University of Leeds

Department of Philosophy

History and Philosophy of Science Division

The Biological Work of Martin Lister (1639-1712)

By J. Carr

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ABSTRACT.

This thesis discusses the life and work of Martin Lister, physician and naturalist, who is best known today for his conchological writings and for his wide circle of aquaintances.

Chapter I gives an account of Lister's family background and connections, which had important effects on his career. Most of Lister's field work was carried out in the years shortly before and shortly after he entered medical practice at York, when he was strongly influenced by John Ray. This period is discussed separately from that at London, when, though publishing several books, Lister became more an indoor naturalist and correspondent. In the final ten years of his life, he was out of touch with his subject and out of sympathy with the general trend of science.

Chapter II describes what was the most original and thorough piece of work Lister ever carried out - his account of English spiders, based on research done in his late twenties and early thirties. This work shows Lister's enthusiasm and diligence at its best; unfortunately it was so different in its approach from anything else at this time that it had little effect on the zoology of the period. This isolation has lead to the almost complete neglect of the work, even though its approach is in many ways similar to that of the early twentieth century.

Associated with this interest in spiders, Lister carried out a number of investigations on insects, all connected with the problems left by the general rejection of the idea of spontaneous generation. This work, on parasites and metamorphosis, together with his later writings on reproduction in higher animals, showed Lister to have been interested in the general problem of generation. As shown in Chapter III, he adopted an ovist outlook.

Lister's reputation has rested mainly on his writings on molluscs, perhaps because of the widespread popularity of shell-collecting for two centuries after his death. This work is described in Chapter IV. Again, Lister's comparative anatomy has been generally neglected in comparison

with the authority conceeded to him as a conchologist. Any loss to science in this case, however, is trifling; though competent by the standards of the time, Lister was no Swammerdam or Malpighi.

A conchologist in the late seventeenth century in England was bound to be drawn into the conflict on the nature of fossils. As described in Chapter V, this controversy has been widely misunderstood by later writers. Because of the lack of a widely-held idea of evolution, and with the short geological time-scale of the period, the differences between fossil and extant forms made it difficult for experienced shell-collectors such as Lister to accept shell-stones as the remains of animals. The contrary view was very widely held in England at the time, but in general not by the men with greatest first-hand knowledge of the subject.

As a physician, Lister was interested in general physiological problems, and left published works on this subject which are comparable in bulk with that of all his other biological writings. This work, discussed in Chapter VI, dates mainly from the later part of his life, and shows Lister in an unfavourable light. His early diligence and concern for first-hand information was replaced by speculation, reliance upon written authority - classical wherever possible - and suspicion of the then modern trends toward mechanical and chemical explanations in physiology. In this field, it is difficult to detect any real influence which Lister may have had on later authors.

Particularly in the early years in Lincolnshire, Craven and York, Lister had very wide interests, covering the whole of natural history. Chapter VII outlines the more important of these minor activities; his work in geology, meteorology and antiquarianism is not discussed in this thesis.

The conclusion discusses the nature of Lister's attitudes and the relationship of his work to seventeenth century biology in general.

Contents

III Generation

Acknowledgements.

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The work involved in this thesis has involved research in the following libraries, to whose staff the author is indebted for assistance: the Brotherton Library, University of Leeds; the Bodleian Library, Oxford; the library of the British Museum; the library of the Royal Society; the library of the Royal College of Physicians; the Lincolnshire County Archives Office, Lincoln Castle.

Some help in the identification of Lister's fossil illustrations was given by Mr. P. J. Hopwood, Huddersfield New College. Otherwise, identifications of animal species are by the author, as are all translations from Latin.

Abbreviations and other conventions.

The following abbreviations are used in footnotes for Lister's main works:

HAA: *Historiae animalium Angliae tres tractatus*, London, 1678.

Goed.: *Johannes Goedartius of insects. Done into English and methodized. With the addition of notes*, York, 1682.

FMA: *De fontibus medicatis Angliae, exercitatio nova et prior*, York, 1682.

LMD: *Letters and divers other Mixt Discourses in Natural Philosophy*, York, 1683.

HC: *Historia sive synopsis methodica conchyliorum*, London, 1685-92.

EA: *Exercitatio anatomica in qua de cochleis maxime terrestribus et limacibus agitur*, London, 1694.

EAA: *Exercitatio anatomica altera de buccinis fluviatilibus et marinis*, London, 1695.

EAT: *Conchyliorum bivalvium utriusque aquae exercitatio anatomica tertia*, London, 1696.

JP: *A journey to Paris in the year 1698*, London, 1699.

Hum.: *Dissertatio de humoribus*, London, 1709.

The following abbreviations are also used:

Birch. T. Birch, *The History of the Royal Society*, 4 vols., London, 1756.

CJR: *The Correspondence of John Ray*, edited by E. R. Lankester, London, 1848.

FCJR: *The Further Correspondence of John Ray*, edited by R. T. Gunther, London, 1928.

Phil. Trans.: *The Philosophical Transactions of the Royal Society*, London, 1666f.

Dates. All dates in the body of the thesis are old style. Wherever possible, dates not in direct quotation are given in the form *1 February 1672-3* to avoid confusion.

References In the body of the thesis, names of authors are given in the appropriate vernacular, and places of publication in English. In the bibliography, names of authors and places are given as on the title page of the work concerned.

References to papers in the *Philosophical Transactions* are given in the thesis only as volume, date and page. Full titles are given in the bibliography.

List of illustrations

Martin Lister, M.D., F.R.S. (1639-1712)

from a photograph of the only known portrait

in Lister Denny, Memorials of an Ancient House

London, 1913.

Chapter I Biography

(1) Family background of the Listers.

Martin Lister came from a long-established Yorkshire family which has possessed land in the Craven area from before the Norman conquest to the present day, and of which the present Lords Ribblesdale are representatives. [1] William Lister, of Thornton and Midhope, who died in 1582, married twice. From his first wife, Anne of Midhope, was descended the main line of the family; from his second, Bridget Pigot, the branch leading to his great-grandson Martin. Bridget Pigot was the widow of Thomas Banister of Brockden, a near neighbour of William, and was originally from Aston Rowan, Oxfordshire. [2] Three of the sons of this second marriage are relevant here: Michael, Martin (here referred to as Martin senior) and Matthew (later Sir Matthew).

William Lister left most of his property to his eldest son by his first marriage, Laurence; this consisted partly of freehold land in Craven, but included many leases, apparently of former church lands and including one for all the coal mines in the parish of Colne, 'maid by the moste famous princes, Phillipe and Mary, lait king and quene of England'. One of these leases, that of Friar Head, Winterburn, was left to Michael. [3] Of the sons of the second marriage, only Michael left offspring. Matthew

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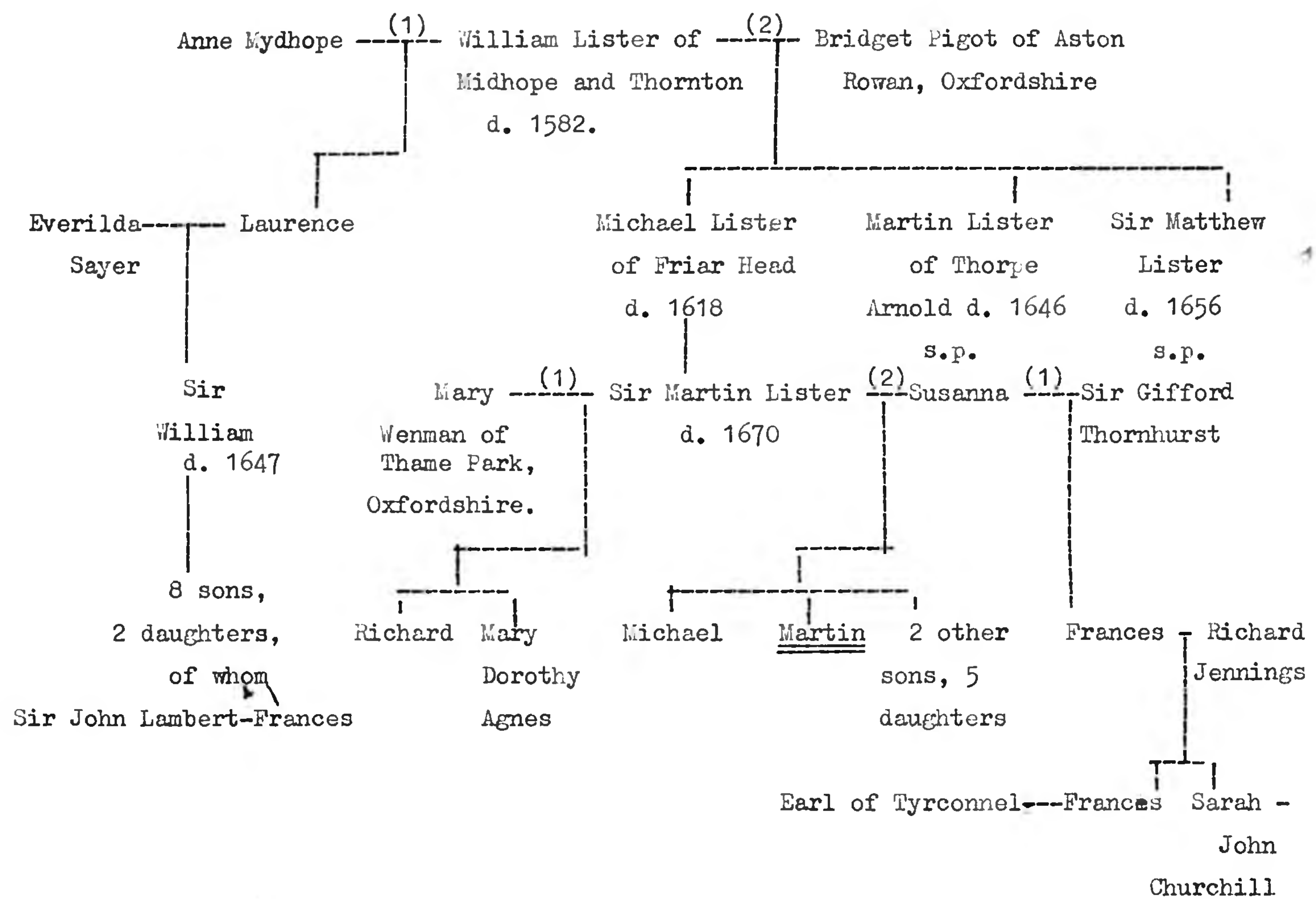
1. J. Foster, Pedigrees of the County Families of England: West Riding, 1 (London, 1874). This is the source for all statements here on family relationships within the family. The family trees in J. Foster, Visitation to Yorkshire made in the years 1563 and 1564, to which is added the subsequent visit made in 1612 (London,1875) and in J. Nichols, History of Leicestershire.2 (London, 1795) are both incomplete, and the biographical accounts of Lister in Dictionary of National Biography and in R. Davies, 'Memoir of Martin Lister', Yorkshire Archaeological Journal 2 (1873) pp. 297 - 320, both contain errors.

2. Foster 1875 p.290.

3. Will of William Lister, 1 September 1582, in Yorkshire Archaeological Record Series: Yorkshire Deeds 3 (Leeds 1922) p. 33.

Descent and relations of Martin Lister.



matriculated at Oriel College, Oxford, on 2 February 1587-8, graduated B.A. 1590-1, M.A. 1595, and M.D. Cambridge (incorporated from Oxford) 1608. He was also M.D. of Basel, Fellow of the College of Physicians 1607, and later censor. He became physician to Anne, queen of James I, and later to Charles I, being knighted 11 October 1636. He lived and practised at Westminster until retiring to Lincolnshire. [1]

Martin senior appears to have been an active farmer, holding a sub-lease of Middle Claydon, Buckingham, from the Gifford family. The holder of the freehold, Edmund Verney, purchased this lease from Martin senior in 1620. [2] It is possible that this removal from Yorkshire was connected in some way with the Oxfordshire origin of Martin's mother. After giving up Middle Claydon, Martin senior is described as being of Goadby Marwood, Leicestershire; [3] this must also have been on lease, as Goadby Marwood was at this period in the possession of the Duke of Buckingham. [4] On 2 February 1626-7, Martin senior and his brother, the future Sir Matthew, bought the manor of Thorpe Arnold, near Melton Mowbray, Leicestershire. The brothers took equal shares in the property, but Martin took up residence while Matthew remained in Westminster. [5]

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1. J. A. Venn, <u>Alumni Cantabrigiensis 3</u> (Cambridge, 1924) p. 90.
2. <u>Victoria County History of Buckinghamshire 4</u> (London, 1927) p. 33.
3. A. E. Goulding, 'History of the Lords of the Manor of Burwell' <u>Architectural and Archaeological Societies' Reports 24</u> (1897) p. 63.
4. Nichols p. 195.
5. Goulding 1897 p. 63; Nichols p. 377.

On 28 October 1641, the two brothers bought the estate of Burwell, near Louth in Lincolnshire, though neither took up residence. [1] On 5 July 1644, Martin senior, being himself without children, settled his third of the Burwell estate on his three great-nieces, Mary, Dorothy and Agnes, the daughters of his nephew Sir Martin, who was the only son of Michael Lister of Friar Head. [2] A week later, he settled on Sir Martin's three younger sons, Michael, Martin and William, the estate of Winterburn, Yorkshire. [3] This would probably include Friar Head and have been received by Martin senior from Michael. [4] The Martin benefiting from this settlement was the future Dr. Martin Lister.

Martin senior died at Thorpe Arnold on 9 September, 1646, leaving his half share in the Thorpe Arnold estate to his nephew Sir Martin. [5] After Sir Matthew's death in December 1656, Sir Martin inherited the other half of Thorpe Arnold and two-thirds of Burwell. [6] He soon bought the other third from his daughters. [7]

This Sir Martin was the only son of Michael of Friar Head, elder brother of Martin senior and Sir Matthew. He matriculated at

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1. The estate was bought from Sir Thomas Glemham for £15,000. This included the manors of Burwell and Calceby, with twenty vills and over 5,000 acres in Eastern Lindsey. Two-thirds of the purchase price was met by Sir Matthew and one-third by Martin senior, and on 28 May 1642 it was agreed that the revenues should be divided in this proportion. On 10 May 1643, the manor house and messuage of Burwell was leased for 21 years to Samuel Hill of Saltfleetby. See abbreviate of Lister of Burwell papers in Massingham Munby collection at Lincolnshire Archives Office; Goulding 1897 p. 80; settlement dated 28 May xvii Charles I (i.e. 1641) between Martin Lister and Sir Matthew Lister, in Lister of Burwell papers.

2. Settlement dated 5 July 1644 between Martin Lister and Robert Gilbert and Will. Gumble, in Lister of Burwell papers.

3. Goulding 1897 p. 81.

4. Martin was guardian of Michael's son from Michael's death in 1618 to 1624. R. H. Lister-Kenny, Memorials of an Ancient House (London, 1913) p. 205.

5. Goulding 1897 p. 82.
6. Ibid. p. 84.
7. [over]

Trinity College, Oxford, on 15 October 1619, and was knighted on 9 July 1625. [1] His means of livlihood at this time are not clear; he had strong connections with the Oxfordshire-Buckinghamshire region. He married Mary Wenman, of Thame Park, Oxfordshire, probably at about the time of his being knighted; their first son, Richard, was born at Ambrosden, Oxfordshire, on 17 July 1628. [2] In 1630, Sir Martin was one of the patrons of the living of Stoke Poges, Buckinghamshire; [3] his sons Matthew and Martin were baptised at Radclive, Buckinghamshire, in 1637 and 1639 respectively. [4] Sir Martin became Member of Parliament for Brackley, Northamptonshire, in the Long Parliament of 1641; [5] Brackley is only just over the Buckinghamshire border. He did not, however, lose all connection with Yorkshire; in 1636, he received fron Charles I the manor of Waddington in Craven. [6] This would be at about the time of his second marriage, to Susanna, widow of Sir Gifford Thornhurst. As Lady Thornhurst, she had been maid of honour to Anne, queen of James I. [7]

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7. He paid each of them £1,000, the money being raised by selling half the Thorpe Arnold estate to the father of his son-in-law; agreement dated 7 January 1656-7 between Sir Martin Lister, his daughters Mary and Dorothy, his son-in-law John Morris, and Sir Thomas Hartopp, im Lister of Burwell papers.

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1. J. Foster, Alumni Oxonienses 4 (Oxford, 1891) p. 918.

2. Lister-Denny p.206.

3. Diocese of Lincoln presentation deed, 1630 no. 37, Lincolnshire Archives Office.

4. Parish register of Radclive, Buckinghamshire Archives Office, Aylesbury.

5. D. Brunton and D. H. Pennington, The Members of the Long Parliament (London, 1954).

6. Lincolnshire Archivist's Report 4 (Lincoln, 1952) p. 26.

7. Nichols p. 377

As Sir Matthew Lister was physician to Anne, this may have been the contact between Lady Thornhurst and Sir Martin. In 1642, Sir Martin was lessee of Deloraine Court in the town of Lincoln. [1] When he inherited half of the Thorpe Arnold estate from his uncle, Martin senior, in 1646, Sir Martin took up residence there, and appears to have become an active farmer, running his own beasts on the land. [2] The family remained at Thorpe Arnold until 1656; the future Dr. Martin entered St. John's College, Cambridge, in that year, from Melton Mowbray school. [3] On the death of Sir Matthew in the same year, Sir Martin moved to Burwell; presumably the 21 year lease of the manor house sold to Samuel Hill had already been bought back, as Sir Matthew had been living there for two or three years before his death.

Sir Martin died in 1670. His will, dated 1669, [4] leaves his property in Leicestershire to his eldest son Richard, some of his Lincolnshire estates to his younger daughters Jane and Barbara, and the rest of the Lincolnshire estates to his second son Michael. The older daughters and the younger sons, including Martin, had already been provided for by Sir Martin's uncle, Martin senior, but it is surprising that there is no mention of these sons in the will, which is long and detailed; even Sir Martin's library, valued at £70, and

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1. Lister-Denny p. 206.

2. Inventory for probate of estate of Sir Martin Lister, dated 31 August, 1670, in Lister of Burwell papers. Mention is made of animals and agricultural produce belonging to Sir Martin which were at the time in Leicestershire, in addition to those in Lincolnshire.

3. J. E. B. Mayor, Admissions to the College of St. John the Evangelist in the University of Cambridge (Cambridge, 1882) p. 122.

4. Will of Sir Martin Lister, XXI of Charles II (i.e. 1669) in Lister of Burwell papers.

which might be thought to have been of interest to a Cambridge Fellow, was ordered to be sold. Among Sir Martin's assets was a sum of £100, representing rent outstanding 'from Yorkshire';[1] as the will makes no mention of the disposal of the property from which this rent was raised, it may be that it was the subject of a separate settlement involving the younger sons.

Dr. Martin Lister's half-sister was the mother of Frances and Sarah Jennings, both of whom were very prominent in the life of the Royal court in the later part of the seventeenth century. Frances, who married the Earl of Tyrconnel, James II's representative in Ireland, does not appear to have any close contact with Dr. Martin, but Sarah, wife of John Churchill, Duke of Marlborough, was on close and affectionate terms with him.[1a] The influence she had in her own right with Queen Anne may have been of importance to Lister.

(2) Political position of the Listers.

The Lister family, at least from William Lister onwards, appear to have expressed no unorthodox religious views. William asked to be buried according to the rites of the Church of England, and religion does not obtrude in family affairs.

Most of the family appear to have taken the Parliamentary side in the Civil War. The head of the main line of the family, Sir William Lister of Midhope, was one of the signatories of the Rothwell petition, in which Yorkshire gentry pledged themselves to

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1. See inventory for probate of estate of Sir Martin Lister. This mentions eight steers of Sir Martin's at Thorpe in the Wharfe - presumably Thorpe near Grassington in Craven. The rent is unlikely to have been from the manor of Waddington, as this was in 1671 in the possession of fees representing the towns-people; see Lincolnshire Archivist's Report 4 (Lincoln, 1952) p. 26.

1a. See letters from Sarah Churchill to Dr. Martin Lister in MSS Lister 4 ff. 5-9.

remain neutral; [1] but he was later an active Parliamentarian, as M.P., first for East Retford and later for the West Riding and City of York. [2] His eldest son, Colonel William, was killed at Marston Moor, and his manor house, which had been used as a Parliamentary garrison, was burned by Prince Rupert, presumably during the Prince's march from Lancashire to Marston Moor. On 9 May 1646, the Committee for Compounding moved that, as Sir William had suffered so much for the Parliamentary cause and had been driven from his estates, he should be granted a pension of £1500, and in December 1646, a further £610 was allowed for the maintainance of the widow and children of Colonel Lister, Sir William's son. [3] Sir William was father-in-law to ~~Sir~~ John Lambert, the parliamentary commander.

The Lincolnshire-Leicestershire branch of the family was also against the King. As M.P. for Brackley, Sir Martin Lister supported Parliament though he was secluded in 1647, [4] and was a commissioner for funds for Leicestershire, of militia for Lincolnshire and Leicestershire, of troops for West Yorkshire, and of assessments for Lincolnshire, over dates ranging from 1643 to 1659. [5] His son, Richard, the brother of the future Dr. Martin, was Colonel of the train bands of Leicestershire during the war,

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1. Lister-Denny p. 205.
2. Brunten and Pennington p. 214.
3. Calendar of the Committee for Compounding, 1643-60, edited M. E. Green (London, 1890) p. 38.
4. Brunten and Pennington, loc. cit.
5. Lister-Denny, p. 206.

and Parliamentary commissioner for militia in the county for 1659/60. [1] At Burwell, Martin senior was making new enclosures from monastic lands in 1647. [2]

The only Royalist in the family was Sir Matthew, who continued to attend on the Royal family throughout the war. He attended Queen Henrietta Maria at the birth of Princess Henrietta at Exeter in June 1644 and was at Oxford with the King. He was fined £200 by Parliament, and his will speaks of 'that poore remnant of plate which is left to me since these troubles'; he appears to have made over his Burwell estates, at least temporarily, to his nephew Sir Martin, to avoid forfeiture.[3] It is likely that the future Charles II would have had personal contact with Sir Matthew during the war, and the position of the future Dr. Martin Lister at the Restoration appears to have been influenced more by this than by the record of his father and brother. The Burwell Listers did not apparently benefit financially from the Commonwealth; the family papers show several mortgages on parts of the estate, and the outright sale of the manor of Calceby, in the later 1650's. They appear to have become high Tories after the Restoration; early in 1690, depositions were taken at Grimsby against Matthew Lister (Dr. Martin's brother and Lord of the manor of Burwell) and Lord Lexington for swearing at King William and drinking to King James. [4]

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1. Ibid. loc. cit.; Nichols p. 376
2. <u>Lincolnshire Archivist's Report 4</u> (Lincoln, 1952) p. 26.
3. Lister-Denny pp. 202-3.
4. Ibid. p. 225, quoting Luttrell, <u>Historical Relation of State Affairs 1678-1714</u>.

(3) Early life, up to the period at York.

Martin Lister was christened at Radclive, Buckinghamshire, on 11 April 1639, and so presumably born there. [1] Although his father had connections with this area, the family's status in the district is not clear; Sir Martin was not the owner of the manor, which at the time belonged to New College, Oxford. [2] He must, however, have lived for some time in the village, as Martin's older brother Matthew was christened there on 17 July 1637. [3] The family appears to have moved to Thorpe Arnold, near Melton Mowbray in Leicestershire, when Sir Martin inherited a half share of the manor from his uncle Martin in 1646. [4] Martin junior attended the Melton School, under Mr. Barwick, until his entry into St. John's College, Cambridge as a pensioner on 12 June 1655; his tutor was Mr. Paman. [5] He matriculated in 1656 and graduated Bachelor of Arts in 1658. [6] There is no indication in any of Lister's writings or correspondence that he had any interest in natural history during his undergraduate period. Natural science at Cambridge under the Commonwealth was on an informal level. Though there were several individuals with scientific interests, such as John Ray, Francis Willughby, Walter Needham, Henry Power and Isaac Barrow, such interest was a personal matter and not a part of the official curriculum. Lister seems to have had no contact with those natural philosophers who were still at Cambridge during his undergraduate days. Five years later, in France, Ray

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1. Radclive parish register, County Record Office, Aylesbury.
2. G. Lipscomb, History of Buckinghamshire (London, 1847) p. 69.
3. Radclive parish register.
4. Goulding 1897 p. 63.
5. Mayor 1882 p. 540
6. Venn[3] p. 90.

and Lister were to be drawn together by common interest and to become close personal friends; that they could be contemporaries at Cambridge for seven years apparently without coming into contact is an indication of the unorganized and very minor place occupied by natural philosophy at the University at this period - even though the two were at different colleges and Lister was the junior by ten years.

At the Restoration, Lister's relationship to Sir Matthew Lister gave him a strong claim to royal favour. In a letter dated from Whitehall on 31 August 1660, the King, having 'received sufficient Testimony of the learning, civill behaviour and abilities of Martin Lister, and of his desire to follow his studies' required St. John's College

> 'forthwith uppon the receipt hereof to preelect and preadmit him to the first Fellowes place that is or shall be voyd in your house; or to take some such course by registring these our Lettres and passing an act in his favour that he may undoubtedly elected to the first voyd place ..... notwithstanding any Custome to the contrary with which we doe by these presents dispence'. [1]

This letter was read on 4 September 1660; Lister was admitted as a Fellow two days later, in succession to Mr. Heron. Other men put forward as Fellows by royal mandate at the same time had to wait up to two and a half years. [2]

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1. J. E. B. Mayor, History of the College of St. John the Evangelist 1 (Cambridge, 1869) p. 540.

2. Ibid. p. 298.

Two years later, Lister took the degree of Master of Arts;[1] by this time he must have decided to take up the study of medicine.

Medical education at Cambridge at this time was unprogressive,[2] being still under the Elizabethan statutes of 1570; these had been replaced at Oxford by the Caroline statutes in 1636. The course for the degree of Bachelor of Medicine took six years, the prescribed authors being all classical. A further five years were needed for a Doctorate in Medicine, during which three anatomies were to be carried out. In addition, it was customary, though not compulsory, to take an arts degree before beginning a medical course. In spite of this formidably long course, standards were low; medical textbooks in the library were classified under 'Miscellaneous' and the eminent Regius professor of Physics, Francis Glisson, is said to have done little or no teaching on his infrequent visits to the University. The low standard of teaching and the length of the courses in England encouraged many to go abroad to qualify in medicine; few Cambridge graduates attempted to practise in London. To do so, a physician had to be a Fellow or Candidate of the College of Physicians, or to be licensed by the College. For this, a doctorate was required, and candidates were also examined by the College. In the early seventeenth century, the doctorate had to be from Oxford or Cambridge, and those who had studied on the continent were required to incorporate at an English University. From 1647 onwards, continental degrees were accepted,[3] but most candidates continued to incorporate in England, this being a formality.[4]

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1. Venn, loc. cit.

2. P. Allen, 'Medical Education in Seventeenth-Century England', Journal of the History of Medicine 1 (1946) p. 115.

3. Sir G. Clark, History of the Royal College of Physicians 1 (Oxford, 1964) p. 280.

4. Allen loc. cit.

Like many of his fellow countrymen, Lister went to study at the Medical school at Montpellier; his father was in France during Martin's undergraduate days. [1] Some fragmentary notes of his journey survive in a pocket almanac left in his papers. [2] He 'left the house' (presumably Burwell) on 11 August 1663, to join the Yarmouth brig 'Matthew' bound for Bordeaux; this would probably be from Boston. The boat was at London on 20 August, and Weymouth on 1 September, where she was stormbound for nearly three weeks; Lister used the time to make observations on Chesil beach. Attempts were made to leave on 5 and 8 September and finally, 'at great hazard to our lives', the boat left for St. Malo on the nineteenth. The weather forced her into Guernsey two days later, and the island was not left until 4 October; St. Malo was reached on the sixteenth, and Lister stayed in the town with Mr. Garet. A fortnight later, on 30 October, the boat docked at Bordeaux, where Lister remained until 6 January 1663-4. He then left for Montpellier by the most direct route, making halts at Cadillac, Lusinard, Finiac, Toulouse, Villefranche, Carcassone and Narbonne, reaching his destination on 16 January. Two days later, he found accomodation at the house of M. Sargeons, 'Maister Apothecaire'.

At the end of March, he began to travel again, and spent four months visiting Nimes, Arles, Avignon and Aix en Provence. At Arles, he bought, together with medical works by Willis, Glisson and Dioscorides, Rondelet's de Piscibus - this being the first reference to any interest by Lister in Natural history.

The pocket-book notes cease at this point, and there remain

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1. MSS Lister 4 f. 31.
2. MSS Lister 19.

only scattered references to the remaining two years of his stay. In December 1664, he visited Sir Thomas Cru [Crew] at his house at Montpellier,[1] and a year later Lister was at the house of Lord Aylesbury, where he met the Danish anatomist Steno, attending one of his anatomy lectures and a dissection and also meeting him informally.[2] In late July 1665, Lister made what was to be his most important contact in the field of natural history when John Ray visited Montpellier and met Lister, together with William Croone, Peter Vivian, Francis Jessop and Samuel Howlett.[3] Ray encouraged Lister's interest in natural history, and the correspondence begun on their return from France contains frequent references to their shared interest in plants, molluscs, insects and birds while at Montpellier.[4] Lister's own papers refer to his presence at a debate between Ray and a Mr. Havers on the site of the virtues of plants - whether it is in their oils, salts or spirits.[5] The two men appear to have lodged together,[6] and they were to become very close friends; after Ray became unemployed at the death of Willughby in 1672, Lister offered to take Ray into his own house.[7] Ray thought highly of Lister:

> 'I judge you to be a person, to speak modestly, as well qualified as any I know in England for such an undertaking [the pursuit of natural history] and so likely to make the greatest advance and improvement, you having taken the right course and method; that is, to see with your own eyes, not

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1. MSS Lister 5 ff. 217-19.
2. MSS Lister 5 f. 225.
3. C.E. Raven, John Ray, Naturalist (Cambridge, 1942) p. 137.
4. CJR pp. 25, 111, 107, 61; FCJR pp. 118, 133, 126.
5. MSS Lister 5 f.226. Clopton Havers?
6. CJR p. 17.
7. W. Derham, Memorials of John Ray (London, 1844) p.29

relying lazily on the dictates of any master but yourself ....'[1]

'Your friendship and affection I do deservedly value at a very high rate, so that I should be loath that through any negligence or ommission of mine it should cool or decay, and shall therefore be careful always with my best endevours by all offices and services on my part to cherish and increase it'.[2]

The two were to remain very close friends for about ten years, and to be on good terms until Ray's death.

Early in 1666, Ray again met Lister at Lyons, in the company of Skippon, Vivian, and Jessop. The men were all on their way back to England following the French intervention in the first Anglo-Dutch war. The party left for Paris on 6 March, and Lister and Skippon reached England on 8 April.[3] Lister had been abroad for nearly three years. In this time, he learned enough French for Ray to seek his advice on translation,[4] though later in life Lister admitted that he could not follow the dialogue in a French play.[5]

Lister returned to Cambridge and took up the study of natural history with enthusiasm; his first letter to Henry Oldenburg was written at this time,[6] and Lister began his extensive correspondence with Ray. This was at first very formal, being in Latin until the end of 1669. On 18 June 1666, Ray visited Lister at Cambridge, and, together with Peter Dent, they left on a botanical

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1. CJR p. 13.
2. Ibid. p. 43.
3. Raven p. 138.
4. CJR p. 123.
5. JP p. 171.
6. Birch 4 p. 316.

tour of Cambridgeshire and Lincolnshire, including a period at Burwell. This journey, taken during the period of the plague at Cambridge, lasted into September, and Lister's notes on it survive. [1]

The next eighteen months were spent at Cambridge, and his letters show Lister to have been actively studying minerals, plants, insects, spiders, molluscs, fishes and birds. There are no letters for July and August of 1667; he may have spent the summer in London. A letter from his Half-sister, Francis Jennings (or Jenyns, mother of the future Duchess of Marlborough), written on 30 May 1667, suggests a meeting in the capital in two months' time. [2] He had some connections at Court as early as this period: a letter from his sister Jane, describing her visit to London, mentioned that 'as for the Court, I must ask you concerning it'. [3]

It seems that as early as 1666 Lister began to consider entering into medical practice. This would raise difficulties at St. John's; his Fellowship was not in Physic, and in fact he may already have been under some pressure because of his not having taken the holy orders required for ordinary fellows. To resign his safe fellowship and rely entirely on professional fees would have been to take a financial risk. Lister tried to obtain a dispensation to avoid taking holy orders, [4] and at the same time persuaded his father to make some kind of financial arrangement for him.

His letters show him to have been at Burwell for three months

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1. MSS Lister 39 f. 421.
2. MSS Lister 4 f. 48.
3. MSS Lister 4 f. 60.
4. MSS Lister 3 f. 30.

'Mentha aquatica sive Sisymbrium aquatica rupra
Park hic et alibi in aquis frequens'
(MSS Lister 39 f. 370)

Mentha aquatica L. in Barwell Brook at Park Farm.

in the spring of the year, and Thomas Briggs wrote to him from St. John's on 28 April saying that

> 'I should rejoice to understand anything from Sr. Martin to your better advantage, but, till assured of that, quit not a certainty if it may be retayned .... if you procure a dispensat lett it be onely till such time as a Phisique place falls voyd, whereby the Coll. will apprhend less infringement of the statutes ....'. [1]

In fact Lister did obtain a royal dispensation dated 19 June 1668, requiring the College to continue him in his fellowship until a Fellowship in Physic became available. [2] This would overcome the problem of holy orders, but by this time Lister had reached an agreement with his father; Briggs wrote on 20 May to welcome a letter of Lister's 'since it carried the good news of Sr. Martin's consenting to your requests; though wee in College shall be loosers thereby, yet the gain will be the better since youle bee a saver ..'. [3] There is no definite record of what these arrangements were; but there is a mention of rent paid to Lister from property in Lincolnshire, [4] and a letter from his sister Anne of 17 August 1668, mentions a settlement which reacts unfavourably on her children. [5] It seems that Sir Martin must have settled land on his son, and that this was the reason for the ommission of any bequests to Lister in his father's will, made in 1669. During this period, he continued to work on his classification of spiders, and Ray wrote that he could not 'admire enough that you have given so much pain and spare time, when with

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1. MSS Lister 3 f. 20.
2. R. T. Gunther, Early Science in Oxford 12 (Oxford, 1939) p. 316.
3. MSS Lister 3 f.21.
4. MSS Lister 4 f. 31
5. MSS Lister 4 f. 39.

such disturbances of the mind that it would have been just, if all study were held up'. [1]

Lister made use of his dispensation to continue at Cambridge for another year, during which he became involved in a controversy with Dr. Hulse over priority in the discovery of the cause of gossamer. In the summer of 1669, however, he resigned his fellowship. There is a break in the Ray-Lister correspondence at this time of five months - much longer than any other gap. Lister probably spent the summer months in Craven. There is no record of his having visited Yorkshire before this, but he had relatives at Midhope, and owned property at Winterburn which had been left to him by his great-uncle Martin. It must have been on this visit that he met Hannah, daughter of Thomas Parkinson of Carleton Hall, about five miles from Winterburn. Their marriage is unlikely to have been arranged before this summer, as Lister's sister Jane wrote to him on the last day of August, complaining that

> 'I believe it is a month sence we heard of you, and you promised to writ to us every weeke. I doe not know what to think of it; shure you are bringing of us a nu sister - if it be that which taks up all your thoughts I am satisfied, but nothing else can excuse you'. [2.]

The couple were married in October in Westminster Abbey - [3] a typically ostentatious gesture, as they set up house, not in London, but at Nottingham. It is probable that Lister attempted,

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1. CJR p. 29.

2. MSS Lister 4 f. 54.

3. Chester, Westminster Abbey Registers (Harleian Society) p. 221, as quoted in Goulding, A. E., 'Martin Lister, Naturalist', Architectural and Archaelogical Societies' Reports 25 (1899) p. 339.

unsuccessfully, to start a medical practice in the town; he had earlier been in correspondence with a Mr. Bayulay of Chichester, with a view to taking up a practice there. [1] He continued to work at his spider list, and began to carry out experiments of the movement of sap in trees; there also survive notes on the birds he observed that Winter on the Trent. [2] Ray asked him to revise the '<u>English Catalogue</u>' of plants, apparently holding a high opinion of Lister's botanical competence. [3] Willughby and Lister met at Nottingham, [4] and discussed birds and insects, but no close friendship was formed. No correspondence was established, and later Ray acted as an intermediary between the two.

In March, 1670, Lister left Nottingham [5] for his wife's home at Carleton. He remained in Craven for the next seven months, with a short visit to London in April. In this period, and on later visits, Lister became very familiar with the district, its geology and natural history; the names of the places in the Skipton - Malham - Gisburn area occur frequently in his published works on spiders, molluscs and fossils. He described, in a letter to Ray, his discovery of <u>Valeriana graeca</u> (now <u>Polemonium caeruleum</u> L., or Jacob's ladder) at 'Malham Coze, a place so remarkable that it is one of the wonders of Craven. It grows there on both sides of the spring in great tufts'. [6] This discovery has sometimes been attributed to Ray, who saw the plant at Malham the next year, and described its situation in Lister's own words.

By October, Lister had moved to York, where he established a successful practice which was to keep him at York for the next thirteen years.

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1. MSS <u>Lister 3</u> f. 10.
2. MSS <u>Lister 5</u> f. 113.
3. CJR p. 43.
4. Ibid. p. 48.
5. Ibid. p. 55.
6. Ibid. p. 57.

(4) Period at York.

It has been suggested that Lister moved to York because of his connections with the influential Fairfax family of that town, who were related to the Listers of Craven by marriage. [1] However, it has already been shown that Lister had contemplated or attempted medical practice in Chichester and Nottingham, so that York was at best his third choice; and though he did correspond with Brian Fairfax, this was not until the late nineties.

York was at this time the third town in England by population, and, as the northern capital, had a social life second only to that of London. [2] It therefore had attractions to an ambitious young physician. Lister moved, by at latest October 1670, to a house outside Micklegate Bar. This was then sufficiently suburban for him to have a garden which he cultivated personally. [3] In 1672 he moved into a larger house in Lendal Street, Stonegate. [4] Most or all of Lister's children were born at York: his sons Martin, Michael (who died in 1676) and Alexander, and his daughters Susannah and Frances. Two younger daughters, Dorothy and Barbara, may have been born at York or at London near the time at which Lister moved to the South. [5]

Lister quickly became part of the medical establishment at York. The physicians of the town had associated themselves into a body with some formal structure, appointing a proctor and holding monthly dinners with the York apothecaries. An agreement was drawn

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1. Davies p. 300.

2. D. Ogg, England in the Reign of Charles II 2nd. edn. (Oxford, 1956) p. 49.

3. Phil. Trans. 7 (1672) p. 4064.

4. MSS Lister 34 f. 29.

5. The parish registers for St. Helen's, Stonegate, are missing for this period.

up regulating the practice of medicine intra hanc provinciam, the signatories agreeing to exclude non-qualified practitioners; to consult one another on professional matters; to keep the pharmacists under control; to ask the Archbishop, whose authority in theology was acknowledged, to recognize their authority in medical matters (including, presumably, the granting of licences); to give the poor the same attention as the rich; and to maintain the integrity of the profession. The document was signed by Stephen Taylor, M.D.; R. Witty, M.D.; Peter Vavason, M.D.; Will. Liscough, M.B.; Martin Lister, M.A.; Glen Corbett, M.D.; and N. Johnston, M.D. Lister, it will be seen, was the least well qualified of these. [1] The group had its own standards, not necessarily the same as those of the London College of Physicians; an extra-licentiate of the College was prosecuted at York by this group for practising without a degree. [2]

The life of an English country physician with a mainly upper-class clientele was at this time quite leisurely and comparatively well rewarded. Lister kept detailed account-books, some of which have survived. [3] These show him to have treated patients at this period at the rate of about one every other day. However, as was common at the time, his practice was spread over a wide area, so that he must have spent a great deal of time in travelling. He appears to have covered a large part of central and eastern Yorkshire, from Teesmouth and Whitby to Doncaster and Pocklington. He also published medical notes from Craven; [4] these would

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1. MSS Sloane 1393 f. 13.

2. Ibid. f. 15.

3. MSS Lister 27, 29, 30, 31, 32, 32^{x}.

4. Phil. Trans. 14 (1684) p. 597.

probably be casual cases seen while visiting the area on business concerning his estates there. For such visits, he kept for his own use the house at Carleton of which his wife had inherited a one-third share fron her father; Lister must at some time have aquired the rest of the property, as he was to leave it all to his son, Captain Martin Lister. As his correspondence at York practically ceased during the summer months, his visits to Craven preumably took place then. His later account books from London show that a physician's business in July was only about a quarter of that in January, so that his travels need have caused little inconvenience to his York practice. In the early eighties, Lister was spending his summers at Bath or in France. It was on these travels that Lister accumulated the practical knowledge and notes which were to provide the basis for his original work in natural history, geology and antiquarianism; he appears to have added little to his experiences during his twenty years in London.

Lister's standard fees were five shillings or half a guinea, with a shilling for attendance on servants; this was enough to provide him with an income, during the middle years of his York practice, of about £180 per year. For comparison, Gregory King's tables of 1688 show an eminent clergyman to have received £72 a year, a lawyer £154, a person in a greater office £240 and a gentleman £280. His medical practice could therefore have given Lister a comfortable though not luxurious life; but he had also an income from his estates in Yorkshire and Lincolnshire. He would, therefore, have been a relatively wealthy man.

After settling at York, Lister began what appears to have been a deliberate attempt to obtain election to the Royal Society. He had already had a letter from him to Ray published in the Philosophical Transactions of 1669 (as from 'an ingenious Cantabrigian'). Three more papers of his were published at the end of 1670, and eleven in 1671. These papers were at first in the form of letters to Ray, who had sent them on to Oldenburg, but Lister began from August 1670 to write to Oldenburg direct. On 18 May 1671, Lister was proposed by Oldenburg as candidate for the Royal Society; this had been suggested by Oldenburg in January, and he was admitted as member on 2 November of that year, the first meeting at which a quorum was present. [1] In these first eighteen months at York, Lister's only surviving correspondence is that with Ray and Oldenburg. These letters and his published papers show his interest at this time to have been concentrated upon spiders, over which he became involved with Dr. Hulse in a dispute over priority in the discovery of the nature of gossamer; problems on the generation of several small animals - cochineal insects, horsehair worms, ichneumons and viviparous flies; and plant juices, their movement and possible economic uses. There is also mention of botany, fishes and fossils. His work on plant juices and their use in dying attracted a good deal of attention at the Royal Society, and apparently some resentment at his secrecy; Prince Rupert asked for samples of his preparations. [2]

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1. Birch 4, pp. 481, 485; MSS Lister 34 f. 14 (according to which Lister had been proposed candidate by 7 May); MSS Lister 34 f. 24 (which gives 2 November as the date of his election.

2. MSS Lister 34, ff. 83, 85, 86; MSS Lister 36 ff. 5 - 6

From his election to the Royal Society until 1675, Lister continued to work at the same general problems, though his interest in spiders declined and that in fossils and molluscs increased; and he extended his circle of acquaintances and correspondents. As the chief social centre of the North, York provided a certain amount of intellectual society; among those with whom Lister is known to have had close personal contact are Francis Place and William Lodge, artists both of whom illustrated papers by Lister; the glass-painter Henry Giles; John Brooke, F.R.S., son of the Lord Mayor of York, and M.P. for Boroughbridge; ~~Henry Giles~~; and Thomas Kirke of Cookridge, Leeds.[1] Lister's scientific acquaintances were mostly from further afield: Dr. Nathaniel Johnstone, of Pontefract, introduced to Lister by Brooke, and who helped him with microscopical observations; Francis Jessop of Sheffield, who had been at Montpellier with Lister, and whose interests were mainly geological; the Reverend George Plaxton, vicar of Barwick in Elmet, antiquarian and minerologist; John Webster, geologist, of Clitheroe in Lancashire; Francis Bedford of Falmouth, another minerologist; John Brearcliffe, a Halifax antiquary and apothecary; Dr. Thomas Townes, an acquaintance of Lister before 1674, when he left Cleveland for Barbados, from where he sent observations on general natural history; Mr. Thoresby of Leeds, father of Ralph Thoresby, the antiquary; the botanist Nehemiah Grew, with whom a rather acrimonious controversy on plant anatomy was conducted, via Oldenburg, and, still much the most important, John Ray, who continued to provide and receive correspondence on plants, birds, fishes, insects, molluscs and fossils. So close were Lister and Ray

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1. H. M. Hake, 'Francis Place and Henry Giles', Walpole Society 10, n.d.; Birch 4, pp. 94, 136, 138, 213, 255, 369, 279; Phil. Trans. 8 (1673) p. 6181; Goed. preface; MSS Lister 34 f. 87.

that in 1672, on the death of Ray's patron, Willughby, Lister offered to take Ray into his own house. [1] Other correspondents at this time included John Wilkins, Bishop of Chester, on medical matters, and Thomas Briggs and John Peck, both of St. John's College, Cambridge.

During the period up to 1675, Lister continued as a keen member of the Royal Society, sending frequent contributions to Oldenburg, some of which were read at Society meetings, and twelve of which were published in the four years from 1672-5. In 1674, he was asked to send a lecture to be read at the Society, £5 being allowed for the cost of demonstration materials. [2] On 22 October 1673, Lister was named as one of 57 members who could be relied upon to pay their fees regularly. [3]

In addition to his published papers, Lister had by 1675 completed the editing and annotation of his English translation of the papers of the Dutch entomologist Goedart, a Latin translation of which had come into his possession; the work was not, however, published until 1682. [4] Most of the material for the Historia animalium Angliae of 1678 must have been assembled by this time. [5]

In the later 1670's, Lister's interest in science appears to have declined abruptly. The correspondence with Ray was maintained until July 1676; on the second of that month, Lister wrote to Ray with no hint of any disagreement, but this appears to have been his last letter, though Ray wrote five times over the next eighteen months. Many years later, Ray wrote that he believed the break to

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1. Derham p. 29.
2. MSS Lister 34 f. 70.
3. Birch 3 p. 481.
4. Goed. preface.
5. HAA Preface.

have been caused by the controversy over gossamer between Lister and Hulse, which Ray had inadvertently provoked. [1] This is unlikely, as Lister had continued to be on close terms with Ray for several years after this event, and he later defended Ray in ways which do not suggest any resentment over matters of priority. In 1693, he wrote to Edward Lhwyd, keeper of the Ashmolean Museum at Oxford, saying that Ray was 'the most generous man in the world that I know, full of honour and civilitie and integritie .... of Privateering men you cannot be overcautious ... what you have communicated to Mr. Ray is well done'; [2] and he specifically defended Ray's rights of priority against John Woodward, founder of the Woodwardian collection at Cambridge, of whom he wrote that 'I have had noe conversation with him in the least this 12 month for Mr. Wrayes sake, whome he openly vilified'. [3] In 1671, Lister had defended Ray on a matter of priority against Swammerdam; and he presented a copy of his Historia conchyliorum to Ray on its completion in 1692. [4]

In fact Lister's scientific correspondence in general had sharply declined at this time. Oldenburg wrote on 3 June 1676 to ask why he had had no recent letter from Lister, and a week later regretted that Lister had now no time for philosophy. [5] Apart from two letters to Ray in 1676 and one to Oldenburg in July 1676, no letters on scientific subjects from Lister survive from July 1675 to January 1680-1; and the only ones to him are the unanswered five from Ray and two from Oldenburg, and one each from Johnstone, Kirke, and G. Witham. There was also a notification from Grew of

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1. FCJR p. 140.
2. MSS Ashmolean 1816 f. 98.
3. MSS Ashmolean 1816 f. 119.
4. Phil. Trans. 6 (1671) p. 2219; G. Wilkins, 'The Historia conchyliorum of Martin Lister', Journal of the Society for the Bibliography of Natural History 3 (1953) p. 198.
5. MSS Lister 34. f. 83.

Oldenburg's death. In January 1677-8, Lister was included in a list of correspondents of the Royal Society to whom a common letter was to be sent, encouraging their further correspondence.[1] All Lister's surviving correspondence in this period is concerned with medical, family, or personal matters. The long delay between the completion of his edition of Goedart in 1675 and its publication in 1682 confirms Lister's lack of interest in science at this period; and though the Historia animalium Angliae was published in 1678, it was, according to the preface, completed several years earlier, and the book was printed from a manuscript which was clearly very badly prepared for publication.

It is difficult to account for this decline in interest. Lister's medical practice continued normally throughout this period, and apart from the death of his son Michael in August 1676 there appears to have been no crisis in his family or personal life. However, it does reinforce a general impression that Lister's scientific interests, though followed enthusiastically at times, were only a peripheral part of his life and always secondary to his medical career. Lister's interest began to revive about 1681, when he was persuaded by Thomas Kirke to publish his edition of Goedart.[2] Lister raised the matter with Francis Aston, secretary of the Royal Society, who received the proposal favourably, commenting that Goedart was often mentioned in Swammerdam's book - 'I suppose you may have seen it'.[3] In the summer of 1681, Lister and Kirke, together with Francis Place, John Fenton and Mrs. Jane Allington

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1. Birch 3 p. 369.
2. Goed. preface.
3. MSS Lister 35, f.45.

and their servants, visited France, [1] and on their way through London Lister attended a meeting of the Royal Society - apparently the first time he had attended in person. [2] He showed his Goedart manuscript, and asked for the Society's approval. This was given, but no financial help was offered at the time. The cost of publishing the book was mentioned several times in Lister's correspondence with Aston and Grew in this year. [3] On February 8, 1681-2, the Society resolved 'to find out some expedient to have the thing done at their charge', and agreed to print the work if 150 subscribers could be found, 50 of them from the Society. [4] Lister had already had the plates engraved, and was to present them to the Society. However, when the work was finally printed in 1682, it was done privately by Lister at York, though the Society did take 50 copies. [5] The Latin edition of 1685 was published by the Society in London.

Lister's book on mineral waters was mentioned in letters to the Royal Society in 1682, and copies were sent early in the following year; [6] and in 1683 he published his Lectures and Mixed Discourses, a collection of his papers previously published in the Philosophical Transactions. Both of these works were published at York. It is possible that he was already thinking of moving to London, and that this literary activity was deliberately intended to give him a reputation which would be a valuable business asset in

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1. MSS Lister 3, f. 1.
2. Birch 4 p. 94.
3. MSS Lister 35, f. 56.
4. Birch 4 p. 124.
5. Ibid. p. 142.
6. Letterbook of the Royal Society 8 p. 247.

such a move; but his interest in natural history does seem to have been stimulated again. He must have begun his major work, the Historia conchyliorum, at about this time; it was completed in 1691, and Lister later wrote that it had occupied him for ten years. [1] Contributions from him were now once more read frequently at meetings of the Royal Society; he published two papers in the Philosophical Collections for 1682, and five in the revived Philosophical Transactions in 1683. His interests at this time were mainly concerned with digestion and lymphatics, geology and antiquities. His suggestion [2] that the Royal Society take in hand the publication of Willughby's History of Fishes, as edited by Ray, began a series of negotiations with which Lister was to be concerned for the next three years.

He was also at this time connected with the new Ashmolean Museum, then being erected at Oxford. This was to house the collections of natural and antiquarian rarities begun by John Tradescant, gardener to James I, and enlarged by his son and by Elias Ashmole, who received the collection from the widow of the younger Tradescant in 1674. In 1677, Ashmole offered the collection to the University of Oxford, on condition that a suitable building was put up to house it, and work began on this in 1679. Lister offered a large number of specimens to the museum, and there were several letters of thanks in 1682 to him from University officers and from Robert Plot, the first custodian. [3] Particularly valued were two large stone altars, thought to be Roman, which were sent

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1. JP p. 105.

2. Birch 4 p. 127.

3. Letters from John Lloyd, Obediah Walker and Robert Plot in MSS Lister 35, ff. 94, 235, 60, 73.

by Lister from York. They were shipped by barge from London with the main Ashmole collection in 1683 and set at the entrance of the museum. [1] So important were Lister's contributions that at one time his name was set over the entrance together with that of Ashmole, the two being described as the principal benefactors of the institution. [2] Later in life he was to make frequent gifts of books and specimens to the museum.

Lister's circle of correspondents began to increase at this time, and in the years 1680-3 included, besides personal and medical correspondence, Francis Aston, Nehemiah Grew, and Robert Hooke, all officers of the Royal Society; Miles Gale, rector of Keighley and a fossil collector; Robert Plot, keeper of the Ashmolean Museum and author of the Natural History of Oxfordshire; Octavian Pulleyn, a microsopist; Dr. John Place, brother of Francis, and physician to the Grand Duke of Tuscany, a naturalist and acquaintance of Francesco Redi; and Tancred Robinson, naturalist and prolific correspondent of John Ray, though Lister's own close relationship with Ray did not revive. Lister also met at this time the Leeds antiquary Ralph Thoresby [3] and the Bristol fossil collector William Coles. [4]

Lister had by this time established a reputation for himself, as the author of four books and numerous papers. Robert Boyle thought him comparable with Redi and Malpighi, [5] and he was held in high regard by the Royal Society. It seems that he wished to take advantage of this by taking up practice in London, which

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1. MSS Lister 35, f.90.
2. MSS Lister 36, f. cxxiv (from Thomas Tanner).
3. The Diary of Ralph Thoresby, 1, ed. Hunter (Leeds 1823) pp. 297-8.
4. MSS Lister 35, f. 99.
5. MSS Lister 35, f. 89 (from Tancred Robinson).

Friar Head House, near Winterburn, in Craven, Yorkshire.
This was the manor house of Lister's Yorkshire estate, though in fact he appears to have used his wife's house at Carleton on his visits to Craven, rather than Friar Head, which was presumably let.

would offer much greater financial reward. It has already been shown that Lister had court connections in the 1660's, and this, together with his reputation as a natural philosopher, would give him reasonable grounds to expect success. In June and July of 1683, he was at Bath, and visited Westminster on his return journey. [1] In August he was at Carleton, and on his return to York he packed his belongings for shipment to London in the 'Alice Bacon'. Part of his packing list survives, and this includes a list of the numbers, though not the titles, of his books, classified according to subject. [2] This gives an indication of the state of his library; it includes natural history 70, botany 46, natural philosophy 128, physic (English) 31, medicine 180, chemistry 42, mathematics 12, philology and classics 72, divinity 34, French, Spanish and Italian (no number given), law 6, maps, papers, MSS 57.

By September, Lister had taken up residence in Old Palace Yard, Westminster

(5) Period at London.

Lister's reputation allowed him to enter the higher ranks of society immediately. On 23 September, a few days after his arrival, he was at a dinner given by the Archbishop of Canterbury and met Elias Ashmole, comptroller of the Exchequer, Windsor herald and founder of the Ashmolean Museum. [3] The two had been in written contact earlier because of Lister's donations to the museum. Next day, Lister was a guest at dinner at Ashmole's house, together with a number of German noblemen. [4] On 31 October, he attended a

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1. Phil. Trans. 14 (1684) p. 489.

2. MSS Lister 27.

3. Elias Ashmole (1617-1692): his Autobiography and Historical notes, 4. Ed. C.H. Josten (Oxford, 1966) p. 1731.

4. Ibid. p. 1732.

meeting of the Royal Society; he had made an isolated visit in 1681, but this was the first of what was to be a series of regular attendances over the next few years. He attended a further seventeen meetings in the next six months, speaking on barometers and meteorology, minerals, fossils and antiquities. At a meeting on 12 December, he clashed with Robert Hooke on the origin of shellstones; Lister was able to use his superior knowledge of the structure of living and fossil shells to bring out differences which Hooke had not noticed. This line of argument was important in reinforcing Hooke's suggestions on the possibility of the gradual modification of species characteristics in the course of time as a way of avoiding this difficulty. [1] On 30 November, Lister was elected to the council of the Royal Society. [2]

His rapid entry into high social and philosophical circles could not at first be paralleled in his professional life. Lister at this time did not hold an English medical degree, even at bachelor's level, and there is no record of any qualification he may have obtained at Montpellier. He was occasionally addressed at York as Dr. but this was purely a courtesy title.

Medical practice in London was under the jurisdiction of the College of Physicians, whose membership was limited at this time to forty, with a maximum of twelve Candidates at any one time, formally recognized as waiting for a vacant place. Entrance to the College required a Doctorate and a public examination, though there was a class of honorary Fellows for whom such examination was thought to be inappropriate. [3] Lister, without a doctorate, was not eligible

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1. Birch 4 pp. 237-8.
2. Ibid. p. 231.
3. Clark 1 p. 340.

for election either as a Candidate or as an honorary Fellow. He would have been able to apply for a licenciateship of the College, open to suitable men without doctorates, but in fact no such application by him is recorded. Though unqualified to practice in London, Lister nevertheless did take medical fees in his first few months in residence there, though only on a small scale. His receipt books show him to have taken £13 in this way in October, November and December of 1683. [1] During this time, he was probably trying to obtain the qualifications needed for legal medical practice in London.

His first degrees from Cambridge and his former Fellowship at St. John's College did not cause him to look in that direction. His donations to the Ashmolean Museum were highly valued at Oxford, and appear to have earned him the goodwill which would be needed by a man who did not possess a bachelor's degree in medicine in his application for a doctorate. He was granted the degree of Doctor of Medicine on 5 March 1683-4, the citation declaring that

> 'He was lately a practitioner of physic at York, now here in London, a person of exemplary loyalty, and of high esteem amongst the most eminent of his profession for his excellent skill and success therein, and hath given farther proof of his worth and knowledge by several learned books by him published. He hath entertained so great an affection for the University of Oxon, that he hath lately presented the library with divers valuable books both manuscript and printed, and enriched the new musaeum with several altars, coins, and other antiquities, whereof several cannot be matched for any price; which

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1. MSS Lister 27.

yet he declares to be but an earnest of what he farther intends'. [1]

With this qualification, Lister was accepted as a candidate by the College of Physicians on 25 June 1684, and so became officially eligible to practice in London. [2]

His practice, from its earliest days, included the highest ranks of society. He attended levees of Charles II, which must have taken place this year, and the King took Lister into his Whitehall laboratory to show him the method of distilling Goddard's drops from raw silk - a secret bought by Charles from the widow of Dr. Jonathan Goddard. [3] Lister was one of the physicians called to the post-mortem of Charles on 7 February 1684-5. [4]

There is an incompletely dated letter from Sarah Churchill, future Duchess of Marlborough, to Lister which suggests that he attended Charles in an unofficial capacity which he wished to convert to an official appointment at the accession of James II, and tried to do so by characteristic seventeenth century methods:

'Deare uncle,

> Tho' you haue not heard from mee I haue not been negligent in your business, but I dout I can't make an interest enough to get you one of the fore physicians to the King becase I beliue hee will take the same that ware to the last King, tho hee has yet declared but three; what I can doe for your

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1. Antony a Wood, <u>Fasti Oxonienses 2</u> (Oxford, ~~1674~~ 1790) p. 391; quoted in Goulding 1899 p. 342.

2. W. Munk, Roll of the Royal College of Physicians (London, 1878).

3. JP p. 28.

4. MSS Lister 3 f. 2.

seruis you may be sure I will, & as to the mony you bed my coson Hartopp tell mee you would be willing to giue to anybody that mought oppose you, if it is my power to doe it there is noe need of that & all that is disired of you is to giue soomthing to your solicitor mis Hartopp who is your own neece & one that wants it & if I can get you to bee to this King as you ware to the last, I shan't dou't but in time to obtain all that you can desire, being your affectionate neece and humble servant,

Churchill.

March the 2.

For Dr. Lister at his hous in the old palace yard in Westminster.'[1]

This letter is unlikely to refer to the accession of William III, as Lister certainly had some contact with Charles, but there is no evidence of any connection with James; further, the latter is signed 'Churchill', whereas letters from Sarah written after the ennobling of her husband at the revolution of 1689 are signed 'Marlborough'.

Lister's pocket-books for the years 1686-90 show his patients to have included Lord Brounkner, President of the Royal Society; the Earl of Tirconnel, Lord Lieutenant of Ireland and husband of Lister's niece, Frances; Lord Vaughan, Governor of Jamaica; Lister's nieces, Sarah Churchill and her husband, John, Duke of Marlborough; Lords Carbury, Thanet, Bellamont and Ashley (presumably the son of the Earl of Shaftsbury); and Ladies Plymouth, Preston, Landsdown, Freshwell, Lascelles, Cartwright, Coventry, Montague, Fleet and Middleton. From medical fees, Lister earned approximately £350 in 1686, about twice as

1. MSS Lister 4 f. 5-6.

much per annum as in his York practice.[1] There were, of course, professional set-backs; in July, 1684, Richard Boyle, second Earl of Cork and Earl of Burlington, wrote to Lister, reassuring him that the 'death of yt worthy and great person' was not his fault, and that he, Boyle, would vindicate Lister all he could, being sorry for having introduced him into company where 'so little ingenuitye was practis'd towards you'.[2]

In spite of his success, the limit of forty on the number of Fellows of the College of Physicians would prevent Lister from quickly reaching the highest ranks of his profession. After the accession of James II, all corporations with privileges protected by Royal Charter came under pressure from James's wish to bring them under his closer control, and the College of Physicians voluntarily surrendered its charter on 19 October 1685, in the hope of obtaining better terms than by holding out as long as possible. James granted a new charter on 11 March 1686-7, and this increased the number of Fellows to a maximum of 80, 76 of whom were named in the charter. These included Lister, and also his close acquaintances, Hans Sloane and Tancred Robinson, both of whom Lister had proposed as members of the Royal Society; and Nathaniel Johnstone, of Pontefract, recently removed to London. The Fellows listed had been checked for political reliability, and not all the former complement of forty were re-admitted; but as Lister had no difficulty at the revolution of 1689, it may be assumed that he did not make obvious any political views he may have had, and was not admitted to the new membership for political reasons. There was a good deal of controversy

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1. MSS Lister 29, 30, 31.

2. MSS Lister 3, f. 16

about the legality of the 1687 charter after the revolution, but a compromise was reached whereby the new members were to be retained without needing to satisfy the requirements of the older, 1647, charter. [1] Some of the 1687 members did, however, find practice impossible after the Revolution. Johnstone was so affected, being a very High-churh Tory, but Lister managed to keep out of these political conflicts.

In the middle eighties, Lister continued his scientific correspondence, though this was not so extensive as it was to become a few years later. New writers to him included Robert Plot, professor of Chemistry and keeper of the Ashmolean Museum; William Coles, the Bristol fossil collector; R. Fitzgerald, a Cheshire salt-mine owner, concerned with the commercial distillation of salt water; and Sir Richard Bulkley, an Irish landowner of Dunlavan, Co. Wicklow, at this time in Holland with the entourage of William and Mary and the Princess Anne, and who wrote on agriculture and gardening.

In 1685, the Royal Society published a Latin version of Lister's edition of Goedart, but much more important was the beginning of his <u>magnum opus</u>, the <u>Historia conchyliorum</u>. This work was issued in parts from 1685 to 1692, and is bibliographically complex because of the great variation between individual copies; also, Lister circulated odd sets of proof sheets to his friends, and these were sometimes bound up into incomplete copies. Two such incomplete sets, both entitled <u>De cochleis</u>, exist in the libraries of the British Museum and the Linnean Society. As Lister claimed that the book occupied him for at least ten years, it must have been begun at York; he also

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1. Clarke p. 373.

claimed that it cost not less than £2,000 to produce, 'of which sum, yet a great share it stood me in, out of my Private Purse'. [1] The book consists almost entirely of copper plate engravings on about a thousand plates, with no text other than a foreword and the descriptive titles and sources engraved on the plates. The lack of descriptions was felt at the time to be a handicap, but Lister claimed that they would make the book too long. [2] The plates are usually said to be the work of Lister's daughters, Susanna and Anna, who signed the drawings; but though Susanna (or Susan) is mentioned in Lister's letters and his will, there is no reference to a daughter Anna.* It is likely that the other artist was in fact Lister's first wife Hannah, whose name would be Latinized as Anna. Lhwyd wrote in 1692 that 'I doe not wonder your workwomen begin to be tired; you have held them so long to it'. [3]

Publication of the <u>Philosophical Transactions of the Royal Society</u> was resumed in 1683, the year of Lister's removal to London, though it was to be issued rather irregularly until 1696. In 1684-5, the journal carried twelve papers by Lister, one of them being an old letter written to Oldenburg. The papers covered meteorology, geology, chemistry, ornithology, physiology and medicine. One of these [4] included his well-known proposal for the compilation of geological maps.

Lister's enthusiasm for the Royal Society lasted for about two and a half years, and he is recorded as having spoken at 23 meetings in 1684, and 28 in 1685, covering a wide range of

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1. JP p. 104-5.
2. MSS <u>Ashmole 1816</u> f. 81.
3. MSS <u>Lister 3</u> f. 154.
4. <u>Phil. Trans.</u> <u>14</u> (1684) p. 739.

* Anna did exist and Anna was one of the artists. Anne, b. at York 13th Oct 1671 Bapt. 24th Oct 1671 at Holy Trinity. Micklegate York.

biological, geological, antiquarian and technical subjects. On 14 January 1684-5, he was elected Vice-president of the Society. [1] One of his principal tasks in this capacity was the organization of the publication by the Society of Willughby's 'History of Fishes', edited by Ray. This was a matter which was to involve the Society in financial commitments which proved to be a serious embarrassment. On 11 March 1684-5, Tancred Robinson showed the work in manuscript to the Society, and the secretary, Francis Aston, read a letter from Robert Plot to the effect that the Bishop of Oxford would finance the publication of the book if the Society would take a hundred copies. Lister was asked to arrange for the collection of illustrations, apparently to be taken from other published works. This was unacceptable to the Bishop, who, according to a letter from Plot of 25 March, insisted on original plates, from life, and all by the same hand. The Society thought that this would take too long, and appointed a committee, consisting of the President, Samuel Pepys, Drs. Lister, Robinson, Tyson, and Messrs. Waller, Hill and Aston, to arrange for the printing of the book at Oxford, at the Society's expense. Subscribers were found to sponsor the plates at a guinea each, and, after protracted negotiation, accompanied by suggestions of fraud on the part of the printer, and much irritation in the Society at the delay, Lister was able to present the accounts for the printing on 21 March 1685-6. The cost was £360, to be recovered by the sale of the book to the public. Ray was given 20 unbound copies, Pepys six bound and a special presentation copy. Lister, Robinson and Aston were thanked for their work. The book sold badly, and it took the Society several years to recover from the financial

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1. Birch 4, p. 355-6.

burden; in 1687, Halley and Hooke, paid servants of the Society, were each offered 50 copies of the book in lieu of wages - including 20 copies formerly left with Smith's, booksellers. [1]

On 9 December 1685, Lister was retained as Vice-president, and occupied the chair on 13 January next. However, this position was occupied by Sir J. Williamson on 10 February, and on 14 February, Robert Plot wrote to Lister of his distress at hearing of the 'frenzy at the Royal Society; pray be not troubled at it, for the shame at last will turn upon their own heads ...' Plot hoped that Aston, Tyson, Robinson 'and some others will follow your example'. There is no record of any controversy at the Society at this time, though it is recorded that 'Mr. Hooke asserting, it was queried by Dr. Lister if the register or journal of the Society mentioned a glass cane of 32' long made for the Torricellian experiment'. As Lister had spoken on barometers and written a paper on the subject for the Philosophical Transactions, and Hooke was often involved in bitter controversy over matters of priority, it is possible that this entry covers a dispute of some kind. [2] It is also possible that some unpleasantness may have arisen over the abrupt resignation of the secretaries Aston and Robinson in December, and the election of Halley rather than Sloane or Papin as paid clerk. The costs of printing the History of Fishes may also have caused bad feeling. [3] Lister is recorded as having attended the Society on only one further occasion, when he presented the accounts for the History of Fishes on 25 March 1685-6; he was not re-elected to the Council ~~until~~ later that year, [4] and in 1708, he wrote to

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1. Birch 4, passim; FCJR p. 89.
2. Birch 4 pp. 443, 449, 452, 459; MSS Lister 35 f. 110
3. Sir H. Lyons, The Royal Society 1660-1940 (Cambridge 1944) p. 100.
4. Birch 4 pp. 467, 481, 505.

Edward Lhwyd that he had attended Gresham College (i.e. the Royal Society) 'not five times in twenty years'. [1] This abrupt change must presumably have been caused by some personal animosity which it is now impossible to reconstruct; but several years later Lister appeared still to be resentful against some of the members of the Society. [2] Writing to Lhwyd in 1695-6 about his (Lhwyd's) plans for publication, Lister warned him that there were those who would endeavour to slight the work; he mentioned that some of Leewenhoek's letters, though of great importance, had been delayed in publication by the Royal Society over twelve years: 'such mean & invidious spirits reigns amongst even Societies founded purposlie for the promoting of learning in all its parts'. [3]

None of Lister's books published after this bore the imprimatur of the Royal Society, though, when the Philosophical Transactions resumed publication, he again published papers in it, having eleven in the last four years of the century; but he kept away from the Society's meetings, though remaining scientifically active throughout the nineties.

Though no longer attending the meetings of the Royal Society, Lister apparently found the more informal company at the Temple coffee-house 'club' more congenial. This group of naturalists met regularly on Fridays at the coffee-house for at least ten years from about 1689. It included, besides Lister, Hans Sloane, Tancred Robinson, Nehemiah Grew, William Sherard (founder of the chair of Botany at Oxford), William Carleton, George London, James Pettiver, Samuel Dale and Leonard Plukenett (superintendent of the gardens at Hampton Court); and Thomas Tanner was an occasional visitor from

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1. MSS Ashmole 1816 of 24/2/1708-9.

2. EAA p. 7.

3. MSS Ashmole 1816 of 6/2/1695-6.

Oxford.[1]

Lister was also active in publishing his work at this period; sixteen of his papers were published in the Philosophical Transactions from 1692 to 1699. Some of these were on work twenty or more years old at this time; they covered medical matters, geology, steel-making, coal-mining, shells and mollusc anatomy, botany and the animalculist theory of generation. Indeed the last decade of the century was the most active period of Lister's life, if measured by the bulk of his correspondence and the number of his published works.

He was now well established in the leading ranks of the medical profession in London, and became a Censor of the College of Physicians in 1694.[2] However, his wish for appointment to the Royal Court was probably hindered rather than helped by his relationship to the Duchess of Marlborough, in view of the hostility between William and Mary and Sarah's close friend, Princess Anne.

The period produced some changes in Lister's family life. One daughter had died in 1688, and was commemorated by a plaque in Westminster Abbey, well-known now for its simplicity: 'Jane Lister, deare Childe, died Oct. 7th, 1688'. His wife Hannah died on 1 August 1695; the Earl of Thanet invited Lister to stay with him for a month to help him overcome his bereavement.[3] There was some trouble with his oldest son and daughter at this time. The son, Alexander, was a student at Balliol College, Oxford, from 1695, and was at first

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1. MSS Sloane 3336 f. iiv; 3961 f. 41; 4020 f. 107; 4039 f. 80; 4066 ff. 277-91; 4067 ff. 81, 144-6; MSS Lister 36 f. cxxiv.

2. Munk op. cit.

3. MSS Lister 3 f.99.

supervised by Edward Lhwyd; Lister allowed him £10 per annum for this.[1] Alexander appears to have been negligent in writing home, and Lhwyd left Oxford in 1697 on his great tour of Britain. Shortly after, he heard from William Williams, librarian at the Ashmolean, that 'Dr. Lister's son is unhappily married to a woman of no fortune, no extraction and scarce reputation'.[2] Lister's daughter Frances also married under strained circumstances in 1698. A correspondent, Thomas Railton, spoke of the disparaging way in which she, as Mrs. Evans, spoke of her father, and J. Braylesford urged Lister to visit her and attempt reconciliation.[3]

Lister's correspondence was very wide at this period. Much of it was personal or medical, as with E. Reading, of Dublin; Archibald Pitcairne, the Scottish professor of medicine at Leyden; George Bennis, of Montpellier; John Howes, Lister's former sizar at Cambridge; F. Bernard, physician to James II; J. Barrow; W. Courtier; J. Braylesford, of Cambridge; and several relatives. There were many letters concerning Lister's books and his bequests to the Ashmolean: from T. J. Almeloven, the Amsterdam publisher; Jacob Tonson, the London publisher; Edmund Gibson, later Bishop of Lincoln and London; and, from Oxford, from Henry Aldrich, the vice-chancellor, Arthur Charlet, Thomas Tanner, and John Hoskyns. Lister remained in contact with some of his Yorkshire friends, as with Henry Gyles and Ralph Thorseby; and his scientific correspondents included Hadrian Beverland, geologist and coin collector, and intermediary between Lhwyd and Woodward; Benjamin Allen, of Essex, author of The Natural History of the Chalybeate and Purging Waters of England (1699); John Beaumont, the Somerset fossil collector; Jabez Cay, minerologist,

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1. MSS Ashmole 1816 f.115.

2. R. T. Gunther, Early Science in Oxford 14 (Oxford, 1945) p. 402.

3. MSS Lister 4 ff. 1-2, 103.

of Newcastle; the Reverend J. Bannister, shell collector, who visited the East Indies and spent 14 years in Virginia, where he died in 1692; Samuel Dale, of Braintree, Essex, collector of insects and snails, and close friend of John Ray; Edmund Halley, astronomer; Thomas Henshaw of Oxford, writer of papers on salts published in the Philosophical Transactions; Francis Jessop, of Sheffield, geologist; A. Lowther, of Marske, Yorkshire, who wrote of mining; Charles Leigh, author of the Natural History of Lancashire, Cheshire and the High Peak of Derbyshire (1700); Bishop Nicolson (later Archbishop of Cashel) of Carlisle, fossil collector and antiquary; John Place, physician to the Grand Duke of Tuscany, brother of Francis Place of York, and acquaintance of Francesco Redi; Charles Proby, medical student and shell collector, of Montpellier; Sir Robert Sibbald, professor of Medicine at Edinburgh, geographer, zoologist and founder of the Edinburgh Royal Botanic Gardens; Robert Steevens, a Quaker shell-collector of Goose Creek, South Carolina; J. J. Scheuchzer, physician and naturalist, of Geneva; Edward Thomas, shell-collector and protégé of Lhwyd at Oxford; Richard Towneley, of Burnley, centre of a group of northern philosophers; J. P. de Tournefort, the French botanist; R. Willbraham, a midlands fossil collector; and N. Witsen, Dutch naval architect, and Netherlands ambassador to England, botanist and shell-collector. Sir Richard Bulkely, now living in Ireland, remained a prolific writer on agriculture and geology; his hypochondrism made him something of a joke to his contemporaries, and after his removal to Ewell in Surrey, a mystic and devotee of faith-healing. A particularly close friend was Dr. Tancred Robinson, whom Lister had known as a schoolboy in York, and who was proposed by Lister as a member of the Royal Society in 1684. Robinson was also a close friend of Ray, and it is difficult to understand why this contact, and that of Dale, did not bring Ray and Lister together again. Lister did advise Ray on medical matters, though apparently only at second-hand, [1] and he passed plant specimens to Ray from Lhwyd, [2] but a brief acknowledgement of the latter from Ray was the only direct communication between them at this time. [3]

Lister's acquaintance with Dr. (later Sir) Hans Sloane, whose collections were to become the foundation of the British Museum, must have begun soon after Lister's arrival in London, when they would have met both at the Royal Society and at the College of Physicians, to which they were both admitted as Fellows by the Charter of 1687. Lister made Sloane a present of books when the latter left for Jamaica in 1688. [4]

Much the most important of Lister's correspondents was Edward Lhwyd (1660-1709). Lhwyd was a generation younger than Lister, but the two became very close friends, each being the principal correspondent of the other; there survive 88 letters from Lister to Lhwyd, and 102 from Lhwyd to Lister. [5]

Lhwyd was at Jesus College, Oxford from 1682-7, his tutor being Robert Plot, and during his undergraduate days he earned a little money as assistant at the Ashmolean Museum. [6] After graduating, he spent some time collecting plants in North Wales, sending collections to Jacob Bobart at the Oxford Physic Garden, and to John Ray. On his return, he became under-keeper at the Ashmolean, spending much time classifying the shell collections. This brought him into contact with Lister, the acknowledged

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1. Raven p. 280.

2. MSS Lister 35 f. 54.

3. FCJR p.136.

4. MSS Sloane 4063 f. 36.

5. The correspondence is contained in MSS Lister 35 and MSS Ashmole 1816.

6. For biographical details of Lhwyd see Gunther 1945.

authority on the subject. The first letter between them which is still preserved is from Lhwyd, of 19 July 1689, but there must have been earlier contact, as shells sent by Lhwyd were illustrated in a section of the Historia conchyliorum published in 1688; and Plot wrote in June 1689 of Lister's 'kindness' towards Lhwyd. [1] Several of Lister's early letters include directions for finding particular fossil localities.

Plot was at this time thinking of retiring from the keepership of the Ashmolean, and Lhwyd, who had no private means, was eager to succeed him. Though he claimed that Ashmole had earlier promised the succession to him, Lhwyd had heard rumours that Ashmole now intended to appoint his nephew, George Smallridge, a man with no interest in natural science. Lhwyd appealed for support to Plot, who appeared to favour him, and to Lister, [2] who, as physician to Ashmole's wife, [3] would have opportunity to present Lhwyd's case. Lister did so, and advised Lhwyd not to mention his plans for further collecting expeditions, as Ashmole required constant attention at the museum. [4] On 7 October 1690, Lister wrote that 'I have at last prevailed on Mr. Ashmole to give you the keeping of the museum', [5] and early in the following June Lhwyd took over the keys. He wrote to Lister that 'I am wholly indepted to yr goodnesse' in frustrating what Lhwyd, apparently wrongly, took to be Plot's opposition. [6]

Over the next few years, the extensive correspondence between the two covered shells and fossils, antiquities, the affairs of the Ashmolean, Lister's books and his bequests to the museum, Lhwyd's

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1. MSS Ashmole 1817a of 10/6/89.
2. MSS Lister 35 f. 56.
3. Josten pp. 1752-3.
4. MSS Lister 36 f. vi.
5. MSS Ashmole 1816 f. 84.
6. MSS Lister 36 f. i.

preparation for his great tour of Celtic Britain in 1697-1701, and his two important books on British fossils and archaeology. In general, Lhwyd's letters are of much greater scientific interest than those of Lister, whose field work at least was now almost over. He was still building up his shell and fossil collection, but he now relied upon his wide circle of correspondents and upon professional collectors. For example, in 1696 he wrote to Ralph Thorseby to ask for enquiry to be made to John Bolland of Halifax for specimens of fossiliferous slate from coal pits to be sent to London. [1]

Most of Lister's important books were published in the 'nineties'. The Historia conchyliorum was completed by 1692, and a second edition appeared from 1692-7. As this work consisted almost entirely of engravings of the shells of molluscs, Lister published an account of the internal anatomy of the phylum. This was done in three parts. The Exercitatio anatomica of 1694 covered the land snails and slugs, with some notes on cephalopods; the Excercitatio anatomica altera of 1695 dealt with the freshwater and marine gastropods; and finally, the bivalves, both marine and freshwater, were covered in the Excertitatio anatomica tertia of 1696. Each work deals in detail with the anatomy of a type species, and includes a good deal of digression on general physiological topics. Shorter accounts are given of the individual characteristics of some of the other species of each group, and each book contains a number of anatomical plates. This work was done in London, Lister keeping specimens alive in his small garden in Old Palace Yard until needed for dissection. [2] These books were to a great extent breaking new ground, being the first attempt at a

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1. R. Lancaster (Ed.) Letters of Eminent Men Addressed to Ralph Thorseby, 1 (Leeds, 1830 and London 1832) p. 259.

2. MSS Ashmole 1816 of 19/10/93.

comprehensive survey of the anatomy of an entire invertebrate group, and the work is of greater zoological significance than the much better known Historia conchyliorum. It was not, however, fully appreciated at the time; in 1694, Lister wrote to Lhwyd that

> 'I have now in the Presse five small Tracts of the most common cronical deseases in London: this I part with unwillingly because it will come out before its time: as belonging to a greater bodie of Phisic, but I find my selfe necessitated to it, to stop the censorious mouthes who thinke and say a man that writes on Insects can be but a trifler in Phisic. After this small essay I hope they will let me alone to pursue Philosophie amongst the inferior sorts of beings ....'[1]

The book referred to here is the Sex exercitationes medicinales.

Lister had at some time made the acquaintance of William Bentinck, Lord Portland, the first Gentleman of the Bedchamber, reputedly 'the King's right-hand man' and the most unpopular man in England. Portland had botanical and agricultural interests, and was Superintendent of the King's gardens; Lhwyd had asked for Lister's support in seeking Portland's sponsorship for collecting expeditions to the Canary Islands and the West Indies. Late in 1697, following the Peace of Ryswick, Portland was sent as English ambassador to Paris, and Lister accompanied him as physician. He kept detailed notes on the journey, publishing them on his return as Journey to Paris in the Year 1698, a work which is still often used as a source of information on the state of society in Paris at this time.

Lister was worried about his own health, and he had found

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1. Ibid. f. 113.

'several times' that the French air was beneficial to him; as the war had prevented recent visits, he 'took first opportunity of Lord Portland's acceptance of my attendance on him in his Extraordinarie Embassie'. Lister went ahead of the Ambassador, with a 'good friend who was sent to prepare matters'. They left London on 12 December, and arrived at Paris on 1 January 1698. Lister had visited the capital earlier, and commented on the changes he saw. His French had evidently deteriorated since the days when Ray, thirty years earlier, had described him as a 'master of the language'; [1] he now admitted that he could not follow the dialogue of a French play.

