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TORONTO

THE EARLY NATURALISTS

THEIR LIVES AND WORK

(1530–1789)

BY

L. C. MIALL, D.Sc., F.R.S.

MACMILLAN AND CO., LIMITED
ST. MARTIN'S STREET, LONDON

1912

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PREFACE

The old naturalists have occupied so much of my leisure of late years that it becomes a pleasant task to write about them. My chief aim is to induce such readers as I may find to make themselves better acquainted with the founders of modern natural history. To succeed in this attempt a rather strict selection of authors is indispensable, and I have been forced to omit many of those workers at details to whom natural history owes so much, in order to give fair space to the pioneers who opened out new fields of inquiry or introduced new methods. I cannot pretend, however, to have been altogether consistent and impartial in my selection. Some old works have been included, not so much because they are important as because they give a lively picture of the state of knowledge in a past age. Insects take up more than their due share of space, partly because they are really prominent in the works of early naturalists, partly because old books about insects give me more than common pleasure. Such preferences are natural, and if not pushed too far, may be advantageous to the reader as well as to the author. No more fatal mistake can be committed by an author who undertakes to handle a wide subject than to fancy that he can attain to completeness unless indeed his work takes the form of an index; and it is almost as unpromising to divide the space impartially among the persons or things to be described; the product, however well-proportioned, is sure to be lifeless.

Some readers will be surprised that I give so wide an extension to the word *early* as to include Buffon and the Jussieus. But the time has already come when hardly any eighteenth-century naturalists, with the exception of a few eminent students of life-histories (Swammerdam, Réaumur, &c.), are searched for biological facts; they are important merely as historical landmarks. Indeed zoology and botany have been so largely recast since 1859 that we shall shortly make Darwin's *Origin of Species* the era of modern biology, and consider all naturalists *early* who precede Darwin.

It would have been a delightful task, had it been possible, to continue the history through the age of evolutionary speculation; to show how Linnæus' rude sketch of the kingdoms of nature has been enlarged; how new studies, of which Linnæus had little conception (comparative anatomy, embryology, geographical distribution and palæontology), have become strong and fertile; how a fairly satisfactory grouping of the genera of flowering plants into families has been devised, how the cryptogams, long despised as casual and unstable, have been proved to rival the flowering plants in practical importance and intellectual interest; and how the history of extinct animals and plants has been illuminated by a theory of continuous descent. I need make no apology for having declined so vast and so difficult an addition.

Some biographers seem to hold that nothing in the career of a man of science signifies very much except his effective contributions to knowledge. His mistakes and failures, however many and grievous, are, they think, no longer a matter of practical concern to anybody. When we examine a building we consider the plan and its execution, but do not care to be told how many bricks were dropped as the work went on. This is the amiable view of official eulogists, and also of some writers who, without being bound to praise, consider nothing but economy of the reader's time. It may appear to others that something besides positive achievement should be recorded. We want to know not merely what was discovered, but how it was discovered. The discoveries, even of great men, have often been vitiated by serious mistakes, which have subsequently been corrected by men of far inferior power. Whether in such cases we give the whole credit to the man who first indicated the process, or to the man who first arrived at a true result, we do some injustice and at the same time misinform our readers, who may fairly claim that in important cases all the essential steps in the discovery should be laid before them. We want to know how some real discoverers began by trying false routes, how others were impeded by time-honoured delusions, or by overbold speculation. These things are part of the story, and cannot be omitted without loss.

The classics of natural history are not very much studied in our own time. Few of them command high prices, except those which treat of birds, or are richly illustrated, or exemplify the history of printing and engraving, and only public libraries take much pains to enlarge their collections. Hence the works of such early masters as Malpighi, Swammerdam, Ray, Leeuwenhoek and Réaumur are still within the purchasing power of ordinary students. I wish that every naturalist might deem some acquaintance with them as part of his equipment.

The time bestowed upon the Early Naturalists by author and reader will have been well spent if it helps them to attain a comprehensive view of biological history, which is indispensable to the appreciation of recent work. History is necessary to the student who practises modern methods and is inspired by modern ideas, for the same reason that embryology is necessary to comparative anatomy; to know what is we must know how it came to be.

I have to thank Dr. B. Daydon Jackson for corrections and elucidations of material value.

L. C. M.

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INTRODUCTION: NATURAL HISTORY DOWN TO THE SIXTEENTH CENTURY

The beginnings of natural history are wholly unknown to us. In a very remote past men made themselves acquainted with some of the properties of plants and with some of the habits of common animals, learned to distinguish a few of the more conspicuous kinds, and gave names to such as seemed to them important or curious. The most interesting to us of these early inquiries were made before the Christian era in Greece; similar investigations were no doubt pursued in Egypt, India and other eastern countries, whose history is less accessible.

The beautiful land of Greece, intersected and indented in many places by the sea, rising into lofty mountains, enjoying a climate propitious to labour, well furnished with small harbours, and having ready access to all Mediterranean ports; more than all the rest, inhabited by a people of singular enterprise, was upwards of two thousand years ago the cradle of the sciences. Neither Asia nor Africa has done so much for the scientific education of the world as the little country of Greece. The heavenly bodies, the seasons, the winds, the life of animals and plants were there observed with eager curiosity. Ploughmen, gardeners, vine-growers, wood-men, shepherds, herdsmen, horse-breeders, dog-fanciers, hunters, fowlers, fishermen and beemasters handed down to their sons the slight improvements which had brought them success. Physicians were highly esteemed, and gave employment to druggists and root collectors, who sought out rare plants, not disdaining to practise superstitious rites, possibly as a means of keeping out competitors.[1] From such informants as these much knowledge concerning plants and animals was collected, and at length recorded in books, most of which are now known only by chance quotations. Herodotus describes the rivers, climates and remarkable animals of the distant countries which he had visited in the course of his travels. Xenophon, who was not only a general, an historian, and a moralist, but an inquisitive naturalist and sportsman as well, shows how much attention had been bestowed upon animals before the age of systematic treatises. He gives lively descriptions of the hares, deer, wild boars and hounds which amused his leisure, and contributes the valuable information that in his day lions and leopards still haunted Thrace, Macedonia or the wild country further to the north.

Some of the Athenian philosophers discoursed upon natural phenomena, and especially upon the phenomena of life, with an acuteness and comprehensiveness which have moved the admiration of all succeeding generations. Aristotle, who dealt with the whole range of science, surprises the modern reader by his knowledge of migration (not only in the easily observed crane, pelican and quail, but in the mackerel and tunny), of the artifice by which the angler-fish captures its prey, of the brood-pouch of the male pipe-fish (in this case the facts were only partly understood), of the laying of eggs by worker-

[1] Theophrastus, *Hist. Plant.*, IX, Ch. 8.

bees (eggs which produce only drones), of the sound-producing mechanism of the cicada and the grasshopper, of the hectocotylus-arm of some Octopod, of sharks attached to the mother by a kind of placenta, of the early stages of the developing chick, and of many more secrets of nature. It is true that much of his knowledge is drawn from other observers, as words like "it is said," continually remind us, and that hardly any of his stories are perfectly right, but what a range of curiosity they indicate! We find too abundance of general remarks on structure, valuable because they go just as far as observation extended and no farther, such generalisations as Bacon called "axiomata media," *e.g.* that horned quadrupeds, with no upper front teeth, ruminate; that birds which are armed with spurs are never armed with lacerating claws; that in poultry the eye is closed chiefly by the lower lid, but in owls by the upper lid; that insects with more than one pair of wings may bear a sting in the tail, but that such a sting is never found in two-winged insects. Aristotle is the real founder of Comparative Anatomy, and perhaps no science ever made so prosperous a start, enriched from its birth with such a multitude, not only of facts but ideas.

No pilot can explore unsurveyed channels without a confidence which sometimes leads to disaster. The Greek philosophers would have been more than men if they had not often tried to explain things which they very imperfectly understood. Aristotle at least well knew the risks which he ran. "This," he says, "seems to be the mode of generation of bees, as ascertained both by reason and observation. All that takes place is not indeed cleared up; even if it were, we must rather rely upon observations than reasoning, and rely upon reasoning only if it agrees with manifest facts (phenomena)."[1] We need not be surprised that, in spite of the warning which he had himself given, and a mental bias towards scepticism rather than towards credulity, he should have taken for granted many beliefs which cannot stand a strict inquiry. Experiment, which modern science regards as a chief test of conjectures and a chief means of gaining new knowledge, was not yet reckoned among the ordinary resources of the natural philosopher.[2]

Theophrastus is not to be compared with Aristotle as a thinker. It belongs to the age in which he lived that he should have shown the same passion for multifarious knowledge and the same lack of acquaintance with the scientific uses of experiment. The two botanical treatises by Theophrastus which have come down to us are founded on a wide knowledge of the plants, not only of Greece, but of Egypt and Persia as well. Some of these plants must have been minutely investigated, for details are noted which were little attended to by the botanists of modern Europe until the time of Malpighi. The natural history of Theophrastus, like that of Aristotle, was far too extensive to be the product of a single life-time, but who his predecessors were, and what learning they transmitted to him, are questions to which no satisfactory answers can be returned.

The botanic garden of Theophrastus is no better authenticated than the royal menageries and the army of collectors, which are said to have provided materials for the zoological studies of Aristotle. It is vouched

[1] *De Generatione*, III, x, 25.

[2] We can only point to two examples of deliberate scientific experiments in Greek authors, those on the reflection and refraction of light, contained in a treatise on Optics often attributed to Ptolemy, and those on the numerical relations of the musical scale, for which Diogenes Laertius gives the credit to Pythagoras.

for by Diogenes Laertius, an uncritical writer, who flourished some five hundred years after the death of Theophrastus, and Diogenes speaks of a garden, not of a botanic garden.[1]

If Greek liberty and civilisation could have endured, Greek philosophy and science would no doubt have overcome many of their early difficulties, among which we must reckon an undue propensity to argument. But a long course of crushing misfortunes arrested their progress. Alexandria now became the great centre of learning and science, and here a Greek and Semitic school of much celebrity laboured to extend the knowledge of geometry, astronomy, optics and geography. Human anatomy also was diligently and profitably studied in Alexandria under Herophilus, Erasistratus and their successors, but after Aristotle and Theophrastus no great progress was made in natural history until science of every kind died out. The most important treatises which have come down to us from the Roman empire are the *Materia Medica* of Dioscorides and the *Natural History* of Pliny.

Dioscorides recorded what was known of the occurrence, form, colour and properties of medicinal plants; he paid great attention to the names of the plants; his classification is utilitarian, being mainly founded upon the useful products which the plants yield. Now and then, however, a succession of plants belonging to the same family (Labiates, Umbellifers, Composites, Borages, or Leguminosæ) shows that real affinities had been perceived and made use of. Close resemblance in leaf and stem did not conceal from him the fundamental unlikeness of the stinging nettle and the dead-nettle, which some botanists of a much later age brought

[1] Meyer, *Gesch. der Botanik*, Vol. I, p. 152.

together again. When botany began to revive, the writings of Dioscorides were considered by French, German and Italian herbalists as one of the most precious legacies of ancient learning.

Pliny's *Natural History* is a vast and uncritical encyclopædia, which probably contains not a single new observation in biology. The book has a value, however, if not the kind of value that we expect. Frequent notices of the practical arts of the ancients supply information which can be found nowhere else, and Pliny abounds in that philosophical eloquence with which in a much later age Buffon was wont to dignify his expositions of natural processes.

After Pliny the decline of European science, art, literature and civilisation was general and rapid. Galen, who died about 200 A.D., is the last of the ancient anatomists, Oppian (contemporary with Galen) the last of the ancient naturalists. The decline in the fine arts may be roughly estimated by comparing the architecture and sculpture of the age of Constantine with those of the times of Augustus or Trajan. The higher Greek literature ends with Lucian (d. about 200 A.D.), the higher Latin literature with Claudian (d. about 410 A.D.); about 600 A.D. the knowledge of Greek ceased in Western Europe.

During the greater part of a thousand years men despaired of progress and of their own powers. It was widely believed, as it has been in less gloomy ages, that man had declined, not only in knowledge and skill, but in strength, stature and longevity. The earth and even the heavens were thought to show signs of decay.[1] But

[1] For ancient opinions on the decay of nature Mayor's *Juvenal* (Vol. II, pp. 374-6) may be consulted. To the modern references given by Mayor and Jonston, *History of the Constancy of Nature*, 12mo. Lond., 1657. Jonston, like Hakewell (quoted by Mayor), takes the cheerful modern view.

this superstition was at length refuted by undeniable facts. About the millenary year (1000 A.D.) faint signs of improvement began to appear; by the year 1200 it is clear to us, though it may not have been clear to men then living, that the winter-solstice was past. It has ever since been the rule in western Europe that every generation should enlarge the knowledge bequeathed by its predecessor.

No doubt the observation of birds, insects and plants never died out among the people, but the scanty literature of the middle ages disdained to learn from the people. Emblems from nature were collected from Latin and Greek authors, used as matter for sermons and commentaries, and carved in wood and stone. The treasury of this sort of learning was Physiologus, who was neither a man nor a book, but a literature in prose and verse, which lasted for a thousand years and was translated into many languages. In the bestiaries, or books of beasts,[1] where Physiologus is the spokesman, the reader is told that the lion sleeps with his eyes open, fears a white cock, and makes a track with his tail, which no beast dares to cross; that the crocodile weeps when it has eaten a man; that the little beast called Grylio is so cold as to put out a fire; that the elephant has but one joint in his legs, and cannot lie down; that the hedgehog sticks ripe grapes upon its prickles, and so carries them home to its children; that Cetus (the whale) spreads sand on its back and goes to sleep, floating at the surface of the sea; that mariners mistake it for an island, land

[1] See Wright's *Popular Treatises on Science written during the Middle Ages* (1841), and Langlois, *Connaissance de la Nature et du Monde au Moyen Age* (1911). The original Physiologus is said to have been written in Greek at Alexandria in the second century, A.D. (Lauchert, *Gesch. des Physiologus*, 1889).

upon it, and begin to get ready a meal, when the whale, awakened by the heat of the fire, plunges and drowns them all; that the eagle can look at the sun when it is at the brightest; that aged eagles fly into the east, dip three times into a certain fountain, and become young again; that the pelican, having slain her own young, tears her body with her beak, when the blood, falling upon the young birds, brings them back to life.

Even in the times when book-learning was well-nigh extinct, some practical knowledge of plants survived. Agriculture and horticulture were attentively pursued wherever the authority of princes or the sanctity of religious houses afforded protection against lawlessness. From the age of Charlemagne, which some historians have regarded as the nadir of learning and literature, there have come down to us the great emperor's edicts for the government of his dominions and estates.[1] One of these (*Capitulare de villis imperialibus*) enumerates the fruit-trees, vegetables, medicinal herbs and flowers which were ordered to be grown in the imperial gardens.

Earle[2] has prepared a list of English names of garden plants, which have come to us from the Latin, not through French or any other modern Romance language, but through intermediate Anglo-Saxon forms. Among the examples are the following:—

Latin.	*Anglo-Saxon.*	*English.*
Cannabis	Hænep	Hemp.
Caulis	Caul	Kale.
Crotalum	Hratele	[Yellow] Rattle.
Febrifugia	Feferfuge	Feverfew.

[1] These are called *capitularia*, because they were arranged under heads (capitula). They are printed in the *Monumenta Germaniæ Historica*, fol. Hanover, 1835 (Legum tom. i.).

[2] *English Plant Names*, sm. 8vo. Oxford, 1880, pp. xlix, l.

Latin.	*Anglo-Saxon.*	*English.*
Ficus	Fic	Fig.
Lactuca	Lactuce	Lettuce.
Linum	Lin[sæd]	Lin[seed].
Napus	Næp	[Tur]nip.
Petroselinum	Petersilie	Parsley.
Radix	Rædic	Radish.

It is evident that these names were introduced by gardeners who understood Latin, and there can be little doubt that the gardeners were the monks, of whose skill in horticulture there are abundant indications in mediæval annals.

Medicine was practised during many generations chiefly by the religious and the Jews; relics and holy water were more esteemed than drugs.[1] When physicians became plentiful, they were nearly always astrologers as well, and during a great part of the middle ages all men of science either called themselves astrologers or were popularly supposed to practise astrology and magic.

To the thirteenth century are generally ascribed the introduction of the mariner's compass, gunpowder, reading glasses, the Arabic numerals and the denary scale. In the fourteenth century trade with the east was extended so far as the Saracen power permitted; central Asia and even the far east were visited by Europeans; universities were multiplied; popular government, ecclesiastical reformation and national sentiment gained strength; the revival of learning and the revival of painting and sculpture proceeded in Italy with unexampled rapidity and force. The fifteenth century is marked by the invention of wood engraving and printing, and by the great geographical discoveries

[1] Meyer, *Geschichte der Botanik*, Vol. III, p. 412.

of the Portuguese in the east and of the Spaniards in the west.

Though intellectual life abounded during all these years, hardly any attention was paid to science. One or two names indeed, such as those of Roger Bacon and Regiomontanus, show that the aptitude for scientific research already existed, though it was liable to be fatally discouraged by the church (which claimed the exclusive right of teaching), the scholastic philosophy and the popular dread of magic. In the remarkable but still imperfectly understood career of Roger Bacon we note the stimulus which he received from Arabian science, his indignant protests against the ignorance and presumption of the scholastics, the interdiction of his lectures at Oxford by one general of the Franciscans, and his imprisonment for many years by another. Brunetto Latino remarked, when he saw Bacon's magnetised needle pointing to the pole, that no navigator would dare to use it for fear of being called a magician. Science was rarely tolerated in the thirteenth, fourteenth and fifteenth centuries, except when it took its least exciting forms, or was patronised by some great churchman.

We form some notion of the state of natural history during the later middle ages by examining the treatise *De proprietatibus rerum*, written by Bartholomew of England, a mendicant friar, before the middle of the thirteenth century, and translated into English in 1397. We are not surprised to find that Bartholomew had but an indistinct notion of Egypt, India and the "mountains hyperborean," of dragons, griffins and sirens, but we are amused to see how little pains he took to observe and interpret the commonest natural facts. The succession of colours in the rainbow is given as red, blue, green;[1] an oar dipped into water seems to be broken because of the swift moving of the water; bees are said to load themselves with small stones that they may be more steadfast against blasts of wind (this is taken from Aristotle); the crab, we are told, waits till the oyster gapes, and then puts a stone between the shells, so that he may gnaw the oyster's flesh; the fox halts, because his right legs are shorter than the left.[2] These things were written about the time when the most beautiful parts of the great churches of York, Lincoln and Salisbury were being reared, and when Merton College was being founded at Oxford. They were not mere popular fables, but the deliberate statements of a man learned according to the highest standard of the thirteenth century, who had taught in the great university of Paris. Nor were they rejected by the readers for whom they were written, but copied, translated time after time, re-edited, abridged, and at length multiplied by printing.

[1] This succession had a mystical meaning; red was a symbol of fire, blue of water, green of earth. For more than five hundred years men went on repeating an error which might have been corrected at once by observation of an actual rainbow.

[2] Some mediæval writers say this of the badger.

SECTION I. THE NEW BIOLOGY

THE REVIVAL OF BOTANY

THE emancipation of the biological sciences from traditional learning was long hindered by the pretensions of an obsolete medicine. It is not difficult to understand that the extraordinary complexity of most questions relating to health and disease should have made medicine slow to adopt scientific methods, or that the medical profession should have been sensitive about its reputation and prone to assert its infallibility. In the sixteenth century botany was regarded as a main branch of medicine, and may be said to have constituted nearly the whole of therapeutics. Euricius Cordus, who was in all things a reformer, laboured to convince his hearers and readers that there were three things which the physician was bound to know: the human body, the disease and the remedy, but in practice knowledge of a reputed remedy was held to be the main thing. It was generally believed that for every ill that flesh is heir to, nature had designated some plant as the appropriate cure. Some believed that Providence had caused particular plants to grow in those districts where the diseases which they cured were prevalent.[1] When

[1] *E.g.* Theodore of Berg-zabern, who is known to science by his Latinised name of Tabernæmontanus.

the treatment of diseases by the plants of the country was held to be so efficacious, it is no wonder that botany should have been in high esteem. Every physician professed to be a botanist, and every botanist was supposed to be qualified for medical practice. For example, all the botanists who are named in this chapter practised medicine, except Clusius, whose modern biographer, Morren, notes it as a singular circumstance that he was *not* a physician. The alliance of botany with medicine provided a livelihood to many a student of plants, brought hearers to his lectures, and helped to sell his books, but it forced him to make pharmacy his main theme. This would have been retarding under any circumstances, all the more when the pharmacy was wholly unscientific, relying simply upon the dicta of ancient and ill-understood authors.[1]

Before discussing the writings of the botanical reformers it will be useful to glance at those which they sought to replace. About the year 1500 the treatises which most nearly answered to the herbals of a later time professed to indicate remedies for all known diseases, and to trace drugs to their sources. Well-known animals and minerals were added, sometimes on very slight grounds, to the plants which yielded the bulk of the remedies, so that the handbook of medicine became an encyclopædia of natural history. The most widely circulated of these books was the *Ortus* (*Hortus*) *Sanitatis*, called in German the *Gart der Gesundheit*, which had been written in Germany before the invention of printing. The original was in Latin, and addressed to

[1] Until our own times the dissecting-room and the lectures of the medical school furnished the only regular training for the naturalist, while he found in the medical profession the likeliest means of earning his bread. Baer and many other nineteenth century naturalists were thus compelled to study medicine.

the professional classes. After printing became common the *Ortus* was reproduced many times, often in popular forms, while it was translated or otherwise adapted to the use of different nations. Though "Dyascorides" is often quoted, as if from the original, the text is really a compilation from mediæval writers, who were themselves compilers. A table of diseases shows where the remedies are described, and takes the place of an index. Plants furnish the bulk of the illustrations; many are drawn from native species, though not one in three could be recognised by the figure alone. Among the animals are fabulous creatures, such as the basilisk. The figure of a snail resembles a horned quadruped peeping out of a bottle. In the baser editions the pictures are such as a child might draw, and sometimes lose all resemblance to the object. A figure of Greek asphalt, for instance, which was unnatural to begin with, after passing through the hands of several copyists, becomes a mere chance collection of strokes and blotches. Failing natural objects, anything might be inserted which caught the fancy of the draughtsman, a monkey perched on a fountain, casks in a cellar, etc.[1] These books indicate a zero of merit, above which rise, not only all the herbals produced after 1530, but all books which contain observations made direct from nature.

Germany soon took the lead in the revival of botany. It was not only Germans who felt the need of improved knowledge. In France Ruel, a physician of Soissons, spent many years upon the elucidation of Dioscorides, and the examination of those native plants which might throw light upon his author. In Italy too scientific

[1] Free choice of subjects, irrespective of the text and of practical utility, was the long-standing tradition of the illuminators of inital letters.

research began to revive. Young naturalists, not only from Italy itself, but from Germany, and now and then one from England, came to Luke Ghini or to some other Italian master to be trained in botany and pharmacy. Pisa, Padua and Bologna had each its botanic garden, and an academy of natural science was founded in Naples. In Italy the new scientific movement was soon quenched by the Church and the princes, but the torch of learning had been handed to Germany, and here it was not allowed to go out.[1]

Along the Rhine, from Switzerland to the Netherlands, civilisation and industry had long flourished together. On the left bank of the river opulent towns had been built even in Roman times. Centuries after the fall of the empire, great trade-routes, connecting Flanders and the Baltic with Lyons on the one hand and Venice on the other, gave the merchants and manufacturers of the Rhine access to the great markets of the world. Here and in the country further to the east had sprung up that powerful union of seventy cities known in the thirteenth century as the Confederation of the Rhine; here too in a later age were found influential members of the Hanseatic League. Printing and wood-engraving established themselves in the fifteenth century at Mayence, Strasburg and Cologne. When the Reformation began to stir, the Rhineland, above the point where the river entered the "priests' lane," and became enclosed by the archbishoprics of Mayence, Trèves and Cologne, contained many sympathisers with Luther, who spread also eastwards into Hesse and westwards into the Palatinate.

[1] Eurich Cordus and his son Valerius are among the Germans who visited Italy in the early or middle part of the 16th century for botanical study. The books of Italian writers on botany and pharmacy were often studied in Germany about the same time.

With the reform of religion some men combined aspirations after an improved social state; others were eager to infuse new life into literature, or to free medicine from the bonds which had long impeded it. Not a few believed, at least in the early stages of the movement, that they were simply labouring to remove the corruptions of later ages, and to restore the purity of ancient times. Those who occupied themselves with the reform of medicine made it a duty to go back to Dioscorides, and to clear away the misapprehensions which obscured his teaching. It was easy to show that the apothecaries had on the slightest possible grounds treated common German plants as identical with certain plants of southern Europe, which ancient pharmacists had celebrated as the source of valuable drugs. While these discussions went on the close study of native species began to spread, and field-botanists multiplied, especially it would seem, in and around Strasburg. Few of them published their observations, but their experience was not altogether lost; among the first to take advantage of the local facilities for printing were Brunfels and Eurich Cordus. The reform of pharmacy and botany long retained its Protestant character, and till the close of the sixteenth century almost every author of a botanical treatise published in Germany or Flanders was a Protestant.

OTTO BRUNFELS[1]

1484–1534

Herbarum vivæ eicones . . . per Oth. Brunf. 3 pts. Fol. Strasburg, 1530-6.
Contrafayt[2] Kreuterbuch . . . durch Otto Brunfels newlich beschrieben. 2 vols. Fol. Strasburg, 1532-7.

Brunfels was born at Mayence, and received an university education, being destined by his parents for the church. At the age of thirty-seven he found himself in a Carthusian monastery, whose restraints now proved intolerable, for he was strongly impelled to join the new humanist and reforming movement. In 1521, the year of Luther's appearance at the diet of Worms, Brunfels fled from the cloister. He soon found employment as a Lutheran pastor, or as a schoolmaster, but settled at Strasburg in 1524, and henceforth employed his learning, which seems to have been considerable, in writing for the booksellers. Pedagogy, theology, medicine and botany by turns engaged his attention. It is not easy to understand how he acquired such a knowledge of medicine as procured for him not only the doctorate in medicine of the university of Basle, but considerable repute as a physician. An enterprising bookseller of Strasburg, named Schott, engaged him to write a new herbal, which was to take advantage of the new learning, and also of the remarkable improvements in wood-engraving, which had been effected of late years. The first volume appeared only six years after Brunfels' settlement in Strasburg, so that his

[1] Fuller biographical information will be found in a paper by F. W. E. Roth in the *Botanische Zeitung*, 1899, pp. 191-232. There is an interesting discussion of Brunfels' botanical work in E. L. Greene's *Landmarks of Botanical History*, *Smithsonian Collections*, pt. 1, 1909, pp. 165-191.

[2] *Contrafayt* means portrayed or pictured, as in the inscription, "Albrecht Durer's Conterfeyt" (1527).

preliminary botanical studies must have been slight, all the more because botany was only one of his occupations during this busy time. He got help from several botanists, who are known to have worked at the plants found in the country round Strasburg; his own share in the work perhaps consisted largely in incorporating with the information supplied by field-naturalists passages from the *Materia Medica* of Dioscorides, which had been lately translated into Latin by Ruel and others.

Brunfels' herbal, in Latin and German, is illustrated by near three hundred figures of plants, drawn by Hans Weydiz, a celebrated artist of the time, or by his assistants. The plants are shown in clear outline, and are sometimes so faithfully copied that it is still possible to pick out those which were set before the draughtsman in a defective condition. Other figures are less adequate; though the species is often determinable, it is not always possible to make out even the genus. Sometimes the figures and the descriptions do not correspond; thus descriptions of Aristolochia extracted from Dioscorides and Pliny are illustrated by figures of two species of Corydalis.[1]

The modern reader will shortly describe Brunfels' arrangement of plants as haphazard, and such it often is. If we come across the trace of a natural grouping, we shall probably find that it is taken from Dioscorides. Like Dioscorides and the pharmacists who succeeded him, Brunfels is inclined to put together plants which are supposed to share the same properties. But we can only account for some of his sequences by supposing that he inserted the species just as they came in.

Brunfels' *Eicones* went through several editions, and

[1] Greene, loc. cit., p. 173.

its success prompted the issue of a German version, which, being ill-fitted for popular use, was never completed. Bock's herbal was the first to meet the needs of the unlearned.

In 1533, when the new herbals were far from complete, Brunfels was offered the post of town-physician at Berne. He accepted the invitation, and entered upon his new duties, but by this time his course was nearly run; he was soon afterwards struck by mortal illness, and died at the age of forty-six.

Brunfels won respect, and made many friends. It is pleasant to find that Bock and Fuchs, both of whom were engaged upon herbals of their own, warmly praised his botanical services, as also did Conrad Gesner. In spite of testimonies like these, we are sometimes disposed to put the question why this old herbal need be studied again. The answer is that by figuring from nature a large number of native plants Brunfels initiated modern systematic botany. Fuchs, Bock, Gesner, L'Obel and many more carried on the work which Brunfels had begun. It was soon discovered that pictures would not suffice without methodical descriptions, that philosophical arrangement renders comparison easier and more profitable, and that philosophical arrangement can be attained neither by logic, nor by ingenious contrivance, nor by consideration of the wants and wishes of mankind, but only by patient study of the groups which actually exist in nature. Study of such groups revealed the existence of *affinity*, a property which, after remaining mysterious during many generations of men, became at last intelligible. Brunfels, without suspecting it, had set his foot on a new land.

HIERONYMUS BOCK[1]

1498–1554

New. Kreutter Buch von underscheydt, würckung und namen der Kreutter, so in teutschen Landen wachsen, &c. Fol. Strasb. 1539. Parts 1 and 2 only.

Kreuter Buch, &c. Second edition, with figures. Fol. Strasb. 1546. Part 3, also with figures, was published at the same time.

Hieronymi Tragi de stirpium, maxime earum quæ in Germania nostra nascuntur . . . libri tres . . . interprete Davide Kybero. 4to. Strasb. 1552.

Bock, a native of Baden, was destined by his parents for the cloister, but when he grew to manhood, he came, like Brunfels, under the influence of the new doctrines, began to study medicine and botany in addition to theology and philosophy, and at length took the decisive steps of removing to Zweibrücken in the Palatinate, setting up as a schoolmaster, and marrying. It was no doubt an important promotion for him when he was called upon to attend the duke of Zweibrücken as physician, and to supervise his botanic garden. Some years later he was rewarded by a sinecure canonry at Hornbach, a few miles from Zweibrücken. Protestantism was then spreading in all parts of the Rhineland, and in all ranks of society. The dukes whom Bock served, and even the abbot of Hornbach, favoured the Reformation, so that Bock, a married man, who had moreover undertaken the functions of a Lutheran pastor, without apparently any ecclesiastical sanction, was allowed for many years to share the emoluments of an ancient monastic foundation. In spite of his varied employments, for he is believed to have practised both divinity and medicine, he found time for oft-repeated botanical excursions, which he

[1] The latest and best account of the life of Bock is that of F. W. E. Roth (*Botan. Centralbl.*, 1898, pp. 265-271; 313-8; 344-7). For information concerning the botanical merits of the *Kräuterbuch* E. L. Greene's *Landmarks*, pt. 1, pp. 220-64, may also be consulted.

generally made in peasant's dress, so as to excite little notice. The excursions gradually took a wide range; many places between the Rhine and the Moselle were visited, besides countries as distant as Switzerland and Tyrol. Bock, who seems to have been a sociable, friendly man, became known in some of the Rhenish cities, especially to Brunfels and other botanists of Strasburg; he corresponded also with Gesner of Zurich. A young man, named Jacob Theodor of Berg-zabern, who was afterwards known throughout Europe as Tabernæmontanus, was first a pupil and afterwards an assistant of Bock's during this Hornbach time. Bock himself tells how his honoured friend Brunfels came out on foot from Strasburg to Hornbach (some sixty English miles) and pressed him to write in the mother-tongue a new herbal for the instruction of the German people.

Bock spent some fifteen years amidst these occupations, disturbed only by symptoms of waning health, but about the year 1548 he was called upon to face changes disastrous to his happiness. By this time the herbal in its German form was complete. The prospects of the Reformation in Germany had meanwhile become clouded; a new duke of Zweibrücken and a new abbot of Hornbach withdrew their support from the Lutheran pastor, who was obliged to remove for a time to Saarbrücken, where the count, whom Bock had treated successfully in grave illness, offered hospitality and countenance. All his ten children except two died before him, and the only surviving son was deprived of the Hornbach canonry, which the father had resigned in his favour.[1] Amidst calamities like these a wasting

[1] Bock returned to Hornbach not long before his death, and was buried there; whether he was reinstated as pastor is not known.

disease, from which Bock had long suffered, carried him off at the age of fifty-six.

The new *Kräuterbuch* (parts 1 and 2) appeared about six years after Brunfels' visit to Hornbach. It was written in German, and at first contained no figures. The inclusion of many more plants, the fuller and more lively descriptions and the homely style gave it a marked advantage over the German translation of Brunfels' *Eicones*, which was soon discontinued. The second edition of the *Kräuterbuch*, besides a new third part, were made more attractive by the introduction of figures, drawn by David Kandel, a young self-taught artist of Strasburg, who worked under Bock's eye at Hornbach. The figures are smaller and coarser than those of Brunfels; many are copied from the herbal of Fuchs, which appeared in the interval between Bock's two editions. In front of the illustrated editions of the herbal we find a portrait of Bock at the age of forty-six, drawn by David Kandel. An arch of florid design occupies so large a part of the page that scanty room is left for anything else. The naturalist is shown in half-length side-view, holding a flowering bulb in his hand. The straight hair is combed down to the neck behind, and over the top of the forehead in front; both chin and cheeks are shaven. The features are good, the well-shaped nose prominent, the eyes a little up-turned, the expression grave but pleasing. In the coloured copies all that is attractive disappears.

Hieronymi Tragi de stirpium libri tres is a translation of the herbal into Latin by David Kyber of Strasburg, the figures of plants being retained. The Latin translation was never reprinted, but seven editions in German appeared after Bock's death; the last bears the date of 1630.

After nearly four hundred years we still read with pleasure Bock's accounts of the pistillate flowers of the hazel, the deciduous calyx of the poppy, the pistil of the bilberry, the rooting stems of water-lilies, the hooks on the twining stem of the hop, and the shooting-out of the seeds of the wood-sorrel. He notes more distinctly than any other botanist of the time the difference between stamens and styles, but has no true notion of their physiological office, not even recognising that one or both may be found in every flower. Particulars of place and environment are added, and the descriptions are enlivened by curious details, which give them in many places a vivacity to which the text of Brunfels or Fuchs makes no approach.

Bock's grouping of plants is largely traditional. He accepts the ancient division into trees, shrubs and herbs. Since he gives no synoptic tables, far less family-names with definitions, it is a matter of conjecture what groups of genera, if any, he regarded as marked out in nature. He inherited from Theophrastus and Dioscorides a few natural groups :—Umbellifers, Thistles, Chicories, Legumina, Labiates, Solanaceous plants, Crucifers, Mallows, Catkin-bearing and Cone-bearing trees, none of them precisely limited, and to this list he added the (unnamed) Borages. He places rosemary and lavender among the Labiates, notwithstanding their woody stem. The nettle and the dead-nettle are described in close succession, though the distinctive generic names of Dioscorides are quoted. We find groups founded on habitat (*e.g.* water-plants), or on usefulness to man (*e.g.* kitchen-herbs), or on habit (*e.g.* Serpentariæ or climbers, a group of Bock's own proposing). The special value of floral characters was then unsuspected.

In his preface Bock shows the importance of associating related or similar plants, and pours contempt on the alphabetical arrangement. We must not however read the modern meaning into his word *affinity*. There is no reason to suppose that the notion of a common descent for the pea and the bean, or for rosemary and lavender, had ever crossed his mind.[1]

LEONHARD FUCHS

1501–1566

De Historia Stirpium Commentarii insignes, maximis impensis et vigiliis elaborati, adjectis earundem vivis plusquam quingentis imaginibus nunquam antea ad naturæ imitationem artificiosius effictis et expressis, Leonarto Fuchsio medico hac nostra ætate longe clarissimo autore, &c. Fol. Basil. 1542.

Fuchs was born at Membdingen in Bavaria in 1501. His original calling was that of schoolmaster, and his favourite study ancient literature. At the university of Ingolstadt, a stronghold of Catholicism, he made acquaintance with the writings of Luther, and was thereby led to adopt Protestantism. Having graduated in arts, he studied medicine and took his doctor's degree in that branch, becoming after a short interval professor of medicine. He was next made physician to the marquis of Brandenburg at Anspach, and became favourably

[1] "Und hab in gedachten büchern gemeinlich disen Process und Ordnung gehalten, nemlich das ich alle Gewächs so einander verwandt und zugethon oder sonst einander etwas änlich seind und verglichen zusammen doch underschiedlich sind. Und den vorigen alten Brauch oder Ordnung mit dem ABC wie das inn den alten Kreutter buchern zu erschen hindan gestelt. Dann die Gewächs nach dem ABC in Schrifften zuhandeln gar ein grosse ungleichheit und irrung geberen, &c." (Bock's Preface.)

The word *affinity* or some synonym appears in Aristotle (*De Partibus*, IV, 6, 3), and in the natural history books of the sixteenth and seventeenth centuries A.D., such as those of Fuchs, Dodoens, Gesner, Cesalpini, Caspar Bauhin, Grew and Ray. Nowhere is a clear distinction drawn between affinity and general similarity of structure or habit.

known by his successful treatment of an epidemic. In 1533 he was invited to resume his professorship at Ingolstadt, but was soon driven away by Jesuit intrigues, and returned to Anspach. On the death of his patron, the margrave, he accepted a call to the university of Tübingen, which had just adopted the Reformed faith, and here he remained from 1535 till his death (1566). Among his published works are treatises on medicine and human anatomy.

Fuchs' first contribution to botanical literature consisted of critical remarks on medicinal plants written for Brunfels' *Kräuterbuch.* He then aspired to produce a herbal of his own, and in 1542 issued his *Historia Stirpium*, which was immediately translated into German.

The text, like that of Brunfels, is drawn chiefly from ancient authors; the descriptions are briefer, and show a much slighter acquaintance with the original texts. The arrangement is alphabetical according to the Greek names of genera. Fuchs says in his preface that he would have liked to associate "congenerous herbs," as Dioscorides had done, had such a sequence been permitted by the pictures; this excuse is unconvincing. Plants are often associated on the ground of a quite superficial resemblance; Viola includes the violet, Hesperis and the snowdrop; Stellaria Holostea and Parnassia come under the Grasses.[1] Fuchs shows

[1] This remark holds good for early botanists in general. Names like *rose* and *violet* had no definite botanical meaning; the Christmas Rose, the China Rose and the Rock Rose have no affinity with the rose of the hedge, nor with one another; the Dame's Violet and the Dog's-tooth Violet no affinity with the sweet violet and the pansy. In the same way primitive medicine gave the name of Hepatica to an anemone, and also to the cryptogamous Marchantia, of Verbena to Verbena officinalis and also to the groundsel; and of Consolida (healing) to a number of quite different herbs, which agreed only in having a reputation for closing wounds (Greene, *Landmarks*, pp. 176, 231).

little interest in living nature, or in adaptation to environment, takes notice of few rare plants, and does not restrict himself to native species; he thought chiefly of meeting the wants of the pharmacist.

The five hundred woodcuts of the *Historia Stirpium* probably surpass in artistic quality any long series of botanical figures that has ever been published, though they are not remarkable for minute accuracy. Each plant fills a folio page, on which no letterpress beyond the name is allowed to encroach. The outlines are clear, and there is little or no shading.[1] Sometimes but not often the flower and fruit are shown on detached branches; the structure of the acorn is displayed in separate figures; on the other hand the flowers of the nettle are indicated by mere dots. A whole tree from the roots to the top branches may be shown in one view; then the leaves are out of all proportion to the trunk. In the drawing of an entire walnut-tree there are only about a score of leaves, each perhaps one-fifth of the total height; it would of course have been better to show only a single branch, as is done in the case of the savin. Greek vase-painters could draw unmistakable olive-trees, with only one or two leaves apiece, but natural history cannot allow such liberties. Fuchs' own portrait occupies the frontispiece, while his draughtsmen (Heinricus Füllmaurer and Albertus Meyer) together with his engraver (Vitus Rodolphus Specklin or Speckle) share a page at the end of the book.

A glossary of difficult terms is prefixed to the Latin *Historia Stirpium*, but omitted in the German trans-

[1] The practice of drawing plants in outline probably originated in the colouring of the figures. Early woodcuts are often coloured by means of stencils, but this is never the case with Fuchs' figures.

lation. The anthers are called by Pliny's name of "apices," but not clearly distinguished from the styles. The "glume" is defined as the sheath which encloses each grain in a grass-spike. The "stipule" is the sheathing leaf of a grass. A bulb is defined as a rounded tunicated root, which is retrograde; Theophrastus knew better than this. When he comes to explain the botanical umbel, Fuchs, like a true scholar, goes a little out of his way to give the history of the word, quoting the Greek *skiadeion* and the Latin *umbella*, "qua mulieres vultum vindicant a sole et æstum arcent." Cesalpini mentions parasols as being used on journeys, and they are figured in Anglo-Saxon MSS.

Fuchs' letters show that he laboured during many years to extend his *Historia Stirpium*. In 1565 he was ready to publish three parts of what he characteristically describes as an excellent, noble work, containing in each part more than five hundred beautiful and carefully drawn figures, together with the histories of the plants. He sought for a wealthy patron to meet the cost, and got the promise of one contribution. But in the following year Fuchs died, and the work was never produced. The manuscript is believed to have been extant many years later, and the engraved blocks were long used to illustrate the works of other botanical authors.

It is unpleasant to have to say of an author who rendered real service to botany that his character lacked modesty. Fuchs was in the habit of blowing his own trumpet, and sometimes he blew it loud, as in the title of his great work. He showed no jealousy of other botanists, and often praised what they had done.

Father Plumier gave the name of Fuchsia to one of the most beautiful of the garden-flowers which we have received from America.

VALERIUS CORDUS

1515–1544

The brief and tragic history of Valerius Cordus (son of the Euricius Cordus already mentioned) can only be glanced at here, because few naturalists can acquaint themselves at first hand with the surviving fragments of his work, which were piously collected by Gesner. Dying at twenty-nine, he had already made his mark in science. He is remembered as the discoverer, or one of the discoverers, of sulphuric ether, as the first to say in print that young ferns spring from the light dust borne on the back of the leaves, as one of the first to trace the origin of coal to long-buried vegetation. The term *pollen*, which had been used by Pliny as the name of meal or any other kind of fine dust, Cordus applied to the dust emitted by anthers. He has a special name (*papilionaceous*) for the flower of Leguminosæ (Gesner had already compared pea-blossom to a butterfly).[1]

CONRAD GESNER

1516–1565

C. Gesneri Opera Botanica... Omnia ex Bibliotheca D[om.] C. J. Trew nunc primum in lucem edidit et præfatus D[om.] C. C. Schmiedel. 2 pt. Fol. Norimbergæ. 1751-71.

Gesner studied at Strasburg, Paris, Basle and Montpellier (under Rondelet), and became skilled in the

[1] Greene (*Landmarks of Botanical History*) has given a detailed and appreciative notice of the botanical work of Valerius Cordus.

ancient languages as well as in medicine and natural history. Like all the German and Swiss botanists of his generation, he was a stout Protestant. His own father fell in battle, fighting with Zwingli to defend Zurich against the Catholics of the forest cantons. Conrad Gesner too perished in the service of Zurich. In 1564 the city was ravaged by a plague, which Gesner, who was the public physician, combated successfully, though to the injury of his health. Next year the plague reappeared, and Gesner as before stuck manfully to his post. This time he did not escape, but was carried off before he had quite reached the age of fifty.

Gesner was the most learned naturalist of the sixteenth century, but he was much more than a naturalist. He had been professor of Greek at Lausanne, and good judges have reckoned him among the best Greek scholars of his age. His *Bibliotheca Universalis*, a bibliographical account of all writers in Latin, Greek and Hebrew, his *Pandectæ Universales*, a methodical index to all the knowledge recorded in books, and his *Mithridates*, an attempt to arrange all the languages of the world according to their affinities, are works of vast extent and labour. He only lived to produce one comprehensive biological work, his *History of Animals*, and that was not quite complete. For a great *History of Plants*, which he was much better qualified to write, he had made preliminary studies of high promise.

Among the many proofs of his multifarious knowledge we may cite his little book on fossils,[1] where he discourses upon all things which are dug out of the earth, and figures not only basaltic columns, encrinites,

[1] De rerum fossilium, lapidum et gemmarum figuris et similitudinibus Liber. 8vo. Tiguri. 1565.

belemnites, &c., but stone-implements and even a lead-pencil! He says that this last is made of what some called English antimony, set in a wooden handle. The figure resembles a modern pencil-case for the waistcoat pocket.

Gesner, laborious, learned, enlightened, unselfish, ever zealous to extend the knowledge of nature, had, like Linnæus two hundred years later, correspondents in every country, and suggested or helped many an inquiry. If somebody was wanted to take up a neglected branch of natural history, or to edit the writings of a naturalist who had been cut off before his time, Gesner, loaded as he was by tasks of his own, was the readiest to lend a helping hand. Belon, Rondelet, Aldrovandi, Valerius Cordus, Caius and Turner are to be found in the long list of those whom he befriended or advised. Even the Congregation of the Index had recourse to his *Bibliotheca* for information concerning heretical authors, though they ungratefully put him into the list along with the rest.

Letters of Gesner give some faint notion of what his *History of Plants* might have done for botany. In one place he explains that flower, fruit and seed afford better indications of affinity than leaves. It can easily be perceived, he says, by the organs of fructification that Staphisagria and Consolida are of kin to Aconite, &c. He asks a friend to send him a drawing of a tulip-fruit (the tulip was then a rarity in western Europe) to show the arrangement of the seeds, which he wished to figure. He recognises genera, or natural groups of species, as many had done before him, and says that there are hardly any herbs which do not fall into genera of two or more species. The ancients had described one gentian, but he knew of ten or more. He distinguished varieties from species, and demanded proof of constancy in the characters before he would allow that they were of specific value.

We are told that Gesner had brought together no fewer than fifteen hundred figures, many of them drawn by his own skilful hand, while nearly four hundred had been engraved on wood. The drawings and wood-blocks were handed down after his death from one botanist or publisher to another. Some were used to illustrate Mattioli's *Epitome* (1586). At last the collection, sadly diminished, was bought by Christopher Jacob Trew, an eminent physician of Nuremburg, who valued good books of natural history. Trew entrusted the thousand figures which came into his hands to the careful editorship of C. C. Schmiedel. Many of Gesner's drawings were now engraved on copper and coloured after the originals; some of the woodcuts were printed off, while others, which had suffered injury, were re-engraved on copper. Two great folios, which include the botanical works published in Gesner's life-time, were thus produced, which give the best notion now to be had of Gesner's industry and skill as a botanist. In these interesting and often beautiful figures we find details of flowers and fruits never so well presented before. It is a question whether he used lenses or not; sharp sight may perhaps have sufficed. Gesner was so short-sighted as to require concave spectacles for the perception of distant objects. Like another short-sighted naturalist (K. E. von Baer), who was remarkable for his power of distinguishing the minute details of living things, Gesner may have turned the imperfections of his eyes to good account.

The pleasing usage of naming the genera of plants after meritorious botanists was introduced by Gesner.

It was extensively adopted in a later generation by Father Plumier (1646-1704).

Gesner's *History of Animals* is noticed elsewhere. His publications, though copious and learned, only partially explain the reverence with which after ten generations naturalists and scholars still regard the name of Conrad Gesner.

MATTHIAS DE L'OBEL[1]

1538–1616

Plantarum seu stirpium Historia . . . cui annexum est Adversariorum volumen. Fol. Antw. 1576.

From the age of sixteen L'Obel was a diligent observer of plants. He betook himself at the age of twenty-seven to Montpellier, in order to study under Rondelet, then at the height of his fame. Here he paid close attention to the plants of Languedoc and the Cevennes, which afterwards yielded him much material for description. Rondelet died in 1566, and his manuscripts were left to L'Obel as his favourite pupil. He did not return home at once, for the terrible Alva was governor of the Low Countries from 1567 to 1573, and many of the unfortunate Flemings were glad to take refuge in England. L'Obel was one of these, and his first botanical work[2] was produced in London. We next find L'Obel in Antwerp, where he practised medicine. His repute

[1] Biographies of L'Obel by Edward Morren are to be found in *Bull. Fédér. Soc. d'Horticulture de Belgique*, 1875, and in *Biog. Nat. de Belgique.*

[2] *Stirpium Adversaris nova . . . autoribus Petro Pena et Matthia Lobelio.* Fol. Lond. 1570. Pena had been a fellow-student of L'Obel at Montpellier and a diligent collector of the plants of Languedoc. Legré (*La botanique en Provence au XVI^e^ siècle*, 1899) has shown that the *Adversaria* was largely the work of Pena. L'Obel and Pena left Montpellier for England together; Pena remained there for several years, and afterwards became very successful as a physician in France.

became so considerable that he was made physician to William the Silent. Not long after the assassination of the prince L'Obel was appointed superintendent of the physic-garden set up at Hackney by Lord Edward Zouche. He now busied himself with English botany, and was the first to note several species native to Middlesex. Among the English botanists whose acquaintance he made was Gerard, whom he esteemed very lightly. One of L'Obel's daughters was married to a London citizen (James Coel, of Highgate), and this connexion may have helped to detain him in England; he died (no doubt in his daughter's house) at Highgate in 1616.

L'Obel can hardly have been an amiable man; he was inclined to boast, and often wrote contemptuously of his predecessors or contemporaries. But he was laborious and sagacious, and botany owes a good deal to him. The Lobelia, named after him by Plumier in 1702, helps to keep his memory fresh.

His botanical works (*Adversaria*, *Observationes*, *Kruydboeck*, *Icones*, &c.) were much esteemed in their day, and went through several editions. The modern reader finds the Latin style dry and clumsy, and the definitions few and obscure, while there is far too much of an obsolete pharmacy. The woodcuts engraved expressly for these works are small and of no great merit. Larger and better ones are often borrowed from the books of Dodoens or Clusius, with both of whom L'Obel lived for some years on intimate terms; Christopher Plantin, who published for all three, was no doubt glad to repeat in a succession of books the blocks which he had paid for.

We shall now notice some features of these volumes which are of biological interest.

L'Obel makes a distinct advance upon the systems of earlier botanists. Not content with tacitly adopting what he took to be a natural sequence, like the early German botanists, he enumerates in synoptic tables the species of one genus, or the genera of one family. His primary division is the ancient one into trees and herbs; then the herbs are divided according to the form of the leaves. Division into Monocotyledons and Dicotyledons is foreshadowed by the separation of plants with narrow, simple, parallel-veined leaves from those with broad, reticulate-veined and incised leaves. His system is really based on leaf-form, and he unites clover, oxalis and hepatica merely because all have trifoliate leaves. He sought to proceed from the simple to the complex, and for this reason among others began with the grasses, which he took to be flowering plants of peculiarly simple structure. From the grasses he went on to irids, lilies, &c., being guided, as he says, chiefly by the pointed and simple leaves. Alisma, Sagittaria, some Orchids, &c. are widely separated on account of their broad leaves. The cereal grasses lead in another direction to the Crucifers; here the point of resemblance is suitability for human food. Cabbages are associated with lettuces, which are of like habit and "fruitio," while both are used in cookery.

Though the shrubs and trees are recognised as distinct groups, the shrubby Leguminosæ are laudably, but inconsistently, put next to the herbaceous genera. The ferns, even such unusual forms as moonwort and adder's tongue, are kept together.

L'Obel's works show an extensive acquaintance with the rare plants of Europe, such as Pyrola, which he had found at Berchem near Antwerp, Cypripedium Calceolus from Switzerland and Tyrol, and many more from

Languedoc or the Cevennes. Sprengel[1] gives a long list of species which L'Obel was the first to describe. L'Obel has also something to say about the Papyrus antiquorum, which he had seen in the botanic garden of Pisa, about Sarracenia, Tillandsia and other newly imported American plants, about the wheat-trade of Antwerp, the manufacture of beer and the trenching of celery. Drugs of recent introduction are of course noted, and many therapeutical experiments are recorded. He tells with pride of plants brought at great cost to Flanders from Constantinople, Greece, Italy, Asia, Africa and America, and cites among the glories of his native land, the eminent botanists and gardeners which it had produced. The highest place is given to De L'Escluse (Clusius).

L'Obel seems to have been the first naturalist to call attention to the fact that the mountain plants of warm countries descend to low levels further north. His words are :—"quæ jugis montium calidarum regionum proveniunt, eadem in planis, silvis, silvosis et depressis regionum septentrionalium exeunt."[2] This observation of L'Obel's was the starting-point of inquiries which have been pursued with ever-widening grasp to our own time. Linnæus[3] showed that alpine plants are nearly the same all the world over, while Ramond[4] observed that the zones of vegetation on high mountains may answer to horizontal zones bounded by parallels of latitude, a relation which Humboldt demonstrated on a far larger scale.

[1] *Gesch. der Botanik*, Vol. I, p. 311.

[2] Preface to *Stirpium Illustrationes*.

[3] *Phil. Bot.*, § 334.

[4] Ramond, a naturalist of no real weight, had the honour of influencing the geological speculations of Cuvier, and is once mentioned in Darwin's *Origin of Species*. His *Voyages au Mont-Perdu* (1801) has some little historical interest.

ANDREA CESALPINI (OR CESALPINO)

1519–1603

De Plantis Libri XVI. 4to. Florent. 1583.

Little is known of the personal history of Cesalpini. He studied at Pisa (where he was introduced to botany by Luke Ghini, a teacher of great reputation) succeeded Aldrovandi as director of the botanic garden at Bologna, and professed medicine and botany in the university of Pisa, where again he had charge of a botanic garden. In old age he removed to Rome, becoming professor at the Sapienza and physician to the Pope.

In Cesalpini's time and in the very city where he taught, ancient beliefs were for the first time submitted to experimental verification. Galileo, who had attended Cesalpini's lectures, investigated the swinging lamps of the cathedral at Pisa in 1583, the year in which the *De Plantis* appeared; in 1588-91 he refuted the Aristotelian doctrine of falling bodies by dropping weights from the leaning tower. We are not told what Cesalpini and Galileo thought of one another, but it is not difficult to guess. Cesalpini is reckoned among the physiologists who anticipated the discovery of the circulation, though he is not known to have made any experiments of his own; he was also one of the few sixteenth-century naturalists who recognised the real nature of animal and vegetable fossils. His published work shows him to have been an acute, observant man, full of such knowledge as was then accessible, and not afraid to express his opinions, even when they differed from those of the people about him. Could he have realised that in botany as in all natural sciences he was but a beginner, he might have done much more than he actually did. But he was confident and over-emphatic. In the dedication of his *De Metallicis* to the Pope we find a passage which shows that though he claimed for himself that liberty of opinion which the Catholic Church grants to those in whose loyalty it has confidence, he made no secret of his inclination to restrict scientific thought in less orthodox teachers. He repudiates an unnamed author because he held opinions contrary to the principles of philosophy, and also as a man condemned (explosus) by the church.[1]

Cesalpini's *De Plantis* gives a short account of plant-physiology as understood by a Peripatetic philosopher of the sixteenth century. We find a discussion of the question whether the seat of life is diffused or concentrated ; it is finally placed just where the stem and root meet, a point which has neither morphological nor physiological importance. The pith, we are told, is the seat of innate heat ; this strange belief was founded on the resemblance of the pith surrounded by a cylinder of wood to a spinal cord enclosed by a vertebral column. The flower is said to exist, partly to protect the young fruit, partly of necessity, because the plant becomes turgid with vapour. Plants have no sexes, because in them the *genitura* is not distinct from the *materia*. The chief function of the leaves is to shade the buds.

Cesalpini's system of plants has been praised by Ray and Linnæus. He threw over the tentative method practised by L'Obel and others, in order to bring forward a new and logical method of his own. The ancient division into trees and herbs is of course respected, and

[1] I suppose that the author aimed at was Bernardino Telesio, who had attacked the doctrines of Aristotle, and tried to supersede philosophy by methodical observation. His treatise *De natura rerum juxta propria principia libri II* (Rome, 1565) was condemned in the Index of Pope Clement VIII, that very Pope to whom Cesalpini dedicated his *De Metallicis*.

the seedless plants, which are imperfect, bred of putrefaction, and intermediate between plants and inanimate things, are separated from the more perfect plants. Then he arranges his flowering herbs by the number of divisions of the seed-vessel, but uses also, without strict subordination, other characters, such as the superior or inferior ovary and the position of the embryo in the seed. These characters, which have proved valuable to later systematists, are not always employed with knowledge. Cesalpini confuses divisions of the ovary with seeds, or even with flowers; he has no conception of any such morphological unit as the *carpel* of modern botany; and his brief characters drawn from the embryo[1] are sometimes unintelligible, all the more because neither figures nor synoptic tables are supplied. The student finds himself compelled at length to depend chiefly on the illustrative genera cited. Thus judged, Cesalpini will be found to have made no addition to the short list of truly natural families already recognised by L'Obel. Instead of increasing the number he destroyed or spoilt some necessary groups, leaving only the Umbelliferæ intact. Cesalpini stood aloof from all the botanists of his time, whom he never quotes, and they paid no attention to him. Reftelius in the *Amœnitates Academicæ* (who is only a cloak for Linnæus) says truly that Cesalpini dwelt alone in the house which he had built.

Cesalpini offers here and there good observations on the biology of plants. He remarks[2] that ants gnaw the embryos of grains of corn, to hinder them from sprouting when stored underground. He tells how

[1] It is possible that Cesalpini got the hint of them from Theophrastus.

[2] Ælian, *De nat. animalium*, II, 25, may have guided Cesalpini in this passage.

weak plants, unable to support their own weight, may clasp other plants with their tendrils, and shows that a tendril may spring from the axil of a leaf, or in the place of a leaf, or from the apex of a leaf. He names clematis as an example of a plant which climbs with the help of its leaf-stalks, ivy as one which climbs by what he calls "hooks," arranged along the stem like the feet of a centipede; others are said to twine like snakes. He remarks that climbing plants appear to have some power of perception, for they feel about for a suitable support, and grasp it when found (Chap. xi). We find also a good account of the way in which wood-sorrel throws out its seeds, of the creeping stem, flowers and fruit of the white and yellow water-lilies, &c. These plants, or most of them, had been carefully studied before Cesalpini by Bock, Fuchs and L'Obel, sometimes by Theophrastus as well.

Cesalpini's account of the seed and seedling is memorable because he clearly states that in many plants there are two seed-leaves, while in the wheat-grain there is only one. He is further aware that the seed-leaves may contain a store of food, and that in leguminous plants they may never leave the seed.[1]

[1] The passages of the *De Plantis* which treat of the flower and the cotyledons were attentively studied by Linnæus, whose annotations can still be read in the library of the Linnean Society.

PIERRE BELON

1517–1564

Les observations de plusieurs singularitez et choses mémorables trouvées en Grece, Asie, Judée, Egypte, Arabie et autres pays estranges redigées en trois livres. 4to. Paris. 1553.

L'Histoire naturelle des estranges poissons marins, avec la vraie peinture et description du dauphin. 4to. Paris. 1551.

De aquatilibus libri duo cum iconibus ad vivam eorum effigiem. Sm. oblong 8vo. Paris. 1553. Three editions of a French translation, in folio, quarto and octavo, appeared in 1555. One is entitled "La nature et diversité des Poissons, avec leurs pourtraicts, &c." Sm. obl. 8vo.

L'Histoire de la nature des Oyseaux, avec leurs descriptions et naifs portraicts retirez du naturel. Fol. Paris. 1555.

Some twenty years after the revival of botany naturalists began to describe and figure direct from the objects the fishes and birds of Europe. Zoological research may have been a little retarded by the absence of that professional motive which impelled physicians to examine closely their native plants. The facilities afforded by the markets, together with the special knowledge handed down, generation after generation, by fowlers, falconers and fishermen, had no doubt their effect in deciding what animals should first be taken in hand. Belon tells us how, when dwelling in foreign cities, he used to study the birds and fishes which were brought to market. During his stay in Padua he was accustomed to leave home every Thursday evening and travel all night by boat, so as to reach Venice next morning. There he stayed on Saturday and Sunday, employing his time with observation of birds and fishes, and discourse with fowlers and fishermen. On Sunday night he took boat again, and was back at his studies by Monday morning. Nothing is said about personal observation of live birds and fishes, but this was not neglected when opportunities offered.

The life of Belon was full of labour and excitement. He was born in Maine, near the city of Le Mans. As a young man he was patronised by the Chancellor of France, by a bishop, and by two cardinals, Tournon and Chastillon. Thus aided, he went to Germany, where he studied botany under Valerius Cordus, among others. After this he set out to explore the Mediterranean countries, travelling in Turkey, Greece, and the Greek islands, Asia Minor, Syria, Palestine, Egypt, and the Sinaitic Peninsula (1546-9). Shortly after his return he paid a visit to England, where he fell in with the Venetian ambassador, Daniel Barbaro, who showed him drawings of three hundred fishes of the Adriatic, and permitted him to copy them. The rest of the naturalist's life was extremely unlucky. The French king granted him a pension, which was left unpaid, and Belon was reduced to great straits. At last he was set upon in the Bois de Boulogne and murdered; the assassin was never discovered.

The chief writings of Belon are :—(1) His travels, in which he sets down all that he could learn or conjecture respecting the remarkable animals named in ancient authors (*infra*, p. 54). (2) His dissertation on the dolphin. (3) His book of aquatic animals, which has been cast a little into the shade by the more exact book of Rondelet; and (4) his book of birds, the best which the sixteenth century could produce. At the time of his premature death Belon was translating Theophrastus and Dioscorides.

Belon's dissertation on the dolphin occupies a large part of his *Histoire Naturelle des Estranges Poissons Marins.* Its primary purpose was to identify and describe the dolphin represented on ancient works of art. The author easily decides that the dolphin of the ancients was the common dolphin of the Atlantic shores, often confounded with the porpoise. True fishes which had been called dolphins, such as the sturgeon, tunny, &c., are figured and shortly described. In a second part the anatomy of the dolphin is discussed; its brain is said to be very like that of a man; the embryo in the uterus is figured. The hippopotamus is described from a live specimen which Belon had seen in Constantinople, and compared with ancient sculptures. The shells of the argonaut and pearly nautilus are figured and compared. —De Aquatilibus libri duo 4to Paris 1553

The book on *Aquatic Animals* aims at giving by means of descriptions and figures some rough notion of the various creatures which inhabit the waters. Cetaceans, the beaver, otter, seal, water-rat, tortoises, true fishes of many kinds, mollusks, crustaceans and brittle-stars are included. There is also a small admixture of animals which Belon did not profess to have met with, such as the fabled horse of Neptune, the sea-wolf, rather like a hyæna, which was thought to haunt the shores of England, and the fish which resembled a monk. Each animal is shortly described, and its names in different languages are quoted. Identification of the fishes mentioned by ancient writers is a prominent feature. The illustrations are somewhat rude woodcuts, which nevertheless give a fair notion of the different species. There is no regular classification, and hardly any definitions of groups, large or small, but animals which would now be referred to the same class or order are usually kept together. The systematic arrangement indicated by the succession of species is based upon Aristotle. As usual in the works of early naturalists, too much weight is given to the general form and the place of abode. The text is largely a compilation, and most of the figures are believed to have been copied from Barbaro's drawings.

Belon's *History of Birds* is the most important of his contributions to natural history, and was during many years the best book on the subject. It is a handsome folio of near 400 pages, illustrated by many hand-coloured woodcuts, one as a rule to each bird that is described. About two hundred birds are included; they are nearly all European, but Belon does not hesitate to describe with them such foreigners as the ibis, the birds of paradise, and parrots. In his preface he claims to be the first to give "naif portraicts des serpents, des poissons et des oyseaux : le naturel desquels nul autre n'auroit encor fait voir avant nous."[1] His draughtsman was Pierre Goudet, of Paris, whose work does not fully deserve the praise that it receives from Belon. The attitudes are often awkward, and the markings of the plumage are but poorly shown. This was no doubt contrary to the author's intention, for he says in his preface that birds differ from one another chiefly in colour; "touts ont quasi les iambes, ongles, bec et plumes de mesmes," which is, of course, far too strong a statement of the case. There is little to mark the scale of the different birds; the ostrich and the sparrowhawk, for instance, are nearly of a size. The descriptions are unmethodical, and often very slight. Belon is not aware that a small difference, if constant, may serve to distinguish one species from another, and the current popular names (in French) are precise enough for all his purposes. Yet he distinguishes a good many kinds or sorts of birds, and brings together all that seem to him generally similar in structure and mode of life. He does his best to amuse his readers by relating bits of his

[1] Gesner's bird figures were published in the same year (1555).

experience in foreign lands, such as the decoying of sparrowhawks on the Propontis, or by discussing the etymology of the French names of common birds, or by giving the points of a good falcon, or by describing the succession of the dishes at a French banquet, or by explaining why the trail of a woodcock is eatable. A few sentences of "Naturel" (natural history) are often introduced into the description, and we find occasional hints as to the use of birds in medicine, such as that the blood of the partridge is good for sore eyes. Ancient authors are regularly quoted, and pains are taken to identify the birds of which they speak. Fabulous stories are mentioned, though with due scepticism; Belon does not believe, for example, that the sparrowhawk is the father of the cuckoo, nor that barnacle-geese are generated from floating wrecks (they have been seen, he tells us, to lay eggs); nor that the chameleon feeds on air.

What we should now call orders of birds are indistinctly recognised, but only as convenient headings. It was far too early for any naturalist to inquire how there come to be natural assemblages of birds, or why one principle of arrangement is to be preferred to another. Belon adopts Aristotle's groups as far as they go; he recognises the birds of prey, the swimming birds, and the waders with long legs, joining with these last the kingfisher and the bee-eater; his remaining groups are the birds which nest on the ground, then a very miscellaneous group (crows, pigeons, parrots, &c.), which agree only in being of fair size and nesting in any situation; his last section consists of the songsters. Tradition compelled Belon to put the bat among the nocturnal birds of prey, but he did not really take it to be a bird.

In his introduction Belon gives on opposite pages large figures of a human skeleton and that of a bird, naming all the principal bones, and thus indicating their homologies. This is an early and interesting example of that comparative method which has since proved so fertile.

Belon was much interested in the enrichment of French gardens by new exotic species, and is said to have introduced the cedar of Lebanon into western Europe.

GUILLAUME RONDELET

1507–1566

Libri de Piscibus Marinis. Fol. Lugd. 1554.
Universæ aquatilium Historiæ pars altera. Fol. Lugd. 1555.

Rondelet was professor of anatomy at Montpellier, then a provincial capital, famous for its medical school. It is only seven miles from the Mediterranean, whose coasts are full in view from the celebrated Promenade de Peyrou. In Rondelet's day the sea-fisheries were important, and offered good opportunities to an anatomist who sought to enlarge biological knowledge. His reputation as a naturalist attracted many students to Montpellier; among the number were Dalechamps, Clusius, John Bauhin and L'Obel—a list of great distinction, which might easily be enlarged.

With Rondelet, as with other writers of his day, *fishes* include aquatic animals of every kind. In his own mind he distinguished, as Aristotle had done long before, the blood-holding (vertebrate) fishes from the bloodless (invertebrate), but by treating all together in his anatomical account, he rendered most of his generalisations unserviceable. Copious extracts from ancient writers weary the reader, and show how imperfectly Rondelet foresaw that his own observations were to lay the foundation of a new ichthyology, which would convert the descriptions of Pliny and Ælian into mere historical curiosities. He discriminates and names such true fishes as were known to him, and often describes in succession several species which are now placed in the same genus or the same family, such as the "brames de mer" (sea-breams), or the different kinds of Turdus, Raia, and Galeus. The invention of the genus was ascribed by Haller and Linnæus to Gesner, but it is probably as old as natural history. Aristotle enumerates two or more camels, eagles, kingfishers, tits, woodpeckers, wagtails, thrushes, &c. What is modern is the use of the word *genus* as a technical term, and the reference of every species to its genus, verbal usages which came in gradually, and were at length formally inculcated by Linnæus. Rondelet indicates groups more extensive than genera, but without subordination or definition. There are no synoptical tables, and the groups are mere headings. Like other naturalists of that age, he was content to reckon the whales as fishes, though he was well aware of the differences between them. He regularly noted the structure and arrangement of the gills in every true fish that came before him.

In these two books nearly two hundred and fifty species are described, most of them being figured, and there is rarely a doubt as to the fish which is meant. The modern names are regularly assigned to his figures in the *British Museum Catalogue of Fishes* (1859-70).

Rondelet was of great use to Willughby and Ray (*infra*, p. 112) and through them to later ichthyologists. The task upon which all were engaged proved to be one of unsuspected difficulty. Though Ray, Linnæus, Cuvier and other zoologists, the strongest of their time, laboured at it, the end has never come in view. It seems that the highly specialised and dominant group of Teleostean fishes has become adapted in most intricate ways to the exigencies of aquatic life, and that no simple principle of division is likely to prove natural here, any more than in the class of Birds. Ichthyologists, like ornithologists, can only remove this or that blot, with little hope of complete success, even in the distant future.

Yet another book on fishes was brought out nearly at the same time with those of Belon and Rondelet, by Hippolito Salviani (1514-1572), a physician of Rome, whose work, *Aquatilium Animalium* [*Historia*], dated 1554, was only completed in 1558, as the colophon shows. The three authors were all physicians, and all were patronised by Cardinal Tournon. Salviani's book is chiefly remarkable for its beautiful engravings on copper, which in some copies are delicately coloured.

THE ENCYCLOPÆDIC NATURALISTS OF THE RENAISSANCE

We must briefly notice a class of writers who were highly esteemed in their day, though most of them did little to advance natural history, because they relied upon other aids than that first-hand study, which is essential to lasting progress in the interpretation of nature. Encyclopædic learning was the passion of sixteenth century scholars, who loved to transcribe copious extracts from ancient authors into their Adversaria in the hope of some day digesting them into books. Zoology and botany were treated like history or philology by writers who failed to perceive that Pliny and Ælian were by no means trustworthy witnesses on matters of biological fact. The encyclopædic naturalists were far more eager to amass information than to sift it. Their works are now and then languidly turned over by some historian of science, who perhaps collects singular fables as indications of the prevailing state of knowledge, until at length he sweeps the whole away as futile, remembering that obsolete encyclopædias, which reflect, not the opinions of the age in which they were compiled, but a medley of opinions of all preceding ages, are not of much value, even as historical documents.

The best of the encyclopædic naturalists of the Renaissance were Gesner and Aldrovandi. Gesner stands high among early botanists, as we have elsewhere (*supra*, p. 30) tried to show. But he was much else besides a botanist, and would have claimed to be called a polyhistor, *i.e.* a scholar who set himself to acquire and expound all learning.

