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– 9 –

E RAY LANKESTER

AND THE MAKING OF

MODERN BRITISH BIOLOGY

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E. RAY LANKESTER

and the Making of Modern British Biology

by Joseph Lester

Edited, and with additional material

by Peter J. Bowler

BRITISH SOCIETY FOR THE HISTORY OF SCIENCE

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ISBN 0-906450-11-X

Typeset in Electronic Font Foundry LondonA using
Computer Concepts Impression II with Acorn Archimedes
by Geoffrey Bennett,
Professional Office Services, 31 High Street,
Faringdon, Oxon, SN7 8LH

Printed in Great Britain by the Alden Press, Oxford

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Photograph of E. Ray Lankester taken around 1907

(Reproduced with the permission of the Natural History Museum)

Editor's Preface

E. Ray Lankester was one of the giants of late-nineteenth-century British science. His work as a morphologist and evolutionist brought him international fame, and his contributions to scientific education and to the organisation of British science were immense. He made many enemies in the course of his only too active pursuit of his goals, but was respected and admired by scientists and by those of a liberal disposition. He knew numerous eminent figures of the time, including many outside the realms of science – Karl Marx, H. G. Wells and Anna Pavlova, to name only three. His work has now been largely forgotten, having fallen into that blind spot of the history of biology, the era between the original Darwinian debate and the emergence of the modern synthesis of genetics and Darwinism. Nevertheless, a cursory glance at the nature of his achievements makes it obvious that he is worthy of a serious study.

The great merit of this biography is that is has been prepared with rhe benefit of access to a major collection of Lankester's papers which is still in private hands. In the course of his life, Lankester wrote many detailed letters ro his mother and to orher members of his family. After his death, these were preserved by his niece, Mrs N. Pain, along with a substantial collection of notebooks, drafts of papers, offprints, and the relatively small collection of letters from others that Lankester preserved. The collection also contains the diaries of E. Ray's father, Edwin Lankester, a fact which was unknown to Dr Mary English until she had finished her recently-published biography of Edwin. Joseph Lester made conract with Mrs Pain in the 1950s and was eventually given access to the papers on the understanding that he would write an 'approved' biography. It must be remembered that Lankester had been a controversial figure, and his family was at first reluctant to permit any access to the papers. The collection is now held by Mrs Pain's daughter, who wishes to respect her mother's decision that Lester's biography should be the first publication based on the material. The collection is designated as the 'Lankester family papers' in the references below.

To prepare the biography, the family papers were supplemented by Lankester's published works and by other archival sources. The latter include the papers of T. H. Huxley at Imperial College, of J. D. Hooker

and W. Thiselton-Dyer at Kew, of H. G. Wells at the University of Illinois at Urbana-Champaign, and of P. H. Gosse in the Brotherton Collection at Leeds Univetsity. Material relating to Lankester is also located in the following archives, not all of which are listed in *The Manuscript Papers of British Scientists* (London, 1982): University College, London; The Bodleian Libraty and the Zoology Department Libraty, Oxford; Cambridge University Libraty (William Bateson papers); University of St. Andrews Libraty (D'Arcy Wentworth Thompson papers); The Marine Biological Association, Plymouth; The Natural History Museum, London; The Stazione Zoologica, Naples; Dr. Williams's Libraty, London, and the British Libraty, London. Our thanks are due to these archives for allowing access to their material and granting permission to quote from it.

Lester worked on his biography during the 1950s and 60s, but was unable to find a publisher. He was an amateur, with no contacts among professional historians of science – who were, in any case, rather thin on the ground at the time. He became understandably ftustrated, and was concerned that his work might be 'scooped' by a professional. Eventually my own interest in Lankester led me to contact Lester and I was able to convince him that the only way that his work would see the light of day was to have it edited by a someone who could ensure that it met modern standards of scholarship. Having seen the manuscript, I decided that the amount of information revealed by the Lankester family papers was sufficient to justify taking on the job of editing myself.

Much of the text of Lester's manuscript has been left intact, with only minor tidying up of a stylistic nature: the extensive quotations from the family papers speak for themselves. A few sections have been repositioned in order to improve the flow of the text. The greatest amount of effort has been consumed by trying to ensure that the references in the footnotes are adequate by modern bibliographical standards. In some cases, I have rewritten interpretive passages in the light of modern knowledge, and have added references to the recent literature (any reference to material published after about 1970 falls into this category).

There are a few more significant changes which need to be specified individually. Chapter 7 on Lankester's evolutionism has been almost entirely rewritten by me, because there have been major changes in how we understand the impact of Darwinism since Lester wrote the original. There are additions, especially to chapter 4, based on my use of the letters from Lankester to Ernst Haeckel. Copies of these were provided for me by Dr. Mario Di Gregorio, with the approval of Dr. Erika Krausse of the

Ernst-Haeckel Haus, Jena. There are extensive changes, especially to chapter 11, based on my use of the Lankester papers in the archives of the Natural History Museum, London. My thanks are due to Dr. Krausse in Jena and to John Thackray in London for permission to use this material. Finally, there are additions to chapter 15 based on my consultation of the Lankester-Reid Moir letters held by the British Library (Add 44968-71).

At a late stage in the preparation of the book I was able to consult the Lankester material in the archives of the Stazione Zoologica, Naples. The archivist, Dr Christiane Groeben, had already provided me with a listing of the correspondence held there, and I am very grateful for her help. Several minor points based on this material were added in proof.

Peter J. Bowler
Belfast, December 1994

- 1 -
Parents and Childhood

Edwin Ray Lankester once wrote playfully to his mother that he had sprung from 'poor but clever parents'. There was point to his remark, for he was always acutely aware of the fact that he did not belong to that privileged group of scientists able to devote a private fortune to the investigation of nature. As late as 1883, he went so far as to say that it was impossible to name a man who had added greatly to scientific knowledge, without enjoying a private fortune.[1] As examples he named Lyell, Darwin, and his own close friend Francis Balfour. Ray Lankester's father, Edwin, was born in 1814 at Melton, near Woodbridge in Suffolk. Edwin's father, a builder, died young in 1818, leaving his widow with a son and daughter, and in rather precarious circumstances. Edwin kept a punctilious diary between the years 1832 and 1845, and tells us something of the circumstances of his early life and adolescence:

> At an early age, I lost that dearest of ties, a parent, in my father, the loss of whom has undoubtedly affected much my affairs in life ... Having at the age of ten acquired a sufficient knowledge of writing, I was placed with an attorney as a writing clerk, with whom I continued about a year. At this time proposals were made that I should become apprentice to my uncle Thomas Taylor at Pontefract in Yorkshire, and was on the point of starting for that place, when Mr Gissing, a surgeon of Woodbridge, intimated a desire that I should be apprenticed to him, although at that time very young.[2]

A strong religious feeling now developed within him, leading to an active life as Dissenter. Much of his diary records his spiritual wrestlings,

[1] E. Ray Lankester, 'Biology and the State', President's Address, Biology Section, British Association for the Advancement of Science, 1883, in Lankester, *The Advancement of Science* (London, 1889), pp. 63-117, p. 72.

[2] Edwin Lankester's notebooks and diaries are preserved in the Lankester family papers. There is a biography of Edwin by Mary English, *Victorian Values: The Life and Times of Dr. Edwin Lankester, M.D., F.R.S.* (Bristol, 1991); unfortunately Dr. English was not informed of the existence of these diaries.

and his attempts to cope with the conviction that his studies in 'natural philosophy' were breeding in him habits of indulgence and infidelity: 'My heart would fain give itself up to literature and science, but whenever these objects gain my attention, then do I seem most distant from my God.'

In 1832, he became assistant to Thomas Spurgin, a cultured surgeon of Saffron Walden, who was so impressed with the young man's ability and integrity, that he raised a loan of £300, enabling Edwin to study medicine and science from 1834 to 1837 in the recently opened University College, London. He attended the lectures of the botanist John Lindley, and those of the zoologist R. E. Grant, whose post was the one to which Ray Lankester succeeded in 1875. He did very well, though he privately castigated his 'love of sociality', which however endeared him to a circle of friends that included W. Jenner, J. C. Bucknill, R. Hardwicke, and W. B. Carpenter, all of whom had distinguished careers in science or medicine. Since he could not afford the expenses of a full degree course, he qualified as a member of the Royal College of Surgeons, and a Licentiate of the Society of Apothecaries.

In 1837, Edwin travelled to Doncaster, to become the resident medical attendant and science tutor to the Woods family of Campsall Hall, a post he owed to the friendship of Lindley. Here, he formed a friendship with a fellow tutor, Leonhard Schmitz, classical scholar, and later tutor to Albert Edward, Prince of Wales. Schmitz eventually became head of a very remarkable and advanced school, the International College of Isleworth, and on his eightieth birthday, when Edwin Lankester had been dead for many years, wrote to his widow recalling the happy days he had spent with her husband.[3]

The Woods family were liberal in outlook, but Edwin found them rather repellent, and quarrelled violently before he left Campsall Hall. He was pained by their indifference to religion, for they were all fervent Owenites, and in 1839 the great man himself paid a visit to Campsall Hall.

> On Monday last Robert Owen the Socialist arrived at Campsall Hall, and I was invited to meet him ... His appearance is not striking; his manner is kind and polite. He appeared in good health, and looks younger then he really is, being now seventy ... He is aiming at reforming the world, attributing its present state to want of the knowledge of the principles on which men should

[3] Preserved in the Lankester family papers.

> act in their relations to each other. He unfolds his views with all the confidence of a mathematician demonstrating a problem ... It is curious to see with what earnestness the family of the Woods receive his statements. Most of them look upon him as an oracle. They however fall short of him in their active benevolence. Whilst he has a heart devoted, I believe, to what he supposes the welfare of mankind, they only look to securing themselves from what they suppose to be the errors of the world ...[4]

In 1839, Edwin left the unbelieving Woods and travelled to Heidelberg to take his M.D. – an aim he attained in six months. On his return to London he divided his time between medical practice, lecturing and literary work. His circle of friends extended to include the Manx naturalist Edward Forbes, the wit Douglas Jerrold, and the botanist Arthur Henftey, who named a plant genus *Lankesteria.*

An entry in Edwin's diary, dated November 29th 1843, gives an indication of the dilemma in which he found himself:

> In my literary life I have been variously employed. I am writing the articles on Botany, Medicine and Surgery for the *Penny Cyclopaedia,* the biographies of naturalists and some physicians for the *Biographical Dictionary,* with occasional contribution to the *Lancet, Gardeners' Chronicle, Athenaeum, Annals of Natural History,* etc. Upon this part of my exertions depends my livelihood, and I must confess I have pursued it more in the spirit of business than of love. I have done but little for the advance of science ... I yet hope to do so ... should professional success attend me. But I find that it is very hard work to live the outwatd life of a physician, and pay for it by the work of a literary man. This leaves me little time to putsue science 'con amore'.

Yet there are signs of what he might have accomplished. In 1841 he made a study of the 'glairine' of sulphurous waters, now known as the sulphur bacteria.[5] He was one of the first to make a thorough microscopical examination of drinking water. All this and other genuine scientific work was buried beneath a mass of popularisations, lectutes and organisational work for science.

[4] Edwin Lankester's diary, 24 October 1838 (Lankester family papers).

[5] Edwin Lankester, 'Notice of Plants and Animals found in Harrowgate and Askern, Yorkshire', *Annals and Magazine of Natural History,* 7 (1841): 105-110.

Edwin was a founder of the Biological Section of the British Association for the Advancement of Science, and remained its President for twenty-five years. He was first Secretary of the Ray Society, publishers of biological monographs, and for a time, Mrs. Lankester functioned as Assistant Secretary. In 1845, Edwin was elected Fellow of the Royal Society, and President of the Royal Microscopical Society. Twenty years later, he was the first President of the notable society of amateurs, the Queckett Microscopical Club, and in his Presidential speech expressed mature and advanced views on the microscope as an instrument of research. It was not to be considered as an aimless, luxurious toy, but as a working instrument, by the use of which the boundaries of science would be greatly extended.

Edwin was a superb lecturer, using fern cases and specimens of all kinds, for he believed that natural science could not be learned from books. He believed that: 'Physics and Chemistry must be taught by experimenrs; Mineralogy and Geology by specimens of minerals and rocks; Botany and Zoology by plants and animals.'[6] It seems obvious enough today, but was not so at that time. When Rolleston, Huxley, Ray Lankester, Bower and others put these ideas into practice, the result was a revolution in science teaching. Edwin also deserves to rank as a founder of the aquarium. His book, *The Aquavivarium*, published in 1856 had a great influence.

There is no doubt at all of Edwin's most notable contribution to science; he was the co-founder of the famous journal which was to be the vehicle for most of his son's contributions to biology. This was the *Quarterly Journal of Microscopical Science*, first appearing in 1853, under the joint editorship of Lankester and the surgeou George Busk. It was partly an independent journal, and partly the organ of the Microscopical Society of London, latet the Royal Microscopical Society. In 1868, the connection was severed, Busk retired, and the journal was carried on by Lankester, at first alone, and later assisted by his son Ray, who became somewhat critical of his father's dilatory editing. Under the guidance of Ray Lankester, it became one of the most celebrated periodicals devoted to biology, and from its pages alone one can obtain a very fair idea of the progress of comparative anatomy, bactetiology, and protozoology. Edwin, like his son, liked to work with the microscope, and in 1857, wrote a little book, *Half Hours with the Microscope*, which became very popular, and was reprinted unaltered as late as 1918.

[6] Edwin Lankester, 'On the Teaching of Natural Science in Schools', *Quarterly Journal of Science*, 6 (1869): 378-88, p. 380.

In 1845, Edwin married Phebe Pope of Highbury, daughter of a former Manchester millowner, and sister to the noted Parliamentary barrister, Samuel Pope, whose massive build made him a favourite target for the cartoonist. The marriage, as Edwin's diary makes clear, was a lovematch. Neither had any private fortune. Phebe, only nineteen when she married, was a woman of culture, ability, and strength of character. Like her husband, she was a competent botanist and microscopist. She published a number of books, mainly for boys and girls, and wrote natural history articles. Huxley liked her, and wrote to her in his most gallanr style.[7] She knew George Eliot, who had great respect for her. Huxley became a close friend of the whole family, and a regular visitor ro the Lankester home. Phebe Lankester died in 1901.

To the Lankester home at 8, Savile Row, there visited a fine array of talent and advanced thought, for Edwin was not only an educationist, and a medical and sanitary reformer of the school of Wakley; he was a Dissenter in religion, and a Radical in politics. The Lankesters knew the philosopher and mathematician W. K. Clifford, whose dictum it was that to believe anything without sufficient grounds was intellectual immorality. In his diary, Edwin makes some interesting observations on the struggles of his time:

> January 11, 1839. Should things come to blows, the grand question is for each individual to choose his side. I am no Chartist, but there lies the interest of the masses, and the inrerest of mankind. Should things come to a rupture, the side of the people will be my choice, however injudicious some of their movements may appear.

He was a strong supporter of the North duting the American Civil War (as was Huxley), and at home supported the movement for the extension of the suffrage. Forbes Lankester wrote in his father's diary: 'Papa very much wanted J. S. Mill to get in for Westminister, but Smith and Grosvenor have got in instead.'[8] In 1868, Edwin and his wife visited Ireland, and Edwin wrote in his (later) diary in the strongest terms of the wretched plight of the Irish peasantry, and of the evils of absentee landlordism.

[7] Three letters from Huxley to Mrs. Lankester are preserved in rhe Lankester family papers.

[8] Melron House Record (Edwin Lankester's later diary), 24 April 1865 (Lankesrer family papers).

The Lankesters had eleven children, of whom eight survived. Despite current scepticism concerning Victorian family life, it really does seem to be true that the Lankesters were a happy family. Edwin was a kindly and indulgent man. The children grew up imbued with a strong family solidarity and social sense, expressed in a dedication to worthy causes. The eldest daughter, Fay, became Secretary of the National Society, and worked for fifty-one years for this organisation, which continued much of her father's work for sanitation and hygiene. Neither Fay nor her sister, Nina, had much in common with the Victorian 'young lady', but could claim to be among the first representatives of the 'new woman'. Nina became one of the first women clerks of the Post Office; she died in 1923. Marion (Minnie) married a parson, Sydney Vatcher, and lived in Stepney; she left an adopted son and died in 1933. Owen Lankester became a well-known Wimpole Street physician. Forbes Lankester followed the law, becoming a London magistrate, and in this capacity displayed the Lankester freedom of mind, defending street traders from police interference. Owen Lankester died in 1933, and Forbes two years later. The only son to found a family was the second, Rushton. Emigrating to Sourabaya in Java, he married Catherina Rica Riesg, daughter of a planter, and reared a family of seven. The daughters Fay, Nina and Jessie remained unmarried, devoted entirely to rheir work.

Edwin Ray Lankester was born on May 15, 1847, at 22, Old Burlington Street. He owed his second Christian name to the fact that his father had been editing the memorials of the famous seventeenth-century naturalist, John Ray, for the recently formed Ray Society. While still a child, Ray Lankester met many of the most celebrated men of science of his time, and long years afterwards recalled with warm affection the 'dear old men who were my father's friends, and under whose charm and inspiration I grew up'. There were the botanists Henfrey and Hooker, the marine zoologist P. H. Gosse, (much admired by the mature Ray Lankester), and the anatomist Richard Owen. A constant visitor was Edward Forbes, the gifted geologist and pioneer marine biologist. Forbes became Professor of Natural History at Edinburgh in 1853, and his death six months later was bitterly deplored. The family long remembered the genial and kindly man of the worn coat and dishevelled hair, and preserved the whimsical sketches he drew for the Lankester children. Charles Darwin, who was soon to throw his bombshell into the world, told Ray of how he had ridden on the back of a giant tortoise. Huxley carried young Ray on his back at Felixstowe. He had not long returned from the voyage of *H.M.S. Rattlesnake*, and was teased by his friends as the

man who had been round the world, and had seen nothing but jellyfishes – a reference to his important work on the coelenterates. Other close friends at this period were W. B. Carpenter, the microscopist, Edwin's old University College friend, and Darwin's friend and mentor J. S. Henslow, whom Edwin had met during a visit to Cambridge in 1835.[9] It was Henslow who introduced Ray to the delights of fossil hunting. Before he was adolescent, Ray had also met Charles Lyell and Roderick Murchison.

Stimulated in this way, the boy's natural talent and interest in science developed precociously. He possessed a microscope when he was seven, and before he was nine knew the scientific names of the common birds, and could easily distinguish them. His father, who was a handy man to have on a country walk, encouraged him to observe and collect, but gave little formal instruction. In 1855, Ray was sent to a boarding school at Leatherhead in Surrey, pleasantly situated close by the River Mole. From here, a constant stream of boyish letters tell of the trials and occasional triumphs of schoolboy life. The mature Lankester thought that he expressed himself best in letters, especially in those he wrote to his mother, to his sister Fay, and to his friend Thiselton-Dyer. Phebe Lankester preserved all the letters sent to her by her son, whose genius she recognised, and whose career she carefully fostered. For forty years, Ray Lankester described, advised, confided, lamented and sometimes chided in the pages of this remarkable correspondence.

The following is an example of a letter from his school, sent on October 24, 1855.

> My dearest Mama,
>
> I am very happy, and my diahrea [*sic*] has quite left me. I shall jump out of my skin when I see you. I could not find Marchantia polymorpha in Babington [probably C. C. Babington's *Manual of Botany* of 1843], I mean the Marchantia, not the Rhinanthus. I am glad you corrected me, for I might have made a very silly mistake. Tell dearest Papa I mean these letters for him as well as you, do not let him think me unkind. I take my roll to bed with me, on Sunday morning I was looking at it, then I put it under my pillow. I am learning a piece of poetry called The Arab to his Favourite Steed. In Latin I have got to the Second declension and am at the top of the class. I have been put back in French, but I am getting on vety nicely. I learn a Duet called long

[9] Edwin Lankester's diary, 6 April 1835 (Lankester family papers).

long ago which you might get for Fay. I will tell you the set next letter. There is a Total eclipse of the moon tomorrow in the morning visible at Greenwich.

Goodbye my dearest mother,
I am,
Your ever loving son,
Edwin Ray Lankester.

Ray soon adapted to school life, although he showed a disposition to answer back and justify himself when reproved. This disturbed the headmaster, Mr. Payne, who would cry, 'Monitor, put down Lankester's name', and thus prevent Ray from obtaining the coveted good-conduct card, and of being numbered with the 'optimi'. His size of body and intellectual superioriry enabled him to make friends with older boys, but earned him the reputation of being conceited. To all his confessions and complaints, his parents replied with long exhortations to master himself and obey his teachers. Sometimes the boy's knowledge of natural history enabled him to impress his school-fellows. On an afternoon walk, a boy found a slow worm (a harmless creature with a superficial resemblance to a snake). He thoughr it poisonous, but Ray told his companions that he kept these animals as pets, 'at which they took heart'. In the pleasant countryside surrounding the school, the boy pursued his nature interests. He wrote constantly of his discoveries (each meticulously named), and filled his cupboard with fossils. Even his Christmas presents to his parents consisted of folders with painstaking coloured drawings; in 1854, a folder of birds; butterflies in 1856, and birds' eggs in 1858.

In January 1858, Ray Lankester proceeded to St. Paul's School, Colet's historic foundation, numbering among its pupils Milton, Camden, Pepys, Marlborough and Nelson. Every morning, Ray walked to school from the family home to receive his daily quota of the strictly classical education presided over by Dr. Kynaston, the High Master. Lankester said many years later that while at school he had never been genuinely taught by anyone.[10] The pupils were all taught in one room and were dismissed at four in the afternoon. Ray would make his way home along Fleet Street and the Strand, or perhaps along the river. He discovered small eating-houses in side streets off the thoroughfares, or dined (for sixpence), at the 'Boiled Beef House', on that comestible, served on pewter plates, and with

[10] E. Ray Lankester, 'Compulsory Science versus Compulsory Greek', *Nineteenth Century and After*, no. 409 (March 1911) (from an offprint in the Lankester family papers).

draughr stout in pewter pots. These places have long since vanished, and St. Paul's School has reappeared in red-brick disguise in Hammersmith, with some four hundred boys added to Colet's original one hundred and fifty-three.

Ray did well at school, being headboy and prizewinner in successive classes. Glaisher remembered him as a prefect, walking up the schoolroom with his silk hat in his hand, and sometimes rather late. Ray was happy at St. Paul's, because it was a day school, and he had leisure to pursue his scientific interests. Ray had a den, his beloved back room at 8, Savile Row. Here, with the assistance of sister Fay, he kept his growing collection of fossils, minerals and natural history specimens.

It was to be expected that the young Lankester would attend the public science lectures, the glory of the time. He heard Tyndall lecture on glaciers at the Royal Institution, and was present when Faraday gave his juvenile lectures in December 1859 and January 1860. He took notes of these lectures, adding his own records and observations. The young Faraday had done the same when he had attended Davy's lectures. In November 1860, Ray noted, 'Dr. Hoffman has been giving a course of lectures on Chemistry, which I have attended'. He heard the great German chemist again in 1864. 'They were the best teaching I ever listened to.'[11] He wrote anxiously to Richard Owen for a ticket to his lectures at the London Institution. However, it was not to Owen, but to Huxley, that Ray devoted a special notebook, and it was on Huxley that he tried to pattern his life. He even played truant to attend Huxley's lectures. 'I followed him with my notebook to the Geological Society, and wherever else I could hear him, and was encouraged at all times by his fatherly advice and assistance.'[12] These early notebooks are indeed a remarkable record of Ray's hunger for the facts of science, and constitute a record of his general intellectual progress.

On the beach at Felixstowe, Ray hunted for marine specimens, while the Felixstowe cliffs gave endless employment for his new geological hammer; for capping these cliffs was a deposit of the 'Red Crag' of Suffolk, rich in shelly fossils. A number of notebooks show Ray's progress. They are the work of a boy who had received less formal teaching in science, than is given today to a boy in any secondary school. The opening entry is dated August 7, 1858.

[11] *Ibid.* This would have been A. W. von Hofmann.

[12] Quoted from the autograph copy of Lankester's speech to the Royal Society on receiving their Gold Medal, 1885 (Lankester family papers).

> Collection on the rocks. Notes on 'zoophytes'. Notes on Felixstowe insects. Fine drawing of a gnat larva. Notes and drawings about jellyfishes. August 26. Note on a porpoise cast ashore, 'dissected by Papa and Mr. Mayor'. Note on a 'mermaid's purse', with embryo fish. Note on the mineral *selenite.* Note on the formation of flints. Notes on algae and protozoa. Butterfly scales. Notes on various walks, detailing geological features noted, and animals and plants collected. Notes of various lectures attended, on fossils, diatoms, etc. Visit to a railway cutting, and discovery of rare fossil fishes.

A second notebook, from July 1861 to 1862, contains more advanced work, and provided material for some of his early articles.

Lankester's first scientific publication was a letter to *The Geologist* in 1861, published (thanks to a mis-print) as being from a Mr E. Ray of Lanbeater. This referred to a new specimen of the Mesozoic mammal *Stereognathus* from the Stonesfield slate of Oxfordshire, which he had obtained from a schoolfellow. Lankester later described the unfortunate fate of this fossil jaw.[13] He took it to Huxley at Jermyn Street for cleaning, but then wore off the vital cusps of the teeth when taking it back home to draw a lithograph. The mutilated specimen was returned to Huxley, but subsequently disappeared.

Meanwhile, he did consistently well at school, writing, 'I am top', 'I am top still', 'I am still top of the class ! ! !' In 1862, he visited Wales and wrote back excited and enthusiastic descriptions, the first of a long series, in which he describes minutely and with great power, his travels at home and abroad. Of course, he collected continually; molluscs, sponges and worms went into the ever-ready bottle of spirit, and the geological hammer found continual employment. In the same year, he made his first trip abroad, visiting France in the company of his aunt Emily. His letters home show amusingly enough, the impact of France on a young Englishman, with a proper contempt for things foreign.[14]

> I can't say on the whole that Paris was such a nice place as I expected. It is all pleasure there, cafees, [*sic*] cafees, cafees everywhere. The sryle of building too, although very beautiful, and

[13] See Lankester's footnote to E. S. Goodrich, 'On the Fossil Mammalia from the Stonesfield Slate,' *Quarterly Journal of Microscopical Science,* 35 (1895): 407-432, pp. 424-425.

[14] Undated letter (Summer 1862) (Lankester family papers).

> white as snow, is not *solid*. ... I talked to Frenchmen about the two countries, and they think there is nothing anywhere to compare to Paris. I passed by the Tuileries on a bus, and a Frenchman who was with me said, Voilà, dere is a place for you! *I merely observed that the Houses of Parliament were equal to it* ... On Wednesday I was at the Palais de Luxembourg. I went all through, and saw the Salle de Throne, where the old fool of an Emperor places his elegant carcase. ... The Museum of the Luxembourg is very fine.

Some letters of 1863 and 1864 were written to Rushton with all the assurance of an elder brother, and a St. Paul's senior.

> I am looking forward to your coming home for several reasons. I shall have left St. Paul's you know, and I think we might go somewhere together ... The examination is on, and on the fifteenth come the speeches, to which his Royal stupidity the *Purrince* of Wales is coming ... also innumerable other swells and bishops and boobies ... I have two papers to be published this quarter, one on fossil fish, and another on microscopical science. My essay on 'Town and Country' was placed second by old Kynaston, which is very good ... The back room where I write is as comfortable as it can be. I have a stunning fire burning away, and plenty of pipes and tobacco, but I want you to sit with me and enjoy them too. Last night we had some fellows up, and we had mulled claret which I made, and coffee and cards ... [Here is appended a sketch showing Ray seated, pipe in mouth, before his fire, while in the background, a master belabours Rushton with a cane] ... I have just rearranged my brachiopods and put in new ones. I am keeping them in the old cabinet ... they occupy eleven drawers now.

When a student at Edinburgh, Edward Forbes had founded a select club called the 'Oineromathika', compounded from the Greek words for wine, love and learning. Ray did the same, although the name caused some uneasiness, and led to an interview with Dr. Kynaston. The group produced a magazine written in an exercise book. It is well-written and amusing, and obviously owes much to Ray Lankester's superior powers. Particularly interesting is a bold and clever cartoon celebrating Colenso's critique of the Pentateuch, the first part of which had appeared in 1862, and created an immense sensation. A top-hatted, gaitered Colenso, whip

in hand, drives before him in panic rout, the animated books of the Pentateuch. The cartoon shows how far Ray had already travelled from the simple faith of his father.

In 1864, three important events occurred in the Lankester household. Ray left St. Paul's; the family moved on the twelfth of November into Melton House, Hampstead, a more commodious dwelling, and Edwin began a vigorous campaign for the Middlesex coronership. Once installed in his new home, Edwin commenced a family diary, the 'Melton House Record'. This is written with a truly Huxleian illegibility, legible passages being supplied by his wife and children in occasional entries. On July 7, Edwin was elected, after a fierce contest, Coroner for Central Middlesex by forty-seven votes in a poll of over a thousand. Edwin proved to be a worthy successor to Thomas Wakley, the first of the medical coroners. He became known as a fearless man, insisting on proper medical evidence, 'watching over the deathbeds of the poor', as one paper put it, and raising the office of public coroner in esteem. Moreover, he continued the practice he had started as M. O. H. of St. James's Parish, of issuing leaflets on health topics for public distribution, and issued detailed annual reports.

Outstanding in Ray's memory was his 1863 visit to the British Association meeting at Newcastle. Here he watched his schoolfriend W. F. L. Glaisher go up in a balloon with his father, a celebrated and daring aeronaut, who was soon to ascend to a height of seven miles in a famous (and near-fatal) balloon attempt. In the following year he went up to Downing College, Cambridge, to begin a new phase in his education.

- 2 -
The Universities

In October 1864, Ray Lankester proceeded to Cambridge, as a Scholar of Downing College. Downing was a small college, founded in 1800, and in the sixties did not yet possess its full staff according to statutes. Besides the College Master, there were two Professors, with three Fellows, and about thirty undergraduates. Ray remained here for five terms, migrating to Christ Church, Oxford, in April 1866. At Downing he was soon cosily installed in his rooms, candlesticks and egg-boilet on the mantelpiece, and his favourite pictures on the walls. On his desk-table, brought specially from home, stood his green-shaded oil-lamp, actually a microscope lamp, which was 'much admired'.[1] In his wardrobe hung his cap and gown, and the 'huge white surplice' in which he attended chapel, for religious observance was compulsory.

The young man was naturally sociable, soon made friends, and entered into the general life of the college. His only fear was that he might become a financial burden on his parents. He wrote:

> It is awfully jolly up here, and I like it very much. I should be perfectly happy did I not think of that d-d money ... What a beastly thing this money is. If it were not for that, I should feel comfortable enough here. What I dread is your running short, and my having to run away. In the beginning of November, there will be five pounds or more for matriculation fee ... What will you do if I don't get a foundation? [Founder's scholarship]

The expenses of a Victorian Oxford or Cambridge undergraduate were indeed heavy, even if, like Lankester, he were a scholar (partly supported by the College), and not a pensioner paying full expenses. The Cambridge Student's Guide of 1874 estimated expenses for tuition and board of about £78 per year, and a total yearly expenditure of £202.[2]

[1] All quotations from various undated letters from Downing College (Lankester family papers). For a general account of the College see Pertit Stevens, *History of Downing College* (London 1899).

[2] *Students' Guide to Cambridge*, 1874 edition, p. 90.

Ray was certainly not as well provided for as some students. He mentions one Greenhill: 'lots of tin, plays billiards all day'. Yet Ray Lankester was in no way a specimen of the really poor student, the 'sizar'. With his fellow students, Ray soon showed his capacity for friendship with men older than himself. As at St Paul's, this was the result of his physical size, intellectual superiority, and vigorous personality. He proudly describes his intimacy with the 'best men of the College', Collins, Maccoll, and Brickwood the rowing Captain. However, it was with T. W. Danby that Ray became most intimate. Danby was seven years his senior, and became a lecturer and Demonstrator in the University Laboratory.

The science teaching at the College, presided over by P. W. Latham, did not impress Lankester. It was all 'cram, cram, cram'. Latham did not lecture, but set sixty pages to read from various textbooks, then set a paper of questions. The science laboratory was in a rudimentary state of organisation. Nevertheless, Ray did much work there, including the dissections for some of his early articles. Science was still at a low ebb at both Cambridge and Oxford – a situation that Lankester himself would try to change in later years. Many dons still regarded natural science as a subject that did not belong in the universities.[3] Years later, Lankester related a conversation with 'a dear old head of an Oxford College' (actually Brodrick of Merton), who thought science was well provided for in his college, since there was one lecturer in Natural Science.[4] Lankester noted that at Downing College no one read the important German periodicals devoted to biology, Kölliker's *Zeitschrift*, and Müller's *Archiv*, which were sent straight to the library to be bound up.

Ray joined the boating club, rowed well, and soon found himself in the full splendour of white flannel and red maltese cross – one item in a list of expenses reads significantly, 'Plaster for rowing sore, one shilling'. Meanwhile, he continued hard at work, but was not satisfied with his progress, showing signs of a self-doubt and self-criticism that were to remain with him throughout his life. Yet his lists of books read show that he studied hard at physiology, anatomy, histology and chemistry. His articles in science periodicals earned him some money – a useful addition to his income. He joined the College Natural Science Society (now called

[3] For illustrations of mid-nineteenth century attitudes to science and Oxbridge, see H. M. and K. D. Vernon, *A History of the Oxford Museum* (Oxford, 1908) pp. 36, 45.

[4] Lankester, 'Museums', in *Science from an Easy Chair: Second Series* (London, 1912), pp. 310-29, p. 312; see also *More Science from an Easy Chair* (London, 1920), pp. 209-28, 211.

the Lankester Society), and became its Secretary. He was also made an Associate of the University Ray Club. During vacations, he did a good deal of travelling abroad. However, his parents were worried about his future. During the vacation of 1865, he was prevailed upon to visit Oxford, and there learned something of the pioneer work of the zoologist George Rolleston, at work in the new Museum. On his return, he was persuaded, though most unwilling, to try for a scholarship at Christ Church, Oxford.

A series of anguished letters to his parents tell of his unwillingness to go. Everything was going well at Downing, where he counted for something.

> Who is the most illustrious man at either Cambridge or Oxford? I suppose anybody would say, Professor Stokes of Cambridge. Well, *he called on me* in my rooms yesterday. I was out and he left his card. It was about the spectrum of the fluid I had been talking to him about at the Ray. They won't do that for me at Oxford.[5]

His parents were insistent, and Huxley supported them. Ray did in fact obtain his studentship, and according to his father's diary, left for Oxford on April 14, 1866, although he did not vacate his rooms at Downing until October. He held a Junior Studentship, tenable for four years. There is little doubt that the change to Oxford was to his ultimate benefit, but his letters home were filled with bitter complaint.[6] Their tenor was simple – Oxford was less democratic and more expensive than Cambridge; how he hungered for 'dear little Downing'. He wrote:

> I feel in a curiously depressed state of mind .... What a fool I was not to stay at Cambridge. There I was as you know in a flourishing state. I had lots of good friends among undergraduates and dons, and was getting on well ... here I have become quite different.

He found it difficult to make friends because of the 'disgusting toadying and aristocratic system'. He was acting on judgement and repenting by feeling, as he so often did. It was now that there began to

[5] Letter of 12 April 1866 (Lankester family papers).

[6] Letter of his 'early Christ Church period', undated (Lankester family papers).

develop his bitter dislike of the whole aristocratic university system. In his second year at Christ Church, he was introduced to a certain freshman, an 'honourable'.

> I felt a great internal writhe when I looked at him, and thought of the superfluous funds he has at his disposal, while I am shut out from the better society of the House, by not having money.

Still, he admitted that everybody was 'wonderfully kind'. Sir Henry Acland liked him, invited him to the observatory to view sunspots, and asked him to 'work the stars' with him in the evening. Acland, Regius Professor of Medicine, was a mild reformer, and the moving spirit behind the foundation of the Oxford Museum. Ray was coached thrice weekly by Vernon Harcourt, who considered him rather independent in his attitude. Sometimes he was invited to a 'perpendicular' (described by Baldwin Spencer as an 'evening dress entertainment most awfully stiff') at Sir Benjamin Brodie's, Professor of Chemistry at Oxford. Rolleston too, recognising the young man's talent and paid him the compliment of inviting him to special dissections. Meanwhile in London, Huxley and Carpenter watched his progress.

Rolleston's position at Oxford was difficult. In 1860, he had been appointed to the newly-founded Linacre Professorship of Physiology and Comparative Anatomy. He was expected to teach human and comparative anatomy, as well as physiology and anthropology, and was badly overworked. In a letter of 1867, Ray Lankester commented: 'He is going away for a holiday on Tuesday. He has not left his work here in Oxford for a year.' Rolleston's wishes for a division of his position were not carried out until after his death. In 1884, Moseley became the Linacre Professor of Human and Comparative Anatomy, while Burdon Sanderson became Wayneflete Professor of Physiology, and Lankester's friend, E. B. Tylor, Reader in Anthropology. Both Rolleston and Acland held the Lees Readership, the germ out of which the science school at Oxford was largely developed. Acland's extensive dissections laid the foundations of the anatomy collections of the Museum, while Rolleston introduced systematic practical activity by the students, instructing them in the dissection of selected animal 'types'. Rolleston was a lukewarm Darwinist, a good lecturer, as much at home with the classics as with the scalpel.[7]

[7] See George Rolleston, *Scientific Papers and Addresses* (Oxford 1884, 2 vols.), vol. 1, 'Introduction'.

Lankester found the libraries somewhat deficient in German scientific literature, but made friends with Clifton, the Professor of Physics. ('He has no Oxford manners, thank God.') Clifton gave him valuable help with spectrum analysis, and showed much interest in Ray's experiments with the fluids obtained from various invertebrates.

After about a year at Oxford, Ray was ready to admit that he had benefited by the change. It was the social side of Oxford that irked him, then as later in the nineties. He complained that he had had to drop friends through not being able to invite them to dinner; he estimated that the mere move to Oxford had cost him £86. He took Rolleston's advice, and quickly passed his 'Mods', the first public examination, roughly equivalent to a modern intermediate.

It was at Christ Church that he made lasting friendships with two men of his own calibre, H. N. Moseley and W. T. Thiselton-Dyer. He met Moseley in Rolleston's laboratory at the Museum.[8] He was the son of the Canon of Bristol, and in 1871 Ray visited his home at Olvaston, not far from the Severn. It was on this occasion that they dissected and prepated the skeleton of a 'grampus', later donated to the Oxford Museum. Lankester said that the smell of that carcass lingered long in their memories! Ray was attracted by Moseley's 'deep-rooted scepticism', his determination to see for himself. They took their degrees together in 1868, and collaborated in an article on mammalian teeth.

Thiselton-Dyer was four years older than Lankester. He thought at first of going to India but was persuaded by Lankester and Moseley to remain and took a first class in Natural Science in 1867. By 1872, he was Professor of Botany at South Kensington, and organised the botanical side of Huxley's science course. He married Hooker's daughter Harriet, and became Director at Kew Gardens in 1885. Many stories were told of his autocratic nature, especially in his later years. Lankester trusted him, and carried on a vigorous correspondence with him for many years. Dyer's work as a botanist was in general parallel to that of Lankester as a zoologist, both introducing German methods into Britain.[9]

When Ray took his final examinations for his degree, he took a gloomy view of his prospects. He feared for his physics, and the physiology

[8] Lankester's obituary notice of Moseley, *Nature*, 44 (1891- 92): 79-80.

[9] For information on Thiselton-Dyer and other botanists of the period see F. O. Bower, *Sixty Years of Botany in Britain (1874-1935): Impressions of an Eye-Witness* (London, 1938); see also Thiselton-Dyer's article in the Huxley centenary supplement to *Nature*, 'Plant Biology in the Seventies', *Nature*, 115 (1925): 709-12 and W. B. Turrill, *The Royal Botanic Gardens, Kew* (London, 1959).

questions were not those he had 'got up'; he felt 'written out and exhausted'. He need not have worried; on June 12, 1868, he wrote home: 'I have just seen the class list. It is, First Class – Lankester, Moseley, Smart. So I have done the trick. It is all over.' He now decided to take the medical course, and spent some time at St. Bartholomew's, and at the Middlesex Hospital, taking his course of 'anthropotomy', all of which he felt boring and absurdly elementary, although he did see the celebrated surgeon Paget operate. Meanwhile the affairs of the *Quarterly Journal of Microscopical Science* had reached a crisis. Edwin Lankester was running the journal since Busk had retired in 1868, and Ray complained bitterly about his father's dilatory editing. In 1869, he was announced as co-editor, becoming sole editor in 1878. Thenceforward, the journal gradually took on its modern form.

On February 19, 1869, Edwin Lankester noted with satisfaction in his diary: 'On Friday, Ray returned from Oxford, having won the Burdett-Coutts Geological Scholarship, which is worth £150.' There was a curious sequel to the winning of this scholarship. During his later Oxford days, Ray Lankester visited London frequently, and on one occasion was taken ill at rooms in Hertford Street, Mayfair. The philanthropic Baroness, whose scholarship he had won, heard of his illness and insisted on sending him dinner every day. 'Every evening a footman in full regalia arrived with silver dishes, filled with delicacies to tempt the appetite of the invalid.'[10] Fifty years later, an Oxford Committee brought forward Lankester's scholarship papers as an example of how much geology a candidate had been able to offer, so many years before.[11]

Refusing an offer from Dilke to write the scientific columns of *The Athenaeum*, Lankester in December became a candidate for the Lees Readership in Anatomy at Christ Church. He set about obtaining the necessary testimonials (a task he hated), and succeeded in procuring letters from Darwin, Huxley, Owen, Allman and others.[12] These were hurriedly printed, together with a list (not entirely accurate) of his scientific papers. On Christmas Day, Edwin penned an enrry in his diary which throws an interesting light on the intellectual atmosphere of the time.

[10] This incident is described in a printed autobiography preserved as proof sheets, although apparently never published (Lankester family papers).

[11] Lankester to H. G. Wells, 3 November 1924 (Wells papers, University of Illinois).

[12] Darwin wrote: 'I have read several of Mr. E. Ray Lankester's papers in various journals, and have been struck by the ability and knowledge shown in them, and by his zeal for science. Therefore in my opinion, he would be well qualified for giving →

> Ray did not get the Lees Readership. A gentleman of the name of Thomas [actually Barclay Thomson] obtained the suffrages of the electors. Ray was decidedly the best man, and ought to have had it. Various reasons were assigned:
>
> 1. Ray's views are *too advanced.* He had testimonials from Darwin and Huxley
> 2. Ray is *personally disliked at Oxford,* on account of his *want of complaisance* with things as they are.
> 3. I wrote an article in the Quarterly Journal of Science which offended the Dons at Oxford, and *they had wreaked their vengeance on Ray.*
> 4. The Fellows of Christ Church, the electors, are utterly ignorant of natural science, and it was quite a chance whether one man or another gor it.

Huxley had already warned Ray that his father's article would injure him at Oxford.[13] The offending article had appeared in July, and boldly attacked the Universities:

> Our school system has grown up from a period when there was little or no natural science to teach ... Under these circumstances, the teaching of the classics and mathematics has become a kind of institution around which the feelings of those who have been educated under their influence have clung, ... as around a political system whose existence is regarded as the palladium of the state ... The only way in which this opposition can be overcome is by attacking the universities. It is here that the natural sciences meet with their first rebuff ... Who has not been examined before a committee of the Houses of Lords or Commons, has not wondered at the ignorance displayed by our legislators of the commonest facts known to the scientific man.[14]

lectures in Biology, and for leading students along the right path.' This letter, dated June 25, 1869, was printed along with other material to form a testimonial that Lankester submitted in support of his application (Lankester family papers). See also F. Burkhardt *et al.*, eds., *A Calendar of the Correspondence of Charles Darwin* (New York, 1985), letter 6803.

[13] Letter of 23 August 1869 (Lankester family papers).

[14] Edwin Lankester, 'On the Teaching of Natural Science in Schools', *Quarterly Journal of Science,* 6 (1869): 378-88, p. 381.

Edwin here anticipated his son's later attacks on the 'classical system' in education. Ray himself was inclined to think that these attempts to obtain a post were 'precocious', and did him harm, and discounted his father's opinion that Rolleston was hostile to him. In fact, there was some friction with Rolleston, although the rupture did not come until 1873, not long before Lankester left for London (see chapter 5 below).[15]

In a letter from Clermont in early April 1869, Ray Lankester for the first time mentioned his work for the Oxford University prize essay on 'Comparative Longevity', saying that he found it difficult, and had not got much further with it. However, the essay was finished by the end of May, duly won the prize, and was in print the next year. It is a pioneer study in a field which has only recently become the object of serious research, and is certainly a remarkable work to have been produced by a man in his early twenties. In March 1870, Edwin Lankester wrote:

> Ray's 'Comparative Longevity', was published the week before last – a good critique in the 'Spectator', and his friends speak highly of it. We have had letters from C. Darwin, H. Spencer, Sir W. Holland, speaking highly of the book as an effort to deal with a very difficult subject.[16]

In a letter to Edward Youmans, Spencer commented on the fact that Oxford University had been willing to award a prize for an essay on evolution.[17]

After a summer visit to Heidelberg University ('better supplied with laboratories than Oxford'), Ray began to work hard for the Radcliffe Travelling Fellowship. This was worth £200 for three years, half of which period had to be spent on the Continent. It was open to first-class graduates in natural science. He duly sat for the examination, which was very severe. Early in 1870 he decided, on the strength of his Burdett-Coutts scholarship, to accompany his friend Moseley to Vienna. His purpose was exploratory, to find out about the important biological work now developing on the Continent. The very day after he set out, his family were informed that he had gained a Radcliffe Fellowship.

15 Thiselton-Dyer to Huxley, letter dated 22 November 1880 in W. R. Dawson, *The Huxley Papers* (London, 1946), but probably dating from this earlier period (Huxley papers, Imperial College).