Lister made many contacts in Paris, mostly of a purely social nature; some of these, such as the wife of the ambassador of Brandenburg, he treated medically. However, he also followed his scientific interests; indeed, he claimed that

> 'You'l easily find by my Observations, that I incline to Nature rather than Dominion; and that I took more pleasure to see Monsieur Breman in his white Wastcoat digging in the Royal Physic Garden, and sowing his couches, than Monsieur de Saintot making room for an Ambassador; and I found myself better disposed, and more apt to learn the Names and Physiognomy of a Hundred Plants, than of 5 or 6 Princes'. [2]

Lister describes the different stones used for building, and visited quarries to watch the making of millstones, which he compared with those made in Yorkshire. He was particularly interested in the plaster quarries of Monmartre, and in fossil beds

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1. CJR p.123.

2. JP p.8.

at Vanre, three miles from Paris, in which he found unusually large Buccinia. He looked at a number of shell collections, and had drawings made of those species which were new to him. Shell collecting was becoming fashionable in Paris, and prices paid for choice specimens were high; 900 livres, or £50 at the current exchange rate, for a single shell was noted by Lister, who also recorded that a collection of 32 shells fetched an unsuccessful offer of 11,000 livres from the Duke of Orleans; and he adds that it is debatable who was the greater fool, he who made the offer, or he who refused it. He met the botanist Tournefort, with whom he had corresponded; the anatomists Verney and Merrie, with whom he discussed their theory of foetal circulation; the zoologist, Poupart; the minerologist Morin; and the English instrument maker Michael Butterfield, resident in Paris thirty-five years. He saw Swammerdam's papers at the house of M. Thevenot, and, though claiming that they were below his expectations, he tried to buy them. Lister described the Royal library, in which he saw an imperfect copy of his Historia conchliorum. Amongst the literary figures he met was the Breton philologist M. Pezron, with whom he arranged a correspondence for Edward Lhwyd, though this did not materialize. As might be expected, Lister gave extensive notes on the organization of medicine and the public hospitals in Paris.

Lister returned to London by June, 1698. Now sixty years old, his life began to change. For a few months, he appears to have been busy. He had a good deal of polite correspondence with new acquaintances in Paris, and he continued to receive letters from many of his English friends. The Journey to Paris was published a few weeks after his return, and was a great success, three editions being brought out within a year. It was quickly parodied in a work, A Journey to London in the year 1698, after the ingenious method of that made by Dr. Martin Lister to Paris in the same year, written by a well-known lampooner William King. King (1663-1712), a doctor of commom law, was secretary to Princess Anne, and a staunch Tory and High churchman. He published satirical attacks on the Marlboroughs, in his Rufinus of 1711; on Sloane, in The Transactioneer of 1700; and on the Philosophical Transactions, in his Useful Transactions in Philosophy and other sorts of learning of 1709; and Lister was to be attacked again by King a few years afterwards; the possible reasons for these attacks will be discussed later.

On 24 October 1698, Lister married his second wife, Jane Cullen [1] (a Samuel Cullen had been a patient of Lister in 1685, and had subscribed for a plate in Willughby's Fishes; and Richard Cullen was to be Lister's lawyer from 1699). Fairly soon after, Lister 'broke up' his house in Old Palace Yard and took lodgings in the country, at Mr. Mitchell's house in Leatherhead, Surrey, for the sake of his health. [2] He continued to visit London to attend to his practice, lodging with his sister, Mrs. Thynne, in Glasshouse Street; [3] but presumably he was now attending only a small number of patients. It is possible that he visited Craven in the summer of 1699; there is a period of three months at this time from which no letters to Lister are preserved, and in April of that year, Robert Parker, of Carleton, wrote that he would be pleased to see Lister at his (Lister's) own house in Craven, and would entertain him well. [4] Such a journey may have been made for health reasons and to show his

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1. Parish register of St. Stephen's, Walbrook, London; quoted in Goulding 1899 p. 362.

2. MSS Ashmole 1816 of 7/1/99.

3. MSS Lister 37 of 13/1/1701-2.

4. MSS Lister 3 f. 183.

Yorkshire estates to his new wife.

Apart from Lhwyd and Robinson, most of Lister's scientific correspondents appear to have ceased writing to him from about 1700; his later correspondence is mainly legal and financial. There is a record of 10 different annuities to the total value of £1,540 paid to Lister's lawyer, Richard Cullen, by his sister Mrs. Susanna Gregory of Burwell. [1] This is probably connected with the financial settlement made for Lister by his father when he gave up his fellowship at St. John's, though it is not clear how many years' income this sum represents. There is also a record of £80 being paid as fee for attendance on Lord Portland in Paris, obtained only after almost three years' effort on the part of Cullen. [2]

In 1700-01, Lister was engaged in the editing for publication of two medical works, the Aphorisms of Sanctorius, and the Aphorisms of Hippocrates. Tancred Robinson helped to see these through the press during Lister's absences at Leatherhead, and he had some difficulty in finding a publisher for books written in Latin. [3] Robinson had had similar difficulties with Ray's History of Plants, and complained to Lister of the decline of learning and printing in England, as compared with Holland. [4] Also in 1701, Lister published his last paper in the Philosophical Transactions - describing an attempt made many years earlier to colour chyle in the lymphatics.

Robinson and Lister had both been concerned in the publication of Lhwyd's monograph, the Lithophylacii Britannici Iconographia. Even though this was based on the Ashmolean collections, the University of Oxford would not publish it, and it was brought out in 1699 at the

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1. MSS Lister 4 f. 18.

2. MSS Lister 2 f. 8.

3. MSS Lister 37 of 13/1/1701-2.

4. MSS Lister 36 f. cxxiii.

expense of a group of private subscribers: the Earl of Dorset, Lords Somers and Montague, Isaac Newton, Martin Lister, Tancred Robinson, Hans Sloane, Francis Aston and Professor Geoffrey of Paris. The work was dedicated to Lister. That Lister was concerned actively in the production, and not only financially, is shown by a letter from Robinson to Lhwyd stating that Lister's marriage had held up publication; [1] and Robinson later asked Lister to try to persuade Lhwyd to publish a second edition. [2]

William III died in March 1702, and was succeeded by Anne, at that time an intimate friend of Lister's niece, Sarah Churchill. Soon afterwards, Lister was appointed fourth physician to the Queen. He appears then to have discontinued the practice of medicine outside the Royal household, and he moved further into the country, to Epsom. [3] For visits to the Court, he at first kept his lodgings at Glasshouse Street, and later at his sister-in-law's in Cecil Court, but he began to find the journey a hardship because of ill-health. From about 1700, most of Lister's letters contain some reference to this, and as early as February 1700-01, he wrote to Lhwyd that 'I may possiblie dwindle on a little time, but I can neither endure the choakie aire of London, nor the piercing colds of a dryer aire ..' [4] By 1709, he was complaining of being housebound because of coughing and shortness of breath caused by asthma,

> ' ... these being hereditarie evills, I bless God, notwithstanding I have laboured under them these manie yeares, I am arrived at the full age of man: but never expect to be better, but dailie worse ...'. [5]

In January, 1709-10, he was appointed second physician to the Queen,

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1. Gunther 1945 p.404.
2. MSS Lister 3 f. 226.
3. MSS Ashmole 1816 of 8/8/1702.
4. MSS Ashmole 1816 f. 126.
5. MSS Smith 52 f. 87.

and the journey home to Epsom from his London lodging, now at Chelsea, took five days, as 'I could beare neither Coach nor chair upon the pavement, so crasie I am growne from 2 or 3 fitts of the stone I have had this last summer in my kidneys ...' [1] However, in October of that year he had written to Smith that he had been at Windsor twice recently. [2]

At Epsom, Lister spent much of his time in his garden. Several planting plans drawn up by him survive, and many of his surviving papers from this period are bills from Samuel Driver, nurseryman. He began to dispose of his books and collections; in 1707, Lister offered his shells and the copperplates of the Historia conchyliorum for sale to Hans Sloane, perhaps after an enquiry from Sloane as to their future. Lister wrote on 29 August 1709, that

> 'My plates and 3 cabinets of shells, I do designe for the Museum at Oxford, & yet, if I could have one to buy ym yt would improve yem for ye good of ye publick, I shall be redie to part with yem. I will not abate of £150, which I am sure, is not ye 3rd part of their value'. [3]

Sloane inspected the shells and plates at Cullen's house, but demurred at Lister's price:

> ' ... I would not for a little matter make any difficulty with a person for whom I have so great a regard, but really the summe is so great tho yet under what the true value may be, that I cannot any ways contrive how to think them worth so much to me who have laid out a greate deale in the same way and am not like to see much of it again ...' [4]

However Lister would not reduce his price, nor would he

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1. Ibid. of 26/1/1709-10.
2. Ibid. 52 f. 87.
3. MSS Sloane 4064 f. 206.
4. MSS Lister 37 ff. 107, 109.

separate shells and plates:

'I do resolve not to part ye shells and ye plates: because they properlie belong one to ye other: and if I sell ym not in my lifetime, I designe them for the Museum at Oxford'.

Though he would be glad to see them in the hands of someone who would care for them, yet

'I shall not take lesse than £150 for both shells and plates: & you may think how great a gainer I am by ym at yt price, they having cost me at least £500'. [1]

However, though the plates were eventually bequeathed by Lister to the University of Oxford, the shells were bought by Sloane, though none of them have been traced in the existing British Museum collection. [2]

Most of Lister's scientific books had been disposed of before his death, probably being included in his frequent bequests to the Ashmolean Museum. An inventory of Lister's books, apparently prepared after his death, consists mainly of a Liste de Tailles douces, light literature in modern languages, to the value of £707, and 228 other books, all medical except for nine of Lister's own works, five of Ray and Willughby, three by Nehemiah Grew, four on husbandry, Mercurius on botany and Freind's Praelectiones chymicae. [3]

Lister kept up his correspondence with Lhwyd until the latter's death in 1709, though the letters became less frequent than formerly; and Robinson wrote until 1700. There were letters

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1. MSS Sloane 4041 f. 23.

2. G. Wilkins, 'The Sloane Shell Collection', Bulletin of the British Museum (Natural History) Historical Series 1, no. 1 (1953).

3. MSS Lister 5 ff. 133-42.

from Charlett, Parry, Tanner and Gower, from Oxford, until 1709, and Tillman Bobart, brother of the keeper of the Botanic Garden, asked for Lister's support and influence at the university in his attempt to succeed his brother in 1701.

From 1700, Lister had been writing to Dr. Thomas Smith, Cotton librarian and bibliographical authority, though the two were described as 'old friends' in 1700, and there is an isolated letter from Smith dated 1688. [1] It seems that Smith was proposing to arrange Lister's papers; in 1708, Lister wrote

> ' .. I am still and ever shall be obliged to thanke you for the great and constant care you take of my poor papers; I wish they were worth your pains. But, Sr, you have put upon me a new taske ... I have begun to collect some few accidents of my life: but find all my friends and relatives dead, that should inform me better: however, such short notes as I could make, I will leave for your perusal ....' [2]

Smith died in 1710, two years earlier than Lister, so that this work was not completed; and this may account in part for the incomplete nature of Lister's papers, in which, for example, none of Ray's letters is preserved. There were some letters from the Dutch publisher Almeloven, a few on medical matters, and a single letter from John Place on his return to York in 1708; but otherwise Lister had now lost contact with his old acquaintances. For example, George Plaxton, Rector of Barwick in Elemet, Yorkshire, wrote to Ralph Thorseby in 1708 that

> 'I would have you visit my old friend, Dr. Martin Lister: tell him I am still alive and have the same value for him which I had in 1672, for so long I have known him'. [3]

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1. MSS Smith 51 f. 31.

2. MSS Smith 51 f. 53.

3. The Diary of Ralph Thorseby 2, ed. Hunter (Leeds, 1830) p. 134.

In spite of his ill-health, Lister published two books while living at Epsom. In 1705 he brought out a limited edition, for a list of subscribers, of an edition of Apicus Coelicus, de opsoniis et condimentis, the well-known Roman cookery-book. He had been working on this at least since 1702, when he asked Lhwyd to search for manuscripts of the book in the Bodleian library. [1] This work, like the Journey to Paris, was pilloried by William King in a parody, The Art of Cooking. [2] King opened with the words:

'Ingenious Lister were a picture drawn,
With Cynthia's face, but with a neck like brawn;
With wings of turkey, and with feet of calf,
Tho' drawn by Kneller, it would make you laugh!.....
Homer, more modest, if we search his books,
Will show us that his heroes all were cooks:........
Oh cou'd that poet live! cou'd he rehearse
Thy journey, Lister, in immortal verse!
Muse, sing the man that did to Paris go,
That he might taste their soups and mushrooms know.'

At one point, Lister attributed these attacks to his relationship, presumably with the Marlboroughs; but in the same letter he claimed that he had no influence at court: 'I have been 5 yeares at Court, and have never received any the least favour for myselfe, or any of my family ...' [3] It seems likely that from about the late 'nineties', Lister had become unpopular amongst a section of the medical

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1. MSS Ashmole 1816 f. 126.

2. The Art of Cooking, in Imitation of Horace's Art of Poetry, with some letters to Dr. Lister and others, occaision'd principally by the Title of a Book publish'd by the Doctor ... by the Author of the Journey to London. Humbly inscribed to the Honourable Beef Steak Club (London 1705).

3. MSS Ashmole 1816 of 19/1/1707-8.

profession. In 1708, he told Lhwyd that he had not been to the College [of Physicians] for twelve years, i.e. since 1696;[1] and he certainly felt that he was being attacked by certain members of the College. He wrote to Smith in 1709 of 'all the contempt and scorne wch I have had these manie yeares from the brotherhood of Phisic'.[2] He described King's work as 'the fresh attack of a Villanous buffoone, set on by the Phisitians as in R. W. [i.e. King William's] time',[3] though he claimed that, as diet was an essential part of medicine, he was justified in writing on this subject. He further wrote to Lhwyd that

> 'There always will be envie in all great Corporations; but that is onlie personal .... none has suffered more than myself; and because they durst not fairlie attack me, they put a buffoon to do their worke ...'.[4]

This feeling against Lister, if it existed, was, no doubt, caused largely by his truculence and intolerance of opinions opposed to his own. Thus, though his own physiology was extremely speculative, he is reputed to have accused Sydenham of 'playing the philosopher by fanciful and precarious interpretations of the nature of diseases and medicines to gain a sort of credit with the ignorant';[5] and he dismissed Harvey as being 'beyond contempt' for not noticing the auricle of the snail's heart.[6] His attitude to medicine was in general very conservative; when Lhwyd wrote on behalf of a relative to ask advice on the study of medicine, Lister replied that

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1. MSS Ashmole 1816 of 24/2/07-8.
2. MSS Smith 51 f. 71.
3. MSS Ashmole 1816 of 29/12/07.
4. MSS Ashmole 1816 of 30/9/05.
5. Quoted in Davies p.318.
6. EA p.26.

> ' ... if he intends to grow ritch upon the practise, he must set up with impudence good store, and neere trouble himself about the rest, but, to be serious, no authors are so well worth studding & with diligence as the antients; I mean Hippocrates, C. Celsus & Dioscorides. These he ought to be extreamlie well versed in; and he will find that all the rest have added little; and what they have of good they have gott out of them. The great improvement lies in Anatomie, and the use of some new drugs'; [1]

Later he recommended Pliny, Celsus, Scribonius, Galen, Hippocrates and Dioscorides; 'most of the moderns are vainlie false', though 'all medical historie is good, and the modern anatomie'. [2] However, as the first of these letters shows, he had some sympathy with 'modern drugs', presumably chemical; as mentioned above, one of the few scientific books Lister kept until his death was Freind's Praelectones chymicae, and his papers contain two Helmontian manuscripts: van Helmont De flatibus, [3] and Praxis Helmontiana. [4] Neither is in Lister's hand, but the latter has an inscription, by Lister: Operā et diligentia amicissimi viri D. Sturdy, and several remedies are marked with a cross or with 'N.B.'

Together with this conservatism, Lister considered himself to be upholding a dignity in his profession not shared by all physicians; he wrote to Smith that

> 'I had incurred the envie of manie by endevouring to affect manie things necessarie to be observed in practice, for the honour of the profession, espeacillie in relation to the methods and decorum, and goode of consultations, wch

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1. MSS Ashmole 1818 f. 124.
2. MSS Ashmole 1816 f. 180.
3. MSS Lister 5 ff. 191-210.
4. MSS Lister 5 ff. 175-90.

the avarice, hastie humour and pride of some men had quit ruin'd'. [1]

While Lister seems to have felt that there was hostility towards him personally among some of the members, the end of the Seventeenth century saw a period of great controversy in the College of Physicians. The establishment within the College saw the apothecaries as serious competitors, and sought to keep them down; amongst other measures, a public dispensary was opened by the physicians to provide an alternative pharmaceutical service, and members of the College were ordered to write directions for apothecaries in English, which the patient could read. On the other hand, a substantial body of opinion in the College, including many of the Fellows admitted when the membership was doubled by the charter of 1687, favoured working with the apothecaries to their mutual advantage. Controversy therefore broke out, initially over the question of English directions, but rapidly becoming a general attack on the College establishment, its power to levy bonds and fines, its secrecy over financial and other affairs, and the restriction to committee members of the power to propose amendments to the constitution. So bitter were the disagreements that only one new Fellow was admitted to the College from 1694 to 1701, for fear of introducing more indigestible 'mutineers'. At Michaelmas, 1704, a paper on the low state of the College was submitted by 13 Fellows, and a compromise settlement was agreed. [2]

Lister does not figure prominently in these disputes, and for most of the period he was not active in College affairs. However, in 1694, he was a Censor, and a member of the committee which

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1. MSS <u>Smith 51</u> f. 53.

2. Clarke chapter 22.

suggested the opening of the Dispensary, [1] and in 1702 the President, Sir Thomas Millington, wrote to Lister to ask for his support against 'intruders within the College', that is, would-be intruders into committee affairs. [2] Support for the establishment would agree with Lister's character, though some of his friends, such as Drs. Slare and Barnard, and in particular, Tancred Robinson, were prominent in the other party.

No doubt there was also some jealousy of Lister because of his Royal appointment obtained through his relationship to the Marlboroughs - a couple who were themselves not universally popular. Such a feeling would be the more understandable because Lister's reputation had been made as a naturalist rather than as a physician; hence King's description, in the Art of Cooking, of Lister's works as

> 'concerning cockles, English beetles, snails, spiders
> that get up into the air and throw us down cobwebs,
> a monster vomitted up by a baker and such like, which,
> if carefully perused, would wonderfully improve us'.

Lister's justification for the publishing of the Sex Exercitationes medicinales, quoted above, may also be mentioned in this context. The work in fact had a hostile reception from the profession, on the grounds that it mentioned venereal cases involving women patients and that it dealt with rare and obscure diseases such as Hydrophobia. [3]

There thus seems to be reason for Lister's claims that he was being attacked by a section of the physicians. He was, in any case, disillusioned with learned societies after his experience of the

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1. Ibid. p. 443.
2. MSS Lister 37.
3. MSS Lister 3 f. 132.

Royal Society and the College of Physicians. He wrote to Smith about 1708 that

> 'Societies are never modest, but must be spitefull, thro ye predominance of some craftie men amongst them; and ye world must be behouden and expect good from single persons onlie'. [1]

Lister's last work was a treatise on body fluids, the Dissertatio de humoribus of 1709. This was a subject in which he had been interested from the days of his earliest writings. Perhaps because Lister's physiological ideas were not in the currently fashionable mechanical style, and he did not wish to risk further pillorying, there was some secrecy over the book until its publication, at London in 1709, with a second edition in 1711 by Almeloven in Amsterdam. [2] The book, wrote Lister, was 'as full & compleat a system of the animal oeconomie, especiallie in relation to the Humours, as I could contrive'. [3] The book is long and almost entirely speculative, with little evidence of experimental or even observational work. In the same year, Ray's edition of Willughby's History of Insects was published.[4] This contained an appendix on English beetles written by Lister, but this had been completed many years before; the manuscript had been sent to Lhwyd in 1692. [5] The section on spiders in Ray's book was a slightly abridged reprint of Lister's work of 1678.

Lister's active days were now over. In the spring of 1709, he wrote to Lhwyd that 'I did go out with the butterflies, which is a

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1. MSS Smith 52 f. 5.
2. MSS Smith 64 f. 10.
3. Ibid. 51 f. 63.
4. John Ray, Historia insectorum (London, 1712).
5. MSS Ashmole 1816 f. 92.

great comfort to me to have lived to see warmer weather once more'.[1] Lhwyd died two months later; there survive a few letters to Lister from that summer, and two letters from Lister to Smith from the following winter, but otherwise nothing more; the last two years are silent. Lister died on 2 February 1711-2, and was buried beside his first wife at Clapham Church, his memorial reading

> Near this place is buried the Body of
> Martin Lister, Doctor of Physick,
> a Member of the Royal Society, and one
> of Queen Anne's Physicians, who
> departed this life the second day of
> February, 1711-12.

In his will,[2] Lister left £10 each to his two older children, Alexander and Frances, both of whom had married against his wishes; £20 to each of his other children, and the rest of his estate (cash, and property in York) to his wife, with his copper-plates from the Historia conchyliorum going to the University of Oxford. Presumably the estates in Craven had already been settled on his second son, Martin, as the latter was to sell them later to Lord Bingley.[3] As Lister's income from Lincolnshire was in the form of annuities, this would cease at his death.

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1. MSS Ashmole 1816 f. 178.
2. Quoted in Goulding 1899 p. 370.
3. T. D. Whitaker, History of Craven (London 1805).

Chapter II Spiders.

(1) Previous writers on spiders.

The principal accounts of spiders existing at the time of Lister's activity in this field were those of Aristotle, Pliny, Aldrovandus and Moufet. The works of Kircher, Redi and Hooke dealt with some aspects of the biology of spiders, and the principal medical authors mentioned the medical virtues of these animals.

Aristotle [1] distinguished between web-spiders and phalangids (i.e. those with a poisonous bite). He gave two species of the latter, one large, black, slow, and spinning a poor web on the ground or on stone walls, and a smaller species, spinning no web, and jumping like a flea. The latter is clearly one of the Salticidae, the former being unidentifiable. He noted that web-spiders spin their webs in the order perimeter: radials: spiral, and that spinning takes place at dawn and dusk, though he was mistaken in saying that only the female spins. He differentiated between one species which watches from the top of the web (i.e. probably a Linyphid or Lycosid) and one which watches from below. Mention will be made of Aristotle's account of the production of silk later in this thesis. He noticed that when an insect strikes the spider's web, the spider first runs to the centre before running out to the prey, and that only the juices of insects are used as food. Like other writers up to and including Lister, Aristotle was impressed with the ability of spiders to withstand prolonged starvation; for proof, he referred to the spiders kept alive in apothecaries' shops. [2] A fairly extensive account of the reproduction of spiders was given. [3] His statement that spiders copulate at the rear, facing away from one another, was

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1. Aristotle, Historia animalium 622 b 28.

2. Ibid. 594 a 12.

3. Ibid. 555 a 26.

taken by Lister [1] and by modern writers [2] as a serious error, though it may be that Aristotle was in fact referring to the head-to-tail posture adopted by spiders in copulation. He regarded the eggs as grubs which metamorphose in three days into small spiders, being contained during this period in a web which is in some species attached to the mother. At four weeks old, the spiderling is fully developed, and in phalangids the mother is killed and eaten by her young. This fate is often shared by the father, who was thought to take part in the care of the young.

Pliny [3] derived almost all his information from Aristotle, his additions being purely literary. His description of the mating position is, however, slightly more accurate (Aranei conveniunt clunibus).

Lister made frequent reference to both classical authors, almost always derogatory, or at least sceptical; for example, when discussing the assertion of both authors that spiders can kill lizards and snakes, he commented that our [English] spiders are not so spirited (tantus animus non est). He did however, allow the truth of Aristotle's statement that spiders eat each other. [4]

Lister's general attitude to Aristotle at this time can be seen in a letter written by him (in Latin) to John Ray on 24 March 1667-8:

> 'I value Aristotle's Natural History enough to concern myself with it; but the distinguished man gives very little help or satisfaction. Certainly, in those things which concern me daily, I find him to err badly, and to be not at all that admirable and splendid author

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1. HAA p. 16.
2. eg. T. H. Savory, Spiders, Men and Scorpions (London, 1961) p. 22.
3. Pliny, Historia naturalis, books 11, 28, and 29.
4. HAA p. 11.

whose works are to be consulted by custom: quite the reverse. It is plain that he was content to build up an elaborate structure on a very slight experimental basis'. [1]

In reply to this letter, Ray admitted that, however learned Aristotle may be, he sometimes made mistakes. [2] It would seem, however, that Ray was rather more sympathetic to the ancients than was Lister. On 22 December 1669, Lister recalled in a letter to Ray that it was Ray who took away his prejudice against Pliny - 'I have ever since looked on him as a great treasure of learning'. [3] This might seem surprising in view of Lister's strong condemnation of those, such as Moufet, whom he accused of merely transcribing the works of others; but Lister in general tended to extravagance in distributing praise or blame, and to be rather fulsome in agreeing with his correspondents. As his references to Pliny are usually critical, we may suppose that his original prejudice was not entirely dissipated by Ray.

Lister professed great admiration for the work of Aldrovandus (1522-1605): [4]

> 'A truly magnificent man, the customary evaluation of whom I cannot accept. I should rather accord him the honour he deserves, but it is the way of men that we prefer a display of immense industry and collection, rather than great accuracy in small things'. [5]

This opinion, as Lister himself admitted, was not, and is not now, shared by other writers. For a modern opinion of the sixteenth

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1. CJR p.11 (This letter is incorrectly dated by Lankester).
2. Ibid. p. 29.
3. Ibid. p. 48.
4. Ulisse Aldrovandi, De animalibus insectis libri septem (Bologna, 1602).
5. CJR p.11 (in Latin).

century encyclopoedist we may quote Raven :

> ' ... apart from a number of not very recognisable pictures and brief descriptions, there is hardly a single fact or observation which is of value; and what there is of first-hand knowledge is swamped in vast accumulations of quotations, epigrams and proverbs, of moralizings, fables and ancient traditions. Of its author's erudition and industry, here as in the rest of his pandects, there can be no question; .... but it is the work of a humanist of the Renaissance, not of a naturalist and observer, a monument to the past, rather than a search-light to the future. [1]

However, even though Lister held this high opinion of Aldrovandus, and quoted him approvingly in some of his earlier papers in the Philosophical Transactions of the Royal Society, [2] the only use made of the work of Aldrovandus in Lister's book of 1678 was as the source of a quotation from Bellon. [3] It may be that his early uncritical enthusiasm for this work declined in later years.

We see a similar extravagance in Lister's condemnation of the work of Thomas Moufet (1553-1604). [4] This book was founded on the notes of William Penny, with additions from Edward Whotton (or Wotton) and Conrad Gesner. It is a work more typical of the Renaissance encyclopoedists than of the later seventeenth century naturalists such

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1. C. E. Raven, English Naturalists from Neckham to Ray (Cambridge, 1947) p. 191.

2. Phil. Trans. 5 (1669) p.1001.

3. HAA p. 13-14.

4. Thomas Moufet, Insectorum sive minimorum animalium theatrum (London, 1634). English translation by John Rowland, published as part of Edward Topsell's Natural History of Four-footed Beasts (London, 1658). Raven (1947, p. 187) believes the section on spiders to be the work of Moufet himself; the rest of the book is based on Gesner, Penny and Wotton.

as John Ray and Lister himself, being a compilation from previous authors with little original matter. The reader is overwhelmed by quotations from 46 different authorities in the section on spiders alone, and of the fifteen pages in this section, six are devoted to the medicinal aspects of spiders (including the effect of their bite), and another four to the praise of their beauty and moral virtues. In this work, which must be regarded as the standard early seventeenth century authority on terrestrial arthropods, we find descriptions such as this:

' ... the same colour that Ovid writes that lovers have, that is, pale; and when she sticks aloft with her feet cast every way, she exactly represents a painted starre, as if nature had appointed, not only to make it round like the heavens, but with rays like the stars ..... the skin of it is so soft, smooth, polished and neat, that she preceeds the softest skin'd Mayds, and the daintiest and most beautifull strumpets .... she hath fingers that the most gallant Virgins desire to have theirs like to them ... has eight feet, which number is next to the most perfect number, as all men know. Their legs are also made in a sesquitertiall proportion, which is the most admirable and venerable'. [1]

He commented extensively on the virtuous family life of spiders, their monogamy and devotion in bringing up 300 children 'to labour, sparingness, discipline and weaving, and love them all alike'. He distinguished several species, but did not give a systematic description of them, and none of them can be recognised. (Savory [2] equates one iluustrated species with Araneus diadematus, but as Moufet

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1. Moufet chapter 13.

2. Savory 1961 p. 36.

says this a wolf spider which does not spin a web, either Savory's identification or Moufet's description is wrong). His illustrations vary from passable sketches to mere characatures, one of which represents the body as having distinct head, thorax and abdomen; this is in keeping with his characterization of spiders as animals with

'A little head and body small,
With slender feet and very tall,
Belly great, and from thence come all
The webs it spins'.

While this work cannot be compared with Lister's own as a piece of serious natural history, it is not on the grounds of its factual content that Lister attacks Moufet, but rather on the latter's integrity. He is

'reluctant to reveal how many names, how many helpers, have contributed: Whotton, Gesner, Clusius, Penny, Knivett, Brugaerius, etc. .. Really, he has put together his whole theatre in such confusion, and without order, and it is not obvious what he has derived from others. I praise him least of many men: not only is he ignorant of almost everything, he sets out everything so truly barbarically. We could forgive that to an incompetent but well-intentioned man, if there were not something else against him, something which I am careful to note when I am reading authors, so that I can understand if their souls are honest and pure. But from his writings I have found something quite different, which I cannot admire. All will judge me to speak correctly when I say that on the natural history of small animals, he has taken almost all his words from Aldrovandus, yet nowhere does he (and who else of our generation?) mention that most ingenious man. Aldrovandus made it all clear thirty years before his work was published .... it is plain that Moufet has read everyone except Goedart'. [1]

This attack is quite unjustified, as Moufet made plain the fact thet the work was not basically his own. The title page of the Latin edition gave the true authors as Penny, Gesner, and Whotton, making clear Moufet's subsiduary role; and, though this title page is missing from the English edition, the preface stated clearly that Moufet had only 'disposed, ordered and added to them the light of oratory' the fragments of Penny, Gesner and Whotton, according to his abilities, 'which I know how small they are'. His own additions were only 'trivial matters'.

The most charitable explanation of Lister's attack would be that it was based on a superficial reading of the English edition which did not extend to the Preface. Ray himself reproved Lister in this 'You are too hostile to Moufet; I think he deserves something from the world of letters, and most learned men agree with me in this'. [2] A further indication of Lister's hasty and uncritical approach at this period is that, in the same letter in which he attacked Moufet, he commended as 'most elegant .... marvellously plain' the work of Whotton, upon which Moufet's book was mainly based.

Athanasius Kircher (1602-80) devoted a single folio page to spiders in his Mundus Subterraneous [3] in which he described the construction of the web, and also made an important point about the

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1. CJR p. 11 (in Latin).

2. Ibid. p. 24.

3. Athanasius Kircher, Mundus subterraneus in XII libres digestes 2 (Amsterdam, 1664) p.368.

use of the male palps in copulation; this is dealt with below. Although Lister made no reference to Kircher in his work on spiders, he mentioned the book when discussing spontaneous generation, [1] and we may be sure that he knew of this reference to the use of the palps.

Francesco Redi (1621-97)[2] discussed spiders mainly from their relevance to the spontaneous generation controversy, but included some observations on feeding and spinning, the method of slinging webs between trees, and their infestation by ichneumons. Lister made frequent reference to this work.

Henry Power (1623-68) [3] was one of the first to use a compound microscope in the investigation of spiders. He noticed that some species have eight eyes and some six; but he thought that others had four, and that the number was in proportion to the bulk of the body and to the length of legs. Diagrams of the eye arrangement of ~~the~~ two species were given; these will be dealt with below. He mentioned three species of true spider and one harvestman. [4] Lister does not appear to have had any contact with Power, who died in 1668, when Lister was beginning the serious study of Natural History in England. It is surprising that Lister nowhere mentions Power's books.

Robert Hooke (1635-1703) gave very good plates and a full description of a harvestman [5] in which he stressed the peculiar eye arrangement. Lister quoted this at length. Hooke also gave a description, sent to him by John Evelyn, of the hunting behaviour of an Italian hunting spider; [6] this repeats the idea that the adults

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1. CJR p. 11.

2. Francesco Redi, Esperienze intorno alla generazione degli insetti (Florence, 1668). References here are to Latin translation (Amsterdam, 1671).

3. Henry Power, Experimental Philosophy, in three books, containing new Experiments, Microscopical, Mercurial, Magnetical (London, 1664).

4. Power pp. 11-12.
5. R. Hooke, Micrographia (London, 1665) p. 198.
6. Ibid. p. 202.

teach their offspring how to hunt, and Evelyn added a description of how the adult ran away and hid in a crevice to hide its shame if it missed its prey in a jump. Hooke added a few general notes on silk and eye arrangement.

The History of Insects of Jan Swammerdam (1637-80) [1] included an account of spiders. He looked on the adults as nymphal forms which, unlike other and higher forms of insects, do not metamorphose into winged forms. He pointed out that the 'teeth' could better be regarded as modified legs. He describes three kinds of spider: *Aranea telarius* or *muscatrix*, *Aranea pulex* or *lupus*, and *Aranea longipes*. These are respectively web spiders, wolf or jumping spiders, and harvestmen. He also mentions seeing plates of thirty-five species of spider by Jacob Hoefnagel; these do not seem to have been published. Lister nowhere mentions Swammerdam's book, though he had seen Swammerdam's treatise on respiration. This latter was written in Latin however, and the History of Insects was available only in Dutch until 1682.

(2) The Development of Lister's interest in spiders.

In the preface to his Historia Animalium Angliae of 1678, Lister states that he began to describe the insects of these islands ten years ago, spending 'not hours, or a few days, but ... many months' in Lincolnshire, studying these 'minute beasts'. This has been taken [2] to mean that Lister spent a single season in the study of spiders, but in fact it appears that his interest must have extended over at least ten years, though gradually declining in this period.

It is unlikely that his interest in spiders had reached the stage of meking detailed observations while he was studying medicine

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1. J. Swammerdam, Historia insectorum generalis ofte Algemeene verhandeling van de bloedeloose dierkens (Utrecht, 1669). All references here are to the French translation, Histoire generale des Insectes (Utrecht, 1682).

2. Raven 1942 p. 140.

at Montpellier; otherwise he would not need to rely 'on other people's evidence' [1] for the information that some of our spiders are common in Europe. There are no other references to countries other than England in any of his writings on spiders, apart from quotations from other authors.

Several of the species of spiders whose distributions were given in 1678 are recorded from Cambridgeshire, one (Titulus XXXIII) specifically from St. John's College. These records probably date from the period of Lister's residence at St. John's as a Fellow (1666-1669). He was in the habit of spending a good deal of his time at Burwell, Lincolnshire, during this period, and several other species are recorded from this county. Lister moved to York late in 1670, so that his first list of English spiders, [2] dated 10 January 1670-1, must have been compiled almost entirely from his work in Cambridgeshire and Lincolnshire. In his early years at York, however, he appears to have continued this work enthusiastically, and added 5 new species to this list, 3 of them in 1670. [3] There are more specific references to Yorkshire in his book of 1678 than to anywhere else. Most of these are not dated to the year (though Lister is careful to give the month of his observations), but there is a single record dated as 'end of August, 1676'. [4] It would seem however, that his interest in this branch of natural history was fading by this time. In his correspondence with John Ray, there are 6 references to spiders in 1668, 3 in 1669, 10 in 1670, 3 in 1671, one in 1672, and none thereafter, though there were frequent letters between the two until 1676. There is a casual reference to the strength of spider silk in a paper of 1684; [5] but in the preface to his English translation

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1. HAA preface, p. (iv).
2. Phil. Trans. 6 (1671) p. 2171.
3. CJR p. 65.
4. HAA p. 63.
5. Phil. Trans. 14 (1684) p. 592.

of Goedart's Insects (published at York in 1682, but body of the text completed in 1675), Lister says that the 'notions I once had of these things, being less fresh in my memory, I had little leisure to instruct myself again'. Though he is here referring to 'insects' in general, and not to spiders in particular, we may take it that by this time Lister was devoting himself to the study of molluscs, geology and antiquities. His work of 1678, with a few minor corrections appended to the Latin edition of Goedart (1685), represents the sum of his work on spiders over the decade 1666- 1676 or thereabouts.

The localities mentioned in the spider section of Historiae Animalium Angliae give an indication of the geographical extent of his activity:

Locality	No. of species recorded
South England (not further specified)	9
Cambridge (shire)	6
Hartford	1
North England (not further specified)	2
Lincolnshire	3
Yorkshire	20
(York 9, Craven 5, Acomb 2, Doncaster 1, Teesmouth 1, Scarborough 1, Filey 1)	

It must be remembered, though , that most of the species in the book are given as being common and widely distributed, without specific localities being mentioned.

Lister's work on spiders appears to have been carried out in almost complete isolation. Few other men were at that time interested in these animals; even John Ray, who received, with apparent interest, frequent accounts from Lister of the latter's work, could write:

'I have made little enquiry and contemplation of spiders, partly because other studies and affairs have not allowed it, and partly because the fear of poison makes these animals almost untouchable, even to me, who has from youth discarded vulgar prejudices; but from these I still feel abhorrence'.[1]

There was, in 1669, a suggestion, apparently on the initiative of Francis Willughby, that he and Lister should co-operate on a History of Spiders; Lister wrote on 22 December of that year to Ray: 'I have transcribed the tables, that he may for the future join with me and assist me in the prosecution of my design ... '.[2] As already mentioned, Lister and Willughby were in direct contact during Lister's stay in Nottingham an 1670, and this may have been a subject for discussion. However, even though, in the same letter to Ray, Lister says that Willughby 'may freely command my papers at any time . .... if you think fit, make him a present of it in my name, or otherwise make use of it' [i,e. Lister's table of spiders], it must be doubted whether Lister's temperament would have allowed such co-operation to succeed; in any case, Willughby's early death prevented the project from making progress.

Lister's jealousy and keen sense of proprietary rights in his own discoveries brought him into conflict with the only other naturalist to publish new work on spiders at this time, Dr. Hulse. An account of this controversy is given below.

Lister's work on spiders can be found in the following letters and publications:

(1) His correspondence with John Ray contains many references to spiders; as already mentioned, these all fall in the period 1668-1672.

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1. CJR p.29.

2. Ibid. p.48.

(2) Four papers in the Philosophical Transactions of the Royal Society; three of these are concerned with the shooting of threads and the aeronatical habit, [1] and the fourth classifying and enumerating the species of spiders found in England. To this is added a list of queries covering a wide range of topics concerning spiders. [2] These papers date from 1669 to 1671.

(3) His major work, the book Historia Animalium Angliae Tres Tractatus, published in 1678.

(4) A few additions and corrections, of a very minor nature, appended to Lister's edition of Goedart's De Insectis of 1685.

The letters to Ray contain little factual information on spiders; the references are mainly comments on Lister's progress in the compilation of his list of English spiders, which was to form the basis of his book of 1678. This list passed back and forth between the two men several times for perusal by Ray and correction by Lister. Three of the papers published in the Philosophical Transactions were originally written to Ray, who sent them on to Oldenburgh.

The first three of these papers, dealing with the production of gossamer, are treated below. The fourth shows that Lister's interests in spiders did not change between the date of this paper (1671) and that of his main book (1678). He raised 23 queries, all concerning matters which were to be extensively treated in 1678. These queries concern: the number of species of spiders in England, and the best way of classifying them (query 1); the generation of spiders - spontaneous, sexual, the function of the male palps, life histories (2 to 9); attacks by ichneumons (10); feeding methods (11-13); moulting (14); thread production and spinning (15 to 20, and 23); and their usefulness in medicine (21-22) and the possibility of extracting

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1. Phil. Trans. 4 (1669) p. 1001; 5 (1670) p. 2104; 14 (1684) p. 592. The latter was written in 1670.

2. Ibid. 6 (1671) p. 2171.

a dye from them (9). This list shows that Lister's interests were already what would now be broadly called ecological, and that his approach was in this way a great advance on that of his immediate predecessors, though his utilitarian interests were more typical of his time. His list of spider species was also in almost its final form by 1671. It then contained 29 species of true spiders , 3 of harvestmen, and one mite. This was expanded by 5 new true spiders by 1678, and there were some alterations to the system of classification.

Lister's first major work, and the only one to deal with spiders, was the Historia Animalium Angliae of 1678. This was a development of the list sent to John Ray ten years previously, and several stages in this development can be traced. The first mention of such a list occurs in a letter from Ray to Lister of 31 October 1668, [1] in which Ray acknowledges the receipt of a table of 30 species, observed and named by Lister. He comments with '... wonder at the art and industry used, that you have been able to observe so many distinct species in so short a time and in so restricted an area'.

This list appears to have been revised almost annually for several years after this. On 12 December 1669, Lister wrote to Ray [2] to say that Willughby had asked for his (Lister's) spider list, which is, however, 'yet imperfect'. This second version contained 31 species - a number queried by Willughby as being far too low. [3] By the end of 1670 three more species were added, and some reclassification was done:

> 'I have this last month writ over a new copy of my History of Spiders (which is the fourth since I put my notes in any order) and inserted therein all the last summer's

1. CJR p.29.
2. Ibid. p. 48.
3. Ibid. p. 60.

observations and experiments. I find only two or three new spiders, and one to be removed into another tribe, to which it more properly belongs'.[1]

It seems that the list was now being thought of as a book. Two years later,[2] Lister wrote to Ray that he had made similar tables of the land and fresh water snails, with an appendix on shell stones; these he had added to the table of spiders. With the addition of the marine shells, this would complete the contents of the book of 1678. Though there were minor additions to the spider section, and though Lister mentions[3] that he has gone through his notes 'last winter' (i.e. 1677-8), it is likely that the spider work was substantially complete by the end of 1672.

The work was offered to Ray in 1676, as a contribution to Ray's and Willughby's History of Insects,[4] but Ray, in what was the final letter of the extensive correspondence between the two,[5] advised Lister not to bury his work in Willughby's, which in any case depended upon Ray's life and health for their chances of ever being completed and published; but Lister should make sure that his own work was complete. The book was published two years later by John Martyn, printer to the Royal Society, though it did not carry the imprimatur of the President of the Society, as did Lister's later works.

(3) Lister's methods and approach to Arachnology.

Lister was not entirely free from the feeling of revulsion from spiders current at that time even among some learned men. Writing of a species of crab spider, he mentioned his abhorrence of the strange form and gait of this pernicious beast, though he knew it to be harmless;[6] and where he suspected the spider may have a harmful bite,

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1. Ibid. p. 73.
2. Ibid. p. 99.
3. HAA preface p. (i).
4. CJR p. 124.
5. Ibid. p. 125.
6. HAA p. 85.

he was careful not to run the risk of being bitten.[1] However, his interest in the animals was strong enough to lead him to a great deal of detailed, personal observation; he was almost always highly critical of recognised authorities, and includes very little information indeed which is not derived from his own work. Considering the extremely poor development of the study of spiders at this date, this cautious attitude towards authority would have been almost unavoidable to anyone prepared to make any serious observation in the field. Lister's book was the first on these animals to be based on this kind of work; indeed, it was to be almost a century before any really serious attention was paid to spiders again, and then without the same stress on field work which is found in Lister.

As mentioned above, Lister was critical of the standard works of Aristotle and Moufet; he was also unwilling to accept unquestioningly the evidence of his contemporaries. Writing on Hulse's claim that he has seen forked and branched silk threads produced by spiders, Lister says

> ' ... far be it from me that I should slight any phenomenon of nature, for I am the gladdest man that can be to hear of any; but I am very cautious not to forwardly entertain such upon trust; neither would I have any man take them from me otherwise as they themselves should find them'.[2]

Though authorities may be fallible, he claims that sensory evidence cannot deceive or produce error.[3] His attitude was commended by Ray: 'You have taken the right course and method, that is, to see with your own eyes, not relying lazily upon the dictates of any master but yourself ... '.[4]

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1. Ibid. p. 27.

2. CJR p. 78.

3. HAA preface p. (ii).

4. CJR p. 13.

This personal observation was directed principally towards the life histories and habits of spiders, rather than to their structure. Lister makes it quite clear that he regards these as the really significant points: ' ... we should pick out, not that which is superficial (extrinsecam) or arbitrary, but make clear their essential nature (propriis earum naturis)'.[1] Again he tells the reader that he has stressed the 'enumeration of the spider kind (genus) and species, and what seem to me to be the distinctive features of their life histories (vitae rationes prae caeteris insigne mihi visum).[2] Indeed, the classification of the animals is, in Lister's view, inseparable from their life history: ' ... I like the making of genuses and tribes ex moribus et vita, though I would not, as near as may be, have the form excluded'.[3] It should be noted that this stress on habits and life history does not mean that Lister was an indifferent anatomist at this period. He was in fact quite perceptive, at least so far as the external features are concerned. His accounts of the arrangement of the eyes in different groups of spiders are an example of this.

Lister's observations were careful and detailed; for example, in discussing the feeding methods in spiders, he points out that exoskeletal remains of insects cannot be found in spider excrement, as would be expected if the hard parts were eaten, and not only the juices of the prey.[4] In order to examine the arrangement of the eyes in a species in which they were very small, he burned away the hairs on the head before using a lens.[5] This kind of detailed attention occurs all through his work on spiders, perhaps most noticeably when

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1. HAA preface p. (iii).
2. Ibid. preface p. (i).
3. CJR p. 73.
4. HAA p. 12.
5. Ibid. p. 55.

dealing with reproduction and life history. Here we find Lister carrying out systematic observations over an extended period of time, covering such points as the number, size, shape and colour of the eggs, the structure of the egg-case, the type of silk from which it is constructed, the hatching period, the appearance of the young, the frequency of moulting, maturation period, and place of hibernation. Most of this work was necessarily done in the field, and Lister's accounts contain many references to the kind of vegetation and general habitat in which his species are found, and descriptions of courtship, spinning and feeding behaviour in field conditions. Lister also kept spiders in captivity, for example in order to obtain eggs which he has been unable to find in the field [1] and to make experimental observations on the diet and method of feeding of captive spiders. [2]

His dissection of these small animals was much less rewarding than his ecological work, but he does appear to have made dissections frequently. For example, he mentioned having dissected more than twenty females of one species in October in an (unsuccessful) attempt to find developing eggs. [3] He does not appear to have used a microscope to any great effect in his dissection; his observations on internal anatomy do not give any detail which could not be seen with the naked eye. His descriptions of eye-arrangements sometimes mention the use of a microscope (microscopium, or often conspicullum [4] and occasionally vitrum). [5] This would probably be a simple lens. There is no evidence that he was at this stage using a compound instrument.

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1. Ibid. p. 27.
2. Ibid. p. 69.
3. Ibid. p. 26.
4. Ibid. p. 33.
5. Ibid. p. 25.

Lister appears to have made collections of specimens in spirits, as he commented that one species fades from green to a yellowish colour when so preserved, though other species do not change much in pattern and colour. [1] This technique was only a few years old at the time. [2]

(4) Spider anatomy.

In general, Lister's description of the anatomy of spiders is good, at least insofar as the external features are concerned, though he did not include microscopical details such as the structure of the palpal organs or the epigyne, which play such an important part in modern spider morphology. He appears constantly to have borne in mind the relationship between structure and function; for example, when discussing the different ratios of the lengths of the pairs of legs in different groups of spiders, he asked: 'Why this variety? I say, in order to take the flies and other insects necessary for food, each in its own natural way'. [3] On internal anatomy, Lister was much weaker, and made only a token attempt at describing the abdominal viscera: 'What is the true configuration of the internal organs, and a correct explanation of them, their very small size prohibits'; [4] 'As for the other viscera, which are in the chest, not even a hint of their place can be given'. [5]

Lister's treatment of anatomical structures concerned with reproduction and spinning, and those which he uses in classification (i.e. eyes and chelicerae) will be considered below. His anatomical terminology is dealt with in the summary list below.

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1. Ibid. p. 36.
2. F. J. Cole, A History of Comparative Anatomy (London, 1953) p. 274.
3. HAA p.3.
4. Ibid. p. 3.
5. Ibid. p. 4.

Terminology.

(i) Divisions of the body.

Lister did not give a distinctive name to the cephalothorax, though he pointed out that the head is not separated from the thorax; he distinguished between the octonocular spiders, with a distinct abdomen, and the binoular spiders (harvestmen) with an integral body (corpus integrum).[1]

Head - caput (p. 2); front of head - frons (p. 24).

Thorax - humeres (p. 2); under surface of, - pectus (p. 2).

Abdomen - alvus (p. 3):

" anterior parts - clunes (p. 24).

" under surface - venter (p. 25).

" region round tip - anus (p. 3)

" central peak of, - summes clunium (p. 23).

'Skin', exoskeleton - cutis (p. 3).

" " when being cast during moult - senectus (10).

(ii) Appendages.

Palps - antennae sive cornicula (p. 2).

Male palpal organs - ' ... quibusdem capitulis sive nodis' (p. 2).

Chelicerae - Telum (p. 1).

" in harvestmen - brachium forcipatum (p. 1).

Eyes - oculi (p. 2), ocelli (p. 24).

Legs - pedes (p. 2);

" below first joint - crus (p. 2).

Joints in legs - ' ... quibusdam juncturis constant' (p. 2).

Segments of leg - internodia (p. 3).

Spinnerets - ani appedices (p. 3); fistulae (p. 3).

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1. Ibid. p. 3.

(iii) Internal organs.

Uterus - uterus (p. 3).

Compartments of uterus - cellula (p. 3).

Ovary - folliculum (p. 3).

Intestine - intestina (p. 3).

Silk gland - fili conceptaculum (4).

Lister appears also to have noticed the lung-book openings, at least in one species, though he did not name them, nor did he offer any suggestion as to their function: 'Belly ... with two crescent-shaped (lunatis) marks, yellowish, with the horns facing inwards'. [1] Similarly, his description of what must be the epigyne in another species is left unexplained: 'on the belly is a crowded circle of wrinkles (crebrae rugae) ...'. [2]

(5) The production of silk.

Probably the most conspicuous characteristic of spiders is their ability to produce silken threads. Lister regarded this as a constant characteristic of spiders of both sexes, all ages and all species of spider, at least so far as the octonocular group is concerned. He was not certain that the binocular spiders (harvestmen) can spin; he had a suspicion that they may do so during reproduction, but ' ... it is a great argument against this, that no injury will provoke these binoculars to produce a thread, as happens with all the octonoculars. [3]

Spinners in spiders has attracted attention from the earliest times. Aristotle [4] described the production of silk as part of the hunting behaviour; he thought of the silk as being drawn from the

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1. Ibid. p. 36.
2. Ibid. p. 84.
3. Ibid. p. 6.
4. Aristotle, Historia animalium 623 b 28.

outer covering of the animal's body, like the bark of a tree or the quills of a porcupine. In doing so, he contradicted Democritus, whom he quoted as saying that silk is an excretion from the internal organs. Aristotle's rather unlikely theory was not accepted by any of the other major writers on spiders. Even Pliny said that the threads are produced in the 'uterus'; [1] Moufet wrote that 'they carry the matter of their webs in their belly'; [2] and Redi agreed with both. [3]

Lister found Aristotle's idea rather puzzling, and was apparently a little doubtful at first that he had been correctly interpreted; although the text may seem 'plain in itself, yet it will not easily enter into our imagination'; [4] 'This is so obscure that many learned interpreters have not been able to follow him'. [5] He referred the point to Ray, asking him to look at the Greek text and give his interpretation, [6] but in 1678 still had to content himself with saying that 'these words are very obscure .... but this is the way in which the greatest of philosophers is seen to explain the emission of threads'. [7] Lister himself flatly contradicted Aristotle's theory; the silk threads could hardly be external and remain attached at the anus.

The abdominal silk glands were found in some species, though not surprisingly, little anatomical detail was made out in these complex structures. Lister pointed out only that the gland is paired (duplex) in some species. [8] He gave no details of the spinnerets

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1. Pliny, Historia naturalis book 11 chapter 24.
2. Moufet ch. 13.
3. Redi p. 170.
4. CJR p. 65.
5. HAA p. 6.
6. CJR p. 82.
7. HAA p. 7.
8. Ibid. p. 4.

beyond mentioning their apparently tubular structure, and using their length as a factor in classification.

Lister appeared to consider that the silk is forced out under pressure from the glands. He uses the word ejaculatione in Latin, and 'darting' or 'shooting' in English. However, he believed that at times the thread may be drawn out passively, 'by hand (manu), forcibly and unwillingly'. [1] He also thought that emitted threads could be drawn back inside the animal, by a power 'not dissimilar to that which the lungs of blooded animals have, which by a reciprocal motion draw in and expel breath (spiritus)'. [2] It was considered that this made it likely that the silk was present in the silk glands as a conglomeration of ready-formed threads, rather than as a viscous mass, even though it had been produced originally from a special body juice (succus corporis singularem). Lister tried to investigate the properties of silk, finding that it was insoluble, even in boiling water, and that if heated, it would not burn, but melted into a glutinous mass. He was the first to recognize that there are several types of silk, differing in colour and texture as well as in thickness. [3] In several species, he pointed out the different uses of the various types of thread; for example, in Titulus VI, he mentioned that the eggs are covered by threads which are pale yellow, stiff, and rough, quite different from those forming the nest, which are light, soft and white, as if they were made by different parts of the body. He noticed that the spinnerets could produce several threads at the same time, [4] and that these threads may be of different types. He speculated as to whether these different threads are produced by different glands, [5]

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1. Ibid. p. 7.
2. Ibid. p. 8.
3. Ibid. p. 9.
4. Ibid. p. 3.
5. Ibid. p. 39.

or by different parts of the same duct, or that perhaps some types may be produced by the uterus.

He did not agree with Hulse's finding that forked threads are sometimes produced; Lister pointed out that the production of several threads together may produca an illusion of a single branched thread.[1] This point formed part of the controversy over the question of priority of the discovery of the nature of gossamer, dealt with below.

The construction of the orb-web.

The orb-web of spiders, as seen in its maximum development in the Argiopidae, has always excited the interest of naturalists, who have often credited the weaver of such a web with a high degree of intelligence, industry and even morality. However, there was little systematic observation of the way in which these webs are constructed, or of the variety to be found in their structure, before Lister's work. Aristotle noted that not all species of spider are web-spinners, and that the web is spun in the order perimeter first, then radials or warp, and lastly the woof. He knew that some species watch from above the web and some from below, but not of any structural variation in the webs.[2] Pliny added nothing of value to this.

According to Moufet, the spider first 'draws semidiameters to places circumambiant, then with no compass but by a natural skill of her feet, she makes 44 circles with her thread from centre to circumference, by equal parts more distant one from the other'. The web finished, 'they smear it over with a bird limey glutinous spittle'. The only variation in plan noted is that the most ignoble spiders have a very coarse web, spun from a grosser thread. He believed spinning

1. CJR p. 78.

2. Aristotle, Historia animalium 622 b 28.

to be an art learned from the parent, and which is improved by practice as the animal grows. [1]

Kircher compared the web to an astrolabe, crediting the animal with great skill in setting it out accurately. Like Moufet, he thinks that the net is made sticky after construction by the application of an adhesive corpus mollis to existing threads. [2]

Lister corrected Moufet's statement that spiders are taught by their parents to weave and pointed out that the web of any species has the very same (ipsissimo) form and structure at any age of the animal. [3] He also rejected Kircher's idea of a carefully orientated structure, though not mentioning him by name. He points out that the web may face east-west or north-south, and that, although the plane of the web is fairly constant for any species, this may be either approximately vertical or approximately horizontal. Further, the web may be laid down from left to right (i.e. clockwise), or from right to left according to the species. [4]

A fairly full account of the method of construction of the orb-web is given. [5] From the description of the hub and the number of radii and turns of the spiral, this appears to have been taken from a species of Meta rather than of Araneus, but Lister stated that the process is almost the same in all orb-web spiders, by which he meant the Argiopidae. A frame is first run around a space which the animal has a mind to occupy (quod is est in anima occupare). These single threads (stamina) are then strengthened by having other threads bound to them to form a thick, compound cable (funes crassiculos). The first radii, each being a simple thread (stamina simpliciora) are laid down, the

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1. Moufet ch. 13.
2. Kircher 2 p. 369.
3. HAA pp. 9-10.
4. Ibid. p. 35.
5. Ibid. pp. 22-3.

centre being marked by a fluffy, woolly (lanuginis) mass of tangled threads. The other radii are then laid down in all directions until the correct number is reached. The stabilizing zone, consisting of about four turns or ranks (ordines) of a spiral , is then laid down around the centre: Lister noted that in one species (Titulus I) this thread is viscid. The temporary spiral, normally of naked (prorsus) threads but occasionally viscid, is next added as the spider spirals out towards the circumference. The permanent, viscid spiral is covered with drops of gum, and is therefore described as being made up of ranks of spotted threads (macularum ordinibus). This is attached to the radii as the spider moves in again towards the centre, but is discontinued before the stabilizing zone is reached, leaving a free zone. Finally, the central mass (floccus) is torn out, and the spider takes up its position in this opening (vestibulum sive foramen patulum), holding a radius with each limb, which are evenly spread, pulling the radii together or holding them apart.

As detail points, Lister pointed out that the number of radii and turns of the spiral never exeed thirty (this is normally so in Meta, but other genera may slightly exeed this number), that the ranks of the spiral are equidistant, and that the radii are evenly spaced. He noted that in weaving the thread is pulled out from the abdomen 'by itself', though the last pair of limbs are used for manipulating the thread, and not for walking during this operation. He was uncertain as to how the threads are fixed to each other; though he noticed the tip of the abdomen making movements, he doubted that they can be knotted, even though a strong pull cannot separate them. He suspects that the spider makes use of the form (sigillo) of the thread when it is newly spun and still fluid. This is in conflict with his earlier suggestion that the thread is preformed in the solid state while still in the silk glands.

There is nothing to compare with this account in earlier authors, though the only technique needed in its compilation is patient watching at dawn, or, in some species, at dusk. Lister's account is comparable with modern descriptions: the only difference being in the early stages of the making of the frame and the fate of the temporary spiral.

He made no mention of the 'first fork' in the construction of the frame, unlike most modern authors. However, some recent authors make no mention of this feature,[1] or specifically deny that it is of constant occurrence.[2] Savory, who accepts its occurrence at least in some species, says it is very difficult to observe.[3] Lister's omission therefore appears understandable.

He did not mention that the temporary, non-viscid spiral is removed at the time the permanent, viscid thread is being laid down.

The repair of broken webs was noted, though Lister was wrong in claiming that this is done with material taken from other parts of the web. It does not seem very likely that by this he means the removal of the temporary spiral; although this is eaten, and might therefore be made re-available, this is done when the web is still being constructed, and not when it is in need of repair.

Lister paid some attention to the differences between the structure of the orb-webs of different species. The stabilmentum of the web of Cyclosa (Titulus IV) is described, without being named, and the habit of this spider in fixing the remains of its prey 'ostentatiously' to the band passing through the centre of the web. He also described and illustrated the web of Zygiella (Titulus X), in which the turns of the viscid spiral are incomplete over one sector, leaving a single radius

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1. Eg., C. Warburton, Spiders (Cambridge, 1912) ch. 3.

2. Petrusewiczowa, Praec. Towarz. przy. nauk. Wilna, 13 (1938), pp. 1-24 (quoted in T. H. Savory, The Spider's Web (London 1952) p. 104).

3. Savory 1952 p. 76.

free from the spiral. Orb-web spiders were divided into two groups (I to V, and VI to X) according to whether or not the spider watches from the centre of the web or from a refuge near the web and connected to it by means of a signal thread, which is described. In Titulus I, however, he mentioned that a spider may sometimes occupy one position, sometimes the other.

Other types of web.

Although Aristotle mentions that some spiders spin only a poor web, [1] Lister was the first writer to pay any real attention to webs other than the fully developed orb-web of the Argiopidae. He made what is apparently the first attempt to illustrate sheet-webs, and claims that 'they surely have beauty, even with their rude and haphazard construction! [2]

Two basic types of non-orb web were distinguished:

(a) the conglobate web (reticula conglobata), a structure formed (according to Lister) from a single thread and extended equally in all three dimensions; this is now called a scaffold web, and is produced by the Therediidae; [3]

(b) the sheet-web or cob-web, running mainly in two dimensions to form a sheet-like structure. These are further sub-divided into three types, beginning with what he apparently regarded as the typical or complete cobweb of the Agelenidae, eg., Titulus XVIII. This large, dense web was described as having a canopy of a few threads fixed vertically or horizontally above the main web, so that

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1. Aristotle, Historia animalium 622 b 28.

2. HAA p. 49.

3. Ibid. p. 48.

flies colliding with them will fall down into this. There is also a refuge tube at the centre of the web (fundus or infundibulum) in which the spider waits for its prey.

The second type of cobweb is characterized by being much more open and tenuous. Here Lister paid too much attention to the closeness of the texture of the web, as compared with the basic structure; he includes as spiders making this type of web two species (XIX and XX), one of which (XIX) makes a refuge tube and would have been much better placed as a weaver of the first type of cobweb; it is in fact an Agelenid, as are the other spiders making webs of the first type. Titulus XX is a Linyphid, making a web with a canopy but no refuge tube, such as is now called a hammock-web.

The third type of cobweb is simpler still, consisting only of a refuge tube and a few irregular threads leading from it. This is described as being made by species XXI to XXIV inclusive; these are spiders of the families Ciniflonidae (Dictynidae), Segestriidae, and Clubionidae. (Lister further separates these, but not on the basis of web structure).

In modern evolutionary terms, the tube web (type 3) is primitively simple, the cobweb of type 1 is a more complex development of this, and the hammock web of type 2 is a further, simplified, development of type 1. To Lister, of course, these two kinds of simplicity were not distinguishable in kind, but only in degree, so that his arrangement of them differs from ours.

The manuscript of Lister's spider classification survives.[1] Later additions to this, in Lister's hand, translate the three main web-forms as wheel-nets, bushy nets and sheet-nets.

(6) Gossamer.

In the early mornings of autumn, after a night of heavy dew,

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1. MSS Lister 5 f. 99.

large quantities of gossamer threads are seen covering the grass and other vegetation. The origin of this had been a mystery since ancient times.

Although Bristowe [1] considers that the word 'gossamer' is related to 'goose-down' rather than to 'gauze', the other favoured derivation, most early references to it relate it to weather conditions, in particular to dew, mist, and clouds. As we now know, spiders only produce gossamer on fine, clear days, such as are likely to be followed by a cold night which will produce a heavy dew; and the gossamer threads are so fine that they are in any case invisible without a coat of dew. They were therefore often thought of as being a solid form of dew. Its mystery, fineness, and romantic association, made it a popular subject with poets. Chaucer [2] tells us

' Sore wondren some on cause of thunder,
On ebb and floud, on gossamer and on mist'.

The connection with dew is seen in Spenser: [3]

' More subtle web Arachne cannot spin,
Nor the fine net which oft we woven see,
Of scorched deaw do not in th'ayre more lightly flee';

and Robert Herrick [4] speaks of dawn as being the time when

' ..... the light
hangs on the dew-locks of the night'.

Even Thompson, writing a generation after Lister's work, could write

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1. W. S. Bristowe, 'Spider superstitions and folk-lore'. Transactions of the Connecticut Academy of Arts and Sciences 36 (1945) p. 65.

2. Chaucer, Squire's Tale 1. 251.

3. According to Warburton ch 3. and Bristowe, W. S., The World of Spiders (London, 1958) p. 7. I have been unable to trace the reference, and this usage is not listed in the glossaries to Spencer of Morris (1907) or Smith and Selincourt (1912), though both give other examples of 'deaw'.

4. Robert Herrick, Corinna's gone a'Maying in The Hesperides (1648).

of gossamer as 'Filmy threads of deaw evaporates' [1] Quarles [2] refers to cobwebs in this connection, but it is not clear whether this implies a real relationship between the two, or whether it is purely metaphorical:

' And now autumnal dews are seen
To cobweb every green.'

A link between clouds and webs is seen in Drayton: [3]

' Thin clouds like scarfs of cobweb lawne'
- which chequer the sky.

This apparent connection between cobwebs, clouds and mists and dew could also arouse philosophical interest; a few years before Lister's work, Robert Hooke wrote:

' Much resembling a Cobweb, or a confus'd lock of these Cylinders, is a certain white substance which, after a Fogg, may be observ'd to fly up and down in the Air; catching several of these, and examining them with my Microscope, I found them to be much of the same form, looking most like to a flake of Worsted prepar'd to be spun, though by what means they should be generated, or produc'd, is not easily imagined: they were the same weight, or very little heavier than the Air; and 'tis not unlikely, but that those great white clouds, that appear all the Summer time, may be of the same substance'. [4]

Oldenburg believed that webs were made not only by spiders, as they could be found in the middle of winter, in 'uliginous' (i.e. swampy) fields, when there were no spiders about.

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1. Thompson, Autumn (1709), 1121-2.
2. Quarles as quoted by Bristowe 1958 p. 7.
3. Drayton, From the Sixth Nymphal, Muses Elizium, 1630.
4. Hooke 1665 p. 202.

In an age when most gentlemen were country gentlemen, the solving of the gossamer problem might be expected to bring a good deal of public recognition; and as Lister's discovery came very early in his career in the world of learning, it is perhaps understandable that he should have been sensitive about the issue over priority in the discovery which developed between him and Dr. Hulse. This produced some tension between Lister and Ray, as the latter was in the difficult position of having received letters from both protagonists describing their discoveries; Hulse's was the earlier, though his account was much less complete than Lister's.

About the middle of 1666, Ray received a letter from Hulse on the emission of threads by spiders; knowing little about these animals himself, he passed the information on to Lister in a letter written (in Latin) on 31 October 1668. The delay of over two years is explained by the fact that this was the first letter between the two in which spiders had been mentioned. Ray writes that a learned friend has communicated to him that some spiders, in spinning, do not draw out (extrahunt et eliciant) their threads so much as protrude (protrudent) and almost project (projiciant) them, to a considerable distance, forward or to the side, and not only downwards. Ray asks Lister's opinion as to how this can be, as the thread is not stiff, but soft. [1]

Lister's reply was written on 22 November 1668. He said that he has noticed the projection of threads, and that this can be seen in the flying of spiderlings, 'which I was the first to see'. He could have shown Ray this phenomenon last September. Lister had noticed, on a clear day, the large number of fine threads which he could feel in the air; he did not connect these with spiders until a

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1. CJR p. 29.

few days later, when, while watching a spider, he saw it turn tail to the wind and squirt out thread, 'like a robust child emptying a full urinary bladder'. Several yards of thread were let out, and the spider was eventually carried off into the air. Lister claimed to have followed this up with laborious observation, in which he has noticed that the spider will roll up its thread while in flight or emit new ones.[1] (He did not mention that this was the mechanism whereby the spiderling can rise or fall while in flight, though he knew that this change of level could occur. The first explanation of the way in which this is done was given by Gilbert White, who mentioned Lister's work on this subject).[2]

The contents of Lister's letter were passed by Ray to Oldenburg, and published in the Philosophical Transactions on 16 August 1669,[3] though there are some additions which must have been communicated by Lister in a further letter which has not been preserved. These additions include an explanation of the tangled masses of threads which litter the grass, i.e. gossamer. This comes from the threads which spiders emit and then break off 'not having a mind to sail'. It is noticed that it is the wind which produces the lift, and that large spiders will hunt while flying, but only in summer. He has noticed the shooting of threads even at Christmas, but only by the young of that Autumn.

Following this, Hulse wrote to Ray again to amplify his previous letter.[4] He described the laying down of a line attached to the surface on which a spider is walking, and the flight of spiders; but he was doubtful as to whether the wind can provide the motive

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1. Ibid. p. 31.

2. Gilbert White, The Natural History of Selborne (London, 1788); letter 23 to Barrington.

3. Phil. Trans. 4 (1669) p. 1001.

4. CJR p. 57.

force, as he had seen spiders flying four times as fast as the air seems to be moving. He appears to have been more concerned with the actual process of ejecting the thread than with flight, and he explained the bridging of wide gaps, as between trees or across a room, by this apparent shooting action. He described the threads themselves as being sometimes forked.

Ray sent this letter to Oldenburg, and it seems that Oldenburg asked for a clarication on the matter of priority. The letter was published on 11 July 1670, [1] with a note by Ray explaining that

> 'concerning the matter of spiders projecting their threads, I received the following account from Dr. Hulse, from whom (to do him right), I must acknowledge, I had the first notice of this particular which was not long after communicated to me by another ingenious friend whose letter I formerly sent you to be imparted to the Royal Society'.

A note by Lister was appended, saying that he had no knowledge of the original letter from Hulse to Ray until he had sent a list of spiders to Ray. Ray had then asked Lister for his comments on Hulse's letter, and then to prepare his notes for the Royal Society. Oldenburg added a further note, concluding that the observation was both Hulse's and Lister's, but that Hulse had first communicated it to Ray.

Lister was apparently not satisfied with this, and must have written very promptly to Ray on the matter, for only six days after the publication of this issue of the Philosophical Transactions, Ray wrote to *L*

> ' deprecate your displeasure for publishing to the world that Dr. Hulse was the man from whom I had the first notice of spiders projecting their threads. The observation is yours as well as his, and neither is beholden to the other (that I know) for any hint of it, only he had the hap

1. Phil. Trans. 5 (1670) p. 2103.

> to make it first, and being questioned about it, I could do no less than own the first discovery of it to me to be from him, who indeed communicated it to me as soon as I saw him, immediately after my return from beyond the sea.' [1]

Lister appears to have been temporarily subdued by this, though he wrote directly to Oldenburg, giving a further full account of his observations. This letter, written on 23 December 1670, was never published by Oldenburg; it appeared in the Philosophical Transactions in 1684. [2] Other friendly letters were exchanged by Lister and Ray, in one of which Lister adds further details on the flight of spiders. He had noticed that even when he climbed to the top of the highest steeple of York Minster, he could still see spiders ascending above him until they disappeared from sight. As he found these were wolf-spiders (lupi) which do not enter houses or other buildings, they could not be supposed to have taken their flight from the steeple itself. [3]

A little later, however, he suggested that Hulse was mistaken in supposing that spider threads were sometimes forked; the shooting of several threads at once could give this impression. [4] A month later he took up the subject again. Referring to Aristotle, he wrote that, on the flight of spiders,

> 'I find the ancients were silent, and I think I was the first who acquainted you with it; but that is best known to yourself, and I challenge it only by way of emulation, not envy, there being nothing more likely than that several persons following the same studies may many of them light upon one and the same observation. I am no Arcana man, and methinks I would have everybody free and

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1. CJR, p. 61-2.
2. Phil. Trans 14 (1684) p. 592.
3. CJR p.76.
4. Ibid. p. 78.

communicative, that we may if possible, considering the shortness of our lives, participate with posterity'. [1]

In spite of Lister's ostentatious denial of purely self-interested motives, Ray appears to have decided to try to maintain an amicable relationship by conciliating Lister's vanity. In reply to this letter, he wrote

> 'The flying of spiders through the air is, for aught I know, your discovery; from you I had the first intimation and knowledge of it. Dr. Hulse acquainted me with no more than the shooting out of their threads. I would not be so injurious to any man, especially to so esteemed a friend, as to rob him of any part of the reward of his ingenious endevours and transfer to another what is due to him, though it be as much commendation to find out a thing by one's own industry, which hath been already discovered by another, as to invent it first; this last being rather a happiness than anything else, though I know the world will hardly be induced to believe that two men should hit upon the same discovery at the same time'. [2]

This was the last reference to the matter in the correspondence between the two; but Ray believed, more than thirty years later, that Lister continued to hold this affair against him, and that this was the cause of the eventual cessation of the correspondence between them. As has been shown earlier, this does not seem to be altogether likely.

Lister continued to claim priority in this discovery. [3] To do him justice, the letter to him from Ray of 31 October 1668, in

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1. Ibid. p. 82.

2. Ibid. p. 129.

3. HAA p. 6.

which Hulse's discoveries were mentioned, makes no reference to the flight of spiders, and Hulse's later letter of 28 June 1670 gives much less detail than Lister's. Even so, the affair does reveal something of Lister's personality.

In any case, the subject was not entirely new. It is possible that Aristotle was referring to the flight of spiderlings when he wrote that ' ... when it is fully developed, the young spider makes a sudden leap and begins to spin its web'. [1] Although they do not mention the flight of spiders, Kircher, [2] Redi, [3] and Swammerdam [4] all refer to the shooting of silk threads into the air, and the drifting of these in the breeze until they touch an object some distance away. This explains how a spider's web may be slung across a stream or road, or between trees. Redi also suggests that the spider itself may be blown to one side while dropping on a thread, or that it could climb down a tree, along the ground, and up another tree, pulling in or giving out its thread while doing so.

In his Historia animalium Angliae, Lister repeats his earlier work, and also indicates which species he has detected as taking flight. These are tituli XIX (a Linyphid), XXIX (a crab spider) and all the wolf spiders (Lycosids). He noticed that although the larger spiderlings, such as titulus IX, produce threads, they do not appear to use them for sailing. Although we now know that most species show this habit, the overwhelming majority (eighty to ninety percent or more) are Linyphids; the wolf and crab spiders have been shown to make up most of the rest. [5]

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1. Aristotle Historia animalium 555 b 4.
2. Kircher 2 p. 368.
3. Redi p. 176-178.
4. Swammerdam 1669 p. 66.
5. W. S. Bristowe, The Comity of Spiders 1 (London, 1939) pp.190, 196.

(7) Food and food capture.

Some easily observable aspects of feeding in spiders had been described by previous authors. Aristotle [1] gives 'flies' as the normal diet, and noticed that only the body juices of the prey are used, the husk being left. He is impressed by the ability of spiders to withstand prolonged fasts. The difference between the hunting methods of wolf spiders and phalangids (jumping spiders) is noticed, and also the variation in the watching position of web spiders.

Moufet gives little information on hunting spiders. One kind 'goes as it were rebounding, like a flea or an ape'; another 'goes horridly with its head always nodding and a heavy belly ... it hath a sting in the top of its neck ..'. [2] The different watching positions of web spiders are said to be differences between the two sexes of the same species. When the prey is caught, the spider does not wound it, but proceeds to 'kiss and tickle it, and smear him with a clammy net'. [3]

Robert Hooke had given a description, sent to him by John Evelyn, of the hunting behaviour of an Italian Salticid spider. This was quite detailed, but Evelyn thought that the adult spiders instructed their young, and he describes how an adult which has missed its prey would run into a cranny and hide, as if ashamed. [4] Lister refers to this account.

Redi [5] made detailed investigation into the ability of spiders, both adult and newly hatched, to withstand starvation, but he gave

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1. Aristotle, Historia animalium 488 a 16, 594 a 12.
2. Moufet ch, 12.
3. Ibid. ch. 13.
4. Hooke 1665 p. pp. 200-202.
5. Redi pp. 159-166.

no other information on feeding habits.

Lister made some attempt to be more specific about the type of prey; he carried out experiments to find which animals could be killed and eaten by spiders, and sometimes noted the kinds of insects to be found in the webs of particular species. [1] He noted the capture and eating of other spiders, of the same or other species, and of spider eggs, both when the animals are confined without other food and when they are free to choose other food. Large and pernicious insects such as bees, wasps and horse-flies (tabani) are given as prey for some species, but he was sceptical about the claims of the ancients that spiders could overcome lizards and snakes, denying that it occurs in England. [2] He claimed that different species of spiders specialize in different kinds of prey. He pointed out that webs tend to contain one kind of prey more than others, and believed this shows that the spider has spun its web where its particular prey is abundant. He does not consider that the opposite may be true. [3]

Lister repeated experiments similar to those of Redi to determine the length of fast a spider could sustain, finding this to be about six months. He believed this power to be necessary to allow the animals to survive the winter, as they do not store food, and neither do they, according to Lister, hibernate. [4]

Some account is given of the way in which the web is used for hunting. As it is usually vertical, insects will fly into it, but horizontal hammock nets have a canopy of verical threads with which insects will collide and then fall on to the net below. This is

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1. HAA pp. 69, 37, 62.
2. Ibid. pp. 32, 35, 12, 11.
3. Ibid. p. 11.
4. Ibid. p. 12.

found in Tituli XVIII and XIX (Agelena labyrinthica and Linyphia sp.). Spiders with orb webs either watch from the centre (Tituli 1-V) or from a nearby refuge (VI-X). Those watching from the centre hold a radius with each leg, except III, (Tetragnatha extensa) in which the first four legs are extended straight forwards from the animal. This is in fact a characteristic of this species. The typical posture of Linyphids, e.g. XIX, which watch from below the web, is also noticed. In those spiders watching from a refuge, the signal thread-ladder is described. Lister noticed that the spider was attracted by the vibration of the web, and that they could be called out by throwing sand on the web.[1] Less detail is given on the hunting methods of wolf spiders. It is stressed that these spiders can produce silk, and that some of them do this to fly and catch prey in the air.[2] The jumping of Salticids is described, but Lister wrongly thinks that the thickened first legs of these spiders provides the power for this;[3] in fact the last pair of legs is used.

The poisonous bite of spiders had always attracted a great deal of attention; of the fifteen pages which Moufet gives on spiders, almost a third are devoted to the effect of their venomous bite and its cure. It was generally thought that only certain species of hunting spiders were venomous; these were called phalangids by Aristotle[4] and this name was in general use, though Lister used it only for jumping spiders. The modern usage of the name is quite different; it is now used to mean Lister's binocular spiders, the harvestmen.

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1. Ibid. p. 29.
2. Phil. Trans. 4 (1669) p. 1001.
3. HAA p. 87.
4. Aristotle, Historia animalium 553 a 4, 555 a and b.

Lister believed that most groups of spiders produce venom. He quoted Harvey's experiment, [1] in which Harvey pricked himself with a needle which had been rubbed against a spider's teeth. This produced an inflammation not found when the experiment was repeated with a clean needle. Lister himself was less daring, and describes how ' ... when I provoked one of them [a web spider], drops of a liquid like pure lymph were quickly and copiously produced. It then wanted to bite something. With the threat of death close at hand, I took great care against this danger, not being willing to run risks, so that if this liquid is poisonous, and to what extent, I am so far incompetent to say'. [2]

This caution is in spite of his statement that cold countries such as England do not contain dangerous spiders; also, he had earlier written to Oldenburg that he had 'handled with my bare hands many 100 of most sorts that I name, yet I never received the least harm'. [3]

On the tarantula, which received so much attention from other writers of the period, Lister is sceptical: 'Its effect, and the method of curing it, are rejected by learned natives as pure figments'. However, there is nothing intrinsically impossible in the stories, in Lister's view; men bitten by rabid dogs try to bark like dogs, so there would be nothing to wonder at if people bitten by a jumping spider began to jump. In that case, it is possible that the exitement of music and dancing might cause the sufferer to sweat so much ~~much~~ as to become cured. Against this, Lister doubted

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1. William Harvey, De generatione animalium (London, 1657) ch. 57.
2. HAA p. 27.
3. Royal Society MSS L 5, f. 24.

that the tarantula is a jumping spider. [1]

(8) Reproduction and life history.

Aristotle gave a good deal of attention to reproduction in spiders. [2] He noticed that there were differences between different species in such things as the appearance of the cacoon and in the maternal care shown, but he made several errors. His account of copulation is not clear; he considered the eggs to be round grubs which metamorphose into fully developed spiders; he said that both parents brood over the eggs, and that, in phalangids at least, both of them are killed and eaten by their offspring. These mistakes were copied in detail by Moufet, who also included an outright statement on spontaneous generation in these animals: 'It is manifest that spiders are bread of some aeriall seeds putrified from filth and corruption', giving as proof the fact that spiders and cobwebs can be found in new houses as soon as they are whitewashed. [3] Aristotle neither accepted nor rejected spontaneous generation in spiders, though he does give these as an example of insects which are generated by ovoviviparity as a result of copulation.

Kircher, as an advocate of spontaneous generation, believed that spiders, like many other insects, are generated by the vis seminalis in dust, earth and water in addition to the sexual method. [4] Redi contradicted this idea as part of his argument against spontaneous generation from inorganic material:

> 'That spiders are generated by aerial seeds coming through the air, or from dirty putrifactions, I cannot believe, without proof being brought forward other than the usual ones, such as that spiders and their webs are to be found

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1. HAA p. 15-16.
2. Aristotle, Historia animalium 553 a 4, 555 a and b.
3. Moufet ch. 16.
4. Kircher 2 p. 369.

in newly limed houses. It would be a marvel if spider's nests were not brought into the house with the lime, dust and dirt; these hatch just when the walls are being finished and build their webs'. Redi also rejected Mathiolus' idea that spiders are produced from plant galls, and that when this occurs it is to be taken as an omen for a contagious and pestilential year. Redi pointed out that small spiders can only be found in perforated galls, and may therefore be assumed to have entered the gall after its formation. [1]

Lister did not give much attention to either of these supposed methods of generation; the two ideas are, however, listed in his table of false traditions, ancient and modern. [2] He did not accept Moufet's moralizing account of the family life of spiders. In his description, the male is only present 'for the sake of copulation, not in order to take care of the eggs (nidificandi) or to feed the young, and he leaves immediately after mating'. [3] He notes, however, that in titulus XVII (Tegenaria domestica) the two sexes remain together and even feed in the same web. Locket and Millidge give this as a characteristic feature of this species. [4]

Lister gave a considerable amount of information about the courtship of these animals, all of which is new, and not all of which has been repeated by modern observers. Among the examples which are confirmed by present-day authorities may be mentioned: in titulus I (Meta segmentata), Lister described the gathering of several males at the periphery of a female's web, which is now known to be characteristic

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1. Redi pp. 171-174.