Gesner's *History of Animals*[1] was written in Latin, and appeared volume by volume from 1551 to 1587, the mammals, oviparous quadrupeds, birds, fishes and other aquatic animals being treated in succession. A volume on serpents and a description of the scorpion, which was to have formed part of the insects, were not published till after Gesner's death. The whole work extended to 4,500 folio pages, and was adorned by several hundred woodcuts. So far as possible, each animal is described under eight heads:—(1) names, in various languages; (2) native country, external characters, &c.; (3) mode of life; (4) habits and instincts; (5) capture, rearing, domestication, &c.; (6) uses as food; (7) uses as medicine; (8) literary and moral uses, historical allusions, &c. The primary arrangement is, of course, Aristotelian, but with a number of changes for the worse;

[1] Historia Animalium. 5 vols. Fol. Tiguri. 1551-87.

beyond this the animals are taken in alphabetical order, though nearly allied forms are often grouped about a type. There is no regular subordination of groups, no precise nomenclature, no anatomical introductions; the figures are largely borrowed. It gives some notion of the state of zoological knowledge in the second half of the sixteenth century that Gesner should have grouped the hippopotamus, whales, fishes, mollusca, &c. as aquatic animals, that the bat should be described among the birds, and that the scorpion should be represented as possessing elytra. The *History* was republished, abridged, and translated, so that it must have been highly esteemed.

Not only Gesner but almost all the naturalists of the sixteenth century put the bat among the birds and the whales (sometimes the seals and the hippopotamus also) among the fishes, or at least in a group of aquatic animals, though the more knowing showed that they were aware of the differences which rendered such associations scientifically indefensible. It is surprising that they hardly ever ventured to throw over the mediæval grouping and go back to Aristotle, whose name commanded so much respect. Wotton and Aldrovandi did so in the case of the bats,[1] but not even Ray dared to separate the whales from the fishes. What is perhaps the last survival of such a grouping is to be found in Artedi's *Ichthyologia* (1738), which was edited by Linnæus.

Less known to fame was Edward Wotton (1492-1555), a London physician, who published a Latin treatise *De differentiis animalium* (fol. Paris, 1552) nearly at the same time with the first part of Gesner's *History*.

[1] Wotton treated the bats as mammals, Aldrovandi as intermediate between mammals and birds; Aristotle seems to have hesitated between the two views.

Wotton methodised the zoology of Aristotle, and drew up the first formal classification of animals. His book is sagacious and careful, but dry. It was little read, and exerted no appreciable influence upon the progress of zoology.

Adam Lonicer (1528-1586), a physician and botanist of Frankfort, published a *Naturalis Historiæ Opus Novum* (2 vols. Fol. Francofurti. 1551), the largest and best part of which is botanical. This work is more remarkable for its longevity than for its quality; it was continually re-edited, and only disappeared from the book-market in the eighteenth century.

Ulysses Aldrovandi of Bologna (1522-1605) was, like so many other early naturalists, a physician and botanist. At first he pursued many different branches of study, but by the advice of Rondelet selected zoology and botany as his own special province. Aldrovandi was director of the botanic garden of Bologna, which he had largely helped to found. He was also a diligent collector, and bequeathed a museum to his native city. In old age he began to publish an extensive treatise on animals, which was to form part of a still wider scheme.[1]

John Jonston (1603-1675) was a weak successor to Aldrovandi, from whom he borrowed largely. His illustrated works enjoyed a great reputation, being republished or translated many times. Jonston was of Scotch descent, though born in Poland; he studied both at Thorn and St. Andrews. To explain how this came about would require a historical discussion, in which the Wyclifites, Hussites and Moravians would all find a place.

[1] Only the birds (Fol. Bononiæ. 1599-1603) and the insects (1602) appeared during Aldrovandi's lifetime. The quadrupeds, viviparous and oviparous, the serpents and dragons, the fishes and whales, the bloodless animals and the *Dendrologia* were edited and published posthumously (Fol. Bononiæ. 1606-7).

SECTION II. THE NATURAL HISTORY OF DISTANT LANDS (EARLY TIMES TO THE CLOSE OF THE SIXTEENTH CENTURY)

Voyages of discovery go back to times whose history is inextricably mixed with legend. Phœnician merchants sailed over the Mediterranean, and beyond the pillars of Hercules to the Fortunate Islands and the western shores of Spain, bringing to Tyre and Sidon the products of Arabia, Egypt and India, as well as of northern countries rarely visited except by barbarian traders. Herodotus, the first Greek historian, travelled in Persia, Egypt and Scythia, and was able to gratify the curiosity of his countrymen by telling them, among many things of greater importance, about the crocodile of the Nile, and the artificially impregnated date-palm of Babylon. Ctesias, a Greek physician, who had lived at the court of that Artaxerxes, whom Cyrus the younger tried to dispossess, wrote accounts of Persia and India, in which elephants, parrots and bamboos are noticed. Greek armies were led by Alexander to the Punjab, returning by the Indus and the Persian gulf. It is just possible that from this last source of information Aristotle learned what he knew about the anatomy of the elephant, and how the Bactrian camel differed from the Arabian.

In the narrative of the voyage of Nearchus (one of Alexander's generals) from the Indus to the Tigris, mention is made of the tiger, of the cotton-plant and of the use of cotton in weaving, of rice, of silk, of the sugar-cane, of tortoise-shell, and of oriental spices and drugs. The fleets of the Ptolemies made regular trade-voyages to Arabia, tropical Africa and perhaps to countries yet more remote. Ptolemy Philadelphus set up a menagerie at Alexandria, in which elephants, rhinoceroses, buffaloes and ostriches were kept. Agatharcides, an Alexandrian scholar of the second century B.C., described strange animals of Ethiopia, the giraffe, the rhinoceros, the baboon, various monkeys and the spotted hyæna. Theophrastus knew something about the banyan-tree, the citron, the tamarind, which was reported to fold up its leaflets at night, and the thorny Mimosa of Egypt, whose leaves droop when touched.

Under the Roman empire trade with distant countries was perhaps as much hindered as encouraged by the Roman passion for dominion. Such books as the *Natural History* of Pliny show that opportunities of enlarging geographical knowledge were not neglected. Roman emperors sent expeditions to the shores of the Baltic for the sake of amber, and to tropical Africa for the sake of birds of rich plumage. Elephants, camelopards and ostriches were exhibited and slain in the circus. Ivory, silk, pearls, spices, dyes and drugs were regularly imported.

During the long decline which followed the downfall of the empire such knowledge as the ancients had possessed about exotic animals and plants shrank to a meagre stock of perverted recollections. Though the elephant was kept in mind by the bestiaries and the first book of Maccabees, it was often confounded with the camel.[1] Monkeys, lions, leopards and lynxes were still well known.

Of the acquisitions made between the years 200 and 1000 A.D. none perhaps was more considerable than the importation of the silkworm in the reign of Justinian. Constantinople was during many centuries the great European emporium of eastern wares.

The wars of Saracens and Christians did little for geographical knowledge or industry to compensate for the interruption of peaceful intercourse which they created. In the thirteenth century the passionate zeal which had stirred up so many Holy Wars died out, but travel and exploration revived as the progressive movement (see pp. 7, 9) gained strength. Towards the end of the thirteenth century Marco Polo and his companions reached China (Cathay, as it was then called) by land, taking advantage of that relaxation of restrictions which followed upon the conquests of the Tartars. The barriers were soon restored, and China became once more impenetrable. Elsewhere geographical knowledge and commerce advanced steadily. Venice, Genoa and Florence became enriched by eastern trade. Dates, balsams and flax were regularly imported from Egypt; the sugar-cane was planted in the islands of the Mediterranean, and cotton in the south of Europe. In the sixteenth century, and indeed long before, the northern parts of Spain supplied Europe with whalebone and train-oil, sending their ships out into the Atlantic to capture the Right Whale.

[1] We read however of an elephant sent to Charlemagne by Haroun-al-Raschid, and of another given to our Henry III. by Louis IX. of France; there is a tolerable though small figure of one in the *Meditationes* of Johannes de Turrecremata, Rome, 1467. A giraffe was imported by the emperor Frederick II. in the thirteenth century.

Few of the innumerable pilgrims to the Holy Land brought home anything better than chance scraps of information about the remarkable animals and plants of Syria. Among the most enterprising was one of the latest pilgrims, Bernard de Breydenbach, a canon of Mayence, who travelled in Palestine and Arabia during 1482 and following years. He wrote an account of what he had seen,[1] which is illustrated by very curious woodcuts. A painter named Remich made one of the party, and drew several strange animals, among which was a giraffe ("seraffa"); no earlier portrait of this animal, taken from the life, is known. Breydenbach was probably the first traveller whose descriptions and figures were multiplied by the printing-press. Menageries, containing remarkable foreign animals, now began to be common ornaments of the courts of Italian princes.

Here would come in order of time the great geographical discoveries of Vasco da Gama and Columbus. We shall however defer this topic until we have tried to show by two or three examples how the new spirit of the Renaissance stirred up explorers to examine more closely the natural products of countries less distant from civilised Europe.

Pierre Belon, of whose life a sketch has already been given (p. 40), visited the eastern end of the Mediterranean during the years 1546-9. In 1553 he published a little book called *Les observations des plusieurs singularitez et choses memorables trouvées en Grece, Asie, Judée, Egypte, Arabie et autres pays estranges*, which was highly esteemed, passing through several editions, and being translated into Latin by the

[1] *Opusculum sanctarum peregrinationum*, Mainz, 1486, often reprinted and translated into several modern languages before 1500. Some beautiful manuscript copies also exist.

celebrated naturalist Clusius, as well as into German. A portrait prefixed to the Latin translation shows Belon as a strong and handsome man with short curly hair and full beard; he was only thirty-two when he returned from the east.

The Turks were then at the height of their power. Belon, like Busbecq (p. 56), admired their hardihood and temperance in this season of conquest and glory. He describes the menagerie of the sultan, which was kept in an ancient temple at Constantinople. Lions were tied each to its own pillar; sometimes they were let loose. Besides lions there were wolves, onagers, porcupines, bears and lynxes. Genets were kept in the houses like cats. Belon says that the Turks loved flowers, and were skilful in gardening. Parsley was called *macedonico* in the market of Constantinople; hence perhaps the *macédoine* of modern cookery. Smilax aspera and Tamus communis were used as salads. The giraffe, buffalo, gazelle, chameleon and Egyptian crocodiles are described, some of them being figured. Belon refutes the popular fable that the chameleon lives on air, but was induced to figure a mummied serpent with wings and clawed feet, which, he tells us, was able to fly from Arabia into Egypt. Much to his surprise, he found the skin of a six-banded armadillo, which must have come, he knew, from South America, in the hands of a troop of wandering Turkish drug-sellers; he secured the specimen and figures it.

We find a particularly interesting description of Crete. Belon begins by lamenting that the Greeks, to whom the arts and learning owe so much, held not a foot of ground as their own, the Turks dominating the inland parts, and the Venetians the shores of what had been the Greek empire. The ancient language was still spoken; though corrupt, it was, Belon thinks, more similar to ancient Greek than the Italian dialects to Latin. He notes some customs which still prevail in Crete, such as the practice of sipping wine, but at the same time quenching thirst with large draughts of water. He did not fail to visit the ruins of ancient cities, all of which the Cretans were inclined to call by the celebrated name of Labyrinth. A particular account is given of the mode of collecting the balsamic resin called Ladanum, which was much esteemed by the ancients, and of which Pliny had related a ridiculous fable, viz. that it was combed out from the beards and shaggy legs of goats which had browsed in the forests of Arabia. There is a lengthy description of his discovery of the parrot-wrasse (Scarus), which Aristotle had said (wrongly, as it happens) to be the only fish that ruminates. The Cretan sheep and goat are described and figured. Concerning the latter Belon makes two startling remarks, viz. that its horns may be four cubits long, and that the number of rings on the horns tells how many years the animal has lived.

In this way Belon goes on pleasantly from one country to another, discussing with little method animals, plants, useful arts, drugs, and the ruins of ancient buildings. One heading runs thus:—"Modestie des soldats turcs, et d'un serpent nommé Jaculus, et de l'oiseau nommé Onocratalus." Many of the woodcuts are fair, but the long-tailed ichneumon, whose tail is cut off and shown separately above the body, makes us smile.

Augier Ghislen de Busbecq (1522-1592) was a Fleming, who was twice sent by the emperor as ambassador to Soliman II. Historians have drawn valuable information from his descriptions of the

Turks in the days when they threatened all Christendom. Being not only learned, but urged by an unbounded curiosity, Busbecq inquired into strange facts of every kind. His love of gardening caused him to send some plants, cultivated by the Turks but unfamiliar in Europe, to his correspondents in Vienna.

Gilles, in Latin Gillius[1] (1490-1554), was a naturalist who made the same venture as Belon, and like him, was unkindly treated by fortune, for his calamities hindered him from bringing home the fruits of his toil. He was a native of Alby in Languedoc, who betook himself to the study of the ancient naturalists, but gained practical experience of zoological research by examining the fishes of the Mediterranean and Adriatic. He was patronised by a celebrated free-thinking bishop, Armagnac, and commissioned by the king, Francis I., to visit the Levant in quest of ancient or modern knowledge. His necessities were not duly provided for, and he found himself left destitute in Asia Minor. All his collections were lost, and he was compelled to enlist in the Turkish army for the sake of a subsistence. At last he made his escape to France (1550), and rejoined his patron, now a cardinal, at Rome. Before setting out on his travels Gilles published Ælian in Latin, rearranging his matter, and identifying the species where possible; after his return he wrote on the topography of Constantinople. Among his publications is a description of an elephant sent from Persia to the sultan.[2] Gilles met with it and its Hindoo mahout at Aleppo, where the elephant died. He notes the gentleness of the animal,

[1] This Petrus Gillius must not be confounded with Petrus Gillius or Ægidius of Antwerp (1486?-1533), who was the friend of Erasmus and Sir Thomas More, and edited the first edition of the *Utopia*.

[2] *Elephanti Descriptio, missa ad R. cardinalem Armagnacum ex urbe Berrhœæ Syriaca, authore Petro Gillio*, 8vo. Lyon. 1562.

supposed to be not more than four years old, and its love of play. He easily refutes the belief that the elephant had but one joint in its limbs. Finding two live elephants at Constantinople, he measured the larger one. With this insignificant contribution recommences the study of the structure and natural history of the elephant, interrupted for some nineteen centuries. The same little book contains notes on the "marine elephant" (hippopotamus), which also he saw alive at Constantinople, the giraffe, and an ichneumon, which last he kept alive for some time.

Siegmund von Herberstein, who visited Moscow in 1516-7 and again in 1526, as ambassador from the emperors Maximilian and Charles V., described Russia for the gratification of the curious.[1] Among other things he mentions some remarkable wild animals, the bison, the elk, the ibex or some allied species, and the onager or wild ass. He says of the Lithuanian bison that it has a mane, long hair about the neck and shoulders and a beard ; the eye is large and fierce, as if on fire ; the horns are wide apart, and there is a hump on the back (not a real hump, but only high withers) ; the animal smells of musk.

Whatever Olaus Magnus (Magni or Stor) titular archbishop of Upsala (b. 1490, d. 1557) may have been as a describer of national customs and a collector of folklore, he sinks to the mediæval level in his descriptions of animals. His *History of the Northern Nations*[2] tells of the glutton, which after gorging himself makes ready for another meal by squeezing his body between two trees, of the kraken, which is able to swallow ships,

[1] *Rerum Muscoviticarum Commentarii*, Fol. Vienna. 1549. Translated as "Notes upon Russia," 2 vols., Hakluyt Soc. 1851-2.

[2] *Historia de gentibus septentrionalibus.* Fol. Rome. 1555.

of the sea-serpent a league and a half long, and of swallows which pass the winter at the bottom of lakes and rivers. Some of these fables long continued to figure in natural history books.

The interest with which Europe received the announcement of a new continent across the Atlantic was heightened by the report that it was peopled by strange animals and plants, unknown to ancient or modern naturalists. The species of North America, it has since been discovered, for the most part belong to genera or families which occur in Europe or temperate Asia, but the West Indian islands, Brazil and Mexico (and it was these of which the Spanish navigators brought intelligence) possess a far more peculiar fauna and flora.

On his return from his first voyage (1493) Columbus exhibited to the court of Ferdinand and Isabella, not only six Indians waiting to be baptised, but live parrots and a few stuffed animals. In subsequent voyages he paid such attention to natural history as his troubled and wandering life permitted.[1] The *Decades* of Peter Martyr Anglerius[2] were a chief source of information to the readers of Europe during the early years of the sixteenth century.[3] Anglerius had never crossed the Atlantic, but his official position as chronicler of Indian affairs and member of council for the Indies made him acquainted with every new exploration. He

[1] Humboldt has remarked the closeness of Columbus' observation of all natural phenomena. Among other things he noted the solitary seed of Podocarpus, an aberrant South American conifer. Hardly any American explorer before Joseph de Acosta, he adds, showed any power of generalising the facts of observation, except Columbus (*Examen Critique*, Vol. III, pp. 20 foll.). The Letters of Columbus do not seem to me to bear out the statement as to his frequent and close observation of natural objects.

[2] So named from his birth-place, Anghiera on Lake Maggiore.

[3] *De Orbe Novo Decades*, Alcala, 1516. There is a translation into Italian in the third volume of Ramusio.

had entertained in his own house Columbus, Sebastian Cabot and other well-known navigators, and took a lively interest in their enterprises. Moreover, he could write from scanty materials interesting sketches of what had been seen in the New World, and these sketches, when collected into Decades, circulated far and wide. He tells how Pope Leo X. liked to read them to his sister and the cardinals.[1] No marvel of the animal life of America interested early explorers more than the opossum, which figures in several narratives. Anglerius describes it as a creature which had the snout of a fox, the tail of a monkey, the ears of a bat, the hands of a man and the feet of an ape. It climbed trees, and carried its young in a pouch, like no other known animal.

The first man to set down in writing something like a connected account of the natural history of the New World was Gonzalo Fernandez de Oviedo y Valdes (1478-1557). Oviedo had in his youth served as page to Prince Juan, son of Ferdinand and Isabella. In 1513 he was sent out to America as inspector of mines, and after this he served the crown in various capacities, residing long in Hispaniola, of which he was alcalde. On his retirement from foreign service he acted as chronicler of the Indies.

Oviedo laboured during a great part of his life at a *General and Natural History of the Indies.*[2] A summary of this was published in 1526, and the first part of the full history in 1535. The whole is now accessible in print.

West Indian Mammals. We are told by Oviedo that when Hispaniola (also called Hayti and St.

[1] Letter of Anglerius, Dec. 26, 1515.

[2] *Historia general y natural de las Indias.* Fol. Salamanca, 1535.

Domingo) was first visited by Europeans, it contained five animals (he means mammals), besides snakes, &c. Four of the five were called by Indian names, Hutia, Chemi, Mohui, Cori, the fifth kind being the native dog. It would require an intimate knowledge of the native mammals, and especially of their quality as food, to identify all of these by means of Oviedo's descriptions, for though he tells us which were good to eat, he says little about teeth and claws, which are more serviceable in the determination of species. Of the native dogs he says that the Indians used to rear them in their houses, but that at the time of writing none were left. They were of all colours; some were smooth-haired, others woolly like sheep. The ears were erect. The dogs of the Indians were used in hunting, but were not equal to those which had been brought from Spain. They were dumb, and did not howl or bark when beaten.

The Tapir. Oviedo's Danta or Beori (Indian name) must be the tapir, but the description is very vague. We are told that it was as big as a mule, that its skin was dark, and that it had no horns. The flesh was good to eat, and the feet delicious when boiled for twenty-four hours. The animal was hunted with dogs, and had to be hindered, if possible, from entering water, where it became formidable.

The Sloth. According to Oviedo the sloth takes a day to travel fifty paces. Its legs cannot support its weight, and the body trails on the ground. It climbs trees, gripping the boughs with its long claws, and sings by night, uttering six notes in regular descending order. It will remain on a tree-top for many days together, and no one knows what it feeds on, but since it keeps its head turned towards the wind, Oviedo thinks that it must live on air. Such tales as these were often repeated by naturalists, some of whom, like Ulloa, had seen a live sloth, while others, like Buffon, had not. Buffon severely criticises Nature for turning out a creature so ill-equipped and so wretched. At last Charles Waterton showed that the sloth is by no means the pitiable object which Buffon and his forerunners had painted; it is in all respects well-adapted to its mode of life, and only becomes grotesque or unhappy when removed from its accustomed haunts, and hindered from using its natural powers.

The Anteater. Of the ant-bear, as he calls it, Oviedo says that it has the skin of a bear, a long snout and *no tail*! It is defenceless, though it sometimes bites (Oviedo seems not to be aware that the anteater has no teeth). It feeds on ants (really on termites), which it manages to secure in spite of the strength of their habitations. In South America, Oviedo explains, the ant-hills are as high as a man, and being alternately moistened by rain and baked by the sun, become as hard as stone. The entrance is close to the ground, and so small as to admit nothing bigger than an ant. But the ant-bear finds cracks on the surface of the fortress, into which it inserts its tongue; by continual licking these are widened more and more until an effective breach is made. He knows nothing of the use of the great claws in demolishing an ant-hill, or in self-defence.

The Manatee. Oviedo describes this animal as a fish, though he is aware that it has a leathery, not a scaly, skin, and teats for suckling its young.

Birds. Oviedo gives Spanish names to the birds of the West Indies and South America, entertaining little suspicion that they were distinguished by peculiarities more important than differences of size or colour. Lively descriptions are met with in his pages, as when he says of the humming-birds that they are no bigger than the top of the thumb, and when plucked, only half as big; that they fly too fast for the movements of the wings to be followed by the eye, and when first seen are taken for hornets. Nest and bird together, he goes on, may weigh no more than twenty-four grains, while the feet and claws are as delicate as in the miniatures of an illuminated prayer-book. The plumage is of all gay colours, such as green and gold, and the bill is as fine as a needle. Though so small, they are bold enough to fly at the eyes of anyone who tries to plunder their nests. It is easy to imagine the delight with which such particulars were read for the first time.

American Plants. Many edible and medicinal plants are described, among the rest, maize, cassava, the pine-apple and the prickly pear. We are told of the singular efficacy of a prickly pear poultice in curing fractured limbs, and of the edible fruit. The carmine colouring matter is also noticed, but no mention is made of the cochineal insect. India-rubber balls are said to be used in an Indian game.[1]

Oviedo's figures of animals and plants are very rude, but much allowance must be made for the clumsiness of the wood-engraver.[2] Maize, pine-apple, cacti, &c. are represented for the first time in a printed book; the manatee is one of the few animals figured.

[1] This last I quote from Darmstädter's *Geschichte der Naturwissenschaften*, having failed to verify his reference, or to discover the passage in the enormous and unindexed volumes of the Madrid edition of Oviedo.

The first mention of a lead-pencil occurs in Gesner's little book on fossils (*supra*, p. 30), but the first mention of india-rubber as useful for erasing pencil marks is as late as 1770 (see Thorpe's *Priestley*, p. 72).

[2] This clumsiness will strike any reader who recollects the high quality of the wood-engraving executed in Germany, Holland and Flanders during the first quarter of the sixteenth century; Italy and France were not far behind. The reproductions of Oviedo's figures in Ramusio are much better executed than the originals.

As a naturalist (I have not read his civil history) Oviedo is not considerable. He does not claim to possess any special gifts, training or experience; indeed we may say that in his time there was no instance of a man who confined himself to so narrow a department of learning as natural history. He writes merely as one who was well acquainted with tropical America, had observed the things about him, and had noted all that was told him. Pliny and Albertus Magnus were still authorities, while the *Elucidarius* and the *Ortus Sanitatis*, though packed with fables, furnished a large part of the natural knowledge of the reading public. But the spirit of enlarged curiosity was abroad, and although Oviedo shared many beliefs at which we cannot but smile, he had the thirst for knowledge which properly belongs to a contemporary of Copernicus and Regiomontanus, of Brunfels and Bock, of Leonardo da Vinci and Albert Durer, of Erasmus and Sir Thomas More. Oviedo exhibits the simplicity of Herodotus; Acosta, who comes next before us, possesses the higher quality of thoughtfulness; exactness we must not expect for another hundred years or more.

J. Acosta's *Natural and Moral History of the Indies*, a concise but interesting sketch of the natural phenomena, useful products and native tribes of America, was first published in Latin at Salamanca in 1588. It was so well received that it was quickly translated into Spanish, with large additions. The *History*, thus recast, was three times reprinted in Spain, and translated into Italian, Dutch, French, German and English.[1]

The author, Joseph de Acosta, was a Jesuit father, who had sailed to Cartagena in 1570, being then about thirty

[1] Grimston's translation of 1605 has been reprinted, with introduction and notes by the Hakluyt Society, 1880.

years of age. He was set over the Jesuit missionary stations in Peru, and resided for many years at their chief settlement, Juli, on Lake Titicaca, from which he ultimately removed to Lima. He sailed to Mexico in 1583, and returned to Spain in 1587. His last years were spent at Valladolid and Salamanca, where he presided over Jesuit colleges, and he died at Salamanca in 1600.

Acosta sets out by proving that the same sky which over-arches Europe extends all the way to America. The glorious Chrysostom had indeed maintained a contrary opinion, but Acosta had sailed as far as the tropic of Capricorn, and seen the northern constellations gradually sink as the southern cross rose. He explains the motion of the heavenly bodies by supposing that the star-sphere revolves about the immovable, spherical earth, just what his contemporary, Tycho Brahe had taught in the same year (1588).

Another preliminary question which Acosta feels bound to discuss is the question how America became peopled. Since all men are descended from Adam, the first human inhabitants of the New World must have been derived from the eastern hemisphere. They could not have crossed the ocean, for they had no compass. But the tribes of men are only part of the problem; America has its animals also, some of them large and ferocious. Saint Augustine[1] had long before pointed out that the presence of such animals in islands is a great difficulty; he thought it possible that they might either have swum across from the mainland, or sprung out of the earth, or even have been carried across by those who took delight in hunting. Acosta rejects all these explanations; he cannot suppose that

[1] *De Civitate*, lib. XVI, cap. vii.

animals could swim across the Atlantic, nor would men have tried to carry fierce beasts across in ships, nor does he think it conformable to nature and the government established by God that lions, tigers and wolves should be engendered of the earth, like rats, frogs, bees and other imperfect creatures. His own solution is much more probable than any of the alternatives of Augustine, viz., that the continents of the Old and New Worlds meet or nearly so, perhaps towards the north pole, where the maps of Acosta's day showed a great widening-out of America.

In another place Acosta shows that those who maintain that the quadrupeds now peculiar to America were created there are at variance with the history of the creation and the deluge. For why should it have been necessary to preserve the animals in the ark, if they could be created anew as required, and how could the sacred history affirm that all was made and finished in six days, if other animals of high grade were still to be created? We are bound therefore to suppose that the peculiar animals of America, such as the alpaca and the llama, came from the Old World. Perhaps all the animals dispersed gradually after the subsidence of the deluge, when such as found countries well suited to their mode of life survived; the rest perished. In the end every region became populated by animals well adapted to the local conditions, and not found elsewhere.

Every race, he goes on, not only of animals but of men, shows peculiarities which are not *essential*, but *accidental*, differences of colour, stature and so forth; some apes have tails, some none; some sheep are short-haired (bare, Acosta says), others fleecy; some are long-necked, others short-necked. But such "accidental" differences can never, he thinks, account for the "essential" differences between the animals of America and those of Europe.

It may be doubted whether any speculator who accepted the literal truth of the book of Genesis could have framed a better explanation of the observed facts.

Acosta smiles at Aristotle and other ancient philosophers, who had taught that the torrid zone was uninhabitable by reason of its heat. I have lived there a long time, he said, and found it very pleasant. Only after much learned disquisition does he bring out one very material fact. Equatorial America is traversed by one of the loftiest mountain-ranges in the world, and Acosta spent most of his time in Peru at a greater elevation than the highest summits of the Pyrenees. But even the shores of equatorial America are habitable, as he shows.

His discussion of the trade-winds is based upon solid facts, and his explanation is quite tolerable, though he is of course wrong in attributing them to the diurnal motion of the celestial spheres, which carry the atmosphere round with them.

Certain plants and animals of Peru and Mexico are described briefly, especially such as are important to man. We miss some remarkable features of the flora and fauna; there is, for example, no mention of the great cactuses, nor of the many singular water-birds of the mountain-lakes, such as Lake Titicaca; nor of opossums, which abound, not in the mountains (most familiar to Acosta), but in the wooded plains; the condor is dismissed in a few words. Acosta would have written a very big book if he had told all that he knew.

The notices of maize, potatoes, cassava, tomatoes, bananas, cotton, and pine-apples we may pass by as long familiar. Of cacao (our cocoa) Acosta says that it was much used in Mexico for the making of chocolate, which had been a favourite drink long before the arrival of the Spaniards; it was not grown in Mexico, but imported from Central America; cocoa-seed passed as money among the Indians. Chili peppers (Capsicum) were a favourite condiment, added to many dishes. The leaves of the Peruvian coca (Erythroxylon) were chewed as a stimulant, like the betel of equatorial Asia. The Mexican pulque, the fermented juice of the agave, is described. Prickly pears and the cochineal which is found on them, the iron-wood which sinks in water, and the brazil-wood used in dyeing are among the curiosities of which Acosta speaks. The Indians grew pulse, whether native or introduced from Europe Acosta does not know. Ginger had been already brought from the East Indies to Hispaniola, where it multiplied greatly, and the sugar-cane was extensively planted in Peru, Mexico and the West Indian Islands; the canes were crushed by machinery.

In the passion-flower people found emblems of the crucifixion; Acosta remarks that they were not wholly wrong, but that some piety is required to believe it all.

Monkeys. Acosta says that he saw on the isthmus of Panama monkeys tying themselves together by their tails for the purpose of crossing a river. This story, retold by Ulloa,[1] who gives an engraving of the monkey-chain, has been repeated in many popular books of natural history. Humboldt[2] says that though he had opportunities of observing thousands of the howler-monkey, which is named as forming a chain, he places no confidence in such tales.

[1] *Viage a la America meridional.* Madrid. 1748. Vol. I, pp. 144-9.

[2] *Personal Narrative*, Eng. Trans., Vol. II, p. 264.

Puma. Of the puma or American lion Acosta says that it is not so furious as it appears in pictures.

Llamas and Alpacas. These he considers to be a kind of sheep. He praises them as of great profit and small charge, for they yield both wool and meat, and carry burdens without either saddle or oats; they are sheep and asses combined. He speaks of their being hunted by a thousand or more hunters at a time, and also of their being lassoed with lines and plummets of lead. The vicuna he compares to a wild goat, but says elsewhere that it cannot be really a goat, for it has no horns.

Manatee. This he saw in the Windward Islands, and describes it as a strange kind of fish, if we may call it a fish, for it brings forth its young alive and suckles them. The flesh was so like veal that he had scruples about eating it on a Friday.

Of other quadrupeds he mentions the peccary, tapir, armadillo, chinchilla, guinea-pig and three-toed sloth, but has nothing interesting to tell about them.

Humming-birds. Acosta often doubted as he watched them whether they might not be bees or butterflies.

Flying-fishes. He saw flying-fishes leaping into the air to avoid the pursuit of the dorado. One fell on his ship, and he examined its wings, which he thought to resemble linen cloth or parchment.

Acosta saw the guano islands, and learned that guano is a valuable fertiliser. He describes from his own experience the symptoms of mountain-sickness.

It does not belong to our undertaking to quote interesting facts, of which there are many, concerning the Incas of Peru, or the ancient civilisation of Mexico. Enough has already been extracted to show how valuable in Acosta's own day must have been the observations of a man of his wide experience, furnished too with a candid and inquiring mind.

We must notice much more briefly the effects of the discoveries in tropical Asia. Here peculiarities of situation, climate, population and history rendered the new acquisitions of geographical knowledge far less important, at least for a time. In the days when there was no Suez canal the East Indies were about three times as distant from western Europe as Mexico or Peru, which is one reason for the comparative slowness of eastern exploration. In America vast tracts of land enjoy a temperate climate, and bear plants which thrive in Europe, but European settlers cannot permanently establish themselves in the East Indies, and tropical plants are unable to endure the cold of our winters. The native races of America were numerically weak, little advanced in the practical arts, and unable to resist European arms; the nations of Eastern Asia on the other hand were populous and capable of an effectual defence; it proved to be a far harder task to explore the East than to conquer the West. Lastly, the East Indies had been long though most imperfectly known, through conquering armies, the reports of travellers, and especially through traders; in America (especially in South America and Mexico) almost everything was new.

Thus it happened that the discovery of a new world across the Atlantic immediately created a thirst for selfish acquisition, accompanied by a far weaker but nevertheless invaluable impulse to learn all that could be learned about the strange new lands. New settling grounds were opened to the Spaniards, the Portuguese, the French, and ultimately, with far greater results, to the English. Of smaller but considerable importance was the introduction of new food-plants to Europe. On the other hand the discovery of a sea-route to India did little more at first than to throw a profitable foreign trade into the hands successively of the Portuguese, the Spaniards, the Dutch and the English. The conquest of America at once began to enlarge the bounds of natural history, but it was long before really valuable knowledge of this sort was brought from Asia. Magellan and his companions were able to see with their own eyes the nutmeg-tree and the clove-tree of the Moluccas, the camphor-tree of Borneo, cinnamon-trees, ginger, sago-palms and bananas. A little later Garcias ab Horto and his pupil Christobal Acosta wrote treatises on the drugs of India, but it was not till the seventeenth and eighteenth centuries that the Dutch naturalists began to publish methodical treatises on the natural history of India and the Malay archipelago, while the British contributions are of still more modern date.

The new food-plants brought over from America (potato, maize, Jerusalem artichoke and probably the haricot[1]) made a very important addition to the resources of Europe. From America too came many ornamental plants, capable of cultivation in our gardens. A few tropical species from Brazil, Peru, Chili or the West Indies, were cultivated in European greenhouses, which were however rare and costly luxuries till the eighteenth century was far advanced; among these the passion-flower and the sensitive plant excited particular interest. But for nearly three hundred years hardly any plants from the Far East were cultivated in Europe.

[1] The origin of the French bean and the scarlet runner, which are both haricots, has not been fully cleared up. See De Candolle's *Cultivated Plants.*

CLUSIUS, CONSIDERED AS A STUDENT OF EXOTIC NATURAL HISTORY

Charles de L'Escluse, better known by his Latin name of Clusius (1526-1609), was a Fleming, who made it one of the occupations of his long and busy life to translate and publish the narratives of travellers and collectors in distant lands. He had studied in several universities, pursued all the principal branches of learning then cultivated, and searched the wilder parts of western and central Europe for rare plants. He had lived at Montpellier in the house of the eminent naturalist Rondelet, and had been encouraged by him to devote himself to botany and zoology, had directed the imperial botanic garden in Vienna, and had been the intimate associate of L'Obel and Dodoens, besides keeping up a correspondence with Busbecq, Gesner and many other men of note. During the latter half of his life he was the great centre of botanical information. Serious troubles, arising partly from his Protestant faith, and partly from an extraordinary proneness to fracture and dislocation of the limbs, did not spoil his power of work. His last years were spent in a quiet professorship at Leyden. He visited England in 1571.

The two books cited[1] contain the most important results of the labours of Clusius. Here we can read the accounts which early Spanish or Portuguese travellers and residents in the East or West Indies had given of the fruit-eating bat, three-banded and six-banded armadillos, the sloth and pangolin, the sperm-whale, the manatee, the cassowary, dodo and penguin, humming-

[1] *Rariorum Plantarum Historia.* Fol. Antwerp. 1601. *Exoticorum libri decem, quibus animalium, plantarum, aromatum, aliorumque peregrinorum fructuum historiæ describuntur: item Petri Bellonii observationes, eodem Car. Clusii interprete.* Fol. Antwerp. 1605.

birds and birds of Paradise, the chimæra, diodon, tetrodon and ostracion, the king-crab and gorgonia, the banyan, mace, nutmeg, spice-clove, cinnamon, pepper, lac, the Egyptian lotus (Nelumbium), the coco-nut, pine-apple, vanilla, arnotto, capsicum, copal and tobacco, besides foreign drugs, such as aloes, assafetida, sarsaparilla, balsam of tolu, castor-oil and opium. His descriptions are often accompanied by woodcuts, which give a fair notion of the objects.

The diligence of Clusius was often rewarded by unexpected and highly curious facts. In his last years especially, residence in Holland, then beginning to send out ships to the Far East, gave him excellent opportunities of collecting information, but he had been long before known throughout Europe as a man learned in every branch of natural history. A Portuguese physician, Christobal Acosta, who had resided at Goa, published a description of the sensitive plant, which Clusius translated, adding a figure taken from a dried specimen brought by the Earl of Cumberland from the island "D. Joannis a portu nuncupata."[1] Another time he was disappointed by the death at sea of a live sloth, shipped to Amsterdam, but managed to draw or procure a drawing of the carcase; which he helped out by the description of Oviedo; it is not surprising that his figure is hardly recognisable. For the sperm-whale he had to trust to a figure given by a Spanish friar (in a catechism!) and to a drawing of a specimen cast up on the Dutch coast.[2] A squadron of eight ships, commanded by Van Neck, sailed from Holland to the East

[1] This island was Cuba, named by Columbus after the son of Ferdinand and Isabella, the infante Juan.

[2] The same whale, apparently, reappears in Visscher's *Piscium Vivæ Icones* (1634) and in Jonston (*De Piscibus et Cetis*, 1650, pl. XLII), but the point of view differs a little from that of Clusius' figure.

Indies in 1598, conquered the Mauritius from the Portuguese, and renamed it after their own Prince Maurice. The narrative of the voyage, published in 1601, makes mention for the first time of the dodo, of which a live specimen was brought home. Clusius was able to copy a sketch made on board, and also to describe a dodo's foot preserved at Leyden. An apothecary of Leyden possessed the skin of a pangolin, which Clusius figures under the name of "Lacertus peregrinus spinosus." Though he calls it a lizard, he mentions that some hairs were found on the body; it was not yet known that a few scattered hairs are the infallible mark of a mammal. In the posthumous *Curæ posteriores* (1611) he tells how Johannes van Ufele, a traveller in Brazil, showed him a book of pictures of Brazilian animals and plants, coloured after nature, and copied for him the drawings of the male and female papaw, which are reproduced as woodcuts. Clusius helped to spread the potato-plant in Flanders, Austria and Germany; the question of the first introduction of the plant into Europe, about which much has been written, is too complicated for discussion in this place.[1] In co-operation with Busbecq and others, he made the horse-chestnut, the lilac, the mock-orange, the tulip and the common laurel, then called "the plum of Trebizond," known to the gardeners of Europe.

The scientific gain which accrued from the multitude of new species was not really so great as it appeared to be. So vast and sudden an accession of facts overpowered rather than strengthened the infant studies of zoologists and botanists. Until the *Systema Naturæ* of Linnæus

[1] The facts are recited by Dr. Daydon Jackson in the *Gardener's Chronicle*, Mar. 17 and 24, 1900.

appeared naturalists had not even pigeon-holes ready to receive the new species. Ray, in the generation next before Linnæus, found it impossible to arrange the new fishes and plants which poured in from America alone. Even the subject of geographical distribution, though more manageable than most branches of biological inquiry, could not be investigated to real profit. Europe was as ill-prepared to grasp the new opportunities of enlarging the knowledge of terrestrial life as politically and morally ill-prepared to use her conquests in Mexico and Peru to the lasting advantage of mankind. The naturalists of Europe were untrained, and training was hardly to be had. Here and there a man like Swammerdam might show how fruitful is the close study of a few well-chosen animals and plants, but the lesson was little heeded. Collectors went on loading their cabinets and folios with ill-described and ill-understood objects, seldom attempting close comparisons of distinct forms, or investigating internal structure, or framing instructive generalisations. It was not till the age of Buffon that comprehensive and daring questions were raised in earnest, and that the new sciences of geology and palæontology began to enforce the pregnant thought that facts unintelligible on the theory of sudden creation might receive an explanation from long-continued development. A new race of travellers (Pallas, Humboldt, Robert Brown and Darwin) appeared, who cared less about making collections than about the acquisition of new ideas and the solution of problems. Even then it was only the few who could restrain the passion for mere acquisition. The infinite wealth of natural facts is to this day an impediment to all naturalists except the few who are content to remain ignorant of many things in order that they may learn what is best worth knowing.

SECTION III. SOME EARLY ENGLISH NATURALISTS AND A CONTEMPORARY FRENCH AGRICULTURIST

WILLIAM TURNER

1510?–1568

Libellus de re herbaria novus. 4to. Lond. 1538. Reprinted in facsimile, with notes and life, by B. Daydon Jackson, 1877.

Avium præcipuarum, quarum apud Plinium et Aristotelem mentio est, brevis et succincta historia. 8vo. Coloniæ, 1544. English translation by A. H. Evans. 8vo. Camb. 1903.

A New Herball, etc., Fol. Lond. 1551. The second part. Fol. Collen (Cologne). 1562. The third part. Fol. Lond. 1568.

Englishmen took no part in the revival of botany and zoology, any more than in the invention of printing, engraving and other useful arts, but were during many years content to imitate as well as they could the example of more advanced countries. Such backwardness might be attributed to intellectual apathy, were it not for the great things accomplished by Englishmen in the same age. The maintenance of the first place among the Protestant powers, the establishment of a maritime strength able to contend with Spain in all seas, and the glorious Elizabethan literature sufficiently attest the vigour of our forefathers during that memorable time.

At last Englishmen began, one by one, to study the natural productions of their own country. It was long before any of them achieved eminence; John Ray, in the latter half of the seventeenth century, was the first who could be compared with the best naturalists of Flanders, Germany and Switzerland.

Turner was a fellow of a Cambridge college, who had, under the influence of Ridley and Latimer, become a stout Protestant. While still at the university he studied botany, and put forth his *Libellus*, which gives the Greek, Latin and English names of all the plants which he knew. "As yet," he explains, "ther was no Englishe herbal but one, al full of unlearned cacographies and falselye naming of herbes" (the *Great Herbal*). He was soon afterwards imprisoned for preaching without a licence. When set free he went abroad, studied botany and medicine under Luke Ghini at Bologna, visited Gesner at Zurich, and botanised along the Rhine. On the accession of our Edward VI. he returned to England, and found employment as chaplain, physician and botanist. He was made dean of Wells in 1550, but was forced to flee again to the continent on Mary's accession. At her death he recovered his deanery, but fell into trouble again in 1564, being suspended for nonconformity in the use of vestments. He died in London in 1568.

Turner's literary activity was chiefly exhibited in religious controversy. His *Herball*, though now interesting to the student of the English language, did nothing for scientific botany. The arrangement is alphabetical, under the Latin or Greek names, much space is devoted to the virtues and properties of the plants, and more than three-quarters of the figures are borrowed from Fuchs. Turner's best work in natural history was his history of birds. The primary object of this book was the determination of the birds named by

Aristotle and Pliny, but its whole value lies in the observations made in England. "It is not too much to say" (we quote from Mr. Evans) "that almost every page bears witness to a personal knowledge of the subject, which would be distinctly creditable even to a modern ornithologist." The following passages, quoted from Mr. Evans' translation, show Turner at his best:—

"There is a certain bird which Englishmen call Creeper, that is Climber, for it always climbs about on trees: this I believe to be the Certhia. It is a little bigger than the Regulus, having a whitish breast, the other parts dull brown, but varied with black spots; its note is sharp, its beak is slender and is slightly hooked towards the tip; it never rests, but is for ever climbing up the trunks of trees after the manner of the Woodpeckers, and it eats grubs, picking them from the bark."

"I know two sorts of Kites, the greater and the less; the greater is in colour nearly rufous, and in England is abundant and remarkably rapacious. This kind is wont to snatch food out of children's hands, in our cities and towns. The other kind is smaller, blacker, and more rarely haunts cities. This I do not remember to have seen in England, though in Germany most frequently."

JOHN GERARD

1545–1612

Herball, or Generall History of Plantes. Fol. Lond. 1597.

The memory of Gerard, the English botanist of the period who is most read and quoted, is tarnished by unscrupulous borrowing. His *Herball* was re-edited by Thomas Johnson, a London apothecary, who greatly extended and improved it, insomuch that Ray called this edition (Fol. Lond. 1633) "Gerardus emaculatus," *i.e.* cleansed from blots. Johnson admits that Gerard was incompetent, and that his herbal was an elaborate plagiarism; the story has often been told in detail.[1]

Gerard is useful to the botanist and gardener, because he tells us what plants were cultivated in English gardens at the time when he wrote. In turning over such books as Aiton's *Hortus Kewensis*, which give the countries from which our garden exotics come, and the year of first introduction, we continually meet with the date 1597, which means that our first knowledge of the plant as an English garden-flower is drawn from Gerard.

JOHN CAIUS

1510–1573

De Canibus Britannicis. 8vo. Lond. 1570. An English translation ("Of Englishe Dogges") was made by Abraham Fleming, student (4to. Lond. 1576).

This account of the Dogs of Britain, together with chapters on rare animals and plants, on Caius' own books, and on the pronunciation of Greek and Latin, form a small book, of which the dogs occupy only twenty-six pages and a table of breeds.

John Caius (the name is supposed to be a Latinised form of Kay) is now best known as the second founder of a Cambridge college. In his lifetime he was renowned as a physician, who served in succession Edward the Sixth, Mary and Elizabeth, and wrote what is considered the best contemporary account of the sweating sickness.[2]

[1] Dr. Daydon Jackson says (*Dict. Nat. Biog.*) that nearly all Gerard's figures are taken from the *Eicones* of Tabernæmontanus; only sixteen are original.

[2] A Boke or Counseill against the Disease commonly called the Sweate or Sweating Sickness. 8vo. Lond. 1552.

It is of interest to note that after completing his Cambridge course he had studied anatomy at Padua under Vesalius.

The treatise on the Dogs of Britain was written for Conrad Gesner, who would have printed it at once, had not Caius demanded time for revision. Meanwhile Gesner died, and in the end Caius printed his little book independently.

He gives a slight and amusing account of the dogs known in the time of Queen Elizabeth, supplying such information as a country gentleman fond of field-sports might pour out in the course of conversation.

The table of British dogs is here quoted in a simplified form :—

First come the Generosi, or well-bred dogs :—

Venatici (hunting dogs)	Terrare (terrier). Harier (harrier). Bludhunde (bloodhound). Gasehunde (greyhound). Leviner or lyemmer. Tumbler.
Aucupatorii (fowling dogs)	Spainel (spaniel). Setter. Water-spainel or fynder.
Delicati (pet dogs)	Spainel-gentle or comforter.

The lower-class dogs follow :—

Rustici (farm dogs)	Shepherdes dogge (sheep-dog). Mastive or bandedogge.
Degeneres (mongrels)	Wappe. Turnspete. Danser.

The following names of dogs occur in the text, but not in the table :—otter-hound, lurcher, brach ; the bull-dog, beagle, pointer and retriever are not mentioned in either.

The *bloodhound* was in Caius' day regularly employed in tracking cattle-lifters. Hector Boece (1527) says :—"the samin ar richt frequent and rife on the bordouris of Ingland and Scotland."

The *gasehound* or *gazehound* has been supposed to be an old English greyhound,[1] which has perhaps disappeared by the steady selection of improved varieties. Caius tells us that it sought its prey by sight, not by scent, that it was more used in the northern than in the southern counties, and more in open country than in woodlands; lastly, that it was more often followed on horseback than on foot.

The *lyemmer, limer*, or *lime-hound* (Fr. *limier*) was a dog led in a *lyam*, or leash.

The *tumbler*, according to Caius, was known by its trick of turning suddenly and seizing its prey in the mouth of the burrow. He speaks of its artfulness in giving no warning. The tumbler was smaller and slenderer than the harrier, and had more erect ears. Paulinus (*Cynographia curiosa*, 1685) and Riedel (*Tabula generalis*, 1780-4) identify the tumbler with the dachshund.

Of the water-spaniel Caius says that it had long, curly hair, and was used to recover birds hit with the cross-bow,[2] or darts which had missed their mark. The water-spaniel was known to Bewick, who gives a figure of it, but has now been completely replaced by the retriever. (See below.)

The *spaniel-gentle* or *comforter* was distinguished from the Maltese dog, which was very small. Sully used to tell how he found the effeminate Valois, Henri III.,

[1] Caius' Latin name for the gazehound, *Vertragus* or *Vertagus*, survives in the modern French *vautre*, a boar-hound.

[2] The Latin word is *scorpio*, which Fleming mistranslated *venomous worm*, a blunder which has been repeated in some recent books.

with his sword by his side, a cape about his shoulders, a little flat cap on his head, and a basket hanging from his neck, in which were two or three dogs no bigger than your fist. The *comforter* is mentioned in Bewick's *Quadrupeds.*

The name of *bandog* for the mastiff implies that it was often tied up. Caius calls it "villaticus seu *catenarius.*" The mastiff was used to guard flocks, to hunt the wild boar, to keep swine from straying, to bait bulls, and to draw water from wells; it was also made into a beast of burden, or chained up as a watch-dog. The loyalty of mastiffs is praised, and we are expected to believe that they were so intelligent as to gather the embers together with their paws, so as to keep the fire from going out, or to heap ashes over them when the flame was too fierce. Bewick's bandog was a small mastiff.

Caius' mongrels were dogs of no particular breed, which had been taught to bark at strangers, to turn spits, or to dance to a tune.

Wappe or *wappet* is the name of a mongrel kept for giving warning by its bark. Caius derives *wappe* from *wau* (our bow-wow?).

The *turnspit* had a long body and crooked legs, but these peculiarities are found in dogs of more than one breed. Bewick (1790) still retains the turnspit among the British dogs, but says that its services were little valued.

We shall notice next the omissions from Caius' table, passing over the otter-hound, which, though not included in the table, is mentioned in the text as a dog that pursues the otter.

The *lurcher* ("canis furax" of Caius) was, he says, a dog that hunted rabbits by scent and did not bark; it was used by night, probably by poachers. Youatt says that the original lurcher was a cross between the grey-hound and the sheep-dog.

The *brach* has been defined as a deerhound (Nares' *Glossary*) or any dog that hunts by scent; according to Caius, brach was not the name of a breed, but of a hunting bitch.

The four dogs which follow are neither named nor described in *The Dogs of Britain.*

There is no hint of a *bull-dog*, and in Caius' day bulls were baited by mastiffs. The poet Gay, who had written on rural sports, speaks in his *Fables* (1726), not of the bull and the bull-dog, but of the bull and the mastiff.[1]

The *pointer* is believed to have been unknown in England until 1688; no such dog is mentioned by Caius. Darwin[2] says that the English pointer changed greatly during the hundred years before 1859, chiefly in consequence of crosses with the fox-hound.

The *beagle* was perhaps reckoned by Caius as a harrier.

The *retriever* is believed to be a cross-breed, first produced in the nineteenth century. The name how-ever is old. Juliana Bernes (*Book of St. Albans*, 1486) says:—"if ye have a chastysed spanyell that wyl be rebuked and is a *retriever*," &c.

Caius has not a word to say about the origin of the breeds of dogs. When such questions were raised, in the eighteenth century, the breeds were supposed to be either independent species, or hybrids.

[1] Quoted from *The Farrier* (1828) by Darwin, *Animals and Plants under Domestication.* R. B. Lee says in *Modern Dogs* that the bull-dog is first mentioned in 1631, when Prestwich Eaton wrote from St. Sebastian to London for a mastiff and two good bull-dogs.

[2] *Origin of Species*, chap. i.

The weapons mentioned by Caius as used in hunting are the cross-bow, the javelin and the arrow.[1] The net was used in fowling in this way. The setter, when he found the game (partridges or quails), crouched and lay down, showing the direction of the birds with his foot;[2] then it was the business of the fowler to draw his net over them. This done, the setter roused the birds, which got entangled in the net. Ferrets were used to drive rabbits from their burrows.

We find a few dog-stories taken from books. Caius quotes from Froissart the story of a greyhound belonging to Richard II., which greeted the Duke of Lancaster and thereby gave a forecast of his master's fate; he tells also how Henry VII. ordered the mastiffs to be hanged which had dared to attack the lion, the king of beasts.

THOMAS MOUFET

1553-1604

Insectorum sive minimorum animalium Theatrum: olim ab Edoardo Wottono, Conrado Gesnero, Thomaque Pennio inchoatum: tandem Tho. Moufeti Londinatis opera sumptibusque maximis concinnatum, auctum, perfectum, &c. Fol. Lond. 1634.

Moufet[3] was the son of a Scotch tradesman settled in London. He studied medicine under Caius at Cam-

[1] Fifty years later (1621) Burton's *Anatomy of Melancholy* shows that bows, arrows and javelins had then disappeared, the gun taking their place. "Fowling is more troublesome, but all out as delightsome to some sorts of men, be it with guns, lime, nets, glades, gins, strings, baits, pitfalls, pipes, calls, stalking-horses, setting-dogs, coy-ducks, &c. or otherwise." Pt. II, sect. ii, memb. 4.

[2] "Pedis indicio locum stationis avium prodit: unde canem indicem [pointer] vocare placuit." Willughby's *Ornithology*, where the training of a setter is described, after Markham and others, makes no mention of pointing with the foot, but says that when the dog "standeth still and waveth his tail, looking forward as if he pointed at somewhat, be sure the Partridge is before him."

[3] The name is variously spelt, and is no doubt the same as Moffat.

bridge, and afterwards abroad. He practised in Ipswich and in London, and was patronised by the Earl of Pembroke, who made him member of parliament for Wilton in 1597. Gesner, who was overpowered by the impossible task of describing animals and plants of every kind, had obtained the assistance of Thomas Penny, who laboured to complete a sketch of a History of Insects made by Wotton. At Penny's death, his notes, untidy and full of erasures, were handed over to Moufet, who in turn died before the work was published. Moufet's manuscript lay long in the possession of Sir Theodore Mayerne, who at last produced in 1634 the belated treatise. Mayerne was a French protestant, who had been physician to Henry IV., king of France. He had incurred the hatred of the Galenists by using chemical drugs, among others calomel. These troubles probably drove him to England, where he became physician to James I. and afterwards to Charles I.

Though Wotton, Gesner, Penny and Mayerne all contributed to the book, it possesses little value. The structure, life-histories and classification of insects are handled without real knowledge, and the authors trusted mainly to what they could find in the books of the learned. The coarse woodcuts are mostly unnamed. Martin Lister[1] in a letter to Ray, criticises the confused arrangement of Moufet's matter, and still more severely his transference of information from Aldrovandi, who is not once named.

Those who care to occupy themselves with Moufet's literary gifts will find a favourable specimen in the thirteenth chapter of the second book, where he discourses upon the virtues of spiders. Of his inability to distinguish between a true and a false narrative one

[1] *Correspondence of John Ray*, 1848, p. 12.

proof will suffice. He relates that on Feb. 24th, 1574, so great a multitude of cockchafers fell into the Severn that the water-wheels were choked. The mills would have been blocked to this day (etiamnum hodie), but for the exertions of men, aided by fowls, ducks, nightjars, sparrowhawks and bats.

A curious word or phrase, and at long intervals a fact which is both credible and worth preserving, ill repay the reader's exertions. We find (Chap. I.) *been*, the old plural of *bee*. Trying to prove that insects need not be contemptible merely because they are small, Moufet has recourse to the singular argument that Drake, though a little man, was more than a match for the biggest of the Spaniards.[1] It would be hard to mention any more valuable information which the book yields to a modern naturalist than the statement that a Spanish galleon captured by Drake was overrun with cockroaches.[2]

Mayerne's dedication is livelier reading than Moufet's text. He dwells upon the wonders of the insect-world with considerable animation. No doubt his disposition was trustful, for he accepts such statements as that the cicadas are fed on dew, and that the scarab rolls its pellet of dung for a whole lunar month, following the course of the sun all the time. There are better things than these, however, in the dedication. Mayerne explains, quite truly, that the green grasshopper chirps by rubbing its wing-covers together, and that its stomach is armed with teeth. We find an interesting mention of glass lenses, which had already been used to demonstrate structural peculiarities of the flea, the movements of the heart and blood in the louse, and the head and feet of the itch-insect.

An English translation of the *Theatrum* is given as

[1] Preface. [2] P. 138.

an appendix to Topsell's *History of Four-footed Beasts and Serpents* (folio. Lond. 1658). Topsell's *History* is an adaptation of Gesner for English readers

CHARLES BUTLER

1569 ?–1647

The Feminine Monarchie ; or a treatise concerning Bees and the due ordering of them. 8vo. Oxford. 1609.

The Feminine Monarchy, or the History of Bees, written out of experience, &c. Third edition. Sm. 4to. Oxford. 1634.

The first account of the economy of the bee-hive which we shall have to notice is Butler's *Feminine Monarchy*, a learned, practical and amusing treatise. Thomas Hill's *Profitable Art of Gardening* (4th ed. Lond. 8vo. 1568) contains a chapter on the ordering of bees, but Hill explains in his preface that he had no practical knowledge of bees; he merely collected statements and opinions from ancient authors.[1]

Butler was parson of Laurence Wotton, near Basingstoke, and master of Basingstoke grammar-school. He wrote other books besides the *Feminine Monarchy*, a treatise on rhetoric, a treatise on consanguinity in marriage, an English grammar with a new phonetic spelling, and a book on the principles of spelling. The *Feminine Monarchy* seems to have achieved success; it reached a third edition, and a Latin translation was published in 1673. I have quoted from the third edition, which adopts the phonetic spelling advocated

[1] Butler explains in his preface that Georgius Pictorius had collected passages about bees from ancient authors, and that one T.H. (Thomas Hill) of London had translated these into English, concealing the author's name. "These and the like, when a scholar hath thoroughly read, he thinketh himself thoroughly instructed in these mysteries, but when he cometh abroad to put his learning in practice, every silly woman is ready to deride his learned ignorance."

in the author's English Grammar. Some readers will value this edition all the more because it contains complimentary verses by George Wither. One sentence in the book shows Butler's politics: "The bees abhor as well Polyarchy as Anarchy, God having shewed in them unto men THE MOST NATURAL AND ABSOLUTE FORM OF GOVERNMENT."[1]

Butler looks up to Aristotle as the chief authority on natural history, though he does not hesitate to correct even Aristotle when there is cause. Aristotle, while admitting that the case was not clear, had called the supposed governor of the hive the *king-bee*; Butler insists that in this community the males bear no sway at all; it is an Amazonian or feminine monarchy. He does not realise that the queen is under normal conditions the mother of the entire family.

Of the drones he says that they are found in the hive during the whole breeding season and then only; they are bred and reared by the workers. Wasps and "dors" (humble-bees) have their drones, as well as hive-bees.

The workers, which he calls the "honey-bees," lay all the eggs from which drones or workers are hatched.[2] In summer only "they suffer their drones among them for a season, by whose masculine virtue they strangely conceive and breed for the preservation of their sweet kind." Proof that the drones are males was of course unattainable as yet. No anatomical investigation of the different inmates of the hive had been made, and

[1] Jerome Cortes (*Libro y Tratado de los Animales*, 8vo. Valencia, 1613, p. 452) practised the same flattery before Butler, as did Joseph Warder (*The True Amazons, or the Monarchy of Bees*, 12mo. Lond. 1712) after him.

[2] Moufet, who died in 1599, had also maintained that the small bees are females, and the drones males (*Theatrum*, p. 13).

nearly two hundred years had still to run before the mating of the queen with the drone became a demonstrated fact. Butler admits (p. 55) that the drones had not been seen to engender with the workers.[1]

The queen, he says, is "perhaps of no kindred with the drones or workers." He seems to have imagined that the offspring of the queen are all queens,[2] but here he leaves much unexplained. Having remarked that the cells are of different sizes, and that they are completed before any eggs are laid in them, he infers that the prolific female (not the queen, but the worker, according to his view) enjoys a peculiar gift; she knows whether she is about to lay male or female eggs, and chooses the cells accordingly.

Butler thinks that the queen is assisted by "subordinate governors and leaders," which are distinguished by a crest, tuft, tassel, or plume, of yellow or murrey colour, turned up in some, down in others. Pollen-grains, clinging in strings to the heads of bees, as they often do, no doubt gave rise to this fancy. There is a hint of some belief of the same sort in Aristotle.

A tolerable account of the structure of the hive-bee is given. The compound eyes are recognised as organs of sight, and the "fangs" (mandibles) and "tongue" (proboscis) are briefly described. The "horns" (antennæ) are said to be used for feeling. We are told that no brain is to be found in the head! The sight of bees, he thinks, is poor, their sense of smell excellent; hearing, feeling and taste they no doubt enjoy. Stinging, he says, is present death to the bee which inflicts the wound—rather too strong a statement of the case.

[1] For Milton's theory of the bee-community, which may have been founded on Butler, see *infra*, p. 186.

[2] Yet he says:—"if the old queen bring forth many princes (as she may have six or seven, yea sometimes half a score or more, &c.)." P. 4.

Among the implements which he recommends to the bee-master is a drone-pot, that is, a weel made of wire, and used like a lobster-pot. The drone-pot is set at the door of the hive, and is so constructed that the drones can enter it, but cannot leave it again. In this way drones can be caught and killed, whenever it is necessary to prevent waste of the store of honey.[1]

Butler, being master of the art of music, as of many other arts, attempts to set down the song of the bees when busy in their hive. He tells us that he pricked down the bees' music with the help of a wind-instrument, but confesses that his dull hearing could not perfectly analyse the confused noise of the buzzing bees, and that he was obliged to make out the conclusion as best he could. He assigns the treble part to the princesses, the bass to the queen, and tries to show how the inner parts are supplied. The glee in four parts and triple time, which he prints as the Bees' Song, must not, of course, be taken for a real transcription of the sounds of the hive; it would be as reasonable to believe that the bees composed the verses to which the song is set. In the engraved music the bass and counter-tenor parts are printed upside-down, so that four singers, each holding his own corner of the book, may sing away together.

Bees may be seen, says Butler, to blow liquid wax from their mouths. Sometimes they do it in such a hurry as to drop the wax in the form of loose white scales on the stool or the "skirts" of the hive; these scales, when warmed, can be kneaded into pellets. Butler was on the way to discover how wax is secreted, but was too impatient to work the matter out properly.

The hexagonal cells (Butler does not call them *hexagonal*, but *six-square*, a convenient English word, which

[1] Pp. 47, 66.

we have unwisely dropped)[1] are thought to be due to the fact that bees have six feet. He notes some of the advantages of the six-sided prism, though not its strength nor its economy. He describes the pyramidal ends, and the way in which every cell adjoins three cells of the opposed layer.

Bee-bread, such as the workers bring home on their hind feet, was then popularly believed to be some kind of wax; Butler, however, calls it "gross honey," or "gross leg-honey," and explains that it has a sweet taste, while it does not melt with heat.[2] Nothing was then known of the use of bee-bread in feeding the larvæ. Liquid honey, he rightly explains, the bees collect with their tongues, and "let it down into their bottels." Virgil (who in this follows Aristotle) misleads Butler on one point, making him say that the purest nectar comes from above ("aera mellis cælestia dona"). This aerial honey is of course honey-dew, concerning which naturalists knew nothing accurately until Réaumur enlightened them.

When he comes to treat of the collecting habits of bees, Butler furnishes some details which show close observation:—"They gather," he says, "on the flowers of the maple a whole month together, and somewhat on the flower of vetch, when his time is, but the greatest store of honey is drawn out of the black spot of the little picked (piked, or pointed) leaf (stipule) of the vetch, which groweth on each side of the two or three uppermost joints. These they ply continually: I never saw vetches, how far soever from hives, that for three months together (if the weather served) were not full of bees.

[1] The same usage lasted through the eighteenth century. Wesley's *Journal* makes mention of rooms or buildings which were *three-square*, *eight-square* and *twelve-square*.

[2] P. 106.

Beans also, which with their flowers have also black-spotted leaves as well as the vetches, on which sometimes they gather."[1]

"The bees gather but one kind of flower in one voyage."

"And in this great variety this is strange that where they begin they will make an end, and not meddle with any flower of other sort until they have their load." Here he quotes Aristotle (*Hist. Anim.* IX, 27). "Insomuch that those which begin with the flower of the vetch will not once touch the rich spotted leaf of the same before they have been at home. Although when they come to a flower that yieldeth both nectar and ambrosia, they will use sometimes the tongue and sometimes the fangs, and gather them both."[2]

These observations, which were confirmed by Ray,[3] appear to be the first mention of extra-floral nectaries.

The making of mead and metheglin is carefully described. In Butler's day no yeast was deliberately added, though the diluted honey was often drawn into a beer-barrel. He notes the formation of a "mother" (impure pellicle of fermentative micro-organisms) on the liquor and the proneness of the mead to pass into vinegar. Mead is still made on the old plan in secluded parts of England. Not long ago, in the dense beech-woods of Buckinghamshire, and only thirty miles from Cornhill as the crow flies, I saw a man and his wife making mead just as Butler used to make it.

Butler thinks honey much better than sugar, and tells how to make with honey, marmalade of quinces, marchpane, and other sweetmeats which bear delicious names.

[1] Pp. 109-10. [2] P. 112.

[3] *Cat. Plantarum circa Cantabrigiam nascentium*, pp. 175-6 (1660).

It will be seen that the reader who spends a day or two upon the *Feminine Monarchy* may expect other pleasures than those of natural history. We now and then come across forgotten English phrases and words, *e.g.* "it is also convenient for each hive to have *his* settle before *him*;"[1] the bees are said to return to the hive *leere*,[2] &c. Butler's lively expression often charms the reader, as in the following passage:—"There remaineth yet another enemy worse than all these; for these all do wrong the bees but by little and little, some in their goods, some in their persons, and there is remedy shewed, if industry be not wanting, against them all. But this," he means the stealer of hives, "when he cometh, playeth sweepstake with them, carrying away both honey and wax and bees and hive, &c."[3]

OLIVIER DE SERRES

1539–1619

Le Théâtre d'Agriculture et Mesnage des Champs, d'Olivier de Serres, seigneur du Pradel . . . Nouvelle edition . . . publiée par la Société d'Agriculture du Département de la Seine. 2 vols. 8vo., Paris, Ans XII-XIV [1804-5].

Though the name of De Serres is rare in English books,[4] he is still fondly remembered by Frenchmen. His chief title to fame is that he did so much to spread the silk-industry in France, but he is also honoured as the author of the *Théâtre d'Agriculture*, which gives a lively picture of country life in Languedoc during the latter half of the sixteenth century.

[1] So too in the *English Grammer* we have the verb and "*his* cases"; but *its* also occurs.

[2] *Leer* (empty) is still used by the peasants of Devonshire.

[3] P. 137.

[4] Arthur Young (*Travels in France*) speaks of him with enthusiasm.

De Serres was prominent among the Protestants of the Vivarais (the modern Ardèche). The *Théâtre* shows that besides being a seigneur who tilled his own lands, he was a wide reader and a practised writer.

Faujas de Saint-Fond,[1] writing in 1802, describes the large and beautiful meadow which gave the name of Le Pradel to Olivier de Serres' estate, a league distant from Villeneuve-de-Berg. It sloped gently from the chateau, was watered by a spring issuing from its upper end, and fringed by a triple row of fine oaks. In the background rose a volcanic mountain, crowned with basaltic columns. The chateau had been thrice fired during the wars of religion, and only one of its towers was standing in 1802. All the trees which De Serres had planted had been cut down, and little remained of his improvements except the fragments of an aqueduct made for watering his gardens and turning his mills. The house still stands, though much altered.

The *Théâtre* opens with a discussion on soils. Some of the indications of fertility are taken from the *Georgics*. De Serres prefers a mixture of plain, hillside and mountain, and praises as "bon et beau" a site which not a little resembles his own domain of Le Pradel.

De Serres is far from vulgar superstition; if he collects old saws about lucky and unlucky days, it is only to laugh at them. Nevertheless he does not reject the prevalent opinion that the heavenly bodies exert an influence upon the affairs of men. The moon especially, as the nearest of them, may, he thinks, affect the weather and the growth of plants and animals. The

[1] An early geologist of great mark (1741-1819), who described the trachytic mass of the Mézenc (*Recherches sur les volcans éteints du Vivarais et du Velay*, 1778), the basalt of Fingal's cave, &c.

tides ebb and flow under the influence of the moon; plants turn towards the sun and are fostered by his rays. Accordingly he would not set at naught the belief that timber-trees must only be felled when the moon is waning, lest he should thereby endanger the safety of his buildings. But he thinks it foolish to wait for a change of the moon when the weather is favourable to the operations of the farm. Diligence to seize opportunity by the forelock is better than all forecasts.

"Que l'homme estant par trop lunier
De fruicts ne remplit son panier."

Science, experience and diligence is the motto of the *Théâtre*, *science* meaning knowledge, and especially book-knowledge. De Serres is careful to show that while book-learning may be futile, experience may be unenlightened, and he trusts neither alone. Hence, while he enforces Virgil's advice:—"Laudato ingentia rura, exigua colite," or quotes Hesiod upon the difference between the neighbour who rushes barefoot to help a friend in trouble and the neighbour who waits till he has finished dressing, he does not disdain to offer bits of practical advice in homely rhymes, *e.g.*

"Celui son bien ruinera
Qui par autrui le manira;" or this,
"On dit bien vray, qu'en chacune saison
La femme fait ou défait la maison."

There is set before the reader a pleasing mixture of history, literature and practical good sense. Obsolete phrases often give a rustic flavour to the book, as here:—"Plus rare present ne pourriez-vous faire à vos amis que de fruits exquis: voire les plus grands seigneurs ont accoustumé de recevoir humainement le plein panier d'abricots bien choisis, et la douzaine de poires ou prunes de remarque que l'homme vertueux leur offre, tant petit soit-il."

The chapter on silk-culture begins with the ancients, telling how Virgil believed that silk grew on trees, how Pliny, though he knew that silk was spun by insects, supposed that the insects were generated from fallen flowers,[1] how under the emperor Aurelian silk was worth its weight in gold, how two monks brought silkworm eggs from Cathay to Byzantium in the reign of Justinian,[2] and how from this time silk-culture gradually spread through the Mediterranean countries.

De Serres traces the introduction of the silkworm and the mulberry-tree into France to the return of Charles VIII from Italy (1494).[3] By 1600 the industry was well-established in Provence, Languedoc and some of the neighbouring provinces, and had been attempted in Touraine, the Orléannais and Normandy. In 1554 an edict had been put forth to encourage the planting of mulberries. De Serres himself had reared silkworms and collected their cocoons for thirty-five years when he began to write about them.

In 1599 Henri Quatre, having remarked that imported silk and silk-stuffs cost the French people "plus de quatre milions d'or" every year, became eager to spread silk-culture in his own dominions. De Serres was invited to report on the subject, and having his *Théâtre* ready for the press (it was published in the following year) he extracted from it one chapter, which was printed as a little 8vo pamphlet of about a hundred pages, called *La Cuillete de la Soye*. A little later, in 1605, Laffemas,

[1] De Serres says *leaves*.