16 Edwin Lankester, 'Melton House Diary', March 20, 1870 (Lankester family papers). The essay was published as *On Comparative Longevity in Man and the Lower Animals* (London, 1870).

17 Herbert Spencer, *An Autobiography* (London, 1904, 2 vols.), vol. I, p. 216.

– 3 –
The Wander Years: Vienna, Leipzig and Jena

Lankester and Moseley spent some days in Paris, then went on to Munich by way of Strasbourg. Munich impressed Lankester by its magnificent collection of paintings, especially those of Rubens, 'the most astounding artist who ever used a brush'. There were scientific as well as artistic diversions, for they called at the Anatomical and Physiological Laboratories and presented a letter from Rolleston to Pettenkofer, the Professor of Medical Chemistry. Max [von] Pettenkofer was a great authority on epidemics and hygiene. Some years later he showed his contempt for Koch's cholera bacillus by drinking a whole tube of the germs – without effect, as it so happened! He showed the visitors over the laboratories, where they saw 'his wonderful chamber for experimenting on the excretions of living animals'. Ray was already becoming bored with Germany, and began to long for the southern country round Nîmes, with its dark olive and cypress trees and intense sunshine. It would be nice to go to Italy too: 'However, I shall stick to my plan. Another year D.V. I go to Italy.'[1]

The friends arrived in Vienna about the second week in March, and took up lodgings in the third storey of a house near the Allgemeines Krankenhaus, the General Hospital. A first sight of Vienna was disappointing – muddy streets, chilling winds, the Danube broad and murky, with enormous masses of snow on its banks. The two young Englishmen delivered their letters to Karl Rokitansky, Professor of Pathological Anatomy. Rokitansky and the anatomist, Joseph Hyrtl, received the young pilgrims kindly and made arrangements for their courses. The immense hospital impressed them very much.

> The hospital is a most gigantic place, though there are only two thousand beds. It is built in nine great quadrangles, and is really splendid. For a man who has finished his studies elsewhere, it must be most capital to come here and take two or three special courses. We went yesterday to the lying-in wards, and saw three

[1] Letter of 6 or 7 February 1870, from Strasbourg (Lankester family papers).

> births ... There were two ladies at the lecture, and about forty male students. The ladies seemed quire at home, and pulled about the preparations, and discussed them with the students.[2]

Ray soon decided againsr a long stay in Vienna. It was fine as a medical school, but other places would be better for physiology, and pleasanter in the way of student life.[3] The food drove him to loud complaints, but there were compensations. Vienna was nothing if not musical; there was the opera, which the two friends attended regularly, and for a few coppers one could hear the King of Hanover's band.

During these first few weeks his visit nearly came to a sudden end. Presented by the police with the customary forms, Lankester wrote under the heading 'religion', the words 'gar keine' (none at all), because 'in their sense I have none'. The police sent the form back, convinced that a joke was intended. Presumably Lankester obeyed the earnest solicitations of his hosts to write something – Catholic, Jew or Lutheran!

Evety morning, Ray rose at half past eight, took his coffee, and passed two hours perfecting his German with Herzfeld, an assistant physician at the Hospital. From eleven to one, he studied human anatomy, then dined at Krieg's Restaurant. Thence he went to Professor Salomon Stricker to work with the microscope until five thirty. 'Today, I have been working at histology with Professor Stricker, to whom I am going every day', wrote Ray to his parents. Stricker, 'a gifted Jew'[4] had in 1868 become Professor of Experimental Pathology, and Director of the new Institute attached to the General Hospital. He was an outstanding investigator and teacher, one of the first to use projection methods in teaching. He had immense energy and patience and on one occasion spent ten consecutive hours at the microscope, watching cell division. Lankester had great respect for his teacher.

Stricker's histological methods were consideted advanced in his day. Microscopic technique was soon to make spectacular advances, but in 1870, methods were still very simple and crude, not much advanced from the observation of unstained tissue practised by J. J. Lister and other pioneers of the achromatic microscope. Lankester championed the methods of Stricker and Schenthauer in the pages of *Nature*, then in its

[2] Letter of 12 or 13 March 1870 (Lankester family papers).

[3] Lerrer of about 14 March 1870 (Lankester family papers).

[4] Lankester pencilled these words after Stricker's name in his copy of his article 'Culture and German Culrure', which appeared in rhe *Strand Magazine*, 49 (1915): 31-39 (Lankester family papers).

second year of publication.[5] These methods are outlined in the manual of *Human and Comparative Histology* edited by Stricker, who contributed the first chapter on 'Genetal methods of investigation'.[6] Stricker pioneered a primitive form of wax embedding of tissues prior to sectioning, bur had not, as Lankester thought, invented ir.[7] Stricker's merhod of staining, clearing and mounting tissues in Canada Balsam disrinctly forecast modern methods. With him, Lankester studied the movemenrs of protoplasm, the effects of vatious stimuli, and especially 'diapedesis', the 'outwandering' of the white blood cells from the blood vessels in inflammation, and the movements of the large connective tissue cells of the cornea.[8] He was able to help his teacher by obtaining an article by Lister on the same subject. A good deal of vivisection was practised in the laboratory. Lankester was rather uneasy, but was reassuted when he found that the animals were anaesrhetised. There was a special machine for holding down animals.

> A little dog was running about thete for some days, (two or three orhers besides); one day I came in and found him having his knee excised under chloroform. Two days after I saw his carcase and the leg injected [with red and blue pigments to fill the blood vessels], and studied mictoscopically. Stricker took up one little dog, patted his head fondly, and said, "Ah, my little Kerl, you are the next who will furnish us with such good results, will you not?". Two men are every afternoon walking about with dogs or rabbits under theit arms, and inserted into the animal a thermometer, which they read every five minutes.[9]

With Schenthauer, Lankester made preparations of various cancers, tumours and tubercles from the enormous collection in the labotatory. It was in Stricker's laboratory that Lankester was introduced to the cheaply produced Continental short-tube microscope. The standard English long-tube instrument was a beautifully constructed, heavy and expensive instrument, out of the reach of most students. Lankester was mainly

[5] Lankester, 'The Microscope', *Nature*, 2 (1870): 213-4.

[6] S. Stricker, ed., *Manual of Human and Comparative Histology* (London, 1870, 3 vols.).

[7] Frison, 'The Origin of Wax-Embedding', *The Microscope*, 9 (1953): 131-33.

[8] Lankester, 'The Foundation of Biological Science', *Nature*, 104 (1919): 198-201. This is from rhe Jubilee edition celebraring fifty years of *Nature*.

[9] Lerter of April 1870 (Lankesrer family papers).

responsible for the introduction of the cheap German microscope into Britain, obtaining two for Hooker and Dyer, at a cost of £9 15s 6d each.[10] As a result of the connection established by the Lankester-Moseley visit, Emmanuel Klein came to London as assistant to Burdon Sanderson, and was, after his appointment at Bartholomew's Hospital, the chief teacher of Continental methods of staining, section-cutting and refined histological methods. It was English workers who invented the Cambridge rocking microtome and the consequent method of producing long ribbons of consecutive sections, later an everyday laboratory process.[11]

Lankester tried to interest Stricker in England, persuading him to promise a visit to the next British Association meeting and an article on embroyology for the *Quarterly Journal of Microscopical Science.* During the Easter vacation, Lankester and Moseley travelled to Sryria, but bad weather spoiled the trip. At the end of April he wrote that he would be returning home, since he had to work for his first M.B. in November and especially wished to attend Daniel Oliver's botanical lectures at University College, London. He left on April 23, leaving Moseley still at work with Stricker. He travelled home by way of Prague, Leipzig, Jena and Würzburg, and thence to France. His letter from Strasbourg, dated May 1, 1870, provides a good example of his familial epistolatory style and describes his meeting with a number of famous men.

> Slap-bang, here we are again in Strasbourg. The preceding is not intended to denote joy, car j'aprouve une douce melancholie, weil ich habe den Deutschland hinter verlassen. It is always painful to me changing place, and I hate railways and having to decide what class to travel by, so I consider myself unfitted to be Radcliffe Travelling Fellow, and intend to throw up the appointment. I should like to have stayed in Leipzic, also in Jena, and I ought to have gone to Berlin (and St Petersburg, Samarcand and Pekin too). However, now I am in France again, and intend to go by the express tomorrow to Paris. It is a choice between hours in the train third class, and one sovereign in pocket, and nine hours in the train first class. I think I must spend the £, as I am quite

[10] Illustrated in W. B. Carpenter and W. H. Dallinger, *The Microscope and its Revelations* (London, 1891), p. 210. The importance of the cheap German microscope is discussed in R. C. Maclean and W. R. Ivimey Cook, *Textbook of Practical Botany* (London, 1952), pp. 1-2.

[11] 'The Foundation of Biological Science' (note 8), p. 199.

sick of railways. I travelled all last night and all today ...

At Leipzic, I proceeded first to Professor Ludwig's magnificenr new physiological laboratory, found that he was away from Leipzic from a man who talked German through his nose, and proved to be a most capital and pleasant American ... Dr. Bowditch of Boston, who proved most kind and agreeable in every way ... I spent most of the three days with him, and we became great friends. He showed me all the various rooms of the laboratory in detail, and we had out different instruments to study. He introduced me to Professor Schweiger-Seidel, Professor of Histology. I experimented with him, or rather watched what he was working on. We dined together twice, and went on the second evening to the theatre, where we saw two capital German comedies, which I understood excellently. Leipzic I liked very much. It was a relief to find oneself outside that beastly Austrian Empire, and in a counrry with some real life and activity of its own, not like an animated corpse, as is Austria.

The great Leipzic Fair was going on, and the University term had just begun, so that the place was full. I saw Victor Carus and had some talk with him. On the second day, after some trouble, I managed to find Professor Rudolph Leuckart, who is Professor of Zoology. He was most pleasant, and a very interesting and enthusiastic man, something like Pengelly in manner. He said, 'What, are you Ray Lankester?' (as he came in, in German), 'the author of the 'Earthworm' and other papers? I thought you were a middle-aged man!' He produced cigars, and we talked a great deal on worms, in a mixture of French, German, and English. He then showed me the fine collection; some invertebrates especially interested me, also his laboratory, where a young Russian Dr. Brandt was at work. He then showed me numerous microscopic preparations of worms and other things, and I left him after four hours, much pleased.

Ludwig arrived in Leipzic the evening before I left, and so I went to see him, and just had a little talk with him about studying physiology. I confess I am in great strait as to the choice between Zoology and Physiology, for on Friday and Saturday, I was at Jena, and was nearly all the time with the most delightful man, Professor Haeckel, the celebrated apostle of Darwinism. It quite upset my physiological ideas to talk with Haeckel of studying

marine forms at Messina, and in the Canaries, and to see his beautiful sketches not only of the wonderful living things, but of the scenery, lovely beyond description ... Why should I not Radcliffize there one winter?

Jena has no railway; you drive for two hours in a post-waggon (a snail-cart would be a better name), from a station some three hours on line between Leipzic and Frankfurt. Jena is a curious town, of a square form on account of the battlements, the towers of which remain all around it, and is beautifully situated among highish hills on a little river. The University buildings, Church, and one or two towers quite dominate the town, which has only four thousand inhabitants. Trees and Gardens are planted round about the old battlements, and also in the streets of the town; an air of the profoundest calm reigns, only broken by the shouts or songs of beer-drinking students, who seem to be *the* inhabitants *par excellence.*

I put up at the 'Black Bear', and first tried to find Haeckel at the Zoological Museum, so I tried to find Preyer, the Professor of Physiology; he was out of Jena, and I regret to say I did not return ... Then I went to Haeckel's house and found him. I was surprised to find a youngish man (he looks younger than he is – thirty-six is his age), with that gleam of intelligence in the eye which you may see in many naturalists. We immediately became most friendly. He speaks English, but not well. He took me to the Museum, showed me over various things, then found his wife so unwell, that he could not give me tea in his house, so came with me to the 'Bear', brought some books with him, and there we talked them over, and had beer and tea, until past eleven o'clock. He does not smoke, nor does a very different man, Stricker.

The next day, he called for me early, and we went again to the Museum, and then to see Professor Gegenbaur in the Anatomical Museum. In the afternoon, he went a long walk with me up in the hills, and I took tea with him ... I left Jena by the snail-cart at ten at night ... If all goes well, this day week I shall arrive in England; if coin does not hold out, before that.

My dearest mother, I feel very glad when I find that I **can** make friends ... Such men as Haeckel, Edward van Beneden, Michael Foster and some others I could mention, I have a real sympathy with, and it is pleasant to be with them, as it is unplea-

> sant to be with Moseley – all good will to him, nothing the less.[12] This American too I liked very much, and think he will turn up next year in England. He is one of those quiet, rather sad Americans. He served four years as a cavalry officer in the War, and now is studying Physiology in Germany. He has no trace of military swagger. Would not an Englishman have it ...

Ray Lankester followed out his plan of attending the lectures of Daniel Oliver, the professor of botany at University College, London and also dissected in Burdon Sanderson's laboratory. In early September, he took lodgings in London, in Frances Street, to work for his first M.B. He later recalled this as a pleasant time, enlivened by visits from his friends, especially Dyer and his medical friend Donkin. His diploma is dated December 8, 1870. Meanwhile in November, he had applied for the Natural History Chair at Belfast, vacated by Wyville Thomson. Although he was able to send in a formidable list of testimonials, backed by a list of forty papers on biological topics, he was not successful.[13] He was not seriously perturbed, having put in his application merely to satisfy his parents. In December Michael Foster wrote to Huxley that William Rutherford and Lankester were both anxious to join Huxley's team at South Kensington.[14]

On New Year's Eve, Ray Lankester crossed the Channel for the second stage of his reconnaissance. He would have liked a short stay in Paris, but the Franco-Prussian War was raging and he decided instead to call on his Belgian friend Edouard van Beneden, with whom he had been exchanging letters and specimens for some years. He spent a pleasant few days for the Benedens were charming hosts, and Lankester always responded to

[12] Too much should not be made of Lankester's occasional remarks about Moseley's pushful manners; G. C. Bourne points out that although the two were rivals from the first for scholarships and fellowships, there was a remarkable absence of jealousy between them, see his 'Memoir of Henry Norridge Moseley' prefaced to Moseley, *Notes of a Naturalist: an Account of Observations made during the Voyage of H.M.S. Challenger* (revised edn., London, 1892), p. xvi.

[13] Lankester preserved copies of his letter of application for the Belfast Chair of Natural History (dated December 10, 1870) and his testimonials (Lankester family papers).

[14] Michael Foster to Huxley, 31 December 1870 (Huxley papers, Imperial College). William Rutherford (1839-1899) was Professor of Physiology at King's College, London from 1869 to 1874 and at Edinburgh from 1874 until his death. According to some he was the original of Arthur Conan Doyle's irascible Professor Challenger, although Lankester himself is also a likely candidate.

kindness and good fellowship.[15] By January 6, he was in Leipzig, and made his calls on the Directors of the two great biological laboratories of the city, Ludwig and Leuckart. The physiological laboratory of Karl Ludwig was a mecca for students of biology. Here Lankester gained his first insights into 'mechanistic' biology. Ludwig had a tremendous reputation as an organiser of his students' work; moreover his laboratory had a distinctly international character. Lankester found there his American friend H. P. Bowditch, who was to leave in September, returning to Harvard to found the first physiological laboratory in the U.S.A. There were also present two Poles, a Russian, an Italian, an Egyptian and three Germans. Lankester tells us their names; Ustimovitch (with whom he became very friendly), Ceradini, Iappeina, Lesser, Huffner, Müller, Kronacher, Boehm, Nawrocki. After some hesitation he decided to 'stick to Ludwig' giving up any idea of working in the laboratory of the parasitologist Rudolph Leuckart. He did sometimes visit this scientist, and struck up a friendship with his assistant Dr. Nitsche, later a Professor in Saxony.[16]

Lankester found Ludwig to be a 'very nice old fellow, soft- hearted and cynical and philosophical and simple at the same time'.[17] He was on excellent terms with all his students. Lankester's day was a full one – up at eight thirty, and at work in the lab. by nine fifteen. There he remained till one; then together with his fellow students, he would lunch at Stolpe's or at the Hotel des Prusses, finding the food very German, and therefore unsatisfactory – the only fish served was carp! From four till five he heard Ludwig lecture. Then came dinner at five thirty, a cigarette or two in a cafe, and then back to lodgings for some private work and reading. As in Vienna, he was devoted to the opera, and heard many symphonic concerts.

In the laboratory he studied, under Ludwig's supervision, the influence of the various cardiac nerves, and learned the methods of vivisection. He also learned the technique of automatic registration by the kymograph, invented by Ludwig in 1847 to record variations in blood pressure. He also performed a good many experiments to be found in the new work on physiology, by Michael Foster and Burdon Sanderson.[18] Some incidental remarks show a certain impatience with the slow and methodical Germans.

[15] Letter of 3 January 1871, from Cologne (Lankester family papers).

[16] The Lankester family papers contain more than twenty letters describing Lankester's sojourn in Leipzig and Jena.

[17] Letter of 26 February 1871 (Lankester family papers).

[18] Letter to Huxley, April 1873 (Lankester family papers).

> This whole morning I did nothing but stick a frog's tongue in a bottle, three precious hours. I was waiting about for Ludwig, and he was engaged with orhers. He 'will' do microscopy with me, and I want to do that for myself, and for him to give what time he does give to me, to vivisection experiments. Tomorrow we are going to work again on a rabbit, or else some experiments on the salivary gland of a dog, to see if, after it is cut out, and an artificial circulation of blood is kept up through it, it will secrete saliva; and if then the secretion can be increased by subjecting it to alternate compression and expansion.[19]

A month later he wrote:

> I worked steadily all the week as usual, frog's tongue and dog's heart ... On Saturday, Professor Preyer of Jena, and Cohnheim of Kiel appeared in the laboratory. Preyer is an Englishman of German parents, and I liked him much. He knew me, and has written and worked on the same things with the spectroscope.[20]

With his fellow student Ceradini, Ray had long talks about Italy, and longed for blue skies and kindly sun. He was beginning to find Leipzig featureless and boring; the six thousand domino-playing French prisoners in the town might well, he considered, be exchanged for an equal number of Parisiennes. When Paris fell, the noisy patriotic celebrations irked him. In this context, it is worth noting that when the Great War broke out, Lankester wrote an attack on German culture, but even here contrasted the attitudes of the old and the new Germany. When he was there as a young man:

> The old traditional culture of Germany was still flourishing; its chatactersitics simplicity of life, love and music, thoroughness, industry and patience in intellectual work. At that time the Prussians were hated in Vienna and Jena and even in Saxony.[21]

During the Easter vacation, he travelled with Bowditch to Würzburg, Heidelberg, Bonn and Jena. At Bonn he met Max Schultze, 'the very first of German microscopists', who received them kindly, and also called on

[19] Letter of 22 January 1871 (Lankester family papers).

[20] Letter of 20 February 1871 (Lankester family papers).

[21] Lankester, 'Culture and German Culrure' (note 4), p. 39.

the celebrated Eduard Pflüger, editor of an important physiological journal.

> Kölliker was not there ... We saw however Professor Fick, Professor von Recklinghausen, Professors Sachs and Semper. The latter is Professor of Zoology, of whom I have heard a good deal. His father died and left him a good deal of money. He spent it all in going out to the Philippine Islands, where he lived for seven years, catching the most beautiful marine animals, drawing them and working at their structure. He married a German lady who draws most beautifully, and has illustrated many of the splendid books he has written ... After we had seen some of his collections, he asked me if I had not a relative of the same name, who had written a great deal on zoology in various journals, and I told him no, but I had written some things myself. He shook hands with me and said, "Dear me, why I thought Mr. Lankester was a much older man". They are always insulting me in this way.[22]

Jena once more sent him into raptures; he could not have enough of the hills, shady walks, beautiful views and cool streams. He saw Preyer again, and Dohrn and Kleinenberg. The latter was busy with his work on Hydra, and Dohrn urged Lankester to come to Jena to study molluscan embryology. Lankester did not need much persuading, for he was convinced that Jena was the 'best place for zoological study now'. He had previously met Anton Dohrn at the Liverpool British Association meeting, when Huxley was President. Dohrn was then thirty, a privatdocent in zoology, known for some good embryological work on the crustacea, in furtherance of which he had passed some months at Naples and Messina.[23] Concerning Dohrn he wrote home,

> Anton Dohrn is a great friend of Huxley; he stays with him whenever he goes to England, and corresponds with Mrs. Huxley. She wishes him to marry Jessie, one of the daughters, but he declines. He tells me, however, that she is very pretty and clever, and he likes the whole family much. Dohrn will go to Naples also at the end of October, so perhaps we shall go down there together.[24]

[22] Letter of 2 April 1871 (Lankester family papers). Karl Semper was Professor of Zoology at Würzburg from 1868. His book *The Natural Conditions of Existence as they Affect Animal Life* (translated 1880) is a pioneering study in what later became known as ecology.

[23] Lankester, obituary notice of Dohrn, *Nature*, 81 (1909), p. 429.

[24] Letter of 29 March 1871 (Lankester family papers).

When Lankester returned to Leipzig, he had made up his mind to move to Jena. By May 1st Lankester could write:

> Here I am at Jena. I left Leipzic yesterday, after immense internal struggles and pains ... Why could I not quietly stay on with dear old Ludwig, and Bowditch and Ustimovitch, and the Knanths [the family of a Leipzic banker with whom he was friendly] ... I have been subjected to the greatest distress as to whether to come here, or to accept this post to assist Huxley. It is not at all improbable that I shall come home at the beginning of the second week of June, in order to do that ... Huxley is Huxley, and is worth all other teachers rolled into one ... I wrote to Huxley, and had a very kind letter from him ... His private opinion was that I had better come to him ... It would be a swell-sounding thing to publish a German paper with Ludwig, though really it would be gross humbug, for nearly all such papers are written entirely by Ludwig, and the experiments devised by him[25] ... It is really a privilege to have a place in his laboratory, and one gets so bound up by seeing him day after day, and then he is a very sensitive man, an in spite of all I said, no doubt feels slighted by my cutting. I really felt my heart in my mouth, as he cast on me a reproachful smile, and said in German, 'So you have really come to take your leave of me! And just as your research was going on so nicely. If you were to stay a month or so, you could experiment by yourself when I could not help you.' I have learned a great deal there during those four months, and I should not have learned so much more by staying[26] ...

In a letter to Rolleston, Lankester doubted about 'going in for physiology'. For a long time his attitude to physiology remained critical. It seemed to him too 'medical', too much based on man, too much an affair of kymographs. As late as 1919, he could still write that physiology had not come under the full influence of the Darwinian doctrine, and that there had been no investigation of the development and survival of functions. It was necessary to study their evolution from simpler types, and to analyse by experiment the progressive series of chemical activities involved in digestion, secretion, excretion, and so on. Physiology, he held,

[25] This is confirmed by Eric Nordenskiöld, *History of Biology* (reprinted New York, 1946), p. 413.

[26] Letter of 4 May 1871, from Jena (Lankester family papers).

was as incomplete as morphology would be if no forms below terrestrial vertebrares had been studied. Lankester, in fact, wanted a *comparative physiology*, such as has developed in our own time.

In Jena, Lankester took up his lodgings in a room overlooking the Prinzessin Garten, where he was kept awake at night by the singing of the nightingales. He took his meals in Dohrn's study, together with the microscopist Ernst Abbe: 'the queerest helter-skelter dinner with nothing to drink. I can't say I admire it'. In the afternoon he would walk with Abbe, Kleinenberg and Preyer in the University 'Paradise', which resembled the Christ Church meadow on a smaller scale. During the weekends, he roamed with Dohrn among the wooded hills, streams and waterfalls of the lovely Thüringer Wald, and discussed exhaustively the eager German's plans for a marine station at Naples. Jena preserved the charm of its historic past, and the entaptured Lankester described it all in his eager, fresh and charmingly naive way.

> You would not be surprised to see Oken and Goethe walking along together, or of Schiller came into the Bear Hotel, and ordered his seidel of ale, or even to go further back in imagination, if you saw a medieval Faust ... I am sure Goethe must have had Jena in his thoughts when he wrote the first part of Faust. There are just the gardens here, which one imagines Marguerite's to have been.[27]

Later he wrote:

> I have just come in. It is eleven o'clock at night. Both my windows are wide open ... It has been very hot in the middle day for the past week, and now a mist very fine and hazy fills the valley, and the moon is shining intensely. It is excessively beautiful, and when there is added to this the thought that these very evenings ... have been witnessed and enjoyed by the greatest men who lived in this place and loved it – Goethe, Schelling, Hegel, Oken and Schiller – you will understand that Jena is a rare and fascinating spot. I shall indeed be sorry to leave it; I feel like a lotus eater, as though I had been dreaming for the past week or two, and shall be glad to wake myself up by being once more in London.[28]

[27] Letter of 29 May 1871 (Lankester family papers).

[28] *Ibid.*

Comparison of Jena University with Oxford made Lankester 'sick with rage'.

> At Oxford, they have the greatest horror of seeing the University Germanised. Well may they, for there is not one in ten of them, who would not be absolutely abolished in the process.[29]

Lankester attended the lectures of Dohrn, Gegenbaur and Haeckel, and was, by Haeckel's kindness, allowed to work in the laboratory. His work here was concerned with the development of the little freshwater bivalve, *Pisidium pusillum*, which he obtained from 'a muddy little stream near Jena'.[30] It was a great privilege, and a tremendous stimulus for the young Englishman to be brought into close and friendly contact with men of the calibre of Haeckel, Dohrn, Abbe, Gegenbaur and Kleinenberg. Jena University did indeed hold a privileged position, for the ducal government was relatively liberal. Gegenbaur, although a Catholic, preferred Jena for its freedom from clerical interference, while Haeckel was able to carry on his anti-clerical campaign for many years. Gegenbaur was a close friend of Haeckel, although he did not approve of his friend's excursions into politics and anti-clericalism.

Lankester was subsequently responsible for the publication in Britain of Gegenbaur's *Elements of Comparative Anatomy*, and of Haeckel's *History of Creation*.[31] The latter was a translation of Haeckel's popular exposition of his evolutionary philosophy, the *Natürliche Schöpfungsgeschichte*. Lankester told Haeckel that he did not feel able to do the translation himself because he was a 'poor German scholar',[32] but supervised the process which led – after some delays – to its publication in 1876. The translation was made by Dora Schmitz, daughter of an old friend of Edwin Lankester. Ray

29 *Ibid.*

30 Lankester, 'Contributions to the Developmental History of the Mollusca', *Phil. Trans. Roy. Soc.*, 165 (1876): 1-48.

31 Carl Gegenbaur, *Elements of Comparative Anatomy* , trans. F. J. Bell, preface by Lankester (London, 1878); Ernst Haeckel, *The History of Creation*, translation revised by Lankester (London, 1876, 2 vols.).

32 Lankester to Haeckel, 25 May [?1872] (Ernst-Haeckel-Haus, Jena). This letter was provisionally dated to 1871 by the Haeckel-Haus, but must be later because it appears to have been written from England, and Lankester was still in Jena on May 25 1871. The Lankester-Haeckel correspondence at the Haeckel-Haus contains a number of letters charting the lengthy process by which the translation was eventually produced. I am grateful to Mario Di Gregorio for providing me with transcriptions of these letters and to Dr Erika Krausse of the Haeckel-Haus for permission to quote from them (P. J. Bowler).

could on occasion be as straightlaced as any of his contempories, and was shocked at many of the things Dora had translated, commenting 'Nice things for a young lady to turn from German into English!'.[33] He chose the title in order 'not to frighten the pious English public' and asked Haeckel for a new preface to replace the original German which he felt too radical for English tastes.[34] After publication, he wrote to Haeckel saying that 'The parsons here at Oxford will be furious when they read it.'[35] The translation thus served many purposes: it introduced a leading German evolutionist's work to the English-speaking public, along with some of the broader implications of the evolutionary philosophy, and also fitted into Lankester's campaign against the Oxford establishment – into which he would soon be dragged even more deeply.

These projects were consequences of the close connections built up with Gegenbaur and Haeckel during Lankester's time in Jena in 1871. Despite the valuable work he had done there, Huxley had advised him to return to London and he soon decided to comply. He wrote to his mother from Jena: 'You may expect me in Hampstead on the 9th or 10th of June.' Soon he would begin his collaboration with Huxley in the modernization of science education in Britain.

33 Letter of 1874 (Lankester family papers).

34 Lankester to Haeckel, 31 December [1872] and 16 November [1872] (Ernst-Haeckel-Haus, Jena).

35 Lankester to Haeckel, 22 November [1875] (Ernst-Haeckel-Haus, Jena).

– 4 –
The Wander Years: from South Kensington to Italy

When he was an old man, Ray Lankester pointed to the portrait of Huxley that hung on his study wall, and said to a friend, 'He is my hero.'[1] For him, Huxley, his 'father in science', stood above every other man, worth all other teachers put together. So it was that he had decided to leave Jena and take part in his mentor's new educational venture. We must imagine Huxley at this time to look not a great deal older than he appears in a photograph of 1857, which Lankester cherished because it showed his teacher in the full prime of manhood and as yet unknown to fame. It was one of a series of portraits of Fellows of the Royal Society. The dark-browed face shows intellect, earnestness and humour.[2]

Much had happened since this photograph had been taken. In 1857 Huxley had been Professor at the School of Mines in Jermyn Street for only two years.

> Up in that street of Jermyn,
> We've been lecturing every night,
> Such Mammoniferous sermons,
> With the room at 98.[3]

Having begun his career as an invertebrate morphologist, Huxley's position at the School of Mines now required him to pay more attention

[1] Berta [Amy Roberta] Buck, *A Storyteller Tells the Truth: Reminiscences and Notes* (London, 1935), p. 310. Lankester expressed his appreciation of Huxley in his 'Memoir on T. H. Huxley', *Natural Science*, 7 (1895): 120-1 and in his contribution to the Huxley supplement to *Nature*, 115 (1925): 696-725, see pp. 702-4.

[2] This is the portrait used as the frontispiece to volume 2 of Leonard Huxley, ed., *The Life and Letters of Thomas Henry Huxley* (reprinted London, 1903, 3 vols.).

[3] Soliloquy', an unpublished *jeu d'esprit* written by Huxley for the 'Red Lions', a dining club linked to the British Association for the Advancement of Science (Lankester family papers).

to fossils. His research for a naturalistic explanation of the relationship between organic forms led him to champion Darwin's theory after the publication of the *Origin of Species* in 1859, although recent studies have shown that his reaction to Darwinism was complex.[4] He was never happy with the theory of natural selection, treating it only as a hypothesis that demonstrated science's ability to tackle the question of the origin of new forms of life. Nevertheless, his outspoken defence of Darwinism in the public arena made him a notorious and eventually a famous figure. Modern studies have reassessed his famous confrontation with Bishop Samuel Wilberforce at the Oxford meeting of the British Association for the Advancement of Science in 1860.[5] It appears that in this case, at least, Huxley's efforts were less effective than later evolutionists have imagined, although in the long run his defence of evolution was more successful. In 1874 Lankester was with him in Belfast when he turned down his notes and delivered his lecture on 'Animals as Automata' extempore.[6]

There can be no doubt that Ray Lankester remained Huxley's most faithful disciple, carrying out not only the biological, but also what may be called the socio-political testament of his master. One biographer has stressed the democratic significance of Huxley's work;[7] in his pupil, this aspect is even more strongly marked. Yet Huxley did not at first think of Lankester as his successor. He considered the brilliant F. M. Balfour the future doyen of British zoology – but Balfour was killed in a climbing accident in 1882.[8] Lankester had become one of Balfour's closest friends, and bitterly regretted his death. It seems probable that Huxley's original preference for Balfour was based on a fear that Lankester's ardent temperament would lead him into difficulties; he feared his combativeness and outspokenness, and could not visualise him as an elder statesman of science.

[4] Recent studies emphasizing the complexity of Huxley's response to Darwinism include Adrian Desmond, *Archetypes and Ancestors: Palaeontology in Victorian London, 1850-1875* (London, 1982) and Mario Di Gregorio, *T. H. Huxley's Place in Natural Science* (New Haven, 1984).

[5] For a modern reassessment see J. Vernon Jensen, 'Return to the Wilberforce-Huxley Debare', *British Journal for the History of Science*, 21 (1988): 161-80.

[6] This event is recalled in Lankester's Huxley memoir in *Nature* (note 1), p. 703. Much later Huxley reminisced to Lankester abour his speech in a lerrer of February 6th 1894, reprinted in Huxley's *Life and Letters* (nore 2), II, p. 134.

[7] See H. C. Bibby, *T. H. Huxley: Scientist, Humanist and Educator* (London, 1959).

[8] On Balfour's career see the biographical introduction by Michael Fosrer ro M. Foster and A. Sedgwick, eds., *The Works of Francis Maitland Balfour* (London, 1884, 4 vols.), I, pp. 1-24.

At Oxford Rolleston had introduced his students to zoology by making use of a selected series of animals to serve as 'types', his students dissecting a specimen of each animal. This was a tremendous advance, as it was still customary to teach by lectures alone. In botany classes, specimens were handed round and picked to pieces, but as late as 1874, S. H. Vines had never seen a plant section.[9] After ten years experience, Rolleston printed his manuscript directions and notes as a book called *Forms of Animal Life* in 1870. Huxley was convinced that the study of organisms formed one discipline, and that a sound knowledge of this unified science could only be gained in the laboratory. Since the School of Mines offered no facilities for laboratory work, he had contented himself for some years with giving a full exposition in lectures of selected animals, plants, and palaeontological types.[10] When new buildings at South Kensington were opened, Huxley was able to give his long-desired course. In the large rooms on the ground floor, laboratories were improvised. For six weeks, June and part of July 1871, Huxley lectured for an hour each morning; then the students went to work with scissors, scalpel and microscope. Besides Lankester, Rutherford and Foster acted as demonstrators. As he jovially wrote to Dohrn: '[Lankester] is helping me as Demonstrator in a course of instruction in Biology which I am giving to Schoolmasters – with the view of converting them into scientific missionaries to convert the Christian Heathen of these islands to the true faith.'[11] A year later, J. S. Parker, G. B. Howes, and Thiselton-Dyer also assisted. Dyer said that it was 'sheer adventure', and Lankester wrote: 'Great enthusiasm prevailed. We went over a number of plants and animals, including microscopic work, and some physiological experiments. The 'types' were more numerous than in later courses.'[12] The South Kensington class really did make history, for it made teaching by lectures alone out-of-date and formed the model for all biology courses in English-speaking lands from that date.

[9] See R. C. Maclean and W. R. Ivimey Cook, *A Textbook of Practical Botany* (London, 1952), pp. 1-2.

[10] See T. H. Huxley and H. N. Martin, *A Course of Instruction in Elementary Biology* (London, 1875), pp. v-viii.

[11] Huxley to Dohrn, 7 July 1871, reprinted in Huxley's *Life and Letters* (note 2), II, pp. 59-60.

[12] See Thiselton-Dyer, 'Plant Biology in the Seventies', in the *Nature* Huxley supplement (note 1), and Lankester, 'Memoir on T. H. Huxley' (note 1). Huxley described the teaching in a letter to Dohrn, reprinted in his *Life and Letters* (note 2), II, pp. 82-3.

Lankester's admiration for Huxley, if anything, grew with the passing years. When Huxley died, Lankester uttered what has been called the finest tribute ever paid to a human being.

> There has been no man or woman whom I have met on my journey through life, whom I have loved or regarded as I have him, and I feel that the world has shrunk and become a poot thing, now that his splendid spirit and delightful presence are gone from it. Ever since I was a little boy, he has been my ideal and my hero.[13]

During his pleasant walks with Dohrn at Jena, Lankester had heard of his friend's plan to start a marine biological station. Dohrn's wealthy father, owner of a sugar manufactory, wished his son to marry and settle down, but Anton had made up his mind to do something large and important for zoology. He had been one of Haeckel's first pupils, had received his first lessons in marine zoology from him at Heligoland.[14] A photograph of 1865 shows him, a stalwart figure in topboots, together with Haeckel, Greef, Salverda and Marchi, each holding aloft a large net.[15] Zoologists were beginning to turn away from preserved and lifeless specimens to the beaches and shallow seas where life swarmed in unimaginable quantity. The Frenchmen Coste and Lacaze-Duthiers had already set up a small station at Marseilles. Dohrn had at first thought of Marseilles as a site, but the Franco-German War had intervened. He therefore determined to secure a site at Naples, in the Villa Reale which stretches along the shore. To his marine station Dohrn dedicated his own inheritance, and appealed for support to the universities of Europe, as well as to outstanding men of science, such as Huxley[16]. He eventually succeeded in obtaining widespread support, especially from the newly-founded Imperial German Government. Lankester promised to meet Dohrn in October. Meanwhile Dohrn came to Naples and took a number of rooms in the Palazzo Torlonia, a large mansion lately built by an Italian nobleman. The plan was that Lankester was to set up a temporary

[13] Huxley, *Life and Letters* (note 2), III, p. 393.

[14] W. Bölsche, *Haeckel: His Life and Work* (London, 1905), p. 59. Theodor Heuss, *Anton Dohrn: A Life in Science* (trans. Lixlotte Dieckmann) (Berlin 1992). C. Groeben, *The Naples Zoological Station at the Time of Anton Dohrn* (Naples 1975).

[15] W. Bölsche, *Haeckel*, facing p. 70.

[16] The Huxley papers (Imperial College) contain many letters to and from Dohrn concerning the financing of the Naples station, see W. R. Dawson, *The Huxley Papers* (London, 1946), pp. 35-8.

laboratory in the Palazzo and pursue embryological work. Dohrn entered into long and vexatious negotiations with the dilatory municipal authority at Naples. On October 8, Edwin Lankester noted in his diary: 'Ray left for Italy on Friday week. We have heard from him in Paris and Switzerland. His destination is Naples, and he expects to remain for six months.'[17] At Paris, Ray travelled to Mount Valerian to see the scars of the recent war, and was told in his favourite restaurant, how they had served dogs for food, and hidden in cellars during the bombardment. He arrived in Naples on Saturday October 13, having spent a day in Rome. His experiences in Naples he describes in the liveliest and most fascinating letters he ever wrote home. His first letter was written to his mother on the seventeenth, from the Palazzo Torlonia.

> You perceive by the above address that we have got into our lodging ... The place is as yet unfurnished, but we are getting one or two necessary things in ... We are about a mile and a half from the centre of Naples. The best houses are here. Dohrn's had already taken a part of this palace, and so we have taken an upper floor of it. It is an immense building, and was built by Prince Torlonia for himself, and is quite new, never before inhabited ... We command the most admirable view of the Bay, of the City and Vesuvius. Vesuvius is directly opposite to us, and at all moments one can look up and see its whole sweep. It is very graceful, like a chameleon in its wonderful change of colour with different degrees of sunlight. At sunset, it becomes a most splendid violet-red ... It is now in a mild state of eruption, that is, there is a small stream of lava issuing from it, and this glows like a coal at night.
>
> Our apartments consist of a floor containing eight rooms ... Besides Dohrn and I, there is the architect to come, and Kleinenberg, who will come later. Mme. Baranowska occupies a wing of the building below, which she is in the act of furnishing. She has loaned us two beds, and bought us a table, wardrobe, chairs etc, and so we are gradually geting furnished ... We take tea, and, (till our arrangements are completed) dinner, with the family below. They are very pleasing ... There is Mme. la Baronne, she is charming, although illness has made her somewhat withered. The two children are very nice girls, contrasting both in face and manners. They speak English, but generally French. The

[17] Edwin Lankester, 'Melton House Record' (Lankester family papers).

> M. Baranowski is expected in three weeks; his brother was here today, a long-haired amiable Russian. [Baron Baranowska had been Governor of Orenburg, and was at this time employed on Russian missions to China.] I felt some embarrassment in calling upon these people, since Dohrn is so exceedingly intimate, and I a total stranger ... Yesterday I went to the National Museum, one of the finest art collections in Italy ... I am quite overwhelmed with all these collections. It is really cooking to find such galleries in every town, and then these richly decorated churches ... The great thing however, which one never tires of, is the Bay and Vesuvius, always beautiful and always changing ... My chief anxiety is as to work; we cannot begin before the tables etc are in place, and I, not before my box comes ...

The elder Baranowska girl, Marie, ('not beautiful, rather careless and awkward in dress'), was a capable girl of winning character, who acted as housekeeper. Eventually, she became Dohrn's wife, and Ray Lankester's close friend. Madame Baranowska was an intense Polish patriot, and used to argue furiously with Dohrn, although the whole family liked him. Lankester found this Russo-Polish family most unusual people. None of them went to church, and they did nor at all believe in Christianity. 'This does not shock me at all', he wrote, 'but it is a curious phenomenon.'

In return for lessons in Italian, Ray gave the girls lessons in microscopy. The house he found to be most uncomfortable, damp and cold with its tiled floors. There were no fires at first, but later English fireplaces were installed and he put down some coconut matting.

Dohrn's forceful personality made a curious, not altogether pleasant impression on the young Englishman. Lankester felt keenly his own youth and inexperience. Everybody commented on his youthful appearance and the Baranowska called him 'Le Bébé', which furiously annoyed him. Of Dohrn he wrote:

> He is a very strange fellow, full of self-confidence, and somewhat boisterous in his manners, yet full of kind attentions, thoroughly honest, very energetic and at the same time clever. It is almost impossible to argue with him, or even talk much; he is so full of his own opinions and thoughts that he will not stop to consider other people's.[18]

[18] Letter of 22 October 1871 (Lankester family papers).

Dohrn, for his part, complained to Huxley that he found Lankester rather selfish.[19]

Lankester gives us some interesting glimpses of his biological work at Naples. On October 22, he wrote: 'I have been on the Bay with the fishermen, the same poor Claparède used to employ. [Edouard Claparède, a Genevan Professor, had just died, aged only thirty-nine.] Lankester's favourite fishermen were Giovanni di Giovanni, and his brother Domenico. They knew where to obtain the best specimens; it was not the first time that they had dealt with 'gli professori'. Giovanni's invariable practice was to take away two glass jars, and bring back one full of specimens; 'nova specie, signore', he would assert. These fishermen collected innumerable molluscs, annelids, fish and polyps. Their collecting instrument was a long pole, with an iron ring and bag attached, or a rake. Some used a net. These were the days before Dohrn's aide, Dr Lo Bianco, brilliantly organised the collection and preservation of specimens for the Stazione Zoologica. Lankester wrote:

> We caught an immense number of *Amphioxus*, [the lancelet, a primitive chordate, and a classical object of anatomical research]; these men know where to get everything. They brought us this morning several cuttlefish, lots of various crabs, shrimps and annelids, a *Balanoglossus*, ['acorn worm'], an exceedingly interesting animal I longed to see, and a *Torpedo* [electric eel].

The day after this was written,

> a hundred gallon seawater tank was installed 'in our back premises'.

Lankester's intention had been to study the embryology of the larger molluscs, but the eggs were not to be found at that season and he had to turn to other things. He became apprehensive, and wrote discouragingly to his sister Fay. However, in the spring of 1872, he was able to obtain copious supplies of the eggs of the cuttlefish *Loligo*. These were kept in a basin into which ran a continuous stream of seawater; others were sunk in a fisherman's basket at Santa Lucia. The eggs were small and transparent, and were easy to examine in the living state. Observations on these eggs formed a notable part of his memoir, 'Developmental History of the

[19] Dohrn to Huxley, 11 June 1871 (Huxley papers, Imperial College); see *The Huxley Papers*, p. 36.

Mollusca'.[20] Nevertheless, his father noted in his diary on the last day of the year, 'Ray is grumbling as usual'.

In February, Ray was writing more cheerfully, 'I am now working on the development of *Aplysia*, and of a cuttlefish, which have both already given definite good results ... I have also material for a memoir on *Sipunculus*, a most interesting worm, which has been over and over again studied by German and other naturalists and wrongly interpreted ...'[21] He lists numerous other animals of which he has made drawings and notes.

Lankester paid a number of visits to the usual places of interest, Pompeii, Capri, Capua, but it was Vesuvius that claimed his chief interest. In a letter enlivened with deft sketches, he recounts how he and his friends ascended to the great crater. A week later he made a night excursion to the volcano, and reported home gleefully on his victory over Dohrn in matters athletical, 'beating him hollow' in a race to the top.[22] They waited for an explosion, and watched the white-hot stones hurled in the air: 'Then we bolted. Soon after, a lot of ashes and stones fell where we had been standing.' He sent to the *Athenaeum* a long account of his visit to Vesuvius, and his book, *Secrets of Earth and Sea* also contains a chapter on 'Vesuvius in Eruption'.[23]

A curious incident happened in connection with Dohrn's negotiations for the building of the Station. The German architect, Kelterborn, began to suffer from delusions, and had to be sent home, where he happily made a complete recovery. In order to conciliate the municipality, Dohrn agreed to employ a Neapolitan architect. This man considered himself insulted because Dohrn refused to promise him a ten per cent commission, instead of the five per cent usual in northern Europe. Lankester relates: 'Dohrn's birthday. He has been challenged this morning to a duel by his Italian architect. Of course we laughed at him.'[24] Lankester met the architect's friend, who was to be his second, and told him gravely that Dohrn was a soldier of the German Emperor, a very deadly man with the sabre, and was determined not to yield to any nonsense. The challenge was withdrawn, and the insulted architect completed his task in a satisfactory manner! Towards the end of his sojourn in Italy, Lankester began to feel the need of change. He was getting tired of the 'incessant microscope', and

[20] Lankester, 'Contribution to the Developmental History of the Mollusca', *Philosophical Transactions of the Royal Society*, 165 (1875): 1-48, see p. 38.

[21] Letter of 22 February 1872 (Lankester family papers).

[22] Letter of 22 October 1871 (Lankester family papers).

[23] See *Athenaeum*, 18 November 1871, and Lankester, *Secrets of Earth and Sea* (London, 1920), pp. 64-73.

[24] Letter of 28 December 1871 (Lankester family papers).

began to consider his prospects. In a letter written sometime in March, he sums things up.