2. HAA p. 17.

3. Ibid. p. 4.

4. G. H. Locket and A. F. Millidge, British Spiders 2 (London, 1953) p. 7.

of this genus; he described (in titulus XIII, Theridion sisyphium) the plucking of the web by which male web-spiders make the initial communication with the female; and in titulus XXV (Lycosa pullata), which is a wolf spider and so cannot use web vibrations, he mentioned the use of the palps as signals in courting:

> 'In early May, on a river bank, I have watched the males courting the females (mares foeminas ad venerem exitantes). They follow the females with their palps (antennae) vibrating, while the females keep them away'.

These points are confirmed by Bristowe.[1] Lister also noted that in titulus IX (Araneus umbraticus) courtship appears to take place at night. In titulus I (Meta segmentata) a description of a fight between two males was given:

> 'Two males were fighting strenuously, their heads held together by their jaws (mordicus), and their legs tangled. In order to separate them, I took them from the web, where they were together with the female, into my hand. They separated themselves, but the instant they were released, they began to fight again'.

This type of bloodless combat between males is well known now as a modification of normal courting behaviour between the two sexes.

The process of copulation in spiders is peculiar because of the use of the modified first appendages of the male to transfer sperm to the female; there is no equivelant of a penis. Lister described the terminal swelling of the palps as a characteristic of the male in eight-eyed spiders, especially marked in Salticids. (He noticed that binocular spiders have a penis which is protruded when the abdomen is pressed).[2] The function of this organ is believed to be to 'make

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1. W. S. Bristowe, The Comity of Spiders 2 (London, 1941) pp. 469-473.
2. HAA pp. 2, 69.

soothing caresses ... each is a penis, or that from which a penis is protruded ... and used alternately in coitus.'[1] Lister observed the mating process in several species and gave fairly full descriptions for tituli III and XIII (Tetragnatha extensa and Theridion sisyphium). That for Tetragnatha may be compared with a modern description:

Lister: 'Both are suspended under the web by threads, their undersides pressed together, copulating; the male below, his abdomen extended straight out, the female's abdomen curved over the male's, the anus touching the upper part of the male's abdomen. I could not see a penis, except for the palps, which I saw continually and alternately applied to the forepart of the female's belly (ventris superiori)';

Bristowe:[2] ' .... they now drop on threads until they are both hanging vertically and belly to belly with her abdomen contorted in a curve so that it touches his. From this position the male inserts his palps alternately for about quarter of an hour and then rapidly disengages ...'.

Lister does not, however, notice the interlocking jaws characteristic of this species. He did not observe the process of sperm induction in the male of any species of spider.

Lister is generally credited [3] with the discovery of the sexual nature of the palps in the male. However, this had been outlined

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1. Ibid. p. 2.

2. Bristowe 1958 pp. 254-5.

3. eg. J. Blackwall, The Spiders of Great Britain and Ireland (London, 1860-64) p. 1; Bristowe, 1941 p. 461, Savory 1961 p. 42 is wrong in stating that Lister did not notice the sexual function of the palps.

by Kircher in a work which we know Lister had read. [1] Kircher wrote that there is no true copulation in spiders, but that there is a caressing with the sexual (geneticus) limbs, which are alternately applied to the abdomen. This is thought to exite a seminal force in the viscous excrement which is 'communicated' after titillation in something like a swollen bubble. This is obscure. Kircher cannot be referring to transference of sperm from male to female, as this cannot be seen. As the 'communication' takes place after the titillation, and as Kircher does not make clear the roles of the two sexes, he may be referring to the process of sperm induction, in which the male recharges his palpal organs from a special sperm-web, though this does not involve the female. Lister must have obtained a useful lead from this, though it is clear that he would in any case have come across the process during his own observations.

For over a century and a half, authorities could not agree on this matter. Although Swammerdam mentioned the use of the palps in his Bible of Nature (he had not mentioned it in the History of Insects of 1669), Leeuwenhoek rejected this idea in 1702. De Geer correctly described their use in 1778, but Cuvier denied their sexual function. [2] It was not until Menge's work in 1843 that the point was fully and finally established. Blackwall summed up the position in 1860:

'Taking anatomy as his guide, Treviranus arrived at the conclusion that the parts in question are used for the purpose of excitation merely, preparatory to the actual union of the sexes by

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1. Kircher 2 p. 369; CJR p. 11.

2. J. Swammerdam, Bybel der Natur of Historie der Insecten 1 (Leyden, 1737) p. 51; Phil. Trans. 23 (1702) pp. 1137-55; C. de Geer, Memoire à Servir à l'Histoire des Insectes 7 (Stockholm, 1778) pp. 179-180; G. Cuvier, La Règne Animale (Paris, 1817). English translation (used here) by Griffith (London, 1833) 13 p. 393.

means of the appropriate organs situated near the anterior part of the inferior region of the abdomen. This view of the subject, which is very generally adopted, is opposed to that derived from physiological facts by Dr. Lister and the earlier systematic writers on arachnology, who regarded the palpal organs as strictly sexual, and recent researches, conducted with the utmost caution, have clearly established the accuracy of the opinion advanced by our distinguished countryman'. [1]

Lister gave a great deal of specific information on the life histories of the species covered. Much of this has not been repeated, and so cannot be checked with modern authorities. He dissected females at different seasons to observe the maturation of the eggs. The date at which the eggs are laid, the number produced, whether they are loose or stuck together, their size, shape and colour, and the colour of their contents are all noted for many of the species. The cocoons (folliculi) are often described: the type of thread, its colour and texture, the closeness of its weaving. In one case this is confirmed by a modern authority. Lister described the eggs of the titulus XII (Theridion ovatum) as being enclosed in a loose, sky-blue thread. Locket and Millidge describe this similarly. [2] The place in which the cocoon is deposited is described: in the web, close by, under stones or tree bark. The carrying of the cocoon by wolf-spiders is dealt with as a characteristic of this group, and Lister observed that a female of titulus XXVI (Trochosa ruricola) would pick up her cocoon again after it had been taken away from her. He realized that the eggs are not incubated, spiders being cold to the touch, and that only the female guards them. Though he gives 21 days as a typical time for the eggs to reach hatching point, he

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1. Blackwall preface, p. (i).

2. Locket and Millidge 1953 p. 76.

realized that there is much variation, and that in some species the eggs laid in autumn do not hatch until the following spring. The carrying and feeding of young wolf-spiders by the mother is described; feeding of the young by her was noticed also in titulus VII (Araneus sclopetarius). Recent authors do not include this uncommon spider or others of the same genus among those in which feeding by the mother has been seen. He denies Moufet's statement that the young are instructed by the mother in spinning or hunting. For several species in which the young stages differ in appearance from the adult, descriptions of the spiderlings are given. In at least one case, titulus I (Meta segmentata) Lister obtained cocoons of eggs and reared the spiders to maturity in order to establish the identity of this type of cocoon and these spiderlings. The best account of the reproduction and life history of one species is that for titulus VI (Araneus cornutus); this occupies more than two pages.

Lister correctly estimated that most spiders are a year old when they reach maturity. During this year, they moult several times, at intervals determined by the rate of growth, the first moult being a few days after hatching. Moufet's idea of regular monthly moults was rejected. Lister devoted a chapter (IV) to moulting, which is stated to include the whole body, even including the teeth. It commences with a split at the chest. He believed all the cuticles which are cast during the spider's life to be present beneath the first when the spider hatches.

For several species, the winter retreat is described - in houses, hollow plant stems, tree bark, special winter webs, etc. He regarded spiders as resting during this time, and not as being torpid, as they respond immediately to disturbance. The long winter fast kills off the great majority of the spiders.

(9) Ecology.

There is almost no sign of an ecological approach to spiders in the writers prior to Lister; Lister's own attitude was that of an enquiring field naturalist. He was concerned to study the animals in their natural environment in a way which was new so far as arthropods were concerned, but without the same attention to the systematic collection of detail which is often seen in his accounts of life-histories. He made some attempt to indicate the geographical distribution of each species, though as his collecting was largely confined to Cambridge, Lincolnshire and Yorkshire, his data for doing this are limited. He often distinguishes between highland and lowland distribution; his apparently frequent visits to Craven while resident at York would give him opportunity to compare two such regions.

The habitat of most species is given in some detail; eg. titulus VI (Araneus cornutus): 'On moors (ericetis) in mountainous regions in the North of England, fairly common; sometimes in gorse in the South of England, and in herbs such as Acanthus and Stephalinus. Often by small streams'. Locket and Millidge [1] give this species as being widely distributed, especially common near water, spinning retreats in reed heads. Lister's other descriptions of range and habitat are similarly in general too restrictive, representing the range of Lister's collecting rather than the range of the spider. Many of the species are given as being found on gorse and heaths near York, reflecting Lister's main collecting ground and also the changes in the landscape which have occurred since the seventeenth century.

The season in which the adult is active was usually given; eg. titulus I (Meta segmentata): ' Very common from early May to the

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1. Ibid. p. 136.

middle of October, sometimes later'. Locket and Millidge say of this species :'Adult in late summer and autumn; some specimens occur in spring, possibly after hibernation as adults'. [1] Lister had noted that this species winters in the egg stage, nearly all the adults dying, and only a few emaciated individuals surviving until spring.

Some species were described as being nocturnal; titulus X (Zygiella atrica) is said to hide in its refuge by day, and titulus IX (Araneus umbratious) as being torpid and immobile all day. Lister tried to feed this species in captivity; by day, the animal did not respond, but food left in the container overnight had been killed and eaten by morning. When free, it would not emerge from its refuge by day, even when a fly struck its net. Locket and Millidge say: 'Rarely appears in daylight, but remains hidden underneath loose bark of trees or posts until dusk'. [2]

The only large predators referred to are birds, which are said to devour them in large numbers. As with early authors, Lister was interested in the parasitism of spiders by ichneumons, and he gave long quotations from Bellon and Goedart on the subject; the Bellon quotation was taken from Aldrovandus. [3] In addition to this second-hand evidence, he pointed out that ichneumons attack the eggs of spiders, and not only the adults. He had published this discovery in 1671 in a letter to the Royal Society; [4] this was in reply to a query from Willughby. [5] He did not, however, notice that ichneumons of different families are involved. He stressed that the

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1. Ibid. p. 117.

2. Ibid. p. 139.

3. HAA pp. 11-14.

4. Phil Trans 6 (1671). p. 2284.

5. Ibid. p. 2279.

wasps develop from eggs laid by other wasps, and that this was not an example of spontaneous generation.

In two species of 'binocular spiders', (XXXV and XXXVI), Lister noticed infestation by the small red ectoparasitic mite Ritteria ('coccineis cimicibus'). This had already been described by Power.[1]

(10) Systematic account.

(a) Taxonomy and nomenclature.

Lister's species names were typical of seventeenth century taxonomy, consisting of short descriptive phrases. These were sometimes very short, as with titulus XXV or Araneus niger (now Lycosa saccata), but usually from ten to fifteen words in length. These are clearly intended to be true names, though it has been claimed that Lister's numbering of the species was intended as a substitute for naming them.[2] Lister's names are given below.

Although these species were arranged in groups which were in turn arranged under larger headings, these groupings were not given rank names, such as genus or family, and there was no implication that different groups can be compared in rank with one another. The word genus was often used, but its scope was not fixed. It was used to cover any grouping of species which have in common some feature under discussion, and, as used by Lister, is best translated as 'kind'. The word was used to include all spiders (Araneorum genus) at one point; to include only web spiders at another; and, in the plural, to include the different kinds of hunting spider.[3] In the preface to the book, it is used as a synonym of 'species': 'I do not want anyone to think I have described all the genera'.

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1. Power 1664 p. 19.
2. Savory 1961 p. 41.
3. HAA preface p. (i); pp. 25, 76.

Lister claimed to have been careful in naming only true species, not multiplying them unnecessarily, and not confusing varieties with species. [1] Most of his descriptions allow his species to be identified with confidence, and the remainder with probability; none is so poor as to be hopeless. The titulus numbers were listed by Blackwall as synonyms in his species accounts. [2] Most of these identifications appear to be correct; some would seem to be wrong, and a few of Lister's species were not named by Blackwall. The list following gives Lister's 1678 titulus numbers and names, their modern names, [3] and their numbers in the list of 1670. Identifications not in agreement with Blackwall are asterisked and commented on below.

<u>Table of Lister's species of spiders together with their modern names.</u>

1678 titulus	1678 name.	Modern name.	1670 titulus
I	<u>Araneus subflavus</u>, alvo praecipue in summe sui parte et circa latera albicante, plena; oculis nigris pellucidis in capite albicante	Meta segmentata (Clerck)	I
II	<u>A. rufus</u>, sive avellaneus, cruciger, cui utrinque ad superiorem alvi partem quasi singula tubercula eminent	Araneus diadematus Clerck	II
III	<u>A. ex viridi</u> inauratus, alvo longiuscula, praetenui	Tetragnatha extensa (L)	VI
IV	<u>A. cinereus</u>, sylvaticus, alvo in mucronem fastigiata, seu triqueta.	Cyclosa conica (Pallas).	VII

1. Ibid. preface p. (2); p. 88.
2. Blackwall, passim.
3. In accordance with Locket and Millidge, 1951-3.

1678 titulus	1678 name	Modern name.	1670 titulus
V	A. viridis, cauda nigris punctus utrinque, ad marginem superne notata, ipso ano croceo.	Araneus cucurbitinus Clerck (Araniella cucurbitina; Larinioides cornutus)	VIII
VI	A. cinereus, alvo admodum plena, ejusque pictura in plures partes quasi divulsa.	*Araneus sclopetarius Clerck	III
VII	A. pullus, glaber, cruciger, alvo plena ovali	Singa hamata (Clerck) (see note below)	IX
VIII	A. flavus, quatuor insignibus maculis albis, aliisque multis exiguis ejusdem coloris in pictura clunium foliacea notatus.	Araneus quadratus Clerck.	IV
IX	A. nigricans, capite quadrato sive phalangio formi, clunibus ad similitudinem querni folii depictis.	Araneus (Nuctenia) umbraticus Clerck	V
X	A. cinereus, capite leviter rotundo, pictura clunium foliacea, ad margines undata.	Zygiella atrica (Koch)	-
XI	A. pullus, glaber, domesticus	Steatoda bipunctata (L)	XII
XII	A. albicans, corona coccinea in alvo ovale.	Theridion (Enoplognatha) ovatum (Clerck)	-
XIII	A. fere subfuscus, interdum varie coloratus, alvo foliaceapictura insigniter, globata.	Theridion sisyphium (Clerck)	X
XIV	(below)		
XV	A. cinereus, e minimis, macula nigra in summis clunibus insignitis.	Dictyna arundinacea (L)	XIII
XIV	A. rufus, clunium globatorum fastigio, in modum stellae radiato, sylvicola.		

1678 titulus	1678 name	Modern name	1670 titulus
XVI	A. pusillus, lividus, pictura clunium nigra et veluti denticulata.	Dictyna latens (Fabricius)	-
XVII	A. subflavus, hirsutus, praelongis pedibus, domesticus.	Tegenaria domestica (Clerck).	XIV
XVIII	A. cinereus, maximus, ani appendicibus insigniter prominentibus.	Agelena labyrinthica (Clerck)	XVIII
XIX	A. niger, aut castaneus, glaber, clunibus summo candore interstinctus,	*Linyphia peltata Wider	XIX
XX	A. fuligineus, et humorem fastigio et clunium pictura candida, ad margines denticulata.	Tetrix denticulata (Olivier)	XVI
XXI	A. nigricans, praegrandi macula nigra in summis clunibus, caeterum iisdem imis oblique virgatis.	Ciniflo fenestralis (Stroem).	XV
XXII	A. cinereus, mollis, sive lanuginosus, cui in alvo oblique virgata macula latiuscula nigricans.	Clubiona corticalis (Clerck).	XX
XXIII	A. plerunque lividis, non raro tamen subflavus, sine ulla pictura.	*Drassodes lapidosus (Walkenaer)	XXI
XXIV	A. subflavus, alvo quasi cylindracea maculis quadratis insignita; item cui alvi latera singulae oblique virgulae flavescentes.	Segestria senoculata (L)	XVII
XXV	A. niger	Lycosa pullata (Clerck).	XXVI
XXVI	A. fuscus, alvo oblique virgata	Trochosa ruricola (Degeer).	XXV

1678 titulus	1678 name	Modern name	1670 titulus
XXVII	A. flavus unicolor, alvo productiori, accuminata.	*Dolomedes fimbriatus (Clerck)	-
XXVIII	A. sublividus, alvo undatim picta, productiori, accuminata.	Pisaura mirabilis (Clerck)	XXIV
XXIX	A. subfuscus, minutissimis oculis e viola purpurascentibus, tardipes, et gressu et figura cancero marino non adeo dissimilis.	*Xysticus cristatus (Clerck)	XXIII
XXX	A. parvus, subrufus velut inauratus, ipsa alvi apici infuscata, levipes.	*Tibellus oblongus (Walckenaer) [Philodromus cespitum Walck.]	XXII
XXXI	A. cinereus, alvo circiter senis fasciis transversis in angulos acutos in medio erectis, argenteis et nigris alternatim dispositis insignita.	Salticus scenicus (Clerck)	XXVII
XXXII	A. ex rufo subfuscus, super clunes praeter duas maculas albas, foliacea quadam pictura, obscure licet delineata insignitus sparsus.	Sitticus pubescens (Fabricius)	-
XXXIII	A. subflavus, oculis smaragdinis, item cui secundum clunes tres virgulae crocea.	*Euophrys frontalis (Walckenaer)	XXVIII
XXXIV	A. subrufus, ericetis, sive in rupibus degens.	*Evarcha sp. falcata (Clerck)	XIX

The binocular spiders (harvestmen) are dealt with separately below.

Comments on the identification of some of Lister's species.

Titulus VI. This is identified by Blackwall as Epeira apoclisa (now Araneus cornutus Clerck), which is a commoner species than the A. sclopetarius given here. However, Lister's illustration of this spider, which is one of the few which is accurate enough to provide useful information, clearly shows the white V on the cephalothorax which is very characteristic of this species.

Titulus VII. Identified by Blackwall as Epeira tubulosa, now Singa hamata (Clerck). Locket and Millidge give this species as being 'very rare; on heather, rushes and grass ...' while Lister gives this titulus as being common on rushes. Even so, a web spider with the short legs, oval abdomen and hairless body described by Lister can only be of this genus, and only this species has the conspicuous cross on the abdomen described by Lister. It may be that he had chanced upon a local colony of this species at York, or that this spider has become rarer since the seventeenth century, as some other species are known to have done.

Titulus XIX. Blackwall gives Linyphia marginata, (now L. montana (Clerck)), but Lister's description, especially of the abdomen, appears to fit L. peltata much better. Both species are common. As there are about 250 English species of Linyphid spiders, most of which are very similar, and as Lister only gives one of these, it is likely that this titulus is in fact a composite of several species.

Titulus XXIII. Blackwall does not list this in the synonomy of any species, but Lister's description of the eyes, proportions, colour and web, and the illustration, fit Drassodes lapidosus in every way. This is a common species.

Titulus XXVII. This is not identified by Blackwall. Lister gives the eye pattern as being the same as in titulus XXVIII, which is certainly Pisaura mirabilis. The only other English member of this family is Dolomedes fimbriatus, the largest English spider. Lister describes this titulus as 'certainly the largest of this kind (genus) of spider' and his illustration shows an animal of the same size and shape as D. fimbriatus. He describes the characteristic habit of this spider of running down a reed stem below the surface of the water in which the reed is growing. The only argument against this identification is that Lister gives the colour as being yellowish (subflavus) with a whitish (subalbida) stripe running longitudinally on each side. The colours of D. fimbriatus are brownish with a yellow longitudinal stripe on each side; but as Locket and Millidge say that the colours are less striking after preservation, and as the other features could hardly fit any other species, it seems safe to make this identification.

Titulus XXIX. Blackwall gives no identification for this. In most respects Xystious oristatus agrees with Lister's description, though this species is common over the whole country and Lister gives this titulus as being common only in the south.

Titulus XXX. Not identified by Blackwall. The eye pattern, colour, shape of the abdomen and active habit agree with Tibellus oblongatus. The illustration is poor, but can be more easily looked upon as showing this species with its slender abdomen exaggerated than as representing anything else.

Titulus XXXIII. Blackwall erected a new species for this on the basis of Lister's description (Salticus xanthogamma) but this has not been accepted by any other authority. It appears to match Euophrys frontalis fairly well. Lister's golden rays radiating from the eyes would seem to agree with Locket and Millidge's fringe of vivid orange hairs, and the eye arrangement and body colourations match; the emerald eyes which impressed Lister are not mentioned in modern accounts of this species, though, probably coincidentally, the metallic green sheen of the legs is.

Titulus XXXIV. Identified by Blackwall as Salticus coronatus (now Evarcha falcata (Clerck)). It seems certain that the spider belongs to this genus, but it is impossible to make Lister's description fit any of the known species.

Of these thirty-four species, tituli X, XII, XVI, XXVII, and XXXII were additions to the list of 1670. Titulus XXVII is local in England, but the others are widespread, XVI being not uncommon and the others abundant. That Lister had not sufficient experience of some of these to include them in his first list can only be because his interest was not of long standing at this date. In 1678, he described the first three as being common; XXXII he had found only at York. The swamp-spider, XXVII, was recorded from thickets and swamps in the south of England, and only rarely in the North. As Lister can have spent very little or no time in the South between publishing his list of 1670 and completing his final list by 1672, this must mean that he had some notes on the species by 1670, though he made no mention of them in his list of that year; and it has already been mentioned that it seems from his description of this spider that he used faded, preserved specimens. This would appear to confirm the point made below: that Lister did not publish all the material he had available, but only that which had been worked up into reasonably complete accounts.

The selection of species described shows that Lister gave his attention mainly to the large and more conspicuous types; the small Linyphid spiders form almost half our spider fauna, but only one of Lister's thirty-four species belongs to this family. On the other hand, the more spectacular orb-web spiders form only one thirteenth of our fauna, but one-third of Lister's list.

The small number of species has attracted comment from modern writers, especially in view of Lister's remark in his preface:

> 'I do not wish anyone to think I have described all the <u>genera</u> of these animals which we have ... nevertheless I can say boldly that no-one will discover by chance any new species not described by me'.

In spite of this remark, Lister was aware that there were other species in England. Ray had told him that Willughby had said that he could find double Lister's number, [1] and Lister himself mentioned [2] having seen several species of wolf spider which he does not describe for lack of leisure to investigate them further. Shortly before the passage quoted above from the preface, Lister mentioned having discarded a great deal of information. It seems probable that he had accumulated a quantity of notes from which he selected the material needed to present reasonably comprehensive accounts of individual species, and discarded odd points about other species which could not be fitted together into such accounts. It is difficult to believe that the large amount of detailed observation which Lister devoted to spiders for a period of five years did not bring him into contact with more than thirty-four species. On this interpretation, the operative words in the passage quoted would be 'by chance'.

(b) Classification.

As with other animals, Aristotle did not give a formal classification of spiders, but he distinguishes between web spiders, wolf spiders and jumping spiders or phalangids. [3] Moufet gave the following system: [4]

Harmless	-	house
	-	wolf (small and large)
Venomous	-	small, jumping
(phalangids)	-	large, hairy

Having more species to classify, Lister's system was more formal and

1. CJR p. 60.

2. HAA p. 82-3.

3. Aristotle, Historia animalium 622 b 28f.

4. Moufet ch. 11.

involved a variety of criteria. These include:

1. The method of obtaining food; this provides the basic division of eight-eyed spiders into web and hunting-spiders.
2. The structure of the web, if present.
3. The place in which the cocoon is found.
4. The method of locomotion.
5. The relative lengths of the pairs of legs.
6. The arrangement of the eyes.
7. The amount of hair on the body.
8. The length of the spinnerets.
9. The presence or absence of a leaf pattern on the abdomen.
10. The shape of the cephalothorax.

The fundamental divisions are not based on anatomical features, but upon the animals' ways of life. This is in accordance with Lister's ideals, as mentioned above, and in his account of titulus XIII:' ... it is of little importance to know the colours of spiders, or even the form of the body, until the other distinguishing features, such as feeding and reproduction, are known'.

The most constantly used of the purely anatomical features is the eye arrangement, which was carefully described in almost all species. Although Lister himself did not regard this as being so important as the feeding method of the animal, its importance to modern araneologists is so great that it is dealt with separately below.

Lister's taxonomic system is inseparable from the organization of his book. The species are grouped into chapters; as he dealt with more than twice as many web spiders as hunting spiders, the section dealing with them has the chapters grouped into three membra: those for orbweb spiders, for scaffold ('globe') web spiders, and cobweb spiders. Each membrum then contains two or more chapters. In the summary table at the beginning of the book, the first two membra are linked together as net-spiders, but this is not repeated in the body of the book. The hunting spiders are divided simply into wolf-, crab-, and jumping-spiders. As with the membra of the web spiders, each chapter here includes a list of characteristics of the included spiders, and it seems that these chapters are intended to represent a higher unit than those of the first section.

Lister's full classification is:

Section 1 Web spiders

Membrum 1. Orb-web spiders having (a) characteristic eye arrangement; (b) relative lengths of pairs of legs in the order 1:2:4:3; (c) almost glabrous 'skin'; abdomen with leaf pattern.

Chapter II - watch from centre of web, no nest. Tituli I-V.

Chapter III- watch from refuge, separate nest. Tituli VI-X.

Membrum 2. Globe-web spiders. (a) very small; (b) leg order 1:4:2:3; (c) eye pattern characteristic.

Chapter II - no cocoons formed. Tituli XI-XII.

Chapter III - with cocoons. Tituli XIII-XVI.

Membrum 3. Cob-web spiders. (a) long spinnerets; (b) characteristic eye pattern; (c) last four legs longer than first four; (d) hairy.

Chapter II - dense web, nest inside. Tituli XVII-XVIII.

Chapter III - thin web, nest nearby. Tituli XIX-XX.

Chapter IV - tube web, Titulus XXI.

Chapter V - reduced web, nest inside. Tituli XXII-XXIII.

Chapter VI - anomalous, six-eyed spider. Titulus XXIV.

Section 2 Hunting spiders.

Chapter II. Wolf spiders. (a) characteristic eye-pattern; (b) last legs much the longest; (c) carry their cocoons; (d) hunt on the ground. Tituli XXV-XXVIII.

Chapter III - Crab spiders, with last legs shortest. Tituli XIX-XXX.

Chapter IV - Jumping spiders with (a) leaping motion; (b) flat frons; (c) characteristic eye pattern; (d) long and thick first legs. Tituli XXXI-XXXIV.

There is also a second part dealing with binocular spiders. The first chapters of the three membra of section 1 and the first chapter of section 2 deal with the general features of those units.

Although this classification is based on web and/or way of life rather than anatomical features, Lister's system agrees fairly well with that of modern systematists. Of the thirteen modern families represented in his list, only three are partitioned, and one of these is a case in which Lister has not been consistent in applying his own principles.

The orb-web spiders include the two modern families Argiopidae and Tetragnathidae; until fairly recently, systematists have included the latter under the former family.

The globe-web spiders correspond to the Theridiiae and Dictynidae less Ciniflo. (This latter is one of the partitioned families, the web of Ciniflo being quite different from that of Dictyna.) These two families are not in fact closely related, but their webs are very similar. Savory says of them: ' .... once again the cribellate spiders provide us with a parallel, the web of Dictyna being almost exactly a pure Theridiid type, with cribellum silk on some of the superstructure'.[1] The eye patterns of the two families are fairly similar, both being simple with no special peculiarities. Some of the Theridiidae (Steatoda bipunctata and Theridion ovatum, tituli XI and XII) are separated from the rest as they do not enclose their eggs in a complete silken cocoon; this point is correct, though not now regarded as being of taxonomic importance.

The sheet- or cob-web spiders are a heterogenous group containing representatives of seven families. Even here, however, Lister's subdivisions usually correspond to family units. The last three chapters represent the genus Ciniflo from the family Dictynidae, (chapter 4), the two closely related families Clubionidae and Gnaphosidae (chapter 5) and the distinctively six-eyed family Dysderidae (chapter 6). These are all fairly primitive spiders. The first two sub-divisions contain the more specialized families Agelenidae and Linyphidae, but Lister divided the former family, putting titulus XX (Tetrix denticulata) into chapter 3 together with a Linyphid on the grounds that they both leave the cocoons outside their webs and have these webs of a very open texture. In doing this, he rejected the evidence of the basic structure of the web and of the eye arrangement, both of which should have shown him that titulus XX should have been grouped with chapter 2, which includes the other Agelenids. This is the worst error in Lister's system, as he here made poor use of his own declared criteria,

The wolf spiders include the two closely related families Lycosidae and Pisauridae; the crab spiders correspond to the Thomisidae; and the jumping spiders to the Salticidae.

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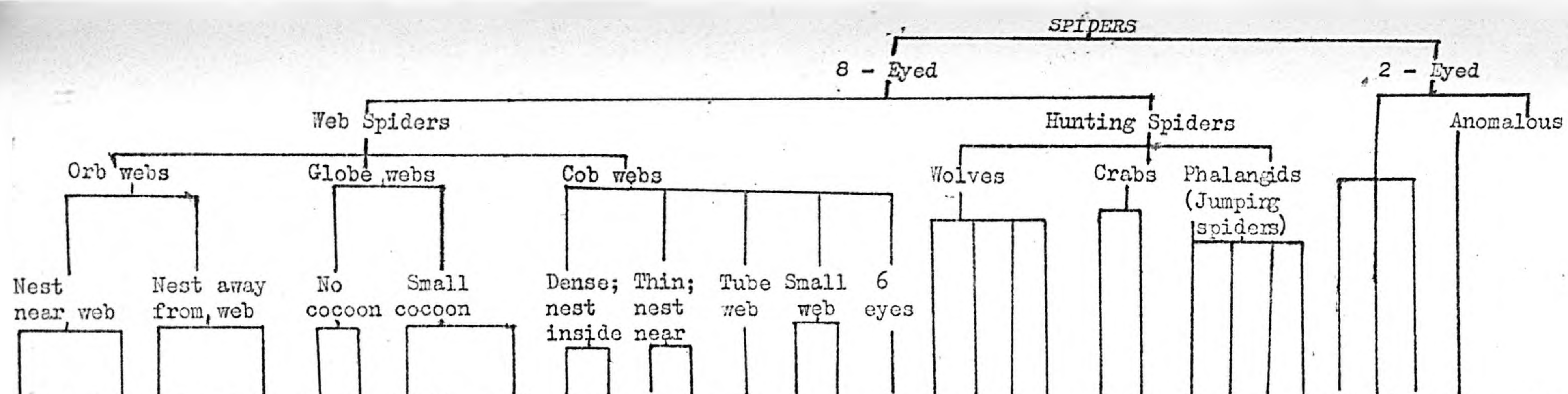
1. Savory 1952 pp. 123-4.



Tituli	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38
TETRAGNATHIDAE			*																																			
ARGIOPIDAE	*	*		*	*	*	*	*	*	*																												
THEREDIIDAE											*	*	*	*																								
LINYPHIIDAE																			*																			
AGELENIDAE																	*	*		*																		
PISAURIDAE																											*	*										
LYCOSIDAE																									*	*												
SALTICIDAE																															*	*	*	*				
CLUBIONIDAE																						*																
GNAPHOSIDAE																							*															
THOMISIDAE																													*	*								
DYSDERIDAE																								*														
DICTYNIDAE															*	*					*																	

Lister's earlier classification of 1670 had fewer divisions. The three membra of web spiders were not subdivided, and the crab spiders were included with the wolves. The different order of tituli in the two lists shows that Lister had not begun to arrange the spiders in their final subdivisions. A comparison of Lister's full system with a modern method is given below.

(c) Eye arrangement.

The earliest writer to comment on the variety of eye number and arrangement in spiders was Henry Power. [1] He noted that some spiders have four, some six, and some eight eyes, 'according to the proportion of their bulk and the longity of their legs'. He linked the number of eyes with the need for a neckless animal to have a wide field of vision.

Swammerdam looked upon the eyes of spiders as being the equivelant of the facets of an insect compound eye; having them dispersed over the head instead of being concentrated at two points gives spiders better vision than any insect other than dragon flies. [2] Hooke [3] also mentioned variation in number. He wrote that some species have fewer, others more than, eight eyes. In fact, no spiders have four eyes, very few (and none of Power's) have six, and none has more than eight.

Lister thought that eight was the normal number in all spiders other than the binocular harvestmen, and he only reluctantly accepted that titulus XXIV (Segestria senoculata) appeared to have only six.

The positioning of the eyes was first mentioned by Power, who gave two diagrams of spider eye-arrangements, without saying which species they represented. They have an improbable appearance and are unrecognisable. [4]

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1. Power 1664 p. 11-12.
2. Swammerdam 1669 p. 61.
3. Hooke 1665 p. 202.
4. Power 1664 p. 13.

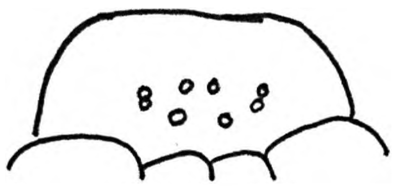
Lister referred to the eye arrangement in almost all the species. A full description of one particular arrangement was given in one account, and species with similar arrangements compared with this. Thirteen different arrangements are given, and these correspond closely with Lister's scheme of classification. The points of difference are that titulus XX (included in membrum 3, chapter 3) is given the same arrangement as chapter 2 of the same membrum; and separate arrangements are described for the two species of membrum 3, chapter 5, and for the two species of crab spider. Of no taxonomic value

<u>The eye arrangements of spiders according to Lister.</u>

The diagrams below are taken from modern sources (Bristowe or Locket and Millidge) and the descriptions from Lister.

<u>Tituli I - X.</u> (Membrum 1, chs. II and III)

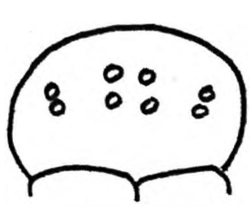
Eight eyes, equal in size, of which the inner four are arranged in a quadrangle, the outer ones in oblique lines, almost touching.



<u>Cyclosa</u> (Titulus IV)

<u>Titula XI - XVI.</u> (Membrum 2)

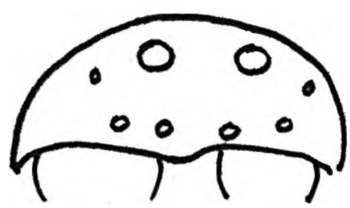
Eight eyes, of equal size. The inner four form a quadrangle, the outer ones are almost touching.



Theridion (Tituli XII - XIV)

<u>Tituli XVII, XVIII, XX.</u> (Membrum 3, chs. II and III (part)).

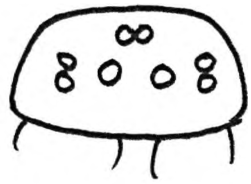
Of the four central eyes, the two lower are closer than the two upper.



<u>Tetrix</u> (Titulus XX)

Titulus XIX. (Membrum 3, ch. III (part))

The eyes appear to be five in number, but with a microscope one can see that the central four near the mouth are single, but the apparently single one above them is really double, and similarly with the four outer eyes which are almost touching.

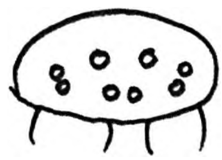


Linyphia

(The reference to 'the central four' is clearly a slip; otherwise a total of ten eyes would result, and had Lister meant this he would have pointed it out).

Titulus XXI. (Membrum 3, ch. IV)

Of the four central eyes, the two upper are a short distance apart; those nearer the mouth are much closer. The other four are on each side almost touching.



Ciniflo

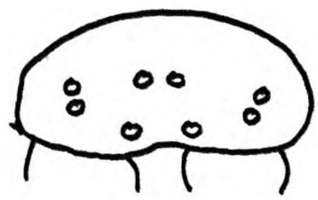
Titulus XXII. (Membrum 3, ch. V (part))

Of the four central eyes, the two upper are fairly wide apart, the two lower almost touching and a little larger. A pair at each side, almost touching.

Clubiona

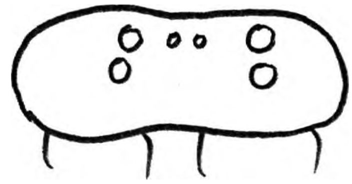
Titulus XXIII. (Membrum 3. ch. V (part))

Eyes of equal size. Of the four central, the two upper are closer together than the two lower. The other four are at the sides, almost touching.



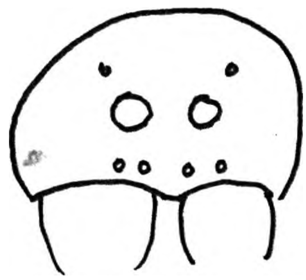
Drassodes

Titulus XXIV. (Membrum 3, ch. VI)
It would be most peculiar if it were true that it only has six eyes, as appears to be the case, but my glass leads me to this conclusion. There are two on the frons, rather small, and a pair at each side.



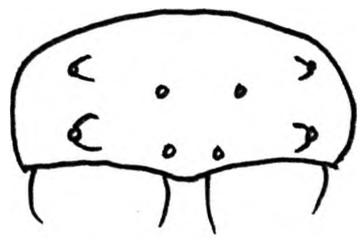
Segestria

Tituli XXV - XXVIII. (Section 2, ch. II)
Four at the front, almost touching. A little behind are two, much larger and further apart. Nearer the shoulders another two, fairly large.



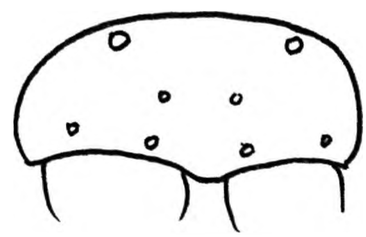
Lycosa (Titulus XXV)

Titulus XXIX. (Section 2, ch. III (part))
Eyes very small. Two at the front, two others above them. On the sides of the frons are two tubercles, an eye on each.



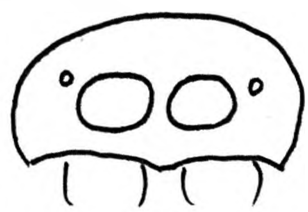
Xysticus

Titulus XXX. (Section 2, ch. III (part))
Four eyes at the front, in a curve. Two further back, then two more, much larger.

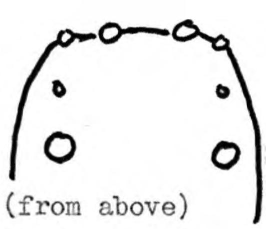


Tibellus

Tituli XXXI - XXXIV. (Section 2, ch IV)
Two in the middle of the frons, very large and easily visible with the naked eye. Two next to them, a little smaller, then two quite small. Finally two others, almost equal to the second.



(from front)



(from above)

Salticus (Titulus XXXI)

(d) Species accounts.

Lister's accounts of individual species follow a set pattern. An indication of the size of the spiders is given, relative to that of other species. No absolute measurements are given, but it should be remembered that Lister's illustrations are all life-size. Then the general body shape and colour are given; the eye arrangement, relative lengths of the different pairs of legs, and often descriptions of the palps, jaws and spinnerets. In the web-spiders, there is a description of the individual features of the web of the species; and usually an account of the hunting method in the hunting spiders. The distribution, habitat and season follow, and finally an account of the reproduction and the life cycle. As a rough indication of the relative importance Lister places on these topics, the average amount of space devoted to them per species is;

Description	- 18 lines;
Distribution, habitat	- 4 lines;
Web (when present)	- 8 lines;
Life history	- 19 lines.

As an example, Lister's account of Titulus I is given as an appendix. This is a little longer than most of the others, but is closer in relative proportions to the above averages than are any of the others.

As already mentioned, Lister in several cases gave descriptions of the juvenile stages. He also noted the differences between the sexes, and in some species gave illustrations of both. (In his preface, he wrote that all descriptions were to be taken as being of the female, unless otherwise stated). Though he mentioned differences in size, proportions and colour, he gave the structure of the palps as the main feature distinguishing the sexes.

Some of the descriptions refer to intra-specific variation. This is said to be more noticeable after a moult, and care is needed not to take these varieties for distinct species.[1] In one case,

1. HAA p. 88.

Titulus I, this variation is linked with the spider's habitat or geographical distribution: 'Those taken in winter by mountain streams are seen to be almost a different species from those living in the plains; they are much darker and longer'. Modern authorities note that this species is very variable, but do not mention any correlation of this with distribution.

(11) Harvestmen.

The harvestmen (Opiliones) had not been distinguished as a group from the true spiders by writers earlier than Lister. Power, for example, described a 'long legged field spider, with its two eyes set on a turret', but placed it among accounts of other, true spiders, and did not mention the undivided body; he in no way implied that the animal was anything other than one of several typical spiders. [1]

Hooke gave a detailed description and excellent drawings of a harvestman or shepherd spider; he also noted the peculiar eye arrangement and the chelicerae, and that the 'head, breast and belly' could not be separately distinguished. As he was not giving a systematic account of spiders in general, however, it is not clear whether or not he looked upon the animal as a true spider. [2]

Lister realized that these animals were a distinct group, and his account of spiders is divided into two parts: one for the eight eyed and one for the binocular spiders. However, the name Araneus is used for 'spiders' of both groups.

Lister gave the following distinguishing features of binocular spiders: [3]

(1) Very long legs.

(2) Crustaceous skin.

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1. Power 1664 p. 14.

2. Hooke 1665 pp. 198-200.

3. HAA p. 93.

(3) No power to produce silk 'so far as I know' - though as he did not know how the animals breed, he could not say whether or not they spin a cocoon.

(4) They have only two eyes.

(5) The head is set into the thorax.

(6) The thorax and abdomen are not distinct.

(7) The chelicerae (tela) are bent and forked at the tip like a crab's pincers.

(8) They do not have a poisonous bite.

(9) The second and fourth pairs of legs are longer than the others.

(10) Their excrement is solid.

He noticed horns on the chelicerae of the male, and he thought this was found in all species; it is in fact a characteristic only of Phalangium opilio (Titulus XXXV). He noticed the penis and ovipositor, though he did not realize the function of the latter. [1] Although he observed mating, in which he noticed the animals pair both facing the same direction, he could not see the rest of the reproductive cycle. He dissected gravid females in August and found fully developed eggs, and saw spiderlings in May, but never saw eggs laid. When he kept females in glass vessels to try to observe this, they all died without laying. [2] Eventually, in the supplement to his book, [3] he mentions a suspicion that harvestmen may be viviparous - 'quod valde suspicior'. In fact they lay their eggs underground by means of the ovipositor.

Lister's descriptions allow the three species to be identified. They are:

Titulus XXXV. Araneus cinereus, cristatus (Phalangium opilio).

Titulus XXXVI. Araneus rufus, non cristatus (Leiobunum rotundum).

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1. Ibid. p. 96.

2. Ibid. p. 95.

3. Published as an appendix to the Latin edition of Goedart, 1685.

Titulus XXXVII. Araneus exiguus, e candido nigroque varius sive maculatus insigniter, cristatus, sylvicola. (Opilio parietinus).

All three are common and widely distributed, though Lister's record of Opilio parietinus for Lincolnshire was the only record for that county until 1948. All three species were included in the list of 1670.

Finally, included with the binocular spiders, but in a separate chapter, was Titulus XXXVIII, Araneus exiguus, coccineus, vulgo Anglice TANT dicte; this is a Trombidiform mite, the red harvest bug. Lister noted the undivided body and eight legs, but did not think he could find any eyes; he did see a number of small black spots round the first legs.

(12) Illustrations.

Lister placed a great deal of emphasis on illustration in all his books. In the English version of his edition of Goedart's Insects he wrote:

> 'Natural History is much injured through the too little encouragement which is given to the artist, whose noble performances can never be enough rewarded; being not only necessary, but the very beauty and life of this kind of learning'.

The standard of illustration in his books is usually high. Those in his book of 1678 were by William Lodge, who was a member of the group of virtuosi at York which included Lister.[1] However, Lister's preface implies that more than one artist was tried for

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1. H. Walpole, Catalogue of English Engravers (London, 1786) p. 100 (quoted by Davies p. 79).

this book:

> 'I have taken care to have almost all the animal figures drawn in my presence, with the best artist producing, not as is usual his own ideas, but, when one of the species was being drawn, I first pointed out the characteristic features, and he readily accepted this. Any of these men, however praiseworthy in other respects, who did not give way in this, were discharged, as this is not a matter of arranging a drawing of an animal in a pleasing way, but of giving it an appearance of real life, and in which the valuable point is quick and easy distinction of the species'.

All but five species were illustrated, sometimes with figures of both sexes or the cocoon or web. Unfortunately, Lister always used life-size illustrations, and though these are usually adequate for shells, they are of little use with spiders. Although Lister's plates are much more naturalistic than those of Moufet, they can only occasionally be used to help identify species, and are much inferior to Hooke's drawing of the shepherd spider (a harvestman). Even so, the following illustrations show the improvement in standard of illustration between Moufet and Lister.

(13) Economic uses of spiders.

Lister devoted a short chapter (VIII) to the medicinal uses of spiders. These can be grouped under three main headings:-

1. Use of the web or the spider itself to prevent bleeding and to heal wounds.
2. Use of the spider to cure fevers.
3. Use of the spider or spider's eggs to relieve pain, such as toothache, earache or gout.

These were all orthodox remedies at the time. Elsewhere in the book, [1] mention is made of Sir Matthew Lister's use of a distillation

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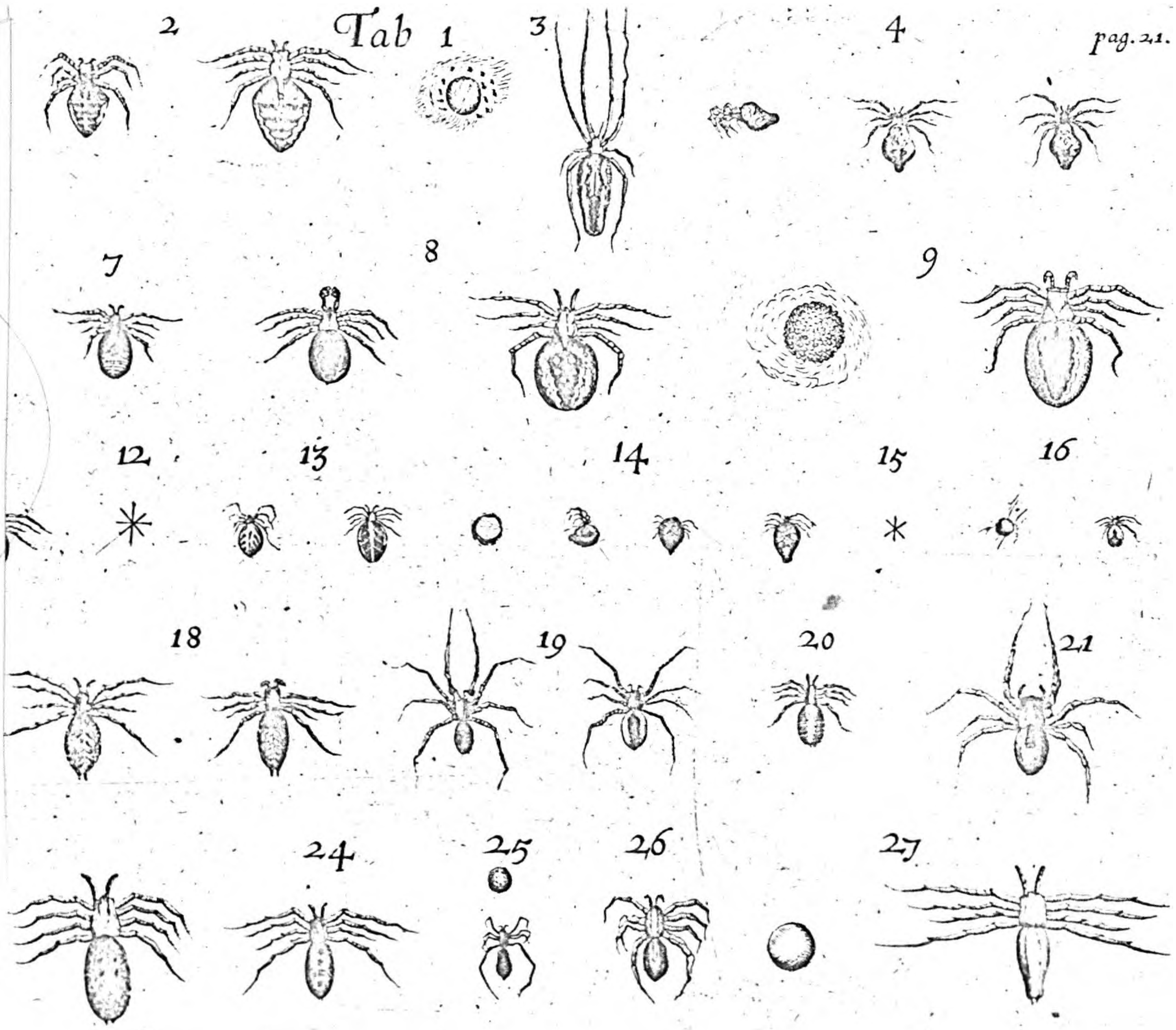
1. HAA p. 78.



Illustrations of spiders from Lister (above)

and Moufet (below).

of black spiders as a most efficient cure.

It is possible that much of the value placed on the curative powers of spiders came from the undoubted value of spider web as an aid to blood clotting, combined with confusion between the words aranea, a spider or a spider's web, araneus, a spider, and araneum, a spider's web.

Lister's writings show a frequent interest in dyes and pigments. In his paper of 1671, query 9 concerned the possibility of extracting and 'striking' colours from spiders so that they could be used as dyes.

(14) The influence of Lister's work on spiders.

Lister's work must have become widely known among the contemporary general public, a man spending his time investigating such obnoxious creatures as spiders being an obvious target for derision. Thomas Shadwell, in The Virtuoso, satirized the apparent futility of the activity of the Royal Society, using the study of insects, and particularly spiders, as the easiest target. The virtuoso is Sir Nicholas Gimcrack, who is said to be

> 'One who has cracked his brain about the nature of Maggots; who has studi'd these twenty years to find out the several sorts of spiders, and never cares for understanding mankind.'[1]

That Shadwell had Lister's work in mind is shown by Sir Nicholas' statement that

> 'I think I have found out more Phaenomena's or Appearances of Nature in Spiders, than any man breathing; wou'd you think it? there are in England six and thirty several sorts of Spiders; there's your Hound, Grey-hound, Lurcher, Spaniel-spider .....'

Of his tame spiders,

> ' one above all the rest, I had call'd him Nick, and he

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1. Thomas Shadwell, The Virtuoso (1675) in The Works of Thomas Shadwell 3, ed. Summers (London 1927) p. 113.

knew his name so well, he wou'd follow me all over the house; I fed him well with fair flesh-flies. He was the best natured, best condition'd Spider, that I ever met with'. [1]

This attitude was common at the time; King's parody of Lister's edition of Apicus [2] refers to a catalogue of

> 'all the Doctor's works concerning cockles, English beetles, snails, spiders that get up in the air and throw down cobwebs, a monster vomitted up by a baker, and such like, which, if carefully perused, would wonderfully improve us'.

Addison commented that

> 'It is natural to laugh at such studies as are employed on low, vulgar objects. What curious observations have been made on spiders, lobsters and cockleshells! Yet the very naming of them is almost sufficient to turn them into raillery'. [3]

As a work of reference, Lister's book immediately replaced Moufet as far as spiders were concerned, and retained its authorative value for a century.

Swammerdam's major work, the Bible of Nature [4] was published posthumously, under the editorship of Boerhaave. Swammerdam himself is usually said to have finished his active work by 1673 or 1675, [5] but this work contains frequent specific references to Lister's work of 1678, mostly approving, though Swammerdam denied

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1. Ibid. p. 141.

2. W. King, The Art of Cookery, in Imitation of Horace's Art of Poetry (London, 1709).

3. J. Addison, Discourse on Antient Medals (quoted by Davies p. 79; the present author is unable to trace this work. Possibly the Discourse on Antient and Modern Learning is meant).

4. Swammerdam 1737 1 pp. 47-56.

5. eg. C. Singer, History of Biology (third edition, London 1959) pp. 163-4; E. Nordenskiold, History of Biology (English translation of 1912 German original, New York 1928).

that all spiders possess palps. It would seem either that Swammerdam continued work on insects until shortly before his death in 1680, or that Boerhaave added substantial sections to the work.

John Ray's History of Insects was mainly the work of Francis Willughby. However, Ray explained [1] that Willughby died before completing his study of spiders, and, as Ray has not studied these insects with industry, he has used Lister's account. The section on spiders is in fact virtually a reprint from Lister's book; some of the general matter is omitted, there are very brief and unidentifiable descriptions of two extra species, and Ray suggests that the black spots at the base of the legs of the mite (titulus XXXVIII) were eyes; the rest is a word-for-word transcript from Lister. A confusing misprint occurs several times. Ray appears to have written in manuscript 8oculi or 8noculi as an abbreviation for Lister's octonoculi. The printer read and printed this as Senoculi - a serious distortion.

Leeuwenhoek carried out micro-dissections of the garden spider, but made no reference to Lister. He denied that the palps could be used in mating. [2]

The first English book on spiders after Lister's was that of Eleazor Albin. [3] He referred to nearly 150 species, with many plates, but the species are not all distinct and are mostly unidentifiable from the poor descriptions. The starting point of modern spider

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1. Pref. p. 11.

2. Letter 143 of 1712, as quoted by de Geer, loc. cit., and Savory 1961 p. 43; PT 23 (1702) pp. 1137-55.

3. Eleazor Albin, A Natural History of Spiders and other Curious Insects (London, 1727).

taxonomy is the work of Clerck, [1] who described about 70 species from Sweden. The book was translated into English by Thomas Martin. [2] Clerck's fellow-countryman, Linneaus [3] included only 39 species in his work and these were drawn from all over the world. No attempt at subdivision was made, all the species being included in the single genus Aranea. Lister's species names are given in the synonomy.

The most important account of spiders at the end of the eighteenth century was that of De Geer. [4] He referred constantly to Lister's work as a source of information, though he did reject Lister's idea of the forcible ejection of threads of silk and their retraction. For one species (Lister's titulus XXXI), De Geer said that Lister's account is so good that no repetition is needed. De Geer's classification follows that of Lister, with fewer subdivisions. His garden, wood and house spiders correspond to Lister's three better-named membra of web-spiders; the wolf, crab and jumping spiders correspond in both systems, and De Geer has an extra group for the water-spider, Argyronecta.

In the same year as De Geer's volume was published, Lister's book was translated into German [5] so it seems that exactly a century after its first appearance the work was not fully outdated. However, progress in this field was now so rapid that Lister's work was coming to have only historical value. Cuvier wrote that 'Lister has laid the basis of a natural distribution which all subsequent authors

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1. R. C. Clerck, Svenska Spindler (Upsala, 1757)

2. Thomas Martin, Aranea, or, a Natural History of Spiders (London, 1793). This includes a revised version of Albin's text.

3. C. Linnaeus, Systema naturae (10th. edition, Stockholm, 1758).

4. De Geer, op. cit. vol. 7.

5. Naturgeschichte der Spinnen, mit anmerkung vermehrt von Friedrich Heinrich Wilhelm Martini (Quedlingen and Blanckenburg, 1778).

have done little more than modify'. [1] Blackwall similarly wrote that

> 'Our celebrated countryman Dr, Martin Lister, in his admirable Tractatus de Araneis has given a classification of the species he has so admirably described, founded on their external organization and economy, which has formed the basis of every subsequent attempt deserving of notice to effect a systematic arrangement of this interesting order of animals'. [2]

Blackwall occasionally used Lister as a source of information on distribution, reproduction and web structure, and gave his titulus numbers in the synonomy.

Appendix. Lister's account of spider titulus I - Meta segmentata.

Yellowish spider, abdomen paler, especially at the summit and round the sides, and full; eyes black and shining on a whitish head.

Description. Among the smaller species; legs fairly long, not too slender, especially the first, yellowish, spotted lightly and with rigid hairs like spikes, easily visible to the naked eye. They can be ordered as follows: the first longest of all, then those behind them, the third shortest of all, and the fourth numbered after the second. Head and shoulders the same colour as the legs, and almost shining; at the front, eight eyes, black and difficult to make out with the naked eye. Four at the front, in a rectangle, evenly spaced. Next to these, a pair at each of the upper corners of the frons, almost touching, forming a slightly oblique line.

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1. Cuvier 1833 trans. of 1817, 13 p. 395.

2. Blackwall preface p. (1).

Abdomen broad near the thorax, tapering gradually from there to a blunt point. On it can be traced out the rather obscure traces of the shape of a leaf. In the centre of the abdomen is a straight line like the midrib of a leaf. Next to the thorax and at the sides, the abdomen is almost brilliantly white; the rest is yellowish or golden, sometimes greyish to rufous. Certainly this, like the other species (and as we do not wish to have to repeat often) has a colouring which is hardly constant in summer, either because the animals are at different stages of moult or some of them are a year old. The belly is black at the centre, yellowish round the sides. This spider is glabrous, and appears to be scaly when viewed through a lens.

Place. This spider is very common from the beginning of May to the middle of October, and sometimes later, among gorse, broom and in woods. When the storms and rainy weather begin about October, they take refuge in houses and under roofs. Those that are found in winter by streams in mountainous country appear to be almost a distinct species; they are blacker and much greater in length than those from the plains.

The males are much leaner and slenderer than the females, and more reddish; at the tip of the palp is an almost round tubercle, which is sometimes distinctly swollen; this is a very sure mark of the sex, in this or any other eight-eyed spider.

It is a peculiarity of the web of this spider, that they first fasten a few viscous threads at the centre of the web, and then fasten the others at large intervals from the outside. This is always so, and not found in any others of this genus of spiders. They draw out a thick thread, one end fixed to the centre of the web, the other under a bank or elsewhere, and this takes the spider back to safety. If this is noted when the web is first seen, and the web has been made by this spider, you will certainly find the spider, if not at the web centre, then certainly under a nearby bank next to the end of the chord, but quite in the open, without any special nest, so far as I have been able to discover.

The female becomes gravid in September. In many of their nets, both male and female can be found together at this time. (I have seen the male making its own net). Two or three males come together round each female, but she continues to occupy the centre as sole owner and hunter of this web, while they, like so many suitors, spread out on the outer ranks.

One morning I made an interesting observation. Two males were fighting strenuously, their heads held together by their jaws, and their legs wonderfully interwoven. I wanted to separate them, and took them from the web, in which they were with the female, into my hand. There they separated themselves, but the instant they were released, they began to fight again.

In October, I have looked into the abdomens of twenty or more of this species of spider. In none of them were there any eggs remaining; they had all been laid a short time before. The abdomen had by then lost its swelling.

I have also noticed that at the end of October, males are met with, with much reduced abdomens (many males hunt in their own webs at this time, as well as females); and, as if in celebration of mating, are moulting, hanging on by their legs.

I have more than once kept gravid females of this species under glass, but they have always died before laying. However, I have obtained the young in another way. By chance, at the end of April, I have found cocoons with several eggs among moss at the foot of large oaks. These cocoons were round, and made of loose white threads, about the size of common peas. I kept them in wooden boxes, and about the middle of May I had no difficulty in recognising the spiderlings as being of this species. Afterwards, I often found and recognised these cocoons elsewhere, in house windows and in gorse.

Some of them hatched before winter, and if my observation is not false, some of these minute offspring amuse themselves in November by ejaculating threads; but much the greater part last all winter, and until the beginning of summer, as eggs.

It is worthy of note that in this, as in some other spiders, most of the adults either perish in winter or have become emaciated by their long fast, so that hardly one in a thousand is not, at the end of April or the beginning of May, much more slender than it was in autumn. I believe that they survive with difficulty or not at all.

As with other eight-eyed spiders, they eject liquid excrement.

Chapter III Generation.

(1) Introduction: the problem of generation: epigenesis, metamorphosis, and preformation: ovism and animalculism.

For seventeenth-century natural philosophers, the fundamental biological problem was that of generation: how do living things come into being? [1] Such a question includes a number of subsidiary problems, which can be grouped under two main heads:

(1) What is the nature of the primordium from which a living thing may develop? Is a special 'life force' needed, or are the properties of the particles of certain kinds of matter adequate in themselves? Need the primordial matter itself be alive or not? If so, must it always develop into an individual of the same species as the animal which produced it - in spite of appearances in some simple animals? Is the material continuity between the generations in higher, sexual animals brought about through the male or the female genital material, or both? What are the features and properties of these genital materials? These problems, that of vitalism apart, were largely the consequences of the limited observational data available in the middle of the century, and in particular the lack of relevant microscopical research.

(2) When, and how, does the organization of the foetus appear? By a rearrangement in, or a sudden imposition of form on, the primordial matter (metamorphosis in the seventeenth century sense)? Or gradually, as new matter is added to the growing foetus, giving an increase in complexity as new organs develop (epigenesis)? Or by the unfolding and

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1. For general reference on this subject, see F. J. Cole, Theories of Sexual Generation (Oxford, 1930); N. J. Needham, A History of Embryology (Cambridge, 1934).

increase in size (evolution in the original sense) of structures already present and preformed in the primordium? In the latter case, it is necessary to discover whether the preformed organization lies in the egg or in the semen of sexually reprodusing animals.

William Harvey (1578-1657) defined two of these alternatives clearly:

> 'In the generation of animals by metamorphosis, forms are created as if by the impression of a seal .... the whole matter is transformed. But an animal which is created by epigenesis attracts, prepares, elaborates, and makes use of the material, all at the same time; the processes of formation and growth are simultaneous..' [1]

The idea of preformation, which was to replace both these theories, did not appear until after Harvey's time.

In the mid twentieth century, the problem of organization has been pushed down from its literal sense of the structure of organs and tissues, through the cellular level to the molecular, and the divisions between these different seventeenth century approaches to the problem have become blurred: we think of preformed genetical information, received from both parents, transforming or metamorphosing organic food material, and building it up, epigenetically, into structures of gradually increasing complexity, which in some animals undergo periods of reorganization. For us, the problem of the nature of the matter involved, and the explanation of its properties, has been pushed back to the sub-molecular level, at which it is remote from the activities of biologists. In the seventeenth century, however, organization had to be explained, by biologists, on the basis of the matter from which the organs were formed; and control

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1. William Harvey, Exercitationes anatomicae de generatione animalium (London, 1651); English translation by R. Willis in 'The Works of William Harvey' (London, 1847) p. 335.

of this organization must either be inherent in the properties of certain kinds of matter, or be the result of preformed patterns already imposed upon the matter of the primordium - unless final causes were acceptable, as they were to Harvey. The former alternative makes it difficult to accept an inorganic origin for even moderately complex animals, and thus led to the rejection of the idea of spontaneous generation for such creatures in the second half of the century. In consequence of this, and the failure of mechanical theories of generation such as that of Nathaniel Highmore (1613-85), [1] biologists were inevitably led to the acceptance of preformation, at least in the wide sense in which the pre-existence of some form of organization in the primordium was assumed, whatever the primordium may be. Epigenesis, in which no organ structure was supposed to exist until the material making up the organ was added, would allow the theoretical possibility of inorganic matter becoming gradually more complex until it reached the level of organization of living things. Further, preformation allows a more 'mechanical' approach in which explanations in terms of cause and effect are possible, and fits in well with the mechanical physiology of the turn of the century, in which growth was thought of as being the addition of fluids to a body composed almost entirely of vessels, which were thereby expanded. Epigenesis appears to be much less amenable to material causes.

The two problems, that of the nature of the primordium and that of the origin of organization, were clearly distinguished by Harvey:

'We ... shall show in the first place in the egg and

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1. Nathaniel Highmore, The Historie of Generation (London, 1651).

and then in the conceptions of other animals, what parts are formed first, and what are subsequently formed .... next we shall inquire into the primary matter out of which, and the efficient cause by which, generation is accomplished'. [1]

Martin Lister made no direct contribution to the solution of the problem of organization in the embryo, but he was involved in a number of investigations peripheral to the problem of the nature of the primordium, and these affected his published pronouncements on the central problem of the, by then orthodox, preformationist attitude to embryology: does the embryo derive its structure from the male or female parent?

The major work on generation in the mid-seventeenth century was Harvey's De generatione animalium of 1651. This concentrates almost entirely on the question of the appearance of organization in the foetus; Harvey had little to say on the nature of the primordium, and of course neither the spermatozoan nor the mammalian ovum were known to him. However, his work on reproduction in deer disproved the orthodox Aristotelian idea that the foetus was formed by the 'setting' of the menstrual blood by a non-material principle (pneuma) carried in the male semen. Harvey showed that no such mass of material existed in the uteri of does after mating; this also disproved the Galenic idea that the matter of the foetus was provided equally by the male sperm and the female menstrual blood. However, Harvey did not suggest an alternative which was satisfactory to later writers. The 'egg' which Harvey is commonly said to have claimed to be the origin of all animals was, though corporeal, a theoretical concept rather than a well-defined reality. It is produced from an 'internal vital force', and not by the uterus, and

1. Harvey 1651 p. 164.

may be formed 'as it were, spontaneously, by chance', and not only as 'fruit or seed, from something else preceeding it'. [1] The word ovum, in other words, is applicable to any structure, whatever its origin, having life in potentia; it is defined in terms of final, not material or formal causes. This was unsatisfactory to many in an age when final causes were widely rejected, and there were many attempts to solve the problem of the physical origin of living things. Highmore, for example, wrote in 1651 of the origin of living things by the chance 'mutual conjecture of atoms', followed by'the addition of new parts'. He made it clear that by the latter phrase he meant the enlargement of existing organs by the addition of new flesh, rather than the formation of new organs, so that he avoided the problem of the control of growth by suggesting a form of instant preformation. [2]

This, and similar theories by Pierre Gassendi (1592-1655) and Sir Kenelm Digby (1603-1665), had little success, being pure speculation which could not be related to observation. [3] In the second half of the century, however, new observations were made which allowed the rejection of Harveian epigenesis in favour of preformation. Some of these led to a growing scepticism towards the idea of spontaneous generation, which is difficult to reconcile with preformation; and the use of the microscope was to be very important. Until this time men must have always felt some lack of confidence in accepting, for bodies too small to be seen, a physical reality in the same sense as that of visible objects. The microscope demonstrated this reality, and, together with the use of the telescope in astronomy, suggested that size and scale may be relative, and not absolute, matters.

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1. Ibid. pp. 457, 281.
2. Highmore pp. 4, 58, 61, 60.
3. See Cole 1930 p. 156.

It was now possible to suggest seriously a minute precursor, too small to be seen with the naked eye, but yet containing all the organs of a complete animal on a miniature scale. The gap left by Harvey's rejection of the Aristotelian foetation could now be filled without recourse to Harvey's teleological epigenesis.

Further, the microscope demonstrated the actual existence of a possible precursor, in Leeuwenhoek's discovery of the spermatozoan in 1678. The theoretical impact of the change in outlook on size and scale is well seen in Leeuwenhoek's writings on the spermatozoan:

> [If we] ' ... consider, what great Wonders can be lodged in such an Animal, we must stand amazed and cannot apprehend the extraordinary smallness of these parts, whereof these Creatures are composed, and say within ourselves, how impervestigible is the depth of Wisdom'. [1]

The existence of these minute organs was necessary at a time when life was thought to function at, literally, an organic level, rather than at the tissue, cellular or molecular levels of later times; and the preformation theory was a natural consequence of this. As a definite doctrine, preformation is usually held to date from Swammerdam's demonstration of the pre-existence of a butterfly in its chrysalis in 1669. The emboitment of Charles Bonnet follows logically from this, and was foreshadowed by Leeuwenhoek himself:

> ' ... in an Animal of the Masculine seed of Man, is

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1. Phil.Trans. 21 (1698) p. 271.

locked up a whole Man, and the Animals of the Seed are all descended from the first created Man'. [1]

De Graaf's discovery of the mammalian 'ovum' in 1672 had suggested another possible precursor, and this was supported by Croone and Malpighi. (That De Graaf had observed what we now call the Graafian follicle rather than the true ovum was, of course, irrelevant to the theoretical impact of the discovery at the time). However, perhaps because of the more clearly animal-like nature of the spermatozoan, the majority of biologists accepted it, rather than the ovum, as the true precursor of the foetus, the ovum being thought to provide a resting place or food for the animalcule. [2] This was to be the generally accepted theory until the middle of the eighteenth century.

(2) The rejection of the idea of spontaneous generation.

Before it was possible to accept either the ovist or the animalculist version of preformation as the universal explanation of generation in animals, it was necessary to reject the idea of spontaneous generation. Until the middle of the seventeenth century, this idea was almost universally accepted, at least for the simpler forms of life. There are frequent references to the idea in Harvey's writings; for example, that 'some animals arise spontaneously, or as is commonly said, by putrefaction'. [3] Of course, generation by such putrefaction, or by fermentation, solar heat, dew, Athanasius Kircher's vis seminalis or Thomas Moufet's 'Plastic force of Caleodick Nature' is not spontaneous in the strict sense, though Highmore's 'chance conjecture of atoms' may be so: but the term must

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1. Ibid. loc. cit.

2. Nicholas Andry de Bois-regard, De la Génération des Vers dans la Corps de l'Homme (Paris, 1700); G. Garden, 'On the Modern Theory of Generation'. Phil.Trans.17 (1691) p. 474.

3. Harvey 1651 p. 286.

taken here in a broad sense as meaning the origin of living from non-living material. It is this which John Ray declared in 1671 to have been 'the constant opinion of Naturalists hereto fore.[1] The rejection of this in favour of biogenesis is usually held to date from the work of Francesco Redi in 1668.[2] Redi wrote of

> 'My belief that the Earth, after having brought forth the first plants and animals at the beginning by order of the Supreme and Omnipotent Creator, has never since produced any kinds of plants or animals, either perfect or imperfect; and everything which we know in past or present times that she has produced came solely from the true seeds of the plants and animals themselves, which, through means of their own, preserve their species'.[3]

The truth of this belief was demonstrated so far as blow flies were concerned by the well known experiments in which meat was protected from egg-laying flies by fine gauze. This work, though of great importance, was not the first in the field. Similar conclusions, derived from the same type of experiment, had been made at the Royal Society a decade earlier; Thomas Sprat wrote of 'experiments of the equivocal Generation of Insects ... of Flesh not breeding Worms, when secured from Fly-blowings'.[4] Further, in spite of the above-quoted statement, Redi's biogenesis limited an organism's origin only to some kind of living matter, and not necessarily to its own species. The parasitic insect larvae in plant gills were thought to be the product of 'that soul or principle which creates the flowers

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1. FCJR p. 56.

2. Francesco Redi, Esperienze intorno alla generazione degli insetti (Florence, 1668); translated by M. Bigelow, Chicago, 1909.

3. Ibid. pp. 26-7.

4. Thomas Sprat, The Historie of the Royal Society (London, 1664) p. 223.

and fruits of living plants; and is the same that produces the worms of these species'. [1] Similar origins were suggested for other parasites. Nevertheless, though parasites continued to present problems, free-living macroscopic animals were no longer thought of in informed circles in the last quarter of the century as arising from non-living matter; but until the problem of parasites was solved, it was not possible to accept a thorough-going theory of biogenesis. As John Ray wrote:

> 'It seems to me at present most probable, that there is no such thing [as spontaneous generation]; but that even all insects are the natural issue of Parents of the species with themselves ... But still there remain two great difficulties. The first is, to give an accompt of the production of Insects bred in the By-fruits and Excresencies of Vegetables, which the said Redi doubts not to ascribe to the Vegetative soul of the plant ... The second, to render an accompt of Insects bred in the Bodies of other Animals'. [2]

And earlier Harvey had written:

> 'What shall we say of the animals which are engendered in our bodies, and which no one doubts are ruled and made to vegetate by a peculiar vital principle (anima)? Of this kind are lumbrici, ascarides, lice, nits, syrones, acari, etc.; or what of the worms which are produced from plants and their fruits, as from gall-nuts, the dog-rose, and various others? .... It certainly cannot be that the living principles of the Animals which arise in the gall-nuts existed in the oak, though

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1. ~~15.~~ Redi pp. 91-2.

2. Phil. Trans. 6 (1671) pp. 2219-20.

these animals live attached to the oak, and derive their sustenance from its juices ..' [1]

Martin Lister left no record of having carried out experimental work on the simpler kinds of suspected spontaneous generation, though he wrote to Ray in 1668 that

'whether such a thing can occur anywhere in Nature I strongly doubt, in spite of the testimonies of Aldrovandus etc ..... I have many experiences which either show the contrary or demonstrate certain errors, which you shall receive from me later'; [2]

and his published works often condemn the idea. In his last work on natural history, he claimed to show that bivalve molluscs were of different sexes, so removing one of Aristotle's arguments for their spontaneous generation. [3]

However, during his years at York, Lister carried out a considerable amount of work on the generation of parasites.

A. Plant Galls.

Plant galls or 'vegetable excrescencies' are superficially so much like fruits or other normal plant growths that it was natural to believe them to be such. It had long been known that insect larvae or 'worms' could often be found in them, even though there was no apparent hole to show where the worm had entered. Galls with worm-holes were always found to be empty, showing the hole to be an exit. Redi took this as evidence of 'the peculiar potency of that soul or principle' of the plant responsible for normal vegetative growth. He saw further evidence of this in that galls always occurred in the young growing parts of the plant, and were visible 'from the first budding out'. Further, the worm failed to develop if the

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1. Harvey 1651 p. 282.

2. CJR p. 11 (in Latin).

3. EAT pp. 81-106.

gall were removed from the plant, so depriving the animal of the vital stimulus and food conveyed into the gall by the plant fibres. [1]

Lister published two papers in the Philosophical Transactions for 1671 on vegetable excrescences, the first being a summary of a letter to Ray which has not survived, and in which Lister criticized Redi's recent opinion that 'some live Plants or their Excrescencies do truly generate some Insects'. [2] Lister admitted that 'it had never been my good fortune to discern Eggs in the centre of Galls', he 'having found worms even at the first appearance of the excrescence'. Nevertheless, he was convinced that the worms were generated from eggs laid by animals of their own kind, as other creatures of the same 'race' (i.e. other insects) were known to be. Further, the adult insects emerging from the galls could be shown, according to Lister, to be male and female, and 'we may argue with Aristotle ... that Nature made not such in vain'. If such insects produced offspring after mating, these offspring would either be of the same kind as their parent, which would argue that the parents themselves had been produced sexually, or they would be of a different kind, which would lead to a multiplication of species ad infinitum - which was absurd. Lister's argument here is that, as there is sex among insects, there can be no spontaneous generation; Aristotle had in fact argued in the other direction, that as insects are generated spontaneously, there can be no sexual distinctions between them, or the absurd contradiction quoted by Lister would arise. [3]

Lister noted that one type of plant might carry several kinds of gall, each of which produced a distinct kind of insect, which 'might rather argue the diverse workmanship of different Insects, then [sic] one and the same principle of vegetation to be the Author of

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1. Redi pp. 91-4.

2. Phil. Trans 6 (1671) p. 2254.

3. Aristotle, De generatione animalium 715b.

several sorts of animals'.

Not only is the worm not generated by some principle of the plant, but, claimed Lister, neither is it nourished by the plant in any active way. Though the worm clearly took its food from the gall, there was no

'Navil connexion, as that Author [Redi] fancies, and which .. to me was unintelligible ... I should be glad of a notion, which might make out to me such monstrous relation, as half animal, half vegetable, or which is all one, Vegetable vessels inserted into an Animal, or, the contrary. Strange Oeconomy!'

As the anatomy of the vessels of even a large tree was not fully understood, wrote Lister, who was also working on plant vessels at the time, it would be even harder to understand the 'filaments' of galls. He apparently did not realize that this is, of course, an argument against him. He was unable to explain, in this paper, how the worm was nourished, since the gall increased greatly in size during the growth of the animal, and did not appear to be at all worm-eaten.

He returned to the problem in his second paper, [1] taken from a surviving letter to John Ray. [2] By comparing them with ichneumon larvae in caterpillars, he suggested that gall-worms probably fed on the juices of the plant, rather than on the fibrous matter; in this, they differ from the grubs found in seeds and the kernels of nuts. The dryness of the material of galls such as oak-apples appeared to confirm this.

Lister proposed to confirm the truth of his hypothesis on the origin of galls by making systematic observations on poppy-head galls, as the poppy plant left a scar following the slightest puncture: he

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1. Phil. Trans. 6 (1671) p. 3003.

2. CJR p. 88.

had noticed such scars on every one of these galls which he had examined that summer. No such observations, if any were made, were ever published.

In spite of his understanding of the true nature of the animal contained within the gall, Lister was still impressed with the fruit-like appearance of the gall itself, and he put forward, as queries, the possibility that such excrescencies as oak apples and the 'shagged balls of the Wild Rose' - presumably Robin's pin-cushions - might be 'grown from the bud', or, in the former case, be acorns 'monstrously perverted'. This would mean, of course, the laying of an egg by the parasite in the unopened flower-bud, and modification of flower and fruit by the activities of the adult or larval insect.

A problem of such contemporary importance, and which was relatively easy to solve, was bound to attract further attention, and Lister's pioneering, though incomplete, observations were followed by Malphigi's monograph on the subject eight years later.[1] It was not until 1700, however, that Valisnieri was able to make the observation which completed the argument: that gall wasps do in fact lay eggs in plant material which then produces a gall.

Another plant excrescence or 'husk' studied by Lister was kermes. This name was used for a group of plant bugs now known as scale insects or coccids, highly modified Homoptera in which the female becomes reduced at successive ecdyses until it consists of a scale, made up of a number of cast exoskeletons glued together, covering a much reduced adult with atrophied legs and antennae, and bearing a number of eggs. Included in this family are Coccus cacti, the cochineal insect, and

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1. Marcello Malphigi, De gallis: de variis plantarum tumoribus et excrescentis in Opera omnia (London, 1686).

Laccifer lacca, which produces shellac. In the seventeenth century, these adults were taken to be plant material, and the young stages were unknown, so that kermes was not looked at in the same light as were plant galls. However, it was 'generally known' that insects could be 'ingendered of this Fruit', according to an anonymous writer in the Philosophical Transactions for 1667, who gave a method for making the 'plant' decay to produce flies. [1] However, treatment of the 'fruit' with acid or vinegar prevented this, according to a M. Verney, apothecary at Montpellier, whom Lister may very likely have met in France.

Early in 1671, Lister sent his first observations on the subject to Oldenburg, but these were not published in full. The letter apparently included a description of 'kermes husks' found on plum trees. Shortly after this, Lister wrote a second letter, published in the Philosophical Transactions. [2] In this, he described finding the 'worm-husks' on oak, vine, cherry-laurel, plum and cherry, always on the underside of branches, in groups, and each about the size of a half-pea, chestnut in colour, and containing four or five maggots like those of bees or wasps. These larvae, whose nature was to be a great puzzle to Lister, were separated from the 'bee-meat' [the adult?] by a partition, and surrounded by excrement. The empty husks gave a purple colour when rubbed on paper. Lister later found husks on rose trees; [3] he insisted that the colouring matter could not be derived from the tree, as it was always the same, no matter what species of tree the scale was taken from. In fact, he had probably observed more than one species of coccid, including, no doubt, Kermes ilicis and Lecanium hemisphaerica. Lister kept some of the husks in a box, and in June 1671, he collected winged insects which had emerged from them. He described

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1. Phil. Trans. 2 (1667) p. 796.
2. Phil. Trans. 6 (1671) p. 2165 (includes abstract of the earlier letter).
3. Phil. Trans. 6 (1671) p. 2177.

them as black bees, 'certainly the least, that I ever yet saw of that Tribe'; they apparently carried stings. It is possible that Lister had observed the winged males, which have abdominal appendages resembling stings; but if his statement that they carried four wings is accepted, then he must have seen the chalcid wasps which parasitize the majority of individual scale insects. Male coccids have well developed anterior wings, but the hind wings are vestigial. Lister entitled these insects, 'according to our custom'.

Apiculae nigra, macula super humeros sub-flavescente insignitae, e patellis sive favis memranaceis, veri Kermes similibus, suaque itidem purpura tingentibus, Cerasi aut Rosae aliarumve arborum virgis adtextis, exclusae

- a long title, even by pre-Linnaean standards.