[2] It was only then, that is, in the sixth century A.D. that Europeans came to know that silk is spun by caterpillars which feed on the mulberry-tree.

[3] Earlier dates are also quoted. The first pope who resided at Avignon, Clement V, is said to have introduced them in 1305.

the minister of Commerce, aided the same cause by writing a manual of mulberry-planting and silkworm-feeding for the use of the clergy. Good advice was not the only means employed, for the king prohibited the importation of silk-fabrics, and ordered the white mulberry to be planted in all his gardens. From fifteen to twenty thousand were sent to the Tuileries, where a large house was built and fitted up for the rearing of silkworms.

Sully opposed the extension of silk-culture in France, on the ground that it would encourage luxury; his objections were not heeded. Colbert, who found time for every enterprise that concerned the interests of France, attended to the improvement of the silk-industry, and it gradually rose to great importance. In 1780 the value of the annual yield of cocoons was estimated at near a million sterling, and by 1848 the figure had risen to four millions.

De Serres calls silk a miracle of nature, and the silk-worm "animal admirable," lacking as it does flesh, blood, bones, intestines, eyes, ears and other things which are found in nearly all animals. He has a good general notion of the life-history. The abstinence from food of the pupa strikes him as surprising. He enters into many practical details concerning the propagation and management both of the mulberry and of the silkworm. The maladies to which the larva is subject are carefully, though not instructively, described. Superstitious or fanciful usages, such as dipping the eggs into choice wine, hatching them in the bosom of a woman, and sipping wine before handling the larvæ, are mentioned, sometimes with approval; the phases of the moon appropriate to every operation are duly noted. The spontaneous generation of silkworms from the flesh of a calf, a survival of the ancient belief in the generation of bees from the flesh of oxen, is described. De Serres finds no inherent improbability in the story, but calls for experimental proof, which is just what a sensible man might have been expected to do in the year 1600.

The mulberry is not valued in the *Théâtre* solely because it yields food for the silkworm; its culture is strongly advocated on account of its bast, which was reputed to be highly serviceable for cordage and cloth. The branches, collected at the time of lopping, were soaked and peeled; the bast was then bruised, dressed and carded, like hemp or flax, and spun.

De Serres practised the artificial incubation of eggs, which had long been familiar to the Egyptians and the Chinese. He furnishes curious information about recently introduced animals and plants (guinea-pigs, turkeys, maize, beet-root), the sugar-cane, which he thought might be acclimatised, artificial meadows, greenhouses, then a luxury of princes,[1] wind and water-mills, and cisterns made of other materials than stone.

[1] The Orangerie at Heidelberg was already famous. Henri IV had one at the Tuileries, and Louis XIV (long after the date of the *Théâtre*) added many to the gardens of Versailles and the Jardin du Roi.

SECTION IV. RAY AND SOME OF HIS FELLOW-WORKERS

JOHN RAY

1627–1705

FRANCIS WILLUGHBY

1635–1672

Francisci Willughbeii de Middleton in agro Warwicensi, Armigeri, e Regia Societate, Ornithologiæ libri tres . . . Totum opus recognovit, digessit, supplevit Joannes Raius. Sumptus in Chalcographos fecit illustriss. D. Emma Willughby, Vidua. Fol. Lond., 1676.

The Ornithology of Francis Willughby . . . in three books . . . translated into English, and enlarged with many additions . . . to which are added, three considerable discourses, I. Of the Art of Fowling, . . . II. Of the Ordering of Singing Birds, III. Of Falconry. By John Ray. Fol. Lond. 1678.

Francisci Willughbeii . . . de Historia Piscium Libri quatuor, jussu et sumptibus Societatis Regiæ Londinensis editi . . . Totum opus recognovit, coaptavit, supplevit, librum etiam primum et secundum integros adjecit Johannes Raius. Fol. Oxon., 1686.

[Francisci Willughbeii] Historia Insectorum, opus posthumum, 4to. Lond. 1710.

Synopsis methodica animalium quadrupedum et serpentini generis. Auctore Joanne Raio, S.R.S. 8vo. Lond. 1693.

Joannis Raii Synopsis methodica avium et piscium; opus posthumum, &c. 8vo. Lond. 1713.

Catalogus Plantarum circa Cantabrigiam nascentium, &c. 12mo. Cambridge, 1660.

Methodus Plantarum nova, autore Joanne Raio. 8vo. Lond., 1682.

Jo. Raii Historia Plantarum. 3 vols. Fol. Lond. 1686-1704.

Joannis Raii Synopsis Methodica Stirpium Britannicarum . . . Editio secunda. Accessit . . . Rivini Epistola ad Joan. Raium de Methodo: cum ejusdem Responsoria, &c. 8vo. Lond. 1696.

A Collection of English Proverbs, by J[ohn] R[ay]. 8vo. Lond. 1670.

John Ray, the son of a blacksmith, was born at Black Notley in Essex in 1627. He probably showed early talent, for he was sent to the neighbouring grammar school at Braintree, and afterwards, at the cost of one Squire Wyvill, to the university of Cambridge. He became fellow and tutor of Trinity; while still a layman, he was selected to preach before the university, and his discourses attracted some attention. At thirty-five he was not only learned in the ancient literatures, and competent in divinity, but known to some few as a naturalist of great promise. He had gathered about him undergraduates who were fond of natural history, some of them heirs to great estates, and had secured the co-operation of Francis Willughby, the ablest and most zealous of the little company, in a scheme for the methodical investigation of the animals and plants of all accessible parts of the world. He had already traversed most parts of England and Wales, besides the Lowlands of Scotland, in search of rare plants and other natural curiosities, and these travels he was afterwards to extend beyond the seas.

Such were Ray's achievements and prospects when his future was darkened by misfortunes heavy enough to crush a man of no more than ordinary courage and patience. Charles II was restored to the throne of his fathers. The change seemed at first propitious to Ray, who was, what he continued to be to the day of his death, a sincere but moderate churchman. He was ordained by Bishop Sanderson a few months after the Restoration, and was looking forward to an honourable career in church and university when the Act of Uniformity was passed. Ray had no scruples about any of the doctrines or offices of the Church of England. He had never signed the Solemn League and Covenant, which was reprobated by the Act of Uniformity; indeed, he considered it an unlawful oath. But the Act required him to declare that the oath was not binding upon those who had taken it, and this Ray could not in conscience do. No scruples of this or any other kind were respected by the High Churchmen who were then dominant in church and state. Ray held no benefice, but he was turned out of his fellowship; Cambridge and the church were closed to him; he was deprived of his livelihood, and forced to seek a new calling.

Henceforth Ray's life was, in the main, a life of poverty and seclusion. For some years he was supported by his wealthy associate, Francis Willughby, whose children he helped to educate, but Willughby died in 1672, and then Ray was compelled to serve as a tutor under less agreeable conditions. At the age of fifty-two he returned to his native village, to subsist upon a small pension bequeathed to him by Willughby. During the last twenty years of his life he was often kept close to the fireside by painful sores upon his legs. He continued to write and publish to the very last. His works were highly valued by those who could judge, and some of them passed through several editions. But the publishing trade was then very imperfectly organised; an author had usually to be satisfied with whatever the bookseller chose to allow him, and Ray was probably wretchedly paid for his labours. At his death in 1705 he had less than forty pounds a year to bequeath to his widow and daughters.

I have found no single passage in which Ray speaks reproachfully of his persecutors. He seldom mentions his sufferings at all, and then uses particularly calm expressions. In the preface to his *Wisdom of God in the Creation* he explains that "being not permitted to serve the Church with my Tongue in Preaching, I know not but it may be my Duty to serve it with my Hand by Writing." After the Revolution he had the satisfaction of congratulating himself and his readers upon the triumph of liberty, the purification of religion, and the restoration of the ancient laws of England, but his solemn thanksgiving makes no mention of private injuries.[1]

The most intimate of Ray's friends was Francis, only son of Sir Francis Willughby of Middleton Hall in Warwickshire and of Wollaton, near Nottingham. The hall at Wollaton, built in 1588, is one of the noblest examples of English domestic architecture. When an undergraduate at Cambridge Willughby came under the influence of Ray, joined him in his journeys, and helped him to frame liberal schemes for the advancement of natural history. After Ray was forced to leave Cambridge, the two friends determined to visit foreign countries together. Adding to their party two pupils of Ray, they travelled through the Low Countries, Germany, Switzerland, Bavaria, Italy, Sicily and Malta. Three of them returned through Switzerland and France, while Willughby protracted his tour by a visit to Spain. Ray tells us that his friend had designed a voyage to the new world in order to perfect his *History of Animals*, but did not live to undertake it. The arrangement of their collections, fresh journeys in the British Isles, and experimental researches engaged the two naturalists for several years more, when Willughby was cut off by a sudden illness.

Ray was made one of the executors under Willughby's will, and charged with the education of two sons. Sixty pounds a year (the amount was afterwards slightly increased) was bequeathed to him for life, and this was henceforth his principal livelihood. He married a young woman resident at Middleton Hall, who helped him with

[1] Preface to the *Synopsis Stirpium*.

the children. Lady Willughby, the mother of Francis, seems to have promoted in every way the plans of her late son. For three or four years Ray was continued in his office as tutor, and money was found for the publication of the first of the unfinished memoirs which Willughby had left behind him. Then Lady Willughby died, Ray gave up his tutorship, and the Willughby connection seems to have been broken off altogether, except that Ray's pension was still paid.[1]

The indefatigable Ray, though now reduced to a humble way of life, determined none the less to complete by himself the vast enterprise to which he had set his hand. Willughby had left behind him an imperfect *Ornithology*, an imperfect *Ichthyology*, and many scattered observations on insects. All of these Ray contrived to publish, having first completed them to the best of his power. Nor did he neglect the share of the work originally assigned to him, which was the description and arrangement of the plants, not only of Britain, but of all countries hitherto investigated.

WILLUGHBY'S ORNITHOLOGY

Ray first undertook to revise and complete the *Ornithology*, making it his main design to describe accurately each species. "We ourselves," he claims, "did carefully describe each bird from the view and inspection of it lying before us." He finds fault with Willughby's excessive minuteness. "Now though I cannot but commend his diligence, yet I must confess that in describing the colours of each single feather he sometimes seems to me to be too scrupulous and particular . . . yet dared I not to omit or alter anything."[2] He

[1] Ray's *Synopsis Stirpium* (1690) is dedicated to the surviving son of Francis Willughby.

[2] Preface.

owns the obligations of the *Ornithology* to Gesner and Aldrovandi, but claims to have corrected many of their mistakes, such as the making of two or three species out of one. Willughby had made a collection of pictures of birds, and caused various species to be drawn for him by good artists; from these and published figures a selection was made which Ray thought were the best and truest hitherto engraved. Neither the sources of the figures nor the scale is indicated.

Birds of all countries are included. The measurements and the weight of the bird are often given, and a careful note is made of its external features. There are usually rough memoranda concerning the internal anatomy (crop, gizzard, intestine, cæcal appendages, gall-bladder, trachea, &c.), and the contents of the stomach are sometimes mentioned. The eggs of many of the birds are shortly described. The species is identified, if possible, with some species of earlier authors, and a large part of the history is sometimes condensed from Aldrovandi, Gesner, Clusius, Jonston, Belon, Olina, Turner, &c. There are also descriptions of foreign birds taken from Marcgraf (birds of South America), Bontius (East Indies), and others. The uses of the bird in medicine, cookery, falconry, &c., are noted. Harvey or Malpighi may be quoted on some point of anatomy or physiology; some trustworthy friend may furnish a description or a note of occurrence. The localities mentioned show a wide acquaintance with British topography, and a singular curiosity (in Ray, no doubt) concerning places, names and odd facts of every kind. The popular English names of the birds are given, and now and then names that have some etymological interest. For instance, the following names for the woodpecker are quoted:—woodspite, pickatrees (N. of

England), rain-fowl, highhoe or heghoe, hew-hole, wit-wall, hickwall. The list might be greatly lengthened; in Wright's *English Dialect Dictionary* the woodpecker figures under almost every letter of the alphabet. *Wierangle* (Germ. *Werkengel*) is a name for the shrike, *flusher* (*flesher*?) for the lesser shrike. The redwing used to be called a *windthrush*, and Dr. Charleton, once known as the author of the *Onomasticon Zoicum* (1668), traced the name to a belief that the redwing came over in the equinoctial gales. Ray's knowledge of languages showed him that this was a mistake; the Germans, he says, call the redwing *Wyntrostel* (*Weindrossel*), because it is fond of grapes, and we have adopted the German name, changing its form to suit an erroneous interpretation.[1]

Among the helpers to whom he returns thanks Ray mentions Sir Thomas Brown, the author of the *Religio Medici*, who owned a collection of birds, and had written an account of birds found in Norfolk; Francis Jessop of Sheffield, Philip Skippon of Wrentham in Suffolk, and Ralph Johnson of Brignall in Teesdale.

The fables which are treated so gravely by his predecessors receive little consideration. Gryphons, harpies, phœnixes and rocs find no place in this book. Ray is incredulous about the transformation of barnacles into geese, the renewal of their youth by eagles, the incessant flight of birds of paradise, the wool-bearing fowl, the antipathy between the lion and the cock, the six months' sleep of the humming bird, or the milking of goats by the nightjar. The wonderful tales of Nieremberg are put by themselves in an appendix.

An introductory section treats of the structure,

[1] P. 190. Turner (*Hist. Avium*) quotes the English name as *wyngthrushe*, and the German as *weingaerdsvogel*.

development, and mode of life of birds. The anatomical part is largely taken from Willis and Harvey. The sclerotic ring in the eye is mentioned. We are told of the curious rule of the regular increase in the number of the phalanges from the first to the last digit of the foot, a rule which "hath not as yet been taken notice of by any naturalist, that I know" (p. 3). Mention is made of the exception furnished by the swift, in which "the least toe has one joint, and the other three two joints each, contrary to the manner of all other birds that we know beside it" (p. 214). The muscles of flight are mentioned, and we are assured that if men ever fly, it must be with their legs. The wonderful protractile tongue and the prolonged hyoid of the woodpecker are explained and figured. This structure was afterwards redescribed by De la Hire and Méry, and became a favourite proof of the wisdom of Providence. The double-shafted feather of the cassowary, which had been described and figured by Clusius, appears here again. Under the head of development we find mention of the fact that a fowl's egg cannot be easily crushed by the fingers, if the pressure is applied to the two ends. It is boldly affirmed that all animals, even viviparous animals, proceed from eggs; there was as yet no suspicion that an animal may be *budded forth* from its parent. Twenty-four questions are printed, which Willughby had proposed to himself concerning birds, *e.g.* whether they cast their claws and bills; whether the iris is always of the same colour in the same species; a few of the questions have answers appended to them. There is a brief account of remarkable breeding-places of British birds.

Aristotle had said[1] that while some birds migrated according to the season of the year, swallows did not,

[1] *Hist. Animalium*, VIII, 15.

but had been found hiding, and entirely bare of feathers, in the winter months. This statement may possibly have encouraged Olaus Magnus,[1] or those who prompted him, to publish the ridiculous fable about swallows lying hid at the bottom of lakes and rivers, two together, mouth to mouth and wing to wing. This delusion once set going, false reports were readily produced to support it, and the theory of hibernation did not die out till the end of the eighteenth century. Gilbert White entertained to the day of his death a suspicion that the swallows of Selborne never left the country, but merely went into hiding for the winter. Willughby and Ray attributed the seasonal disappearance of the cuckoo, swallows, fieldfare and woodcock to migration, though they had to admit that the truth or falsehood of the tales about hibernation had not been certainly determined. They say of the nightingale that it does not endure cold well, and either goes into hiding in the winter, or departs for warm countries.

In the seventeenth century the bustard and the crane were still well-known English birds. Willughby and Ray tell us that in their day the bustard frequented the heaths and plains of Cambridgeshire, Suffolk and other parts of the kingdom.[2] Great flocks of cranes were to be seen in summer in the marshes of Lincolnshire and Cambridgeshire.[3]

Seventeen domestic races of pigeons are named and described, several being figured ; Aldrovandi had already given a systematic table of the breeds of pigeons.

Mention is made of the aviary in St. James' Park, an aery of sea-eagles belonging to the Countess of Pem-

[1] *Hist. de gentibus septentrionalibus* (1555). See *supra*, p. 59.

[2] P. 178. The last British-bred bustard was killed in May, 1838.

[3] P. 274.

broke at Winfield in Westmoreland, the menagerie in the Tower of London, the repository of the Royal Society, and the museum of the Tradescants. A decoy for wild ducks is described with the help of rude figures.[1] The snaring of pheasants and the daring of larks are explained, as well as the classification of hawks by falconers.

Willughby and Ray had little sense of the relative value of zoological characters; it gives a sufficient idea of their judgment in this matter that they sometimes divide their birds according to size, colour, the nature of the food, the length of the leg, wildness or tameness. For the mere naming of such species as are not finely graded their divisions and sections, which are accompanied by a key, may have answered pretty well.

They are not careful to mark the birds which are described for the first time, but the Whooper Swan, the Herring Gull and the Black Diver (our Common Scoter) they claim as hitherto undescribed. They note that the trachea enters the keel of the sternum in the Whooper, as previously observed by Aldrovandi, but not in the tame swan.

Willughby and Ray explain in their preface the usage which they adopted in this and other books on natural history with respect to the names of what we now call genera and species. They took little trouble, they say, about nomenclature, and usually followed Gesner and Aldrovandi, being unwilling to disturb accepted names. Their chief care was to make it quite clear what bird was denoted by a particular name. They were indifferent whether the name was Latin or English, of one word or several. Thus we have "Fulica, the Coot," but the

[1] P. 372. More elaborate figures are given in Bewick's *Water Birds* and in Yarrell's *Birds*.

Black Tern is "Larus niger fidipes noster." They followed the practice, then usual, of giving a name of one word to what we now call a genus, or to a species which stands alone in its genus, but a name of two or more words to each of the species in a large genus.

The following is a general view of the classification of Birds adopted by Willughby and Ray.

LAND FOWL

Rapacious Diurnal (include shrikes and cuckoo).
Rapacious Nocturnal (include nightjar).
The Crow Kind.
The Woodpecker Kind (includes wryneck, nuthatch, creeper. hoopoe).
The Poultry Kind (includes the landrail).
The Pigeon Kind.
The Thrush Kind (includes the starling).
Small Birds with slender bills.
Small Birds with thick bills.

WATER FOWL

Cloven-footed (Waders).
Birds of a middle nature between Swimmers and Waders (water-hen, coot, grebe).
Whole-footed (Web-footed).

Some passages are next extracted, which either yield useful information, or illustrate Ray and Willughby's handling.

Lapwing

"It builds its nest on the ground, in the middle of some field or heath, open and exposed to view, laying only some few straws or bents under the eggs, that the nest be not seen. The eyes [eggs] being so like in colour to the ground on which they lie, it is not easy to find them though they lie so open. The young, so soon as they are hatcht, instantly forsake the nest, running away, as the common tradition is, with the shell upon their heads, for they are covered with a thick down, and follow the old ones like chickens. They say that a lapwing, the further you are from her nest, the more clamorous she is, and the greater coil she keeps; the nearer you are to it, the quieter she is, and less concerned she seems, that she may draw you away from the true place, and induce you to think it is where it is not."[1]

Bittern

"They say that it gives always an odd number of bombs [booms] at a time, viz., three or five; which in my own observation I have found to be false. It begins to bellow about the beginning of February, and ceases when breeding-time is over. The common people are of opinion that it thrusts its bill into a reed, by the help whereof it makes that lowing or drumming noise. Others say that it thrusts its bill into the water, or mud, or earth, and by that means imitates the lowings of an ox. It hides itself, commonly among reeds and rushes, and sometimes lies in hedges with its neck and head erect. [at which time the booming sound is uttered.] "In the autumn after sunset these birds are wont to soar aloft in the air with a spiral ascent, so high till they get quite out of sight, in the meantime making a singular kind of noise, nothing like to lowing."[2]

Kingfisher

"It is a vulgar persuasion that this bird, being hung up on an untwisted thread by the bill in any room, will

[1] P. 308.

[2] Pp. 282-3. "Few people in Britain have ever heard its loud and awful voice" (Newton, *Dicty. of Birds*). The last bittern's egg was taken in Norfolk in 1868, and the "boom" was last heard in 1886 (Southwell, *Notes and Letters of Sir T. Browne*, p. 17 n.). Since this note was written the bittern has again bred in Norfolk, as Miss E. L. Turner states in *British Birds*, Sept. 1911.

turn its breast to that quarter of the heaven whence the wind blows. They that doubt of it may try it."[1]

Blackbird

"The blackbird builds her nest very artificially with-outside of moss, slender twigs, bents and fibres of roots, cemented and joined together with clay instead of glue, daubing it also all over withinside with clay. Yet doth she not lay her eggs upon the bare clay, like the mavis, but lines it with a covering of small straws, bents, hair, or other soft matter, upon which she lays her eggs, both that they might be more secure and in less danger of breaking, and also that her young might lie softer and warmer."[2]

Kites

"They are very noisome to tame birds, especially chickens, ducklings and goslings, among which espying one far from shelter, or that is carelessly separated a good distance from the rest, or by any other means lies fit and exposed to rapine, they single it out, and fly round and round for a while, marking it; then of a sudden dart down as swift as lightning, and catch it up before it is aware, the dam in vain crying out, and men with hooting and stones scaring them away. Yea, so bold are they that they affect to prey in cities and places frequented by men, so that the very gardens and courts or yards of houses are not secure from their ravine. For which cause our good housewives are very angry with them, and of all birds hate and curse them the most."[3]

WILLUGHBY'S HISTORY OF FISHES

Beyond the statement on the title-page that the first and second books, which treat of fishes in general and of

[1] P. 146. [2] P. 191.

[3] P. 75. For an earlier account of kites in London see p. 78.

the cetaceans, were written by Ray, we have no distinct indication how much of the book is Ray's, how much Willughby's. Here, as in the *Ornithology*, we can only guess whose are the thoughts and statements which we have before us.

The authors had visited all the best-known fishing stations in England, Holland, Germany, France, and Italy.[1] They were however unable to make themselves personally acquainted with more than a small proportion of the fishes known to Belon and Rondelet. Most of the figures are copied;[2] among such as are original a few, *e.g.* that of the perch, are life-like. It cannot be said that this is a very important contribution to natural history. Much is taken from Rondelet. The "cetacean fishes" are retained,[3] and Lophius keeps its old place among the cartilaginous fishes. It is something that invertebrate aquatic animals are excluded, and that classification by habitat is abandoned. Useful characters are drawn from the texture of the fins and the number of dorsal fins.

We find a description of the uses of whalebone, which reminds us how long certain fashions have lasted: "Laminis illis corneis, quæ palata hujus piscis innascuntur, fissis et perpolitis, politiores mulierculæ sua pectoralia communire, vestiumque fibras rigidiores et rotundiores continere; atque apparitores publici virgularum ac fascium loco gestare solent, ut recte *Bellonius*."[4]

[1] In a letter to Dr. Tancred Robinson (Ray Correspondence, ed. of Ray Soc., p. 166) Ray laments that all his notes on the animals of High and Low Germany had been lost.

[2] Belon, Rondelet, Salviani, Gesner and Marcgraf are among the authors drawn upon.

[3] *Supra*, p. 49.

[4] P. 38. "Bellonius" is, of course, Belon. The whalebone occasionally mentioned in mediæval authors was white, and cut from walrus-teeth (Wright, *Homes of other Days*, p. 119).

After saying that sharks have the mouth so placed that they must turn over to seize their prey, Willughby and Ray explain that this is a provision of nature for securing the safety of other fishes, and also for preventing the sharks from perishing by their own greediness.[1] In the same way De Geer admires the providential instinct which causes scorpions to kill one another whenever they meet, and so hinders these "insectes malfaisans" from becoming too numerous.[2] Natural theology, which undertakes to justify all the arrangements of nature, finds it necessary to maintain that she sometimes parries her own blows.

Our authors think that the fish which swallowed Jonah must have been a shark, not a whale, because (1) the whale has a very contracted throat; (2) whales are very seldom seen in the Mediterranean; and (3) the fish in whose belly Hercules spent three days was, according to Lycophron, a shark.[3]

THE HISTORIA INSECTORUM, 1710

The weakness of attempting too much appears more strongly in Ray's *History of Insects* than in any other of his writings. The indefatigable old man laboured to the last to complete his gigantic task, which included an ornithology, an ichthyology and a history of plants. It was in his eyes a sacred duty to rescue from oblivion even the scattered observations on insects made forty years before by his friend and fellow-worker. These fragmentary materials Ray supplemented by new observations of no great moment, such as brief descriptions of caterpillars (whose later stages were still undiscovered, and which are arranged only by the year of observation), or short notes by Lister and Derham. Meanwhile the founda-

[1] P. 45. [2] *Hist. des Insectes*, Vol. VII, p. 337. [3] P. 48.

tions of a real History of Insects had been laid by Swammerdam (*infra*, p. 181). The classification adopted by Ray may be passed by without remark; the ancients and Swammerdam furnish all that is of value. I find no original passages which call for extraction, only at long intervals such curiosities of natural history as Pappus' remarks on the advantage for storing of the hexagonal cells of the honeycomb, Mentzel's account of the vinegar-fly (Drosophila), Derham's notices of gnats, blood-worms and the Corethra-larva, which he calls an "animated shadow."

Some of Willughby's papers in the *Philosophical Transactions* are of interest, such as his account of ichneumons,[1] and of the leaf cutting bee ("bees bred in cartridges").[2]

THE CATALOGUE OF CAMBRIDGE PLANTS

In his preface Ray speaks of the ill-health which had obliged him to spend some of his time in riding or walking, and had thus favoured his open-air studies. The riding (alas!) became impossible in later years, but Ray continued to be an ardent field-naturalist, until he could walk no more. When he began to pay attention to plants, no one in the university had a passable knowledge of botany, and even the smatterers were few. He seems to have thought that botany had actually declined, for he speaks of it as "extinctum pæne et intermortuum."

Ray adopted the nomenclature of J. and C. Bauhin, and of the English herbalists Gerard and Parkinson, simply because their books were so well known. John Bauhin's *History* he cannot praise too highly, and Caspar Bauhin's *Pinax* was of great use to him; Gerard and Parkinson he thought little of.

[1] *Phil. Trans.* No. 76 (1670). [2] *Ibid.* Nos. 65, 74. Cf. Lister, *ibid.* No. 160.

The mention of the two Bauhins reminds us that in the interval between L'Obel and Ray systematic botany had made real progress. Reverence for the ancients had grown less servile, the demands of pharmacy less exorbitant, figures of common plants less indispensable. Caspar Bauhin had rectified the intolerable confusion of plant-names. Jung (*infra*, p. 123 n.) had framed a well-considered botanical terminology. The sense of natural affinities had become more enlightened, though the best naturalists were still misled by adaptive resemblance, and plants were brought together merely because they agreed in being aquatic, or in climbing, or in possessing trefoil leaves. One old and laudable practice was firmly adhered to; botanists sought first to discern, and then to define, the groups which exist in nature; no one imitated Cesalpini in proposing a new classification simply because it struck him as precise and logical. The natural families recognised were however so few (less than a dozen), that Ray at first employed an alphabetical sequence of genera, and this is what we find in the *Catalogue of Cambridge Plants.*

The *Catalogue* of 1660 contains 671 species of wild plants. An appendix (1663) added 37 species, and Peter Dent's edition of this appendix (1685) 59 more. In the *Catalogue of English Plants* (1670, 1677) Ray marked the Cambridgeshire species by the letter C, so that this catalogue is a kind of supplement to the Cambridge one. The Martyns (father and son), Relhan and Babington have since revised the flora of Cambridgeshire, and the last-named botanist has noted the changes due to the interference of man since Ray's time.

To many of the plants Ray appends notes, and here he allows himself great latitude, quoting sayings of authors, observations on insects which feed upon the plants, and anything else which might prove useful to his pupils. These notes are often curious, and throw light upon the state of natural history in 1660, but the want of arrangement is so troublesome that the reader may be obliged to make an index for himself. Some notable passages will now be extracted in a condensed form.

Uncertainty of Nomenclature

It is startling to find Ruta, Salvia, Paronychia, Saxifraga and Empetron given as synonyms of Adiantum; or Tussilago and Chrysanthemum as synonyms of Caltha, but parallel cases are to be found in Caspar Bauhin's *Pinax.* When it was thought legitimate to group plants by the shape of the leaf, or the medicinal virtues (often quite imaginary), the most eccentric associations were possible.

Both in this *Catalogue* and in the later *Synopsis Stirpium* a species is usually an indivisible group of plants, while a genus is a group of species. The ten species of Willow, for example, are arranged in the *Catalogue* under two genera. This sense of the word *genus* is not, however, invariable; *genus* may mean no more than *kind* or *sort.*[1] What we call the generic name is with Ray "the name," to which he affixes an adjectival "epithet" characteristic of the species. Different genera do not necessarily bear different "names," either in the *Cambridge Catalogue* or in the *Synopsis.* Thus clover and wood-sorrel are both called Trifolium, though they are placed (in the *Synopsis*) in different parts of the system. Ray quotes from

[1] Examples occur in *Cat. Cantab.* under the heads of Lupulus, Hordeum, Triticum, &c.

Spigel the remark that no name is applied to so many "genera" as Trifolium.[1] He apparently thought it no more necessary to alter the generic names of plants to suit his views on system than to alter the names of towns or men so as to make them accord with some logical method. Wood-sorrel had long before Ray's time been distinguished from Trifolium as Oxys (by Valerius Cordus), but Ray cared little about the name so long as it was clear what plant was denoted.

Fern-seed

Under Filix fœmina authorities are quoted on both sides of the question whether ferns bear seeds or not. Ray supports the negative side, and argues that various plants lack one of the organs which are considered to be essential. Gelsemimum (Jasmine) has no fruit, Fig no flower, Asparagus and Dodder no leaves, Mistletoe no root.

Cuckoo-spit (*Frog-hopper*)

Woodseare is given as a synonym; where does it come from? Ray understands the purpose and the mode of formation of the froth, though he is wrong in supposing that it comes from the *mouth* of the larva; he had wiped away the froth, and seen it form again. The leaping of the full-grown insect had led some naturalists to call it a grasshopper. (See Papaver spumeum, p. 112.)

Ichneumons

Under Dipsacus and Rapum we find observations upon ichneumons, which were in the seventeenth century generally believed to proceed from eggs, not laid by insects, but directly generated in the tissues of the host.

[1] *Cat. Cantab.* p. 169.

Willughby's account of the ichneumon of the cabbage butterfly is given. Some of the caterpillars are said to have produced, not ichneumons but flesh-flies (Tachinidæ?). Ray and Willughby remarked the similarity of the ichneumons to ants and gall-flies. They are cautious about drawing conclusions from what they thought they had seen. "De his nobis nondum satis perspectis nihil temere pronunciare audemus." Another season they would make more careful observations. A few years later we find Martin Lister[1] quite clear that ichneumons are bred from eggs laid in caterpillars or the eggs of spiders by flies of the same species. Willughby[2] thought Lister's explanation ingenious and true, but desired fuller proof.

The Androgyny of Snails

Under Deadly Nightshade Ray observes that the poisonous properties of this plant do not keep off snails and slugs, and goes on to say that these mollusks are androgynous. The details which he mentions leave no doubt that he had witnessed and understood the reciprocal union, of which he says that neither Aristotle, nor, so far as he knew, anyone else had made mention. Seven years later Swammerdam noted the same remarkable fact, which he further elucidated by dissection;[3] he was ignorant at this time that he had been anticipated by Ray; it was not to be expected that a discovery relating to snails would be found in a catalogue of Cambridge plants.[4] The *Biblia Naturæ* contains excellent figures, with a full description.

[1] *Phil. Trans.* 1671; Translation of Goedart (1682).

[2] *Phil. Trans.* 1671. [3] *De respiratione*, p. 117 (1667).

[4] Swammerdam's *Historia Insectorum Generalis* (1669) recognises Ray's priority.

Historical Curiosities

A glimpse of old usages is now and then given by some chance word in the catalogue. Mentha cattaria (the greater Catmint) grows "in a lane on the right hand of Barnwell in going thither, which leads down to the moor on which stand the *pest-houses*." Ebulus grows "along the *balks* of the plowed fields next the *closes*," a reminder that cornfields were often (in still earlier times always) unenclosed. The balks were uncultivated strips, which served to divide the "yard-lands." They were common in Cambridgeshire till the end of the eighteenth century. There are also some curiosities of language. Why should cudweed (Filago germanica) be called "the herb impious"? The answer may be found in Pliny; it is because the younger flowering heads overtop the older ones, as undutiful children overtop their parents. Gerard had explained the old name of wood-sorrel (Alleluia) by the singing of Alleluia in the churches at the season (Easter to Whitsuntide) when wood-sorrel flowers, but Ray, following Scaliger, rejects this with some contempt, and derives the name from the Italian Juliola.[1]

The references to the gardens and flower-shows of Norwich,[2] and to Dr. (afterwards Sir Thomas) Browne's *Garden of Cyrus*[3] are not without interest.

Experiments on the Flow of Sap

Ray and Willughby published in the *Philosophical Transactions*[4] some early experiments on what they called the sap of trees. Any liquid which was contained in the tissues of a living plant was then called sap.

[1] *Cat. Cantab.* p. 165, and Glossary, p. 39.

[2] P. 97. [3] P. 171.

[4] No. 48, p. 963 (1669); No. 70, p. 2125 (1671).

They studied the bleeding of fresh-cut truncheons of birch and sycamore, and also of roots of the same trees. When the truncheon was held vertically, it might be made to bleed at either end. Water supplied by means of a cell of wax to the upper end of a truncheon of birch could be made to run out at the lower end, and this happened whether the truncheon was held in its natural position or inverted. The temperatures are only casually mentioned, and the same work had afterwards to be repeated with greater exactness. They showed that "sap" exudes, not only between the bark and the wood, and from the "pricked circles" [of dotted ducts] "between the coats of the wood," but also from the "very body of the wood." They also investigated the flow from holes bored in a birch and a sycamore.

Many years later Ray, in his *Wisdom of God manifested in the works of the Creation* (1691), adds this interesting note:—"That there is a regress of the Sap in Plants from above downwards, and that this descendent Juice is that which principally nourisheth both Fruit and Plant, is clearly proved by the experiments of Seignor Malpighii, and those of an ingenious Country-Man of our own, Thomas Brotherton, Esquire,[1] of which I shall mention only one, that is, if you cut off a ring of Bark from the Trunk of any tree, that Part of the Tree above the Barked Ring shall grow and increase in Bigness, but not that beneath." The reader who turns to Malpighi's *Anatome Plantarum* in order to see what the experiments were by which he established the existence of a descent of the sap, will be disappointed; it was not by experiments but by arguments that Malpighi reached his conclusion.[2]

[1] *Phil. Trans.* No. 187.

[2] *Anat. Plantarum*, pp. 38-9.

THE METHODUS PLANTARUM

A great part of this little book is occupied by tables, in which the families are laboriously compared with respect to a number of characters.

Long before 1682 real progress had been made towards a natural system of flowering plants. Ray was of opinion that the best of the families (" summa genera ") inherited from earlier botanists were the Fungi, Mosses, Algæ, Ferns, Umbelliferæ, Labiates, Borages, Stellates, Leguminosæ, Pomiferæ, Composites (in three divisions), Bulbous plants, Grasses and Grass-like plants (united).[1] He might have included the Crucifers, which were usually kept more or less together, and some others. Botanists of every age had informally and without attempt at definition recognised, as country-folk still do, poppies, mallows, pines, nightshades, &c. Ray did not succeed in making permanent additions to the number. Nevertheless his life-long exertions were not in vain, for as we shall see, he was able (1) to name and characterise the Monocotyledons and Dicotyledons, and (2) to discover an important principle of natural classification, which was afterwards to guide the practice of Linnæus.

It was no doubt from Malpighi that Ray got the notion of a difference in seedlings, which might serve to divide the flowering plants into large sections. In his tract *De Seminum Vegetatione* Malpighi had described and figured the seedlings of cucumber, French bean, common bean, pea and wheat.[2] It is probable that Ray studied in gardens the germination of many common

[1] Some of these, *e.g.* the umbellifers, leguminous plants, bulbous plants and two divisions of the composites had been more or less clearly recognised by the ancients.

[2] See also *Anatome Plantarum*, p. 77.

plants.[1] From a collection of examples, no doubt very inadequate, he drew a sweeping inference, which wider knowledge has luckily confirmed, and gave definiteness to L'Obel's unnamed series of grasses, bulbous plants and the like, a series hitherto characterised only by the form and venation of the leaves. It was not till the second edition of his *Methodus* (1703) that he gave the names of Monocotyledons and Dicotyledons to his new divisions.[2] Even then, he did not make them primary divisions of flowering plants, for he clung to the last to the ancient separation of trees and herbs. He remarks, however, in the edition of 1703, that the distinction of monocotyledons and dicotyledons might be extended to trees, if it should appear that palms differ from other trees in the same way that monocotyledonous herbs differ from other herbs.

Ray's contribution to the philosophy of natural classification consists in this, that after trying a variety of expedients, some of them very unpromising, he at last recognised that single-organ characters often break up natural groups. He found, for instance, that he had at one time relied too exclusively upon the fruit; the palms, which constitute a truly natural group, may bear either a succulent berry, or a drupe with a hard stone. Rivinus, going solely by the flower, had unnaturally separated Tormentil from the other Potentillas, Echium from the Borages, &c. Tournefort had relied too exclusively on characters taken from the corolla; Hermann on the

[1] Ray's mention of the variety of structure exhibited by the cotyledons, and his advice that his readers should study for themselves the contrivances of the germinating seed, imply personal knowledge on his part. His definite statement that bulbous plants have only one cotyledon (*Synopsis Stirpium*, 2nd ed. 1696) would be unjustifiable if he had never verified the fact.

[2] The distinction is recognised in the table on pp. 56-9 of the first edition of the *Methodus*, where the flowering plants are divided into "foliis seminalibus binis," and "singulis aut nullis."

divisions of the seed-vessel. Ray, in a late stage of his career, found it better to take his characters wherever he could find them, from flower, fruit, seedling, or anything else.[1] Whatever logic might seem to teach,[2] there were natural affinities which must on no account be violated. "Ab una parte quacunque, sive Flos ea sit, sive Fructus, non posse Plantas omnes in Genera seu Classes dividi, ita ut nulla vis Naturæ inferatur, hoc est, ut quæ manifeste cognatæ sunt non separentur, nec quæ alienæ consocientur."[3] When he said[4] that a perfect method is not to be expected, "cum natura (ut diximus) intra limites Methodi cujuscunque coerceri repugnet," he was feeling after the truth which Linnæus was not long after to express more clearly :—"Quæ in uno genere ad genus stabiliendum valent, minime idem in altero necessario præstant," and "Scias characterem non constituere genus, sed genus characterem."[5]

The great fault of Ray's different classifications of plants is of course the retention of the division into trees, shrubs and herbs. Jung[6] and Rivinus had shown

[1] *Reply to Rivinus and Tournefort* (1696).

[2] J. D. Titius, professor of mathematics and physics at Wittenberg, who was also the first to announce that what is called Bode's law of planetary distances, objected to the employment of heterogeneous characters in Linnæus' *Systema Naturæ*, because it conflicted with the rules of sound classification (*De divisione animalium generali*, 1760). Darwin, by showing that natural classifications all rest upon one and the same property, viz. affinity, due to common descent, nearer or more remote, reconciled the claims of practical systematists with logic (*Origin of Species*, Chap. XIII).

[3] Preface to *Methodus Plantarum*, 2nd ed. 1703. Linnæus was evidently too concise when he said that Ray from a Fructicist became a Corollist (*Phil. Bot.* § 59); in his last years Ray strove to detach himself from these and all other sects.

[4] *Hist. Plant.* Vol. I, p. 51. [5] *Phil. Bot.* § 169.

[6] Joachim Jung, physicist, botanist and in the end schoolmaster at Hamburg (1587-1657), exerted a distinct and beneficial influence on the teaching of botany. His *Doxoscopiæ Physicæ Minores* and his *Isagoge Phytoscopica* were published by pupils after his death. Jung's improved botanical terminology,

how unnatural was the separation of, for example, the arboreal from the herbaceous Leguminosæ, but Ray, though he admitted the similarity of structure, could never bring himself to discard a division so obvious and so generally received.[1] In the same way he refused to separate the cetaceans from the fishes, although he was well aware of the important peculiarities which they share with quadrupeds.[2] Buffon is the only naturalist of eminence who has since maintained that system must conform to common usage, instead of guiding it. Ray strangely asserted that while trees and shrubs are furnished with winter buds, herbs are not.

Whatever his deficiencies, Ray did a useful service to systematic botany by gathering up all that he found valuable in his predecessors, producing thereby the best arrangement of plants hitherto published. Long afterwards it facilitated the more lasting system of A. L. de Jussieu.

THE HISTORIA PLANTARUM

is a vast compilation, a proof no doubt of Ray's industry and candour, but little memorable on other grounds. The accounts of exotic plants, taken from Marcgraf, Bontius and many other authors, are quite unreadable. The introduction expounds the structure and physiology of plants, as understood in the latter part of the seventeenth century. Linnæus studied this introduction with

based upon a far sounder knowledge of structure than had hitherto prevailed, was made known to Ray by Samuel Hartlib, a young German, who also diligently propagated the teaching of Comenius (*Cat. Camb. Plants*, p. 87). Ray adopted some of Jung's reforms in his *Historia Plantarum*, and Linnæus in a later generation drew valuable suggestions from the same source.

[1] The ancient division reappears in Tournefort (*Institutiones rei herbariæ*, 1700) and Magnol (*Character plantarum novus*, 1720) and was only finally expelled by Linnæus.

[2] *Supra*, p. 112.

care, and Cuvier thought that it might be worth reprinting separately. But historical inquiry has now cut deeper channels, and the only passages which the reader of the *History of Plants* finds valuable are those in which Ray states his own opinions on questions which were then occupying the attention of botanists for the first time.

Sexuality of plants

In 1686 Ray was not fully persuaded of the sexuality of plants.[1] He says of the stamens that their use is doubtful; some thought them merely ornamental; others (Malpighi) that they eliminated matter detrimental to the seed; Grew, however, had maintained that they effect the fertilisation of the seeds. Ray supports Grew, pointing out that some animals, *e.g.* fishes, are hermaphrodite, and fertilise their eggs without congress, thus furnishing an analogy with the majority of flowering plants. Some plants too are clearly sexual (date-palm, willows, nettle, &c.). Though he inclines to Grew's opinion, Ray does not consider it proved as yet:—"nos ut verisimilem tantum admittimus." In his *Synopsis Stirpium* (1690) and his *Sylloge* (1694) he shows himself fully convinced that the stamens are really male organs.

Definition of a species

Ray considers the coming-up true from seed ("distincta propagatio ex semine") to be the mark of a distinct species, which would make true species of many plants which are known to have been produced in our gardens. Elsewhere he unwillingly admits that degeneration or transformation of species occurs, if the testimony of persons believed to be worthy of credit is to be received.[2]

[1] Vol. I, p. 17. [2] Lib. I, Ch. XX, XXI.

THE SYNOPSIS STIRPIUM

is the first systematic account of British plants, the first British flora in the modern sense of the word. The old herbalists had included all the plants which they could find, either in field or garden. William How (*Phytologia Britannica*, 1650) restricted himself to native species, but adhered to an alphabetical arrangement, as did Ray in his *Catalogus Plantarum Angliæ* (1676). In the *Synopsis* the plants are arranged according to his notion of their affinities, and he scrupulously excluded doubtful natives. The *Synopsis* soon became the manual of field-botanists, and the model of all later floras. After Ray's death it was re-edited by Dillenius (1724), and translated into English by Wilson (1744). Hudson's *Flora Anglica* (1762) was the first to adopt the Linnean arrangement and rules.

Ray gives no synoptic tables, and his book must have been troublesome to work by. His "genera" are for the most part what we should call families, or aggregates of families. The smallest collections of species which he recognises (our genera) are left undefined, nor do they always receive distinctive names. Thus the clovers are called Trifolium, but in another part of the book wood-sorrel and buckbean receive the same name.

Two examples of Ray's descriptions follow; the mixture of Latin and English is singular.[1]

"Erysimum latifolium Neapolitanum, Park. [Parkinson], latifolium majus glabrum, C.B. [Caspar Bauhin]. Irio lævis Apulus Erucæ folio, Col. [Columna]. Smoother broad-leaved Hedge-Mustard. Circa Londinum variis in locis; as between the City and Kensington in

[1] As a rule, English is used only for the localities.

great plenty, also about Chelsey. After the great Fire in London, in the Years 1667, 1668, it came up abundantly among the Rubbish in the Ruines. I have also observed it elsewhere, as about the House of my honoured Friend, Edward Bullock, Esq. at Faulkbourn in Essex; also on the Walls of Berwick upon Tweed [where it still grows].

"This hath small yellow Flowers, and cods [pods] longer by much than those of the common Erysimum, not clapping close to the Stalk, as in that, but standing out from it; it's also a much lesser and lower Plant." [This plant is still called London Rocket; it is the Sisymbrium Irio of text-books.]

Our second example is Ray's description of Jacob's Ladder:—"Valeriana Græca, Ger., Park. [Gerard, Parkinson]. Græca quorundam, colore cæruleo & albo, J.B. [John Bauhin], cærulea, C.B. [Caspar Bauhin], Greek Valerian, called by the vulgar, Ladder to Heaven, and Jacob's Ladder [Polemonium cæruleum]. Found by Dr. Lister in Carleton-beck, in the falling of it into the river Air; but more plentifully both with a blue flower and a white about Malham-Cove, a place so remarkable, that it is esteemed one of the Wonders of Craven. It grows there in the Wood on the left hand of the Water, as you go to the Cove from Malham plentifully; and also at Cordil [Gordale] or the Whern, a remarkable cove, where comes out a great stream of water, near the said Malham.

"Foliis longis pinnatis Viciæ in modum, floribus amplis deorsum nutantibus, vasculis in terna loculamenta divisis ab aliis hujus generis differt [the "genus" is Ray's very miscellaneous Pentapetalæ vasculiferæ]."

Bits of curious information abound in the *Synopsis*. Ray has remarked the occurrence of sea-plantain in inland parts of Cornwall and the bishopric of Durham;

also the capillary submerged leaves of Sium and Sagittaria, which easily escape notice. We are told that lycopodium powder was used in the manufacture of fireworks, the ashes of the female fern for scouring cloth, and kelp for glass-making.

RAY'S ENGLISH PROVERBS

In the grave and laborious life of John Ray we find very few indications that he knew how to amuse himself. His travels, for instance, are relentlessly instructive. Some little idle time, as he would no doubt have reckoned it, he bestowed upon a *Collection of English Proverbs* (1670), and we may hope that his wrinkles were now and then relaxed as he wrote down such sayings as these :—

"Grantham gruel, nine grits and a gallon of water." [1]

"Stay, quoth Stringer, when his neck was in the halter." [2]

"Cut off the head and tail, and throw the rest away." [3]

"Put a miller, a weaver and a tailor in a bag and shake them ; the first that comes out will be a thief." [4]

THE BOOK CALLED "RAY'S TRAVELS"

Library catalogues reckon as one of Ray's works a *Collection of curious Travels and Voyages* (2 vols., 8vo. Lond. 1693), with which, I believe, he had little to do. Comparison with Belon's *Observations des plusieurs singularitez*, &c. first led me to suspect that Ray could not have written this meagre and unintelligent summary. The title-page seems to announce Ray as the author, but this is a mere matter of typography. The words—"By John Ray, F.R.S.," which are promin-

[1] P. 319. [2] P. 82. [3] P. 346. [4] P. 85.

ent, immediately follow upon *Three Catalogues of such Trees, Shrubs and Herbs as grow in the Levant*, which are a very subordinate part of the book, and the booksellers' preface claims no more than these three catalogues as Ray's work, while the actual compilers of the *Collection* are named on the title-page. Smith and Walford, the publishers, published for Ray, and no doubt employed him to make the catalogues. If they had submitted their title-page to him in manuscript, he would have found nothing to object to. The prominence given to his name in printing may have been meant to catch purchasers.[1]

The account of Ray's own travels in Germany, Italy and France is entitled *Observations, topographical, moral and physiological, made in a journey, &c.* 8vo. Lond. 1673.

ESTIMATE OF RAY

Natural history unquestionably owes a great deal to Ray. During his long and strenuous life he introduced many lasting improvements—fuller descriptions, better definitions, better associations, better sequences. He strove to rest his distinctions upon knowledge of structure, which he personally investigated at every opportunity. No doubt he was old-fashioned in some things, clinging for example to the retention of the cetaceans among the fishes, and to the division of the flowering plants into trees, shrubs and herbs, after it had become clear to his mind that such concessions to usage were scientifically indefensible. His greatest single improvement was the division of the herbs into Monocotyledons and Dicotyledons.

Zoological and botanical systems owe comparatively

[1] Derham's *Life* explains the origin of the book.

little to individual lawgivers; they have been built up piecemeal by the incessant proposal of amendments and the retention of such as proved satisfactory in practice. Without claiming for Ray that he possessed a genius for the discovery of hidden relations, we may rank him as the worthiest representative, with respect to knowledge at least, of systematic natural history in the seventeenth century. He made things much easier for Linnæus, as did Linnæus in his turn for naturalists who now smile at his mistakes. Both were capable of proposing haphazard classifications, a fact which need not surprise us, when we reflect how much reason we have to suspect that the best arrangements of birds, teleostean fishes, insects and flowering plants known to our own generation need to be largely recast.

MARTIN LISTER

1638–1712

Historiæ animalium Angliæ tres tractatus . . . de araneis . . . de cochleis tum terrestribus tum fluviatilibus . . . de cochleis marinis. 4to. Lond., Ebor. 1678-81.

J. Godartius of Insects, done into English . . . with notes. 4to. Lond. 1682.

Historia Conchyliorum. Fol. Lond. 1685-92.

Lister came of a Yorkshire family, which held the manor of Thornton-in-Craven. He entered St. John's College, Cambridge, in 1655, and was made fellow in 1660. Ray was teaching at Cambridge during these years, and his example was no doubt influential in causing Lister to occupy himself with natural history. Being intended for the medical profession, he betook himself to Montpellier, and falling in with Ray at that place, shared his journey through France. In after-life they were frequent correspondents, and it was no doubt by arrangement with Ray that Lister undertook to study spiders and shells, in furtherance of the great project of a general natural history of plants and animals. Lister married the daughter of a Yorkshire landowner, and lived for some years at Carlton, near Skipton. During this time he seems to have been fond of rambling over the Craven hills, observing the fossils of the limestone, the mollusks, spiders, insects and plants. Local naturalists still recall his discovery of Cyclostoma elegans at Thorp Arch on the Wharfe, and at Burwell in Lincolnshire, of Sphærium rivicola at Doncaster, and of the fossil Aviculopecten of the lower coal-measures. In 1670 he began to practise as a physician at York, and in the same year was elected into the Royal Society. He continued to busy himself with natural history, paying attention also to antiquities, tracing the Roman wall of York and writing a description of the multangular tower, besides collecting altars, coins and other objects of curiosity. In 1683 he removed to London, and there became a fashionable physician; he made one of the "medicorum chorus," which surrounded Charles II during his last illness; in 1698 he was attached to Portland's embassy to the French court, and at a later time served queen Anne as second physician. Professional work withdrew him from all close study of natural history after his removal to London.

In his younger days Lister was an accurate observer, by no means disinclined to speculation. He stimulated faunistic work in England, and threw out from time to time a sagacious hint or a profitable criticism. A few detached notes will sufficiently indicate the nature of his contributions to biology.

Gossamer

He tells how a spider while fabricating her web may suddenly cease working, and turning the hinder end of her body towards the wind, dart out a thread, which in a moment attains a length of some fathoms; at last the spider leaps into the air, and the thread carries her away. Cases are quoted of both old and young spiders sailing on their threads; some of the threads are not simple, but "snarled," and form woolly locks.[1] The following passage from a letter to Ray, Jan. 1670, has become well known by frequent quotation. "As to the height they [gossamer spiders] are able to mount, it is much beyond that of trees, or even the highest steeples in England. This last October the sky here upon a day was very full of webs; I forthwith mounted to the top of the highest steeple in the Minster, and could thence discern them yet exceeding high above me."

Ichneumons

Lister was clear that the ichneumons bred in caterpillars were the offspring of ichneumon-parents. He had seen ichneumons lay their "young" [eggs] in the very egg-cake of spiders, or pierce galls to reach the maggots within. Goedart had said, in accordance with prevalent belief, that an animal may be generated of a fat juice, but Lister corrects him:—"There is but one way," he says, "that of animal parents."[2]

Insect Hairs

"Naked caterpillars are a more acceptable food to birds than such as are hairy, as I have found by

[1] *Phil. Trans.* No. 50 (1669).

[2] *Translation of Goedart on Insects*, pp. 5, 64. Lister made the mistake of supposing that the piercing instrument of the ichneumon was the tongue.

experience in feeding redbreasts; I guess the reason to be that the hair is noxious to their stomachs. And indeed it is my opinion that the vesicating faculty of insects is much more in the hair than in any other part; I, having blown into my boxes, where sometimes I kept a sort of hairy Cimices, had in a few minutes after all my face blistered. These naked, and therefore more innocent caterpillars, by the instinct of nature, seek to preserve themselves by getting underground in the daytime, when the birds are stirring."[1]

Frozen Caterpillars

"I can witness that in the depth of winter and in very deep snow I have found both caterpillars and hexapod worms lying upon the snow, and therefore have crawled out upon it. I say these caterpillars were so hard frozen that thrown against a glass they made a sound like stones, but put under the glass and set before the fire, did quickly crawl about, and bestir themselves nimbly to get away."[2]

Proposed Soil or Mineral Map

Lister proposed[3] to mark on a map the boundaries of what we should now call the different geological formations. Taking his native county of Yorkshire as an example, he indicated definite tracts which could be laid down:—1. the wolds; 2. the moors with their beds of sandstone; 3. Holderness; 4. the western mountains. "Such upper soils," he says, "if natural, infallibly produce such under minerals, and for the most part in such order." The first actual geological maps were published by Christopher Packe (country

[1] *Trans. of Goedart*, pp. 53-4. See also *supra*, pp. 107, 110.

[2] *Trans. of Goedart*, p. 36. [3] *Phil. Trans.* (1684).

round Canterbury, 1743), Guettard (northern France, 1751), Füchsel (Thuringia, 1762) and especially Guettard and Monnet, *Atlas descriptif et minéralogique de la France* (Fol. Paris, 1780).[1]

The Nature of Fossils

Like some other Cambridge naturalists,[2] Lister speculated freely concerning the nature of fossils. He doubts whether what he calls "cochlites," (fossil shells) had ever formed part of living animals, because (1) some of them exceed in size all shells found in our seas. (2) They occur just as much in inland as in maritime places. (3) They are formed of mere stony substance. (4) They are often imperfect. He adds that some had conjectured that the living animals of such cochlites may some day be found at great depths in the sea.[3]

[1] Geikie, *Founders of Geology*, 2nd ed., pp. 449-55, etc.

[2] Cudworth's *Intellectual System* (1678) expounds a theory of the origin of fossils which strikes us as amazing when we reflect that it was put forth in the life-time of Newton, Leibnitz and Boyle.

[3] *Hist. Anim. Angl.*

SECTION V. THE MINUTE ANATOMISTS

ROBERT HOOKE

1635–1703

Micrographia: or some Physiological Descriptions of minute bodies made by magnifying glasses, with observations and inquiries thereupon. By R. Hooke. Fol. Lond. 1665.

We now reach a point in the history of biology at which the microscope becomes an important instrument of research. Hooke, Malpighi, Grew, Swammerdam and Leeuwenhoek constitute a group of seventeenth-century naturalists who may be called the Minute Anatomists. All of them, either regularly or frequently, investigated minute objects, and their ordinary instrument of research was the microscope. They were not properly histologists, for they studied entire animals and plants as readily as tissues; they might be zoologists, botanists, or physiologists, as well as minute anatomists; opportunity chiefly directed the course of their work; they were little given to experiment (Swammerdam is the most notable exception), and were greater as observers than as thinkers. Hooke (in his natural history work; he was much more than a naturalist) and Leeuwenhoek might be called micrographers; they changed the object continually, turning from a nettle-leaf to the eels in vinegar, or from an insect to a crystal, as the fancy took them; they loved to work out special mechanisms, the sting of a bee, the barbs of a feather, the claw of a spider, the spore-case of a fern; in a word, their biology was unmethodical. The work of Swammerdam, on the other hand, exhibits concentrated effort, while Grew, a man of far inferior power, devoted himself almost entirely to the structure of the higher plants. In the eighteenth century, when the microscope had become an ordinary biological appliance, the best of the naturalists who employed it set narrow limits to their inquiries; Lyonet, for example, was accustomed to proceed from one definite investigation to another one of the same kind.

THE DISCOVERY OF THE MICROSCOPE

Perhaps the first mention of anything that can be called a magnifying glass is to be found in Seneca, who speaks of a glass ball filled with water, and used to make small letters larger and clearer.[1] A passage in Pliny, which has been thought to describe Nero's emerald lens, relates, not to a lens, but to a mirror. Prof. Govi, an eminent Italian physicist, who has carefully examined all the accessible evidence,[2] finds no mention of crystal lenses earlier than the *Opus Majus* of Roger Bacon (1276), where they are described as instruments "useful to old men and to those whose sight is weakened, for by this means they will be able to see the letters sufficiently enlarged, however small they may be." Govi considers Roger Bacon to be the inventor of convergent lenses, and therefore of the simple micro-

[1] *L. Annæi Senecæ ad Lucilium Naturalium Quæstionum Libri*, I, vi, 5.

[2] *The Compound Microscope invented by Galileo* (*Atti R. Accad. Sci. Fis. Nat. Napoli*, Ser. II, Vol. II, 1888. Translated in *Jour. Roy. Micr. Soc.* 1889, pp. 574-598). The present account of the discovery of the microscope is based upon this memoir.

scope. Spectacles were evidently familiar to Chaucer[1] and Lydgate.[2] Telescopes were invented in Holland about the year 1608. Galileo soon heard of them, and proceeded to make some for himself, with which he discovered, in 1610 or shortly after, the satellites of Jupiter, the phases of Venus and the mountains of the moon. He seems to have immediately perceived that the telescope might be transformed into an instrument for the enlargement of minute objects. The narrative of a Scotchman, named Wedderburn, then resident in Rome, quotes from Galileo's own mouth the description of an insect's eye, when magnified. The Dutch microscopes of Drebbel came several years later.[3]

By 1625 there were many working opticians in Holland, Paris and London,[4] who made compound microscopes in a variety of forms, some very elaborate, but all faulty and cumbrous. Simple lenses, very diverse in style as in magnifying power, came to be more and more favoured by working naturalists, and it was with these that the best researches of the seventeenth and eighteenth centuries were carried out.

Hooke was distinguished, even as a boy, by his mechanical genius.[5] He was educated at Westminster School and afterwards at Oxford, where he became known to Wilkins, Seth Ward and Willis. In 1654 Robert Boyle came to reside at Oxford, and employed Hooke first as his instructor, afterwards as his assistant.

[1] *Tale of the Wyf of Bathe*, v. 1203; *Squier's Tale*, v. 234.

[2] *London Lickpenny*, st. 7.

[3] Some authors attribute the discovery of the compound microscope to the Dutch optician, Zacharias Janssen, and put the date as early as 1590.

[4] Descartes tells of the opticians of Paris; most of Drebbel's microscopes were made in London.

[5] See his *Life*, by Richard Waller, prefixed to his *Posthumous Works*, 1705, and the notice by Miss A. M. Clerke in *Dict. Nat. Biog.*

In 1662 Hooke was appointed Curator to the Royal Society, which had been founded two years before; his duties were defined in these words:—"to furnish the Society every day they meet with three or four considerable experiments, expecting no recompense till the Society get a stock enabling them to give it." Hooke seems to have performed faithfully his part of this exorbitant bargain. The numerous experiments demanded were performed; and performed "with the least embarrassment, clearly and evidently."[1]

Hooke was ready to attack by reasoning or experiment every kind of physical question, and the list of his discoveries or anticipations is a long one.[2] He invented air-pumps, diving bells, micrometers, hygroscopes, arithmetical machines, triple writing machines, wind-gauges, rain-gauges, watch-movements, a screw-divided quadrant, and a variety of optical instruments. He was also an architect and a surveyor. His employment as city-surveyor after the great fire brought him in thousands of pounds, much more, doubtless, than all his experiments.

Waller tells us that Hooke was despicable in person, being crooked and low of stature. He went stooping and very fast. He was of an active, restless, indefatigable spirit, and slept little to the day of his death, often continuing his studies all night. His temper was "melancholy, distrustful and jealous." He could not forget that he had anticipated much that others carried nearer to perfection, and his claims to recognition as the first discoverer involved him in endless controversy of a sordid kind. Newton was driven to resolve never to publish his *Optics* so long as Hooke lived.

[1] Weld's *History of the Royal Society*; Waller's *Life of Hooke.*

[2] Several are mentioned in Rouse Ball's *History of Mathematics*, Chap. XV.

Hooke used glasses "of our English make"; his compound microscope is figured. The body, probably of brass, was 6-7 in. long, and was screwed by means of a ball-joint into a ring, which slid on an upright pillar. The tube of the microscope could be moved in any direction; the object was fixed to a jointed arm. The unsteadiness of the arrangements must have been a great hindrance to work. Sunlight, diffused daylight or lamplight was used, the light being reflected, if required, from paper or ground glass. A glass globe filled with water or brine served as a condenser; a sub-stage condenser is also shown, which consists of a conical tube capable of being filled with water and provided with a plano-convex lens at each end.

The following passage from the *Micrographia* shows how the simple microscopes, which were to play so great a part in the natural history investigations of the next two hundred years, were first made. "Could we make a *Microscope* to have only one refraction, it would, *cæteris paribus*, far excel any other that had a greater number. And hence it is, that if you take a very clear piece of a broken *Venice* glass, and in a Lamp draw it out into very small hairs or threads, then holding the ends of these threads in the flame, till they melt and run into a small round Globul, or drop, which will hang at the end of the thread; and if further you stick several of these upon the end of a stick with a little sealing Wax, so as that the threads stand upwards, and then on a Whetstone first grind off a good part of them, and afterward on a smooth Metal plate, with a little Tripoly, rub them till they come to be very smooth; if one of these be fixt with a little soft Wax against a small needle hole, prick'd through a thin Plate of Brass, Lead, Pewter, or any other Metal, and an Object, plac'd very near, be look'd at through it, it will both magnifie and make some Objects more distinct then any of the great Microscopes."

The chapters or sections of the *Micrographia* are called Observations, and are numbered in order. Oldenburg in a notice of the book says that the figures were drawn by Hooke's own hand.[1]

Obs. 1. Hooke begins by illustrating the perfection of the works of nature and the comparative grossness of the works of man. The point of a needle, a printer's full stop, the edge of a razor, a piece of fine lawn, &c. are shown magnified.

The next sections discuss certain properties of matter which we may be excused for mentioning without comment. Explanations are offered of what is now called surface-tension, of Prince Rupert's drops, of the colours of thin films, of the nature of light and heat. Heat is defined as "a property of a body arising from the motion or agitation of its parts."

Obs. 8. The fused globules obtained by striking fire with flint and steel are shown.

Obs. 11. The shell of a Foraminifer (Rotalia), from white sand, is engraved. This is probably the first figure of any Rhizopod, or indeed of any Protozoan.

Obs. 14. The "figures of snow" are well shown. The snow was collected on a black hat or a piece of black cloth.

Obs. 16. The structure of wood charcoal is demonstrated. Hooke shows that the charcoal is traversed by pores, arranged circularly and also radiately. It is easy, he says, to blow through a piece of charcoal.

Obs. 17. Petrified wood, &c. are discussed. Hooke rejects the "plastic virtue," which had been supposed to produce fossil shells.

[1] *Phil. Trans.* Vol. I, p. 28 (1665).

Obs. 18. Cork is examined, and a thin section figured. Hooke describes it as made up of "little boxes or cells," which reminded him of a honeycomb. This is believed to be the first mention of histological cells. The term has since been thoughtlessly extended from the cavities to the live particles which they contain. The epidermic cells of the nettle-leaf (Obs. 25) are more typical of cells in the extended sense of the word. Hooke does not distinguish between cells and pores (*e.g.* of wood). He looked in vain for passages leading from one cell to another, or for valves. He could find no cells in seeds, where Grew and Malpighi soon afterwards discovered them in multitudes. Cork he supposed to be a kind of fungus, sucking its nourishment from the bark of the tree.

Obs. 19. A leaf-fungus (Erisyphe) is engraved, the sporocarps and stylo-gonidia being shown. Hooke thinks that this fungus "may have its equivocal generation, as I have supposed moss or mould to have," and explains its origin by putrefactive and fermentative heat. Mistletoe, mushrooms and mosses may, he thinks, be developed in the same way; the putrefaction of slime and juices may produce worms, the process being aided by the seminal principles of animate bodies; new species might arise casually in this way. Hooke is in all this a link between Aristotle and Buffon.

Obs. 20. One of the common moulds is figured. Its mycelium is described as a "water-mushroom of a multitude of little ramifications, like a thicket." Hooke is prepared to trace a gradual passage from fluidity through orbiculation, fixation, angulization, crystallization, germination or ebullition, vegetation, plantanimation, animation, sensation, to imagination! Did the scholastic philosophy, which he condemns in his preface as a mere matter of discourse and disputation, ever pour forth words without knowledge at such a rate as this?

Obs. 21. A moss is figured with its capsule and peristome.

Obs. 23. A bit of Flustra, which he calls a seaweed, is figured. This is probably the first notice of any Polyzoan.

Obs. 25. Part of the under side of a nettle-leaf is well delineated. The stinging-hairs are figured and explained. Epidermic cells are shown, and there is something very like a nucleus in one of them, but this may be accidental.

Obs. 27. The hygroscopic property of the beard of wild oat is described, and a hygrometer is shown, which may be used "for the discovery of the various constitutions of the air as to dryness and moistness."

Obs. 33. The scale of the eel is beautifully figured.

Obs. 34. The sting of the bee is engraved, but no hint is given that the parts are separable.

Obs. 35, 36. These sections are devoted to the structure of feathers. The barbules and their hooks are shown, and the way in which the bird adjusts the hooks, by stroking or drawing the feather through the bill, explained. A peacock's feather with unconnected barbs is represented; its colours are explained by the "curious and exceeding smallness and fineness of the reflecting parts."

Obs. 37. Hooke here figures the foot of a fly, and shows how the insect is enabled to walk on vertical glass. He tells us that the fly "suspends itself very firmly and easily without the access or need of any such sponges filled with an imaginary gluten as many have, for want of good glasses perhaps or a troublesome and diligent examination, supposed." This "imaginary gluten" has not been refuted, but confirmed, by the modern microscope.

Hooke remarks the "strength and agility of these creatures (insects) compared to their bulk, being, proportionable to their bulk, perhaps an hundred times stronger than an horse or man." This way of stating the case is founded upon a fallacy, which it seems impossible to dispel, and Hooke's mistaken comparison is still reproduced in popular books of natural history.

Obs. 38. The wings of insects are here dealt with, and the scales of a moth's wing well figured.

Obs. 39. The plate shows the head of a "grey drone-fly,"[1] drawn on a large scale. Hooke tells us that the corneal facets looked like holes, but that by observing the reflections from their surfaces he was able to correct the false impression. He shows that the drone-fly has its compound eyes "bisected," to use the modern term, the lower part being composed of lenses distinctly smaller than those of the upper part.[2] Each of these "pearls or hemispheres" is, he doubts not, a perfect eye.

Obs. 42. A good figure of a "blue fly" is given.

Obs. 43. The "water-insect or gnat" is described. The larva and pupa figured are those of Culex, but the fly is what he calls a "tufted or brush-horned gnat," *i.e.* a Chironomus. Much additional information concerning the early stages of these insects was afterwards supplied by Swammerdam and Réaumur. Hooke compares the gnat-larva hanging by its tail from the surface-film to an opossum which he had seen in London, and which, he tells us, had been described by Piso in his

[1] The figure is so imperfect that the fly is not determinable.

[2] Such bisected eyes occur not only in some Diptera but in insects of other orders; their special use is unknown.

Natural History of Brazil. The opossum sleeps hanging by its tail, as the gnat-larva also may do for all that we know. When he comes to give an account of the generation of the gnat, Hooke shows the same uncertainty as other naturalists of the time. He is at one moment ready to believe that the living things which were supposed to be bred of corruption all proceed from eggs. But he is also ready to believe that the sun may cause a parcel of earth to fly on wings in the air, as it causes water to rise in vapour. Science in 1665 had by no means freed herself from the tradition of ancient philosophy.

Obs. 46. A plume-moth (Pterophorus pentadactylus) is shown, but the hind-wing is represented with two divisions only.

Obs. 48. An animated description of the capture of prey by a hunting spider is quoted from John Evelyn.

Obs. 50-2. A chelifer and a Lepisma are figured, probably for the first time. Lepisma is called "the small silver-coloured book-worm." The lustre of pearls is explained.

Obs. 53. A gigantic figure of the flea is given.

Obs. 54. Here is a similar figure of a louse. A mite is figured, and Hooke remarks that mites are found among moulds, so that moulds are probably their food, "spontaneous vegetables seeming a proper food for spontaneous animals."

Obs. 56. The coccus of the vine and its eggs are shown.

Obs. 57. The eels in vinegar are discussed.

Obs. 58-60 treat of inflection of the rays of light, of the fixed stars, and of the moon.

Hooke relates experiments on the sensitive plant.[1]

[1] Pp. 116-121.

He attributes the drooping of the leaves or leaflets either to the circulation of a liquid, or to "a constant pressing of the subtiler parts of it to every extremity of the plant." This is a conjectural anticipation of what is now called turgidity, which has been experimentally demonstrated to be a cause of change of figure in plant-tissues.[1]

MARCELLO MALPIGHI

1628–1694

Marcelli Malpighii Opera omnia. 2 vols. Fol. Lond. 1686-7. Opera posthuma, quibus præfixa est ejusdem vita a seipso scripta. Fol. Lond. 1697.

The above are often found collected in one or two volumes. They were reprinted without editing, and the pagination is not continuous, so that reference is troublesome.

Several of the treatises were published separately during Malpighi's lifetime. The following are of special interest to the naturalist:—*De Bombyce.* 4to. Lond. 1669. *De Ovo incubato,* 4to. Lond. 1672. *De formatione pulli in ovo.* 4to. Lond. 1673. *Anatome Plantarum,* 2 pts. Fol. Lond. 1675-9. A preliminary sketch (*Anatomes Plantarum Idea*), transmitted to the Royal Society in 1671, is prefixed to the last-named.