> You write of that Exeter Fellowship as if I were sure to get it. I cannot deny to myself that I ought to have a Fellowship at Oxford, and to have teaching work to do there if I desire it ... It will enable me to go on with my work, and pave the way for something better. I have got a great deal of material now in notes and drawings; how it will all come out, when I try to put it inro shape for publication, I don't know. I look forward with great pleasure to breaking off here, and yet I can't say I do not enjoy what I am doing here ... A thing which gave me some satisfaction the other day was that in the new edition of the *Origin of Species*, Darwin quotes me twice, once as to Longevity, and once as to the significance of homologies; he calls what I wrote, a very remarkable paper! I confess that it is one of the greatest consolations of my existence, when I see such an approbation from such a man ... If I were to get in at Oxford, I might perhaps do the science for the *Athenaeum*, as Dilke proposed before.

In March, Lankester acted as guide to Huxley (returning from his visit to Egypt), in expeditions up Vesuvius. In April he fell seriously ill with what he describes as typhoid fever, but was most probably paratyphoid. Phebe Lankester was dreadfully alarmed, and determined to come to Italy to nurse her son; but by the rime she arrived, the danger was past. Ray had been devotedly nursed by Marie Baranowska. He used later to assert, perhaps not too seriously, that had his mother not arrived, he would have married Marie. In the meantime, Moseley had informed Edwin Lankester that he did not intend to go in for the recently instituted Exeter College Fellowship in Natural Science.[25] Early in May, Phebe wrote from Naples that she had found Ray weak and exhausted, but out of danget. Rolleston wrote that that Moseley was definitely going on the *Challenger* survey expedition, that Ray's most formidable opponent for the Fellowship was out of the way, and that there was plenry of time for him to recover for the Fellowship examination.

Ray Lankester saw nothing of the Vesuvius eruption of 1872, except for the dust in the streets. He lay in bed listening to the deep humming sound, and wondering at rhe darkness. When he could get up, he saw the

[25] Edwin Lankester, 'Melton House Record', 26 April 1872 (Lankesrer family papers).

great cauliflower-like cloud, and the vacant place where the ashcone had been. Ten days afterwards, he visited one of the villages in the path of the lava stream, a journey picturesquely described in his *Secrets of Earth and Sea*.[26] Accompanied by his mother, he set off for home about May 4, travelling by Rome, Florence and Paris, and arrived back on the 20th May, 'thin but well', as his father noted.

Meanwhile, Edwin Lankester's affairs had reached a crisis. He was forced to come to an agreement with his creditors, a proceeding attended with unpleasant publicity. 'Since then', he wrote; 'my life has been one of continued and deep anxiety. The events of this period are too painful to contemplate, and I put it in record here as one of the saddest enteries in this diary.'[27] In May he wrote; 'My affairs are bothering me sadly, and I feel very poorly.'[28] At the end of 1871, he had contemplated extensive literary activity, but this now became impossible. There can be no doubt that these troubles hastened his end.

In February, Ray's sister Minnie was married. In June, Ray went up to Oxford to take his Fellowship examination. He felt rather unwell and depressed. Nobody at Oxford knew him; the papers were much as he expected; he was not sure if he had done well; if not Rolleston and Huxley would be annoyed, as they had set questions obviously intended for him – so ran his complaints. He looked back with some regret to the Palazzo Torlonia. If only Dohrn had been a little less overbearing, and there had been a little English society, things would have been very satisfactory there. Poulton has stated that there were two candidates for the examination, Lankester and Sharkey (later Sir Seymour Sharkey),[29] but Lankester himself records that there were *three* others, 'who appeared to be rather puzzled with the paper – one disappeared without writing anything. Sharkey wrote, but complained of the paper.'[30] Sharkey it appears, wrote on one topic only, while Lankester wrote a more strictly zoological paper. No award was possible on the examination, and Lankester was elected as senior, probably because his subject was thought to be more useful.[31] This was the uncertain period that preceded the second University Commission, and the College Visitor had agreed to the conditions of this Fellowship, namely that if the Fellow elected should have taken part as tutor or lecturer in the College for seven academic years, he should retain

[26] See note 24 above.

[27] 'Melton House Record', 11 April 1872 (Lankester family papers).

[28] *Ibid.*, 19 May 1872.

[29] See Poulton's obituary of Lankester, *Nature*, 124 (1929), p. 310.

[30] Letter of 18 June 1872 (Lankester family papers).

[31] See Poulton (note 29).

his Fellowship. In fact Lankester did not become full Fellow of Exeter College until 1873.[32] He took part in Huxley's second class at South Kensington, then in July, took his M.A. In December, he travelled to Plymouth to see Moseley off on the *Challenger* expedition.

It was not until 1874, that Lankester was able to make another visit to Italy, to see how the Marine Station was progressing. At his request, domiciliary regulations were waived, and he was enabled to spend from March to July 1874 in South Italy and Sicily. When he had left Naples in May 1872, the walls of the Stazione Zoologica wete already a couple of feet above the foundations. When he arrived at Naples in 1874, accompanied by his friend Balfour, the laboratory was completed. The Baranowskas were now in Poland. Dohrn was now engaged to Marie Baranowska, but his martiage was delayed; 'he thinks of nothing else', wrote Lankester. On subsequent visits, Lankester was able to renew his friendship with Dohrn and his wife, and some surviving letters from Marie testify to his status as a close family friend. After their visit to Naples, the two friends crossed to Sicily, and made an extensive tour, penetrating to the centre of an island, at that time almost unknown to tourists. Their plan was to go on to Syracuse, then back to Catania; thence up to Etna, then across the interior to Girgenti to see the Greek temples; from there across country to Palermo, and then back to what Ray Lankester described, according to mood, as 'dolce Napoli', or 'the horrid old plague-spot called Naples'. He described his journey in two delightful letters, the first, suitably didactic and descriptive, to his brother Owen; the second to his sister Minnie.[33] Sicily was plagued with brigands, and while travelling by 'diligence' to Girgenti, they came on the body of a soldier who had been shot dead by brigands only a short time before. After these experiences Lankester returned to Oxford, where he was already deeply involved in the struggle to reform rhe teaching of science.

32 This information was supplied to J. Lester by J. P. V. D. Balston, Senior Tutor of Exeter College.

33 Lankester family papers. The archives of rhe Stazione Zoologica, Naples, holds microfilm of a number of letters from Lankester ro Dohrn in the period 1872-74 and original letters from later years. Lankester consulted Dohrn on rhe plans for building the Marine Biological Association's laboratory at Plymouth (see Chapter 9). The letters record at least one later visit by Lankester to Naples, in 1886, when he had to leave suddenly because he was affected by rhe hear.

- 5 -
Exeter College: The Young Scientist and Humanist

After taking his M.A. in June 1872, Lankester busied himself with preparing his Naples material for publication. He projected a number of articles, including a general one on his Naples work. This appeared in the *Annals and Magazine of Natural History*. It is one of his most readable articles, full of the sun and sights of southern Italy, and the fauna of its seas.[1] Darwin had followed Lankester's progress with attention, and wrote to him enthusiastically: 'What ground work you did at Naples! I can clearly see that you will some day become our first star in Natural History.'[2]

Ray's Exeter Fellowship was a source of deep gratification to his parents, but it brought the son little satisfaction for two reasons; he found little to inspire him in his official duties, and he hated Oxford society. He felt his life to be abnormal, propped up year after year by scholarships and stipends, entailing no work of a regular character. One sees in his letters his profound dislike of the aristocratic milieu of Oxford. Late in 1873, he confided to his parents his longing to escape from the society where young men were taught to be 'flunkeys, snobs, spendthrifts and social bullies'. Those who escaped after four years could recover themselves. Others were entrapped by the snare of the 'Resident Fellowship'. He depicts the reality of his Fellowship as he sees it.

> Expenses much greater than anywhere else, society stale, old broken-down, one-sided pedagogues or pedants; dwelling rooms wretched, stuffy, sickening. Teaching duties a weariness of the flesh ... enforced pretence of teaching; microscopic inspection on all sides of everything you do. No liberty of action, a tyrannical majority which shows its opinion of you, even if you stay away from church.

[1] Lankester, 'Summary of Zoological Observations made at Naples in the Winter of 1870-71', *Annals and Magazine of Natural History*, 11 (1873): 81-98.

[2] Darwin to Lankester, 15 April 1872; in F. Darwin, ed., *More Letters of Charles Darwin* (New York, 1903), II, p. 336.

He describes a dinner party at which Bonamy Price, the professor of Political Economy argued about miracles, and claimed to have converted the scientist Tyndall. He concludes: 'If you hear of an appointment of £250 a year in London, that I could fill, remember that I should jump at it.'[3] He was soon to receive his London chance, and at a rather better figure than the one he mentioned.

At the end of 1873 Huxley suggested that Lankester be invited to contribute to the new *Encyclopaedia Britannica.* The editor, Thomas Spencer Baynes, wrote to ask if he could provide articles on various biological topics within six months. Lankester replied that he could not contribute immediately, but would like to write a series of articles that could be pieced together to form an introduction to the study of life.[4] The production of the new edition of the *Encyclopaedia* turned out to be a slow business, and he was able to contribute a number of important articles to various editions of the work.[5]

To his friend and teacher Huxley, Lankester poured out his heart in letters in which he rehearses his complaints against the University. Addressing Huxley as his 'father-in-science', he adjures him not to suppose that he (Lankester) has a genuine career before him.[6] The Colleges were nothing but large proprietary schools, with Fellows pushing stupid and lazy youths through the degree examination. Another letter to Huxley was written from St. Andrews, where Lankester had gone to seek the elusive worm *Echiurus.* He wanted advice about a post at Owen's College, Manchester.[7] Huxley counselled moderation and patience, as he would do many times in the future. A little later he wrote to Lankester:

> I could make out just as black a case against my life in London, as you do against yours in Oxford ... Ecclesiastical odours may not be so strong in London as in Oxford; plutocratic and aristocratic odours are a good deal stronger.[8]

[3] Undated letter to his parents, late 1873 (Lankester family papers).

[4] Lankester to (presumably) Baynes, 3 December 1873 (Library, University College).

[5] Lankester's articles for the 9th edition of the *Encyclopaedia Britannica* are best known, including 'Zoology', vol. 24 (1888): 799-820 and the articles on individual groups collected in Lankester, ed., *Zoological Articles Contributed to the Encyclopaedia Britannica* (London, 1891).

[6] Lankester to Huxley, 18 December [1872] (Huxley papers, Imperial College).

[7] Lankester to Huxley; Huxley papers, Imperial College. This is the letter no. 3 dated April [1880] in Warren, *The Huxley Papers,* but Lankester's diary (Lankester family papers) confirms that he went to St. Andrews looking for *Echiurus* in April 1873.

[8] Huxley to Lankester, 27 April 1873; Lankester family papers. Lankester preserved a number of letters from Huxley which were loaned to Leonard Huxley for the preparation of *Life and Letters* and returned with thanks.

Edwin Lankester's health had meanwhile been sinking gradually, and did not benefit much by a trip to Switzerland in 1873. He went to Margate and hopes were entertained for his recovery, but he was suffering from diabetes, in those days an incurable disease. He died in a coma on the 26th October 1874, and was buried in the churchyard of Hampstead Church, his friend Stopford Brooke conducting the service.

The shock of his father's death, and his added responsibilities, depressed Ray Lankester a great deal. Whenever the nightmare spires of Oxford became too much for him, he fled for a time to Felixstowe, a resort that never failed to please him.

About this time, J. D. Hooker enquired of Thiselton-Dyer if Lankester would consider a post in Ceylon, in the new Museum at Colombo. Lankester declined, alleging his dislike of a hot climate.[9] On June 3rd, 1875, he was elected Fellow of the Royal Society, the citation stressing his work on Molluscan embryology. Lankester's entty into the scientific élite was not always smooth, however, thanks to his controversial opinions. Later in the same year he was blackballed from the Linnean Society, forcing Darwin, Hooker and others to mount a campaign to get him elected. The second proposal was seconded by Darwin, Huxley and other eminent figures, and Lankester was elected at the beginning of February 1876.[10]

One outlet for Lankester's dislike for his life at Oxford was the fight for University reform, then engrossing a good deal of the time of Huxley, Playfair and other leaders of science.[11] At South Kensington, Huxley had created a new form of biology teaching, but he felt that it was equally important for the great universities to be brought into the movement. Everyone was aware of the immense contributions being made by the German universities, no one more so than Lankester, whose extended studies at Leipzig and Jena had introduced him to the new methods of teaching. After returning to England in 1872 he had written to Haeckel

[9] Lankester to Hooker, undated (Hooker papers, Kew).

[10] Osrensibly Lankester was blackballed because the council had agreed to remit his fees; see Darwin to Wallace, 23 December 1875 (Darwin papers, Cambridge University Library; item 10322 in F. Burkhardt *et al.*, eds., *A Calendar of the Correspondence of Charles Darwin*). The Form of Recommendation for Lankester's eventual election is preserved in the archives of the Linnean Society.

[11] See D. S. L. Cardwell, *The Organization of Science in England* (revised edition London, 1972), although this is generally weak on biology. Lankester's involvement in the reform of science at Oxford is covered in Janet Howarth, 'Science Education in Lare Victorian Oxford: A Cutious Case of Failure?', *English Historical Review*, 102 (1987): 334-71.

asking for information about Jena university, his object being to amass information that would allow him to attack the current Oxbridge system where so much was seized and wasted by the Church.[12] He later recalled that Huxley had sent him to Oxford to *fight* for a proper use for science in the university.[13] In defending the role of marine biological stations (discussed below, chapter 9), he made clear the thinking behind the new demands fot proper resources.

> We claim for biology now a place of far higher importance in the scheme of human knowledge than she had occupied hitherto. She has proved her claim to the respect and most serious attention of men by the discovery of the principles and detailed laws of evolution, – a discovery which has more widely influenced human thought than has any other product of human science, and must continue long to do so. We are no longer content ro see biology scoffed at as 'inexact', or gently dropped as 'natural history', or praised for her relations to medicine. On the contrary, biology is the science whose development belongs to the day. At this moment, she is deserving of more artention, more material aid, more assistance in her young growth, than any other human science ...

Inevitably, Lankester would come up against vested interests, and equally inevitably his combative nature led him to become embroiled in controversies that were not necessarily the best way forward. Lankester's notebooks from this period contain frequent references to the topic of university reform, and he contributed a number of articles to various periodicals.[14] Here he struck out without fear or favour. Even the new Oxford Science Museum he regarded with distaste, remarking that £80,000 had been spent on a palatial edifice for natural science, and had been devoted to an architectural monstrosity ...

[12] Lankester to Haeckel, 25 May [1871] (but almost certainly 1872) (Haeckel-Haus, Jena).

[13] Quoted from Lankester's autograph copy of his speech to the Royal Society on receiving their gold medal, 1885 (Lankester family papers).

[14] Lankester's notebooks for 1872-73; Lankesrer family papers. See Lankester, 'The Furure of the English Universities', *Nature* 8 (1873): 61-2. The Lankester family papers also conrain an offprinr of the following article, also presumably by Lankester, signed 'An Oxford Man': 'The Universities Bill', *Nature*, 15 (1877): 391-4.

[facing page 54



Cartoon of Lankester published in *Vanity Fair*, 12 June 1905.
'His religion is the worship of all sorts of
winged and finny freaks.'

> which though pleasing to the aesthetic persons [Ruskin and Acland!] who invented it, does not provide the accommodation which the subjects require, nor even so much as could, in the absence of aesthetic meddling, have been obtained for a fourth part of the sum quoted with so much assurance.[15]

He believed that the government had no real intention of making Oxford and Cambridge into real universities. There the professor was in an ambiguous position. By right the director of the studies connected with his chair, he was, Lankester believed, shorn of the privilege and function of his office, because of the eager competition of colleges for examination honours and tutorial fees. The theological faculty reigned supreme. Medical study had ceased. The Universities had become a congerie of finishing-schools for the sons of the wealthier classes.

> The question is this. Are Oxford and Cambridge to remain as institutions exclusively for the elegant education, the 'culture' of the upper classes who may choose and can afford to allow their sons to while away certain years there; or any they to be made engines of a national education, where a poor man may go with as much reason as a rich one, and profitably spend his time in acquiring knowledge and training which have a real value in rhe world, and place their possessor in the position to earn his bread and his standing among men?[16]

He saw the remedy in the reconstitution of the faculties, and summed up his programme in the following points:

> The creation of new professorships. (Leipzig had 115 to Oxford's 43);
>
> Sole control of the curriculum of a Faculty by its official members;
>
> Imposition of a thorough matriculation examination on all seeking admission;
>
> The abolition of the 'college monopoly of boarding undergraduates';
>
> Special revenue for missions, travels and researches;

[15] 'The Universities Bill' (note 14).

[16] 'The Future of the English Universiries' (note 14).

General control by the State;
The continual control of a richly endowed corporation by the State, is an indispensable safeguard;
No University funds for any ecclesiastical purpose;
Removal of religious tests.

In 1872, the Society for the Organisation of Academical Study was formed on the initiative of Dr. Appleton, a young Oxford man who had studied in Germany. The discussion that led ro the formation of the Society took place in the Freemason's Tavern. Mark Pattison was in the Chair, and Rolleston was the first speaker; Carpenter and Burdon Sanderson also spoke. The Society was abortive, perhaps because of university indiffetence and concern with the extension movement.[17] Lankester's position was made all the more difficult by the fact that he soon came into conflict with Rolleston, the Linacre professor of physiology and comparative anatomy. Although Rolleston had helped to pioneer the teaching of comparative anatomy through dissection of selected types, he resisted many efforts at further reform. Lankester tried to get permission to teach in the museum, but was refused. He proposed a system for spreading the teaching among the trained college fellows available, but this was also rejected. A letter from Thiselton-Dyer to Huxley gives an account of the resulting controversy, indicating the problems created by Lankester's tempetament.

I am quite prepared to believe that Ray Lankester had played his game at Oxford with but little tact, and not a little indiscretion. Notwithstanding all this, and however one may esteem Rolleston's undoubtedly excellent personal qualities, it is impossible to approve his attitude or policy as head of his branch of scientific teaching at Oxford. One has only to look at what Foster had done at Cambridge, to see what is possible. What has Rolleston done to found a school comparable to that growing up around Foster. I believe his mind to be an utter blank to the modern developments of animal morphology. Lankester for his part, is keenly anxious for the progress of biological study at Oxford, and Rolleston's really reactionary attitude has undoubtedly been extremely galling to him. It was technically out of Lankester's power to compete in any way with Rolleston, and had the latrer taken him up as Foster has done with Francis Balfour, I

[17] Cardwell (note 11), p. 152.

> do not see why they should not have worked smoothly together ... I have had a little talk this morning with Moseley ... and I found that he is unflinchingly with Lankester as against Rolleston, though he has had no rupture with the latter.[18]

Dyer goes on to say that he wished that Lankester had 'a little tincture of the opportunism which is the fashionable political rule of conduct'. Let Lankester leave Rolleston and Acland alone. Oxford was dead set against 'the strong meat that Lankester and all of us would willingly administer to it, if its stomach would bear it'. Dyer was right about Lankester; he was not, and never would be an opportunist.

In assessing Lankester's views on Oxford, it must be made clear that his dislike of the aristocratic university system did not blind him to the historic role of the University, nor did it prevent him from making many friends at Exeter. When his Fellowship expired in 1889, he was elected to an Honorary Fellowship of the College, 'in recognition of his scientific eminence and past services to the College'. That he was genuinely moved, is clear from the graceful acknowledgement he made to the Rector.[19] Lankester's portrait still hangs in the Hall, above the panel door to the Senior Common Room, and the Lankester Society still flourishes in the College.

Lankester's often intemperate attacks upon the Oxford establishment arose from deep personal convictions. His notebooks from this period contain many reflections on philosophy, politics and religion, while his letters to his parents made it clear that he derived his campaigning spirit from them. Throughout his active life, he was oppressed by the difficulty of establishing himself as a man of science in an environment he felt to be, in some fundamental way, hostile to the progress and development of scientific knowledge. Unlike his friend Balfour, and unlike Darwin, he was not a favoured and protected member of the social order. He often described himself as a 'poor man', relative to the aristocratic university society of his own time. These impressions poured in on a nature at once emotional, strong and sensitive. All his friends recognised that Ray Lankester felt, spoke and thought strongly. He loved fairness and justice, had a hatred of cruelty and hypocrisy, and above all, was possessed by an unconquerable desire to speak his mind. Lankester could be diplomatic

[18] Thiselton-Dyer to Huxley, 22 November 1880 (Thiselton-Dyer papers, Kew).

[19] Letter in the College Register, 20 June 1889 (courtesy of the Keeper of the Archives, Exeter College).

enough on occasion, but he could not be silent in the face of hypocrisy, untruth and fraud. He scorned the doctrine of *quieta non movere*, and was forever kicking sleeping dogs into yelping activity. When Dyet temonstrated with him, he retorted: 'It is all very well about 'assaulting people', but it is difficult to say anything without antagonising *somebody*.'[20]

Lankester's pugnacity distressed even the combative Huxley, whose letters are long invocations to his pupil to 'simmer down'. But this was no mere foible of temperament; it sprang from perception of the deep contradiction between the progress of science, and the conditions of contemporary society. Fraud of any kind aroused him. When he was a very old man, he wrote in opposition to Professor Tillyard: 'It is not true that science has nothing to fear from fraud ... All humanity has much to fear from fraud, and to all human progress, fraud is, and has been, a deadly enemy.'[21] Lankester's reputation for combariveness had its effect. Many said that his outspokenness damaged his worldly prospects. Certainly, he was always prepared to rush in, where even the least angelic of his colleagues feared to tread. If he was *fortiter in re*, he was frequently *fortiter in modo* as well.

Lankester was an early participant in the controversy over vivisection which raged during the 1870s and 1880s. His training with German physiologists made him aware of the value of this technique, although it was of little significance in his own later work. When the German physiologist Moritz Schiff was attacked by the British community in Florence for his vivisection experiments, Lankester sprang to his defence. He wrote letters to *The Times* in December of 1873 and January 1874 resisting the charge of unnecessary cruelty made against Schiff.[22] Over the next few years he worked actively to defend the scientists' position in this debate, attacking the anti-vivisectionist *Spectator* and being attacked in return. He received a good deal of abusive mail as a result of his stand, and – typically – pasted his collection in a scrapbook.[23] Modern historians have interpreted the vivisection debate as a symbol of resistance to the scientists' wider demand for social authority. Lankester's participation thus

[20] Lankester to Thiselton-Dyer, 2 September 1879 (Thiselton-Dyer papers, Kew).

[21] Pencil notes of 1924 (Lankesrer family papers).

[22] Lankester in *The Times*, 29 December 1873 and 1 January 1874. On these episodes see Parrizia Guarnieri, 'Moritz Schiff (1823-96): Experimental Physiology and Noble Sentiment in Florence', in Nicolaas A. Rupke, ed., *Vivisection in Historical Perspective* (London, 1987), pp. 105-124, and Rupke, 'Pro-Vivisection in England in rhe Early 1880s', *ibid.*, pp. 188-208. See also R. D. French, *Antivivisection and Medical Science in Victorian Society* (Princeton, 1975).

[23] Lankesrer family papers.

reflects not only his training as a biologist, but also his activity in the general campaign by which science sought to displace traditional sources of authority.

A letter written to *The Times* later in his career gives a good idea of Lankester's outspokenness, his radicalism, his capacity for invective, and the fundamental humanity of his outlook.

> Lord Meath [who had attacked the 'idleness' and 'indiscipline' of the masses] confuses 'idleness', 'neglect of duty', and 'Indiscipline', with a very different quality, *viz.* the healthy emancipation of the individual from mechanical, unreasoning 'obedience'. Lazy, self-seeking, incompetent, stupid persons who, to the misfortune of their fellow-citizens, hold by accident or tradition positions of authority ... naturally regret the increasing difficulty which they experience in exacting slavish compliance with their unreasonable demands. The mechanical obedience of the whipped dog or horse is not a thing to be desired either from children or grown men, however humble their social position. There is a discipline which is based on such obedience. It serves only to maintain worthless and greedy persons in unmerited comfort, luxury and authority, at the expense of the 'indisciplined class', to arrest the development of intelligence, and to bring about national disasrer. There is another discipline, which is based upon understanding and affection. The jack-in-office, the pompous official, the petulant commander, the ignorant pedagogue, the unworthy parent, cannor bring it into existence. Ir is they who cry out for 'obedience', and deplore the departure of a servile spirit which enabled them to mismanage public and private affairs ...[24]

Lankester could certainly be brusque on occasion, and liked to have his own way. Eden Philpotts, who knew him well, has a reminiscence of a visir to the Torquay caves, when rhe brusqueness of a bored Lankester made the Curator of the caves remark, that although the cavebear was supposed to be extinct, there seemed to be one still alive.[25] Lankester was no respecter of persons. On one occasion, Edward VII paid a visit to the Natural History Museum, when Lankester was Director. The King ventured to express an opinion on some biological topic; we ate told that the Director's sonorous voice boomed out, 'Sit, the facts are not so; you

[24] Lankester in *The Times*, 7 January 1907, p. 9.

[25] Eden Philpotts to J. Lester (personal communication).

have been misinformed'. One likes to think that Conan Doyle's irascible Professor Challenger was founded on Ray Lankester. Doyle certainly had Lankester in mind when he wrote *The Lost World*, since he makes Challenger refer to a copy of *Extinct Animals*, 'by my good friend Professor Lankester'.

It is possible to discern the following themes in Lankester's writings of the early seventies. He shows a strong reaction against dogmatic Christianity, priestly pretention and fraud. To this theme he returns many times. In this he was a true follower of Huxley, who had written:

> I desire that the next generation may be less fettered by the gross and stupid superstitions of orthodoxy, than has been, and I shall be well satisfied if I can succeed to however small an extent in bringing about that result.[26]

It is easy to forget what the humanists of the period had to fight against – the fanatical hatred of 'infidelity', the fierce sectarianism, the 'hellfire sermons', which even drove people to suicide, and the prevalent gross ignorance and superstition. While at Jena, Lankester had many philosophical conversations with his German friends, and one finds traces of these discussions in his notes and letters. In general, he transferred his father's piety to Nature, sublimating a strong religious feeling into a Spinozistic regard for the wonders and beauties of Nature. While he desired to see the pretensions of organised religion curbed, Lankester was never a 'propagandist atheist' in the Bradlaugh sense. It was theology he disliked rather than religion.

In a note written in 1873, entitled 'Development of Human Character and Beliefs', he gives a concise and fiery summary of his outlook, from which we extract the following.

> It is awful to think what European nations have gone through, tolerated, endured, at the hands of the Church, in consequence of certain quite gratuitous religious beliefs ... such beliefs as,
>
> 1. A dread of the Unseen God, who will punish us, if not now, yet after death.
> 2. That all physical enjoyment is degrading, and will bring down vengeance from the Creator.

[26] Huxley to Lankester, 22 May 1883 (Lankester family papers).

> 3. That copulation is especially a wrong thing.
> 4. That women are inferior creatures, used by the devil to bring men to misery.
> 5. That there is a God who can be 'got round', so to say, by duly servile treatment, but who has a Hell ready for most men. Oh, if we could only all clear our minds, and return to the bright and joyous beliefs of the Greeks ... The belief that Nature is not good, that God is a devil and man his victim. Such Institutions as Monarchies and Aristocracies, Priesthoods, with the poor starving for the pleasure of the rich; prostitutes thronging the streets that others may be 'chaste', or that marriage may tyrannise. Curse all these things! I know they will pass away. They are doing so. May I help to smash them.[27]

Lankester disliked his youthful appearance, with his round, boyish face, and longed for maturity. When it came, the inner and outer man combined to produce a personality of extraordinary power. He was well over six feet in height, and built in proportion; his two brothers were even larger. A friend who once saw the three together in a Turkish bath remarked that it was in impressive sight, not easily forgotten. What astounded most people who met Lankester for the first time was this astonishing union in one person of sheer physical size and obvious intellectual power. Sir Arthur Keith recorded that Lankester made him feel small.[28] Some, like Keith, were rather repelled by this forceful personality, but none could deny its power. Olive Schreiner met him at a dinner party in 1881, and commented: 'I have just come back from Dr. Philpotts. Ray Lankester was there. He is the most powerful human being I ever came into contact with; he is like those winged beasts from Nineveh at the British Museum. What you feel is just immense force.'[29]

[27] Notebooks in Lankester family papers.
[28] A. Keith, *An Autobiography* (London, 1950), p. 262.
[29] Copy in Lankester's own hand (Lankester family papers).

- 6 -
The Professor: University College London, 1875-90

In the seventies of last century, no finer opening for Lankester's talents existed than University College London, which was free from the theological domination which he deplored at Oxford and Cambridge. On its foundation, it had been denounced as the 'infidel college', since it did not impose religious tests upon aspirants for the University of London degrees. Edwin Lankester once failed to obtain a much-desired post because he had taken courses at University College.[1] The type of student at the College was different; Ray Lankester described them as 'poor determined young fellows', and they appealed to his democratic instincts. Thanks to Lankester's efforts, University College emerged as one of the most effective centres of zoological research and teaching in the late nineteenth century. As D. M. S. Watson later wrote:

> During the tenure of the chair by Ray Lankester, University College possessed by far the most active School of Zoology in Britain, Cambridge being the only possible competitor ... Whilst at University College Lankester trained a great series of zoologists who filled very many of the chairs in that subject, both at home and in the Dominions, and he thus influenced the whole course of zoology in the British Empire...[2]

It comes as a surprise that the College very nearly refused Ray Lankester's services. The Senate had appointed a committee to consider applications.[3] The report of the committee was favourable to Lankester, but there was trouble with The Committee of Management, and much bandying of reports between Senate and Council. Huxley and many other scientists lobbied strongly on Lankester's behalf; a letter to his mother by

[1] Edwin Lankester's diary, Wednesday 15 September 1841 (Lankester family papers).

[2] Quoted in Hale Bellor, *History of University College* (London, 1929), p. 392.

[3] Universiry College, Senate Report, 13 October 1874.

the physiologist W. B. Carpenter testifies to their efforts.[4] Not till February 20, 1875, was Lankester appointed Professor in accordance with Senate recommendation.[5] His appointment came at a propitious moment, since a government report on Science and Education, had recommended the completion of the College buildings, and the augmentation of professorial incomes.[6]

The chair of zoology at the College had become vacant through the death of its first incumbent, Robert Edmond Grant. Grant was an expert on sponges who had moved from Edinburgh to take up the chair in 1827. He was still lecturing and attending Senate meetings up to a month before his death on August 23rd 1874. Adrian Desmond has highlighted the role played by Grant in the introduction of the 'transcendental anatomy' of E. Geoffroy Saint Hilaire into Britain.[7] He was also a Lamarckian evolutionist who had startled the young Darwin with support for this theory while the latter was a medical student at Edinburgh. Grant continued to support evolutionism, but made few major contributions in the later part of his career. After moving to London in 1827 his radical views on the relationships between living forms, coupled with his challenge to the medical establishment, embroiled him in numerous debates. He was poorly paid and overworked, which accounts for the decline in his published output after his move to London. Although a transmutationist, he did not participate in the development of evolutionary morphology in the years following the publication of the *Origin of Species*.

It was Lankester who was to turn University College into a leading centre for teaching and research into evolutionary relationships. Like Balfour at Cambridge, he imported the latest continental methods to establish an active centre of research. There is some evidence that his initial reaction to University College was negative. He wrote to Haeckel in 1876 explaining that he still kept his books at Oxford, but lived in London with his mother and sisters. At this point he was going into the College as little as possible, partly because his pay was poor and partly because it was so dirty there.[8] Matters soon began to improve, however,

[4] W. B. Carpenter to Mrs Lankester, 16 November 1874 (Lankester family papers).

[5] University College, Council Minutes, 9 January and 20 Februaty 1875.

[6] *Ibid.*, 7 November 1874.

[7] See Adrian Desmond, *The Politics of Evolution: Morphology, Medicine and Reform in Radical London* (Chicago, 1989).

[8] Lankester to Haeckel [September 1876], (Haeckel-Haus, Jena). Over a year later, however, Lankester indicated that he was still living in Oxford and spending only two days a week in London; see the letter to Anton Dohrn, 3 October 1877 (Stazione Zoologica archives, Naples).

and Lankester began the process of encouraging students to participate in the latest developments.

It is sufficient to compare the last examination set by Grant with the first drawn up by the young Lankester to understand the latter's originality. He compelled his students to cast a broad synthetic glance over their science.[9] The very first question reads:

> Define the science of Biology. Under what subdivisions would you class its subject matter? What do you understand by a natural classification? Give arguments for and against the genealogical tree of the animal family. Give examples of retrogressive development, and discuss them in connection with the question.

Lankester's greatest contribution consisted in submitting all his students to a rigorous discipline of practical laboratory work, on a scale not envisaged, though foreshadowed, by Huxley's classes at South Kensington. For his students' anatomical drawings, Lankester even introduced special intetleaved notebooks.[10] The laboratory was situated in the museum housing the Zoological Collection. Research was certainly encouraged:

> The Professor will be glad to make arrangements with gentlemen who, having passed through an elementary course [of dissection of selected animal types], and paid the Laboratory fee, are anxious to enter upon original investigarions in Comparative Anatomy and Zoology. He is prepared to receive a limited number of pupils who wish to devote themselves ro such researches, and will furnish them with the necessary marerial ... The Zootomical Laboratory in rhe Gallery of the Museum will be open Tuesday to Friday, 10 a.m. to 4 p.m.[11]

So it was that there developed the standard of zoological laboratory, enduring without material change till the end of the First World War. Not till then did genetics and biochemistry begin to loom as large as anatomical studies; a university chair of biochemistry was not founded until 1921.

Lankester received good support from the Council of the College, which voted £400 for the purchase of museum specimens and afforded

[9] University College Calendar, 1878.

[10] This point is mentioned in Lankester's journal for 1896 (Lankester family papers).

[11] University College Calendar, 1875-76, p. 53.

opportunity for the extended practical work. He became friendly with Sir George Young, President of the Council until 1886, who imbibed a number of Lankester's ideas on the organisation of science teaching. In the session of 1875-76, Lankester energetically refitted and rearranged the Museum, which, according to Burdon Sanderson, was in a very confused condition. Specimens were named with large tickets, and students were encouraged to read in the Museum and study the specimens. Facing the north window were cases containing preparations of the skeletons and viscera of specimens to illustrate the works of Gegenbaur and Huxley. So much in use was the Museum, that the Professor had to arrange for extra cleaning at his own expense. Lankester spared no pains to obtain rare and interesting specimens, such as the Thylacine (Tasmanian wolf) and other marsupials, *Limulus* (the King Crab, which he made the subject of a historic essay), *Protopterus* and other lung-fishes, together with many invertebrates which he donated to the Museum. Each year, Lankester wrote a report on the progress of the Museum in a book still preserved there. In October 1879, he notes that the money awarded by the Museum Committee was nearly exhausted. The next year he wanted the Committee to spend a further £150. The cost of specimens would make a present-day curator's mouth water. The hand and foot of a gorilla and orang cost £2-12sh. and the skeleton of the primitive mammal, Echidna (the spiny ant-eater) a mere £5! In October 1884, A. G. Bourne was given six months' leave to work at Naples and bought many specimens for the Museum. In 1886, work was started on a catalogue, giving the classification of specimens, and so printed that it could be cut up for labels. This catalogue was in the main completed a year later.[12] The care of the Museum involved Lankester in a great expenditure of time, the compilation of the Catalogue alone necessitating frequent visits to the Natural History Museum. There were sundry annoyances, such as the constant leak of water from the Physiology Laboratory, causing a roof fall, and smashing a case of reptilian specimens.[13]

Lectures were at first held on four days a week, from three to four in the afternoon, but in the session of 1886-87, Lankester had the temerity to fix his lectures for one o'clock. The courses were carefully graded, and always included an elementary course of surprising comprehensiveness,

[12] *A Label Catalogue of the Zoological Collection now under the care of the Jodrell Professor ... Printed for Private Circulation* (London, 1890). A copy of this pamphlet marked 'Professors Copy' in Lankester's hand is preserved at University College.

[13] Museum Report for 1890, University College, Council Minutes.

dispelling the idea of a narrowly morphological teaching.[14] The number of lectures rose from fifty to one hundred and twenty, and in the year of his retirement from the College (1890), Lankester was arranging courses for commencing students, occasional students, a full B.S. curriculum, and work for his advanced research students, who paid no fee and were considered as assisrants of the Professor. In 1882, the first class for women was organised. They did well, and the Professor especially remembered Philippa Fawcett, who went to Cambridge, after working brilliantly at the College.

The teaching of zoology by the use of selected animal 'types' has come in for much criticism. Sir Arthur Keith in his *Autobiography* congratulated himself that he had not received his education in biology in this way.[15] Lankester himself was by no means blind to the abuses of this system: 'At present students learn the anaromy of a mussel, and never think of it as representing a large group, and don't care whether there are other things more or less like it than not.'[16] The study of selected types is not enough, he claimed. 'Morphology is essentially comparative. It becomes daily more obvious that the histology, morphology and physiology of the organism must be consideted and treated without regard to the arbitrary separation of adult and embryonic conditions, and without the exclusiveness which the selection of types involves.'[17]

Lankester denounced the coercion of examination schedules and the multiplication of cram books. Practical work had been a great step forward, but was becoming a tyranny and impediment to knowledge. Teachers of a certain class were ignoring the generalisations of biology and were plying their pupils with the regulation details of the scheduled organisms. At a slightly later point in his career, Lankester noted with delight how Moseley's daughter, Margery, found a hitherto unnoticed gill-plume in the red-footed cray fish (*Astacus fluvatilis*). The demonstrator in her elementary biology class refused even to look at her discovery, and she was forced to send drawings directly to Lankester as a prelude to publication.[18]

[14] Details of the courses may be found in the University College Calendars, 1875-1890. An illustrated set of lecture notes taken by one of Lankester's students, A. J. Harris, probably in 1889, is preserved at University College.

[15] Arthur Keith, *An Autobiography* (London, 1950), p. 73.

[16] Lankester to Hooker, 5 July 1879 (Hooker papers, Kew).

[17] Lankester, 'Progress in Elementary Biology', *Nature*, 44 (1891): 290-92, p. 290. This is a review of T. J. Parker, *Lessons in Elementary Biology* (London, 1891).

[18] This incident is recounted in Lankester, *Secrets of Earth and Sea* (London, 1920), pp. 122-3.

A more serious criticism of the teaching of this period is that it was concerned more with the dead than the living animal. Huxley has been accused of being a 'necrologist', or mere laboratory anatomist.[19] Valid or not, this criticism cannot be made of Lankester, who himself regarded direct observation of the living animal as a necessary and fascinating part of his studies. It was he who introduced the term 'Bionomics' to denote the study of the living animal in its environment, anticipating what would later become known as ecology. However, it cannot be denied that students at this period expended their energies chiefly on the examination of preserved specimens. The Cambridge Professor of Comparative Anatomy, Alfred Newton (a noted ornithologist), remarked that although Darwin's work dealt with living creatures, the obsession of the time was with the dead, with bodies preserved and cut into the most refined slices.[20] Lankester was a keen critic of the textbooks that multiplied with the increasing popularity of biological studies. He found them deficient because they did not follow the best way of study and exposition of a problem, which was to trace its actual historical development. Lankester's success as a teacher was a result of his intense desire to arouse interest (as he believed, the basic factor in all education), and to share his knowledge with others. This in turn sprang from his passionate belief in the liberating power of scientific knowledge. As a lecturer he was superb, unlike Newton of Cambridge, who declared that he would give up part of his salary to pay a lecturer. Students were inclined to be rowdy, as some of Lankester's colleagues knew to their cost; but it never occurred to any student to misbehave during Lankester's lectures. These were delivered in a large theatre which penetrated to the second floor, and was lit by a skylight. On two sides were large curtain screens for the display of diagrams (of which the Professor had about five hundred), whilst a special arrangement of sliding boards gave him ample room for the use of coloured chalks, and gas burners were placed strategically, to counteract the London fogs.

Before the lecture commenced, Jessup would enter to hang up the diagrams. The dean of laboratory supervisors, Jessup, began his duties in September 1879, and actually remained laboratory steward till after the Great War, dying at the age of ninety-five. Promptly at one, greeted by applause, the Professor entered – well-dressed, a powerful confident figure with strong black hair and muscular torso, his height harmonising his bulk – in no way the learned man of popular conception.

[19] E. W. MacBride, *Huxley* (London, 1934), p. 131

[20] A. F. R. Wollaston, *Life of Alfred Newton, 1866-1907* (London, 1921) p. 102.

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Mezzotint from the portrait of Lankester by John Collier, 1907.

(Reproduced with the permission of the Natural History Museum)

> He looked round the room, surveyed us (as we felt somewhat as if we were cockroaches), and gave for an hour a clear constructive account of forms and conceptions wholly new to us, with such skill that we were unconscious of the marvellous scope and concentration of his lecture, and unconscious of difficulty in the subject. At the same time, apparently without effort, he drew on his many blackboards with firm sweeps of wide lines clear diagrams which were left untouched during the day for those who were unable to copy as quickly as he drew. I have no recollection of seeing him refer to notes, except to dictate the definition of groups, or rarely for some drawing. For us in 1881, he was infallible.[21]

Another pupil recalls his astonishment at seeing his Professor so young, healthy and well-dressed, and describes the resonant voice, and clear decisive sentences.[22]

The students who thus hung breathlessly on the sonorous tones of their teacher, or marvelled at the speed with which he worked, could only guess at the toil that lay behind it all. Early in 1877, he wrote to his mother that he worked all day and half the night, and that even on Sundays he could hardly find time for a walk. Seven years later, he could still write: 'I have been at work ever since five ... Just time for dinner, and I had a long day at the College too.'[23] Despite his grumbles, there is no doubt that his 'London period', was the happiest and most fruitful of Lankester's life. He knew it: 'London is the only happy place for me, and will always remain so.' He soon found it necessary to have an establishment of his own, and in 1879 took apartments at 11, Wellington Mansions, North Bank, 'in order to give all my attention and strength to my University College teaching.'

He also found it necessary to have assistance at the College. In 1879, R. H. Peck, D'Arcy Power, and A. G. Bourne functioned as demonstrators. Bourne became Professor of Biology in Madras, resigning his College post in December 1885. Lankester made his assistants work hard; G. C. Bourne says that they slaved for him, and he was merciless towards people who worked carelessly. But he was mindful of their interests, and did his best to obtain for them the promotion he considered their due. When Lankester resigned in 1890, he moved in the Senate that

[21] G. P. Bidder, Obituary of Lankester, *Nature*, 124 (1929): 345-6.

[22] S. J. Hickson, Obituary of Lankester, ibid., pp. 312-3.

[23] Letter of 6 October 1884 (Lankester family papers).

W. S. Benham be made Assistant Professor, and tead a long statement of his claims. Once he was assured of his pupils' worth, he admitted them to terms of close though always respectful intimacy, and those who had the privilege of being included among his protegées could recall many an evening in which wit and wisdom were blended in discussions that lasted well into the small hours of the morning. Many of those who were, for a longer or shorter time his pupils, both at London and later at Oxford, filled important zoological posts in universities all over the British Empire. The list of his pupils who attained eminence includes A. G. Bourne, G. C. Bourne, S. F. Harmer, S. J. Hickson, R. Gunther, W. B. S. Benham, A. Willey, and above all, E. S. Goodrich, described by Charles Singer as 'Lankester's greatest discovery'.[24] Comparative anatomy in Britain, may be said to have reached its culmination with Goodrich's *Studies on the Structure and Development of Vertebrates* of 1930. Originally a studenr at the Slade School of Arr in Universiry College, Goodrich went to listen to Lankester, was impressed, and decided on zoology. When Lankester migrated to Oxford, he took Goodrich with him as assistant, recognising his outstanding ability, and forging an imporrant link in the history of zoology. Ray Lankester was rhe virtual founder of the College Biological Society, taking the chair at the two inaugural meetings, and inviring his most eminenr friends to lecrure ro the members.[25] His Chair at the College soon received a considerable boosr. In 1874 Huxley's wealthy friend, T. Phillips Jodrell, had subsidised the Physiology Chair in substantial fashion. Impressed by Huxley's account of Lankester's work, he decided to subsidise rhe Zoology Chair as well. With the help of Sir George Young, the matter was arranged, and in the session of 1880-1881, Lankester's Chair became, as ir remains today, the 'Jodrell Chair of Zoology and Comparative Anaromy'.

In the same session, an extensive building project was completed at the College. Lankester's departmenr received additional space by the erection of a new wing, though only one room (his own private laboratory), actually formed part of the new building. The Department now consisted of a Lecture Theatre (already described), a North Museum, a South Museum (with an Aquarium), the Professor's Laboratory, and rooms on the second floor, conriguous to the Lecture Theatre, to which some of them formed a gallery. On the second floor were the large classrooms for practical work, the Attendants' Gallery, the N.W. Laboratory, and the

[24] Personal communication to J. Lester.

[25] *Universiry College Gazette* (26 November 1885), pp. 55-6 and 17 December 1885, p. 15.

Assistant's Laboratory and Storeroom. The Lecture Theatre could comfortably accommodate eighty students. In the new adjoining North Museum, the larger osteological specimens were now kept. In the South Museum were freshwater and marine aquaria in carefully oxygenated tanks. Some of Lankester's most important research was carried out in his new Private Laboratory, for its time an elegant and practical workplace. The students dissected in the 'Large Practical Classroom', fitted with twenty-five tables, each suitable for two students. Each table was equipped with an Argand gas burner. In the remaining N.W. and N.E. Laborarories, there worked the advanced students, and Professorial Assistants. A notable feature of these laboratories was a gas-fired incubator, with an internal chamber in which microscopes could be introduced, allowing a specimen to be incubated on the microscope stage. In the Assistant's Laboratory were kept the specimens for dissection, the larger in a slate tank. Apart from his private investigations, and work as Demonstrator, the Assistant had the arduous task of keeping specimens in good repair, mending osteological specimens, and replenishing bottles with the alcohol which was the universal preservative of the time, not yet displaced by the pungent formalin.[26] Lankester summed up his eatlier work in a letter to Hooker in 1879:

> I have been at this [lecturing and organising practical classes], for some years, and at University College, the Secretaty and my colleagues can bear witness to the continual increase in the attendance of my classes, and the success of my pupils at Burlington House [the London University exams]. I have entirely made rhe Museum, lecture diagrams, dissecting rooms and apparatus ar University College, when there was nothing of the kind before I came ...[27]

Much later, when he had left the College, Lankester wrote soberly: 'I fancy that I did more that was original and good when I went to University College, than I know of.'[28]

Increasing fame as a man of science, and success as a teacher could not but be gratifying to one who believed that recognition was the one reward desired by most men of science who had not attained to the heights of

[26] A description of the labotatories may be found in a pamphlet, *The University College of London: Extension of the North Wing. Description of the Rooms now provided for the Departments* (London, 1881).