Lister had written to Ray on the subject, but the latter appeared to be unconvinced that the kermes husk was of animal origin. He wrote that the 'matrices' of many insects found on plant leaves and stems were excrescencies of the plant itself, caused by the puncture of the insects' bites or by some virulent juice from the animals' bodies. [1] Lister, however, believed in 1671 that the husks were produced by small black bees as shelter for their young larvae, which later developed into new bees; they were not the equivelant of plant galls. He wrote to Oldenburg that he was convinced that the English purple kermes, like the imported scarlet kermes or cochineal, are

> 'only contiguous to the Ilex-branches, and are not excrescencies of the Tree, much less fruit or berries; by which abusive names they have been too long known; But that they are the artifice and sole work of the mother-Bee in order to the more convenient hiving and

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1. CJR pp. 86-7; FCJR p. 130.

and nourishment of her young'. [1]

In a paper probably written late in 1672, [2] he noted that what he had previously taken to be a mass of powdery excrement was in fact a mass of small mites; he acknowledged the help of Dr. Johnstone of Pontefract in this microscopical discovery. Despite his earlier reference to 'bee-meat', he now suggested that the grubs fed on these mites, 'there being no other food for them'; presumably the possibility of the larvae feeding on plant juices was not considered. However, as he suggested that

> 'there are probablie different sorts of Mites in these Husks, making possiblie different species ... the whole Husk ... serving only for a Cap or Cover to that company of mites of Kermes'

and as he wrote to Ray that the powder was in fact a mass of eggs, from which the mites hatched, he appears to have been uncertain at this stage as to which animals were the original occupants of the husk, He noted that these mites were 'to be distinguish't from the Bee-grubs, which are changed into the skipping Fly, that is, the Bee .. by us described formerly'. Not surprisingly, Lister confessed to Ray that he found the nature of kermes 'a greater Puzzle this year than I expected'. [3] His'mites' were probably the newly hatched coccid larvae; the 'maggots' were, no doubt, the larvae of chalcid parasites, though it is possible that when Lister found only a single 'grubb' in a husk, he may have been looking at the female coccid. He realized that cochineal was made by gathering scarlet kermes early in the year and drying it; as the material was imported in its final form, its exact nature had been a mystery.

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1. Phil. Trans. 6 (1671) p. 2197.

2. Phil. Trans. 7 (1672) p. 5059.

3. CJR p. 96.

By 1673, Lister could write to Ray that 'I have much changed my thoughts on kermes'. [1] Having found them on old ropes and dead boards, he was now 'pretty confident that it was an animal of the multipede kind, which does fix itself in order to the laying of its eggs'. These were fastened about its belly, as under the 'tail' of a crayfish. Lister claimed to have taken the animal before it attached itself.

This is an approximation to the truth, though he leaves unanswered the problem of the nature of the 'mites' and 'grubbs': which of them was the larva of the 'bee' which Lister had reared from the husk? He told Ray that he had sent 'further discoveries' to Oldenburg; these, like several other papers by Lister, were never published. It may be that they may have given an answer. Certainly, as Lister was also at this time working on ichneumon wasps, the nature of which he well understood, he could have given one possible explanation; but it is unlikely that he would ever have solved the whole problem without the use of a microscope, an instrument he appears to have neglected. The extreme sexual dimorphism of coccids was something for which he would have been unprepared, and which was not understood until Leeuwenhoek's microscopical work of 1702.

Robert Hooke had earlier [2] described another scale insect, apparently unparasitized, and gave an explanation for the scale similar to that of Lister's. Lister in fact referred to Hooke's work in his last paper; but as we can reconstruct the development of his ideas over the preceeding three years, it seems clear that Lister reached his conclusion independently.

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1. CJR p. 103.

2. Robert Hooke, Micrographia (London, 1665) p. 215.

B. Internal parasites - Worms.

Internal parasites were even more difficult to understand than gall-insects; as they are enclosed in an animal's body, they appear to be cut off from similar free-living creatures. Further, in a teleological age, it was difficult to see what these creatures were for; and was Adam created with an infestation of worms?

Thomas Moufet, while accepting that parasitic worms are 'bred of all humours', suggested an explanation which showed 'how far is it that they should be accounted by physicians amongst diseases'; he claimed that 'there are collected in us some putrefied excremental superfluous parts, which the more bountiful hand of Nature changeth into Worms, and so cleanseth our bodies'. [1]

Elsewhere, he claimed that tapeworms (Taeniae) were 'not properly living' - a suggestion which does not, of course, conflict with his hypothesis. [2] William Harvey claimed that 'no one doubts' that parasites, including 'lumbrici, ascaroides' and several ectoparasites, are 'made to vegetate by a peculiar vital principle' by which they are 'ingendered in our bodies'; [3] and Redi believed that his vital force was responsible not only for gall-wasps, but 'in this manner, I am inclined to believe, tapeworms and other worms arise, which are to be found in the intestines and other parts of the human body, also in the gall and liver of sheep; and likewise those other disgusting little worms found in the head of deer and sheep'. [4]

He even suggested that fleas and lice might be formed in this

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1. Moufet p. 1111.

2. Ibid. p. 1106.

3. Harvey 1651 p. 282.

4. Redi p. 116.

way, but eventually rejected this idea.

With the tendency to reject the idea of spontaneous generation in the later part of the century, the suggestion that the eggs of the worm had been swallowed by the host was made. This was more credible for some parasites than for others. Edward Tyson (1650-1708) accepted it for the Lumbricus Teres, a nematode, but would not commit himself for Lumbrious latus, a tapeworm. It did not resemble free-living worms, so how could it develop from their eggs? Even so, he claimed to distinguish two sexes in these parasites. Hyatids had to be classed as 'a set of Worms or Insects sui generis'. [1] However, by the end of the century, the swallowing of eggs was being accepted as the explanation of intestinal worms. [2]

As objects of both medical and biological importance, parasitic worms were of interest to Lister. A paper published by him in the Philosophical Transactions of 1673 dealt with tapeworms, and showed his typical attention to detail. [3] He noted that the tapeworms (lumbrici lati) of dogs and mice were specifically distinct, and gave a clear description of that of the dog. He realized that the small, fixed end was the 'head'; in this, he differed from Tulpius. [4] He noted that the worms were restricted to the duodenum and ileum, and were never found in the stomach or colon. As he could not provoke any movement in the worms, 'they then appeared to me as things without motion or sense'. Though he appeared to look on the worms as single animals, Lister described one dog tapeworm in which 'there is great reason of Suspicion, that this is a chain of many Animals linked together'. However, he gave no real evidence for this apparently perceptive statement, beyond

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1. Phil. Trans. 13 (1684) pp. 113-44 and 154-70; 17 (1691) pp. 506-10.
2. Andry de Bois-regard, passim.
3. Phil. Trans 8 (1673) p. 6062-5.
4. Tulpius, Med.obs. 2 (1652) chapter 42; as quoted by Lister.

noting that the chain was often to be found broken into smaller units.

This paper contained no speculation on the origin of the parasites. In 1675, Lister sent on to Oldenburg a latter from Francis Jessop, in which the latter described some 'hexapod worms' vomitted by a girl, and which Lister identified as beetle larvae. [1] He added descriptions of other 'worms' vomitted by a patient of his which he recognized as moth caterpillars,

> 'the very same for kind that have many times been seen on Plants, and no doubt, these (as those others) would in due time (if the place had not hindered) have shrunk into Chrysalis's and changed into moths. As also those mentioned by Mr. Jessop would have changed into Beetles'.

In 1681-2, Lister published in the Philosophical Collections of the Royal Society a description of a monster, now unidentifiable, vomitted together with a mass of flesh and blood by a patient of his. [2] As an explanation of the origin of this creature, he stressed what he had already noted in his paper of 1673: that 'we often drink what is alive; and it is certain some things will live on in our Stomachs in despight of concoction' - giving as examples gut-worms and ichneumons. Lister suggested that such swallowed creatures, if able to survive and especially

> 'if it shall be young and tender, and yet growing .... may have its designed form and shape monstrously perverted, so as to appear to us quite another thing than naturally and really it is'.

The vomitted flesh and blood were, he suggested, the remains of an ulcer analogous to the galls caused by plant parasites.

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1. Phil. Trans 10 (1675) pp. 393-5.

2. Philosophical Collections of the Royal Society 6 (1681) p. 164.

Such a theory would account for the differences between parasitic and free-living worms as well as explaining the method of infection. However, Lister had already shown the previous year that at least one group of internal parasites had a free-living stage of identical form. [1] These were Nematomorph worms, probably Gordius spp., which were usually thought to originate as animated horse-hairs in fresh water; Lister quoted Aldrovandi's account of this idea. He then described how he dissected 'medium-sized black Beetles' which he found while digging in his York garden. In the 'bellies' of these were animals which he identified as the horse-hair worms hitherto only known from freshwater ponds. There were one to three worms in each beetle, of two distinct types. These were well described, and Lister noted that the legend that they were amphisbaenae (having a head at each end) was false. The worms crawled out of the opened beetles themselves, and lived in the water for many days. These appear to be the first observations of Nematomorphs spending part of their lives in the body cavities of terrestrial insects; [2] however, Lister appears not to have known that the adult worms leave their hosts spontaneously and move into freshwater to breed, the larvae then reinfecting new insects. In view of his other writings on parasites, it seems likely that Lister would have thought that the worms had been swallowed by the beetle, probably as eggs or larvae, and quite accidentally, so that they would have developed normally as free-living individuals if left to themselves.

These worms are found in the haemocoel of the insects, and not in the gut. Lister's reference to the 'belly' of the beetles is ambiguous on this point, but he was aware that worms do occur in

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1. Phil. Trans. 7 (1672) p. 4064.

2. According to L. H. Hyman, The Invertebrates 3 (New York, 1951) p. 455, nematomorph worms were first found in arthropods in 1828 by Dufour.

positions other than the gut. He described worms (lumbrici teretes, or nematodes) which he had found in an ulcer on the ankle of a York girl. [1]

C. Ichneumons.

These internal parasites of insects had attracted Lister's attention early. In a paper published in 1671, he wrote that

> 'the Observation of the Vespae Ichneumones, as it hath relation to Spiders, I willingly reserve for other papers: yet I may tell you in general, that this kind of Insect is one of the greatest puzzels in nature; there being few Excrescencies of Plants, and very many births of Insects, wherein these slender Wasps after divers strange ways are concerned'.[2]

However, Lister's answer to the puzzle was already known to Francis Willughby, who had earlier written that 'Mr. Lister's opinion is, that the Muscae Ichneumones lay their Eggs in the bodies of Caterpillars' - an observation confirmed by Willughby. [3]

At this period, the word 'ichneumon' was used in a wider sense than at present. As used by Aristotle, in what Lister thought was the only example of the use of the word in Classical authors, it refers to solitary wasps feeding on spiders. [4] Pierre Belon (1517-64) used the word in this sense, [5] and Willughby referred to Ray's observations of solitary wasps as being of ichneumons. [6] In addition, Lister mentioned several times that some of the gall wasps

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1. Phil. Trans. 8 (1673) p. 6064.
2. Phil. Trans. 6 (1671) p. 2281.
3. Phil. Trans. 6 (1671) p. 2279.
4. Aristotle, Historia animalium book 5 chapter 20.
5. See HAA pp. 13-14.
6. Phil. Trans. 6 (1671) p. 2279

were either ichneumons or else closely similar; they are in fact usually chalcid wasps.

Thomas Moufet used the word in Aristotle's sense, but he also quoted, among other examples of 'putrefaction', an account by Sir Thomas Knivett of the

> 'corrupted body of a Catepillar .... converted into an imperfect Aurelia, then from that, not into a Buterfly, but three black eggs are cast out that are somewhat long fashioned, from whence proceed ordinary Flyes, or others like unto them; and sometimes the Aureliae being putrefied, neither Buterfly nor eggs come from it, but white worms (sometimes one, sometimes many) come forth, whence are generated very small Flyes. [1]

These flies were not described under the name Ichneumon. It may be that the modern use of the word to describe parasitic wasps originated in Lister's extension of the word to cover, not only those free-living wasps which use spiders as food for their young, but also those laying eggs in living spiders, and from this to include those parasitizing insect larvae of many kinds. So long as these parasites were looked on as examples of spontaneous generation, such an extension of the use of the word would have been pointless.

Lister wrote four papers on ichneumons, which were published in the Philosophical Transactions; he mentioned the subject often in his early letters to John Ray and in his edition of Goedart; and his work on English spiders included a section on their infestation by these insects. [2]

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1. Moufet pp. 924, 933.

2. Phil. Trans. 6 (1671) pp. 2281; 3002; HAA ch. 5.

Ichneumons presented two main problems to Lister; what kind of food do they find in the body of the caterpillar? And how do they infect the host? It seems that he never considered the possibility of their spontaneous generation.

The method of feeding was difficult to understand, as the parasites are not found in the gut of the host, and yet do not appear to attack the body structure; Lister pointed out that the caterpillar's 'bowels' were not destroyed, and that it was able to live for several days after the parasites had erupted. [1] He suggested that the ichneumon larvae perhaps fed on the body juices of the host; in this they would resemble their congeners, the gall-wasps:

> 'It is observable (to endeavour a Solution) that some of the Ichneumons delight to feed of a liquid matter, as the Eggs of Spiders, the juices (if not Eggs) within the bodies of Caterpillars and Maggots: Whence we conjecture, that those of the same Genus, to be found in Vegetable Excrescencies, may in like manner suck in the juices of the equivalent parts of Vegetables'. [2]

The alternative suggestion contained in this quotation, that the eggs of the future butterfly, which the caterpillar may already contain, could serve as food for the parasites, was to be returned to later. [3] It was probably suggested by Lister's observation that the eggs of spiders, as well as the adults, are often infected. Francis Willughby thought that Lister was wrong on this point, and that the similarity between the spiders' eggs and the wasp cocoons had caused confusion. [4] Lister was in fact

1. Goed. pp. 11-12.

2. Phil. Trans. 6 (1671) p. 3004

3. Goed. p. 58.

4. Phil. Trans. 6 (1671) p. 2279.

correct, though the wasps attacking the adults and eggs of spiders are of different genera.

A third possibility was the rather vague idea that the ishneumon larva was fed 'like an Infant in the womb'. [1]

Lister's ideas on the method of infection of the host are difficult to understand. Willughby's reference to Lister's views appear to show him as understanding the problem correctly; and in 1678 he wrote of ichneumons as bearing (pariunt) their young in the egg-cases of spiders. He also described the ovipositor, without naming it as such, as a 'prolonged, pointed anal extension, between two appendages which act as a sheath (vaginae)'. [2] Further, his papers on gall-wasps show him to have understood that the eggs are inserted into the plant by the parent insect. Yet by 1675 he was writing of the possibility that

> 'Ichneumon eggs may be licked up and swallowed down by Catterpillars in feeding, and escape digestion, and hatch within the Catterpillar's body: but I affirm nothing, not having seen the Seed, or Eggs of any one Ichneumon layd'. [3]

It may be that his interest in the gut parasites of vertebrates was at this time influencing his ideas more effectively than was his earlier work on galls. Certainly his reliance on the similarity between the methods of nutrition in the two kinds of animal host, rather than on the similarity of structure of the two kinds of wasp parasite, caused him to miss the opportunity of being the first to understand fully the nature of Ichneumon parasitization. However, he did firmly reject the idea that 'they

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1. Goed. p. 57.
2. HAA ch. 5.
3. Goed. p. 78.

are generated by the Catterpillar'; they are produced 'by their respective Parents; the Catterpillar which bore them, serving only as food to them, not a Mother'. [1]

D. Metamorphosis.

The word 'metamorphosis' has changed its meaning since the seventeenth century. To Aristotle, it meant the production of a foetus by the organization of the menstrual blood in the uterus, under the influence of the pneuma carried in the male semen. This organization appeared in all parts of the body at more or less the same time. Harvey understood the word in this sense, and contrasted it with epigenesis, in which new parts are added successively, the matter of each being organized as it appears: ' ... in generation by metamorphosis, the whole is distributed and separated into parts ..' [2]

The apparent reorganization of the living material of one kind of creature into the body of a new organism, as seen particularly in insects, could have been seen as a particular form of metamorphosis. The word is now used only in this sense, though the interpretation of the changes concerned which was given in the middle of the seventeenth century was quite different from that of the present day.

To Aristotle, the problem of insect metamorphosis and the relationship of larva to imago could be solved by looking on the former as a pre-egg stage, needed to accumulate the nourishment required for the production of a perfect egg - now called the pupa. Insects were therefore less highly developed than birds or reptiles, which lay eggs already in a perfect state. Such an interpretation has an element of truth in it, as it emphasizes the role of the

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1. Ibid. pp. 11-12.

2. Harvey 1651 p. 336.

larva as the feeding and growing stage of the animal, and Aristotle was giving more than a merely material connection between larva and adult. In contrast, Thomas Moufet looked on the larva as being a 'worm' of a quite different nature from that of the imago or the pupa produced from the flesh of the worm. Moufet described caterpillars and butterflies under separate headings in different parts of his book, and he makes it clear that what we now describe as three stages in the life of a single animal are for him three different animals, each owing its existence to the death and corruption of the earlier stages, or, in the case of the caterpillar, to the corruption of the leaves or dew:

> 'An Aurelia is no Egge, and it ought not to be called a Generation, but a transformation of a Caterpillar into this, and of this into a Butterfly ... they have their original from the death of Caterpillars, which as they do waste by degrees in certain dayes, so by degrees their covering becomes continually more hard, and changeth into an Aurelia. These again the next Spring or Autumn, by degrees losing their life a Butterflie comes forth of them that is bred by the like metamorphosis'; [1]

and, of butterflies, 'they appear in the Spring-time out of the Canker-worms Aureliae, growing by the heat of the Sun'. [2]

The aurelia was for Moufet a living thing, which must die before its flesh can be transformed; its lack of movement and inability to feed are no more a certain sign of its inanimate nature than are the same features in a hibernating dormouse. Further, he mentions what are clearly ichneumons or chalcids as evidence that

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1. Moufet. p. 1124.

2. Ibid. p. 1040.

the corruption of an aurelia does not always produce the same result. [1] To anyone not understanding the life history of these parasites, this must have been a strong argument for the distinct natures of pupa and imago and the corruptive nature of the change from one to the other.

Such attitudes to the problem of insect metamorphosis must be taken as orthodox for the middle of the seventeenth century. Jan Swammerdam, though disagreeing with this interpretation, wrote, when dealing with this problem in 1669, of Moufet's good reputation and of the many authorities consulted by him. Swammerdam also quoted John Ray's catalogue of Cambridgeshire plants as giving a similar opinion to that of Moufet: '_Illud alio animali deponitur, exors actualis vitae et motus_: _Aurelia a nullo deponitur, sed ab uno in aliud transformatur_'. [2] William Harvey also appeared to agree that the caterpillar represents a source of material from which the imago is produced by metamorphosis in the contemporary sense. He refers to the animals

> 'whose generation takes place by metamorphosis, and of what kind is the pre-existent material of insects which take their origin from a worm or caterpillar; a material from which by metamorphosis alone, all their parts are similarly constituted and embodied, and a perfect animal is born'. [3]

Elsewhere, however, he refers to the larva in Aristotelian terms:

> ' ... in respect of a fly, moth or butterfly, whose

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1. Ibid. p. 1040.

2. Swammerdam 1685 trans. of 1669 pp. 29-30.

3. Harvey 1651 p. 338.

primordium it is potentially, it is as a creeping egg .. ceasing motion, it is like an egg, an animal potentially.' [1]

It is ofcourse, impossible to impose on to seventeenth century thought modern ideas of the individuality of organisms and of species, and equally, Harvey's description of the caterpillar as being a primordium with the potentiality of a butterfly or moth cannot be fitted precisely into modern terms.

That the interpretation of insect life cycles was still confused up to the 1660's is shown by some of the statements in the work of Goedart; though realizing that caterpillars may emerge from eggs laid by butterflies, Goedart also believed that they may also be the offspring of other, larger caterpillars. [2]

He regarded larva and imago as distinct animals, the latter of which may, but need not, be metamorphosed from the body of the former, which in turn may or may not be hatched from eggs laid by the adult.

Even the work of Robert Hooke shows a similar confusion. One of his series of microscopical observations contained a detailed account of the metamorphosis of the gnat, including a description of the emergence of the imago from the nymph: Hooke claimed that he was the first writer to describe this. He appears to have thought of the larva as being simply a young gnat with a different form from that of the adult. However, he insisted that not all gnats, even of one species, were so produced:

> ' .... indeed, so various and seemingly irregular are the generations or productions of Insects,... for not only the same kind of creature may be produc'd from several kinds of ways, but the very same creature may produce several kinds ... the All-wise God of Nature

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1. Ibid. p. 459.

2. Goed. p. 46; Lister wondered whether the Latin original may be faulty here.

may have so ordered and disposed the little Automatons, that when nourished, acted or enlivened by this course, they produce one kind of effect, when by another they act quite another way, and another Animal is produced'.[1]

Such a mixture of attitudes towards this subject might be expected from a man of such ability and originality whose ventures into Biology were, though fruitful, sporadic and unsystematic.

However, from about 1670 it was becoming generally accepted among naturalists that the change from caterpillar to pupa and imago was internal to the life history of a single individual, and not a metamorphosis in the old sense; the attitude taken by Swammerdam in 1669 was substantially modern. This change was no doubt brought about by the rejection of the idea of spontaneous generation, and the acceptance of an egg of some sort as the common origin of all animals - even of man. If larva and pupa are stages which regularly separate such an egg from an adult similar to that which laid the egg, they must be stages in the life cycle of the adult itself, and not distinct individual organisms. Though it is impossible to be sure exactly what was meant at this time by saying that a caterpillar 'changes into' a butterfly, it is likely that Francis Willughby, writing of such a change as being 'natural' and 'according to the usual course of Nature', no longer accepted interpretations such as that of Moufet.[2]

Lister's period of interest in entomology coincided with

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1. Hooke 1665 pp. 93-4.

2. Swammerdam 1682 translation of 1669; Phil. Trans. 6 (1671) p. 2279.

this change in outlook, and no doubt his work helped to establish the new interpretation. His work on plant galls and kermes assumed that larva and imago were the same individual, and his studies of ichneumons removed one of Moufet's chief arguments in favour of the word 'metamorphosis' - the observation that similar larvae and pupae often produce different imagines. Lister was able to adopt the view that these changes were constant and predictable for any one kind of larva; his early notes on kermes mentioned that 'none of the Maggots were yet in Nympha, so that you cannot expect from me a description of the Bee or Wasp they will turn into, when they come to perfection'.[1]

Lister's interest in the life histories of insects must have been among the earliest of his activities in natural history. He referred to caterpillars which he had found at Calais and Montpellier, and wrote papers on the life history of a viviparous fly which he observed in May 1666, very shortly after his return from France, and on a bee, made during his stay at Bessenburn, Cambridgeshire, during the plague summer of the same year.[2] By 1675, when his edition of Goedart was completed, he understood the situation well.

Lister observed carefully the way in which adult butterflies lay their eggs after having carefully selected the correct food-plant, so rejecting the still widely-held idea of their spontaneous generation:

> ' ... this Catterpillar hath its beginning from the Egg of such a Butterfly; and so, probably, have all Catterpillars whatsoever their beginning from the Eggs of their respective Butterflys ... for I cannot think, that this or any Animall else is spontaneously produced by the plant, or any cause

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1. Phil. Trans. 5 (1670) p. 2166.
2. Phil. Trans. 14 (1684) pp. 595-6.

else whatsoever, but the Animall parent'. [1]

He also noticed the viviparity which is common among flesh-flies, and the laying by butterflies of infertile eggs, which would have been normal had the 'Male first made them Prolifick'.

The larvae hatching from these eggs were seen to be continuous with the pupa and the adult, though the changes were admitted to be profound:

> 'I am moreover conceited, that the change of a Catterpillar is not superficial only, but goes deeper yet, and that the intestines are in some sort changed also ... the inside of the Gutts being indeed an outside too in all animals'. [2]

The idea that the lining of the gut is part of the body surface is one which Lister often made, though it rather contradicts his point here. He realized the relationship between pupa and imago and the nature of the change between them: 'All the parts of the Butterfly are budded in the Chrysalis; But are not sprouted, Explicate and hardened'; even the eggs are present in the female pupa. [3] Lister had clearly carried out dissections of chrysalids. That his outlook on the nature of metamorphosis and the functions of the different stages was essentially modern can be seen in his statement that

> 'the Butterfly is the Mother insect in perfection, and the Catterpillar, its Aurelia or Chrysalis, are but certain disguises for a time, wherewith one and the same Animal is by Nature invested for divers ends, viz. that of the Catterpillar to eat such and such food; This of the Aurelia to perfect and harden its limbs'. [4]

It is difficult to be certain whether Lister was indebted in

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1. Goed. pp. 2, 13, 23, 79.

2. Ibid. p. 6.
3. Ibid. pp. 56, 26.
4. Ibid. p. 2.

any way to the work of Jan Swammerdam (1637-80). Swammerdam's History of Insects was published in 1669, but in Dutch, which Lister could not read; French and Latin translations were brought out in 1685. His Ephemerae vita was published in Latin in 1675, by which time Lister claimed to have completed his Goedart edition, with an English translation in 1681.[1] Swammerdam is usually said to have originated the preformation theory with his demonstration of the imago of a butterfly contained within the exoskeleton of the pupa. In fact, he did not advocate extreme emboîtement, but he did vigourously attack what he called transformation theories, in which living flesh was thought to be transformed from one form to another. He included in his condemnation the works of Aristotle and Harvey, who both advocated metamorphism in the old sense, and also the less subtle ideas of Moufet. Instead, Swammerdam stressed that the larva and pupa both possess buds of tissue which expand slowly like a flower bud, into the organs of the adult, giving complete continuity of structure without any fundamental break during the animal's life history. In support of this, he claimed that his dissections showed the complex structure of the pupa in which the adult organs were almost fully formed.

Swammerdam's understanding of metamorphosis in the modern sense was much deeper than that of his contemporaries; he stressed the similar natures of what are now called obtect and exarate pupae, and distinguished between apodous, campodeiform and eruciform larvae; he realized the difference between true and false legs in the latter, and saw the limb buds in the apodous forms.[2] His view of the changes between larva, pupa and imago were similar to those of Lister, but were supported by dissections of a standard far in advance of the latter's. However, Lister certainly had not read Swammerdam's book

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1. J. Swammerdam, Ephemerae vita (Amsterdam, 1675); English translation by E. Tyson, Ephemera vita, or, the Natural History and Anatomy of the Ephemeron (London, 1681).
2. Swammerdam 1685 trans. of 1669 pp. 7-41.

in 1671, and he makes no reference to it in his edition of Goedart. Lister was always jealous about his own rights of priority, and was equally careful with other people's rights. In 1696, he was to write that the fact that the fly was contained within the pupa had been demonstrated by 'myself and others, and most of all by Swammerdam'. [1] He made other remarks disparaging himself in comparison with Swammerdam, and in the absence of any reference in his earlier works to the latter, it is likely that Lister was unaware of this work while he was studying the problem of insect metamorphosis in the early 1670's.

Lister appears to have been abreast of rather than ahead of the best contemporary opinion in this matter. In any case, the significance of the changes of insects was largely a matter of change of outlook produced by new ideas on generation rather than the result of anaccumulation of observational detail; we would therefore expect it to be realized independently by more than one competent naturalist at this time. Nevertheless, the understanding of problems such as those of gall-wasps and ichneumons, in which Lister was prominently involved, must have helped in this change in outlook.

E. Barnacles.

A special case of the changes in body form in animals was provided at this time by the barnacle goose legend. According to this, barnacles of the genus *Lepas* (now known as goose barnacles) were supposed to metamorphose into geese of the genus *Branta*, two species of which occur in Western Europe in winter: *Branta bernicla*, the brent goose, and *Branta leucopsis*, the barnacle goose. However, as their scientific names indicate, there has been much confusion between the two species, and they were not clearly distinguished until

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1. EAT p. 106.

the nineteenth century. The legend must be taken as applying to both species. The birds nest in the high arctic, the breeding grounds of the barnacle goose being particularly remote, on inland cliffs in north-east Greenland and Spitzbergen, and not discovered until the twentieth century. The birds winter in north-west Europe, particularly Scotland and Ireland, but their breeding grounds were a mystery for centuries.

The goose barnacle is found on ships and floating logs, and this gave rise to the legend that they were the fruits of waterside trees whose limbs had dropped into the water. The shells of the barnacle form a black and white pattern on an egg-shaped body, attached to the log by a long stalk, the effect being strikingly similar to a miniature, headless black and white barnacle goose attached by its neck. The effect is heightened by the feather-like cirri, used as a food filter, which protrude slightly, at about the position in which wing-plumes might be expected in the goose.

It has been claimed that some designs on Mycenaean pottery illustrate this legend, though the story is absent from classical writers, and the barnacle goose does not occur in the Mediterranean. However, the story is known from very many sources in medieval and early modern times, from Neckam (1157-1217) onwards. [1] Down to the middle of the eighteenth cebtury the legend was being repeated by reputable naturalists, such as Hans Egede, and as late as 1783 Guettard found it necessary to attack the widespread acceptance of the story.

As an example of the credibility given to the fable in the

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1. E. Heron-Allen, Barnacles in Nature and Myth (Oxford, 1928) has collected a comprehensive list of writers on this subject from earliest times to the eighteenth century.

seventeenth century, the account of the 'Goose-tree, Barnacle tree, or tree bearing geese' given in Gerard's Herbal may be mentioned. This was published in 1597, but the description is not changed or commented upon in Thomas Johnson's Edition of 1633; the account clearly refers to Gerard's own observations, and not to those of Dodoens, whose work formed the basis of Gerard's book.

Gerard mentions stories that in Orkney and the north of Scotland, there grow trees bearing white shells, which, falling into the water, become geese. He does not vouch for this himself, but declares that

> 'what oure eies have seene, and hands have touched, wee shall declare. There is a small Island in Lancashire ..... wherein are found the broken pieces of old and bruised ships ... whereon is found a certain spume or froth that in time breedeth unto certain shells ...which in time cometh to the shape and form of a Bird ... in a short space it cometh to maturitie, and falleth into the sea, where it gathereth feathers, and groweth to a fowle bigger than a Mallard, and lesser than a goose ... for the truth thereof, if in any doubt, may it please them to repaire unto me, and I shall satisfie them by the testimonie of good witnesses.' [1]

Even in Lister's time, the Philosophical Transactions carried a paper by Sir Robert Moray, describing how the author found, washed up in Uist, logs covered with

> 'multitudes of little Shells, having within them little Birds, perfectly shaped, supposed to be Barnacles ....

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1. J. Gerard (1545-1612) The Herbal, or Generall Historie of Plants (London, 1633 edition of 1597 original) book 3 page 171. The barnacle goose is considerably smaller than other species.

the Bird in every Shell that I opened ... I found so curiously and completely formed, that there appeared to be nothing wanting ... for making up a perfect Sea-Fowl ... that the whole looked like a large Bird seen through a concave or diminishing Glass, colour and feature being everywhere so clear and neat'. [1]

There were many variations in detail; Athanasius Kircher, for example, thought that the barnacles grew from material spilled into the sea from broken goose eggs. The matter was of theological importance, as a bird growing from shellfish could be eaten in Lent, and there were several papal pronouncements on the subject. It was also of general biological interest, especially in relation to the general problem of metamorphosis; the barnacle goose change was in fact compared directly with that of the silkworm to moth, by Bertius at the opening of the seventeenth century. [2] However, some scepticism was growing by the middle of the century. There had always been some opposition, though some of this was ill-founded; Albertus Magnus and Pierre Belon claimed to have seen the birds breeding, which is unlikely, and William Harvey confused the birds with the gannets or solan geese which breed in Britain. However, brent geese were found breeding in Nova Zemblya by the Dutch in 1596, and the changing outlook on generation and metamorphosis slowly brought about a rejection. Martin Lister made the first real attempts to study the anatomy of barnacles, and this certainly played a part in this rejection. For example, Robert Moray's account was used by John Hill in his attempt to discredit the Royal Society:

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1. Phil. Trans. 11 (1677) p. 926.

2. Heron-Allen p. 47.

'The World ... did not know the Character of the Royal Society so well as it does at present: People believed it to be a Set of sensible and honest Men: And what could happen in Consequence of such an Account ... but a general Belief ... that the Geese were in reality thus bred'. [1]

Hill was, of course, writing propoganda; but in discrediting Moray he went on to describe the nature and anatomy of the goose barnacle in a way which shows that he used, without acknowledgement, Lister's description of the animal of 1696.

In his Historiae animalium Angliae of 1678, Lister illustrated and described, as Titulus XLI of the marine molluscs, the Balanus Cinereus, velut e senis laminis striatis compositus, ipso vertice altera testa, bifida, rhombide occluso. This was the modern Balanus balanoides, an acorn barnacle, which was not connected with the goose legend. It was classified as:

Marine molluscs: Section I Turbinates ie. snails
Section II Bivalves
Section III Univalves
a. Mobile - limpets
b. Immobile - Balanus

Ten years later, in the Historia conchyliorum, both forms of barnacle were illustrated, with a crude dissection of Balanus showing limbs, visceral mass and penis. The taxonomy had been changed, and now showed:

Book III Marine bivalves
Part I Uneven bivalves, eg. oysters;
Part II Even bivalves, eg. mussel;

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1. John Hill, Memoir of the Royal Society (London, 1751) p. 105.

Part III Multivalves

Section I Pholids

Section II *Concha anatifera*

Section III *Balanus*

Pholids are bivalve lamellibranchs with, in addition to the normal two valves, a small and slender piece of shell along the crest of the hinge (*cardo*). On the strength of this, the multivalve bivalves (sic) were placed together, apparently in an attempt to integrate the five-shelled goose barnacles and six-shelled acorn barnacles with the molluscs. As will be seen below, Lister appreciated the problem of the cirri, but this was not apparently enough to make him attempt to classify the barnacles with any group other than the molluscs. In this, he resembled all later zoologists until the nineteenth century.

Lister's main published account of the goose barnacle was that in his *Exercitatio anatomica tertia* of 1696.[1] This contained dissections of a dozen bivalves, including the *Concha anatifera*; this was still placed with the pholids. The five shells (four principal, one in the hinge) were described, and the cirri were categorically stated not to be feathers. They were carefully described, and their relative lengths and biramous nature noted. Lister's comparison, in function at least, was with the limbs of a squid (*polyporum genere*), which was of the same group (*mollibus*) as the barnacles were supposed to be. In both animals, the mouth was at the centre of the limbs. Detailed descriptions were given of the peduncle and the large adductor muscle, but the description and plates of the anatomy of the viscera are below Lister's usual standard. The *meconium* (a word usually used to describe the so-called liver of molluscs) which he describes appears to be the testis, and the large 'duct' which he

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1. EAT pp. 94-101.

figure is almost certainly the seminal vesicle. Lister admitted that he had never seen live goose barnacles, as the freshwater of the Thames killed those carried by ships before they reached London.

Lister appears to have thought of publishing a comprehensive paper on the nature of barnacles. There are three manuscript drafts, all incomplete, in the Lister papers; two are in English, one in Latin. They are undated, but they contain ideas not found in Lister's account of 1696, and they probably date from about 1705, when Lister was in correspondence with Robert Sibbald of Edinburgh, asking for information on the Concha anatifera.[1]

Lister names the authorities he has consulted on these animals, listing Lobelius, Licetus, Mayerus, Sennertus, Wormius, Margravius, Clusius, Rondelet, Bartholinus, Aldrovandus, and Fabio Colonna. He lists the species of barnacle described by these writers, giving four:

(1) the Balanus of Rondelet, whose description is quoted. It is presumably Chthamalus stellatus of modern writers, but the length given by Rondelet (an impossible five inches) is so great that Lister supposes he has not seen this animal. Presumably the length was misquoted, mistranslated or wrongly converted from one kind of unit to another.

(2) the Concha anatifera major of Bartholin;

(3) the Vulgaris concha anatifera minor of Bartholin; and

(4) the Datteri Genuensibus sive concha tenuis testa of Aldrovandus - also not seen by Lister.

Lister's own Balanus cinerea of 1678 is not listed.

These animals are classified as belonging with

'ye musculi or pinne: The common nature of which are to

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1. MSS Lister 39 ff. 124, 126, 33.

be immovable fastened to a place to put out cirri or seres, both which the Conch anat. have in common with them'.

He lists the valves contained in the shell, the fifth shell (the carina) being an appendix. Though the pholads show that other molluscs may have such a shell, Lister was still unsettled that the barnacles should have as many as four other shells. To get over the problem, in these papers he makes the original suggestion that each barnacle is in fact a double animal, consisting of a male and female joined together, each contributing two valves to the common shell, the pair being 'tackt together by fifth common shell'. The two animals were supposed to be joined at the mouth, so explaining why this was single. Lister suggested that the male was above (superior) the ventral female; as he says the male is shorter, and with shorter cirri, this would make it the anterior part of the animal. This attachment of male and female was thought to be necessary to allow mating, otherwise impossible because of the sessile nature (loca fixi) of the animals:'for that probablie without ye coition of male and female there is noe birth of any animal in nature', so we may disregard the idea of spontaneous generation for barnacles (persenerabimus in sponte natis). It is, of course, purely coincidental that in many species of barnacles the female carries a small, parasitic male attached to her and used only to fertilize the eggs. This does not apply to Lepas or to any barnacle found in British waters.

Lister wrote that 'Most say they produce Birds of the Goose kind'. He had already rejected this as a fable in 1696; in his Latin manuscript he notes that the cirri, one of the main arguments in favour of the goose legend, in fact resemble insect limbs more than feathers: non tantum in avibus sed et in insectis ut innumeri exemplis constat. This is a very valuable observation, though without a clear conception of the differences between the natural and artificial systems of classification, it could not lead to barnacles being correctly classified with the arthropods.

Lister may therefore be supposed to have contributed to the decline of the barnacle legend by making the first anatomical study of the animal, and by supposing it to be adult, and therefore to be incapable of the sort of metamorphosis needed by the legend. On the other hand, his own wild speculation on the double nature of the animal, unaccompanied by any anatomical evidence other than that of the number of shell valves, is rather out of keeping with his normally conservative and cautious, not to say unimaginative, approach.

(3) The nature of the primordium of living things.

Like most observant naturalists after 1670, Lister rejected the idea of spontaneous generation. It was therefore necessary for him to give his opinion on the nature of the primordium from which animals develop.

a. The origin of the genital secretions.

Aristotle defined semen as 'that which comes from the generating parent ... and it is that in which a generative principle is first found'.[1] He claimed that it was not a coliquescence formed by decomposition within the body, but a concoction from the residue left over from those useful parts of the food which are normally distributed over the whole body. Menstrual fluid was a similar residue, but less highly concocted, and, like ordinary blood, it provided the material for the growth of the body. The form of the body was provided by the semen, which contained potentially all that the offspring showed in actuality. This theory failed to account for the resemblence between mother and offspring, and Galen therefore suggested that matter and form were derived equally from the genital secretions of both parents.

Both these theories were shown to be false by Harvey's

1. Aristotle, De generatione animalium 274 b.

demonstration that the uterus of the pregnant doe does not contain any mass of material which could be derived from either male or female secretions; but as Harvey gave no alternative explanation of the nature of these secretions, there was a gap in this part of the theory of generation about 1670.

Lister's first attempt to deal with the problem was in De fontibus medicatis of 1684. This account of English medicinal mineral waters contained a description of Lister's theory of the formation of the body fluids, including the semen. He claimed that the chyle absorbed from the gut was desalinated by the kidneys and skin, the pure residue being used to form pituita (phlegm, mucous, and intestinal secretions), the purest of all being used for the production of semen. The fineness and refinement of this fluid are shown by its exceptional softness, and by the weakening suffered after its loss. In gonorrhoea, the diseased semen is made in large quantities from vitiated lymph, because of the disturbance caused to the purifying mechanism; this is similar to the production of large amounts of diseased phlegm in catarrh.

This is the same kind of explanation as that of Aristotle, though the latter's 'concoction' is more active than the rather negative process of purefication suggested by Lister.

This account of 1684 contains no suggestions as to the nature of the fertilizing power of the semen. The discovery of the spermatozoa by Antoni van Leeuwenhoek (1632-1723), in 1678, was ignored. It is likely that this is because Lister's book was written before about 1676. As shown elsewhere, Lister did little work from this date until his move to London in 1684, and his increased literary activity in 1682-5 appears to have been no more than the publishing of old work brought out at this time to help to establish his name in London medical and scientific circles.

Lister returned to the subject of the nature of semen in two of his major works, the Exercitatio anatomica tertia of 1696, and the

Disertatio de humoribus of 1709. These accounts overlap in subject matter, but are not identical; they deal with the origin and nature of the semen and of the seminal animalcules.

It was again stressed that the semen originates from the chyle carried in the blood; this is the reason that castrated animals, which produce no semen, become so much fatter than entire males. [1] Lister rejected Francis Glisson's claim that the semen originates in the nerves, since, though the blood vessels to the testes are large and numerous, the nerves running to them are few and minute. He quoted the authority of Diemerbroek for this, so that he had presumably not made special dissections for this purpose himself. Further, the testes are glandular by nature, as de Graaf had shown them to be dense masses of tubules and blood vessels, as are other glands. [2] The semen boils out (excoquitur) from these tubules and the epididymis and seminal vesicles; there is only one kind of semen, as the prostate secretion is for lubrication only. [3]

Lister stressed a point in connection with the semen which he had earlier made about the production of humours in general: that the changes produced in digestion and chyle formation were fundamentally the same as putrefaction, accelerated by body heat. The partly putrid nature of semen, shown by its bad smell, was important to Lister, as he used it to account for several effects in pregnant women thought by him to be caused by the seminal fluid. It produces headaches and vomitting, dizziness, and changes in appetite and imagination, in the production of bile and in the mammary glands: 'Haec omnia mulierum tanta mala a

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1. Hum. 398.

2. Ibid. pp. 392-4.

3. Ibid. p. 396.

tantillo humore, quale est semen virile, summa putrefactione, orta sunt'. These effects are caused by the semen, and not by the foetus, as they disappear later in pregnancy, when the embryo is largest. Lister overlooks, of course, the fact that these changes are not produced by the semen in a woman who does not conceive. [1]

b. The fertilizing power of the semen.

Lister had less to say about the power of the semen to produce ferilization. He made a suggestion that at the start of life, the beat of the heart in the egg may be accelerated by the burning or tearing action of the semen, the heart being preformed in the egg and already beating imperceptibly before coitus; this does not of course give any explanation as to how the effect would be produced. He also suggested that the power of the semen might be similar in some ways to that of poisons. Some of what Lister considered to be side effects of semen were thought by him to be caused, as already mentioned, by its putrid and partly poisonous nature. It was claimed that the genetic powers of the fluid were also

> 'not out of harmony with the infective powers of many poisons, of which many instances can be collected, such as that in the bite of a mad dog, which produces changes in many ways approaching those of dogs, as in the gape of the jaws, the method of eating and drinking, and even in the voice'. [2]

The subject of poisons and their action was of concern at the time, and Richard Mead's (1673-1754) important book on the subject referred to Lister's work on hydrophobia. This disease was one of those dealt with in Lister's Sex exercitationes medicinales of 1694, and he had written a paper on the subject in the Philosophical

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1. Ibid. p. 395.

2. EAT p. 117-18.

Transactions.[1] It would have been important to Lister to develop this suggestion, since, like other ovists, he was concerned to explain the genetic influence of the father on the offspring. He particularly mentioned this problem in connection with the mule and in Leeuwenhoek's description of crosses between male wild and female domestic rabbits, in which the male parent appeared to produce the greater effect. However, Lister contented himself with writing that 'So far, the male virtue is obscure, and I am not one who delights in conjectures'.[2]

The explanation accepted by the majority of Lister's contemporaries was that Leeuwenhoek's spermatic animalcules were the precursors of the embryo. Lister, however, accepted the rival ovist theory, which, though slightly pre-dating animalculism, was not to be generally accepted until the mid-eighteenth century.

c. Ovism and animalculism.

Shortly after the discovery by Regnier de Graaf (1641-73) in 1672 of what he took to be the mammalian ovum, Lister described in manuscript how he

> 'kept a bitch whelp until it was 7 or 8 months old in order to ye most clear discovery of the eggs in the ovaries, I dissected har about 10 dayes after she had taken the Dogg, and I found, as I expected, the inflamed cicatricula mentioned by de Graaf, but more especially the eggs in this young Bitch were very clearlie visible through the fine and tender skin which covered them, and continued so in spirits of wine, where I kept ym to admiration.[3]

A further clear statement of Lister's ovist outlook was included in his English edition of Goedart, completed by 1675. After

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1. Phil. Trans. 13 (1683) p. 162.

2. EAT pp. 116-18.

3. MSS Lister 39 f. 92; probably about 1674.

describing the laying of eggs by an unmated butterfly, he wrote that

'the Eggs of Butterflyes are to be found in their bodies whilest they are in the Disguise of a Chrysalis, and are undoubtedly essential parts of the Female, as much as her legs and Wings, and in no wise generated by the Male'.[1]

- though he recognized, of course, that the eggs could only be fertilized by the male.

Lister referred to his dissection again in 1696, repeating the claim that what he had seen were just as much eggs as were those of birds, fishes and insects.[2] He admitted that the transference of the egg from ovary to uterus had not been demonstrated, but

'do you deny the circulation of the blood through the minute vessels in humans because it has not been demonstrated, even though it has been clearly shown in many insects, birds and fishes?'[2]

Lister refused to believe that these eggs were 'altogether useless'; the ovaries were not 'rudimentary testes, like the male nipples'; and the experiments of Swammerdam, de Graaf, Malpighi, Payer and others of good faith were not mere dreams. 'Certainly the foetus is to be looked for in all eggs, and not to be found elsewhere'; further, it was doubtless formed in the egg before mating occurred.[3]

Nevertheless, in view of the ovists' inability to answer satisfactorily the part played by the semen in generation, the claims

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1. Goed. p. 26.

2. EAT p.111.

3. Ibid. pp. 109-10.

of the animalculists had to be answered.

The more extreme animalculists, such as Leeuwenhoek, claimed that the foetus originated solely from the spermatic animalcule, after tha tail had been discarded. [1] Lister asked why, in that case, do androgynes (that is, hermaphrodites) such as snails, need to copulate? Each animal has its own sperm; why should anything else be needed? And why are the animalcules only observed as minute, tailed structures? If they grow into embryos, why are they never found to have lost their tails and to have grown larger? And, in particular, for what reason could they be produced in such enormous numbers? It is 'absurd that, to make one animal, innumerable thousands of living creatures are wasted'. In oviparous creatures, the difficulty of reaching the goal (ie. an egg in which to grow) and of entering it could be pleaded; but in humans, in which there is no (shelled) egg, what excuse can there be for the 'loss of so many fine hopes for children?' [2] As a minor point, Lister claimed that doves lay eggs which produce male chicks, alternately with eggs which produce females; how could such regularity result from a chaotic mass of animalcules? [3]

Besides these points of detail, there was an important matter of principle. If the foetus develops from an animalcule how are the animalcules themselves produced? They are so clearly independent animals that Lister, not having a cell theory at his disposal, ignores the possibility that they are a part of the structure of the man himself; in any case, to say that would have been to evade the question

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1. <u>Phil. Trans 12</u> (1678) pp. 1040-45; <u>15</u> (1685) pp. 1120-34.

2. EAT pp. 113-14.

3. Hum. p. 397. This is still a widespread piece of folk-lore; hence the expression 'pigeon-pair' for a boy-and-girl set of twins.

of generation. The animalcules are clearly produced continuously over a period of many years, and the only possible origins for them, in Lister's view, were spontaneous generation within the seminal liquor, or by reproduction from other seminal animalcules. The former was contrary to the general outlook of Lister and most of his contemporaries, including, as Lister pointed out, Leeuwenhoek himself; but if the second alternative be accepted, the animalcules must themselves be mature animals. How then can they grow into a new infant human being and undergo a second maturation? This, it seems, would be to revert to an idea of metamorphosis such as that recently discarded. Leeuwenhoek, claimed Lister, had not faced up to this dilemma.[1]

A less extreme version of the animalculist theory suggested that, though the spermatozoan was the true primordium, an egg was produced by the female, and this provided an essential nidus in which the animalcule grew. Leeuwenhoek rejected this, but the idea was widely accepted at the end of the seventeenth century.[2] But Lister claimed that, though the animalcules had been observed in the uterus, they had never been seen in an egg, nor could any mark of entry be demonstrated in a fertile egg. Such an entry would, said Lister, be occult: 'this connection or marriage of yours between egg and animalcule is plainly marvellous, because they would be making a native out of a stranger (ex advena plane indigena fint)'. By this, Lister appears to mean that the animalcule, to him a distinct living organism, must according to this theory, be amalgamated with

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1. Phil. Trans. 20 (1698) p. 337; EAT p. 102.

2. Andry de Bois-regard, op. cit; Garden, Phil. Trans. 16 (1691) pp. 474-82.

the product of a different species, a human ovum. Even without a clear species concept, this would be hard to accept; but it does, of course, involve the acceptance in advance of Lister's view of the spermatozoan, and so can hardly be used as an argument in favour of such a view. [1]

Even if it were suggested that the egg were no more than a source of food for the animalcule, in the same way that some grubs feed on nut kernals, Lister still claimed that this would not agree with observation. Such grubs, as he knew from his own investigations, produce a large amount of excrement; yet such an accumulation, and the corresponding wasting away, is never seen in fertilized (impraegnatis) eggs. In any case, such a theory would not be an explanation of generation, as it would not show how the organs of the foetus originated: 'on this you are silent'. It is not sufficient to show where the matter of these organs comes from; an account of generation would have to explain organization, which is the essence of an organism. If the animalcule already has organs, which merely grow as a result of the consumption of the egg, then generation in the strict sense must already have occurred at an earlier stage; if the animalcule has no organs, then mere demonstration of the source of the material for their growth is no explanation of their origin. [2]

As a final argument against the entry of the sperm into the egg, Lister noted that, after a single copulation, a hen will lay fertile eggs for several days in succession. It is clear from this, and presumably also from dissection, that the eggs are at different stages in development; where do the sperms live until each egg reaches full development? In any case, Lister claimed there were no spermatozoans in the semen of the cock. [3]

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1. EAT p. 110.

2. Ibid. p. 112.

3. Ibid. loc. cit.

This is a strange oversight, as Leeuwenhoek had already mentioned 'animals of the Semen of the Cock' in the Philosophical Transactions.[1]

Leeuwenhoek replied to Lister's criticisms of 1696 in a paper sent to the Royal Society in 1699.[2] None of his replies appear to be well thought out or to answer Lister's points. Thus, Leeuwenhoek claimed that, as size is relative, the smallness of the spermatozoan is no bar to their being human primordia; after all, we cannot see an apple tree in a seed, but it must be there, However, the last argument assumes in advance the point which Leeuwenhoek assumes to be at issue; and Lister had made no objection based on the small size of the spermatozoans as such, but only on this small size being constant, with no larger individuals in the process of growth being seen.

Leeuwenhoek also pointed out that if a cod be stripped of its (soft) roe, more is produced, so that some remnant must be left behind which is able to grow. This hardly answers Lister's comments on the origin of the spermatozoans; indeed, it appears to concede the point that the animalcules are generated from themselves. Leeuwenhoek then contradicted himself by denying that the sperms could be generated 'of themselves', since they must carry the qualities of the parents, which otherwise would become mixed. By this he presumably means that hybrids would occur unless parental characteristics were were transmitted en bloc. This argument could be used equally well in support of ovism, and in 1709 Lister wrote that he could make nothing of this point.

4. The nature of the spermatazoa.

If the spermatic animalcules are not involved in generation, what then is their nature? Lister was quite certain: they are parasites, whose relation to the host is the same as that of intestinal

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1. Phil. Trans. 15 (1685) p. 1126.

2. Phil. Trans. 21 (1699) p. 270-71.

worms. Indeed, there is a great similarity between the two and their environment in the body, as both flourish in putrid material. Digestion, for Lister, is a process of accelerated putrefaction, and so is the production of semen, some of whose properties are, as mentioned above, due to its putrid nature. This does not mean that putrefaction is the cause of the generation of the parasites - they are both stated to be produced by their own kind; but in these conditions food is more readily available:

> 'in this refuse (sentina) of the spermatic ducts, they generate abundantly, as this seminal humour accumulates daily in large quantities, so that there is more opportunity for the generation of insects than in other body fluids which are evacuated continuously or at frequent intervals; such as saliva, urine, milk, sweat, visceral phlegm and bile, in which, as even Leeuwenhoek admits, no worms are to be found'. [1]

In the gut, however, conditions are similar, and here worms are to be found in all animals. Both the gut and the seminal ducts, like the gums and teeth, open directly to the outside air, and are for this reason more likely to be wormy and evil-smelling; and this is why mature animals and humans smell worse than infants. (Lister ignored the fact that several other glands open directly to the air). The remains of dead sperms were thought to add to the putrefaction inthe ducts, as, unlike waste from the host, they are non-perspirable - a point in which the influence of Lister's editorship of Sanctorius can be seen. [2]

No explanation is given of the means by which the man becomes infected; Lister was similarly unsure of the origin of gut parasites, as has been shown.

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1. EAT p. 114.

2. Phil. Trans. 20 (1698) p. 337; Hum. p. 397.

The parasitic nature of the spermatazoa does not, in Lister's view, prevent them from having a definite function (usus) in reproduction. His attitude seems inconsistent here: though the animals are adventitious to man by nature, they are essential to reproduction. They are needed to incite sexual desires by their movements in the seminal ducts, and this is confirmed by the lassitude and dejection produced after emission of the stimulating 'worms'. [1]

Lister fully admitted his inability to explain the fertilizing power of the semen, and the consequent difficulty in relying upon the ovist position; in particular, the problem of the genetic influence of the male parent. However, he pointed out that strict animalculists have a similar difficulty in accounting for the influence of the female parent. [2]

e. The female genital secretions.

Lister rejected the notion of female semen. There are no testes in women (no attempt is made to compare ovary and testis, either in structure or function) and the vaginal secretions are merely pituita, secreted as lubricant.

The menstrual blood was, for Aristotle and Galen, a source of material from which the foetus was formed. Harvey had shown this to be untrue, and to both animalculists and ovists the menstrual fluid was irrelevant in this sense. Lister looked upon it as a secretion from a mass of uterine glands - which is a reasonable description of the endometrium and its tubular glands. The fluid is not a simple haemorrhage; though it contained the 'red globules of blood', it was not itself blood, being much thicker. It was thought by Lister to be a secretion, built up from chyle by the glands for the nourishment of the embryo. Menstruation occurred only in women because, in other mammals, the uterus wall was more turgid, so holding the uterine glands

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1. EAT p. 116; Hum. pp. 396-7.

2. EAT p. 117.

closed. In a non-pregnant woman, the wall loses its turgidity at intervals. However, the inflamed parts and coloured pituita often seen in other female mammals showed that 'women may not be the only animals to have menstruation in the strict sense'. [1]

4. Summary of Lister's work on generation.

The second half of the seventeenth century saw an intense search for causal explanations in all fields of science. Hopes for such explanations of the problem of generation were raised by the observations of men such as Redi, Swammerdam, Malpighi, Leeuwenhoek and Lister; however, science needs more than the collection of observation. Imaginative speculation, keeping in contact with known facts but leading to a controlled search for others, is also required.

Lister's familiarity with his subject-matter, including, in his early days, practical field-experience as well as dissection led to the accumulation of a number of pieces of factual information, to the rejection of some of the more extravagant ideas current at the time, and towards a solution of certain problems requiring practical observation rather than theoretical interpretation for their solution. On the other hand, his enthusiasm and involvement with detail often made it impossible for him to take a sufficiently wide view of his problems, and he was over-prone to look for analogy where none need exist. It was, for example, the success of his observation on plant parasites which led him to reject the idea of the egg providing food for the spermatazoan; if a gall-wasp grub left a mass of excreta in such circumstances, so should a spermatazoan. His only venture into radical speculation, on the matter of the double nature of barnacles, was unfortunate since, having refused to speculate for so long, he was now unable to do so in a controlled manner.

Lister's methods often led him to a successful understanding of

1. Hum. pp. 401-4.

one problem and to failure in what he regarded as a related problem and to which he had therefore transferred his methods and ideas. His suspicion of radical, semi-occult changes in the bodies of animals, his clear understanding of the meaning of maturity in individuals and his careful observation led him to a solution of the problem of insect metamorphosis; and he was able to distinguish this situation from that suggested by the barnacle legend. Similarly, his careful field-work produced an almost complete explanation of insects parasitic on plants and on other insects, and an understanding of the separate individuality of host and parasite and the independence of their generation.

However, transference of these ideas of metamorphosis and parasitism to the problem of the seminal animalcules led Lister into error. This cannot be counted as a major blunder on his part, since a correct explanation could not be put forward at the end of the seventeenth century, for both theoretical and technical reasons; indeed, this remained true until well into the nineteenth century. Nevertheless, men with less first-hand knowledge of parasites and metamorphosis than Lister were able to adopt explanations of the nature of spermatozoa which were closer to the truth.

Besides this uncritical tendency to analogise, Lister sometimes selected his arguments in a way which was self-contradictory, as with his comments on the possibility of gall-wasp larvae being nourished from plant vessels.

Lister, of course, shared with his contemporaries certain limitations in biological theories which made it impossible to give an adequate explanation of generation. Lack of a cell theory inevitably makes the entire animal body appear to be the only biological unit, so that the spermatozoan appears to be comparable directly with the complete human being. The idea of conversion of one to the other was bound to be troublesome to anyone such as Lister who had worked on metamorphosis with some success. Lack of a cell theory must also have reinforced the idea that an organism is essentially a set of organs. A suggested primordium such as the spermatozoan cannot therefore be the ultimate source of generation if it already has organs; but if it has not, a still more fundamental explanation is needed. It was this contradiction which was to lead others to the idea of emboîtement.

In any case, on questions involving microscopical detail, Lister was at best using second-hand evidence. He was no microscopist himself, and he had to call on the assistance of Nathaniel Johnstone of Pontefract when such work was needed. Indeed, he seems not to have taken such evidence very seriously or to have had much respect for it; in 1696 he wrote, rather cynically, that such observations had produced contempt for the ancients, and made the minutest animals equal to the greatest and finest.[1]

The strengths and weaknesses of Lister's outlook show up more clearly in the field of generation than in any other, and his work here demonstrates that enthusiastic and original observation, though capable of producing useful results in natural history, are by themselves inadequate in the more fundamental problems of biology.

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1. EAT p. 107.

Chapter IV Molluscs.

(1) Conchology before Lister. [1]

The molluscs form the second largest phylum in the animal kingdom, and are found in the sea, in freshwater and on land. Nevertheless, the study of the anatomy of these animals made little progress, except in the works of Lister, until the publications of Lamark and Cuvier in the early nineteenth century. This was to some extent because of the apparently strange structure of the animals, which not only usually lack limbs, but often do not have so fundamental a structure as a head or even a recognizable front end. Further, the organs which do obviously exist, notably the reproductive organs, are so complex as to defy simple explanation.

However, molluscs do usually have one striking, beautiful and sometimes economically valuable feature: the shell. So great is the contrast between the obscurity of the animal and the obviousness of the shell that the latter has dominated the study of these creatures until modern times. Even Lister, the pioneer of molluscan anatomy, and a man who well understood the importance of strictly biological criteria in systematics, based his classification of the molluscs on the artificial features of shell and habitat - a marked contrast with his insightful and natural spider classification.

No writer before the seventeenth century showed any greater understanding of the molluscs than Aristotle, some of whose best work was done on the cephalopods. These were thought by him to be a group

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1. The term 'conchology' is used here rather than the later word 'Malacology', though neither was in use in Lister's time. At present, the former is generally used to refer to the study of the shells and the latter for the study of the complete animal, though this useage cannot logically be justified; see P. Dance, Shell Collecting (London, 1966) pp. 270-4. The older term seems more appropriate in the context of the Historia conchyliorum.

distinct from the other molluscs, though he did realize that there were some basic similarities between the groups. The higher bloodless animals (invertebrates) were divided by Aristotle into

<u>Malakia</u> (molluscs, ie. cephalopods)

<u>Malakostraka</u> (crustaceans)

<u>Entoma</u> (insects)

<u>Ostrakoderma</u> (testaceans, ie. shelled molluscs) [1]

The Ostrakoderms were divided into univalves (limpets etc.), turbinates (snails) and bivalves, the relation between the three being emphasized by Aristotle's belief that the operculum of gasteropods was the equivelant of the second valve of the bivalves. He gave the basic testacean body a four-part structure: foot, head, mantle-sac and fins, where present. The simplicity wrongly attributed to these animals was thought to be because of their stationary habits. Aristotle claimed that, as bloodless animals, molluscs could have no viscera, which in his view were formed from blood; in his writings, the word covers liver, kidney, spleen, lungs etc. but not the gut. He did make some observations on the alimentary canal and apparently left drawings of it, now lost. These descriptions were detailed enough to differentiate between the guts of squids and cuttlefish. The liver was thought to be an excrement. Though he did not find the true heart - mistaking the cepholopod liver for this - he insisted that a heart, as the source of life, must be present. He believed that no molluscan respiratory organ, blood vessels or urinary system existed. Though he made some observations on the testacean reproductive system, he believed that these animals are spontaneously generated, and that the egg masses are not the source of new animals. On the other hand, his account

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1. Aristotle, <u>Historia animalium</u> 685a, 523b.

of reproduction in cephalods were in some aspects ahead of any work published before 1852, and represent some of his most striking observations. He realized that a shell was necessary for sluggish or sessile animals, but, beyond noting that they may be smooth, rough, ribbed, thick or thin, he paid little attention to these shells. Some of Aristotle's names survive as those of modern genera: Tellina, Nerita, Purpura, Haliotis, Solen, Eledone, Ostrea. [1]

Molluscs attracted little further attention until the sixteenth century, when the large and spectacular shells brought back from the Indian and Pacific oceans became some of the principal items in the new pastime of natural history collecting. By the early seventeenth century, shells were a recognized article of commerce in Paris and Amsterdam. [2] This new interest was reflected in the writings of the encyclopaedic naturalists of the period, notably these of Rondelet, Gesner and Aldrovandi.

Guillaume Rondelet (1507-66) [3] divided bloodless 'fishes' into three groups: the soft-bodied (cephalopods, slugs and sea anemones), hard skinned (lamellibranchs) and shelled (gasteropods). He gave six genera and nine species of cephalopods, one genus and three species of sea-slug, about twenty-five genera and forty-seven species of lamellibranchs, and fifteen genera and thirty-five species of gastropods. The descriptions show more personal observation and rather less compilation from classical authors than was fashionable at the time; the illustrations are often easily recognizable. However, the work is anecdotal rather than systematic and it ignored the anatomy of the animals. Though Lister did not refer to Rondelet in his own work, he bought a copy of the de Piscibus in Arles in 1664. [4]

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1. Ibid. 524-8, 546, 678-80, 683.

2. Dance pp. 36-7.

3. Guillaume Rondelet, Libri de piscibus marinis (Lyon, 1554-5), libri 17-19.

4. List of books bought in France, MSS Lister 19 (no page numbers).

The work of Conrad Gesner (1516-65) [1] was of slight importance for conchology. The book includes a few well-known molluscs, arranged alphabetically and mixed in with fishes, otters and other aquatic animals. No attention at all was paid to anatomy, and the work was not mentioned by Lister.

Ulisse Aldrovandi (1522-1605) produced a series of enormous works which were intended as a compilation of all previous knowledge on the subjects treated. That including the molluscs [2] devotes 340 folio pages to the shelled forms, and another 90 to the cephalopods. The work was, however, almost entirely secondary, relying in particular on Rondelet. Though Lister uncritically over-valued Aldrovandi early in life [3] there is no sign of any such influence on his own later writings.

Another Italian, Fabio Colonna (1567-1650) gave what were then the best available plates of mollusc shells, being the first not to be reversed by the engraving and printing process. [4] Lister had used this work, but as it was limited in scope and concerned mainly with foreign species, it had little influence on him. (Colonna's ideas on fossil origins did, however, concern Lister considerably). The 1675 edition of Colonna's work, edited by Meyer, contained very valuable additions, including in particular a comprehensive classification of the molluscs, comparable in many ways with that of Lister, though more rigidly dichotamous. Though his classification was artificial and purely conchological, Meyer appears to have realized that classification

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1. Conrad Gesner, Historiae animalium liber IV qui est de piscium et aquatilium animantium (Zurich, 1556); edition used here, (Frankfurt 1604).

2. Ulisse Aldrovandi, De reliquis animalibus exanguinibus (Bologna, 1606).

3. CJR p. 11.

4. Fabio Colonna, De purpura (Rome, 1616).

serves two purposes: to show fundamental distinctions and to allow easy identification.[1] Lister made no reference to Meyer's work.

The only prominent authors of the time attempting to deal with the internal structure of the molluscs were Willis and Redi.

Thomas Willis (1621-75)[2] was concerned with the anatomy of the nervous systems of animals, which he thought to contain a corporeal soul in the form of subtle particles. He could not find a nervous system in the oyster, one of the invertebrates dealt with in the book, though he did suspect that it might be represented by the crystalline style, which he thought to resemble the spinal marrow. He made notes on the heart and gut, and believed the gills to remove nitrous particles from the water, and so to resemble the lungs of higher animals, which were a 'fireplace, chimney or breathing-hole of the flame cherished within them'. Lister used Willis's work extensively, and reprinted his plates in the Historia conchyliorum and in the Exercitatio anatomica tertia.

The work of Francesco Redi (1636-97)[3] shows the first clear illustrations of the complex snail reproductive system. Redi did not realize that the organs were hermaphrodite, though neither did he make it clear that he was dissecting only one sex. His remark concerning a large gland 'il quale, ne maschi, petrebbe dirsi il testicolo' ('which, in the male, could be called a testis') suggests that he may have thought the organs were similar in both sexes; all individuals do, of course, have reproductive organs of the same structure. The bisexual nature of the organs was made more obscure

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1. Fabii Columnae ... opusculum de purpura Romae primum an. 1616 editum nunc iterum luci datum opera ac studio Johann-Danielis Majoris (Cologne, 1675) p. 81.

2. Thomas Willis, De anima brutorum (Oxford, 1672); translated by S. Pordage as Two Discourses Concerning the Souls of Brutes (London, 1683).

3. Francesco Redi, Osservazione intorno agli animali viventi (Florence, 1684).

by Redi's failure to find the outlet of the vas deferens or to suggest a function for the spermatheca. Lister was able to improve very considerably on this work, but he relied heavily on Redi for his account and illustrations of the more straightforward anatomy of the cephalopods.

(2) Lister's early work on shells.

Lister's interests in molluscs must have had as early a beginning as that in spiders, though his letters in the early 1670's contain more on the latter subject. Lister was certainly interested in snails on his first visit to France; he referred to his titulus III (ie. *Helix aspersa*) as having been found by Ray and himself at Montpellier. He described his (marine) titulus XVI (*Haliotis*) as common in Guernsey. As he was there in September and October of 1663, and as the animal is not found north of the Channel Islands, it is likely that this record of the species was made then. At Arles in June 1664, he bought Rondelet's *de Piscibus*. [1]

His collecting perhaps became systematic on his return to England. Lister's first published paper was on dexterorotatory snails. The two snails mentioned are probably *Balea* sp. and *Clausilia* sp.; both are fairly common and likely to be met with fairly soon by a careful collector. Lister thought this type of spire to be important, as it showed that the usual story that the turn of the shell was caused by the movement of the sun was an idea of those 'who consult not the stores of nature but their own fancy'. [2]

By March 1673-4 Lister's catalogue contained 27 species of land and freshwater molluscs. When published in the *Philosophical Transactions* [3] it was printed under the title *Tabulae cochlearum Angliae*

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1. CJR p. 111; MSS Lister 19.
2. Phil. Trans. 4 (1669) p. 1001.
3. Phil. Trans. 9 (1674) p. 99.

tum terrestrium fluviatiliumque tum marinarum quibus accedunt lapides ad cochlearum similitudinem figurati. Lister had, it seems, also been working on his lists of marine shells and fossils, though they were kept back for the present, as 'they increase upon my hands daily'. He emphasized that he was concerned to show that fossil 'shells' were not really molluscan remains, and that the most convincing way to show this was to emphasize the differences through a 'comparative view'. For this reason, the plate, by Edward Lodge, of York, did not include the slugs named in the list; these animals shed no light on the fossil question. A number of miscellaneous queries was appended to the list.

The list of species was added to and corrected over the next few years. After six months, Lister changed the name of one species (titulus III) in a way which showed he now had more experience of the variability of this snail (Cepea); he also wished to correct the illustration of another species, and had added

> 'many species found in these northern lakes ... I am not so thoroughly stocked with sea shells as I wish and endeavour. I aim not at exotics, but those of our own shires'. [1]

Two years later, he had added a further five new species, and was debating

> 'whether to put them out separately, if they deserve it, or throw them into Mr. W's [ie. Willughby's] store, if perchance anything has escaped his diligence'. [2]

By this time, the material for the Historia animalium Angliae had almost certainly been assembled.

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1. CJR p. 111.

2. Ibid. p. 124.

Molluscs in the Historiae animalium Angliae.

(a) Taxonomy and species list.

Living molluscs occupy two of the four sections of this book, the second part dealing with land and freshwater forms, the third with marine shells. Accounts of 73 species are given, in less space than was devoted to half that number of spiders. Lister naturally concentrated on the more conspicuous species; thus, he described eight large land snails out of the dozen or so species now known to be fairly common, but only six small species out of twenty-five genera and many species. He discovered ten freshwater snails, which is about a third of the total number of species other than very rare forms, and three freshwater bivalves out of six genera and about thirty species, most of which are exceedingly difficult to distinguish. In each case, the species described are usually the larger members of the genus.

Lister's coverage of marine shells was generally adequate. He found some members of all the larger groups, including most of the whelks, pectens and cockles. He failed to distinguish a few abundant species such as Littorina rudis and Patella aspera from their relatives though on the other hand he subdivided wrongly some other species such as Littorina obtusata. Being drawn from the rather impoverished fauna of northeastern England, Lister's collection did not include several common southern forms.

Lister's list of 1674 included only land and freshwater forms, suggesting that he had not yet arranged his marine material. This is confirmed by lists of localities mentioned for each of the three groups of molluscs. For the land forms he mentions Hertfordshire (once), Cambridge (once), Kent (2), Lincolnshire (4) and Yorkshire (4). For freshwater forms, he mentions Cambridgeshire (once), Westmorland (once) and Yorkshire (nine times), and four of the five new species added between 1674 and 1678 were freshwater. For marine shells, his records are almost all northern (Yorkshire 25, Durham 5, Lancashire 3 and

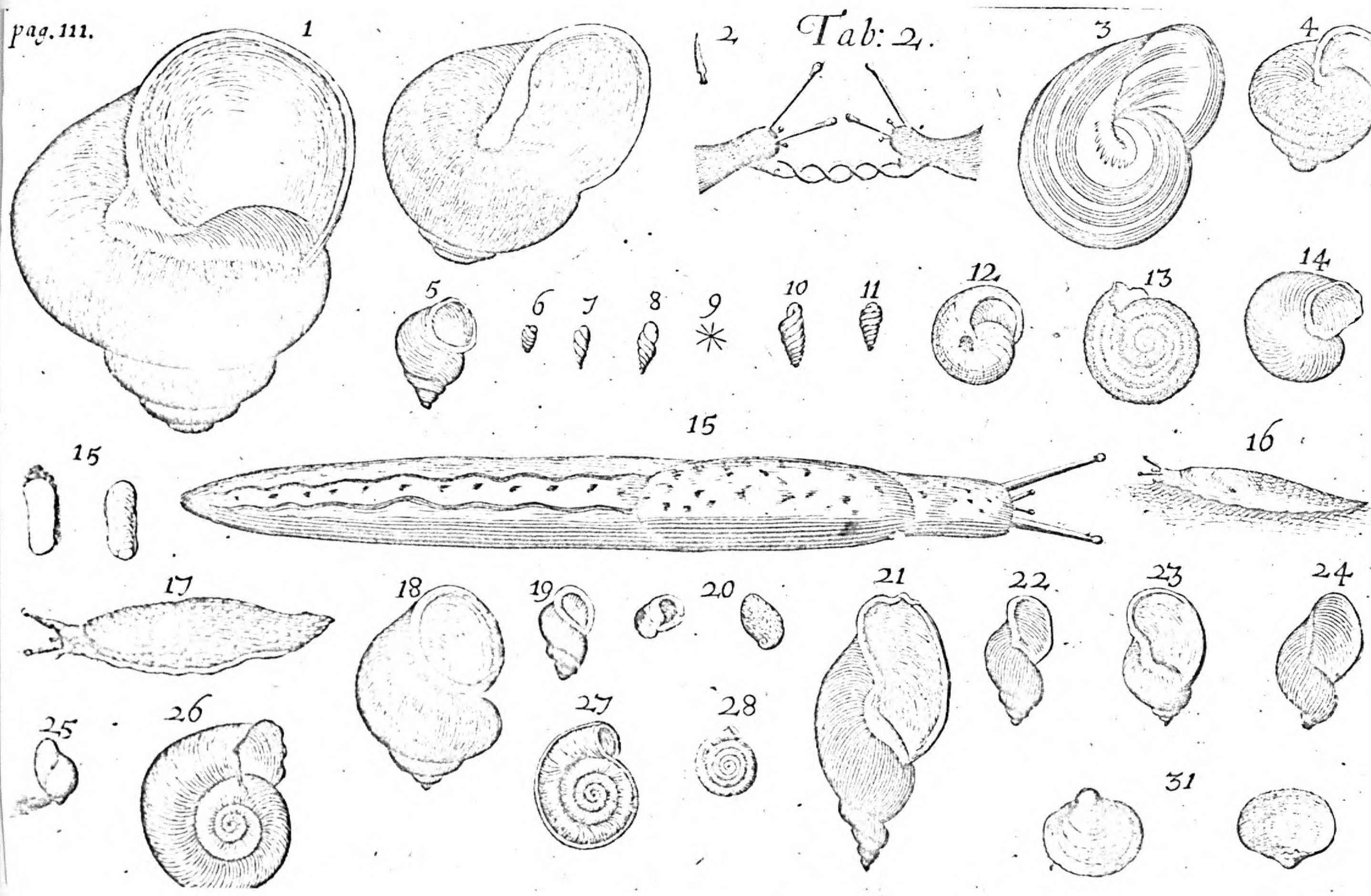
Lincolnshire 2). The exceptions are from Portland in Dorset and from Guernsey, both clearly from his first journey to France. Further, his accounts of marine species are merely scrappy descriptions of the shells, and have no ecological content; they appear to have been made at a time when Lister's interest in field work was declining. This does not conflict with the fairly comprehensive list of marine shells in his book. These shells are very much easier to collect than are land or freshwater forms, and a few days' collecting by the shore would be enough to assemble such a list. For several forms, Lister admits that he had only seen empty shells washed up.

Lister's species list of 1678, with abbreviated titles and modern nomenclature, follows; the species numbers in the list of 1674 are given in parentheses.

Titulus	Name	Modern name
I (1)	Cochlea cinerea maxima	Helix pomatia
II (5)	Cochlea vulgaris major	Helix aspersa
III (3)	Cochlea citrina aut leucophaea	Cepea hortensis + C. nemoralis
IV (4)	Cochlea maculata	Arianta arbustorum
V (2)	Cochlea cinerea interdum .. rufescens	Pomatius elegans
VI (6)	Buccinium exiguum subflavum	Lauria cylindricea
VII (7)	Buccinium exiguum quinque anfractum	Cochlicopa lubrica
VIII (8)	Buccinium rupium	Ena sp.
IX (9)	Buccinium parvum	Acme fusca?
X (10)	Buccinium pullum	Clausilia sp.
XI (11)	Buccinium alterum pellucidum	Balea perversa
XII (-)	Cochlea dilute rufescens	Monacha cantiana
XIII (12)	Cochlea cinerea albidave	Helicella itala

Titulus	Name	Modern name
XIV (13)	Cochlea pulla sylvatica	Helicigona lapicida
XV (14)	Limax cinereus maximus	Limax maximus
XVI (15)	Limax cinereus, parvus	Arion intermedius
XVII (16)	Limax ater	Arion ater, A. rufus
XVIII (17)	Cochlea maxima fusca	Viviparus sp.
XIX (21)	Cochlea parva subflava	Bithynia tentaculata
XX (-)	Nerita fluviatilis	Theodoxus fluviatilis
XXI (22)	Buccinium longum	Limnaea stagnalis
XXII (20)	Buccinium minus fuscum	Limnaea palustris
XXIII (19)	Buccinium pellucidum	Limnaea auricularia
XXIV (18)	Buccinium subflavum	Limnaea pereger
XXV (-)	Buccinium exiguum	Physa fontinalis
XXVI (23)	Cochlea pulla ... cava	Planorbis corneus
XXVII (24)	Cochlea fusca	Planorbis carinatus
XXVIII (25)	Cochlea exigua	Planorbis spirorbis
XXVIX (-)	Musculus latus	Anodonta cygnea
XXX (27)	Musculus angustior	Margaritifer
XXXI (26)	Musculus exiguus	Sphaerium spp.
Marine forms.		
I	Buccinium album	Buccinum undatum
II	Buccinum crassum rufescens	Neptunes antiqua
III	Buccinum tenue leve	As titulus I
IV	Buccinum angustius	Oceanabra erinacea
V	Buccinum minus albidum	Nucella lapillus
VI	Buccinum minus ex albo subviride	Nassarius reticulatus
VII	Buccinum crassum 2 acutis ... striis	Turritella
VIII	Buccinum tenue dense striatum	Bittium
IX	Cochlea fusca fasciis crebris	Littorina littoralis
X	Cochlea furescens	Natica catena
XI	Nerita ex fusco viridescens	Littorina obtusata

XII	Nerita fasciatus	As titulus XI
XIII	Nerita reticulata	As titulus XI
XIV	Trechus albidus	Calliostoma zizyphinum
XV	Trechus crebris srtiis	Gibbula cineraria
XVI	Auris marina	Haliotis
XVII	Concha veneris exigua alba	Trivia monacha
XVIII	Echinus marinis	Echinus esculentus
XIX	Concha longa lataque	Venerupis sp.
XX	Concha quasi rhomboides	Mya obtusa
XXI	Pholas noster	Hiatella arctica
XXII	Concha e maximis ... rotunda	Cyprina islandica
XXIII	Concha tenuis subrotunda	Tellina crassa
XXIV	Concha crassa	Spisula solida
XXV	Concha parva subrotunda	Tellina tenuis
XXVI	Ostreum vulgare	Ostrea edulis
XXVII	Ostreum parvum	Anomia ephippum
XXVIII	Musculus ex caerulea niger	Mytilus edulis
XXIX	Pecten maximus	Pecten maximus
XXX	Pecten tenuis	Chlamys opercularis
XXXI	Pecten minimus	Chlamys distorta
XXXII	Pectunculus maximus	Cardium norvegicum
XXXIII	Pectunculus echinatus	Cardium aculeatum
XXXIV	Pectunculus vulgaris	Cardium edule
XXXV	Tellina intus ex viola	Donax vitatus
XXXVI	Concha laevis	Gari faroensis
XXXVII	Concha fusca	Ensis siliqua
XXXVIII	Concha altera parte dimidia	Zirfaea crispata
XXXIX	Concha candida	Pholas dactylus
XL	Patella ex livido cinerea	Patella vulgata + P. aspera
XLI	Balanus cinereus	Balanus balanoides



Illustrations of snails from <u>Historiae animalium Angliae</u>.

(b) Methods of Classification.

Lister's mollusc classification compares very unfavourably with his treatment of the spiders, being exclusively conchological. This was presumably because of the presence of the shell, obvious, easily preserved and showing great variation easily studied by the naked eye, and giving an easy approach to taxonomy. With the spiders, Lister had been successful largely because of the difficulties presented by these small and relatively uniform animals, which had compelled him to be more discerning. However, even this does not account for Lister's adoption of a tripartite division into land, freshwater and marine forms. This was an obsolescent approach, even at this time, and was not followed, for example, in Johann Daniel Meyer's shell classification of 1675, in which habitat was ignored.[1] As shown above, Lister's interests in molluscs developed in the order of these three habitat sections, and the text of the Historia shows that his notes were assembled with little revision or preparation for publication; but Lister's failure to integrate his three sections can only be seen as showing lack of insight.

Within each group, a similar pattern was followed. The shells are divided into spiral, bivalve and univalve forms - the latter, including limpets, sea urchins and barnacles, being seen as uncoiled gastropods. As goose barnacles were not included, there was no section for what Lister was later to see as multivalve shells. Slugs were counted as naked snails; sea-slugs were not mentioned. The sections were further subdivided on conventional conchological characteristics - the shape of the spiral, direction of coil, width of mouth, smooth or textured surface, shape of valves, and so on. The larger number of marine shells made necessary a greater degree of subdivision, and

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1. Colonna ed. Meyer, op. cit.

here a number of proper nouns in general use for 'generic' groups were used, eg. nerite, top-shell (trochite), oyster, pecten and cockles (pectunculite). The system may best be described as undistinguished, though the fairly large number of species given, and the absence of distinctive exotic forms, did oblige Lister to pay close attention to relatively minor points in order to separate the individual species. Perhaps its happiest features were in the snails, where Lister separated the prosobranch land snail Pomatius; this was on the grounds of the presence of an operculum in the former, and not for anatomical reasons. Lister also distinguished the aquatic Basommatophora from the land-living Stylommatophora by the position of the eyes on tentacles in the latter. Here he was using genuinely anatomical arguments, and he did realize that conchological characteristics were sometimes linked to other features; for example, he pointed out that all those freshwater snails having flat spiral shells also had red body fluids, red horns and sessile eyes.[1]

Lister was more original in his approach to the species problem. As in his treatment of spiders, he realized that intraspecific variation was often so considerable that morphological features alone were unreliable in fixing species; wherever possible he used a biological definition. Thus, he realized that his two grey slugs, Limax cinereus maximus ... (titulus XV, now Limax maximus L) and Limax cinereus parvus (titulus XVI, now Agriolimax reticulatus (Müller)) were distinct species, and not merely large and small individuals of the same species, as each copulates only with its own kind. Less fortunately , Lister also distinguished two very similar marine nerites, tituli XI and XII, on the similar grounds that copulation does not occur between them, though he undermined his own position by admitting that he had never seen titulus XII copulating at all, even with its

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1. HAA pp. 103, 120, 107, 133.

own kind! In fact, the two are only colour phases of the same species, Littorina obtusata. [1]

Conversely, Lister realized that the many forms of his titulus III, Cochlea citrina ... were all of the same species in spite of their great differences in colour; he had found ashy-coloured and lemon, plain and striped, mating at random. On the other hand, his titulus IV, Cochlea maculata ...(now Arianta arbustorum (L)), though conchologically very much like the varieties of titulus III, was reproductively isolated and therefore a distinct species. In fact, Lister went a little too far in this matter, as some of his eight varieties of titulus III are a species, Cepea (Helix) hortensis (Muller), distinct from the others, which are all varieties of the very variable species Cepea nemoralis (L). These species are well known for the work on them by Cain and Sheppard on the effects of natural selection and fertility in producing a balanced polymorphism. [2]

It should be noted that Lister's use of the terms dextral and sinistral is the reverse of that of the present day; he referred to the direction of rotation, whereas we describe the position of the mouth.