Malpighi was born near Bologna, and studied philosophy at the neighbouring university,[2] until the death of both his parents threw upon him the care of a family of eight brothers and sisters. He had to arrange for

[1] See the discussion of Brücke's treatise on the leaf-movements of Mimosa (1848) in Sachs' *History of Botany*, English trans. pp. 557-8.

[2] Bologna has done what it can to show its pride in Malpighi. In the upper arcade of the quadrangle of the old university an allegorical fresco has been painted in his honour. There is a Piazza Malpighi and also (but such monuments are fugitive) a Birraria Malpighi.

the disposition of a small family estate, and soon became involved in a boundary dispute, which was never to be settled during his lifetime, and caused him great trouble. These matters occupied him for two years, after which he returned to the university as a student of medicine under Massari. Massari was not content with lecturing, but gathered about him a little company of junior professors and students for the prosecution of research. Malpighi was one of the number, and soon rose from the position of pupil to that of associate and friend. He married Massari's sister, and not long afterwards became professor of medicine. The opposition of unprogressive teachers made his position at Bologna uncomfortable, and in 1656 he was glad to remove to Pisa, where the Grand Duke of Tuscany had set up a new university on a liberal scale. Here Malpighi became acquainted with Borelli, who was appointed professor of mathematics in Pisa at the same time. Borelli was able to apply his physics to certain physiological problems, with great advantage to Malpighi and other students. His treatise on Animal Mechanics is still full of instruction; he is also honourably known in the history of mathematics as the discoverer of the Arabic MS. of Apollonius. Borelli and Malpighi learned much from one another, and during their residence in the same city kept up an intimacy, which was unfortunately impaired at a later time by scientific differences. It is related that during one of Malpighi's lectures the views put forth offended the audience to such a point that one after another withdrew until only Borelli was left. Three years' residence in the damp valley of the Arno affected Malpighi's health, which was always delicate, and obliged him to return to the hill-country. In 1662 he was invited to fill the professorship of medicine at Messina, and there he worked until he accepted an invitation to return to Bologna in 1666. Ever since the old days when he had studied under Massari, he had laboured at anatomy and physiology, and his discoveries had now made him famous throughout Europe. In 1667 he received an invitation from Henry Oldenburg, secretary to the Royal Society in London, to enter into a regular correspondence with that young but enterprising body. He gladly consented, and was next year made a fellow. Oldenburg suggested that he might make observations on the silk-worm and its economy. To this request Malpighi replied in 1669 by sending his dissertation *De Bombyce*, and in the same year the Council of the Royal Society resolved :—" That the History of the Silke Worme, by Signor Malpighi, dedicated to the Royal Society, be printed forthwith by the printers of the same." This was the first of a long series of scientific communications which were at last collected (collected, but not edited, as Haller truly says), and published by the Royal Society under the title of the *Works of Malpighi.*

After thirty years of incessant labour Malpighi's career as a scientific explorer came to an end. A letter from Dr. Tancred Robinson to John Ray, dated Geneva, April 18th, 1684, relates the destruction of all Malpighi's unpublished works:—" I had several conferences with S[ignor] Malpighi at Bononia. . . . He honoured me with two visits at my inn, where once he took occasion to be a little angry with Dr. Lister (whose history he had by him), for his opinion of the origin of stones and shells resembling animal bodies.[1] Just as I left Bononia, I had a lamentable spectacle of Malpighi's house all in flames, occasioned by the negligence of his old wife. All his pictures, furniture, books, and manuscripts were burnt.

[1] *Supra*, p. 134.

I saw him in the very heat of the calamity, and methoughts I never beheld so much Christian patience and philosophy in any man before; for he comforted his wife, and condoled nothing but the loss of his papers, which are more lamented than the Alexandrian Library, or Bartholine's Bibliothece at Copenhagen."[1] During a great part of his life Malpighi suffered from calculus in the kidney, and more than once mentions his poor health as an excuse for the imperfections of his writings.

In 1691 he was pressed to remove to Rome, and become physician to Pope Innocent XII, who had known him during a residence in Bologna some years earlier. Malpighi very reluctantly consented, but his time of service was short. He was seized with a slight apoplectic fit, after which he spent a few weeks in arranging his papers for the printer. In this interval he lost his wife, and a second fit carried him off in November, 1694.

His portrait by Tobar, presented by Malpighi himself, is still in the possession of the Royal Society. It is a good painting, and shows such a face as might belong to a thoughtful and amiable man.

THE ANATOMY OF PLANTS

Every biographer of Malpighi feels bound to repeat the story of the chestnut bough which set him on to study the anatomy of plants. In the memorial volume[2] the incident is related three times in slightly different forms. In the year 1663 Malpighi, it is said, was walking in the garden of his friend, the Visconte Ruffo, when he noticed a broken bough of a chestnut tree. From the broken end a number of threads projected, and on examining these with a lens, Malpighi found

[1] *Correspondence of John Ray*, p. 142.

[2] *Marcello Malpighi e l'opera sua.* Milan, 1907.

that they consisted of spiral vessels filled with air. This incident is supposed to have led him on to study the structure of wood, and the general anatomy of plants. It has no more claim to belief than the famous legend of Newton and the apple. In his own narrative Malpighi drops all the picturesque circumstances, merely saying that while lodging in the villa of the viscount he examined the structure of plants, and in a bit of chestnut wood met with wide air-ducts or tracheæ, which he also found in other vegetables. It is probable that what Malpighi had seen (perhaps in a shaving of chestnut wood) were not true spiral vessels, which are found only in the primary wedges, but pitted ducts. Finding that these ducts were often full of air, which escaped when the parts were cut under water, and that in some cases they were packed with cells (the tüllen or tyloses of modern botanists), he was struck with the resemblance to the wind-pipe of an air-breathing vertebrate, the air-tubes of an insect and the air-cells of a vertebrate lung, and wrote to his friend Borelli that he had discovered the respiratory organs, which served also to carry the sap. He further remarked that by snapping a green stem across and drawing the cut ends gently apart, the spiral thread of the vessels is easily demonstrated.[1] Many generations of succeeding botanists have been accustomed to verify this fact upon the young shoot of the elder.[2] In 1671 he sent to the Royal Society what he called the Idea or preliminary sketch of an

[1] *Anat. Plant. Idea*, p. 3.

[2] Sprengel, Cuvier, Sachs, and perhaps other historians of Botany mention Henshaw as the discoverer of spiral vessels in walnut-wood (1661). The only ground for this statement, and so far as I can find out, the only record of Henshaw's work in botany, is this minute of a meeting of the Royal Society (July 31, 1661):—"Mr. Henshaw exhibited the spirals of nut-trees, showing that they grow snail-wise" (Birch, *Hist. of Roy. Soc.* Vol. I, p. 37). These spirals must surely have been hazel-stems strangled by honeysuckle.

Anatomy of Plants. This was followed up a year later by his fuller treatise, the *Anatome Plantarum.* Both were published together in 1675, and a second part of the *Anatome* in 1679.

Most of the numerous figures which accompany the *Anatome Plantarum* are good, some of them surprisingly good; we are often astonished that so early an investigator should have seen so much and understood so well what he saw.[1] Some of these figures remained unintelligible to botanists until the structures which they depict were rediscovered in modern times. Malpighi shows practical good sense in his choice of subjects, and also in refraining from labour which would not tell. Other naturalists, Grew for example, would laboriously draw half or even the whole section of a stem; Malpighi is satisfied with a small sector.

The text is by no means so good as the figures. False analogies with animal structures and functions abound, as in Aristotle and Cesalpini; the spiral vessel reminds Malpighi of an insect's air-tube and of a vertebrate wind-pipe, which, regarded as an exploratory suggestion, was natural and proper, but he goes on to infer that spiral vessels are respiratory, calls the wood the thorax of the plant, and compares the cells of a tylosis with the air-cells of a lung; he discovers a peristaltic motion in the spiral vessels; the ovary of the plant is an uterus, and the cotyledons are placentas.[2] There is a great deal also of a *Natur-philosophie*, barren and delusive;

[1] One would like to know for certain whether the beautiful drawings in red chalk preserved by the Royal Society were executed by Malpighi's own hand or not. In three separate places (preface to *Anatomes Plantarum Idea*, *Anatome Plantarum*, and *Opera posthuma*, p. 18) he speaks of the figures as having been drawn by himself, but it is of course possible that he handed them over to a professional artist to be copied.

[2] But also leaves of the embryo-plant (seminalis plantula).

he was deeply impressed with what he took to be the important truth that Nature is one, and that the same purpose guides her operations in all things.

In 1671 the Cartesian principle that investigation must proceed from the simple to the complex (a principle which often passes unobserved into the dangerous shape of proceeding from the general to the particular) was prevalent. It led Grew astray, and either this or some older form of the same doctrine had its effect upon Malpighi also. He supposed that complex structures can only be mastered by first mastering the simpler ones upon which they depend, or from which they are derived. The study of plants ought, he thought, to precede that of animals, the study of minerals to precede that of plants. It was impossible for him to foresee that minerals could not be studied to much purpose until chemistry and optics were far more advanced than they were in 1671, but one would have thought that the discovery of the circulation of the blood, in which he had a share, might have opened his eyes to the importance of opportunity and a definite practical purpose, things which have as much to do with the order of investigation as relative simplicity. The vascular mechanism was by no means a simple thing, such as on the Cartesian principle should have been taken in hand early, but men felt how immensely important it was that the course of the circulation should be properly understood, and they found, by a piece of good fortune, that this question could be decided without wide or accurate knowledge of physiology at large, or even of the physiology of the vascular system.

The maxim which should guide our work is not *from the simple to the complex*, nor yet, as some philosophers have taught, *from the more needful to the less needful*, but *from the known to the unknown,* from truths either discovered by steady effort or stumbled on by accident to new truths springing out of past acquisitions, and verified by observation or experiment. Biologists, like other scientific discoverers, have a rugged peak to climb, and are often urged to try this or that infallible method of Bacon, Descartes or Comte, a method which generally turns out to be either misleading or impracticable. There is but one way—to wriggle up as you can, sometimes taking to the right, sometimes to the left, sometimes turning back, because what looked like a promising opening proves to lead nowhere. It is a great thing to possess natural aptitude for the work, a great thing too to be obstinately bent on getting to the top, but the successful climber often owes much to good-luck wisely turned to account.

Malpighi had the notion of verifying his conjectures by experiment, but most of his experiments on plants were so crudely devised, and so imperfectly followed up that they came to nothing. When he wished to discover whether the earth could of itself bring forth plants, he dug up earth from a depth, covered it with a piece of silk, and watched to see whether plants would appear on it. When a question arose as to the use of the cotyledons, he cut them off to see whether the seedling would develop without them. He investigated nutrition through the roots by setting plants in pots, and covering the earth with sulphur, sea-salt, slaked lime, wine, &c. Beans were laid in water, which was covered with oil, to see whether they would germinate. A worker with a talent for experiment might try such things as these, but he would not have accepted failure so easily as Malpighi did, nor would he have printed a string of experiments which had taught him nothing.

The Stem of the Flowering-plant.

The figures of the *Anatome Plantarum* show that Malpighi had by his own labours attained to a fair general notion of the herbaceous dicotyledonous stem, of the woody dicotyledonous stem, and of the stem of maize. Among other things they illustrate the scattered bundles of monocotyledonous stems, the annual rings of dicotyledonous wood, the medullary rays, the rearrangement of fibres at a node, dotted ducts (with a spiral fibre instead of the characteristic marking), and wood-fibres. Cells, which he calls utricles, sacculi and globules, were familiar to him, both when filled with cell-sap and dry. He knew also resin-passages and what he called "lactiferous" vessels. He had compared transverse sections of dicotyledonous branches in successive years.[1] Sap-wood, which he calls "alburnum," was in his day as now rejected by carpenters, because it did not last.[2] He gives a recognisable figure of a tylosis in chestnut wood,[3] together with a fanciful explanation of its function. The lenticels of a young branch are mentioned, though without explanation.[4] The pits of coniferous wood are described as swellings, and tolerably figured;[5] the central opening is not shown.

It is almost superfluous to say that there are many mistakes, sometimes serious ones. The bast he supposed to be a special tract of cortical fibres, adjacent to the wood and gradually converted into wood, a view which prevailed till the beginning of the nineteenth century. The cambium he barely noticed, and did not recognise it as a growing layer. His explanation of the course of the sap is based on the direction of the bundles of

[1] Pl. VIII, Figs. 32-6. [2] P. 20. [3] Pl. IV, Fig. 23.

[4] Idea, p. 2. [5] P. 10; Pl. VI, Fig. 25.

vessels and rows of cells, and helped out with suppositions, which are altogether misleading.

Buds

An interesting and useful object-lesson might be founded upon Malpighi's explanations and figures. He knows that the new buds become visible as soon as the buds of the preceding year have expanded. He is quite clear as to the nature of the bud; it is an undeveloped branch, a "compendium plantæ." He gives a long series of instructive figures to illustrate the development of leaves and stipules. One misses a full account of the bud-scales, and some discussion of their nature.

Leaves

Malpighi's account of the functions of leaves is unsupported by a single experiment. He is guided by analogy to the conclusion, that they are organs for the elaboration and elimination of the crude sap, and are analogous to the skin of an animal. The comparison is, he thinks, borne out by the fall of the leaves, when choked by waste products, which resembles the periodical casting of an insect's skin.[1] The sap, purified in the leaves, supplies, he says, the young shoots and buds.

Malpighi's theory of plant nutrition may be summed up thus:—Water and certain dissolved substances are absorbed by the roots and ascend by the wood-fibres; these materials are elaborated in the leaves, superfluous moisture being exhaled and waste matter eliminated; the elaborated sap passes down the stem by the vascular bundles and may either supply new growths or be stored

[1] Aristotle (*De Gener. Anim.*, V. 3) had compared the fall of the leaf to the fall of hairs, the moulting of feathers, &c.

up in the cortex and medullary rays. He does not speak of a regular circulation of liquids, though this has been attributed to him. The large vessels of the wood, which he supposed to be all spiral, conduct air; the laticiferous vessels are filled with elaborated sap.[1]

Stomates

Malpighi's *Anatome Plantarum*, Part I (1675) contains the earliest account of stomates known to me.[2] Here we find figures of the sunk spaces in the oleander leaf, each lodging several stomates, and also of the stomates of poplar, chestnut, mulberry; in the mulberry they are shown attached to the leaf-veins like berries on stalks. Citron, orange and lemon are said to possess similar organs. Malpighi did not pursue his inquiries so far as to discover that stomates are usual in leaves, nor did he reveal their structure or their function. He thought it enough to say that they opened to the air, and discharged *either* a vapour *or* a liquid ("inter utriculos et fibrorum rete, in plerisque foliis peculiares folliculi seu loculi disperguntur, qui patenti hiatu foras, vel halitum, vel humorem fundunt"). Had he resolutely attacked this last question, he would have found it easily soluble; nothing more was required than to tear off a piece of the epidermis, moisten it with a drop of water, and then examine it with a lens. The reproach of having missed so great an opportunity does not rest upon Malpighi alone; his facts, his engraved figures and his question lay before the whole botanical world for nearly two centuries without starting effective experimental investigations.

[1] Cf. Sachs, *Hist. of Botany*, pp. 458-60.

[2] *Anat. Plant.* pp. 36-7, Pl. XX-XXI, Figs. 106, 107, 109.

The Flower

The parts of a flower Malpighi calls calyx, with its foliola, flos with its leaves, stamens, style. The construction of different flowers is illustrated by numerous well-chosen examples; the accompanying figures are particularly instructive. He has no true notion of the function of the stamens, but supposes that the "globules" (pollen) are a kind of excretion, whose removal purifies the sap. The outer whorls were removed by Malpighi from unexpanded flowers, especially tulips, to see whether in such cases the ovary would ripen; sometimes its development seemed to be retarded; sometimes it was normal. The experiment was of course inconclusive, as no precautions were taken to prevent pollination from other flowers, and Malpighi found himself still at liberty to entertain his favourite speculations, that the outer floral organs either keep off the sun's rays, or purify the sap.[1]

He remarks with astonishment that Nature should have provided receptacles on the petals, in which to store honey. The honey-pouches of Crown Imperial are described.[2] The honey, Malpighi clearly sees, must be secreted by the petal, not brought from without. Naturalists had hitherto explained it as a deposit from the atmosphere.

Staminate and pistillate flowers (nettle, maize, cucumber, Amentiferæ, &c.) are noted. It shows either a momentary lack of acuteness or undue haste that the occurrence of stamens in certain flowers which possessed no ovary did not lead Malpighi to question his theory that stamens protect the ovary or else supply it with a purer sap.

Malpighi traces with some detail the analogy between

[1] P. 55. [2] P. 47; Pl. XXVIII, XXIX, Figs. 162, 167.

the constituent parts of the flower and the sexual organs of mammals, identifying the styles with the Fallopian tubes, the ovary with the uterus, &c.

Double flowers and other monstrous forms are mentioned, and the transformation of the petals of the Polyanthus into foliage-leaves is figured.

Development of the Plant-embryo

We owe to Malpighi the first good account of the development of the seed and embryo. The minuteness of his scrutiny into all visible structures is shown by his many figures of the embryo-sac, its cellular contents (endosperm), and the attached "vessel" (empty part of embryo-sac).[1] There is a plate of the embryo of the walnut in various stages of development which is surprisingly good. He also figures and describes the seedlings of cucumber, kidney bean, common bean, pea, wheat, and millet. In the *Opera Posthuma* we find careful accounts of the germination of a bean, a laurel and the date-palm. The figures are such as an intelligent student, who chose to dispense with the help of books, might make in a modern biological laboratory. I select the description of the seedling date-palm[2] as an example of his treatment. He begins with a notice of the hard endosperm, as we should now call it, which occupies most of the kernel; this he calls the placenta, and says that it plays the part of the cotyledon of wheat or oat; it is composed, he says, of rows of minute cells, which enclose a small chamber for the embryo (plantula). When the seed is planted in the earth, the endosperm softens; the embryo enlarges and begins to protrude. A long whitish stalk, surmounted by a rounded knob, descends into the soil.

[1] Pl. XXXVII. [2] *Opera Posthuma*, pp. 72-5, Pl. VII-IX.

This is, of course, the single cotyledon, which is laid open to show the radicle and plumule within; the knob is the organ for absorption of the endosperm. Malpighi's figures are excellent, and together with the figures of a germinating wheat-grain given in the *De Seminum Vegetatione*, would furnish matter for a capital exposition; the descriptions however are less adequate; the two types are not closely compared, and the names of the parts are not consistently employed. We find no better account of the development of a monocotyledonous plant until we come to Mirbel's memoirs of 1809.[1]

Tubercles on Leguminous roots.—Valerius Cordus had already noted that small tubercles sometimes appear on the roots of lupine; Malpighi[2] figures them in haricot and bean. The interpretation of these curious and important structures belongs to recent plant-physiology.

Malpighi's figures and descriptions show that he had studied objects so various and so little likely to catch the eye of an ordinary observer as the scutellum of a grass-seed, the replacement of the radicle of a grass by lateral roots, the adhesion of earthy particles to root-hairs, the poppy-fruit with its valves, the flower of an orchis, the oblique fibres of a leguminous pod and the spore-cases of a fern.

THE DEVELOPMENT OF THE CHICK

The tracts on the development of the chick in the egg[3] would thoroughly establish Malpighi's power as a biological investigator, even if they had been his only published researches. The subject was not quite a new one; Aristotle, Eustachio, Coiter, Fabricius of Aqua-

[1] *Ann. du Muséum*, Vol. XIII. [2] *Anat. Plant.* Pt. II, Pl. II, IV.

[3] *De Ovo Incubato*, 1675; *De Formatione Pulli in Ovo*, 1673.

pendente, and Harvey had gone before; none of them, however, had made more than a sketch of this memorable history. Aristotle had remarked the beating heart of the early chick and the direct absorption of the yolk. Fabricius made little further progress; he derived the embryo from the chalazae (twisted cords of albumen which project from the yolk-bag). Harvey did much more; he described the ovary and oviduct of the hen, the new-laid egg, the sequence of events during the early days of incubation, and the escape of the chick from the egg. He also noted the vessels which bring nourishment from the yolk, and the first steps in the formation of the brain. Malpighi enjoyed one great advantage over his forerunners; he was able to use magnifying glasses in the work. In his memoirs on the chick we find clearly delineated the vascular area, the dorsal folds, the mesoblastic somites, the vesicles of the brain, the amnion and allantois, the development of the heart (traced in great detail) and the aortic arches, which in a particular stage are seen to encircle the gullet. More minute and technical details are also shown and described.

In his treatise *De Formatione Pulli* Malpighi notes that an embryo can be distinctly seen in an unhatched egg, and figures a second-day chick extracted from such an egg. He mentions one circumstance which renders all plain; this particular egg was opened in August, and in a season of unusual heat, even for Italy, a heat sufficient, no doubt, for development. Malpighi inferred that the rudiment of the embryo goes back further than had been supposed.[1]

[1] "Quare pulli stamina in ovo præexistere, altioremque originem nacta esse fateri convenit, haud dispari ritu ac in Plantarum ovis" (*De Formatione Pulli*, p. 2). This observation formed a foundation-stone of the amazing theory of *predelineation* and *emboîtement* (*infra*, p. 289).

THE ANATOMY OF THE SILKWORM

The treatise *De Bombyce* (1669) was the first thorough account of the structure of any insect. So laborious did Malpighi find the work of dissecting a small animal, with no help to the eyesight but a simple lens, that he suffered afterwards from an inflammation of the eyes. In spite of this trouble, his delight in the new structures which revealed themselves to him was so great that he could not tell it in words. He was the first to observe the air-tubes and spiracles of an insect, the many-chambered heart, the silk-glands, the Malpighian glands (named after him), the gangliated nerve-cord, the reproductive organs and the development of the wings and legs of the moth. He demonstrated experimentally the function of the spiracles, showing that when silkworms are placed in hot water many air-bubbles are given off from these openings, and that when they are smeared with oil the insect dies "in the time that one can say the Lord's prayer."[1] He remarked the rhythmical contraction and dilatation of the last three segments of the caterpillar, and doubtfully regards them as respiratory movements, which they really are.

The figure of the nervous system of the silkworm[2] shows that Malpighi did not understand the brain, which he divided into completely separated halves, nor the relations of the œsophageal ring. The ventral ganglia are too few, one being left out. It would be rather like Malpighi to have drawn the nervous system from a single dissection, and thus to have fallen into errors which a less confident man would have easily avoided. Swammerdam immediately detected these slips,

[1] Aristotle had taught that insects do not breathe, although he knew that an insect dies at once when smeared with oil.

[2] Pl. VI, Fig. 2.

and said in his treatise *De Respiratione* that Malpighi had omitted the brain, but this was unjust. Malpighi complained that his anatomy of the silkworm, the first of its kind, had been censured "rigorose et austere nimis."[1]

MINOR ZOOLOGICAL DISCOVERIES

Malpighi's smaller discoveries are so many that it would weary the reader even to name them. Among other things he observed the liver-fluke, the cystic stage of the tapeworm and the development of a feather.[2] We learn from his diary that he had dissected many animals which he never found time to describe. The day before his death he dictated a short account of the ear of an eagle, and signed it with a trembling hand, begging that it might be added to his anatomy of the eagle.[3]

PHYSIOLOGICAL DISCOVERIES

Malpighi's revelation of the capillary circulation and the structure of the lung was his first, or almost his first, and (there can be little doubt) his greatest discovery ; it was moreover the first important discovery made with the help of the microscope.

In the year 1660, shortly after his return from Pisa to Bologna, Malpighi, now in his thirty-third year, undertook a re-examination of the structure of the lung, in company with his colleague Fracassati. The lung had been hitherto supposed to be a mass of flesh and blood, infiltrated with air, but Malpighi soon found reason to suspect that it was really cellular. A washed-

[1] *Op. Posth.* p. 61, first pagination.

[2] This last research is of later date than that of Poupart on the same subject (*infra*, p. 232).

[3] Already printed in his letters to the Royal Society, which were issued in 1697 as his *Opera Posthuma.*

out lung became sufficiently transparent to reveal a network of fibres or vessels binding together the cells. When water was injected into the pulmonary artery, a stream issued from the pulmonary vein. How, he asked, do the branches of the artery end; by anastomosis with the vein, or by open mouths, leading into air-filled spaces, as most physiologists then held? Before seeking an answer to this question, he wrote an account of his preliminary inquiries to his master and friend Borelli; this letter is the first of two on the lungs, which were printed and became famous. He then betook himself to close study of the lung of the frog, which is both unusually transparent and of comparatively simple structure.

In January 1661, only a few days, it would seem, after despatching his first letter, Malpighi had important news for Borelli. He had found in the frog's lung air-cells of simple form, enclosed by folds of the lining membrane, and subdivided by smaller folds. Upon the edges of all the folds ran blood-vessels of various size.[1] In a lung, still connected with the beating heart, he had seen the blood-stream coursing through finer and finer branches, and becoming paler as it broke up. Then the branches seemed to reunite into a vein, but Malpighi was not certain of the fact until he examined by the microscope a dried frog's lung, in which the finest vessels were naturally injected with blood. It then became clear to him that the blood never escapes from the vessels, but passes from artery to vein through a closed capillary network, and so returns by the pulmonary vein to the heart. The irregular cavities, into which, according to Fabricius, the pulmonary

[1] Malpighi's figures show that he did not perfectly understand the details of structure, and his description has been amended here.

artery discharged its blood, to be there mingled with air drawn in from the windpipe, and then collected by the branches of the pulmonary vein, had no real existence. The vascular networks of the mesentery and the bladder of the frog showed the same kind of communication between artery and vein as the vessels of the lung.

Fracassati's share in the discovery seems to have been unimportant, and two of his letters, printed by Malpighi, present him to us as a rhetorical man, not likely to give much effective help. Malpighi, in his second letter to Borelli, writes in his own name, and relates just what he had seen with his own eyes. Borelli advised instant publication, and a printed account, consisting of the two letters, was put forth with the least possible delay.[1]

Thus was Harvey's doctrine of the circulation completed. Few great discoveries in physiology have been made so rapidly, though Aselli's detection of the lacteals furnishes a fair parallel. Malpighi had a clear question in his mind, and knew upon what object he could best direct his lens; to a man thus prepared a few minutes may well have sufficed.

Malpighi was the first to demonstrate the vesicular structure of the lung, the sensory papillæ of the skin, the minute structure of the liver, kidney and spleen. He saw what must have been red blood-corpuscles in the omentum of the hedgehog,[2] but took them to be fat-globules.

[1] *De Pulmonibus observationes anatomicæ.* Fol. Bonon. 1661. I have not seen this rare tract, but only the reprint in Malpighi's *Opera.* The *De Pulmonibus* and the *Opera Posthuma* (pp. 4-6, 104) contain Malpighi's account of his discovery. Foster's *History of Physiology* gives a translation of the most important passage in the letters to Borelli.

[2] *De omento,* &c. (1665).

ESTIMATE OF MALPIGHI

Malpighi was above all things a physiologist, but his physiology was of that unspecialised kind which is prosecuted in an age when hardly any branches of science have been pushed far. From the study of man and the higher animals he passed quite easily to the study of insects; plants then seemed to him an attractive and practicable field of investigation; he only regretted that he had not time to attend to chemistry and mineralogy as well. He was skilled in anatomy, but his distinctive merit among contemporary physiologists was his continual resort to the microscope. He was a close observer, and whatever he saw he pondered over, but experiment was not his strong side.

His own generation was not competent to follow up all the work of Malpighi. In human anatomy and physiology, indeed, a body of trained students already existed, and his discoveries were not suffered to fall to the ground. Of his contributions to these sciences I have not ventured to speak at length; they doubtless constitute his greatest claim to the respect of later ages. The treatise *De Bombyce* inaugurated a new study, that of insect anatomy, which was to be prosecuted with greater power by Swammerdam. Malpighi's admirable work in embryology was for the time unproductive; only after a long interval was it resumed by Haller, Wolff, Pander, and Baer. Notwithstanding the merit of his observations on the structure of plants, they fell upon unprepared ground, and it may be doubted whether they did much real good. Their seeming completeness and their general agreement with the teaching of Grew, led men to the totally unfounded belief that the organs and functions of plants had now been effectively dealt with. For another century no one seems to have suspected that experimental inquiries into such elementary and vitally important processes as the nutrition of green plants, the transport and storage of food-materials, and the fertilisation of the ovule, had still almost to be begun.

If it be true, as I think it is, that Malpighi's work in natural history produced no effect answerable to its real value, it would be instructive to discover the reason. Influence is gained most easily by those reformers who neither move fast nor attempt many things at once. Harvey, Swammerdam, Linnæus, and Cuvier, unlike one another in so many things, each pursued one purpose until it was accomplished. Harvey effected the discovery of the circulation, Swammerdam explained the transformations of insects, Linnæus adapted the "system of nature" to new and urgent wants, Cuvier revealed the structure of many extinct animals, unlike any that now survive. What else these great men did, and whether they did anything else, were matters of less importance. Moreover their discoveries were timely; the world was ready to receive them and to turn them to account. Malpighi, on the other hand, opened out many new paths, but soon quitted them. He explored the anatomy of the flowering plant, the development of the seedling, the development of the chick, the anatomy of the silkworm, the structure of glands and many other parts of the animal frame, but this extraordinary fertility really diminished his influence. Few workers received from him such practical training as enabled them to occupy the territories which he had discovered, and he became not so much a leader as a pioneer, who planted his standards so far ahead and so far apart that they could not serve as rallying-points, but merely as proofs that in several different directions he had outstripped all competitors.

NEHEMIAH GREW

1641–1712

The Anatomy of Vegetables begun. 8vo. Lond. 1672.
The Anatomy of Plants. Fol. Lond. 1682.

The botanist Grew was son to the Puritan minister of St. Michael's, Coventry, one of those ejected in 1662. Like Ray, he was a pious man, who thought it a duty to trace the hand of God in the visible creation, and it was this which caused him to attend to the structure of plants. Wilkins, afterwards bishop of Chester, one of the founders of the Royal Society, brought Grew's work under the notice of his colleagues, who encouraged him to print his first book. He removed to London, and set up as a physician there ; he served as secretary to the Society in 1677. His communications to the Society formed the basis of separate publications, and these, corrected and expanded by further reflection as well as by hints taken from Malpighi,[1] were at length collected into a folio volume, called *The Anatomy of Plants.*

The Anatomy of Vegetables begun, which ultimately formed the first part of the *Anatomy of Plants*, is an 8vo book of 198 pages, with three folding plates. Chapter I describes the germinating seed of the garden bean. Then we go on to the trunk (stem), the germen

[1] The publications of Malpighi and Grew on plant-structure are sometimes almost simultaneous, and it is often hard to decide which was the first to observe a fact of special interest. Questions of priority are not raised by either ; in the seventeenth or early eighteenth century it was not the practice to acknowledge help received from other authors. Locke and Newton do not quote Bacon, nor Galileo Kepler, nor Descartes Kepler or De Dominis, nor Spinoza Hobbes. Linnæus is very careless about debts of this kind.

(bud), the leaf, the flower, the fruit and the seed before germination.

Grew's description of the bean-seed may be called an object-lesson, one of the first object-lessons that was ever written. The parts are skilfully displayed and well figured.

He notes the "vest or coat" of the seed, and the foramen (micropyle)[1] against which the tip of the radicle lies. The foramen "is not a hole casually made, or by the breaking off of the stalk; but designedly formed, for the uses hereafter mentioned." It is found not only in beans, but in other pulse, and "in many seeds not reckoned of this kindred." "That this foramen is truly permeable, even in old setting-beans, and the other seeds above named, appears upon their being soaked for some time in water. For then, taking them out, and crushing them a little, many small bubbles will alternately arise and break upon it." He afterwards explains that the general cause of the growth of the bean is fermentation, and that the foramen serves for the supply of airy particles which excite the fermentation; it also gives easy issue to particles and steams, and plays the part of a bung-hole to the new-tunned liquor in a barrel.[2]

[1] The name of micropyle is Tournefort's. The modern reader may be puzzled for a moment by Grew's statement that the micropyle will admit a "small virginal wyer," but will soon recollect that the virginal or spinet was the piano of those days.

[2] Malpighi in his *Anatomes Plantarum Idea* (p. 9) explains that the micropyle serves for the entrance of moisture. His speculations, like those of Grew, were refuted when it was shown that seeds whose micropyles have been plugged with wax or varnish germinate perfectly well (Turpin, *Ann. du Muséum*, Vol. VII). C. J. Geoffroy (Acad. des Sciences, 1711) showed that the fertilisation of the ovule is effected through the micropyle, though he erroneously believed that "le petit grain du poussière (pollen) peut tomber naturellement par cette petite ouverture dans la cavité de cette vésicule qui est l'embryon de la graine." The pollen-grain was, he thought, the germ of the future plant.

Within the coat Grew finds the main body of the seed, and notes the radicle, plumule ("plume") and two seed-lobes. Some seeds, like those of corn, are not divided, but entire; and some few, he says, are divided into more than two lobes.[1] The embryo is covered with a cuticle,[2] within which is found the parenchyma,[3] which is not "a mere concreted juice," but "a body very curiously organised, consisting of an infinite number of extreme small bladders." He finds branching vessels in all parts of the embryo, which appear in sections as specks; and he figures very carefully the vessels of the seed-lobes. He is aware that in some seedlings (lupine, &c.) the seed-lobes turn green, and push into the air, while in others (bean, &c.) they remain shut up in the seed; he knows what becomes of the plume and radicle, and recognises that the seed-lobes are only a particular kind of leaves ("dissimilar leaves," he calls them).

Grew's clear and useful account of the structure of the bean-seed and seedling is sadly marred by his guesses as to the function of the parts. We have noticed what he says about fermentation; his explanation of the sap in the vessels is equally baseless. It comes, he says, from without, filters through the seed-coats, becoming fermented in the inner one, enters the parenchyma, and so gains the vessels of the radicle and plumule.

Plant Cells

Grew, as well as Malpighi, was aware that a young plant is wholly composed of cells, which he compares to

[1] His example is not a pine, but the common cress, whose seed-leaves are three-lobed.

[2] In his Anatomy of Seeds (*Anat. of Plants*, p. 207) Grew shows that in a soaked bean the cuticle can be separated; if examined by the microscope, it is seen to consist of cells ("bladders") "all radiated towards the centre."

[3] The word is not new; Erasistratus had applied it nearly 2000 years before to a substance supposed to be poured out from the veins.

the vesicles of bread, and supposes to have originated in the fermentation of a "coagulum." Hooke had called the cells of a parenchyma a "heap of bubbles"; Grew's favourite term is "bladders." He knows that "a single row or file of bladders, evenly and perpendicularly piled, may sometimes . . . break into one another and so make one continued cavity" (p. 118), an anticipation of Mohl's discovery of 1831. His notion of the cell-wall, suggested by the thickenings so frequent in vessels, was that it consisted of a network of fine threads.

Vessels

Grew discovered and traced the vessels of the bean-seedling and other plants, but first learned from Malpighi that some of them possess a spiral thread. He remarks that spiral vessels never branch, and that they may extend to great distances. His "lymph-ducts" are not true vessels, but bast-fibres, &c.

Monocotyledonous Stem

We find a good figure of a transverse section of a maize-stem, and Grew notes the scattered vascular bundles, as well as the lack of a distinct bark and pith (p. 104, Pl. XVIII, Fig. 2).

Resin-passages

He recognises and correctly interprets the resin-passages in the cortex of a pine-stem (p. 110).

Resistance to Bending of Hollow Structures

The rigidity of a circumferential zone of vascular bundles, as well as the rigidity of quills and hollow bones, when compared with solid rods of the same length and the same weight, is pointed out (p. 23).

Medullary Rays

Grew had a good anatomical notion of the medullary rays of the dicotyledonous stem; he calls them "insertions," because they are inserted or wedged into the wood. He remarks their continuity with the cortex and pith, their use in bracing together the rings of the wood, and their emergence from the outside of the woody cylinder (pp. 12, 20; Pl. III, Fig. 8; Pl. XXXVII).

Hooks for Climbing

The use in climbing of the hooks of bramble and cleavers is described (p. 149). Mention is also made of the wings and feathers of fruits or seeds, and of plants which spurt or sling away their seeds; in short, Grew gives a preliminary sketch of the account of dispersal which Linnæus was able to furnish in 1751 (*infra*, p. 322).

Buds

The security of the axillary position, the protection and economy of space gained by the imbrication of the bud-scales, and by the folding of the foliage-leaves, various modes of leaf-rolling, and the defence of leaves by means of hairs are dwelt upon (pp. 145-9). Grew points out that buds are formed months before they expand (p. 157), and mentions the burying of the sumach-bud in the cortex of the branch (pp. 146-7). "A bulb is, as it were, a great bud under ground" (p. 58).

Stomates

He notes the occurrence of "orifices or passports, either for the better avolation of superfluous sap, or the admission of air" (p. 153). By a strange oversight he says that the stomates are found only on the *upper* side of the leaf. There is a tolerable figure of the stomates of a lily-leaf (Pl. XLVIII). It is singular that Grew does not notice Malpighi's earlier discovery of the stomates.[1]

Alternation of Floral Whorls

The relative position of the parts of the flower is explained; they are not "filed one just over another, but alternately," thus admitting of closer packing and more complete protection of the inner organs (p. 164).

The Flower

In the flower Grew distinguishes the calyx ("empalement"), corolla ("foliature"), and stamens ("attire"); the pistil he does not consider a part of the flower, but of the fruit; this had been the view of Theophrastus and also of the sixteenth-century botanists. He tells us that the anthers ("semets") when they burst scatter their powders, which are a "congeries, usually, of so many perfect globes or globulets, sometimes of other figures, but always regular" (p. 38). The uses of the pollen Grew can only guess at; it may serve "for food, for ornament and distinction to us, and for food to other animals" (pp. 39, 40). The "primary and private use" of the pollen is discussed more fully in the fourth book; Grew is convinced that it must be great and necessary. He has no experimental evidence to bring forward, but makes one interesting remark. "In discourse hereof with our learned Savilian Professor, Sir Thomas Millington, he told me he conceived that the attire doth serve as the male, for the generation of the seed. I immediately replied that I was of the same opinion; and gave him some reasons for it, and answered some

[1] *Supra*, p. 155.

objections, which might oppose them" (p. 171).[1] Unfortunately Grew did not seek to verify his opinion by experiment. He goes on to say that as every plant is both male and female (an unwarranted guess) the attire must discharge other functions as well, such as the separation and "affusion" of parts (probably chemical change of some kind). In another passage he notices what he calls the secondary use of the attire; it serves, he thinks, for ornament and for distinction (of species); further, it provides food for "a vast number of little animals" (p. 40). Elsewhere he remarks that pollen-grains "are that body which bees gather and carry upon their thighs, and is commonly called their bread. For the wax they carry in little flakes in their chaps, but the bread is a kind of powder, yet somewhat moist, as are the said little particles of attire" (p. 171). Swammerdam had not been able to discover what bee-bread really was (see p. 192).

The Fructification of a Fern

The sporangium ("seed-case") of a fern is described as "girded about with a sturdy tendon or spring," whose surface resembles a fine screw; "so soon as by the innate air of the plant or otherwise this spring is become stark enough, it suddenly breaks the case into two halfs, like two little cups, and so slings the seed" (p. 200). The sporangia are barely recognisable in Grew's figure (Pl. LXXII). Fern-sporangia had already been described by William Cole of Bristol (1669). Valerius Cordus (*supra*, p. 28) had found reason to affirm that young ferns spring from the brown dust

[1] Ray in his *Wisdom of God* (1691) speaks of "the masculine or prolific seed contained in the chives or apices of the stamina," but like Grew, he had no clear proof for what he said. Five years earlier he had treated the question as an open one (*supra*, p. 125).

on the under-side of fern-leaves. Perhaps he had no clearer proof to adduce than the springing-up of young ferns around old ones, and the emission from the fronds of fine particles, which would naturally be called seeds. The learned botanists of ancient times had denied that ferns bear seed, but popular belief in the middle ages inclined the other way.

The passages just quoted show Grew at his best. It is natural that he should have mistaken many things. Like Malpighi, he failed to recognise the cambium-layer of the stem, which is very excusable. His propensity to put forth untested speculations sometimes makes us smile. Thus he explains that sap ascends because its motion is ascent, and because motion is more noble; he distinguishes solar plants which twine with the sun from lunar plants which twine with the moon. In these things Grew was the man of his generation—a generation which no one will affect to despise who recollects that it was also the generation of Swammerdam, Boyle and Newton.

Grew's talent really lay in the use of those aids which, he was sanguine enough to suppose, would render the microscope almost superfluous, viz. a good eye, a clear light and a keen knife (p. 107). His reputation with posterity would stand higher if he could have laid aside all his philosophy, and imitated his friend Boyle in testing current interpretations by well-devised experiments.

In the *Philosophical Transactions*[1] there is a good account by Grew of the ridges and sweat-pores of the human hand. He saw the sweat exude from the pores, and gives good figures of the patterns formed by the ridges, of a ridge with its pores, &c. Tyson[2] a few years

[1] No. 159 (1684). [2] *Anatomy of a Pygmie*, p. 12, 1699.

later observed in the chimpanzee "those spiral lines which are usually in a man's." Grew and Tyson had between them laid the foundations of a scientific account of finger-tips, but these hints lay unnoticed for nearly two hundred years.

JAN JACOBZ SWAMMERDAM

1637–1680

Historia Insectorum Generalis, ofte Algemeene Verhandeling van de Bloedloose Dierkens. Sm. 4to. Utrecht. 1669.

Ephemeri vita, of afbeeldingh van's menschen leven, vertoont in de wonderbaarlycke historie van het vliegent ende eendaghlevent Haft of Oever-aas, &c. 8vo. Amst. 1675.

Biblia Naturæ, sive Historia Insectorum . . . accedit præfatio in qua vitam auctoris descripsit Hermannus Boerhaave . . . Latinam versionem adscripsit Hieronimus David Gaubius. 2 vols. Fol. Leydæ. 1737-8.

Swammerdam was born at Amsterdam. His father was an apothecary, who had expended much time and money upon a private museum, for which his own trade and the world-wide commerce of Amsterdam gave special opportunity. The boy was originally intended for the Protestant ministry, but when he grew up, he got leave to study medicine instead. Natural history, which was afterwards to become his passion, was even in youth one of his chief occupations, and he collected and studied insects with enthusiasm. His father, the apothecary, probably found the services of a zealous young naturalist very advantageous to the growth of the museum, and Swammerdam continued to live at home till he was twenty-four, when he was sent to Leyden as a medical student. Here he made the acquaintance of the Dane, Nicholas Stensen, whose name when Latinised became Steno, and who, in 1661, the very year of Swammerdam's matriculation, discovered the duct of the parotid gland.

Regner de Graaf (Graaf of the Graafian follicles) was another friend of Swammerdam's student days, but rivalry embittered their relations before Graaf's early death. Stensen, Graaf, and Swammerdam were all pupils of Franciscus Sylvius (François de la Boe), who professed medicine at Leyden with great distinction from 1658 until his death in 1672. This Sylvius (who must not be confounded with the anatomist, Jacobus Sylvius, who taught Vesalius) was a zealous and skilful anatomist; he was also one of the best chemists of that early time. To carry on his studies and enlarge his experience, Swammerdam next travelled, as was the rule with young students of adequate means. Perhaps it was the existence of a great Protestant college[1] which directed his course to Saumur (1663), where he carried on his insect studies. Here, in the year 1664, he discovered the valves of the lymphatic vessels, and sent an account of them with drawings to his friend Stensen. Not long after Ruysch announced the very same discovery, and Swammerdam was inclined to believe that his drawings had been shown to Ruysch. Visiting Paris, Swammerdam became acquainted with Melchisedec Thévenot, traveller, French ambassador at Genoa and a lover of science, who brought many scientific men together at his pleasant villa. Both Stensen and Swammerdam were hospitably received by Thévenot, who continued a true friend to Swammerdam throughout all his subsequent troubles. Amidst the lively throng which gathered at Thévenot's house Swammerdam always remained silent; his powers were only revealed when he could be induced to exhibit his marvellous skill in unravelling the intricacies of insect

[1] Suppressed, together with the colleges of Die, Vitré, Castres, Orthez, Sedan, Nismes, and La Rochelle by Louis XIV in 1685. Montauban alone was allowed to survive.

structure. On his return to Amsterdam Swammerdam threw himself seriously into human anatomy, and worked at the structure of the spinal cord. He also practised various modes of injection, and became expert in this difficult art. In 1667 he was made Doctor of Medicine of Leyden, presenting as his dissertation a treatise on Respiration.[1]

In 1668 Cosmo, duke of Tuscany visited Amsterdam, and was taken to see Swammerdam and the museum. With delicate tools and a sure hand Swammerdam repeated a favourite demonstration. Opening a caterpillar which was ready to pass into the pupal stage, he showed the butterfly with its wings, legs and proboscis packed up within the larval skin. The duke was delighted, and bid 12,000 florins for the collection, on condition that Swammerdam would accompany it to Florence, and accept an appointment in his service. Swammerdam was at this time vexed with worldly cares, and the prospect of a secure post, with unlimited leisure for study, must have had its charms, but he was Dutch and Protestant, accustomed to think his own thoughts, especially about matters of religion. Life at court, and in the service of a Catholic prince, was impossible for him, and he refused the duke's offer. Swammerdam's friend, the anatomist and physiologist Stensen, became the duke's physician, and not very long after underwent a sudden conversion. He was moved, not only to embrace the Catholic faith, but to take orders, and having been advanced to the dignity of a bishop, was sent back to the Protestant countries of the north as a missionary. The labours and privations of

[1] This was at first a brief abstract of Swammerdam's results in scholastic form; it was enlarged in a second edition (1679), and reprinted with annotations by Haller (1738).

his remaining years at least attested his complete sincerity.[1]

Swammerdam's *General History of Insects* was published in 1669, when the author was thirty-two, and still entirely dependent upon his father, who began to grow impatient of the burden. A severe attack of ague had impaired the naturalist's health, and from this time the disease continually returned. He was bent upon completing fresh insect studies, among the rest, a treatise on the Ephemera, and a fuller account of the structure and life-history of the hive-bee than had hitherto appeared. But the father would have no more of these unproductive labours, and Swammerdam was forced to promise that he would now give his undivided attention to medical practice. To conciliate his father, he first wrote out with immense labour a complete catalogue of the museum. The ague came back, and Swammerdam had to go into the country to recruit. It was June, and the butterflies were abroad. The temptation proved irresistible, and he relapsed into natural history. How he propitiated his angry father we do not know, but the printing of the history of the Ephemera in 1675 points to further pecuniary assistance. During these troubled years the laborious observations and exact delineations which fill the two large folios of the *Biblia Naturæ* were incessantly accumulating. Boerhaave (who did not, we must note, write from personal knowledge) gives us a vivid picture of the toil bestowed upon them. "Swammerdam's labours were superhuman. Through the day he observed incessantly, and at night described and drew what he had seen. By six o'clock in the morning in summer he began to find enough light to enable him to trace the minutiæ of natural objects. He

[1] Foster, *History of Physiology*, Lect. IV.

was hard at work till noon, in full sunlight, and bareheaded, so as not to obstruct the light, and his head streamed with profuse sweat. His eyes, by reason of the blaze of light, became so weakened that he could not observe minute objects in the afternoon, for his eyes were weary."

Boerhaave describes from Swammerdam's papers his methods of work. The dissecting table was of brass, and had two arms, which could be turned in any direction; one arm carried the object, and the other the lens. Swammerdam's lenses were of various sizes and powers. His skill in the use of forceps and scalpel was surprising. Some of his tools were so minute that he had to whet them under a magnifying glass. He was skilful in injection and inflation. Swammerdam knew how to render anatomical preparations transparent by balsam, so that the course of the vessels could be traced without dissection; it was considered an important advantage that the parts remained soft and flexible. The tissues, which were often of considerable bulk, were first soaked in turpentine, for months if necessary. After long soaking in turpentine, balsam or mastic was added. A moderate heat was sometimes employed.[1]

In 1673 Swammerdam came under the influence of Antonia Bourignon, who is commonly described as a religious fanatic, and who was bitterly persecuted as such by the Dutch and Danish Lutherans. Swammerdam was henceforth guided by her advice, and to some extent shared her sufferings. At one time he thought of selling all his insects and preparations, and retiring upon whatever income they would yield. But no purchaser could be found. The duke of Tuscany was approached through

[1]An account of Swammerdam's method is to be found at the end of Schrader's *Observationes et Historiæ*, 12mo., Amst. 1674.

Stensen, but acceptance of the Catholic faith was laid down as a necessary preliminary, and Swammerdam declared that he would not sell his soul at a price. In 1675, the very year in which the *History of the Ephemera* appeared, Stensen sent to Malpighi some of Swammerdam's figures of the silkworm, together with a short letter:—" Swammerdam begs you to receive kindly these figures,[1] since he is about to give up the study of natural objects. He had undertaken a work of the same kind as yours (the treatise *De Bombyce*, 1669), but has destroyed it, keeping only the figures. He is seeking after God, but not as yet in the church of God." Swammerdam's father died in 1677, and bequeathed property to his son.[2] But the will was disputed, and amidst anxieties of every kind, money-cares, religious controversy, continual illness and disappointed hopes, the naturalist's life came to a sad close in 1680; he was only forty-three.

During his life-time Swammerdam had published, besides academical theses, a *General History of Insects*, a description of the life-history of Ephemera, and an account of the chameleon. Both the *General History* and the *Ephemera* were afterwards incorporated with the *Biblia Naturæ*.

THE *BIBLIA NATURÆ*

Of the *Biblia Naturæ*, as of other works whose excellence lies in the mass of finished detail which they offer, no account in the least adequate can be given. We have

[1] The figures sent to Malpighi may have been those which now appear in the *Biblia Naturæ*, Pl. XXVIII, Figs. i-iii.

[2] Copies are still extant of the sale-catalogue of the elder Swammerdam's museum (143 pp. in Latin and Dutch, 8vo., 1679), and of the collection of insect-preparations, anatomical preparations, injections, &c., of the son. A note at the end informs us that both collections were offered for sale at the same time.

here the history of the ephemera, the dragon-fly, the bee, the gnat, ants, the rhinoceros-beetle, the tortoise-shell butterfly, the cheesehopper, the gall-insects, the hermit-crab, the water-flea, the tadpole, the snail, the sepia and many more. Under each we find the life-history, the anatomy down to the last detail visible by Swammerdam's microscope, and in many cases observations upon allied forms. No delineations so exact and beautiful as these appeared until Lyonet, seventy years after the death of Swammerdam, produced his *Anatomy of the Goat-moth.* The *Biblia Naturæ* is of permanent interest as a collection of facts, as a monument of industry and sagacity, and as a measure of the high level which biological knowledge had attained in the latter half of the seventeenth century.

We have little information respecting the time and manner of production of the beautiful plates which accompany the *Biblia Naturæ.* It is not easy to understand how Swammerdam, amidst the distresses of his last years, should have found money to pay for fifty folio plates. Did Thévenot find the money?

Swammerdam bequeathed all his manuscripts and drawings to his friend Melchisedec Thévenot. The painter Joubert bought the papers from Thévenot's heirs, and sold them again to the anatomist Duverney, who kept them for many years, talking about a French edition, but doing nothing. Forty years after Swammerdam's death his chief contributions to natural history still remained in manuscript, their very existence known only to a few. At length the famous Dutch physician, Boerhaave, came to know that Swammerdam's papers were still preserved in Paris. He begged his friend William Sherard, who was about to visit France, to find out all about them. It was discovered that the manuscripts still existed, and proofs of some of the engraved plates were sent for Boerhaave's inspection. He bought the whole at a low price, and in 1737-8 had the satisfaction of issuing Swammerdam's works in two magnificent folios, illustrated by fifty-three plates, and accompanied by a new Latin version by Gaubius of Leyden. It is not easy to explain how Swammerdam came to give to any book of his so significant a title as *The Bible of Nature.* His purpose of magnifying the works of God, which appears in the pious ejaculations at the end of every chapter, may have seemed to sanctify his labours. *Buch der Natur* had been the title of an illustrated book of animals and plants published at Augsburg late in the fifteenth century.

Insect Transformations

The *General History of Insects* (1669) includes a long discussion on the nature of insect transformations, which was afterwards included in the *Biblia Naturæ.* One main object with Swammerdam was to refute the mistaken doctrines of Harvey, the great Harvey, whose account of the circulation of the blood was now generally accepted. Harvey's knowledge of development was based entirely upon his study of vertebrate embryos, and his account of the development of insects and other animals then called "bloodless," was darkened by vain philosophy. According to him, an insect could not be properly said to grow; it was a lump of organic matter which had been stamped with a definite form. Insects, he made bold to say, were generated by chance, and were not constant to their kinds. Swammerdam might well be angry to find notions so preposterous disseminated under the authority of a great name. He combated Harvey with all his vigour, and at wearisome length. Insects grow, he explains, just as much as any other animals. Their changes of form are exactly analogous to the changes by which a frog's egg becomes first a tadpole and then a frog. But the frog has blood, and even Harvey must have admitted that it grows. Swammerdam loses patience when Harvey attributes to insects a metamorphosis which has nothing to do with growth, a kind of transmutation, like that by which, it was supposed, a base metal can be changed to gold; an Ovidian metamorphosis, such as that by which a flying nymph is turned to a laurel tree. The development of the winged insect, says Swammerdam, is merely growth with change of form, and the wings, legs and proboscis can be discovered beneath the larval skin long before the fly emerges. The insect is one and the same animal throughout and has never been anything but an insect.

Swammerdam returns to the question again and again,[1] explaining how the legs, wings and other appendages of a butterfly form beneath the larval skin, how they become visible at the time of pupation, how they are glued down till they have acquired due firmness, and how, having cast yet another skin, they become functional when the butterfly enters upon its free existence.

In the section entitled "Animal in Animali,"[2] Swammerdam shows how to demonstrate that the butterfly is contained within the last larval skin. A full-fed caterpillar with swollen thorax must be taken and tied by a thread to a stick; it must then be time after time dipped for a moment in boiling water until the skin becomes loose, when it can be easily stripped off, leaving the

[1] See *Biblia Naturæ*, pp. 34, 578 (tortoise-shell butterfly), 603 (large cabbage-white).

[2] *Biblia Naturæ*, p. 603.

butterfly exposed. The expression "animal in animali," we remark, incorporates the very notion which Swammerdam had vehemently repudiated in his controversy with Harvey, where he says as emphatically as he can that the larva or pupa is not changed into a butterfly; it is itself the butterfly in another form.

He protested also against Harvey's doctrine that the pupa is an egg, a doctrine which, ill-supported as it is, still reappears from time to time.

In one place[1] Swammerdam, after disposing of these misleading hypotheses, brings out one of his own. There is perhaps, he says, no true generation anywhere in nature; what goes by that name is merely continuous growth, a budding out of new parts, the same process as that by which the legs and wings of the butterfly are formed in the larva. This, he thinks, explains how a mutilated parent can produce unmutilated offspring, how Levi could pay tithes to Melchisedec, before he was born, and how the sin of Adam can be laid to the charge of all his posterity. Upon this sandy foundation was long afterwards erected the theory of "emboîtement" (*infra*, p. 289).

Swammerdam recognised four modes of larval development, and made use of them to divide insects into four orders. In the first order development is direct, and there is no transformation. Here he placed the lice, besides the centipedes, spiders, scorpions, earthworm and slugs. In his second order Swammerdam placed the insects which gradually acquire wings, and pass through no resting-stage. Of these he quotes the dragon-fly, the cricket, the cockroach and others. In the third order come the insects whose wings develop beneath the larval skin and which pass through a

[1] *Biblia Naturæ*, p. 34.

pupal or resting-stage. Here Swammerdam places the bees, beetles, moths, butterflies, and certain two-winged flies. But those flies which, like the bluebottle or the house-fly, pupate within the dry larval skin he separated to form his fourth order, associating with them, on slight grounds, other insects of quite different kinds.

The Hive-bee

The *Biblia Naturæ* does not admit of brief abstract, and probably the only way in which a notion of its quality can be given to a modern reader is by a leisurely account of one or more chapters as a specimen of the rest. The description of the hive-bee[1] recommends itself to our choice. It is full and interesting; it reveals the practical methods and some of the philosophical views of its author. Moreover the hive-bee has attracted the notice of observers and minute anatomists in every age; Swammerdam, Réaumur and the two Hubers kept the inquiry alive for a century and a half without nearly exhausting it, as may be seen by the fact that it was still left for Dzierzon to prove the following new and vitally important points:—that female bees (queen and workers) proceed only from eggs fertilised by drones; that drones proceed from unfertilised eggs; that the queen is fertilised once for all, and can fertilise or not fertilise her eggs, seemingly at her own pleasure.

Swammerdam tells us that the chief part of his work on the hive-bee was done in 1673, one of those "cruel years," as he calls them, of the French invasion, when the dykes were cut to save Amsterdam. The very bees were ruined in Holland, and hardly any queens could be procured. He had worked at bees before this date, and

[1] *Biblia Naturæ*, p. 367.

the anatomy of a drone had been one of the marvels of nature which he had laid before the duke of Tuscany in 1668. He now spent several months upon the investigation, dissecting by day, and writing his descriptions at night. All his favourite anatomical methods were brought into play—dissection with simple lenses, inflation, injection with coloured liquids, and mounting in balsam. The life-history, the anatomy of the male, female and neuter bees in every stage, and the whole economy of the hive, are carefully described. To the discussion of special points of structure or function Swammerdam brings a considerable knowledge, not only of insects but of animals in general, and often enters upon fruitful comparisons. The engraved figures would do credit to the most skilful anatomists of any age. This, the first extensive and truly scientific memoir on the hive and its inhabitants, carries the exploration a long way at a single bound, and biology can hardly produce a second example of a research so comprehensive and disfigured by so few faults.

In spite of his extraordinary sagacity and the most scrupulous pains, Swammerdam's account of the hive-bee contains some errors of fact and of interpretation. The naturalist who undertakes to describe for the first time so complex a thing as a social insect reminds one of the pilgrim who had to go along the Valley of Humiliation; Bunyan tells us that Christian went down very warily, yet he caught a slip or two.

Swammerdam had announced in his *Historia Insectorum Generalis* (1669) that the queen, hitherto commonly called the "king of the bees," is the only effective female in the hive; that the drones are the males and the workers neuters. This identification rested upon his careful and detailed anatomical examination of each sex.[1] He is positive that the workers have no reproductive organs, though it is now known that they occasionally lay eggs. He remarks that both the queen and the workers possess a sting, while the drones have none, and that in structure and disposition the workers resemble the queen rather than the drones.

The external features of the bee are described in much detail, and Swammerdam put forth all his skill in the account of the proboscis, compound eyes and sting. We are surprised to find no detailed account or enlarged figure of the legs of the worker-bee, on which we should have expected that particular care would have been bestowed.

The large figure of the proboscis might be copied almost without alteration in any modern text-book of entomology; but in the description we observe some mistakes. Swammerdam thought that the long slender tongue was a real tube,[2] that it was the only entrance to the mouth, and that the sucking-up of liquids was effected by the movement of the abdomen. Many a bee would have perished by starvation had it been furnished with a capillary inlet so liable to become choked with viscid nectar and pollen. The tongue is actually grooved and not tubular; the suctorial channel is made up of distinct pieces, which can be separated at pleasure; moreover there is a large and distensible

[1] Others had suspected that the "governor" of the bees was a female, but without attempt at strict proof, *e.g.* Butler (*supra*, p. 89). Milton's *Paradise Lost* was published in 1667, and can have taken no advantage of Swammerdam's discoveries. Yet it celebrates

> "The female bee, that feeds her husband drone
> Deliciously, and builds her waxen cells
> With honey stored." (vii, 490-1.)

[2] "Cavi instar tubuli pervia" (*B.N.*, p. 445; pl. xvii, fig. v). Swammerdam's mistake was pointed out by Réaumur, *Hist. des Insectes*, tom. v, pp. 320-1.

mouth-opening, which forms no part of the tongue. The proboscis of the bee leads Swammerdam to say a word about that of a moth, which he shows to be double, the halves being co-articulated by innumerable fine hooks. He briefly describes the short proboscis of a wasp, but gives no hint that it is made up of parts answerable to those of the bee, except that he figures the two sets of organs side by side.

When he comes to the eyes of the bee he remarks that they are larger in the drone than in the queen or the worker, and that besides compound eyes there are three peculiar eyes (now called "simple eyes") on the top of the head. That the compound eyes are true organs of vision had already been demonstrated by Hooke, who had blinded insects by cutting out or injuring their eyes. Swammerdam resorted to the less cruel expedient of smearing the bee's eyes with black paint, and found that bees so treated could not find their way about. By careful figures and descriptions he gives as good a representation of the eye of a bee as could be attained in the days when there were no transparent sections, when the development of the parts had not been studied, and when the best optical knowledge was but elementary. In spite of all defects, Swammerdam's account of the compound eye is much the best which was offered to physiologists then or for many years to come. He remarks that certain ingenious but hasty philosophers ("sommige subtiele ende gaauwe geesten"), among them the illustrious Hooke, had supposed the compound eye to be a collection of numerous simple eyes, each fashioned like the eye of man. Swammerdam saw that this comparison gives no true idea of the insect-eye. To the inner surface of each corneal facet is applied a long, slender cone, which, though it may superficially resemble an iris in being pigmented, has no pupil, and absorbs almost all the light which enters it. The humours of the vertebrate eye, which Hooke thought he had found in the compound eye, are not really to be recognised therein. Swammerdam derides certain naturalists, to whom he had demonstrated the facets of the bee's eye (Leeuwenhoek is no doubt one of these), and who had found in their hexagonal form a reason why the cells of the honeycomb should be hexagonal. On the same principle, says Swammerdam, mankind with their circular pupils ought always to make circular dwellings! Among other details he notes the fine branching air-tubes which run between the elements of an insect's eye—a striking proof of the goodness of his microscopes and the closeness of his observation. How can insects see with their compound eyes? Swammerdam answers that there is no image formed upon a retina, as in the vertebrate eye; there is no regulated aperture for the admission of light; in short, the compound eye is not in any respect such a camera obscura as the human eye. The luminous rays must strike direct upon the cones, and there produce sensations. He goes on to say that the vision of insects is thereby rendered more acute; it is because they possess compound eyes that bees can see in the dark, and that dragon-flies can take their prey on the wing. But here Swammerdam places himself among those "ingenious and hasty philosophers" who explain what they do not really understand. Vision by means of a compound eye was first made in some degree intelligible by Johannes Müller a hundred and fifty years after the date of Swammerdam's account of the hive-bee.

The sting, he tells us, is straight in the worker, curved in the queen, and wanting in the drone. Though concealed in the winged bee, it is external in the pupa, and its hard parts are cast together with the pupal skin. His figures show all the essential parts of the sting—the sheath, the two darts with their levers, the barbed teeth, the poison-sac with its gland and duct. He notes the firmness of the poison-sac, and says that if the abdomen is opened and the sac grasped, the whole sting may be plucked out without tearing. He was accustomed to mount the sting in balsam—a method of his own devising. He examined the action of the darts and their barbs by causing bees to sting leather gloves and the thicker parts of the human skin; he remarked the alternate action of the darts, and the tendency of the sting to penetrate deeper and deeper. In his thirst for knowledge he often pressed a bee's sting into his own skin, and found that if the poison is prevented from entering, the pain is nothing. When bee's venom was taken into the mouth, he observed that it caused a flow of saliva, and an action upon the tongue like that of the root of Pyrethrum or of spirits of wine. The poison of the queen was more virulent than that of the worker, and that of a wasp more potent still.

I must forego the opportunity, so seductive to an insect-anatomist, of discussing Swammerdam's description of the internal organs of the bee. They are wonderfully exact and detailed, and would rank with the best work done in modern times. General observations now and then show how wide was his knowledge of insects and allied animals. He remarks that insects, spiders and crabs cast the skin, and, unlike vertebrates, have their muscles enclosed by the skeleton.[1] Few naturalists of his own generation could have penned such a sentence as the following, though it has since

[1] Pp. 403, 444.

become matter of common knowledge:—"Whatever size insects have attained when they undergo their transformation, they retain ever after." He had closely studied the change of skin in insects. The new segments and appendages, he tells us, are at first soft and wrinkled, but expand greatly as soon as they become free, owing to distention by air or blood. He is well aware of the close connection between instantaneous or protracted egg-laying, and the shorter or longer duration of the imaginal state.

The life-history of the hive-bee is described with much care. Swammerdam noted the changes of skin which the larva undergoes, and the closeness of his observation is shown by his statement that when the last larval skin is cast the old tracheæ are withdrawn from the spiracles, and the chitinous lining from the alimentary canal.

Having worked out with incredible labour the structure of the reproductive organs of the queen and drone, and having convinced himself that the queen lays all the eggs by which the hive is replenished (but see p. 186), Swammerdam must have confidently expected to get proof of the fertilisation of the queen by a drone. But the difficulties of observation happen to be unusually great in the case of the hive-bee, and Swammerdam was unluckily led astray by difficulties of his own contriving. He never came to a knowledge of what actually occurs, and put forth a delusive explanation which does him little credit. To begin with, he could not understand the action of the male parts, which seemed to him to hinder effective copulation. He remarked too that the queen is surrounded by workers, which do not permit the access of drones; even at swarming-times, when she is allowed to leave the hive, she is closely guarded.

There is, as we now know, one occasion in the life of the queen when she flies abroad unaccompanied by workers, but this was unknown to Swammerdam and all other naturalists until the time of the elder Huber. In this emergency Swammerdam might have profitably recollected the maxim which he quotes from Bacon, that it is not our business to devise, or to think out, but to *discover* the method of Nature. He betook himself to thinking out, and the result was his theory of an *aura seminalis.* Drones shut up in a box or bottle give out a strong odour; during the summer months a hive contains thousands of drones; if the queen is not fertilised like other female insects, was it not possible that she might be fertilised by the effluvium of the drones? This supposition, at least in the seventeenth century, when seeds were thought by some to be fertilised by an effluvium from flowers, and when strange fables as to the generation of saints were not yet wholly discredited, had sufficient plausibility to justify further examination. An experiment was hit upon by Swammerdam, the very experiment by which long afterwards Huber demolished Swammerdam's theory, viz. that of exposing a virgin queen to the emanations of drones enclosed in a perforated box. It impairs Swammerdam's character as a scientific man that he never tried his own experiment; he ought either to have thoroughly tested his theory, or to have withheld it. He speculates also upon the possibility that the eggs of bees, like those of many fishes, may be fertilised after laying, but here again he had no facts to go upon, nor did he try to procure any.

Swammerdam's description of the comb is tolerably full, but too familiar for repetition. He notices the remarkable constancy in size of the cells, and tells us that some Frenchman had proposed to make them the basis of an international system of measurements.[1] He thinks that he could make out how the bees construct their cells if he could spare half a year for quiet observation.

He has much to say about honey, bee-bread and wax, but his knowledge is still imperfect. The honey, he thinks, undergoes a sort of partial digestion in the crops of the workers, and is afterwards regurgitated into the honey-cells. Bee-bread he examined microscopically, but failed to discover that it is derived from the pollen of flowers, though he recognised its identity with the pellets brought home on the legs of working bees. The globules which he found in the bee-bread puzzled him completely; at one time he thinks that they look like fat-globules; at another he thinks they may be dew or the effluvium of flowers and fruits, condensed into globules by the pressure of the atmosphere! Nor is he more fortunate in explaining the uses of the bee-bread; he does not find out that it is the same thing as the whitish, almost tasteless paste, which he elsewhere mentions as the food of the larvæ; he has a strong suspicion that it is the raw material out of which wax is formed, and throws out conjectures that saliva, venom or honey may be able to change bee-bread into wax. He justly doubts whether the workers ever return to the hive with wax on their legs, and mentions a reward which he offered in vain to any Dutch beemaster who would bring him a bee so laden.

The whole economy of the hive is discussed, and such matters as the rearing of brood, swarming, and the production of new queens receive due attention. Most of

[1] The Frenchman, as Réaumur says, was Swammerdam's friend, Thévenot (*Hist. des. Insectes*, Vol. V, p. 398). Réaumur observes that the pendulum offers a far more exact standard of length. A degree of the earth's surface is proposed as a unit of length in Burton's *Anatomy of Melancholy*.

what Swammerdam has to say about these things had been known for ages to beemasters.

Aristotle, Virgil and Pliny tell how bees in windy weather load themselves with small pebbles, to save themselves from being blown away, and Swammerdam tries to show how this belief may have originated.[1] When he was living in France in 1666 he observed the habits of the mason-bee (Chalicodoma), and saw it carrying small pebbles for the strengthening of its stony nest.

He tries to make intelligible the tale of Sampson and the swarm of bees which made combs in the carcase of a dead lion.[2] Perhaps the bones of the lion had first been cleaned by maggots. But he adds the significant fact that there are flies so like bees, and with maggots so like bee-larvæ that they may easily be taken for true bees. In our own time Osten Sacken has more fully worked out this notion of Swammerdam's, and put it beyond doubt that the resemblance of drone-flies to bees is the basis of the ancient and wide-spread belief that bees may be generated from putrefying carcases.

Whatever poets and philosophers may have imagined, Swammerdam holds that all the acts of the workers in the hive are governed by necessity; they have no real government, no virtues, no rewards nor punishments. Elsewhere he says that the bees learn their duties from nature, not by copying others.

There is a short but useful account of humble-bees and their nests, which was afterwards greatly expanded and improved upon by Réaumur.

Swammerdam is far from methodical in his statement of facts, and the reader is sometimes put to much trouble by his habit of modifying in one place what

[1] *Biblia Naturæ*, p. 525. [2] *B.N.*, p. 527.

he has already said in another. He is fond of digression, and whatever the matter in hand, chance remarks on a variety of subjects are to be expected. If the references were lost, one would hardly look in the chapter on the hive-bee for the two passages next cited.

All animals, he says, even man himself, proceed from eggs. In another place he speaks confidently about mammalian eggs, though such things were not actually demonstrated before the nineteenth century.

A chance mention of the egg-masses of the lackey-moth leads him to say that though such eggs might be expected as a matter of course to yield caterpillars, they sometimes yield flies instead, which he considers the most surprising fact in natural history. This must be a very early mention (perhaps the very first) of egg-parasites.