[27] Lankester to Hooker, 1 September 1879 (Hooker papers, Kew).

[28] Lankester's journal for 1896 (Lankester family papers)

philosophical contempt for the regard and sympathy of fellow labourers. Yet there were shadows. Behind the forcefulness which was, at any rate in part, his armour against the world, lay the torment of a sensitive nature, a prey to indecision, self-doubt, and prone to self-criticism. Two problems loomed before him; the attainment of economic security, and the 'awful worty of matrimony', the necessity of ending a bachelor existence he felt to be sterile and lonely. 'I only feel one desire, and that is to get into some permanent line of work which shall bring me an income.' So he wrote in 1877, when he was earning not much more than £500 a year. Eight years later, when he was at the height of his powers, he complained that he was still uncertain as to income, and had lived long enough in his 'very small reriring bachelor way'.

Where marriage was concerned he was not attracted by the Victorian 'young lady'. Early in 1875 he confided to his mother:

> If I could only find some woman who had soul, a belief in things not believed in by society, and who could take a pleasure in existence apart from gossiping and empty distraction, and who would help me to do the same, I should be happy wherever I might be.

In 1876, it seemed he might attain his desire. He had for some time been a visitor at the home of Sir Benjamin Brodie, Oxford Professor of Chemistry. In July, he became engaged to one of Brodie's daughters, Ethel. He wrote home in delight, 'I have asked Ethel Brodie to marry me, she has accepted, and Sir Benjamin has given his blessing.' Ethel Brodie appears to have been a pious and conventional girl. Ray soon discovered in her a lack of spontaneity, a reference of every thought and action to the inane goddess of propriety and superfine young-lady-ism. He blamed it on her upbringing among sisters and aunts. At first he could not believe that things would end. In October he wrote, 'I am sure of her love, but there are shadows which arise and give me trouble.' He had a horrid feeling that he had lost a dream. The scandal would be great at Oxford if he broke the engagement, yet he came to feel it inevitable; in December, the engagement came to an end. He felt despondent, but was convinced that he had acted rightly.

> I don't care if I never marry, if I can't have what I want. I daresay I am very wrong, that is to say, my nature is perhaps a bad one ... but there it is, and I will not do violence to it.

He consoled himself by taking a close interest in the romance of his friend Dyer, now coming to a successful outcome with Harriet, the daughter of Sir Joseph Hooker. Dyer wrote to Mrs. Lankester, 'Poor dear man, he has been more successful in steering me into the right haven, than in navigating his own bark.'[29]

During the eighteen eighties, Ray Lankester's circle of friends extended enormously, his vigorous personality and brilliant conversation making him welcome in common-room, club and mansion. He was well-liked by his colleagues, among whom were men who left their mark on science: Burdon Sanderson the physiologist, Sidney Ringer, Professor of Medicine (a physiological saline is still known as 'Ringer's Solution'), Wilson Fox, Professor of Clinical Medicine, the anatomist G. D. Thane, the surgeon and obstetrician Wharton Jones, and Daniel Oliver the botanist.

Like a number of his colleagues, Lankester counted as a member of both the Faculty of Science and of Medicine. There was some friction between Lankester and the members of the latter Faculty. He was anxious that medical students should obtain a thorough grounding in biology, and at the end of 1876, served on a Senate Committee empowered to determine whether medical students were in fact sufficiently instructed in biology. Sanderson considered that his own department was being interfered with, and wrote angrily to the Senate.[30] When, in 1883, Lankester wrote to the Senate criticising the Medical Faculty pamphlet, *Suggestions for Students*, Wilson Fox blocked a Senate motion by Lankester, and forced him to withdraw 'every expression . . . offensive to any member of the Senate'.[31] In October 1884, Lankester moved in the Senate, that Lectures on the Course of General Biology be open to women.[32] The Medical Faculty would not accept this, but eventually it was arranged that the lectures should be opened to women, but must not be reckoned among those given by the Faculty. Then the Medicals succeeded in excluding Lankester from their Faculty, on the grounds that the Professor of Anatomy had no classes in the Faculty of Medicine![33] Lankester tried to reopen the matter in 1885, but failed.[34] There was little personal antagonism in all this, and Lankester, on a number of occasions, mentions his good relations with his colleagues, including Burdon Sanderson.

[29] Thiselton-Dyer to Mrs. Lankester, 31 May 1877 (Lankester family papers).

[30] Letter of 4 July 1878 quoted in University College Senate Report for that year.

[31] *Ibid.*, 16 November 1883.

[32] *Ibid.*, 22 October 1884.

[33] *Ibid.*, 11 December 1884.

[34] *Ibid.*, 16 April 1885.

In 1879, Lankester made a notable American friend. A young student named Henry Fairfield Osborn, came to England primarily to meet Balfour. He was introduced to Lankester, and records that he spent many delightful hours in his rooms, Lankester seemed to him to represent above all intense conviction, and he was astonished at the heat of the arguments that took place. Osborn returned home to become an outstanding palaeontological authority, but could never persuade Lankester to visit the U.S.A. because of his feat of seasickness.[35]

Lankester had a constitutional need of fresh air and exercise, preferably obtained by a round of golf. Continued work without a break he found intolerable. 'I have written MSS all day, and I am melancholy for lack of exercise,' he complained in a letter of 1889. 'I should like to abandon myself to a real open-air life, and only do such work as the spirit moved me to – a few hours a week.'[36] It was an ideal he never realised. He had too much intellect for the life of a country squire. He liked congenial company. To be alone was terrible, since it led him to introspection, the analysing of his maturer history, till he felt quite ready to die on the spot![37] Solitary travel to him was like the 'retreat' practised by Catholics. Most of his holidays he spent abroad. Paris he visited at least once each year. He often travelled to Switzerland, sometimes meeting Continental scientists, and sending home his delightful descriptions of natural scenery. In 1880, he spent some time with Ranvier, the French microscopist, and visited Pouchet and Perrier. He liked to combine his holidays with some work, and often carried his mictoscope with him. Thus in Switzerland, he discovered a new amoeba, subsequently described in the *Quarterly Journal of Microscopical Science.*[38]

> One ought to have a principle in rravelling; one ought to go especially aftet some object, no matter what. The mental inactivity resulting from simply loafing without any desire to know about the people or products or art of the country, is a very bad thing, and makes one miserable.[39]

His paint box accompanied his microscope on his travels. He liked to paint beetling Rhine cliffs, towering Alpine peaks or lush valleys, majestic

[35] H. F. Osborn, Obituary of Lankester, *Nature*, 124 (1929): 124.

[36] Letter of 27 January 1889 (Lankester family papers).

[37] Letter probably of 1885 (Lankester family papers).

[38] Lankester, 'Lithamoeba discus', *Quarterly Journal of Microscopical Science*, 19 (1879): 484-87.

[39] Letter of 9 September 1885 (Lankester family papers).

glaciers, or the calm blue of Lake Lucerne. Detailed and precise, he painted with the eye of an anatomist.

During this period, Lankester spent much time as an examiner for various bodies. He could say with truth that few people knew as much as he did about examinations. When he came to make a critical survey of exams in 1888, he divided them into four groups.[40] Examinations at school or university he felt to be valid if conducted by the actual teacher; but he disliked the institution of 'impartial examiners' and rigid examination schedules. These, he maintained, led to the degradation of teacher and pupil alike. His ideal was the German system: 'There is no more enviable position than that of the university professor as realised in Germany.'[41] This was the system of *Lehrfreiheit* and *Lernfreiheit*, founded upon Fichte's sketch for a University of Berlin. Fichte had shown that a university was not merely a place where instruction is given, but an institution for the training of experts in the art of making new knowledge, the end being attained by associating the pupil with the professor in the research which the latter initiates and pursues. Examinations in a university would thus be useful when conducted by the teacher as simple tests for a degree, not competitive, and not conferring distinction. Competitive exams for scholarships and fellowships Lankester considered to be thoroughly objectionable. Scholarships should be awarded only to those who would not be able to exist at a university without them.[42] This might need a legally certified statement of poverty from each candidate, but this would do no harm: 'There is too little poverty and too much luxury among the Oxford and Cambridge undergraduates at present.' He was very satisfied with the University of London examinations, and wrote to Huxley that the candidates had all had serious laboratory training, and that the average attainment was very good.[43]

No consideration of Lankester's attitude to examinations would be complete without some account of his row with the Vice Chancellor of Oxford University, concerning the viva voce exams. Lankester did not like 'vivas'. The London vivas were exhausting affairs lasting five days, but the Oxford vivas were very different, the sole relic of ancient scholastic exercises. The written exams had steadily encroached on the 'responsions', 'prelections' and 'wranglings', till the orals had become the mere ghost of

[40] Lankester's article on 'Examinations', originally from the *Universal Review* (November 1888) is reprinted in his *Advancement of Science* (London, 1890), pp. 175-92.

[41] *Ibid.*, p. 184.

[42] *Ibid.*, p. 191.

[43] Lankester to Huxley, 27 July 1892 (Huxley papers, Imperial College).

an obsolete academic past. Now Ray Lankester was the sole examiner in Animal Morphology in the Oxford School of Natural Science. A small part of the exam was oral. Lankester considered it to be a mere farce, causing expense and inconvenience, and admitted to make no difference to the result.

At the midsummer examination of 1887, Veley, Senior Resident Examiner in Natural Science, requested Professor Lankester to take his part in the vivas fixed for June 7 and 8. Considering the orals to be a useless relic, ripe for abolition, Lankester determined to force the issue. Accordingly, he informed Veley he would not attend. Lankester was ordered to return the exam papers in his possession, but was away on holiday. Thereupon the Vice Chancellor directed that Lankester be removed from his office. Lankester now applied to the Queen's Bench Division, calling upon the University to reinstate him. The University fought the case, and won.[44] The case attracted some attention, and *The Times* commented in a leading article.[45] Lankester replied in an angty letter, nettled by the suggestion, that even a junior colleague must not be handled as merely a dried preparation, and the Vice-Chancellor and Proctors as so much protoplasm. Lankester's letter began:

> It is bad enough to have failed in spite of the patient efforts of able counsel, to produce upon the minds of Mr. Justice Mathews, and Mr. Justice Smith, any more satisfactory result than that of bewildering ... without having one's posirion, intentions and arguments incorrectly reported to the public.[46]

He had, he said, been discharged for not complying with the regulations. No examiner did, for it was impossible to do so. There was something beneath the surface – the disobliging spirit of junior examiners, and a Vice Chancellor impatient with modern innovations, and subjects he did not like or understand.

During the eighteen seventies and eighties, Lankester became very satisfied wirh the progress of the *Quarterly Journal of Microscopical Science*, although it was not a pecuniary success. In 1878 he became sole editor, though he had a great deal of help from various scientists, especially Balfour and Klein. By 1883, the Journal had reached its final form. From 1863 to 1919, Lankester himself published 78 memoirs in it, all but three

[44] Reported in *The Times*, 9 February 1888.

[45] *The Times* leader, 10 February 1888.

[46] Lankester, letrer in *The Times*, 17 February 1888.

in his own name.[47] Most British and many foreign biologists contributed to the *Journal*, or supplied the best work of their pupils. To young biologists seeking publication, Lankester was kindness itself. Lister, Moseley, Milnes Marshall, Goodrich, Poulton, Weldon, Benham, Willey, Hickson, Minchin, Graham Kerr, MacBride all contributed – their names read like a roll of honour of British biology. Osborn, Packard, Wilson, Van Beneden, Pelseneer, Ranvier, Hubrecht, Grassi, Bütschli, Kovalevsky, all contributed. Some of the papers that appeared in the *Journal* are classics. In 1883, Thomas published his important discovery concerning the life-cycle of the liver fluke, now a textbook commonplace. Lankester was actually with him in the laboratory when the discovery was made, and watched excitedly as the tiny miracidia larvae penetrated the skin of their host, the freshwater snail *Limnaea truncatula*, collected by Thomas from an Oxford meadow.[48]

In the summer of 1879, Lankester and Donkin were in Paris, and took the opportunity to visit the Salpêtrière, where the physician Charcot was carrying out investigations into hysteria. Strange doctrines of magnetotherapy and metallotherapy prevailed at the Salpêtrière. A susceptible female patient held an electromagnet in the hand, the magnet energised by a bichromate battery. As soon as the wire was connected up, the hand or arm of the woman holding the magnet lost all sensation, and large carpet needles could be thrust into the hand or arm. Lankester was convinced that electricity had nothing to do with the phenomenon. While Charcot was out of the room, he emptied the essential chemical fluid out of the batteries. When Charcot returned and repeated the experiment, the patients showed the same anaesthesia. Lankester explained what he had done, fully expecting to be shown the door; but the great investigator seized his hand, crying 'Well done Monsieur'. A close friendship developed between the two men, Lankester spending much time at Charcot's home.[49]

It was to be expected that Ray Lankester would carry over to University College his passion for university reform. He liked to remind people that it was a strange feature among the many anomalies characterising the greatest and richest city in the world, that it had no university – adding

[47] G. C. Bourne, 'Fifty Years of the Q. J. M. S.', *Quarterly Journal of Microscopical Science*, 64 (1919): 1-17.

[48] Lankester, *Great and Small Things* (London, 1923), p. 72.

[49] Lankester, *From an Easy Chair* (London, 1908), pp. 61-4; reprinted in *Great and Small Things*, pp. 150-55. For background see Anne Harrington, 'Hysteria, Hypnosis and the Lure of the Invisible: the Rise of Neo-mesmerism in *fin de siècle* Freud Psychiatry', W. F. Bynum, Roy Porter and Michael Shepherd (eds.), *The Anatomy of Madness. Essays in the History of Psychiatry*, (London, 1988), pp. 226-46.

maliciously that this was a distinction it shared with Constantinople! A University of London did exist, but it was a mere examining body. Lankester stated the problem with his accustomed urgency.

> If London does not within the next decade see a true universiry established and carrying on its beneficent work in its midst, it will be because the powerful and wealthy men who use the great city for their profit and pleasure are so intently set upon the games of commercial speculation and party politics that they are careless of that which is the one and indispensable foundation of national greatness.[50]

Lankester's ideal for London was the German or Scottish model; a university with its various faculties, each numbering some twenty professors, and the combined sixty forming the Senate, in which the absolure governing authority would be vested. In 1884, he joined the Association for the Promotion of a Teaching University in London, and was soon recognised as one of its most active members. Huxley would not support the extreme 'professorial' view of reform, any more than he would accept Lankester's view that the medical student should receive a very extensive general biological education.[51] Huxley, it must be borne in mind, was a governor of University College. It was actually Lankester who was responsible for the motion that the Council of University College should seek affiliation to the Victoria University, the former Owen's College of Manchester.[52] He spoke in rhis sense to the General Meeting of the Members of the College, and saw his motion accepted, but in the January of the year following, it was defeated by the opposition of the Medical Faculty.[53] Then he began to work for the view that the College should be granted the right to confer its own degrees. He gave evidence befote the Selbourne and Gresham Commissions, steadfastly adhering to the 'professorial' school of thought. He was a member of the Committee appointed by the Senate to report upon the findings of the Selbourne Commission, and one sees his influence in the statement in which the Senate rejected the report of the Commission. Not till 1900 was the Act passed which formed the Statutes by which the University of London was governed. Under the new scheme, the teachers were kept in a minority. Huxley's Royal College of Science remained free from the University for many years.

[50] Lankester, 'A University for London', *Pall Mall Gazette* (22 May 1884): 1-2.

[51] Lankesrer to Huxley, 4 April 1892 (Huxley Papers, Imperial College).

[52] University College, Senate Report, 20 November 1886.

[53] *University College Gazette* (22 February 1887): 99.

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The Anatomist and Evolutionist

Lankester gained an international reputation as a biologist, but his name is largely forgotten today. He came onto the scene just too late to be involved in the Darwinian debate, and his creative period was over before the great revolutions of the early twentieth century associated with the advent of Mendelian genetics. He belonged to a generation whose work has been largely dismissed as derivative, a mere filling in of the basic details of how life evolved without reference to the problems of the evolutionary mechanism.

Evolutionary morphology was one of the great scientific enterprises of the late nineteenth century. By transmuting the experiences gained by their predecessors in the light of the theory of evolution, morphologists such as Lankester threw new light on the nature of organic structures and created an overview of the evolutionary relationships that might exist between different forms. Their work has been dismissed as of minor importance by later generations, but that is because so much of it is now taken for granted, while the fundamental problems they encountered have been pushed aside rather than solved in the twentieth century. Lankester himself believed that the phenomena of life would ultimately be explained in terms of the chemical properties of elements, but he played no part in the emergence of molecular biology.

Although he wrote a number of popular books and edited a multi-volumed survey of zoology, Lankester gained his reputation primarily through the publication of articles and memoirs in the technical journals. There is no single work which stands out as his major contribution. As the editor of one of the leading journals of the day, the *Quarterly Journal of Microscopical Science*, he had strong opinions on the techniques of scientific publication. He believed that authors should strive for clarity, and insisted that predecessors in the field should be carefully cited. Lankester himself was familiar with the work of continental, especially German, biologists, and he was instrumental in bringing English-speaking biologists up to the level of competence that had long been taken for granted elsewhere. He insisted on good quality illustrations, a requirement of particular importance when the chief purpose of many articles was the

elucidation of the minute internal structures of living things. A good indication of his own style can be gained from his articles in the *Encyclopaedia Britannica*, which were reprinted in 1891 with the aim of rendering them more accessible to university students.[1]

Although Lankester gained fame as a morphologist dealing with living animals, it is easy ro forget that he had an early interest in fossils. His first scientific publication – at the age of fifteen – was a letter to *The Geologist* on the head shield of the Palaeozoic fish *Pteraspis*. In 1870 he published a monograph on the Cephalaspids, the armoured fish of the Palaeozoic. Differing from most authorities, who interpreted these fish as armoured relatives of the modern lamprey, he saw them as a detached group of primitive jawless vertebrates unrelated to any living fish. His influence in palaeontological circles may be judged from the printed menu of a Palaeontographical Society dinner in 1874, which shows Lankester writing his name on the top of a gigantic fossil plant, while a dinosaur looks on through binoculars.[2] His popular book, *Extinct Animals* of 1905 was thus by no means a venture into unknown territoty.

Lankester delighted in the observation of narure in the wild, but most of his reputation was built on work in the laboratory or dissecting room. Animal morphology, the detailed comparative study of animal form, had developed extensively in the first half of the nineteenth century.[3] Morphologists sought the underlying relationships between different kinds of animal structure, conceived not in an evolutionary light, but in terms of idealized archetypes, basic patterns of which the structure of real species was but a superifical adaptive modification. Following the acceptance of evolutionism in the 1860s, the search for archetypes was replaced by the search for common ancestors. Morphology became, in effect, an attempt to reconstruct the tree of evolutionary relationships through an evaluation of the underlying structural resemblances between different forms.

[1] Lankester, *Zoological Articles Contributed to the Encyclopaedia Britannica* (London, 1891).

[2] Lankesrer family papers. Lankesrer's first article was his 'Restoration of Pteraspis', *Geologist*, 5 (1862): 451-2. His monograph is *The Fishes of the Old Red Sandstone of Britain. Part I. The Cephalaspidae* (London, 1868). For an outline of his views on these ancient fish see his 'The Taxonomic Posirion of rhe Preraspidae and Cephalaspidae', *Natural Science*, 11 (1897): 45-7.

[3] On the history of morphology see E. S. Russell, *Form and Function: A Contribution to the History of Animal Morphology* (London, 1916). More specifically on evolutionary morphology see William Coleman, 'Morphology between Type Concepr and Descent Theory', *Journal of the History of Medicine*, 31 (1976): 149-75, and Peter J. Bowler, 'Development and Adaprarion: Evolurionary Concepts in Brirish Morphology, 1870- 1914', *British Journal for the History of Science*, 22 (1989): 283-97.

Originally, morphology had depended on the analysis of adult structures, but by Lankester's time it was increasingly realized that comparative embryology offered an even better possibility of discovering evolutionary relationships. Two forms whose adult structure differed widely might have embryos which exhibited close similarities, and this was interpreted as evidence of an evolutionary link between them. Ernst Haeckel proposed his biogenetic law, otherwise known as the recapitulation theory, according to which the growing embryo actually passes through stages corresponding to the sequence of adult forms in its species' evolutionary history. Studies based on these techniques owed comparatively little to Darwin's biogeography, or to his detailed efforts to understand the interaction between the organism and its environment. Morphologists realized in principle that changed habits and environments must play a role in the shaping of forms through evolution, but in practice they were concerned only to reconstruct the path of evolution, not to explain the adaptive forces at work.

Some of Lankester's research involved the description of newly discovered species, but much of it was devoted to known species whose detailed structure was as yet not fully described. It might seem that mere description was a fairly routine process, but Lankester was well aware of the difficulties involved:

> It is astonishing how many good observers it requires to dissect and draw and record over and over again the structure of an animal, before an approximately correct account is obtained.[4]

The observer tended to see what he or she was expected to see, and once a particular structure was in the textbook, students regularly saw it even when later research showed that the textbook was wrong. Controversy could erupt when the proponents of two different interpretations of evolutionary ancestry disagreed over the true significance of the structures they observed.

In the mid 1860s Lankester had written a number of papers on earthworms that attracted the attention of both Darwin and Richard Owen. He did important work on the physiology of worms and other invertebrates, investigating the chemical constitution of their blood. His later morphological studies also tended to concentrate on invertebrates, and particular attention needs to be paid to his studies of molluscan

[4] Lankester, 'Zoology', *Encyclopaedia Britannica*, 9th edn., 24 (London, 1888), pp. 799-820, p. 800.

anatomy and his reassessment of *Limulus*, the king-crab. Lankester's 86-page memoir 'Limulus an Arachnid' of 1881 was widely cited as a classic.[5] The king crab had, as its name implies, been widely regarded as a crustacean. Lankester affirmed, as other biologists had already begun to suspect, that in fact it was far more closely related to the scorpion and other arachnids. He compared the two forms organ by organ, showing for instance how the lung books of *Scorpio* are analogous to the gills of *Limulus*. Thus, although the scorpion is specialized for life on land, while the more primitive king crab lives in water, there is a fundamental identity of structure proclaiming an evolutionary relationship. The historical link between them was the extinct sea scorpions of the Palaeozoic, the Eurypterids. Lankester's memoir did far more than merely transfer a known species from one branch of the tree of evolutionary relationships to another. It forced biologists to reassess the steps by which the the various branches of the Arthropoda (crustaceans and arachnids) had evolved, a topic which remained controversial throughout the rest of the century.

Lankester's researches on the structure and development of the molluscs also led to far-ranging conclusions. At a practical level, he discovered the shell-gland of gasteropods. More fundamentally, he investigated the embryological development of various molluscs with a view to ascertaining the primitive type from which the different membets of the phylum had diverged.[6] He defined the ctenidum as the most primitive molluscan organ. Along with Paul Pelseneer, who wrote the volume on the molluscs in the *Treatise on Zoology* edited by Lankester, he proposed a theory of the origin of the molluscs from segmented annelid worms. This theory made use of the theory of neotony (discussed below) to propose that the annelid ancestor had lost its segmentation by dropping the original adult stage from its life history. Lankester also made fundamental contributions to the understanding of the evolutionary significance of the earliest srages in the development of the embryo, and introduced many new terms to denote stages in the developmental sequence. An earlier generation of embryologists had identified the layers of cells in the early embryo from which the various organs developed. Huxley proposed that these germ layers were homologous throughout the animal kingdom and saw the coelenterates (jellyfish etc.) as two-layered animals ('diploblastic' in Lankester's

[5] Lankester, 'Limulus an Arachnid', *Quarterly Journal of Microscopical Science*, 21 (1881): 504-48 and 609-49. The Lankester family papers contain a note dated 1921 in which Lankester listed what he considered to be his chief contributions to evolutionism. In America, *Limulus* is usually known as the 'horseshoe crab'.

[6] Lankester, 'Contributions to the Developmental History of the Mollusca', *Philosophical Transactions of the Royal Society*, 165 (1875): 1-48.

terminology). This diploblastic stage was passed through in the development of all the higher animals, and in 1867 Kovalevsky had described the creation of the two-layered stage from a single, spherical layer by a process of invagination, or in-folding.

Haeckel had proposed that a unicellular egg develops by cleavage into a blastula, or hollow single-layered sphere, which by invagination turns into a two-layered gastrula with an ectoderm and endoderm. In his theory the gastrula stage in individual development corresponded to a primitive organism existing at the dawn of life, the ancestor of all the higher animals. Lankester challenged the significance of Haeckel's findings, insisting that the mouth of the gastrula (the 'blastopore') often closed up in the course of development, a new mouth and anus being produced at a slightly later stage. Thus the gastrula lacked the phylogenetic significance attributed to it by Haeckel (*i.e.* there was no corresponding stage in the evolutionary development of the higher animals). Lankester's own theory of the 'planula' served as an alternative explanation of the early stages of development via a solid mass of cells.[7]

In addition to his work on the early stages in the development of higher organisms, Lankester followed the progress of bacteriology very closely. He made an important contribution in his 1873 article on the 'peach-coloured bacterium', *Bacillus rubescens*, where he suggested that the natural species of bacteria are capable of changing their external form within certain limits.[8] Because of this phenomenon of pleomorphism, bacteria would have to be characterized not by a simple external form, but by the ensemble of their morphological and physical properties exhibited in their complete life-histories.

In addition to his *Encyclopaedia Britannica* articles, Lankester long harboured a desire to produce an English-language equivalent of the great continental works on comparative zoology. It was only after his move to the British Museum (Natural History) that he began to fulfill this ambition through his editing of the *Treatise on Zoology*. This was a trying

[7] Lankester, 'On the Primitive Cell-Layers of the Embryo as the Basis of Genealogical Classification of Animals', *Annals and Magazine of Natural History*, 4th series, 11 (1873): 321-38; 'Notes on the Embryology and Classification of the Animal Kingdom; Comprising a Revision of Speculations Relative to the Origin and Significance of the Germ Layers', *Quarterly Journal of Microscopical Science*, 17 (1877): 399-454. Paul J. Weindling, *Darwinism and Social Darwinism in Imperial Germany: The Contribution of the Cell Biologist Oscar Hertwig (1849-1922)* (Stuttgart, 1991), pp. 86-108. Stephen J. Gould, *Ontogeny and Phylogeny* (Cambridge, Mass., 1977), pp. 76-85.

[8] Lankester, 'On a Peach-Coloured Bacterium, B. rubescens, N.S.', *Quarterly Journal of Micoscopical Science*, 13 (1873): 408-25.

time for Lankester (see chapter 11 below), and the task of lining up contributors and seeing their texts into print proved very difficult. Lankester became impatient with contributors and made impossible demands – W. T. Calman was told that his volume on the Crustacea was wanted within one year. The volumes eventually began to come out in 1900, and Lankester himself wrote important introductions to parts I and II of the series. The last volume to appear was E. S. Goodrich's on the Cyclostomes and fishes in 1909. Eight volumes had been produced in all, but the impetus had now departed from the editor and the series remained incomplete. This was in contrast to the rival *Cambridge Natural History*, which was completed and reprinted several times. Lankester's *Treatise* forms a magnificent fragment, an authoritative summary of the period when morphology was king, but which was already being bypassed by advances underway elsewhere in biology at the time of its production.

In the course of his researches Lankester addressed a number of major issues in evolution theory and made several important contributions. In the early part of his career he was strongly influenced by Haeckel, and although he was never an uncritical follower, he sometimes found it difficult to break away from the German biologist's image of evolution as the ascent of a hierarchy or ladder of developmental stages. He was well aware of the problems associated with the recapitulation theory, in which the evolution of life was modelled on the development of the individual organism toward maturity. In his work on molluscan embryology, for instance, he stressed the extent to which the presence of nutritive material in the egg disturbed the tendency for development to recapitulate ancestral stages.[9] Yet without such disturbing factors, he accepted that recapitulation could occur.

Linked to Haeckel's recapitulationism was a tendency to view the lower modern organisms as relics of earlier stages in the ascent of life towards mankind. In principle, Lankester warned against the dangers of this assumption.

> The belief that the lower forms are ancestral to the higher arises from a very common tendency of the mind, against which the naturalist has to guard himself – a tendency which finds expression in the very widespread notion that the existing anthropoid apes, and more especially the gorilla, must be looked upon as the ancestors of mankind, if once the doctrine of the

[9] Lankester, 'Contributions to the Developmental History of the Mollusca' (note 6), p. 33.

> descent of man from ape-like forefathers is admitted. A little reflexion suffices to show that any given living form, such as the gorilla, cannot possibly be the ancestral form from which man was derived, since ex hypothesi that ancestral form underwent modification and development, and in so doing, ceased to exist.[10]

This passage nicely illustrates Darwin's image of evolution as an ever-branching tree rather than a ladder – yet Lankester himself then goes on to claim that *Balanoglossus* is only a slightly modified form of the ancestral vertebrate.

Elsewhere, Lankester tended to visualize the tree of evolution as having a central trunk aimed at the production of mankind. In 1877 he warned against the dangers of treating evolution as a linear process, but then went on to claim that:

> the general doctrine of evolution justifies us in assuming, at one period or another, a progression from the simplest to the most complicated grades of structure; that we are warranted in assuming at least one progressive series leading from the monoplast to man; and that until we have special reason to take a different view of any particular case we are bound to make the smallest amount of assumption by assigning to the various groups of organisms the place which they will fit on the supposition that they do represent in reality the original progressive series.[11]

Lankester thus defined a 'grade' of organization by postulating a main stem of evolution: divergent branches were unified in a single grade by the fact that they all branched off the main stem at the same point. He did not envisage a grade as a level of organization that could be achieved independently by several different lines of evolution.

At the same time, Lankester was strongly aware of the factors that could mislead the morphologist seeking the evolutionary links between distinct forms. Earlier in the century Richard Owen had distinguished between 'analogy' and 'homology.' Analogous organs have the same function, but have different structures and show no genuine affinity; an example would be the wings of a bird and a bat. Homologous organs may be applied to different functions, but have the same underlying structures and (in

[10] Lankester, 'Vertebrata', in Zoological Articles (note 4), pp. 173-83, see p. 175.

[11] Lankester, 'Notes on the Embryology and Classification of the Animal Kingdom' (note 7), p. 440.

evolutionary terms) indicate descent from a common ancestor; here the human hand, the forelimb of a horse and the flipper of a whale may serve as an example. In 1870 Lankester argued that embryological research was now showing that some apparently homologous structures had, in fact, been produced independently by different lines of evolution adapting to similar circumstances. He proposed to distinguish between 'homogeny' (organs which are similar by virtue of inheritence from a common ancestor) and 'homoplasy' (where the similarity is due to mechanical adaptation).[12] The morphologist should assume the unity of type as long as there is no evidence to the contrary, but must be constantly on the alert for the deceptive effect of homoplasy. The term 'homoplasy' was used by some late-nineteenth-century naturalists, but was eventually replaced by the modern term 'convergent evolution'. Equally important, in view of Lankester's own tendency to think in terms of a developmental hierarchy, was his emphasis on degeneration. This theme had been developed by his friend Anton Dohrn in his *Ursprung der Wirbelthiere* of 1875. On 22 August 1879 Lankester gave an address on the topic to the British Association for the Advancement of Science which was published as a small book in the following year under the title *Degeneration: A Chapter in Darwinism.* He claimed that naturalists had assumed that natural selection has 'invariably acted so as either to improve or elaborate the structure of all the organisms subjected to it, or else has left them unchanged, exactly fitted to their conditions, maintained as it were in a state of balance'.[13] But simple preservation of earlier stages was not the only explanation for the continued existence of primitive structures. Evolution could lead to degeneration, especially when a species was adapting to a less rigorous way of life such as parasitism. The Tunicates or sea-squirts were degenerate offshoots of a primitive stage of vertebrate evolution, adapted to a sessile instead of an active life.

The rival theories of the origin of the vertebrates debated in the late nineteenth century all made use of the concept of degeneration in one way or another. Arthur Willey, a one-time student of Lankester, dedicated his *Amphioxus and the Ancestry of the Vertebrates* of 1894 to him. But Lankester was also anxious to stress the social implications of the idea. He

[12] Lankester, 'On the Use of the Term Homology in Modern Zoology, and the Distinction between Homogenetic and Homoplastic Agreements', *Annals and Magazine of Natural History*, 4th series, 6 (1870): 34-43.

[13] Lankester, *Degeneration: A Chapter in Darwinism* (London, 1880), p. 26. This is reprinted in Lankester, *The Advancement of Science* (London, 1890) and in William Coleman, ed., *The Interpretation of Animal Form* (New York, 1967).

argued that any conditions 'which render its food and safety very easily attained, seem to lead as a rule to degeneration; just as an active healthy man sometimes degenerates when he becomes suddenly possessed of a fortune; or as Rome degenerated when possessed of the riches of the ancient world'.[14] What was to prevent our own industrial civilisation degenerating? Lankester's answer was science, which could generate a never-ending source of stimulus for the human mind. There is some evidence, however, that the degenerate future envisioned by Lankester's friend H. G. Wells in his *The Time Machine* of 1895 owes something to the influence of Lankester's book.[15]

In an addition to the printed version of his talk on degeneration Lankester also commented on the evolutionary significance of what he called 'super-larvation', or what would later be called neotony. Noting that the Axolotl can breed while still in the aquatic, gilled form (*i.e.* without maturing into a terrestrial form like other amphibians), he argued that the same effect could have occurred at various points in evolution:

> By super-larvation, it would be possible for an embryonic form developed in relation to special embryonic conditions and not recapitulative of an ancestry, to become the adult form of the race and thus to give to the subsequent evolution of the race a totally different and otherwise improbable direction.[16]

Lankester thus anticipated the views later developed by Walter Garstang in the 1920s.[17]

Some of the strongest advocates of the theory that ontogeny recapitulates phylogeny interpreted it in terms of the Lamarckian mechanism of the inheritance of acquired characteristics. Ancestral stages were recapitulated because evolution took place through the adding on of developmental stages acquired by the adult when adapting itself to new conditions. In some of his writings of the 1870s Lankester himself seems to imply that variation might be affected by the interaction of the organism and its environment, but in later decades he emerged as a champion of the Darwinian selection theory against Lamarckism. He was always careful to give credit to Lamarck himself as one of the founders of

[14] Degeneration, p. 33.

[15] See Peter J. Bowler, 'Holding Your Head Up High: Degeneration and Orthogenesis in Theories of Human Evolution', in J. R. Moore, ed., *History, Humanity and Evolution: Essays for John C. Greene* (Cambridge, 1989), pp. 329-53.

[16] Lankester, *Advancement of Science* (note 13), p. 57.

[17] See Stephen Jay Gould, *Ontogeny and Phylogeny* (Cambridge, Mass, 1977), part 2.

evolution theory. But in the 1880s some natutalists wete setting up Lamarckism as an alternative to Darwinism, and this polarized opinions on the question of the inheritance of acquired characters.[18] August Weismann had now developed his theory of the 'germ plasm' in which the transmission of characters via heredity could not be affected by changes to the adult body. Lankester joined the ranks of the 'neo-Darwinians' who were attempting – by no means successfully at first – to stem the tide of Lamarckism.

One of the earliest supporters of Lamarckism against Darwinism in Britain was Samuel Butler, whose *Evolution Old and New* of 1879 began a campaign which alientated him from the scientific community. Lankester wrote a lerter to *The Athenaeum* in 1884 criticizing Butler's views and giving a clear statement of his growing conviction that there was no inheritance of acquired characters.[19] In the course of the 1890s he debated with Herbert Spencer and othets in the columns of *Nature* and *The Times*, insisting over and over again that the evidence for Lamarckism was unsound.[20] He was the first to point out that Lamarckism involved a conceptual difficulty; Lamarck had proposed two laws (1) that individuals can adapt themselves to new conditions and (2) that these adaptations can accumulate through inheritance. As E. B. Poulton put it in a follow-up to Lankester's letter: 'The first law assumes that a past history of indefinite duration is powerless to create a bias by which the present can be controlled; while the second assumes that the brief history of the present can readily raise a bias to control the future.'[21] In an article written for the *Nineteenth Century and After* in 1910 hc pointed out that sclection could enhance a general 'educability' which might give the impression that specific skills were being inherited.[22]

[18] On Lamatckism see Peter J. Bowler, *The Eclipse of Darwinism: Anti-Darwinian Evolution Theories in the Decades around 1900* (Baltimore, 1983), chapter 4; more generally on the non-Darwinian characet of much late-nineteenth-century evolutionism, see Bowler, *The Non-Darwinian Revolution: Reinterpreting a Historical Myth* (Balrimore, 1988).

[19] Lankester, letter in the *Athenaeum* (1884), p. 412. See Philip J. Pauly, 'Samuel Butler and his Darwinian Critics', *Victorian Studies*, 25 (1982): 161-80.

[20] Lankester wrote a large number of letter on Lamarckism, see for instance *Nature*, 41 (1889-90): 129-30 and 415; 47 (1892-3): 389 and 486; 51 (1894-5): 102-3; see also *The Times*, February 16th 1889. More substantial is his 'Heredity and the Direct Action of the Environment', *Nineteenth Century and After 19-20 (1910)*: 482-91. Lankesrer held Lamarck himself in high esreem, see his letter supporting the erection of a sratue of Lamarck, *Nature*, 77 (1907-08): 149.

[21] Poulton, letter on 'Acquited Characters', *Nature*, 51 (1894-5): 126-7.

[22] Lankester, 'Heredity and the Direct Action of the Environment' (nore 20).

Although Lankester rejected Lamarckism, he did not follow Darwin in assuming that variation among individual organisms was completely random. Like Huxley, he preferred to believe that all the members of a species inherited a common tendency to vary in certain directions. This was what he called 'correlated variation', because it affected the whole structure of the organism rather than individual characters. In some notes on the subject prepared in 1896 he wrote:

> It is a most misleading and erroneous view which has somehow got its head up, that any given animal varies in all its parts in every conceivable way, so that any result may be produced by selection. On the contrary, variation in every group is characteristic, is limited by the already selected and emphasized characteristics of the group. Every part therefore of an animal which varies, varies not 'equally round a norm,' but varies in accordance with the constitutional tendency of the organism, which may be called its ancestral bias, or group bias.[23]

Lankester did not go as far as the advocates of 'orthogenesis,' who maintained that such trends could produce nonadaptive and even harmful characters, but like many morphologists and palaeontologists he thought that evolution was controlled by something more definite that the reaction of the population to changes in the local environment.

In the early twentieth century, Lankester was one of the first Darwinians to note that the discovery of radioactivity solved the problem created by the physicists' views on the age of the earth. Physicists such as Lord Kelvin had argued that the earth was gradually cooling down from a molten state and could be no more than a hundred million years old.[24] This was not time enough for natural selection to produce the vast range of living forms now inhabiting the earth, thus encouraging those biologists who looked for non-Darwinian mechanisms of evolution. Once it was realized that the radioactivity of the rocks of the earth's core could produce heat over a long time period, the objection to Darwinism was negated. Lankester pointed this out clearly in his presidential address to the British Association for the Advancement of Science in 1906.[25]

[23] These 'Notes on Variation' are contained, along with many other items, in a large diary heavily bound in red; Lankester family papers.

[24] See Joe D. Burchfield, *Lord Kelvin and the Age of the Earth* (New York, 1975).

[25] Reprinted as 'The Progress of Science' in Lankester, *The Kingdom of Man* (London, 1907), pp. 66-158, see p.p. 80-81.

Like many Darwinians of this period, however, Lankester had major reservations about the advent of Mendelian genetics. Although the synthesis of genetics with the selection theory would eventually solve many of the problems of Darwinism, the immediate aftermath of the 'rediscovery' of Mendel's laws of heredity was the rejection by many Darwinians of what was presented as the basis for an alternative, saltationist, theory of evolution. In 1917 he wrote a review of T. H. Morgan's *Critique of the Theory of Evolution* in which he denied the significance of the chromosomal effects that Morgan had demonstrated. Seizing upon the Morgan school's neglect of the process of individual development, he insisted:

> The question as to how the [Mendelian] factors arise, and how they influence the development of the embryo, cannot, as Professor Morgan somewhat arbitrarily states, be excluded from the solution of the real, traditional and actual problem of heredity.[26]

Some modern biologists would say that Lankester's caution was justified, but his refusal to go along with the new emphasis on the transmission of hereditary characters meant that he was unable to keep up with the most active developments of early-twentieth-century biology.

Like many biologists of his generation, Lankester assumed that the characteristics of living things were determined by the nature of the 'protoplasm' of which they are composed. He had little to say on the nature of protoplasm, but made an important suggestion concerning the earliest origin of living things. To his *Encyclopaedia Britannica* article on 'Protozoa' he added some notes on this topic:

> Briefly stated, the present writer's view is that the earliest protoplasm did not possess chlorophyll, and therefore did not possess the power of feeding upon carbonic acid. A conceivable state of things is that a vast amount of albuminoids and other such compounds had been brought into existence by those processes which culminated in the development of the first protoplasm; and it seems therefore likely enough that the first protoplasm fed upon these antecedent steps in its own evolution, just as animals feed upon organic compounds at the present day.[27]

[26] Lankester, review of Morgan's *Critique of the Theory of Evolution*, *Nature*, 99 (1917): 181.

[27] Lankester, Zoological Articles (note 1), p. 3. See John Farley, *The Spontaneous Generation Controversy from Descartes to Oparin* (Baltimore, 1977).

Here Lankester anticipated the theory later put forward by Oparin and Haldane, in which the earliest steps in the gradual evolution of life were eliminated by their descendants.

Lankester's views on this topic were influenced by his prevailing belief that the nature of life would ultimately be explained in terms of the physical properties of matter. His dislike of Lamarckism was probably influenced by the claims of Samuel Butler and others that this should become part of an anti-materialist philosophy that would preserve the purposefulness of life as a basic characteristic. He also attacked Lord Kelvin for advocating a creative and directing force in living organisms.[28] Although not a simpleminded reductionist, his distrust of vitalism and the growing tendency toward an almost mystical view of life was so strong that he sometimes supported the narrowest exponents of mechanism. In 1912 he wrote a preface to to H. S. R. Elliot's *Modern Science and the Illusions of Professor Bergson*, a book which attacked Bergson's concept of the *élan vital* so vigorously that it denied any significance to consciousness. In fact, Lankester believed that mental functions had evolved as a consequence of the increasing complexity of living things. But he was determined to resist those philosophers whose metaphysical speculations led them to make statements about matters of what he considered to be scientific fact.

[28] Lankester, letter to *The Times*, 17 May 1903, reprinted in *The Kingdom of Man* (note 25), pp. 62-65.

– 8 –
Slade and Superstition, Edinburgh Fiasco, Norwegian Success

Among the treasured possessions of Ray Lankester was an object that had been given to his father by Michael Faraday. It consisted of two glass rods, with thin boards on either side held firmly in place by rubber bands. It was the 'Plancherte', which Faraday had devised in 1855 to show that the cause of table-turning phenomena was the pressure of the hands upon the table. This and other spiritualistic pastimes spread like wildfire in the sixties and seventies of the nineteenth century. Men of science were not always exempt from the prevailing credulity. Two outstanding scientists professed a belief in spirit manifestations: William Crookes and Alfred Russel Wallace. In 1876 Wallace sponsored the reading of a paper on spiritualism by William Barrett at the meeting of the British Association for the Advancement of Science. Lankester wrote to *The Times* suggesting that Wallace's position on this matter was questionable and that the British Association had been degraded. This naturally elicited a reply from Wallace.[1]

Also in 1876, there appeared in London a tall, thin and drooping American medium, Henry Slade. He installed himself at no. 8 Upper Bedford Place, and with his coadjutor Simmons (whose job it was to listen to visitors' anteroom conversations) practised spirit-writing on a slate at a guinea a seance. Although Ray Lankester did not know it, Slade's trickery had already been discovered in the U.S.A. by Truesdell; to him Slade had 'generally confessed' in private, but Truesdell did not make the facts public until 1883.[2]

[1] This incident is described by Malcolm Kottler, 'Alfred Russel Wallace, the Origin of Man, and Spiritualism', *Isis* 65 (1974), pp. 145-92. Accounts of scientific involvement with spiritualism include Ruth Brandon, *The Spiritualists: The Passion for the Occult in the Nineteenth and Twentieth Centuries* (London, 1983), chap. 3; and Janet Oppenheim, *The Other World: Spiritualism and Psychic Research in England, 1850-1914* (Cambridge, 1985), part 3.

[2] Joseph McCabe, *Spiritualism: A Popular History from 1847* (London, 1920), p. 160.