It will be seen that Lister's nomenclature was purely descriptive, any names used in the long titles having no taxonomic significance. Terms such as cochlea, buccinum, concha and nerita were used at several points in Lister's tables, but the animals so described by each of these were in different classificatory units and these names cannot be thought of as being the equivelant of modern generic names,

(c) Internal anatomy.

Lister gave several pages of anatomical description in the Historia, though clearly he had not yet begun the systematic series of

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1. Ibid. pp. 131, 165.

2. Ibid. pp. 117-19; Cain, A. J. and Sheppard, P. M., 'Natural Selection in Cepea' Genetics 39 (1954) pp. 89-116.

dissections which was to occupy him twenty years later. He equated the foot in bivalves with that in gastropods, and realized that the head in the latter is a division of the foot. He paid some attention to the antennae and to the often stalked eyes in snails, using them as taxonomic characteristics, and agreed with Hooke's description of the teeth as forming a single fused mass.[1] The effects of torsion of the position of the anus (anterior in gastropods, posterior in lamallibranchs) was noted; Lister realized that the opening of the mantle cavity served the lung as well as the gut, pointing out that when this opening was closed, the animal ceased moving, and that freshwater snails protruded the lips of this aperture into the air when visiting the surface.[2] He gave no details of the gut, beyond noting that there was considerable variation here. Conversely, he mentioned that the reproductive organs were very similar in all species; he understood the function of the dart. Because of his interest in body fluids, the red humour of freshwater snails, the pale blue blood of land snails and their sticky mucous caught his attention. The saliva was apparently thought to be the same as the fluid used in locomotion - 'swimming in their own humours' - as well as that used as a seal for the shell. The body fluid was quite different - an 'extra-vasated blood'.[3] Lister found that it did not clot on standing, but would do so in alcohol or when mixed with the saliva. The red humour found in the flat spiral freshwater shells (Planorbis) but not in those with turbinate shells (Limnaea) was thought not to be blood, but a kind of saliva or other special fluid.[4]

(d) Ecology.

Lister paid much less attention to the ecology of molluscs than to that of spiders; this was especially true for the marine forms,

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1. Hooke 1665 pp. 180-1.
2. HAA p. 106.
3. Ibid. p. 107; Phil Trans. 9 (1674) p. 98.
4. HAA pp. 115, 118; 115, 144.

for which almost nothing other than an account of the shell was given. He made some reference to the habitat of most of the snails, noting for example, the effect of running or stagnant water on the distribution of pond snails. [1] Lister's interest in the life histories of animals showed clearly. He felt it necessary to stress the hermaphrodite nature of snails, the credit for this recent discovery being given to his 'learned and close friend, John Ray'. [2] An illustration of the intertwined penes of two snails was given; though unacknowledged, this appears to be a copy of part of the title page of Swammerdam's <u>De respiratione</u>. [3] Since Lister at several points felt it necessary to deny that colour variation in snail shells is related to sex, it seems that the sexual nature of these animals was not widely understood.

Copulation was noticed in a number of species, including two marine forms; the date and the effect of the weather on this behaviour was noted, and the use of the dart as a stimulant. [4] Lister dissected snails after copulation in unsuccessful searches for eggs, but he could only find these several months later when laid. Descriptions of the eggs of two species were given, and, as with spiders, Lister kept eggs under observation until the young emerged, and noted peculiarities of the immature shell. He recognized the viviparity of <u>Viviparus</u> sp. though conceding priority to Robert Plot. [5]

Hibernation interested Lister a good deal, as he thought it showed a voluntary control by animals of their heart-beats. [6] He noted the dates at which several species went into hibernation, and

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1. Ibid. p. 141.
2. Ibid. p. 118.
3. J. Swammerdam, <u>Dissertatio de respiratione</u> (Leyden, 1667).
4. HAA pp. 107, 114, 129, 138, 162, 164.
5. Ibid. p. 135.
6. JP pp. 69-70.

that, though one species, titulus V (now Pomatias elegans, a prosobranch) was operculate, the others sealed their shells with a mucoid saliva. By bringing hibernating snails into the warmth, Lister was able to revive them; as he found the gut to be full of excrement by the end of winter, he doubted the ability of the snails to hibernate much longer. [1]

Occasional notes were made on the food of slugs and snails and on predation on them by beetles and thrushes, including the use of a stone as an anvil (rostrum). [2]

(e) Economic uses.

Lister was always careful to note possible uses for his animals. Apart from the use of snails, mussels and barnacles as food and as manure, Lister mentioned their mucus as a binding agent in plaster for building and as a glue. He suggested the possibility of making pigments from the red and black colouring of freshwater snails and slugs. [3] Medical authors were quoted on the use of ground-up snail shells or the whole animal for treating fevers or bladder stones, and as purgatives or, in the case of the neck region containing the genitals, as aphrodisiacs. He did, however, deny that the vestigial shell of slugs could be used to cure baldness. [4] Lister noted the frequent occurrence of small pearls in tituli XXIX and XXX (now Anodonta and Margaritifer), comparing them with (non aliter quam) renal stones in man - an important point in Lister's theory of fossils. [5]

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1. HAA pp. 112-14.
2. Ibid. pp. 113-14, 132, 134, 118, 125, 131.
3. Ibid. pp. 108, 112, 116, 182-3, 196, 132.
4. Ibid. pp. 108, 141, 150, 140.
5. Ibid. pp. 148-50.

(f) Lister's classification of English shells (1678).

1. Land shells.					
	With shells				
		Short			
			No operculum		I-IV
			Operculate		V
		Long (whelks)			
			Sinistral		VI-IX
			Dextral		X-XI
		Flat			XII-XIV
	Naked (slugs)				XV-XVII
2. Freshwater shells					
	Spiral	Thick, operculum			XVIII-XX
		Thin, open (whelks)			
			Sinistral		XXI-XXIV
			Dextral		XXV
		Flattened			XXVI-XXVIII
	Bivalves (mussels)				XXIX-XXXI
	Univalves (Limpets)				XXXII
3. Marine shells					
	Spiral				
		Twisted			
			Siphon groove (whelks)		
				Smooth	I
				Striated	II-VI
			Plain opening		
				Slender	VII, VIII
				Compact	IX, X
				Nerites	XI-XIII
				Topshells (Trochi)	XIV, XV
			Wide, perforate		XVI
			Cleft-like aperture		XVII
		Untwisted			XVIII
	Bivalves				
		Closed			
			Smooth		XIX-XXV
			Rough (oysters)		XXVI-XXVII
			Hairy (mussels)		XXVIII
			Striated		
				Pectens	XXIX-XXXI
				Cockles	XXXII-XXXIV
				Tellins	XXXV
		Open			
			Smooth		XXXVI, XXXVII
			Striated		XXXVIII, XXXIX
	Univalves		Mobile		XL
			Fixed		XLI

A comparison of Lister's classification of molluscs with that of the present day. (Land and freshwater only; Lister makes no mention of the anatomy of any of the marine forms on which this could be based).

	Lister's group and number	Gasteropods				Lamellibranchs	
		Pulmonates		Prosobranchs		Schizodonts	Sphaerids
		Baso- -matophora	Stylo- -matophora	Archaeo- -gasteropoda	Meso- -gasteropoda		
LAND FORMS	Short spirals: no operculum 1		*				
	2		*				
	3		*				
	4		*				
	operculum 5				*		
	Long spirals sinistral 6		*				
	7		*				
	8		*				
	9		*				
	dextral 10		*				
	11		*				
	Flat spirals 12		*				
	13		*				
	14		*				
	Naked 15		*				
	16		*				
	17		*				
FRESH - WATER FORMS	Thick spiral oper-culum 18				*		
	19				*		
	20			*			
	Thin, spiral, open 21	*					
	23	*					
	24	*					
	25	*					
	26	*					
	Flat spiral 27	*					
	28	*					
	Bivalves 29					*	
	30					*	
	31						*
	Univalve 32	*					

Lister's most valuable points are the separation of the Stylommatophera and Basommatophera, which was done not merely on habitat, and of the aquatic Prosobranchs from the Pulmonates. Though he also separated the land Prosobranch from the other snails, he thought of this as a minor point, at least at this time.

(3) The Historia conchyliorum.

This is the book by which Lister is principally remembered, though in fact it contains little original work. The book consists almost entirely of engravings of as wide a range of shells as Lister could find in his own and his friends' collections; there is no descriptive matter, few names, and a number of anatomical plates taken from his other works. The engravings are usually said to be by Lister's two daughters Susanna and Anna.[1] However, there is no record of a daughter named Anna, though Lister's wife was called Hannah. The former name would be used as a Latinization of the latter, and only Latin is used on the engravings (eg. delineavit Anna Lister). It is much more likely that the plates were by Lister's daughter Susanna and his wife Hannah.

The book is biographically extremely complex.[2] There has been confusion over the number of editions published, and few copies of the earlier editions are identical. It appears that the first edition was printed by Lister and his family themselves, and that sets of proof copies of some of the sheets were given to several people. Some of these were given a title page and preface, and this is often listed as a distinct work, the De Cochleis of 1685. However, Lister himself did not include it in an otherwise complete list of his works, written towards the end of his life,[3] and it seems better to think of it as

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1. G. L. Wilkins, 'Notes on the Historia conchyliorum of Martin Lister (1638-1712)', Journal of the Society for the Bibliography of Natural History 3 (1953). p. 196.

2. See Wilkins 1953a op, cit.

3. MSS Smith 31 f. 80.

an experimental and preliminary printing. Continuation of this practice of issuing incomplete sets continued when the main work was in print, and it was to embarrass Lister on his visit to Paris in 1698; he found that the beautifully bound copy of the Historia in the Royal Library was one of these incomplete volumes, and he made up for this by presenting a complete copy when he returned to England. [1]

The first edition appeared between 1685 and 1692, in four 'books' and two appendices, though the work is nearly always bound as a single volume. This edition was entirely a domestic production, being drawn, engraved and apparently printed by Lister's family. Lister later claimed that it would have cost anyone else £2,000 to produce this work, and that even as it was it cost him the greater part of this sum. [2] He also claimed that it had occupied ten years of his time. This cannot include work he had done on the second edition, which was not complete until 1697, so that Lister must have begun the enterprise about 1682, a time when his interest in natural history was reviving after a period since 1676 in which he had done little work. He was then living at York, where he would probably have little more than his own collection for reference. However, on moving to London in 1684 he would be able to consult the collections of other conchologists, such as those of William Courton or Charlton (1642-1702), Edward Lhuyd (1660-1709), James Petiver (1663-1718) and Hans Sloane (1666-1753). Some of the specimens from these collections still exist, and can be identified with plates in Lister's work. [3] Though reduced at times to copying from Buonanni and Colonna, Lister would therefore often be able to find new material, and this is probably the reason for the

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1. JP p. 104.

2. Ibid. p. 105.

3. G. L. Wilkins, 'A Catalogue and Historical Account of the Sloane Shell Collection', Bulletin of the British Museum (Natural History) Historical Series **1** no. **1** (London, 1953).

frequent alteration of figures and numbering, and the insertion of new sheets as the work progressed. Indeed, in later copies the numbering of the sheets was abandoned, and numbers already engraved were erased.

The second edition, in two volumes, was much more regular and apparently professionally produced. The plates were now all numbered, with 64 new plates of shells and 22 anatomical plates from Lister's other works published since the first edition. The upper part of the title-page was altered, the title itself being changed from Historia Conchyliorum to Historiae sive Synopsis Methedica Conchyliorum. However, the lower part of the title-page was unaltered, and as this bore the date 1685 this has caused some confusion, this edition being taken by some to be merely one of the variants of the first.

The third edition, edited by William Huddesford and printed at Oxford in 1770, was essentially a reprint of the second, with some regularization of numbering of the plates. The fourth edition, printed at Oxford in 1823, was a reprint of the third, with an index of Linnaean names by L. W. Dillwyn and a new title-page bearing the words editie tertia apparently because of the confusion over the first two editions.

The work is divided into the following sections:

Book I (1685) Part I Land snails (de cochleis terrestribus) from plate 3

Part II Slugs (de Limacibus) from plate 100

Book II (1686) Freshwater shells (de turbinibus et bivalvibus aquae dulcis)

Part I Freshwater snails (de turbinibus fluviatilium) from plate 106

Part II Freshwater bivalves (de bivalvium fluviatilium) from plate 144

Book III (1687) Marine bivalves (de bivalvibus marinis) from plate 161

Appendix (1688) Fossil bivalves (de conchitis) from plate 446

Book IV (1688) Marine snails (de Buccinis marinis) from plate 524

Appendix (1692) Fossil snails (de buccinitis) from plate 1026 to 1082

The plate numbers refer to the second edition. Within each section, the species are arranged in a fairly systematic order, apparently as Lister would have arranged them in a system of classification. However, not only was no such system given, but the illustrations were not even given titles. The only written text consists of the title pages to the books, sectional headings, and the preface; these were engraved, not set in movable type. Species were referred to by later users of the work by plate numbers, but Dillwyn's index gives the Linnaean names for all except a handful of unidentifiable species (mainly fossils). Because of their length, Dillwyn's lists are not given here.

Lister's work became the standard text until the Conchylien-cabinet of F. W. Martini (1729-1778) and J. H. Chemnitz (1730-1800) was published between 1769 and 1795. [1] Even after this, it was still thought useful enough to justify an index in 1823 by Dillwyn, who wrote in his preface that

> 'The second edition of the Historia Conchyliorum is a work which has been so long and universally referred to by every naturalist who has published on either recent or fossil shells ...' [2]

(4) Lister's anatomical works.

The Historia conchyliorum contained pictures only of the shells of molluscs; not even the external features of the animal were shown. This, of course, was caused largely by practical difficulties, as foreign molluscs were not generally available in the flesh. However, Lister realized the limitations of the Historia:

> '.. some critics will judge it as nothing if I do not exhibit something of the anatomy of these animals ...' [3]

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1. F. W. Martini and J. H. Chemnitz, Neues Systematisches Conchylien-cabinet 11 vols. (Nurnberg, 1764-95).

2. L. W. Dillwyn, An Index to the Historia Conchyliorum of Lister (Oxford, 1823), preface.

3. EAA p. 4.

He undertook to do this by giving a detailed account of the anatomy of a wide range of at least the English species accessible to him and his friends - some species being provided by Edward Lhuyd, keeper of the Ashmolean Museum at Oxford. Though concentrating on a few representative types, he did make an attempt to cover a wide range of species in rather less detail. Though there was some excellent contemporary work on invertebrate anatomy by Malpighi and Swammerdam, and some genuinely comparative vertebrate anatomy, as by Grew, [1] Lister's was the first attempt to cover a whole invertebrate group in detail.

It appears that the dissections were carried out shortly before the works were published. The books contain additions and corrections which would have been incorporated more systematically had the work been prepared for publication more leisurely, and, according to Lister,

> ' .. these exercises which long ago in my youth were my pleasure and delight are now a comfort to me as an old man. Though I must now use a lens, because of bad sight, I am glad that with its help I can again follow those studies which for a long time I could not do without assistance'. [2]

The first volume, the Exercitatio anatomica in qua de cochleis maxime terrestribus et limacibus agitur of 1694, contained a detailed account of the anatomy of the edible snail Helix pomatia (the Cochlea cinerea maxima or Cochlea pomatia edulis of Lister) and of the slug Arion ater (the Limax niger of Lister) together with less detailed

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1. Swammerdam 1675 op. cit.; Malpighi, Dissertatio epistolica de bombyce (London, 1669); N. Grew, Museum Regalis Societatis ... whereunto is subjoined the comparative anatomy of stomach and guts (London, 1681).

2. EAA p. 2.

accounts of other species. The full list is

Lister's name (1694)	Species no. (1678)	Modern name.	Plate (1694)
Cochlea pomatia	I	Helix pomatia	1
C. vulgaris majoris hortensis	II	Helix aspersa	2
C. multifaria fasciata et non fasciata	III	Cepea hortensis C. nemoralis	5
C. maxima hortensis maculata	IV	Arianta arbustorum	5
C. terrestris umbilicata	XII	Monacha cantiana	2
C. albida sive cinerea, compressa	XIII	Helicella itala	2
C. fusca, compressa, extime orbis ambitu acute	XIV	Helicogona lapicida	5
Cochlea nigritis	-	Hygromia sp?	
Limax maximus cinereus striatus	XV	Limax maximus	3
L. minimus cinereus	XVI	Arion intermedius	3
L. niger	XVII	Arion ater (not rufus)	3
L. succinus colore maculis albidus	-	Agriolimax reticulatus	3

Plate 4 is of a crayfish 'liver'; plate 6 is of a snail from Surinam and its eggs; and plate 7 shows figures, from Redi, Swammerdam and Aldrovandus, showing squid anatomy.

It will be seen that Lister dissected most of the larger snails and slugs known to him.

The following year Lister published his second anatomical work, the Exercitatio anatomica altera in qua maxime agitur de buccinis fluviatilibus et marinis. The dissections and drawings in this are of a lower standard than in the earlier work; Lister appears to have become less enthusiastic for his project, which must now have begun to appear rather repetitive. The species treated here include the land prosobranch Pomatius elegans, which, according to Lister, ought to have been included in the first volume with the land snails. The other species were given a more even treatment than those in the first volume. The list is:

Lister's name (1695)	Species no. (1678)	Modern name	Plate (1695)
Cochlea terrestris striata, operculata	V (land)	Pomatius elegans	1
C. maxima, vivipara	XVIII (of F. W.)	Viviparus sp.	2
Buccinum fluviatile sex spiralis	XXI (of F. W.)	Limnaea stagnalis	2
B. pellucidum	XXIII (of F. W.)	L. auricularia	-
Cochlea fluviatile compressa	XXVI (of F. W.)	Planorbis corneus	3
Buccinum marinum crassum	II (of marine)	Neptunea antiqua	4
B. minor albidum	V (of marine)	Nucella lapillus	-
B. alterius minoris ex albo subviride	VI (of marine)	Nassarius reticulatus	-

Plate 5 showed crayfish anatomy and a beetle.

Lister's third volume, the Conchyliorum bivalvium utriusque aquae exercitatic anatomica tertia of 1696, dealt with bivalves. In this field Lister did have precursors, and he relied heavily on Willis and Heide, [1] rejecting possible charges of plagiarism by giving long, verbatim but acknowledged quotations from these authors; these take up a large part of Lister's book, though he claimed to use these quotations only for what he had seen with his own eyes. [2] He showed no ambition to improve on these writers, and the original parts of the work are of a low standard. He had clearly lost enthusiasm for the project. The death of his wife in August 1695 may have been one cause of this; she had illustrated the first two books, but the third contained only two original plates, both unsigned and of a lower quality than those in the earlier volumes. There were four other plates of bivalve anatomy, taken from Willis, Leeuwenhoek and Heide, and two plates on cephalopods, which appear to have been in part copied from Lister's own first book, where they were attributed to Redi, Aldrovandus and Swammerdam. The

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1. Willis, 1672 op. cit.; A. Heide, Anatomia mytuli (Amsterdam, 1683).

2. MSS Ashmolean 1816 f. 113.

mussel was dealt with in most detail, the full list being:

Lister's name (1696)	Number (1678)	Modern name
Musculus latus	XXVIX	*Anodonta cygnaea*
M. angustior	XXX	*Margaritifera*
M. citrinus, angustissimus	-	*Unio pictorum*
Pectunculus vulgaris	XXXIV	*Cardium edule*
Tellina	XXXV	*Donax* or *Tellina* sp?
Chama	XIX	*Venrupis* sp.
Musculus	XXVIII	*Mytilus edulis*
Ostrea vugaris	XXVI	*Ostrea edulis*
Pholas	XXI	*Hiatella arctica*
Concha anatifera	-	*Lepas anatifera*
Balanus	XLI	*Balanus balanoides*

Original figures were given only of *Pectunculus* (*Cardium*) and *Pholas* (*Hiatella*). Lister was more casual with identification and nomenclature than in the previous volumes.

The last two of these three works were both issued together with medical treatises, the *Exercitatio medicinalis de variolis* and *Disertatio medicinales de calcule humane* respectively. This was done partly to increase sales and partly because Lister was worried by comments that 'a man who writes on Insects can be but a trifler in Phisic'.[1] The first two volumes were published by Samual Smith and Benjamin Wallingford, with the *imprimatur* of the College of Physicians, but the third was brought out at Lister's own expense, presumably because the series had not been a commercial success.

1. MSS *Ashmolean 1816* f. 113.

(5) Lister's comparative anatomy.

(a) Approach and methods.

Lister's work was one of the first serious attempts to deal with the anatomy of a whole group of invertebrates, and he felt a need to justify his efforts and to show that the anatomy of such mean (vilioribus) subjects does not defile (sordescit) the character. He quoted Bacon on the value of small things, and pointed out Aristotle's work on lower animals, even though Galen had not dealt with 'insects'. [1] He found much more variety in the anatomy of molluscs than in their shells, but this in itself was no justification, as he rejected the Baconian idea of the value of simple accumulation of facts in their correct order. It was necessary to compare the structure of a wide variety of animals in order to work out the function of the organs, and this extended to the use of lower animals to give a proper understanding of our own bodies; it is dangerous to say that the anatomy of any animal is useless. [2]

> 'The use of parts may probablie be better understood, or at least to have as good a light given to understand you, from Insects as from beasts: and this advantage is to be reaped from comparative anatomie. For though they are different animals toto genere, there is yet an analogie of parts & consequentlie of their uses, if rightlie understood'. [3]

Lister tried, with varying degrees of success, to apply this approach to a number of organ systems, notably the liver and the circulatory and respiratory systems.

Lister's dissection was of an acceptable standard for the period, being an improvement on that of Redi and Willis, the only other

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1. EA p. 1; EAT p. (iii)

2. EAA pp. 3-7.

3. MSS Lister 39 f. 24v.

significant workers on molluscs at the time, but not to be compared with that of Malpighi and Swammerdam.

Most of Lister's illustrations show isolated organs or systems, only plate 1, figure 1 in Exercitatio anatomica being of a general dissection. From this, it can be seen that he opened the mantle cavity by cutting alongside the rectum, rather than at the other side as is done today. He then folded the mantle back, away from the animal's head, rather than towards it. This method shows the branchial vessels more clearly, but the main arteries attached to the heart less clearly, than our method. As his illustrations of isolated slug hearts do show these main arteries and not the branchials, Lister must either have removed the organs from the animal or dissected them by our method. As his accounts of the circulatory systems of the two animals are significantly different because of this, it seems that Lister did not take sufficiently into account the effect of his techniques when interpreting the structures he found. The only significant omissions, however, were the nervous system in the snail and the main recurrent part of the ureter. Had he noticed the latter, he might have correctly interpreted the function of the kidney. Lister's skill and knowledge were not enough to allow him to understand the nervous system of his subjects, but he was able to produce plausible, if not always correct, accounts of the structure of the alimentary, circulatory and reproductive systems. After the completion of his first anatomical work, however, he began to lose some of his enthusiasm, and the quality of anatomical description in the third volume is poor.

Lister does not appear to have used a microscope with much success, though he did use one to examine snail blood and to compare the size of its 'globules' (ie. cells) with those in human blood. His statement that failing sight now compelled him to use a microscope does not suggest that he looked on the instrument as a powerful new tool of investigation. However, his technique did include more than simple dissection and description. He often noted the effect of boiling on tissues and sometimes described their tastes. As with spiders, though to a much lesser degree, Lister made observations on living animals, as on their sense organs and reproduction.[1]

(b) Lister's use of comparative anatomy.

Lister claimed that a study of mollusc shells alone brought sparse results and that internal anatomy must also be studied. A few types only would be inadequate, as there was so much variety, and in any case comparison of a wide range of species was needed to work out the function of the organs.

Lister's ideals were not always fully realized, and he made some bad mistakes. Thus, though he claimed that different animals have different movements of their vital fluids, Lister's account of the circulatory system in snails and slugs is very confused because of his assumption that, as in fishes, blood flows from the hearts of these animals directly to the gills. As this is the reverse of the situation in molluscs, Lister's account shows the blood circulating in the wrong direction, with most of the veins labelled as arteries.[2] His lack of knowledge of the functions of the nervous system allowed him to claim that the equivelant of a brain was present in slugs but missing in snails - though later in the same work it was said merely that diligent search with a microscope failed to reveal a brain in the latter animals.[3] It might be thought that, even without understanding its function, Lister should have realized that the brain was too important an organ to be present in one, and missing from the other, of two such closely related animals. Similarly, he was unable to explain the structure of the foot in the mussel, in which the visceral and muscular parts of the organ are separated. He suggested that the muscular part might be a reproductive organ, even though a little care would have shown

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1. EA pp. 95; 2; 28-9; 78,95; 81; 151; EAA pp. 32-7.

2. EA pp. 31-9.

3. Ibid. pp. 71, 15, 12.

him that the organ has no duct and is made of nothing but muscle, being only slightly different from the feet of related molluscs. [1]

On the other hand, Lister's comparative method did have its successes. Using sound comparative arguments, he showed that the 'liver' of molluscs did not correspond to that of mammals. Of these arguments, listed later, perhaps that with greatest insight was his note that the blood supply to the gut and to the 'liver' are both arranged in the same way in the snail. His descriptions make it clear that he was making a distinction from the mammalian position, in which the liver is unique in receiving most of its blood, not directly from the heart, but from the portal vein after it has passed through the gut walls. (Lister's confusion over the direction of the flow of blood does not, of course, affect the soundness of his reasoning.) As a result of this and other arguments, Lister was able to distinguish two types of 'liver': the intestinal liver (<u>jecur intestinalis</u>) of snails, crabs, lobsters and spiders, made up of gut diverticula and usually found near the front of the gut; and the true liver (<u>jecur</u>, <u>hepar</u> 'properly so called'), the bile producing organ found in vertebrates. Some cold-blooded vertebrates (actually only some fishes) were said to have both types; Lister was here thinking of the pyloric caecae of some teleosts. [2]

In discussing the blood supply to the gut and liver, Lister noticed that lacteals were missing in molluscs; this was also true (as far as was then known) of birds and reptiles, and he thought that this comparative evidence showed that it was possible to carry chyle in the blood. [3]

At one point Lister considered that the snail kidney might be a true liver, but comparison with slugs, in which the kidney is larger

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1. EAT pp. 47, 52.

2. EA pp. 79-88.

3. Ibid. p. 34.

and not positioned so close to the middle of the gut, convinced him that this was not so. The close contact between this organ, called by him the grey viscus, and the heart puzzled him, though he suggested it might take the place of the mammalian mesenteric (lymph) glands and so help to nourish the heart. He did not consider it as a kidney, as he thought that excretion in molluscs was via the skin, the sweat of higher animals being similar to urine and snails being notable for the copiousness of their skin secretions. [1]

Besides using comparative anatomy as a means of investigating the function of organs, Lister sometimes used it as an aid to taxonomy. He placed the molluscs next to the vertebrates in the scale of perfection, using the structure and motion of the heart as his standard. He noticed that molluscs resembled locusts and spiders, and differed from vertebrates, in having the heart above the viscera, a superficially modern distinction. [2]

Lister paid much attention to the blood itself, noting that snail blood did not clot for several days, and, when examined with a microscope, had globules (cells) much larger than those of mammals. He believed the blue colour of snail blood to be an essential feature, and not a simple effect of temperature, as Glisson had said. Heating the blood caused no change, and in any case oysters from Madeira still had blue blood, in spite of the warm climate there. Lister classified blood into several classes:

1. Red, thick, clotting, and warm - in 'animals with lungs';
2. Red, thick, clotting, and cold - in fishes and serpents;
3. Thin, perpetually fluid
    a. Blue - in molluscs;
    b. Yellow - in 'some crustatorum'

The red blood of some molluscs was thought by Lister to be a

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1. Ibid. p. 24, 92, 142.
2. Ibid. pp. 4, 68.

kind of saliva found in ducts, since it resembled mammalian blood in nothing other than its colour. [1]

Besides using anatomy to relate molluscs to other groups, Lister also used it in some cases as a help in the internal classification of molluscs themselves. Thus, he claimed to find a foot in the nautilus, this being a better indication of its relationship to the snails and mussels than its shell, as this last is no more than an extra (cauda). However, he more frequently used anatomy to separate groups then to unite them, and carried this down to the species level. He listed many minor differences between the gonads of those species he dissected, and used the tentacles, position of the eyes and presence of red 'humour' to separate what are now the Stylomatophora and Basomatophora. Less successfully, he tried to use the 'ossicle' as a taxonomic characteristic. Under this name, he confused the snail dart with the vestigial shell of slugs and cephalopods, and the crystalline style of bivalves, and this bad mistake caused him some confusion. [2]

Though the features of the nervous system which distinguish Pulmonates and Prosobranchs were beyond Lister's skill and knowledge, the absence of a gill and operculum in the former group were easily seen by him. They were, surprisingly, not used in Lister's system of classification, though their presence in the marine form was mentioned without comment. However, when describing the land prosobranch Pomatius elegans (the Cochlea terrestris striata, operculata of Lister), he noted that the viscera were unlike those of any of the land snails, and the eyes and tentacles were intermediate between those of land and aquatic forms. Like the marine forms, it has separate sexes and operculum. Lister concluded that the species was 'something of a hybrid', though he could not, of course, find a typical prosobranch gill in this

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1. EA pp. 94-100.

2. Ibid. pp. 20, 128, 12; EAA p. 2; EA pp. 122-4; EAT p. 52.

terrestrial form. [1]

(6) Lister's anatomy of the Mollusca - systematic account.

(a) External features.

Lister did not discuss the nature of the molluscan shell, claiming that he had said enough on the subject elsewhere; by this, he meant his theory of the precipitation from a calcareous lapidifying juice, as with bones and teeth in mammals. Pearls in molluscs, like kidney stones in man, were seen as the result of faults in this process. Lister suggested that pearls could be produced by keeping oysters and mussels well-fed in artificial ponds. Pearls were said not to be produced in gastropods because the contact between shell and body was not so close as in bivalves. In these animals, the columellar muscle was thought to be the nutritive connection between the shell and the rest of the body. [2]

Lister observed the foot carefully enough to see that movement in a snail is brought about by a succession of undulations passing forwards along the foot. He identified the foot of most bivalves, but was confused over the mussel, in which the foot is divided into muscular and visceral parts; he suggested that the former was a tongue, proboscis or genital member, and believed it to secrete the byssus threads. In the pecten, movement is entirely by means of the shell adductor muscles, the foot being mainly visceral; Lister described it as an extension of the ovary. [3]

The mantle in bivalves was well described, and in several species the siphons are clearly shown. However, though Lister carried out simple experiments with fine particles of sand, which should have made clear the inhalent and exhalent siphons, he appears to have thought of both siphons as emitting water, without explaining how water enters

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1. EAA pp. 2-4.
2. EA pp. 126-7, 18.
3. Ibid. p. 7; EAT p. 47; Phil. Trans 19 (1698) p. 567.

the animal. The mantle in slugs was wrongly thought to be the equivelant of the collar in snails, as both are penetrated by the branchial opening. Good descriptions were given of the head tentacles and eyes of gastropods, with attention to the detail differences between those of the Stylomatophera, Basomatophera and Prosobranchia. He thought the oral tentacles of mussels to correspond to (non aliter quam) these gastropod tentacles. [1]

(b) Gut and feeding.

The alimentary canal of molluscs is relatively simple. Lister found that snails and slugs would eat, besides herbs and fruit, bread, cheese, meat, fresh and salted fish. Like Hooke, he found the upper jaw which he referred to as a mass of fused teeth, found, as in ruminants, in one jaw only. His rostrum in the lower jaw may be a reference to the radula. [2]

Lister was vague about the feeding of bivalves. He appears to have thought that the current brought in by the gills might assist feeding in the immobile oysters, and in young animals still in the parent's gill cavity; but though he quoted Leeuwenhoek's account of animalcules in the gills of mussels, he did not suggest that they may form the animal's food (As an ovist, Lister would not have agreed with Leeuwenhoek's inference that the animalcules were connected with reproduction). He suggested that Pecten might use its powerful shell muscles to crush fishes. [3]

Lister gave a detailed description, with excellent plates, of the snail gut, noting the radula sac (as a fistula) and distinguishing the crop (ingluvies sive stomachus superior) from the true stomach (ventriculus) - a point often missed by many texts even in the early

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1. EAT 7-8, 39.

2. EA pp. 89-90; Hooke 1665 p. 180.

3. EA pp. 69-70; EAT pp. 86; 41-2; 56; Phil. Trans. 19 (1698) p. 567.

twentieth century. The salivary glands were described as a fatty <u>omentum</u>, and their ducts as ligaments. [1]

The account given of the liver [2] is one of the more original parts of Lister's work, and shows an insight not found in many other invertebrate anatomists until this century. The true functions of the liver were not of course understood for any animal at this time, though the secretion of bile and some form of concoction of absorbed food or chyle were attributed to it. Its connection with the gut in vertebrates was known to be only via the portal vein. Lister found the snail 'liver' to be connected to the intestine by two ducts, through which the organ could be inflated if air were blown into the gut. The flesh of the 'liver' was formed of many grape-like lobes - a point on which he agreed with Malpighi. Lister suggested that this organ began the process of digestion, claiming that this could be shown by squeezing out the partly-digested material from the cavities of the organ. He also believed that the chyle produced by this activity could be stored in the 'liver'; the idea of intra-cellular digestion was, of course, unknown to him. In support of his idea, Lister noted:

1. that the organ does not produce bile, as shown by its taste;
2. the intimate connection between the 'liver' and gut, and the large size of the ducts;
3. the position of the organ near the front of the gut;
4. the fluid contents of the organ, which are clearly partly-digested food, and not excrement, as the ancients believed - one of the few occasions on which Lister ever criticized Aristotle;
5. the similar arrangement of the blood supply to gut and 'liver'. In modern terms, the two are in parallel in molluscs, with blood passing directly from each to the heart, whereas in

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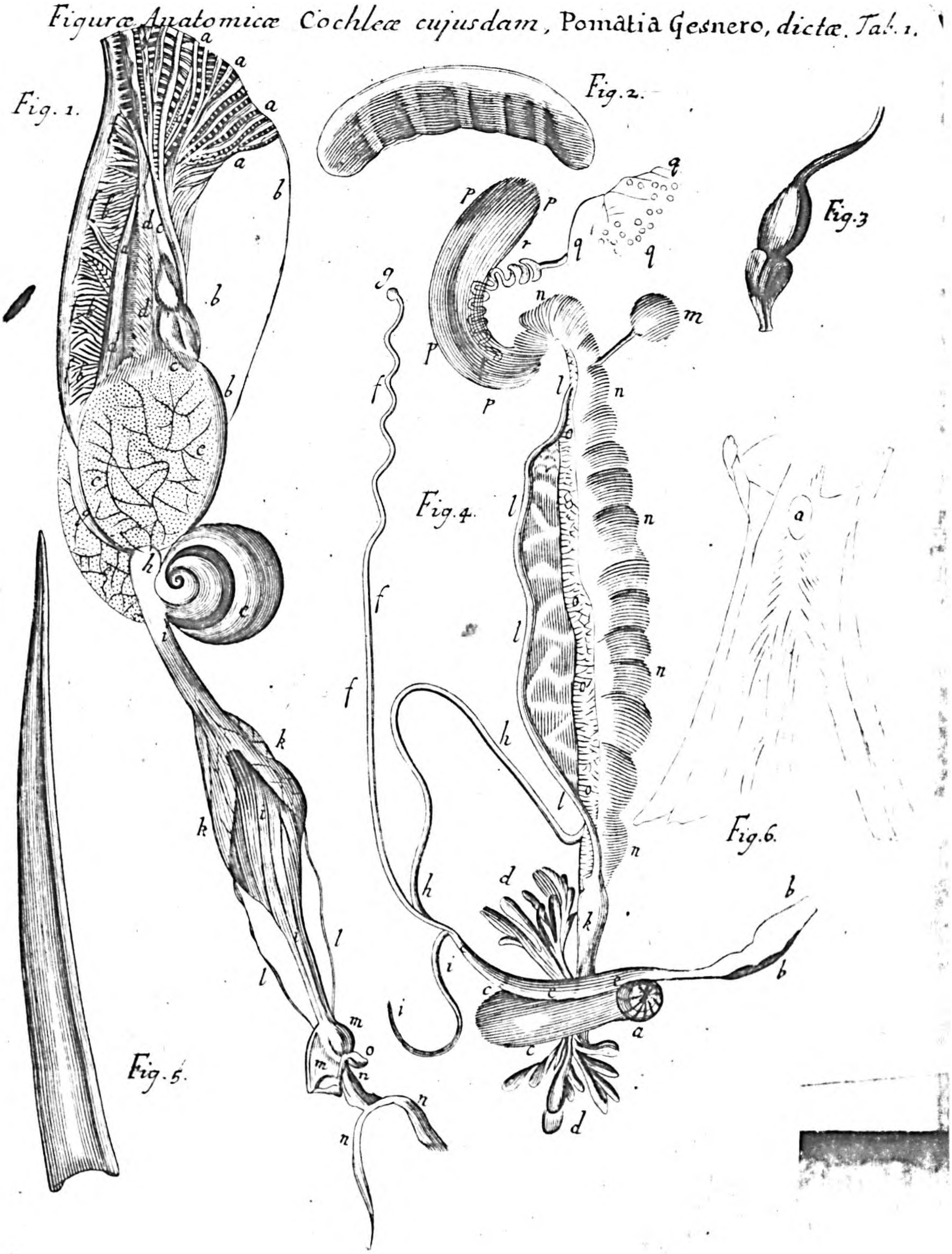
1. Ibid. pp. 70-73.
2. Ibid. pp. 77-88.



Anatomical plate from the <u>Exercitatio anatomica</u>.

Fig. 1 a. branchiae; b. branchial membrane; c. heart; d. grey viscus (ie., kidney); e. liver; f. rectum; g. intestine; h. stomach; i. gullet; k. omentum; l. omental ligaments; m. head; n. retractor muscles; o. blind process under chin. Labels for fig. 4 (reproductive organs) are given on p. 238. Fig. 2 shows the teeth, fig. 3 the heart and fig. 5 the dart.

vertebrates they are in series, with blood passing from heart to gut to liver to heart again. (Lister believed that the vertebrate liver, like the lung, acted as a blood reservoir to buffer the heart against excessive loads)

That Lister believed the blood to flow in what is in fact the wrong direction does not affect the excellence of his comparative reasoning in the last point.

Lister concluded that the snail 'liver' corresponded, not to the bile-producing true liver (jecora or hepar) of vertebrates, but to the digestive structures (jecora intestinalis) found near the front of the gut in crayfish, crabs and spiders. He rejected the idea that these organs, of which he gave figures, could be some form of pancreas, since they clearly took material from the gut. Finally, he pointed out that some fishes had both kinds of organ - by which he meant the pyloric caecae and the liver.

The rest of the gut was straightforward; Lister described the coils of the intestine and the forward opening of the rectum. During the passage of food through the gut, juices were squeezed out, the solid matter being egested largely unchanged, as in higher herbivores. The juices were absorbed from the 'liver' and intestine in silvery veins which carried the chyle straight to the heart, there being no intervening true liver and lacteals. The grey viscus (kidney in modern terms), which Lister claimed as his own discovery, was thought to be a replacement for the mesenteric (lymph) glands and to supply chyle to the heart and gonads, (The three organs are intimately connected in molluscs because of the restriction of the coelom to little more than the pericardium). He denied that a kidney was present, as he thought that excretion was carried out through the skin in the abundant mucous; this shows the influence of Sanctorius, of whose work Lister had brought out an edited version. [1]

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1. Ibid. pp. 91-3.

Lister noted minor differences between the guts of different species of snail and slugs. The bivalve gut presented two special problems. The position of the rectum, which passes right through the ventricle of the heart, puzzled him, though he suggested that it might stabilize the heart in some way. He rejected Willis' idea that the crystalline style took the place of the spinal medulla, but his own suggestion that it was the equivelant of the gastropod dart was almost as bad a mistake. [1]

(c) Circulatory system.

Lister referred to the pale blue fluid found in snails as vital humour. He showed that, like mammalian blood, it would coagulate when heated, and described its large 'globules' (cells), but would not accept it as blood, though recognizing an analogy between the two. This was because of the failure of the snail fluid to clot on standing and because of its permanent blue colour. This could not be the result of its low temperature, as Glisson had claimed, as many cold-blooded animals, including worms, did have red blood, and molluscs, even in hot climates, never did. As with some other vital fluids, such as the yellow liquid of the crustatorum, its lack of redness was the result of low pressure in the ventricle. The red fluid found in some molluscs was not blood, as it was too thin and permanently fluid; Lister claimed that it was restricted to its own ducts, whereas the vital fluid was found everywhere. [2]

A good deal of attention was given to the heart, which Lister thought to consist of a single ventricle. He doubted the existence of an auricle, though his plate shows one very distinctly. As he suggested that the auricle might be incorporated in the aorta, he appears to have been confused over the nature of the heart-beat. He condemned

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1. Ibid. pp. 52-3.

2. EA pp. 27-9, 94-108; 99, 110, 94.

Harvey for suggesting that the snail heart consisted of an auricle without a ventricle. Lister noted the slow and irregular heart-beat in these animals, and suggested that it showed the heart to be under the control of the animal's will, and not to have a 'natural' beat, as in higher animals. The ability to restrain the heart-beat was the reason that snails and slugs outlasted all other animals in the Boylean vacuum. [1]

Lister's description of the arteries and veins was badly at fault because of his over-reliance on analogy; he assumed that, as in fishes, blood must flow from the heart to the gills. (Lister thought of the vessels lining the lung as a flattened gill). Consequently, he assumed the blood to flow in what is in fact the wrong direction. Furthermore, in molluscs blood returns from the tissues to the gill or lung through large venous sinuses, not along veins. Not knowing this, and not understanding the difference between single and double circulatory systems, Lister was therefore unable to demonstrate a complete circulation. He had to assume that some of the vessels he found were arteries and others veins, running in parallel and indistinguishable. This would explain what Lister assumed to be a minor circulation through the respiratory organs, but not that through the other organs, which each had only a single main vessel. He had to leave the mystery of venous return unsolved in snails. In slugs, in which he was able to find more vessels (though his figure omits the branchials) he claimed to find both an aorta and a vena cava attached to the heart. However, he did not point out that each organ was served either by an artery or by a vein, but not by both. [2]

Lister believed that these vessels carried lymph, separate lacteals not being necessary, as in birds and reptiles, in which

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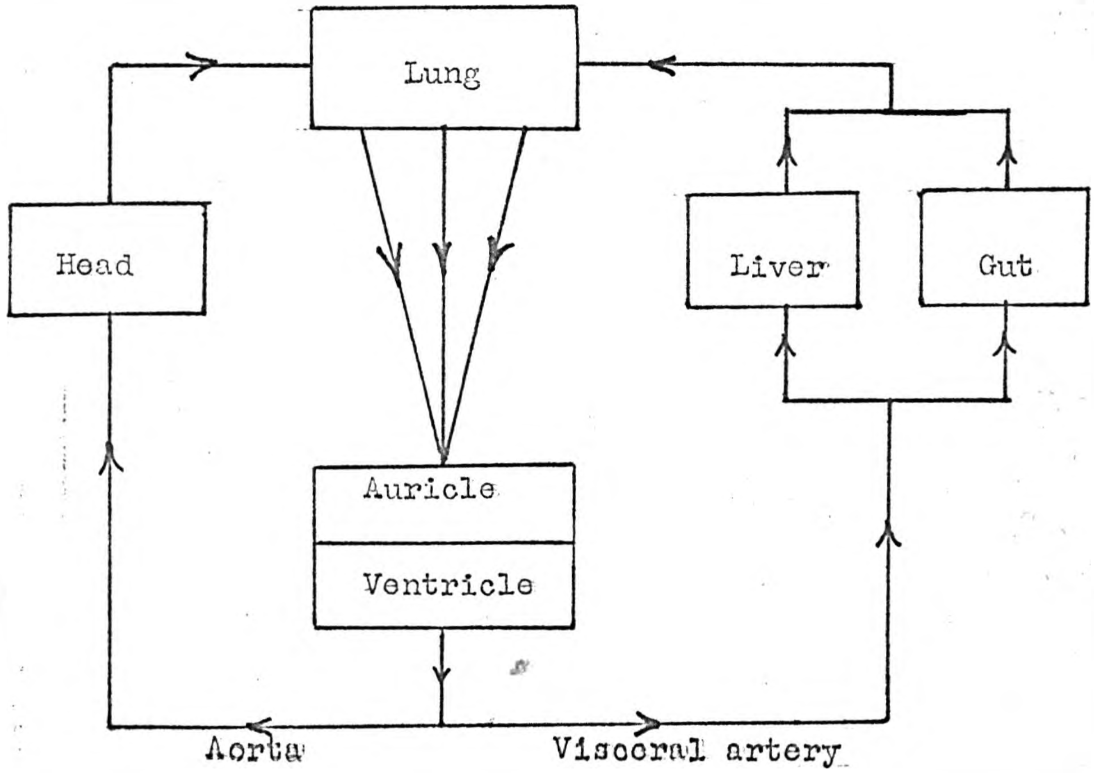
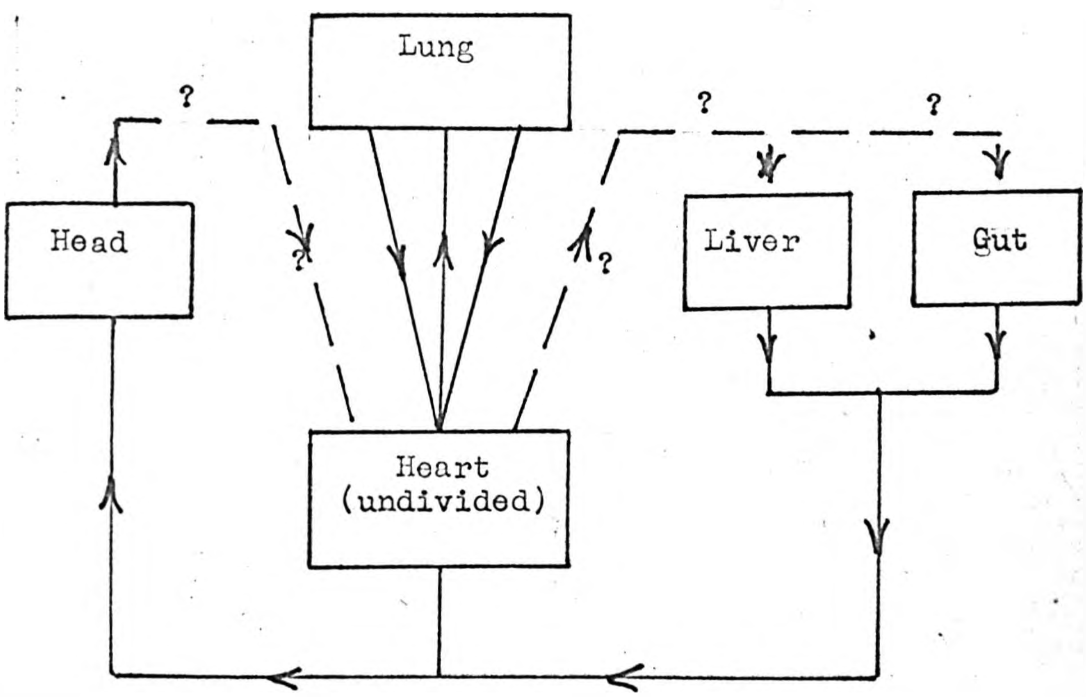
1. Ibid. pp. 24-8, 38; plate 1 fig. 3.

2. Ibid. pp. 30-35, 94.



Actual layout of the gastropod blood system

(Blood flow to the lungs is by venous sinuses)



Lister's interpretation of the gastropod blood system
(Branchial vessels were not seen in the slug, nor the aorta in the snail)

lacteal vessels had not at that time been demonstrated. [1]

(d) Reproduction.

Lister realized that the land gastropods he dissected were hermaphrodites (androgynes), and acknowledged John Ray's discovery of this. He was unable to give a satisfactory explanation for this, though he speculated that perhaps the fact that each snail has only one testis might in some way make it necessary for two individuals to contribute male matter, though he did not follow up the consequences of this surprising idea. He noticed that the land prosobranch Pomatius and most of the bivalves, though not Pecten, had separate sexes. [2]

Lister was fairly successful in interpreting the complex snail reproductive organs. The following table refers to his plate 1 figure 4 in EA, and gives Lister's and the correct labelling:

Reference	Lister's label	Correct label
a	Vulva	Genital aperture
b	Penis	Penis
c	Dart sac	Dart sac
d	Mucoid follicles	Mucous gland
e	Penis root	Penis root
f	Flagellum	Flagellum
g	Flagellum apex	Flagellum apex
h	Sperm duct	Vas deferens
i	Penis retractor	Penis retractor
k	Vagina	Receptaculum seminis
l	Vas deferens	Receptaculum seminis
m	Testis	Spermatheca
n	Uterus	Female duct
o	Uterine ligament	Male duct

1. Ibid. p. 34.

2. Ibid. pp. 6, 143; EAA p. 7; EAT pp.9, 56; Phil. Trans 4 (1669) p. 1012; Ibid. 19 (1698) p. 567.

p	Uterine gland	Albumen gland
q	Ovary	Hermaphrodite gland
r	Ovarian duct	Hermaphrodite duct

Having made the understandable mistakes of not realizing that one organ produced both eggs and sperms, or that the organs stored sperm for some time after mating (though he had observed that mating occurred at a time when eggs were not present, [1] Lister naturally took the spermotheca to be the testis and the receptaculum seminis to be the vas deferens. Atart form this, his only serious fault was to suggest two vasa deferentia, one of which opened, not at the penis, but at the structure Lister took to be the vagina. He has been criticized [2] for not using a microscope to investigate the gonads; but in fact he did observe ova in the hermaphrodite gland with a microscope, and, not being an animalculist, he would have drawn no conclusion from the discovery of spermatozoa in the gonads. His description was much superior to that of Redi, who interpreted the structures as being purely male and who cut the vas deferens. [3]

Lister realized the use of the dart as a stimulator for animals not having teeth or claws, and he gave good accounts of the spermatophore (capreolus) and its transfer during copulation. He described the eggs of the main species and gave a good account of viviparity in Viviparus sp. He emphasized that the uterine cavity is morphologically outside the body, and that the young shelter, but do not receive nourishment, there. He counted eggs and young in the uterus over an 18 month period, and found that the peak numbers of eggs were reached in September, but that the young were most abundant in January. The eggs and young were therefore developing in winter, when the adults were torpid. He also noticed the larvae of pond mussels on the outer surfaces of the gills,

1. EA pl 147.
2. Cole 1944 p. 235.
3. EA p. 140; Redi 1684 op. cit.

and suggested that they were passed out in the breathing movements. [1]

(7) The reception of Lister's anatomical work.

The eighteenth century saw a great increase in interest in shell collecting, and as Lister's Historia conchyliorum was the standard work for most of this period, there can be no doubt that it was this which earned him his reputation as a man of science. In fact it is scientifically perhaps the least valuable of all his publications. His pioneering work in anatomy appears to have been completely ignored during the eighteenth century - an indication of how completely did the shells of these animals occupy attention to the exclusion of any interest in the animals themselves. Even so well known a name as Lister's could not stimulate an interest in mollusc anatomy.

In spite of suggestions from Adanson and Müller that anatomy should be the basis of taxonomy, eighteenth century mollusc classifications, such as that of Linnaeus were entirely conchological, and as Lister's Historia did not give any formal classification, he was of little interest in this field. [2] At the end of the century, Cuvier began to introduce anatomical considerations into mollusc classification, particularly among the bivalves. His Mémoires pour servir à l'histoire et à l'anatomie des mollusques (Paris 1817) are the starting point for modern molluscan morphology, and deal with the same subject matter as Lister's neglected works of over a century before. Cuvier had looked at Lister's books, but apparently only superficially. He commented that Lister had made many mistakes, but only mentions one: that he had described lacteal vessels on the snail gut. In fact Cuvier was quite wrong. Lister specifically stated that

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1. EA pp. 122, 146, 115; EAA pp. 32-44; EAT p. 10.

2. M. Adanson, Histoire Naturelle du Senegal: Coquillages (Paris, 1757); C. Linnaeus, Systema natura (10th. ed., Stockholm, 1758).

lacteals were missing from molluscs, as from birds and reptiles. Though Cuvier's work was both wider and deeper than Lister's, he would have done well to have paid more attention to the earlier worker; Lister's anatomical distinctions between Stylomatophoran, Basomatophoran and Prosobranch snails and his interpretation of the mollusc liver were missed by Cuvier.

Though Cuvier made such mistakes as including tunicates, barnacles and brachiopods among the molluscs, his work changed the approach of other zoologists, as can be seen in Lamark's classification of 1819. [1] It now became possible to see Lister's work in better perspective, as in the comment of Johnston:

> 'Lister, then, greatly advanced conchology, by rescuing it from the charge of frivolity, by an unrivalled series of illustrations of species, by many novel remarks on their habits, by a very complete history of the species of his native land, and chiefly by giving us some excellent essays on the structure and physiology of the Mollusca, which had been neglected since the time of Aristotle, for the isolated notices of a few species by Willis, Redi Harderus and Swammerdam, however good, had no influence on conchology, while those of Lister are epochal'. [2]

However, by this time Lister's work was much outdated, even though, as Johnston pointed out, 'his opinions relative to the functions of the liver in Mollusca appear deserving of more attention than they have yet received'. [3] It was Lister's misfortune to have done his best work in a field in which he was ahead of his time so far as interest was concerned, but in which he was much within his time in respect of ability.

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1. J.B. de Lamarck, Histoire Naturelle des Animaux sans Vértèbres (Paris, 1815-22); see also G. Johnston, Introduction to Conchology (London, 1850) p. 524.

2. Johnston pp. 502-3.

3. Ibid. p. 501.

Chapter V   The origin of fossils.

(1) Introduction.

It is commonly thought that, until the work of Steno and Hooke in the seventeenth century, fossils were normally thought of as non-organic growths, produced in the rocks in which they are found, and never having formed part of a living animal. Secondary sources contain many such statements as:

> 'It is astonishing to find how tenaciously, until the middle of the eighteenth century, so many authors clung to such absurd ideas ... fossils were ... usually treated as mineral curiosities, or as illusions of nature'; [1]

and, more recently:

> 'Kircher's views on magnetic and terrestrial forces for the growing of fossils in situ were adopted more or less intact by most of Hooke's contemporaries'. [2]

This idea is misconceived in several ways. It is wrong to think that the origin of fossils was a matter of any great concern before the seventeenth century; it is something of an anachronism to try to distinguish between organic and inorganic theories of fossil origins until that time; and of writers who did speculate on the subject, so many looked on fossils as being plant or animal remains that this opinion can hardly be thought to be in any way unorthodox.

Our definition of the word 'fossil' is historical, referring to what has happened in the past. Because of our evolutionary outlook on

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1. K. von Zittel, Geschichte der Palaontologie und Geologie(Munich and Leipzig, 1899); English translation by M.M. Ogilvie-Gordon (London, 1901) pp. 16-17; But see M. J. S. Rudwick, 'Problems in the Recognition of Fossils as Organic Remains', Proceedings of the Tenth International Congress of the History of Science (Paris, 1964).

2. C. Schneer, 'The rise of Historical Geology in the Seventeenth Century', Isis 45 (1954) p. 258.

the world in general, we see things as being the result of a developmental process which can be understood by reference to activities going on around us. Until the seventeenth century, any definition of a fossil would have been a description, concerned with what the object is, with no reference to origins. This would fit in with the static world picture of the time, in which the only natural changes were cyclic - birth, growth and death; summer and winter; an outlook in which uniformitarianism was of effects, whereas we have uniformitarianism of causes. The importance of the seventeenth century fossil controversy is that a developmental approach in Biology and Geology first appeared in this field, together with the inevitable controversy.

The origin of fossils was not their most important aspect, and gave rise to no sharp controversy, until well into the seventeenth century. Little attention was given to the subject, even in books devoted entirely to stones, metals and crystal - all fossils in the usage of the time. For example, the most widely used of the lapidaries of the time, that of Camillus Leonardus, [1] could give accounts of the appearance and virtues of fossil shells without it being thought necessary that any speculation on their origin was called for - even in the edition of 1750. Such a voluminous and original writer as Agricola (1494-1555) showed no intellectual curiosity about the origin of formed stones.

It is impossible in any case to try to distinguish two contrasting theories at this time. Before the general rejection among philosophers of the idea of the spontaneous generation of comparatively large and complex animals such as molluscs, no clear line could be drawn between the organic and inorganic worlds. The medieval Scala Natura did not permit the drawing of such sharp distinctions; organisms such as sponges, corals and lichens blurred such divisions, and, among 'fossils', objects

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1. Camillus Leonardus, Speculum lapidum (Venice, 1502 English translation London, 1750).

like crinoids and belemnites formed a link between animal-like stones and crystals, spars and ores.[1] Many mineral objects show a chance resemblance to parts of human or animal bodies, to a hand or an ear or a head. It would not be immediately clear why these should represent a different class of objects from those stones resembling shellfish; it might have been natural to look at them both as being examples of the same phenomenon, to be explained in similar ways. The growth of mineral ores, stalagmites, crystals, and the production of saltpetre were sometimes seen as the expression of some rudimentary vegetative faculty in the mineral world, and, conversely, the growth of stones in the bodies of animals appeared to show the mineralizing ability of living things. The question of generation is crucial here; if a snail is thought to be generated, together with its shell, in and from the mud at the bottom of a pond, there is no clear reason why the shell alone should not be produced in the earth by comparable forces. Indeed this may appear to be the simpler case. Aldrovandus (1522-1605) wrote that if fish and frogs could be generated from air, and mice and worms from mud, then shells could be generated from stone.[2]

There was, of course, speculation about the origin of fossil animal remains, though it was not a matter of fundamental importance, and the different theories were fitted into a continuous range of natural philosophies. Hypotheses on fossil origins covered all gradations from an almost modern outlook, through mineral and plastic virtue theories to astrological and occult explanations. All these shades of opinion were represented from the pre-Socratics down to at least the middle of the seventeenth century. Those writers treating fossils as animal remains were not at all exceptional or ahead of their time. The argumentative Bernard Palissy (1510-90) accepted a more or less modern

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1. A. O. Lovejoy, The Great Chain of Being (Cambridge, Mass., 1936).

2. As quoted by Thorndyke, L., History of Magic and Experimental Science 6 (New York, 1941) p. 291.

view of the nature of fossils, but he was concerned, not so much to establish their organic origin, as to give a 'uniformitarian' explanation for their distribution, not dependent on supernatural causes. His arguments are therefore directed, not against authors favouring a non-organic theory, but against Cardan, who, like Palissy himself, accepted fossils as animal remains, but who suggested that they were dispersed by the Noachian flood. Palissy apparently thought it unnecessary to argue in favour of his ideas on the nature of shell-stones; they were already widely accepted.

(2) Ideas on fossil origins up to the middle of the seventeenth century.

The earliest surviving opinion on fossils is that of Xenophanes (born c. 570 B.C.)

> 'Shells are found inland, and in the mountains, and in the quarries in Syracuse ... These .. were produced when everything was long ago covered with mud, and the impression was dried in the mud'. [1]

This was basically the orthodox Greek view, although not all writers of classical times used fossils as evidence of such extensive geological change. Xanthus, Strato & Eratosthenes mention them as showing at least local recession of the sea [2] and Herodotus quotes his own observations of shells in the hills of Egypt as evidence that the country had been built up from silt washed down to the sea by the action of the Nile. [3]

These authors usually refer to fossil molluscs as 'shells' but in fact they are usually of a stoney material as was recognized by Eratosthenes.[4] This apparent petrification was ascribed by Avicenna (980-1037) to a mineralising virtue in the earth, [5] and this view was

1. Quotation from Hippolytus in G. S. Kirk and J. E. Raven, The Pre-Socratic Philosophers (Cambridge, 1960) p. 177.
2. Strabo, Geography, introduction, Book 1, ch. III para. 4; Loeb edition (London, 1917) pp. 181-7.
3. Herodotus, History book 2, II; Penguin edition (London, 1954) p. 106.
4. Strabo p. 181.
5. P. Duhem, Études sur Leonardo da Vinci 2 (Paris, 1950) p. 307.

accepted by Albertus Magnus (1206-80):

'If the bodies of these animals are in places where a mineralising power (<u>vis lapidificativa</u>) is being exhaled, they are reduced to their elements .. then the mineralising power converts the terrestrial element into stone'. [1]

The first writer to suggest Noah's flood rather than recession of the sea as an explanation for the inland occurrence of fossil shells was Ristoro d'Arezzo, in 1282:

'Almost at the summit of a very high mountain, we have found a large quantity of shells of those fishes we call snails and cockles ... also a great mass of sand and rounded pebbles ... as if they had been deposited by a river. This is a certain sign that this mountain has been made by the deluge'. [2]

Vincent de Beauvais also accepted an organic origin for fossils in his <u>Speculum naturale</u> of about 1250. This encyclopoedic work had a wide influence in the late middle ages, [3] and several Italian writers of the fourteenth and fifteenth centuries, such as Cecco d'Ascoli, Boccacci, and Leon Battista Alberti, repeated similar ideas. [4]

Speculation on fossil origins became more widespread in the sixteenth century. The manuscript notebooks of Leonardo da Vinci (1452-1519) [5] contain several references to fossil shells. Leonardo rejected the flood as the cause of their distribution. A relatively slow rise in

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1. See Albertus Magnus, <u>Book of Minerals</u>, translated by D. Wyckoff (Oxford, 1967) p. 52; also quotes Avicenna.

2. Duhem <u>2</u> pp. 318-19.

3. According to Duhem loc. cit.

4. R. Barratta, <u>Leonardo da Vinci ed i Problemi della terra</u> (Turin, 1903) pp. 223-8.

5. I. Richter <u>Selections from the Notebooks of Leonardo da Vinci</u> (London, 1952) passim.

sea level could not move heavy shells upwards, especially as some of the commonest fossils, such as oysters, grow firmly attached to their substratum. Only a raging torrent could move these, and this would deposit the shells haphazardly, in mixed masses, whereas they are in fact found in beds mainly of one species, as they are in their natural habitats. Further, Leonardo found these shell beds to occur at at least four different levels in Lombardy, so a single flood would be insufficient. He could not have supposed the mountains to have been covered by a universal flood, because of the difficulty of explaining where all the water has gone. Finally, since the Noachian flood was caused by rain, which would flow downhill, it could not have moved anything uphill. Instead, Leonardo adopted the idea of many Greek writers, and also of Albert of Saxony, [1] that the waters of the earth are gradually retreating because of evaporation, so that former breeding grounds of marine molluscs are now left inland.

Retreat of the sea was also suggested by Andrea Cesalpino (1519-1603), though as he claimed that living molluscs can still be found embedded in rock, his ideas appear less well thought out than Leonardo's. [2] Gabriele Fallopio (1523-62) wrote that he was 'of that opinion' which recognised fossils as animal remains, thereby implying that in doing so he was following a definite body of opinion. [3] Like Leonardo and Cesalpino, he rejected the 'common idea' of the flood as a geological agent for fossil dispersal. Girolamo Fracastoro (1484-1553) also accepted an organic origin for fossils, [4] and Girolamo Cardano (1501-76)

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1. Duhem 2 pp. 327-31.

2. Andrea Cesalpino, De metallicis libritres (Rome, 1598).

3. Gabriele Fallopio, De metallicis in De medicatis aquis atque de fossilibus tractatus (Venice, 1564).

4. Girolamo Fracastoro Homocentrica (Venice, 1538) (According to F. D. Adams, The Birth and Development of the Geological Sciences (New York, 1954) p. 261.) Adams does not give a page reference, and I have been unable to find the passage in the original.

reverted to the flood to explain their distribution, at least in part; he supposed that Noah's was only one of many such deluges.[1]

Cardan's work was very widely known, and was translated into French. This brought it to the notice of the Huguenot potter Bernard Palissy. Though taking the view that fossils are animal remains, he accepted neither of the previous explanations of their distribution. He was impressed by the restriction of fossil shell beds to a relatively small number of places; this he thought was because they represented the dry beds of former inland lakes, whose waters have become petrified together with their living contents. As a subsidiary explanation, he suggested that very dense beds of fossil shells were the refuse dumps of men who had formerly lived by the shores of these lakes. He appears to have been the first writer to realize and attempt to deal with the problem that fossil species are different from present-day animals, some, such as ammonites, markedly so. Palissy pointed out that all extant species are restricted to certain areas, and that a species can become extinct, at least locally, as he had observed with salmon in some French rivers. He accepted total extinction for forms such as ammonites, and put this down to overfishing in the past. The shells were thought to have become petrified by the action of a sal congelatif which later converts the water of the lake into rock. Palissy did not feel it necessary to argue against non-organic ideas of fossils. Even though he claimed not to be able to read Latin, his prominent part in the intellectual life of the French court and his public lectures on geological subjects must have brought him into contact with such ideas; that he ignores them must mean that, though they were clearly quite widespread, these theories were not in any way orthodox opinion. Palissy concentrated his

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1. Girolamo Cardano De subtilitate (Nurenberg 1550); French translation by Richard le Blanc (Paris, 1556) pp. 152-3.

arguments entirely against Cardan's use of the flood to explain the inland occurrence of fossils. [1]

The main systematic accounts of fossils and other stones in use in the sixteenth and early seventeenth centuries were those of Conrad Gesner (1516-65), Ulisse Aldrovandi (1522-1605) and Georg Bauer (Agricola) (1490-1555). Discussion of fossil origins was complicated for these authors by their own comprehensiveness. They had to account for crystals, spar, stalagmites, flint nodules, and other bodies in addition to shell-stones, fossil bones and teeth and objects of intermediate degrees of complexity. Aldrovandus, though recognizing the alternative, preferred to give a single explanation for all 'fossils', and so rejected an animal origin which was widely considered as possible for some of them. He suggested a 'hidden force' which created imitations of plants, animals, parts of the human body, ecclesiastical and religious objects and other things. [2]

Gesner made the first attempt to classify fossils by their form, dividing them into 15 classes. [3] Five of these were 'plant-like' (including coral) and two were 'animal-like'. There are some animal fossils, such as belemnites and crinoids, which are very unlike living forms; Gesner included these in a group of objects 'like artificial things'. This sub-division of 'fossils' would have allowed Gesner to accept different explanations for the origins of different groups, though in fact his precise opinion on the matter is not easy to find from his writings.

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1. Bernard Palissy, Récepte veritable par laquelle tous les hommes de la France pourront apprendre à multiplier et augmenter leurs thresors ... (La Rochelle, 1563); in Oeuvres complètes de Bernard Palissy, edited by P. A. Cap (Paris, 1961) p. 37; Discours admirables de la nature des eaux et fontaines (Paris, 1580); in Cap, pp. 219, 272, 274-5, 277-81.

2. Ulisse Aldrovandi Musaeum metallicum in IIII libros distributum (Bologna, 1648).

3. Conrad Gesner, De rerum fossilium lapidum et gemmarum (Zurich, 1565).

Although the works of Agricola [1] dealt with the origins of minerals, and he was the first to classify them according to their physical properties, only passing reference is made to fossil shells, and no account of their origin is given in these voluminous and authoratative writings; this is a further indication of the comparatively non-controversial position of the problem of fossil origins in comparison with the stress placed on their medical and other virtues.

At the end of the sixteenth and the beginning of the seventeenth centuries, the works of Fabio Colonna (1567-1650) included fossil shells together with living forms. [2] Because of the high standard of his accounts of the modern animals, in which systematic classification by genera and species was attempted for the first time, Colonna's work was influential. His main arguments in favour of the organic origin of fossils are given in his account of glossopetra (fossil shark's teeth).

For those rejecting an animal origin for fossils, there was more than one alternative explanation. As long as spontaneous generation was accepted, some natural force had to be postulated, capable of forming such bodies as molluscs and their shells. For Palissy, this was the generative salt, released by the decay of plants in Autumn, responsible for the growth of new plants, and also of stones, and, when present in water, for the growth of shellfish. [3] It is not a fundamental change to visualize such a force, virtue or substance as operating in rock and producing only part of the animal: bone without muscle, teeth without jaw, or shell without molluscs; even in

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1. Agricola (Georg Bauer) De natura fossilium (Basel, 1546); De ortu et causis subterraneorum (Basel, 1546); Italian translation of latter by G. Tramezzino, Di Giorgio Agricola de la Generazione delle cose (Venice, 1550) used here.

2. Fabio Colonna, De glossopetris dissertatio in De purpura ... cum iconibus (Rome, 1616)

3. Palissy 1580 in Cap p. 282.

water and soil, animals of varying degrees of perfection are generated, and there were in fact several stories of living shellfish being found embedded in rock. The 'hidden force' of Aldrovandus can be seen as such a natural, terrestrial force; these forces sometimes have a pseudo-biological nature, even when concerned with the growth of ordinary stones. Cardan wrote of the growth of these as being like that of a chick in the egg, this being the reason that gems cannot be grown artificially. [1] Fallopio suggested a fermentation in the earth, producing vapours from which living shell-fish, whose remains are left as fossils, were generated. [2] This is also a non-occult, terrestrial theory, and fermentation was thought of as having strong biological links.

The idea of astral influences on the growth of stones originated with Aristotle. [3] The planets and constellations produced vapours in the earth which congealed to form fossils or metals. These ideas were widely held in modified form, down to the seventeenth century, and they fitted easily into the renaissance neoplatonic world of sympathies and antipathies, macrocosm and microcosm; though, ironically, they were not so easily reconciled with the reformed, hard-line Paduan Aristotelianism of the period. Some stones were held to show astral influences in their shapes, astroites (fossil corals) having star-like markings, and selenites (crystals of borax) being supposed to show a reflection of the full moon when held to the light. Aristotle had not made clear whether or not 'fossils' were to be taken as including shell-stones. His only specific mention of such objects, a reference to fishes being found motionless in the earth, appears to show that he, like other Greek writers, looked on

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1. Cardan p. 153.

2. Fallopio p. 109.

3. Aristotle, <u>Meteorologia</u> 378a.

them as animals rather than mineral productions. Other writers, however, explained all formed stones as products of astral influences and sympathies. Paracelsus included all stones resembling animals or human artefacts (arrowheads, axes, etc.) under the name Gamahey (a plural noun). According to him, they contain the virtues of the constellations under whose influence they were formed.[1] Such ideas were given by other sixteenth century writers - Megenburg, Reisch, Savonarola, Maffei,[2] without distinguishing shellstones, fossil bones and teeth from crystals and other less-organized formed stones; and even Gesner was not free from such basic attitudes of his time.[3]

We see then, that until well into the seventeenth century, there could be no real conflict of ideas on the origin of fossils. The conditions needed to provoke such a conflict were missing - a concern for developmental, and not merely descriptive, history, and a clear distinction between the organic and inorganic worlds. In the sixteenth century, the idea of an animal or plant origin for some fossils was a by no means insignificant part of a wide but continuous range of hypotheses on the subject. The organic theories of men like Palissy and Leonardo, are linked, through the idea of spontaneous generation, with natural, terrestrial theories of non-organic fossil origin such as those of Fallopio and Aldrovandus. The vapours and exhalations of these ideas in turn show no insuperable cleavage from Aristotelian theories of stellar radiation which, in the hands of neoplatonic writers such as Paracelsus, become thoroughly occult. This continuity was further emphasized by the width of meaning of the word 'fossil'; a writer could accept explanations from different parts of this range to account for different kinds of fossil. Ristoro d'Arrezo accepted

1. Bombast von Hohenheim (Paracelsus) Liber de imaginibus in Opere omnia medico-chemico-chirurgica (Geneva, 1658) p. 502.

2. See Adams p. 84.

3. Conrad Gesner De omni rerum fossilium genere, gemmis, lapidibus metallis et huiusmodi (Zurich, 1565); quoted in M. J. S. Rudwick, 'The Meaning of Fossils' (London, 1972), chI.

an astral explanation for the growth of most kinds of fossil, but an animal theory for shell-stones.[1] Even within the formed stones, there was a wide range of organization; from complete skeletons to odd bones and teeth, shells very similar to modern forms, shells unlike any extant species, mysterious objects such as belemnites, fossil leaves, dendritic growths of pyrites, and single crystals. This paralleled the continuity of the animal and vegetable kingdom; it was impossible to draw clear lines between the different kinds of fossils, or to differentiate distinct 'organic' and 'inorganic' theories of their origin.

(3) The fossil controversy in the second half of the seventeenth century.

About the middle of the seventeenth century the position began to change, at least in England; there appear to have been two main reasons for this. There was a growing scepticism towards the idea of the spontaneous generation of all but the simplest animals. The work of Redi in Italy was paralleled in intent, if not in quality of experimental technique, by several English observers, including Lister; there was much discussion of the subject at the Royal Society in its early years, and several papers on generation appeared later in the Philosophical Transactions about 1670. This sharpening of the difference between living and non-living things tended to separate non-organic theories of fossils from those accepting an animal origin for the more complex of these stones. The idea of a petrific seed, which was fairly widespread at this period, can be seen as an attempt to maintain continuity between the animal, vegetable and mineral worlds while recognizing the change in biological outlook.

Secondly, many learned men were losing confidence in occult

1. Duhem p. 32; Adams pp. 84, 339.

explanations at this time. It is true that there was a great deal of interest in astrology and alchemy in the second half of the seventeenth century, and that explanations of this kind could be offered for the problem of formed stones. Elias Ashmole, for example, wrote in 1652 that

> 'As for the use of such Characters, Letters, Words, Figures, &c. Formed or insculpted upon any Matter we make use of, we are led to it by the president of Nature, who Stampes most notable and marvelous Figures upon Plants, Roots, Seeds, Fruits, nay even upon rude Stones, Flints, and other inferior Bodies. Nor are these remarkable Signatures made and described by Chaunce ... but are the Characters and Figures of those Starrs, by whom they are principally governed, and with those particular Stamps, have also peculiar and different vertues bestowed upon them'. [1]

Nevertheless, such opinions were becoming restricted to men with certain attitudes of mind, and were no longer almost universally acceptable. Thus, John Webster (1610-82), himself interested in the occult, wrote in 1671 that it was the common opinion that minerals did not grow in the rocks, but remained in the form in which they had been created at the origin of the earth. [2] Though fossils may have been collected as curios by men prepared to accept an astrological explanation for them, there is no sign of such opinions among the writers engaged in the fossil controversy in the second half of the seventeenth century in England. Even a writer such as Robert Plot,

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1. Elias Ashmole, Theatrum chemicum Brittanicum (London, 1652) (reprint, London and New York, 1967) p. 463.

2. John Webster, Metallographia, or, an History of Metals (London, 1671).

known to be a practicing alchemist, [1] made no attempt to introduce occult or alchemical ideas into his writings on fossils.

Although this change need not necessarily affect such terrestrial, non-occult theories as that of Fallopio, in fact all theories of the growth of formed and other stones in situ appear to have been weakened by their isolation, and, at least in England, to have been discarded by most natural philosophers by the middle of the century. Sir Thomas Browne used the idea of a plastic nature to explain the growth of (quartz) crystal in 1646, [2] but when Henry More attempted to bring such ideas into physical science in the 1650's he was strongly opposed by Robert Boyle. [3] More was not a natural philosopher in any real sense, but he wrote of a 'spermaticall power' or spirit of nature which was a

> 'substance incorporeal pervading the whole Matter of the Universe and exercising a plasticall power therein ... raising such Phaenomena in the World, by directing the parts of the matter and their Motion, as cannot be resolved into meer Mechanical powers'. [4]

Such ideas had little success in England at this time. An attempt was made in 1664 at the Royal Society to attribute fossil shells to such a power; it was apparently not well recieved. Birch records that

> 'the Secretary presented the Society from Mr. Beal with a box full of several sorts of stones, by which the latter conceived it might be seen what is the process

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1. F. Sherwood Taylor, 'The Alchemical Papers of Dr. Robert Plot', Ambix 4 (1949) p. 67.

2. Sir Thomas Browne, Pseudodoxia Epidemica (London, 1646) in Keynes 2 p. 78.

3. Robert Boyle, The Usefulness of Natural Philosophy (London,) (printed in The Works of the Honourable Robert Boyle, edited by Thomas Birch, London, 1744).

4. Henry More, The Immortality of the Soul (London, 1659) p. 450; quoted by L. A. Green 'Henry More and Robert Boyle and the Spirit of Nature', Journal of the History of Ideas 23 (1962) pp. 451-74.

of the plastic spirit in shaping perfect cockles, muscles, scollops, headless serpents, thunder-stones etc. But several members were of opinion, that these shells had filled with clay or mud, which from them received the impression of their figure, and was in length of time hardened into a stony substance ...' [1]

A plastic virtue was suggested as late as 1691 by John Ray, [2] as the probable intermediary between God and His handiwork; but Ray, though careful and painstaking, was conservative, and he wrote of this virtue only in connection with the growth of plants and animals (though he did not specifically restrict it to this field). It appears that Beal's was the last attempt in England to explain shell-stones by such a semi-occult force.[3]

As a result of the rejection of spontaneous generation and of astrology, the general opinion of philosophically-minded men in England in the 1660's was that fossils such as shell-stones were the remains of living things. The rival, geochemical theory associated in the 1670's and 80's with a group of English naturalists was seen at the time, not as a reactionary movement opposed to a new, progressive idea, but as an innovation challenging the generally accepted view. This was specifically stated to be so at the time by writers of both viewpoints. Woodward, a vigorous supporter of the organic view, wrote in 1695 that

'I found myself necessarily obliged to take off a Difficulty started by some learned Men who have wrote now lately upon the Subject, and assert that these

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1. Birch 1 p. 457.

2. John Ray, The Wisdom of God Manifested in the Creation (London 1691); eighth edition used here (1722) pp. 52, 77.

3 Beale maintained his non-organic view of fossils in a letter to Boyle in 1671, though he described his ideas as an 'obstinate heresy'. See The Works of the Hon. Robert Boyle 4 (London, 1772) p. 433

> Shells are not real: that they were never bred at Sea: but are all of Terrestrial Original, being mere stones, though they bear a Resemblance of Shells, and formed, in the Places where they are now found, by a kind of Lusus of Nature, in Imitation of Shells';

and that fossil shells 'are the real spoils of once living Animals; and not Stones, or natural Fossils, as some late Learned Men have thought'. [1]

John Ray wrote in 1692 that fossil shell-stones had formerly been called petrified shells, but were 'now a-days' referred to as formed stones; he can only mean that the organic theory had been challenged by a new interpretation of fossil origins. [2]

Edward Lhwyd, writing in 1686, said that 'Naturalists contend much about the original of these stones; ffor most of them affirme they were once shells, and therefore call them petrified shells, not cochlites or shell-stones ...' [3] He wrote to Ray of his difficulty in accepting either the opinions of Plot or 'the general opinion, that they have been reposited in the places we find them at Universal deluge, and so preserved to our time'. [4]

At the Dublin Philophical Society in 1684, Mr. Ashe 'produced some formed stones resembling petrify'd shells, these we hear are thought of late by some to be lapides sui generis'. [5] John Beaumont wrote in 1676 of 'that opinion which generally solves those various Phenomena of the several figur'd Stones, which we find in Mines and

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1. John Woodward, An Essay towards the Natural History of the Earth (London, 1695) pp. 14, 16.

2. John Ray, Miscellaneous Discourses concerning the Dissolution and Changes of the World (London, 1692). Second edition published as Three Physico-theological Discourses (1693).