The Snail and other Mollusks

Swammerdam takes the apple-snail (Helix pomatia) as his first example of a mollusk.[1] He remarks that this species was a pest to the wine-growers of France; in Holland, however, it was a curiosity, with which people liked to furnish their grottos, and he thinks it worth while to explain how they can be most readily imported. Then he describes and figures the shell; unfortunately, this and some other figures of snails have been reversed by the engraver. He calls it an operculate shell, though he knew that the so-called operculum is only to be found in winter. The uses of snails as food and medicine are noted. They are, he thinks, a kind of insect, and he places them in his first order of insects, viz. such as undergo no transformation; elsewhere he speaks of "Scarabaei et alia Testacea."[2] The

[1] *Biblia Naturæ*, p. 97. [2] *B.N.*, p. 197.

shell, he explains, is not the house of the snail, but part of its body; it is formed of "true bone," and has muscles inserted into it. The tentacles are carefully and admiringly described. He shows how they are withdrawn by special retractor muscles, and protruded by a kind of peristaltic action of their annular muscles. The eye on the tip of the tentacle receives close attention; Swammerdam finds in it all the layers of the vertebrate eye, and even believes that it possesses an iris, though he admits that he has not seen it. The eyes of the snail are, he tells us, ineffective for the perception of near objects. He notices in passing the eye of the mole, which also is of little or no use; to this keen-sighted anatomist it was so plain that he could dissect it without a microscope. He figures the jaw of the apple-snail and its odontophore, but he does not seem to have found the lingual ribbon,[1] one of those exquisite contrivances in which he was accustomed to take delight. It was not only Swammerdam's eyes which were quick to perceive; he speaks of hearing the sound which the snail makes in feeding. Then he goes on to the organs of respiration and circulation. His knowledge of the circulation is not quite complete; for example, he takes the pulmonary vein to be the vena cava. He tells how from this vessel the heart and arteries can be inflated, or filled with a coloured liquid. He knows that the blood of the snail is coagulable, and that it turns milky when mixed with water. This leads him to expose the mistake of speculative writers in saying that small animals "of this kind" (he means invertebrate animals) have no blood; they really have blood, but, except in the earthworm, it has no crimson colour. He mentions and figures the renal organ, which

[1]He describes elsewhere the lingual ribbons of Paludina and Sepia.

he says discharges a calcareous secretion into the intestine.

Swammerdam's account of the complicated reproductive organs of the snail is a marvel. If it were now an untouched inquiry, and if a trained naturalist, armed with modern appliances, were to spend a summer in elucidating it, he would do well to turn out so good a history as this. The darts are the only things which completely puzzle Swammerdam, and this is not surprising, for their function is by no means cleared up even now. Only a single blameable weakness is to be remarked; being unable to explain the spermatheca, he throws out the wholly unjustifiable speculation that it answers to the cavity in which the murex stores its purple dye.[1]

Swammerdam gives a careful representation of the nervous system of his snail, noting particularly the œsophageal ring, which he had found in moths also and all other insects examined by him. He notes the great mobility of the nervous ring upon the œsophagus, and figures the muscles by which its position can be altered. The lateral œsophageal connectives answer, he thinks, to the spinal cord.

Of the shell he has much to say. It is a tube spirally wound about a small central space, which is sometimes closed in the apple-snail as in many others; he notes that in Vermetus a number of the spires are unconnected and irregular. In some aquatic snails the shell can be recognised even in the egg. It is invested by a "periosteum." When the snail is about to add to its shell, it cleanses the old periosteum with its mouth, removing pieces and swallowing them; at these times it remains long motionless. The new shell is formed,

[1] For Swammerdam on the androgyny of the snail see *supra*, p. 118.

he says, of fibres secreted by the mucous glands of the skin, especially by the thickened edge of the mantle; these fibres coalesce to form membranes or laminæ, which are afterwards calcified. Bones, teeth and the shells of crustaceans are, he thinks, formed in much the same way, and injuries to the shell are repaired like fractured bones. He thinks it wonderful that the shells of aquatic snails and the cocoons of some aquatic insects should form under water, for he attributed the hardening of the mucous secretion to the action of the "ambient air."

A number of other mollusks, chiefly land or freshwater species, are described with more or less detail. The account of Paludina is interesting.[1] Swammerdam notes the horny operculum, the eyes on lateral pedicels outside the tentacles, and the lingual ribbon. He grows enthusiastic about the viviparous reproduction of this snail, of which he gives a circumstantial account.

The Frog and the Tadpole

This memoir[2] is a marvel of patience and anatomical skill. The passage of eggs from the ovary to the exterior of the body is discussed by Swammerdam at great length, but he could not make out how they gain the narrow mouth of the oviduct (a question which still has its difficulties). A large and elaborate drawing displays the anatomy of a tadpole furnished with internal gills. The early stages of development are described, the process of segmentation naturally escaping observation, and a stage is expressly mentioned in which the embryo consists entirely of small granules (*greynkens* or *klootkens*, our *cells*).[3] The "vermiculi viventes" which

[1] *Biblia Naturæ*, p. 169. [2] *Biblia Naturæ*, pp. 789-860.

[3] Leeuwenhoek made such a stage known in 1688, eight years after Swammerdam's death, but long before the publication of the *Biblia Naturæ* (*infra*, p. 203).

Swammerdam found in the lung, are the parasites now called Rhabdonema.

Here too we find Swammerdam's account of his discovery of the red blood-corpuscles of the frog. Foster's translation[1] follows:—"In the blood I perceived the serum in which floated an immense number of rounded particles, possessing the shape of as it were a flat oval, but nevertheless wholly regular. These particles seemed however to contain within themselves the humour of other particles [or rather, *another humour besides*—the nucleus?]. When they were looked at sideways, they resembled transparent rods, as it were, and many other figures, according no doubt to the different ways in which they were rolled about in the serum of the blood. I remarked besides that the colour of the objects was the paler the more highly they were magnified by means of the microscope."[2]

The chapter ends with a discussion of muscular contraction, which can be studied in the frog with peculiar advantage, partly because the nerves and muscles are so readily exposed and separated, partly because the power of contraction, as in all cold-blooded animals, persists long after removal from the body. Swam-

[1] *History of Physiology*, p. 99. *B. N.*, p. 835.

[2] The wrong date of 1658 is assigned to Swammerdam's discovery of the red blood-corpuscles of the frog by Foster (*loc. cit.*), by Darmstädter, and probably by other writers. In 1658 Swammerdam had not begun his regular anatomical studies; he went to Leyden for this purpose in 1661. No date is assigned, so far as I know, either in the *Biblia Naturæ* or in Boerhaave's Life prefixed thereto, to the discovery of the red corpuscles, but on p. 839 of the *Biblia Naturæ* the wrong date of 1658 is given to Swammerdam's demonstration of a muscle-nerve preparation before Cosmo III, Duke of Tuscany; it is well-known that the Duke's visit took place in 1668. Swammerdam's observations on the red corpuscles of the frog cannot therefore, it would seem, be dated at all. In *B.N.*, pp. 69, 70 he speaks of human blood as having been examined by him, and found to contain reddish corpuscles floating in a clear liquid; he was not certain that they occur in arterial as well as in venous blood. See also *infra*, p. 204.

merdam proved that the irritation of a nerve, completely severed from the brain, may excite a muscle to contract, and further, that a muscle does not increase in volume during contraction, as physiologists had hitherto supposed. He placed the muscle-nerve preparation in a glass tube, drawn out into a fine neck, and filled with water. At the moment of contraction there was no rise of water in the tube, but if anything a fall. He concluded that no material substance passes along the nerve to the muscle, but a mere impulse.[1]

ESTIMATE OF SWAMMERDAM

We may claim for Swammerdam (1) that he offered the first scientific account of those transformations of animals which had hitherto been so anomalous and perplexing; (2) that he gave a powerful impulse to the *comparative* study of animal structures; (3) that he did something for the improvement of zoological system; (4) that he illustrated by a series of examples admirably worked out that method of studying structure and life-history by means of concrete animal types, which still holds its ground as the best form of elementary instruction in biology, and (5) that he made important contributions to experimental physiology and embryology. His short and troubled life was assuredly not spent in vain.

[1] Glisson's plethysmographic experiment demonstrated in a different way that muscular contraction is not accompanied by an increase of volume, and this was probably the first to be published (Foster's *History of Physiology*, p. 290). It would be interesting to learn more precisely what were the muscle-experiments which Swammerdam demonstrated to Stensen and others somewhere about the years 1666-8.

ANTONY VAN LEEUWENHOEK

1632–1723

Leeuwenhoek's papers, most of which appeared in the *Philosophical Transactions*, were collected and reprinted from time to time in small volumes, a list of which is given below. These volumes, not all issued by the same publisher, are generally found bound in four volumes, which bear the misleading title of *Opera Omnia.* They were not re-edited, and each collection has its own name, pagination and index. We shall quote the collections by the abbreviations given in the following list, adding the volume and page of the collection most often met with (4 vols. Lugd. Batav., 1722), but bad arrangement and confused pagination will sometimes make it hard to find the passages cited.

Anatomia, seu interiora rerum cum animatarum tum inanimarum (sic) ope, . . . microscopiorum detecta. 2 pts. Lugd. Batav. 1687. (*Anat.*)

Arcanæ Natura detecta. 4to. Delphis Batav. 1695. (*Arc. Nat.*)

Continuatio Arcanorum Naturæ detectorum. 4to. Delphis Batav. 1697. (*Cont. Arc. Nat.*)

Epistolæ physiologicæ. 4to. Delphis 1719. (*Ep. phys.*)

Epistolæ ad Societatem Regiam Anglicam . . . seu Continuatio Arcanorum Naturæ detectorum. 4to. Lugd. Batav. 1719. (*Ep. Soc. R.*)

Continuatio Epistolarum . . . ad Regiam Societatem Londinensem. 4to. Lugd. Batav. 1689. (*Cont. Epist.*)

Nearly all the great naturalists of the seventeenth century (it will suffice to mention the names of Malpighi, Redi, Swammerdam and Ray) were learned men, who had studied under eminent professors. Among them Leeuwenhoek, a man who owed nothing to any university, and knew no language but his own, won a high place.[1] In the new age of scientific discovery which had just opened such examples were to become frequent. Leeu-

[1] Leeuwenhoek in a letter to the Royal Society, dated Jan. 22, 1676, explains that he can read Dutch only.

wenhoek was born at Delft of a family of burghers, some of whom had been brewers, and was first put to business in Amsterdam. Afterwards he obtained the office of sheriff's chamberlain in Delft, which yielded him a small income with apparently little labour, and to this occupation he settled down for life. In his leisure-time he began to make and use magnifying glasses, and before he had reached middle life his microscopic demonstrations had become celebrated. De Graaf introduced him to our Royal Society, which printed his first paper in 1674. Swammerdam repeatedly examined his preparations, and any distinguished person, such as the duke of York or Peter the Great, who happened to visit Holland, was taken to look at Leeuwenhoek's microscopes as chief curiosities of the country. In these placid occupations his life was passed; in his eighty-fifth year he wrote his last published letter, as he says, with a torpid and trembling hand, and died at ninety-one.

Leeuwenhoek did not methodically study any science; his curiosity led him to examine a great variety of minute objects, and he found something new in every one. An attentive inspection, perhaps a drawing made by another hand, a few reflections, sometimes remarkably penetrating, and then he sits down to indite another page of the *Secrets of Nature.* Next week or next month he may be busy with something quite different. Desultory work like this reminds us of Hooke's *Micrographia.* Both inquirers resemble men who have found their way into a place rich in fragments of ore, and pick up whatever happens to catch their eye, without attempting to sink shafts or run galleries. When we are inclined to disparage Leeuwenhoek's hasty methods it is well to recollect that he initiated biological inquiries of the greatest interest, *e.g.* the parthenogenesis of aphids and the revivification of dried microscopic organisms, while he gave the first notices, or the first worth mention, of rotifers, Hydra, infusorians, yeast-cells and bacteria.

The microscopes which Leeuwenhoek made for himself were double-convex lenses of various power. So much light was lost that the higher powers were only effective when the object was transparent and directly illuminated by the sun. Leeuwenhoek found that the adjustment of one of his lenses to the object was too hard a task for inexperienced persons, and when he sent preparations to a distance it was his practice to devote a separate lens to every object, and fix everything in its place. His experience showed that the most considerable discoveries were made with lenses of moderate amplification.

The Royal Society formerly possessed a set of microscopes and objects, bequeathed by Leeuwenhoek, and described by him as "a small black cabinet, lackered and gilded, which has five little drawers in it, wherein are contained thirteen long and square tin boxes, covered with black leather. In each of these boxes are two ground microscopes, in all six and twenty; which I did grind myself, and set in silver; and most of the silver was what I had extracted from minerals, and separated from the gold that was mixed with it; and an account of each glass goes along with them."[1] This cabinet of microscopes, which Baker had before him when he wrote his *Microscope made easy* in 1743, disappeared long ago.

Leeuwenhoek had no means of measuring small lengths with precision, and his estimates are sometimes ludicrously wrong; his standard of comparison was a grain of sand, which he took to be the hundredth of an

[1] Weld's *History of the Royal Society*, I, p. 245.

inch in diameter. Dr. James Jurin, a London physician, improved upon this. A fine silver wire was wound in a close spire about a slender cylinder, such as a pin; the length of the spire divided by the number of the turns gave the diameter of the wire. Short lengths of this wire, strewn about the field of view, served as measures of length.[1] Benjamin Martin adopted an expedient which is still much used, that of a glass disk, ruled with fine lines and fitted in the focus of the eye-glass.[2]

The Tadpole[3]

The circulation occupies a large part of the letter, but Leeuwenhoek mentions some interesting details concerning other parts of the frog's history. He notes one use of the jelly which coats the eggs, viz. that it becomes loaded with minute air-bubbles, which, by rendering the eggs buoyant, expose them to the heat of the sun. The mouth of the tadpole with its many rows of teeth, the suckers and the external gills are described. Like Swammerdam, whose account was not yet printed,[4] Leeuwenhoek remarks that the young tadpole is entirely composed of cells ("globules").

Blood and the Circulation[5]

Malpighi (*supra*, p. 161) had extended the knowledge of the vascular system by showing that innumerable capillary vessels connect the arteries and the veins in the lung, the mesentery and the bladder of the frog.

[1] *Phil. Trans.*, No. 355 (1718) and *Dissert. physico-math.* 8vo. Lond. 1732, pp. 45-6.

[2] *New and compendious system of Optics.* 8vo. Lond. 1732, pp. 45-6.

[3] Epist. 65, *Arc. Nat.* (1688). Vol. II, pp. 163-172.

[4] *Supra*, p. 197.

[5] *Phil. Trans.*, 1674; Epist. 65-8, *Arc. Nat.* (1688-91). Vol. II, pp. 153-217.

Leeuwenhoek demonstrated the capillary flow more advantageously by using the tail of the tadpole and fish, the web of the frog's hind foot, the membrane of the bat's wing, &c. Malpighi in 1665 (*supra*, p. 163) had seen the red blood-corpuscles of the hedgehog, mistaking them however for fat-cells; Swammerdam at some unknown date (*supra*, p. 198) had found them in the blood of man and of the frog; Leeuwenhoek in 1673 examined a drop of his own blood, and saw red corpuscles floating in it; his discovery was published in 1674. He went on to show that the mammals examined by him contained circular red corpuscles, the birds, amphibians and fishes oval ones; that the frog's corpuscles are nearly colourless, but reddish when superposed, and exhibit a bright oval spot in the centre; further, that the redness of blood is due to their presence. He also found uncoloured corpuscles circulating in small transparent crustaceans.

Spermatozoa

None of Leeuwenhoek's own discoveries made quite so much stir in the scientific world as the discovery of the spermatozoa, which was popularly attributed to him. He tells us[1] that in the year 1677 a young physician named Hamm demonstrated the spermatozoa in his presence; Leeuwenhoek lost no time in transmitting the news to scientific friends and to the Royal Society. He supplied figures of the spermatozoa, some of them ludicrously like human beings, with heads, arms and legs.[2] It was Leeuwenhoek's belief throughout life that

[1] *Phil. Trans.* No. 142 (1678). The name of the discoverer is quoted by Leeuwenhoek as "Dominus Ham," by others as Ludwig van Hammen, who is said to have been a pupil of Leeuwenhoek and to have used a microscope made by him, and again as Stephen Hammen of Stettin.

[2] *Cont. Arc. Nat.*, Vol. III, p. 68.

the sperm was the veritable germ, which was only hatched by the female, and he was ready with arguments to prove that the "imaginary ova,"[1] as he calls them, have little or nothing to do with the first formation of the embryo. He also believed that he could distinguish two kinds of spermatozoa, which he set down with characteristic boldness as the germs of male and female embryos.

The Crystalline Lens[2]

Leeuwenhoek showed that the crystalline lens of vertebrates is composed of many thin laminæ, and that fibres radiating from the anterior and posterior poles form a three-rayed star in the lens of mammals.

Vertebrate Histology

There is perhaps no mention by earlier writers of the transverse striation of muscles,[3] the canaliculi of dentine, the fibres of the crystalline lens, or the cells of the epidermis, but it is hard to speak positively in such cases. Leeuwenhoek describes and figures the epidermic cells of his own skin; he took the cell-nuclei for the ducts of glands.[4]

The Compound Eye[5]

Hooke, Malpighi and Swammerdam had already made observations upon the compound eye of insects, but

[1] These "imaginary ova" were Graafian follicles. *Arc. Nat.* (1680). Vol. II, p. 27.

[2] *Arc. Nat.* (1684). Vol. II, pp. 66-81.

[3] *Phil. Trans.* (1681); *Anat.* (1682); *Arc. Nat.* (1683?). Vol. I, p. 45 (2nd pagination); Vol. II, pp. 30-1.

[4] *Epist. physiol.*, Epist. 43 (1717). Vol. IV, pp. 403-8.

[5] Epist. 83, *Arc. Nat.* (1694); Epist. 35, *Ep. phys.* (1717). Vol. II, pp. 434-7. Vol. IV, pp. 340-7.

Leeuwenhoek had his own remarks to offer, sometimes well-founded, sometimes not. He showed that repeated inverted images are formed by the corneal facets, but that we need not suppose each image to be separately perceived ; we do not see double because we have two eyes. He throws out the bold speculation that the hexagonal cells of the honeycomb are due to an impression received from the hexagonal facets of the bee's eye, a supposition which Swammerdam gravely refuted.[1] As a proof of the quickness of sight which may be conferred by a compound eye, he relates how he watched a swallow chasing a dragon-fly over the surface of a large pond, and how the swallow was baffled by the speed and unexpected turns of the insect, which kept it always several feet in front of the enemy. Leeuwenhoek points out that compound eyes are not peculiar to insects, but occur in crustaceans also.

Viviparous Reproduction by Unfertilised Aphids[2]

Leeuwenhoek remarked that expanding buds of currants, cherries and peaches were sometimes distorted, and the unexpanded leaves crumpled. On close examination he found that the affected buds were beset by aphids or plant-lice. Intending to investigate their life-history, he sought for eggs, but could find none. When he opened the bodies of the aphids, he found to his surprise no eggs but young aphids, resembling the parent in all but size. An aphid no more than a fortnight old might contain as many as sixty young ones, so that propagation went on with extraordinary rapidity. The birth of the young was observed. No males were

[1] *Biblia Naturæ*, p. 490, and *supra*, p. 188.

[2] Epist. 90, *Arc. Nat.* (1695) ; Epist. 94, *Cont. Arc. Nat.* (1695) ; Epist. 104, *Cont. Arc. Nat.* (1696) ; Epist. 134, *Ep. Soc. R.* (1700). Vol. II, pp. 486-502 ; 2nd pagination, pp. 9-11, 148-156 ; Vol. III, 263-280.

found, and Leeuwenhoek concluded that the viviparous females were unfertilised. Winged aphids appeared among the rest, and the gradual protrusion of wings beneath the skin was studied ; both winged and wingless forms were viviparous. Leeuwenhoek evidently believed that all, when mature, acquired wings, as his experience of other insects would naturally suggest, but his supposition is not confirmed by observation.

He notes all sorts of facts concerning aphids in the simple order of discovery, the casting of the skin, the excretion of honey-dew, hitherto believed to fall from the sky, the different species and the restriction of each to a particular plant, the Hymenoptera parasitic upon them, &c. A great deal of new and surprising information was suddenly thrown out for the consideration of naturalists in these unmethodical and almost extempore letters. One figure of an aphis shows the antennæ, the proboscis and the abdominal tubes, with a drop of liquid exuding from one of them.

The inquiry into the viviparous reproduction of unfertilised aphids was afterwards pursued by Réaumur, but soon handed over to Charles Bonnet (*infra*, pp. 284, 286).

Hive-bee

Leeuwenhoek gives a very fair set of figures of the sting of the bee, and also of the mouth-parts, though in the latter case the bases are not shown.[1]

Fleas[2]

Leeuwenhoek hatched the eggs of fleas, saw the larvæ curled up within them and afterwards observed their

[1] *Phil. Trans.* Nos. 94 and 97 (1673).

[2] *Arc. Nat.* (1680, Epist. 76, 1693). Vol. II, pp. 20, 324-343.

emergence. He fed his larvæ on fresh-killed flies, and after twelve or thirteen days observed them change to pupæ. He utterly repudiates the doctrine of Kircher that fleas are generated out of dust and filth. If Kircher is to be trusted, he says, we must suppose that fleas are generated in Italy after a very different fashion from that which prevails in Holland.

Hooke, Roesel and De Geer are among the other naturalists who investigated the flea.

Ants[1]

Ants are sometimes seen to carry about large, white, rounded bodies, which are popularly known as "ant-eggs." Leeuwenhoek copies and criticises Griendel's figure of one of these, which shows an eight-legged ant walking about in a relatively large chamber. He shows that the supposed egg is really a pupa, and that the real eggs are much smaller. He rejects the belief that ants store up food against the winter, which is nevertheless true of the ants of warmer countries.

The Nature of Cochineal

Cochineal had been regularly grown by the Indians of Mexico before the arrival of the Spaniards, and since careful transfer of the insects from old Opuntia-plants to new ones is an essential part of the process of cultivation, they must have known, one would think, what was the real nature of the grains exported to Europe. It was however long debated by the learned men of England, France and Holland whether cochineal-grains are insects or fruits. One anonymous writer of 1668 gave it as his opinion that the dye-stuff consisted, not of fruits of the

[1] *Cont. Epist.* (1687). Vol. I, pp. 75-90.

"prickle-pear," but of insects "engendered of the same."[1] Lister[2] conjectured that it might be a sort of kermes, which was a near approach to the truth. Swammerdam[3] satisfied himself, both by the examination of soaked cochineal and by the reports of unnamed persons, that cochineal is composed of the dried bodies of insects, but his report was not published till 1738, when the question had been finally settled. Tyson[4] gave a figure of the "cochineel-fly," which exhibits decisive characters. Robert Boyle in 1685 invoked the judgment of Leeuwenhoek, who replied that cochineal was a fruit containing a multitude of small seeds. Boyle sent back the opinion of a governor of Jamaica, to the effect that the so-called seeds were really the eggs of an insect. Upon this, Leeuwenhoek re-examined the cochineal, and found distinct proofs of its insect-nature, which were published in 1687. Still the dispute went on. At last a lawsuit was instituted in Amsterdam to decide a wager on the point; depositions were taken in Mexico; the result was to convince everybody that cochineal is really an insect.[5]

Spiders[6]

Leeuwenhoek is at his best when occupied with a small animal of complex structure. To work out the whole anatomy and trace the whole life-history was too much for his patience, but he may be counted upon for good or fair figures (not drawn by his own hand), ingenious experiments, and bold interpretations, which

[1] *Phil. Trans.*, No. 40 (1671). [2] *Phil. Trans.*, No. 87 (1672).

[3] *Biblia Naturæ*, p. 420. [4] *Phil. Trans.*, No. 176 (1685).

[5] *Hist. Nat. de la Cochinelle, justifiée par des documens authentiques.* Amst. 1729.

[6] *Epist.* 138, 143, *Ep. Soc. R.* (1701-2). Vol. III, pp. 312-345, 375-9.

sometimes give the clue to future discoveries. It is not surprising therefore that a spider should furnish the occasion for one of his most interesting letters, written, we may remark, in his sixty-ninth year.

He happened to begin with the microscopic examination of a spider caught in his house, and the first thing which he remarked was the blood coursing through the legs. The spider managed to escape, so Leeuwenhoek went on to examine the next spider which came to hand, a garden-spider. He had remarked that when dropping by its line from a height, a spider may often be seen to pause, and support itself by grasping its line with one of its hind feet. This led him to look carefully at the structure of the foot. He found a pair of serrate claws, and between them a smaller, non-serrate claw, which he supposed to be that which grasped the line. Next he examined the parts of the mouth. The poison-fangs are well described and figured, the piercing terminal joint, the double row of spines between which the terminal joint folds up, and the minute orifice of the poison-duct being all clearly shown. The four pairs of eyes are drawn. He remarked that captive spiders, when fairly matched in size, fight with great determination, and that if the central part of the body is wounded by the poison-fangs, the injury is mortal.

We find here the first tolerable account of the spinning apparatus of the garden-spider. Having fixed a spider on its back, Leeuwenhoek with a pair of forceps drew out a thread, noting that it was composed of innumerable parallel filaments. At the extremity of the abdomen he found five triangular valves meeting in a point; the uppermost (really the tip of the abdomen) emitted no threads, but the other four (the spinners; there are actually six, but two of them are concealed) bore hair-like spinnerets, each of which emitted its own filament.

In October spiders were enclosed separately in glass tubes to see whether they would lay eggs. Some of them spun webs, within which they laid rounded masses, half an inch in diameter, composed of yellow eggs. Leeuwenhoek found to his surprise that the eggs are not passed out from the extremity of the abdomen, but from the under surface of its fore end. In a warm room little spiders hatched out during the winter, before any food was ready for them ; some devoured the bodies of their companions or the unfertilised eggs. Letter 143 tells how an unnamed friend had seen what were no doubt the palps of the male spider used to fertilise the female ; anatomical and other considerations made it impossible for Leeuwenhoek to accept this true story.

An Inquiry into the Generation of the Edible Mussel (Mytilus)[1]

It was with the intention of refuting the doctrine that shell-fishes are generated from the mud of the sea-bottom that Leeuwenhoek took up this inquiry ; he would, as he says, have found it just as easy to believe that a whale could be generated from mud. He quotes testimony to prove that mussel-spawn or milt at certain seasons makes the sea white, and thinks it certain that the spawn settles at the bottom, to replenish the exhausted mussel-beds. But until the spawn was clearly traced to the mussel the evidence did not carry conviction. Leeuwenhoek dissected mussels, and searched in vain for their eggs, though he found other things to interest him, such as the play of cilia on the gill, which

[1] *Epist.* 83, *Arc. Nat.* (1694). Vol. II, pp. 417-439.

he had previously observed in the oyster,[1] the byssus by which the mussel anchors itself, and the crystalline style, which he supposed to be accessory to reproduction. At last he found what he took to be the eggs adhering in clusters to the outside of a mussel-shell. Each egg occupied one of a number of cells which were arranged in regular rows, and was furnished with sixteen long filaments disposed in a circle. What he had really found was a Polyzoan colony (Membranipora); his embryos were the polyps, and the circle of filaments the lophophore.

He thought that the crystalline style of the mussel was used to arrange the eggs (polyzoan polyps) on the outside of the shell, and compared it to the ovipositor of an insect, but in a postscript he assigns this function to the foot.

The naturalist's attention was next caught by what he called pustules on the mussel-shells, which a glance at his figure shows to have been acorn-barnacles. On the top of the conical barnacle-shell he found an aperture guarded by two valves, which opened when dipped in sea-water, but closed and retracted when touched with a needle. The eggs of the barnacle were discovered; other individuals which contained no eggs he naturally but erroneously took to be males.[2]

In spite of his unlucky mistakes, Leeuwenhoek, as we have seen, drew from his unsuccessful quest of mussel-eggs two capital discoveries. Early investigators no doubt labour under special difficulties, but they also enjoy advantages of their own.

[1] *Phil. Trans.*, Feb. 1681-2. Heide (*infra*, p. 213) announced the same discovery in 1683.

[2] A description of a stalked barnacle (Lepas), about as good as Leeuwenhoek's acorn-barnacle (Balanus) was given a hundred years earlier by Fabius Columna in his *Piscium aliquot plantarumque novarum historia*. 4to. Neapoli. 1592.

A countryman of Leeuwenhoek, Antony van Heide, who practised medicine at Middleburg, had several years before this written an account of the anatomy of the mussel,[1] which we might have expected to be quoted in this letter ; it was valued by Martin Lister and others. Heide described the crystalline style and the foot, and believed himself to have discovered the " motus mirandus " (ciliary motion) in the gill of the mussel, but Leeuwenhoek's description of the cilia of the beard of the oyster is a year or two earlier (see p. 212).

Anodon Embryos[2]

Among the many objects which engaged the attention of Leeuwenhoek was Anodon. His account is slight and unsystematic. He mentions the ciliary motion in the gills of this and a marine bivalve (Mactra ?), but the best of his attention was bestowed upon the eggs. Ovarian eggs were studied by the microscope, and the naturalist, his daughter and the draughtsman watched the rotating embryos for hours together with great delight. He was aware that the eggs are transferred from the ovary to the gill.

Swammerdam too made a cursory examination of the structure of Anodon, from which he prepared a meagre and largely erroneous description.[3] Poupart and Méry (*infra*, p. 234) were the first to treat the anatomy of Anodon with any thoroughness.

[1] *Anatome Mytili, Belgice Mossel, structuram elegantem ejusque motum mirandum exponens.* Prefixed to a *Century of Medical Observations.* 8vo. Amst. 1683.

[2] *Epist.* 95, *Cont. Arc. Nat.* (1695). Vol. II, pp. 14-29 (2nd pagination).

[3] *Biblia Naturæ*, p. 190, pl. X, figs. vi, vii.

Rotifers[1]

Having macerated bruised pepper in rain-water, Leeuwenhoek found animals of the kind which we now call rotifers in the infusion ; the description agrees with that of Rotifer vulgaris, but few details are furnished. He observed that the tail-end was furnished with a forked grasping apparatus, the head-end with a peculiar organ which set up whirlpools in the water. The animalcules were able to creep like leeches, attaching each end by turns to the supporting surface. Near the middle of the body was a structure which seemed to pulsate, and was taken by Leeuwenhoek for the heart, but it was no doubt the mastax, or gizzard.

Fifteen years later Leeuwenhoek returns to his rotifers.[2] In a leaden gutter the rain-water took a reddish colour, and when examined by the microscope was found to contain red or green rotifers. Some contained embryos, one of which was seen to free itself and swim about. Hot weather came on, and the gutter dried up. When all the water disappeared there was no sign of life, but it occurred to Leeuwenhoek to put a little of the dry mud into a glass tube and add rain-water. In an hour rotifers were seen clinging to the glass or swimming about in the water. They revived equally well when boiled rain-water was used to moisten the mud, and even after the mud had been kept dry for twenty-one months.

Leeuwenhoek sagaciously remarks that the cuticle of the animalcules must be singularly impervious to water, for if the tissues had really become dry they must have perished. The possibility that minute organisms may be

[1] *Cont. Ep.* (1687). Vol. II, pp. 94-6.

[2] *Epist.* 144, *Cont. Arc. Nat.* (1702). Vol. III, pp. 380-394; *Phil. Trans.*, Nos. 283, 295, 337 (1703-13).

superficially dried, transported by wind or flying animals, and afterwards revived by moisture, explained many of the cases which had been thought to prove the generation of animals from putrid matter.

The means employed by gutter-rotifers to protect their bodies from complete desiccation are more elaborate than Leeuwenhoek was aware of. They not only contract their bodies, but seal up the ends with gelatinous plugs; if the process of drying is too rapid for this, as for instance when naked rotifers are dried on a glass slip, they perish at once. It has been found possible, by placing strips of paper in a wet gutter, to procure groups of rotifers glued to one another and to the paper, as many as a hundred together. When thus protected, they show remarkable power of resisting extremes of temperature; they can also endure the vacuum of an ordinary air-pump, though not the more complete exhaustion of the Sprengel pump, and they revive after drying in vacuo over sulphuric acid.[1]

Long after Leeuwenhoek's death Spallanzani[2] showed that the moss-haunting Tardigrades exhibit the same power of enduring superficial drying; so do Infusoria and other Protozoa, moulds, bacteria, seeds and eggs. Preyer has compared the organism whose life is thus temporarily arrested to a clock which has been wound up and then brought to a stand; a push is enough to set it going again. But an organism which has once been thoroughly dried is like a clock whose spring is broken.

In his Royal Society papers Leeuwenhoek describes and figures Limnias ceratophylli and Melicerta ringens.[3]

[1] H. Davis, *Month. Micr. Journ.*, 1873, p. 287; Hudson and Gosse, Vol. I, p. 95; Hartog, *Camb. Nat. Hist.*, Vol. II, p. 227.

[2] *Opuscules de physique animale et végétale* (1776).

[3] *Phil. Trans.*, Nos. 295 (1705) and 337 (1713). In 1713 the naturalist must have been over eighty years of age.

He points out that what seem to be two wheels are really the lobes of a single disk, and that the ciliary play is continuous, the direction being counter-clockwise. He discovered that the tube of Melicerta is composed of pellets moulded by the ciliary disk, and laid one by one on the edge of the growing tube.

Hydra [1]

Leeuwenhoek was the first to discover and describe (very briefly) this deeply interesting animal. He tells us that it possesses from six to eight horns (tentacles), which can be extended so far that under the microscope they seemed to be several fathoms long! He found two small polyps attached to a parent, and saw them become free, thus anticipating the most important fact in Trembley's discovery, though forty years were to pass before its significance could be perceived. A parasite (Trichodina) was seen running about on the polyp. The figure of Hydra, though recognisable, is not good.

Unicellular Animals (*Protozoa*)

Hooke's Rotalia and Leeuwenhoek's Nonionina (the latter found in the stomach of a shrimp) were the first recent Foraminifers to be noticed. The observations of living Rhizopods begins with Roesel, who in 1755 described and figured an Amœba. Except for a slight notice of Euglæna by Harris (1696), the study of the flagellate Infusorians begins with Leeuwenhoek's account

[1] *Phil. Trans.*, No. 283 (1702). Better figures are given shortly afterwards by "a gentleman in the country" (*Phil. Trans.*, No. 288, 1703). The chief faults are that the body of the Hydra is shown as segmented, and that there is an outlet at the attached end. The same paper contains the earliest figure of a diatom (Tabellaria) which I have met with; the writer at first took the cells for "salts" (crystals), but afterwards thought that they might be plants.

of Volvox, Hæmatococcus and Polytoma. It is again Leeuwenhoek who starts the literature of the ciliate Infusorians by his descriptions of organisms found in rain-water and pepper-infusions, of species parasitic upon the frog, and of Vorticellid colonies. He remarked that no Infusorians could be found in fresh rain-water collected by a leaden gutter, but that they appeared in the course of a few days.

Volvox[1]

In water from a ditch Leeuwenhoek remarked a number of green spheres, which moved slowly about, rotating as they moved. Closer examination showed that each sphere was composed of a multitude of particles beset with small protuberances. Several small spheres might often be seen within a single large one, and Leeuwenhoek was fortunate enough to see them escape one by one through an opening in the parent sphere, until none remained behind; as soon as they became free they began to swim about; he detected the germs of a third generation within the daughter-spheres. The small spheres grew rapidly after liberation. Notwithstanding its power of locomotion, the mode of propagation led Leeuwenhoek to decide that Volvox was more like a plant than an animal.

Henry Baker in 1753 showed that the protuberances which Leeuwenhoek had seen on the green particles of Volvox bore "short moveable Hairs or Bristles" (pairs of cilia), and that these set up the movements of the spheres. The name of Volvox was given by Linnæus (1758).

[1] Epist. 122, *Ep. Soc. R.* (1700).

Reverse Planting[1]

Constantyn Huygens, father of the celebrated physicist and mathematician, told Leeuwenhoek that the gardeners of the Elector of Brandenburg were in the habit of planting trees upside down. Leeuwenhoek said that twenty years earlier he had bent down a vine shoot and caused it to enter the earth; when it had rooted itself, the connection with the parent plant was cut, and thus a second vine was obtained; he had treated branches of gooseberries, currants and willows in the same way with complete success, and experiments were now made on lime-trees. The young tree was laid on the ground, the roots at one end and the branches at the other being sunk in the earth. In time the buried branches began to send out roots. As soon as these were well established, the roots were cut through, and the trunk raised to a sloping position, the original lower end being now uppermost, and the original upper end rooted in the earth. Buds formed on what had been the roots, and in the course of a little more than a year grew into branches of good length.

Ray and Willughby[2] had made similar experiments. Slips of willow were set in the ground with the growing ends downwards; briars which had taken root at the small end were cut through; all grew and flourished.

Malpighi[3] had observed that shoots of fig, prune and bramble will grow if planted upside down, and yield trees, though not full-sized ones. His conclusion is given in these words, "unde alimenti via invertitur," (the path of the nutritive sap is reversed).[4]

[1] Epist. 64, *Arc. Nat.* (1688). Vol. II, pp. 141-6.

[2] *Phil. Trans.*, No. 48 (1669). [3] *Anatomes Plantarum Idea*, p. 13 (1671).

[4] Theophrastus mentions the growing of pomegranates upside down (*De causis plantarum*, Bk. II, ch. 9).

Reverse planting became a favourite diversion in the eighteenth century, and Miller's *Gardener's Dictionary* gives instructions for practising it.

The interest of such experiments is not solely practical. It is well-known that brambles, Forsythia, &c. enlarge at the extremities of the rooting branches; some Aroids, &c. form tubers on their aerial branches. Further investigation may possibly throw light, not only upon the origin of tubers, but also upon that reversal of the sap-current which struck Malpighi as remarkable.

Minute Structure of Wood

Leeuwenhoek's figure of a piece of lime-wood cut longitudinally is believed to be the earliest representation of dotted ducts.[1]

Yeast[2]

The first microscopic study of yeast-cells was made by Leeuwenhoek. He remarked that they give off bubbles in great numbers, as do crabs' eyes when placed in vinegar, and that many of them are compound, consisting of several particles united together; he did not however discover that the compound globules are produced by budding. Upon a few rapid observations he founded a number of speculations, which are set down without much attempt at verification. Thus he states that not only all yeast-cells, but blood-corpuscles also, consist of six component particles apiece. Finding that a tube of rain-water, when set in a window, contained after a few days green globules, he persuaded himself that these too were almost all sixfold. He concludes that the component particles come from the air. The

[1] Epist. 74, *Arc. Nat.* (1692). Vol. II, p. 302, pl. 289, fig. 19.

[2] *Arc. Nat.* (1680). Vol. II, pp. 1-14.

substance of a burning candle turns to water, which may coagulate and ultimately compose the sixfold corpuscles, perhaps combining with soot particles as it descends. The really important discovery made by Leeuwenhoek was that fermenting wort contains rounded bodies which give off bubbles of gas.

Bacteria[1]

In 1683 Leeuwenhoek wrote a letter to the Royal Society which contains the first mention of bacteria. He had been writing and speculating upon saliva, and had searched the saliva of the human mouth for animalcules without finding any. It then occurred to him to ask whether the teeth might lodge animalcules discharged from the salivary ducts. He tells us that, though his own teeth were scrupulously clean and particularly sound for his age (about fifty), the lens revealed a white deposit upon them. This deposit was found to contain minute rods, some of which showed either a steady or a gyratory movement. Others were very minute, of rounded form, and moved with remarkable velocity. The largest of all, which were either straight or bent, were motionless. The teeth of an old man, which were never cleansed, contained among others large rods which exhibited snake-like undulations. Rubbing the teeth with strong vinegar did not kill the moving bodies, but they became quiescent when detached and placed in a mixture of vinegar and saliva, or vinegar and water. Nine years later Leeuwenhoek returned to the subject. Living particles were no longer met with in his teeth, and he was at a loss to explain why, until it occurred to him that he was now accustomed to drink hot coffee

[1] *Phil. Trans.*, No. 159 (1684), also *Arc. Nat.* (1683); Epist. 75, *Arc. Nat.* (1692); Epist. 110, *Ep. Soc. R.* (1697). Vol. II, pp. 39-43, 307-311, Vol. III, 35-6.

every morning. This, he thought, might have killed the animalcules, and his conclusion was confirmed by finding that on the back teeth, which were less exposed to the hot drink, plenty of them were still to be found. In this letter of 1692 he describes and figures angulated rods which moved by rotation on their long axes. In 1697 he tells how he pulled out a decayed tooth, and found that the cavity abounded in moving particles.

Here Leeuwenhoek's study of bacteria comes to an end, except that in 1713, being then over eighty, he speculated a little as to the possibility of bacteria being introduced into the mouth by the rinsing of drinking-vessels in water abounding with infusorial life. After Leeuwenhoek nothing more was done to elucidate the bacteria till 1786, when O. F. Müller described and figured several kinds.

A Microscopical Fraud

It may relieve the reader's attention to mention a curious fraud which Leeuwenhoek exposes. A long-forgotten writer, Noel Argonne, who used the nom-de-plume of Vigneul-Marville, and whom Voltaire describes (not quite accurately) as the only Carthusian monk who ever made a contribution to literature, relates[1] that on arriving in London he and his friends were solicited to buy curiosities. Among these was a microscope, *i.e.* a lens mounted on tortoiseshell, which he was assured was "si excellent, qu'il ne faisoit pas seulement voir les cirons (mites) les plus imperceptibles; mais aussi les atomes d'Epicure, la matière subtile de Décartes (*sic*), les vapeurs de la terre, celles que notre corps transpire, et les influences des Astres." On looking at the exhibi-

[1] *Mélanges d'histoire et de littérature.* 12mo. Paris. 1699-1701. Vol. II, p. 426.

tor's coat from a distance of five or six paces, an infinity of little worms seemed to be devouring the cloth. When the same trick was practised upon Leeuwenhoek, he discovered that holes had been ground in the lens, into which minute objects could be placed. The objects, whatever they were, would of course appear enormous when judged to be several feet off.[1]

ESTIMATE OF LEEUWENHOEK

The modern biologist, whose task is lightened by the improvements of many preceding generations, who has at command microscopes which do not fatigue the eyes and text-books which summarise what is already known, finds it hard to put himself in the place of the minute observer of the seventeenth century. The animal and vegetable kingdoms seemed vast and intricate even to those who never gave a thought to animals smaller than insects, or to plants smaller than duckweed. Before the objects easily visible without a lens had been tolerably classed, the microscope revealed a new world of minute organisms, many of them small enough to be wafted by the lightest summer breeze. We need not wonder that Leeuwenhoek should have studied many things superficially; it is enough for his fame that he studied some things carefully, kindled curiosity, and opened out inquiries which others have pursued much farther.

No one would reckon Leeuwenhoek among the great philosophers. He held, however, decided opinions on two great biological questions which already engaged attention, the question of spontaneous generation, and that of the origin of species. The reasoning of Redi, supported by his own observations, convinced him that when living things seemed to arise independently in

[1] Epist. 139, *Ep. Soc. R.* (1701). Vol. III, pp. 346-354.

tissues or infusions, they had always been really introduced from without. He had no doubt of the fixity of species, and expressed himself in language very similar to that employed by Linnæus in the next generation :—"Omnia animalcula," says Leeuwenhoek, "quantumvis vilia ac despecta, originem suam debent illis animalculis, *quæ initio rerum creata fuere, iisdemque gaudent perfectionibus*."[1]

[1] *Cont. Arc. Nat.*, Epist. 121 (argumentum).

SECTION VI. EARLY STUDIES IN COMPARATIVE ANATOMY.

A CONNECTED history of Comparative Anatomy, which is impossible in a volume like this, would be bound to dwell upon the labours of the anatomists and physiologists of the period between 1545 and 1650. Some of the most eminent belong to the school of Padua, which was founded by Vesalius and continued by Falloppio and Fabricius; Coiter, a Dutchman, and our own William Harvey got their training in Padua. The pupils of Falloppio and Fabricius, besides anatomists, who in other cities of Italy or of France pursued the same studies, compared monkeys and animals of still lower grade with man; some attended to the development of mammals and birds. The naturalist Belon set the example of close comparison by figuring on opposite pages the skeletons of a man and a bird, and lettering the corresponding bones by the same letters; this was as early as 1555. In the second half of the seventeenth century the succession was kept up by Malpighi in Italy, by Perrault in France, and by Tyson in England.

FRANCESCO REDI[1]

1626–1698

Osservazioni intorno alle Vipere. 4to. Firenze. 1664.

Esperienze intorno alla generazione degl' Insetti. 4to. Firenze. 1668. Translation by Mab Bigelow. Chicago. 1909.

Esperienzi intorno diversi cosi naturali. 4to. Firenze. 1671.

Osservazioni intorno agli animali viventi che si trovano negli animali viventi. 4to. Firenze. 1684.

Redi, a naturalist of wide curiosity, is remembered as having examined a variety of animals, and investigated experimentally the question of spontaneous generation. He studied at Pisa, attracted notice by his talents and learning, and was made physician to the dukes of Tuscany, among others to Cosmo III, Swammerdam's duke.

Observations on Vipers[2]

Once when a large number of vipers had been procured in order to make a theriacum for the duke of Tuscany, the question was raised whether the viper's tooth merely made a wound, or whether poison was injected. One learned man assured the company on the authority of Pliny and Galen that the smallest drop of viper's venom, if swallowed, would kill man or animal. A viper-catcher, who was present, took some of the fresh venom, put it into a glass of water, and swallowed it, offering to drink as much more as they pleased. Venom was administered by the mouth to various animals without effect. Viper's gall, reputed to be a deadly poison, was spread harmlessly upon fresh wounds. Animals were caused to swallow all the liquids which could be extracted from a viper's head, but

[1] Redi does not come very well into this section; I can only plead that he would not have come better into any other.

[2] *Oss. intorno alle Vipere.*

no ill effects were observed. Only when fresh venom was applied to wounds did death ever follow. Redi, who no doubt made the experiments, went on to examine the poison-gland and poison-tooth of the viper. He seems to have concluded that the tooth was only channelled, instead of being perforated by the poison-duct, as it really is.

The Generation of Insects[1]

This treatise is historically important because it dispelled ancient superstitions by direct experiments. It is a pity that Redi's decisive proofs, which might have been related very concisely, should be loaded with two hundred pages of discussion. The question to be settled was whether, as Aristotle had taught, insects could be generated spontaneously by putrefaction. Redi proved experimentally that the flesh of the same animal might yield more than one kind of fly, while the same fly might be hatched from different kinds of flesh. He saw the flies laying their eggs in flesh, and dissected eggs out of their ovaries. The larvæ and pupæ of common flesh-haunting flies were noted and compared. It was thus proved that the generation of a particular maggot or fly in flesh did not depend upon the kind of flesh, but on the kind of fly which had got access to it. Flesh was placed in bottles, some of which were left open, while others were closed with paper or gauze. The open bottles produced larvæ, pupæ and flies, while the closed bottles produced none, though flies, attracted by the odour, strove to enter, and in some cases laid their eggs on the gauze.

Among the baseless fables handed down from ancient authors was the Bugonia fable, well-known to readers of

[1] *Esp. intorno alla generazione degl' Insetti.*

the fourth Georgic. Redi demolished this by pointing out that flies, and not bees, appear in corrupt flesh. He did not however go on to explain the old belief by pointing out that one flesh-haunting fly (Eristalis) is so bee-like that it easily deceives an ordinary observer. Pliny's statement that buried crabs produce scorpions was tested, but not confirmed; it was the same with another belief drawn from Pliny, viz. that the hind legs of the frog are formed by the splitting of the tail of the tadpole.

Unfortunately Redi did not trust in every case to the experimental method, which he had indirectly learned from Galileo; his scientific reputation suffers by one unconfirmed speculation. Having tried to explain insect-galls on the supposition that they always begin with an egg laid in the tissues of the plant, he was perplexed by the cases in which no hole or scar showed where the egg had been passed in. In order to explain how a grub might occupy a nut whose shell seemed to be intact, Redi threw out the suggestion that the same virtue which produces flowers and fruits may also produce grubs, and this guess he was rash enough to publish without verification. By the facile hypothesis of a "vivifying principle" he explained the worms in the heads of sheep and deer. One of his pupils, Vallisnieri, afterwards showed how the egg was brought into the rose-gall, while Malpighi examined the young nut, and found both the hole and the egg. Redi was then obliged to apologise for his random guess. In spite of a few unlucky mistakes he did good service by furnishing a simple explanation of cases of propagation which had been reputed mysterious. His experiments impressed Swammerdam, Leeuwenhoek and Ray, and before many years had passed scientific minds at least were persuaded that only microscopic organisms, and perhaps some internal parasites, originated spontaneously. The rooting-out of the theory from these last refuges does not come within the period covered by our history.

Birds' Gizzards[1]

Redi ridicules Ælian's explanation of the swallowing of stones by the crane, viz. that they serve partly for food and partly for ballast, for the stones, he says truly, are found in birds that never fly. Borelli had investigated the matter, and found reasons for supposing that the stones aid in digestion. He passed glass bulbs into the gullet, and found that they were pulverised in the gizzard. Redi shows that solid glass balls and bullets are scratched; open bulbs, if they happen to pass through uninjured, are filled with an acid fluid, which he takes to be the secretion of the œsophageal glands. Trituration, he remarks, does not suffice for digestion; there must be "fermentation" as well.

Life-history of the Eel[2]

Redi satisfied himself by personal inquiry that eels visit the sea in autumn, for the purpose, as he supposed, of egg-laying. He had also seen the elvers ascending the rivers in spring. Aristotle had taught that eels lay no eggs, and may perhaps be generated from earthworms.

There is much more to tell about Redi, but we must content ourselves with glimpses of his work.

[1] *Esp. intorno diversi cosi naturali.*

[2] *Oss. intorno agli animali viventi, &c.*

PERRAULT AND HIS COLLEAGUES IN THE FRENCH ACADEMY OF SCIENCES

The French Philosophical Transactions (*Mémoires de l'Académie des Sciences*), from 1666 to the middle of the eighteenth century, contain many curious dissertations on natural history. The early volumes, which are of most historical interest, are unfortunately rare, but pretty full abstracts are to be found in Berryat, *Recueil des Mémoires, partie Françoise.*

The writers from whom the following notes are taken will first be enumerated :—

(1) Claude Perrault (1613-88), though educated as a physician, turned architect, translated Vitruvius, and designed the colonnade of the east front of the Louvre. He was elder brother to Charles Perrault of the *Contes des Fées.* French historians of science reckon Perrault as the reviver of Comparative Anatomy, but this is to ignore the anatomists of the school of Padua, besides Eustachio, Coiter, Riolan and Swammerdam.

(2) Jean de Méry (1645-1722) was educated as a surgeon, but anatomy and natural history absorbed his attention to such a point that he neglected practice and society. His anatomy of the ear and his account of the fœtal circulation gave him a great professional reputation.

(3) François Poupart (1661-1709), though nominally a surgeon, was devoted to natural history, geometry and philosophy. He was an excellent anatomist and a close observer, who long endured the neglect which befalls those who fail to behave like other people. It was a surprise to many when this retiring and ill-drest man was brought into the Academy of Sciences at the time of its reorganisation in 1699.

(4) Geoffroy the younger (Claude Joseph, 1685-1752) was younger brother to Étienne François Geoffroy, and uncle to Étienne Louis Geoffroy, who described the insects of Paris.

(5) Joseph Pitton de Tournefort (1656-1708), nephew to the great physician, Fagon, was a celebrated botanist, professor of botany at the Jardin du Roi. He was a great traveller, who explored the Alps, the Pyrenees, Greece and Asia Minor in search of plants. In this and other ways he acquired a great knowledge of flowers, though he was only superficially acquainted with their structure; he rejected, for instance, the sexuality of flowering plants. His system, which was long popular, and was only driven out by the Sexual System of Linnæus, was founded on the general form of the flower. Like Ray, Tournefort retained the old division into trees and herbs; he abandoned or else overlooked Ray's division into Monocotyledons and Dicotyledons. His classification was methodically exhibited; the genera, which he was long thought to have invented, had their characters clearly set forth, while figures of the flower and fruit of every genus gave welcome help to the reader. He succeeded also in retaining a number of natural families which had been established by his predecessors. Tournefort's patron, the abbé Bignon, is commemorated by our Bignonia (so named by Tournefort); Aubriet, the artist who accompanied him to the east, and who drew the beautiful figures of plants for his *Elements of Botany*, by our Aubretia.

(6) During the first half of the eighteenth century Réaumur (see p. 245) was a leading contributor to the *Mémoires de l'Académie des Sciences*, discoursing upon the growth of molluscan shells, the locomotion of

mollusks, starfishes and medusæ, the fixation of Mytilus, the silk of spiders, and a world of other things.

Comparative Anatomy, chiefly of Mammals

Perrault's descriptions and dissections of animals which had been kept in the royal menagerie were reprinted in two magnificent folios,[1] intended to set forth the glory of Louis XIV as well as the wonders of nature. As a rule they do no more than set down the facts of structure. Luminous general propositions are not to be expected in an age when museums were few, and anatomical records meagre. Perrault's *Essais de Physique*[2] contain popular discussions on the mechanism and senses of animals. Here are found short notices of the retractile claw of the lion, the pointed papillæ on its tongue, the barbs and barbules of a feather, the ruminant stomach, the spiral valve of a shark's intestine (previously described by Swammerdam and Willughby), the tracheæ of an insect, &c.

Duverney (1693) contributes a comparative study of the human hand and the lion's paw.

Birds

Perrault (1666, 1671-6) investigates with much success the structure of feathers, illustrating the subject by excellent diagrams, drawn as if from enlarged solid models, which show the barbs and barbules of a flying bird and an ostrich.[3] He explains the difference between the proximal and distal barbules, the use of the hooks, and

[1] *Mémoires pour servir à l'histoire naturelle*, Paris, 1671-6.

[2] *Physique* is not limited to what we call *Physics*, but includes all the physical and natural sciences.

[3] Perrault's figure of an ostrich feather is copied in Owen's *Comparative Anatomy*, Vol. II, p. 234.

the interlocking, which he compares to that of a latch and its hasp. All these features are illustrated by an excellent model, better than those usually given in recent text-books. Perrault dwells too on the advantages of the curvature of the vane, on the power of inclining the wing-quills, and on the facility with which ruffled barbs can be adjusted by stroking; in the ostrich, as he shows, the barbs are unconnected, and the quills cannot be variously inclined. He points out the action of the head, neck and legs in the flight of large birds, and dispels the fallacy that birds fly by virtue of their low specific gravity.

Poupart (1669) figures late stages of developing feathers, showing among other things the formative papilla and the pile of dry cones (German *Seele*), into which it ultimately shrinks.

Méry (1689) describes the respiratory movements of a bird. He remarks that in a live goose the thorax dilates during inspiration, the sternum receding from the backbone, and the ribs from one another. The air-sacs, which had been described and figured by Perrault, become distended as the thorax expands.

The most interesting bird-paper of this age is Méry's description of the woodpecker's tongue (1709). Borelli and Perrault had already treated the same subject, but Méry aspired to give a more exact description than either of his predecessors, and his account has become celebrated as a masterpiece in the delineation and interpretation of natural contrivances. The adaptation which he explains makes it possible for the woodpecker to protrude at pleasure a stiff and slender tongue, long enough to probe the burrows of wood-boring insects. When the mouth is opened, nothing is seen of the tongue-apparatus except a pointed, horny scale, which [in the purely insectivorous woodpeckers] is armed with minute, backward-pointing teeth [Méry says six on each side, but the number may be much greater than six]. To render the tip of the tongue yet more effective in picking up insects, it is covered with a viscid fluid, the secretion of large salivary glands. The hyoid bone, which, as usual, supports the tongue, is straight, slender, and about two inches long.[1] It gives off behind a pair of very slender branches (cornua), which are no less than six and a half inches long. When the tongue is at rest, the cornua curve round the sides of the neck, pass over the top of the head, and then, bending to one side, end together in the right nostril [sometimes in the left one]. The top of the skull is excavated by a groove, in which the cornua lie, and the hyoid, with the bases of its cornua, are enclosed in a sheath, whose cavity opens into the mouth.[2] Méry goes on to describe the muscles of the tongue. There is a pair of protrusors, which connect the cornua with the lower jaw; when they contract, the cornua and tongue are drawn forwards and protrude from the mouth. Two elastic ligaments attach the tips of the cornua to the nostril; these become stretched during protrusion, and during retraction help to return the cornua to their position of rest. A pair of retractor muscles make the windpipe their fixed point, and are inserted into the sheath. Other muscles raise, depress, or bend the tongue to one side. The long protrusors are wound about the cornua, and the long retractors about the windpipe—curious examples of economy of space. By these arrangements the requisite length in both cornua and muscles is obtained, while the parts are so

[1] The dimensions given relate to Méry's "piver," our green woodpecker.

[2] In humming-birds, which like the woodpeckers have a protrusible tongue, both the furrow on the skull and the sheath are double.

disposed as not to interfere with the movements of the head, neck, or jaws. It was impossible for Méry to trace the origin of the bony supports of the woodpecker's tongue, but in our evolutionary age no naturalist can fail to remark that they are an extreme modification of one part of the gill-bearing skeleton of a fish.[1]

The Frog

Méry (1684) describes the skin and tongue of the frog. He remarks the large subcutaneous cavities and the slight connection of the skin with the underlying muscles.

The form and position of the frog's tongue[2] lead Méry to the conclusion that it is darted out of the mouth for the capture of prey. He took little pains to verify his supposition, observing only a single frog, and not venturing to put forth his explanation as a positive fact.

Mollusca

Mytilus and Anodon were both favourite studies about this time. Van Heide (*supra*, p. 213) had already described Mytilus, and Swammerdam[3] had made notes on Anodon, but these early attempts left plenty of room for more elaborate studies. Poupart, Méry and Réaumur each added something to what was previously known.

Poupart (1706) explains the action of the ligament and adductors in opening and closing the bivalve shell. He describes the heart of Anodon, but finds neither

[1] Readers who cannot examine Méry's figure of the woodpecker's tongue will get a notion of the parts from Owen's *Comp. Anat. of Vertebrates*, Vol. II, pp. 58, 152, and figs. 33, 77. See also Newton's *Dictionary of Birds*, articles *Hyoid*, *Tongue* and *Woodpecker*.

[2] The peculiar attachment is remarked by Aristotle (*Hist. Anim.*, IV, 9), who wrongly explains that the tongue is in the frog a vocal organ.

[3] *Biblia Naturæ*, p. 189.

arteries nor veins in connection with it. The following statement is remarkable: "J'ay vu par hazard que la grande espèce de Moule d'étang, dont j'ay parlé, voltigeoit sur la superficie de l'eau."

Méry (1710) notes the enclosure of the rectum of Anodon in the heart, and describes the pericardium. He too believes that Anodon can rise to the surface of the water. He calls the external gills the ovaries, not an unnatural mistake, seeing that in the breeding season they are loaded with eggs.

Insects and Crayfish

Poupart (1704) describes the structure, life-history and mode of life of the ant-lion. He is struck with the resemblance of the imago to the dragon-fly, and calls both of them "demoiselles." His figures are excellent. In another memoir he gives an account of the frothing hopper (the insect found in cuckoo-spit).

Geoffroy the younger gives in 1709 an account of "crabs' eyes," calcareous concretions found in the crayfish, and at this time regularly prescribed by physicians. Gesner, Agricola and Belon had said that the concretions form in the brain; Geoffroy verifies the statement of Van Helmont that they form in the stomach. He shows that they are corroded and ultimately dissolved at the time of moult, when the lining of the stomach and intestine is shed; he goes too far, however, in saying that both stomach and intestine are destroyed at this time.

The astronomer Maraldi (1712) determined the angles of the rhombic plates which form the bases of the cells of the hive-bee. Kœnig (1739) showed the minute correspondence of these angles with the form most economical of wax. There is reason, however, to believe that the accuracy of the work of the bees has been exaggerated.

De la Hire and Sédileau (1730) describe and figure the scale-insect of the orange-tree.

The Earthworm

Poupart (1699) demonstrates the hermaphroditism of the earthworm, having observed two individuals in congress and noted the reciprocal insertion of "petits boutons" into "petites ouvertements."

Plants

Tournefort (1693) describes the ejection of seeds. Among other things he shows how the oblique fibres of a leguminous pod (which he calls *muscles*) twist the valves and throw out the seeds. The power of movement in plants depended, he thought, upon such muscles.

These French memoirs of natural history spread a conviction that some knowledge of the world of life should be brought within the reach of everybody. Rollin, author of *L'Histoire Ancienne*, desired that the discoveries of the academicians, which he had read with pleasure, should be explained to young persons, and his friend Pluche wrote a *Spectacle de la Nature* (8 vols. 12mo. Paris. 1732 &c.) which attempted to realise Rollin's purpose. Richard Bradley's *Philosophical Account of the Works of Nature* (8vo. Lond. 1721 and 1739) and Templeman's *Curious Remarks and Observations extracted from the History and Memoirs of the Royal Academy of Sciences at Paris* (2 vols. 8vo. 1753-4) were English publications of the same character. Bonnet was incited to study natural history by reading Pluche's account of the ant-lion, and

Humboldt many years later found that the *Spectacle of Nature* was highly esteemed in Spanish America. Professed students make light of such books, but those who appreciate the importance of beginnings will not despise these early attempts to diffuse more widely an elementary knowledge of the natural sciences.

SOME ENGLISH CONTEMPORARIES OF REDI AND PERRAULT

Thomas Willis' treatise *De Anima Brutorum* (8vo. Lond. 1672) interests the naturalist because it contains an anatomical description of certain invertebrate animals. Willis was too busy to undertake this part of the book, and handed it over to Edmund King and John Master, who produced much better accounts of the oyster, cray-fish and earthworm than had been seen before; the anatomy of an insect (silkworm) is added, but this is taken from Malpighi. The illustrative figures, based on careful dissection, surpassed all previous work of the kind, and were often quoted or copied by foreign naturalists.

In the oyster the four gills, the labial palps, the adductor muscle and the typhlosole are shown, but the nervous system is neither figured nor mentioned. It is pointed out that in the crayfish the flesh is covered by the "bones," not as in vertebrates, the bones by the flesh.[1] Attention is called to the inverted position (in comparison with a vertebrate) of the chief organs of the crayfish, to the inclusion of the nerve-cord within the sternal ossicles, and to the gastric teeth. In the

[1] This is remarked also by Perrault (*Essais de Physique*, Vol. III, pp. 80-1 and by Swammerdam (*Biblia Naturæ*, p. 444).

earthworm the locomotive function of the setæ is explained, and the reproductive organs are figured and discussed; it was not discovered, however, that the earthworm is hermaphrodite.[1] The typhlosole and the dorsal pores are mentioned.

King, afterwards Sir Edmund (1629-1709), took a chief part in the celebrated operation of transfusing the blood of a sheep into a man (1667); he is also remembered as the physician who bled Charles II with a penknife at the outset of his fatal illness. There is much to say about Willis, both as an anatomist and as a physician, but it is not for a naturalist to say it.

Edward Tyson (1650-1708), a London physician, made careful studies of the structure of a chimpanzee (which he calls an orang-outang,[2] an opossum, a porpoise and a rattlesnake. His account of the chimpanzee was published separately; the rest are to be found in the *Philosophical Transactions*, where also he figured, but without description, the "cochineel-fly." It was something to recognise, as early as 1685, that cochineal was an insect and not a fruit (*supra*, p. 208). Tyson figures also a Tænia, with its head and hooks. He has no doubt that there is a transition from minerals through plants and animals to man,[3] but this was not a conclusion drawn from his own observation and reflection. Such a transition had been a common theme of philosophers, occasionally of anatomists and naturalists also, ever since the time of Aristotle.

An *Essay on Comparative Anatomy* was published anonymously in 1744 by Alexander Munro primus, the

[1] A general statement of the fact, without precise details, was published by Redi in 1684. See also Poupart on the reproduction of the earthworm (*supra*, p. 236).

[2] *Orang-Outang, or the Anatomy of a Pygmie.* 4to. Lond. 1699.

[3] Epistle Dedicatory to *Anatomy of a Pygmie.*

first of three anatomists of the same name ; it is believed to be the earliest formal treatise on the subject. The *Essay* gives a slight and popular account of the anatomy of quadrupeds in general, of the dog, cow, fowls in general, a "stenhil" (also called *stannell* and *staniel*—kestrel) and fishes. By 1744 sufficient materials existed in print to furnish a much better elementary sketch than this.

SECTION VII. THE SCHOOL OF RÉAUMUR

JOHANN LEONHARD FRISCH

1666–1743

Beschreibung von allerley Insecten in Teutsch-land, &c. 13 pts. 4to. Berlin. 1720-38.

Réaumur is of course the chief figure in this Section, but before we enter upon Réaumur it will be convenient to notice his forerunner Frisch, who studied insects in the same spirit, though with less power, and with none of the advantages of wealth and position which his great successor enjoyed.

Frisch, the writer of the first important German treatise on insects, was rector of a gymnasium in Berlin, author of several dictionaries, and director of historical studies under the Berlin Academy of Sciences. He is also remembered as the introducer of the mulberry into Prussia. When past fifty he began to publish his Insects of Germany, which appeared in parts for eighteen years, and at length included three hundred different insects. It found plenty of readers, and more than one edition appeared in his life-time. Then he undertook the Birds of Germany, and had worked at them for eight years when death put a stop to his varied labours; the memoir was completed by one of his sons.

Frisch was a careful and diligent observer, bent not merely upon observing, but upon interpreting the facts of structure and habit. He takes one insect at a time, describes its external features, figures it in all its stages, if possible, and tells all that he has been able to discover about its mode of life. There is hardly any attempt at systematic arrangement. The simple microscope is regularly employed, but it is unusual to find any mention of internal anatomy. He was solicitous to study his insects alive, and reared many in his own house. Writing for the unlearned as well as for professed naturalists, he used German names instead of Latin and Greek. He introduced into Germany the word *insect*, regularly used by Pliny, and by this time quite familiar in France and England. The plates were engraved from Frisch's own drawings, the early ones by his son, Philip Jacob, who was only a boy when the publication began.

The Field-Cricket[1]

Frisch begins with the field-cricket. The external parts are mentioned and figured, and the differences of the sexes noted. A good account follows of the sound-producing organs on the wing-covers of the male, and the act of shrilling is described from life. The file and resonator are recognised, but the minute structure of the file is not discovered. The cerci are said, probably with truth, to serve as feelers, which in the darkness of the burrow perceive any moving thing which may approach the cricket from behind. The hind legs and their use in leaping or digging, as well as the ovipositor of the female, are shortly noticed. Frisch is very circumstantial about the mode of life, having kept crickets in captivity from hatching to old age. He tells us that

[1] Pt. I, ch. i-v.

they feed upon all kinds of vegetable matter. They drink much, but do not choose standing water, preferring dew, or raindrops on the grass. The burrows of the field-cricket run in a horizontal or an inclined direction; vertical burrows might fill with water in rainy weather. The crickets throw out the earth with their hind legs, or pull it out with their jaws. Their burrows are excavated in a dry, sunny place, not overgrown with grass; those of the male cricket are easily found, for they are quite open, and enlarged towards the entrance. Here the amorous male sits and sings, to charm any female that may be within hearing, and his burrow is made wide enough to lodge his mate as well as himself. The female cricket closes the burrow when she has laid her eggs. Young crickets make deep holes, but crickets of the second year make shallow holes as if they foresaw that they would die before winter. Crickets live near together, but do not share the same holes, except for a short period when mated. The females are prone to bite, and even to devour the males. The males show rivalry; one will pursue or defy another, and they sometimes butt at one another like goats. The impregnation of the female by a spermatophore and the action of the ovipositor in egg-laying are carefully described. The long yellow eggs are laid in batches of about thirty. Fresh-hatched crickets cluster together; they are at first pale, but soon darken. Four moults are gone through before maturity is attained; the cricket eats up its cast skin. After the third moult the wings and the ovipositor become evident.

Readers of the *Natural History of Selborne* will remember the letter on the field-cricket. Frisch has not the charm of Gilbert White, but on this particular topic he has more information to communicate. His writings, like those of nearly all foreign naturalists, were unknown to White.

Hydrophilus[1]

Frisch tells us that he had not meant to study aquatic insects, but that they drew him insensibly to the water, and even into it. The large size of Hydrophilus no doubt attracted his attention, and he kept the beetles long in captivity for the purpose of studying their mode of life. Throughout the autumn and winter they lived happily (in some kind of aquarium, I suppose), but when spring came round they strove to leave the water, and died if prevented from doing so. Frisch recognised the larva and pupa, but never found the egg-cocoon. He noticed the arched attitude which the pupa takes when resting in its underground cell, as well as the spines which keep it from touching the ground, and demonstrated in the adult beetle the presence of an air-space beneath the elytra, into which the spiracles open. When he removed one of the elytra a rhythmical pulsation of the air beneath the folded wing was clearly seen.[2]

German Cochineal[3]

German cochineal is the product of a scale-insect (*Porphyrophora polonica*), which makes galls on the roots of *Polygonum cocciferum*. Frisch describes the galls, the gall-forming insect, and the cottony threads which issue from its back. His figures are so minute and so rude that they are barely recognisable. He quotes ancient monastic writings to prove that the scarlet dye yielded by this insect was once largely used

[1] Pt. II, ch. vii, viii.

[2] The pulsation can be observed without injuring the beetle.

[3] Pt. V, ch. ii.

in Germany, and blames the laziness of those who preferred to buy the costly exotic cochineal instead of gathering the dye-stuff of their own country.

From the scale-insects which Frisch kept for observation ichneumons were hatched; he supposed that this was the regular transformation, the scale-insect being the larva and the ichneumon the imago of the same species. This mistake is the more surprising because Frisch had already described the parasitic habits of the ichneumons. Breyn some years later corrected Frisch's error, and pointed out the true winged male of the scale-insect. In the preface to the last part of his Insects of Germany Frisch humbly confesses his error. One thread of his warp had been broken; he could only hope that the rest of the web was sound. In this last preface the old man (he was now past seventy) says that if his activity and his eyesight should be preserved a little longer he would set about the fourth hundred of his descriptions of insects. But this was not to be; he published no more upon insects.

RENÉ-ANTOINE FERCHAULT DE RÉAUMUR

1683–1757

Mémoires pour servir à l'histoire des Insectes. 6 vols. 4to. Paris. 1734-42.

I have found no better account of the life of Réaumur than that written by Cuvier for the *Biographie Universelle*, and offer a translation of this short biography as an introduction to the *Histoire des Insectes.* The words in square brackets are additions.

"Réaumur, one of the most ingenious naturalists and physicists that France has produced, was born at La Rochelle in 1683. He was the son of a judge of appeal in that city. After beginning his education in his native place, he went on to the Jesuits at Poitiers, and afterwards studied law at Bourges. A propensity to observe nature then took possession of him, and ample means enabled him to pursue this taste with youthful eagerness. He laid a foundation for his future career in the serious study of mathematics, and when he felt himself prepared to try his strength with professed naturalists and physicists, betook himself to Paris. This was in 1703, when he was not yet twenty years old. President Hénault, a relative, made him known to men of science, and in 1708, being then twenty-four and having already contributed some geometrical papers to the Academy of Sciences, he was admitted to that learned body.

