comprehend how it should happen that the yellow, which is the original colour of the blood, should always be seen at the extremities of the vessels; nor was my embarrassment lessened by observing, that this fluid, which was red in the heart, on arriving at the arterial vessels should at first assume a rust colour, and afterward become yellow when it had reached their extremities. Neither could I better understand how, after preserving its yellowness in the most minute vessels, it should assume a rust colour as they encrease in size, and again become perfectly red on its entrance into the heart.

These three colours in the same blood, circulating in the same vessels, made me hesitate respecting the reality of their appearance, and induced me to believe that they must be the effect of some optical illusion which had not hitherto been observed. I could not view these vessels without having the eye dazzled by the yellow colour of the yolk beneath them, which is evident through their transparent coats, and which appeared to me to be the cause of this fallacy. I conjectured, therefore, that, although the blood be wholly of a red colour, it may yet, however, assume a yellow appearance in the extremities of the arteries and veins, from their minuteness being such as readily to transmit the colour of the yolk: this hue becomes also less evident in proportion as the vessels augment in size, doubtless because their coats are not only thicker and more opaque, but because they contain a greater quantity of blood, which may impart to it a rust colour. In short, this fluid resumes its redness in the heart, because, from its greater abundance, it is less influenced by the colour of the yolk.

§ XXXIV.

This doubt appeared to me well founded, because it is built on a fact. The small cutaneous vessels which creep on the shining yellow bands upon the breast and belly of the salamander, do not appear of the same colour. The largest are slightly red, the middle-sized of a yellowish red, and the smallest nearly yellow (Exper. 63). This diversity of colour, however, is produced by the greater or less impression made on these vessels by the colour of the ground over which they are disposed; since it has been demonstrated that each genus of vessels in the salamander contains only red blood, as I have myself observed in all my experiments on these animals.

§ XXXV.

Another still more direct fact, furnished by the chick, confirmed me in this opinion. If the blood, which appears through the coats of the vessels of a yellow or rust colour, assume a red tint in escaping through wounds made in these vessels, and if it preserve this redness after coagulating round these openings (Exper. 115), I may affirm that those colours are not proper to the blood, as the subsequent experiment will demonstrate.

In the third dissertation of the same work, I pointed out the proper mode of removing and placing the vessels of the chick, upon a glass plate, without impeding the circulation (Exper. 37). In performing this process, I was anxious that not the smallest portion of the yolk should remain on the plate, but then the yellow and rust colour disappeared, and the blood assumed its original redness, only it was somewhat paler in the small extremities of the vessels (Exper. 37).

§ XXXVI.

In addition to the above facts, I shall

mention another which I discovered since the conclusion of those experiments. Sometimes, although seldom, I have observed, before the fortieth hour of incubation, a beginning circulation, when I could only frequently discern a small yellowish spot, which appeared reddish on being removed to the glass plate without any portion of the yolk attached to it, and when viewed in this situation, by means of a very powerful lens, it seemed to be composed of a network of very small nascent vessels. These facts sufficiently prove, that in the chick the original colour of the blood is not yellow, but a pale red; because in it the blood has yet only received the first impression of nature, and it is only in the progress of time that it acquires a greater degree of intensity, and at last assumes the scarlet hue proper to this fluid.

§ XXXVII.

Cold-blooded animals resemble, in this respect, those with warm blood; for as no coloured body is interposed beneath the vessels in this last class of animals, we observe not that mixture of rust colour or yellow, as in the chick: I allude here to tadpoles, during the first three days after exclusion, the blood of which is wholly colourless (Exper. 145, 146). On the fourth day it begins to redden where a great number of globules are accumulated, as in the heart, but its colour still continues extremely faint (Exper. 147, 148): so necessary, indeed, is a considerable number of molecules to render the colour evident, that on the sixteenth, and even on the eighteenth, day, the blood in the smallest vessels still preserves its transparency (Exper. 154, 155). On the twenty-second day, the red tint is diffused throughout the whole mass of blood, although it be still extremely pale in the most minute vessels (Exper. 156). In short, it is only by slow degrees that this fluid acquires its highest intensity.

I shall here only remark, the important difference observable between the redness of the blood in cold and warm-blooded animals. In the last we perceive, on the second day of incubation, a beginning redness in the blood, although for some time it continues pale; whilst in the former the same appearance is only discernible on the fourth day. In warm-blooded animals this fluid is of a fine purple, from the first day (Exper. 127); whilst, in those with cold blood, it is still pale on the fiftieth day (Exper. 162); it is only on the seventy-first, or rather the seventy-second, that it acquires all the redness it possesses in the full-grown animal (Exper. 164, 165). The small degree of heat present in the tadpole, and the high temperature of the chick, concur, I believe, in a great measure, to produce this difference.

§ XXXVIII.