A contemporary wit told of Lankester's involvement in very blank verse:

> And on a day it chanced a party came,
> A female half believing, half in doubt;
> And passed she in, and Simmons shut the door.
> Now Mrs. Lankester – for she it was –
> Was tolerant of what she half disdained,
> Yet fixed her eyes so clearly on the slate,
> That Slade was somewhat nonplussed for the nonce;
> But he had heard what she had said without,
> And used this knowledge cutely on the slate,
> And showed the names she knew and thought upon.
> Thus worked he on her wavering mood unril
> She half believed him true, and went
> Away less dubious than she came, and told her son.
> And he – his mother called him simply Ray;
> But dubbed Professor was he by the world –
> Rose up indignant as she told her tale,
> And burned his cheek, and flashed with ire his eye,
> And murmuring 'Oh, mamma for shame!' he went,
> Called for a friend and so the doctor sought.[3]

As a matter of fact, Mrs. Lankester's visit to Slade took place independently of, and a little later than her son's first descent on the medium, her curiosity no doubt aroused by her son's account. Lankester described his two visits to Slade in a letter to *The Times*, written immediately after his second visit on the 15th of September 1876. Commencing with a thrust at Wallace for introducing spiritualism into the British Association, he described how his friend, the barrister Cox, President of the Psychological Society, had urged him to see Slade, and how he had obtained an appointment for Monday, September 11. After watching Slade at work, he had come to the conclusion that the 'medium' was himself writing the supposed spirit messages. He left Slade with a promise to return on the Friday, then wrote at once to Cox, offering to put his suspicions to the test by seizing the slate from Slade's hand, when he professed it to be void of writing, and at the moment when the 'spirit-writing' was about to commence.

[3] From the ephemeral journal *Truth*, 1 March 1877.

It was now that Ray Lankester consulted the 'friend' mentioned in the verses. This was another 'doctor', a genuine physician, his friend and Continental travelling companion, Horatio Bryan Donkin. Donkin was two years older than Ray, and a graduate of Queen's College Oxford. He was at this time Assistant Physician at Westminster Hospital, but later set up in private practice. He became a physician of eminence, attaining a knighthood, but will probably be remembered as Karl Marx's last physician, introduced to Marx by Ray Lankester in 1880.

In his *Times* letter of 16th of September, Lankester explains how he unmasked Slade.[4] At the crucial moment he seized the slate, and showed the message already written. After a lively papet war, Lankester decided to summons Slade and Simmons. The hearing opened on Octobet 2nd in the little court in Bow Street, which was fantastically crowded owing to the intense public interest. Wallace (who makes no mention of Slade in his memoirs) would not accept Lankester's explanation, and spoke in Slade's favour. The illusionist Maskelyne showed how the trick could have been performed. The proceedings dragged on till the end of the month. The magistrate decided he could not call on the defendants to answer the more serious charge of conspiracy, but found Slade guilty under the Vagrancy Act, and sentenced him to three months with hard labour. Slade gave notice of appeal, the hearing of which did not take place until March 1877. Slade's counsel, Ballantine, got his client off on a technicality. Lankester and Donkin immediately took out a fresh summons, but Slade had had enough, and departed quietly for his native land. Lankester had now become the best known professor in the land. Cartoons appeared in the illustrated journals, depicting the unmasking of Slade by the burly young man with the chubby face framed in fashionable sideboards. The spititualist journal *Medium and Daybreak,* issued a reverential Dr. Slade number, and a good deal of very bad verse celebrated Lankester's victory and his subsequent discomforture at the hands of Ballantine.

For Lankester, the history of the occult was enveloped in an atmosphere of self deception. In his criticism of Conan Doyle's *History of Spiritualism* he says:

> I am definitely convinced that rhe existence of spirits and miracles attributed to them is in a very high degree improbable, whilst human liability to deception is so common as to be well-nigh universal.

[4] Lankester, letter to *The Times,* 16 September 1876.

His final conclusions are as valid today as when he wrote them in 1926:

> There is urgent need for the improvement and mental training of all men and women in order to enable them to weigh evidence and form rational opinions upon matters of high importance. In the absence of such training, large sections of our fellow citizens are unresisting victims of fantastic illusions.[5]

In 1922 Lankester wrote an article for the *Rationalist Annual* discussing whether there had been a revival of superstition.[6] He was optimistic; the mental attitude of the poorer class had much improved. They were now less supetstitious than the idle rich. Rationalists could now work freely to destroy superstition, without incurring legal penalties. Education remained the best cure for superstition. Few things angered Lankester more than to see rationalists turning from the true path; in this regard, even close friends like H. G. Wells came in for castigation. It has been said that Lankester was always ready to pursue any sheep straying from the rationalist fold – to turn it into mutton!

It may be noted that when Lankester and Donkin became involved in a telepathy controversy with A. J. Balfour in 1925, the tone of the press was decidedly hostile. According to the *Westminster Gazette* in a leader headed 'Science at a full stop', the old note of cerritude in science had given way to the recognition of a sense of mystery which could not be explained away by known material processes. Lankester and Donkin represented the survival of the Victorian dogmatic school in science, which used to pair off with the dogmatic school in theology. Lankester's strict integriry was now considered old-fashioned, and superstition was now a reserve force to be called on to support a threatened social order.[7]

Something has already been said concetning Lankester's philosophical outlook. Formally he remained a Huxleyan agnostic all his life, fully sharing his teacher's views on the necessity of destroying clerical pretensions. On this matter, Huxley had written to him in the strongest terms.[8] However, it is typical of the emphasis that Lankester gave to his views that the faithful found his indifference more galling than Huxley's combative agnosricism. Moreover, Lankester had an annoying habit of adopting rhe

[5] Lankester, review of A. Conan Doyle, *The History of Spiritualism* (London, 1926, 2 vols), *Sunday Times*, 15 August 1926.

[6] Lankester, 'Is there a Revival of Supersition?' *Rationalist Annual* (1917): 3-10.

[7] *Westminster Gazette*, 1 May 1925. See also letters to *The Times* by Lankester and Donkin, 24 April and 30 April 1925, and by Balfour, 30 April 1925.

[8] Huxley ro Lankester 23 May 1889 (Lankester family papers).

language of religion. Quoting Bishop Creighton's dictum that 'Religion means the knowledge of our destiny and the means of fulfilling it', he adds blandly that science can say no more and no less. 'Science like othet faiths can be preached as a gospel. It is the salvation of man we announce.'[9] He never denied that a supreme intelligence might exist, but unless one made a number of other assumptions about this entity, it was of no practical consequence. In his theoretical and philosophical statements, Lankester was careful to define the 'limits' of science, and to deny it, in Kantian fashion, any knowledge of ultimates. But like all agnostics, he did not behave as if he really believed this to be true – substantially he was a good philosophical materialist.

Nevertheless, his view of Christianity was different from that of Bradlaugh and other atheists and rationalists of the time. Fraud had played its part in the propagation of religion, but 'It is improbable thar the belief in an unseen world of hostile agencies was originated by designing adventurers'.[10] His good historical insight prevented him from seeing nothing but error in Christianity. He held that Christianity was a compound of two things; a system of morality approved by the united conscience of civilised man, and a fantastic mythology. The latter was based on Jewish traditions curiously interwoven with non-Jewish polytheism, and with a poetic development of the widely-spread fables of a divine incarnation and immolation of the god-holding victim. Christian morality had given a value and currency to the Christian religion which would persist when the mythological vehicle had lost all value. Lankester became a member, and was later made an honorary Associate, of the Rationalist Press Association. In 1906, he signed a petition, along with Bury, Jane Harrison, and others, urging that secular instruction alone should be given in the state schools.[11]

* * *

In 1879 Lankesrer heard that Sir Charles Wyville Thomson, head of the famous *Challenger* expedition, was to resign his Regius Chair of Natural History at Edinburgh University. The news excited him because he believed that Edinburgh would offer an advantage over his post at University College. Apart from his stipend, the Edinburgh Professor was

[9] Footnote in the notes for Lankester's Romanes Lecture of 1905, 'Narure and Man' (Lankester family papers).

[10] Lankester 'Is there a Revival of Supersirion?' (nore 6), p.4.

[11] See F. G. Gould, *Pioneers of Johnson's Court: A History of the Rationalist Press Association* (revised ed., London, 1935), p. 62.

permitted by the unreformed statutes of the University to gather the fees of some five hundred students, mainly medical, who attended his courses. This privilege was reputed to be worth well over two thousand pounds a year. Lankester had often said that all he wanted was an assured income with any amount of hard work. Here was his opportunity to attain financial security. Mrs. Lankester discerned a brilliant opportunity for her son, and wrote (without his knowledge) to her old friend Sir Richard Owen a letter marked 'Private and Confidential'. 'I greatly fear', she wrote, 'that Conservative influence is being used quietly with the government to make the appointment.'[12] Owen's reply was friendly, though he doubted his ability to help.[13]

Early in November, Wyville Thomson himself told Lankester he would resign. Huxley warned that Lankester might not find things at Edinburgh all to his liking, but Lankester determined to press on with his application. The general opinion was that there were only two possible candidates – Balfour and Lankester. Balfour announced that he would not stand, and out of respect for Lankester, other candidates stood down, except Cosser Ewart of Aberdeen and Patrick Geddes. Paddy Kitchen's biography of Geddes alleges that an anonymous article in the *Academy* attacking Geddes' ideas was by Lankester, supposedly part of a campaign to discredit Geddes' candidature.[14] Lankester's application was in by November 1881, supported by an astonishing collection of testimonials and a list of precisely one hundred published papers.[15]

However, Her Majesty's government remained strangely reluctant to appoint. Mrs. Lankester's intuition had been correct; Rosebery, the Under-Secretary for the Home Department (who was Rector of Edinburgh University in 1880) was known to be hostile. Lankester's friends became anxious, and Lankester himself wrote to Hooker:

> I believe that reports are current in Edinburgh unfavourable to my personal character, and if you could say anything which would cut away the ground for such idle chatter, I should be deeply indebted to you.[16]

[12] Mrs. Lankester to Owen, June 1879 (Owen papers, Natural History Museum).

[13] Owen to Mrs. Lankester, 17 June 1879 (Lankester family papers).

[14] See Paddy Kitchen, *A Most Unsettling Person: An Introduction to the Ideas and Life of Patrick Geddes* (London, 1975), p. 74.

[15] Pamphlet entitled 'The Edinburgh Chair of Natural History: Testimonials in Favour of E. Ray Lankester, M.A., F.R.S.' (Lankester family papers).

[16] Lankester to Hooker, 7 November 1880 (Hooker papers, Kew).

What form this chatter took is illustrated by an incidental remark in a letter to his mother: 'Rutherford writes that I am reported to be an Atheist, and must contradict the report.'[17] Lankester was oppressed as much by fear of success, as by fear of failure. He liked his life in London, and was perturbed by the thoughr of leaving his friends, his specimens, all his arrangements, then getting into order after long struggles. Still, he felt that he was now committed. His friends increased their pressure, notably Wilson, his chief supporter among the Edinburgh Professors. Rutherford urged Lankester to visit Edinburgh, and he did pay a short visit at Christmas 1881, but seemed to have found little opportunity of inspecting the University. In Februaty, Lankester wrote to Thiselton-Dyer that an official had confirmed to him Rosebery's hostility.[18] So much opinion was now on the side of Lankesrer that Rosebery finally gave way. The Home Secretary, Vernon Harcourt, offered him the post, and Lankester accepted. At the beginning of April 1882, he went once more to Edinburgh, accompanied by Donkin. It is clear that he was by no means easy in his mind, and would grasp at any excuse to sray in Gower Street. On April 3 he wrote home:

> As far as I can make out, there is no lecture room or museum of any kind for me: they want me to make bricks without straw, and will try to make me lecrure in the winter as well as the summer ... I dislike the whole business intensely.

After a few days Lankester left the unfriendly capital and spent some time in the Lake District. Edinburgh weighed on his mind like a lump of lead. He received the Royal Warrant of appointment, containing a proviso that resolved all his doubrs. He was determined to resign.

He wrote to Harcourt and the newspapers, and with less formality, to Huxley. He rehearsed the complaints already quoted, and added others – the zoological collections were completely detached from the care of the Professor, and the Warrant Appointment announced an impending Executive Commission with power to alter the conditions of tenure of the Chair.[19] Lankester went to see Huxley personally on May 27. Huxley recieved him mildly enough, and only asked him why he had not gone ro Rosebery, or to himself. Lankester could only reply 'Quem deus vult perdere, etc'. He had indeed, in his method of renouncing his newly-

[17] Lankester to his mother, 26 July 1881 (Lankester family papers).

[18] Lankester to Thiselton Dyer, 21 February 1882 (Thiselton Dyer papers, Kew).

[19] Lankester to Huxley, 17 April 1882 (Huxley papers, Imperial College).

gained post, committed a grave tactical error. His friends felt that he had let them down. Dyer wrote apologetically to Huxley, Hooker was angry, and Huxley wrote to *The Scotsman* disclaiming responsibility and confirming that Lankester had been informed of the potential difficulties before accepting the appointment.[20] Lankester too wrote to this journal, and in almost identical terms to *Nature*.[21] He repeated the main points he had made in his explanations to Huxley and Harcourt, and added:

> I am aware that my action must appear abrupt, and wanting in consideration for others. But on the other hand, the reflection that my resignation must cause considerable disappointment and annoyance to those whom I had most reason to spare such feeling, led me to hesitate in taking action until the necessity of making arrangements both in Edinburgh and London, was so pressing, as to make the immediate statement of my intentions to all persons concerned, imperative.

An attempt at further explanations met with a blunt refusal from Harcourt.[22] Rosebery appointed Cossor Ewart, who, undeterred by inhibitions, paid a brief visit to Scotia's darling seat and netted £2,400 in fees.[23] On May 11, a Senate meeting of University College considered a letter from Lankester applying for his old post. In the interim, only one candidate had applied, and he had withdrawn. On the motion of Croom Robertson, Lankester was triumphantly re-elected to his old Chair. One can imagine the satisfaction with which he gazed upon the familiar environment of University College and the sights and sounds of the London he loved so well. In July, he wrote a full account of the whole affair, and thought of publishing it, but gave up the idea as unlikely to produce a useful result.[24]

The affair ruined his credit in ruling circles, at any rate for the time being. James Geikie wrote to Patrick Geddes complaining about Lankester's behaviour.[25] Three years later, Lankester's friend Michael Foster could write:

[20] Huxley, letter in *The Scotsman*, 22 April 1882. The manuscript of the letter is preserved in the archives of the Natural History Museum, London.

[21] Lankester, letter to *The Scotsman*, 28 April 1882.

[22] Vernon Harcourt to Lankester, 28 April 1882 (Lankester family papers).

[23] Lankester, undated letter to his mother (Lankester family papers).

[24] There is a draft of this account in the Lankester family papers.

[25] Cited in Paddy Kitchen, *A Most Unsettling Person* (note 14), p. 75-6.

> In the first place, apart from the rights and wrongs of the case, H. M. Government are angry with you about the Edinburgh business, and that, as I told you some time ago, seems to close the chapter.[26]

Huxley, on a visit to Italy, told Lankester's friend Giglioli, that only Lankester was fit to succeed him at S. Kensington, and added, 'When Lankester resigned his Edinburgh Chair, he simply cut his own thtoat.'[27] Lankester can perhaps be blamed for not heeding Huxley's warnings concerning the Edinburgh Chair, but it is entirely to his credit, and a measure of his integrity, that he retreated immediately from a position that would have given him no opportunity for the research to which he had dedicated his life.

* * *

For some time after the Edinburgh fiasco, the mere sound of a Scots accent was sufficient to throw Lankester into a profound despondency. It is thus not surprising that he welcomed the prospect of his favourite kind of holiday, combining fresh air, exercise and research. He had heard from his friend, Canon A. M. Norman of Durham (another of the great band of clerical naturalists), that there was a Norwegian farmhouse at Lervik on the isle of Sterdo, near the mouth of the Hardanger fiord, where one could stay and where a boat could be hired for dredging in the deep waters of the fiord. In 1868 an experienced sea dredger had obtained near Lervik a curious colonial animal attached to the stems of the white coral dredged up from 150 fathoms. Norman had originally found this animal off the Shetland Isles, and his preserved specimens had been examined by Allman, who had assigned them to the *Polyzoa*, which he was a recognised authority. He had named it *Rhabdopleura* (rod-walled), because of the rod-like cord which runs along the inside of the delicate branching tube, only one-twentieth of an inch wide, that the little animal constructs and inhabits.[28] A more complete description was given by the Scandinavian biologist G. O. Sars, from specimens found off the Lofoten Islands.[29]

[26] Foster to Lankester, 5 July 1885 (Lankester family papers).

[27] This information was transmitted to Lankester in a letter from Gigioli, 1 May 1885 (Lankester family papers).

[28] G. J. Allman, 'On Rhabdopleura, a New Form of Polyzoa, from Deep-Sea Dredging in Shetland'. *Quarterly Journal of Microscopical Science*, 9 (1869): 57-63, see p. 57.

[29] G. O. Sarrs, 'On Rhabdopleura mirabilis', *Quarterly Journal of Microscopical Science*, 14 (1874): 23- 44.

Rhabdopleura was quite unlike other polyps, and obviously needed much further study. Lankester sensed its importance and wished to examine live specimens. In a brief criticism of Sars, he had come to the conclusion that there was:

> ... no important feature in which Rhabdopleura approximated to the Hydrozoa, and that it was separated by a hugh gulf from the Diploblastica, from the animals devoid of true body cavity, and possessing extensive gastric ramifications.[30]

Continued study has shown that Lankester's perception was justified. *Rhabdoplleura*, and the related *Cephalodiscus*, have been removed from the Polyzoa, and are now classed as Hemichordata, remotely connected with the Chordata; recent study even suggests a possible connection with the extinct and enigmatic graptolites. Thus a special interest attaches to Lankester's journey to Norway in 1882.

He sent on ahead his equipment, which included a windlass with six hundred fathoms of rope. On July 14th, he set out to meet his assistant A. G. Bourne, who was already on the Continent. Lankester has described his venture in a charming essay in his book *Diversions of a Naturalist*, but the following account is drawn mainly from his letters home, more spontaneous if less literary, and conveying better the excitement of discovery. His first letter was written from Copenhagen, dated July 18th. The distances to be travelled he found alarming, the Danes 'not interesting, being very like English'. When he reached the Hardanger fiord, he was in a more cheerful mood. Norwegian scenery impressed him, and he rhapsodises about the mountains, the air, and the clear rushing waters.

> Such water I have never seen. Huge rivers boil down over precipices on every side as you drive along, and the water is not milky as in Switzerland, but since it does not come from glaciers, is absolutely clear and pure. I have never seen anything more splendid than the deep green pools and rushing oceans of this pure untainted water.

Lankester and Bourne reached Lervik on the first of August. While on the way, they had learned the tragic news of the death of Francis Balfour, the result of a climbing accident in the Alps. Balfour had belonged to the

[30] Lankester, 'Remarks on the Affinities of Rhabdopleura', *Quarterly Journal of Microscopical Science*, 14 (1874): 77-81.

inner circle of Ray Lankester's closest friends; he was deeply distressed. Balfour had charmed and impressed all who knew him. In a letter written on the day he arrived at Lervik, Lankester mourned his friend.

> No one can replace him. He was most valuable for science in every way. Not only was his work so good, but personally he had so much influence owing to his earnest and courteous character, and he always wished for the right things, and would have been more and more able to gain the day for such things. At Cambridge they can never replace him. All is over there with the study of animal morphology. What a fiendish and cursed institution is this Alpine Club ... How often have I urged Frank Balfour not to climb? Last year I wrote to him seriously to beg him not to, and this year I very gravely begged him to give it up as too dangerous. I cannot think properly on the subject, for when I remember his earnest eager way, and his benevolent beautiful eyes, as I have so often seen them as we discussed late into the night in my study or at Cambridge, and then remember that he has been cruelly smashed, and that I shall never see him again – I can only cry.[31]

They were welcomed at the farmhouse by its owner, sister to the M.P. for the surrounding region, which was twice the size of Yorkshire. The prospect of dredging in deep water filled Lankester with a pleasurable excitement, but the weather at first was bad and they could not venture out in their fishing boat with its crew of three stout young Norwegians. Lankester and Bourne went to Bergen, and returned with the naturalist Friele, who gave them advice on dredging methods. The dredging was carried out some ten miles along the fiord, precisely at the spot indicated on Norman's map. Norman himself soon joined the party, and confirmed his skills as a dredger. On August 9th, Lankester wrote home that the dredging started at seven in the morning and went on till three, and that he was feeling very nautical and sea-hardened.

> We let down the dredge ... To each side great tassels of hemp are tied to entangle the corals. Down it went to a hundred and fifty fathoms, that is about fourteen hundred feet, as high as from the top of the Malvern Hills, into the valley of the Severn. I felt when it reached the bottom, and it began to catch in the masses of coral and polyps. It struck two or three times very tightly, so after

[31] Letter of 1 August 1882 (Lankester family papers).

> twenty minutes we hauled in. We have a very large wheel for winding up, and it is very hard work for the three men; they take about forty minutes to get it up. At last it came up, and I was immensely pleased to see masses of a beautiful coral, quite white, and as big as my head (and bigger), sticking in the tangles.

Here Lankester added some sketches for his mother's benefit, and added,

> Lophohelia, the Norwegian white coral ... When put into water, the tentacles came out of the stony white cups like a very delicate sea-anemone. It was a most beautiful sight. Growing on the coral are all sorts of sponges and polyps, and it is here that I hope to find Rhabdopleura.

At first they found only the dead tubes of the animal. At last, on 21st August, he was able to write home that he was 'getting plenry of Rhabdopleura', and was 'making out new things about it'. The weather became very bad, and Bourne fell ill. Lankester wrote thar the little polyps had now shed their ova, and were in a sickly state. It would be nice to drop in on the little creature at the end of May, when the eggs were being laid. He was not disappointed, and would have a memoir for the Linnean Society. The other rhings too, the corals and worms and starfishes had interesred him greatly. The memoir on *Rhabdopleura* did not appear until 1884 in his own journal.[32] Lankester described the body cavity, the gonads, the methods of budding, and Allman's 'blastophor', the 'axial chitinous rod' which had given the animal its name.

Lankester brought home large numbers of specimens, but of particular interest was the discovery of numerous specimens of the worm *Hamingia,* of which only two damaged specimens were known. Lankester's original note of this discovery has been preserved. While examining a similar worm, the Mediterranean *Boniellia,* Lankester's friend Kovalevsky had found minute males crawling on the body of the much larger female. Lankester found on *Hamingia arctica,* the same kind of diminutive male, one-twelfth of an inch long, within the dilated pharynx of the female. He was able to show that the Norwegian worm possessed red blood cells carrying haemoglobin.[33]

[32] Lankester, 'A Contribution to the Knowledge of Rhabdopleura', *Quarterly Journal of Microscopical Science,* 24 (1884): 622-47.

[33] Lankester, 'On Specimens of the Gephyrean, Hamingia arctica, from the Hardanger Fiord', *Annals and Magazine of Natural History,*11 (1883): 37-43.

- 9 -
The Marine Biological Association

Ever since his Neapolitan days it had been an ambition of Ray Lankester to see a flourishing marine zoological laboratory in England, similar to that presided over by his friend Dohrn. Unfortunately, the government of the wealthiest country in the world remained as yet indifferent to the advantages of subsidised biological research, and private benefaction for this purpose had not yet been sounded. There were many amateur naturalists with expert knowledge of their local sea-shore, and in the early part of the century important work was done by naturalists using dredges to bring up material from the sea-bed.[1] Two small marine stations existed in Scotland, one established by Professor M'Intosh at St. Andrews, the other at Granton on the Moray Firth. This latter was transferred to Oban in 1882, but later moved back to the east coast. There was some small financial support for this station from the British Association, Ray Lankester serving on the committee for this purpose.[2] In 1882, the modest grant of £40 was not applied for in time, and work at the station stopped, except for some researches carried on by Schafer. Occasionally the British Association made grants for marine work in other parts of the country, and an occasional financial windfall led to useful work. Thus, after a fisheries exhibition at Edinburgh in 1882, a surplus of about £3000 was devoted to a study of the sea bottom of some Scottish fishing grounds.[3]

If there was little official interest in marine stations, the push and pull of economic interest was strong in the fishing industry. From about 1860, a stream of complaints and demands for legislative interference poured in via the parliamentary representatives of the fishing ports. In 1862, Huxley served on a Commission for two years, and assembled a mass of evidence

[1] See Philip F. Rehbock, 'The Early Dredgers: 'Naturalizing' in British Seas, 1830-1850', *Journal of the History of Biology*, 12 (1979): 293-368. On later developments see Eric L. Mills, *Biological Oceanography: An Early History* (Ithaca, N.Y., 1989).

[2] 'Report of the Committee appointed for the Maintenance of the Scottish Zoological Station', *Report of the British Association for the Advancement of Science*, (1881), pp. 177-8.

[3] Lankester, letter to *The Times*, 3 November 1883.

exceeding in bulk, as he tells us, a large family bible.[4] The Report was unfavourable to the demands for sectional interference, and favoured the abolition of old legislative restrictions on fishing. A few years later, a second Commission came to similar conclusions, and there was a Commission on trawling in 1883, on which an irritated Huxley was laughed to scorn for daring to suggest to a room full of cod-fisherfolk, that cods' eggs floated on the surface, instead of lying on the bottom, as the fisherfolk asserted. Hence their belief that nets dragged along the sea bottom destroyed the eggs of food fishes. Huxley was too ill to take part in drawing up this last report, which did raise the question of the possible exhaustion of trawling grounds.[5]

The International Fisheries Exhibition at South Kensington in 1883 attracted great popular attention. Thousands of Londoners wandered amidst the well laid-out gardens, listened to the military bands, and gazed with varying degrees of comprehension at Dr Dohrn's four hundred bottles containing the fauna of the Bay of Naples, at Dr Day's collection of Indian fishes, at the big collection of American molluscs and crustacea, and at the varied fishing gear sent from Hull and Grimsby. Prizes were offered for essays on fishery topics, conferences were held, and lectures delivered. In general, it was noted that Britain's exhibits made a poor show when compared with those of the U.S.A. and the Continental countries, being hardly at all concerned with zoological science.

In June 1883 Ray Lankester, soon to deliver a slashing attack on government supineness in his British Association address, 'Biology and the State', gave a brilliant lecture on the scientific results of the Exhibition, pointing out that no zoologists were employed by government to investigate fishery problems.[6] He stated his complaints even more pointedly in a lecture to the Society of Arts:

> It is a striking fact ... that whilst the agriculturist has very largely availed himself of scientific knowledge, the fisheries of our coasts, which provide an almost equal amount of food, have never been carried on with any regard to an accurate knowledge of the fishes on which they depend ... In fact, our fishery industries are still barbaric; we recklessly seize the produce of the seas, regardless of the consequences of the method, the time, or the extent of our

[4] Huxley, letter on 'Sea Fisheries' in *The Times*, 8 January 1889.

[5] *Ibid.*

[6] Lankester, 'The Scientific Results of the International Fisheries Exhibition, London, 1883', in Lankester, *The Advancement of Science* (London, 1890), pp. 193-222.

> depredations. In the same ignorant fashion as the nomadic herdsmen of Asia, who descend on a fertile valley, and after exhausting it, leave it to time and natural causes for its recuperation, so do we treat the fishing banks of our coasts.[7]

More effective fishing methods were leading to exhaustion he claimed:

> We do not know why soles are getting scarcer year by year; nothing is known about the spawning of the sole, the male fish is not even recognised. We do not understand oysrers in the precise and detailed way we understand pigs and chickens. We do not understand why some fishes swim in shoals in certain seasons in certain spots, then to the dismay of the fishermen give up ever passing that way. We do not know whether we could catch the young turbot, soles and cod, and stock the sea with them, as we do our rivers with trout and salmon. We do not know whether we could favour the increase of such fishes by cultivaring in the sea their favourite food. In many cases, we do not know what their food is ... In fact, we know exceedingly little about the minute details of the life of marine animals, and if we wish to deal with sea fishes like rational men, we must find out these minute details, and gradually apply our knowledge so gained. The whole field of knowledge must be cultivated in the simple faith that the increase of knowledge is the greatest good which human effort can achieve.

Lankester's Exhibition Lecture was the opening shot in the campaign for a matine laboratory, for he judged that the time was now ripe. At the Southport meeting of the British Association, his plea for governmental support for biological research attracted much attention. He commented to Huxley:

> We had a fairly good meeting at Southport, and as I dare say you saw my address excited a good deal of criticism ... but several papers were decidedly favourable to increased provision for scientific investigation. I hope you do not think that the dust I have been kicking up is inopportune.[8]

[7] Lankester, 'Value of a Marine Laboratory to the Development and Regulation of our Sea Fisheries', *Nature*, 32 (1885): 65-7.

[8] Lankester to Huxley, 12 October 1883 (Huxley papers, Imperial College).

The General Committee of the British Association tried to obtain surplus funds remaining from the Fisheries Exhibition, for the purpose of founding a marine laboratory.[9] Lankester's propaganda was beginning to bear fruit, and he threw himself with zest into the struggle, however much he might complain that he was being overworked. It has been said that it was due to Ray Lankester's initiative that the Marine Biological Association was planned upon a scale that made it of national importance, and that it was through his influence, persuasiveness, and perseverance, that the necessary funds were raised. As an obituary noted:

> In retrospect, it is sufficiently remarkable that so young a man – he was only thirty-six years old – should have commanded so much influence, not only among his scientific colleagues, but among persons of all ranks and callings, as to be able to bring a scheme conceived on such generous lines, to a successful issue.[10]

At the close of the Exhibition (the end of October 1883), Lankester obtained the signature of a number of leading naturalists and others to a memorial to the Executive Council of the Exhibition, echoing the British Association's demand that a portion of the proceeds might be devoted to the foundation of a laboratory.[11] This memorial, signed by Carpenter, Flower, Sclater, Moseley, Allman and eleven others, was published in *The Times* of October 30th. Carefully worded, it pointed to the flourishing Naples station, and repeated the British Association request for an interview with the Exhibition Committee. *The Times*, later highly praised by Lankester for its support, devoted a sympathetic leader to the matter, and in the same issue printed a long and closely reasoned letter from Romanes. Shortly afterwards, Lankester weighed in with a criticism of the Exhibition, which, he asserted, had not really advanced our knowledge of the subject.[12] Since the Exhibition had not been organised as a private speculation, but had royal patronage and semi-governmental sanction, it should devote its surplus to the worthy cause of a marine laboratory.

[9] See the resolution referred to the council, *Report of the British Association for the Advancement of Science*, 1883, p. lxvii.

[10] Obituary of Lankester, *Journal of the Marine Biological Association*, n.s. 16 (1929-30): 365-71, p. 365. See also 'The History of the Foundation of the Marine Biological Association of the United Kingdom', *ibid.*, 1 (1887): 17-44.

[11] Appendix to Lankester, 'The Scientific Results of the International Fisheries Exhibition', (note 6), pp. 219-22.

[12] Lankester to *The Times* (note 3).

Lankester feared that Huxley might disapprove of his activities, and wrote to reassure him.[13] Huxley was not unfavourable, but the appeal to the Exhibition Council was unavailing, and the surplus was applied to other purposes. What was to be done? To Dyer, Lankester put forward a daring scheme to raise money under Linnean Society auspices to found not one, but a whole series of biological laboratories (see chapter 14 below). It was Lankester's friend Albert Gunther, the British Museum ichthyologist, who suggested to him the formation of a marine biological association.[14] No time was wasted; the Royal Society was prevailed upon to loan its rooms, and an inaugural meeting was arranged for March 31st. Ray Lankester worked frantically to ensure that everybody who counted, both scientists and men of rank and influence, should be present. To Sir Joseph Hooker he wrote:

> It is hoped that you will speak to the first resolution on Monday, (the meeting is at 4.30), which will be moved by the Duke of Argyll. It is a general resolution to the effect that it is desirable to found a laboratory on the British coast for the study of marine life, with a view to the improvement of zoology and botany, and of our knowledge of food fishes and molluscs ... Besides the Duke, there are coming Lord Dalhousie, Lord Arthur Russell, the Lord Mayor, Sir Lyon Playfair, and many others. As a leader of Biological Science, you will be able to give us a great help by coming to meet those non-scientific men who are good enough to take an interest in the scheme. Professor Huxley is taking great interest in the matter, and will preside. Sir John Lubbock, Professor Flower, Dr Carpenter, and others promise to speak.[15]

Representatives of the Fishmongers' Company also attended. The ground had been well prepared, and all propositions were adopted unanimously. A provisional committee was elected. Frank Crisp, Treasurer of the Linnean Society, became Hon. Treasurer of the Association, while on Lankester fell the onerous duties of Hon. Secretary. Huxley became President, on the understanding, as he jocularly told Lankester, that he would do as little as possible to deserve the honour. The Prince of

[13] Lankester to Huxley (note 8).

[14] See the letter from Lankester to Albert Gunther of 14 March 1884, quoted in A. E. Gunther, *A Century of Zoology at the British Museum through the Lives of Two Keepers* (London, 1975), p. 396.

[15] Lankester to Hooker, 28 March 1884 (Hooker papers, Kew).

Wales became a patron. Annual membership was set at one guinea, life membership at £100. Donations were invited, and a committee of May 2 set an aim of a basic capital of £10,000 and an annual income of £1500. In a letter to the Times Lankester surveyed progress and appealed for further help. The provision of one good laboratory on the seashore, he stated, would enable a large number of volunteer investigators to do a vast amount of good work without further expense; but some hundreds of well-equipped institutions were wanted in this country, before Britain could stand on the same level as Germany.[16]

Lankester was convinced that governmental aid would be granted, if they could make a good start on their own and thus satisfy *laissez-faire* scruples. He sent out many urging-cum-begging letters, similar to one he sent to Hooker. About £1000 had come in. It was necessary that eminent men should make personal application to those who had money to spare. Sir Joseph had raised several thousand pounds for the Royal Society by writing to his friends. Would he do the same for the M.B.A.? If only they could make a good start on their own, they would be able to get the 'Fisheries' £3000, and an annual £200 from the government.[17] Lankester asked the Royal Society for £500, but was glad to receive £250. At the first annual meeting of the Association in June 1885, the Secretary was able to announce over three hundred members and a total of £8,000 in subscriptions. By the time building started, nearly £17,000 had been collected. It is noteworthy that a number of biologists, none of them wealthy men, paid their £100 for life membership – Moseley, Poulton, Weldon, and Lankester himself. The London Companies donated fairly generously, the Fishmongers alone giving £2,000, and most scientific societies donated as generously as their means permitted. Finally, in view of the great public interest, the government relented, as Lankester had hoped. Parliament voted, on Treasury recommendation, an initial sum of £5,000, and £500 annually for five years, on the understanding that the M.B.A. would pay due attention to matters of economic importance.

Weymouth and Torquay had been considered as possible sites for the new marine station. However, the Inspector-General of Fortifications offered a suitable site at Plymouth. Sound reasons for locating the laboratory at Plymouth were also offered by the brothers Bayley, two philanthropic residents of Plymouth who were also generous patrons of the M.B.A., and by C. Spence-Bate F.R.S., another resident. Lankester wrote to Anton Dohrn for advice on the construction of the laboratory,

[16] Lankester, letter to *The Times*, 10 May 1884.

[17] Lankester to Hooker, 15 May 1884 (Hooker papers, Kew).

particularly on how to maintain an adequate supply of sea water at a site that was situated 110 feet above high water.[18] He reassured Dohrn that support would continue for British research at Naples. Although overworked by his many projects, he found time to write a review of M.B.A. progress in *The Times*.[19] To Dyer he wrote:

> I expect to receive very soon the official announcement of the grant of £5,000 and £500 a year to the M.B.A. We must then pass the building plans, hold a general meeting, and set the workmen hard at it. Also I think it would be a good thing to appoint Heape Resident Superintendant at a salary of £200 a year, at once, i.e. in January. He can then visit Naples to learn some tips as to management, and on his return take up residence at Plymouth, and keep an eye on the works, whilst also commencing the very important and necessary task of dredging and getting to know about the water in the Sound, (best time for pumping into aquarium reservoir), the fishes taken, the fishermen and such things. He can in fact, begin that exploration of the Sound, which we shall have to carry on in detail afterwards.[20]

Under the direction of J. C. Inglis, a handsome building arose on Citadel Hill overlooking Plymouth Sound. It was built of the Devonian coral limestone of which Plymouth Hoe is formed. It consisted of a main block, connecting with a north block containing the laboratories.[21] The total building cost was £12,500 – very moderate, even by Victorian standards. On June 30th, 1888, the Plymouth Marine Laboratory was officially opened in the presence of a large and distinguished company, presided over by Sir James Lawrence, Warden of the Fishmongers' Company. The toast of the M.B.A. was proposed by Lord Morley, who coupled with the Association the name of Professor Ray Lankester. In his reply, Lankester outlined briefly the history of the Association, and paid tribute to all who had assisted in the maturation of the plans for the laboratory. Characteristically, he closed his speech by pointing out that the Station needed a sea-going steamer, and hoped that those who could,

[18] 28 March and 7 August 1884 (Stazione Zoologica Archives, Naples).

[19] Lankester, letter to *The Times*, 18 January 1887.

[20] Lankester to Thiselton-Dyer, 14 November 1885 (Thiselton-Dyer papers, Kew).

[21] See W. Heape, 'Description of the Laboratory of the Marine Biological Association at Plymouth', *Journal of the Marine Biological Association*, 1 (1887): 96-104; also F.S. Russell, 'The Plymouth Laboratory of the M.B.A.', *Proceedings of the Royal Society* (B) 135 (1947-48): 12-25, p. 21.

would assist.[22] The proceedings terminated with a cruise round the harbour in the Port Admiral's yacht. In July 1888, a fund was officially opened for the purchase of a steamboat. The next year a small steam launch was purchased, but was not very seaworthy. Lankester once again assumed his role of money-raiser, and wrote to the Royal Society:

> The Royal Society has from the first treated the M.B.A. as a favoured infant, and now we are buying an excellent steam yacht for trawling ... at a price of £600.

He then asked for £100, after detailing private donations.[23]

Lankester became President of the M.B.A. in 1890, holding the position for the rest of his life. The first President, Huxley, resigned because a request for further Treasury financial aid had not first been sent to him for approval. In a placatory letter to Huxley, Lankester described the serious financial plight of the M.B.A. in these early days.[24] They would have to cut down on staff, cease buying books for the library, and would be unable to print reports or accounts of work done. The arrangements and staffing at the laboratory were at first modest enough. The only salaried officers were W. L. Calderwood, (soon succeeded by G. C. Bourne), and a naturalist, J. T. Cunningham, who had come from the Scottish station. Cunningham, and voluntary helpers such as Weldon and G. H. Fowler, had actually begun researches well before the completion of the building, as Lankester himself had done in Italy.

The successive Council and Directors' Reports, published in the early issues of the Journal of the Marine Biological Association show that considering the slender resources of the Station, its activities were astonishingly wide. Lankester was a frequent visitor, entering with great minuteness into all the details of organisation. He had a high standard of efficiency, and was inclined to underrate the difficulties of the struggling and underpaid Directors of the pioneer days. Lankester subsequenrly gave a sum of money to the M.B.A. which was used to fund an 'Investigator' for many years. He also donated a substantial collecrion of offprints to the library.

[22] Lankester's speech as the opening of the laboratory, *Daily Chronicle*, 2 July 1888.

[23] Lankester to the Secretary of the Royal Society, 10 February 1891 (Royal Society, London).

[24] Lankester to Huxley, 1 March 1890, Huxley papers, Imperial College. Additional correspondence and documentation from Lankester's work during this period is held in the archives of the Marine Biological Assocation, Citadel Hill, Plymouth.

In January 1895 E. J. Allen became Director, the fourth in eight years, and was without doubt the second maker of the Laboratory. Under his leadership, there began a steady progress and achievement.[25] G. C. Bourne paid tribute to Lankester's 'unremitting exertions' in maintaining the work of the M.B.A. at a high level of efficiency. When in 1899-1901, the Government was asked to cooperate in an international investigation of the North Sea, the only organisation with a staff trained to undertake that particular kind of work was the Marine Biological Association, with its Plymouth Laboratory.[26]

[25] See the obituary of Allen, *Journal of the Marine Biological Association*, 25 (1941-43): 671-84.

[26] As noted in the obituary of Lankester (note 10).

- 10 -
Linacre Professor: The Uneasy Chair

As the years wore on, Ray Lankester became more and more uneasy about the situation at University College, becoming convinced that his Chair was 'rickety' and his future problematical. There was some reason for worry. At the end of January 1887, the Senate discussed a serious fall in the number of students. Certainly, as Huxley admitted, his £500 a year was not a very liberal reward.[1] For a time, Lankester considered attempting to obtain the Assistant Secretaryship of the British Association, and even toyed with the idea of leaving the country. From all this, Huxley gently dissuaded him. Describing the qualities necessary for a British Association Secretary, Huxley made a noteworthy comment about his pupil:

> Looking at things solely from the point of view of your interests, I should strongly advise you against taking it – even if it were offered. My pet aphorism, 'Suffer fools gladly', should be the guide of the Assistant Secretary ... You do not suffer fools gladly; on the contrary, you gladly make fools suffer.

In this same year, the medical students at University College (who all took a preliminary biology course) were transferred at Huxley's request to the Royal College of Science. Lankester became sick with worry; it was the only time in his entire life that he felt hostility towards his teacher. Even the award of the Gold Medal of the Royal Society for his work in morphology and embryology did little to assuage his resentment. Nevertheless, his speech on the reception of the award contained a graceful tribute to Huxley, who was President.[2]

In 1887 Lankester's friend Moseley, Linacre Professor of Comparative Anatomy at Oxford, fell seriously ill, and it became necessary to appoint a deputy. Lankester's thoughts now turned towards Oxford, which might supply the security he felt he lacked at University College. Many of his

[1] Huxley to Lankester, 22 May 1889 (Lankester family papers).

[2] Citation of the Royal Society on the award of the medal kindly supplied by the Society's Library, Mr I. Kaye (11 October 1957). The autograph of Lankester's speech has survived (Lankester family papers).

friends thought it feasible, despite his recent tussle over the 'vivas' with the Vice-Chancellor, although they considered that Acland and the religious parties would oppose him. They would use the row with the Vice-Chancellor to influence the outside electors such as the Primate, who was thought to want the Linacre post for a cousin.[3] Lankester wrote to Huxley:

> I am trying to get back to Oxford to take poor Moseley's place. There is some difficulty, as they do not at all rightly value the blessing of having someone among them to prosecute the Vice-Chancellor, and otherwise make things lively.[4]

Huxley replied that it was a good thing; Oxford science needed stirring up, and Lankester could do with a little more stirring down, so that the conditions for a transfer of energy were excellent. He concluded with an earnest appeal to Lankester to settle down, and put his fighting behind him.[5]

Lankester's first attempt to become Moseley's deputy failed, however, the electoral board choosing Halchett Jackson. Lankester complained again to Huxley:

> If Madam Science would give me a Chair to sit on less rickety than that of University College, I would be her mosr domesticated adorer. All the classes at University College have fallen twenty per cent since 1880-82; that is the result of competition and underselling. One can't devote oneself coolly to prolonged investigation, while one's very bread and butter is being snatched from one's fingers.[6]

Moseley had applied for leave of absence at the end of 1887, and Jackson was soon assisted by a second deputy, S. J. Hickson. Jackson retired from his post in 1890, and in July Lankester was elected Deputy Linacre Professor. Announcing his success, he acknowledged it to Huxley's intervention.[7] Lankester's chief rival for the post had been William Bateson, but the latter had made it plain that if Lankester were to stand,

[3] Lankester to Thiselton-Dyer, 22 October 1888 (Thiselton-Dyer Papers, Kew).

[4] Lankesrer to Huxley, 5 December 1888 (Huxley papers, Imperial College).

[5] Huxley to Lankester, 6 December 1888, quored in L. Huxley, *The Life and Letters of Thomas Henry Huxley* (London, 1903, 3 vols.), III, p. 93.

[6] Lankester ro Huxley, 7 December 1889 (Huxley papers, Imperial College).

[7] Lankester to Huxley, 2 July 1889 (Huxley papers, Imperial College).

he would withdraw.[8] When the Archbishop quibbled at the thought of the lively London Professor occupying the Linacre Chair, Huxley told him that although Ray Lankester was certainly given to playing astonishing pranks, he always selected the time for playing them, and the persons on whom they were to be played, in such a manner as to be most damaging to his professional interests. A form of ingenuity so rare ought to be encouraged! "Now", he admonished his pupil, "having nothing more to lose, you are expected to turn over a new leaf".[9]

It was a sorrowful Lankester who took his leave from University College. His farewell speech pays graceful and sincere tribute to his colleagues and students. The regard of his students had been his greatest encouragement, and his most constant source of happiness.[10] Moseley died on November 10, 1891, and Lankester became full Linacre Professor just before Christmas. When he went to Oxford in the October of the previous year, he continued for a time to lecture at University College, until his successor W. F. R. Weldon, was able to take over. At first he felt satisfied with his new post, although he felt it strange to turn up at Oxford alone, like an undergraduate in his first term.

> Everyone here was very kind and friendly, but of course I am uncomfortable, having nothing I am used to ... My chief anxiety, next to that of getting a house, is about my teaching. There are a number of students who want to be crammed for their examination in one term – this is the system invented by Sanderson, and I cannot fall in with it. It would be the height of folly to leave London, in order to do such dirty work. It may lead to trouble with Sanderson, but I hope not ... I had a good class, and lectured well, and feel better for having done so.[11]

A little later he wrote that he was contented with Oxford, although:

> It gives me a pang when I see my class at University College, a larger one than usual, all rapt attention and hanging on my words. Every Friday, they give me a little applause when I appear. At Oxford there is a dead silence, and twenty young asses look at

[8] See Beatrice Bateson, *William Bateson, Naturalist: A Memoir* (Cambridge, 1928), p. 37.

[9] Huxley to Lankester, 11 July 1889 (Lankester family papers).

[10] University College Calendar, 1890.

[11] Letter of 4 October 1890 (Lankester family papers).

> me with a critical eye, having been told by Sanderson that my reading will be quite useless to them. But I am going to beat Sanderson, you will see![12]

In another letter, he depicts himself ironically as retiring to the peace and tranquillity of this sainted city, and awaiting undismayed the scythe of time.