"For nearly fifty years Réaumur was one of the most active and useful members; his labours dealt with industrial arts, general physics, and natural history successively, and hardly a year passed in which he did not publish some memoirs of great importance or interest. He was early pledged to co-operate in a description of the mechanical arts, which the Academy had undertaken. Not confining himself to a mere record of the state of the various industries, he sought to improve them by fresh applications of scientific principles, while at the same time he enlarged our knowledge of natural phenomena by his industrial experience. In his account of rope-making (1711), he proved by conclusive experiments that, contrary to the prevalent opinion, torsion diminishes the strength of cords. In 1713, when engaged upon a description of gold wire-drawing, he demonstrated the extraordinary ductility of certain metals. In 1715 the investigation of the colours of false pearls made him acquainted with the singular substance which gives a lustre to the scales of fishes, and this led him to study the development and growth of the scales themselves. These inquiries became linked to researches which he had carried on ever since 1709, into the formation and growth of the shells of mollusks, which he showed not to arise by intussusception [or incorporation of new matter with every part of a pre-existing structure; Réaumur maintained that the shell grows by the addition of layers]. In 1717 he attended to pearl-formation, and sought to compel bivalves to produce pearls. When he had occasion, in 1715, to describe the turquoise mines of the south of France, and the methods in use for producing the blue colour, he discovered that turquoises are the teeth of the large [extinct] animal, since described under the name of mastodon [this is true only of the so-called occidental turquoise, which forms on teeth and bones after long burial in the ground]. But his most important researches in technical science were those upon iron and steel, published as a separate work in 1722 under the title *Traité sur l'art de convertir le fer en acier, et d'adoucir le fer fondu.* Our forges were then almost in their infancy, and we made no steel; all that was required in the arts was brought to us from abroad. It was only by innumerable experiments that Réaumur came to discover the art of steel-making. The Duke of Orleans, then Regent of France, proposed to remunerate him for this service by a pension of twelve thousand livres. At this date no tin-plate was made in France; all came to us from Germany; Réaumur succeeded in making it by a cheap method, which he published in 1725. In his numerous experiments he had more than once occasion to remark that cast metals, in cooling, assumed regular forms; and this led him in 1724 to give a first sketch of metallic crystallography. The manufacture of porcelain also interested him ; he sent to China for the materials, and busied himself in searching for similar minerals in France. His memoirs on this subject date from 1727 to 1729 ; his attempts were not entirely successful, but Darcet, and especially Macquer, following the indications given by him, were more fortunate, and succeeded in discovering the fine hard porcelain which we now employ for so many purposes. Réaumur also devised the hard white glass, still known as Réaumur's porcelain, of which he published an account in 1739. We owe to him the first attempts made to introduce into France the artificial incubation of eggs, practised from time immemorial in Egypt. He showed how to preserve eggs by smearing them with fat, how to hinder the evaporation of spirituous liquors by mercury, and suggested many other processes of greater or less practical utility. He improved the hanging of carriage bodies and the fitting of axles. In 1711 he rediscovered a mollusk which yields a dye answering to the purple of the ancients. He sought to turn to account the silk of spiders,[1] and it is a singular fact that his memoir on spiders' silk, dated 1710, was translated into the Manchu language at the request of the Emperor of China, who wished to read in his own language a paper whose title had moved his curiosity. In physics Réaumur is best known by his thermometer, which he brought out in 1731. The freezing and boiling-points of water are taken as fixed points, and the interval divided into 80 degrees, that number being chosen on account of the accidental circumstance that the alcohol which he used dilated $\frac{80}{1000}$ of its bulk [on being brought from the freezing-point to the boiling-point of water];

[[1] Spiders' silk proved to be too fine and therefore too costly.]

this mode of graduation has since been abandoned in favour of a centesimal division. As the fixed points adopted by Réaumur are still retained, all modern thermometers are in a sense Réaumur thermometers; it must, however, be admitted that the original conception belongs to Newton. In the course of the numerous experiments which this invention involved, Réaumur made curious observations on the increase or diminution of heat produced by the mixing of liquids, and also on freezing mixtures. He collected with great care observations on the temperature of various places as registered by his own thermometer, and gave an active impulse to this branch of meteorology. He observed that a freezing temperature does not prevent the evaporation of snow.

"In spite of the importance and practical utility of the publications of which we have just given a very brief account, there was yet more novelty and interest in his natural history memoirs. Besides what he had already written about the scales of fishes, the growth of shells and petrified teeth, he described in 1710 the modes of locomotion of many mollusks, star fishes, and other invertebrate animals.[1] In 1712 he made known the singular phenomena relating to the reproduction of cast limbs of crayfishes and lobsters. In 1715 he gave a detailed account of the torpedo shock, and of the organ by which it is produced, but electrical phenomena were then too imperfectly understood to make a thorough explanation possible. He examined several of our rivers, whose sand contains gold, and wrote a memoir on them in 1718. The vast layers of fossil shells, known in Touraine as Faluns, did not escape his notice; he described them in 1720. He investigated in 1723 the

[1] [The first descriptions of the ambulacral feet was given by Réaumur.]

light emitted by certain mollusks, and especially by the Pholadidæ. It will be seen that Réaumur was by no means unacquainted with physiology. Experiments as ingenious as they were decisive, showed him in 1752 the singular difference in the digestive organs of birds of prey, whose stomach acts on the food only by means of a solvent liquid, and those of grain-eating birds, in which a very powerful muscular gizzard exerts a pressure sufficiently great to crush and pulverise extremely hard substances."

[Réaumur's experiments on the digestion of birds,[1] are memorable in the history of physiology. Seeking to clear up the question whether food in the stomach is merely triturated, or whether it undergoes a chemical change, and in that case whether the change most resembles solution or putrefaction, he made a number of ingenious experiments on a captive kite. Small metal tubes, whose ends were closed by fine gratings, were filled with pieces of meat and offered to the bird in its food. When the kite, according to custom, rejected the tubes with other indigestible matter, the contents were carefully examined. The meat, though protected against trituration, was found to be partly dissolved; it exhibited no signs of putrefactive change. Even fragments of bone were more or less corroded. Vegetable matter on the other hand was hardly acted on. The tubes contained besides the remains of food a yellow fluid of bitter taste; large quantities of this were collected by tubes filled with pieces of sponge, which could be squeezed after rejection from the stomach. With this fluid Réaumur attempted artificial digestion, and though complete success was not achieved he was at least able to show that gastric digestion differs altogether

[1] *Mém. Acad. Sci.*, 1752.

from putrefactive change. When his kite died, Réaumur carried on the experiments with dogs and sheep. These results were not forgotten, and Réaumur's method became still more productive in the hands of Spallanzani.]

"But of all the works of Réaumur, the most remarkable are the *Mémoires pour servir à l'Histoire des Insectes*, of which six quarto volumes were issued between 1734 and 1742. This history can never cease to be studied with the keenest interest by those who would frame an exact notion of nature, and of the marvellous variety of means which she employs in the preservation of organisms apparently so frail and easily destroyed. Réaumur displays extraordinary sagacity in observing the special instincts which ensure the safety of these feeble creatures, and keeps our attention alive by a continual succession of new and striking contrivances. His style is somewhat diffuse, but so clear as to render everything plain, and the facts which he relates are rigorously true. The *History of Insects* can be read with all the interest of the most absorbing romance. Unfortunately it remains unfinished; the manuscript of the seventh volume, bequeathed at the author's death to the Academy of Sciences, was in such disorder and so incomplete as to be unfit for publication. In it Réaumur had intended to speak of crickets and grasshoppers, while the beetles were to have occupied the eighth and following volumes. The six volumes which actually appeared treat of the remaining orders of insects. [Cuvier's rapid summary of the *History of Insects* is omitted.]

"The *History of Insects* had placed Réaumur in the front rank of naturalists by the time that the first volumes of the *Natural History* of Buffon began to appear. Buffon somewhat eclipsed by the brilliancy of his style the popularity of the elder naturalist. Réaumur seems to have been weak enough to feel some jealousy, and was perhaps concerned in the publication entitled *Letters to an American*, the anonymous production of an Oratorian named Lignac, who lived not far from Réaumur's country seat, and often visited him. In this work Buffon and his associate, Daubenton, were treated contemptuously, while Réaumur, his works, and his collections were highly praised. Réaumur was the first man in France to form tolerably extensive collections of animals. Brisson, his curator, drew from these collections the chief material for his work on quadrupeds, and more particularly for his great *Ornithology*, in six quarto volumes. These specimens, though imperfectly cured, and most of them simply dried, passed into the Royal Cabinet after the death of their first owner, and long constituted the principal part of the bird collection. Many of the coloured plates of Buffon were also drawn from them, which explains the occasional resemblance between Buffon's and Brisson's figures.

"Réaumur's life passed in tranquillity, part being spent on his estate in Saintonge, part at his country house at Bercy, near Paris. He held no official post, and devoted every moment of his time to science. Public esteem and influence with the government sufficed to gratify his ambition. In order to oblige a relative who had been driven to resign the place of intendant of the order of St. Louis, Réaumur purchased the office, but only assumed the insignia himself, relinquishing the emoluments to his relative. It does not appear that he was ever married. A fall, which he met with in 1757 at the chateau of Bermondière in Maine, where he was spending his vacation, hastened his death. He died on 18th October, 1757, at the age of 74.

Besides the numerous memoirs which he contributed to the *Receuil* of the Academy (where also is to be found his *Eloge* by Grandjean de Fouchy) and other published writings, he left behind him 138 portfolios filled with complete or imperfect works and observations, as well as innumerable other papers. Among them was found a large part of his *History of the Useful Arts*, almost fit for publication, besides many memoranda for what still was left unfinished."

[During the years 1742-57 Réaumur hardly published anything, and there is no reason to suppose that he wrote much. We are left to conjecture why a life which had been so strenuous should close in comparative inertia, while the concluding volumes of the great *History of Insects* remained unwritten.]

THE *HISTOIRE DES INSECTES*

In 1734, when the first volume of the *Histoire des Insectes* appeared, Réaumur was fifty-one, and had still twenty-three years to live. Linnæus was a young man of twenty-seven, who had just completed his journey to Lapland; Buffon was already a member of the Academy of Sciences, but had not as yet paid any attention to natural history. Swammerdam's *Biblia Naturæ*, though the author had long been dead, was still unpublished; it did not appear until 1737-8.

The *Histoire* owed its origin to memoirs contributed by Réaumur to the Academy of Sciences, which were subsequently enlarged and popularised. The six volumes which he lived to complete are handsomely printed in quarto and illustrated by excellent plates. Réaumur did not draw with his own hand, but he tells us that he closely directed his artists. The style is flowing and animated, and few books on natural history are so pleasant to read. Réaumur found that the descriptions of insects published before his time were too concise and dry, and made a point of suppressing technicalities and discussing questions in which readers who are neither collectors nor anatomists might be expected to take an interest. Charm of style is however the least of his merits; he was one of the best observers that ever lived and enriched every topic with a profusion of new facts.

Réaumur's most notable predecessors, as he remarks, were more successful in depicting than in describing insects. Madame Mérian had issued a hundred beautiful plates of insect-transformations, and had also painted many of the remarkable insects of Surinam;[1] Eleazar Albin had figured many English lepidoptera with their larvæ;[2] Goedart[3] and Frisch had bestowed much labour upon the life-histories of insects. But these works, though useful to the naturalist, left almost untouched that field of inquiry which Réaumur called "the industries of insects," and this he determined to occupy.

He does not by any means restrict the word *Insect* to annulose animals, and it does not shock him that slugs, star-fishes, sea-anemones, reptiles, and amphibians should be ranked as insects. All animals, in short, except quadrupeds, birds, and fishes, come under Réaumur's "Insects." His reason for bringing reptiles into the insect-class is apparently only this, that reptiles (or some of them) creep on the ground. "Un Crocodile seroit un furieux insecte; je n'aurois pourtant aucune peine a lui donner ce nom."[4]

[1] *Der Raupen wunderbare Verwandelung und sonderbare Blumennahrung* (1679); *Insecta Surinamensia* (1705).

[2] *Natural History of English Insects* (1720).

[3] *Metamorphosis et Hist. Nat. Insectorum* (1662-7); translated into English by Martin Lister (1682).

[4] Vol. I, p. 68.

Caterpillars [1]

The methodical account of caterpillars describes the variety of external features which they present. How dreary we find such a collection of obscure differences when handled by a dull man! Réaumur however knows how to enliven his account by graphic touches. When he has to speak of the food-plants of caterpillars, he remarks that some feed with impunity upon acrid leaves; this leads him to mention one of the hawk-moths, whose larva refuses all food except the leaves of spurge. Réaumur put some of the milky juice of the spurge upon his own tongue, and at first noticed no distinct sensation. But in the course of an hour or so his mouth seemed on fire, and repeated rinsing with water could not allay the pain. The larva of the hawk-moth however was ready to drink several drops in succession of this caustic juice, and experienced no ill effects. Again, he tells us that many kinds of caterpillars which he attempted to rear perished because provision had not been made to satisfy their special instincts. Some are in the habit of hiding underground by day, and must therefore be supplied with loose earth. Réaumur made this discovery by noticing that some caterpillars of the cabbage moth (Mamestra brassicæ) and the common dagger-moth (Acronycta Psi), which he had supplied with young cabbage plants, were not to be found upon them next day. A fresh supply of caterpillars was procured, and these also disappeared, but the leaves had been much gnawed, so that Réaumur began to suspect that the caterpillars had not gone far away. On searching the earth in the pots, they were found buried, but at night a visit paid by candle-light

[1] Vol. I, Mém. ii-iv.

discovered them feeding on the leaves. It is little observations of this kind which keep alive the attention of the reader. In the same memoir we have a lively description of the arts practised by different caterpillars when alarmed. Some curl themselves up, others sham dead; others let themselves fall to the ground; others run away. A few stand on the defensive, and execute movements of various kinds, which possibly inspire terror.

Réaumur had to contend with one very serious difficulty. Many of the insects which he wished to point out had no fixed names, either popular or scientific. Sometimes he hits upon a name of his own, and these are often happily chosen, but now and then it is impossible for the reader to make out with certainty what insect Réaumur had in his mind. The concise characters of genera and species, which he found so dry, have their value after all.

From the external structure of a caterpillar Réaumur proceeds to consider its internal organs. He had Malpighi's admirable description of the silkworm as a guide, but the dissections figured in the *Biblia Naturæ* were as yet inaccessible. No better elementary account of the legs, head and spiracles of the caterpillar than Réaumur's could be set before a young student of the present day. Now and then, of course, some faulty or deficient explanation reminds us that he wrote nearly two centuries ago. He knows, for example, nothing about the chemical properties or mode of formation of the substance (chitin), which forms so large a part of the external skeleton of an insect.

The silk-glands, the spinneret, and the silk itself are thoroughly investigated. A simple expedient is pointed out which greatly facilitates the dissection of the glands.

Malpighi had found this a troublesome operation, but Réaumur discovered that soaking the caterpillar in alcohol coagulates the fluid silk, and makes it so firm that the glands can be removed entire. Réaumur discourses upon the physical properties of silk with great animation, for here his technical knowledge of glues, resins, and varnishes furnished him with many useful notions. Silk, he says, is remarkable for three properties; it sets instantly when exposed to the air; when once it has set, it is not softened by water or any other natural solvent; lastly, it is not softened by heat. It is easy to see how the value of silk, not only to the insect, but to ourselves, is enhanced by these peculiarities. Réaumur in his easy conversational way slips from one topic to another, and gives many bits of curious information that one does not look for in a chapter on caterpillars. Here is a single specimen. Many rocks, he says, are soft when first exposed, and only harden as they dry. When they have once become thoroughly dry, water does not easily penetrate them again. Fresh-wrought slates can be split into thin laminæ, but when the water has been allowed to escape they no longer split with the same facility. A little later he treats us to an entertaining discussion on the possibility of manufacturing thin sheets, not composed of threads, but with a perfectly smooth and bright surface, fit to take paint or gilding. Some of the packets made out of sheets of gelatine to hold sweetmeats perfectly realise Réaumur's notion.

He gives an interesting account of the alimentary canal, the heart, and the air-tubes of a caterpillar. Here he owes much to Malpighi, though he has looked into everything for himself. He thinks that air enters into the body of an insect by the spiracles, but does not issue by the same openings. There are, he supposes, a vast number of pores in the skin which allow the air to escape. It is easy with an air-pump, or even with a little strongly heated wax, to prove that the air taken in for respiration is unable to issue at any other points than the spiracles, and it is a little surprising that Réaumur did not think of this or some other decisive experiment.

The fourth memoir discusses change of skin in a caterpillar. Réaumur was at first in doubt whether the new hairs were enclosed within the old ones or not, and devised the following simple experiment to settle the point. A day or two before the change was due, he cut a number of the long hairs close to the skin, choosing the hairs just behind the head, the hairs of one side of the body, or some other definite group. The caterpillars so treated cast their skins exactly like any others, and the new hairs were found to be entire. Immediately after the skin is cast, Réaumur remarks, the body is damp externally, showing that a liquid had existed between the old and the new skin.[1]

Moths[2]

The external anatomy of moths occupies the fifth memoir. The wings, eyes, antennæ, and proboscis receive special attention, and are illustrated by excellent figures. To appreciate the importance of this memoir, we must bear in mind that these things had never been adequately investigated before. Malpighi had dismissed the silk-moth briefly, though he investigated thoroughly the reproductive organs in both sexes. Swammerdam's *Biblia Naturæ* (unpublished in 1734) gives a better

[1] A similar liquid facilitates the process of moulting in some reptiles.

[2] Vol. I, Mém. v.

account of the compound eye than Réaumur's, but is not nearly so full in respect to the wings, antennæ, and proboscis. Réaumur's memoir is therefore the foundation of nearly all our knowledge concerning the external structure of a moth or butterfly. The scales of the wings, overlapping like the slates of a roof, and inserted by their points into regular rows of holes, the different kinds of Lepidopterous antennæ, and the construction of the sucking proboscis, are described with great distinctness and spirit. So much has been done since 1734 to elucidate these interesting structures that it would be unfair to dwell upon the omissions inevitable in a first study. Happily there is very little to correct in these descriptions, and whatever Réaumur tells us is easily remembered.

The Transformations of Moths

The second half of Volume I (Memoirs 8-14), is chiefly occupied with the change of the caterpillar into a chrysalis, and the change of the chrysalis into a moth. These luminous descriptions are now reproduced with cruel abridgement in all popular works which treat of insect-transformations. It is a pity that so few readers take the trouble to make themselves acquainted with the original narrative, which is infinitely more interesting than any of the abstracts. The only important additions which naturalists have made to Réaumur's account of the transformations of Lepidoptera relate to the internal changes, and these demand a minute acquaintance with insect anatomy.

The first memoir of the second volume shows that the duration of the different stages of an insect's life may be greatly affected by temperature. Thus a caterpillar which is hatched out from the egg in July may pass the winter in the chrysalis form and only die as a spent moth in the following June; whereas a second caterpillar, born in May, may reach the end of its life-history in two or two and a half months. In the same way winter-sown wheat and spring-sown wheat ripen at the same time after growth-periods of very different lengths. Réaumur found that pupæ kept in hothouses in the depth of winter yielded moths long before other pupæ which were kept in a cool place. He also tried the singular experiment of enclosing some pupæ in an egg-shaped glass flask, and giving them to a hen to be sat with her own eggs. The time of year was June and July, and the pupæ set under the hen hatched out in four days, while pupæ of the same species kept in the open air required fourteen. Then he tried to retard development by keeping the pupæ in a cellar. They were kept alive without undergoing change twelve months beyond the usual time, and might probably have been retarded even longer by a lower temperature. Réaumur goes on to quote examples of the suspended animation of hibernating quadrupeds; of eggs of the silk-worm, which in a cool place remained long unchanged, but hatched out very rapidly when kept at the temperature of the human body; and of seeds which germinated after lying dormant for twenty years. He speaks, with a contempt which has since proved to be unjust, of the magazines of grain supposed to be laid up against winter by the industrious ant. Instead of enjoying the fruits of their labours in the dead season, the ants, he says, are then crowded together, and unable even to move. This is perfectly true of the ants of France and England, but the agricultural ants of warmer countries have been shown in modern days to be no fable.

A Digression on Eggs[1]

What he had seen of the retarded development of insect-eggs put it into Réaumur's head to try whether the eggs of fowls might not be kept fresh and capable of development long beyond the ordinary term. The fowl's egg spoils in consequence of putrefactive change; if such change could be hindered it might be possible to keep the egg fresh for an indefinite time.

What happens, he inquired, when an egg goes bad? In spite of egg-shell and shell-membrane, the egg loses moisture, and as it loses moisture it spoils. In a fresh egg the contents fill the whole space, but a continually increasing cavity, which can be seen by holding the egg up to the light, is found in all stale eggs; it is due to the loss of water by evaporation. Bellini and Vallisnieri had proved that the egg-shell is porous, for when the egg was placed in the receiver of an air-pump and surrounded by boiled water, the air rushed through the pores in the form of small bubbles. In the last days of hatching the chick utters a cry, which proves that its lungs are then filled with air; this air must have entered by the pores of the egg-shell. The countryman, continues Réaumur, knows how to keep autumn-laid eggs till winter, and then sell them as fresh eggs. He packs them in barrels with close-pressed wood-ashes; the ashes choke the pores of the egg-shell, and render evaporation slow. It occurred to Réaumur that varnishing the eggs would be simpler and more certain. Accordingly he took fresh-laid eggs in April, and varnished them with shellac dissolved in alcohol; next day he varnished them a second time. After two and a half months, that is, early in July, he boiled and

[1] Vol. II, Mém. i. Issued as a separate work in 1749.

opened the varnished eggs, and found that they had the appearance and flavour of fresh eggs. Eggs which had been kept in water for two or three days looked fresh, but had already lost the natural flavour. Eggs which had been boiled when fresh, and were merely warmed up again before eating, were not well-tasted. Réaumur kept varnished eggs for two years, and found that they still appeared fresh, though their taste was not so pleasant as that of fresh eggs; they resembled eggs which had been kept in water for a few days. He goes on to explain that a particular way of dipping the eggs in varnish is much less laborious than painting them with a brush. He fixed a thread to one end by means of sealing-wax, and was thus enabled to dip the egg and afterwards hang it up to dry.[1]

Réaumur thought it possible that his varnished eggs might be hatched and made to yield live chicks. He knew that the varnish must be removed before the eggs were set under the hen, and he could think of no good way of doing this. It is not surprising that his varnished eggs failed to produce live chicks. One such egg, however, one of four which had been very carefully cleaned from the varnish, did contain a chick with feathers nearly ready to hatch out; whether accidentally or not, it proved to be a monstrous chick with four legs.

The effect of temperature upon the fowl's egg is not, Réaumur perceived, a simple or direct action. In air which is cooled far below the temperature of the blood the egg may remain long unchanged; a rise of temperature within certain limits promotes putrefaction. A temperature may be reached which starts the development of the chick within the egg, and this hinders putrefaction;

[1] Dipping in boiling water, so as to coagulate a superficial film of albumen, has been found beneficial.

in an unfertilised egg, however, putrefaction would go on faster when the egg was moderately warmed.

Réaumur, we see, had attained a clear knowledge of some of the elementary conditions of the problem; it was not possible for him to explain why putrefaction should set in only between certain extremes of temperature, and should become more energetic as blood-heat is approached. The part which bacteria play in putrefaction was not suspected as yet.

Pupæ confined in a glass flask sometimes exude a considerable amount of moisture, which condenses on the sides of the flask. The exudation seemed to Réaumur a necessary accompaniment of the consolidation of watery liquids into permanent tissues. Exudation is a sign of life and growth; more remotely a sign of approaching death and decay. All growth, all activity, is a step towards the wearing-out of the body. It occurred to him that life might be indefinitely prolonged if the exudation was checked. Accordingly he varnished the body of a pupa, carefully leaving the spiracles unobstructed, and found that the emergence of the moth was retarded by some weeks. He went on to infer that in man himself a similar prolongation of life was conceivable. Cold, or a retardation of the insensible perspiration, might possibly be employed to protract life indefinitely, if such a result were truly desirable.[1]

Réaumur carried his speculations to a length that was not quite prudent, and thereby laid himself open to a mischievous critic. Maupertuis, a friend and ally of Réaumur's, seized upon the notion of protracting life by checking the insensible perspiration, and published his thoughts in a volume of letters, together with many

[1] Bacon (*Hist. of Life and Death*, ch. xviii) had explained that there is in the bodies of animals a vapour analogous to flame; if this is prevented from escaping, by unction with oil, life will be prolonged.

wild suggestions for the advancement of science and the improvement of the human race. Voltaire, who had a spite against Maupertuis, avenged himself in the *Diatribe du Docteur Akakia*, which holds up Maupertuis, and indirectly Réaumur himself, to the ridicule of mankind. The reader does not soon forget the resinous varnish which is to prolong our days.

Leaf-Rollers and Leaf-Folders [1]

Réaumur has a pleasant chapter on the caterpillars which roll up or double in two the leaves of plants. Oak, the common fruit trees, many garden shrubs, and some herbs, yield plenty of familiar examples. One leaf-folder can nearly always be procured, at least in summer, upon ivy, and another is plentiful on lilac in June. Most leaf-rollers and leaf-folders are Tortrices, Pyralids, or Tineids.

Réaumur gives a practical hint to those who desire to watch with their own eyes the operations of such larvæ. Having got a supply of rolled or doubled leaves, take fresh branches of the same plant and stick the cut ends into damp soil. Then turn out the caterpillars from their retreats and lay them on the leaves; they will make haste to conceal themselves in their accustomed way, whatever it may be, and every detail of the work can be observed. We cannot even name all the caterpillars described by Réaumur, and shall have to content ourselves with his account of the tortrix of the oak, which is common in early summer, sometimes so common that the trees are completely stripped.

One chief purpose of leaf-rolling is no doubt protection from birds and other enemies, but the same leaf which provides shelter to the larva and the chrysalis also yields

[1] Vol. II, Mém. v.

food, so long as food is required. The tortrix, screened from view by the outer turns of its green case, is able to devour the inner turns at its leisure.

These small larvæ are not strong enough to roll an oak leaf without artifice; moreover the leaf must not only be rolled, but hindered from unrolling again. In case of attack by a more powerful enemy, the larva must have a ready way of escape not too obvious to the pursuer. All these requirements are met by the instincts of the leaf-rolling tortrix.

A leaf is generally chosen which has some tendency to curl, and silken threads are spun across the bight. The threads are not scattered at hazard, but collected into short, stout bands, each of a hundred or more separate filaments. While spinning, the larva swings its head and the fore part of its body to and fro. When the band is half finished the larva climbs upon it, and then proceeds to spin the second half. Its weight, as Réaumur explains, slightly deflects the first mass of threads, and so tightens a little the folded edge of the leaf; the new filaments secure it in its position.[1] Each new band does something to increase the tension, and as row after row is added, what was at first a slight concavity becomes a close spiral, the turns being just so far separated as to allow the caterpillar to move easily between them. It is thus enabled to spin fresh bands, to feed upon the inside of its tube, and to conceal itself from an enemy. Now and then a main rib is gnawed through in two or three places to make it more flexible.

When alarmed the larva throws its body into rapid

[1] De Geer (*Hist. Nat. des Insectes*, Vol. I, p. 425 foll.) thinks that the weight of the larva is too inconsiderable to produce much effect. He remarks that the larva pulls the last-spun threads towards its body with its hooked feet. Every new thread visibly draws the edge of the leaf inwards, and all the old threads are lax.

undulations which puzzle or even alarm an enemy. It can move backwards and forwards with great agility, and often escapes at one of the open ends of the tube. It is always prepared to let itself down by a silken thread; when all is quiet again it climbs back to its leaf, coiling the thread against its breast, and, last of all, eating up the coil.[1]

Whenever the tube proves too narrow for the growing larva a new one of greater diameter is begun, and the leaf-roller moves to a fresh leaf as soon as may be requisite. The last tube of all, in which the change to the chrysalis takes place, is lined with silk, and the overlapping edge is secured by a continuous silken hem. The pupal stage lasts about three weeks, and then the moth emerges from one end of the tube.

Some Peculiar Caterpillars[2]

Under the heading of caterpillars which take peculiar attitudes or shapes, Réaumur describes the privet hawk-moth, the puss-moth, the iron-prominent (Notodonta dromedarius), the zigzag prominent (N. zic-zac), and the hook-tip (Platypteryx lacertinaria). He notices the protective resemblance of the lappet moth, and tells us that having shown this moth to several persons they all pronounced it to be a bunch of dead leaves. In the same memoir he describes the death's head moth in all its stages. The people of Brittany, he tells us, consider this moth a precursor of epidemic diseases. One ominous feature is the appearance of a skull upon the thorax; another is the cry which it utters, a strong and shrill note, resembling the squeak of a mouse, but more plaintive. We do not know, says Réaumur, any insects

[1] Some additions have been made to this paragraph.

[2] Vol. II, Mém. vi.

which possess a true organ of voice; the sounds which they emit are always due to vibration or friction. In this case the wings take no part in the production of the sound, for when they are held the sound does not cease, but becomes louder than before. It seems to proceed from the head of the moth. Réaumur satisfied himself that it was due to the rubbing of the palps against the proboscis. When the proboscis was forcibly extended with a pin, the sound ceased altogether; when only one of the palps was allowed to act the sound was weakened.

The Blow-fly

Réaumur describes with careful detail the external features of the winged blow-fly. Among other things he discusses the mechanism which enables the feet to adhere even to a vertical glass surface, though without remarking the exudation which is now known to be essential. The eyes, simple and compound, the balancers (halteres), and the membranes by which they are covered in certain flies, the spiracles, and the air-reservoirs all receive careful consideration. He gives an elaborate account of the proboscis, and compares it with that of a moth.[1] He is not satisfied with an enumeration of the parts; he must show them in action. His method of study was to smear the inside of a glass vessel with syrup, introduce some flies, and then watch them with a lens. He saw the terminal disk applied to the sugared surface in a hundred changing attitudes, expanded or contracted, pressed flat, inclined, or hollowed into a funnel; he saw undulations travelling along the grooves, liquid sucked up the tubular stem, and now and then an outflow of saliva (made more evident by bubbles of air), which served to dilute the syrup.

[1] Vol. IV, Mém. v.

Then we get a full description of the maggot,[1] including such minute details as the anterior spiracles, which are easily overlooked by an observer who has no means of enlargement beyond a simple lens. He shows how the pupa forms within the larval skin and is well aware that the head and other appendages of the future fly are telescoped into the body, from which they can be caused to protrude by gentle pressure. A student who intends to study all the details of the transformation would be well advised to verify Réaumur's account as a preliminary. He calls attention (Vol. IV, Mém. vii, viii) to the peculiar method of pupation which is found in the blow-fly. At first sight the pupa of such a fly seems to resemble a Lepidopterous chrysalis, but it differs notably from all ordinary insect-pupæ in one respect. Réaumur tells us that when the maggot is full-fed, it buries itself in the earth, if it can, and then contracts its body until it assumes the figure of an elongate egg. The larval skin is not cast, as in the pupation of a caterpillar; it persists as an outer defence, which we shall in this abstract call the *shell*. Though it was soft and flexible when it served as the skin of the maggot, it now turns dry and firm; its colour changes from white to red, and ultimately to a deep maroon. The maggot imprisoned within the shell is incapable of movement, its head is withdrawn into the thorax, and the hooks, by which it used to tear its food, are shed. Réaumur found that when the shell was opened, a new and delicate skin, the proper pupal skin, was found within it. Within this inner skin he naturally expected to find a pupa, but in a maggot which had just pupated he could discover nothing but a milky pulp.[2] Five or six days later he

[1] Vol. IV, Mém. iii.

[2] It has been ascertained since Réaumur's time that the larval tissues are to a great extent consumed by phagocytes shortly before and after pupation,

found, on opening the shell, a white pupa, in which the parts of the fly could be distinctly recognised. By a brief immersion in boiling water the pupa, in its later stages at least, could be readily extracted without injury, and he was thus able to study its growth with facility. Two or three days after the maggot became motionless three pairs of short legs appeared on its thorax; next day the wings could be distinguished, while the legs had apparently grown longer; a little later the proboscis became visible, and so on. Réaumur next discovered that the new organs begin to form some time before they appear externally, for they are at first telescoped into the body in such a way that only their extremities are free. By gentle pressure he succeeded in causing them to protrude from the deep infoldings in which they were at first concealed. "In fact," says he, "I was able myself to complete the development of the pupa, effecting in a moment a transformation which ought to have occupied several days." He made the acute suggestion that the protrusion may be accomplished in the living insect by blood-pressure, and this suggestion has now been confirmed by actual observation.

Réaumur next explains how the fly makes its escape from the shell (hardened larval skin). At the head-end is a horizontal cleft, which divides the thoracic and first abdominal segments of the larval skin into dorsal and ventral halves; this cleft is a line of weakness, prepared in advance to facilitate the emergence of the fly. By pressing a late pupa between the finger and thumb the cleft can be made to gape; the dorsal and ventral flaps part, and may fall off, for they are easily detached along

and that the milky pulp which Réaumur observed is largely made up of gorged phagocytes. Something of the same kind has been observed in many other insects, but it is a peculiarity of Muscid larvæ that they are almost entirely dissolved in this way.

the transverse lines where the segments meet. Réaumur took advantage of this arrangement whenever he had occasion to extract a pupa from its shell. It still remained to explain how the fly is enabled to open the shell from within. Réaumur made the remarkable discovery that the head of the fly is at this time capable of great and rapid dilatation. All the fore part between the compound eyes and the antennæ can be distended so as to double the size of the head. The fly, says Réaumur, when it is ready to emerge, dilates its head with *air*, and thus exerts a pressure sufficient to burst open the shell. This statement contains the only mistake that I have discovered in Réaumur's account of the transformation of the blow-fly; the head is not distended with air, but with blood. Then the fly wriggles out, relying at first upon its body-segments, but employing its legs as soon as they become free. The delicate pupal skin, which was closely fitted to every part of the surface of the fly, is left behind in the empty shell, and with it the linings of the air-tubes, which were drawn out through the spiracles. The fresh-emerged fly is soft and pale-coloured, and its wings are crumpled. It looks too big for the shell in which it was imprisoned, and is really so, for it enlarges after its escape in a surprising way. Réaumur discovered that the enlargement is simply due to inflation by air, and by the prick of a pin he was able to reduce the fly to its former size. In the course of a few hours the skin darkens and turns hard, the wings expand, and the insect becomes capable of flight.

Aphids [1]

Réaumur was able to make important contributions to that investigation of the aphids, which Leeuwenhoek

[1] Vol. III, Mém. ix.

had started, and which had attracted the notice of several other naturalists during the forty years' interval between the *Continuatio Arcanorum* and the *Histoire des Pucerons.* He showed that both the winged and the wingless aphids may be viviparous ; Leeuwenhoek had concluded too hastily that the wingless forms are immature insects, destined afterwards to acquire wings. Réaumur was aware that the unfertilised females can bear young, and he endeavoured to rear successive generations of viviparous aphids, but was accidentally hindered. Leeuwenhoek had concluded from his own observations that ants devour aphids, and are the great natural check upon their inordinate increase ; Réaumur, however, confirmed and corrected the view which Goedart had put forth long before and which Frisch had defended ; he showed that when ants seek out aphids, it is not usually in order to prey upon them, but to drink their sugary exudation. He found small red ants, probably Formica rufa, dwelling underground in company with grey aphids. All honeydew, Réaumur maintained, is the product of aphids, a statement which has been confirmed by later observers. He showed that a fluid is exuded from the tubes which in most aphids stand up from the hinder part of the abdomen, but he could give no account of the use of the fluid.[1]

Réaumur's Aphis-studies enabled him to suggest to his young friend, Charles Bonnet, the inquiry which has immortalised his name, and in the last volume of the *Histoire des Insectes*[2] Réaumur had the satisfaction of noticing Bonnet's discoveries with warm praise.

[1] Bonnet (*Traité d'Insectologie*) was the first to observe that aphids defend themselves by directing the extremity of the abdomen (which bears the tubules) towards an enemy.

[2] Vol. VI, pp. 523-568.

The Hive-Bee

The last part of Volume V is devoted to bees, and Réaumur begins with a full account of the hive-bee, the most interesting of all insects.

Swammerdam, as we have seen (p. 185), had proved by anatomical examination that the so-called "king of the bees" was really a queen, the only functional female, as a rule, in the hive. This discovery was the most considerable addition made to the knowledge of the honey-bee since ancient times. It was, however, far from being the only one which we owe to Swammerdam, who had worked out in rich detail the general anatomy and the life-history. Réaumur now comes in, simultaneously with the long-delayed publication of the *Biblia Naturæ*, to enlarge and correct the studies of his predecessor. Far inferior to Swammerdam in anatomical knowledge, he enjoyed the advantage of coming after him, and as an observer of the living animal he surpassed not only Swammerdam, but, it is hardly too much to say, all other naturalists who have occupied themselves with insects. After the *Histoire des Insectes* no great step was taken towards the elucidation of the economy of the hive until Schirach in 1771 proved that the workers are imperfect females. This brings us in sight of the researches of Huber.

Réaumur was indefatigable in devising experiments to ascertain how bees behave in various contingencies. He made much use of glass hives, so narrow that bees shut up in them could never be very far from one or other of the glass faces. The idea of glass hives was not new. Réaumur quotes Pliny to show that sheets of horn had been used in ancient times to facilitate observation of the work of the bees. Cassini, the first and greatest astronomer of that illustrious name, had kept glass hives in the garden of the observatory at Paris, and these had been used by his nephew Maraldi for observations on the construction of the honeycomb. Réaumur's hives and the work done with their help speedily superseded all Maraldi's results.

The account of the external features of the honey-bee in the *Histoire des Insectes* is an improvement even upon that of Swammerdam, and the description of the proboscis there given has never been mended except in minor details. One mistake of his predecessor Réaumur was careful to correct. Swammerdam had represented the so-called tongue, which forms the tip of the proboscis, as a tubular organ, which sucks up the honey like a pump. Réaumur showed that it is not a complete tube, but rather an elongate gutter. The cavity along which the honey flows lies between the tongue and the ensheathing blades of the maxillæ.

When Réaumur comes to discuss the source from which bees get their honey, he gives the credit of the discovery of the nectaries of flowers to "M. Linéus." Linnæus had recently visited Paris, and made himself known to the Jussieus, from whom Réaumur may have learned something about him. The nectaries of flowers are mentioned in his Linnæus' *Fundamenta Botanica* (1736) (pp. 10, 13), but were described long before his day by Malpighi (see p. 156).

Many particulars are added to Swammerdam's account of the pollen-collecting of the workers. The collecting hairs are figured; the comb on the hind leg and the bread-basket are well explained. In studying the movements of the legs in a bee which was combing out the pollen, Réaumur found that the bee worked her legs too rapidly for the convenience of the observer; he got over the difficulty by watching bees that were partially benumbed by the cold of a spring morning. He failed to note the antenna-cleaning comb on the fore tibia, or the spike on the mid leg.

Maraldi had supposed that the bees found their wax ready-made on leaves and flowers. Swammerdam, as we have seen, thought that wax is, in some fashion or other, elaborated by the bees out of the bee-bread, which he did not recognise as a mass of pollen. By the time of Réaumur the origin of the bee-bread presented little difficulty. The use of pollen in the fertilisation of flowers was now generally known, and Geoffroy had described the pollen-grains of a number of common flowers. Réaumur still held the old belief of Swammerdam, that bee-bread is the raw material from which wax is made.

In ancient times Pappus had illustrated the advantage of the hexagonal cells of the honeycomb, by proving that of all elongate regular solids, which could be employed in the construction of honey-cells, the hexagonal prism is the most economical of material. Réaumur proposed to the geometer Kœnig to determine what angles are most suitable for the rhombic plates which form the pyramidal ends of the cells, and Kœnig's reply, agreeing exactly with Maraldi's measurement of the angles in an actual honeycomb, became universally celebrated. This incredibly exact correspondence of bee-instinct with mathematical theory has however since been broken down; the typical form of the cell is rarely, if ever, realised.

Réaumur did not discover where or how wax is formed; he thought that the bees regurgitate it from their mouths as a soft paste. He gives useful information about propolis and its use in stopping chinks.

The old question, how the eggs of the queen are fertilised was rediscussed by Réaumur, who got nearer to the truth than any of his predecessors or contemporaries. Maraldi had maintained the ancient opinion that the eggs of the queen are fertilised after being laid, like the eggs of fishes. Swammerdam had propounded the theory of an *aura seminalis* (see p. 191). Réaumur shut up together a queen and a drone, and got indications that the queen is impregnated like other insects. It amused him to remark that the queen made all the advances. The curious history of the wedding-flight was first cleared up by Huber.

Réaumur found out methods of transferring all the bees of a swarm to a new hive, or of dividing them into companies. He discovered that they can be revived after long immersion in water, and employed this expedient to search a whole swarm, bee by bee. In this way he proved that a hive ordinarily contains but a single queen, and no drones except for a few weeks in the year. He gives a brief but accurate account of the swarming of bees, shows that wholesale destruction at the approach of winter is needless, and recommends the occasional transport of the hives into a flowery country —a practice for which he is able to quote examples of great antiquity.

At this point the inquiry into the structure, life-history and economy of the hive-bee passes out of our notice. Since Réaumur it has been fruitfully carried on by many naturalists, among whom the Hubers, father and son, Schirach and Dzierzon are pre-eminent.

Wild Bees[1]

Réaumur's accounts of the moss-carding bee and the leaf-cutting bee have been often quoted in popular

[1] Vol. VI, Mém. i-v.

books. Their history as related in Kirby and Spence is taken almost wholly from Réaumur.

Animals which Increase by Budding[1]

Biological discoveries of the highest interest not relating to insects (in the modern sense), nor to Réaumur's own work, occupy the concluding pages of his last preface. Trembley had lately made known the existence of an animal (Hydra), which increased by budding, and when cut in two, gave rise to two new animals. Trembley's discovery, communicated to Réaumur in 1740, was shortly afterwards set forth by Trembley himself in his classical *Mémoires pour servir à l'histoire d'un genre de Polypes d'eau douce* (1744). The name *Polype*, since so extensively employed, was suggested by Réaumur.

In this same preface Réaumur was able to give a preliminary notice of Bonnet's discovery of the multiplication of a fresh-water worm by artificial division. Bonnet's fuller account appeared three years later (1745) in his *Traité d'Insectologie.*[2]

The discoveries of Trembley and Bonnet were followed by a number of experiments on the multiplication of animals of low grade by artificial fission. Réaumur found that planarians ("sangsues limaces"), Stylaria, and earthworms could be increased in this way. Guettard and Bernard de Jussieu experimented on starfishes, and made it clear that they could at least reproduce lost rays. Trembley investigated a fresh-water polyzoan which he called the *polype à pannache* (Lophopus), and showed that it produced new individuals by budding.

At this time a question was reopened which had been

[1] Vol. VI, preface. [2] *Infra*, p. 284.

long under discussion, viz., whether corals and sea-anemones are animals or plants. Réaumur had hitherto accepted and enforced the teaching of Count Marsigli, who had described the polyps of corals as their flowers. Peyssonel, a native of Marseilles, who had diligently studied the corals of the Mediterranean, tried in vain to get a hearing for more sensible views. He had seen the polyps extend their tentacles, open and close their mouths; had ascertained that they were not, like flowers, restricted to a particular season, but were always to be found on living coral, and had rendered it highly probable that they had the chemical composition of animals, one proof being that when they putrefied they gave out the odour of decomposing animals. Réaumur, who had been invited to bring Peyssonel's paper before the Academy of Sciences, did so with the greatest scepticism, did not even name his correspondent (for fear, as he afterwards explained, of bringing ridicule upon him), and immediately laid before the Academy a paper of his own, in which he supported Marsigli's erroneous interpretation of the corals.

Trembley's discoveries caused Bernard de Jussieu to examine closely examples of different kinds of zoophytes (Alcyonium, Tubipora, Flustra, Cellepora), all of which he showed to be unquestionably compound animals. Réaumur was now converted. He accepted the conclusions of Trembley and Bernard de Jussieu, apologised for his incredulous reception of Peyssonel's observations, and employed all his gifts of exposition to spread the belief in the animal nature of the corals and zoophytes.

Peyssonel, who received hard treatment all through, had gone on a forlorn errand to Guadeloupe as physician-botanist to His Most Christian Majesty. When Réaumur withdrew his opposition, Peyssonel made a last effort to obtain recognition; he wrote from his place of banishment a lengthy treatise on corals, which he addressed to the Royal Society of London. From the abstract published in 1752 it would appear that this treatise contained little new matter. The unfavourable opinion of one Dr. Parsons, though containing no experiments or observations, was held to be decisive, and Peyssonel's treatise was never published. The clear evidence brought forward by John Ellis was required to convince naturalists of the soundness of the views which Peyssonel and Bernard de Jussieu had advocated.

These extracts and notes from Réaumur must now come to an end. It will be readily understood that they are mere chance samples of the rich mass of observations which fills the *Histoire des Insectes*. They cannot but fail in giving any just impression of that clear and sprightly style by which Réaumur fixes our attention upon the details of a thousand natural contrivances.

DE GEER'S *HISTORY OF INSECTS.*

Réaumur's *History of Insects* was followed up by the seven volumes of De Geer,[1] which adopt the methods and even the form of their model as closely as a work based on independent observations could do. De Geer (1720-1778) came of a very notable and public-spirited Dutch family, which had been long settled in Sweden, and had grown wealthy by the possession of iron mines and furnaces. He was educated in Holland, and afterwards attended the lectures of Linnæus at Upsala. He was only sixteen when he made his first observations on the water-spider, and his first paper was read when he

[1] *Mémoires pour servir à l'histoire des Insectes.* 7 vols. 4to. Stockholm. 1752-1778.

was no more than twenty; it contains the first published account of the water Podura and other spring-tails. This excellent beginning was followed by more studies of the same kind, and after some years' labour Réaumur and Muschenbroek advised him to prepare a methodical account of his discoveries. He took Réaumur's *Histoire des Insectes* as his model, and wrote in French, though he was aware that his style was "pas trop Français." Réaumur assured him that due allowance would be made for a naturalist who wrote in a foreign language, but little allowance was called for. The numerous plates were engraved from the author's drawings. Buffon's first three volumes had lately appeared, in which Réaumur was reproached for his blind admiration of the works of nature, and especially of insect contrivances. "On admire toujours d'autant plus, qu'on observe davantage et qu'on raisonne moins." De Geer retorted by explaining that he had made a point of telling what he had seen "sans trop de raisonnemens." His first volume found so few purchasers that de Geer in a fit of disgust burned a large part of the edition; this volume has been scarce ever since. Happily for us his good temper soon returned; he lived long enough and worked hard enough to deal with all the orders of insects. His classification of insects by wings and mouth-parts was better than any that had previously appeared, and resembles in many respects that which we still employ.

ABRAHAM TREMBLEY

1700–1784

Mémoires pour servir à l'histoire d'un genre de Polypes d'eau douce, à bras en forme de cornes. 4to. Leyden. 1744.

The *Polypes d'eau douce* is a book of 324 pages, illustrated by thirteen plates and four vignettes. It is printed in the handsome style of Réaumur's *Histoire des Insectes.*

The author was a Genevese, related to Charles Bonnet. When he wrote the book he was tutor to the two boys of the Hon. William Bentinck, English resident at the Hague. He afterwards followed Bentinck to London, and served as travelling tutor to the young duke of Richmond. In 1757 he returned to Geneva, and became a member of the Council, which led him to pay attention among other things to economic entomology.

The four vignettes which enliven the book show Bentinck's country-house (Sorgvliet, a mile from the Hague), the artificial lakes in which Trembley was accustomed to fish for polyps, and the schoolroom of the mansion. Trembley and his pupils are brought into each picture.

At the time of his first arrival at Sorgvliet (summer of 1740) Trembley happened to collect some water-plants, which he put into a glass vessel and set in a window. Being occupied with aquatic insects, he paid little attention to a small green stalk, barely visible to the unaided eye, which he found attached to one of the plants, until the slow movement of its thread-like tentacles attracted his notice. When he gently shook the vessel, the stalk and tentacles contracted, but soon extended themselves again. Was this new object a plant or an animal? Its form and colour were those of a plant, and sensitive plants were known, which droop when touched or shaken. But when he found that the supposed plant could move from place to place, he was inclined to change his first opinion. It then occurred to him to cut the stalk in two, and see whether the halves would live when separated; if they did, the natural inference would be that it was a plant. The halves gave at first no signs of life beyond movements of contraction or expansion, but on the ninth day small prominences were found to have formed on the cut end of the basal half; these gradually grew into new tentacles. Trembley had now two complete organisms in place of one; each was able to extend or contract its body, and to move about.

Hydra was therefore capable of increase by artificial fission; in other words, it could be multiplied by cuttings, like a plant. Leeuwenhoek[1] had proved in 1702 that Hydra also buds spontaneously, a fact which Trembley abundantly confirmed. These points of resemblance between Hydra and a plant were however balanced by the discovery that Hydra fed upon small aquatic animals, capturing and devouring live water-fleas (Daphnia) with avidity. No wonder that Trembley wavered in opinion, regarding this puzzling object now as a plant, then as an animal, and again as neither plant nor animal, but something intermediate. When the water was warm and "pucerons" (Daphnia) abundant, the polyps formed colonies of great size, each consisting of countless attached animals.

Trembley found the ovary of the Hydra, and observed the liberation of the ovum. He suspected that a small polyp which afterwards appeared had proceeded from

[1] *Supra*, p. 216.

the ovum, but the proof was not complete, as other polyps had been kept in the same vessel. Roesel a few years later figured the ovary and the ripe ovum within it, but was not fortunate enough to see polyps emerge from the ova; he thought, quite truly, that the young polyps which are plentiful in spring originate in this way, but was too cautious to affirm what he could not prove.[1]

Live specimens were sent to Réaumur, who had no hesitation in placing them among animals, and calling them *polyps*, the old Greek name for cuttle-fishes. Linnæus afterwards proposed the generic name of *Hydra*, suggested by the fabulous monster which sprouted out new heads, as fast as the old ones were cut off.

In his memoir Trembley describes three species, the green, the common brown, and the long-armed brown Hydra. He says that, besides preying upon Daphnia, his Hydras devoured Nais, Cypris, insect-larvæ and pupæ, and even very small fishes. Fishes and also the whirligig-beetle rejected the Hydra after once biting it; the irritating thread-cells were of course unknown as yet. Trembley mentions the power which the polyp possesses of fixing its body by means of its tentacles, and shows that it can travel looping-fashion, attaching the fore and hind ends alternately; he was not aware that it can also move slowly by means of the foot alone. He describes the suspension from the surface-film, and shows that a single tentacle can thus hold up the body. A drop of water let fall upon a Hydra floating in this way sends it to the bottom at once. There is also mention made of the infusorian parasite (Trichodina), which runs about on the Hydra.

[1] *Insecten-Belustigungen*, Theil III, pp. 500, 513-4, pl. 83, figs. 1, 2; 89, fig. 7.

Trembley's worst mistake was that he supposed the base of the polyp to be perforated. When we recollect that he worked with a simple lens, and lacked all the appliances which histological experience has now devised, it is surprising that he should have ascertained that the body-wall is composed of two layers, the colouring matter being confined to the inner one; that both layers extend into the hollow tentacles; and that the cavity of a bud is continuous with the cavity of the parent polyp.

Trembley carefully abstains from the guessing which so often spoils the work of early discoverers. "Je ne m'arrêterai point," he says, "à expliquer par quel mécanisme le corps des polypes s'étend et se contracte. Je risquerais de ne donner que des conjectures." He would no doubt have gone wrong if he had offered an explanation.

His manipulative skill is astonishing. There are very few who could lay open a Hydra in this way: "Je mets un polype sur ma main, et je le fais contracter le plus qu'il est possible; après quoi j'introduis dans sa bouche une pointe de ciseaux très fins, et je la fais sortir par le bout postérieur. Je ferme ensuite les ciseaux, c'est-à-dire, je coupe un côté de la peau du polype suivant toute sa longueur, j'ouvre d'un bout à l'autre le canal qu'elle forme, et en abaissant après cela de côté et d'autre cette peau que j'ai séparée, je découvre la superficie intérieure de la peau du polype, les parois de son estomac." Trembley cut Hydras into very minute pieces, which completed themselves into normal polyps; he divided them longitudinally as well as transversely, and actually succeeded in cutting a polyp into four strips, each of which yielded a perfect animal. By cutting and reuniting he produced extraordinary monsters, seven-headed hydras, double hydras, in which one body enclosed another, &c. He found it possible to join up the fragments of different individuals. His turning of a polyp inside out was a still more wonderful feat. The base was pushed inwards until it emerged through the mouth, a bristle being the only instrument employed. The polyp had a great inclination to turn itself back again, which Trembley sometimes prevented by spitting it on a bristle. The most extraordinary feature of the story is that polyps thus treated could live, feed and bud out new individuals. For the truth of his statement he appeals to the witnesses who saw it done, to Allamand, professor of natural history at Leyden, who successfully repeated the operation, and indirectly to Lyonet, who made the drawings. How a polyp so treated could digest its food is an unsolved mystery, and modern zoologists find it hard to believe that there is not some mistake in the account.

Baer[1] has remarked that Trembley's discovery appreciably modified the teaching of physiology by showing that an animal without head, nerves, sense-organs, muscles, or blood may perceive, feed, grow and move about.

The *Polype d'eau douce* contains good figures of another compound animal, the Polyzoan Lophopus, which Trembley calls the "polype à pannache."

Eight of the thirteen plates were engraved by Lyonet, who was already a keen and experienced naturalist, living at the Hague, and warmly interested in Trembley's work. Though a skilful draughtsman, Lyonet had never attempted to engrave, nor had he even seen how engraving is done. A Dutch engraver, Wandelaar, being struck by the beauty of his drawings, persuaded him to try

[1] *Reden*, Vol. I, pp. 109, 154.

what he could do on copper. His first figure, that of a dragon-fly, was successful; he went on to engrave three moths, and then without further apprenticeship executed the charming plates of the *Polype d'eau douce.*

CHARLES BONNET

1720–1793

Traité d'Insectologie, ou Observations sur les Pucerons. Par M. Charles Bonnet, de la Société Royale de Londres, &c. A second part, published at the same time, contains Observations sur quelques espèces de vers d'eau douce, qui coupés par morceaux, deviennent autant d'animaux complets. 8vo. Paris, 1745.

About the year 1745 all well-read naturalists, and many people who were not naturalists at all, were strangely excited about the *pucerons* or aphids. It became known that a young man named Bonnet had just proved that the aphids produced new generations without fertilisation, and this singular exception to the ordinary course of nature created almost as great a stir as the seminal animalcules of Leeuwenhoek or the polyps of Trembley. The story of the aphids occupies the first volume of the *Traité d'Insectologie.* Though spaced so widely as to occupy 228 pages, it is not longer than many a review article, and may easily be read through in an evening. It is clear and interesting, devoid of technicalities, and suited in all ways to readers who are intelligent without being learned.

A short account of Leeuwenhoek's work on the aphids has already been given (*supra*, p. 206). Several other naturalists had engaged in the further study of these insects, so common and yet so interesting. Among them was Hyacinthe Cestone (1637-1718), who communicated his observations to Vallisnieri, by whom they were published. Then came the important memoir by Réaumur, published in the third volume of his *Histoire des Insectes* (*supra*, p. 269). Leeuwenhoek and Cestone had found no male aphids, and had got no proof that the females were ever fertilised. Neither of them firmly established the fact, but they offered an explanation, which was that aphids were self-fertilised—a daring speculation, seeing that not a single case of a self-fertilised animal could be quoted! Cestone had thought that the scale-insects too were self-fertilised, but Réaumur was able to refute him by discovering the male insect. Réaumur now thought of rendering the fertilisation of an aphid by another individual impossible; he meant to isolate it from birth, and see whether it would still be able to propagate. Owing to unlucky accidents, none of the aphids which he thus isolated came to maturity, but he did not lose sight of the inquiry, and when Bonnet begged him to suggest a subject for investigation, Réaumur proposed this experiment as a promising one.

Bonnet came of a French Protestant family, which had been driven by religious persecution to take refuge in Switzerland. He was brought up to the law, but found time for other studies, and among the rest for natural history. Réaumur's *Histoire des Insectes* early fixed his attention, and he was only twenty years old when he undertook the aphis-experiment. The result was communicated to Réaumur, and through him to the Académie des Sciences, which begged him to repeat and extend his researches. He did so, and followed up his aphis-work by studying another case of abnormal reproduction, the multiplication of worms by section. A weakness of the eyes hindered him from attempting further work in natural history. Henceforth he gave his chief thoughts to philosophy, and wrote much that was once highly esteemed, though it has failed to endure. Bonnet and his wife (a daughter of the celebrated De la Rive family), being childless and wealthy, adopted a nephew of Madame Bonnet, who made for himself a great name as a botanist, a geologist and an explorer of Mont Blanc. This was Horace Benedict de Saussure (1740-1799).

Experiments on Aphids

To the *Traité d'Insectologie* is prefixed a short introduction, for which Réaumur's *Histoire des Insectes* furnishes all the facts, and all the figures of insects. Bonnet then proceeds to explain the experiment proposed to him by Réaumur, and the measures taken to carry it out. He filled a flower-pot with earth, and plunged into it a phial of water, intended to supply the food-plant. A new-born aphis, whose birth had been observed, was placed on the plant, and all was covered up by a bell-jar, which was pressed into the earth, so as to exclude other insects. An aphis found upon the spindle-tree was selected for the first trial, which began on May 20, 1740. Bonnet kept an exact diary of his observations, which were made hourly or oftener during the day; a good lens was continually employed. The aphis changed its skin four times, and came to maturity on June 1, when the first young one was born. By June 21 the unfertilised female had produced 95 aphids, all born alive. She was then accidentally lost, having by this time assumed a peculiar shape (flattish, narrowed in front and rounded behind), which Geoffroy had mistaken for the male aphis, but which Réaumur had shown to indicate an exhausted female. Next year the experiment was repeated. Two new-born aphids were tried, one of which produced 90, the other 49 young, but a fever prevented Bonnet from carrying the trial further.

Trembley, who was at this time living with the Bentinck family at the Hague, corresponded with Bonnet about the aphis-experiments, and suggested that one act of fertilisation might conceivably suffice for several generations. In order to investigate the matter to the very bottom, he thought it desirable to isolate individuals of each successive generation until either males appeared or the reproductive powers became exhausted. Bonnet carried out Trembley's suggestion, and reared in succession five generations of the aphis of the elder, all without the participation of a male insect. At last the bark of the elder grew too hard to be penetrated by the beaks of the young aphids, and further propagation became impossible.

Bonnet also observed two species of the genus Lachnus, aphids which are common on the oak. They are remarkable for their large size, 6 mm. ($\frac{1}{4}$ in.); one of them had a beak longer than the whole body. Both the winged and the wingless individuals reproduced viviparously so long as the season was mild, no act of fertilisation being observed. At the approach of winter, however, small, active, winged males appeared; the females were fertilised and laid eggs. These eggs were kept, but they produced no young, possibly because they were not kept till spring. Trembley soon wrote from the Hague that Lyonet, "qui voit tout," had found fertile eggs of Lachnus on the oak in April. Bonnet was now able to show that the aphids reproduce viviparously so long as food is plentiful, but that when severe conditions prevail, viviparous reproduction ceases, and eggs are laid, which outlast the winter; all such eggs are fertilised.[1] Bazin of Strassburg, Trembley and Lyonet were all invited by Réaumur to repeat Bonnet's experiments, and all three got confirmatory results, as did Réaumur himself, though his trials were less complete. Long afterwards it became known that as early as 1701 Albrecht of Hildesheim had observed reproduction in an unfertilised moth.[2] He had picked a brown pupa from a currant-bush, and placed it in a glass vessel. A yellowish-white moth emerged, which was left all winter without attention. In the following April Albrecht was astonished to find that the eggs of this moth had hatched, and produced a number of small black caterpillars.

The Multiplication of Worms by Section

In Part II of the *Traité d'Insectologie* Bonnet tells how he searched without success for the freshwater polyp according to the indications furnished by Trembley. A long aquatic worm was however found, and upon this the experiment of section into two (suggested by Trembley's work on Hydra) was tried ; each piece became a complete worm. In the end Bonnet succeeded in cutting a worm into twenty-six parts, most of which yielded complete individuals. He believed that he had experimented on six different kinds of worms ; Nais does not appear to have been one, in spite of statements to the contrary, which are frequent in text-books. Bonnet takes to himself the credit of these observations, but it is plain that it was not he but Lyonet who first showed that there are worms

[1] See also Trembley, *Polype d'eau douce*, preface, p. xi, which shows that Lyonet had anticipated some of Bonnet's conclusions.

[2] The case is reported by Siebold in his *Wahre Parthenogenesis*, p. 16 (1856).

which, when cut up, form as many animals as there were pieces.[1]

Bonnet remarked that his worm (Lumbriculus) occasionally budded out a second head; if he had happened to study Nais instead (as is often alleged), it is probable that he would have observed the frequent formation of a zone of division, and the spontaneous segmentation of the worm into two complete individuals. Both artificial and spontaneous fission were observed in Nais by Roesel[2] a few years later.

Bonnet's once-celebrated theory of *emboîtement* may be sufficiently discussed in a few words, since it has long ceased to occupy the thoughts of biologists. The facts on which it rested were these:—insect-larvæ and pupæ often contain eggs; aphids can reproduce for many successive generations without fertilisation; Malpighi had found a rudimentary chick in an egg which had never been sat by a hen (*supra*, p. 159). From these facts Bonnet drew very wide conclusions. All female animals, he said, whether fertilised or not, contain embryos, which are capable of development, and enlarge simply by absorption from the surrounding tissues. The embryo is "preformed," and exists before the body by which it ultimately becomes enclosed. It was easy to trace in imagination the invisible rudiment to the egg, to the parent, to the grandparent, and so on; Bonnet

[1] The preface to Réaumur's sixth volume (p. lvi), published three years before the *Traité d'Insectologie*, makes this clear. We have also the testimony of Trembley:—"M. Lyonet, qui est le premier qui ait coupé des vers, et qui ait vu chaque partie devenir un ver parfait, &c." (*Polype d'eau douce*, p. 223). It is not easy after this to understand Bonnet's account of the matter, nor his assurance that he had no direct knowledge of Lyonet's experiments when the *Traité* was sent to the press (Preface to *Traité d'Insectologie*, Vol. II). No doubt Lyonet had Bonnet in mind when he complained of those who had anticipated him by publishing the results for which he had laboured (Preface to *Traité Anat. de la Chenille*, &c.).

[2] *Insecten-Belustigungen*, Vol. III, pp. 567-584; pl. 92-3.

supposed that the process was started at the Creation, and has been in operation ever since; there is really no such thing as generation, but only the expansion of germs which are as old as the world.[1] Swammerdam (*supra*, p. 183) had already published a speculation which has much in common with Bonnet's.

The formation of hybrid animals and plants, and the resemblance of the child to the father, show that the development of an embryo may be largely affected by events immediately preceding, but awkward facts like these might perhaps be got over by the ingenuity of a Bonnet. The process of embryo-formation is however absolutely different from that which he pictured to himself. Even in 1759 Wolff saw enough of this process to satisfy himself that the embryo is new-formed in each generation, and his conclusion is now reinforced by the far more complete and rigorous demonstrations of modern embryology.

The Scale of Creation

Like other philosophers, Bonnet was prone to affirm things that can never be known. He had picked up from Leibnitz[2] the saying, *Natura non facit saltum*, and not realising the enormous difficulty of proving so comprehensive a negative, he applied instead of scrutinising the maxim. Thus he came by his chain of organised beings, a linear series of insensibly graded natural objects, which leads from the four elements to man. Between the different "classes and genera" Bonnet finds connecting links ("points de passage ou de liaison").

[1] See Bonnet's "Accroissement des Germes," *Œuvres*, Tom. V, pp. 1-11 (1781).

[2] "Rien ne se fait tout d'un coup, et c'est une de mes grandes maximes et des plus verifiées, que *la nature ne fait jamais de sauts*. J'appelais cela *la loi de la continuité*, lorsque j'en parlais autrefois dans les *Nouvelles de la république des lettres*." (Leibnitz, preface to *Nouveaux Essais*.)

Hydra, for example, is a link between plants and animals, the snails and slugs connect mollusks and serpents (!); flying fishes connect ordinary fishes and land vertebrates; the ostrich, bat and flying fox connect birds and mammals.

PIERRE LYONET

1707–1789

Traité Anatomique de la Chenille qui ronge le bois de Saule. 4to. The Hague. 1760.[1]

The Larva of the Goat-moth

The *Traité Anatomique* is perhaps the most laborious and beautiful example of minute anatomy which has ever been executed. All the details of structure are given with extraordinary fidelity. The dissection of the head of a caterpillar is a feat which will never be surpassed. Nearly the whole interest of the volume lies in the plates, for the text is little more than a voluminous explanation of the figures.

It is not without surprise that we find that Lyonet was an amateur, who had received no regular training either in anatomy or engraving, and that he had many pursuits besides the delineation of natural objects. He came of a French Protestant family, driven out of Lorraine by the tyranny of Louis XIV, was brought up for the Protestant ministry, turned to the bar, and finally became cipher-secretary and confidential translator to the United Provinces of Holland. He is said to have been skilled in eight languages. His first published work in natural history consisted of remarks and drawings contributed to Lesser's *Insect Theology* (1742). About

[1] Copies dated 1762 have a plate representing the microscope and dissecting instruments used by the author.

the same time, Trembley was prosecuting his studies on the freshwater polyp, and Lyonet gave him some friendly help. Those who care to turn to the preface of Trembley's famous treatise (see p. 279) will see how warmly Lyonet's services are acknowledged. He made all the drawings, and engraved eight of them himself, while Trembley is careful to note that he was not only a skilful draughtsman, but an acute and experienced observer.

Lyonet complains in his preface that while he had been engaged upon a *Recueil Historique* (apparently a general account of Insects), others had managed to publish interesting facts which he had discovered. He resolved to set about a work of smaller compass, and fixed upon the anatomy of the Goat-moth larva. Even here he was anticipated by De Geer, whom he did not consider a very formidable rival. After five years of labour on the goat-moth larva, official work and intrigues caused him to break off. Six more years passed, during which he almost forgot the use of the burin; then he returned to his insect studies, and in two years and a half completed the *Traité Anatomique* with its eighteen beautiful plates.

At the time when the *Traité Anatomique* was published Lyonet meant to proceed with the description of the pupa and imago of the goat-moth, and to trace in detail the anatomical changes which accompany transformation. He made some progress with the second part, but found at the age of sixty that his eyesight was impaired to such an extent as to make it impossible to dissect or engrave any more. Twenty years later he still hoped to put forth a volume of insect anatomy. He had by him the materials collected for the second part of the *Traité Anatomique*, the notes and drawings intended for the *Recueil Historique*, and a study of the sheep-tick, earlier than that on the goat-moth. He did not live to carry out this intention, but long after his death the papers, illustrated by fifty-four plates, were published in the *Mémoires du Muséum*.[1] The plates deal with the sheep-tick; Mallophaga (Anoplura); mites and ticks; the house-spider; Dytiscus, Hydrophilus and some other beetles; the life-histories of various Diptera and Lepidoptera; and the later stages of the goat-moth. The student of life-histories finds valuable details in these posthumous memoirs, but they are too difficult and too dry for ordinary readers.[2]

AUGUST JOHANN ROESEL VON ROSENHOF

1705–1759

Die monatlich-herausgegebene Insecten-Belustigungen. 4 vols. 4to. Nürnberg. 1746-61.

A short biography by his son-in-law, Kleemann,[3] supplies nearly all the information which we possess concerning the personal history of Roesel, whose *Insecten-Belustigungen*[4] (Insect Recreations) is well known as an extensive collection of life-like pictures of insects and other small animals. During most of his career he bore the name of Roesel only, though he was entitled to use the addition of "von Rosenhof," and this he did in his last five years. He was brought up as a painter and engraver, and became expert in the fashion-

[1] Tom. XVIII-XX. 1829-32.

[2] Some account of Lyonet's memoir on Hydrophilus is given in my *Aquatic Insects*.

[3] *Insecten-Belustigungen*, Vol. IV, p. 3.

[4] Roesel sometimes uses the singular, sometimes the plural form in his title-pages.

able art of miniature. From an early age he took an interest in the life of insects, which he was destined to study with passionate devotion. When not much above twenty years old, he visited Denmark and Hamburg. At Hamburg he examined the beautiful insect-drawings of Madame Merian, and determined to draw insects himself. Settling at Nürnberg he became a favourite painter of miniatures, and was much employed by passing travellers. Greatly to the surprise of his friends, he began to collect and rear insects. "Why trouble," asked all Nürnberg, "about mischievous and revolting creatures, which can never have been made by the beneficent Creator, but probably by the devil himself?" Even cultivated persons were slow to respect so eccentric a taste, and agreed with Malebranche that "les hommes ne sont pas faits pour considérer des moucherons." But Roesel persisted in studying and drawing insects until he had attained a skill equal to that which he had admired in Madame Merian. He had many difficulties to contend with; he was unlearned, and knew no foreign language; he was untrained in anatomy or natural history; he had no microscope. But difficulties like these disappear before a firm resolution. Roesel got hold of Derham's *Physico-Theology*, Swammerdam's *Biblia Naturæ*, and Réaumur's *Histoire des Insectes*. A friendly physician taught him to dissect, and helped to give to his descriptions what they called "Reinigkeit des Styli." A professor of mathematics showed him how to grind lenses, and to make a solar microscope, which had never been seen in Nürnberg before. Roesel and some few like-minded associates searched the country for caterpillars and beetles, and a portfolio was soon filled with exquisite drawings. The thought of publishing naturally suggested itself, and in 1741 appeared the first number of a monthly serial called *Insecten-Belustigungen*, which consisted of a few pages of description and a single quarto plate. The work became popular, and sold so well that before long two plates instead of one were issued with each part. In time the monthly parts were collected into volumes, of which three and an imperfect fourth were ready by 1759, the year of his death. Roesel's plates, all engraved by his own hand, amount to over three hundred, many of them crowded with detail. Not even this laborious undertaking sufficed for the author's energy; he studied the comet of 1744, and illustrated its changes by an engraved plate; he investigated the frogs, salamanders, and lizards of Germany, and exhibited their structure and development in twenty-four double-folio plates (1758). His patient labours ceased only with his life. In the year 1752 his health failed; he was paralysed, and it seemed as if his labours were at an end. But some strength returned. Though unable to go abroad, he sent into the fields for water-weeds, and with his unimpaired right hand engraved the beautiful figures which fill the last twenty-seven plates of the third volume of the *Insecten-Belustigungen*. By the use of electrical and other treatment, his powers were so far restored that he was able to go about the house again. The sudden death of his wife in 1757 was a heavy blow, but he went on bravely with his fourth volume, and had completed forty plates, with their descriptions, when the end came, March 27th, 1759.