3. R. W. T. Gunther, Early Science in Oxford 14: The Life and Letters of Edward Lhwyd (Oxford, 1945) p. 79.

4. MSS Ashmolean 1820a f. 28.

5. R. W. T. Gunther, Early Science in Oxford 12: Dr. Plot and the Correspondence of the Philosophical Society of Oxford (Oxford, 1939) p. 14.

elsewhere, by saying that they are parts of Plants and Animals, or whole ones, petrified ...' [1] The reviewer in the Philosophical Transactions of Plot's Natural History of Staffordshire wrote of fossils as 'those stones like Testaceous fishes, which have long been thought to be petrifyd shells, tho' some Naturalists are persuaded to the contrary'. [2]

Lister certainly considered his opinions to be unorthodox and unlikely to find favour with those 'content to acquiesce in ... general notions', [3] and he wrote of 'the finding of Cockles or Shells, as most writers are pleased to call them, upon Mountains'. [4]

The widespread acceptance of an organic theory of fossils had been reached largely by the default of opposing ideas rather than as a result of any discussion of positive evidence. The situation began to change after about 1660, and apparently particularly so in England. Several causes can be traced for this change. It has been argued that the increase in interest in fossils at this time was a result of the English interest in antiquities. [5] This seems too narrow. None of the prominent antiquarians of the seventeenth century [6] - James Ussher, William Dugdale, George Hickes, Humphrey Wanley, Thomas Hearne, Thomas Gale, Henry Wharton - was involved in the fossil controversy, though many of them may have collected fossils as curios. [7] The only important overlap between the two fields is to be found in the work of Edward Lhwyd. However, all Lhwyd's early work was in Chemistry and Geology, and this occupied him almost exclusively until about 1697.

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1. Phil. Trans. 11 (1676) p. 737.
2. Phil. Trans. 16 (1686) p. 209.
3. Phil. Trans. 6 (1671) p. 2283.
4. Phil. Trans. 14 (1684) p. 742.
5. Schneer loc. cit.
6. As listed by Schneer.
7. For example, Ralph Thoresby, the Leeds antiquary, had an extensive collection which included fossils, though he does not appear to have been involved in any controversy. See catalogue of his collection.....

After the publication of his work on fossils in 1698, he began his extensive travels in Wales, Scotland, Ireland, Cornwall and Brittany and devoted himself almost entirely to Philology and Archaeology. His fossil interests pre-date his antiquarianism, and, though both subjects might appeal to the same kind of person, it is clear that Lhwyd's activity in Archaeology, coming from his intense Welsh patriotism and concern for the history of his people and their language, could not have been the cause of his fossil interests.

Lister himself wrote several papers on antiquities, [1] and presented specimens, including two large altars, to the Ashmolean Museum. His contributions in this field are all dated several years after his earliest work on fossils, and there is no sign of antiquarianism in his early writings - even at Nimes, Avignon and Arles during his French travels in 1665-6. [2] It seems that the two fields of interest were both among the products of the intellectual interest which Englishmen were beginning to take in their country, rather than that one gave rise to the other. This was the period in which the English began to show a great interest in natural history in its widest sense: in their country; its people and their languages, its buildings and antiquities, its animals and plants, minerals, soils and waters, studied largely for their own sakes, and not only for their medical value. This was the sense in which Boyle understood the term in his outline for a specimen natural history of England, [3] and in which it was used in the county natural histories which began to appear at this period: for Oxfordshire, Staffordshire, Northamptonshire, Lancashire and Cheshire, and Ireland, with similar outline plans suggested for Wiltshire,

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7. (cont.) ...in Ducatus Leodiensis (London, 1715) pp. 21-30.

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1. Phil. Trans. 13 (1683) p. 70; Ibid. p. 238; Philosophical Collections of the Royal Society 4 (London, 1681-2) p. 87.

2. MSS Lister 19.

3. Phil. Trans. 1 (1665) p. 186.

Middlesex and Somerset. [1] There can be no doubt that Lister would have produced a similar volume had he taken up the suggestion of John Peck in 1681 that he should write a natural history of Yorkshire, a work which was in fact attempted by Lister's correspondent Dr. Johnstone of Pontefract. [2] One important aspect of this interest was the beginning of systematic collection of objects, and these included formed stones. Collections such as those of Lister, Plot, Coles, Beaumont, and Lhwyd provided detailed and accurate information which had previously been lacking. It was now possible, for example, to compare shellstones with shells from living animals, and to compare fossils from different rocks and from different parts of the country, and these comparisons were necessary if shellstones were to be systematically arranged. It was only then that a distinction between organic and inorganic origins became meaningful and important. The idea of an animal origin for fossils was already well established; it could not properly be challenged without a knowledge of the detail differences between living and fossil forms which was only now available. It is significant that the main opponents of the organic theory in England were the collectors and cataloguers just named -

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1. Robert Plot, The Natural History of Oxfordshire, being an Essay toward the Natural History of England (Oxford, 1677;)Plot, The Natural History of Staffordshire (Oxford, 1686); for proposed natural histories of Middlesex and Kent by Plot, see MSS Ashmolean 1817a; John Morton, The Natural History of Northamptonshire (London, 1712); Charles Leigh, The Natural History of Lancashire, Cheshire and the High Peak of Derbyshire (Oxford, 1700); John Aubrey, An Introduction to the Survey and Natural History of the North Division of the County of Wilts (London, 1714); for a proposed natural history of Somerset by John Beaumont, see Gunther 1939 p. 276.

2. MSS Lister 35, f. 47; Diary of Ralph Thoresby ed. Hunter 2 pp. 137, 142, 164, 167.

precisely those with greatest first-hand knowledge of the subject matter. The only prominent collector adopting the organic view was Woodward, who did not give a detailed or systematic account of his collection, and whose understanding of the subject was inferior to that of most other participants in the dispute; he was in fact something of an embarrassment to other writers holding an organic view.

Another characteristic feature of seventeenth century intellectual life was a strong interest in natural theology. Such an interest had been widespread in Europe in the Renaissance, but declined greatly after the Reformation; it almost disappeared in Catholic countries, and was not prominent in Germany until the end of the eighteenth century. In England, on the other hand, there was a continuous tradition in the seventeenth century which can be traced through the works of Hakewill [1] and Sir Thomas Browne, and which was developed in detail in the works of Burnet, Woodward, Ray, Whiston and Derham [2] at the end of the century and the beginning of the next. In these works, which had very varied receptions from theologians, arguments for or against geological change, and in particular those bearing on a universal flood, were vital, and fossils could play an important part in this. They were not necessarily used as evidence; Burnet and Whiston do not mention them; and they could be interpreted in quite different ways, as by Ray and Woodward. Even so, fossils now had more than medical or merely curiosity value. If they are organic, it must be accepted that there have been in the past great changes in

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1. George Hakewill, The Power and Providence of God in the Government of the World (London, 1627).

2. Thomas Burnet, Telluris theoria sacra (London, 1680), with author's translation, The Sacred Theory of the Earth (London, 1684); Ray 1692; Woodward 1695; William Whiston, A New Theory of the Earth (London, 1696); Whiston, Astronomical Principles of Religion (London, 1717); William Derham, Astro-theology (London, 1715).

the face of the earth. The question of timing was crucial here. To account for the finding of fossils in mountains, one must assume either that an immense length of time has passed since the Creation, during which the seas have retreated or the land has been in some way slowly built up or uplifted, or that geological change has been extremely violent in the past. The former was in conflict with the generally accepted date of about 4,000 B.C. for the creation, and would need an allegorical interpretation of Genesis; the alternative could be reconciled with the Noachian flood, and in this way fossils could be, and were, used in support of Genesis. This was perhaps one reason for the general acceptance of the organic view of fossils, though the detailed distribution of fossils presents difficulties for a diluvial theory. Another theologically acceptable solution was to assume that all change had been more violent in the youthful, vigourous earth.

Problems of biological change were less obvious, and were only apparent when close attention was paid to the form of the fossil. It then becomes clear that they do not represent extant species. Though it was suggested that the fossil forms still exist in remote places, it was impossible to explain the absence of living forms from the rocks. It would appear to those able to understand the evidence that either extinction or modification of species characteristics must be assumed in order to give an adequate explanation. Though not strictly in conflict with Genesis, both these were against tradition, and involved the revision of God's creation or the rejection of part of it.

These problems need not be faced if fossils were assumed to be inorganic productions; on the other hand, to look on them as organic allows them to be used as evidence for the flood, and this was attractive to many.

The growth of natural history and of natural theology made fossil origins a matter of importance; a theory to rival that of organic origins became possible with the development of a non-occult and 'mechanical' mineralogy.[1] Crystals were no longer being looked upon as produced by astral or plastic forces, and salts and spar were no longer thought to show a vegetative force in the ground. Crystals were now being thought of as products of the mechanical packing of their constituent parts; spars, the 'shooting' of salts and the growth of other mineral bodies were seen as comparatively straightforward processes of deposition of particles from a solution or suspension of the solid matter. The change in attitude which occurred early in the second half of the century can be well seen in a comparison of Sir Thomas Browne's description of the growth of (quartz) crystal, formed by'a seminal root and formative principle of its own',[2] and Robert Hooke's purely mechanistic interpretation of 1664.[3] This new minerology made possible the development of a new, 'mechanical' theory of fossil formation, needed to avoid the difficulties presented against the organic theory by the new natural history, the whole dispute being sharpened by the growth of natural theology.

(4) Writers on fossils in the second half of the seventeenth century.

Though the Dane Steno, working in Italy, and the Italians Agostino Scilla and Paolo Boccone wrote on fossils in the years around 1670, the controversy was centred in England in the last forty years of the century, around the work of Hooke, Ray, Woodward, Plot, Lister, Beaumont, Cole and Lhwyd.

Niels Stensen (Nicolaus Steno) (1638-86) was born in Copenhagen and studied medicine at the university there from 1656 to 1660. He continued his studies in the Netherlands, at Amsterdam and Leyden, and at Paris. In December 1666 he was at Montpellier, where he met Lister; this visit has not been noted by the biographers of Steno.[4] Lister

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1. J. G. Burke, The Origins of the Science of Crystals (Berkeley, 1966) pp. 20-43.
2. Browne 1646 in Keynes 1964, 2 p. 84.
3. Hooke 1665 pp. 82-8 and plate 7.
4. eg. J. G. Winter, introduction to Prodromus to a Dissertation concerning a Solid Body enclosed by a Process of Nature within a Solid (Ann Arbor, 1930), being an English translation of Steno's De solido

wrote that he 'had the honour to assist at an Anatomie lecteur .. made by Mr. Steno the Dane himselfe in my Lord of Aylesburies cabinet...' Lister found Steno 'infinitely taking and agreeable in conversation ... very much of the Galant and honest man ... as well as of the Scholler ...' At this time, neither had made any public declaration on fossils, and the subject was not raised between them, though Lister met Steno privately after the lecture; the discussion appears to have been entirely anatomical.[1]

From 1666 to 1677 Steno lived mainly in Florence as Physician to the Grand Duke of Tuscany and as tutor to the son of the next ruler, Cosimo III. As a Catholic convert, he spent the years from 1677 to his death in the service of the church in northern Germany. He published many medical works from 1661 to 1675, and two geological works, in 1669 and 1671, much the more important being his Prodromus to a Dissertation on a Solid Naturally Occurring Enclosed within a Solid of 1669.[2] The work is important mainly for its treatment of the laying down of rock strata under water, and the formation of rivers and valleys; he used as evidence for this the occurence of fossil shells and crystals embedded in now solid rock. He dealt with the petrifaction of the shells, but provided no arguments against an inorganic theory of their origin. Steno's work was quickly translated into English by 'H. O.' - presumably Henry Oldenburgh, and this provoked Lister into his first public statement on the nature of fossils.[3]

Paolo Boccone (1633-1704) of Sicily was also interested in fossil shells as evidence of earth movements.[4] He presented a geological

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4. (cont.) intra solidum naturaliter contento dissertationis prodromus (Florence, 1669).

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1. MSS Lister 5 ff. 224-6.
2. Steno 1669 in Winter 1930.
3. Phil. Trans. 6 (1671) p. 2282.
4. Paolo Boccone, Recherches et observations naturelles (Amsterdam, 1674); Museo di fisica e di esperienze (Venice, 1697).

collection to the Royal Society in 1673; the accompaying account claims that it included pieces which showed the alteration and petrification of sea-shells; the resemblance between glossopetra and sharks teeth; and others produced by the impression of real shells. [1] Another Sicilian, Agostino Scilla, wrote at length on the organic nature of fossils. Though the book was in Italian, and was not translated into English, his often original arguments had a wide influence in England because of the very long review the book received in the Philosophical Transactions; this amounted to a precis of the work. [2]

In England, the chief proponents of the organic view were Robert Hooke, John Woodward, and John Ray.

Hooke (1635-1703) was Secretary to the Royal Society and professor of Geometry at Gresham College; his primary interests were not biological or geological, and he was not a systematic collector of fossils; but his use of crystals and 'petrified wood' as objects for microscopical study produced the first modern discussion of the nature of formed stones in English. [3] His acceptance of the organic nature of fossils was coupled with a more radical approach to the history of the earth and of organic life than any of his contemporaries. He accepted, not only drastic changes in the earth's surface, but also the extinction of some animal species and the production of new genetic varieties of others. These ideas were contained in addresses given to the Royal Society and published posthumously. [4] One of these was dated by Hooke as 1668, which would pre-date the work of Steno; but

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1. Birch 3 pp. 116-17; Phil. Trans. 8 (1673) p. 6158.
2. Agostino Scilla, La vana speculazione distingannata dal senso (Naples, 1670); reviewed in Phil. Trans. 9 (1674) p. 181f.
3. Hooke 1665 pp. 107-12.
4. The Posthumous Works of Robert Hooke, edited by Richard Waller (London, 1704).

Hooke's editor, Richard Waller, suspected that this address, which was never read to the Society, was not in fact written until about the same time as the others, ie. in the 1680's. Hooke spoke against Lister's views when they were first read at a meeting of the Royal Society in 1671. [1]

John Woodward (1665-1728), professor of Medicine at Gresham College, shared Hooke's view on the nature of fossils, but drew a very different conclusion from this. [2] Woodward accepted the Noachian flood literally, and claimed that fossil organisms provided a demonstration of its having occurred, and even the season of the year in which it happened. Otherwise, he rejected any significant geological or biological change. Woodward, whose collection was to form the basis of the Woodwardian Museum at Cambridge, was the only prominent collector to reject an inorganic theory of fossil origins, though the catalogue of his collection shows that it contained <u>fossilia</u> of all kinds, the formed stones making up only a small part; they were not classified into a systematic arrangement of genera and species. [3] His relationship with other contemporary naturalists became very bad; Lister wrote of him as 'the Arch-pirate' and as a spreader of malicious rumour in the London coffee-houses. [4] Woodward, an'idle, vain, despicable wretch', 'vilified' Ray in public, and broke off his correspondence with Lhwyd with some unpleasantness, claiming that Lhwyd was taking bread out of his mouth by writing another book on formed stones. [5] His public quarrels with Dr. Richard Richardson and Sir Hans Sloane became so acrimonious that the Royal Society had to take the unprecedented step of expelling Woodward from membership. [6]

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1. Birch <u>2</u> p. 487.
2. Woodward 1695 op. cit.
3. <u>An attempt towards the Natural History of the Fossils of England, in a catalogue of the English fossils in the collection of John Woodward, M.D.</u> (London, 1729).
4. MSS <u>Ashmolean 1829</u> f. 43.
5. MSS <u>Ashmolean 1819</u> f. 119.
6. (over)

John Ray(1627-1705) was a careful and enquiring naturalist, though he did not attach much importance to minor differences between fossil and living shells, not being a collector: 'For, although I have often seen, and myself also sometimes gathered of these bodies; yet I did never curiously note the texture, parts and differences of them'. [1] He was in general convinced of the organic origin of formed stones, and gives a well-argued and well-known case for this view. [2] However, the existence of belemnites, ammonites and other forms very different from any living animal, even in the eyes of a non-specialist, puzzled him, as he was unwilling to accept the idea of any revision of God's creation. Though he suggested the survival of these animals in the depths of the oceans, the idea did not apparently completely convince him, and he admitted that 'In fine, these Ophiomorphous stones do more puzzle and confound me, than any other of the formed Stones whatsoever...' [3] It is very significant that Ray's other doubts lay in the field of fossil plants, in which, as England's greatest botanist, he was more competent to judge the evidence. Though at first doubtful about the nature of these objects, [4] he later became inclined to regard them as inorganic:

> 'I must not dissemble, that there is a phenomenon in Nature, which ... seems strongly to prove, that Nature doth sometimes ludere and delineate Figures, for no other end, but for the ornament of some Stones ... That is, those elegant impressions of the Leaves of

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6. R. Weld, History of the Royal Society 1 (London, 1847) p. 337; see also V. A. Eyles, 'John Woodward, F.R.S., F.R.C.P., M.D. (1665-1728): a life and work Journal of the Society for the Bibliography of Natural History 5 (1971) pp. 399-427.

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1. Phil. Trans. 10 (1675) p. 278.
2. Ray 1692 p. 106f.
3. Ibid. p. 125.
4. FCJR pp. 204-13

Plants upon Cole-slate...' [1]

It seems that his religious beliefs prevented him from following too far the problems set by an organic theory of fossils; he was far from being a radical in natural philosophy. He insisted on a universal flood when many were prepared to accept an inundation restricted to Palestine, and he was hostile to attempts to rationalize Genesis, as by Burnet, Woodward and Whiston. As he accepted a plastic virtue in nature, [2] he would have had little difficulty in accepting one of the older non-organic theories of fossils. At the end of his life, he seems to have lapsed into complete uncertainty; in spite of his definite statements of 1692, in each of the five editions of the Wisdom of God published between 1691 and his death, he said of the forms, which 'inanimate mix'd bodies' can take, that 'any of them shoot ... into those that are more elegant and compounded, as those form'd in imitation of the Shells of Testaceous Fishes of all sorts, Sharks Teeth and Vertebres &c. If these be original stones, or primary Productions of Nature in Imitation of Shells, and Fishes Bones, and not the Shells and Bones themselves petrified, as we have sometimes thought'. [3]

Ray's work shows clearly that, with a non-evolutionary world picture, an expert's familiarity with the subject-matter of the fossil controversy was an obstacle to the acceptance of their organic origin.

The English naturalists putting forward what we may now for the first time call a truly in-organic theory were Lister, Plot, Cole, Beaumont and Lhwyd.

Robert Plot (1640-96) was the first professor of Chemistry and first Keeper of the Ashmolean Museum at Oxford, and at one time

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1. Ray 1692 p. 106.

2. Ray 1691 (1722 edition) pp. 77, 102.

3. Ibid. pp. 69-70.

Secretary to the Royal Society. His Natural History of Oxfordshire contained the first attempt to give a comprehensive, illustrated account of formed stones, being published very shortly before Lister's work. Plot gives these stones, as mineralogical objects, a place in his systematic description of the Heavens, airs, waters, earths, stones, plants, animals and people of his country. For him, the problem of the origin of shell-stones is one of crystal chemistry. Plot was on good terms with Lister. [1]

William Cole (d. 1701) was a wealthy customs and excise official of Bristol. Though he published nothing on the subject of fossils, [2] he was influential through his correspondence with Ray and Lhwyd, and because of his collection, which appears to have been the best in the country. He appears not to have had a pleasant personality, and was disliked by Lister and Lhwyd. After Lister had visited him to see his collection, Cole tried to establish a correspondence between them, but Lister did not accept. He thought Cole a 'fickle and verie conceited man, and not to be dealt with'. However, as Cole was rich, without dependants, and had promised to bequeath his collection, with an endowment, to the University of Oxford, Lister advised Lhwyd to humour him. [3] Cole died without making these arrangements, and the collection was broken up and sold off by the executors while Lhwyd was trying to persuade the University to buy it. Most of his letters show Cole to have been quite scathing in his attitude towards those thinking fossil shells to be animal in origin, though he finally suggested a theory which included both an organic origin for fossils and also an explanation for

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1. Plot's Natural History of Staffordshire (Oxford, 1686) contains similar ideas.

2. Cole did publish an account of living Murex (actually Nucella) shells: Phil. Trans 15 (1685) pp. 1278-86.

3. MSS Ashmolean 1816 ff. 111, 117.

the differences they show from modern shells, without involving extinction or species modification.

Dr. John Beaumont (d. 1731) was a Somersetshire physician and naturalist, and a correspondent of Lister, Lhwyd and Plot. He projected a Natural History of Somersetshire, which according to his preliminary draught, was to be modelled on Plot's Oxfordshire. [1] He was to include 'stones curiously wrought by nature', but as the work was never published, his theory of fossil origins can only be found in two long papers in the Philosophical Transactions, giving a careful and detailed account of 'rock-plants' (crinoids). [2] He compared these structures with coral, itself thought at the time to be a kind of stone. They were thought to be in some respects intermediate between the mineral and vegetable kingdoms, and he pointed out that the ducts found in these fossils show that they have grown not merely 'by juxtaposition'. However, he stressed that the best way to explain their growth is by comparison with spars (of which coral is one), crystals, snowflakes and coal-leaves - it being assumed without question that the latter are inorganic in origin, and he insisted that the earth can produce shells as well as the sea. He rejected the flood as an explanation, though allowing the 'rock-plants' to have been formed under water, and he attacked Burnet for introducing biblical matters into Geology. [3]

Edward Lhwyd (1660-1709), Plot's successor at the Ashmolean Museum, was perhaps the most active field collector of the period. His position brought him into contact with most of the naturalists of the time, and he became a close friend of Lister's. Each was the principal correspondent of the other. His catalogue of British fossils [4] was the first published work to be devoted entirely to formed stones. It had 23

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1. Gunther 1939 p. 276.
2. Phil. Trans 11 (1676) pp. 724-41; Phil Trans 13 (1683) pp. 276-80.
3. MSS Lister 36 f. ix.
4. Edward Lhwyd, Lithophylacii brittanica iconographia (London, 1699).

excellent plates, and gave a first-class descriptive account of all the fossil plants and animals known at the time. The work would have been even more influential than it was had it not been so rare. Although it was based on the Ashmolean collection, Lhwyd could not persuade the University to publish it; it was eventually brought out by a group of nine private sponsors, including Lister, Isaac Newton and Sir Hans Sloane, but only 120 copies were printed. At first, Lhwyd was firmly convinced that fossil shells were mineral growths,[1] but he began to have doubts after about 1692, though he could not accept the organic theory. His letters show a great deal of uncertainty, and for many years he would not commit himself to a firm opinion. Eventually, he compromised with a theory usually regarded as his own.

According to this, formed stones had grown in situ, and were not evidence of any great geological change. They had been generated from the spawn (seminum) of sea-creatures, evaporated from the sea together with water vapour, and brought down to the land in rain to percolate through the rocks. The unnatural conditions under which they grew often distorted their growth, so that the shell-stones are not identical with those growing in the sea. The letter containing this theory [2] was appended to the Lithophylacii Britannici iconographia. The theory is fact almost identical, though more detailed, with older ideas such as those given by Sir Matthew Hale (1609-1675), almost thirty years before.[3] Hales held the conventional view that fossils were the remains of shells left behind by the flood; the semina in the rain-water were part of an alternative explanation given by him. Another correspondent of Lister, Jabez Cay of Newcastle, was a practical mineralogist and lead miner; he collected and classified shell stones, distinguishing, for example,

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1. Gunther 1945 p. 79.

2. MSS Ashmolean 1820 f. 28.

3. Sir Matthew Hale, The Primitive Origination of Mankind (London, 1677) p. 193.

different species of ammonites, and he noted the differences in the anatomy of ostracites and oysters. He did not subscribe to any theory of fossil origins; as a practical man, he was sceptical and refused 'to burn my fingers over the intricate question, what they are?' [1]

(5) The growth of Lister's geological interests.

Lister's interests in Geology developed early in life and were sustained as long as he was writing on philosophical subjects. The diary of his journey to Montpellier in 1663, [2] the oldest of Lister's surviving writings, shows that he spent some of the 19 days in which he was stormbound at Weymouth in examining the 'south-west beach', ie. Chesil Bank. He was struck by the difference in water level on the two sides of the bank - 'many fathoms', even though water soaked through the shingle; and also by the gradation of pebble size on the beach - a problem not yet solved. [3] Lister's explanation was that the small pebbles low down on the beach were in the region of greatest water turbulence, and so had suffered more rapid grinding. During the land journey from Bordeaux to Montpellier, he recorded the nature of the soil at each halting place, apparently in an attempt to correlate this with the quality of the wine produced in the district. At the other end of his life, during his visit to Paris in 1698, Lister spent much time examining mineral collections, discussing magnetism and visiting quarries to observe the rocks from which millstones were made and to look for fossil shells. [4] Of his last four papers in the Philosophical Transactions, in 1699-1701, three were on geological subjects.

A large part of this interest was utilitarian. He wrote on medical springs, leaving manuscripts, papers in the Philosophical Transactions

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1. MSS Sloane 4025 f. 70

2. MSS Lister 19.

3. See J. Steers, The Sea-Coast (London, 1953) p. 158.

4. JP pp. 77, 8083, 84-7, 144-5, 226-7.

and a full-length book, and was interested in the commercial distillation of salt-water; [1] he showed much interest in metal ores and smelting. This was at least partly because he hoped to find and exploit mineral resources on his own land; he wrote to Ray in 1669:

> 'You will be pleased, at your best leisure, to send me an account of the best Authors that have written on minerals and fossils, for I am, as I said, but a beginner in this part of natural history, and I have great encouragement, besides my profession, not to be ignorant in this part especially, having great hopes of considerable mines in my own Lordship in Craven; therefore I would furnish myself with the best Authors'.

This must refer to Lister's inheritance of the Manor of Winterburn from his grandfather Michael. In reply, Ray recommended Agricola and Lazarus Erker; and possibly Kircher, though Ray was 'not improved' by this. Lister also asked Ray to send samples of iron ore from Sussex and tin ore from Cornwall.[2] He wrote a paper on the modern and ancient methods of making steel, [3] and presented to the Royal Society a collection of specimens showing the history of iron ore. There are frequent references to Lister's minor metallurgical contributions at the meetings of the Royal Society during the period of his active participation from 1683-6; he also gave advice on the making of cement. [4]

However, Lister also showed a more academic interest in geology. He read a paper on magnetism to the Royal Society in February 1683-4, [5] and published a series of papers in the Philosophical Transactions on the

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1. MSS Lister 35 f. 114.
2. CJR pp. 48, 52; FCJR pp. 38, 134; CJR p. 112.
3. Phil. Trans. 17 (1692) pp. 865-7.
4. Birch 4 pp. 202, 287.
5. Ibid. pp. 261-2.

nature of earthquakes, which he thought were caused by the spontaneous ignition of large subterranean masses of pyrites. [1]

His work on the classification and distribution of rocks and soils is of more relevance to the fossil problem, and Lister was on the brink of adopting a stratigraphical approach to the subject. He prepared a manuscript 'On the fossils of England, more particularly such as are found in these Northern parts', [2] written, it seems, about 1671-3; the classification of sands and clays is more elementary than in his later paper of 1674. The manuscript is a 113 leaf ledger, most of whose pages are about half blank, apparently with the idea of completing them for publication. The classification adopted in this work is reproduced here. It shows the same method of repeated sub-division that Lister used for animals, and demonstrates the position held by formed stones in his mineralogy. The fossils we now recognize as organic in origin are not treated in detail in this work.

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1. Phil. Trans. 14 (1684) pp. 512, 515, 517.

2. MSS Lister 7.

- Earthy concretions
  - Soften in water
    - Cole
    - Fuller's earth
    - clay
    - marle
  - Have been manifestly in a liquid state
    - gur
    - soapstone
    - sulphur
    - stalactites (water-wrought stone)
  - Float, in-flammable
    - bitumen
    - peat
- Stones
  - Hard and friable
    - Huge masses, mostly horizontal but sometimes perpendicular or at an angle
      - Endure fire
        - rag, finstone
        - pebles
        - sandstone
        - crawstone (coal rock)
        - alum rock
        - ironstone
        - white flint
        - black flint
      - Fuel
        - coal
        - jet
        - amber
      - Burn to lime
        - gypsum
        - lapis specularis
        - limestone
    - Heaps accidentally brought together, broken fossils
      - sand
      - gravel
  - Contained within hard masses
    - In veins
      - Oar
        - lead
        - silver
        - copper
      - Bright, like oar
        - black lead
        - sparr
        - lapis calaminaris
    - transparent
      - crystals - various
    - Opaque, animal or plant shape
      - troch [ites]
      - astro [ites]
      - glosso [petra]
      - conch [ites]
      - lap [is] jud [aica]
      - belemn [ites]
      - cornu per [sica]
      - lignum fossile

<u>Lister's classification of the fossils of the north of England.</u>

In 1684, Lister published a similar table in which a more detailed classification of sands and clays was given; [1] he wrote that this was drawn up about ten years earlier. The table includes twenty-nine sands and twenty-one clays, the main divisions being:

- Sand
  - Sharpe
    - fine
      - white
      - grey
      - reddish-browne
    - course
      - greisly
      - browne
  - Soft
    - from lime-stone with mica
      - silver-like
      - gold-like
- Clay
  - pure
    - greasy
    - harsh when dry
    - stony when dry
  - mixed with round sand
  - mixed with flat or thin sand

Lister realized that these 'fossils' were not distributed in a completely random manner. He had traced the course of a fossiliferous bed, 'that black dirty vein which crosses England through Bedfordshire and Cambridgeshire' into Huntingdonshire; [2] he had followed the distribution of a particular belemnite through the Wolds of Lincolnshire and Yorkshire, 'above a hundred mile in Compass'; [3] and he knew that the chalk hills of the south of England were continuous with those of northern France. [4] Lister was the first to suggest that observations of this nature could be used in the compilation of a 'Soil or Mineral Map of England', on which the boundaries would be marked of areas showing different kinds of soil; colours or etchings were to be used to show these. Yorkshire, for example, would be divided into areas which would include

> '(1) The Woolds, Chaulk, Flint and Pyrites, &c. (2) Black moore; Moores, Sand-Stone, &c. (3) Holderness; Boggy, Turf, Clay, Sand &c. (4) Western Mountains; Moores, Sand-stone, Hall-playster, or

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1. Phil. Trans. 14 (1684) p. 739.
2. MSS Ashmolean 1816 f. 82. Lister was perhaps writing of the Ampthill clay.
3. HAA p. 228.
4. Phil. Trans.14 (1684) p. 741.

Gypsum, &c'.

It was suggested that

'something more might be comprehended from the whole, and from every part, than I can possibly forsee, which would make such a labour very well worth the pains'.[1]

This interest in the classification of and distribution of 'fossils' did not lead Lister into a modern attitude towards historical geology, even though he had realized the correlation between fossil shells and the type of rock in which they are found. There were two key elements missing from Lister's outlook - the ideas of the stratigraphical arrangement of rocks and of geological time. Although the beginnings of stratigraphy can be seen in Steno's work, the idea was not really developed until the second half of the eighteenth century. Lister did not differentiate between what we now call solid and surface geology; he was 'of the opinion, such upper Soiles, if natural, infallibly produce such under Minerals, and for the most part in such order'.[2] His fossil-collecting expeditions to quarries did not give him any real understanding of the three-dimensional arrangement of rocks in the earth's crust; Lister was concerned almost entirely with the surface of the earth. As will be shown below, he appears to have had a vague idea of time stretching back over immense eras, but at the same time failed to recognise the magnitude of the geological changes which could take place over such periods.

Lister's interest in formed stones appears to have developed at the same time as that in general natural history and geology. He wrote in 1671 of his theory of shell-stone formation as having been formed 'several years ago',[3] though there is no mention of these fossils in the manuscript dealing with his French travels of 1663-6, and

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1. Ibid. pp. 739-40.

2. Ibid. p. 740.

3. Phil. Trans. 6 (1671) p. 2283.

he makes no mention of fossils in connection with his meeting with Steno. Lister's collecting localities are mostly in Lincolnshire and especially in Yorkshire, with some in Nottinghamshire, Leicestershire and Northamptonshire. There is only very occasional mention of Cambridgeshire. From this it would seem that Lister's collecting was begun towards the end of the period in which he was a Fellow of St. John's, and spending his summers at Burwell and in Craven, but after his return from France; that is, probably about 1668. This agrees with evidence from his correspondence with Ray. After settling at York, Lister began to collect and describe shell-stones in a systematic way; most of his localities are in the eastern half of Yorkshire, or in Craven, where he had property and which he seems to have visited often. In 1672, he described how he 'purposely visits' quarries to look for shells, and by 1674, his list was of 'near 30 species'. [1]

In addition to his own searches, Lister received specimens from his correspondents, as from Miles Gale of Keighley and John Bolland of Halifax. [2] It is not usually possible to tell from his accounts whether a specimen was collected by Lister himself or not, but he does sometimes plainly describe his own collecting of specimens, as at Bugthorpe, East Yorkshire, and Beauvoir Castle, Northamptonshire. The directions he gave to Lhwyd for finding particular fossil localities in Lincolnshire and Huntingdonshire show that he had visited them personally. [3]

At some time in the 1670's, Lister must have presented specimens, which would almost certainly have included shell-stones, to the Royal Society; in 1681, the Secretary, Nehemiah Grew, wrote to Lister, promising to have them put in order, they having been neglected. Soon after he had

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1. CJR p. 99; Phil. Trans. 9 (1674) p. 96

2. MSS Lister 35 f. 76; Diary of Ralph Thoresby 3 p. 259.

3. Phil. Trans. 10 (1675) p. 274; 8 (1673) p. 6187; MSS Ashmolean 1816 ff. 82, 88.

Two of Lister's Craven
fossil-collecting
localities:
right, limestone
quarry between
Friar Head and
Eshton Tarn;
below, Middop Wood.

moved to London, it was agreed by the Society that Lister should be allowed to supervise the building of a drawered cabinet to house all the Society's mineral collections, and to reduce them to order and method. [1]

By the 1690's, Lister appears to have almost ceased active field work, but he continued to add to his collection. He was a member of a syndicate which employed a professional collector to travel through England and search for fossil shells. Lhwyd was a member of the group, which included three others, from Wales, Yorkshire and Cumberland. The names of these three are not given in any of the manuscripts, but judging from Lhwyd's correspondents, they were probably Dr. Williams, Archdeacon of Cardiff; Dr. Richard Richardson, of Bradford; and Bishop Nicholson of Carlisle, (or perhaps Hugh Todd, Fellow of University College, Oxford, and holder of several livings in Cumberland). Each member was to contribute 1/6d each week to pay the man's wages, and the fossils obtained were to be shared. Lister offered 1/- a week 'over and above' this, and asked that the man be sent to Lincolnshire, sending directions for this to Lhwyd, who acted for the group in their dealings with the man. The arrangement lasted more than two years, until August, 1695, when the man, by the name of Smith, was dismissed in Lincolnshire for refusing to part with the shells he had collected. [2]

Lister's collection must have been extensive, and as early as 1674 was housed in a specially built cedarwood cabinet; he appears to have been jealous of it. When, in 1686, the Royal Society asked him and Sir Hans Sloane to send specimens to Dr. Rudbeck at Upsalla, Sloane agreed, but Lister asked to be excused. Similarly, when Lhwyd asked for specimens from Lister's collection for the Ashmolean Museum, Lister

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1. MSS Lister 35 f. 49; Birch 4 p. 250.

2. MSS Lister 36 ff. xvii, xxv, lxviii; MSS Ashmolean 1816 f. 99.

protested that his collection was poor, and would contain nothing that the museum did not have already, even though Lister had been very generous in donating books and archaeological specimens to the museum. Nevertheless, when Lister offered his collection of shells, and probably shell-stones, for sale to Sloane, he asked a high price. [1]

Lister's writings on fossils are scattered through unpublished manuscripts, letters, papers published in the Philosophical Transactions, and in printed books. There are two important manuscripts in the Lister collection, both of which appear to date from the early 1670's. One, the 'Account of the fossils of England', already described, includes an elementary classification of formed stones. The other [2] is a refutation of the arguments put forward by Fabio Colonna in favour of an organic origin. There are several references to fossils in Lister's letters to Ray, and much of his correspondence with Lhwyd deals with specimens, and with books and personalities concerned with formed stones. By this period, however, Lister had little new to say on the subject, and there is little of controversy in this correspondence.

Four of Lister's papers in the Philosophical Transactions deal with the origin of organized fossils; these were dated 1671 (refuting Steno's theories), 1673 (on fossil 'plants', actually crinoids), 1674 (on shell classification) and 1675 (on star-stones; that is, fossil corals). His most important treatment of the subject in a printed book is that given in Historia animalium Angliae. This contains a fifty page appendix on shell-stones; a systematic account of the species known to Lister and a preface dealing with their origin are included. His theory of the growth of mineral bodies is treated in more detail in his account of English mineral waters; though formed stones are not

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1. MSS Sloane 4064 f. 206; 4041 f. 23.

2. MSS Lister 5 ff. 118-9.

specifically included here, Lister makes clear in his other writings that his theories do include them. His major work on shells, the Historia conchyliorum, includes a section on shell-stones, but this work consists entirely of plates, with no text; and he outlined his ideas once more in the Exercitatio anatomica tertia.

(6) Lister's systematic work on fossils.

Lister's manuscript on English 'fossils' distinguished eight 'animal or plant-shaped stones': trochites (crinoids), astroites (corals), glossopetra (sharks' teeth), conchites (shellstones), lapis judaica (cystids), belemnites, cornua persica (ammonites) and lignum fossile. Although his paper of 1673 on trochites mentioned and illustrated several types of these fossils, only the conchites and ammonites were ever classified, named and described. This was in his book of 1678; he had been compiling a list of shell-stones since at least 1673 and it had reached thirty by 1674.[1] These fossils are given an arrangement based entirely on their form, and not upon their mineralogy, so that Lister's classification is very similar to those which he gives for living molluscs, often using the same features. The number of valves, form of spiral, if present, the pattern of grooves and striations, the position of the hinge, form of valves and presence of 'ears' in bivalves are the characteristics used in Lister's classification. Individual species descriptions also include the size, weight and material, number of turns of the spiral in univalves, and the presence or absence of a spine and umbilicus in ammonites. Lister also pointed out any imperfections in the shellstone, any mineral growths on its surface, signs of wear and tear, marks of muscle attachments, worm tubes, and whether or not bivalves occur complete or as one valve only; many of these features were looked on as being important in deciding the origin of the stones. Lister's classification is given below.

This is the first real attempt at a classification of fossil shells. Plot, though giving a certain amount of detail in his

1. CJR p. 99; Phil. Trans. 9 (1674) p. 97

descriptions, did not set a formal division into groups, being content to refer to cockle-shellstones, oyster-stones, and so on. The echinoids and belemnites were dealt with as bodies associated with the heavens, though Plot rejects the vulgar idea that they are thunder-bolts; the old names and order are retained 'though ill applyed to the nature of things, [rather] than put my self to the trouble of inventing new ones'. Lister's classification does not of course separate molluscs from brachiopods and echinoids, but this had not been done for the living forms at the time, and even if it had, this would not necessarily affect Lister's classification, which need depend only on the form of the stone.

Lister made an attempt to provide a comprehensive account of all the English shell-stones, 'not wishing to omit anything which the earth may produce anywhere'. He realized that this was too large a task to be done successfully at the time: 'I have no doubt, however, that these islands contain many more kinds of these stones than either he [Plot] or I have described. I have myself many fragments of shell-stone which are not fully described, as they would be a blemish in such a history as this. I shall put them off for another time'. [1]

To make the book more complete, Lister added figures and accounts of species not seen by him, taken, with acknowledgement, from Plot's Natural History of Oxfordshire, which was published very shortly before Lister's own work. Of Lister's 59 species, 20 are taken straight from Plot, the plates being copied exactly and therefore being printed in reverse. Plot only gave figures of his types, with little or no description, and did not name them. Lister added appropriate names to these 20 species, and in addition identified 13 of his own species with some of Plot's drawings.

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1. HAA p. 202.

Lister's species cannot usually be identified with any real certainty. Even though his descriptions are a marked advance on those of his predecessors and contemporaries, and his plates (apparently by William Lodge of York) are very good for their period, they do not reveal the minute differences needed to differentiate between the tens of thousands of known species of fossil molluscs in England. Some of Lister's 'species' probably represent several true species. The geological information given is of little help, as Lister gives only the general geographical location in which the specimen was found, and this can cover a wide range of geological formations; as already mentioned, Lister had no clear idea of stratigraphy. A list of Lister's species is given, with their localities, probable geological formations and possible modern names.

Lister distinguished five species of trochites (crinoids) in his long paper of 1673, and illustrated fragments of them in the 37 figures illustrating the paper. He similarly described three species of astroites (corals) in 1675, but in neither case was there any attempt to classify or name the individual types. In 1674, he gave a detailed description of a glossopetrum, and noticed the occurrence of several species of lapides judaici (cystids), but none of these was named, and only one described in any detail. [1]

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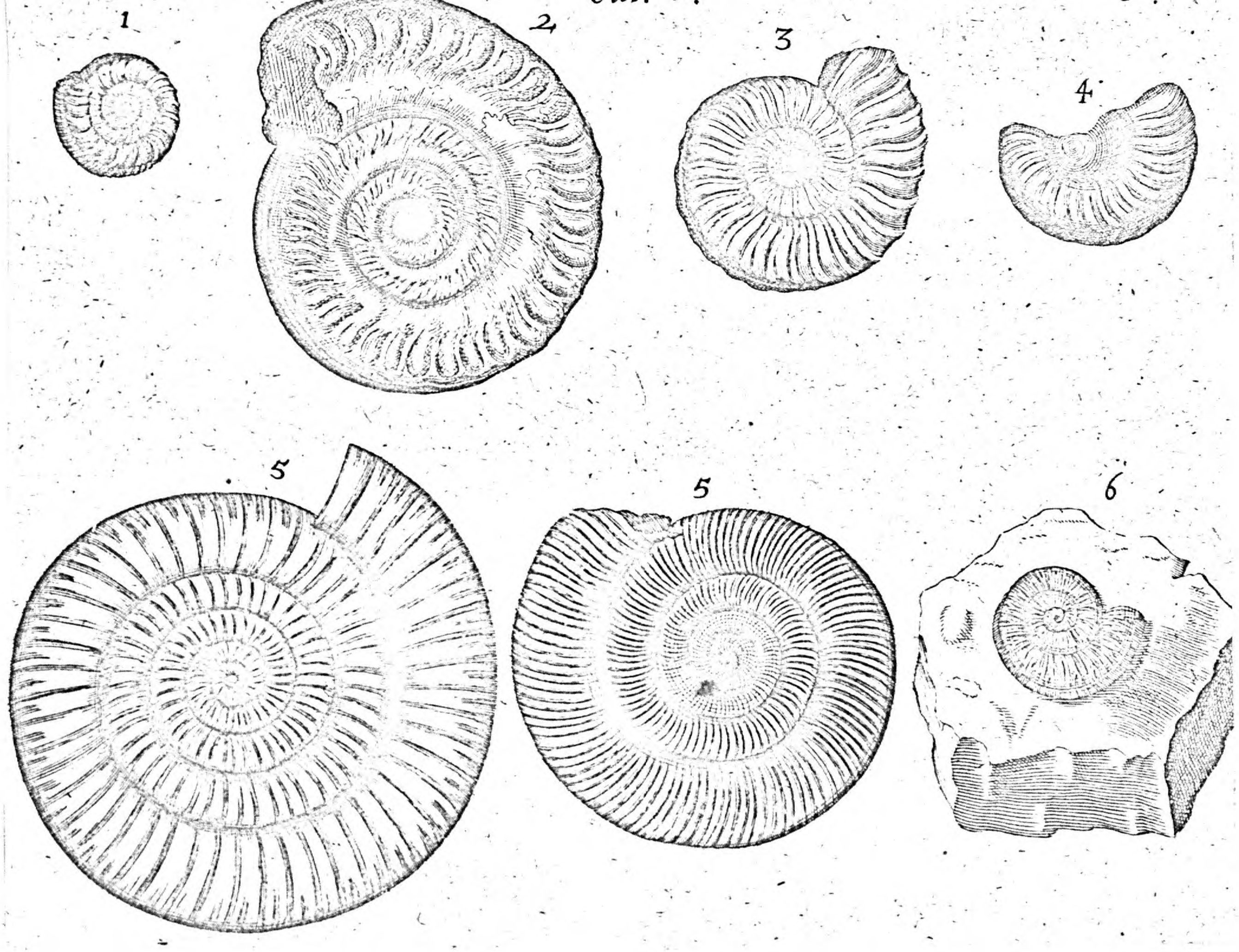
1. Phil. Trans. 8 (1673) p. 6181; 10 (1675) p. 274.

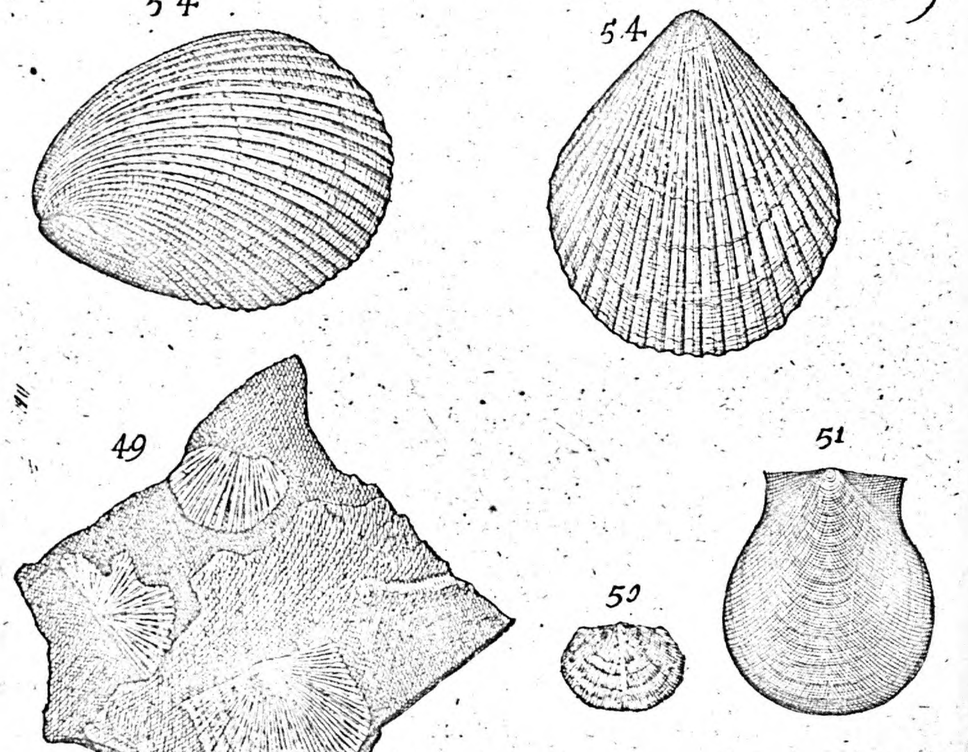
Part I Cones	Section I Spirals	Membrum I Wound around them- selves	Article I Equally con- cave on both sides	Chapter I Striated	Tituli I-VII
				Chapter II Smooth	VIII
			Article II One side more concave.		IX
			Article III Umblicus prominent both sides		X
		Membrum II Extended like a whelk	Article I Striated		XI-XIII
			Article II Smooth	Chapter I Elongate	XIV, XV
				Chapter II Compact	XVI, XVII
	Section II No spiral		Article I Striated, round	Chapter I evenly striated	XVIII-XXV
				Chapter II Unevenly striated	XXVI-XXX
			Article II Smooth, slender, pointed		XXXI-XXXII
Part II Bivalves		Membrum I smooth	Article I both valves evenly curved	Chapter I Asymetrical	XXXIII-XXXV
				Chapter II Symetrical, not deep	XXXVI
				Chapter III Deep from hinge to mouth	XXXVII-XXXIX
				Chapter IV Bellied	XL-XLII
			Article II unequal valves	Chapter I Ostracites	XLIII, XLIV
				Chapter II with rostrum	XLV-XLVII
		Membrum II striated		Chapter I Eared (Pectens)	XLVIII-LI
				Chapter II no ears (Cockles)	LII-LIX

<u>Lister's classification of Shellstones.</u>

Tab: 6.



Tab: 9.



Fossil

plates from

<u>Historiae</u>

<u>animalium</u>

<u>Angliae</u>

Lister's fossil list of 1678.

Number, name, locality, suggested geological formation and identification.

I. Ammonis cornu maximum striis crebris in ipso ambituparum eminentibus (Nunnington Speeton; Middle/Upper Oolite) Aspidoceratus.

II. Ammonis cornu spina in ambitu eminente striis lateralibus paulo ultra mediam (Whitby; Lias) Hildoceras.

III. Ammonis cornu spina in ambitu eminente striis lateralibus ex toto orbem extimum trajicientibus (Bylands, Bugthorpe; Oolite) Cosmoceras.

IV. Ammonis cornu striis lateralibus in medio ambitu ad acutes angulos concurrentibus (Bugthorpe, Speeton; Cornbrash) Macrocephalites.

V. Ammonis cornu 5 anfractum singulis striis spinam trajecientibus (Whitby, Wansford; Oolite) Perisphinctes.

VI. Ammonis cornu striis lateralibus versus ambitum furcatis (P) (Cleydon, Oxford; Lias) Dactylioceras.

VII. Ammonis cornu striis lateralibus versus spinam concurrentibus (P) (Great Rowlwright; Lower Oolite) Stephanoceras.

VIII. Ammonis cornu, laeve, pellucidum crebris articulis (Bugthorpe; Lias) ?

IX. Ammonis cornu laeve ex altera parte ad umbilicum cavam (Eshton; Carboniferous (Clitheroe) Limestone) ?

X. Ammonis cornu vix duorum orbium fere reticulatum quodam opere depictum (Colne, Halifax; Lower Coal Measures) Homoceras.

XI. Buccinites magnus ventricosus et striis et rugis quibusdam inordinatis donatus (Bugthorpe; Lias) Pleuratomaria.

XII. Buccinites exiguus striatus stria media singulorumorbium paulo eminentiore (Bugthorpe; Lias) ?

XIII. Strombus eleganter striatus dimidium digitum longitudine vix explens (P) (Heddington; Oolite) Cerithium.

XIV. Buccinites laevis albidus spiris numerosis inter se haud contiguis (Hinderskelfe, Pickering; Oolite) Pseudomelania.

XV. Buccinites laevis sublividus spiris octonis arcte inter se conjunctis (Craven; Carboniferous Limestone) ?

XVI. Cochlites laevis ore ad amussim rotundo exiguo (Whitwell; Oolite) Bathrotomaria.

XVII. Cochleomorphites sex spirarum (P) (Teynton, Shotover) ? Pleurotomaria.

XVIII. Echinites siliceus vertice fastigiato ('South' ~~Norfolk~~; Chalk) Conulus

XIX. Echinites orbiculatus depressus siliceus quibusdam Ombria (Norfolk; Chalk) Plagiostoma.

XX. Echinites parvulus strii capillaceis undique insignitus (P) (Teynton; Oolite) Anabacia.

XXI. Echinites vertice planiore striis e tuberibus quibusdam grandioribus conflatis donatus (Hinderskelfe; Oolite) Hemicidaris.

XXII. Echinites albido-cinereus extra ex parte interna siliceus nigricans (P) (Stonorhouse; Chalk) Cidaris.

XXIII. Echinites ovarius (P) (Teynton; Oolite) Stomechinus.

XXIV. Echinites ovarius parvus (P) (Teynton; Oolite) Pseudodiadema.

XXV. Echinites velut laminis quinangularibus distinctus (P) (Stonorhouse; Chalk) Cidaris.

XXVI. Echinites e lapide selenite quinis radiis e duplici serie (Newton Grange; Oolite) Echinobrissus.

XXVII. Echinites praeter quinus strias annulis exiguus innumeris insignitus (P) (Tangley, Burford; Oolite) Clypeus

XXVIII. Echinites praeter radios et annulis duplicatis insignitus (P) (Aston Rowant; Chalk) Micraster.

XXIX. Echinites radiorum puntis versus marginem annulis ovalibus inclusis (P) (Brightwell; Chalk) Echinocorys vulgaris.

XXX. Echinites punctis prominentibus (P)(Pyrton; Chalk) Echinocorys scutata.

XXXI. Belemnites niger maximus basi forata (Bugthorpe; Lias) Acrocoelites.

XXXII. Belemnites minimus fere cuiusdam succini instar et pellucidus et coloratis (Yorkshire and Lincolnshire wolds; Chalk) Belemnites listeri.

XXXIII. Conchites major rugosus ad figuram triquetram accedens (Scarborough; Jurassic) Exogyra.

XXIV. Conchites sublividus ex altera parte velut mucronatus ex altera subrotundus e rupibus aluminosus (Whitby; Jurassic) ?

XXV. Conchites leviter rugosus depressior figura quodammodo musculorum e fluviatilibus (Halifax; Bentley; Coal Measures) Carbonicola.

XXXVI. Conchites albidus oblongus et angustus (Hemsley; Oolite) Gryphaea.

XXXVII. Conchites maximus margine lato et ad alteram ejus partem sinu ample donatus (Bugthorpe; Corallian) Plagiostoma.

XXXVIII. Conchites rugosus ad figuram quodammodo musculi marini e silice proprie sic dicto (Bugthorpe; Lias) Liostra or Isognomon.

XXXIX. Conchites mytiloides (P) (Cleydon; Lias) Modiola.

XL. Bucarditis ex albido flavescens laevis (P) (Heddington; Oolite) Cardium.

XLI. Bucarditis costis donatus (P) (Whitney; Lower Oolite) Pholadomya.

XLII. Bucarditis reticulatus (P) (Shetford; Lower Oolite) Homomya.

XLIII. Ostracites maximus rugosus et asper (Hinderskelfe, Huntingdon; Chalk) Ostrea.

XLIV. Ostracites minor cardine angustiore et ad imum paulo minus latus quam ipsa parte media. (Bugthorpe; Lias) Liostrea.

XLV. Conchites anomius rugosus rostro subteriti et insigniter adunco donatus (Hinderskelfe, Lincolnshire; Lias) Gryphea.

XLVI. Conchites anomius rostro prominule et veluti pertuso donatus (Grantham; Lias) Terebratulus.

XLVII. Conchites anomius tenuis et compressior latusculo et rotundo ambitu (Broughton; Carboniferous limestone) Pterinopecten (Clitheroe).

XLVIII. Pectinites rarioribus striis (Hinderskelfe, Pickering; Oolite) Pecten vagans.

XLIX. Pectinites membranaceous dense striatus e pyrit in lapide nigro fisilli (Halifax; Lower Coal Measures) Dunbarella.

L. Pectinites minor striis capillaribus donatus e saxo calcario plumbifero (Stocks; Carboniferous limestone) Chonetes.

LI. Pectinites striis duplicibus et tenuissimus et densissimis et aeque profundus insculptis (P) (Heddington; Corallian) Pecten leus.

LII. Pectunculites densissimis et minus profunde insculptis striis Listriis donatus (Bugthorpe; Lias) Pecten cretosus.

LIII. Pectunculites cinereus striis fere ad alteram e vertice partem inclinatus (Whitwell; Oolite) Lima.

LIV. Pectunculites albidus striis admodum distinctis et elatis ex utraque parte gibbus (Royston; Upper Cretaceous) Spondylus.

LV. Pectunculites subsphaericus e saxo calcis plumbifero (Keighley, Craven; Millstone Grit, Carboniferous L) Productus ~~Limestone~~.

LVI. Pectunculites anomius cui insignis quaedam lacuna per medium dorsum recta procedit (Stocks, Scarborough; Carboniferous Limestone, Oolite). Tetrarhyncia.

LVII. Pectunculites anomia trilobus (Grantham, Speeton; Lias, Chalk ~~Gault~~) Rhynconella.

LVIII. Pectunculites striis latiusculis undatis (P) (Great Rowlright; Gault) Inoceramus?

LIX Pectunculites striis densis et minutis transversis circularibus (P) (Heddington; Oolite) Ceromya.

Species marked (P) were named by Lister from descriptions in Plot's Natural History of Oxfordshire, as they had not been seen by him. Plot had not named his species.

Apart form Plot's Oxfordshire records, there are several fossil localities cited from the Carboniferous limestone area of Craven, from the Coal Measures of the West Riding and Lancashire, and from the chalk of Cambridgeshire and Norfolk. The rest of Lister's records are from a strip of country in Lincolnshire and eastern Yorkshire covered by the Triassic and Jurassic formations. In this area, a distance of only a few miles will cover the Rhaetic, Liassic, Oolitic and Corallian beds, and as Lister gives only approximate geographical locations, it is not usually possible to be very sure of the geological formation from which any particular fossil was derived.

(7) Lister's ideas on the origin of formed stones.

Although in 1671, Lister wrote that he had formed his theory of the origin of shell-stones 'some years ago', his attitude to the question was not entirely rigid. He distinguished between deposits of unchanged sea-shells in inland regions not too far away from the sea, and the shell-stones found in inland rocks and consisting of the same material as their matrix. He saw no reason to doubt the animal origins of the former. Even in the case of stony fossils, he occasionally had doubts as to his ability to decide the problems of their origin:

> 'What light may be hence had, I leave to more judidious persons, acknowledging myself at present not to be able to demonstrate (if they are not Stones of their own kind) what they have been before petrifaction'. [1]

Lister made no attempt to conceal observations which might favour the idea of the organic origin of these fossils; of one species, titulus XXXIII, he wrote 'The figure of this species clearly shows the attachment scar of an adductor muscle' and this was illustrated on the plate of this species. He often points out details of wear and tear on the stones, and, as with titulus XLIII, the presence of worm tubes. He was also prepared to accept the petrification of some kinds of organic matter, even including shells. Nevertheless, he never accepted the idea of an animal origin for shell-stones; '... these Cockle-like stones ever

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1. Phil. Trans. 10 (1675) p. 275; HAA pp. 220-21.

were, as they are at present, Lapides sui generis, and never any part of an animal'. [1]

On the other hand, he never at any time considered semi-occult plastic forces, nor the whims and sports of playful nature. For him, shell stones, like the growth of crystals and spars, were part of the general principles of geochemistry.

Throughout his life, Lister was interested in crystals and salts. He published papers on the growth of alum crystals, and on spar; on salt springs, the distillation of salt waters, and the formation of crystals of salts of several kinds; on the properties of pyrites and their being a cause of earthquakes; on the freezing of different salt waters and the crystals so produced; and on vitriol crystals. [2] He gave a long discourse to the Royal Society on the growth of vitriol crystals, disputing Dr. Vincent's statement that crystals grow by taking nitre from the air; Lister said that they 'feed as plants did, which grow upon the earth'. [3]

His book on medical mineral waters was in two parts, the first being geological and chemical, the second physiological and medical. The book treats salts and their crystals in considerable detail; Lister recognizes five types: vitriolum e pyrite, alumen, sal commune, nitrum or salpetra, and nitrum murale or calcarium. He describes the structure of their crystals, with a well-drawn plate, and gives their distribution and properties. There are chapters on iron ores and limestones, which to Lister represent the two basic types of stony material. He speculates on the growth of these two materials, and attributes their growth to the action of lapidifying juices. This action produces the heat responsible for hot springs.

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1. Phil. Trans. 6 (1671) p. 2282.
2. Phil. Trans 9 (1674) p. 221; 14 (1684) pp. 489, 512-7; 15 (1685) p. 836; 21 (1699) p. 331.
3. Birch 4 pp. 275-7.

Lister looked on formed stones as being the products of the same kinds of action. He directly compared the growth of trochites with the shooting of antimony and with spar, and specifically referred to shell-stones as 'crystals'.[1]

This does not mean that Lister regarded mollusc shells and fossil shellstones as completely unrelated. There was at this period an enormous interest in the growth of stones in the human body, particularly in the kidney and bladder, and Lister contributed papers to the Philosophical Transactions on this subject.[2] He referred to the growth of stones in animal and human bodies as 'shooting', and thought it to be almost instantaneous. He pointed out that a broken intestinal stone in the repository of the Royal Society showed lines radiating from the centre to the circumference; this he took to show that the stone had not been built up slowly by deposition in concentric layers, but had rapidly crystalised out from a central point.[3] This attitude towards the generation of stones in the body is not to be taken as an indication that Lister thought of the action as vitalistic, and that the growth of stones in the soil and rocks was an expression of a vegetative faculty in the mineral world. Lister took the opposite view: that these phenomena proved the existence of 'inorganic' processes at work in the bodies of animals. These stony growths were thought to be caused by the action of lapidifying juices (succi lapidescentes). Such juices were a standard part of sixteenth and seventeenth century mineralogy. Lister used them in this way and also in physiology. He recognised two of these juices, one vitriolic, associated with pyrites, and one calcareous; these correspond with the two basic kinds of stones.[4] Both are responsible for the production

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1. Phil. Trans 8 (1673) pp. 6186-7; HAA p. 200.

2. Phil. Trans. 7 (1672) p. 4062; 15 (1685) p. 882

3. Birch 4 p. 371.

4. HAA p. 201.

of stone within the body, and Lister claimed to be able to distinguish the two types externally by the use of a loadstone. These juices were primarily responsible for the production of the hard parts of the body, and their production of stone was a perversion of their normal function.[1] Lister quoted in support the opinion of Dr. Slare, who, in a paper in the Philosophical Transactions, had also suggested a link between the bones and stone.[2]

There were sometimes claims at meetings of the Royal Society that these stones were at times in the form of shells. For example, in 1663 Charleton mentioned the 'fact, known to many persons', that a Frenchman, M. La Verdure, having eaten nothing but cheese while at sea for several days, had voided a 'great many cockle shells from his bowels'; and Lister described a turbinated stone found by Dr. Pearse in the kidney of a woman.[3]

Lister included normal shells among the products of these petrifying juices, and it is clear that he thought of the shooting or crystallization of the shell as independent of the animal's vital activity, once the juice had been secreted. He carried out an experiment in which he extracted the body juices of snails and applied it to the surface of a shell, inside and out. He found no change in measurements or weight, but remained convinced of his idea, dismissing his failure by saying, rather inconsequentially, that it was because he had used a land snail.[4] These same juices were thought of as pervading the earth, and were important in Lister's outlook on the origin of mineral waters. They are clearly thought of as non-vital.

The Kentish physician Griff Hatley also believed that shellstones could be formed from salts responsible for the normal growth of the shells of animals, but in his hypothesis the fossil forms grew from salts washed

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1. Birch 4 pp. 243-4.

2. Phil. Trans. 14 (1684) p. 532.

3. Birch 1 p. 251: 4 p. 335; Phil. Trans. 15 (1685) pp. 1018-19.

4. HAA p. 201.

down into the earth from the bodies of animals; unlike Lister, therefore, Hutley thought that animals were essential for shell-stone formation. [1]

Lister's position can be summarized as follows. There exist certain kinds of inorganic, mineral juices, of a calcareous or pyritic (vitriolic) nature, found throughout the earth and its rocks. Under certain conditions, they will shoot or crystallize out into stony bodies of varying degrees of complexity, including stalagmites, ores and spars, crystals, and objects closely resembling in form, though not always in material, structures produced by animals, such as teeth, bones and shells. These juices are also to be found in the bodies of animals, in which they function in the same way, and produce very similar objects which are of use to the animal, though occasionally their normal function is disturbed, and they then produce hard bodies of other types, such as crystals, stones, or shells, in abnormal parts of the body. A shell- or tooth-like object can therefore be produced by these inorganic forces either in association with an animal's body or quite separately. If such an object is found in rock, it is likely to have grown *in situ*, though it is possible in some cases for it to have grown round or in an animal's body and been buried later. Far from shell-stones being seen as a demonstration that semi-organic, plastic forces are found in the mineral world, Lister believed that the hard parts of animals showed the existence of mineral, inorganic processes in the bodies of animals; in this way he provides an early example of chemical reductionism.

(8) Arguments used in the fossil controversy.

Both opinions in the controversy used arguments from the form of fossils, their material and their position, and sometimes claimed to have detected them in the process of formation.

The shapes of these stones was, of course, the cause of the whole

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1. *Phil. Trans. 14* (1684) pp. 463-5.

dispute. Fossils such as belemnites, which bear no resemblence to any extant living thing, aroused no controversy. It was the fossil plants, shark-teeth, bones, and above all, the shell-stones which, because of their similarity to present-day forms, became the subject of argument. Until the seventeenth century, the appearance of shell-stones had not been subjected to close scrutiny; Fabio Colonna was the first author to look at them with any care. He was impressed in particular with the internal structure of glossopetra, which he found to be complex, and not homogenous, as a mineral body would be expected to be. [1]

All the proponents of the organic theory took as their point the very close resemblance between living and fossil shells, Steno saying they are as alike as two eggs. [2] Some, such as Ray, accepted this without too close scrutiny. Hooke had a similar attitude, apart from his detailed study of ammonites, which are so peculiar that they cannot be confused with extant species. Even the very critical Lhwyd found this resemblence impressive. In particular, he noted that the stones showed considerable minor variation, but were almost always recognisably shell-like. [3] This regularity was accompanied, as Ray pointed out, by great complexity of form, including curves not found in any crystals, and the variety of form is so great that it was difficult to imagine a sufficient number of kinds of salts. Ray also stressed the unlikelihood of crystals growing together in pairs to form a hinged joint, such as we find in bivalves. [4] These fossils often show signs of having been in the sea, being broken or worn, and may carry worm-tubes or pearls; Scilla claimed to have found a fossil crab with a mollusc

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1. According to Ray 1692 pp. 111-13.
2. Steno 1669 in Winter 1930 p. 254.
3. CJR p. 226.
4. Ray 1692 p. 115; CJR p. 154.

grasped in its claw, and Woodward thought he could distinguish features in fossil plants which allowed him to date the flood responsible for their fossilization as having occurred in the spring of the year.[1] Hooke and Steno pointed out that internally the shell-stones show no structure, unlike crystals, which retain an ordered structure throughout, and cleave accordingly. Scilla repeated Colonna's argument that the internal structure of glossopetra was the same as that of a shark's tooth. Woodward and Steno claimed that the close fit between the shell-stone and its matrix showed the latter to have been liquid when the fossil was formed. However, this does not necessarily favour the organic view, and Ray was giving only negative arguments when he asked why, if shell stones were crystalline, they could not be produced artificially.[2]

Each of these arguments was answered by those holding the inorganic view. There was no reason to refute the occurrence of real shells inland; this was admitted by both Lister and Plot. Plot, for example, explained the origin of a large bed of oysters at Reading by pointing out that Ethelred and Alfred besieged the Danes in that town. The Danish ships would be able to reach Reading by the Thames and bring in supplies of oysters from the Thames estuary as food. Lister accepted inland oyster shells in England and

> 'particularly along the shores of the Mediterranean Sea, there may all manner of Sea shells be found promiscuously included in Rock or Earth, and at good distances too from the Sea'.[3]

He also accepted as true sharks' teeth some non-petrified objects

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1. Woodward 1695 p. 280; Steno 1669 in Winter 1930 p. 255; Hooke 1704 p. 318; Lhwyd in MSS Ashmolean 1820a f. 28; Phil. Trans.9 (1674) p. 193; Woodward 1695 p. 82.

2. Ray 1692 p. 116.

3. Plot 1677 pp. 119-20; HAA p. 200; Phil. Trans. 6 (1671) p. 2282.

from the Isle of Sheppey; but he contrasted these with a glossopetrum made of stone from Yorkshire. [1] These and the fossil shells made from the same material as the rock in which they are found, are quite another matter. The material is the important point; hence, the argument that the great variety of fossil forms makes an inorganic origin unlikely loses its point. As Beaumont pointed out, snowflakes show almost unlimited variation, but are all made from the same material. Lister reversed the point completely; as the variety of, for example, fossil Ombriae (echinoids) is much greater than the number of extant species, this is an argument for their not being organic, and the large number of individuals found together shows that their occurrence is not fortuitous, but caused by definite conditions found in that same place. The complexity of fossil forms in comparison with crystals was not dealt with by Lister; Plot pointed out that a combination of two crystalline materials shooting across one another at an angle would produce a flat spiral, as two streams of water produce a turbo. He claimed that many fossils are in fact regular, astroites showing rays at intervals of 72°. Lister stressed the symmetry of formed stones, which is sometimes paralleled in what were then undoubtedly mineral productions, such as belemnites. [2]

Formed stones which have apparently been broken before enclosure were not necessarily proof of a previous independent existence; Lhwyd believed, correctly, that a fossil echinoid discovered by him, with a marked indentation at the rear, was naturally so. Beaumont saw the numerous 'broken and imperfect pieces' of rock-plant as 'only little essays of Nature towards the production of this Stone'. Lister noted carefully whether his specimens were fragmentary or entire, sometimes

1. Phil. Trans. 9 (1675) pp. 222-3.

2. Phil. Trans. 11 (1676) p. 734; 15 (1685) pp. 79-80; MSS Lister 5 f. 119; Plot 1677 pp. 122-3; Phil. Trans. 15 (1685) p. 779.

admitting, as with titulus XXXVIII, that he has never found a complete example of a particular species. He stressed that no conclusion can be drawn from these broken shell-stones, as small stones, pebbles, and crystals can all be found in the same state and embedded in rock. He would no doubt have given the same explanation for these undoubted mineral productions as he did for the shell-stones; incomplete stones are caused either 'by the restricted (angustiam) position or shortage of material'. 'Thus', concluded Lister confidently, 'with any kind of imperfect shell, there is nothing obscure, certainly not for anyone with experience'.[1] Other points made by Fabio Colonna and Steno - the hollowness of some fossils, the covering of other mineral matter found on some specimens, the way in which they appear to have been layed down in a liquid medium - are similarly met by Lister with the argument that the same is true of some bodies which are indisputably mineral. Lister noted that internally, several species show a smooth fracture, like flint. Belemnites are an exception to this; he described the concentric layers of material, traversed by radial streaks, which give these objects such a mineral-like appearance.[2]

One of the strongest arguments in favour of an organic origin for fossils is that presented by bivalve shell-stones, as Ray pointed out, claiming that such apparently purposive structures must have been functional as living animals at one time. Lister appears to admit that this is a great difficulty for him, by writing that he could demonstrate that all shell-stones, 'even Bivalve-shells' were not of animal origin. Plot had claimed that some types were found only as the flat, others only as the gibbous, valve. Lister was careful to note whether one or both valves were present in his specimens, and if only one, he said which.

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1. Lhwyd in MSS Lister 35 f. 54; Phil. Trans. 11 (1676) p.730; MSS Lister 5 f. 118; HAA p. 200.

2. MSS Lister 5 ff. 118-19; Phil. Trans. 8 (1673) p. 6182; 9 (1674) p. 224; HAA p. 227.

As he assumed that where he has found only one, the other will be similar to that of similar species, as with titulus XLIII, he appears not to have accepted Plot's argument. He did claim that he had found one bivalve shell in which the hinge was so constructed that it could never have opened, so that the argument could not be used in all cases. [1]

Hooke and Ray had pointed out the restricted range of animal parts found as fossils. Only the hard parts of the body, those which would be expected to survive best, are found. Lister's theory of fossil development would explain this; but nevertheless Hooke asked why, if Nature imitates animal and plant forms, do we not find pseudo-roses in the rocks? And, conversely, why does no one suggest that hoards of coins and other man-made objects found hidden in the ground are similarly not genuine? (In fact, it had been suggested, as by Paraclesus, that some apparently human artefacts had grown in the soil.) Ray claimed that only marine animals were found as fossils; why should not land animals be imitated? This is surprising, as Plot had devoted a good deal of space to the problem of 'elephant' bones and teeth found in England, and Ray quoted extensively from him in other places. [2]

In addition to refuting many of these arguments from the form of fossils, Lister and those thinking like him put forward positive points of this kind in favour of their own view. There are some stones having a definite form, such as selenites (crystals of borax) and belemnites, and which are admitted by everyone to be mineral; why, asked Plot, could the same not be true of all formed stone? It does become difficult to draw a rigid line between two classes of formed stones, the mineral and the formerly organic. Beaumont stressed the

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1. Phil. Trans. 9 (1674) p. 99; Plot 1677 p. 116; MSS Lister 5 f. 118.

2. Hooke 1704 pp. 318-19; Paracelsus 1658 p. 502; Ray 1692 p. 114; Plot 1677 pp. 131-5.

complexity of snowflakes, and the similarity of frost and dendritic crystalline growths to plant leaves was well known. Even Hooke was impressed by this, and Lhwyd must have been asking a very common question when, in describing the shooting of a piece of silver ore, he asked why such 'fossils' should not naturally shoot into leaf-like and shell-like forms.[1] Lister did not use such arguments; his writings show no sign of any interest in establishing such continuity between the mineral and the animate worlds. He was interested in efficient and material causes, not in formal affinities.

The most telling arguments against the organic theory were those concerning the species of animals and plants represented as fossils. In a non-evolutionary climate of opinion, these points could not be answered satisfactorily. Very many modern forms are not represented as fossils. Plot stressed this: he lists the

> 'bones of Whales, Sea-horses, and the bones of all the squammeous kind; the great shells of the Buccina, Murices, Conchae Veneris and Solenes; the sword of the Xiphias or Sword-fish, and almost all the crustaceous kind, such as Crabs, Congers, Lobsters &c which last having locomotion, I should much rather expected to have found petrified on the tops of mountains, than any of the Testaceous kind, and yet of these we meet the fewest of any'.[2]

To this, Ray could only reply that many of these animals are not found living in England, and so could not be fossilised here; perhaps the others are not so durable as those we do find in the rocks. Woodward also thought that lobsters and crabs have decayed, in this case because they were left, after the flood, on the surface of the land, and so rotted, while denser shells were buried and so preserved. Neither

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1. Plot 1677 p. 115; <u>Phil..Trans. 11</u> (1676) pp. 731-4; Hooke 1665 pp. 88-92; CJR p. 291.

2. Plot 1677 p. 114.

argument is fully convincing. [1]

The point stressed most by Lister and the other collectors was that, as close inspection shows fossil shell-stones not to be identical with any extant species, the fossils cannot be the remains of any such animal. This had been admitted by Fabio Colonna, [2] and Ray and Hooke both recognised the point. Because of this Ray had doubts over fossil plants, in which he was well able to judge: Hooke was able to avoid any difficulty by accepting a degree of organic evolution. In general, however, not being collectors and cataloguers, neither was as impressed by this argument as were Lister, Plot, Cole, Beaumont, and Lhwyd, whose activity in this field brought them continually face to face with the distinctiveness of fossil forms, both as to species:

> ' .. the real shell-fish .. called Conchylia striata, though thus lineated without, are always, saith Aldrovandus, plain and smooth within, contrary to what we find in these Conchites striata ... which shows the inside .. not only lineated from the commissure to the rim, but adorned also with four or five transverse fillets, not made of one, but of several conjoyned lines, which seems also to conclude it to be Lapis sui generis, and not to have been moulded by a striated Cockel-shell;' [3]

and also as to larger groups:

> 'It must be granted we have a great many formed stones that resemble noe shells at all; such are v:g: Cornu Hammonis, Belemnites, Asteriscus, Entrochus, Dentes Lamiarum Centronites, Crystal &c...' [4]

Lister, whose systematic works formed the standard texts on the

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1. Ray 1692 p. 127; Woodward 1695 pp. 32-3, 87.
2. See Plot 1677 p. 114.
3. Ibid. pp. 104-5.
4. Lhwyd in Gunther 1939 p. 263.

subject, paid the strictest attention to detail in his descriptions; though he admits a resemblance between fossil and extant shells so close that the 'inexperienced and undiligent' may be misled, nevertheless he claims that 'all shells have shapes different from all shell-stones', and that 'such animals as they clearly represent are not in nature generated.' [1]

It would seem that the collectors were conscious that their critics were speaking with a lack of first-hand knowledge. Cole, in a letter to Plot on the structure of ammonites, wrote that

> 'These Phaenomena ... will puzzle Mr. H. and the rest of those ingenious Gentlemen who will have them to be petrified shells. I know notwithstanding, they will have some fine-spun notions to solve them'. [2]

Beaumont wrote of the organic view that 'it seems not to be grounded on practical knowledge'; and Lhwyd admitted that on a superficial view fossils resembled living forms, yet a close look showed them to be only'mock-shells and counterfeit teeth' [3] Lister had a similar view of the opposition, which he described as

> 'those persons, that think it not worth the while exactly and minutely to distinguish the several species of the things of Nature, but are content to acquiesce in figure, resemblance, kind, and such general notions; but when they shall please to condescend to heedful and accurate descriptions, they will, I doubt not, be of that opinion, which an attentive view of these things led me into some years ago'. [4]

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1. HAA p. 199.
2. Gunther 1939 p. 263.
3. Phil. Trans. 11 (1676) p. 737; CJR p. 226.
4. Phil. Trans. 6 (1671) p. 2283.

The naturalists sharing Lister's opinion appear to have considered themselves as cautious, dependable workers, in contrast to the extravagant speculators who were prepared to ignore the factual details and to suggest sweeping biological or geological changes on the basis of a superficial interpretation of shell-stones. Lister was able to conclude a dispute with Hooke at a meeting of the Royal Society by using his detailed knowledge of the structure of living and fossil shells. When Hooke referred to some specimens as 'petrified oysters', Lister was able to point out that they 'had no striae on the outside going from the valve to the rim', as had both European species of oyster. [1] Hooke appears to have had no answer to this at the time, though he had already made suggestions about possible changes in animal forms in the Micrographia.

There was little that could be said in reply to these arguments. Few were prepared to accept the idea of extinction of whole species, or Hooke's suggestion of transformation. The only other argument was that fossil forms still survive in the ocean depths or in remote areas. [2] It was, of course, impossible actually to disprove such negative arguments, though Lister pointed out that he had found fossil shells in submarine rocks (presumably in north-east Yorkshire); if the fossils were formed by marine animals, these should still be found locally, as the sea had not retreated from the animals' original habitat. [3] This argument could only be used by someone like Lister and most of his contemporaries, who had no real idea of geological change.

The material from which fossil shells are made did not provide clear-cut evidence in favour of either view, both sides trying to make use of it. Hooke pointed out that shell stones differed from ordinary stones only in their shape. This was constant for any one type, but

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1. Birch 4 pp. 237-8.

2. Eg., Ray 1692 p. 120; Woodward 1695 pp. 27-9.

3. HAA pp. 198-200.

the material varied, sometimes even between different parts of the same specimen; he claimed to have found one formed stone made up of clay, stone and marcasite. It was difficult to see how a mineral production could vary so much in everything but shape. Plot tried to reverse this argument; if shell-stones varied so much in material, this is proof that they were not laid down at the same time; that is, at the flood They must have grown in the ground at different periods. [1]

Woodward placed a great deal of emphasis on the physical properties of the material of fossils: in paricular, their specific gravity. He claimed to have demonstrated that the specific gravity of the shell-stone was proportional to the depth at which it was found in the rock; this showed that the densest animal remains sank furthest through the mud formed by the solution of the earth's crust in the waters of Noah's flood. He did not differentiate between the specific gravities of the original shell and the stony fossil produced from it - he apparently assumed they were the same. [2]

Lister paid much attention to the chemical composition of his specimens. He noted the effects of burning, friction, vinegar and nitric acid on them, and sometimes attempted to fit them into his basic calcareous and pyritic divisions of mineral matter. Although at one point he wrote that all fossils are formed from the pure and unadulterated (mera) material by which they are surrounded, elsewhere he admitted that the two often differ. He pointed out, however, that this is true of many other kinds of mineral object, it being the nature of one stone to cover another, as rag (ie. grit) covers limestone in the mountains of Craven.

In his first paper on fossils, Lister stressed the link between the type of stone in which the fossil is found on the one hand, and both the

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1. Hooke 1665 p. 110, 1704 pp. 288-91; Plot 1677 pp. 109-10.
2. Woodward 1695 pp. 32-3, 87.

material and the form of the fossil on the other:

> 'our English Quarry-shells ... have no Parts of a different Texture from the rock or quarry they are taken, that is, there is no such thing as shell in these resemblances of shells, but that all Iron-stone cockles are all Iron-stone; Lime or marble all Lime-stone and Marble; Sparre or Chrystalline-shells all Sparre &c ... Quarries of different stone yield us quite different sorts or species of shells, not only one from another ......but, I dare boldly say, from any thing in Nature besides....' [1]

His argument here seems to be that as both the form and material of fossil shells are correlated with the nature of the rock in which they are found, their origin must be found in forces responsible for rock-formation. The 'shells' are therefore mineral, not animal, productions. Unfortunately, he did not here make it clear whether he had at this time found any one species of fossil shell in more than one quarry of the same kind of stone; this would greatly have reinforced his thesis. Later, he was to trace the distribution of one kind of belemnite for a hundred miles through the chalk of eastern England, [2] but he did not use this argument from correlation after 1671.

The position in which fossil shells are found raised problems as fundamental as their structure. If shell-stones are of animal origin, they must be seen as evidence of drastic geological change in the earth's surface at some time in the past, the Noachian flood being the most popular agent. However, when carefully considered, the distribution of the fossils presents difficulties for this explanation, and, as few people considered any other kind of major geological change, these

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1. Phil. Trans. 6 (1671) p. 2282.

2. HAA p. 228.

difficulties were used as arguments for the mineral nature of all fossils. The finding of fossil shell-stone inland, especially in the mountains, above the level at which alluvial deposits might have filled in arms of the sea, was the evidence most often put forward for the flood; Palissy's suggestion of the drying-up of inland lakes was not normally accepted, though the human action also suggested by him was used by Plot, as described above.