Indeed the Linacre Chair offered numerous advantages including a salary of £900 and six months' leisure in the year. There were also difficulties; inevitably there had been a tendency to minimise the subject during Moseley's illness. Lankester fought hard and successfully to save and extend his department, and he claimed that in the process he made no overt enemies.[13] The less strenuous life was an advantage, for he had several bouts of illness. In August 1889, while in Paris, he fell ill with 'typhilitis' (appendicitis), which left him feeling very weak, although he had strength enough to write home furious denunciations of his French physician. In 1892, he had a severe attack of influenza, and the opening of the Lent term for his students had to be deferred for a few weeks.

Lankester's first task at Oxford was the reorganisation of the Museum. He altered the method of displaying specimens so that they were now adequately labelled and arranged in systematic order. The fossil forms were included with the recent, something on which Lankester always insisted.[14] His chief acquisition for the Museum was a cast of the dinosaur *Iguanodon* from Belgium. This cost him a good deal of trouble, as the authorities at the Brussels Museum were not very cooperative and he had to hint at a diplomatic démarche.[15] He was fortunate in having excellent assistants. W. B. Benham, one of his best University College students, became Aldrichian Demonstrator in Morphology, while E.S. Goodrich served him worthily as Assistant. Later, as Linacre Professor himself, Goodrich was to become the most eminent exponent of the Lankester tradition.

[12] Letter of the period October-December 1890 (Lankester family papers).

[13] Lankester to E. B. Poulton, 23 July 1898 (draft in Lankester family papers). For a very different assessment of Oxford's response to Lankester see Janet Howarth, 'Science Education in Late-Victorian Oxford: A Curious Case of Failure?' *English Historical Review*, 102 (1987): 334-71, esp. pp. 347-8.

[14] E. S. Goodrich, Obituary of Lankester, *Proceedings of the Royal Society*, 106B (1930): x-xv.

[15] Director of the Brussels Museum of Natural History to Lankester, 16 January 1897 (Lankester family papers).

It was inevitable that Lankester should engage in the (more or less) gentle stirring of the Oxford pudding advocated by Huxley. In March 1893, he issued an 'Appeal to the Governing Bodies of the Colleges within the University of Oxford'. In this he complained that graduates in science were ignored to an incredible degree by the Colleges. The total of open fellowships in the Oxford Colleges was three hundred. Of these, only thirty were for Natural Science graduates, and these were held mainly by physicists and marhemaricians, and only one by a zoologist. He declared that until some change in the system was instituted, rhere was no hope of a due recognition of Natural Science at Oxford.[16] On one occasion, when travelling to Oxford by rail in the company of various dignitaries – all noted conservatives in marters of university reform – he remarked that there was a great opportunity for a university reformer to push a big stone on the rails! However, resulrs of the pudding-stirring were soon forthcoming. The University vored funds for new laboratories for the Linacre Professor; these were opened without ceremony in 1892. At the same time, expenditure was approved for a new laboratory of Human Anatomy, opened the next year. At Lankester's suggestion, the Linacre Professor was now relieved from specifically medical studies, while retaining overall control of the anatomical collections of the Museum.[17] So far, Oxford had done handsomely by the new Professor.

In 1878, in an address on 'Medical Research and the Endowment of Science,' Lankester had strongly attacked the separation of theory and practice in medicine. He held that the knowledge of diseased and healthy conditions formed one compact, interwoven body of knowledge, and upon this the medical art was erected. Lankester now claimed that Oxford and Cambridge had shamefully neglected the medical student.[18] His stricture was justified. There was at that rime a Professor of Medicine at Oxford, but he was by no means followed by eager troops of students. There was in fact hardly any medical teaching until 1891, when the German Karl Menge gave some lectures on pathology. In 1895, Burdon Sanderson gave a regular course on pathology, while J. Ritchie gave practical instruction. A special pathological laboratory was begun in 1899 and completed in 1901. Giving evidence before the Selborne Commission

[16] Anonymous, 'Natural Science at Oxford', a commentary on Lankester's 'An Appeal to the Governing Bodies of the Colleges within the University of Oxford', *Natural Science,* 2 (1893): 161-2.

[17] Lankester, 'Human and Comparative Anatomy at Oxford', *Nature,* 48 (1893): 616-7.

[18] Lankester, 'Human and Comparative Anatomy at Oxford', *Nature,* 49 (1893-94): 29.

in 1880, Lankester had expressed his wishes for the development of a great school of medicine at Oxford. Thirteen years later, he could say with some justification, that Oxford had been aroused to a sense of obligation to medical study by a movement in which he had taken a prominent part.[19] Insofar as his own department was concerned, he was able to persuade the University to vote £5,000 for a new lecture room in the last year of his term at Oxford.

Not everyone was impressed by Lankester's efforts, and the President of Magdalen College, Herbert Warren, responded in the *Oxford Magazine*. R. T. Gunther, who was studying under Lankester, wrote to his father Albert, Keeper of Zoology at the British Museum (Natural History) noting that Lankester was always complaining and had written to the M.P. for Oxford, the Home Secretary, Arthur Balfour and others.[20] Gunther also mentioned that the students had asked Lankester to lecture with greater regularity. In 1898, Lankester wrote a preface to the Linacre Reports that annoyed G. C. Brodrick, the Warden of Merton College. Brodrick called science the spoiled child of the University, and claimed that Lankester's demands were excessive.[21] He considered that Lankester's attacks on the College and tutorial system were unjust, and called them the 'lifeblood' of the English universities. It was unwise of him to use a biological metaphor. Lankester reported that the universities were suffering from a blood disease resembling leucocytosis. The tutorial system was not analogous to the blood-vascular system of an organism. It was always something adventitous, and of the nature of a tax levied by the resident masters of arts upon the youth who desired to be presented for a degree.

> Its organisation in Oxford during the last sixty years has given it a varnish of respectability, but it is essentially a boarding-house keeper's scheme for securing a lucrative monopoly (fees to the amount of sixty thousand pounds a year) to themselves ... With the abolition of this system, that healthy compound, freedom of the teacher and freedom of the student, must some day in Oxford as in Germany, replace the sickly fluid which the Warden of Merton, with pathetic candour, declares to be at present circulating in the vital channels of our Universities.

[19] *Ibid.*

[20] R. T. Gunther to Albert Gunther, 29 October and 16 November 1895, quoted in A. E. Gunther, *A Century of Zoology at the British Museum through the Lives of Two Keepers* (London, 1975), p. 440.

[21] Brodrick in the *Saturday Review*, 7 March 1900; Lankester's reply, *ibid.*, 31 March 1900; Brodrick's response, *ibid.*, 14 April 1900.

It was in 1898, when his propaganda for science was meeting with some success, that Lankester suddenly retired from the Oxford scene. How is it possible to account for this? It is plain that the reason is to be sought in his bitter loathing of Oxford society. He was appalled at the pomposity and priggishness he found around him, and, as in his earlier sojourn at Exeter College, he abominated Oxford dinner parties, describing them in the most vitriolic phrases he ever used.

His domestic affairs also offered him little satisfaction. In 1894, it seemed that his heartfelt desire to settle down in a happy marriage, might at last be realised. He became engaged to Mary Eleanor Corbett, an American actress to whom he had been introduced in London. The marriage was fixed for the twenty-fifth of July. Some of his former University College students presented him with an illuminated address, and a fine loving-cup of Irish silver; but the wedding did not take place. Lankester's letters say little about it, but refer obliquely to 'the fiasco.' It appears that there was a quarrel, and that his fiancée broke the engagement shortly before the wedding. He knew now he would never get married. He was, he ruminated, getting older and uglier every day; it had not come in the past, and it would never come. Work and travel no longer brought him happiness. His favourite author Maupassant had fled on his travels to escape the pangs of the disease that was to destroy him; but the disease that afflicted Lankester was not physical; it was the malaise of loneliness. It is significant that he began a Journal on July 22nd, 1896.

> I determined to keep this book in order to enter not so much the events of my life, which insofar as they would be interesting to record are more in the past than in the future, but to preserve for myself some of my thoughts and impressions ...[22]

Although there was an introspective and self-mortifying streak in his nature, Ray Lankester's gaze was in the main, sturdily directed outwards to the world. He became bored with his journal, and it did not outlast the August of 1897, giving place to notes on evolution theory. What does emerge, both in his Journal and in his letters, is his detestation of Oxford society. How different it was from London.

> There you walk to your lecture-room through the streets, and see a thousand kinds of people, lovely women, old swells, dirty children, country cousins. You perhaps meet an acquaintance you

[22] The Journal is preserved in the Lankester family papers.

> have not seen for a year, or are bowed to by the pretty girl you dined with the evening before. Here I walk to my lecture-room. I meet two or three stiff, pert, ugly-looking Oxford ladies and perhaps a broken-down old don or a beefy-looking parson. Then, after one's work in London, the two hours before dinner can be utilised in a wonderful way – a picture gallery, a call on kindly and amusing friends, a game of whist at the club, or in spring, the park or a cricket-field. Here there is absolurely nothing, not a soul to speak to, not a thing to see. One must take a walk along a suburban road and back again, or go to one's study, or read a novel. Then dinner in London – the club, or with a friend at a restaurant, or a dinner party with amusing people. Here either solitary mastication or a company of silly, pompous fools with nothing interesting about them ... It is curious to note how all the older and better people here are in despair over the utter failure of the University – £400,000 a year is wasted here, and they all admit it, and calmly say they look to me to do something towards improving things.[23]

It is possible that something of Lankester's outlook on Oxford found its way into a celebrated novel. Lankestet often met Thomas Hardy at the Savile Club, and Hardy in his diaty records a conversation with Lankester. The two men soon became close friends.[24] Hardy conceived the scheme of his novel *Jude the Obscure* in 1890, revisited 'Chtistminster' in October 1892, wrote the outline of his work in the following year, and the complete work in 1893-94. If, as seems likely, he discussed his plan with Lankester, there can be no doubt that the latter would have forcibly expressed his views on Oxford. Lankesret could certainly say with Sue Bridehead in the novel:

> I have no respect for Christminster whatever, except in a qualified degree, on its intellectual side ... The medievalism of Christminster must go, be sloughed off, or Chtistminster itself will have to go.[25]

In spite of his loathing of Oxford social life, Lankester loved and reverenced Oxford the alma mater, the historic University.[26] He had some

[23] Letter to his brother, Owen, 16 April (1894?) (Lankester family papers).
[24] See F.E. Hardy, *The Later Years of Thomas Hardy* (London, 1930), p. 34.
[25] Thomas Hardy, *Jude the Obscure* (London, 1951 edn.), p. 180.
[26] Lankester's Journal (Lankester family papers).

good friends at Oxford who did something to mitigate his isolation. From 1891-96, he lived next door to his friend E. B. Poulton, the Professor of Zoology, at 2, Bradmore Road, breaking down the back garden wall for ease of mutual access. He particularly liked Poulton's two young sons, and often relaxed in a game of golf with their father. He got on well with York Powell, the unconventional Oxford historian, who was reputed to have socialist leanings, and who certainly counted some socialist workingmen as his friends. Lankester states that there was an almost complete coincidence of views between Powell and himself. Powell was responsible for inviting the French poet Verlaine to lecture at Oxford. Lankester was a great admirer of the poet, and was able to visit him a few weeks before his death, in the company of Rothenstein the artist. They arranged a birthday party for the poet, Lankester arriving with a bottle of fine wine, the artist with a bouquet, and between them they did something to cheer up the sick and depressed Frenchman.[27] It was Powell who founded an Oxford Rabelais Club, which included Lankester, W. P. Kerr, Rothenstein, Herbert Fisher, and Herbert Green. Members presented papers on various knotty points in the works of the Master, and distributed a host of Rabelaisian stories.[28]

Lankester got on extremely well with foreign scientists, aided by his excellent linguistic capabilities. In 1889 he had served on the Committee of Patronage of the International Zoological Congress in Paris. In 1898 he was one of the Vice-Presidents of the fourth International Zoological Congress at Cambridge. In 1897 he relieved the tedium by a visit to Spain, and talked with de Buem, the Biology Professor at Barcelona. He saw a bullfight, thought it dull and monotonous, sympathised with the bull, and wrote an account of how it might appear to the animal, if it were gifted with human speech. When he could get to London he passed much time at the Savile Club, where he had many friends. He was a leading member of the Saturday night 'conclave' of wit and brilliance that included Edmund Gosse, Hardy, Saintsbury, Rider Haggard, Walter Besant, Anthony Hope, and sometimes Kipling.

The first years at Oxford, lecturing, Museum reorganisation, and his rather poor health, left him little time for research. The stream of articles begins at last to dry up. Not till 1893, did he publish two papers, one an important work on the coelom and vascular system of the molluscs and arthropods.[29] The next year, he wrote about an animal for which he always

[27] William Rothenstein, *Men and Memories: Recollections* (London, 1931-2, 2 vols), I, p. 26.

[28] *Ibid*, p. 138.

[29] Lankester, 'Notes on the Coelom and Vascular System of Mollusca and Arthropods', *Quarterly Journal of Microscopical Science*, n.s. 34 (1893): 427-32).

retained a peculiar affection, and concerning which he had already written five papers – the tiny freshwater medusa, which had appeared in 1880 in the lily tank of the Royal Botanical Society in Regents Park. He had called it *Craspedacusta sowerbyi*; a week later, Allman described the organism in the same journal, *Nature*, as *Limnocodium victoria*. Although Lankester offered to withdraw his own name for the species, a rather unpleasant squabble followed, which gave a handle to Lankester's enemies.[30] Samuel Butler rejoiced in the 'delightful piece of scientific scandal against Mr Ray Lankaster, and how he had misbehaved about a medusa found in a water tank.'[31] Be that as it may, Lankester studied several hundred specimens of the little jellyfish.[32] All told, he wrote no more than a dozen papers while at Oxford.

Towards the end of 1895, there occurred an incident which has passed into legend. In the small hours of Saturday, October 6th, Ray's brother Owen (the physician), was awakened by a knocking at the door of his house at 5, Wimpole Street. He was confronted by a policeman, who told him that his brother Ray was under arrest at Vine Street station. A little later, Owen was able to bail out his sober but agitated brother. Ray recounted how he had left the Savile Club at a quarter to one. He had seen, opposite Bond Street, a screaming woman being removed from a cab by the police, who dragged her towards Vine Street. Brutality always aroused Lankester. Noticing a group of women discussing the matter, he stopped and asked them what had happened. When asked to move on by a constable, he refused. Another constable appeared, and it is highly improbable that they recognised a professor in the tall, florid, well-built man-about-town who confronted them. Lankester was arrested, led off to Vine Street, and charged with obstructing the police, despite attempts at explanation to an indifferent inspector.

[30] Lankester, 'On a New Jellyfish of the Order Trachomedusae Living in Fresh Water', *Nature*, 22 (1880): 147-8 and 'The Freshwater Medusa', *ibid.*, 177- 8; G.J. Allman, 'On 'Limnocodium victoria', a Hydroid Medusa of Freshwater', *ibid.*, 178-9. The publication of Lankester's paper 'Physiology of the Freshwater Medusa' in the *Proceedings of the Royal Society*, 30 (1880): 554-6 was deferred and the pages left blank. The archive of the Natural History Museum, London, contains a cartoon of Lankester as a medusa by Worthington G. Smith entitled 'Scientific Unworthies, No. 1: A Dirty-Water Medusa'. For research on medusa see P. J. Weindling, *Darwinisim and Social Darwinism in Imperial Germany: the Contribution of the Cell Biologist Oscar Hertwig*, (Stuttgart, 1991), pp. 81-97.

[31] H. Fesring Jones, *Samuel Butler: A Memoir* (London, 1919, 2 vols.), I, p. 411.

[32] In addition to several technical articles, Lankester described his work on the freshwater medusae in his *Science from an Easy Chair* (London, 1910), chapter 8 and in *Diversions of a Naturalist* (London, 1915), pp. 91-2.

After being bailed out, he dashed off notes to various friends, one of which has been preserved.

> I was seized in Piccadilly last night in the most outrageous way, and marched into Vine St., and charged with 'resisting the police', apparently because I enquired of a prostitute what another had done, who was being dragged along to the police station. I was never more astonished or outraged. I want you, if you will, to be ready to appear, to say that I left you at the club perfectly sober at 12.35. It is possible that the police might say that I was drunk, which would be a very injurious charge if it appeared in the papers. I am altogether astounded.[33]

According to Chalmers Mitchell, Lankester should have apologised, given a false name, and forfeited a few pounds bail. Instead, there was a gathering of the Lankester clan, and Sir George Lewis was sent for to defend. Since Lewis was a most eminent advocate, his appearance in court suggested that there was something to be 'got over.'[34] Later on Saturday, Lankester appeared at Marlborough Street police court, and an adjournment was granted for one week. Lankester immediately wrote a letter of explanation to *The Times*.[35] On the 13th October, Lankester appeared before the magistrate, Newton, brother of the anatomy professor well known to him, It was a misfortune for Lankester to appear before Newton, who was notorious for his partialiry towards the police. The case opened with an argument between Newton and Lewis, the advocate accusing the magistrate of listening with his hands in his pockets, as if he (Lewis) were telling fables. After a sharp exchange, Lewis threw down his papers, and announced that, because of magisterial obstruction, he would retire from the case, and allow his client to make his own statement – a course not very helpful to his client. Lankester made his statement, but could not prevail against the police, and was bound over in the sum of £10, not to break the peace again.

Lankester was furious, and wrote a long letter to *The Times* explaining his case and openly accusing Newton of gross partiality.[36] He also

[33] Lankester to Bryce, dated '4 a.m., Friday night' (Lankester family papers).

[34] Peter Chalmers Mitchell, *My Fill of Days* (London, 1937), p. 168.

[35] Lankester to *The Times*, 7 October 1895, p. 2; the details of the court case are given on the same page, and of the proceedings on the following Saturday *ibid.*, 14 October 1895, p. 13.

[36] Lankester to *The Times*, 14 October 1895. Clippings of these letters and other papers associated with the incident are preserved in a scrapbook (Lankester family papers).

demanded an enquiry into the conduct of the police, who were in fact very venal, and known to levy blackmail on women of the streets and others. Press comment was in general favourable to Lankester though the *Eastern Daily Press* thought that the case showed it to be inadvisable for gentlemen to be quixotic in Piccadilly late at night. In an interview with a representative of the *Sun*, Lankester announced his intention of writing to the Home Secretary. Some surviving correspondence illustrates Lankester's efforts to obtain an official enquiry, but Lord Salisbury, although sympathetic, would not act – and there the matter rested. There can be no doubt that Lankester was quite innocent of any kind of disgraceful behaviour, but the incident was very harmful to him. Many of his friends, and all his enemies, got the impression that he had been extricated rathet cleverly from an unfortunate, and possibly disgraceful, scrape.

Chalmers Mitchell recounts how he arranged a club dinner for Lankester in May 1897, on the occasion of Lankester's fiftieth birthday. Kipling turned up and made a brilliant and humorous speech. He had, he said, noticed an unusual number of police about, and asked what the trouble was. He was told that there was a dinner down at the Savile for a man called Lankester; rhey knew all about him. They advised him not to go to the club, but to go home quietly, as they did not want him to get mixed up in any trouble.[37]

[37] Mitchell, *My Fill of Days* (note 34), p. 170.

– 11 –
The Director: South Kensington

In 1898 it seeemed to Lankester that he might at last be able to shake off his 'Oxford burden.' On June 11th Sir William Flower resigned his position as director of the Natural History Museum at South Kensington by reason of ill-health (he died in July 1899). Lankester expressed immediate interest in the possibility of replacing Flower, moving in effect from teaching to administration.

Many years earlier he had expressed to Huxley his views on the relationship between 'scientific investigation' and 'scientific administration.' He held that it was necessary to distinguish between the two spheres of endeavour, although many men with a scientific education made good administrators, while certain matters of government required the possession of scientific knowledge and aptitude. In such cases it was better to select a man of science with minor administrative capacity, rather than vice-versa. He gave Thiselton-Dyer and Flower as examples, and mentioned efforts which had been made to put Kew Gardens under a non-scientific administrator. He continued:

> It is on the whole probable that a distinguished original investigator will make a better administrator than the casual place-man, the son of an earl, or the protégé of a leading politician, from the mere fact that the former is of necessity a man of energy, industry and determination.[1]

His own bid for the directorship of the Museum was to encounter much opposition, but his campaign was not solely a matter of personal ambition. He was determined to block any effort to reduce the status of the Museum's director, as part of his continual struggle to establish men of science in their proper place in the social fabric.

To understand the situation into which Lankester projected himself, it is necessary to know something of the Museum's background.[2] Since its

[1] Lankester to Huxley, 26 May 1886 (Huxley papers, Imperial College).

[2] On the history of the Museum see William T. Stearne, *The Natural History Museum at South Kensington* (London, 1981) and Albert E. Gunther, *A Century of Zoology at the British Museum through the Lives of Two Keepers*, 1815-1914 (London, 1975).

founding in 1757 there were three Principal Trustees, the Archbishop of Canterbury, the Lord Chancellor and the Speaker of the House of Commons. By the end of the nineteenth century there were nearly fifty Trustees, from whom were chosen a Standing Committee of about twenty. In 1856 Richard Owen had been appointed Superintendant of the Natural History Collections, but he did not get on well with Pannizi, the Principal Librarian, who was the Museum's administrative head. Owen had exerted little control over the heads of the natural history departments, and thus his duties had been largely nominal, leaving him free to concentrate on his research. Owen was, however, responsible for the creation of a separate building to house the Natural History Collections. After a long campaign, in which he was actually opposed by Huxley, Owen succeeded in getting the new building erected at South Kensington.[3]

The real maker of the Museum was Owen's successor, William, later Sir William, Flower.[4] It was Flower who created an integrated display of living and fossil specimens which illustrated the underlying relationships revealed by comparative anatomy and evolution. He made a clear distinction between the public display of choice specimens and the study collections intended for serious students. Flower had supported Lankester in his proposed rearrangements of the Oxford collections, and Lankester followed his precepts when he himself took over at South Kensington. Flower himself was an expert on whales and arranged an impressive whale room at the Museum. He had been granted the title of Director in 1884, and had also become Keeper of Zoology when Albert Gunther retired in 1885.

When Flower resigned, a group of the Standing Committee decided to reorganize the administration of the Natural History Collections. It seems clear that the moving spirit behind this plan was the Principal Librarian, Sir Edward Maunde Thompson, a noted palaeographer. Chalmers Mitchell describes Thompson as an efficient and dominating figure, who really did intend to take effective control of the whole Museum, including the Natural History Collections.[5] Lankester himself heard about these plans from Sir John Evans (antiquary and father of the discoverer of Knossos) during a conversation in the quadrangle at Burlington House. Evans had told him:

[3] See Nicolaas A. Rupke, 'The Road to Albertopolis: Richard Owen (1804-92) and the Founding of the British Museum of Natural History', in Rupke, ed., *Science, Politics and the Public Good* (London, 1988), pp.63- 89.

[4] See C. J. Cornish, *Sir William H. Flower, K.C.B.: A Memoir* (London, 1904).

[5] P. Chalmers Mitchell, *My Fill of Days* (London, 1937), pp. 170-1.

> I do not think you would care for the post as it will be in future. In fact, some of the Standing Committee desire practically to do away with the post of Director of the Natural History Departments. We think it better to govern the whole institution from Bloomsbury through the Principal Librarian, who is acrually the Secretary of the Trustees ...[6]

Flower had actually invited Lazarus Fletcher, the Keeper of Mineralogy (and the senior Keeper under 65) to apply for the post of Director. Flower himself denied that there was any intention to change the administration of the Museum and defended the existing arrangements in a letter to *The Times*.[7] But rhere was a widespread feeling among scientists that Fletcher's appointment would be the mechanism by which the effective control of the Museum would pass to Thompson. How much of this feeling was generated by Lankester's supporters is a matter of debate.

Whatever the source of the protest from leading men of science, it was overwhelming and ultimately successful. Lankester's version of the events is recorded in a letter to his literary friend Edmond Gosse:

> I think I told you that I had written to Salisbury as soon as I heard (about the 20th of June) that Flower had sent in his resignation. I received the kindest possible letter from S. saying that he would express to those concerned 'his sincere conviction that I was the right man.' Well, of course, if he stuck to that, I was pretty safe, as I was sure of dear old Gully [the Speaker of the House], and Halsbury [the Lord Chancellor] follows Salisbury's indication.[8]

Lankester also went to see the other Principal Trustee, the Archbishop of Canterbury, who said 'I hear you are a very quarrelsome man.' This charge was refuted by E. B. Poulton in a letter to Gully, who had asked Lankester to obtain some supporting letters from men of science.[9] The letter to Gosse continues:

[6] Report of the conversation in Lankester family papers.

[7] See Natural History Museum archives, file DF 930/17. Flower's letter to *The Times* was 11 July 1898.

[8] Lankester to Edmond Gosse, 6 August 1898 (Brotherton collecrion, Leeds University).

[9] Lankester ro Poulton, 23 July 1898 (Lankester family papers).

> Then, however, arose the rremendous barrier. I was told by the two Trustees that the Standing Commirtee had deretmined to alter the management of South Kensington. They had made Maunde Thompson Director of the whole B.M. show, and increased his salary, and placed the genetal expenditure of the N.H.M. under him, and they only intended to have a director with a little 'd' there, and had recommended that the post be considered as a promotion for the senior Keeper. In fact, the Museum had never been financially independent of Bloomsbury, although such independence had been offered to Flower when he became director and refused. But Trustees may propose – it is the three Principal Trustees who dispose – and my special friend among them (Gully) told me that they were not going to be dictated to by Standing Committees etc. etc.

A memorial was suggested, protesting against the abolition of Flower's post. No sooner said than done. Faithful young friends did it – Weldon, Sedgwick and Mitchell. This manoeuvre provides clear evidence that – as Lankester's enemies deduced – the campaign was engineered directly at his request.

The result of these efforts was a Memorial to the Trustees prepated by the Royal Sociery. Its purpose was to stress that all the advantages gained under Flower's regime would be lost if the Directorship did not pass to an eminent biologist. The memorial was prepared on July 5th; the council of the Royal Sociery met two days later to endotse it and one hundred and twenty signatures were obtained. It was published in *The Times* of July 9th. A subsequent letter to *The Times* by 'one of the original signatories' made its purpose explicit: to prevent the appointment of someone from within the Museum staff who was not a naturalist.[10] The Standing Committe now began to back down. Maunde Tompson wrote to *The Times* on July 10th insisting that there was no intention of abolishing the post of director, but inisisting that the Principal Librarian had always been the chief accounting and executive officer of the Museum. In this he was supported by Flower.

Even so, efforts to block Lankester's appointment continued. The oceanographer, John Murray, was proposed as a candidate, but he would

[10] *The Times*, 14 July 1898. There is what appears to be a draft of this memorial in the Lankester family papers, suggesting that Lankester himself may have played a role in its composition. For derails of this and the other debates see Srearne, *The Natural History Museum* (note 2), chaprer 8.

not stand against Lankester. The Archbishop of Canterbury allowed himself to be persuaded, and Lankester was duly elected to the position, entering office on October 1st. Considering the circumstances of his appointment, his position was bound to be uncomfortable. Lankester wrote to Albert Gunther asking his advice on his duties and responsibilities, but Gunther and Flower were by no means sure Lankester was suited to the job.[11] Maunde Thompson was hostile from the beginning, and although some of the Keepers may have welcomed Lankester's appointment, others – including Lazarus Fletcher – were inclined to side with Thompson. Nor was Lankester himself inclined to be conciliatory. He had definite plans for the Museum and expected to be given the freedom to put those plans into operation. In 1904 he likened his position to that of the captain of a ship; the Trustees were equivalent to the owners who should not expect to put to sea with the ship.[12] Thompson was, in effect, the captain of another ship who should stick to his own affairs. This was not, however, Thompson's teading of the situation, and Lankester complained that the Standing Committe watched him as a cat warches a mouse: 'We have a sub-committee on Entomology tomorrow, and Maunde Thompson has the impudence to come to it, merely to annoy me, and get in the way as far as he can.'[13]

The routine work of running the Museum was, in any case, much heavier than Lankester had anticipated. In an article written in 1906 he gave an outline of his duties.[14] Under him were thirty-eight higher officials and one hundred and forty attendants, messengets, masons, artisans, labourers, police and commisionaires. Letters constantly arrived asking for information as to the gift or purchase of specimens, the collection or identification of specimens, and guidebooks. Applications for posts at the Museum, for permission to draw or study, arrived every day. Answers to these had to be dictated or indicated, and other letters from colonial governors or from other British or foreign officials had to be answered by

[11] Lankester to Albert Gunther, quoted in Gunther, *A Century of Zoology* (note 2), p. 439.

[12] The memorandum is headed 'Memorandum by the Director of the Natural History Departmenrs, submirred ro the sub-committee appointed to enquire inro the present working of the Museum. May 1904.' Memorandum no. 2 is dared 10 June 1904 and no. 3, 27 June. See memorandum no. 2 (printed copies in Lankesrer family papers; these memoranda are nor preserved in the archives of the Natural Hisrory Museum).

[13] Lankesrer ro Thiselton-Dyer, 23 November 1900 (Thiselron-Dyer papers, Kew).

[14] Lankester, 'Eight Years in the Natural History Museum', *Fortnightly Review*, 80 (1906): 1039-54, pp. 1041-2.

the director himself. Letters of thanks for gifts had to be signed, and written instructions signed and sent to the staff. Every bill from every department, from that for a stuffed elephant to that for washing dusters, had to be examined and initialled. The reports of the entire staff, and especially of the Zoological Department, as to illness, increase of pay *etc.*, had to be examined; the complaints, suggestions and enquiries of members of staff had to be attended to by personal interview or memorandum; and directions and decisions of the most diverse kind had to be given, from the posing of an animal group to the choosing of paint for shelves. Then came consultations with the Keepers and inspections of work in progress in all parts of the Museum (hearing, lighting, ventilation, building), of the taxidermists' room, of the modellers' studio, of new specimens being unpacked, and of the workrooms of the Keepers, assistants, and voluntary workers.

In a letter written to Dyer after a bout of illness Lankester described a typical day:

> Today I went into the Museum at eleven, and the following variety occurred. 1. Answer the letter from Sedgwick. 2. Letter as to fish from Victoria Nyanza to Elliot. 3. Letter to Bowlder Sharpe to remonstrate at his delay in sending MS. 4. Elwes to see me about American government and collection of Marine Biological. 5. Guido Gigioli ftom Flotence to see me. 6. New animals from the Antarctic seas – dissections, photographs, drawings to superintend. 7. Book not to be found in library: order same. 8. Elephant photos and tapir to arrange. 9. Sir I. Hampson, head of Insect Dept., wants to know about his appointment. 10. Smith Woodward walks with me through the gallery, talk as to Antarctic and Egyptian things. 11. Lydekker wants me to decide as to colour of paint for cases in Museum. 12. Bell as to Antarctic staff, whether to be employed or not. 13. Beddard's collection of earthworms. 14. Messenger with account books of official and private letters ...[15]

One of Lankester's first acts as Director was to obtain statements from the various Keepers of the number of specimens in each zoological collection, both identified and unnamed. The collection of vertebrates was found to be in a satisfactory condition, but the insects and arachnids were in a backward condition so far as species determination was concerned.

[15] Lankester to Thiselton-Dyer, 13 October 1904 (Thiselton-Dyer papers, Kew).

Lankester tried to get the Trustees to allow parts of the collection to be loaned out for examination by various experts, of whom he had a list compiled by the heads of foreign museums. This was rejected as being incompatible with the Act of 1753 which contained a clause stating that the specimens were to remain in the Museum 'to all posterity.' It was also suggested that an enquiry into the matter would be unwise at a time when the ownership of certain Celtic gold ornaments was being debated between the Bloomsbury Museum and the Treasury.[16]

It was four more years before the Lord Chancellor gave his opinion that the Act did not possess a restrictive sense, so that the Director was at last able to send specimens out for identification. The work was soon begun, and Lankester was later able to claim this as a major improvement under his regime.[17] One consequence of this change was that it encouraged Lankester to propose a history of the Museum's collections, subsequently published as the *History of the Collections contained in the Natural History Departments of the British Museum.* The first volume (1904) dealt with the libraries and the departments of Botany, Geology and Minerals. Volume 2 (1906) covered the various collections of Zoology. Albert Gunther produced an appendix in 1912 giving a history of the Department of Zoology in the late nineteenth century.

The dispute over the sending out of specimens was but one in a long series of conflicts between Lankester and the Principal Librarian and his party. In October 1899 Maunde Thompson wrote to Lankester pointing out that he had been on vacation for longer than allowed for in the regulations and demanding to be informed when he intended to be away. Lankester replied offering to leave an address where he could be contacted. Thompson wrote back saying:

> It seems to me that you wrote this letter under a misapprehension of the relative positions of the Director and Principal Librarian of the British Museum and the Director of the Natural History Departments. On reconsideration you will, I hope, recognize that the tone of equality you have assumed is incorrect ...[18]

[16] Principal Librarian's report, 28 October 1899 (Natural History Museum archives, file DF 930/18).

[17] Lankester, 'Eight Years at the Natural History Museum' (note 14), p. 1145.

[18] Maunde Thompson to Lankester, 21 October 1899 (Natural History Museum archives, file DF 930/18).

He went on to list the holiday entitlements of the staff and to require Lankester to state the number of days he had been away. This must have been particularly galling to Lankester, who travelled extensively on the continent every year. He replied with a simple acknowledgement, provoking Thompson to ask if this was the only reply he was going to receive. Lankester responded in a long letter of 27th October setting out his view of the situation:

> Dear Sir Edward,
>
> Your view of our official relationship differs, I am very sorry to be obliged to say, from that which, I think, is warranted by the instructions and information furnished to me by the direction of the Trustees ... I believe I am not mistaken in thinking that you did not consider it your duty to ask my predecessor, Sir William Flower, to report to you concerning his vacations, and that neither you nor your predecessor in office ever did so. I have regarded the position to which I was appointed at the Natural History Museum as identical with that held by Sir William Flower, and have, to the best of my belief, conformed to his practice in regard to vacations, whilst allowing nothing to interfere with the full discharge of my work at the Museum ... I think that I should be wrong to acquiesce in the new departure which it seems to me that you propose in regard to the relationship of the Director and Principal Librarian and the Director of the Natural History Departments ... I am obliged with great reluctance to object to your belief that I should report to you. I am under the belief that I should report directly to the Trustees and not to you.[19]

The Standing Committee ordered Lankester to prepare a statement of his view of the relationship, which he did in an eight-page memorandum. In fact Thompson did have the powers he assumed, even if he had not exercised them with Flower, and the Trustees backed him in a resolution of 11th November.

Lankester responded by trying to repeat the tactic which had originally gained him his position. In May 1900 his friends drafted a memorial which was circulated for signature before being presented to the Council of the Royal Society. This attacked the management of the Natural History Departments, implying that Lankester needed freedom from external constraint in order to bring things to order. But this time the

[19] Lankester to Thompson, 27 October 1899; *ibid.* Quoted in Stearne, *Natural History Museum* (note 2), p. 82.

tactic failed. Lazarus Fletcher, the Keeper of Mineralogy whose candidature for the directorship had been blocked by Lankester, and George Murray, the Keeper of Botany, whose department was threatened with removal to Kew, responded with a counter-memorial in July. This pointed out that many scientists had signed both the 1898 and the 1900 memorials despite the fact that they made contradictory claims. In 1898 it was said that the Museum was well-run, now it was alleged that it was badly run, although the system of management had, at least ostensibly, remained unchanged. This allowed Fletcher and Murray to allege that many of the signatories had been persuaded to sign without reading the document they were putting their names to. They went on to argue that the memorial had been drafted by Sir Michael Foster, who, as a secretary of the Royal Society, could not sign it:

> Of course Sir Michael has a perfect right ... to shoot from behind a boulder, if he is of the opinion that such a method of attack is the safest and most likely to be successful; but we submit that there is a great lack of candour and fair play in this matter of dealing with the Royal Society Council and in this mode of attempting to secure the official help of the Royal Society in support of an attack on the Trustees of the British Museum or on ourselves.[20]

Faced with a demand for an enquiry that would have produced embarrassing results, the Royal Society abandoned its support for the original memorial, leaving Lankester in an even worse position than before.

The atmosphere in the Museum following this affair made Lankester's job even more difficult. To Dyer he wrote:

> I am as usual subject to plots and devilries ... E. M. T. coolly takes my functions out of my hands and creates disorder and muddle by so doing. Some day they will say that I am useless, and that the system won't work, and retire me at sixty years of age without a pension, or with about two hundred pounds a year.[21]

[20] Lazarus Fletcher and George Murray, 'Memorial to the President and Council of the Royal Society, 4 July 1900' (Natural History Museum archives, file DF 930/16); also quoted in Stearne, *Natural History Museum* (note 2), p.83. The same file contains a comparison of the 1898 and 1900 memorials with an analysis of the motivations behind them, very hostile to Lankester and his friends.

[21] Lankester to Thiselton-Dyer, 11 February 1901 (Thiselton-Dyer papers, Kew).

He was to see this prophecy fulfilled almost to the letter. Another letter of October 1901 described increasing troubles. He was unwell and had a 'terrible time' with the Trustees.

> It is now a regular programme of baiting me, assayed beforehand by Thompson, Walsingham, Rothschild, Dillon and Evans. They *refuse* to *discuss* anything, and bring forward cut-and-dried resolutions, which Dillon puts from the chair *en bloc* ... The Bishop of Winchester and Avebury stood up for me, but I was indignant. Walter Rothschild interrupted me continually, and the place was like a bear garden. Walsingham also attacked me ... Their object is to manage the place themselves, and refuse even to hear my suggestions or opinion. What is to be done? The members of the Standing Committee who are not actually members of the conspiracy against me, such as Peel, Avebury, Northumberland, the Bishop of Winchester, only come occasionally, and are dumbfounded by the proceedings. The conspiracy passes resolutions and rejects my proposals without *pretence* of *discussion*; the other members look on and don't understand. John Morley comes, and although he does not actually belong to the inner 'ring', always throws in a word and a vote with them.[22]

Lankester came to dislike Morley very much. Some time after an especially violent meeting, he was in France with William Heinemann and Chalmers Mitchell and gave them a diverting impression of how Morley would appear when hanged by the neck, head on one side and tongue hanging out.[23] A later meeting of the committee rejected out of hand Lankester's plan for a much-needed lift from the basement to the top floor.

In 1901 Lankester came directly into conflict with an important Trustee, Lord Walsingham, who wished to donate his substantial collection of micro-lepidoptera to the Museum. He laid down impetious and rigid conditions which Lankester found unacceptable, considering it impolitic to allow donors to dictate terms. He thought it best to buy the collection on the open market and use it as was thought best. He complained that the whole matter was put through at four successive

[22] Lankester to Thiselton-Dyer, October 1901 (ibid.).

[23] Mitchell, *My Fill of Days* (note 5), p. 41.

meetings *at Bloomsbury*, in the absence of the Director, who had not been consulted.[24] In this case, however, his objections did have some effect, because acceptance of the collection was delayed for some years, although Walsingham became a bitter opponent of Lankester through the rest of his tenure of the directorship.

In 1904 relations with Maunde Thompson became so strained that Lankester demanded that the instructions of the official 'Red Book' of 1886 be applied again: the Principal Librarian should not attend meetings at the Natural History Museum. Thompson had apparently begun attending meetings during Flower's illness, and had kept up the habit in order to oppose Lankester. Lankester found Thompson's presence an obstacle to the development of sentiments of goodwill towards him. More and more of his proposals had been turned down, including a plan to send Arthur Smith Woodward to Patagonia to study the remains of recently extinct ground sloths. He felt that his cup was full when the Entomological sub-committee, having met without the Director, presented proposals for changes in the department at a formal meeting. The Standing Committee refused to hear Lankester's report on the matter, and the sub-committee proposed a resolution drafted by Thompson that the Director's report not be read. 'And they are ignorant of its contents to this day', Lankester complained bitterly.[25]

It was also in 1904 that the Trustees appointed a committee to enquire into the affairs of the Natural History Museum. Lankester submitted three memoranda to the committee defending his methods.[26] Witnesses were examined and the Director himself questioned. A report was submitted to the Trustees on July 9th in which it was claimed that Lankester, like Owen, was neglecting the administration of the Museum to pursue his own research.[27] He did not consider it his duty to conduct regular and systematic inspections of the whole Museum. Lankester was instructed to carry out his duties as per statute.

At the first meeting of the committee, Sir John Evans accused Lankester of not accepting 'the general responsibility of the Director for the conduct of the various departments'. Lankester replied that the imputation was absurd:

[24] From the 'Parry document', a long account of Lankester's troubles at the Museum dated 22 June (? 1904) to a Mr Parry, apparently a civil servant or official (Lankester family papers).

[25] *Ibid.*

[26] From the memoranda of 1904 (note 12).

[27] Part of the committee report is quoted by Stearne, *Natural History Museum* (note 2), pp. 85-6.

> What I did state in answer to Sir John Evans was that the Director could not be held 'responsible,' in the strict and usual sense of that word, for the management of the several departments, as he had not the authority or control, placed in his hands by the Trustees, enabling him to be really 'responsible.'[28]

Morley accused Lankester of admitting that he did not make a regular and systematic inspection of every department, to which Lankester replied that he frequently visited every part of the Museum.

> What I did repudiate ... was the making of a regularly fixed and recurrent inspection of every department. That has not been the method of supervision handed on to me as the custom of my predecessor. I wish on the other hand to state most emphatically that I never said either that I did not consider it my business to inspect every department from time to time, or that in fact I did not visit and inspect every department from time to time.[29]

This point was repeated in Lankester's formal response to the committee's report, in which he also drew attention to the large increase in the amount of administrative duties that now fell upon the Director.[30]

To the accusation that he regarded himself as another Owen, he responded publicly some years later:

> I do not know what theory the late Sir Richard Owen held as to his duties ... It would require greater audacity than I possess to propose to oneself to emulate the marvellous industry and fertility in scientific research of that eminent man. As a matter of fact, I have had no such illusion as that with which I am credited ... It will be seen that I have but a small amount of time for discovering new things, though I have managed to do a little in that direction ... My constant and strenuous labour in improving the arrangements of the Museum in almost every direction, and in facilitating and stimulating the work of the staff, has not been placed on record, and is little known to the general public, as it would seem to be to those who have invented the claim that I wished to pose as another Owen, and have withdrawn myself from administrative

[28] Memorandum No. 3 (note 12).

[29] *Ibid.*

[30] The complete text of Lankester's reply is quoted in Stearne, *Natural History Museum* (note 2), pp. 86-8.

> work. The public has indeed recognized the large developmenrs and improvements in the exhibition galleries, which I have introduced, and for which I am responsible; but the work done 'behind the scenes', and in the way of general administration and of technical publications, is not so obvious to the world at large.[31]

The public was indeed well aware of Lankester's improvements, and the bright and interesting exhibitions he arranged. Great excitement was created when Andrew Carnegie presented a cast of the skeleton of the giant dinosaur, *Diplodocus*, to the Museum in 1905. Lankester's popular book *Extinct Animals* was published in the same year. He also superintended the revision of a number of Museum guidebooks to the various departments, and had them displayed in a table case at the entrance to the Museum. He planned temporary exhibitions on matters of contemporary interest, including the tsetse fly and the trypanosome. He would have liked to give a series of public lectures, but there was no suitable lecture room.

At the same time, foreign academies were beginning to honour him with memberships. In Brussels he was elected successor to Leuckart; in Paris he followed Steenstrup, beating his friend van Beneden by thirry-eight votes to eight. He found it all gratifying, but embarrassing too:

> Now really and truly he [van Beneden] is a more meritorious worker than I am. I am simply astonished at being preferred to him. It makes me inclined to blush, and also to feel sad. I have a sort of sense that it is the end of the play ...[32]

The strain of rhe disputes at the Museum was indeed begining to tell. In 1905 he had ro have leave of absence because of an ischio-rectal abscess. This was unsuccessfully operated on in July and continued to give trouble until a second operation. Even so, he was left weak and ill, and as late as October was 'not quite healed up'.[33] The final act in the drama of his Directorship was sprung upon him without warning in 1906, the year in which he was President of the British Association for the Advancement of Science.

On May 25th of that year he was informed by the Standing Committee that, in accordance with the Civil Service Superannuation Act, which

[31] Lankester, 'Eight Years at the Natural History Museum' (note 14), pp. 1052-3.

[32] Lankester to Thiselton-Dyer, 6 March 1899 (Thiselton- Dyer papers, Kew).

[33] Lankester to his nephew, Felix Vatcher, 9 October 1905 (Lankesrer family papers).

empowered the head of a department to ask for the resignation of his officials at age 60, he would be called upon to retire from his post in the following May, on such a pension as he was entitled to receive by regulation (£300 per annum). In answer to a formal request, the Committee refused to give any reason for their action. On July 28th the general meeting of the Trustees confirmed the Standing Committee's resolution, subject to establishing that the committee had legally the power to act under the legislation cited.[34]

Faced with this demand, Lankester decided to take the public into his confidence, sending a long letter to *The Times* which was published on August 8th. He pointed out that the rule was applied only to individuals who had, by forty years of employment, earned the maximum pension of two-thirds of their salary. The normal retiring age was 65, and might be extended to 70; Flower had continued until age 68, while Owen had been 80 when he retired. He went on:

> it will be admitted by everyone, that to remove suddenly a scientific public servant from a position of active occupation, with working rooms, collections and libraries at his disposal (and a salary of £1200), to isolation and a pension of only £300 a year, is an extraordinary measure, and that even were such a removal necessary, the retiring allowance offered is inadequate and unreasonable ... At the age of sixty, a man, however vigorous, cannot expect to enter upon new occupations of a remunerative kind, and this is more especially the case when he has devoted his life, as a man of science and a teacher, to a peculiar kind of work, for which there is but limited demand.

An editorial on the same day was strongly in Lankester's favour. It spoke of 'cavalier treatment' and 'derisory compensation', and attributed the dismissal to 'reasons of a more personal kind' arising from Lankester's refusal to adopt a conciliatory tone in the face of interference. The editorial concluded:

> The Museum may manage its affairs in its own way, but it is a disgrace to the nation to treat a distinguished man of science, entering its service in exceptional circumstances, as if he were an ordinary clerk, merely because an absurd technicality places him in the same category.