It was thus I expressed myself respecting the original colour of the blood, in demonstrating it to be red, but extremely pale, and that it assumes by degrees a deeper tint, until in the end it becomes of a deep purple. I wished to express why this colour is in the first instance so extremely pale, when I said *that the blood had yet only received from nature the first impression.* This explanation was, however, too vague and general; but it could not then be more precise, because at that period the great discovery had not been made of the two component principles of atmospheric air, oxygen and azotic gas. Since, therefore, the base of the oxygen gas augments, by its combination with it, the redness of the blood in these animals, it likewise produces it, in the first instance, after their exclusion. The same effect is produced, on the embryos of birds in the egg, by means of the oxygen which penetrates through the shell, and which, combining with the blood, imparts to it a red colour; but the oxygenation being thus begun, and continued by degrees in proportion to the quantity of this principle which enters into the egg, the fluid assumes a darker and darker hue until it becomes perfectly red. This progressive encrease of colour is observable, on the same principle, in several metals, during the process of oxygenation.

§ XXXIX.

I had remarked that this encrease of colour is produced much more slowly in cold-blooded animals, such as the tadpoles of frogs, before either the principle of oxygenation or its laws were discovered. I have the satisfaction to think, that to a certain point I had therefore attained the truth, when I affirmed that, if the blood in the chick is of a deep red, whilst it is very pale in tadpoles, the difference must probably proceed from the different degrees of heat in these animals: but we know, at present, that oxygenation is more readily accomplished in proportion to the higher degree of temperature.

§ XL.

From the land we proceed to the water testacea, which likewise merit some consideration. Several of them are furnished with true gills, in order to respire water, as the duck and swan muscles; whilst others are wholly destitute of them, as the viviparous aquatic snail: oxygen, which it should seem is necessary to their preservation, is absorbed not only by these two tribes of animals themselves, but by their shells. Observations have not been sufficiently multiplied to ascertain whether the infinite number of different kinds of fresh and salt-water testacea be furnished with respiratory organs; as, however, those which are destitute of such organs, equally as the others, require a supply of that principle, we are led, by the strictest analogy, to conclude, that the same law must prevail among those we have not been able to examine.

It is this principle, therefore, which preserves in existence this immense number of aquatic and land animals. The sea water attracts the carbonic acid; which is evolved in a prodigious quantity under a gaseous form by the process of respiration, combustion, and by the decomposition of animal and vegetable bodies. This acid is taken in with the food by the testacea, and combines not only with their bodies, but with their calcareous shells. The water very slowly attracts atmospheric oxygen, which by penetrating into their shell readily concurs towards their preservation.

§ XLI.

But what must we conclude respecting that innumerable multitude of other marine worms, termed zoophytes from their external resemblance to that of plants although they be real animals; such as the following genera: Alcyonium, Sertularia, Pennatula, Madrepora, Tubularia, Millepora, Coralium, Eschara, Gorgonia, Cellularia? We know, likewise, that each of these genera is divided into several species. Are they provided with gills, or other analogous respiratory organs? What shall we affirm respecting that other class of small marine worms, not less numerous than the zoophytes, but which, like them, are not attached to any extraneous body? The extreme minuteness of these animals, the simplicity of their structure, and the attention of those who have discovered or described them having been rather directed to some other object than that of ascertaining if they possessed respiratory organs, are, perhaps, the true cause of the obscurity which prevails in this part of the animal economy.

I have myself examined several genera of these animalcula, which are found in the Mediterranean, Adriatic, and various other seas, an account of which is published in the *Mémoires de la Société Italienne.* Several of these were formed under my own eye, in salt water. By means of small fibres from their bodies, a slight current flows into the opening destined by nature to receive the food; and at the same time, some particles mixed with the water enter by a small canal into these animalcula: it is hence evident that this agitation in these fibres serves to introduce by that current nourishment to this class of animals. A very great number of them, as zoophytes, are attached to a body either calcareous, or of a different nature: they cannot even move in search of food, but they are so organised by nature that the aliments naturally flow into them.

It is true that, except this current of water towards the mouth of these animals, no other motion is perceivable in the surrounding fluid to make us suspect they are furnished with any organ analogous to gills; but are we certain that this respiratory organ may not open internally? that it may not be situated within the mouth, against which the current continually beats? Besides, when this innumerable tribe of small animals receive not from nature any proper respiratory organs, we may conclude that their whole body is destined to fulfil this office, by sucking, so to speak, the oxygen from the surrounding water. Is not an example of this kind furnished by the viviparous snail?

§ XLII.

Doubtless, oxygen is necessary to these marine animalcula. Analogy leads us to believe that fresh-water animalcula greatly resemble in their habits those of salt water.

I speak here of the animalcula infusoria, so termed because they are produced, encrease, and multiply, in water wherein animal or vegetable substances are infused, even when decomposed by fermentation, and which serve as food to these small beings. The organisation, at least of several of them, is extremely simple; being composed of a very small bladder enclosing a number of minute grains, without the least appearance of viscera, or any vascular system, although these animals are for the most part very transparent.

When occupied several years ago with the system of Needham and Buffon on the generation of these animals, I was induced to study the nature of these microscopic animalcula, by considering them under different points of view. I endeavoured, in particular, to ascertain if they could live independently of atmospheric air: and I discovered that, on being confined in the exhausted receiver of an air-pump, they lived longer than other animals; but in this situation they, however, perished in a longer or shorter space of time. Consequently, if these beings, which are justly considered as occupying the lowest degree in the class of worms, require a supply of oxygen, I am warranted to suppose that the same may be affirmed of all the animals ranked in this class.

FINIS.

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