After Roesel's death his son-in-law, Kleemann, engraved and published many more of the drawings, mixed apparently with new ones, and thus completed a fourth volume, his supplement being often counted as Vol. V. Translations, continuations, and abridgements were attempted in French, Dutch, and English; all but one of them remained imperfect. Schwartz published a *Nomenclator*, in which modern names are given to the figures. Roesel's original drawings are still preserved.

Aquatic Beetles

Roesel begins his account of the water-insects with the Dytiscus family, and Dytiscus marginalis is his first example. He employs no scientific nomenclature, and merely distinguishes two "genera" of water-beetles, viz., the flattened forms and those with prominent rounded back; then he goes on at once to the species, for which he has no accepted names, either Latin or German. The egg, larva, pupa, and imago of Dytiscus marginalis are described and figured; the changes of the larval skin and other minute particulars of the life-history are pointed out. So unfamiliar at that time were the habits of a Dytiscus that Roesel had at first no notion what to feed his captive larvæ upon. Nevertheless he remarks many things which might have escaped a less attentive observer; he knows for instance that the larva comes up to the surface to breathe, and hangs head downwards by a pair of tail-projections. He watched the capture of a victim, and the sucking of its juices, though he does not mention the perforated mandible. Frisch taught him that the larva quits the water before pupating, but he had to learn by experience that the earth into which it retreats must be damp. He knows the marks of the sexes, and gives an excellent account of the suckers on the forelegs of the male. On the whole the description is quite as good as that given in most popular books of our own day, but the figures are much better than those which we are accustomed to see.

Roesel distinguishes another Dytiscus, of which he says no more than that it is very like the first in all its stages. He then goes on to describe and figure the Cybister (since been named after him) and an Acilius.

The last of the water-beetles which Roesel describes is what we now call Hydrocharis caraboides, for which he has no more convenient name than "the middle-sized black water-beetle with convex back." Of this he gives a full and interesting account, the best which I know.[1] He tells us that long after he became acquainted with the full-grown beetle its larva and pupa remained unknown to him. One day, while peering into the water of a ditch, he saw a new insect-larva, which had several pairs of feathered appendages sticking out from the segments of the abdomen. He brought it home, inferred predatory habits from the form of the mouth-parts, and succeeded in keeping it alive until it underwent its transformation, when he discovered that he had been rearing a Hydrocharis. His account gives hints which would be useful to any modern investigator of an aquatic insect of unknown habits.

The Goat-moth

So much of the *Insecten-Belustigungen* is occupied with life-histories of Lepidoptera that it seems indispensable to give a specimen of them. The Goat-moth has been chosen because Roesel's account of the natural history forms an excellent complement to Lyonet's anatomical memoir of the same insect.[2]

Roesel has no name for the Goat-moth (*Cossus*

[1] Hydrocharis caraboides had been described by Swammerdam. See his dedication of *De Respiratione*, his *Hist. Insect. Gen.*, p. 144, and his *Biblia Nat.*, p. 286 and pl. xxxii, 5.

[2] Vol. I, Sammlung iv, pp. 113-128, pl. xvii, xix. Roesel's account of the Puss-moth larva is cited *infra*, p. 306.

ligniperda),[1] and is obliged to speak of its larva as "the large red and flesh-coloured wood-eating caterpillar." It was long before he succeeded in rearing the pupa and moth. When he captured the larvæ, which he found creeping on the ground under trees, he offered them leaves of all the plants which grew about, but they refused to feed on any of them, and soon perished of hunger. A friend happened one day to take Roesel to see an oak tree which was beset with these larvæ ; many of them lay between the bark and the wood in a dark, ill-smelling slime ; others had eaten their way into the solid wood. Having thus learned that his large red caterpillars were wood-borers, Roesel was able to keep them alive and study their habits. When alarmed, they sought to defend themselves by biting and ejecting a reddish fluid from the mouth. Some captive larvæ escaped from a glass dish, which had been left for a few minutes. Being curious to find out how they travelled up the smooth surface, Roesel put them back into the dish, and watched their proceedings. He found that they paid out a silken thread from the mouth, attaching it to the glass on the right and left sides alternately ; this gave a sufficient foothold, and enabled the larvæ to climb up the glass. It was not easy to keep them imprisoned ; when the glass vessel was closed by a wooden lid they gnawed the wood, and it was found necessary to use a metal lid, and bind it firmly to the vessel. Roesel was surprised to find that his caterpillars remained unchanged through the winter, and that their life-history occupied at least two years. During the

[1] Vol. I, Sammlung iv, pp. 113-128, pl. xviii, xix (1741). Réaumur (Vol. I, pp. 308-311, pl. xvii, figs. 1-7) had already given a slight account of the Goat-moth. Roesel does not quote Réaumur, nor does Lyonet quote either of them ; the practice of reference to earlier writers was not yet established, though not unknown.

cold season they rested in a temporary cocoon, which they made in the wood. When food ran short, the stronger ones ate the weaker, leaving nothing behind, for even the hard integument of the head was crushed by the powerful jaws and swallowed. At pupation a cocoon was made of sawdust (or rather of gnawed wood) held together and lined by silk. The long larval stage was followed by a short pupal stage, lasting three weeks or less. When the time of emergence arrived, the pupa burst through its cocoon, and lay with the fore part of its body exposed until the moth was ready to escape. Moths of both sexes were obtained; the males were distinguished by their pectinate antennæ. Roesel tells us that the fertile female lays her eggs in the cracks of oak bark, so that the issuing larvæ can easily procure a supply of the sap, which is their first food.

The Crayfish

Many readers of Huxley's *Crayfish* will remember that he extracts some amusing remarks from Roesel, whose account of this "insect" is almost worthy of Swammerdam. Roesel figures the external form, the chief viscera, the scaphognathite (which he describes as an implement with which the crayfish brushes its teeth), and the leech-like Branchiobdella, which attaches itself to the gills. But why does he give the crayfish the colour of a boiled specimen?

Dispersal of Fruits and Seeds

Having finished his account of the water-beetles, Roesel should have taken up the dragon-flies, which come next in his plan of description, but here we find interpolated, much to our surprise, an account of winged and plumed fruits and seeds. Roesel apologises for his digression in these words :—"While I was engaged upon the dragon-flies, I accidentally caught sight of the winged fruits of the sycamore. It struck me that there was a resemblance between the wings of plants and the wings of insects. Since some writers had compared insects and plants without noticing this point, I thought it might not be disagreeable to my readers if I were to offer my reflections thereupon."

He figures the winged fruit of the sycamore, the winged seeds of pines, and the plumed fruits of several composites. He shows that wind will carry them far away, and hinder them from lying heaped about the parent plant; that the sycamore fruit and the pine seed revolve as they descend, giving time for the wind to act upon them; and that the plumes which make up the "Feder-Ballen" of a dandelion revolve as they drift across the fields, bearing along the heavier fruits which hang beneath.

Freshwater Polyps

Vol. III ends with a supplementary chapter entitled *Historie der Polypen der süssen Wasser und anderer kleiner Wasserinsecten diesiges Landes.* Roesel begins by telling us that when Trembley's discovery of Hydra became known to him, he determined to procure fresh-water polyps and study them. But diligent and long continued search in the pools and streams of Bavaria brought to light no Hydra until many years had elapsed. Roesel's quest was not, however, fruitless. Though he found no Hydra, he found many compound animals which were new to him, and these he described and drew. When he became paralysed, and unable to leave his room, he sought to console himself with the microscope, and sent out for a supply of pond vegetation.

A bowl covered with duckweed was brought to him, and on searching the duckweed he found abundance of the long-desired Hydra. He was now able to study the structure and activities of the polyp, and to discriminate the various species. These investigations gave him full employment during months of seclusion.

Trembley had seen the ovary of Hydra and noted the discharge of the eggs, but had not clearly traced the development of the polyp to the egg. Roesel took much trouble to complete the life-history, but without altogether succeeding.

The floating duckweed also yielded a freshwater polyp of a very different kind, which resembled Trembley's "polype à panache" and Baker's "bellflower-animalcule,"[1] but differed in various details, and especially in the mode of branching. The figures of the three naturalists now make it clear that Trembley and Baker had before them the polyzoan called Lophopus ; Roesel's polyzoan was a Plumatella. The small rounded particles about as big as pins' heads, which developed plumes, and which Roesel calls "der kleinere Federbusch-polyp," are now recognised as the young of the polyzoan Cristatella.[2]

We cannot fail to admire the enthusiasm and industry of Roesel, his delight in the study of living things, and his skill as a draughtsman. But we must not omit to mention that his judgment is often unsound, and that he is too fond of putting forth what can only be called guesses. He was, like nearly every naturalist of the eighteenth century, an untrained amateur, and as he had engaged to supply monthly descriptions of unfamiliar animals, it is not surprising that he should now and then publish

[1] *Employment for the Microscope* (1753), pl. XII, figs. 15-22.

[2] Allman, *Freshwater Polyzoa*, p. 77.

explanations which do him no credit. He observed for example in his Plumatella the bodies which are now called *statoblasts*, a peculiar kind of gemmules. These must, he thinks, be seeds which had been taken in as food. He persuaded himself that they were the seeds of duckweed, and supported the identification by a figure which is totally fallacious. Since his supposed seeds of duckweed were found lying loose in the body-cavity, he concluded that Trembley's description of a continuous alimentary canal, which has been confirmed by all modern observers, must be wrong.

The "History of the Freshwater Polyps" contains an account of Nais, to which several plates are devoted. We can recognise Stylaria proboscidea,[1] Nais serpentina,[2] and two others.[3]

Seven plates are occupied with Stentor, Vorticella, Carchesium, and other ciliate Infusoria. Leeuwenhoek had discovered these things long before; Trembley, Baker, Schaeffer, and Brady had called them polyps, and placed them next to Hydra. Roesel gives much better figures than his predecessors. The history of the Polyps ends with Volvox (which had been already described by Leeuwenhoek and Baker) and a minute, colourless animalcule, which is named the lesser Proteus, because Roesel took it to be a smaller species of Baker's Proteus.[4] He gives many careful figures of this, and shows that it frequently changes its shape, that its granular contents are enclosed by a firmer external layer, and that it occasionally divides spontaneously into two. If he had recognised the nucleus and the contractile vacuole, his account would have been fairly complete. This is the

[1] Pl. 78, Figs. 15-18; pl. 79, fig. 1. [2] Pl. 92. [3] Pl. 93.

[4] Baker's Proteus was a ciliate Infusorian. *Employment for the Microscope*, ch. v., pl. X, fig. 11.

first description of that Amœba which has since become so famous.

Imitators of Roesel

Martin Froben Ledermüller published at Nürnberg in 1760-3 a *Microscopic Delight for mind and eye* (*Mikroskopische Gemüths- und Augen-Ergötzung*), which is chiefly remembered as the first book in which the Infusoria are separated under that name. Martin Slabber's *Natural History Recreations* (*Natuurkundige Verlustigingen*), published at Haarlem in 1778, contains the first figures of a Sagitta, of a rock-barnacle nauplius, of a stalked-barnacle nauplius, and of the Noctiluca which makes sea-water luminous.

THE INVESTIGATION OF THE PUSS MOTH

1634–1892

Ever since the days of Ray and Willughby careful observers had been noting the structure and habits of common insects, each adding some interesting detail to those which he found already recorded. Let us take a single case as an example of many. It would not be easy to make a better choice than the Puss Moth, a large and conspicuous insect common in Central Europe, and so peculiar in its early stages as to tax whatever powers of observation, description and delineation the naturalist may happen to possess, besides raising hard questions, which demand a knowledge of other things than natural history.

Moufet[1] gives six lines and a rude figure to the Puss-moth caterpillar, making mention of its coal-black eyes

[1] *Insectorum Theatrum*, p. 183 (1634).

("oculi anthracini"), eyes, we may remark, which are such only in appearance, for they are unfurnished with lenses, and are not borne on the head, but on the thorax. Izaak Walton[1] translates Moufet's Latin into picturesque English, but adds no further particulars. "You shall find him" (the Puss-moth caterpillar) "punctually to answer this very description: His lips and mouth somewhat yellow, his eyes black as jet, his forehead purple, his feet and hinder parts green, his tayl two forked and black, the whole body stain'd with a kind of red spots which run along the neck and shoulder-blade, not unlike the form of Saint Andrew's Crosse, or the letter X, made thus crosse-wise, and a white line drawn down his back to his tayl; all which adde much beauty to his whole body."

Goedart, Madame Merian and Albin figured the larva, and Goedart remarked the appearance of a human face given to the fore part of the body by an arrangement of black dots; he mentions also the cocoon made of willow wood.

Frisch[2] shortly described the stages, and gave tolerable figures of them. He noticed the protrusible tails of the larva, and the fact that they can be directed to any part of the body which is touched; this led him to the conclusion that they are defensive, but what the enemy might be, and how the tails ward off an attack he did not undertake to explain. Frisch mentions the retractile head of the larva, and figures the round black spots which look like eyes, but laughs at Goedart for having seen in them the resemblance of a face.

Réaumur[3] gives a much fuller and better account of the caterpillar, noticing the rose-coloured hood within

[1] *Compleat Angler*, chap. v., 3rd ed., 1661.

[2] *Beschr. Ins. Teutschland*, Pt. VI, ch. viii. [3] Vol. II, Mém. vi.

which the head can be enclosed, but passing over the resemblance to a face. He explains the mechanism by which the tails can be protruded and withdrawn, offering the very probable conjecture that they are used to drive away ichneumons. He compares the cocoon to a wooden box, in which the pupa lies secure, and shows that it is made of fragments of wood cemented together by saliva. Then comes the puzzling question :—How does the moth extricate itself from the hard shell, within which it undergoes its transformation? Réaumur could only guess (rightly, as it happened) that it secretes a liquid which softens the cement.

De Geer[1] discovered a transverse slit beneath the head of the larva, from which an irritating fluid could be ejected. Once when he happened to touch the larva, it shot into his face two jets of a clear liquid, some of which entered his eye, and for a short time caused sharp pain. He dissected out the reservoir from which the liquid is discharged, and remarked that captive larvæ soon lose the secretion or the power of ejecting it.[2] He makes also the interesting observation that the puss-moth larva is sucked by small flies, which become distended by its greenish blood. The caterpillars seemed to feel little or no pain, and no after-effects were perceived.[3]

Bonnet[4] describes the slit behind the head of the

[1] *Hist. des Insectes*, Vol. I, p. 324.

[2] In another place De Geer describes the ejection of a liquid by the larva of the willow saw-fly, but his observations seem to have escaped the notice of those naturalists who have in recent times discussed the case of the puss-moth larva. He says that the saw-fly larva, when touched, can throw out several jets from the sides of its body, in a horizontal direction and to a distance of a foot or more. The liquid is clear, of greenish colour, and of a disagreeable odour. Captive larvæ lose the power of ejection. The pores by which the liquid issues are situated just above the spiracles. (Vol. II, pp. 936-7.)

[3] The flies were perhaps those of Simulium.

[4] *Sav. Etrang.*, Vol. II, p. 276 (1751); *Œuvres*, Vol. II, pp. 17-24.

larva (between the jaws and the first pair of legs); the conical protrusible papillæ, and the pungent liquid which is discharged. It has, he says, the taste of strong vinegar, and causes acute pain when rubbed into a slight wound; it reddened blue paper (coloured, no doubt, with litmus, which had been introduced by Duclos in 1680) and the blue flowers of chicory. The liquid was stored in a chamber with contractile walls. Bonnet believed that it was used to soften the cocoon when the moth is ready to come forth, a conjecture which has not been confirmed. He found similar papillæ in many other Lepidopterous larvæ, which he identifies as well as he can.

Lyonet[1] worked out more fully the modifications of the pair of anal feet in caterpillars, showing that they may disappear altogether, or as in the puss-moth larva, be converted into protrusible tails. He remarks the retractile head, the appearance of a face (a cat's face, he thinks), the raising of the hinder part of the body from its support, so that the tails may be brought over the head, when the larva is threatened, and the brandishing of the tails. A singular proof is given of the strength of the jaws of this larva. One which was kept in a lead-lined box bit off pieces of lead, and made a hard cocoon, partly composed of lead; after this, Lyonet says, he would not have been surprised if it had made a cocoon of brick. Examination of a cocoon from which a moth had issued showed that in one place the shell had been softened and dissolved. His figures are life-like, and exhibit the characteristic attitudes of the larva, as well as the unmistakable face.

Roesel[2] tells us that after searching long and without

[1] *Anat. de diff. Espèces d'Insectes*, p. 318, pl. xxxiv, figs. 1-15.

[2] Vol. I, Sammlung iv, No. 18.

success for this caterpillar, he came unexpectedly upon a good-sized specimen feeding upon a willow. He was about to seize it when the threatening attitude of the larva and the protrusion of a pair of long filaments from the hinder part of its body made him pause. He thought for a moment of grasping the head-end, which looked less formidable than the other, but as his hand came near, the larva bent its filaments in that direction, and Roesel shrank from provoking it. At last he found it prudent to cut off the twig on which the larva stood, and let it drop into his collecting-box. Three days later a number of parasitic grubs crept out of the unlucky caterpillar, the sure token of an early death, but Roesel soon got more specimens, and completed his study of the life-history. Use made him bolder, and he came to think that the threatening attitude was a mere feint; the larva had, he thought, no real power of injuring an assailant; modern inquiries have shown that this conclusion was premature.

When the Puss-moth larva is alarmed, says Roesel, it contracts its body, makes the hump on the third thoracic segment more prominent, raises its fore part a little from the ground, protrudes its filaments, and turns its head towards the assailant. The head is set off by a red hood, formed out of the segment next behind, and a pair of dark spots, which look like staring eyes. Neither Roesel nor Réaumur dwells upon the resemblance to a face. Roesel recognises that the tail-filaments or their sheaths represent the last pair of false feet, found in other Lepidopterous larvæ; he mentions their rose-colour, their frequent retraction, and the curious fact that the full-grown larva is very unwilling to brandish them.

The tails of the Puss-moth larva reminded Roesel of the projectile filaments behind the head of the swallow-tail larva, which, as he remarks, emit a disagreeable odour. He did not suspect that the Puss-moth larva possesses a very similar organ behind the head.

The young larva, he tells us, is uniformly dark, but at each change of skin the colour becomes modified, until a leaf-green surface, diversified with streaks and patches of other shades, is obtained. Male larvæ are usually distinguished in their last stage by rose-coloured patches on the hump, the seventh segment, and elsewhere. The larvæ are very sluggish, and move about as little as possible.

In our own generation the most remarkable peculiarities of the Puss-moth larva have been interpreted as defensive structures; it escapes notice by its general protective resemblance to the food-plant, and deters its enemies by its terrifying appearance and attitude, as well as by its power of ejecting an irritating fluid. All these points have been worked out by Prof. Poulton; I will not weaken by abridgement his interesting discussion, which is readily accessible to every naturalist.[1]

The irritating fluid, shot out from the slit behind the head, has been chemically examined by Poulton, who finds that it is a strong solution of formic acid, the same acid which constitutes the poison of ants and of the stinging-nettle.

In order to soften the wooden cocoon, the moth emits from its mouth an alkaline liquid, which contains between one and two per cent. of caustic potash; it uses part of the pupal skin as a shield which it can press against the softened shell of the cocoon, without risk of injury from the corrosive liquid.[2]

An expansion of knowledge similar to that which has

[1] Poulton, *Colours of Animals*, chap. xiv.

[2] Latter, *Trans. Entom. Soc.*, 1892, pp. 287-292; 1895, pp. 399-412.

been traced in the case of the Puss Moth has gone on in every branch of natural history, new contrivances being perpetually discovered and old ones better described. Two or three centuries of such labour have made us so rich in natural knowledge that we grieve at times to think how small is the fraction which the best memory can retain.

SECTION VIII. LINNÆUS AND THE JUSSIEUS

CARL LINNÆUS (LINNÉ)

1707–1778

Systema Naturæ. Fol. Lugd. Batav. 1735; ed. X, 2 tom. 8vo. Holmiæ, 1758-9; ed. XII. 3 tom. 8vo. Holmiæ. 1766-8.

Classes Plantarum. 8vo. Lugd. Batav. 1738.

Genera Plantarum. 8vo. Lugd. Batav. 1737.

Species Plantarum, 2 tom. 8vo. Holmiæ. 1753.

Philosophia Botanica. 8vo. Holm. et Amst. 1751.

Hortus Cliffortianus. Fol. Amst. 1737.

Flora Lapponica. 8vo. Amst. 1737.

Flora Suecica. 8vo. Lugd. Batav. 1745.

Fauna Suecica. 8vo. Lugd. Batav. 1746.

Amœnitates Academicæ. 8vo. Holm. et Lips. 1749-85. Three more vols. published at Erlangen, 1785-90.

Lachesis Lapponica, or a tour in Lapland . . . from the original manuscript journal of . . . Linnæus. By J. E. Smith. 2 vols. 8vo. Lond. 1811.

Carl Linnæus was born in 1707, in the same year with Buffon and Bernard de Jussieu, one year after the death of Tournefort, and two years after the death of Ray. His father was co-minister, afterwards minister, of the parish of Stenbrohult in the province of Småland. The province of Småland in southern Sweden is occupied by a monotonous succession of rocky knolls, lakes, swamps and forests; it is intensely glaciated, and the ice-worn rocks are either covered with a scanty moorland vegetation or smothered in drift. Wooden houses, painted red and roofed with live turf, and churches with wooden spires and detached belfries, are scattered about. The trees are chiefly firs and birches, with an occasional clump of beeches. Lakes, such as Lake Möckeln, on which Stenbrohult is situate, often shine through the branches. Linnæus thought this a fit birthplace for a naturalist.

Many of his kindred were farmers, doctors or pastors, several of whom took names (Lindelius, Linnæus, Tiliander) from a tall lime-tree, near which they lived.

The parsonage at Stenbrohult had a good garden, and the pastor, who was something of a naturalist, taught his son botany, besides the ordinary learning of the grammar-school. Linnæus' teachers, whether at home or at the gymnasium, thought him a dunce. There was even talk of binding him to a shoemaker, but a doctor, who happened to be consulted, pointed out that the boy's enthusiasm for natural history was a promising sign, and advised that he should be brought up to medicine.

At the age of twenty Linnæus entered the university of Lund, and now began a bitter struggle, which was destined to last for years. Poverty, insufficient teaching, and paltry jealousies long hindered him from rising. He went to Lund with hopes of assistance from a relative, who was a professor of the university, but the first sight which met his eyes on arrival was the professor's funeral. After a year at Lund he migrated to Upsala, taking with him a small sum of money, his only patrimony. At Upsala he found that no lectures in anatomy, botany or chemistry were to be had; the professor of divinity, Olaf Celsius, was, it is true, a botanist, almost the only one in Sweden, but he was at the time living at Stockholm. Among the undergraduates however was a young man, named Peter Artedi, who was working hard at chemistry and natural history; Linnæus sought his acquaintance, and the two students encouraged and helped one another. When Celsius at last returned to Upsala, he took Linnæus into his house, and allowed him to work in his library.

At this time Linnæus read a review of a pamphlet by Vaillant,[1] which advocated the view that the stamens are the male organs of the plant, a view which had been slowly gaining ground ever since the days of Grew. It used to be thought that the reading of Vaillant was a chance spark which kindled in Linnæus a mighty flame. He himself believed, and allowed his pupils to announce, that the hint taken from Vaillant had incited him to prove "with infinite labour" that the stamens and pistil are sexual organs; and that the discovery of the real function of organs so characteristic of the flowering plants justified their use in the definition of classes and orders. We are now obliged to recognise that neither Vaillant nor Linnæus made any solid contribution to the doctrine of the sexuality of plants, and that the merits of the sexual system are independent, or nearly so, of the functions of the parts employed in it.

Olaf Rudbeck, the younger, professor of botany at Upsala, being now seventy years of age, invited Linnæus to become his adjunct. He began to lecture, set up the botanical excursions which afterwards became famous, and with Rudbeck's help improved himself in ornithology. In 1731 the Academy of Sciences at Upsala desired to promote the exploration of Lapland, and invited Linnæus to undertake the journey. He set out from Upsala on May 13, 1732 (O.S.), being that day twenty-five years old. He describes his equipment in these words: "My clothes consisted of a light coat of West Gothland linsey-woolsey cloth without folds, lined

[1] *Infra*, p. 343.

with red shalloon, having small cuffs and a collar of shag, leather breeches, a round wig, a green leather cap and a pair of half-boots. I carried a small leather bag, half an ell in length but somewhat less in breadth, furnished on one side with hooks and eyes, so that it could be opened and shut at pleasure. This bag contained one shirt, two pair of false sleeves, two half-shirts, an inkstand, pen-case, microscope and spy-glass, a gauze cap to protect me occasionally from the gnats, a comb, my journal and a parcel of paper stitched together for drying plants, both in folio, my manuscript Ornithology, *Flora Uplandica* and *Characteres generici.* I wore a hanger at my side, and carried a small fowling-piece, as well as an octagonal stick graduated for the purpose of measuring." Noting everything remarkable, he followed the post-road to the north, passing along the Baltic coast to Umeå. From Umeå he visited southern Lapland, then made his way to Luleå and visited Lulean Lapland. Ascending the river from Luleå, he crossed the mountains to the Norwegian coast near Bodo. On July 15 he set out to return, and by a laborious and dangerous journey made his way back to Luleå. From this point he proceeded to Torneå, at the head of the Gulf of Bothnia, then turned south and followed the eastern coast of the gulf to Åbo. On October 10th he returned safe to Upsala.

The journey occupied five months, during which nearly four thousand English miles had to be covered, mostly by riding on bad roads, or traversing wild country on foot. In descending the River Luleå he and his guide were on one occasion forced to trust themselves to a rude raft, and to steer their course in the dark. The timbers of the raft parted, and it was with great difficulty that the travellers gained the shore. The Laplanders had a propensity to fire at strangers, and one of them aimed at Linnæus; though the bullet missed its mark, it struck close to the place where he was standing.

He was repaid for his labours and risks by many curious sights. Reindeer and lemmings came repeatedly under his notice. Once he was able to study a fresh beaver. His biographer, Stoever, says that Linnæus discovered a hundred undescribed plants during the journey. He attended to many things besides natural history, observed the mode of life of the Lapps, their tents and furniture, their huts mounted on poles, the dress of the men and women, the threads of reindeer sinew, the leather cradles lined with moss and hair, the great herds of reindeer, the bread, made of flour mixed with chaff, the inner bark of pine-trees, or the powdered roots of buckbean, the different ways of making curds and whey, sorrel-leaves and the dried leaves of Pinguicula being among the expedients employed, the various sorts of cheese, the traps for grouse and ptarmigan, the salmon-pots, bows and arrows, sledges, snow-shoes and walking-poles, the calendars made of splinters of wood, the entertainments of the Lapps, their music and their marriages.

The journey to Lapland was the first of a series of Swedish explorations made by Linnæus, either alone or as the chief of a small scientific staff. In 1734 he made a six weeks' tour in Dalecarlia, in 1741 he was sent to report on the Baltic islands of Oland and Gothland; in 1746 he visited West Gothland, and in 1749 Scania; the last three tours were state-surveys of a simple kind.

On returning from Dalecarlia Linnæus made a short stay in Falun, where he became acquainted with the daughter of Moræus, the town physician. He proposed to her, and was accepted, on condition of becoming qualified to practise medicine; this was to prepare the way for settling in Falun as assistant and successor to his future father-in-law. In April, 1735, he set out for Holland, in order to graduate at the ancient university of Harderwyck; Moræus (probably) and some other friends provided a scanty supply of money. The business of graduation was soon accomplished; a thesis on intermittent fevers (which Linnæus attributed to particles of mud taken up with the drinking-water) being accepted as proof of competence. He was now at liberty to visit botanical gardens and professors. Gronovius was so much struck by the merit of a first sketch of the *Systema Naturæ* that he asked leave to print it. Boerhaave, the first physician as well as the first naturalist in Holland, provided him with letters of introduction. Burmann, professor of botany at Amsterdam, took him into his house for some months, and then a wealthy banker, named Cliffort, who had a country-house and fine gardens at Hartenkamp, near Haarlem, entertained him as long as he could be induced to stay. Such was the respect paid to a knowledge of plants, which was already recognised as very uncommon.

The next three years, most of which Linnæus spent in Holland, were years of extraordinary activity. He now published nine books, which were destined to give a powerful impulse to natural history; among them were the *Fundamenta Botanica*, the *Genera Plantarum* and the *Flora Lapponica*. In Amsterdam he fell in with his old friend and fellow-student, Peter Artedi, who was at the moment without subsistence or prospects. Linnæus obtained for him an employment suited to his taste, the description of the fishes in the collection of Albert Seba, an apothecary of Amsterdam.

The work was nearly finished when Artedi, returning by night from Seba's house, fell into a canal and was drowned; he is still remembered as one of the founders of Ichthyology.

In 1736 Linnæus paid a three-months' visit to England, the cost being borne by Cliffort. Sir Hans Sloane, though seventy-six years of age, was still president of the Royal Society. Linnæus called upon him, but Sloane cared nothing about innovations in botany, and gave his visitor a cool reception; so did the Westphalian Dillenius, Sherardian professor at Oxford, and Philip Miller, gardener to the Apothecaries' Company at Chelsea, though a little later Dillenius would have been glad to keep Linnæus in England, to help him with his botanical undertakings.

On returning to Holland Linnæus once more took up the old laborious life, but it was not long before his health gave way. He quitted Hartenkamp, intending to return home, but allowed himself to be detained for a year at Leyden. A rumour that the young lady whom he had left in Falun was being pressed by another suitor then reached his ears, and after this nothing could detain him. He set out for Paris, where he met Bernard de Jussieu, Réaumur and other prominent French naturalists, took ship for Sweden, and arrived there in July, 1738. At this moment his best prospects seemed to lie in the direction of medical practice. His merit as a botanist was known only to a few, and was deliberately lowered by his rivals. Count Tessin however exerted himself to procure pensions and offices for him. Linnæus was married at Falun in 1739, but the wife for whom he had waited so long proved to be selfish and disagreeable.

The death of Rudbeck in 1740 created a vacancy in the chair of botany at Upsala. Linnæus was a candidate, but Rosen, an old rival, got the post. Next year Linnæus was elected to the professorship of physics and anatomy. The two professors now agreed to change places, and in 1742 Linnæus attained the position which had been his ambition for years, the chair of botany in the chief Swedish university; he was then nearly thirty-five years old. After this his life was one of steady routine. His instructions for botanical excursions[1] give us a glimpse of one part of his work. Even the dress of the student is prescribed; he is to wear a short tunic and thin trousers reaching to the ankle, besides a cap or hat with broad brim; he is to carry Linnæus' *Systema Naturæ* and other useful books, a lens, a botanical penknife and needle, a lead-pencil, a Dillenian vasculum of sheet copper, a bundle of botanical paper, and an insect-box. The excursions are to occupy one or two days a week in the summer-season, to begin at seven in the morning, and to last twelve hours. The botanic gardens at Upsala were another chief interest with Linnæus. After the great fire of 1702 they had been neglected, but were speedily restored by the new professor, who tells with pride of the great improvements which he introduced.

No part of his professorial labours was better planned or better executed than the sending-out of competent pupils to investigate the natural history of distant lands. To every naturalist some of their names are familiar, either as daring travellers, or as the discoverers of remarkable plants and animals. In the *Systema Naturæ* Linnæus commemorates the services of fourteen, to which several more might be added. One-third of the number died abroad in their youth, and half of them

[1] *Philosophia Botanica.*

left published accounts of their discoveries. Hasselquist explored Syria, Egypt and Palestine. Forskål accompanied Carsten Niebuhr to Arabia and Egypt. Sparrman travelled in Kaffirland, and shared Captain Cook's second or antarctic voyage. Thunberg visited the Cape, Java, Deshima (in the harbour of Nagasaki, where the Dutch had been permitted to establish a small factory) and Ceylon. Kalm, after whom the Kalmia is named, spent three years in studying the botany of North America. Solander is remembered as the companion of Cook and Banks.

The old age of Linnæus was sweetened by prosperity and honour. He became wealthy, according to the standard of professors, and was able to spend part of the year in a pleasant country-house at Linné's Hammarby, a few miles to the south-east of Upsala. Here, amidst boulders and pine-woods, which a little resemble those of his native Småland, he built himself a handsome dwelling, with a summer-house or garden-study in the grounds. Many a naturalist has ridden out from Upsala to see the place, which is kept up, as nearly as may be, in the same state as when the great naturalist died there. Every mark of distinction which is thought appropriate to a man of learning was bestowed upon him by his university, his sovereign and his nation. Gout troubled him, but he kept it off by a diet of wild strawberries. His powers began to fail at sixty, and paralysis followed, but he was able to work almost to the last. He died in 1778 at the age of seventy. His son was made professor in his stead, but was unable to maintain the dignity of so great a name ; he died in 1783 at the age of forty-one. Thunberg then filled the chair with greater distinction. The eldest of the four daughters of Linnæus, Elizabeth Christina, published a short paper in the *Transactions of the Swedish Academy of Sciences* for 1762, describing flashes emitted by the flowers of Tropæolum, first observed in her father's garden at Hammarby. At the death of his only son the collections and books of Linnæus were offered by his widow to Sir Joseph Banks, and ultimately purchased by J. E. Smith. They are now the property of the Linnean Society.

The name of Linnæus is commemorated after the custom of botanists by the beautiful little Linnæa borealis, one of the commonest flowers of the Scandinavian woods.

THE *SYSTEMA NATURÆ*

The twentieth century naturalist finds himself tolerably at home in the *Systema Naturæ* of Linnæus. It is true that many of the classes and orders, especially of flowering plants, strike him as unnatural, and he sees that Linnæus did not know enough about the lower invertebrates or the lower cryptogams to classify them properly, but the genera and species wear a familiar look, and both the method and the language are those of modern natural history. The canons of the *Philosophia Botanica* are here practically exemplified, and we see how great is the resulting improvement. Ray was in some important respects more enlightened than Linnæus, as in his separation of the Monocotyledons from the Dicotyledons, but how slow and ambiguous does his system appear to any one who has worked by the Linnean definitions! It was Linnæus who first assigned every known animal and plant to its class, order, genus and species. Uniform binary nomenclature had been gradually making its way, though not without resistance, ever since the time of the Bauhins. Rivinus in 1690 had ridiculed the prolix names then frequent, and had shown that a name of two words at most, the first being the generic name and the second a qualifying adjective, would suffice to denote any species.[1] Linnæus further regulated the usage to be employed,[2] and his authority won universal acceptance for the much-needed reform.

The three Equidæ then known are thus defined in the *Systema Naturæ*:—" *Caballus*. Cauda undique setosa. *Asinus*. Cauda extremitate setosa, cruce nigra supra humeros. *Zebra*. Fasciis fuscis versicolor." Note the terseness which Linnæus gains by dropping all the verbs.[3] The extreme conciseness of Linnæus is not in this instance possible to the modern zoologist, who has at least seven recent Equidæ to consider, besides a long series of finely graded fossil forms.

The description of the habits of the cuckoo is a characteristic example of the Latin style of Linnæus. " Coccyx, incubandi ipse impotens, semper parit in alienis nidis, imprimis in Motacillæ, majori ex parte singula ova, aufuratis prioribus; educat subditum adulterato fœta nido et sequitur nutrix fidelissima mensibus æstatis pulcherrimis frondescentiæ, florescentiæ, grossificationis, dum ille aridis insidens arborum ramis famelicus semper cuculans advocat nutricem, donec sub ortu caniculæ ingratus eam occidat devoretque, unde orbus victitet rapina Avicularum Larvisque Brassicæ aliarumque; non tamen in Falconem transformatur." The Latinity of Linnæus is very likely as strange to the classical scholar as the old fables cited in this quotation

[1] When the genus included but one species, Rivinus thought that the generic name by itself would suffice, but on this point he was overruled by Linnæus.

[2] *Philosophia Botanica*, 1751.

[3] Ray adopts the same practice, but not uniformly.

to the modern naturalist, but both will admit the spirit and picturesqueness of the language. Here is a writer who can say forcibly and concisely whatever he has in his mind. In the description of his Amphibia almost every word expresses repulsion:—"Amphibia pleraque horrent corpore frigido, colore lurido, sceleto cartilagineo, vita tenaci, cute nuda, facie torva, obtutu meditabundo, odore tetro, sono rauco, loco squalido, veneno horrendo." Contrast the cheerful words in which the horse is described:—"Animal generosum, superbum, fortissimum in currendo, portando, trahendo, aptissimum equitando, cursu furens; sylvis delectatur, &c." The mournful wail of the cat ("clamando rixandoque misere amat") is equally well hit off. His biographers tell us that in his youth Linnæus had read a good deal of Pliny the elder. He thought little of grammar or style in comparison with accuracy of fact, and would rather, he said, have his ears boxed thrice by Priscian than once by Nature.

Besides the animal and vegetable kingdoms Linnæus recognised, as the alchemists had done before him, a *Regnum Lapideum.* He went beyond the alchemists in classing the minerals and rocks by genera and species. These misleading analogies have been long abandoned. Mineralogy and Petrology cannot adopt the methods of Biology, and the terms *genus* and *species* lose all meaning when applied to lifeless objects, which are incapable of descent or true relationship.

Some disapprobation was caused by the place assigned to Man in the *Systema Naturæ*, where he is included in the same order with the apes, and in the same genus with the orang. Haller remarked that Linnæus could hardly forbear making man a monkey or the monkeys men. Cuvier gave Man an order to himself, and Owen a sub-class, but these attempts to isolate him have not commended themselves to later zoologists.

THE *AMŒNITATES ACADEMICÆ*

These graduation-theses are of interest to the historian of biological progress. Each bears the name of some graduate of Upsala, but the hand of Linnæus is everywhere apparent. We must suppose that he suggested the topics, furnished the learning required for handling them, enriched them with his mature thoughts, selected and edited them. Sometimes he quotes them as his own.[1]

In Vol. II of the *Amœnitates* (Biberg on *Œconomia Naturæ*) and again in the *Philosophia Botanica*, published in the same year (1751), Linnæus discusses the natural methods of dispersal of seeds with abundant knowledge and acuteness. He enumerates seeds dispersed by the wind, classifying them according to the part which is specially modified, mechanically ejected seeds, hooked seeds and fruits, seeds swallowed by animals, seeds dispersed by running water, &c. Then follows an account of the defences of seeds, most of which we should now place among means of dispersal (deceptive appearance, burying in the ground, spines, &c.).

The thesis entitled "Sponsalia Plantarum" (J. G. Wahlbom) is admitted both by Wahlbom and Linnæus

[1] The *Sponsalia Plantarum*, the *Vires Plantarum* and the *Flora Economica* are thus quoted in the *Philosophia Botanica*, preface and § 52. Dr. Daydon Jackson, who has paid close attention to all matters connected with Linnæus and many other naturalists, informs me that it is now impossible to decide which of the papers in the *Amœnitates Academicæ* are to be attributed to Linnæus. Some are the work of the respondents, except for emendations supplied by the professor, while in other cases, according to the testimony of one of them, J. G. Acrel, the matter of the thesis was dictated by Linnæus, the candidate for graduation having merely to supply the Latin wording.

as the work of the latter. After a review of the history of the sexual theory the *Fundamenta Botanica* of Linnæus receives undue praise, for in this treatise, we are told, the sexes of plants are established with such certainty that no one could hesitate to build upon so solid a foundation an extensive system of botany. The functions of the various parts of the flower are explained. One fact is mentioned which seems to have impressed Linnæus as a boy of sixteen. A vegetable-marrow grown at Stenbrohult had the male flowers removed as they appeared, with the result that not a single fruit was matured.

The contrivances which promote fertilisation are then described. Linnæus seems to regard self-pollination as the rule, though he is aware that pollen may be transferred from a distant plant. He affirms that the anthers and stigma ripen at the same time, and that their relative position is usually such as to facilitate self-pollination. This last notion is taken from Morland and Geoffroy (*infra*, p. 342). In Campanula the pollen is said to cling to the outside of the hairy style, and to reach the stigma (of the same flower) by "certain channels," which the describer would have been puzzled to demonstrate. Sprengel half a century later became convinced by his own observations that it is a mistake to suppose that all bisexual flowers are self-fertilised; it would be nearer the truth to say that "nature does intend that any flower should be fertilised by its own pollen."[1] Pollination, when not brought about by the structure of the flower, is, according to Linnæus, effected either by wind or by insects. He makes the interesting remark that mon- and diœcious plants, especially trees, often flower before leafing, so that pollination is

[1] *Entdeckte Geheimniss der Natur*, pp. 4, 43.

not hindered by the foliage. Sprengel observes that this is true only of *wind-fertilised* trees; the lime, which is insect-fertilised, flowers in the height of summer, though the branches are then clothed with leaves.

Linnæus suspected that the nectar of flowers promotes pollination by insects. Relating after Quintilian the story of a lawsuit between neighbours, one of whom complained that his flowers were rifled by the other man's bees, he adds :—" But in my opinion the bees do more good to the flowers than harm, since by their ceaseless labours they scatter the pollen and bring it to the pistil." He goes on to say that "it is not yet clear what part the nectar plays in the physiology of the flower." It is sometimes said that Linnæus was the first to distinguish the nectaries, but Malpighi had described them in the crown imperial seventy years before. B. M. Hall in the *Amœnitates* discusses their function. He thinks that their secretion moistens the ovary, and in this way favours the growth of the embryo. He admits however that nectaries occur in male flowers, which of course contain no embryo. In the end he adopts the suggestion previously thrown out by Linnæus, viz. that the nectar is a food for insects, which disperse the pollen by fluttering in the flowers.

The *Sponsalia Plantarum* contains some observations on hybrid tulips and cabbages. It is shown that two-coloured tulips may be got by artificial fertilisation from parents which are pure red or pure white. Cauliflowers are liable to be fertilised by cabbage-pollen, and to produce common cabbages. By way of proof the story is quoted from Ray's *History of Plants* of an English gardener who sold a great quantity of what he supposed to be cauliflower-seed, and had to pay damages when only common cabbages came up.

In another thesis there is a discussion of Linnæus' wonderful speculation concerning the Metamorphoses of Plants, and what he calls Prolepsis (Anticipation), a speculation based on the ancient doctrine that the pith is a vital organ, which furnishes the substance of which the seed is formed. Leaves, bracts, calyx, corolla, stamens and pistil are supposed to represent the growth of as many years. The bracts and calyx are outgrowths of the cortex, the corolla of the bast, the stamens of the wood, the pistil of the pith; elsewhere the identification is a little different.[1] Linnæus got the suggestion of his Prolepsis from Cesalpini.

THE SEXUAL SYSTEM OF PLANTS

It was natural for Linnæus to suppose that organs universal in flowering plants, *i.e.* stamens and carpels, would furnish the basis of a simple logical classification of flowering plants. The number of the parts would of course yield particularly easy characters, as Cesalpini had seen long before, and Linnæus accordingly made number prominent in his system. He may possibly have remarked that another part universal in flowering plants, viz. the embryo, had yielded characters serviceable for primary divisions, and that here too the divisions had been founded upon number (of the cotyledons). Linnæus did not however trust to number alone, nor get all his classes by counting stamens. The insertion of the stamens, their inequalities of length, their occasional union, their fusion with the styles, and the more or less complete separation of the sexes were all attended

[1] *Prolepsis Plantarum*, by Ullmark and Ferber in *Amœn. Acad.*, Vol. VI; introduction to *Systema Naturæ*.

to. Some old-established natural groups were thus preserved. Linnæus avoided the association of the Rosaceæ with the Ranunculaceæ, and kept the Labiates, Crucifers, Composites and Orchids (with some few exceptions) free from mixture with alien genera. Not even his own class-definitions could induce him to break up the Leguminosæ into Monadelphia and Diadelphia, though he separated the decandrous genera.

The real test of any classification of living things is:—What sort of groups does it yield? Some of the Linnean classes and orders were soon seen to be unnatural. To separate Anthoxanthum, Holcus and Zea from the Grasses, Veronica from the Scrophularineæ, Salvia and Lycopus from the Labiates, Sanguisorba, Poterium and Alchemilla from the Rosaceæ; to associate Mercurialis with Hydrocharis, Valerian with the Irids, Potamogeton with Holly, and Arrowhead with Oak and Beech; to make an order out of Paris, Adoxa, Elatine and Sagina, and a class (Polygamia) out of Musa, Veratrum, Holcus, Atriplex, Chamærops, Morus, Fraxinus, Rhodiola, Empetrum, &c. must have troubled Linnæus himself. Four of his twenty-three classes are natural assemblages, but all these had been recognised before under other names; two more might have been rectified so as to become tolerable; the rest were unnatural. Linnæus could find consolation only in the facility of his system, which was almost indispensable to the progress of systematic botany at that time. How a handful of European botanists could have dealt with the continual accession of new species from every quarter of the globe, if they had been forced to ponder difficult questions of affinity at every turn, it is not easy to conceive.

Facility of course became less indispensable in time, and the anomalies of the Linnean system less tolerable. Then the foresight of Linnæus became evident. At the very time when he was putting forth a system which his admirers supposed to be immortal, he himself looked beyond it to a system constructed on better lines.[1]

If we are inclined to dwell upon the unnatural combinations of the Linnean system, it may be profitable to remember that De Candolle's grouping of the natural families, which at a later day seemed almost indispensable to rapid determination, had faults of the same kind, and that no substitute for De Candolle's grouping has so far met with general acceptance. Some artificial simplification may long persist in all practical systems of flowering plants. The Linnean method gave present ease at the expense of future improvements. Without condemning it outright, or separating it from all other methods by calling it "artificial," we may admit that Ray's *Synopsis* and the Linnean Fragments and Bernard de Jussieu's garden at Versailles indicated a better path. This was in substance the declared opinion of Linnæus himself.[2]

Linnæus lectured on his natural orders, and late in life expounded his philosophy of a natural system to a favourite pupil, Giseke, who has left an interesting record of his master's theory of classification.

Paul Dietrich Giseke *On the Natural Orders*[3] was never seen by Linnæus, and was not published till fourteen years after his death. It was Giseke's plan to set forth the doctrines of his great master without

[1] The sexual system and the Fragments of a Natural Method are both given in the *Genera Plantarum* (1737) and the *Classes Plantarum* (1738) of Linnæus.

[2] "I have never pretended," he wrote to Haller, "that the method [of the sexual system] was natural." *Epist. ad Hallerum*, Vol. I, p. 284.

[3] *Caroli a Linne Prælectiones in Ordines Naturales Plantarum.* E proprio et J. C. Fabricii manuscripto edidit P. D. Giseke. 8vo. Hamburg, 1792.

criticism or important amplification. Jussieu's *Genera Plantarum*, which had appeared in 1789, is barely mentioned. Some description of each order and of its principal genera is attempted, but no distinctive characters are given, for reasons which will shortly appear.

Affinity was of course not yet traced to descent from common ancestors. Linnæus had compared species to the provinces of a map,[1] a comparison which not only suggests the regular subdivision of primary into secondary, secondary into tertiary groups, and so on, but also the possibility that a group of any rank may directly link more than two others. Giseke exhibits his own "genealogico-geographical map," and this map shows fungi, algæ, mosses and ferns, leading up to conifers and amentaceæ on one side, to palms and other monocotyledons on the other; it is a dim forecast of a phylogeny of plants.

By far the most interesting passage in the book is the report of a conversation between Linnæus and Giseke, which belongs to the year 1771. The conversation has been quoted in Whewell's *History of the Inductive Sciences*, from which the following abridgement is made (3rd ed., Vol. III, pp. 269-271):—"Giseke began by conceiving that an order *must* have that attribute from which its name is derived—that the Umbellatæ must have their flower disposed in an umbel. The 'mighty master' smiled, and told him not to look at names, but at nature. 'But,' (said the pupil) 'what is the use of the name, if it does not mean what it professes to mean?' 'It is of small import,' (replied Linnæus), 'what you call the order, if you take a proper series of plants and give it some name, which is clearly

[1] "Plantæ omnes utrinque affinitatem monstrant, uti Territorium in Mappa geographica." *Phil. Bot.*, § 77.

understood to apply to the plants which you have associated. In such cases as you refer to, I followed the logical rule, of borrowing the name *a potiori*, from the principal member. Can you' (he added) 'give me the character of any single order?' *Giseke*. 'Surely, the character of the Umbellatæ is, that they have an umbel?' *Linnæus*. 'Good; but there are plants which have an umbel, and are not of the Umbellatæ.' *G*. 'I remember. We must therefore add, that they have two naked seeds.' *L*. 'Then, Echinophora, which has only one seed, and Eryngium, which has not an umbel, will not be Umbellatæ; and yet they are of the order.' *G*. 'I would place Eryngium among the Aggregatæ.' *L*. 'No; both are beyond dispute Umbellatæ. Eryngium has an involucrum, five stamina, two pistils, &c. Try again for your character.' *G*. 'I would transfer such plants to the end of the order, and make them form the transition to the next order. Eryngium would connect the Umbellatæ with the Aggregatæ.' *L*. 'Ah! my good friend, the transition from order to order is one thing; the character of an order is another. The transitions I could indicate; but a character of a natural order is impossible. I will not give my reasons for the distribution of natural orders which I have published. You or some other person, after twenty or after fifty years, will discover them, and see that I was in the right.'"

ESTIMATE OF LINNÆUS

Cuvier[1] sets forth the merits of Linnæus in a passage which is both just and instructive, so far as it goes:— "Aimable, bienveillant, entouré de disciples enthousiastes, dont il se faisait autant de missionnaires, attentif

[1] *Eloge d'Adanson*, p. 289.

à enricher de leurs découvertes des éditions multipliées, favorisés par les grands, lié par une correspondance active avec les savans en crédit, soigneux de faire paraître la science aisée, plus que de la rendre solide et profonde, le naturaliste suédois voyait chaque jour étendre sa doctrine, malgré la résistance des amours propres et des préjugés nationaux." Sachs in his *History of Botany* gives a much more penetrating estimate.

Industry, enterprise, sagacity and love of order are conspicuous in all the work of Linnæus. Rapidity of execution he carried to a point incompatible with excellence in detail. Though, like many men of a strong practical bent, he had a quick eye to his own interest, he was habitually guided by high motives. His enthusiasm in the pursuit of knowledge was boundless. He could not sleep for thinking of the treasures brought back from the southern seas by Banks, and endangered, as he thought, by the apathy of Solander. Knowledge that could be applied to the wants of mankind had a special value in his eyes. Not a few of the theses in the *Amœnitates Academicæ* relate to such every-day matters as esculent plants, plants used in dyeing, the ravages of crops by insects, &c. During his journeys in Sweden, according to the instructions of those who sent him out, he accumulated notes and sketches relating to rural industries. In his journey to Oeland and Gothland he remarked the power which marram-grass possesses of binding blown sand, and recommended the planting of this grass on dunes, &c. It has since been used with advantage on the shores of the North Sea and the Baltic, on Cape Cod, in New Zealand, &c. The oak timber stored in the Swedish dockyards was damaged by some boring insect, upon which Linnæus was asked to report. He identified it as a beetle (Limexylon navale), and pointed out that by sinking the timber under water at the season when the beetles emerge, the plague would be abated. It is said that this simple remedy was completely successful.[1]

Early in his career he acquired by unsparing labour a knowledge of the species of plants and a judgment in arranging them which was very uncommon. When he went to Holland as a young man of twenty-eight, he was already qualified to direct the progress of systematic botany, and the lead which he then took he never lost. He had a fair knowledge of birds, and had worked at quadrupeds, fishes and insects; his acquaintance with ores, crystals and petrifactions was slight, but he regularly attended to them on his many journeys.

In his own opinion and that of many of his contemporaries the greatest service which he rendered to natural history was the creation of the so-called sexual system of plants, a method which had no merit except that of facility. He gave a far better proof of his insight by insisting upon the necessity of framing at some future time a natural system, and by indicating as early as 1738 sixty-five natural families of flowering plants. Cataloguing and a rough arrangement were in his opinion the indispensable preliminary to a truly natural system of plants. His zoological system was the fullest and best that had then been devised. His binomial nomenclature, and many of his classes, orders, genera and species form part of the permanent fabric of zoology and botany. He laboured at the improvement of museums and botanic gardens, and left behind him a careful description of the flora and fauna of his own country.

Linnæus was deficient in the patience and candour

[1] Linnæus, *Iter West-Gothland*, p. 173; *Systema Naturæ* (under Cantharis).

necessary for the profitable discussion of deep questions of biology. He was, for example, utterly unable to deal with the great unformulated question of the nature of affinity. He did indeed undertake to explain how affinities arose, but no practical naturalist could have explained it worse. He propounds the general principle that all the species which now exist were created in the beginning.[1] But doubts of various kinds and different degrees of weight had occurred to himself and others; we shall mention only one. The water-gentian (Villarsia or Limnanthemum) has the fruit of a gentian, but the leaf of a water-lily. This could, Linnæus supposes, mean nothing less than that the water-gentian is a hybrid between a gentian and a water-lily, and with incredible rashness he affirmed that the pollen of a water-lily had fertilised the pistil of a gentian. No proofs were adduced, and without pausing to substantiate his crude speculation, Linnæus went on to extend it without limits. The Creator, he tells us, had originally fashioned a few independent forms, which he allowed to commingle; thence came genera. Nature then took the genera in hand, and commingled them, thereby producing species. Lastly, chance operated on the species, and produced varieties. The wonder is that a naturalist who, *stans pede in uno*, put forth so daring and unsupported a theory, should ever have been listened to again on the affinity question. It is a trifle that he contradicts his own general principle (quoted in the last foot-note).[2]

[1] "Species tot enumeramus, quot diversæ formæ in principio sunt creatæ.' *Phil. Bot.*, § 157.

[2] See Linnæus, *Phil. Bot.*, § 157 and passages there cited, *Genera Plantarum*, 6th ed., 1764, p. v. (not in earlier editions), Fundamentum Fructificationis and Plantæ Hybridæ in *Amœn. Acad.*; also Sachs' comments in *History of Botany*, Eng. trans., pp. 105-7.

At the close of the first half of the eighteenth century the prospects of biology were unusually bright. The anatomy of accessible animals, and especially of insects, had been diligently pursued. Minute anatomy had been explored by Malpighi, Leeuwenhoek and others. Swammerdam, Réaumur, De Geer, Trembley and Lyonet had shown how life-histories can be profitably studied. Ray had laboured at the classification of plants and animals. In two cases indeed a promising start had not been followed up; Malpighi and Grew's work on the anatomy of plants and Malpighi's work on the developing chick failed to incite other students to pursue the study with the necessary concentration. The impulse which Linnæus gave to systematic natural history in the middle of the seventeenth century had no doubt its special value. But more than this was wanted; nothing less than the harmonious development of every side of biology could really suffice, but biologists were too few and too ill-instructed for so great a task. They made system not so much the natural complement of other biological studies as a substitute for them. To collectors, gardeners, travelling naturalists and pharmacists a ready method of naming plants seemed to be the thing of chief interest, and they demanded (the Linnean correspondence furnishes instances) that the method should be made as easy as possible. Linnæus was convinced that a truly natural system of plants, very different from his own temporary expedient, was both essential and attainable; he had also a strong conviction that natural history must make itself the servant of mankind for important practical purposes. But he was not uniformly true to his convictions; in his elementary books, which were very widely read, he used language which was taken to mean that system and system alone is the chief aim of botany. Readers of our own age are startled by the emphatic language in which Linnæus seems to teach that the chief business of the botanist is to name and place plants. He would have us to believe that the more species a botanist knows the worthier (præstantior) he is; that they are true botanists who can name all plants; that training in botany aims at turning out in a single year men who can tell all plants at a glance, without teacher, plates or description.[1] Such language is evidently exaggerated; without explanation it is not even intelligible. Method is no doubt of fundamental importance to natural history, but it is absurd to claim that a student of Upsala, who had learned to tell a hundred native species by the help of the Linnean key, was a greater botanist than Malpighi, who possessed only the first rude outlines of a botanical system; or that a collector with his cabinet of two or three hundred Lepidoptera, all of which he could name without book, was a greater zoologist than Réaumur, who could hardly define any species of insects with precision.

For about a century (say from 1750 to 1840 or 1850) those naturalists who treated the authority of Linnæus with servile respect made it their chief, almost their sole business, to catalogue, arrange, name and define. This was strictly true only of the botanists, for in zoology the Linnean method, far less peculiar and exclusive, was less obstinately clung to. Nowhere was the deterioration more evident than in England, where botanists thought of little beyond the naming of plants. Even the teachers of botany rarely used the microscope, and knew little or nothing of minute structure. The experimental study of plant-physiology was pursued

[1] *Philosophia Botanica*, §§7, 151, 256; introduction to *Genera Plantarum*.

only by rare and isolated individuals, among whom Stephen Hales (1677-1761) and Thomas Andrew Knight (1758-1838) were conspicuous. The remarkable though inconclusive experiments of Priestley on the nutrition of green plants were relinquished without an effort to foreign chemists. Robert Brown (1773-1858), the one great English botanist who devoted himself to the tracing of natural affinities, to structure and development, was in all things anti-Linnean. France never accepted the Sexual System of Linnæus. Adanson and the Jussieus kept their attention fixed on the enterprise conceived and partly realised by L'Obel, Ray and Linnæus himself, viz. the recognition and delimitation of truly natural families of flowering plants. De Candolle, a Genevese, but French by training and long residence in France, became the most luminous exponent of their views. France did much more than maintain a protest against the narrower Linnean spirit; she was during almost the whole period of obscuration the leader of biological thought, and her influence tended to enlargement and emancipation. Germany, which had at first received the Sexual System coldly, became more and more Linnean as time went on, and exhibited in something like the same proportion the feebleness of the English botanists.

It would be unjust to lay the whole blame of this temporary and partial arrest of development upon any one man. No scientific worker is so many-sided or so prophetic that he can be trusted to legislate for a great department of natural knowledge. Aristotle, Linnæus and Cuvier were three of the most fruitful labourers in the field of biological science; if each of the three was allowed to obstruct progress, we must seek the cause in that unreasoning submission which mankind so readily accords to those who have succeeded in overcoming its unreasoning distrust.

When we look back from the point of view familiar to the biologists of the twentieth century, we cannot help observing how completely Cuvier's estimate of Linnæus (*supra*, p. 329) ignores the philosophic weakness of Linnæus, and especially his inferiority in this respect to his great contemporary, Buffon, who grasped the conception of a reign of law, and hailed the faint dawn of an evolutionary theory. Linnæus had advanced no further in this direction than St. Augustine.

Our regard for this practical, vivacious, rapid, laborious man, is enhanced by such a little domestic picture as that which Fabricius gives of him:—"We only resided at Upsala for his sake. We did not see nor hear anybody but him and his family. He loved likewise the company of his young friends, as he called us, and every day, in town or on his estate, he came to our room with his pipe, and stayed three or four hours in liberal discourse, but always on topics of natural history. He always took our observations in public and private with true benevolence, refuted or reproved them, and laughed heartily when we could find ingenious arguments to puzzle him."[1]

All defects in the mind and character of Linnæus seemed to be made good by his boundless energy. Confident of his own powers, he dealt rapidly, sometimes impatiently, with every difficulty as it arose, passing on without delay to another and yet another task. Men of this temper accomplish great things in business or politics; in science too they often win distinction, but it is not they who read the deeper secrets of nature.

[1] Freeman's *Life of Kirby*, p. 200.

SOME EARLY STUDIES OF THE FLOWER

It must have been known from time immemorial that fruits are preceded by flowers, and that when the blossom is nipped no fruit is to be expected, but though knowledge of the fact is ancient, knowledge of the cause of the fact is as modern as the seventeenth century A.D. Herodotus, in the middle of the fifth century B.C., brought back from his travels the highly suggestive information that the date-palm is of two sexes, and that in Assyria the female tree was fertilised by dusting it with the branches of the male, but this clue was not followed up, partly no doubt because of the difficulty of the inquiry, but partly because a philosophy which treated general propositions as the source from which particular truths are to be drawn was allowed to override observation. Aristotle showed how imperfectly he understood the case of the date-palm by teaching that in plants the male and female elements are united, so that fertilisation is unnecessary. Had the date ripened its fruit on the northern shores of the Mediterranean, it must continually have reminded botanists that it depended upon artificial fertilisation, and truer notions might have come to prevail. Theophrastus was no better informed than Aristotle; he supposed that both male and female dates bear fruit, and shows no real knowledge of the functions of the parts of the flower, not even distinguishing stamens from styles. His diligence in observing and describing far exceeded his acuteness in explaining, and he made no experiments.

Pliny described the flower of the white lily as possessing a slender pistil (*pilum*, or as others read, *filum*) and anthers (*croci staminis*, according to one reading), which stand up in the centre; elsewhere he notes the yellow-tipped stamens of the rose, and has a special name (*apices*) for the anthers. The case of the date-palm was now better understood than it had been in the time of Theophrastus. Pliny says that the most attentive observers were of opinion that all trees and herbs were sexual. This does not, however, prevent him from saying that many were flowerless, while some bore no fruit.

The revivers of botany in the sixteenth century were too much occupied with the identification and description of medicinal plants to study plant-physiology. Not even in the seventeenth century did it occur to Malpighi, familiar as he was with physiological experiment, that an experimental test of his explanation of the function of stamens was required, or that it might be possible to find out whether native diœcious plants, many of which he knew, ripened their fruit and seeds when the males were excluded. It was probably by mere reflection upon the presence of minute grains in the anther, their liberation by bursting of the capsule, and their adhesion to the stigma, that Millington and Grew[1] were able to throw out the conjecture that the anther is the male organ. Ray[2] reinforced their argument by citing once more the classical instance of the date-palm, confirmed by the recent testimony of Prosper Alpinus (1592), who had seen for himself that the date does not ripen its fruit without pollination. In the desert, says Ray, where artificial fertilisation cannot take place, the wind may possibly answer the same purpose. He pointed out that the parts which Grew had called the male organs are sometimes borne on separate plants, and mentioned common examples, though he failed to

[1] *Supra*, p. 171.

[2] *Hist. Plantarum*, Vol. I, cap. x.

profit by the strong suggestion of a conclusive experiment which his own words conveyed.

Some nine years after the appearance of Grew's *Anatomy of Plants* one man struck the right path, and proved experimentally what Millington and Grew had divined, but failed to establish, viz., that the anthers are the male organs of the flowering plant. Rudolph Jacob Camerarius, professor of botany at Tübingen, was led to attend to the sexes of flowering plants by noticing that a female mulberry-tree, growing at a distance from males, on one occasion bore fruit, though the fruit only contained abortive seeds. This led him to experiment on another plant with separated sexes, the annual mercury. Two female plants, when isolated, produced only abortive seeds.[1]

Fuller and more connected experiments are given in a letter *De Sexu Plantarum*, published in 1694.[2]

In the seventeenth century an important scientific communication, dealing with a question of long standing, was expected to traverse the learning of the topic, and so large a part of the *Epistola* of Camerarius is occupied by the views of ancient writers on generation that it takes an hour or so to discover the truly signicative passages. One is here quoted, which of itself suffices; others are given in Sachs' *History* (Bk. III, ch. i).

"Two examples," says Camerarius, "show how serious are the effects of removing the anthers. When I pulled off the first clusters of [male] Ricinus-flowers, while the anthers were still unexpanded, being careful to prevent the growth of fresh ones, and to leave entire the rudi-

[1] *Ephem. Leopold. Carol. Acad.*, 1691.

[2] Reprinted by Mikan in *R. J. Camerarii Opuscula Botanici Argumenti*, 8vo. Prag. 1797. The Linnean Society possesses a copy of the original edition, which once belonged to Linnæus, and is annotated by him.

ments of the seeds [fruits] with their stalks, not a single perfect triple seed [fruit] was produced, but only empty vesicles, which at last withered away. In like manner the fringe of styles of maize having been carefully cut through, the two cobs [spicæ], which afterwards appeared, were devoid of seed [granum], though there were many empty vesicles."[1]

From these and similar experiments Camerarius concluded that the anthers are necessary to the production of embryos; they are in fact the male organs, the ovaries being the female. He added that botanists should endeavour to find out how far the pollen penetrates the female organ. Sachs justly commends him for faithfully recording his failures, due no doubt to access of pollen in unsuspected ways.

It was natural that Camerarius should take it for granted that ovules are fertilised by pollen derived from the same plant. Ray[2] had indeed remarked the wafting of pollen by the wind, but the first distinct mention of pollination by insects that I can call to mind is that made by Philip Miller in 1724.[3] Sprengel, a hundred years after Camerarius, was the first to show that many flowers are regularly cross-fertilised.

Little attention was paid at the time to these

[1] "In secunda plantarum classe, quibus flores a fructu in eadem modo planta semoti sunt, binis quoque mihi exemplis patuit, quam ægre ferant plantæ apicum defectum. Cum enim primos Ricini globos, antequam apices panderent, detraxissem, et novorum proventui caute occurrissem, salvis, quæ aderant, seminum principiis cum suo thyrso, nusquam perfectum semen tricoccum obtinui, sed vacuas vesiculas hæsisse, tandem exhaustas et corrugatas periisse conspexi. Similiter coma Frumenti turcici jamjam pandenda dextre resecta, binæ postmodum spicæ, omni prorsus grano destitutæ, comparuerunt, utut inanium vesicularum maximus esset numerus." (Mikan's reprint, pp. 75-76.)

[2] *Hist. Plantarum,* loc. cit.

[3] *Infra,* p. 345. It is hard to be sure of a first mention, and earlier observations than Miller's may turn up.

important results; botanists went on surmising and explaining, relying a good deal on the sagacious conjecture of Grew, but thinking, and probably knowing, little of what Camerarius had proved.[1] The few who could judge his work saw that his experiments were decisive as to the fact of fertilisation by means of the pollen of the anthers. Kölreuter, the most competent among these few, strongly asserted that it was Camerarius who had founded the doctrine of the sexuality of plants by his observations and experiments.[2]

Samuel Morland[3] started a theory of the process of fertilisation in flowering plants which, though largely unfounded, influenced later contributors to the discussion. He supposed that the pollen is "a congeries of seminal particles, one of which must be conveyed into every ovum before it can become prolific." The style is a tube (this is taken from Tournefort) designed to convey these "seminal plants" into their nest in the ova.[4] In the Crown Imperial the anthers are "so

[1] In this very year 1694, Tournefort was teaching (*Elémens de Botanique*, p. 47), that "on peut regarder ces étamines comme les vaisseaux excrétoires, qui se déchargent dans les sommets (anthers), c'est-à-dire, dans les bourses particulières; où il se dessèche, et se réduit ordinairement en poussière très-menue"; and (p. 55) that "le pistille du Coquelicot est orné dans le haut de quelques bandes veloutées. Ceux de la *Populago*, de la Gentiane, de la Campanule, et presque tous les pistilles des fleurs sont veloutés dans leur extrémité; c'est-à-dire couverts de poils fistuleux, ou parsemés de petites vessies, qui servent apparemment à verser ce que le suc nourricier contient de moins propre pour la nourriture des jeunes fruits. Les fentes qui sont à leurs extrémités servent peut-être à donner entrée à l'air, qui s'insinuant dans chaque loge, contribue à l'accroissement des graines, et ce suc gluant en défend l'entrée aux insectes qui pourraient les ronger, comme le remarque Mr. Malpighi."

[2] "Præprimis, quia inde patebit, Kœlreuterum fuisse, qui demonstravit, quod R. J. Camerarius fuerit, qui primus sponsalia plantarum per observationes et experimenta cognovisset" (Mikan's preface).

[3] *Phil. Trans.*, No. 287 (1703).

[4] The "seminal particles" of the preceding sentence have become "seminal plants." We remark that Morland writes under the influence of Leeuwenhoek.

artfully placed that they turn every way with the least wind," they are of about the same length as the style, whose top is "villous," so as to catch the pollen as it flies out of the anther. The rain washes, or the wind shakes the pollen down the tube, till it reaches the ovary. In garlic the anthers overtop the style, so as to shed the pollen more easily into its orifice. Morland recommends those who possess good microscopes and skill in managing them to find out "whether the ova or unimpregnated seeds are ever to be found without a seminal plant [embryo]." He mentions the perforation (micropyle) of beans, peas, &c., "at which, I suppose, the seminal plant did enter."

Claude Joseph Geoffroy[1] developed a little further the explanation offered by Morland. His chief contribution to the discussion is the mistaken statement that the anthers and stigma are regularly so placed that the pollen can fall from one to the other. Whether the flower is erect or drooping, the anthers, he says, stand higher than the stigma.[2] He saw that in drooping flowers the stigma will not face the anther from which the pollen is to fall, but this difficulty did not lead him to see whether he had not made some mistake. It is surprising that Linnæus should have adopted twenty-five years later, and apparently without inquiry, a statement so questionable.[3]

Richard Bradley, who was afterwards professor of

[1] "Observations sur la structure et l'usage des principales parties des fleurs." *Acad. des Sciences*, 1711, pp. 210-234. A short notice of Geoffroy will be found on p. 230.

[2] Geoffroy's supposition would have been disproved by a tolerably extensive survey of actual flowers, which would have shown among other things how often the relative positions of anther and stigma vary with the stage of ripeness; in many cases the ripe stigma occupies the very same place which was previously occupied by the ripe anthers.

[3] *Supra*, p. 323.

botany at Cambridge, published in 1717 his *New Improvements of Planting and Gardening*, in which he showed that tulips may be sterilised by complete removal of the unripe anthers, hazels by removal of the catkins. If however the female flowers of the hazel are afterwards dusted with catkins from another tree, their fertility may be restored. Bradley mentions hybrids produced by the cross-fertilisation of Auriculas and other flowers.