[34] Natural History Museum archives, files DF 930/19 and 20.

The *Evening News* of the following day stated more bluntly that the reasons for dismissal could only be private envy and hostility, for that was the only reason the Trustees would need to conceal in the formal summons to retire. Other papers called upon the Trustees to give their version of the events.

Foreign opinion was also shocked and incredulous. Dr Choate wrote from America to say that opinion there was on Lankester's side as long as the Trustees remained silent. The *Revue scientifique* was indignant, while Edmond Perrier wrote that Lankester would retain the affection and esteem of biologists. The tribure that Lankester esteemed most highly came from the Institut Pasteur, signed by Roux, Metchnikoff, Laverny, Mesnil, Borrel, Laveran, Nicolle, Delezenne and Besredka.[35]

Nevertheless the Trustees remained unmoved. Lankester bombarded them with demands for details, but got little response. He threatened legal action, and the Trustees were forced to have their opinion backed up by the Treasury and a legal opinion by the Attorney General.[36] There was a long debate over whether it was the whole body of the Trustees or merely the three Principal Trustees who had the power to act in the matter. Unfortunately for Lankester, his arguments were unavailing as long as the Trustees chose to stick with the law. In only one respect did the public outcry affect the situation. The Archbishop of Canterbury wrote to Lankester offering an additional pension of £250 from the Civil List, and he was forced to accept resignation on these terms. The Zoology Organizing Committee of the British Association prepared a memorial to the Treasury demanding a pension commensurate with Lankester's contribution to science, but by this time it was already too late.[37] Even now, the Treasury managed to block the atrangements made by the Trustees for some time, although in rhe end Lankester got his £550 a year.[38]

Lankester did have friends in high places, even at Court, and from this source came an important compensation. An occasional visitor at the Museum was Anise Gosford, lady-in-waiting and sister of the Earl of Gosford. Edward VII was known to favour the bestowal of some token of honour, and on 22nd June Lankester received the following letter from 10, Downing Stteet, marked 'Secret'.

[35] Letters from Choate, Perrier and the Insitut Pasteur in Lankester family papers.

[36] Copies in Natural History Museum archives, file DF 930/19.

[37] Copy of the printed memorial ibid., file DF 930/20. The printed date is October 1907 but this has been crossed out and 'July 1907' inserted.

[38] W. W. Jackson to Lankester, 5 July 1907 (Lankester family papers).

Dear Professor Lankester,

It will be a great pleasure to me if you will allow me to submit your name to the King for the honour of K.C.B. in recognition of your eminent scienrific attainmenrs. You will be good enough to treat this as quite private until His Majesty's pleasure is taken.

Yours very truly,
H. Campbell Bannerman

Lankester was indifferent to honours, but his family were insistent and friends pointed out that it could be taken as a measure of triumph over the Philistines.[39] In due course he knelt in front of Edward to receive the honour.

Lankester's dismissal remains an astonishing incident in the administrative history of British science. The Archbishop of Canterbury assured him that there was no charge of neglect or misbehaviour against him, but merely a desire to have a different class of man as Director.[40] Lankester was convinced that attacks were being made on his personal character, and asked Anise Gosford to make enquiries.[41] After his retirement he asked the Archbishop point blank what was being said about him. He received the following reply:

> I certainly have never heard of the calumnies which you think must have been somewhere astit to the discredit of your personal character. They have not reached my ears.[42]

There was certainly some gossip based on incidents such as the arrest in 1895, but more pertinent was the hostility of many Establishment figures towards a man who expressed radical religious and social opinions and would not adopt a conciliatory manner towards those who obstructed him.

These same feelings stood in the way of the one remaining chance for Lankester to retain an active career in science. The Oxford chair was now vacant once again owing to the death of W. F. R. Weldon, and he tried to secure it. Efforts were made on his behalf by friends outside Oxford, especially by Thiselton-Dyer and Chalmers Mitchell, who obtained

[39] *E.g.* A. Dendy to Lankester, 29 July 1907 (Lankester family papers).

[40] Lankester to Henry Higgs, July 1906 (Lankester family papers).

[41] This is obvious from Anise Gosford to Lankester, 27 October 1907 (Lankester family papers).

[42] Randall Davidson to Lankester, 22 December 1908 (Lankesrer family papers).

signatures on a circular letrer to Lankester himself to be used in support of his application. But opinion ar Oxford itself was divided. E. B. Poulton wrote to the Electors describing Lankester as 'by far the greatest leader of zoological thought among the English-speaking peoples', and comparing his 'master mind' with that of the 'ordinary type of conscientious hardworking man'.[43] But even those such as Albert Gunther's son, Robert, who were anxious ro see the role of science improved, realized that Lankester had made too many enemies among the establishment and might do their cause more harm than good.[44] In the end, the chair went to Gilbert C. Bourne.

Lankester's retirement did not end the efforts of the scientific community to reform the management of the Museum. In September 1907 a memorial signed by many prominent scientists was sent to the Prime Minister. This asked that, in view of the approaching vacancy of the Directorship, an enquiry be held into the adminisrration of the Museum. In July of the following year a deputation led by Adam Sedgwick went to see the Prime Minister. Francis Darwin and others urged the need for the Director to have a free hand. The Prime Minister refused to consider any alteration to the legal status of the Trustees, but promised to convey the deputation's feelings to the Trustees.[45]

This was not enough for the scientists, and in April 1909 Sedgwick and several others sent a powerful letter to *The Times* demanding a Royal Commission to investigate the Museum's administrarion.[46] Maunde Thompson, himself now on the verge of retirement, counterattacked by sending to *The Times* copies of a correspondence between the geologist Sir Archibald Geikie and one of the Principal Trustees.[47] Geikie claimed to have made a careful investigation and concluded that 'the agitation has no substantial justification, but has arisen from misapprehension and ignorance' ... He concluded with some pointed remarks about 'personal idiosyncracies'. Sedgwick and Lankester replied promptly, claiming that the independence of the Director had been revoked in 1898, and that Geikie himself had been appointed as a Trustee only recently and did not know what Lankester had had to endure.[48]

[43] Printed copy preserved in Lankester family papers.

[44] Letrer to Robert Gunther from rhe President of Magdalen, quoted in Gunther, *A Century of Zoology* (note 2), p. 441.

[45] Printed pamphler, 'Natural History Museum: Account of Proceedings at the Deputation to the Prime Minister on 28 July 1908' (Natural History Museum archives, file DF 930/16).

[46] *The Times*, 19 April 1909.

[47] *Ibid.*, 13 December 1909.

[48] *Ibid.*, 28 December 1909.

The new director was, in the end, Lazarus Fletcher, the Keeper of Mineralogy, with S. F. Harmer being appointed Keeper of Zoology. Perhaps Fletcher was the 'little d' director that Lankester had feared. Agitation about the Museum certainly continued, and after the War Lankester demanded a Royal Commission on the status of all the nation's museums.[49] He was especially critical of the 'new so-called Science Museum.' He wanted to see a separation of the agglomeration of national museums to form a series of independent institutions, each with its own expert director.

The early years of Lankester's retiremenr were difficult and depressing, although he found some outlet for his energies in the popular articles he began late in 1907 for the *Daily Telegraph.* In 1908 he received the Darwin-Wallace medal struck by the Linnean Society in commemoration of the historic papers read fifty years before. 1909 was an especially black year, a time of stress and strain when all his troubles seemed to culminate. Even so, he attended the Darwin celebration at Cambridge and delivered an appreciation of Darwin that he reprinted in *Science from an Easy Chair.* He was also elected president of the Royal Microscopical Society, an honour bestowed on his father over half a century earlier. He gave a speech at his inaugural meeting, but felt unable to attend any further meetings. In 1913 he received rhe Copley Medal of the Royal Society. The President, Archibald Geikie – his antagonist in the Museum controversy of 1909 – paid him a glowing tribute and commented on his habit of livening things up with caustically-written letters to the press.

[49] Lankesrer in *Nature,* 105 (1920): 180.

- 12 -
Medicine and Economic Zoology

Although Lankester was revolted by the idea that scientific knowledge should be cultivated solely for the sake of its practical benefits, he was not insensible to its economic importance. His work towards the founding of the Marine Biological Association (chapter 9) had been in part inspired by the knowledge that here was an area of zoology with direct practical applications. His work in microbiology also had obvious implications for medicine, and during the 1890s he became involved in the setting up of what became the Lister Institute of Preventative Medicine. At the Natural History Museum he encouraged the study of insects suspected of carrying disease. He also made important contributions to the origin of the conservation movement, in addition to describing a number of newly discovered species from regions soon to come under threat.

In 1887 Lankester visited Louis Pasteur's laboratory in Paris and struck up a friendship with the French biologist. A letter from Pasteur in 1889 makes it clear that it was Lankester who encouraged the Lord Mayor of London, Sir James Whitehead, to visit the Institut Pasteur, and to set up a fund to help support its work:

> Comment vous remercier de votre si aimable lettre! Je suis invité au banquet du Président M. Carnot, demain jeudi, auquel assistera, disent les journeaux de ce matin, le Lord Mayor de Londres. J'irais tout exprès pour voir votre premier magistrat de la cité de Londres et lui rappeler, s'il est possible de l'approcher, la promesse qu'il vous a fait de venir visiter l'Institut Pasteur, au moment de nos innocularions. Ce serait un grand honneur et un inappréciable bienfait qu'il snt prendre l'initiative d'une souscrition nationale en Angleterre, ainsi qu'il vous l'a fait trésot. Je ne manquerai pas de prier M. Dr. Chautemps, président du conseil municipale de Paris, qui est de mes amis, de l'accompagner dans sa précieuse visite.[1]

[1] Pasteur to Lankester, 8 May 1889 (Lankester family papers).

The Lord Mayor examined the record of treatment for rabies and was impressed. He undertook to do something to show the gratitude of the British people for the treatment of some 250 British sufferers, the largest foreign contingent. From this initiative arose not only a fund to help the Paris institute, but also the campaign for a similar institution in Britain.

Lankester was convinced that a state laboratory similar to the Institut Pasteur was an urgent necessity. But on the particular question of rabies, he urged the eradication of the disease through the muzzling of dogs and strict quarantine.[2] Much correspondence passed between Lankester and the Lord Mayor, leading to the calling of a meeting at the Mansion House to discuss the whole question.

The anti-vivisectionists were determined to block any effort to found an institute equivalent to Pasteur's, and Lankester had to warn the Lord Mayor to have police on hand to deal with any disturbance. The meeting was held on July 1st, with many eminent scientists present. Huxley, Tyndall and the Prince of Wales sent messages of support. It was decided to establish a fund to support the Institut Pasteur, and to pay the expenses of British sufferers too poor to afford the cost of the journey to Paris. Lankester was made treasurer of this fund.[3]

The meeting also expressed support for the idea of founding an institute in Britain. Lankester was convinced that this would only be possible with government support. But others felt that a research institute was unnecessary – all that would be needed was a facility for preparing innoculations. Professor Roy of the Department of Pathology at Cambridge at first took this line, but soon followed Lankester and Michael Foster in campaigning for a research laboratory. As donations began to come in, it was planned to found the laboratory at Cambridge, but conditions attached to a large donation forced a move to London. The British Institute of Preventative Medicine began work in 1894 in the building of the College of State Medicine in Great Russell Street, while a new building was erected at Chelsea. The name was changed first to the Jenner and then to the Lister Institute of Preventative Medicine. Lankester served on the council of the institute for many years. At first it seemed that his warnings about a privately-funded operation were justified, and it was not until 1912 that a gift of a quarter of a million pounds by Lord Iveagh made the project secure. A letter from Iveagh to Lankester indicates that

[2] On these developments see A.N. Drury, 'The Lister Institute of Preventative Medicine', *Proceedings of the Royal Society*, 135 B (1947-48): 405-18, which quotes from several Lankester letters.

[3] Lankester, preface to Olga Metchnikoff, *Life of Elie Metchnikoff, 1845-1916* (London, 1921), p. vii.

the latter had been endeavouring to obtain this support at least since 1899.[4]

It was in areas connected with medicine that Lankester pioneered efforts to exploit the economic potential of zoological knowledge. He recognized that botany had already advanced much further than zoology in this respect. On several occasions he discussed this question with Thiselton-Dyer, and in 1899 he wrote:

> I am more and more convinced of the necessity of a department here of Economic Zoology, to work side by side with the systematic department, and make use of their information. It would lead to the formation of a splendid collection, and would also give us an increased position of utility in public matters.[5]

After his appointment at the Natural History Museum, it was Lankester's influence which led to the publication in 1903 of the First Report on Economic Zoology by F. V. Theobald (whose name is commemorated in the mosquito genus *Theobaldia*). This book contained reports to the Foreign and Colonial Offices on matters of economic zoology. Lankester added an introductory classification of animals from the standpoint not of phylogeny, but of their relationships to mankind.[6] There was extensive material on tsetse-fly disease, pearl fisheries, and the marine resources of the West Indies, which arose (Lankester wrote) 'from my being consulted in my official capacity by the Secretaries of State for Foreign Affairs and for the Colonies'.

In the year in which he became Director of the Museum, Lankester was appointed a member of the newly-formed Tropical Diseases Committee of the Royal Society. This had been formed at the request of Joseph Chamberlain, who was less hidebound and more energetic than his predecessors at the Colonial Office. Lankester became chairman in 1903. Soon he was able to put the Museum's resources to work on the mosquito, once its role in the spreading of malaria had been recognized. It was Lankester who first described a protozoan parasite of the type which was later shown by C. L. A. Laveran to be the causative agent of malaria. This discovery was made in 1882, when he described in the *Quarterly Journal of Microscopical Science* the haemogregarine *Drepanidium ranarum*.[7] This had

[4] Iveagh to Lankester, 17 January 1899 (Lankester family papers).

[5] Lankester to Thiselton-Dyer, 16 March 1899 (Thiselton-Dyer papers, Kew).

[6] Lankester, 'Introductory Scheme of Economic Zoology', in F. V. Theobald, *First Report on Economic Zoology* (London, 1903).

[7] Lankester, 'On Drepanidium ranarum , the Cell-Parasite of the Frog's Blood and Spleen', *Quarterly Journal of Microscopical Science*, 22 (1882): 53-65.

already been seen by the German microscopist Gaule, who considered it to be a normal cell inclusion. In 1892 Labbé renamed the patasite *Lankesterella.* Lankester's intetest in the parasitic protozoa is also commemorated in the genus *Lankesteria* of Manganizzini, tiny coccidian parasites of the Tunicates.[8]

As a member of the Royal Society's Tropical Diseases Committee, Lankester was closely associated with the work of Ronald Ross and Patrick Manson on the malaria parasite. He was in constant contact with Ross during his work in India and followed the work step by step in the reports that Ross sent home. Laveran had already realized that the mosquito might be the carrier of malaria, and Lankester saw rhat the Museum could give important help in this area. Manson wrote to Ross that Lankester was instituting a systematic investigation of the Culcicidae or mosquitos.[9] Lankester himself described these developments in his preface to Theobald's 1901 monograph.[10] He sought the aid of the Foreign, Colonial and India Offices to supplement the small collection at the Museum. A circular was senr to every official of the colonial service, and many other organizations and individuals were asked to cooperate. An extensive collection of mosquitos was assembled for Theobald to work on. The report – the first comprehensive account of the mosquitos – was published with numerous fine lithographic illustrations.

Lankester missed Ross and Manson when they firsr called at the Museum in April 1899, but met them soon after. They also made the acquaintance of the Museum's diptera expert, E. E. Austin. Lankester was at first sceptical of Ross's work on the bird-malaria parasite *Proteosoma.* He invited Ross to visit the Institut Pasteur in Paris with him, where they met Laveran, Mesnil and Metchnikoff. All three accepted Ross's work and persuaded Lankester that Ross had really found the parasite of the 'million murdering death' inside the mosquito.[11] Later in 1899 Lankester suggesred that Ross prepare illustrations of the malaria parasite Plasmodium for the *Quarterly Journal.* This historic material appeared in July 1900, with a note by Lankester; one of the plates was subsequently reprinted in Ross's Memoirs.[12]

[8] See P.P. Grass, *Traité de zoologie* (Paris, 1953), I, p. 625 and p. 787.

[9] See Ronald Ross, *Memoirs, with a Full Account of the Malaria Problem and its Solution* (London, 1923), p.374.

[10] See Lankester, Preface to F.V. Theobald, *A Monograph of the Culicidae or Mosquitoes* (London, 1901).

[11] Ross, *Memoirs,* pp. 374-6. Ross used the phrase 'million-murdering death' in a poem written to commemorate his discovery, see *ibid.*, p. 226.

[12] Plates from R. Ross and R. Fielding-Ould, 'Diagrams Illustrating the Life-History of the Parasites of Malaria, with a Note by E. Ray Lankesrer' from the *Quarterly Journal of Microscopical Science* reprinted in Ross, *Memoirs,* between pp. 114 and 115.

Lankester, who was an inveterate destroyer of correspondence, preserved one letter from Ross, written from the Liverpool School of Tropical Medicine on 18 December 1902:

> My Dear Professor Ray Lankester,
>
> I was delighted to get your kind congratulations about the Nobel Prize, and also to learn that I am indebted to you for recommending me. The presentation was a grand affair, and the Swedish people treated us right royally. Prof. Retzius and his charming wife were particularly kind. Many of them told us that they were very glad an Englishman had got the thing in preference to a Dutchman. The prize consisted of a gold medal, a beautifully designed album, and £7,800 and a little over. There is no doubt that the Nobel bequest will work wonders for science. This sum of money is more useful to a man than a small professorship and so on. The advance of pathology has hitherto been so slow simply because it does not pay a doctor to take it up; but with such a good prize going every year, things will get better. The Nobel Committee considered the rival claim [by Grassi and others] regarding the mosquito theory very carefully, and the result agreed with Nurtall's decision. Hoping to see you soon.[13]

Just as Lankesrer's discovery of the haemogregarine links him with malaria research, so his discovery, or rather rediscovery, of another parasite links him with other researches in pathology. In 1871 he had described a spindle-shaped parasite in frog's blood under the name *Undulina*, presenting it as the type of a new group of 'infusoria'.[14] It turned out that this parasite had already been described in 1843 by Gruby, as *Trypanosoma rotatorum*. These haemoflagellates have great medical significance, since they include the parasites of the various sleeping sicknesses, Chaga's disease (which some authorities think Darwin may have contracted in South America), and the closely allied genus *Leishmania*, causative agent of oriental sore. Many species are found in the blood of vertebrates of almost every type, although for the most part they do not produce clinical symptoms in wild animals.

[13] Ross to Lankester, 18 December 1902 (Lankester family papers). G.H.F. Nuttall had supported Ross's claims against his rivals; see Ross, *Memoirs*, chap. XVIII.

[14] Lankester, 'Undulina, the Type of a new Group of Infusoria', *Quarterly Journal of Microscopical Science*, 11 (1871): 387-9.

Lankester subsequently developed a profound admiration for the work of David Bruce on trypanosome disease, describing it at length in his *Kingdom of Man*.[15] He believed that Bruce was not given sufficient credit for his work. For Lankester, Bruce was a man of genius who had traced the cause of Malta fever to a germ found in goats' milk, and whose extensive work in Africa had given us solid knowledge of the tsetse fly and trypanosome disease. With the cooperation of the Foreign Office, Lankesrer initiated a survey of the tsetse fly, resulting in the publication of the fine monograph on these deadly insects by E. E. Austin.[16] Bruce also did important work on tetanus and trench fever during the world war. A close friendship had grown up between the two men, Bruce visiting frequently at Lankester's house. A letter of 1917 gives some idea of Lankester's growing isolation at this later stage in his life:

> It is a pity I do not belong to the Savile, as then I would have an opportunity of meeting you sometimes. You have chucked the Athenaeum, and you very seldom go to the Royal Society nowadays, so that except for the Savile, you have become quite a hermit.[17]

In his more active years, Lankester worked hard for the creation of a permanent and well-endowed chair of protozoology in the University of London, rejecting various proposals for a temporary post.[18] He was very pleased when a chair was established, especially as the first holder was E. A. Minchin, who had held his old post at University College and was well known for his painstaking research on sponges. Lankester also praised his work on the Uganda tsetse fly.[19] Along with Chalmers Mitchell, Lankester persuaded Minchin to stand for election to the Royal Society, and supported his work on the tsetse fly to ensure his election. Unfortunately, Minchin died in 1915 at the age of only forty-nine.[20]

Lankester was by no means satisfied with the development of relations between science and government. In one of his *Daily Telegraph* articles he

[15] Lankester, *The Kingdom of Man* (London, 1907), pp. 159-91.

[16] E. E. Austen, *A Monograph of the Tsetse Flies – Genus Glossina*, (London, 1903).

[17] Sir David Bruce to Lankester, 3 March 1917 (Lankester family papers).

[18] Lankester to Sir Archibald Geikie, 16 May 1905 (Royal Society, London).

[19] Lankester to Sir David Gill, 19 January (? 1910) (Royal Society, London).

[20] Obituary of Minchin, *Journal of the Quekett Microscopical Club*, ser. 2, 17 (1913-15): 669-71.

pointed out that no official entomologist was employed by the Board of Agriculture in Great Britain, or by any local government body. The Colonial Office was trying to get this kind of work done for nothing by an unpaid 'entomological committee'. He went on:

> It is a serious necessity for the professional biologists to organise rhemselves as the engineers, chemists and medical men have done, and to refuse to be exploited any longer by governments ... It would be interesting ro see an unpaid committee of lawyers or engineers appointed to do serious work for the public service ... Even good work obtained by a government office withour proper payment is not valued by officials, and is in the end neglected and wasred.[21]

Besides his interest in economic biology, Lankester played a role in early efforts to conserve the natural environment. In May 1900 he was an official representarive at a conference of European powers for the protection of wild animals, held at the Foreign Office. He believed thar the advance of civilisation inevitably caused the disappearance of the larger wild animals. The question was whether it would be possible to create an African reserve in a position so isolated that it would nor be a source of disease and danger to the herdsmen and agriculturalists of adjacent rerritories.[22]

Curiously, it was just at this rime that European scientists were confronted by evidence that there were still new species waiting to be discovered in the African forests. Several examples can be cited, the mountain gorilla, the pigmy chimpanzee of the Ituri forest, the giant forest-hog, nor seen until 1904, and the Congo pea-fowl. But it was the discovery of the okapi which created most excitement and which brought Lankester considerable fame. Skins of the animal reached him in June 1901, and he began an extensive study along with Sclater and Boule. It was agreed that the okapi resembled most closely the extincr *Samotherium*; it was one of a family of Giraffidae found in late Tertiary times in Africa. Lankester considered that it should constitute a separate genus, and named it *Okapi johnstoni*, after Sir Harry Johnston, who had sent him the skins. The clinching anatomical feature deciding the close affinity of the okapi and the giraffe was rhe curious shape of the ourer tooth on each side of the

[21] Lankester, 'Prevention and Cure', *Daily Telegraph*, 22 April 1911.

[22] Lankester, *Diversions of a Naturalist* (London, 1915), pp. 21-22.

eight teeth in the front of the lower jaw. This had a broad, chisel-like crown divided into two by a deep vertical slit, unlike the tooth of any other hoofed animal.[23]

Great care was taken to mount the skins for public showing at the museum, but as yet no European had seen a live okapi. The Barnum and Bailey circus was in town, and Lankester brought some pigmies from the circus to view the new exhibit. With great delight he heard their shouts of 'okapi, okapi,' assuring him that the Museum's idea of the animal's appearance was correct.[24] The whole affair aroused intense public interest, and a *Punch* cartoon showed a portly and cheerful Lankester riding an okapi, with the caption 'A light Ray on darkest Africa'.[25] This was not the only time that Museum acquisitions brought Lankester this kind of fame. Even greater interest was aroused by the arrival in May, 1905 of a cast of the gigantic dinosaur *Diplodocus*. This was a gift of Sir Andrew Carnegie, who attended a lunch at Lankester's house at Thurloe Place following the much publicized official presentation.[26] *Punch* featured Lankester and the dinosaur in another cartoon.[27] The Yankified *Diplodocus* greers Lankester with the words: "Wal! If he ain't a daisy!! Quite'n interesting specimen of the British Professor! Carnegie'll just have to send a cast o' him over to the States right away."

[23] Lankester's most substantial account of the Okapi is his 'On Okapia, a New Genus of Giraffidae from Central Africa', *Transactions of the Zoological Society*, 16 (1902): 279-314. see also his *Monograph on the Okapi: Plates* (London, 1910).

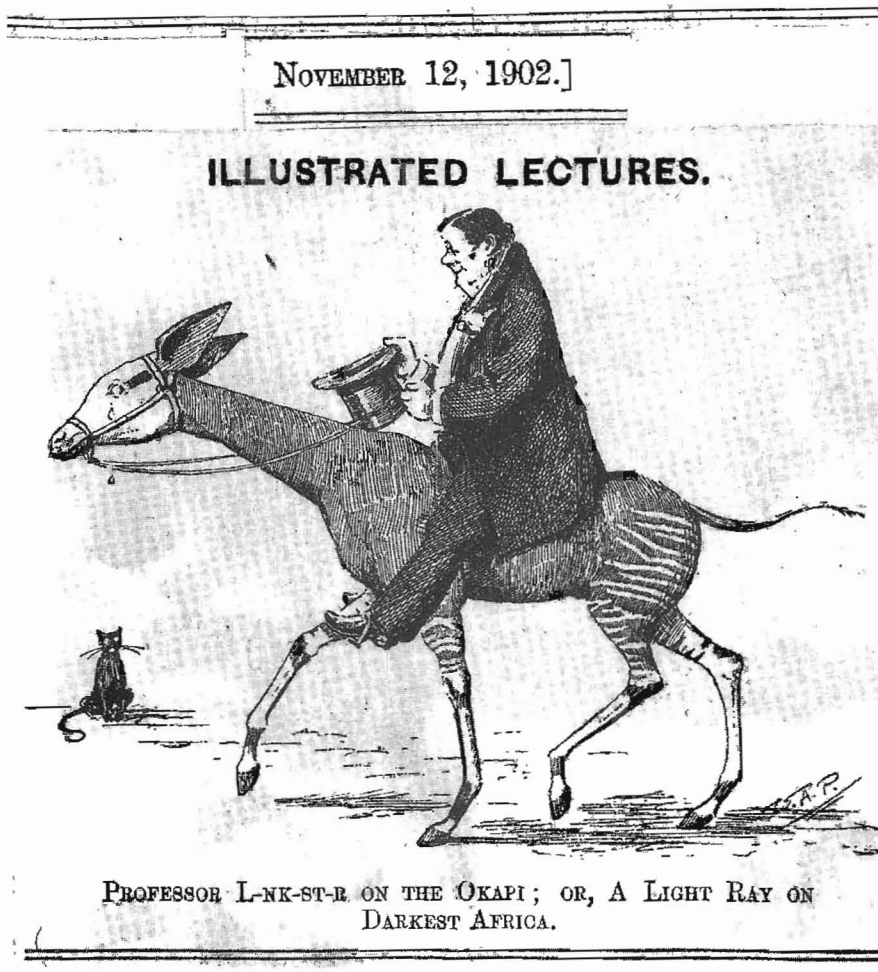
[24] A recollection by Lankesrer's niece, Mrs. N. Pain (J. Lester).

[25] *Punch*, 12 November 1902.

[26] A recollection of Susan Barrack, an old servant of the Lankester family (J. Lester).

[27] *Punch*, 24 May 1905.

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Cartoon published in *Punch* of Lankester riding on an okapi.

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From an Easy Chair: The Populariser

One of Lankester's close friends in later life noted that he liked to be asked questions about science. From his youth onwards, Lankester had adopted Huxley's dictum that the more rapidly truth was spread among men, the better it would be for them. There was a strong tradition of artisans who had taught themselves science and had even made original contributions, men such as the stonemason-turned-geologist Hugh Miller. The literate Victorian artisan was better able to appreciate the work of scientists such as Huxley and Lankester than the classics-trained man of the middle or upper classes. One of the reasons advanced by Acland for the necessity of fostering science at Oxford was that the clergy and upper classes needed it 'in order to retain their proper relations to the lower and middle classes who have this knowledge'.[1] When a worker wrote to a newspaper concerning the scientific aspects of his trade, Lankester thought it worthwhile to cut our his remarks and send them to Thiselton-Dyer, commenting: 'I am anxious to preserve the record of the observations of 'an intelligent workingman' on the colouring matters used in his trade'.[2]

Unlike his friend Milnes Marshall, Lankester did not play a role in the University Extension Movement. But he did a good deal of popular lecturing from the 1870s onwards. In 1873 he gave a series of lectures on Darwinism at Oxford, and these were repeated at the Royal Institution in 1875. He deputized for Rolleston, lecturing on 'Growth from the Egg'. At various dates he lectured on 'Rots and Ferments', 'The Simplest Living Things', and 'Germs – Unseen Enemies', describing the work of Cohn, Koch, Lister and Pasteur. He also gave courses at the Royal Institution at a more advanced level.[3]

[1] Brian Simon, *Studies in the History of Education, 1780-1870* (London, 1960), p. 307.

[2] Lankester to Thiselton-Dyer, 28 September 1883 (Thiselton-Dyer papers, Kew).

[3] These more substantial lectures include: 'Some Recent Biological Discoveries', four lectures (1889) and three lectures (1892); 'Thirty Years' Progress in Biology', four lectures (1895); 'The Simplest Living Things', eleven lectures (1898); 'The Mollusca', ten lectures (1899); and 'The Classification and Structure of Fishes', twelve lectures (1900) (compiled from information in the Lankester family papers).

Lankester insisted vigorously on the need to distinguish the creation of knowledge from the distribution of existing knowledge.[4] Men of science should not depend on their popular reputation for a livelihood. Public discussion and popular display displaced the solid work of investigation and led to a lowering of the tone of discussion, as in the Virchow-Haeckel controversy.[5] There was a belief that a scientist could make a living by writing popular books and articles, but:

> There is not a sufficiently large public interested in the progress of science to enable a researcher to gain an income by writing books, however great his literary facility ... He who becomes the popular exponent of scientific ideas, except in a very moderate and limited degree, must abandon the work of creating knowledge.[6]

When he wrote this in 1878, Lankester could not guess that circumstances would one day turn him into a distributor of knowledge, and that he would play an honourable part in building up a 'large public interested in the progress of science'.

In 1902 he discussed with his literary friend Edmund Gosse the possibility of writing a series of popular science articles.[7] Work at the Museum left him with little time for popular writing, with one notable exception. This was the Royal Institution Christmas lectures of 1903-04 on 'Extinct Animals'. The corrected shorthand report of these lectures for juveniles was issued as a book in 1905, with the lantern slides converted into process blocks.[8] This book helped to establish a new level of sophistication in scientific works for children, in format as well as content.

During the latter half of 1907 Lankester became depressed to an extent that worried his friends. Financially he was hard hit, 'kicked and robbed by the Trustees [of the Museum]'. It was now that his Savile Club friend Harry Lawson, editor of the *Daily Telegraph*, suggested that he write a weekly article for the paper. Lankester accepted thankfully, and the first article appeared on Saturday, October 5th 1907. It consisted of a few brief

[4] Lankester, 'Biology and the State', in Lankester, *The Advancement of Science* (London, 1890), 61-111, p. 89.

[5] Lankester, 'Popular Science', *The Times*, 5 September 1878.

[6] *Ibid.* See also Lankester's 'Biology and the State', in his *The Advancement of Science* (London, 1890), 63-117, pp. 91-92.

[7] Lankester to Edmund Gosse, 17 August 1902 (Brotherton Collection, Leeds University).

[8] Lankester, *Extinct Animals* (London, 1905).

notices modestly lodged in the final column of page fifteen. Lankester introduced his series as follows:

> I do not address my readers from the professor's chair, but from an easy chair. Just as in the club, or my friend's smoking room, I might talk of these things, so do I propose to talk here. My hope is that what I have to say will interest those who are not experts in science, and yet have a desire for trustworthy information and opinion on the vast variety of topics which come up day by day for consideration and discussion, and can only be explained or rightly understood by the aid of that systematised knowledge which is called science.

Lankester commences with a clear idea of the common faults of popularisers. They were the uninhibited use of technical terms, a lack of sympathy with the uninstructed reader, and the attempt to tell too much within the available limits of time or space. This last he considered most important. There was no harm in the use of the special jargon of science, but technical terms should be introduced gradually and in limited number. 'We do not expect cricketers or golfers to talk in plain English; why should we expect chemists and naturalists to do so?'[9] In this respect Lankester may be compared with his friend Grant Allen, who had written a series of articles in the *Strand Magazine* under the title 'In Nature's Workshops.'[10] Allen was also a devoted Darwinian, but his articles lack the variety and lightness of touch of Lankester's.

The snippets of information in the early 'Easy Chair' pieces soon developed into genuine articles each dealing with a single topic, all connected by one central idea: the theory of evolution. Very soon the articles were transferred to the centre of the Saturday page, under a more impressive heading. No illustrations of any kind appeared with these articles; the newly-invented half-tone process was reserved for pictures of royalty and dreadnoughts. The original articles must have reached a fairly wide circle of readers. The *Daily Telegraph* claimed a sale of 500,000 per week, more than any other penny paper, although it was not read by the masses. Lankester's articles reached middle class people of moderate education, or those with a good classical education. A whole generation of such readers received its idea of science from the 'Easy Chair'. The articles' influence penetrated even further when they were gathered into book form.

[9] Lankester, *From an Easy Chair* (London, 1908), p. 1.
[10] *Strand Magazine*, vol. 17 (1899).

Lankester disliked interference with his column. Lawson tried it once, as a placatory letter from him makes clear.[11] On one occasion a sub-editor decided to hold an 'Easy Chair' article over the weekend. Apparently the famous Lankester wrath burst forth in all its fury, if one can judge from a diplomatic and conciliatory letter from the editor.[12] After that, only very occasionally did the series stray from the Saturday page. This last letter contains an excellent characterization of Lankester as a populariser:

> It strikes me that you have done what very few men have been able to do, that is to say, made yourself easily intelligible to the man in the street, without taking anything from the high scientific value of your work.

From 1907 to 1914 Lankester wrote an article every week without intermission, and resumed the series for a few months in 1919. To these must be added slightly less popular articles in the *Field*, the *Illustrated London News* and *Country Life*.[13] The articles which were later reprinted in book form represent only a selection from approximately four hundred that he wrote for the *Daily Telegraph*. The range of these articles is very wide: botanical and zoological topics of all kinds, pathology, bacteriology, physiology, palaeontology, evolutionary theory, as well as excursions into physics and chemistry. Some articles deal with folklore and anthropology. His mentors in these matters were chiefly E. B. Tylor, and J. G. Frazer, whose *Golden Bough* he studied carefully and much admired. He also knew personally the brilliant Jane Harrison, who wrote on Greek religion and mythology. In their entirery Lankester's corpus of articles constitutes a large and comprehensive guide to biology and general science. In 1910 he contemplated writing a book to be called 'Needful Knowledge', a pocket reference book of scientific facts containing the basic knowledge which should be the equipment of every citizen. He wrote a brief sketch of it but did not develop the idea further.[14]

The articles are still readable today, although much of the factual material they contain is outdated. Lankester conveys to the reader his own passionate interest in natute. He respects his readers, laying no claim to

[11] H. Lawson to Lankester, 31 October 1907 (Lankester family papers).

[12] H. Lawson to Lankester, 11 December 1910 (Lankester family papers).

[13] Lankester wrote articles under the title 'Science and Leisure' in *Country Life* in 1914 and 1915, mainly on Crag fossils. His articles 'About a Number of Things' appeared in the *Illustrated London News* in 1919. There were occasional articles in other journals such as the *Sphere*.

[14] From a notebook opened in 1910 (Lankester family papers).

superior insight other than that conferred by application of the scientific method. He wishes to make science easy because he understands the terrible limitations of the ordinary person's life. The articles sparkle with originality and human interest; bright but not flippant, simple but not childish. He makes the life history of the curious annelid *Chaetogaster*, or his discovery of haemoglobin in the snail *Planorbis*, as graphic and readable as a novel. He was a great admirer of the 'incomparable Guy' [de Maupassant], and many of his essays possess something of the quality of a good short story. A solid kernel of ascertained fact mingles with a personal reminiscence, an anecdote or a description. The narrative often ends on a sustained note, a peroration which is impressive and sometimes moving, demonstrating what Gosse called his 'solid and supple' manner of writing English.

In his essay 'Far from the Madding Crowd', for instance, he describes the emotional effect of a film of the Australian aborigines, with a gramophone recording of their songs and speech.

> To hear is a greater means of revelation than to see. One feels even closer to those Australian natives as their strange words and songs issue from imprisonment from the phonograph, than when one sees them in the film pictures actually bearing time with feet and hands and imitating the movement of animals. To receive, as one sits in a London lecture room, the veritable appeal of these remote and inaccessible things to both the eye and the ear simultaneously, is indeed the most thrilling thing I can remember. With a feeling of awe, almost of terror, we recognise as we gaze at and listen to the records brought back by Mr Baldwin Spencer, that we are intruding into a vast and primitive nature reserve, where even humanity itself is still in the state of childhood, submissive to the great mother, without the desire to destroy her control or the power to substitute Man's handiwork for hers.[15]

Natural beauty also inspired Lankester to eloquence, as in his description of the Jungfrau.

> The vast mountain, with its dazzling vestments of eternal snow, and its slowly creeping, green-fingered glaciers, towers above into the cloudless sky. In an instant the mind travels from the microscopic details of organic beauty, which but a moment

[15] Lankester, *Diversions of a Naturalist* (London, 1915), p. 31.

> ago held it entranced, to the contemplation of the gigantic and elemental force whose tremendous work is even now going on close to where we stand ...[16]

Throughout the essays there runs a clear thread of propaganda. There is a definite philosophy at work, although it does not obtrude like the monism of Ernst Haeckel's widely successful *Riddle of the Universe.* Lankester states the case for objective rhinking, for rhe adequate financing of science, for practical benefits such as cheap and pure milk, and for nature reserves. He attacks the educational system, spiritualism, divining and all kinds of superstition. He tackles controversial topics such as eugenics, and few of his women readers would have been satisfied with his remarks on feminism. His motto is always *savoir, prévoir, pouvoir,* – to know, to foresee, to act. He does not believe a little knowledge to be a dangerous thing, provided it be sound as far as it goes. Increase of knowledge is not increase of sorrow; on the contrary, it provides the only possible escape from sorrow. It has been suggested that Lankester's articles were intended for entertainmenr,[17] but their underlying purpose is, in fact, profoundly serious. The form is dictated by the readership, which demanded that its science be sugar-coated.

Lankester's friends urged him to publish a selection of his popular writings in book form. The first step in this direction was *From an Easy Chair,* which appeared in 1908 as a slim volume without illustrations. It reproduced articles from the six months of October 1907 to April 1908. The title page reproduced Stevenson's couplet:

> The world is so full of a number of things,
> I am sure we should all be as happy as kings.

This volume is now quite rare, but Lankester soon began to reach out to a wider audience. The first series of *Science from an Easy Chair* appeared in April 1910, reprinting some of the articles from 1908 and 1909. This was well-illustrated and ran ro 54,000 copies.[18] A second seties in 1913 was followed by *Diversions of a Naturalist* in 1915. The first portion of this was reprinted in 1925 under the title *Some Diversions of a Naturalist.* The ninth edition of *Science from an Easy Chair* of 1916 was a special cheap

[16] Lankester, *Science from an Easy Chair* (London, 1912), p. 6. A picture of the Jungfrau forms the frontispiece to this volume.

[17] J. G. Crowther, *The Social Relations of Science* (revised edn., London, 1947).

[18] Information supplied to J. Lesrer by rhe publishers, Methuen.

edition, and this was still being reprinted as late as 1925. *More Science from an Easy Chair* came out in the same cheap format in 1920. *Secrets of Earth and Sea* was published in the same year, followed by *Great and Small Things* in 1923. Some of the best articles were reprinted in Methuen's *Modern Classics* in 1927 under the title *Essays of a Naturalist.* Finally, the Rationalist Press Association reprinted selections from the *Easy Chair* series revised by C. M. Beadnell in its *Thinkers' Library* series under the title *Fireside Science.* By 1925 about 88,000 copies of Lankester's works had been sold; the *Thinkers' Library* edition added 40,000, and the total sales of all the editions was probably not far short of 140,000. No other science writer of the time enjoyed the same level of success.

In a letter to Edward Clodd, Lankester wrote of his desire to see a sixpenny Rationalist Press Association edition of his *Kingdom of Man.*[19] This paperback appeared in the famous R.P.A. cheap reprint series in 1912. Lankester also took an interest in other scientists' efforts to reach a wider audience. He assisted W. Pycraft, introducing him to Clodd and writing the introduction to his *History of Birds* of 1912. In 1924 Lankester wrote an introduction to a little book by C. H. Murray, a young airman who had been killed in action in 1918. The biologist J. A. Thomson, who wrote a number of popular works, was a great admirer of Lankester. He wrote a brief account of Lankester's life and work in his *Great Biologists* of 1932.

Lankester realized that the Great War had created an increased demand for scientific knowledge. But only a few men of science knew how to state the facts of their speciality 'in plain English, with due sense of proportion and never-failing sympathy with the uninstructed but eager reader'.[20] The ordinary basis of supply and demand was not enough to create a self-sustaining movement for the popularization of science. What was needed, he believed, was a society or a pioneer publisher. To some extent, the mantle of Lankester fell on the shoulders of J. B. S. Haldane, whose popular articles in the *Daily Worker* were reprinted in book form by Pelican. Haldane certainly admired Huxley and Lankester,[21] although his own popular writings were aimed more directly at the masses.

[19] Lankester to Edward Clodd, 1907 or 1908 (Lankester family papers).

[20] Pencil notes, probably of 1919 (Lankester family papers).

[21] Charlotte Haldane, *Truth Will Out* (London, 1949), p. 21.

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The Place of Science in Society, and the Struggle for a Rational System of Education

Lankester once complained that the term 'science' was used to describe the skill of a boxer, the text of Aristotle's writings, the highest mathematics, the twaddle of Mrs. Eddy, and the elements of chemistry. He thought that the best term for a man of science would have been 'physiologist' (in the sense of Cicero's *physiologia natura ratio*, the reasoned story of nature), had this not become too closely associated with medicine.[1] He argued against the use of the term 'scientist', and was quite happy to call himself a 'naturalist', since this linked him to his father's friends.

Lankester also liked to call himself a 'rationalist'. He was annoyed by the attempt to divest the term 'humanism' of its historical meaning by restricting it to historical and classical studies. In 1916 he carried on a vigorous controversy on this latter topic with Professor J. J. Findlay.[2] In Lankester's opinion, 'humanism' should be used to denote 'the attitude or state of mind which is opposed to ecclesiastical or theological authority in intellectual matters'.

Whatever the terminology, Lankester had a clear conception of the role that science should play in society and in the intellectual life of mankind.

> Science is no handmaiden, but in reality the master – the master who must be obeyed. The sooner and more thoroughly the people recognise the fact – and insist upon its acceptance in practice by its representatives and governors – the better for them and their posterity.[3]

[1] Lankester, ' The Word 'Scientist' or its Substitute,' *Nature*, 114 (1924), p. 823.

[2] There are letters on this topic in *The Times* from Findlay, November 23 and December 7 1916 and from Lankester, November 16, November 30 and December 14 of the same year. Lankester produced a printed circular on the topic which he sent to his friends for signature. The copy surviving in the Lankester family papers has the signatures of William Hunt and Edmund Gosse.

[3] Lankester, *From an Easy Chair* (London, 1908), p. 29.

Throughout his life he campaigned against the forces that resisted the spread of science and of rational thought. The nation must take more account of its scientists and offer more support for scientific research. In addition, the education system must be reformed so that science became an accepted part of everyone's way of thought.

How was this enormous expansion of scientific research to be financed and organized? In 1883 Lankester delivered an address on 'Biology and the State' to the Southport meeting of the British Association for the Advancement of Science. This was a vigorous denunciation of government indifference, and a scathing comparison with government support for science in Germany. There were only thirty-eight salaried positions for scientists in Britain, while Germany – admittedly with nearly twice the number of inhabitants – had three hundred. There were a hundred publicly-funded laboratories for the study of biology in Germany.[4] The British left science to men of independent means, or expected them to support themselves by full-time teaching or by writing. Toward the end of his address, Lankester painted a picture of a world in which science was given its full due.

> Through [science] we believe that man will be saved from misery and degradation, not merely acquiring new material powers, but learning to use and to guide his life with understanding. Through Science he will be freed from the fetters of superstition; through faith in Science he will acquire a new and enduring delight in the exercise of his capacities; he will gain a zest and interest in life such as the present phase of culture fails to supply.[5]

Those who tried to resist the advance of science were blocking the path to greater human happiness.

At the time of his 1883 address, Lankester was already promoting what would become the Marine Biological Association (see chapter 9). It is clear that he saw a marine biological station as but part of a much wider scheme for the setting up of biological laboratories. He talked of forty new institutes, which would cost £160,000 to build and £60,000 annually to run.[6] In 1884 he wrote to Thiselton-Dyer outlining a grand design for a

[4] Lankester, 'Biology and the State', reprinted in Lankester, *The Advancement of Science* (London, 1890), pp. 63-117, see p. 82.

[5] *Ibid.*, pp. 108-09.

[6] *Ibid.*, p. 94.

series of biological laboratories, enclosing a memorandum on the topic to be circulated to the Council of the Linnean Society.[7] Evidently, he did not expect the government to shoulder its responsibilities by footing the bill. He proposed setting up a 'highly responsible money box' into which the public could cast their contributions. The fund would be administered by trustees who would organize lectures and appeals with the aim of acquiring donations and annual subscriptions. The aim would be to raise £100,000 in five years. Needless to say, nothing came of this scheme.