A major difficulty for the idea of the flood as a dispersive agent for sea shells was that, according to Genesis, the flood was caused by prolonged rain. This would bring things down from the mountains, not carry them up, and the resulting slow rise in sea level would not have enough momentum to carry shells upward with it, especially in the case of those shells, such as oysters, which grow attached to the rock. If violent tidal waves are assumed, we should find the shells scattered and mixed, but in fact they are found in beds composed mainly of one species, as in life. [1] These beds are often not even approximately horizontal, as would be expected if they were relics of the flood, and the fossils are sometimes found in the roofs of caves, where they could hardly have been deposited in this way. [2]

That these beds should be enclosed in solid rock is also surprising; even if the rock is thought to be petrified clay, it is not clear why the shells should penetrate the clay, and to different distances. With plant remains the problem is greater; far from forcing their way through thick clay, they would be expected to float. Woodward's theory of the dissolving of the rocks of the earth's crust (but not the shells) explains some of the difficulty, but not that of the plant leaves, nor Lhwyd's point that it could not account for some rocks containing large numbers of fossil shells and others none at all. [3]

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1. Leonardo da Vinci in Richter 1952 pp. 27, 30; Palissy 1580 in Cap pp. 272-3; Plot 1677 p. 112; Ray 1692 pp. 130-1.

2. Plot 1677 p. 113; Lhwyd in MSS <u>Ashmolean</u> <u>1820a</u> f. 28.

3. Gunther 1939 pp. 383, 338.

Direct observation of the formation of fossil shells from either source would have settled the matter; there were occasional claims from both sides to have done this.

Plot described the reputed growth of cockle stones at Glympton in Oxfordshire, and describes his own observations of what he thought were partly-grown shell-stones. Lhwyd thought that variation in size of any one species of shell-stone showed their growth in the rock, though on the other hand he reported to Lister, with disbelief, an account of cockles and other seashells being generated in the water of the river Mole; this story originated from the landlord of the King's Head at Epsom. [1] Naturally enough, however, no convincing direct evidence could be put forward for the mineral origin of fossil shells, and Lister never made any claims in this direction. Hooke, on the other hand, described the finding on the south coast of England, of sea shells, buried, and in varying stages of petrification, and Boccone presented to the Royal Society a series of specimens claimed to show several stages in the process of petrification. [2]

In the absence of such conclusive direct evidence, the argument was also conducted by the common seventeenth century method of analogy, between observable inorganic activity and unobservable shellstone formation. Plot noted that selenites would grow while embedded in solid matter, as they are often found with particles of clay inside them, and stalagmites and the one-sided growth of certain salts provided comparisons. Beaumont and Lhwyd similarly compared fossil animals and plants with silver and other ores, and Lister directly compared the rays to be seen in trochites with the shooting of antimony. He also made detailed observations on the shooting of lumps of crude alum and marcasite, investigating the size, shape, and general appearance of the strands

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1. Plot 1677 p. 102; MSS Ashmolean 1820a f. 28; MSS Lister 36 f. xiii.
2. Hooke 1704 p. 292; Birch 4 p. 116.

produced, their solubility, taste, reactions to gall and to sunlight and damp air; the two substances were related to his theory of two stony matters and the shooting of shell-stones from their juices. [1]

The other side could also argue by analogy, Steno saying that objects with the same appearance must have the same origin. [2]

(9) Petrification.

Petrification had aroused interest since the earliest times, as it appears to be a conversion of one material to another. Avicenna attributed the change to a power, the vis lapidificativa, and Palissy suggested a material, the sal generatif et congelatif; this is attracted by a vertu salsatif in the shell, which is thereby hardened. [3]

By the second half of the seventeenth century, petrification was looked on as a penetration by mineral matter of the interstices of the material being petrified, which is itself unchanged, though it may later be removed. This fits in well with a corpuscular philosophy; Boyle wrote:

> ' ... among the Kinds of those Liquors, I have observed a sort that is of so fine a substance, and yet of so Petrifying a Virtue, that it will penetrate and petrifie Bodies of very differing Kindes, and yet scarce, if at all, visibly increase their bulk ... ' [4]

He described a cream cheese so perfectly petrified that even the mould was preserved, and stressed that

> 'Petrifick Agents may insinuate themselves into the pores of various Bodies, and turn them into Stone, without otherwise destroying their pristine Nature, or so much as

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1. Plot 1677 p. 84; CJR p. 291; Phil. Trans. 11 (1676) p. 734; 8 (1673) p. 6182; 9 (1674) p. 228.

2. Steno 1669 in Winter 1930 p. 225.

3. See Albertus Magnus translated Wyckoff op. cit. p. 52; Palissy 1580 in Cap pp. 219, 275.

4. Robert Boyle, An Essay about the Origin and Virtues of Gems (London, 1672) p. 124.

their former figure'.[1]

This interest in petrification was part of a wider interest in the penetration of one substance by another, examples of which were given in support of the corpuscular hypothesis. Steno and Hooke both used this penetrative petrification to explain the changes which have occurred in fossil shells. Steno wrote of juices or exhalations which seeped through cracks in the earth caused by movement of strata, dissolving mineral matter in doing so. They were able to penetrate the interstices of animal shells after material had been moved from these by a subtle matter. The juices then deposited their mineral matter, either because of cooling, evaporation or reaction between different juices which Steno compared to reactions between acids and salts. He stressed the porous, fibrous nature of the shells which made this process possible.[2] Hooke wrote of similar volatile exhalations, coming from subterranean explosions or volcanoes. These caused congelation of shells by crystallization of mineral matter in the pores of the shell. Alternatively, bituminous matter could be supposed to bind sandy matter, moulded by the shell, into hard stone. Even apparently pure water could produce petrification in this way, and Hooke thought that river sand was a precipitate from these petrifying waters; he described the waters of the High Peak in Derbyshire as an example. Hooke thought that petrification was a sign of the old age of the earth, in contrast to the spirituousness and inflammability of youth.[3] Hooke and Boyle both paid attention to petrified wood, which was often mentioned at meetings of the Royal Society.

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1. Ibid. p. 125.

2. Steno, Elementorum myologiae specimen ... cui accedunt canis carchariae dissectum caput (Florence, 1667); translated by A. Garboe as The earliest geological treatise (1667) by Nicolaus Steno (London, 1958) pp. 21-3, 27, 29, 31, 41; Steno 1669 in Winter 1930 pp. 234-250.

3. Hooke 1704 pp. 290-4, 427.

Lister made no attempt to deny petrification as such, and showed active interest in the phenomenon. Indeed, when the Royal Society of Dublin suggested that subterranean trees found in Irish bogs were arbores sui generis, Lister did not take this as support for his theory of fossils; he commented that he had often found them on Pinna-moor in Craven (Pinnow Hill, just above his wife's home at Carleton Hall), that they were almost always found upright, and that close inspection showed them to be birch trees. He investigated several kinds of fossil wood, claiming he had found a piece of petrified ash which was magnetic. The Lough Neagh fossil wood was attracting attention at this time; Lister claimed he could distinguish two types, a limestone petrification of holly, and an ironstone ash - again agreeing with his idea of two lapidifying juices, but in this case involving their action on other bodies, and not simply their crystallization. [1]

Lister even showed a petrified shell to the Royal Society, though he stressed that the petrification consisted only in the laying down of stony substance on the surface of the shell. In his systematic accounts, he often pointed out such instances of mineral matter, usually pyrites, adhering to the fossil shells, and carried out observations on petrifying waters at Knaresborough. What Lister did deny was that petrification changed the substance of the shell; and he denied that petrification in Steno's sense took place. [2] As Lister realized that Steno was describing the replacement of shell by stone, and Lister was prepared to accept the addition of stone to shell, it would seem that he could only be objecting to the removal of the shell material. He insisted that what we see as shell-stone has grown in situ by the action of the lapidifying juices, without having to have a preformed

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1. Boyle 1672 op. cit; Phil. Trans 14 (1684) pp. 552-4; 15 (1685) pp. 1108-12; Birch 4 pp. 306, 186.
2. Phil. Trans. 6 (1671) p. 2282.

mould formed by the animal. Rather contradictorily, however, he elsewhere admitted that some petrifications are not accompanied by an increase in weight. [1]

(10) The purpose of fossils.

To an age not yet free from Aristotelian teleology, a strong argument in favour of the organic origin of fossils was the apparent pointlessness of purely mineral bodies of such complex and seemingly purposive shapes. Fabio Colonna stressed that Natura non facit frustra; so why should the earth produce teeth? Hooke asked why Nature should play the mimic, and imitate what she had done in greater perfection elsewhere, in the animal and vegetable kingdoms; [2] and

> 'It has a long time been a general observation and maxime, that Nature does nothing in vain; It seems, I say, contrary to that great Wisdom of Nature, that these prettily shaped bodies should have all those curious Figures and contrivances.. generated or wrought by a Plastick virtue, for no higher end than onely to exhibite such a form....' [3]

Ray argued similarly:

> 'Now that Nature should form real Shells, without any design of covering an Animal, is indeed so contrary to that innate Prolepsis we have of the Prudence of Nature (that is of the Author of Nature) that without doing some Violence to our Faculties, we can hardly prevail with our selves to believe it: and gives great countenance to the Atheists assertion, that things were made or did exist by chance'. [4]

However, the answer to such criticism was clear enough, and Plot used it as a direct reply to a quotation from Hooke:

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1. Birch 4 p. 237.
2. In Ray 1692 p. 110; Hooke 1704 p. 289.
3. Hooke 1665 p. 112.
4. Ray 1692 p. 109.

'To which I answer, that Nature herein acts neither contrary to her own Prudence, human ratiocination, or in vain, it being the wisdom and goodness of the Supreme Nature .... that governs and directs the Natura naturata here below, to beautifie the world with these varieties; which I take to be the end of such productions as well as of most Flowers, such as Tulips, Anemones &c, of which we know as little use as of formed stones. Nay, perhaps there may proportionably, number for number, be as many of them of Medicinal or other use ... as there are of plants: so that unless we say also (which I guess no body will) that these are produced contrary to the great wisdom of Nature, we must not of Stones.' [1]

Lister appears to have had no comment to make on this speculative part of the subject.

(11) Fossils and the question of change.

Fossils are now looked on as providing evidence for change of a fundamental and far-reaching nature in the geological and biological worlds. In the second half of the seventeenth century, only the most radical of natural philosophers, such as Hooke, and, to a lesser degree, Steno, even approached such an outlook, and even they did not always see the importance of points of detail. The chief obstacle to the acceptance of the idea of change was the complete lack of any real sense of time as a dimension in which real change can happen. If the duration of the earth is thought to be in the region of 6,000 years, then the degree of change which is possible is so slight that men do not begin to think in this way, and there is no place for fundamental change, whether or not such a revision of God's creation is acceptable. The difference is one of kind, not of degree; the dynamic and formative relationships which we

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1. Plot 1677 p. 121.

see between our world and its occupants at present, and their predecessors at different times in the past, were represented at this time by a system of static, structural and non-causative relationships between geological phenomena and between living things and their contemporaries. Fossil animals and plants in this world picture must be either the result of supernatural intervention, such as special creation or the flood, or be mineral structures, or remain a mystery. There can be no satisfactory answer until the time scale is changed, and this did not occur until the second half of the eighteenth century.

Lister appears to have had vague ideas of long periods of time needed to account for erosion, as will be shown below; but he did not see the consequences of this, and was both ambiguous and inconsistent, as with his argument, quoted above, from fossils found in submarine rocks. He did not there begin to contemplate the possibility of even slight geological change, though in other cases he accepted the erosion of large masses of rock; and when he stressed the large amount of silt washed down by the Nile every year, and the slight changes in the delta since the time of Herodotus, the implication is that very long periods of time must have been needed to build up the whole delta. [1] Lister clearly had no real and properly thought out conception of time and change. Even so, his fumblings in this direction were not typical of the time. As an example of the more general outlook of his contemporaries, Steno found it necessary to put forward historical evidence to show that the earth must be at least three or four thousand years old, and that it is possible for fossil shells to survive in the soil for such long periods. If the age of the earth is thought to be comparable with that of human history, such historical evidence is permissible, so that we find Plot using the absence of any record of the flood in England as an argument against diluvial theories of fossil

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1. Phil. Trans. 14 (1684) p. 740.

origins. Hooke was more sceptical of the value of historical evidence; though accepting the flood in England, he argued that as we have no record of what the country was like before the deluge, we must rely on our reason to answer our problems. [1] Nevertheless, he compiled as much historical evidence as he could in favour of his own ideas, including a long interpretation of Ovid's Metamorphoses.

The use of Genesis as a source of historical evidence was, of course, standard until the early nineteenth century. The flood as an explanation for the distribution of fossil shells, and the reciprocal use of fossil shells as evidence for the flood, were both widely accepted in the seventeenth century and this appears to have become the orthodox attitude from the beginning of the eighteenth; for example, in the papers of De la Pryme in 1700. [2]

During the period of the fossil controversy, however, the flood was regarded by many natural philosophers as unacceptable as an explanation of fossil origins. The universality of the flood was questioned by many writers, [3] and detailed criticism of the diluvial theory of fossils had been put forward, even by those favouring the organic view, from Leonardo and Palissy onwards. Although Steno wrote that it was 'certain that the formation of many molluscs which we find today must be referred to times coincident with the universal deluge', the flood as a dispersive agent was rejected by Ray and Hooke as well as by Plot, and all for similar reasons. Only Woodward among those involved in the dispute at this period used Noah's flood as a central part of his fossil theory. [4]

Although Lister was prepared to quote Biblical evidence in support of his ideas, as on the circulation of water in nature, he ignored biblical accounts of the flood; he did not need such evidence

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1. Steno 1669 in Winter 1930 pp. 266, 259; Hooke 1704 pp. 310, 319.
2. Phil. Trans. 22 (1700) pp. 677-87.
3. E. Stillingfleet, Origines sacrae, or, a rational account of the grounds of the Christian faith (London, 1680).
4. (over)

for his theory:

> 'I am well aware, that the finding of Cockles or Shells, as most writers are pleased to call them, upon Mountains, and sand also there, is by the same Harodotus used as an Argument of a great Deluge, or inundation of water; but as I have elsewhere I think demonstrated, that the Rock-Cochlites are no Shells, so neither can I grant that the Sand was adventitious to the Mountains...' [1]

(12) Geological change.

If fossils are thought to be organic, and the flood is rejected as an explanation, a great deal of geological change must be accepted; this is a problem if the earth is thought to be only a few thousand years old. If a mineral origin is accepted, the difficulty is avoided, and Woodward claimed that it was in fact this difficulty which

> 'laid out on all Hands for some new Expedient to solve and put an End to the Perplexity. And 'twas this last Effort that brought forth the opinion, that these Bodies are not what they seem to be: that they are no Shells, but... only Semblances or Imitations of Shells.' [2]

The question of fossil origins and geological change are inseparable.

Palissy was prepared to accept minor changes in the earth's surface - the drying up of inland lakes, the silting up of arms of the sea and estuaries: this he based entirely on fossil evidence. Steno suggested much more extensive changes, comparable in many ways with modern ideas, except for his restricted time scale. Fossils played an essential part in his theory of the origin of rock strata, as the title of his book shows, and he wrote that his ideas were first stimulated by

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4. Steno 1669 in Winter 1930 p. 258; Plot 1677 p. 112; Ray 1692 p. 130, Hooke 1704 pp. 328, 341, 401; Woodward 1695 passim, especially pp. 32-4, 78-81.

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1. Phil. Trans. 14 (1684) pp. 493, 742.

2. Woodward 1695 p. 40.

his observations of fossils. The Italian Paolo Boccone (1633-1704) also suggested uplift of the land to account for inland fossils. [1]

Hooke, though not providing such a detailed example as Steno's geological history of Tuscany, had a very radical approach, suggesting the destruction and creation of whole countries by inundations of the sea, subterraneous fires, earthquakes and volcanoes, and changes in the earth's centre of gravity and its poles. Fossils were for him an essential piece of evidence:

> 'There is no coin can so well inform an Antiquary that there has been such or such a place subject to such a Prince, as these will certify a Natural Antiquary, that such and such places have been under the Water, that there have been such kinds of animals, that there have been such and such preceding Alterations and Changes of the superficial Parts of the Earth'. [2]

A belief in the animal origins of fossils did not lead Woodward into accepting geological change; for him, the claimed correlation between the specific gravity of fossils and their position was sufficient to justify rejecting all non-diluvial change.

The writers accepting a mineral theory of fossils did not need to accept any real geological change. Plot had little to say on the subject, beyond pointing out that earthquakes are too small and infrequent in England to have much effect on land elevation. Lhwyd's travels convinced him of the effectiveness of erosion in mountain areas, but he made no mention of land uplift, and his attitude towards the nature of fossils was too uncertain to allow him to make use of them in this field. [3] Similarly, Lister, though prepared to accept a

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1. Palissy 1563 in Cap p. 37; Palissy 1580 in Cap, pp. 276-8; Steno 1669 in Winter 1930 p. 206; Thorndike 8 p. 37.

2. Hooke 1704 pp. 327-321.

3. Plot 1677 p. 114; CJR p. 240.

considerable amount of geological change, did not base this on fossil evidence; his theory makes them irrelevant here. He realized that the hills in Craven had been produced by erosion. This area is mainly Carboniferous limestone, with a very obvious cap of Millstone Grit on all the main hills. Lister suggested that these caps were once continuous, and that vast quantities of sand had been carried away by streams and rivers, to be deposited at river mouths and on the shore. He realized the alluvial nature of the lowlands and the great length of time needed to bring the material down from the hills, but he appears to have had no idea of any cyclic geological changes involving land uplift. [1] He was relatively uninterested in theories of the origin of the earth; a manuscript criticism by Lister of the theory of Thomas Burnet survives. [2] This is purely destructive, and no mention is made of fossil evidence, the only reference to animal life being a suggestion that fishes would have been unable to breathe in water covered entirely by a layer of rock, as Burnet's theory of the original state of the earth demanded. Lister's other arguments were geological: many rock strata are in fact horizontal, so contradicting Burnet; these rocks are often grained, not smooth, and they differ from place to place, so that they cannot represent one originally continuous and smooth covering; the salt of the sea is unique, and does not correspond to any derived from the land; and earthquakes and erosion are sufficient to account for existing land-forms. Lister did not put forward any theory of the earth of his own, and was careful to avoid becoming involved in his published work with what was then a controversial subject. His general attitude to such speculation can be seen in his comments to Lhwyd on Woodward's book:

'I read so far of the booke till I came at the world being

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1. Phil. Trans. 14 (1684) pp. 740-42.

2. MSS Lister 39 ff. 231-2

dissolved into a mudd, the shells excepted; wch wild fancie so shockt me, that I desid to have to doe no more neither with the author nor his writings. These bold stroakes have been in all ages the bain of Natural Philosophie and they will prevail for aught I can see for ever with the idle and prating part of mankind'. [1]

(13) Biological change.

Many fossils, such as ammonites, are very different from any living animals, and most of the others can be distinguished from extant forms with reasonably close attention. It follows that if these fossils are organic in origin, then either the animals they represent are now found only in unexplored areas, or there has been some form of biological change. The former was the more acceptable alternative, but this is a negative argument, and Ray in particular was uneasy with it. Nevertheless, extinction was unacceptable to most people:

> '... that many species of Shell-fish are lost out of the World [is something] which Philosophers hitherto have been unwilling to admit, esteeming the destruction of any one Species a dismembring of the Universe, and rendring it imperfect; whereas they think the Divine Providence is especially concerned to secure and preserve the Works of the Creation: and that it is so, appears, in that it was so careful to lodge all Land-Animals in the Ark at the time of the general Deluge'. [2]

This problem was the main cause of Cole's acceptance of the mineral theory; if ammonites are animal remains, 'then they must be of a species lost, which can never be without dishonour to the greate Creator of all ...'; extinction was something 'which an eminent

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1. MSS Ashmolean 1819 of 7 April 1695 f. 79.

2. Ray 1692 pp. 119-20; see also Woodward 1695 pp. 27-9.

member of the Royal Society was driven to'. The eminent member was Hooke, who denied that the loss of a species was derogatory or contrary to the scriptures or philosophy; after all, even the heavens change. [1]

A second possible kind of biological change is the transformation of specific characteristics; only Hooke appears to have considered this as a possible solution to the fossil problem. New varieties could be generated

> 'as alteration of Climate, Soil and Nourishment doth often produce a very great alteration in those Bodies that suffer it ... and this I imagine to be the reason of that great variety of Creatures that do properly belong to one Species; as for instance, in Dogs, Sheep, Goats, Deer, Hawks, Pigeons &c...'

If individuals can grow, change, die and corrupt, why not species? [2]

As with geological change, the problem can be avoided by assuming a mineral origin for fossils, and this seems to have been the main factor in influencing the collectors, Lister, Plot, Beaumont, Cole and Lhwyd. Lister appears not to have accepted biological change. He did not mention extinction, and his only reference to transformation is negative and concerns plants: he allowed 'accidental' changes such as colours in tulips and other flowers, and multiplicity of leaves, as in gilly-flowers; but [he claimed] that one plant did not change into a distinct species'. [3]

(14) Later development of ideas on fossil origins.

The geochemical theory of fossils did not outlast the authors associated with its origins. It was restricted to the quarter-century after 1670; by the end of the century it was being deserted even by some

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1. MSS Ashmolean 1830 f. 25; Hooke 1704 pp. 435-6.

2. Ibid. pp. 327-8, 436.

3. Birch 4 p. 427.

of those responsible for it. Beaumont was adopting an organic theory in 1691, [1] and in 1695 Cole developed a theory according to which animal shells were modified after petrification, so reconciling an organic origin with an appreciation of individual differences, without involving extinction or transformation. He claimed to have a series showing the gradual change from an oyster to an ammonite; presumably he had collected specimens of Gryphaea. Further, he claimed that the specimens showing the greatest change in form showed the greatest petrification, and that he had shown such changes experimentally. [2]

Lhwyd had always shown great uncertainty, but by the end of the century he appears to have adopted the compromise theory of development of fossils *in situ* from semina originating from marine animals ; as mentioned above, this was probably a development of earlier ideas such as those of Sir Matthew Hale. Lister himself, though never actually rejecting his earlier ideas, appears to have become less certain late in life, though still suspicious of theorising without first-hand knowledge:

> 'Fanciful men may think what they please; sure am I, until the History of Nature, and more particularly that of minerals and fossils, is better looked into, and more accurately distinguished, all reasoning is in vain. It is to be observed, where men are most in the dark, there imprudence reigns most, as upon this subject. They are not content fairly to dissent, but to insult everybody else ... How many scribblers have there been, without any knowledge of fossils?' [3]

It is difficult to account for this reversal. No new evidence was produced, and no influential book was published at this period. The

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1. MSS Lister 36 f. ix.
2. MSS Ashmolean 1830 ff. 10, 15, 16, 22, 25.
3. JP p. 267.

old arguments were still unanswered, but from the beginning of the eighteenth century the organic theory was unopposed in England. It may be that the change was caused by a simple loss of interest in the problem, though this itself cannot be accounted for. This decline can be seen in the numbers of papers on fossil invertebrates published in the Philosophical Transactions, to the middle of the century:

1670-94 (Nine years with no publication, only one volume for 1/88 to 7/91)	6 papers
1694-1702	6 papers
1703-1712	5 papers
1712-1723	0 papers
1724-1734	0 papers
1735-1743	0 papers
1744-1749	7 papers (including 2 on belemnites)

The dates given are those of the volumes of the abridged version of the Philosophical Transactions.[1] In addition there were four papers on fossil vertebrate bones between 1712 and 1744. From 1700 onwards, these papers, with one exception, refer to fossil shells as animal remains. The exception is the work of DaCosta on belemnites in 1747. These fossils had always been regarded as minerals, even by Ray, Hooke and Woodward, because of their peculiar internal structure and form, though Erhardt had realized their true nature in 1724.[2] Da Costa's other contributions on fossil shells show the normal organic outlook.

As the mineral or geochemical theory had been the product of the collecting and systematic work of the late seventeenth century, and the organic view had been held mainly by those with less practical acquaintance with the subject, it is perhaps natural that a decline in interest in the subject would affect the mineral theory most; but this

1. Edited by C. Hutton, G. Shaw, R. Pearson (London, 1809). This edition refers to every paper published in the full edition.

2. Balthasar Erhardt, De belemnites suevicis dissertatio (Leyden, 1724).

would not explain the apparent weakening in the attitudes of those originally responsible for the idea. It may be that, having discovered the distinctiveness of fossil shells, the collectors were led by the logic of a non-evolutionary world picture into a position too uncomfortable to hold once the first flush of enthusiasm had worn away, and the authority of Lister was no longer enough to support reason against common sense.

The position was quite different on the continent. The older, semi-occult ideas on fossil origins had not been discarded, and were still to be found, together with the equally traditional organic theories,[1] until the second half of the eighteenth century. English natural history and natural theology had no real counterparts on the continent at this time, so that the facts were neither so well known nor of such a controversial nature. The works of such serious and scholarly writers as Beringer in Germany, Mercati in Italy, and Bertrand in France, were not only denying the organic origins of fossils well into the eighteenth century, but were doing so, not with the geochemical arguments of Lister and Plot, but in ways which were obsolescent in England in 1660.[2] Bertrand, writing only a generation before Cuvier, believed that God created fossils in the rocks to beautify the earth; though he showed signs of having read some of the English naturalists, he has not been influenced by them fundamentally. Mercati's book listed Colonna, Steno, Scilla, Boccone and Scheuchzer as the principal writers on fossils; of English writers, he mentions only Woodward, and then only casually. English naturalists seem to have had less influence in Europe than Kircher,[3]

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1. Eg. J. J. Scheuchzer, Phil. Trans 24 (1704) pp. 1604-6; A. van Leeuwenhoek, Ibid 24 (1704) pp. 1774-84.

2. Beringer, Lithographiae Wirceburgensis (Wurzburg, 1726); M. Mercati, Metallotheca ...opus posthumum ... opera J. M. Lancisii (Rome, 1717; Mercati lived from 1514-93, but the many references in it to later writers show that this work was revised and annotated up to the date of publication); E. Bertrand, Memoires sur la structure interieure de la terre (Zurich, 1752).

3. Athanasius Kircher, Mundus subterraneus in XII libros digestos (Amsterdam, 1664).

who suggested a variety of origins for fossils: some being animal remains, other specially created by God, formed by a special succus petrificus or a vis seminalis in the earth, by special individual circumstances, or by pure chance.

(15) Summary of Lister's position in the fossil controversy.

Of the five major figures in the geochemical school of thought - Lister, Plot, Cole, Beaumont, and Lhwyd - Lister appears to have been the central figure. Lhwyd changed his position frequently after his first two or three years of activity, and in the end tried to compromise; Cole similarly attempted in the end to avoid the difficulties of both theories by leaving the mineral group for a theory of his own; and Beaumont, besides eventually joining the opposition, tended to think in terms of vegetation in the mineral world. This has a sixteenth century ring, but this is more apparent than real; he gave a thoroughly chemical explanation of his 'seminal root', and his use of the term 'rock-plant' shows he meant it as a rock with a plant shape rather than as a plant growing in rocky material. Plot also spoke of a plastic virtue, but again he made it clear that this is only a matter of terminological convenience. His explanations are completely chemical. [1]

Lister never made any concessions to the older, semi-occult theories, even in vocabulary. Though he was unable to give detailed explanations or experimental demonstrations of the growth of fossils, his geological and mineralogical interests set the background for his theories and gave an air of credibility to his chemical theory of the origin of formed stones. His main contribution was undoubtedly his systematic work, which was standard for a century. Lister was the authority on shells and shell-stones; his opinion on their origin must have carried weight for this reason. His work shows his opinions to have been reached after careful investigation

1. Plot 1677 p. 111.

of the material, and unprejudiced consideration of the evidence. Where he has doubts, he expresses them, and he scrupulously points out evidence which appears to weaken his case. It may even be that his arrangement of the plates of fossil shells next to those of the living forms most resembling them in his Historia conchyliorum was a major factor in persuading conchologists of their affinity; his works were used as standard sources for identification even by men holding an organic theory. [1]

However, though Lister was a diligent and intellectually honest worker, he showed no interest in speculation on broader issues such as geological and biological change, or the final causes of such complex mineral bodies; he complained of the world being 'intoxicated with subterraneous hypotheses, as well as super-lunary ones'. [2] His theory of fossils made them irrelevant to these problems, and he did not use them in the small amount of geological speculation in which he indulged; but it is difficult to say whether he refrained from wider issues from scientific caution or lack of imagination. He provided the basic facts, and attempted to give a 'mechanical' or chemical explanation of them; he was not a philosopher in the wider sense. He would no doubt have thought of himself as a rationalist, trying to give a scholarly account of something too often explained on a basis of a hasty and superficial survey.

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1. Eg. Samuel Dale in Phil. Trans. 24 (1704) pp. 1568-77; J. Morton, Ibid. 25 (1704) pp. 2210-13.

2. MSS Lister 37 f. 129.

Chapter VI

Physiology.

Lister's written works on physiology and medicine are of a greater bulk than those by him on other biological topics. They are of only slight historical interest, and do not fall entirely within the scope of this thesis. For the sake of completeness, however, an outline of Lister's physiological ideas in their context is included.

(1) Galenic physiology and its rejection in the seventeenth century.

Until the seventeenth century, the writings of Galen (A.D. 130-200) formed the basis of almost all physiological thought. His system,[1] like that of other ancient writers, was based on a number of fluid secretions or humours permeating a fibrous body. Though Galen did not see these humours as being so distinct from each other as has often been thought, and though he did not insist on precisely four humours, they are conventionally given as below:

Humour	Qualities	Element	Season	Age	Temperament
Blood	Moist, hot	Air	Spring	Childhood	Sanguine
Yellow bile	Dry, hot	Fire	Summer	Youth	Choleric
Black bile	Dry, cold	Earth	Autumn	Maturity	Melancholic
Phlegm	Moist, cold	Water	Winter	Old age	Phlagmatic

Precise details of the site and method of production of these humours were not given, and other secretions such as semen, sweat and milk complicated the picture. Disease, other than mechanical malfunction, was generally caused by an imbalance between the humours.

The material from which the body and its humours are derived was thought to be obtained from the food. In the gut, the heat of the body

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1. General statements on Galen's ideas are on the authority of Rudolph E. Siegel, Galen's System of Medicine and Physiology (Basel and New York, 1968)

brings about a transformation of the qualities of the food matter, converting it into chyle which can be absorbed into the portal vein. Here, and in the liver, the chyle was thought to be transformed into venous blood, carried by the veins to the heart, lung, brain and other organs. The idea of a natural spirit or pneuma physikon is often attributed to Galen in connection with this production of blood, but it is not clear that this was intended to be at all comparable with the other pneumata; in any case, the word pneuma sometimes appears to mean a material substance and sometimes a force.

The obvious body heat of mammals was thought of as a fire without flame, centred on the heart, using heat from the inspired air drawn in through the lungs. Secondary functions of the lungs were the promotion of some blood flow from the right to the left heart, via the lungs, during inspiration and expiration, and also to allow the escape of toxic qualities from the heart by a back-flow in the pulmonary vein. Though Galen did accept the idea of the absorption of material from the air, for him the essential point was the transfer of qualities between the body and the air; he knew that the volume of the air was not changed appreciably by breathing, and that air could not be detected in the arteries.

In the heart, a vital spirit or pneuma zotikon was thought to be incorporated into the blood as a result of the reaction between the heat of the air (from the lung) and the venous blood. The exact site of the change from the dark, thick venous blood to the bright, thin arterial blood was not given so precisely as is sometimes suggested.

This blood, laden with vital spirits, was carried by the arteries to all parts of the body, so that the tissues received both arterial and venous blood. There was some intermingling of the bloods in the organs, which therefore received both food and quality of heat.

In the brain, which also received air directly from the nose, a cerebral pneuma, animal spirit or pneuma psykikon, was thought to be made by the choroid plexus in the third ventricle. It was passed to the tissues along the nerves, leaving cerebro- spinal fluid as a waste product.

Excretion of waste matter from the other organs was seen as a process of active secretion by the kidney; filtration would soon cause the loss of all the blood from the body. The urine, mainly water with some bile, was thought to be separated from the blood as curds separate from whey.

Galen's works were not completely known in the West until the sixteenth century, and by the mid-seventeenth they were being seen by many as not providing an adequate theoretical explanation of the facts of physiology in that they did not give causal explanations in terms of the mechanical properties or chemical nature of the particles making up the body. Indeed, Galen rejected so elementary a mechanical explanation as that which Aristotle gave of the production of urine by the filtration of blood in the kidney.

The state of chemical knowledge at the time made it inevitable that mechanical analogies and explanations were more successful in science in general than were chemical theories. In physiology, however, the available comparisons were limited. The most obvious modern analogy is the internal combustion engine, with its emphasis on combustion and energy production; this, of course, was lacking at the time, and clocks, grinding mills and hydraulic devices were used instead. Here, the emphasis is on mechanical motion rather than on chemical transformation within a machine. There therefore existed something of a dichotomy between mechanical and chemical explanations which has now disappeared. The atheistic implications of mechanism were, in the seventeenth century, not so marked as they were to become later; the celestial clock needs a divine clockmaker. The rigid dualism of Descartes [1] allowed the

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1. R. Descartes, De homine (Leyden, 1662); English translation by T. S. Hall, Treatise of Man (Cambridge, Mass., 1972).

theologians unlimited scope in spiritual matters while attempting to give purely mechanical explanations of the workings of the flesh. It provided, however, severe philosophical problems concerning the interrelations of mind and matter; in the well-known words of Joseph Glanville (1636-80):

> 'How the purer Spirit is united to this Clod is a knot too hard for fallen Humanity to unty ... How should a Thought be united to a marble statue, or a Sunbeam to a lump of Clay! ' [1]

Attempts to solve the problem by introducing the idea of a material soul, at least to cover part of animal activity, as by Henry More (1614-87) [2] and Thomas Willis (1621-75) [3] were philosophically unsatisfactory, requiring such concepts as incorporeal matter, and theologically suspect, as leading to mortalism. [4]

Mechanical physiology was much more successful in certain of the simpler bodily functions, most notably in the work of Giovanni Alphonso Borelli (1608-79) on locomotion and the function of the skeleton. [5] Borelli also tried to explain digestion as being the mechanical grinding of solid food into a mass of fine particles, and secretion as being the result of the size of particles in relation to the pores through which they were being forced by fluid pressures. Although accepting two kinds of animal spirits, Borelli claimed that they were material fluids whose functions depended on their physical properties; they were not capable of acting at a distance. Other authors, such as William Coles (1635-1716) used the idea of the mechanical filtration of particles to account for

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1. Joseph Glanville, <u>The Vanity of Dogmatising</u> (London, 1661) p. 20.

2. Henry More, <u>The True Notion of a Spirit</u> (in Glanville's <u>Saducismus Triumphatus</u>, London,1681).

3. Thomas Willis, <u>De anima brutorum</u> (Oxford 1672); English translation by S. Pordage in <u>Dr. Willis's Practice of Physick, being the whole Works of that Renowned and Famous Physician</u> (London,1684).

4. See Richard Overton, <u>Man's Mortalitie</u> (Amsterdam,1644).

5. G. A. Borelli, <u>De motu animalium</u> (Rome,1680).

secretion,[1] while Francis Glisson (1597-1677) used a different kind of mechanical property in explaining the same phenomenon as being caused by the attraction of like particles of matter.[2] Even the complex metabolic processes lumped under the name of fermentation were given mechanical explanations by Daniel Sennert (1572-1637) who thought that they showed the separation and re-union of particles of matter - which, of course, they do.[3]

By the end of the seventeenth century, a group of English mechanical physiologists had emerged - James Keill (1673-1721), John Freind (1675-1728), Richard Mead (1673-1745), George Cheyne (1671-1743), and the rather older Scotsman Archibald Pitcairne (1652-1721). Keill was confident enough to write that

> 'The Animal Body is now known to be a pure machine, and many of its Actions and Motions are demonstrated to be the necessary Consequence of its Structure'.[4]

Even though the necessary data were lacking in many as yet unsolved problems,

> 'There is all the reason in the World to believe we shall have Success if we consider the Progress that has already been made, notwithstanding the mechanical Philosophy as applied to Physick is still in its infancy'.[5]

This attitude was shared by the other members of this group of writers, and bodily functions were explained as being a breakdown, movement, filtration and build-up of particles by mechanical forces and attractions. Quite elaborate calculations were made, most of which

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1. W. Coles, De secretione animali (Oxford,1674) p. 3.

2. F. Glisson, Anatomia hepatis (London,1654).

3. L. Thorndyke, A History of Magic and Experimental Science 7, (New York,1958) ch. vii.

4. James Keill, Essays on the Several Parts of the Animal Oeconomy (London,1717) (Second edition of Account of Animal Secretion of 1708) p. iii.

5. Keill 1717 pp. 101-2.

were meaningless, as these authors did not distinguish between force and pressure. In Keill's case, attractive forces inversely proportional to the third or fourth power of the distance between the particles were suggested.

There was, of course, a possible objection to such theories: the nature of the forces governing the interaction of particles, particularly attractive forces, were unknown and apparently unknowable. Freind, at least, was aware of this argument but saw it as leading only to the abandonment of all investigation and the rejection of all the work of Archimedes, Galileo and Newton 'as having unknown Causes; and cannot be explained without admitting Occult Qualities'.[1]

Chemical physiology or iatrochemistry is generally taken as having originated with Paracelsus (1493-1541). He was concerned more with cures than with explanations, and lack of factual chemical data encouraged the mystical tendency in his ideas. Indeed, his archeus, a non-material, invisible force regulating the activity of the body, meant that chemistry could never provide fundamental physiological explanations.

The work of J. B. van Helmont (1579-1644) contributed two valuable ideas to physiology: those of gases and of ferments. Although his idea of gases was not free from mystical overtones, the idea that there existed several kinds of invisible, particulate matter did open the way towards further progress, even though he did not see the air itself as a gas, but rather as something able to absorb these gases. Though still to some extent mystical, van Helmont's concept of ferments was not unlike the modern concept of enzymes in its effects: it gave an explanation as to why certain chemical actions take place in the body but not outside. The famous experiment on the growth of a small tree in

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1. J. Freind, Chemical Lectures (Oxford 1712) pp. 177-80.

a tub, supplied with nothing but water, had convinced him that the matter of the tree was nothing more than transformed water. By distilling both live, fermenting and dry, dead grapes, and finding that the former produced a gas and the latter only water, he believed he had shown that the living material produced ferments directing changes in the nature of the water - in this case, changing it into gas. Similarly, he explained the changes taking place in the nature of the food as it is converted into flesh in the body as the result of six fermentations: those in the stomach (acid); in the duodenum (neutral); in the mesenteric veins; in the heart; in the brain, and in the individual organs. [1]

Van Helmont retained an overall mystical factor, the Blas, controlling the activities of the body. Franciscus Sylvius de la Boe (1614-1672) was more thoroughly chemical, removing much of van Helmont's mysticism and reducing metabolism to the same kind of chemistry as that found outside the body, placing special emphasis on reactions between acids and alkalis. He was an influential teacher at the important medical school of Leyden. [2]

Georg Ernst Stahl (1660-1734), the originator of the phlogiston theory, was less of a reductionist than was Sylvius; he was impressed by the general stability of the body in spite of the many changes occurring in it, and this convinced him that some form of overall control by a sensitive soul was necessary, and that the resemblances between inorganic and physiological chemistry were superficial. [3]

Iatrochemistry in England had rather a chequered career in England in the seventeenth century, largely because of social and political

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1. J. R. Partington, A History of Chemistry 2 (London 1961) pp. 177-80.

2. M. Foster, A History of Physiology in the Sixteenth, Seventeenth and Eighteenth Centuries (Cambridge 1924) pp. 144-51.

3. Ibid. P. 165.

factors in this country.[1] The College of Physicians held a legal monopoly of medical practice in London; it was therefore subject to attack by all who wished to see a reform of medical services. It was complained that the College physicians served the rich and neglected the poor; that it stifled competition by seeking to exclude perfectly reputable physicians not in membership; and its insistence on Latin prescriptions made it difficult for patients to understand what was being given to them. There were constant disputes with the apothecaries who, though in constant touch with patients who were too poor to have the benefit of attendance by College physicians, were not allowed to prescribe for them. The College, being conservative, was more favourably inclined towards Galenic rather than to the newer Helmontian medicine, which tended to be more popular with some of the younger reformist physicians who found themselves discriminated against by the College. There was also some puritan prejudice against the pagan Galen and for the Christian van Helmont; Galenic medicine had in any case not shown itself to be a complete and reliable system, and the new chemical medicine had an appeal for reformers who then, as at any other time, wished to see quick results. Chamical medicine was therefore favoured by a large body of anti-establishment political and professional opinion.

These opinions became important in the Commonwealth period, Samuel Hartlib, the puritan advocate of reform in the social and educational systems, acted as an unofficial organizer and go-between for this movement, the most important medical members of which were George Starkey (1627-65), Frederick Clodius; William Rand (1617-63), J. S. Kuffler (1595-1677) and A. O. Faber. There was little original work of an

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1. C. Webster 'English Medical Reformers of the Puritan Revolution' Ambix 14 (1967)pp. 16-41.

iatro-chemical character published in England at this time, but many translations into English were made of such authors as Paracelsus, van Helmont, J. C. Schroeder, Sennert, Du Clos, Basil Valentine and Glauber. The orthodox learned medical literature was, of course, almost entirely in Latin.

During the Commonwealth period, several German iatrochemists took advantage of the inability of the College of Physicians to exclude unregistered practitioners, and established themselves in London, introducing a more mystical element. This was welcome to the puritans, who stressed the value of inspiration by inner light at the expense of reason and logic; but it alienated men such as Robert Boyle, who had earlier shown some sympathy.

Helmontian medicine was not eclipsed at the Restoration, as it still had strong support from the apothecaries and from some influential physicians, such as Thomas Williams and Thomas Shirley, the king's personal attendants.[1] However, the College of Physicians was still strong enough to oppose successfully the moves for the establishment of a rival society for the advancement of 'Hermetick Physick'; it could point out that the London Pharmacopoea already included many chemical remedies.

Furthermore, the gap between the exaggerated claims of the Helmontians and their achievements was becoming apparent in the later part of the century, even though so influential a writer as Thomas Willis adopted an iatrochemical approach, with ferments and five principles. As early as 1646, Sir Thomas Browne had wished

> 'the chymists had been more sparing, who, overmagnifying their preparations, inveigle the curiosity of many, and delude the security of most'.[2]

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1. P. M. Rattansi, 'The Helmontian-Galenist Controversy in Restoration England', Ambix 12 (1964) pp. 1-23.

2. Sir Thomas Browne, Pseudodoxia Epidemica, (London, 1646) vol. I ch. 7 (in Keynes op. cit.).

The rudimentary state of chemistry made almost impossible any attempt to give physiological explanations in these terms: the influential Archibald Pitcairne described the Helmontian system of ferments as

> 'that nauseous Doctrine ... which Hypothesis made the Art of Physic ... an unattainable Intricacy'. [1]

John Freind rejected explanations of metabolism in terms of acids and alkalis

> 'words which are now in everybody's mouth. But I cannot see what Definition can be adequate to them .... in vain we endeavour to fix the boundaries which separate each kind... if we cannot understand the terms, what use are the theories?' [2]

Ferments were rejected by the standard early eighteenth century authors Boerhaave and von Haller. [3]

(2) Lister's attitude to chemical and mechanical physiology.

Lister was very interested in inorganic and particularly mineral chemistry, having published several papers on the subject in the Philosophical Transactions and a book on the chemistry of mineral waters. Further, his ideas on the growth of mollusc shells and stones, teeth and bones in the human body showed that he was quite prepared to consider chemical explanations in certain fields of physiology. His papers contain two Helmontian manuscript works and several notes in his own hand on chemical remedies. As described later, he quoted in support of his ideas on the cause of the difference in colour between arterial and venous blood, an experiment in which 'blue vitriol, in volatile spirits of urinary salts, extracted from copper', changed colour when exposed to a vacuum.[4]

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1. A. Pitcairne, Collected Works of Archibald Pitcairne (London, 1727) p. 64
2. Freind 1712 pp. 13-15.
3. Hermann Boerhaave, Praelectiones academicae in proprias institutiones rei medicae (Gottingen 1739) pp. 242-44; Albertus von Haller, ...

Nevertheless, he made no special attempt to give a chemical basis to his comprehensive physiological theories, and even resisted the idea that the humours were corporeal and particulate. [1]

Lister was in fact very scathing in his attacks on mechanical explanations. He commented that they were a revival of Democritean ideas, as if that were condemnation enough in itself, and that they changed medicine from an art into a science, when the degrees of uncertainty in the subject made this impossible. In its existing poor and immature state, any attempt to give a universal theory was an excuse to avoid the industry of experiment and observation; childish, arrogant, inane, dreams as dangerous as those of astrology, ropes of sand which would lead to the overthrow of all ancient medicine and the abandonment of herbal remedies. [2]

In his attitude to individual writers, Lister appears to have been more tolerant than his general attacks might indicate. Though proud to feel that he

> 'had ye honour of setting many of their mathematicians heads at work, as Morelands, Drake, Chenies, Wainwright, Keils ...',

he did allow that

> 'Meade has more learning and modestie than them all .. Pitcairne writ against me verie civillie and candidly, his writings being verie ingenious and most elegantlie writ, as far as I can understand them! ' [3]

Freind had 'writ like a schollar and well, considering the prejudice the doctrine of humours lies under', even though Lister later

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3.(cont) First Lines of Physiology (First edition 1747; English translation used here, Edinburgh 1786, part ii p. 87).

4. EA p. 102.

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1. Hum p. 7.

2. Ibid. preface.

3. MSS Smith 51, ff. 56-7.

described Freind's theory of particle attraction as 'crasie'.[1]

Lister's works show no sign of any use of quantitive or mechanical reasoning, even where this would have helped his case. Thus, his theory of the use of the lungs was that they were needed to contain the surplus blood which it was beyond the capacity of the veins to contain. Some attempt could easily have been made to check the quantities involved, but this was not done. It is difficult to avoid the conclusion that Lister was unable to think in quantitive terms, and could not begin to challange the mechanists in their own field.

(3) Digestion.

The processes by which food is converted into new body material were beyond the powers of explanation of ancient chemistry. For Galen, the changes consisted of the transformation of the qualities carried by the matter of the food, and this was brought about by the heat of the body. The iatro-chemists introduced the idea of ferments to explain changes, such as this, which occurred only in organic matter. Van Helmont, for example, suggested a series of six fermentations in the conversion of food: (1) an acid fermentation in the stomach, the ferment being derived from the spleen; (2) that in the duodenum, brought about by a ferment in the bile; (3) one in the liver, producing a dark, crude blood; (4) the conversion of this into a light, volatile blood in the heart; (5) the addition, also in the heart, of a vital spirit or archeus; and (6) a final fermentation in the tissues, specific to the organ concerned.

For van Helmont, these fermentations were not purely chemical; a mystical blas was thought to be in control of the changes. For Sylvius, physiology was a branch of chemistry, and these changes were no more than a sequence of chemical changes in which acid/alkali balances were important, as shown by the reactions of gastric and pancreatic

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1. MSS Smith 52, f. 11.

juices. Similar ideas, in which ferments were thought to exite movements of the particles of matter, were put forward by Libavius, Sennert and Sala. [1]

The theory of the addition of ferments to the gut contents was advanced by the discovery of the ducts of the pancreas (Wirsung, 1647) and of the submaxillary (Wharton, 1656) and parotid glands (Steno, 1662), though no such duct could be found, however, for the spleen.

There were many variations on the idea of fermentations in the stomach. Acid ferments were usually suggested, but William Musgrave (1655-1721) believed it to be alkaline; Charles Leigh (1662-1701), a mixture of salts and nitro-aerial particles; Clopton Havers (d. 1702), the result of the interaction of several kinds of saliva; and William Cowper (1666-1709) a complex series of reactions to which he refused to apply the term 'fermentation'. [2]

There were also attempts, such as that of Borelli, to explain digestion on the basis of a mechanical grinding away of the food particles and their absorption through the pores of the gut wall. This idea was widely used by the English mechanical physiologists, but it was not easy to elaborate or to confirm by the techniques of the time. By the middle of the eighteenth century, a compromise was reached: the standard text of von Haller stated digestion to be brought about by heat, acid, and mechanical disruption caused by the release of air from the food, though no special ferment was necessary. [3]

Lister took up an attitude which was already outdated in the later seventeenth century, rejecting both mechanical and fermentative theories. He claimed that digestion was exactly the same process as putrefaction,

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1. Partington 1961 pp. 119-228, 265-280.

2. Phil. Trans. 14 (1684) pp. 699-701; Ibid. p. 694-8; 21 (1699) p. 233-258; 19 (1696) p. 231.

3. Haller ii pp. 86-7.

and that in saying this he was reviving the opinion of the ancients, as shown in the works of Celsus, Pliny and Empedocles. There was no fundamental difference between the putrefaction of meat in the air outside the body and its digestion in the stomach, since food in the gut was in fact still outside the body, not having passed through a body surface. Lister emphasized this point frequently in his writings, and it is of course a valuable point essential to the understanding of the problem.

Digestion was thought to be much faster than putrefaction because of the heat of the body, which accelerates any change; the natural souring of wine into vinegar takes place much faster in the stomach than outside the body. Corruptive changes are also favoured by other kinds of heat, as can be seen by comparing the putrid and unhealthy waters of African and Indian rivers with the 'fresh, limpid and healthy' Thames, and in the much higher incidence of putrid fevers in the tropics.

The similarity between digestion and putrefaction is shown by the Welsh habit of eating putrid meat, which they find more easily digestible. Similarly, though English sailors and merchants in the Gold Coast found the local fish (mainly shark) inedible, the negroes were grateful for it, at least when it was half-decayed and so more easily digested. It was noticeable that the most-easily digested foods, such as milk and some fish, especially ling, were also the most liable to decay; and English law made it compulsory to bait bulls before slaughter, so that the heat of the struggle would make the flesh more easily digestible. [1]

Lister was not very explicit on the nature of the changes involved in decay and digestion. Air was closely concerned in addition to heat, as when it is excluded, decay ceases, and so, presumably, would digestion, though Lister gave no evidence for this. His anti-mechanical bias showed in his rejection of the idea that the weight and elasticity of the air may

1. Hum. pp. 48-56.

be concerned in these changes. Lister distinguished between a nudus aer, a subtle, weightless, invisible fluid (liquidus) and aer atmosphericus, which was contaminated by fire or sulphur, derived mainly from volcanoes and responsible for the spring of the air, and by water, coming from the oceans and responsible for giving air its weight. There was no nitre in the air, as this is a non-volatile salt, so that Lister rejected the ideas of Mayow, though without mentioning him by name. He thought that atmospheric air entered the guts in the fluid secreted by the lining of the stomach; this was compared with perspiration from the skin, which he also thought to help in the loss of excess air. Lister referred to this as vital air, though he stated that it was in no way different from atmospheric air. After bringing about the digestive changes, this air was expelled from the gut. [1]

Lister discussed other 'fermentations', as in wine, beer and bread, claiming that, as with digestion and putrefaction, they were all accompanied by the production of gas and warmth, and by the thinning and corruption of the material. [2]

(4) The lymphatic system and absorption.

There was much interest in the mid-seventeenth century in the lymphatic system. The 'lacteal veins' of the mesentery had been discovered by Aselli in 1622 by opening a dog after giving it a meal. He also noticed the valves in the vessels. [3] Pequet showed that these lacteals, after having passed through the receptaculum chyli, emptied into the subclavian vein. In 1652, Bartholin showed that the lymphatic vessels were found in all parts of the body, though only those in the mesentery (the lacteals), ever took on a milky appearance. [4]

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1. Ibid. pp. 79-88.
2. Ibid. pp. 122-29, 135-39.
3. G. Aselli, De lactibus sive lacteis venis... (Milan, 1627).
4. J. Pequet, Experimenta nova anatomica (Paris, 1651); translated as New Anatomical Experiments of J. Pequet ... also an Anatomical Dissertation by T. Bartholinus. (London, 1653).

The lacteal system provided a route for absorbed food different from that suggested by Galen, who had assumed that the food was carried by the hepatic portal vein to the liver. Aselli had supported the Galenic view, as did so late a writer as Francis Glisson.[1] Other writers did not believe that the liver received chyle through the lacteals; some of these were mislead by the fortuitous passage of some of the lacteals through the pancreas in many mammals into thinking that the chyle was filtered in some way by this organ. The discovery of the pancreatic duct by Wirsung in 1642 appeared to confirm this by providing an excretory duct for material rejected from the chyle.

There had been attempts, such as those of Henry Power and William Musgrave[2] to show directly the uptake of material into the lymphatics. Lister made several attempts in the early 1670's, nearly all unsuccessful. He injected dyes into the duodenum of a number of dogs:

> ' ...good Barbados Indigo in fair water, and filtrated; also lumps of Ingido thrust down his throat; good broath (as they call it) of a blew fat; Indigo in Milk, Saffron in milk. Again, we tried in some dogs fed beforehand and injected the liquors in the very height of the Chyle's distribution, into others yet fasting, and that for a longer or shorter time. The Success was so constant, that we cannot say, we ever did find the least discolouring of the Chyle on the other side the Guts, that is, within the Lacteal Veins, but ever white and uniform. Whence we judge it not very feasible to tinge the Venal chyle in well and sound animals'. 3

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1. Francis Glisson in MSS Sloane 3308 ff. 273-281.

2. H. Power Inventio Aselliana de Venis Lacteis et de Motu Chyli, (1654) in MSS Sloane 1343 f. 42; Phil. Trans. 14 (1684) pp. 812-19.

3. Phil. Trans. 8 (1673) pp. 6061-2.

However, he noticed that the urine of persons having eaten prickly pear was coloured exactly as the juice of the plant, and that a glyster of turpentine caused the urine to smell for several days; he believed that these observations showed that these substances could be taken up with the chyle from the gut.[1] He therefore persisted with his experiments and eventually succeeded.

He starved a dog for 40 hours, gave it a small meal with no liquid, and then injected indigo into the duodenum. After 3 hours

> ' ... we found many Lacteal Veins of an Azure Colour; and cutting some of the biggest, we plainly saw a thick blewish Chyle to issue forth and spread itself over the transparent Membranes of the Mesentery ... whence, although it hath been doubted by some, yet it is most evident, that the Lacteal Veins receive what they carry, from within the Cavity of the Intestines'.[2]

The way in which the chyle was taken up was not made clear, though Lister believed that it may be some kind of suction, and that the 'gut glandules' may be absorptive as well as secretory and excretory as are other parts of the body surface such as the skin. (These glandules were clearly Peyer's patches, and not lymph nodes; Lister in fact claimed priority over Peyer in their discovery.[3]

The remnants of the non-absorbed food were thought by Lister to be passed to the caecum to 'drained, baked or hardened'. This process was aided by the colon, but the animals with the driest faeces were seen to have the largest caeca.[4]

(5) Body fluids.

Galen had believed that the tissues of the body are made of three

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1. Ibid. loc. cit.; MSS Lister 39, f. 160.

2. LMD p. 75. (Letter written to Oldenburg in 1673, also published in Phil. Trans. 13 (1683) p. 9).

3. FMA pp. 16, 42-3; MSS Lister 39 f. 160.

4. LMD pp. 77-80. Also published in Phil. Trans. 14 (1684) pp. 455-7.

basic elements: fibres, membranes and flesh, and that these were bathed in fluids derived from the blood. [1] The microscopical work of Malpighi and the injections of Ruysch in the seventeenth century showed the existence of minute blood vessels in all parts of the body, and by the eighteenth century orthodox opinion, as seen in the teaching of Hermann Boerhaave, was that the body was a mass of minute fibres or vessels, the terms being interchangeable at this level.[2] The idea of undifferentiated 'flesh' had little meaning about 1700, though Haller was to call attention once more to the material between the fibre-vessels later in the century. [3]

Lister supported the idea of the body being made up entirely of vessels, giving a good deal of space to a discussion de fibrarum doctrina hodierna, quae multum praevalet. [4] He even found it necessary to reprove Ruysch, who could not find blood vessels in the bile duct. The liver, Lister claimed, was a double viscus, made of two interlocking sets of vessels: the blood vessels (mainly distributories of the portal vein) and the tributaries of the bile duct; the latter were just as much vessels as the former. Those who claimed that certain organs, such as the appendix, had no vessels, were unobservant, and Harvey was only unable to see anastomoses between arteries and veins because he did not use a microscope. Lister discussed several lower animals, claiming that useful information could be obtained from them; the liver of the crab, for example, was obviously fibrous even to the naked eye.[5]

It is clear to the most casual observer that fluids of several kinds form an important part of the body. As outlined above, to Galen

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1. Siegel pp. 234-5.

2. L. S. King, The Medical World of the Eighteenth Century (Chicago 1958) pp. 59-93. See also Keill p. 35.

3. Haller I pp. 1-20.

4. Hum. p. 190.

5. Ibid. pp. 175-203.

these fluids or humours were seen as carriers of the qualities of heat and cold, wetness and dryness, rather than as material substances in a modern sense. In the seventeenth century, the chemical properties of the body fluids, such as their acidity or alkalinity, were being stressed by many writers. Furthermore, both black and yellow bile and often phlegm were being seen as excretory materials rather than as essential body humours, [1] so that blood was coming to have an even more important role than it had even in the Galenic system, particularly after Harvey's discovery of the circulation of the blood. By the early eighteenth century, the mechanical physiologists were emphasizing the pressure of the blood, the size of its particles and of the pores in the walls of its vessels and in the glands, and attractive forces between these particles. [2]

Lister accepted a wide and material concept of the humours. He classified tham as follows:

Primary			Blood proper [ie. cells]
			Serum or lymph [synonyms]
By concoction			Chyle
By secretion	Useful		Milk
			Genital secretions
	Waste:	from chyle	Urine
			Sweat
		from lymph	Phlegm
		from blood	Bile

Lister ignored any distinction between black and yellow bile. Black bile, unlike the other Galenic humours, was a theoretical construct

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1. Eg. Willis, preface to *De febribus*, being part ii of *Diatribae duae medico-philosophicae* (London, 1659), in Pordage.

2. Freind pp. 71-79; Keill pp. 96, 196-202, 209-10.

which could not be directly demonstrated. It was thought to be made in the liver and removed by the spleen. As this organ had no duct, it had to be supposed that black bile entered and left the spleen through the blood. The liver and spleen are connected by veins, but the splenic vein is a tributary of the hepatic portal, so that it had to be assumed that the blood in it flowed in a direction opposite to that in the other factors of the portal vein. Similarly, the connection between spleen and stomach (thought to be responsible for the excretion of the black bile) is by branches of the coeliac artery, again needing a counter-flow. In spite of these difficulties, the idea of black bile was still supported in the middle of the seventeenth century by such writers as Willis and Glisson; but by the end of the century it was obsolete. Lister nowhere mentions it.

According to Lister, the opaque white chyle in the lacteal vessels retained its identity for some time after entering the blood vessels, even though the two fluids were so finely mixed as to be inseparable to the eye.[1] This chyle was thought to contain a high concentration of urinous salts, since it had been produced by the putrefaction of the food, which, like all decay processes, produces these salts. Indeed, the whiteness of the chyle is caused by these salts, according to Lister, and it can be removed by dilution with water.[2]

Before the absorbed food can be used, these urinous salts must be removed from the blood; the humours are the result of this process of purification. Arterial blood was said to be made up of three parts: the grumy part, consisting of the 'globules' (red cells in modern terms); the new chyle, recently taken in to the vascular system from the lacteals; and the purified lymph or serum. This arterial blood must be treated by the kidney or skin, which remove most of the urinous salts and excrete them in the urine and sweat. Lister here stressed the role of insensible perspiration, being influenced by Santorio Sanctorius (1561-1636), whose

1. Hum. pp. 282, 277.
2. FMA p. 14; LMD p. 80.

work he had edited. Some urinous salts were also thought to be lost in the breath. [1]

Newly purified chyle was seen as the basis of milk, whose white colour showed it to have been relatively little changed from chyle. There is a discrepancy here, as Lister had stated that the whiteness of the chyle was caused by the salts, which he believed were removed by the purification taking place before the milk was secreted; and he did not explore the differences which would be expected on his theory between males and non-lactating females on the one hand and lactating females on the other. [2]

The removal of these materials by the mammary glands left the chyle in the blood in a state in which it could be called lymph or serum. This was supplied to the remaining organs, many of which used it as the source of material for their own specific secretions, useful or excretory. The phlegm (pituita) secreted by most of the gut and intestinal glands, for example, was an excretory product eliminated from the gut; any interference with this elimination, such as the reabsorption of the phlegm, would cause disease. Finally, the most refined lymph, left after all the other organs had removed impurities, formed the material from which the genital secretions, principally semen, were formed. All these secretions depended upon a regular inflow of chyle from the gut; in starvation, the organs cut down their activity, so that the stock of chyle in the arterial blood was not much reduced. [3]

As each organ received more lymph than it needed for its activities, there was a special return system of lymphatic vessels carrying the lymph or old (vetus) chyle back to the blood vessels, where it would now form the blood serum; these vessels also received new chyle from the lacteals.

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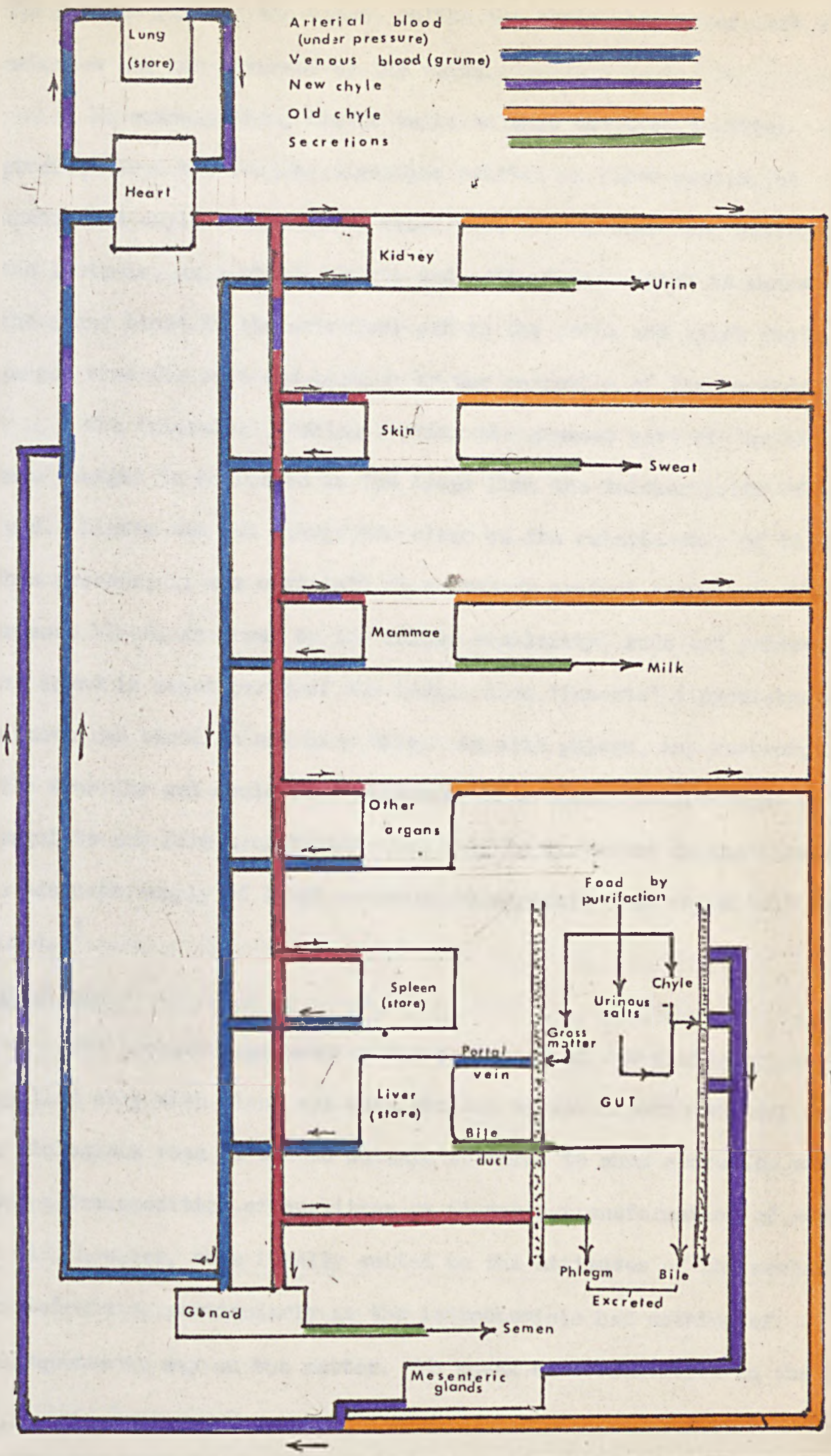
1. Hum p. 278; FMA p. 35.

2. Hum. pp. 426-30; FMA p. 31.

3. Hum. pp. 277-85; 387-92; FMA pp. 26-35.
It will be seen that Lister's system requires either that the organs be supplied with blood in series (they are in fact in parallel) or that some means exist for directing salts in the blood to particular organs.

Lister's system of body fluids



The grumous part of the blood, unlike the chyle, had never left the arteries and was returned by the veins. [1]

In summary then, Lister believed that the liquid matter produced from the food by digestion existed in three states: as unabsorbed chyle in the guts, mixed with the thicker food material; in the lacteals, as a white, opaque and salty fluid, which is mixed with the grumy blood in the arteries; and in the veins and lymph ducts, purged from its salt and opacity by the secretion of the humours. [2]

The 'globules' (cells) forming the grumous part of the blood were thought to be formed in the liver from the thicker parts of the food. Lister was not altogether clear on the relationship of bile to this process; it was certainly an excretory product connected with the grumous blood, as shown by the colour similarity; reds and yellows were not found in other parts of the body. Also 'insects' (invertebrates) without red blood do not have bile. As with phlegm, any reabsorption of bile from the gut could cause disease. The blood globules tend to coagulate and form a clot; this can only be prevented in the vessels by an adequate supply of lymph or serum to separate them and dilute the blood. [3]

(6) Secretion.

The problem presented by the secretion of new fluids by an organ supplied only with blood was more obvious to seventeenth century physiologists than it was to Galenic writers, to whom secretion was as much a transposition of qualities as it was a transformation of material. It did, however, seem ideally suited to the attitudes of the mechanical physiologists, particularly as the iatrochemists had nothing of consequence to say on the matter. To speak of fermentation in the glands

1. FMA pp. 24-5.

2. Ibid p. 19.

3. Ibid pp. 30-31, 48-52.

as the cause of secretion was, as Cole pointed out, merely to exchange one word for another; and in any case, soluble ferments would ~~not~~ be washed out of the gland and insoluble ones could not be carried there.[1] A filtration theory was, in the words of Pitcairne, 'more agreeable to the Mechanics, and the new Philosophy'.[2]

Such mechanical ideas were adopted by Cole (1673) and Borelli (1680), expanded a generation later by Keill, Pitcairne and Freind, and adopted by the standard eighteenth century authors, Booerhaave and Haller. By these writers, secretion was a simple straining or separation of particles from the blood, caused by the pressure of the pulse, the size and shape of the particles in the blood, the size of the pores in the vessels, the diameter of the vessels, the angles at which they branch, and so on. 'These conditions, Nature is able variously to join together or separate, and impart to every strainer in greater or lesser degree; and thus by various methods to modify the secreted humours'.[3]

Lister was in general much opposed to mechanical physiology. Nevertheless, in this matter he felt compelled by the inadequacy of fermentative ideas to adopt explanations of this kind, though without much elaboration or obvious enthusiasm: '*de modo autem Secretionis Animalis adhuc quidem ambigo et in re tamen difficili haud libenter aliquid affirmo*'.[4] He accepted that the pulse was the prime cause of secretion, though his example of ligaturing the renal artery and showing cessation of urine production does not necessarily lead to this conclusion. He did not discuss pore size, though he added that capillary suction in the vessels

1. Cole pp. 52-55; Pitcairne p. 41.
2. Pitcairne p. 43.
3. Haller I p. 127.
4. FMA p. 37.

that make up a gland may be an additional factor. He also speculated a little on the possibility of magnetic attractions between glands and certain particles in the blood. Such ideas were developed in more detail by Keill; they had obvious similarities to theories of gravitation, but were also liable to more mystical and less mechanical interpretations. [1]

(7) Respiration and the blood.

The close connection between the lungs and heart was clear to the ancients, but so long as the nature of the air was not understood and the arterial and venous blood were thought to be substantially distinct fluids, the reason for this connection could not be understood. Aristotle had believed that the lungs cooled the heart, but Galen thought that their function was more complex. They were principally for the transference of qualities from the blood to the air: heat, and toxic qualities from the blood in the left heart. This was the reason that animals would suffocate even with the lungs filled with air. There was also some transfer of material, but in very small quantities. Air (pneuma) was taken to, and vapours from, the left heart, so that the pulmonary veins were thought to carry blood at least to some extent in both directions. Galen also believed the lungs helped to move blood from the right to the left heart, in addition to that passed through the inter-ventricular septum. The difference between arterial and venous blood was caused by the presence of pneuma in the former, though Galen was not clear on the exact site of the change. [2]

The discovery of the circulation of the blood by William Harvey (1578-1657) considerably changed the problem of respiration. Harvey himself said little about the function of the lungs, adopting an Aristotelian explanation: hot blood is carried to the lungs, to be tempered by the inspired air and to be freed from bubbling to excess. [3]

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1. EMA pp. 35-39; Keill pp. 99-105.
2. Siegal pp. 29, 35-40, 58, 154.
3. W. Harvey, Exercitatio anatomica de motu cordis et sanguinis (Frankfurt, 1628); English translation by K. J. Franklin, The Circulation of the Blood and Other Writings, (London, 1963) p. 50.

However, now that the venous and arterial bloods were shown to be the same fluid constantly being transferred from one side of the vascular system to the other, the reversible colour change between them appeared more fundamental, Harvey himself did not stress the difference; he believed it to be to some extent an artefact, depending on the way the blood spurted or oozed from the wound, and was very suspicious of contemporary explanations in terms of 'spirit' or pneuma in the arterial blood. Such spirits were, in his opinion, mere verbal tricks which explained nothing. [1]

Later in the century, a clearer idea of the air as a mixture of different materials began to emerge. George Ent (1604-1689) claimed in 1641 that the air provided a nitrous quality for the vital flame of the heart, just as it did for an ordinary flame. [2] Similar ideas were held by Ralph Bathurst and Malachi Thruston. [3] In 1665, Robert Hooke (1635-1703) spoke of 'nitrous particles' from the air. [4]

The first experimental demonstration that the air was actually essential to life was that by Robert Boyle (1627-91), who showed that a variety of animals were killed by an exposure of more than a few seconds ~~exposure~~ to a vacuum; the action of the pump in removing air showed that waste matter could not be the cause of death. [5] However, it could still be claimed that death was caused by the cessation of the chest movements rather than the reverse, and thus a mechanical function for breathing was still possible. Robert Hooke removed this possibility in 1667 by opening a living dog, puncturing the lungs, and keeping them inflated by a continuous blast from a pair of bellows attached to the trachea, just

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1. W. Harvey, Exercitatio anatomica de circulatione sanguinis (Cambridge, 1649) in Franklin, pp. 147-51.

2. H. Guerlac, 'John Mayow and the Aerial Nitre', Actes du 7me Congrsse Internationale d'Histoire des Sciences 1953 pp. 332-49, quoting Ent, Apologia pro circulatione sanguinis (London, 1641).

3. Partington II pp. 573-87.

4. R. Hooke, Micrographia (London, 1665) p. 103.

5. R. Boyle, New Experiments Physico-Medical touching the Spring of the Air (London, 1660); Phil. Trans. 5 (1670) pp. 2011-31, 2026-56.

sufficient to keep them inflated. The dog lived as long as this blast was kept up, even though there were no rhythmic chest movements. [1] Breathing could now be seen as being necessary only for keeping the lungs supplied with fresh air.

It was still necessary to show that the change from venous to arterial blood took place in the lungs and not in the left heart. This was done by Richard Lower (1631-90) in 1669. [2] He used Hooke's experiment to show that the blood in the pulmonary vein was bright red when the bellows were in use, but became dull when ventilation of the lungs ceased. Further, a clot of blood was brighter on its exposed surface than where it was covered and not in contact with the air. Lower concluded that the change in the blood is caused by venous blood absorbing air in the lungs on its way into the arteries.

Thomas Willis, who had worked with Lower at Oxford, on this problem, agreed that the colour change was caused by the uptake of nitre from the air into the blood, where it reacted with sulphureous particles from the food; waste matter from this reaction was expelled through the lungs. [3]

A more complete attempt to give a theoretical explanation for these facts was that of John Mayow (1640-79) in 1674. Mayow rejected the old ideas that the lungs cooled the heart (they are too complex), and that they churn the blood and move it from the right to the left heart (the heart continues to beat even when the lungs are at rest). He believed that the processes of growth and decay are brought about by the reaction of saline, sulphureous and nitrous particles. The growth of

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1. Phil. Trans 2 (1667) pp. 539-40; Birch 2 p. 198.

2. R. Lower, Tractatus de corde (London, 1669); edited by R. W. T. Gunther as 4 volume 9 of Early Science in Oxford (Oxford, 1932).

3. T. Willis, De sanguinis incalescentia (part i of Diatribae duae medico-philosophicae (London, 1670), published in Pordage.

plants involved the reaction of light, nitro-aerial particles from the air, and compound saline-sulphureous particles from the soil; the nitrous particles detach and combine with the saline matter, leaving the sulphureous matter in the form of oil and other inflammable substances. The reverse process takes place during decay, with saline-sulphureous particles and free nitro-aerial spirit being released. In the blood of animals, a fermentation takes place in which, as in plant growth, saline-sulphureous particles from the food react with nitro-aerial particles. Because of this, air must constantly be taken into the venous blood in the lungs, so changing its colour. After removing the nitro-aerial particles, the remaining air and 'fumes' are given out. In aquatic animals, the particles are taken from the air 'interspersed' in the water. [1]

Mayow's ideas have a superficially modern sound, but they were by no means generally accepted, and in fact were largely ignored. Lower's demonstration that the change in colour occurred in the lungs, essential for a proper understanding of lung function was not easy to repeat because of practical difficulties, the pulmonary veins being short and difficult of access. In 1698 so reputable a physician as William Musgrave, secretary of the Royal Society, could still suggest that the purpose of respiration was to keep the blood in motion, [2] and even in the middle of the next century, the standard textbook of Haller denied that it had been established that any change in the blood took place in the lungs. He fell back on older explanations such as a mechanical grinding of nutrients in the blood in the lungs during breathing. [3] Such explanations had been used by Borelli and the English mechanical physiologists. Pitcairne, for example, rejected the ideas of

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1. J. Mayow, Tractatus quinque medico-physici (London, 1674); English translation by 'A. C. B.' and 'L. D.' (Edinburgh, 1957) pp. 202-4, 35-46, 73-87, 179.

2. Phil. Trans. 20 (1698) pp. 178-80.

3. Haller pp. 90-1, 161.

Lower and Mayow, as it could not be shown experimentally that air did in fact enter the blood from the lungs. He claimed that

> 'every Particle of the last-formed Blood is so broken and comminuted, so separated from each other or reduced to so small a Degree of Cohesion, that it is easy for any one Particle to pass off into some secretory Vessel answerable to its Bulk'.[1]

This reduction in particle size was thought to be enough to cause a colour change.

Lister dealt with respiration at some length, both in De humoribus and as a digression in Exercitatio anatomica. He rejected the Borellian mechanical comminution of particles in the blood; the 'globules' (cells) were already as small as the smallest vessels, and were solid and apparently hard, and lungs were absent in fishes, in which the blood was nevertheless similar to that of quadrupeds. Chemical explanations were also inadequate. Nitrous particles could not occur in the air, as it is non-volatile salt; in fact, Lister claimed, it could not be shown that the air contained anything needed by the animal. Death in the Boylean vacuum could be caused by many factors, as the whole body was involved. All the blood vessels were affected, for example, and lack of air would prevent putrefaction of food in the gut; cold-blooded animals, with a slower rate of putrefaction, were therefore the least affected by the vacuum.[2]

Lister's theory of the use of the lungs was peculiar to him. He believed that the total volume of the blood was greater than the capacity of the blood vessels, and varied according to the state of the contraction of the muscles and the rate of inflow of chyle from the gut. The surplus blood had to be taken up somewhere and a steady return made to the heart.

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1. Pitcairne p. 94.

2. Hum. pp. 15–16; EA pp. 40, 29.

(Lister wrote: ut pulmo plenus perpetuo existat sanguinis continuo affluxu ad auriculam dextram - presumably a slip for auriculam sinistram). The lungs performed this storage function in the chest; the liver and spleen acted as reservoirs in the abdomen. Even though he described secondary functions for liver and lungs, Lister went so far as to say that these organs were without other essential activities, and were not vital for life. In the embryo, in which the blood short-circuits the lungs, the placenta was said to take over the storage function; the sudden removal of its capacity at birth forced the excess blood into the lungs. [1]

This theory is open to objections which it would be reasonable to expect Lister to have seen. The lungs, liver and spleen have blood supply systems on plans which differ from one another more than do those of any other organs. As Lister had used this line of argument in discussing the function of the molluscan liver, it is surprising that he missed it here. The liver and spleen resemble one another in having open blood sinuses, thereby being fitted for use as blood reservoirs, which is, of course, one of the principal functions of the spleen. However, the lung is histologically very different, as was known at the time from the injections of Ruysch. Lister failed to account for the intricate vascular supply to the lung, and the intimate contact between the blood vessels and air ducts in the organ.

A secondary use of the lung given by Lister was that of insensible perspiration; by equating this with loss of 'fumes', he was able to claim the support of the ancients here. He did not, however, restrict the idea of loss of fumes to the lungs and heart, but also saw the lungs, like the skin, as being part of the general body surface through which perspiration occurred. As the tubes connecting the lungs with the outside

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1. Hum. pp. 2-3, 6; EA p. 101.

air were blind, ventilation was necessary to remove perspiration. In the embryo, loss of saliva, presumably to the amniotic fluid, took the place of lung perspiration. [1]

A third, minor function of the lungs was a general cooling of the body as a whole, though Lister stressed that there was no special 'inner flame' in the heart. The air is usually, though not always, cooler than the body; when it is so, it must cool the inner surface of the lung. Lister noted that cold-blooded animals breathed less frequently than warm-blooded, as would be expected on this theory. [2]

Lister paid more attention to cold-blooded creatures than did most writers of the time. Though there is no lung in fishes, he claimed that the storage function was taken over by the extremely large (ingens) auricle, by which he must mean the sinus venosus. The gills were needed to take over the perspiratory function of the lungs. Lister thought of this insensible perspiration as a form of excretion rather than as a cooling mechanism; as fishes cannot spit, and have a thin and narrow rectum and insignificant kidneys, they need gills for this excretory function. He correctly realized that the reason for the small size of the gills in relation to that of the lungs of mammals was their more efficient ventilation, gills being a projection into a channel open at both ends and so carrying a through current. Land invertebrates, such as insects and snails, had organs analogous to lungs, allowing perspiration and, in the latter animals, production of mucous to coat the body. [3]

Lister's ideas on lung function included no reference to the problem of the change from arterial to venous blood. He rejected the works of Lower and Hooke, for the rather inadequate reason that all other authors stated that the blood in the pulmonary vein is similar to that in the other veins; he does not appear to have investigated the matter himself.

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1. Hum. pp. 8-10.

2. Ibid. pp. 11, 16; EA p. 46.

3. Hum. pp. 8-9; EA pp. 45-9.

Lister claimed that Hooke's bellows increased the pressure of the air in the lungs, unlike natural breathing, and the air did not enter the lungs 'in this fashion' (habitum). Lister appears to mean that a living animal draws in air by suction, so that air in the lungs is never at a pressure above that of the atmosphere, whereas the bellows forced air in under pressure. Less valid is his criticism that if the air entered the blood in the lungs, it would not have been necessary to puncture the lung to allow the air to escape. The lungs, being like the skin, an external body covering, are impervious to air, which could therefore only enter the body in the chyle and leave it in secreted humours.[1]

As in his account of the respiratory organs, Lister was concerned with the blood of the lower organisms. His discussion on the changes in the colour of mammalian blood began with a description of snail blood. He observed only a few, very large globules, and saw that the blood consisted essentially of only a fluid part, blue, and remaining fluid when kept in a vessel for many days. He classified animal bloods into three groups:

(1) Warm, red, with globules and fluids; clots on standing. Found in animals with lungs.

(2) Cold, otherwise as in (1). Found in fishes.

(3) Blue or yellow, fluid only, not clotting, cold. Found in exsanguineous animals (invertebrates).

The differences between these types is not caused by heat, as some writers, such as Glisson, have claimed. Oysters from Madeira still have blue blood, and in fishes, the blood, though cold, is still red. Lister also rejected the idea that blood heat was caused by the vigour and difficulty of the circulation. Hedgehogs, whose vascular system was as complex as that of any other tetrapod, had blood which was distinctly cooler than most of these others, and, as Lister had found by dissection,

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1. EA pp. 51-2, 104-5.

was quite cold in winter. [1]

Lister claimed that the difference between arterial and venous blood was caused by the presence of air which had entered the body in the chyle from the gut via the lacteals, 'the only true mouths of the body':

> 'It can be granted that great colour changes can be produced in bodies (corporibus), especially those which are light and fluid, by greater or less amounts of air'. [2]

Some such changes, however, are permanent. These changes can be produced experimentally, as had been shown by Frederick Slare (1647-1727). Slare had put blue vitriol prepared from volatile spirits of urinary salts into a sealed chamber and then pumped out the enclosed air. The blue colour disappeared, to re-appear when air was readmitted. [3]

The reaction probably involved the oxidation and reduction of complex copper ions such as cuprammonium:

$Cu\ (NH_3)_2^{++}$	$\leftrightarrow$	$Cu\ (NH_3)_2^{+}$
Oxidised (Blue)		Reduced (Colourless)

In the blood, such changes of colour were said to be brought about by the heart systole forcing air into closer contact with the blood:

> 'It is certain that the blood of arteries and veins is not at all different in structure, but only in one way, and that is whether or not air is mixed with it. It is not changed by any addition or fermentation, and is therefore not altered in constitution, but only impregnated (incuneatio) with air by the strong and vigorous, though intermittent, pressure, mainly of the left ventricle of the heart, and

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1. Ibid. pp. 95-9.