Sebastien Vaillant put forth in 1718 a theory of the flower,[1] which is known to have produced an effect upon the mind of Linnæus some ten years later.[2] Vaillant had been a pupil of Tournefort (who was now dead) and a protégé of Fagon, who is remembered not only as chief physician to Louis XIV, but also as one of those who laboured to turn the Jardin du Roi into a well-equipped botanic garden. Fagon had been professor of botany and director of the Jardin; when he retired Vaillant took his place, which Tournefort had aspired to. The king had been persuaded by Fagon to build one hothouse in 1714 and another in 1717, the money for the second being advanced by Fagon. In June 1717 there was a formal opening of the re-organised Jardin, and on this occasion Vaillant delivered a discourse, defending and expanding the doctrine of Grew. The experiments of Camerarius are not mentioned, and Vaillant adduces no experiments of his own. The

[1] *Discours sur la Structure des Fleurs.* 4to. Leyden, 1718, in French and Latin. An earlier edition (Paris, 1717) is mentioned by Fée in the *Nouvelle Biographie Universelle.*

[2] Dr. Daydon Jackson informs me that Linnæus' earliest utterance on the sexuality of plants is his tract *Præludia sponsalia plantarum*, written in 1729, but first printed in 1908. At the time of writing Linnæus had not seen Vaillant's *Discours*, but only a review of it in the Leipsic *Acta*. He recognised that the stamens and pistils were the essential parts of the flower, and determined to found a new method upon them.

flower is defined as a collection of sexual organs, usually enclosed by protective tunics. Like Malpighi, Vaillant pushes very far the analogy with the reproductive organs of the higher vertebrates, making the filament of the stamen a vas deferens and the styles Fallopian tubes. "Il est très certain," he says, "que le germe se rencontre dans les semences des plantes qui n'ont point été fecondées, et avec le parenchyme desquelles ce germe ne fait qu'un continu." He exposes with merciless sarcasm the real or supposed mistakes of Tournefort, Geoffroy and Leeuwenhoek, the fallacies of the hollow style, of pollen passing into the micropyle, and of fertilising filaments. His contempt rouses at last indignation in his readers, who begin to ask whether Vaillant himself is altogether above criticism. Before the *Discours* comes to an end the question receives its answer. Vaillant cites Malpighi (at second-hand) to prove that frogs and chicks may develop in unfertilised eggs,[1] and goes on to assure us that a "souffle" (the "aura seminalis" of Swammerdam, the "subtle and vivifick effluvium" of Grew) passes along the lax tissues of the style, thence by the tracheal vessels to the placentas, and so by the umbilical cords to each little germ or embryo (which, it is to be remarked, Vaillant supposes to be ready-formed *before* fertilisation), "qui présente sa radicule au trou de la coque de l'œuf avec lequel s'abouche le cordon umbilical, pour recevoir de ce cordon et le souffle et la nourriture" (p. 20).

No words are required to show the futility of such an explanation as this, and we shall not discuss further Vaillant's contribution to the sexual theory of plants. It is better worth while to note that explanations very

[1] The passage in Malpighi will be found in the *De Formatione Pulli in Ovo*, p. 2. I find no mention here of frogs, nor any hint that the egg of the chick was unfertilised.

like his were long in favour. Grew, Leeuwenhoek and above all Camerarius had thrown out hints which ought to have prompted an inquiry into the changes which take place in pollen-grains lodged on a stigma; the figures of Malpighi ought to have prompted further inquiry into the origin and growth of the plant-embryo, but neither clue was followed up. Botanists chose rather to discuss the hasty surmises of Grew and Malpighi than to strain their eyes over lenses. Not many years after the publication of the *Discours* the dominance of Linnæan botany cast all close study of plant-physiology into the shade. During the next half-century and more Needham, Adanson, Mirbel and others went on discussing the *aura*, and the entry of fertilising particles by the base of the ovule, and the possibility of embryo-formation without previous fertilisation. Pyrame de Candolle[1] in 1827 was not much better instructed than Vaillant in 1718. At last a new tide of inquiry set in; old theories were critically revised; better microscopes were introduced; and by 1846 Amici and Robert Brown had demonstrated that pollen-grains send out tubes, which enter the micropyles of ovules, and set up the changes which convert simple egg-cells into multicellular embryos. But between 1682 or 1694 and 1823 hardly any progress was made towards an improved knowledge of the process of fertilisation.

Philip Miller, gardener to the Apothecaries' Society, relates some experiments of his own in his *Gardeners' and Florists' Dictionary* (1724).[2] The only new point of interest is that having removed the anthers from tulips, he saw a bee fly into the flower and deposit

[1] *Organographie Végétale*, Vol. I, p. 468.

[2] See article on "Generation." Sachs quotes the fuller but much later article in the *Gardeners' Dictionary*, 6th edition, 1752.

pollen on the stigma, verifying the fact by his microscope.

Linnæus appears as a public advocate of the sexual theory of the flower in his *Fundamenta Botanica* (1736). Long before this Camerarius had demonstrated that the anthers are necessary to the production of fertile seeds, while Ray and Bradley had remarked the possible transference of pollen by wind and the formation of hybrids by cross-fertilisation. More recently Miller, as we have seen, had noticed the transference of pollen by bees. All that Linnæus was able to do was to collect more instances of the same kind, to discuss the question at every opportunity, and to employ the number and disposition of the stamens and styles in his Sexual System, whose validity turned, not upon the functions of the parts of the flower, but solely upon the naturalness of the resulting groups.[1]

THE PARTS OF THE FLOWER.

Theophrastus, and still more distinctly the revivers of botany, were led by their descriptions to discover that precise language was necessary if ambiguity and verbiage were to be avoided. Glossaries of technical terms appear as early as the *Historia Stirpium* of Fuchs (1542). Spigel (*Isagoge in rem herbarium*, 1606), Jung (*Isagoge phytoscopica*, a posthumous work, 1679), Linnæus (*Fundamenta Botanica*, 1736 and *Philosophia Botanica*, 1751) and the elder De Candolle (*Théorie Elémentaire de la Botanique*, 2nd. ed., 1819) each in his own generation extended and rectified the language of botany.

Flower. It would not be easy to find earlier than

[1] Sachs, *History of Botany*, English translation, p. 82, &c. It may be fair to quote also the different opinion of Axell (in Hermann Müller's *Fertilisation of Flowers*, Eng. trans., p. 27) that "the masterly collection of proofs of the sexuality of plants given by Linnæus" did much to settle the question.

the year 1600 a collective name for all the parts which are concerned with seed-production. Spigel enumerates them as we should now do, except that he includes the flower-stalk. Hitherto neither the calyx nor the immature seed-vessel was usually reckoned part of the flower, which (as with Theophrastus) meant *corolla* and nothing more.

Calyx and sepal. The word *calyx*, which is found in Aristotle, Theophrastus and Pliny, long denoted any cup-like structure which enclosed seeds; a pod, for example, might be called a calyx. Fuchs calls it a kind of bag which encloses first the flower and afterwards the seeds—a definition which excludes the deciduous calyx. Valerius Cordus, like Fuchs, hesitated to give this name to a whorl of separate leaves. As late as 1720 we find Pontedera, in his *Anthologia*, explaining that the calyx belongs rather to the fruit than to the flower, which is just what Theophrastus would have said. It was only by degrees that shape, texture and duration were ignored, and that the term became purely morphological. *Sepal* was introduced by Necker (1790); before that date the divisions of the calyx were called *leaves*, *foliola*, &c.

Corolla and petal. What we now call *corolla* bore in ancient times the name of *flower*, which included sometimes the stamens and styles, but not knowingly the immature fruit. *Corolla* seems to have been introduced by Linnæus; De Candolle indeed traces it to Tournefort, but I have been unable to verify this statement. *Petal*, which is merely an adaptation of a Greek word for *leaf*, goes back to Fabius Columna (1592), but long after this date it continued to be called *folium*.

Stamen, anther and filament. Ancient botanists bestowed little attention upon the parts which occupy the centre of a flower, rarely distinguishing the stamens from the styles, but grouping all as *capillamenta or flocci.* Pliny, as we have seen, distinguishes in one place the *pilum* (some read *filum*) from the stamens. He also uses the word *apex* (hat, cap, diadem) for the anther. Fuchs (1542) adopts Pliny's name of *apices*, but calls both the anther-bearing filament and the style *stamens*, according to ancient usage. Ray (1660) gives *anthers* as another name for *apices.* The seventeenth century name for stamens (*attire*) which is used by Grew, &c. has not lasted. Linnæus (1736) has *filament and anther.* *Pollen* is perhaps first mentioned by Valerius Cordus[1] as a yellow dust with which the anthers are besprinkled; the dust of lily-anthers he calls a fine powder of rusty colour (rubiginosus pulviusculus). Pollen is used by Pliny and other ancients as a name for fine meal.

Pistil, carpel, ovary, style and stigma. Bock (1552) studied the large flower of the lily, and described its parts. In the bilberry he names the *pistil* from its resemblance to an apothecary's pestle, perhaps taking the hint from Pliny (see above).[2] Jung and many of his successors call the divisions of a pistil either *pistils* or *styles*; *carpelles* had come into use by 1819, the date of De Candolle's *Théorie Elémentaire*, ed. 2. Linnæus adopted or introduced the physiological division of the pistil into *ovary* or *germen*, *style* and *stigma.*

Superior and *inferior* ovaries were distinguished by Theophrastus, though of course he did not use these words; *monœcious* and *diœcious* were proposed by

[1] In Gesner's volume of botanical treatises, 1561.

[2] In his *New Kreutterbuch* (1546) Bock calls the pistil *schwengelin* (clapper), or *zäpfchen* (pin or peg).

Linnæus; *hypogynous*, *perigynous* and *epigynous* by A. L. de Jussieu (1789), though he was by no means the first to note the differences of structure which they denote. Pontedera (1720) uses *monopetalous*, *polypetalous* and the like; whether he invented or adopted them he does not say.

HISTORICAL TABLE OF BOTANICAL TERMS (FLOWER).

CALYX.	Greek and Roman writers; the term is not defined, nor used with precision.
COROLLA.	Linnæus, *Fundamenta Botanica*, p. 12 (1736).
PISTIL.	Bock, *Stirpium . . . libri tres*, p. 974 (1552). Compare *New Kreutterbuch*, III, xiv (1546). Pliny's *pilum* may have suggested the word *pistillum* to Bock.
SEPAL.	Necker, *Element. Bot.*, *Coroll.* p. 18 (1790) and *Phytologie Philosophique* (1790).
PETAL.	Fabius Columna, *Phytobasanos*, p. 1, 1592, and later works by the same author.
STAMEN.	Pliny.
STYLE.	Spigel, *Isagoge*, pp. 14, 15.
FILAMENT.	Linnæus, *Fundamenta Botanica* (1736). Jung (1679) uses *pediculus*, and is followed by Ray.
ANTHER.	Called *apex* in Pliny, Fuchs, &c. Ray (*Cat. Plant. Camb.*, p. 56) uses *anthera* after Jung, whose *Phytoscopia* had been communicated to him in MS. by Samuel Hartlib. *Capitulum* is another name for anther, used by Jung, and after him by Ray.
OVARY.	Vaillant, *Discours sur la Structure des Fleurs*, 1718.
CARPEL.	De Candolle, A. P., *Théorie Elém.*, 2nd. ed., pt. III (1819).

The last hundred and fifty years have made a new thing of the study of the flower. Kölreuter (1761-6) threw a beam of steady light upon the process of fertilisation, the production of hybrids by the union of distinct species, and the co-operation of insects with flowering plants.[1] Christian Konrad Sprengel (1793) opened up

[1] For Kölreuter's researches we must refer the reader to the pretty full summary given in Sachs' *History of Botany*, Bk. III, ch. i.

that inquiry into the sexual economy of the flower which has since been pursued with such remarkable success by Darwin and Hermann Müller. In the early nineteenth century microscopes, far better and more convenient than any which Malpighi or Swammerdam could command, made it possible to examine in detail the internal changes which constitute the process of fertilisation, and to follow the growth of the most delicate embryonic tissues.[1] We are now fortunate enough to possess something that really deserves to be called a morphology and physiology of the flower, based upon elaborate investigations, not only of a multitude of flowering plants, but of the principal types of the higher cryptogams as well. Even the remote history of the flowering plant, which long seemed to be unsearchable, has been illustrated by the close study of coal-measure fossils, and with such effect that the descent of the flowering plant from cryptogamic ancestors, which had been inferred from facts of recent structure, can be verified at some points by the production of long-extinct transitional forms.[2] These vast extensions of knowledge, which cause modern botany to differ so conspicuously from the botany known to the ancients or the revivers of science, may be said to take origin from the experiments of Camerarius. Then and not till then was it definitely proved that stamen and pistil co-operate to produce the embryo, and a clear reason could at last be given for the long-familiar fact that the fruit is in all higher plants preceded by a flower. What the supposed flowers and fruits of ferns, mosses and seaweeds might be, and whether cryptogams really produce anything

[1] Amici in 1823 observed the pollen-tube emitted from the pollen-grain; in 1830 he discovered its entry into the micropyle.

[2] Oliver and Scott, *Phil. Trans.*, 1904.

which can properly be called by such names as these, were questions that could only be seriously attacked in the middle of the nineteenth century.

BERNARD DE JUSSIEU

1699–1777

ANTOINE LAURENT DE JUSSIEU

1748–1836

A. L. de Jussieu. Examen de la Famille des Renoncules. Mém. Acad. Sci., 1773, pp. 214-240 (pub. 1777).

——— Exposition d'un nouvel ordre de Plantes. Mém. Acad. Sci., 1774, pp. 175-197 (pub. 1778).

——— Genera Plantarum secundum Ordines Naturales disposita. 8vo. Paris. 1789.

Five Jussieus are known to botanical history, and two of the five were among the first naturalists of their age.[1] The founder of this botanical succession was Antoine de Jussieu (1686-1758), a pupil of Tournefort and professor at the Jardin du Roi.

Bernard de Jussieu, younger brother of Antoine, showed more original power than any other of the five. He was able to extend Trembley's discovery of the branching animal, Hydra, by producing examples of permanent animal-colonies (Alcyonium, Tubipora, Flustra, Cellepora), all of which he had examined on the coast of Normandy; he was one of the first to pronounce that corals are animals and not plants; and he devised an arrangement of the plants in the botanic garden of the Petit Trianon at Versailles, which is reckoned as an epoch in the history of botany. He was remarkable for

[1] The eight Bernouillis, all mathematicians, and the four Cassinis, who one after another superintended the Paris Observatory, furnish parallel cases of hereditary scientific genius.

modesty and unselfishness. When his discoveries were appropriated by others, he refused to make any reclamation, asking, What does it signify who gets the credit, so long as the truth becomes known? He thought himself unworthy to hold the professorship of botany vacant by his brother's death, and recommended Lemonnier for the post. He asked nothing and got nothing from the court for his work at Versailles, not even out-of-pocket expenses.

A. L. de Jussieu was nephew to the three last. He was trained by his uncle Bernard, whose example he strove to follow, and whose teachings he expounded.

The list of orders or families of flowering plants which Bernard de Jussieu had drawn up while laying out the garden at Versailles in 1759, was printed for the first time by A. L. de Jussieu in his *Genera Plantarum.* Sixty-one families are here enumerated, of which about forty have endured with some rectification. No characters are given, and the families are not grouped under wider headings. In preparing this arrangement Jussieu was able to make as much use as he pleased of Linnæus' *Fragmenta Methodi Naturalis,* published in the *Classes Plantarum* (1738), as well as of the *Philosophia Botanica* (1751), and of conversations with Linnæus in Paris (1738, when the two botanists were much together).[1] It is equally possible that Linnæus may have picked up hints from Jussieu during his stay in Paris, and that his *Fragmenta* was all the better for them. Jussieu's general arrangement is :—Water-plants (Naiades); monocotyledons with inferior ovary; monocotyledons with superior ovary; dicotyledons (taking first the monopetalous families with inferior ovary,

[1] B. de Jussieu's "Éloge" (*Hist. Acad. Sci.*, 1777) says that he founded his families on the *Fragmenta* of Linnæus. Prof. Vines (*Clark Fasciculus*, 1909) thinks that Ray's *Methodus* was the foundation of Jussieu's Natural System.

and ending with the diclinous and apetalous genera); conifers. If we find Bernard de Jussieu's grouping unnatural in many places, we must not fail to remember how difficult was his task. Down to the present time no really satisfactory grouping and succession of the families of flowering plants has been discovered. While botanists agree in recognising many affinities between particular families, they differ much as to the larger groups, and only the following points can be considered as universally accepted :—(1) that the flowering plants divide naturally into Gymnosperms and Angiosperms; (2) that the Gymnosperms should come next to the Vascular Cryptogams; (3) that the Angiosperms divide naturally into Monocotyledons and Dicotyledons.

Bernard de Jussieu's method of arrangement may be called experimental. Starting with no formed plan, except to keep the monocotyledons and dicotyledons distinct, he tried and tried again to group his genera so as to avoid unnatural associations and successions, until at last he hit upon an arrangement which he thought tolerable.

A. L. de Jussieu on the Ranunculus Family.[1] The explanations which Bernard de Jussieu's modesty would not allow him to give were supplied to some extent by his nephew, who began by taking a single family as his text, the large and varied family of the Ranunculaceæ. He points out briefly the difference between an artificial and a natural method. The framer of an artificial method chooses for himself a single organ, from which all his class-characters are taken. This apparently precise method is liable to the objection that accidental (we should now say, *adaptive*) modification in the organ selected may cause the plant to be referred to a wrong

[1] *Examen de la Famille des Renoncules.*

class; thus kindred plants may be separated, or plants of no near kindred united. Tournefort was led by his artificial system to unite the cinquefoil with Ranunculus, and to separate columbine. Linnæus was led in the same way to unite Colchicum with sorrel, and to separate Persicaria. The natural system, though less facile than an artificial one, avoids these blots; all the organs of the flower are taken into account, and no genera are separated by an arbitrary definition. A. L. de Jussieu proceeds to investigate the Ranunculaceæ. In order to complete his task he would have to determine the characters of the family, and also to fix its place in the series; but for the present he limits himself to the discovery of the characters, which are first enumerated without selection. Some are constant, others more or less variable; each may occur by itself in other families, but the combination of all is peculiar to Ranunculaceæ, and constitutes its essential character. The characters of the different families are not always drawn, Jussieu goes on to explain, from the same organs, though logic would seem to require that this rule should be strictly observed.[1]

Some few families of flowering plants, Jussieu says, are so plainly marked out in nature that they are

[1] So Linnæus:—"Quæ in uno genere ad Genus stabiliendum valent, minime idem in altero necessario præstant." (*Phil. Bot.*, § 169.) "The conflict between natural classification and logic is apparent only. Logicians say that in classifying books, for instance, you may take any property you please, subject, size, &c., as the basis of your arrangement, but having made your choice, you must adhere to it for all divisions of the same rank. Naturalists seem to say something different, for they are agreed that what they call 'single-character classifications,' in which one property is adhered to throughout, are unnatural. The fact is that a natural classification always rests upon one and the same property, viz. *affinity*, *i.e.* relative nearness of descent from some common ancestor. Every natural classification, like every logical classification, proceeds upon a single basis, and the failure of the single-character classifications is due to their replacing affinity by some definition." (Miall, *Hist. of Biology*, p. 126.)

recognised by every botanist; among these are the Labiates, Umbelliferæ and Leguminosæ. He adds the instructive remark that these families, which are so easy to define, are hard to divide, the agreement of the whole family in many particulars of structure leaving only minute differences for the subdivisions. He shows that in Ranunculaceæ the separation of the sepals and petals, the insertion of the stamens and the attachment of the anther-lobes are constant. On the other hand, Ranunculaceæ vary in the character of the stem (herbaceous or woody), in the insertion of the leaves (alternate or opposite), in the number and function of the petals, in the number of the stamens, in the number and cohesion of the carpels, and in the number of seeds to a carpel. The fruit may be an achene, a follicle, a capsule, or a berry. Doubtful cases must be decided by the aggregate of all the characters. Anomalous Ranunculaceæ suggest to Jussieu instructive reflections. Thus Actæa and Podophyllum have single carpels which ripen to berries, yet they are undoubtedly near of kin to the Ranunculaceæ; whether they are to be placed in that family, or in the allied family of the berberries, signifies little in Jussieu's opinion; they make a transition from one family to another, and such transitions, which are faults in an artificial system, are merits in a natural system.[1] Elsewhere he traces a transition in the petals of different Ranunculaceæ.[2] Such reasoning, supported by well-chosen instances, justifies the respect in which the *Examen* has been held by subsequent generations of botanists.

In his *Exposition* A. L. de Jussieu gives a table of

[1] "Cette transition, qui seroit regardée comme un défaut dans les systèmes, est une perfection dans l'ordre naturel." *Examen*, p. 236.

[2] *Examen*, pp. 225-6.

his system, of which we need say no more than that the principles which had guided the constitution of the families are thrown over in the larger groups; the succession of the families is therefore to a great extent arbitrary.

Until the philosophy of classification was remodelled by Darwin it was always thought that the younger Jussieu had grasped for the first time relations which his uncle had but dimly perceived; a different conclusion may force itself upon some modern biologists. Bernard de Jussieu may have been really the wiser man of the two, for in the presence of extremely complex facts and relations he simply laboured at a natural arrangement of living and growing plants, adding no word of theory, "parce qu'il s'est cru trop peu avancé dans la science." A. L. de Jussieu must needs expound, and his expositions are laid down with emphasis, especially in the introduction to his *Genera Plantarum*, a later work than either the *Examen* or the *Exposition.* If his positions seem open to criticism, we shall not judge them severely when we recollect that the *Genera Plantarum* is now over a hundred and twenty years old.

A. L. de Jussieu believed it possible to distinguish beforehand primary and essential characters, constant in a high degree, from such as are only available for minor divisions. He tells us that the embryo is the most essential part of the plant, all the other parts serving to produce, nourish, or defend it, existing for this end only, and withering as soon as it is accomplished; that we must accordingly seek the characters of the primary divisions of plants in the embryo; that other essential characters must be drawn from organs necessary to life and the reproduction of the species, while external characters will only be available for subordinate divisions.[1]

Augustin Pyrame de Candolle[2] subsequently drew the important distinction between ancestral ("morphological") and adaptive ("physiological") characters, though it would be too much to expect that he should have applied his own distinction with perfect consistency.[3] Like all botanists before 1859, he was still inclined to believe that primary divisions must rest upon characters of great physiological importance, an assumption which, plausible as it is, further experience has much impaired. We now believe that no line can be drawn between the two sets of characters; those which are now ancestral were once adaptive, a sufficient reason for dropping De Candolle's terms, "morphological" and "physiological," which imply a difference in kind.

The thirteenth chapter of Darwin's *Origin of Species* has enlarged and rectified the teaching of both A. L. de Jussieu and Pyrame de Candolle by demonstrating:—That valuable characters cannot be indicated beforehand by any rules; that the physiological importance of a structure is no measure of its systematic value, which depends entirely upon the probability of its unbroken transmission from a remote ancestor; that embryonic characters are not always distinctive of large groups; that characters drawn from the heart, which A. L. de Jussieu believed to be of the first importance in the classification of vertebrates, worms and insects, are of uncertain, sometimes of very little, value in those groups; that some external characters (the trimerous flowers and parallel venation of monocotyledons, the

[1] *Exposition*, p. 183. [2] *Théorie Elémentaire de la botanique*, 2nd ed., 1819.

[3] Sachs' *History of Botany*, Eng. trans., pp. 128, 135.

hair of mammals, the feathers of birds) are distinctive of classes; that "no character must be supposed to be natural until it is proved to be so";[1] that "the less any part of the organisation is concerned with special habits the more important it becomes for classification."[2]

Sachs[3] considers that the chief merit of A. L. de Jussieu consists in this, that he first assigned characters to the families, as Caspar Bauhin had done for the species, and Tournefort for the genera.

The *Genera Plantarum* was little read for many years, and no systematic work or elementary treatise adopted the new method until De Candolle introduced it into his text-books. The arrangement of flowering plants proposed by Jussieu and De Candolle still prevails in English-speaking countries.

The *Genera Plantarum* of A. L. de Jussieu marks the fullest development which the natural system of flowering plants attained during the period covered by this volume. We have seen the humble beginnings of that natural system in the herbals of Brunfels, Bock and Fuchs (or still earlier in the Greek botanists), and its partial effacement by the Sexual System of Linnæus; the hidden germ, which had been kept alive by the *Fragmenta* of Linnæus himself and the experimental arrangement of Bernard de Jussieu at Versailles, now starts into vigorous growth once more.

In spite of the inevitable difficulties which arise from vast gaps in the historical succession of the types, botanists are steadily labouring at the improvement of system without misgiving. The phylogenetic discoveries of the last half-century have convinced them that the method of the Jussieus is both valid and indispensable.

[1] Waterhouse, quoted in the *Origin of Species*.

[2] *Origin of Species*, chap. xiii.

[3] *Hist. of Botany*, English trans., p. 116.

SECTION IX. BUFFON

GEORGES LOUIS LECLERC, COMTE DE BUFFON

1707[1]–1788

Histoire Naturelle, générale et particulière, avec la description du Cabinet du Roi. 44 vols. 4to. Paris, 1749-1804. The last eight vols. were posthumous.

BUFFON was born at Montbard, near Dijon, in the same year with Linnæus, and educated at the Jesuit college of Dijon. As a youth of nineteen he travelled in France and Italy, in the company of the young Duke of Kingston and his tutor, who had struck up an acquaintance with him during a stay at Dijon. At the age of twenty-five he came into the enjoyment of his mother's fortune, and from that time made it a rule to divide his time between Montbard and Paris. He became known to the members of the Académie des Sciences as a mathematician and as the translator of Hales' *Vegetable Staticks* (1735) and Newton's *Fluxions* (1740). To the same early period (1732-1749) belong

[1] The following coincidences and successions will help the memory of historical students :—

Linnæus and Buffon were born within four months of each other (1707).

Linnæus, Bernard de Jussieu, Haller, Voltaire and Rousseau died within eight months of each other (Nov. 1777-July, 1778).

The year of Bacon's death was that of Boyle's birth ("Sol occubuit, nox nulla secuta est"), and the year of Galileo's death that of Newton's birth. Napoleon, Wellington, Cuvier and Humboldt were all born in 1769.

Harvey was the pupil of Fabricius, Fabricius of Fallopius, Fallopius of Vesalius.

three papers on the strength of wood. The most interesting general result was that the strength of wood is increased by barking before the tree is felled. In 1738 he paid a three months' visit to England, and with this, unfortunately for his scientific career, his travels came to an end; it would have enlarged and corrected his knowledge of zoology and geology, had he been able to examine other countries in a leisurely way. At this time his attention had not been specially directed towards natural history, for which he might seem to have been physically disqualified; he was short-sighted in a measure which made the close examination of plants and animals tedious. This was his only bodily defect, for he was tall, strong, and handsome.

In 1747, not long before the first three volumes of the *Histoire Naturelle* appeared, Buffon published an account of his experiments on burning glasses. The ancient story that Archimedes had set the Roman fleet on fire by means of burning glasses was thought highly improbable; Descartes had said that the thing could not be done. Buffon resolved to try how near he could come to performing such a feat. He set up a compound mirror, consisting of 168 silvered glass plates, each 8 inches by 4 inches, and capable of separate adjustment. With this he concentrated the sun's rays, so that he was able to kindle wood at a distance of 200 feet, and to melt metals at a distance of from 25 to 40 feet.[1] He next turned his thoughts to the concentra-

[1] *Hist. Nat.*, Suppt., Vol. I, pp. 399-516. "The story that Archimedes set the Roman ships on fire by an arrangement of burning-glasses or concave mirrors is not found in any authority earlier than Lucian" (T. L. Heath, *Works of Archimedes*, p. xxi, 1897). For the alleged repetition of a similar feat by Proclus in the time of Justinian, see Gibbon's *Decline and Fall*, chap. xl. Gibbon says that the silence of Polybius, Plutarch and Livy is decisive in the one case; that of all sixth-century historians in the other.

tion of the sun's rays by means of lenses. There was in his day a very narrow limit to the size of lenses of glass; even if large lenses could have been cast, they must have been enormously heavy, and would have intercepted much light. Buffon proposed to replace the solid lenses by stepped lenses (*lentilles à échelons*), in which the necessary deflection of the rays was to be effected by concentric glass rings, ground out of a single piece of glass, but he was unable to get his invention practically tried. Condorcet,[1] in 1773, showed that the stepped lens might be built up of separate rings; Fresnel, in 1822, actually constructed such lenses, and used them to condense the beams of light emitted from the Tour de Cordouan. It is well known that modern lighthouses are furnished with powerful lenses, built up either of rings or prisms.

THE JARDIN DES PLANTES

A Jardin du Roi was founded in 1626 as a small garden of medicinal plants. The king's physician, Guy de la Brosse, gave up for its use his own house and grounds, which stood just outside the city. For the first hundred years the garden achieved no fame. The physicians who were one after another called upon to manage it were busy with more remunerative employments, and the reputation of the garden had sunk very low when, in 1732, the Académie des Sciences advised that a practical man of science should be put at its head. Du Fay, whose experiments on vitreous and sulphurous or resinous electricity, and on the conduction of electricity through wet string for a distance of over 1200 feet, are still remembered, and whom Fontenelle commemorates as the only man who had ever

[1] *Éloge de Buffon*, Paris edition of 1804, p. 35.

presented to the Academy of Sciences memoirs worthy of publication in each of its six departments, was appointed, and was busy with extensions and plans when he was struck with mortal illness. Buffon aspired to the post of Director, soon to be vacant. His qualifications were not exactly those which the Jardin du Roi might have seemed to require, for he was above all things a mechanic and a mathematician. But he already belonged to the Académie, having been received six years before as a member of the class of Mechanics,[1] and Du Fay's appointment furnished a good precedent for the selection of a man who was practised in experimental science, without being a professed naturalist. Buffon was also known to possess some skill in gardening, and he was ready to promise great things for the future, not nearly so much, however, as he actually performed. His friends used all their diligence on his behalf, and the chemist Hellot even brought a warm recommendation of Buffon to be signed by the dying Du Fay. They carried their point, and on August 1st, 1739, Buffon was made intendant of the Jardin du Roi. "Que dites-vous de l'aventure de Buffon ?" wrote President De Brosses to a friend. "Je ne sache pas avoir eu de plus grande joie que celle que m'a causée sa bonne fortune, quand je songe au plaisir que lui a fait ce Jardin du Roi. Combien nous en avons parlé ensemble ! Combien il l'a souhaité ! Et combien il était peu probable qu'il l'eût jamais, a l'âge qu'avait Du Fay !"

The Jardin du Roi, when Buffon became intendant, covered only a small part of the present Jardin des Plantes. An old country-house lodged the herbarium, the pharmacy, and the collections of Réaumur. Green-

[1] In 1739 he was exchanged into the class of Botany.

houses had just been erected, which Réaumur called "magnifiques."[1]

Buffon was not long in turning out the physicians. He extended the grounds by purchasing (with his own money) the Clos Patouillet and the glebe of the Abbaye de St. Victor. His 12,000 livres of annual pay were swallowed up in the cost of the gardens, and at his death the King owed him no less than 600,000 livres. He kept up a correspondence with Frenchmen resident in distant countries, who sent him cases of live plants, rocks, and minerals. When he bought for the Cabinet du Roi a collection which the King's treasury could not afford to pay for, he only said :—"Que voulez-vous? Le Jardin du Roi est mon fils ainé." He refused to make a private collection, and the visitor to Montbard was astonished to find no museum there.

Buffon was always ready to spend his money, not only on the Jardin, but on his workmen and all who needed help. The iron gates and palisades of the Jardin were supplied from his own forges at Buffon, near Montbard, no doubt at his own expense; they helped to keep his people employed. At Montbard he laid out great sums upon a terraced garden, though the soil was ungrateful. It was useless to point out that the expenditure was not likely to be remunerative; the reply was :—"Mes jardins ne sont qu'un prétexte pour faire l'aumône."

Buffon had meant that his son, "Buffonet," as he called him, should become intendant at the Jardin; with this view he had sent him with Lamarck to visit the chief botanical gardens of Europe. But Buffonet had no great talent, and a more adroit man, the Comte de la

[1] One of them is figured in a vignette prefixed to the fourth volume of his *Mémoires des Insectes*. They superseded older ones built by the celebrated physician and botanist, Fagon.

Billarderie d'Angiviller, who by his own confession had only "les connaissances superficielles d'un homme du monde," got the promise of the succession. In a letter to the disappointed father, he described himself as "un homme d'état supérieur à celui des sçavans." The Comte not only procured the next appointment, but also the survivorship for his brother. It will save reference to another page if we briefly relate in this place all that remains to be said about the history of the Jardin. Buffon died in 1788, and Billarderie d'Angiviller became intendant. He knew nothing about the work and left everything to his subordinates. The expenses soon exceeded any sum which the treasury, loaded with debt, could bear. D'Angiviller resigned,[1] and in 1792 Bernardin de St. Pierre, the author of *Paul et Virginie*, was appointed in his place. Strange to say, he proved himself a good administrator, and did all that could be done at a time when the government of France was disorganised. It was he who founded the menagerie at the Jardin by removing thither six animals which had been kept in the park of Versailles, adding such others as happened to be in the market; it was he, too, who made a beginning with the library of the Museum. But in a short time the post of intendant was suppressed, and in 1793 the Jardin was completely reconstituted, in the agony, be it observed, of the Terror and the revolutionary war. Twelve new professorships were founded, with the condition that the lectures should be free. Daubenton, A. de Jussieu, Geoffroy St. Hilaire, Faujas-Saint-Fond, and Lamarck were on the first staff. This reconstruction brings us within sight of the time

[1] D'Angiviller turns up again in the *Diary* of Crabb Robinson. In 1807 his tall person, very dignified manners, rank, and advanced age combined to render him an object of universal interest at Altona.

when Cuvier was to raise the Jardin and the Museum to the height of their renown.

PLAN OF THE *HISTOIRE NATURELLE*

A man of great ambition, practised in literary work and conscious of literary gifts, but deficient in the ordinary acquirements of the working naturalist, was beyond all others the one to signalise his reign at the Jardin du Roi by some great literary enterprise. Short sight and a memory which did not long retain minutiæ made it impossible for Buffon to study natural history as Ray, Linnæus and Réaumur had studied it. "J'ai la vue courte," he said, "j'ai appris trois fois la botanique, et je l'ai oubliée de même." System he looked down upon, at least when he began his labours, as a mere matter of words. We all know what a dog is; why should Linnæus persuade us to call it *Canis familiaris*?

Buffon was forty-two when the first volumes of the *Histoire Naturelle* appeared (1749), and had already spent ten years upon his preparations for the work.

DAUBENTON AND OTHER HELPERS

He was glad to reinforce his theories and descriptions by the systematic knowledge of a professed anatomist, and found the collaborator whom he needed in Louis Daubenton, a young doctor of Montbard (1716-1799). For the early volumes of the *Histoire Naturelle* Daubenton dissected and described nearly two hundred mammals, but after the completion of the fifteen volumes assigned to the quadrupeds he wrote no more. A popular edition was put forth in which Daubenton's contributions were omitted; Daubenton took this as a slight, and withdrew. He served for many years as keeper of the cabinet of Natural History at the Jardin des Plantes, and continued to study, write, and lecture with unwearied diligence until he died in extreme old age. Guénau de Montbéliard and his wife were also employed upon the *Histoire Naturelle.* Guénau was thought to have caught the trick of his master's style, and some of his descriptions, such as the peacock and the swan, were greatly admired as the work of Buffon himself. After Guénau the Abbé Bexon became a principal contributor to the Birds. Barthélemy Faujas-Saint-Fond (1741-1819) had travelled and written on extinct volcanoes, and to him the minerals were entrusted.

BUFFON AT WORK

There have been accounts of Buffon's mode of work at Montbard which have caught the fancy of the public. Everyone has heard, for instance, how he dressed himself as if for a visit of ceremony before sitting down to write. Buffon himself tells us of his "voûte antique," a vaulted room in a tower at Montbard, which was approached by a succession of garden terraces. Standing at a desk in a nearly empty room he meditated and wrote. He also worked in a more comfortable room in the chateau. His daily allowance of literary labour was eight or nine hours. He made a point of destroying his notes and extracts as soon as they were done with, for fear of becoming buried beneath his papers (a dangerous practice !). Order and temperance were the rule of his life.[1] Mallet du Pan, who had seen him at Montbard in his later years, gives his own impression. "Buffon vit absolument en philosophe ; il est juste sans être généreux, et toute sa conduite est calquêe sur la raison ; il aime *l'ordre*, il en met partout."

Buffon took no notice of criticism. "Il faut laisser la

[1] Humbert, *Mémoires*, p. 27.

calomnie retomber sur elle-même." There were criticisms which were not calumnies, but his pride treated all alike with silence.

No corrected editions of the *Histoire Naturelle* were issued, though many new impressions were called for. Buffon was unwilling to impair the value of early copies, perhaps also unwilling to let it appear what extensive correction was needed. He now and then restated with fuller knowledge and greater care doctrines which he had outgrown, or put forth disquisitions which contradicted his early opinions. These revised and improved chapters were issued as Supplements.

BUFFON AND THE SORBONNE

The fourth volume of the *Histoire Naturelle* contains a curious correspondence, dated 1751, between Buffon and the doctors of the Sorbonne. The faculty of theology had marked fourteen passages as reprehensible, but Buffon, having heard of their proceedings, made haste to declare his submission. Some of the passages seem innocent enough to the non-professional reader, such as his prediction that the present continents will in time be submerged, and that new continents will rise from beneath the sea; or that the sun will probably cool down and cease to shine. Again, the Sorbonne need not have shuddered to hear that a comet striking the sun may have detached planets, which continued to revolve in the same direction as the sun, or that the earth was once liquid by reason of intense heat. Most of the propositions relate to abstract points of philosophy, and might well have been allowed to pass, whether theologically sound or not.

The college of the Sorbonne, founded in 1257, became affiliated to the university of Paris, whose theological faculty it long controlled. The theological censorship of books was entrusted to it by the crown, for the papal Index was never recognised in France. Pascal's *Lettres Provinciales* struck a heavy blow at the prestige of the Sorbonne; all Paris laughed at the faculty which found it "bien plus aisé de trouver des moines que des raisons." In Buffon's day the weight of the Sorbonne had still further declined. Marmontel in 1767 was able to defy with impunity its censure of the tolerant views which he had put forth in *Bélisaire.* But an author who was not only in the king's service (this consideration muzzled neither Marmontel nor Voltaire), but pledged to the production of a long series of costly volumes, could not prudently quarrel with the Sorbonne, which might get the license for printing revoked.

Buffon declared that his hypotheses about the formation of planets were pure philosophical suppositions, that he had no intention of contradicting Scripture, and that some of the suspected passages were capable of a harmless meaning. He undertook to publish his recantation at the first opportunity, and was spared all further proceedings. The passages censured were never cancelled. Intolerance breeds insincerity, and Buffon thought it fair to treat the theological tribunals as a man treats the brambles in which he has become entangled, gently disengaging himself to avoid getting torn. "Buffon sort d'ici," said the President De Brosses; "il m'a donné la clef de son quatrième volume, sur la manière dont doivent être entendues les choses dites pour la Sorbonne." In a letter of 1779 Buffon speaks of the foolish explanation which he had been forced to sign nearly thirty years before. The time was then close at hand when the clergy in their turn were to endure a rigorous and unjust persecution.

Though Buffon often dined with Diderot, D'Alembert and Helvétius in Quesnay's rooms at Versailles, he took care not to be reckoned in what he called "l'escadron encyclopédique."[1]

Descartes, Voltaire and Buffon had all been educated by the Jesuits—a proof that early education may be powerless to restrain speculation.

BUFFON'S LAST YEARS

After his sixtieth year was passed Buffon's life was marked by sorrows such as commonly befall those to whom, in Juvenal's phrase, the over-indulgent gods have granted length of days. He married late (at forty-five), but twenty years before his own death he lost his wife, to whom he had been deeply attached. From about this time a painful and hopeless disease embittered his existence. His son, "Buffonet," having been disappointed of the succession to the Jardin du Roi, entered the army, and served under the Duke of Orleans, afterwards Philippe Égalité. The duke corrupted Buffonet's young wife, who became notorious to all France. Even the *Histoire Naturelle* brought many anxieties; collaborators often failed to satisfy him; the labour of correspondence became oppressive; and Buffon was gradually forced to admit that his great work was destined to remain a gigantic fragment. The endless detail of the animal kingdom surprised and disheartened him. In 1780 he complains to the Abbé Bexon of "ces tristes oiseaux d'eau, dont on ne sait que dire, et dont la multitude est accablante." He found only one remedy—to toil on. His last piece of writing was the account of the magnet, which he had specially reserved for his own pen, and on which he bestowed great pains.

[1] Marmontel, *Mémoires*, liv. v.

All the honours that can be bestowed upon a successful author were showered down upon Buffon. He was ennobled by the King; in 1753 he was admitted to the Académie Française without a canvass. Naturalists, with eyes focussed upon the details of their science, might perhaps withhold their approval, but some of the most eminent men in Europe, famous writers and members of learned academies, helped to spread his praise.

He died on April 16th, 1788. On the 11th of December there was a solemn meeting of the Academy, when Vicq d'Azyr was chosen in his room, and delivered an *éloge* which was thought worthy of the great naturalist. This was almost the last occasion on which an orator dared to speak of the king of France as the head of an enlightened people, the benefactor and restorer of the State. Six months later the Bastille was stormed, and some five years afterwards the orator himself lay on his death-bed, fancying in his delirium that he saw Bailly and Lavoisier summoning him to mount the scaffold. Buffon was at least spared the horrors of the Revolution and the execution of his only son as an aristocrat.

Buffon on System [1]

At the outset of his undertaking Buffon made a particularly grave mistake. He attacked, needlessly and ignorantly, two naturalists whom he ought to have conciliated, for each, in a way of his own, was labouring diligently and successfully to promote Buffon's cause. They were not men to be trampled upon with impunity; the one who had most ground of complaint was Linnæus; the other, who is dismissed with a few contemptuous

[1] Premier Discours. De la manière d'étudier et de traiter l'Histoire Naturelle.

phrases, was Réaumur. Nowhere does Buffon expose his own weakness more irremediably than in carefully studied passages which were meant to wound.

His first sentences set forth the difficulty of dealing with the infinite variety of natural objects. "On doit donc commencer par voir beaucoup et revoir souvent." The chain of nature, descending by imperceptible gradations from man to the simplest mineral, has of course to be mentioned; it was thought in 1749 to be an obvious fact. Then he opens his attack on the systematists.

He has no suspicion that *natural* groups of animals and plants may exist, groups so plainly marked that complete unanimity respecting them is attainable. Ray and Linnæus had already founded groups which are still recognised; both had dimly perceived the relation which we call *affinity*, and had sought to give effect to it. But to Buffon the only ground for preferring one grouping to another is convenience. All groups are mere abstractions; "il n'existe réellement dans la Nature que les individus; les genres, les ordres, et les classes n'existent que dans notre imagination."[1] In some philosophical sense this may be true, but for the zoologist to treat mammals and birds as imaginary groups is much as if the politician were to treat Englishmen and Frenchmen as imaginary groups. To Buffon the arrangement of animals and plants was a problem of the same kind as the arrangement of the books in a library. All groups are as arbitrary as the alphabet, and he prefers that arrangement, whatever it is, which is most serviceable to a popular writer. The search for a perfect system is, he says, as chimerical as the search for the philosopher's

[1] Ray had said the same thing long before; it had been a maxim of certain schools of philosophy.

stone; in each case the seekers for what does not exist may find no end of useful things.

The ancients, especially Aristotle and Pliny, were, he thinks, far better qualified to write upon natural history than any of the moderns. They did not trouble themselves about *useless insects whose manœuvres gratify some modern observers* (this is meant for Réaumur), nor count the stamens of plants which possess no medicinal virtues. It would be better to do as Pliny did, and simply name in alphabetical order at the end of the series all the plants which have no useful properties.

Tournefort's classification of plants was, he thinks, good enough. Linnæus, who needlessly tried to upset Tournefort, had mixed up trees and herbs, and put into the same class the mulberry and the nettle, the tulip and the barberry. All was now drawn out in Greek, and the names of all the plants were changed. One could not stir without the microscope; size, shape, and all evident parts of the plant were ignored; classification had become a matter of counting stamens. What were we to do if the plant had no stamens, or if the stamens varied in number? Species, he goes on, should not be distinguished except where the differences are quite obvious, and every obvious difference should be denoted by an adjective. He repudiates binary nomenclature, and likes every animal and plant to have a single name of its own, a vernacular and intelligible name.

The Linnean classification of animals pleases him no better than that of the plants. He complains that the serpents, shells, and crustaceans are not ranked as primary divisions. The number of the classes should be increased. Instead of unfamiliar groups, which take no note of place of abode, he would retain the obvious divisions of quadrupeds living on the earth, birds living in the air, reptiles, amphibians living in both air and water; cetacean, oviparous and boneless fishes, crustaceans, shells, land insects, marine insects, freshwater insects, &c. He thinks it absurd to put the domesticated dog into the order *Feræ*, whose name implies that it consists of *wild* animals. It is better to call an ass an ass, and a cat a cat, than to pretend that the ass is a horse and the cat a lynx.

Had Buffon kept his satire for what was really objectionable in the Linnean classification, he would have shown himself a more useful critic. There was fair ground for complaint against any system of Mammalia which put the bats with the monkeys, mixed up the elephant, walrus, manatee, and edentates in one order; the carnivores, insectivores, and opossums in another. Nor were the Linnean classes and orders of plants less open to reproach. But Buffon in his wrath condemns the whole without discrimination; his objections, if they could be maintained, would destroy, not the Linnean arrangement only, but every other scientific arrangement that has been proposed.

I do not know that Linnæus ever mentions the name of Buffon in his treatises. In his fragmentary Autobiography he says that Buffon was at last obliged, *nolens volens*, to arrange the plants in the Jardin du Roi according to the Linnean system. According to De Blainville,[1] Buffon never allowed the Linnean system to show itself in the garden, and only consented to allow the Linnean names to be used on condition that

[1] *Hist. des Sciences de l'Organisation*, Vol. II, p. 386 n. It appears from A. L. de Jussieu's *Exposition d'un nouvel ordre de Plantes* (*Mém. Acad. Sci.*, 1774, p. 177) that the system of Tournefort was retained in the Jardin du Roi until 1773, when A. L. de Jussieu's system was substituted, both the binary nomenclature of Linnæus and the natural families of the Jussieus being then adopted.

they were to be painted on the *under* side of the labels.[1]

Then Buffon sketches such a natural history as he would himself approve. Knowledge is divisible into Civil History and Natural History.[2] Natural History should mainly consist of descriptions of natural objects, not minute, not formal; they should above all be readable. The grouping should be such as would suggest itself to the first human observer, and the succession should be determined by the closeness of the relation to man. It is a mistake to let the description of the zebra, which is foreign and unfamiliar, follow that of so well-known and useful an animal as the horse; the dog, which we are accustomed to see running at the horse's heels, might much more fitly come in this place. The natural and ordinary way of looking at things is the best.

As he continued his descriptions Buffon discovered that all this was impracticable. In his chapter on the Degeneration of Animals he stated his views on the arrangement of the quadrupeds, without a hint that he had ever discussed the question before, and put forth as his own a classification which is substantially Ray's.[3] Every useful group which it contains is taken from Ray's *Synopsis Methodica Animalium Quadrupedum.* Ray's mistakes are left uncorrected, but Buffon adds a few of his own. The Cetacea are still excluded from the Mammalia; the hippopotamus is still separated from the swine; Buffon cannot tell, any more than Ray,

[1] The story goes that Linnæus revenged himself upon Buffon by naming the toad *Bufonia*, which he never did.

[2] This division is much older than Buffon; it occurs for instance in Bacon's *De Augmentis.*

[3] It is not, however, to be forgotten that he had given an account of Ray's classification of quadrupeds in his fourth volume (*Discours sur la nature des animaux*).

where to place the edentates, moles, shrews, or bats. He has only two suggestions of his own to propose, both of them unfortunate. He puts together the porcupine and the hedgehog, for no better reason than that both are defended by prickles, and he unites the otter, beaver, desman, and seal into a new amphibious family. When he comes to the Quadrumana, we find him working at system just like any of the naturalists whom he had derided.

Sainte Beuve said very truly that Buffon was "un grand esprit éducable." He was ever learning, all through his fifty years of writing and his eighty years of living, and one can only blame him for not having had the candour to admit his early mistakes, or the grace to thank those who had helped him to correct them.

THE ORIGIN OF SPECIES

Buffon had not proceeded very far with his descriptions of quadrupeds before the ass and its resemblance to the horse raised some important questions in his mind. We might feel disposed to call the ass an inferior horse, like a horse in general structure, but with differences which are perhaps due to climate or food. Horses are more variable in colour than asses—an indication, he remarks, that their domestication is of more ancient date.[1] Wild horses may exhibit some of the characters of the ass, such as the low stature, the greyish brown colour, the tail tufted at the end, and the black stripe across the shoulders. Are the two animals allied in blood? Is it proper to make them, as Linnæus does, two species of the same genus, or have they not been

[1] Buffon neglects the possibility that more pains may have been taken with the selection of horses for breeding.

distinct from the beginning?[1] Is there blood-relationship between quadrupeds, birds, reptiles and fishes? Are there true families among plants and animals? If so, had each of these families a common ancestor? Was there a common ancestor to all animals?[2]

The questions are daring, but the answers which Buffon gave in 1749 are orthodox enough even for the Sorbonne. Revelation teaches us, he says, that all animals sprang fully formed from the Creator's hands. What they once were that their descendants are now. From the time of Aristotle no new species has been known to make its appearance. If the ass was produced by the degeneration of the horse, why do we not find stages intermediate between the two? Buffon defines a species as a succession of similar and interfertile individuals, excluding by the word *similar* the possibility of progressive change. But by the time that his descriptions of quadrupeds were drawing to a close, he was ready to treat families and genera as *souches*, stocks from which living branches had sprung. All his two hundred quadrupeds might have been derived, he now thought, from about forty original forms,[3] and still stronger opinions might be quoted from his later volumes.

What Buffon came at length to see distinctly amounts

[1] "L'âne et le cheval viennent-ils donc originairement de la même souche? Sont-ils, comme le disent les nomenclateurs, de la même famille? Ou ne sont-ils pas, et n'ont-ils pas toujours été, des animaux différens?"

[2] "Si l'on admet une fois qu'il y ait des *familles* dans les plantes et dans les animaux, que l'âne soit de la *famille* du cheval, et qu'il n'en diffère que parce qu'il a dégénéré, on pourra dire également que le singe est de la *famille* de l'homme, que c'est un homme dégénéré, que l'homme et le singe ont eu une origine commune comme le cheval et l'âne, que chaque famille, tant dans les animaux que dans les végétaux, n'a eu qu'une seule souche, et même que tous les animaux sont venus d'un seul animal, qui, dans la succession des temps, a produit, en se perfectionnant et en dégénérant, toutes les races des autres animaux." Tom. IV.

[3] "Dégénération des Animaux," *Hist. Nat.*, Vol. XIV, p. 374, &c.

to this:—(1) that a common plan of structure pervades such vertebrates as he was acquainted with;[1] (2) that some animals reputed to be of different species may closely agree in structure, and be interfertile, at least for one generation. His *degeneration* means simply *progressive change* (of any kind).

He believed (after his first confident statement to the contrary) in the filiation of species, and was ever inclined to extend it farther. He has much to say about the origin of new species by degeneration, but neither this nor any other hypothesis enables him to throw a clear light upon the process of derivation of distinct species from a common ancestor.

A hybridising experiment.—More interesting than all Buffon's speculations about hybridisation is an experiment which, though unsuccessful, was well worth making. A captive she-wolf, two or three months old, was shut up in a courtyard with a mastiff of the same age, and Buffon vainly hoped to get a hybrid litter. The fellow-prisoners, at first friendly, came to dislike one another extremely; in the end the dog killed the wolf, and his own temper had grown so fierce that a few days later he too had to be destroyed. Captive dog-foxes were tested with a succession of bitches, but the repugnance of the animals was not to be overcome.[2] A negative result was apparently well made out. Buffon applied it, with great satisfaction no doubt, to the refutation of Linnæus, who had placed the dog, wolf, and fox in the same genus (Canis). It is needless to enter into the question between Buffon and Linnæus, for a few years

[1] Buffon thought he saw in *all* animals a common plan of structure, but this is explained by his ignorance of all but vertebrates. Réaumur, in the *Lettres à un Américain*, which bore the name of the Abbé de Lignac, had no difficulty in pointing out animals which have no skeleton, no heart, &c.

[2] *Hist. Nat.*, Vol. V, p. 210.

later the Marquis de Spontin supplied Buffon with the particulars of a case in which a she-wolf had produced a litter of which a dog was the father.[1]

GEOGRAPHICAL DISTRIBUTION OF ANIMALS

The description of the lion in the *Histoire Naturelle* (Vol. IX) led Buffon to remark that the so-called lion of settlers in South America (the puma) was not the lion of the Old World, but an animal peculiar to the New World, as are most others found there. To support this statement, he prepared and published in the same volume lists of mammals (*a*) peculiar to the Old World, (*b*) peculiar to the New World, (*c*) common to both. From these lists he drew the interesting general conclusion that no large and conspicuous tropical animal is shared by the eastern and western hemispheres. With these chapters[2] the connected discussion of the geographical distribution of animals may be said to begin; at least I am not aware of earlier remarks which are more than statements of bare facts, such as the brief notes contained in the *Systema Naturæ* of Linnæus.

The species common to the two great land-masses are not found, says Buffon, in the southern, the equatorial, or the north temperate regions, but only in the extreme north,[3] where alone two continents approach one another and make it possible for animals to cross over.

He makes bold to say[4] that birds and fishes, being

[1] Suppt., III, p. 10, and VII, p. 161; Geoffroy, *Ann. du Muséum*, Vol. IV, p. 102, gives another instance. For more recent evidence see Darwin's *Variation of Animals and Plants under Domestication*, Chap. I.

[2] Vol. IX, pp. 97-128 (1761), and Vol. XIV (1766).

[3] Linnæus, speaking in Biberg's name, had already remarked that the species of plants common to the Old and the New World are all of northern range (*Amœn. Acad.*, Vol. II, 1751).

[4] *Hist. Nat.*, Vol. IX, p. 106 (1761).

able to cross at pleasure from one continent to the other, cannot be characteristic of either—a striking example of speculation without facts!

Buffon's information respecting the distribution of animals was of course extremely defective. The voyages of Captain Cook, the founding of the British Empire in India, the occupation of all parts of America by civilised races, the colonisation of Australia, New Zealand, and South Africa, events which have enormously extended geographical and zoological knowledge, were still in the future when these chapters were written (1761 and 1766). The Himalayas were still unexplored, and the Andes were believed to be the highest mountains on the surface of the globe. Zoological system was so imperfect that Linnæus himself had no special orders of elephants, edentates, or marsupials (*Systema Naturæ*, 12th ed., 1766). Buffon had been strongly inclined to reject genera, families and orders altogether, but this was a freak of his own, which he was now ready to relinquish. He counted some two hundred species of mammals, of which about seventy were found in the New World. No naturalist suspected that the continents are divisible into six or seven distinct zoological regions. It was still possible to maintain that animals spring from the soil; according to Buffon, the American continent, undrained and drenched with rain, could not develope the "germes" or "principes actifs" of the larger quadrupeds. There was no science of palæontology, and only one indubitable example of an extinct animal (the mammoth) could be quoted.

He is inclined to believe that in the New World nature is less energetic, less varied, and less powerful; the largest quadrupeds are inferior to the largest of the old world; man himself is less numerous and less enterprising. Only such low things as spiders, insects, frogs, and toads thrive best in America. He assures us, without offering any proof, that all animals which have been introduced by man, or have migrated into America, have diminished in size. Then comes the *pourquoi*, for Buffon seems to have forgotten his own maxim (p. 386). He attributes to the humidity and low temperature of America the poor development of its quadrupeds and the rank growth of its reptiles and insects. The humidity and low temperature he explains by the shape of the American continent, the direction of its mountain-chains, the longer duration of its submergence, the greater antiquity of its high land, and the more recent formation of its plains. It is true that the largest *living* mammals of America are small when compared with the largest of the Old World, but Buffon's explanation is demolished by the gigantic *extinct* animals of America (elephants, Uintatherium, Megatherium, Diplodocus), as Darwin[1] remarked.

BUFFON'S ORGANIC MOLECULES

Everybody knows that a tree produces a great number of parts (buds and seeds), each of which is able to produce a new tree. Buffon carries the analysis still further (in his imagination), and arrives at what he calls the "organic molecules" as the smallest *living* units of a tree or any other organism. He had some ground for believing in the material existence of such organic molecules, though he does not profess to have seen them with his own eyes. While the first three volumes of the *Histoire Naturelle* were in hand he had been reading Leeuwenhoek, and had there found such things as the

[1] *Naturalist's Voyage*, chap. viii.

cells of which the tissues of animals and plants are composed, in addition to corpuscles which circulate in the blood, sperms, and bacteria. Out of these he seems to have framed his organic molecules, concerning which he proceeds to lay down a number of confident statements. He tells us that they are primitive and incorruptible; that they exist both in plants and in animals; that they travel about the organism, but may collect in special reservoirs. Nutrition and reproduction are due to their combination; the destruction of the organism may result from their dissolution; they explain the regeneration of lost parts.[1] They are plentiful in the chyle and other products of digestion, in the seeds of plants, in the eggs and fertilising fluids of animals, and in the *interstices of the teeth* (this last points to Leeuwenhoek's bacteria). They increase in number and activity under the influence of light, a remark which may have been suggested by observation of the green growth which forms in vessels exposed to the sun's rays. The moving particles seen in animal infusions are organic molecules set free by the decay of the tissues. Fermentation is perhaps set up by the activity of the same molecules. Infusions of potent drugs swarm with the molecules, which appear far sooner than in ordinary infusions. The poison of vipers depends upon molecules in an exalted state.

New individuals originate in masses of organic molecules, and take their shape from *moulds* existing in the parent. These moulds determine not only the external form, but also the internal structure of the new individual, a statement which has its mechanical difficulties. We perceive at length that Buffon's moulds are purely

[1] Réaumur had forty years earlier maintained that when a crayfish loses a limb, the egg or germ of a new one germinates and developes (*Mém. Acad. Sci.*, 1712).

philosophical; the whole body or any part of it may be such a mould.[1] He boldly affirms that new plants and animals can arise by the fortuitous association of his organic molecules. Mushrooms, internal parasites, and earthworms are, he says, generated spontaneously.[2]

This theory of organic molecules undoubtedly contains hints for a true doctrine of cells. Buffon seems to teach that both animals and plants are built up of cell-units; that the cells may exist as separate organisms, or move about within some more complex organism, or be combined into tissues, or contain smaller organised units, such as we now call *plastids.* He is within sight of the truth that both germ and sperm, when reduced to their lowest terms, are cells. It might seem necessary to give Buffon handsome credit for such anticipations as these, but we are checked by reflecting (1) that he did not discover or observe for himself any of the cell-structures which he mentions; and (2) that Buffon's theory, in the completest form which it ever attained, was quite as likely to lead a student wrong as to lead him right. He never attempted to verify or correct his speculations by such experiments as Spallanzani devised—experiments which demolished Buffon's supposed proofs of spontaneous generation.

There is a strong, but merely superficial resemblance between Buffon's organic molecules and Darwin's gemmules, and Darwin found whole pages of Buffon laughably like his own.[3]

[1] *Hist. Nat.*, Vol. II, pp. 41-2.
[2] *Hist. Nat.*, Vol. IV, p. 335, &c.
[3] *Hist. Nat.*, Vol. II, pp. 322, &c.

BUFFON ON FOSSIL REMAINS

In Buffon's earlier days it was not yet universally admitted that the earth contains the remains of animals which are now altogether extinct. Some, and among these was Buffon himself, thought it possible that ammonites might still survive in the depths of the sea. Less than a century before it had even been gravely maintained that fossils are not necessarily the remains of animals or plants which once lived, but the first volume of the *Histoire Naturelle* shows that common-sense had at last settled this question in spite of the philosophy of the schools. Buffon there explains that fossil shells may retain the colouring and texture of recent shells; some are bored by whelks; old and young are often found together; the fossil teeth of fishes often show signs of wear.[1]

In his ninth volume (1761) he was able to declare that "le prodigieux *mahmout* (mammoth) que nous avons jugé six fois au moins plus grand que le plus fort éléphant, n'existe plus nulle part" (p. 126); but, in spite of his exaggerated notion of its superior size, he was not certain that it was specifically distinct from the existing elephants. A few years later he was able to quote American mastodons as indubitably extinct, and was by this time convinced that some mollusca (ammonites, orthoceratites, belemnites), as well as some fishes, had died out altogether.[2]

SPECULATION ON THE GROWTH OF STAGS' HORNS

Buffon's propensity to unguarded speculation often leads him into absurdity, as for example when he attempts to explain the growth of a stag's horns. He

[1] *Hist. Nat.*, Vol. I, pp. 291-292. [2] *Hist. Nat.*, Suppt., Tom. V (1778).

tells us confidently, but without pretence of experimental proof, that the growth of horns in the stag is due to an accumulation of organic molecules derived from its food. The lichen on which the reindeer feeds is, he makes bold to say, a richer food than the leaves, bark, and buds which nourish the stag; hence the more abundant secretion of horn in the reindeer. The organic matter is not, he tells us, perfectly assimilated in deer, and this explains why their horns resemble the branches of trees in form and texture; they are vegetable structures grafted upon the bodies of animals. He suggests that the periodical shedding of the horns is due to their vegetable origin; it is analogous to the fall of a ripe fruit. Ancient naturalists had believed that ivy will grow round the horns of a living stag, and Buffon thinks that if the fact were established, it would constitute an interesting proof of the fundamental identity of stags' horns and wood. He accepts without misgiving the old belief that the beaver gets his scaly tail by feeding on fishes (this is mentioned in Harrison's *Description of Britayne*, 1577, and doubtless in earlier books also).

THE RANGE OF BUFFON'S STUDIES

Buffon did more than fix the attention of his generation upon "les idées générales sur les animaux," and "l'histoire de l'homme," though these were his chief themes; he has left the marks of his powerful mind upon such problems as the amelioration of wool and the strength of wooden beams, as well as upon problems which have no direct reference to natural history. It was Buffon who explained to the world what was meant by the expectation of life at different ages, and how it was ascertained.[1] Again it was Buffon who first propounded and solved the celebrated mathematical needle-problem.[2] Upon a surface ruled with equidistant and parallel straight lines a rod, such as a needle, which must be shorter than the distance between the lines, is thrown at random ; what is the probability that it will intersect one of the lines? Buffon shows that the answer involves a number which expresses, among many other things, the ratio of the circumference of a circle to its diameter, and that this ratio may be experimentally determined by a long series of trials with the needle. He announced, not quite for the first time, that the stone axes which were popularly called "pierres de foudre," or "thunder-stones," were the work of early races of men.[3] These are but specimens of contributions to knowledge which told with great effect upon that class of alert and speculative men, of whom Erasmus Darwin is a familiar example.

BUFFON'S MAXIMS, DEFINITIONS AND DESCRIPTIONS

Some of Buffon's forcible sentences have become universally current, like the happiest phrases of national poets. How many writers have either quoted or adapted his description of the horse :—"La plus noble conquête que l'homme ait jamais faite, &c.!" Who does not know his definition of genius :—"Le génie n'est qu'une plus grande aptitude à la patience"?[4] "Le style est l'homme même" is not less familiar.[5] Another maxim

[1] *Hist. Nat.*, Suppl., Vol. IV, pp. 46-148 (1777).

[2] *Ibid.*, pp. 100-3.

[3] *Ibid.*, Suppl., Vol. V, p. 225 (1778).

[4] Quoted in this form by Hérault de Séchelles. Buffon's *published* definition is very different :—"La vue immédiate de l'esprit."

[5] Some give it in this form :—"Le style est *de* l'homme même," and declare that the other version misrepresents Buffon's meaning (Vapereau's *Diction-*

which has proved serviceable is this:—"Le but de la philosophie n'est pas de connoître le *pourquoi*, mais le *comment* des choses."[1] Some few will find the following sentence full of meaning:—"Tout sujet est un, et quelque vaste qu'il soit, il peut être renfermé dans un seul discours."[2] Even Universal History may be so handled as to leave a simple and powerful impression, if there is a Montesquieu or a Bossuet to handle it. Professor Huxley used to say that the principles of sound Geology were embodied in the words:—"Pour juger de ce qui est arrivé, et même de ce qui arrivera, nous n'avons qu'à examiner ce qui arrive."[3]

The reader who wishes to see Buffon at his best may turn to his description of the horse and stag, which are praised by Sainte-Beuve,[4] or to those of the horse and camel, which are praised by Gibbon.[5] A. P. de Candolle (*Mémoires et souvenirs*, p. 83) reports, but not of course from personal knowledge, that Buffon liked to find out which of his descriptions a new acquaintance admired most, and that those pleased him best who hesitated between the ass and the horse.

ESTIMATE OF BUFFON

During Buffon's lifetime his merit as a naturalist was hotly debated. It was admitted that he was no botanist;

naire, art. Buffon). But the "Discours à l'Académie Française," printed in *Hist. Nat.*, Vol. IV, omits the *de*. Whatever authority may exist for the insertion of the particle, its omission can hardly be wrong.

[1] *Hist Nat.*, Vol. V, p. 104.

[2] "Discours a l'Académie," *Hist. Nat.*, Suppt., Vol. IV, p. 5.

[3] *Théorie de la Terre*, *Hist. Nat.*, Vol. I.

[4] *Causeries du Lundi*, Vol. IV. Sainte-Beuve originally added the swan, but afterwards became convinced that "le *Cygne* tant vanté pourrait être du pur Bexon." Buffon's letter to Bexon, Dec. 24, 1779, puts the matter beyond question; he speaks of "votre beau Cygne."

[5] "Read (it is no unpleasing task) the incomparable articles of the Horse and the Camel in the Natural History of M. de Buffon," *Decline and Fall*, note to chap. 50.

was he really a zoologist? Linnæus never quoted him, or seemed to know that he existed. Réaumur instigated the anonymous *Lettres à un Américain*, in which the Abbé de Lignac treated Buffon with scorn. Linnæus and Réaumur had personal reasons for disliking Buffon, but the adverse opinion of judges so eminent, however it might be explained, was damaging in a high degree. The majority of professed naturalists in France, Germany, and England held that no loftiness of thought and diction, no liveliness in description, no startling theories of creation could atone for Buffon's contempt of system, or for the blunders which disfigured his pages. Many contrasted the accuracy of Daubenton with the carelessness of Buffon, or said with D'Alembert that Buffon was "le grand phrasier, le roi des phrasiers."

With the general reading public the case was very different. Buffon's handsome and costly volumes sold in large editions throughout his long life. The *Histoire Naturelle* was not only bought but read, and all those passages which a thoughtful reader, ignorant of zoological details, could understand were studied and remembered. Men of letters, such as Diderot and Gibbon, spread his fame. Gray thought that his general view of the surface of the earth and of the nations which occupy it was the best epitome of geography which he had ever met with. What Buffon had to say about the expectation of human life at different ages, about the duration of life in animals and its ratio to the duration of the period of growth, about the history of the earth and the history of continents, entered into the common stock of knowledge.

Having got the ear of mankind, Buffon so used his opportunity as to heighten their interest in natural history. During the first half of the eighteenth century science had already made good its claim to the attention of cultivated people, but the only science which counted so far was the science of Descartes and Newton. Réaumur, and still more emphatically Buffon, showed that the problems of life are not less interesting nor less important than the mechanics of the universe. Buffon's animated descriptions and bold speculations did much to prepare men's minds for Cuvier and Darwin, for geology, palæontology, and the theory of descent.[1]

Buffon was far in advance of his own generation in teaching that all geological phenomena can be explained by the long-continued action of ordinary causes. Cuvier's *Revolutions de la surface du Globe* was written expressly to refute this doctrine.

It is almost unnecessary to say that the *Histoire Naturelle* swarms with errors. All comprehensive works in natural science are found to do so, when they come to be examined by the light of a later age. Buffon is only to be blamed for presumptuous and wanton mistakes, of which there is no lack.

It is now more than a century since Buffon laid down his pen for ever, and we are able in some degree to estimate the value of his labours. He left behind him thirty-six volumes[2] of the *Histoire Naturelle*, enriched by a profusion of plates, as well as by the notes of skilful collaborators. The work formed a vast, though most incomplete encyclopædia of zoology, at once fuller and more entertaining than any which the world had ever seen. At Buffon's death the mammals, birds, and minerals had been dealt with, the reptiles had been begun, and preparations had been made for the volumes on fishes. The original plan extended to the whole

[1] Erasmus Darwin was among those who drew ideas from the *Histoire Naturelle*, whether to the gain of science or not may be a question.

[2] Not counting the two volumes on Reptiles, by Lacépède, the second of which appeared just after Buffon's death.

realm of nature—animals, plants, minerals, and the theory of the earth; fifty years of unparalleled industry had been unable to cover such a programme. Buffon's death and the Revolution which immediately followed were heavy blows to the undertaking, and though the publication was resumed in quieter times, the *Histoire Naturelle* became antiquated long before the end was in sight.

Cuvier[1] has said that the history of quadrupeds is the most complete, the history of birds the most agreeable, the history of minerals the most defective. In the twentieth century the student of scientific history finds it worth while to read all that Buffon has to say about familiar quadrupeds, the dissertations, and also the *Epoques de la Nature*, which exhibits him as a founder of Geology.

Buffon is intellectually the man of his century, the century of Montesquieu and Gibbon. He is enlightened and rational, free from the bonds of theology and traditional philosophy and verbal learning. It was his delight to account for everything. Madame Neckar says that he was ready to account for every word in his own writings down to the smallest particle. Facts he values as the source of ideas ("rassemblons des faits pour nous donner les idées"[2]), but he perceived very inadequately how minute and toilsome must be the collection of the facts if false ideas are to be avoided.

Fontenelle, Réaumur and Buffon rank, as popularisers of science, among the civilising influences of the eighteenth century; Voltaire may perhaps be allowed a place among them, not for the merit and originality of his scientific work, but for its effect; Cuvier and Humboldt continue the tradition. Buffon has of course lost much

[1] *Biographie Universelle.* [2] *Nat. Hist.*, Vol. II, p. 18.

of his former vogue, by reason of the great bulk of his *Natural History* and the lowered credit both of his facts and his speculations. But, like Réaumur and Cuvier, he still finds readers, and few scientific writers of his age are so well known. His maxim:—"les ouvrages bien écrits seront les seuls qui passeront à la postérité" does not by any means always hold good of scientific works, but even among these "les ouvrages bien écrits" have one chance more of escaping oblivion.

1789 AND LATER

From 1789 we look forward to an age comparatively familiar to the modern naturalist. Cuvier, a Wurtemberger of twenty, who had studied in the Caroline Academy at Stuttgard, was in 1789 acquainting himself with the marine zoology of Normandy; in 1795 he was to be summoned to Paris, there to enter upon the palæontological studies which are now reckoned his most valuable contributions to science. Humboldt, born in the same year with Cuvier, was to sail for South America in 1799. Robert Brown, the great founder of nineteenth century botany, was born in 1773; Baer in 1792; Darwin in 1809; Pasteur in 1822.

The nineteenth century has surpassed all predecessors in the extent and importance of its scientific achievement. Which of the branches of science thus enlarged and renovated has done most for the welfare of mankind is a question on which opinion is divided. Some of us attach the highest importance to those sciences whose industrial applications are most evident; others think that mathematics is yet more important, because more fundamental. Biologists have something to urge in support of their own conviction that no science concerns mankind so deeply as that, which since 1789 and especially since 1859 has thrown a flood of new light upon the infinitely varied forms and activities of Life.

There is perhaps no more hopeful augury of the future of Biology than the increased and ever-increasing sobriety of biological speculation. Bold hypotheses are no doubt framed as profusely as ever, but the speculator is made to feel that he must not set them forth in print until he can support them adequately.

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