2. Ibid. p. 99.

3. Ibid. pp. 104-5; Slare, Phil. Trans 15 (1685) pp. 898-908.

also of the arteries. In veins, however, the compressed air expands and disentagles (extricat) itself'. [1]

Slare had believed that his experiment confirmed the idea that the function of the lungs was to supply air to the blood. Lister did admit that a little of this change may be caused in the lung, as this does exert pressure on the blood during expiration; but he believed that the lungs are not essential for the change, and that they were not the source of the air involved. This was shown by the reddening of the blood of fishes, which do not have lungs, and by the foetus, in which the lungs are bypassed. [2]

As mentioned earlier, the function of the lungs and their effect on the blood passing through them remained a matter of doubt for a century after Lister's death, only to be solved by the work of J. Priestley in 1775 and 1786, A. Lavoisier in 1789 and 1790, and M. Magnus in 1837. [3]

(8) Animal Spirits.

The vegetative aspects of animal physiology were, in the seventeenth century, open to a certain amount of direct observation and experiment. The search for an understanding of sensitivity and movement, however, presented even more serious problems, and discussion of this part of physiology was conjectural rather than factual.

The Galenic system involved three kinds of spirits or *pneumata*: natural spirits (*pneuma physikon*), produced in the liver; vital spirits (*pneuma zotikon*), from the heart; and animal spirits (*pneuma psychikon*), made in the brain. It seems that Galen himself did not suggest a distinct natural spirit; this was read into his account of the function of the liver by later writers seeking to correlate Galen's system with the Platonic and Aristotelian vegetative, vital and rational souls. It is also debatable as to whether Galen meant by the word *pneuma* a

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1. EA. p. 100.

2. Ibid. p. 102.

3. G. J. Goodfield, *The Growth of Scientific Physiology* (London, 1961).

material substance or some kind of force; it clearly did not mean 'soul'. [1]

The pneuma psychikon was thought to be produced in the brain by the choroid plexus from vital spirits brought in by the blood, cerebro-spinal fluid being produced as a by-product. The pneuma acted in some way as an intermediary between the structure of the brain and the individual's psychic processes; it eventually seeped out through the skull sutures. Galen was uncertain as to its function in nerve transmission. He thought that the pneuma could move into the solid but porous nerve, either on to the end of a previously empty filament, or increasing the pressure if the nerve were previously full; or perhaps a non-material force could move out into the muscle. [2]

The idea of animal spirits in the Renaissance was so vague that it could have almost any meaning; as Harvey wrote, [3] it was no more than a verbal convenience. In the Cartesian system, however, it came to have a more material meaning. Descartes drew a sharp distinction between material and immaterial, between body and soul, and in this system a material, corpuscular animal spirit provided a means for the mechanical operation of nerves and muscles. In man alone, this mechanism was under the control of the soul. The problem of how an immaterial could influence the movement of the material spirits was, however, philosophically insoluble. Ideas such as that of the 'immaterial substance' of Henry More, acting as intermediary between soul and spirits, could, of course, only be the start of an infinite regression. [4]

The degree to which the spirits were thought of as being material varied considerably in the second half of the century. For Francis Glisson, they were the counterpart of the Paracelsian element of mercury; [5]

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1. Siegal pp. 184-8.

2. Ibid. pp. 114-25, 192-5.

3. Harvey 1649 in Jackson p. 149.

4. H. More, An Antidote against Atheism (1653) in A Collection of several Philosophical Writings (London 1662) p. 40.

5. F. Glisson 1654, chapters 8, 39.

to Henry Power, part of a series of very volatile and aetherial matters responsible for fermentation, growth and movement in minerals, plants and animals.[1] The influential Thomas Willis had a more restrictedly material and corpuscular idea of the animal spirits as a liquid full of volatile salts, produced in the brain by distillation from the blood. This assemblage of spirits was equated with the sensitive soul of animals and man, as distinct from the uniquely human rational soul. Movement of these subtle particles to and from the central nervous system could be so fast as to give a rebound or reflex action; they controlled the movement of local spirits from the tendinous parts of muscles into the muscle fibres, where their effervescence caused a contraction of the muscle.[2]

John Mayow attempted an explanation as to how the muscles produced their effects:

> 'There can be no doubt that the influx of animal spirits is necessary for the performance of the motive function, inasmuch as if a nerve is cut or obstructed, the muscle to which it is distributed refuses to contract.'

Mayow related the effect of the nerve to the increased blood flow to the muscle by supposing that the blood brings in a supply of saline-sulphureous particles. These react with nitro-aerial particles, derived from the air, and 'exalted and put in a condition of the highest vigour' by the brain and brought into the muscle by the nerves. The resulting effervescence causes the muscle to contract in length. The chemistry involved is the same as that occurring, more slowly because of lower concentrations, during growth and fermentation in the rest of the body.

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1. Power 1664 pp. 61-77.

2. T. Willis, Diatribae duae Medico-Philosophicae, preface; De anima brutorum (Oxford 1672); De motu musculari (London 1654); all in Pordage.

'From these things I conclude that it is to some extent made out that nitro-aerial particles, transmitted by means of respiration to the mass of the blood and thence to the brain, are the animal spirits themselves'. [1]

It was, then, a widely-held view in the mid-seventeenth century that the animal spirits existed as material particles which moved along nerves to produce a contraction in a muscle or a sensation in the brain; there was, however, dispute as to the nature of the spirits and their mode of action.

Lister did not share this view:

'There remains one other humour, the animal spirits, the subtlest of all; credited with an infinite number of properties, and not merely the bringing about of sense and motion. For solution of any difficult problem, they bring in this deus ex machina and take refuge in this animal spirits carried about by the nerves'. [2]

Lister's objection to the idea was based on the structure of the nerve. Modern authors, he wrote, claimed that the nerves had an imperceptibly narrow cavity, allowing them to carry the fluid animal spirits. Willis had even described two kinds of such spirits, those controlling voluntary and involuntary motion, made in the cerebrum and cerebellum respectively; and Cole gave details of the structure of the pores by which the spirits left the nerves. Others, such as Borelli, suggested a porous structure like elder pith rather than a continuous lumen. Many properties were ascribed to the spirits themselves: they were sulphureous, saline, acid, soft, volatile, elastic, placid, vibrating, fermenting, and so on. All this, Lister wrote, was mere supposition with no foundation in fact. Nerves are quite plainly solid. He agreed with

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1. Mayow op. cit. pp. 239-255.

2. Hum. p. 451.

Diemerbroek that no microscope could demonstrate any kind of cavity in a nerve. With this, the whole hypothesis collapsed, and with it many other medical fictions stemming from this cornucopia. Galen, Hippocrates, Harvey, Ent and Steno were quoted in support of this attitude.

Willis and Cole were particularly attacked, since they gave details of the movement of spirits in and out of the nerves without explaining any control mechanisms. Willis's rebound theory meant that the particles must have mass (pondus), which conflicted with some of their other properties.

A common argument for spirit transmission in nerves was the paralysing effect on a muscle of a ligature on its nerve; but, argued Lister, nerves are invariably accompanied by arteries or arterioles, which are also affected by the ligature, and it was easy to show that blocking an artery (though not a vein) also paralysed a muscle served by the vessel. The assumption here is that the artery in the nerve supplies the muscle and not the nerve itself, and no evidence was given for this. Lister believed that another likely effect of the ligature was to destroy the natural tension of the nerve (tensio naturalis tollitur). He admitted that to reject animal spirits was to leave a serious gap in the theory of the animal economy; however, this would have to be left to posterity. To accept false ideas as a mere convenience was to encourage a self-satisfied inactivity.[1]

(9) The value of Lister's physiological work.

Lister was rather an isolated figure in seventeenth century physiology, with a peculiar blend of conservatism and scepticism and an inability to understand or sympathize with some of the main trends of the period. Superficially his work often reads like that of Haller, as, for

1. Ibid. pp. 450-458.

example, on digestion and the function of the lungs. This is because, while Haller rejected the more extreme chemical and mechanical explanations of the late seventeenth and early eighteenth centuries, Lister retained many of the attitudes of an age before these views became widespread. Though driven by want of an alternative into accepting a mechanical explanation of secretion, he was otherwise extremely hostile to such ideas. He confessed himself unable to follow the very elementary mathematics of Pitcairne's work, and rejected the Democritean atomic theory out of hand. Though Lister nowhere states this explicitly, it seems that his idea of the humours was Galenic in its concentration on the qualities of the non-particulate fluids, even if he no longer accepted the simple four-humour theory. He appears to have thought that chemical explanations were more sympathetic to this approach than were mechanical; but, though he sometimes used chemical analogies and thought of the growth of teeth and bones as a form of crystallization, it is difficult to see his account of the effect of air on the blood as a description of a chemical reaction. Lister's theory of digestion was reactionary in this: though the idea of fermentation did at least point the way to further research, Lister's use of putrefaction as an explanation was a retreat into mere verbalism. On the other hand, his rejection of black bile was progressive and his classification of humours was acceptable.

Lister's work in physiology was in general speculative, and shows evidence of real practical work only in those topics on which he worked early in life, as with the lymphatics. As he grew older, he relied more and more on the accumulation of authorities, as shown by his rejection of Lower's work. He retained some scepticism, and could be radical at times, as with his realization that the gut wall is an outer surface and in his frequent use of lower animals. However, his theory of lung function shows that he could be very rash at times, and he was in general out of date even in the 1680's; by 1710 he was quite out of touch. This is presumably the reason that Lister's physiological works were almost completely ignored by his contemporaries and by later writers. Pitcairne, who was on good terms with Lister, made a passing reference to his ideas on the gut, and John Quincy, in his translation of Sanctorius in 1720, mentioned Lister's commentary on the same work - but only to say that it was even harder to understand than the original.[1] Otherwise, Lister's books on the subject do not appear to have been cited at the time, and no mention is made of him in the standard modern histories of physiology by Foster and Rothschuh.[2]

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1. J. Quincy, Medicina statica, translated into English, London, 1712.

2. Foster, op. cit; K. Rothschuh, A History of Physiology (New York, 1973); translation of German original Geschichte der Physiologie, (Stuttgart, 1953).

Chapter VII Miscellaneous biological topics.

(1) General natural history.

When John Ray wrote his first letter to Lister, after their return from France in 1667, he mentioned with apparent surprise that

> 'I was much pleased to understand that you do not confine your studies and enquiries to Phytology, but take in Zoology and the whole latitude of Natural History'. [1]

Presumably the two had concentrated mainly on botany during their stay at Montpellier. However, Lister's interest in plants did not last more than a few years. At Burwell and in Craven in the late 'sixties he collected enthusiatically, compiling a species list for the Burwell area, [2] and sending new records to Ray, who acknowledged these in the preface to his Historia plantarum.[3] Perhaps the best-known of Lister's records was that of Valeriana graeca (Poloemonium caeruleum L., Jacob's ladder) at Malham Cove. This has sometimes been attributed to Ray, [4] but Ray's description of the site is almost verbatim from Lister:

> '... Malham Coze [sic], a place so remarkable that it is one of the wonders of Craven. It grows there on both sides of the spring in great tufts'. [5]

Ray recorded it only on the left side of the cove; it now grows only on the right. [6]

Ray's confidence in Lister's botanical abilities was so great that he sent Lister the proofs of the English catalogue for revision. [7]

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1. CJR p. 13.
2. MSS Lister 39, ff. 370-412.
3. John Ray, Historia Plantarum (London, 1686).
4. F.A. Lees, The Flora of West Yorkshire (London, 1888) p. 328.
5. CJR p. 57; letter from Lister to Ray of 4 June 1670.
6. J. Ray, Catalogus plantarum Angliae et insularum adjacentum, second edition (London, 1677) p. 299. (First edition, London, 1670).
7. CJR pp. 43, 47, 50; FCJR p. 123.

However, Lister's interests in botany were now moving from collecting to anatomy and physiology, and he had little further to say on his field work.

In zoology, his main interests were always in spiders, molluscs and insects, discussed elsewhere; but he also had early interests in birds and fishes. During his stay at Nottingham in the winter of 1670, Lister compiled accounts of birds seen, though, being without textbooks, he could not name them. [1] His correspondence often refers to birds; he received accounts of the birds of Barbados from Thomas Townes and identified specimens for Ray, and his account of 'heath throstles' (ring ouzels) in Craven show that he made field observations. [2] Some of Lister's observations, included in the preface to Ray's edition of Willughby's Ornithology, show that he had made detailed observations of the feeding habits of birds; thus, he knew that robins will eat smooth but not hairy caterpillars, and that buntings will hull oat grains but do not crush them. He corrected Ray's idea that the jack snipe (Lymnocryptes minimus) is the male of the common snipe (Gallinago gallinago), as dissection showed that both male and female jack snipes existed. [3]

Lister also published a brief paper on birds in which he commented on the migration of shore larks and dotterels. [4] The idea of bird migration was not universally accepted among naturalists even a century after this paper; [5] Lister believed that it was caused by a shortage of food in the breeding area, and he opened a number of birds' crops to try

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1. MSS Lister 5 f. 113
2. CJR pp. 54, 116, 124.
3. CJR p. 116; F. Willughbeii, Ornithologiae libri tres (London, 1676); English translation, The Ornithology of Francis Willughby (London, 1678).
4. Phil. Trans. 15 (1685) pp. 1159-61.
5. See Gilbert White, The Natural History of Selborne (London, 1789 and later editions); Letters 10, 13 and 23 to Thomas Pennant.

to find evidence for this. The paper also included notes on waxwings seen at York in 1680 and on five genera of wild geese; these were the brent, barnacle, grey-lag and probably pink-footed and bean geese.

In spite of the limited nature of Lister's published work on birds, his reputation forty years later was high enough for James Petiver and Hans Sloane to suggest that he should revise Ray's edition of Willughby or write a new catalogue. [1]

It was presumably because of his reputation that Lister was appointed by the Royal Society to supervise the publication of Willughby's History of Fishes, edited by Ray, in 1685. He contributed the description of Raia radiata, gave a classification of Orbes (porcupine fishes) and made minor suggestions for the text. [2] Ray had believed that Lister had discovered a new species in the rudd (Scardinus erythophthalmus), but in fact Baltner had previously described it in Holland. [3]

(2) Plant anatomy and physiology.

For a period from 1669, Lister was involved in an inconclusive investigation into the movement of sap in plants. This work also involved Ray, Willughby, and Israel (or Ezerel) Tonge (1621-80), John Beal (1603-83), Richard Reed and John Wallis (1616-1703), and was communicated by them to Oldenburg, who published the work in the Philosophical Transactions in 1670 and 1671. [4] It was believed possible that the sap in plants circulated in a manner similar to that of blood in animals.

Ray and Willughby had noted that a frosty night caused a temporary increase in the rate of 'bleeding' from a wound in a tree. [5] Lister

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1. MSS Sloane 1064 f. 206; MSS Lister 37 f. 171.

2. FCJR p. 149.

3. FCJR pp. 144, 146.

4. Phil. Trans 3 pp. 853, 877; 4 (1669) pp. 913, 963-4; 5 (1670) pp. 1165, 1199, 2070, 2072.

5. Phil. Trans 5 (1670) p. 1200.

confirmed this, having made over forty trials in the winter of 1669-70 at Nottingham on sycamores. This bleeding occurred only at new wounds, but old wounds did bleed when the frost cleared. However, the following winter in York he could not repeat the experiment successfully, though Ray in Warwickshire was able to. Lister speculated that differences in soil might cause irregularities, and that the sycamore (and walnut, which he also used) were foreign trees and so perhaps had 'an injurie done to their natures from an unkind Climate'.

Lister also found that sap flowed more freely from branches cut in frosty weather when they were brought inside, close to the fire, and here he used a wide range of species - sycamore, walnut, maple, willow, hazel, cherry, woodbine, bladdernut, vine, elder, barberry, apple, ivy, quicken-tree (rowan) and eggberry (bird cherry?). Raspberry and briars gave little bleeding, and ash none at all. At first Lister claimed that the sap ran more quickly from a branch if the tip were cut off, but after Willughby's disputing this, he admitted that the effect was probably the result of rough handling.[1]

Experiments of this nature suggested the existence of a circulatory system in plants, and so helped to support the tendency of the period to reduce the importance of the differences between animals and plants and to stress their similarities. The classical and renaissance view of nature saw a difference in nature between plants, with only a vegetative soul, and animals, which had also a sensitive soul. In the late seventeenth century this division was blurred by attempts to find animal-like properties and structures in plants as a whole, and not merely in a few intermediates such as zoophytes. Henry Oldenburg, for example, described Malpighi's work as showing plants to have '... not only...veins, but Arteries, Trachea's, lungs, Peristaltick motion, Uterus and what not'.[2]

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1. Phil. Trans. 6 (1671) pp. 2069, 2123, 2121, 2125-6.

2. MSS Lister 34 f. 12; C. Webster, 'The Discovery of Plant Sensitivity by English Botanists in the Seventeenth Century'; Isis 57 (1966) p. 5.

Lister suspected that a circulatory system existed in plants, as vessels analogous to veins could clearly be seen, and these were often full of a distinctively coloured liquid, as in greater celendine (Chelidonium majus). Cutting the stem often caused 'springing' of the sap in these vessels. On the other hand, he could find no evidence of a one-way movement of sap by ligaturing the stem of spurge; both sides of the band swelled equally. Nevertheless, careful skinning of a plant and careful removal of some of the parenchyma caused no bleeding, which 'made against the general opinion of one only sap loosely pervading the whole plant, like water in a sponge'. He tried to dissect out the veins, beginning at the leaf. He saw that in the leaf and stem, the veins were always accompanied by fibres, and did not vary much in thickness throughout the plant; instead of running together as tributaries, the small veins ran parallel but separately down to the roots, where their arrangement became more complex. Though the microscope could not show details of the walls of the veins, they must be membranaceous and non-rigid, as, when the accompanying fibres were removed, the vessels would stretch by one third. The juice in the veins remained fluid when the liquid in the rest of the plant was frozen; this, according to Lister, was because the venous fluid contained a higher degree of fermentation: 'that is, the plant owes its life to it'. [1] (Later in life, Lister was to reject the idea of fermentation). All these observations led Lister to the idea that plant veins contained a special fluid, distinct from the general tissue sap, moved about the plant under pressure and essential for its life processes. Though the mechanism for the circulation of this fluid was not obvious, it must exist, as, like the blood of an animal, the sap would clot without constant movement; Lister had prepared several pastilles of these clotted juices. On a theoretical level, he believed that the roots of the

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1. Phil. Trans. 6 (1671) p. 2119.

plant were, like the lacteals of an animal, the 'true mouths', through which food was taken, the plant being like an animal with the guts turned outward. [1]

Lister could, therefore '...hope...the Analogie betwixt Plants and Animals be in all things else, as well as the motion of their juice, fully cleared'; after all, there was even evidence of 'Acts of Sense' in plants, as in the movements of some flowers. [2] John Wallis later suggested that the fasicular arrangement of vessels in plants, demonstrated by Lister, was more like that of nerves in animals than of blood vessels, which join and divide; they may be responsible for the acts of sense noted by Lister, rather than for circulation. [3]

Other authors were working in the field of plant anatomy at the same time and producing more comprehensive accounts. The books by Marcello Malpighi and Nehemiah Grew [4] reached the Royal Society at more or less the same time in October 1671. Malpighi's manuscript was sent on to Lister after he had read Grew's book. Lister, who claimed to have worked on plant anatomy for several years previously, found Malpighi's work the more acceptable of the two, as it confirmed his ideas on plant veins.[5] Grew had believed that plant tissues were fibrous structures with many large pores, and, according to Lister, he made little or no mention of vessels. Lister pointed out that the vessels could easily be seen, both in transverse section and by dissecting them along the length of the stem; and on theoretical grounds such vessels must exist, as otherwise the undoubted rise of sap in the stem could not be

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1. MSS Lister 5 f. 120.
2. Phil. Trans. 7 (1672) pp. 5132-7; Webster 1966 loc. cit.
3. Phil. Trans. 8 (1673) p. 6060.
4. N. Grew, The Anatomy of Vegetables begun (London, 1672); M. Malpighi, Anatome plantarum (London, 1675).
5. Royal Society MSS L 5, n. 44.

directed. They occur universally in plants (Lister mentioned fifteen species by name) and so must fill a fundamental need. The fatal effect of ring-barking showed that these veins must carry a succus nutritus. [1]

Lister promised further work on the subject, as his notes contained many details on the 'Position, Order, Number, Capacity, Distributions, Differences, Figure etc. of these Veines'. Together with the writings of Grew and Malpighi, this would, according to Oldenburg, mean that, within this season,

> 'the Doctrine and Philosophy of ye vastest part of ye Sublunary World, wch relateth to Vegetables, is like to be satisfactorily stated'. [2]

However, Lister's promises in this direction were not fulfilled, and he became involved in a dispute with Grew, conducted via Oldenburg, on the merits of Grew's book. These papers were not published, but the manuscripts of Grew's three replies to Lister's criticisms survive. [3] These papers contain twelve large sheets, finely written, with no significant botanical material. Grew denied that he had rejected the idea of plant veins, and he dismissed as insignificant Lister's objection that the pith is not always completely filled with 'bubbles'; but otherwise the dispute was apparently conducted on both sides on mere debating points. It was disputed as to whether 'not always' and 'often' had the same meaning; whether 'leaf' and phyll were exact equivelants; whether or not 'more oily' meant 'containing abundant oil'; and Grew objected to Lister's repeated use of 'possibly' and 'probably'. Grew appeared to feel himself the injured party in this exchange of

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1. Phil. Trans 6 (1671) pp. 3052-5.
2. MSS Lister 34 f. 12.
3. MSS Sloane 1929 ff. 7-8: MSS Lister 34, ff.57, 90.

trivia, brought about by Lister's jealousy; it was

> 'as if men wrote, not with Ink, but Aqua Fortis... as if their Pens were nothing else but arrows, nor yr words but bullets: yt to be a Philosopher were only to be a good Fencer and ye seat of ye Muses were but a Martial Field ... how odious a refutement I have of a contentious person, and how very loath I am to be accounted such'. [1]

Nevertheless, the later correspondence between Lister and Grew during the latter's secretaryship of the Royal Society was civil enough, and they were both members of the informal Temple coffee-house botanical 'club'. However, Lister's interest in botany seems to have ceased abruptly, and it may be that the thought that he had done significant work in this subject but that he had been forstalled by Grew and left the field in disappointment.

He did publish, many years later, a long and rambling paper on the juices of plants, [2] but this had been written thirty years earlier, ie. about 1666. This paper attempted some form of classification of plant juices, based on Lister's investigation of some seventy species. After stressing that several juices may be found in one plant, he distinguished between watery juices which coagulate rather like blood or curds and whey, such as that of wild lettuce; watery juices solidifying entire, forming a cake, as in thistles and onion; gums, or sticky juices remaining fluid for some time, as in rhubarb; non-sticky oils, as in olive; and sticky juices quickly setting into a rosin, as in ivy. He also distinguished between fermentable and nonfermentable juices. His

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1. MSS Sloane 1929 f. 8v.

2. Phil. Trans.19 (1696) p. 365.

treatment of the subject is discursive, unlike his usual formal dichotamous classifications.

At some time, Lister must have paid attention to the composition of plant juices, as in 1685 he mentioned at a meeting of the Royal Society that he, like Leeuwenhoek, had found the 'figures' (crystals) of common salt and alum among the salts of wormwood and some other plants; [1] however, there is no other record of this work in printed or manuscript form.

(3) Economic Biology.

Lister's writings in the early 'seventies show some interest in the possible commercial applications of natural history; this was, of course, a part of the Baconian outlook used in justification of the activities of the Royal Society.

Dyestuffs were, before the nineteenth century, mainly of vegetable, or occassionally animal, origin. Though living things show a wide range of often brilliant colours, relatively few permanent dyes could be made from them, and these were usually rather drab. Any addition to the narrow range of permanent bright dyestuffs then available would have been commercially very valuable.

Sir Thomas Browne, Henry Power, Robert Boyle, Richard Reed and Christopher Merret had all investigated the effects of various salts on plant and animal colours, and Ray published work of a similar nature sent to him by Edward Hulse (1636-1711) and Samuel Fisher of Sheffield. These experiments were generally concerned with the effects of acidic or alkaline materials on pigments acting as indicators. [2] Lister wrote a similar paper, showing

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1.. Birch 4 p. 383; Phil. Trans. 15 (1685) pp. 1073-89.

2. A. G. Debus, 'Sir Thomas Browne and the Study of Colour Indicators' Ambix 10 (1962) pp. 29-36; Power 1664 pp. 71-77; Robert Boyle, Experiments and Considerations Touching Colours (London, 1664); R. Reed, Phil. Trans. 6 (1671) pp. 2132-5; Christopher Merret, Pinax rerum naturalium brittaniarum (London, 1666); Phil. Trans. 6 (1671) p. 2063

'two things I conceive are chiefly aimed at in this Inquiry of Colours, ye one to encrease ye Materia Tinctoria and ye other to fix, if possible, those colours we either have or shall hereafter discover for use'. [1]

– the fixing of pigments being, of course, the main difficulty in their use. Lister distinguished between apparent plant colours, as in the flowers, and latent colours, needing treatment by salts to bring them out. He discussed a number of colour changes in plant materials, concluding that red, blue and white flowers usually become green or yellow with alkaline salts, while yellows remained unchanged. Some plant colours produced by treatment with lye, as the red from Lactuca sylvestris (wall lettuce, now Lactuca muralis), and the purple from Catapputia minor (skull-cap, now Scutellaria minor) changed to yellow on standing; others, such as the green produced by the action of lye on woad, and the purple from the same plant with oil of vitriol, were permanent. Lister admitted, however, that none of these colours would strike with cloth. He also investigated some animal colours, finding a red colour from the juices of caterpillars, beetles and ants with lye. He claimed, against the general opinion, that cochineal was an animal product. [2]

One much desired colour was a rich permanent black, suitable for use as an ink in place of the usual brownish fluid made from oak galls and alum. Lister claimed in 1671 to have found such a pigment 'comparable to ye best Inke even for ye Use of ye pen & wch will not change either by fire or salts'. The plant from which it was derived was easy to grow, but the colour was difficult to keep liquid. It caused some interest at

1. Phil. Trans. 6 (1671) p. 2132.

2. Phil. Trans. 6 (1671) p. 2132; Ibid 7 (1672) p. 5059.

the Royal Society, and Oldenburg repeatedly asked Lister to send a sample or to reveal the secret. Lister promised to send a sample as soon as the season permitted, but only a few grains of the resinous substance were produced, and Lister begged to be excused from naming it:

> 'Yet I beseech you not to look upon me, as one yt delights to treasure up secrets; methinks I would have all the world free and communicative in their notions and inventions, yt soe we might hasten and participate, if possible, even with posteritie. 'Tis an argument of a disingenuous Spirit, to be secret, or at least a weake stock in ym yt shall seeke to uphold his fame thereby'.

However, Lister was clearly thinking of the commercial value of his discovery, and still did not reveal the name of the plant. He did send a small sample of the colour, and this was divided between Boyle and Prince Rupert on 16 November, but Boyle found the sample too small to test, and Prince Rupert made no report on it. No more was heard of the resin. [1]

A further example of Lister's early interest in applied natural history was his paper on plants suggested as possible hay crops. [2] His account, which included comments on the value of liming and claying farm land and notes on his own cultivation of asparagus and lettuce, show him to have been quite observant in his travels in Yorkshire. He recommended a variety of leguminous plants, particularly for use on dry, sandy soils; and he thought that perennials would in the end make up for

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1. Phil. Trans. 6 (1671) p. 2136; Royal Society MSS L 5, n. 41; MSS Lister 34 ff. 9, 11; Phil. Trans. 6 (1671) pp. 2126-28; Royal Society MSS L 5, n. 39; MSS Lister 34 ff. 43, 12.

2. Phil. Trans.19 (1696) p. 412 (Described as 'a paper of my youth').

their toughness and slower growth rate by their lower labour needs as compared with annuals. Furthermore, it was possible that the plants may improve in these respects under cultivation. The species recommended were

Vicia multiflora nemorensis perennis	(Tufted vetch, *Vicia cracca*)
Vicia sylvatica semine rotundo nigro	(Commonvetch, *V. sativa*)
Vicia sylvatica multiflora maxima	(Wood vetch, *V. sylvatica*)
Astragalus sylvaticus	(Milk vetch, *A. hypoglottis*)
Orobus sylvaticus	(Tuberous bitter vetch, *Lathyrus montanus*)
Lathyrus major latifolius	(Everlasting pea, *L. latifolius*)
Lathyrus luteus sylvestris	(Meadow vetchling, *L. pratensis*)

These were all native species, and so thought by Lister to be likely to fare better under cultivation in England than would cultivated foreign species.

Chapter VIII Summary.

At his death, Martin Lister was remembered by the public chiefly as a physician. Elkanah Settle devoted most of his memorial poem Threnodia apollinaris [1] to this part of Lister's life:

'Thus Lister's Monumental Name shall stand,
Health's Patriot Champion down to Time's last Sand.'

Indeed, Lister himself would probably not have claimed that his scientific work was the central part of his life. In spite of his extensive correspondence (most of which is of slight scientific importance) he was very much an individual worker, except for the two years after his move to London. Perhaps because of this ~~inclination~~ isolation, he does not fit easily into the generally accepted picture of seventeenth century science.

The conventional view of the development of science in the seventeenth century sees it as the replacement of an orthodox scholastic Aristotelianism by a rational experimentally-based mechanical philosophy, centred largely outside the universities and built round the organized collection of data advocated by Francis Bacon, the craftsmanship and selective experimentation of Galileo, and the clear reasoning of Descartes.

As with most conventional views, this is an oversimplification; it overlooks, for example, the wide range of ideas covered by the phrase 'scholastic Aristotelianism', and in particular it neglects the value of the refined Paduan Aristotelianism, as seen, for example, in the works of William Harvey. This picture also ignores the importance of occult elements and renaissance neoplatonism and natural magic in the intellectual world displaced by the mechanical philosophy. Furthermore, it ignores the variety in the outlooks of the mechanical philosophers themselves, notably the differences between Baconians and Cartesians, and the real elements of Aristotelianism surviving in late seventeenth century

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1. London 1712.

science.[1]

Nevertheless, it remains true that constant acknowledgement was made to Bacon as the figurehead of the new philosophy, and this not only in England; that this new philosophy did to a considerable extent rely on the collection of new data by the use of experiment and that an increasingly rigorous and mathematical logic was being applied to the creation of explanations for these new facts. The result was a general desire among philosophers for a mechanical world of cause and effect from which final causes and supernatural agencies were being excluded. This understanding of the world was to be reached by an organized co-operative effort and was to be used for the general benefit of mankind.

Lister cannot easily be fitted into this pattern. Certainly he accepted the need for careful observation, and at least in his earlier work frequently acknowledged the lead given in this by Bacon. He stressed the value of even the smallest and meanest of observations and the infallibility of sense evidence, and was prepared to criticize Bacon for not valuing plain histories:

> 'without doubt, the greater number we have of particular histories, the plentifuller and clearer light we may expect from them. For my part, I think it absolutely necessary that an exact and minute distinction of things precede our learning by particular experiment ... all these [experiments, knowledge of virtues, applied science]

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1. Debus, A. G. 'Paracelsus and the Puritan Revolution,' Ambix II (1963) pp. 24-32; Schmitt, C. B. 'Towards a reassessment of Renaissance Aristotelianism', History of Science II (1973) pp. 159-193; Pagel, W. 'The reaction to Aristotle in seventeenth-century biological thought', Science, Medicine and History I (edited E. A. Underwood, London, 1953), pp. 489-509; Randall, J. H. 'The development of scientific method in the school of Padua', Journal of the History of Ideas I (1940) pp. 177-206; Yates, F. E. 'The Hermetic tradition in Renaissance Neoplatonism', in Art, Science and History in the Renaissance (edited C. S. Singleton), Baltimore, 1967.

are things subsequent to natural history'. [1]

In this, Lister at least lived up to his ideals. A man who would burn off the hairs on a spider's head with a burning glass in order to examine the eye arrangement, who could distinguish between the tapeworms of dogs and mice, and who could dissect chrysalids to search for butterfly eggs cannot be accused of lack of attention to detail. This capacity for hard work and his enthusiasm were perhaps Lister's main assets as a naturalist, and when applied to spiders produced results of outstanding quality.

It would be more correct to see Lister's concern with detail as an obsession which in some cases prevented him from seeing the truth which a larger and less meticulous view might have allowed. This is well seen in his theory of fossil formation, in which his attention to specific minutiae led him into an intellectual cul-de-sac. In the same way, it is difficult to see as anything other than unimaginative his objection to the idea that spermatozoa grow into a foetus while lodged inside the egg: Lister claimed that this would result in the accumulation of excreta, which was observably not the case.

Later in life, Lister was prepared to reverse his position and criticise Bacon for being over-concerned with the simple acumulation and ordering of facts: active experimentation was also needed to achieve a true understanding. [2] In this, however, there was a considerable gap between Lister's principles and his practice. He did leave accounts of some minor and unoriginal experiments, notably on the lacteals, and he investigated the behaviour and food preferences of spiders and molluscs; but Lister appears to have taken the meaning of 'experiment' in his field as nothing more than dissection. This, of course, is valuable and necessary in itself, but it lacks the element of control needed for

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1. CJR p. 48; EAT pp. (iii)-(iv); MAA p. (ii).

2. EA p. 4.

experiment, and would now be seen as refined observation. Furthermore, Lister's understanding and interpretation of experimental situations was often unsound, as seen in his misunderstanding of the effects of ligaturing the nerve supply to a muscle; and his objection to Hooke's experiment on the ventilation of the dog's lungs does not suggest any profound mechanical imagination. Even in dissection, a field in which he was ahead of many of his contemporaries, he did not realize the effect of his technique in producing different ideas on the circulatory systems of snails and slugs.

Though Lister used a microscope to examine blood cells of animals and to search for the brain of snails, he was in general reluctant to rely upon an instrument which, by making the minutest animal equal to the greatest and finest, led to contempt for the ancients - surely an odd sentiment for a Baconian entomologist. [1] His neglect of this instrument is surprising in view of the use made of it by Hooke, Grew, and Malpighi - men with whose work Lister was in constant touch - and the improvements in the instrument made by Richard Reeves and Christopher Cocks.

In spite of these important deficiences in experimentation, Lister certainly shared with many of his contemporaries a nominally Baconian concern for detail and observation. However, in his attitude to the use of reason and logic in building a theoretical world picture, such as that of the Cartesians, he took up an extreme Baconian position.

It is in fact difficult to trace any positive influence on Lister of such comprehensive theorizing, even though he might be expected to have been exposed as a student to the most influential Cartesian movement in England - that of the Cambridge Platonists under Henry More (1614-87) and Ralph Cudworth (1617-88). [2] Nor can he be seen to have reacted

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1. EAT p. 107.

2. Nicolson, M. 'The early stages of Cartesianism in England', Studies in Philology 26 (1929) pp. 356-514.

positively against any such influence in his early years. In his first writings, Lister was prepared to accept such a theoretical concept as that of the ferments, and he actively searched for similarities between animals and plants. In later years, however, he moved towards the position of Robert Boyle, who wished that

> 'men would forebear to establish any theory, till they have consulted with a considerable number of experiments, in proportion to the comprehensiveness of the theory to be erected on them'. [1]

Lister took this attitude further than most of the other prominent natural philosophers of his day. In the writings of his middle and later years, he rejected most of the theoretical concepts used in the explanations of many of his contemporaries - animal spirits, ferments, nitro-aerial salts, geological catastrophe, and even material corpuscles. In his view, the correct function of the natural philosopher at that time was to accumulate knowledge for the benefit of posterity; attempts at explanation were premature: many were 'calling for birth, when conception had not yet occurred', and

> 'the World at present is intoxicated with Subterraneous hypotheses, as well as with Superlunary ones, and attractions at immense distances'. [2]

By 1709 his condemnations of theorizing had become intemperate, and were extended even to such work as that of Hooke and Lower on respiration. In his insistence on the need for observation unsullied by the intervention of the Idol of the Tribe, Lister would have agreed with Bacon that

> '...the mind of man is far from the nature of a clear and equal glass ... nay, it is rather like an enchanted glass,

1. Robert Boyle, Certain Physiological Essays, (London, 1661) p. 9.
2. MSS Lister 37 f. 129; see also Hum. preface, EAA p. 7.

full of superstition and imposture ... let us consider the false appearances that are imposed upon us by the general nature of the mind ... the spirit of man, being of an equal and uniform substance, doth usually suppose and feign in nature a greater equality and uniformity than is in the truth'. [1]

Lister's comments on mechanical models in physiology, quoted above from the preface to De humoribus, show similar, if more extravagant, sentiments.

Nevertheless, Lister was not free from the fault he so vigorously attacked in others. Though he accepted the unproven hypothesis of a petrifying juice, he did attempt, unsuccessfully, to demonstrate its existence in animals; and his one clear use of reason in a scientific controversy - the reductio ad absurdum of the double maturation of the spermatozoa - was logical enough, and is invalid only because of the lack of a cell theory at the time. However, some of his other speculations were rather wild and ill-founded, as with the nature of goose-barnacles and the function of the lungs; and at times his ideas verged on the ludicrous. He suggested, for example, that the trade winds may be produced by the loss of vapour from plants which were turning to follow the sun's daily track through the heavens. [2] Lister appears to have been inclined to make a virtue of his lack of imagination in philosophical explanations, and the resulting lack of sound, controlled speculation lead to wild extravagancies on the few occasions when he did attempt to give explanations.

The natural philosophy associated with Boyle, Hooke and Newton saw the world as a mechanical system made up of particles interacting by natural forces strictly according to cause and effect - the causes being

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1. Francis Bacon, The Advancement of Learning, London, 1605; edition used, London, 1915, pp. 132-3.

2. Phil. Trans. 14 (1684) p. 494.

material and efficient and the interactions being at least potentially capable of mechanical analysis. It is perhaps here that Lister's Baconianism was weakest. He rejected atomism out of hand, and his conception of the humours of the body, though nowhere precisely stated, appears to be as easily reconciled with a Galenic qualitative approach as with mechanical and chemical ideas. Even the mildly chemical explanation of fossil origins given by Lister is much less thorough than those of Plot and Beaumont. In all his writings, Lister appears never to have made a mathematical calculation. To do so would, in his view, have been to 'leave the beaten road of Nature' [1] and he claimed the authority of Bacon in this:

> 'Lord Bacon says right, yt Mathematicks are but an other form of Logic, and not at all inventive of Philosophie, they must be behouden to the Drogerie of Experiments and Observation, and they can make nothing of it'. [2]

Furthermore Lister by no means rejected the authority of the classical writers. Though critical of Aristotle in his early letters to Ray, he was as early as 1671 saying that 'we may argue with Aristotle that ... nature does not make such in vain, [3] this tacit admission of final clauses being later made explicit:

Finales tamen causas libenter admitto, cum natura nihil frustra faciat. [4]

Lister's 'Baconianism' appears from this analysis to have been very imperfect and not to have fitted what has until recently been the conventional idea of the Royal Society mechanical philosopher. However, study of the seventeenth-century volumes of the Philosophical Transactions shows clearly that Lister's contributions, though exceptionally

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1. MSS Smith 64 f. III.
2. MSS Smith 51 f. 57.
3. Phil. Trans. 6 (1671) p. 2254.
4. EA p. 143.

voluminous, were in fact quite typical of the general run of papers published; in his moderate intellectual qualities and often confused and ambiguous overall attitude, Lister appears to have been a more typical member of the Royal Society than were the handful of better-known men whose work might give a misleading impression of intellectual rigour to the general work of the Society.

Where Lister was untypical of most of his contemporaries was in his enthusiasm and hard work. Though he lacked the staying power of John Ray, and though his work was spasmodic and liable to change direction every few years, his capacity for quite intensive work on, successively, spiders, mollusc taxonomy and mollusc anatomy was impressive by the standards of the time.

An essential part of the Baconian outlook was the organisation of science into a co-operative effort to harness knowledge for the general good and for the relief of man's estate. In his early years, Lister did show some concern for improvements in technology and agriculture, though self interest was perhaps never far from his mind and was sometimes made explicit. His membership of the Royal Society was enthusiastic enough - so long as he was a corresponding member at York. Within two years of his move to London, however, he ceased active participation, even though he had in that period occupied the position of Vice-president. As he had similar personal disagreements in the College of Physicians, again after reaching a position of influence, it seems likely that personality traits made it difficult for him to fit into such groups of men. These experiences were the causes of the bitter comments to Lhwyd and Smith quoted above. Lister certainly believed the fault to be that of others, and agreed with Tancred Robinson that

> 'The R. Society may have great men in their Number, but alass, very little Souls and narrow Minds'. [1]

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1. MSS Lister 37 f. 151.

It must be remembered, however, that the period around 1690 saw the Royal Society at its lowest ebb. There were severe financial problems, the Philosophical Transactions could not be published regularly, and the membership was down to barely 100, only one-third of whom were men of science. Meetings were often hours late in starting, in the hope of a larger attendance, and until Sloane's election as secretary in 1694, the officers were frequently changed and often unsatisfactory or inactive; at Council meetings only six or seven of the twenty-one members would normally attend. [1] There was often much personal conflict between the members; in 1700, Tancred Robinson reported to Lister that quarrels between Cowper, Bidloo, Woodward, Harris, Sloane, Petiver and Plukenet threatened the destruction of the Society. [2] It is understandable that a man of Lister's personality would prefer to detach himself from such a body of men.

On an individual level, Lister built up a very wide circle of correspondents, but of these, only Ray and Lhwyd and, to a less extent, Tancred Robinson, appear to have been close friends. He also had difficulties with some of his children, and it seems that Lister must have been a difficult man to get on with. He appears to have been an aloof and austere individual, with a strong sense of dignity and propriety. His intellectual integrity is well seen, for example, in the care he took to point out features in his fossils which argued strongly against his own theory of their origin. He was also prepared to point out the superiority of other workers in his own field when this was appropriate, as with Swammerdam's own work on insect metamorphosis, and he was meticulous in avoiding possible charges of plagiarism when making use of the writings of

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1. Sir H. Lyons, The Royal Society 1660-1940 (Cambridge, 1944) ch. 3.
2. MSS Lister 37 f. 26.

other authors. This fault, he believed, was the 'bane and pest of learning'. [1]

On the other hand, he was very sensitive about his own position when he felt that he had earned the right to be considered an authority; he made it clear that in any co-operation with Willughby on a natural history of spiders, Willughby would have to consider himself as no more than an assistant. Though prompt in pointing out Ray's priority in the discovery of the hermaphrodite nature of snails, he was perhaps excessively sensitive in his own rights in the gossamer controversy, and Grew certainly considered him a contentious person; Ray suppressed his own ideas on respiration for fear of offending him. [2]

Lister saw himself as maintaining intellectual and professional standards which were being allowed to slip by many of his contemporaries; hence his comment to Smith that he had

> 'incurred the envie of manie by endevouring to affect manie things necessarie to be observed in practice for the Honour of the Profession ... which the avarice, hastie humour and pride of some men had quit ruined'. [3]

In France he had noted with strong disapproval the lack of respect shown to parents by children, to husbands by wives and to masters by servants; and on a private visit ~~to~~ at Montpellier he was offended when the host presented readings from a 'play of intrigues' and from love letters of the King of France. There were better ways to entertain'persons of honest and genteel conversation', paricularly when there were house servants present. [4]

Lister's stiff and conservative character showed itself in his attitude towards the classical authorities. In the early years, he could write to Ray of other men's observations that 'I am very cautious not to

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1. CJR p. 124.
2. FCJR p. 302.
3. MSS Smith 51 f. 53.
4. MSS Lister 5 ff. 218-23.

forwardly entertain such upon trust'. [1]

and Ray could comment that

> 'you have taken the right course and method, that is, to see with your own eyes, not relying lazily on the dictates of any master but yourself'. [2]

This attitude changed considerably in later life, as shown by his suspicion of the microscope on the grounds that it led to contempt for the ancients. He rejected most moderns as 'vainlie false' [3] and would not accept Lower's demonstration of the change in blood colour in the lungs on the grounds that all other authorities gave the heart as the site of the change.

This general conservatism also showed itself in Lister's use of Latin. The last decade of the seventeenth century saw a general change from Latin to English for scientific works published in England. There were, of course, a few Latin works published in the early eighteenth century, such as Ray's Historia insectorum, but many books issued in English at this time, such as the medical and physiological works of Mead, Pitcairne, Freind and Keill, would surely have been in Latin had they been written a few years earlier. The issue of Newton's Principia in Latin in 1689 and his Opticks in English in 1704 shows the tendency of the period.

All Lister's works were published in Latin, except for the collection of papers from the Philosophical Transactions published as Letters and Mix'd Discourses and his edition of Goedart. In 1678 he justified this use of Latin by claiming that it made his work useful on the continent, but in 1709 he used the argument that Latin was the only proper tongue for a learned work. This insistence on Latin caused

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1. CJR p. 28.

2. Ibid. p. 23.

3. MSS Ashmole 1816 f. 180.

difficulties in the printing of works by Lister and Ray, as most English printers could not cope with such work. Because of this, Lister at first planned to publish de Humoribus in Holland, where in fact the second edition was printed. [1]

In spite of the high standards of intellectual morality which Lister advocated and observed, in his professional life he showed himself to be as aggressively ambitious as most of his fellows. He does appear to have been a successful physician on his own merits, and Ray wrote to Robinson that the 'hitting of distempers' was a gift, at which Dr. Lister was happy, [2] but it is difficult ~~not~~ to see his gifts to the Ashmolean Museum as anything other than an attempt to buy himself a doctorate, and his attempt to bribe his way into a royal appointment shows him to have been no more moral in this respect than were his contemporaries. His style of living was also, at least in his earlier years, not frugal; Ray had to commend to him the example of the animals Lister himself studied, which ate and drank no more than was necessary, and reproduced only once a year. [3]

The value of Lister's biological works is not easy to assess, largely because his interests were not those of his fellow workers. There were several competent comparative anatomists working at the same time as Lister: men such as Swammerdam, Malpighi and Tyson. However, their work consisted of unsystematic forays into the field; apart from Grew's undistinguished account of vertebrate guts, Lister's was the only attempt to obtain useful information on function through the comparison of a wide range of related types. Furthermore, though his analogies were sometimes unfortunate, he did realize that there were limits to the amount of direct comparison that could be made between vertebrates and

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1. MSS Lister 36 f. cxxiii; MSS Smith 64 f. 97; MSS Smith 52 f. 7.
2. FCJR p. 288.
3. CJR p. 17; FCJR p. 113.

invertebrates; without a theory of evolution, of course, Lister could not distinguish homology from analogy. Lister's comparative anatomy, sound though not spectactular, could have served as the foundation of a developing branch of biology. It was, however, neglected by most workers then and since.

Even further from the spirit of the time was Lister's ecological work on spiders. This detailed and painstaking work, carried out in the field and involving systematic and organized research rather than casual and anecdotal record, was completely ignored by philosophers and ridiculed by other sections of society, as in the work of Shadwell and King; this was an age in which a will could be challenged on the grounds that its author must be insane, as he collected insects. Either because of this reception, or because of loss of vigour and enthusiasm, Lister produced no more field work of this standard.

It is as a systematist of mollusc shells that Lister has been remembered. To the present-day biologist, this is the least valuable part of his work, of importance mainly for the trivial and commercial values of the shell-collector. However, this attitude does not do justice to the seventeenth and eighteenth century naturalists. For a biologist with an evolutionary outlook, the precise classification of a wide range of animal types is a matter of detail, of value mainly as an illustration of evolutionary change and adaptation. We search for unities in a mass of diversity; indeed, the search for these unities is now the very subject-matter and purpose of science. In the seventeenth century this diversity was itself the principle of science; the baroque mass of detail had a virtue and meaning of its own. This was the real world, the object of the natural philosopher's interest. Particularly in the biological sciences, a non-evolutionary mind searching for principle in the world will find it in the diversity of which Lister illustrated and arranged a part.

'For Natural History describeth the variety of things; physique, the causes, but variable or respective causes; and metaphysique the fixed and constant causes'. [1]

It was this now trivial aspect of his work which was remembered by his immediate successors. Because of this, the reputation of this not outstanding naturalist has suffered perhaps more than it should; it was Lister's misfortune that it was his most ordinary work which was of greatest interest to his fellow biologists. Even on this subject, the influence of Lister's field work on his taxonomy, as seen at its best in his approach to a biological definition of the species, has been overlooked.

Martin Lister, then, was in most respects an ordinary and unremarkable seventeenth-century man of science; his work, though notable for its quantity, was of a quality which has been over-valued in part in the past and neglected in its entirity in the present. He was an ultra-Baconian in his youth, but an advocate of classical authority in old age; an enthusiastic member of societies who became an embittered individualist; hard-working and painstaking, but spasmodic in his activities; conservative and austere, but sometimes original in outlook and worldly in behaviour; generally overcautious to the point of unimaginativeness, but wildly unrealistic in his own occasional speculations; a hardworking man who greatly valued observation, neglected experiment, and had no theoretical framework for his outlook.

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1. Bacon p. 93.

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(1) Lister's works: printed books

-Historiae animalium Angliae tres tractatus. London, 1678.

In spite of the title, this work has four sections, covering spiders, land snails, fresh-water and salt water molluscs, and fossil shells; the latter has a separate preface and all have individual title pages. An appendix to this work was issued in 1685, bound in with the Latin edition of Goedart. A German translation of the spider section by J. A. E. Goeze was published as Naturgechichte der Spinnen, Quendlingburg und Blankenburg, 1778.

-Johannes Geodartius of insects. Done into English and methodized. With the addition of notes. York, 1682.

Lister's notes are a substantial part of the whole and Place's plates are new. A Latin version was published by the Royal Society at London in 1685 as J. Goedartius de insectis in methodum redactum.

-De fontibus medicatis Angliae, exercitatio nova et prior. York, 1682.

An account of medical mineral waters, including an outline of Lister's physiological system. Revised and expanded editions were published as De thermis et fontibus medicatis Angliae, London 1684; Excercitationes et descriptiones thermarum et fontium medicatorum Angliae, London 1685 and 1689.

-Letters and divers other Mixt Discourses in Natural Philosophy. York, 1683.

A collection of papers, almost all of which had been published in the Philosophical Transactions of the Royal Society.

-Historia sive synopsis methodica conchyliorum. London 1685-92, second edition, London 1692-7.

This work is bibliographically extremely complex. It was published in parts, and few copies appear to be identical. A number of bound sets of samples of the earlier sheets, issued with the title De cochleis, about 1685, survive; it is debatable whether they should be looked upon as a separate work. See G. L. Wilkins, (cont.)

Journal of the Society for the Bibliography of Natural History, 4 (1953) pp. 196-205.

A third edition , edited by G. Huddesford, was published at Oxford in 1770; and a fourth at Oxford in 1823, edited by L. W. Dillwyn, with a correlation of Lister's arrangement with the Linnaean system. This last edition is entitled editio tertia.

-Excercitatio anatomica in qua de cochleis maxime terrestribus et limacibus agitur. London, 1694.

-Excercitatio anatomica altera de buccinis fluviatilibus et marinis, London, 1695.

(Published bound with Exercitatio medicinalis de variolis. Some copies of the first part were issued separately as Dissertatio anatomica altera ...., London, 1695, and the second part was issued as Disquisitio medicinalis de variolis, London, 1696.)

-Conchyliorum bivalvium utriusque aquae exercitatio anatomica tertia huic accedit dissertatio medicinalis de calculo humano. London, 1696.

These last three works were intended as an anatomical supplement to the Historia conchyliorum.

-Sex exercitationes medicinales de quibusdam morbis chronicis ..... London, 1694.

A revised and enlarged edition published as Octo exercitationes medicinales, London, 1697.

-A Journey to Paris in the Year 1698, London, 1699.

Two further editions in the same year. Reprinted in Pinkerton's General Collection of the best and most interesting Voyages and Travels .. ... London, 1809; and a revised edition by George Henning, An Account of Paris at the close of the Seventeenth Century ...., London, 1823. Henning's edition was translated into French as Voyage de Lister a Paris.. .., Paris, 1873. A facsimile reprint of the third edition with notes by R. P. Stearns was published in 1967 by the University of Illinois.

-S. Sanctorii de statica medicina ... cum commentario, London, 1701.

Another edition, London, 1728.

-Commentariolus in Hippocratem, London, 1702.

Republished as part of Hippocratis aphorismi cum commentariolo, London, 1703.

-De opsoniis et condimentis sive arte coquininaria, London, 1705; second edition London, 1709.

Lister's edition of Apicius Caelius' work.

-Dissertatio de humoribus in qua veterum ac recentiorum medicorum ac philosophorum opiniones et sententiae examinantur, London, 1709; another edition, Amsterdam, 1711.

-Lister's De scarabaeis Britannicus was printed as part of John Ray's publication of Francis Willughby's Historia insectorum, London, 1710.

(2) Lister's published papers.

'Some observations concerning the odd turn of some shell snails and the darting of spiders. By an ingenious Cantabrigian. In a letter to Mr. John Ray.' Phil. Trans. 4 (1669) p. 1011.

'Another insect that is likely to yield an acid liquor ... and of the bleeding of the sycamore.' Phil. Trans. 5 (1670) p. 2067.

'Some experiments about the bleeding of the sycamore.' Ibid. p.2069.

'On the manner of spiders projecting their threads'. Ibid. p. 2104.

'Inquiries and experiments touching the motion of sap in trees'. Phil. Trans. 6 (1671) p. 2121.

'An observation concerning certain insect-husks of the kermes kind.' Ibid. p. 2165.

'Account of a kind of fly that is viviparous, together with a set of curious enquiries about spiders, and a table of the several sorts of them to be found in England, amounting to at least 33.' Ibid. p. 2170.

'An account of an insect feeding upon henbane, the horrid smell of which is in that creature so qualified thereby as to become in some measure aromatical; together with the colour yielded by the eggs of the same.' Ibid. p. 2176.

'The kind of insect hatched of the English kermes, also the use of these purple insect husks for tinging, together with a comparison made of this English purple kermes with the scarlet-kermes of the shops.' Ibid. p. 2196.

'A considerable account touching vegetable excrescences.' Ibid. p. 2254.

'Confirmation of Mr. Ray's observations about musk scented insects, adding some notes upon Dr. Swammerdam's book of insects, and of that of Steno concerning petrified shells.' Ibid. p. 2281.

'A letter enlarging his /Lister's/ communications about vegetable excrescencies and ichneumon worms'. Ibid. p. 2284.

'Some additions to his letter about vegetable excrescencies and ichneumon worms, together with the discovery of another musk scented insect; and an inquiry concerning tarantula's.' Ibid. p. 3002.

'An account of veins observed in plants analogous to human veins.' Ibid. p. 3052.

'An account of a stone cut out from under the tongue of a man.' Phil. Trans. 7 (1672) p. 4062.

'Letter concerning animated horse hairs, rectifying a vulgar error.' Ibid. p. 4064.

'Enlargement and correction of his former notes upon kermes; and withal insinuating his conjectures of cochineals being a sort of kermes.' Ibid. p. 5059.

'A description of an odd kind of mushroom yielding a milky juice much hotter upon the tongue than pepper.' Ibid. p. 5116.

'A further account concerning the existence of veins in all kinds of plants; together with a discovery of the membranous substance of those veins, and of some acts in plants resembling those of sense; and also of the agreement of the venal juice in vegetables with blood of animals, etc.' Ibid. p. 5132.

'Anatomical observations and experiments concerning the unalterable character of the whiteness of the chyle within the lacteous veins together with diverse particulars observed in the guts, especially some sorts of worms found within them.' Phil. Trans. 8 (1673) p. 6060.

'A description of certain stones figured like plants, and by some observing men esteemed to be plants petrified.' Ibid. p. 6181.

'A letter containing the first part of his table of snails, together with some queries relating to those insects, and the tables themselves.' Phil. Trans. 9 (1674) p. 96.

'Observations and experiments made for the Royal Society.' Ibid. p. 221.

'Observations of the astroites or star-stones.' Phil. Trans. 10 (1675) p. 274.

'Some observations about damps, together with some relations concerning some odd worms vomited by children.' Ibid. p. 391.

'Observations made at Barbadoes.' Ibid. p. 399. [Comments on a letter from Thomas Townes.]

'Several curious observations about antiquities near York.' Philosophical Collections of the Royal Society 4 (1682) p 87

'An account of a very strange case in physic, together with the description of a monstrous animal cast out of the stomach by vomit.' Ibid. 6 (1682) p. 164.

'An account of an experiment made for altering the colour of the chyle in the lacteal vein.' Phil. Trans. 13 (1683) p. 6.

'An account of a Roman monument found in the bishoprick of Durham, and of some Roman antiquities at York.' Ibid. p. 70.

'A relation of a man bitten with a mad dog, and dying of the hydrophobia.' Ibid. p. 162.

'Observations upon Roman antiquities at York.' Ibid. p. 238.

'Some probable thoughts of the whiteness of the chyle.' Ibid. p.242.

'On the use of the intestinum caecum.' Phil. Trans. 14 (1684) p.455.

'Observations of the midland salt-springs of Worcestershire, Staffordshire, and Cheshire; of the crude salt ... dejected by the said brines; of the specific differences betwixt sea salt and common salt; with a way (which seems to be the true method of nature) of distilling sweet and fresh water from the sea-water by the breath of sea plants growing in it; and the breath of the sea plants probably the material cause of the trade or tropick winds.' Ibid. p. 489.

'Of the nature of earth quakes; more particularly, of the origine of the matter of them, from the pyrites alone.' Ibid. p. 512.

'Concerning the spontaneous firing of the pyrites.' Ibid. p. 515.

'Concerning thunder and lightening being from the pyrites.' Ibid. p. 517.

'Concerning the projection of the threads of spiders, and bees breeding in cases made of leaves, as also, a viviparous fly etc.' Ibid. p. 592.

'An account of some very aged persons in the North of England'. Ibid. p. 597.

'An ingenious proposal for a new sort of maps of countries, together with tables of sands and clays, such chiefly as are found in the North of England.' Ibid. p. 739.

'On the rising and falling of the quicksilver in the barometer.' Ibid. p. 790.

'Experiments about freezing, and the difference betwixt common fresh water ice and that of/the sea water; also a probable conjecture about the original of the nitre of Egypt.' Phil. Trans. 15 (1685) p. 836.

'An account of a stone grown to an iron bodkin in the bladder of a boy.' Ibid. p. 882.

'A Letter to Mr. Ray concerning some particulars that might be added to the ornithology.' Ibid. p. 1159.

'An answer to Mr. Dale's three queries relating to shells.' Phil. Trans. 17 (1692) p. 641.

'An account of transparent pebbles mostly of the shape of ombriae or brontiae.' Ibid. p. 778.

'The manner of making steel, and its temper; with a guess at the way the ancients used to steel their picks, for the cutting and hewing of porphyry.' Ibid. p. 865.

'A description of certain shells found in the East Indies.' Ibid. p. 870.

'An account of the nature and differences of the juices more particularly of our English vegetables.' Phil. Trans. 19 (1696) p. 365.

'An account of several plants which may be usefully cultivated for producing grass or hay.' Ibid. p. 412.

'An account of the long worm which is troublesome to the inhabitants of Fort St. George in the East Indies.' Ibid. p. 417.

'The anatomy of the scallop.' Ibid. (1697) p. 567.

'On a venomous scratch with the tooth of a porpoise, its symptoms and cure.' Ibid. p. 726.

'An opinion of Dr. Clarke's observations on the polypus of the lungs.' Ibid. p. 779.

'An account of two boys bit by a mad dog.' Phil. Trans. 20 (1698) p. 247.

'An objection to the new hypothesis of the generation of animalculae in semine masculino.' Ibid. p. 337.

'An account of coal borings in Yorkshire.' Phil. Trans. 21 (1699) p. 73.

'A remark on Dr. Cay's account of the virtue of the ostracites.' Ibid. p. 81.

'On the origin of white vitriol, and the figure of its crystals.' Ibid. p. 331.

'On powdered blues passing in the lacteal veins.' Phil. Trans. 22 (1701) p. 819.

(3) Manuscript sources.

a. MSS Lister (Bodleian Library, Oxford).

A collection of letters, pocket books and mixed papers left by Lister, some of which have no other connection with him. The authors of the letters to Lister are listed together at the end of this section. Many letters must have been lost or placed elsewhere; there are, for example, no letters from John Ray here, these having been obtained by Tancred Robinson, and given to William Derham for publication. Some of them are now in MSS Herb. B.M. (British Museum).

MSS Lister 1.

'A method for the historie of iron.' Notes by Lister on the extraction and working of iron.

MSS Lister 2, 3, 4.

Letters to Lister from several correspondents.

MSS Lister 5.

Miscellaneous notes on medicine, cookery and philosophy, not in Lister's hand; papers by Lister on gems, gardening, personal matters, medical waters, lists of books in his library, notes on spider classification and the Historia conchyliorum, and on his visit to Montpellier in 1664-5.

MSS Lister 6.

A legal treatise, not in Lister's hand.

MSS Lister 7.

An incomplete account Of the Fossils of England, more particularly such as are found in these Northern Parts, apparently prepared with a view to publication.

MSS Lister 8.

A medical notebook in Lister's hand.

MSS Lister 9.

The original drawings for the plates in the Historia conchyliorum, by Lister's wife and daughter.

MSS Lister 10.

A notebook on religious affairs, not by Lister, and with some items dated after Lister's death.

MSS Lister 11.

The Parson's Law: an account of church law, written by W. H. in 1637.

MSS Lister 12.

A preliminary manuscript of the Exercitatio anatomica, entitled Conchyliorum anatomiae pars prima.

MSS Lister 13.

The manuscript of Lister's edition of Sanctorius Sanctorius' De statica medicina.

MSS Lister 14.

A medical notebook in Lister's hand.

MSS Lister 15.

A notebook of extracts from Aristotle and other classical writers, in Lister's hand.

MSS Lister 16.

An account, not by Lister, of the state of the Royal Navy in 1705.

MSS Lister 17.

Notes, not by Lister, on copper mining in the Lake District.

MSS Lister 18.

An account, dated 1607, of the embassy of the Earl of Nottingham to Spain in 1605.

MSS Lister 19.

A pocket traveller's guide Every Man's Companion, with notes on the plain sheets by Lister concerning his journey to Montpellier in 1663.

MSS Lister 20.

A medical notebook by Lister, also containing a copy by John Whiteside of Lhwyd's De stellis marinis.

MSS Lister 21 and 22.

The preliminary manuscript of the Journey to Paris.

MSS Lister 23.

A translation into English by Lister of Terence's Eunuchus.

MSS Lister 24.

A copy, by Lister, of part of Apicius Coelius' De opsoniis.

MSS Lister 25.

A translation by Lister of part of Celsus' De medicina.

MSS Lister 26.

A notebook of memoranda and book-lists connected with Lister's stay in Paris in 1698. Some of the early material is in Italian and French and not in Lister's hand.

MSS Lister 27, 28, 29, 30, 31, 32 and 32*.

Pocket almanacs for the years 1683 (two), 1686, 1689, 1690, 1692 and 1676 respectively. They have been used by Lister for rough notes of fees taken in his practice. MSS 28 does not contain fee lists, but has notes on natural history specimens in the Ashmolean, perhaps presented to the museum by Lister.

MSS Lister 33.

Notes by Lister on the correct use of Latin in medicine.

MSS Lister 34, 35, 36, and 37.

Letters to Lister from several correspondents.

MSS Lister 38.

Medical papers, mostly in Latin and not by Lister.

MSS Lister 39.

Medical and theological papers in Latin and French, not by Lister; lists of books and coins; a preliminary manuscript of the Exercitatio anatomica altera, and notes on plants in Cambridgeshire and Lincolnshire, compiled by Lister in 1666.

MSS Lister 40.

The manuscript of Lister's account of beetles, published in Ray's Historia insectorum.

b. Other manuscript sources.

MSS Ashmolean 1813, 1816, 1817, 1819, 1820, 1829 and 1830 (Bodleian Library, Oxford).

These contain letters to Edward Lhwyd from Lister and from William Cole and John Ray.

MSS Smith 31, 51, 52, 55, 56 and 64 (Bodleian Library, Oxford)

These contain letters from Lister to Thomas Smith, the Cotton librarian.

MSS Stowe 745, 746, and 747 (British Museum).

These contain letters to Lister from Thomas Kirke, John Ray, Samuel Dale and John Beaumont; and from Lister to these four and to Ralph Thoresby and Miles Gale.

MSS Sloane (British Museum).

These contain a manuscript of Lister's account of English beetles (783a); an agreement between the York physicians, including Lister, regulating practice (1393 f. 13); comments by Nehemiah Grew on Lister's criticism of his Anatomy of Plants Begun (1929 ff. 1-11); letters from Lister to Hans Sloane (4002, 4036, 4039, 4041, 4042, 4059); letters from Lister to Jabez Cay and in return (4025); and from Lister to James Petiver (4064).

British Museum Additional MSS 22596.

This contains biographical material on Lister compiled by William Huddersford.

Royal Society MSS L 5.

This contains the original letters from Lister to Henry Oldenburg, most of which were published in the Philosophical Transactions.

Massingham Munby Collection (Lincolnshire Archives Office, Lincoln Castle).

This contains legal documents concerning the Listers of Burwell.

Parish Register of Radclive (Buckinghamshire Archives Office, Aylesbury).

The following manuscripts, not directly concerning Lister, were also consulted and are referred to in the body of this thesis: Henry Power, Inventio Aselliana de venis lacteis et de motu chyli (MSS Sloane 1343 f. 42).

Francis Glisson, MSS Sloane 3308 ff. 273-281 (Essay on production of blood in the liver).

c. Lister's correspondents.

Letters from Lister survive in MSS Ashmolean (to Lhwyd, Musgrave and Plot), Smith (to Smith), Sloane (to Sloane, Cay and Petiver), Stowe (to Beaumont, Dale, Gale, Kirke, Ray and Thoresby); and in Royal Society MSS L 5 (to Oldenburg). Letters written to Ray have been published by Lankester (The Correspondence of John Ray London, 1848) and Gunther (The further Correspondence of John Ray, London, 1928).

A much wider range of letters to Lister survives in MSS Lister 2, 3, 4, 34, 35, 36, and 37. These are listed below in alphabetical order of correspondent together with the number of letters surviving, their period and location. This latter is given (in parentheses) only as the number of the Lister MSS except for the small number of letters in other collections; full reference is given for these. An indication is given of the general nature of the correspondence by the letters L (literary and antiquarian), M (medical), P(personal, family, legal and financial) and S (scientific).

Adams, T., vice-chancellor of Oxford University; 1 (n.d., 3), L.

Aldrich, Henry (1647-1710), Dean of Christ Church, Oxford; 1 (1695, 36), P.

Allen, Benjamin (b. 1666) of Essex, writer on mineral waters; 1 (1696, 36), S.

Allone, -, resident at The Hague; 7 (1694-1709, 2, 36), M, P.

Almeloveen, T. J., publisher at Amsterdam; 32 (1694-1710, 2), L, M, P, S.

Amyott, Charles, of Paris; 4 (1698, 3), P.

Aston, Francis (1645-1715), First Secretary to the Royal Society, 1682-5 and Council member, 1694-1711; 15 (1681-1709, 3, 35, 37), L, S.

Aylesbury, Robert Bruce, Earl of (d. 1685), Lord Chamberlain, 1685; 1 (n.d., 3), P.

Bannister, Rev. J. (d. 1692), traveller to the East Indies and resident in Virginia for 14 years; 1 (1690, MSS Sloane 4002), S: see also EA p. 27.

Barnard, Allen; 2 (1695-6, 36), L, M.

Barrow, -, a member of the Temple Coffee-house group; 1 (1696, 36), P, S.

Basket, Robert; 1 (n.d. c.1705, 3), L.

Bayulay, J., of Chichester; 1 (n.d., c. 1670), P.

Bearse, J., of Windsor; 1 (1684, 3), M.

Beaudelea, -, of Paris; 3 (n.d., 2), P.

Beaumont, John (d. 1731), surgeon, writer on fossils; 1 (1674, MSS Stowe 747 f. 23), S.

Bedford, Francis, of Falmouth, mineralogist; 2 (1674, 3, 35), S.

Belwood, R.; 1 (n.d., 3), P.

Bennis, George, of Montpellier; 2 (1698, 36), S.

Bernard, Francis (1627-1698), physician to James II in exile, collector of medical books; 2 (1696, 36), L, M.

Beverland, Hadrian, geologist and coin collector; 1 (n.d., c. 1695, 2) L. S. P.

Bobart, Tilleman, brother of Jacob, first Danby professor of Botany at Oxford; 2 (1701-5, 37), P.

Bollard, John, geologist; 1 (n.d., 3), S.

Borrow, Peter; 1 (n.d., 3), P.

Bralesford, J., of Christ's Church, Cambridge; 1 (1698, 4), P.

Brearcliffe, John, apothecary, of Halifax; 1 (1679, 3), P.

Briggs, Thomas, of St. John's College, Cambridge; 8 (1665-75, 3), P.

Brooke, John, F.R.S., M.P. (d. 1691); 15 (1670-73, 34), P. S.

Brounker, Lord, (1620-84) first president of the Royal Society, and a patient of Lister; 1 (1684, 3), M.

Bulkeley, Sir Richard, F.R.S. (1644-1710), of Dunlaven, Co. Wicklow; 36 (1686-1707, 3, 35, 36, 37), M. P. S.

Burlington, Earl of (1612-97), brother of Robert Boyle; 1 (1684, 3), M.

Butterfield, Michael, English mathematical instrument maker resident at Paris, 8 (1698-9, 3), M. P.

Cay, Jabez, of Newcastle, mineralogist; 3 (1696-8, 36), S.

Charlett, Arthur, (1655-1722), Master of University College, Oxford; 14 (1698-1709, 3, 36, 37), L.

Child, S.; 1 (1708, 3), M.

Cole, William, (1646-1701), Bristol customs official and fossil collector; 1 (1683, 35), S. (Another letter from Cole in 36 is not to Lister).

Comber, Thomas (1645-99), precentor of York and Dean of Durham; 1 (1698, 3), M.

Courten, William (1642-1702), merchant and natural history collector; 1 (1697, 3), P.

Cressener, Henry; 1 (1709, 37), L. M.

Cullen, Richard, Lister's lawyer and apparently a relative of his second wife; 6 (1699-1712, 4), P.

Cullen, Samuel, a sponsor of one of the plates in Willughby's History of Fishes; 1 (1685, 35), M.

Dale, Samuel (1659-1739), physician and naturalist, close friend of John Ray; 9 (1693-8, 35, 36, and MSS Stowe 747 f. 24), S.

Danby, A.; 1 (1686, 3), P.

Danvers, J.; 1 (1704, 3), M.

Delaune, William (1659-1728), president of St. John's College, Oxford, chaplain to Queen Anne; 1 (1705, 37), P.

Driver, Samuel, nurseryman; 10 (1704-6, 3, 4), P.

Drouyn, Abbē, of Paris; 5 (1699, 2), L. S.

Edgerton, Charles; 2 (1700, 3). M.

Fairfax, Brian (1633-1711), equerry to Charles II and William III; 1 (n.d., 3), L. P.

Fitzgerald, R.; 1 (1686, 35), S.

Flamstead, J. (1646-1719) first astronomer royal; 4 (1702-3, 3, 37), P, S.

Fuller, Samuel (1635-1700), Dean of Lincoln, fellow of St, John's College, Cambridge, Chaplain to Charles II; 1 (pre 1676, 35), L, P.

Gale, Miles (1647-1721), mineralogist and antiquary, Rector of Keighley, cousin of Thomas Gale, secretary of the Royal Society; 2 (1682-3, 3, 35), P, S.

Gibson, Edmund (1669-1748), antiquarian, librarian at Lambeth Palace, later bishop of Lincoln and London; 1 (1694, 36), L.

Giles (or Gyles), Henry (1645-1709), glass painter of York; 3 (1693-6, 3, 36), P.

Gower, Humphrey (1638-1711), fellow and later master of St. John's College, Cambridge; 3 (1682-5, 3, 35), L, P.

Grandage, Christopher; 1 (1708, 3), M.

Greathes, E.; 1 (n.d., 3), M.

Gregory, George, of Burwell, a relative by marriage of Lister; 5 (1679-84, 4), P.

Gregory, Richard, of Burwell; 1 (1697, 4), P.

Gregory, Susanna, Lister's sister; 1 (1696, 4), P.

Grew, Nehemiah (1641-1712), botanist, secretary to the Royal Society; 10 (1673-82), L.

Grove, Robert (1634-96), fellow of St. John's College, Cambridge, later Bishop of Chichester; 4 (1667-83, 3, 35), M, P.

Halley, Edmund (1656-1742), assistant secretary to the Royal Society, editor of the Philosophical Transactions, later full secretary and astronomer royal; 1 (1687, 3), S.

Hartopp, Dorothy, Lister's sister; 3 (1667, 4), P.

Hartopp, Martin, a nephew of Lister; 1 (1690, 4), M.

Hatton, C.; naturalist; 1 (1698, 36), L.

Hautefeuille, S. de, of Paris; 3 (1699, 2), P.

Hechstetter, David; 1 (1699, 3), M.

Henshaw, Thomas (1618-1700), historian, diplomat and formerly secretary to the Royal Society; 1 (1695, 36), L.

Hooke, Robert (1635-1703), secretary to the Royal Society; 2 (1678-82, 34, 35), L, S.

Hoskyns (or Hoskins), Sir John (1634-1705), former president and secretary of the Royal Society; 1 (1696, 36), L.

Howes, John, Lister's former sizar at Cambridge; 1 (1698, 3), M.

Jessop, Francis (1638-1694?) of Sheffield, mineralogist; 10 (1673-93, 34 35, 36), S.

Johnston, Nathaniel (1627-1705), physician, naturalist and antiquary, of Pontefract and later of London, Jacobite; 8 (1672-6, 35), S.

Kenrick, Daniel, poet; 1 (1699, 3), L.

Kempe, E., of Burwell; 1 (n.d., 3), P.

King, R., visitor to Lister at Montpellier; 1 (1664, 3), P.

Kirke, Thomas (1650-1706), of Cookridge, near Leeds, Trinity College, Cambridge, F.R.S.; 2 (1677-9, 34, MSS Stowe 745 f. 137), P.

Lhwyd (or Lhuyd), Edward (1660-1709), antiquary, philologist and fossil-collector, keeper of the Ashmolean Museum; 102 (1689-1702, 3, 35, 36, 37, though from Lister's letters it can be seen that the correspondence between the two continued until Lhwyd's death in 1709), L, P, S.

Lister, J., nephew, traveller to China, observer of vaccination; 1 (1700, 37), M, P.

LLoyd, John (1638-87) principal of Jesus College, Oxford, later vice-chancellor, Bishop of St. David's; 1 (pre 1676, 35), L.

Lodge, William (1649-89), artist, of Leeds and Jesus College, Cambridge; 13 (1672-74, 3, 34), L, S.

Lowther, Anthony, of Whitehaven, one of the few puritans in the early Royal Society; 2 (1698, 36), P, S.

Lybbe, James; 1 (n.d., 3), P.

Mander, Robert; 1 (1701, 36), M.

Marlborough, Sarah Churchill, Duchess of (1660-1744), Lister's niece; 2 (n.d., c.1690 and 1707, 4), P.

Middleton, M. principal of King's College, Aberdeen; 1 (n.d., 3), M.

Monson, James, student at Montpellier; 2 (n.d., c.1685, 3), S.

Montagu, Charles (1661-1715), first Lord Halifax, Lord of the Treasury, president of the Royal Society, founder of the national debt; 1 (1693, 36), S.

Moulin, Lewis du (1606-80), antiquarian, Camden professor of ancient history at Oxford; 7 (1667-80, 2), L, M, P.

Musgrave, William (1655-1721), physician, antiquary and secretary of the Royal Society; 4 (1685-1709), L, P, S.

Newcomen, Thomas (1603-1665), prebendary of Lincoln, formerly of St. John's College, Cambridge; 1 (1662, 3), P.

Nicholas, Ann, a relative of Lister; 2 (1696-8, 4, 36), P.

Nicolson, William (1655-1727), antiquary, archdeacon and later Bishop of Carlisle; 2 (1696-7, 36), L.

Oldenburg, Henry (1615-77), first secretary to the Royal Society; 54 (1671-6, 34), L, S.

Parker, Robert, a tenant or employee on Lister's Yorkshire estates; 1 (1699, 3), P.

Parry, D., Lhwyd's assistant; 1 (1709, 37), L.

Pascall, Andrew; 1 (1694, 36), L.

Paston, Sir Robert, Earl of Yarmouth (1631-1683); 1 (1683, 3), M.

Paynter, William (1637-1716), rector of Exeter College, Oxford; 1 (1699, 36), L.

Peck, John, of St. John's College, Cambridge; 3 (1667-81, 3, 35), L, P.

Pepys, Samuel (1633-1703), diarist, secretary of the Admiralty and president of the Royal Society; 1 (1685, 35), L.

Peterborough, Earl of (Henry Mordaunt, 1624-97); 1 (n.d., 3), S.

Petiver, James (1663-1718), naturalist and apothecary; 1 (n.d., 3), S.

Pierrepont, E. (a member of the family of the Earl of Kingston?) 1 (1682, 3), M.

Pitcairne, Archibald (1652-1713), physician, of Edinburgh, formerly professor of physic at Leyden; 2 (1695, 36), L, M, S.

Place, John, brother of the artist Francis Place, and physician to the Grand Duke of Tuscany; 9 (1682-1708, 3, 35, 37), L, S.

Plaxton, George, Vicar of Barwick in Elmet, Yorkshire; 2 (1674-5, 35), S.

Plot, Robert (1640-96), antiquary, first keeper of the Ashmolean Museum and professor of chemistry at Oxford, secretary of the Royal Society; 8 (1682-6, 35), L, S.

Prideaux, Humphrey (1648-1724) historian, archdeacon of Suffolk; 1 (1699, 3), M.

Prior, Matthew (1664-1721), poet and diplomat; 2(1698, 3, 36), L.

Proby, Charles, medical student at Montpellier; 3 (1698-9, 36), S.

Pulleyn, Octavius; 1 (1682, 35), S.

Pye, Richard, a nephew of Lister; 2 (n.d., 4), P.

Railton, Thomas, a family friend; 2 (1701-2, 4), P.

Ray, John (1627-1705); 39 (1667-1676, MSS Herb, B. M.), S. Also 1 (1689, MSS Stowe 746 f. 113) P. These letters have all been published in CJR and FCJR.

Reading, E., of Dublin; 2 (1688, 35), P.

Robinson, Tancred (c. 1658-1748), physician, of York, St. John's College, Cambridge, and London; secretary to the Royal Society; 27 (1683-1710, 3, 35, 36, 37), L, P, S.

Scheuchzer, J. J., Swiss writer on fossils and minerals; 1 (1698, 2), S.

Sibbald, Sir Robert (1641-1722), professor of medicine at Edinburgh, antiquary and naturalist; 2 (1695-8, 36), S.

Sloane, Sir Hans (1660-1753), physician, secretary and later president of the Royal Society; 2 (1707, 37), S.

Smith, Thomas (1638-1710), Cotton librarian; 22 (1704-10, 37), L.

Steevens, Robert, of Goose Creek, South Carolina; 2 (1695, 36), S.

Sprengel, C., of Germany; 1 (1706, 37), S.

Standfast, Henry; 1 (1709, 37), L.

Stretton, John; 1 (1694, 36) L, P.

Strozzi, Leo; 5 (1698-1700, 2), L.

Sturdy, J.; 1 (1675, 34), S.

Tanner, Thomas (1674-1735) antiquarian, future Bishop of St. Asaph and one of the Temple coffee-house group; 1 (1699, 36) L.

Thanet, Lord; 1 (n.d., 3), P.

Thomas, Edward, a protege of Lhwyd; 1 (1694, 36), S.

Thomson, E., of Aberdeen; 1 (1698, 36), L.

Thoresby, Ralph (1658-1725), antiquary, of Leeds; 3 (1699, 36), L.

Threapland, Samuel, of Halifax; 1 (n.d., 35), S.

Thyme, Jane, a sister of Lister; 1 (n.d., 4), P.

Tonson, Jacob (1656-1736), publisher, of London; 1 (1699, 36), L.

Tournefort, J. P. de (1656-1708) French naturalist; 2 (1687, 2), S.

Townes, Thomas, physician, of Cleveland and Barbados; 6 (1674-5, 3, 35), S.

Towneley, Richard (1629-1707) of Burnley; 1 (1694, 36), L.

Vernon, W., entomologist, of Peterhouse, Cambridge; 1 (1695, 36), S.

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Warton, H.; 1 (n.d., 3), M.

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Witham, G., of Methley, Yorkshire; 1 (1678, 35), S.

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Willbraham, R., fossil collector; 1 (1694, 36), S.

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