There had, in fact, been a very mixed response ro the 'Biology and the State' address in the press. The usual vetdict was that Lankester was a 'Tory democrat' or a 'State socialist'.[8] Lankester wrote a response in the *Pall Mall Gazette* outlining once again the scheme he envisaged. 'What we need is not occasional patronage ... We do not ask for more generals, but for an army'.[9] It was useless to argue that the state could not undertake the task. Experience had greatly modified conceptions of what the state could undertake in the interests of the community. For Lankester, as for many other scientists, the age of *laissez-faire* was over; progress could only come about if the state began to take its responsibilities seriously.

Lankester was given another opportuniry to exptess his views when he was asked to give the Romanes Lecture at Oxford in 1905. He chose as his topic the Baconian theme 'Nature and Man'. The discourse was delivered in the Sheldonian Theatre before a fashionable and distinguished gathering. Though diplomatic and persuasive in rone, the address conrained a scarcely-veiled attack on Oxford that could hardly have pleased his audience. Lankester had spent much time in preparing his speech, which was in a sense a vindication of his life's work. His preparatory notes have survived, and are worth quoting at length to give an impression of his real attitudes.

> Why have science? Because it is rhe saviour of mankind. It is the one hope – Religion is no use, tradition, wealth and strength are no use ... [There are] false prophets of science and false teachers of religion. Those who offer wealth and power as a bribe to induce national culture of science are false advocates. Wealth and power are of no value as compared with the gifts really coming with science, viz. truth and undersranding ... Oppose the mercenary view. Probably would pay England best or quire well to

[7] Lankester to Thiselton-Dyer, 28 January 1884 (Thiselton-Dyer papers, Kew).
[8] *Ibid.*
[9] Lankester, 'The Endowment of Science', *Pall Mall Gazette*, 38 (5 October 1883).

> go on getting her science from abroad. Division of labour – you do the science and we will do the bullying. We can always import Germans and even Americans ... Pay a German to sell his discovery ro you ... Science is not the golden key by which rreasure and luxury are opened to capiralists, and ease and plenty rendered widespread among the masses. This is the accidental and often injurious application of science. Science is the increase of understanding, the essential condition for rational philosophy and the conducr of the communiry ...
>
> The science of the nineteenth century dispelled superstition and above all established 'free thoughr', a great and necessary step. Now for the next thing. It is not, as some seem to think, the mere utilisation of Nature for commerce and manufacture, and it is not national competition and triumph, but the happiness of many, of the whole human race. Guard here against the charge made against Bacon and other improvers of man's estate. We do not mistake the end and purpose of human life. It is not an end and object to be merely triumphant ovet the ills of nature. Healthy, well-fed, well-grown humanity with nothing else is no end, no object. The thought of man is greater than his life – noble deeds, love, art, greatness of soul, these are above mere triumphs over the pains and discords of man versus nature. But the conquest of nature is the necessary step.
>
> It is only rhe rich who create an artificial environment of pseudo-conquest of nature, at an enormous expense, who can indulge in the amusements and delights of art and history. Mankind as a whole can't do it, and won't put up with the people who only aim at that.[10]

The printed text of the address makes the same points, if a little more diplomatically.

> Within the last few years, an attempt to sput the will of Englishmen in this direction has been made by some who have represented that this way lies gteat fortunes, national ascendancy, imperial domination ... I speak for those who would urge the conscious and deliberate assumption of his Kingdom by Man, not as a matter of markets and of increased opportunity for the cosmopolitan dealers in finance, but as an absolute duty, the

[10] Preparatory notes in a small notebook (Lankester family papers).

> fulfilment of Man's destiny, a necessity the incidence of which can only be deferred and not avoided.[11]

The version of the lecture printed in Lankester's *The Kingdom of Man* of 1907 became very popular with socialists and progressive thinkers. It was quoted at length, for instance, in Upton Sinclair's *The Profits of Religion.*[12]

Press reception of 'Nature and Man' was cool. *The Times* of 15th June 1905 devoted a leading article to the address, and was not supportive. Lankester had described man as 'Nature's insurgent son', but *The Times* called Lankester himself 'Oxford's insurgent son'. He was nothing if not aggressive, and seemed to care little for the gentler arts of persuasion and reconciliation. 'If we taught them science as badly as we teach them most other classics, and taught them as little of it, should we be any better off?' The leader reduced the whole matter to 'science versus Greek' and showed no appreciation of Lankester's plan for the proper integration of science into the social fabric. On 17th June G. J. Romanes' widow wrote to *The Times* criticizing Lankester for attacking 'all studies save his own', and dissociating the founder of the lecture series from such views. Lankester strongly defended himself against these claims in letters of 16th and 19th June.

In 1912 Lankester renewed his propagandist efforts to improve the financing of science. He contributed an article to a symposium edited by his friend H. G. Wells entitled *The Great State.* Lankester was not sanguine. Having spent nearly fifty years and the best part of his energies in fighting for the better organization of science, he was disappointed with the result. The present British governing class, whether they labelled themselves with one political name or another, could not understand the point of expenditure for the sole purpose of creating new knowledge. Nor did he see much hope in democracy:

> I must leave it to others among my fellow essayists to suggest how or when the toiling millions of the British Empire or of all civilised Europe, Asia and America together are going to arrive at, first of all, an understanding of what the 'progress of science' really means, ... and secondly, at such a desire for that progress, as will lead them to sanction the annual expenditure out of public

[11] Lankester, 'Nature's Insurgent Son' in Lankester, *The Kingdom of Man* (London, 1907), pp. 1-61, see p. 31.

[12] See Upton Sinclair, *The Profits of Religion* (London, 1927), pp. 306-07.

> resources, on the making of new knowledge, of as large a sum as we now spend annually on the army and navy.[13]

Lankestet then gave an outline plan for the state's role in the organization of science. The unit was to be the professor of genius, surrounded by his pupils and disciples. The net must be cast as widely as possible, without distinction of class or sex, since the special mental qualities of the discoverer are rare. Likely young people would proceed to State Research Institutions as probationers. Each institute would have its director, the most capable and active discoverer in his own sphere. Lankester gave Hofmann as an example of his ideal type ('his inrellectual heritage [is] still with us'), along with Rayleigh, Maxwell, Stokes, Kelvin and J. J. Thomson. Each director would have a suitable staff of assistants, all paid a reasonable salary. Publication would be by the state. Lankester wished to avoid the doling out of inadequate scholarships. Young people must have the opportunity of becoming makers of new knowledge as long as they have shown their capability.

Then came the Great War, and everything that Lankester had been preaching for years was shown to be true. The country suffered immensely, not only from the general anti-scientific mentality of the upper classes, but specifically from the lack of an adequate scientific cadre. A young scientist, eager to place his knowledge at the service of the government, was registed for exemption from national service by being classified as a labourer, because there was no category of scientist or research worker. Lankester rubbed home this lesson in a series of powerful letters to *The Times*.[14] It was now, he wrote, a matter of life and death that Britain should change its attitude to science. There should be not merely better knowledge for the few, but also a widely spread knowledge in all classes: manufacturers, workers, civil servants, and members of the legislature. This would involve a complete revolution in the system of education.

> A terrible responsibility rests upon those who, owing to sheer ignorance or misapprehension, or to fatal tenderness to vested interests, may endeavour to prevent or delay the drastic reform

[13] Lankester, 'The Making of New Knowledge', in F. E. Greville (Lady Warwick), H. G. Wells and C. R. S. Taylor, eds., *The Great State: Essays in Construction by H. G. Wells* ... (London, 1912), pp. 123-39, see pp. 127-8.

[14] Lankester, ' The Neglect of Science', in *The Times*, 14 January 1916. See also his 'Science and Empire', *ibid.*, 30 March 1916. There is a draft of an unpublished 'Easy Chair' article on this topic in the Lankester family papers.

> which alone can save the nation's life ... In science as in war, small professional armies are no longer adequate. The scientific knowledge of a few experts can no longer guarantee the intelligent and progressive development of our industries, or keep us abreast of our rivals in manufacture and defence.

Lankester had long been opposed to the 'classical system in education', and had argued for compulsory science instead of compulsory Latin and Greek. Now the need for a reformed system of education became part of the war effort. In 1916 he became chairman of a newly-formed 'Committee on the Neglect of Science'. On February 2nd 1916 a memorandum of this committee, composed by Lankester, appeared in *The Times*. Copies were also sent to individuals and organisations likely to be sympathetic. The covering letter asked societies to appoint representatives to attend a meeting to be held in the Linnean Society's rooms on May 3rd. The meeting took place, with Lord Rayleigh presiding. Two hundred were present, and two resolutions were passed, demanding that natural science be made an integral part of education, and calling for alterations in the Army and Civil Service examinations.[15] The speeches made at this meeting were published as a pamphlet, and ten thousand copies were distribured. An interview was obtained with Lord Crewe, a member of the government and chairman of the governors of the Imperial College of Science and Technology, where the professors had signed the original manifesto *en masse*. Discussions were also begun with the Civil Service Commissioners. Two government committees did give some tiny concessions, but Lankester complained that there was no real break in the classical monopoly of school education.

Lankester's calls for the practical application of science seem at variance with the loftier ideals he had expressed earlier. He overcame this contradiction by asserting that science develops through a series of stages, from its cultivation by a few isolated individuals to its eventual use by and for society as a whole. This view is expressed most clearly in the preface he wrote to the symposium, *Natural Science and the Classical System of Education*, organized for the Committee on the Neglect of Science in 1917.

> Some of those who are devoted workers in one or another field of science ... desire to see knowledge and the discovery of

[15] See Lankester to H. G. Wells, 20 March 1916 (H. G. Wells papers, University of Illinois). See also the preface to Lankester, ed., *Natural Science and the Classical System of Education: Essays Old and New* (London, 1918).

> truth pursued for their own sake. One may, it seems to me, agree with such a view of the ultimate claims and requirements of science, and yet hold that when this view has failed to gain attention, ... and that the very existence of the spirit, methods and results of science is unknown not merely to the masses, but to the highly educated leaders of the commonwealth, it is right to compel attention to the nature and power of science by an appeal to patriotism, the almost universal desire for national prosperity and national defence ... Patriotism is indeed very generally esteemed as a high virtue, necessary in the present phase of human development, although it may hereafter be replaced by universal philanthropy. Science must stoop to conquer. Its cultivation and diffusion, enforced by patriotic and even purely commercial motives, will ensure respect for, and some understanding of, its significance. Then, and not until then, we may hope for the permanent enthronement of the duty of 'thinking truly' ... Soon, for very many, as now only for a few, the pursuit of science will become part of the moral law.[16]

The same point was made in some notes on industrial research written about 1920:

> In order to assist in the development of the higher aims of science, we must help the merely money-making cultivation of science ... The appeal to lower nature and material inducement must precede the ideal ... Say then as to manufacturers' science; separate labs yet close allies. Great industries to promote their own laboratories of research, self-supporting.[17]

Lankester's essay in the volume on *Natural Science and the Classical System in Education* went on to outline his views on the need for educational reform in the current situation. He summed up his scheme as follows:

> The cultivation of the aesthetic faculty, the knowledge and love of literature, the gift of thinking truly, the effecrive preparation for 'vocarional training', the access to the career of a

[16] Preface to Lankester, ed., *Natural Science and the Classical System in Education* (London, 1918).

[17] Notes in Lankester family papers.

> discoverer of new knowledge and ... the acquirement of a philosophy – a scheme of the relations of man with nature and of existence itself – these are the aims of school and college education.[18]

Lankester took as his motto Kant's slogan of 1774: 'Not slow reform, but swift revolution is needed in our schools'. His essay was written in his usual brisk and direct style, showing no sign of wartime strain. The education of the middle and upper classes, he declared, was in the hands of monopolists who peddled the 'classical system' in which three quarters of school time was devoted to drill in Latin and Greek, and the translation and memorizing of scraps of Latin and Greek authors. He quoted Lord Houghton's dictum that this was the distinguishing mark of the aristocracy, now filtered down to the middle classes. The more important educational endowments had been appropriated by the richer classes. (Lankester was particularly angry at the way in which Sir Thomas Gresham's notable benefactions had been appropriated, with parliamentary consent, by the London companies.) Political parties had systematically neglected this great instrument of national development.

Lankester commented on the inadequacy of popular education. A timid start had been made in educating the masses, but it had been hampered by the crude and inept ideas of the organizers. Money was stinted and:

> a harmful confusion of the activity of the state on the one hand as controller of religion, and on the other as provider and director of the intellectual education necessary for the health and prosperity of the community, has survived from the earlier conditions when the two were inseparable ...
>
> A nominal sum is spent on the teaching of rudiments to the poorest class, while the public endowments of higher education are deliberately assigned to be dealt with capriciously and without any considered or approved purpose by irresponsible and uncontrolled corporations.
>
> Meanwhile the immense power of education in developing the character, capacities and prosperity of the nation when wisely applied, is neglected, and for all that our statesmen care, this gigantic source of strength and happiness might be non-existent.[19]

[18] Lankester, 'The Aim of Education', in *Natural Science and the Classical System in Education* (note 16), pp. 250-68, see p. 263.

[19] *Ibid.*, p. 257.

Lankester wrote to H. G. Wells arguing that science was not just a matter of new dyes and processes, but a philosophy and outlook on life based on an understanding of nature and a belief in the reasoning process.[20] Boys were being trained as slaves of authority and tradition, looking backwards rather than forwards. The classical system was, for Lankester, not only a means of continuing the neglect of science, but also a system designed to ensure the domination of religion, class privilege, and an outmoded disciplinary system. He wanted a secular, egalitarian, humanistic system of education firmly based on knowledge of nature. He was always careful to point out that the sweeping away of the classical system would not interfere with the serious study of classical literature and archaeology. On the contrary, he argued, it would assist its development.[21]

Lankester was not satisfied with the work of the Committee on the Neglect of Science, although its efforts were not entirely in vain. In 1918 he gave evidence on behalf of the committee before the Royal Commission on Science and Education. In 1920 the newly-created government Department of Scientific and Industrial Research began intensive propaganda to persuade industrial firms to set up research laboratories. By this time, however, Lankester had resigned the chairmanship of the Committee on the grounds of ill-health. As he wrote to Wells, the committee met occasionally thereafter, changed its name to 'League', quarrelled, and soon virtually ceased to exist.[22] Perhaps it had served its purpose.

[20] Lankester to H. G. Wells, 27 June 1917 (H. G. Wells, papers, University of Illinois).

[21] Introduction to Lankester, ed., *Science and Education: Lectures at the Royal Institution by Faraday, Tyndall and Others* (London, 1918), p. 9.

[22] Lankester to H. G. Wells, 16 August 1922 (H. G. Wells papers, University of Illinois).

- 15 -
Nature and Man

Lankester was in Leipzig when the *Descent of Man* appeared in 1871, and Darwin sent him a copy. He found the book 'most entrancing' and sent Darwin a photograph of his friend Dr Nitsche's ears, which had a pronounced 'Darwin's point'.[1] In his early work *Comparative Longevity* he gave a sketch of the history of mankind, concluding with the remark that in human society, Darwin's law of the survival of the fittest operated among the varied social groups, not through the struggles of the isolated individuals composing them.[2]

More generally, Lankester was at first influenced by the progressionist social philosophy of Herbert Spencer.[3] Progress was inevitable, like the development of the individual. Humanity itself was perfectible, but in the meantime, evil resulted from the non-adaptation of the human organism to its conditions of life in society. Civilised man retained characters suited to his earlier, predatory lifestyle. As Lankester's view matured, he moved to a conception of society less constrained by biological analogies. Progress, he now believed, was real, but not automatic, and can only be gauged against the whole course of mankind's development from the animal. Finally, he came to realize that human progress depends upon our productive capacity, and on the formation of a fabricated cultural heritage.

In his *Degeneration: A Chapter in Darwinism* of 1880, Lankester argued that progressive evolution resulted from the response of individual organisms to the challenge of their environment. Degeneration occurred in those species which took up a less stimulating way of life. He explicitly compared this with the degeneration which had overtaken Rome after she had conquered the known world. He also warned against the threat of degeneration in modern civilization, but hoped that science itself would provide a constant stimulus to the human mind.[4] Fear of degeneration

[1] Lankester to his sister Fay, 19 March 1871 (Lankester family papers).

[2] Lankester, *Comparative Longevity* (London, 1870), p. 90.

[3] On Spencer see J. D. Y. Peel, *Herbert Spencer; The Evolution of a Sociologist* (London, 1971).

[4] Lankester, *Degeneration: a Chapter in Darwinism* (London, 1880), p. 33 and pp. 58-62. This is reprinted in Lankester, *The Advancement of Science* (London, 1890), see p. 27 and pp. 46-50. On the impact of Lankester's ideas about social degeneration →

would become a prominent theme of late-nineteenth-century intellectual life, but Lankester remained convinced that progress, although not uniform, would be inevitable in the long run.

> Arrest and decay are not the rule in human history. This is but the superficial view of the historian who limits his vision to the last four or five thousand years of man's development. It is not confirmed when we trace man from the flint-chippers of 500,000 years ago to the present day. We must be prepared for, and accept without desponding, the ups and downs as well as the triumphs, which inevitably characterise the natural process of evolution.[5]

If progress has been inevitable, how has it been accomplished? Lankester came to believe that the views of Spencer and Darwin must be re-examined.[6] Natural selection suffices to explain the origin of the human race's bodily structure, but cannot explain our subsequent history. In an article written for the Jubilee Volume of the Paris Société de Biologie in 1899 he discussed the significance of the increasing brain-size in Tertiary mammals.[7] The Titanotheria of the Eocene posseessed only a fifth of the brain-size of a modern rhinoceros. The product of this increase in brain-size, especially in mankind, was an increased plasticity of behaviour. Inherited instincts became less important than 'educability'. This in turn became the basis of our specifically human heritage. Educability can be inherited, but the results of education have to be acquired afresh in each generation. Mankind can be distinguished from the animals by our economic activity and our extra-corporeal heritage of tradition preserved in speech and writing.

Lankester thus was able to demarcate the boundaries of biology and sociology in a way that transcended the problems encountered by an earlier generation. Huxley had eventually come to see the essential

on writers such as H. G. Wells, see Peter J. Bowler, 'Holding Your Head Up High: Degeneration and Orthogenesis in Theories of Human Evolution', in J. R. Moore, ed., *History, Humanity and Evolution: Essays for John C. Greene* (Cambridge, 1989), pp. 329-53.

[5] Lankester, preface to *Secrets of Earth and Sea* (London, 1920).

[6] Lankester, 'Progress', in *Great and Small Things* (London, 1923), pp. 74-82, see p. 79. This was originally a review of J. B. Bury's *The Idea of Progress*, *Nature*, 105 (1920): 733-44.

[7] Lankester, 'The Significance of the Increased Size of the Cerebrum in Recent as Compared with Extinct Mammalia', reprinted from the Jubilee Volume of the Paris Société de Biologie, *Nature*, 61 (1900): 624-5.

characters of mankind as something standing apart from the natural process of evolution.[8] Wallace too had assumed that many human characters could not have been produced by natural selection, and argued openly for supernatural intervention. Criticizing Wallace's *Darwinism*, Lankester wrote:

> We do not need to fall back on metaphysical assumptions in tracing the development of man's intellectual faculties. Does the luxurious development of some professor's mathematical faculty, as compared with the poor numerical conceptions of an Australian black, offer any really greater difficulty of transformation than do the nine-foot long tail feathers of some Japanese barndoor fowls, as compared with the shorter feathers of other varieties?[9]

Lankester recognized both the separation of mankind from nature and our unity with nature, achieving a synthesis epitomized in his description of the human race as 'Nature's Insurgent Son.'[10] In his presidential address to the British Association in 1906 he stressed as an important advance the realization that the human mind was a social product which could only be understood in terms of its interaction with the social environment.[11]

In stressing the relative autonomy of the social sciences, Lankester was abreast of the reaction against nineteenth-century 'social Darwinism.'[12] But the effort to see the evolutionary process as the determinant of human behaviour was increasingly moving to stress the competition between

[8] See Huxley's 1893 Romanes Lecture 'Evolution and Ethics', reprinted in his *Collected Essays*, vol. 9, *Evolution and Ethics* (London, 1894).

[9] Lankester, review of A. R. Wallace's *Darwinism*, *Nature*, 40 (1889): 566-70, see pp. 569-70. On Wallace's spiritualism and its effect on his ideas on human origins see Malcolm Kottler, 'Alfred Russel Wallace, the Origin of Man, and Spiritualism', *Isis*, 65 (1974): 145-92.

[10] The title given to his Romanes Lecture when reprinted in his *The Kingdom of Man* (London, 1907).

[11] Lankester, President's Address, *Report of the British Association for the Advancement of Science*, 1906 meeting, pp. 3-42, see pp. 23-26.

[12] On the reaction against biological models of culture in the early twentieth century see Hamilton Cravens, *The Triumph of Evolution: American Scientists and the Heredity-Environment Controversy* (Philadelphia, 1978). On social Darwinism the classic study is Richard Hofstadter, *Social Darwinism in American Thought* (rev. edn., New York, 1959). This includes a discussion of the transition to an imperialist version of the theory, although many aspects of this latter version can be traced to non-Darwinian modes of thought, see Peter J. Bowler, *The Invention of Progress* (Oxford, 1989), chapter 4.

groups, rather than the individual struggles postulated by Spencer and the apologists for *laissez-faire*. Lankester's friend Chalmers Mitchell explained the international antagonisms of the day as a struggle of races, although he later retracted this view.[13] Lankester rebutted this kind of social Darwinism in a review of Karl Pearson's *National Life from the Standpoint of Science*. Pearson preferred the metaphor of the crucible to that of the pruning hook and wrote of the struggle for existence among nations by war as the 'fiery crucible out of which comes finer metal'. Lankester retorted that Pearson's views:

> seem rather to originate in a hasty attempt to generalize from certain preliminary results of biology, in regard to an enormous unexplored field of human phenomena ... It has yet to be shown that there is any strict analogy between the struggle for existence of the countless individuals in each generation of a non-social species, and the competition of great races or nations of mankind ... In these later days, the struggle is for the supremacy of a special form of political organisation, and not for the extinction or survival of the breed.
>
> Moreover, the new and strangely significant factor of oral and written tradition is operative in civilised communities, so that a race, though dead or merged in other races, yet continues to speak in the works of its greatest men.[14]

The production of talented individuals, he argued, is hindered only by the diversion of natural resources to aggressive struggle. In conclusion he proclaimed: 'I do not despair of humanity. I see no reason to suppose that progress is impossible in the absence of bloodshed and international hostility.'

In his Romanes lecture of 1905 Lankester outlined his views on the origin of mankind. His views are summed up in the preparatory notes which have survived:

> Nature is a vast set of rules. Animals and plants don't know the rules; they are killed by the millions in consequence, and only those who happen to obey or fit into the rules survive, and so they become at an enormous expense very close fitted, 'beautifully

[13] See Peter Chalmers Mitchell, *My Fill of Days* (London, 1937), p. 64.

[14] Lankester, review of Karl Pearson's *National Life from the Standpoint of Science*, *Nature*, 63 (1900-01) supplement, p. iii.

> adapted', as we say, to the tules. But man, as soon as he became man, found out some of the rules, and has ever since handed on by speech and then by writing, his accumulated knowledge, by means of which he has managed to evade the death penalty, and the mechanical shaping to the rules. He has learned to set one rule of nature to counteract another, and that to an enormous extent. But what he has done is nothing to what he can do. It has brought him to a very dangerous pass, and has created what he calls pain and unhappiness unknown before. The fall of man and the ascent of man are identical. By further knowledge of the rules, and proper use of his knowledge, man can get rid of pain and unhappiness.[15]

He continued this theme in the version of the text printed under the title 'Nature's Insurgent Son.' Here he declared that mankind is a new departure in the scheme of things. The will or volition of man has made him an *imperium in imperio,* which has modified the face of the planet.

> If we may, for the purpose of analysis, extract Man from the rest of Nature of which he is truly a product and a part, then we may say that man is Nature's rebel ... According to the law previously in universal operation, man should have been limited in geographical area, killed by extremes of cold or of heat, subject to starvation ... and should have been unable to increase and multiply ... without losing his specific structure and acquiring new physical characters ... But Man's wits and his will have enabled him to cross rivers and oceans, to clothe himself against cold, to shelter from heat and rain, to prepare an endless variety of food by fire, and to increase and multiply ... without submitting to the terrible axe of selection. And as he has more and more obtained this control over his surroundings, he has expanded that unconscious protective attitude towards his immature offspring which natural selection had already favoured and established in the animal race, into a conscious and larger love for his tribe, his race, his nationality and his kind ... He has formed communities, built cities and set up empires. At every step of his progress, Man has receded further and further from the ancient rule exercised by Nature ...

[15] Lankester, preparatory notes to his Romanes Lecture 'Nature and Man' (Lankester family papers).

> In spite of frequent assertions to the contrary, it seems that neithet the more ancient wars of mankind for conquest and migration nor the present and future wars for commercial privilege have any real equivalent to the simple removal by death of the unfit and the survival and reproduction of the fit, which we know as 'natural selection'. The standard raised by rebel man is nor that of fitness to the conditions preferred by extra-human nature, but is one of an ideal comfort, prosperity and conscious joy in life – imposed by the will of man, and in important respects, involving a control and subversion of what were nature's methods of dealing with life, before she had produced her insurgent son.[16]

In a later discussion of progress he argued that when mankind took control of its own future, 'Progress will cease, to a large extent, to be a blindly attained outcome of natural selection; it will acquire new characteristics as the conscious purpose of rational man'.[17]

The population question certainly worried Lankester. In his notes for the Romanes lecture he referred to the human race's tendency to outstrip nature's limits and added: 'This cannot go on; man must come to a limit. Then the real science will come in'.[18] In a letter to H. G. Wells he observed that the increase in population was threatening to turn the world into a sort of formicarium or acaricarium, like an old cheese full of mites. Wells's proposed world republic would have to contol population. Even atomic motors and the reclamation of deserts would not be sufficient.[19]

At the same time, Lankester insisted that control of the population must be based on certain and indisputable knowledge, and it was this which brought him into conflict with the Eugenics movement. This had been founded by Darwin's cousin, Francis Galton, to argue for the purification of the race by controlling the breeding of the least fit individuals. Under Galton and his disciple, Karl Pearson, an active campaign for legislation to restrict the breeding of the feebleminded was underway.[20] Lankester's friend Donkin was a member of the Eugenics

[16] Lankester, 'Nature's Insurgent Son', in his *The Kingdom of Man* (London, 1907), pp. 26-8.

[17] Lankester, 'Progress' (note 5), p. 82.

[18] Preparatory notes for the Romanes Lecture (Lankester family papers).

[19] Lankester to H. G. Wells, 28 August 1909 (Wells Papers, Universiry of Illinois).

[20] On the eugenics movemenr see G. R. Searle, *Eugenics and Politics in Britain, 1900-1914* (Leiden, 1976); Donald Mackenzie, *Statistics in Britain, 1865-1930* (Edinburgh, 1982), and Daniel Kevles, *In the Name of Eugenics: Genetics and the Uses of Human Heredity* (New York, 1985).

Society. But Lankester himself remained adamant that before any general prohibition of reproduction could be enforced, even against persons with hereditary tendencies to certain diseases, a vast amount of additional knowledge was needed. He took the Eugenics movement to task in 'Easy Chair' articles published in 1908 and 1911.[21] What do we mean by 'racial quality' and 'improvement'? he asked. The supporters of Eugenics had not defined their terms, while the biometrical studies of Pearson and others confused inborn characters with those due to education. They had based conclusions as to the existence of a law of hereditary transmission on statistics concerning the frequency of characters reproduced by imitation and education. If the cessation of selection led to racial degeneration, then it was in the richer sections of the community where the effects would be most obvious, not in the 'half-starved, struggling poor'.

In 1908 Lankester engaged in a controversy with H. M. Hyndman over the question of the condition of the masses of humanity.[22] Hyndman was the leader of the Social Democratic party, whom Lankester had met years before at the house of Karl Marx. Hyndman wrote of the 'physical degeneration' of the masses as the result of poverty. Lankester answered that the socialists must be careful of their facts. There was widespread suffering, but this was not the same as the physical degeneration of the race. For Lankester, the theory of a separate, uncontaminated germ plasm, unaffected by a vile environment, was a source of hope for humanity.

Lankester's critics often accused him of neglecting the importance of historical and humanistic studies. It is true that he despised much of what passed for history at the time. He believed that what was needed was a serious study of the races and nations of mankind, as a contribution to knowledge of human evolutionary origins. When this was further developed as a distinct discipline of anthropology, it would generate results of great value. Nor was he without interest in specific historical aspects of cultural development. A letter to Edward Clodd discussed the nature of the ox-harness of ancient times, a topic later taken up by V. Gordon Childe, who showed how the ancient harness had been inappropriately applied to horses until the horse collar had been invented

[21] Lankester's 'Easy Chair' articles in the *Daily Telegraph*, 22 January 1908 and 30 October 1911.

[22] H. M. Hyndman's 'The Socialist Victory' appeared in *The Times*, 24 January 1908; Lankester's reply, 'On Physical Degeneration and Socialism', on 27 January. Hyndman responded on 29 January, with a further reply from Lankester on 3 February.

in the so-called Dark Ages.[23] Lankester also called for the establishment of genuine anthtopological museums.

At the end of World War I Lankester learned that H. G. Wells was planning a history of mankind for the general reader in the hope of throwing light on the post-war chaos. Wells approached his task with great misgivings, and did not anticipate how successful his venture would be. The *Outline of History* appeared originally in fortnightly parts, and as the title page informs us, was written with the advice and editorial help of Ernest Barker, Sir H. H. Johnston, Sir E. Ray Lankester, and Professor Gilbert Murray. Lankester gave most of his advice in the fotm of long letters, dealing chiefly with the difficult problems associated with the otigin of mankind. His influence is obvious in the early chapters, for example in the account of the 'btain gtowth' of mammals. Wells also quoted at length Lankester's letter on the Piltdown finds.[24] Wells had been impressed with Wood Jones' theory that the human race had evolved from a tarsier-like creature, the apes being dismissed as an entirely separate branch of Primate evolution. Lankestet scolded him severely for this, but admitted that it was a 'hellishly difficult enterprise' to tell the story of eatly man because it was so full of uncertainties. He also offered shtewd criticisms of later parts of the *Outline*. He atgued, for example, that it should have told more of the high Byzantine and Islamic civilisations, which had been kept out of common histotical teaching by the prejudice favouring the Latin Church.[26] Nevertheless, when Wells sent him the completed volume in August 1920, Lankester wrote that it had been 'a gigantic task, and wonderfully carried thtough.'

Lankester's 'Easy Chair' articles contained several discussions of human origins. He emphasized the impottance of the opposable thumb as the chief chatacteristic leading to the differentiation of humans from apes. The erect posture of mankind was essential to allow the petfection of the hand as a means of exploring and controlling the environment.[27] In another article he argued that the origin of mankind was a recent event as measuted by the standards of biological evolution. The frequency with

[23] Lankestet to Edward Clodd, 16 March 1912 (Lankestet family papers). on the ox-collar see V. G. Childe, *What Happened in History* (London, 1941), p. 74.

[24] H. G. Wells, *The Outline of History* (London, 1920), I, pp. 37-8.

[25] Lankester to Wells, 8 Decembet 1919 (Wells papers, Universitv of Illinois). On Wood Jones' theory see Peter J. Bowler, *Theories of Human Evolution: A Century of Debate, 1844-1944* (Baltimore and Oxford, 1986), chaptet 5.

[26] Lankestet to Wells, 26 April 1920 (Wells papers, Universitv of Illinois).

[27] Lankestet, 'From Apes to Man', in *Diversions of a Naturalist* (London, 1915), pp. 236-44.

which modern human suffer from hernia shows that the walls of the abdomen have not yet adjusted to the upright posture.[28] He also insisted that the human foot could not have evolved directly from that of the gorilla or chimpanzee.

Although the origin of mankind was recent in biological terms, Lankester believed that it was immensely distant as measured by the standards of prehistoric archaeology. One of his chief scientific interests of later life was his championship of new ideas extending the antiquity of mankind further back into the past. As a young man, he had paid a visit to Boucher de Perthes, the French pioneer of prehistory. Prehistoric studies had been transformed in the 1860s by recognition of the vast antiquity of stone-age cultures, but it had become accepted as dogma that the earliest stone tools were those of the Chellean period. Attempts to identify various crudely chipped flints from earlier geological horizons as primitive tools or 'eoliths' had been dismissed on the grounds that natural forces could have produced the effects.[29]

In his *Kingdom of Man* of 1907 Lankester published illustrations of eoliths found in Kent and expressed the view that they were both man-made and of high antiquity.[30] On October 17th 1910, J. Reid Moir wrote to *The Times* announcing his discovery of humanly-worked flints in the 'bone bed' underlying the East Anglian crag deposits. With some trepidation he wrote to Lankester, who was known as an expert on these deposits. Lankester visited Ipswich and was convinced; it was no longer true that the earliest human relics to be found in England were from post-glacial deposits. Lankester was the only expert to back Reid Moir, and he was soon embroiled in the resulting controversy.

Reid Moir subsequently told the story of Lankester's involvement in his project in a book, with copious quotations from a whole series of letters from Lankester.[31] Moir was the son of an Ipswich tailor of Scottish descent. His interest in archaeology had only begun in 1904, and it was thus of immense benefit to him to get an expert of Lankester's standing on his side. He repaid his mentor with undying gratitude. When he sent Lankester a copy of his book *Pre-Palaeolithic Man* in 1919, he wrote on

[28] Lankester, 'The Skeleton of Apes and of Man', *ibid.*, pp. 245-52.

[29] See Donald Grayson, *The Establishment of Human Antiquity* (New York, 1983), and on eoliths, Bowler, *Theories of Human Evolution* (note 25), p. 30.

[30] Lankester, *The Kingdom of Man* (London, 1907), pp. 18- 21.

[31] J. Reid Moir, *Prehistoric Archaeology and Sir Ray Lankester* (Ipswich, 1935). The original letters from Lankester to Reid Moir are preserved in the British Library Manuscript Department (Add. Mss. 44968-71).

the title page: 'To Sir E. Ray Lankester, to whom the author owes more than he can exptess'.

Reid Moir first wrote to Lankester in May 1910 and soon sent him a box of the flints. Although cautious at first, Lankester wrote on 30 May that at least some of the flints 'are worked in such a way that I cannot considet it otherwise than in the highest degree probable – in fact reasonable to conclude – that man has deliberately worked them'.[32] Once convinced, Lankester entered the struggle with his accustomed vigour, and a tendency to bludgeon his opponents that was becoming more marked with age. In November 1911 he read to the Royal Society his paper 'On the Discovery of a Novel Type of Flint Implement below the Base of the Red Crag of Suffolk, Proving the existence of Skilled Workers of Flint in the Pliocene Age'.[33] Here he described the flints for which he suggested to Moir the name 'rostro-carinate' (keel or beak-shaped). Lankester was convinced that flint implements must be subjected to a more intense, even a micrsocopic, examination. He also recommended practical experiments in toolmaking.

There was much opposition to the claim that the flints were worked by human hands. On many occasions Lankester, not himself a patient man, had to urge patience on Reid Moir. He was sometimes scathing in his remarks about those who refused to accept his conclusions: W. J. Sollas, the Professor of Geology at Oxford, was a 'hopeless little beast' who would have to 'eat his words' when the full story was published.[34] Lankester was also deeply involved in the effort to convince the two leading continental experts, Marcellin Boule and the abbé Breuil, that the flints were genuine tools. The two French archaeologists visited Ipswich in September 1912 and seem to have been unimpressed: Lankester urged Moit 'pray keep a calm attitude about Boule and Breuil'.[35] A draft letter to Boule has been preserved – hopefully a draft and not a copy, since it is written in very forthtight terms.[36] It was 1925 before the last of the doubtets was silenced,

[32] Lankester to Reid Moir, 30 May 1910 (British Library, Add 44968).

[33] Published *Philosophical Transactions of the Royal Society*, 102 B (1912), pp. 283-336. See also Lankester, 'The Test Specimen of the Rostro-Carinate Industry found beneath the Norwich Crag', *Royal Anthropological Institute, Occasional Papers* no. 4 (1914), and 'On Some Rostro-Carinate Flint Implements and Allied Forms', *Proceedings of the Royal Society*, 92 B (1921): 162-7.

[34] Lankester to Reid Moir, 3 September 1917 and 31 July 1912 (British Library Add 44970 and 44969).

[35] Lankester to Reid Moir, 15 October 1912 (British Library, Add 44969).

[36] Lankester to Boule (Lankester family papers).

and Moir's election to the Royal Society confirmed the respectability of his opinions. Modern work has tended to overturn Lankester and Moir's views, suggesting that the rostro-carinte fragments are indeed of natural origin, although Lankester's position on the antiquity of the human species has been amply confirmed by fossil discoveries from Africa.

Like many of his contemporaries, Lankester accepted the Piltdown finds, and he appears on the far right of the famous collective portrait of those involved in the affair painted by John Cooke, R.A. in 1915.[37] His initial reaction was one of enthusiasm; the discovery would 'take the shine out of the French,' and would fit nicely with Reid Moir's findings: 'It seems quite possible that it is our Pliocene man – the maker of rostro-carinate flints'.[38] Neverthless, he was also aware of the anomalies that were only explained by the subsequent exposure of the affair as a fraud. It did not escape him that

> the astounding thing about this half-jaw from Piltdown, is that it is definitely and obviously more like that of a chimpanzee – especially a young chimpanzee – than it is like that of a man ... If it had been found under other circumstances, it might quite well have been described as the jaw of a simiid – a large ape allied to the chimpanzee, with some unimportant resemblances to a human one.[39]

When the fraud was eventually exposed by J. S. Weiner in 1955, it was found that the jaw was indeed that of an ape, with its teeth filed to give a human pattern of wear and its surface stained to give the appearance of age. The discoverer, Charles Dawson, is usually assumed to have been the culprit, although many suggestions have been offered as to a possible scientific expert behind the affair.[40] On reading of the exposure, Sir John

[37] The portrait is reproduced in, for instance, J. S. Weiner, *The Piltdown Forgery* (London, 1955), facing p. 50.

[38] Lankester to Reid Moir, 25 October 1912 (British Library, Add 44969). Another letter of 18 December 1912 shows Lankester already expressing disappointment about Piltdown.

[39] Lankester, 'The Missing Link', in *Diversions of a Naturalist* (London, 1915), pp. 274-91, see p. 285.

[40] On the exposure see Weiner, *The Piltdown Forgery* (note 37). The most recent of the many attempts to identify the culprit picks out Sir Arthur Keith, see Frank Spencer, *Piltdown: A Scientific Forgery* (London and Oxford, 1990).

Squire, who had known Lankester well, wondered what Lankester would have said had he known:

> At the start he would have exploded with rage. His gigantic, almost pachydermatous frame would have shaken with anger; he would have pounded the table with his fist, and he would have said, 'The damned fellow ought to have been hanged.' After a few weeks he would have subsided, and a chuckle would have arisen out of his depths, and he would have said, 'The damn fellow diddled us ...'[41]

[41] Sir John Squire in the *Illustrated London News*, 19 March 1955.

- 16 -
Lankester, Marx, and Politics

Lankester was not primarily interested in politics, but much that he did had implications for the political life of his time. As a scientist and a publicist for the claims of science, he blamed politicians for hindering the progress of his profession by their parsimony. He was also adamant that the educational system must be changed in ways that most politicians distrusted. Among his friends and acquaintances were a number of politicians and statesmen. He had met, to name only the most eminent, Disraeli, Gladstone and Lord Salisbury, and knew A. J. Balfour quite well. As a young man he got to know Karl Marx, a fact ignored in his obituaries, but which has now begun to attract the attention of historians.

A letter of 1884 describes his meeting with Gladstone at the most select dinner part he ever attended.

> I talked with old Gladstone to my heart's content ... He has just read Mark Pattison's and George Elliot's memoirs – deeply interested in both. He laughingly said to me with great assurance, "The absurd thing about poor Pattison was that he actually came to the conclusion that physical science was the only really true knowledge. Ha ha." I told him that I thought so too, which made him sit up.[1]

Lankester was no supporter of Gladstone, though, and penned the following epigram on hearing the news that Woolner was preparing a bust of him from a block of marble presented by the Greeks:

> Whilst Woolner's hand in the classic att,
> Shaping the Premier's pate is,
> The joyous Greeks with grateful heart,
> Send him the glad stone gratis.
> Oh! could our country stone for stone,
> Return the gift genteely,
> and doubly grateful send our own
> Gladstone to Hell as freely.

[1] Lankester to his mother, 1884, and epigram on Gladstone (Lankester family papers).

What, then, were Lankester's own political beliefs? We have seen that he was not a 'social Darwinist', but from the start he realized that Darwinism had implications for human affairs. At the Linnean Society's Darwin-Wallace celebration of 1908 he spoke as follows:

> When the Progressive members of the Austrian Parliament shouted in a stormy debate that they were 'für Darwin', the significance of the science of living things, including man, became evident, and it was recognized that 'Darwinism' must influence future statesman and politicians as well as men of science.[2]

Lankester had inherited and improved upon his father's radicalism, and this led him to declare in a letter to his mother in 1873:

> I really believe that common property and free love, subject to certain regulations for the common good, will one day have a great development, perhaps not in these old lands, but in new ones, which will increase in population and prosperity as Europe dies of old age. I don't say that I should like to try the experiment in the midst of another kind of life, but it cannor be rhat men will go on, some deprived of proper food, education, and all that makes life endurable ...[3]

During the 1870s Lankester impartially criticized both the parties dominating British political life. 'The Liberals are a sham', he declared to Thiselton-Dyer, 'and the conservatives genuine unpretending swindlers'.[4] Nevertheless, in 1880 he was asked to stand for parliament for the Liberals. The invitation came from a colleague, G. Carey Foster, professor of engineering at Universiry College. His opponent would have been John Lubbock, larer Lord Avebury, who actually won the election. Fosrer told Lankester, 'I believe there is a very fair chance of success. I believe we could at least make a good fight, and could probably win'.[5] The inviration shows thar some of Lankester's colleagues looked on him as a potential leader in this area, but he declined to stand. Lankester in parliamenr would have been interesting, to say the least, although it is doubtful if he

[2] Lankester's speech on receiving rhe Darwin-Wallace medal; in *The Darwin-Wallace Celebration held on Thursday 1st July 1908 by the Linnean Society of London* (London, 1908), pp. 27-32, see p. 29.

[3] Lankester to his mothet, 1873 (Lankester family papers).

[4] Lankester to Thiselron-Dyer, 14 Febtuary 1880 (Thiselton-Dyer papers, Kew).

[5] G. Carey Foster to Lankester, 3 July 1880 (Lankester family papers).

would have been popular with his party whip. Sir Arthur Keith once told Reid Moir that if Lankester had gone in for politics, he would have finished by becoming prime minister, and Reid Moir reflected that few who knew him would have disagreed.[6]

About this time, Lankester became a close friend of Karl Marx, a fact that has been highlighted by the historian Lewis Feuer.[7] Feuer suggests that the friendship may have indicated that Marx was becoming increasingly interested in science toward the end of his career. However, it cannot be doubted that Lankester himself had radical political views that would have appealed to Marx. The two men almost certainly discussed both science and politics, although any clear influence in one direction or the other is difficult to substantiate.

Lankester liked to meet outstanding men and women, whether or not they were acceptable in polite society, and he would not have been repelled by Marx's reputation. He could have been introduced by his colleague at University College, the professor of history, E. S. Beeseley, for many years a close friend of the Marx family. By September 1880, Lankester was on a friendly footing with Marx, who was by no means as isolated as it is sometimes made out, although he was subjected to a formidable intellectual boycott. He appears to have become a fairly regular visitor at the Marx home, and on one occasion Marx's daughter Eleanor dined with him.[8]

Four notes from Lankester to Marx survive. On 19 September Lankester wrote from Eastbourne, fixing a date for Marx to visit him at Wellington Mansions. On 27 September he apologizes for his inability to visit Marx, owing to a previous engagement. A note of 25 December is of more interest:

> I must thank you very much for remembering to send me your great work on Capital. I am reading it with the greatest pleasure and profit. I trust that you have had satisfactory medical advice in the matter of which you lately spoke to me, and that the case is going favourably.[9]

[6] J. Reid Moir, *Prehistoric Archaeology and Sir Ray Lankester* (Ipswich, 1935), p.151.

[7] Lewis S. Feuer, 'The Friendship of Edwin Ray Lankester and Karl Marx: The Last Episode in Marx's Intellectual Evolution', *Journal of the History of Ideas*, 40 (1979): 633-48. For a critique of Feuer's position see Dianne B. Paul, 'Marx's Darwinism: A Historical Note', *Socialist Review*, 13 (1983): 113-20.

[8] See Yvonne Kapp, *Eleanor Marx* (London, 1972-76, 2 vols.), vol.1, p. 236.

[9] Lankester to Marx, 25 December 1880. Copies of the four letters were supplied to J. Lester by the Marx-Engels-Lenin Institute, Moscow, although Feuer obtained them later from the International Institute of Social History, Amsterdam.

Lankester, who read German fluently, may have been one of the first British scientists to read *Kapital*. He was familiar with E. B. Tylor's views on cultural progress, which were also noted by Marx, and the two men could certainly have discussed the historical development of society.

They must also have discussed Darwinism, and we know that Marx had mixed feelings about the theory which Lankester championed. As he wrote to Engels in 1860 concerning Darwin's *Origin of Species*; 'Although it is developed in the crude English style, this is the book which contains the basis in natural history for our view'.[10] Marx relished the materialism of the evolurionary theory's implications for the human race, but appreciated the analogy between natural selection and the operations of the free-enterprise system. We now know that Marx did not, as was once assumed, offer to dedicate a volume of *Kapital* to Darwin.[11]

Lankester's short book *Degeneration: A Chapter in Darwinism* had appeared shortly before the two men became friends, and Marx seems to have been impressed with the fact that Lankester drew attention to the possibility of human societies degenerating. In fact, Lankester had explored this question more fully in some notes from the same date, which form a kind of appendix to the published work:

> Certainly in the case of human societies, it is to be supposed that ultimately a degenerate society would be beaten repressed, and eventually annihilated by other societies ... The struggle is so close among civilised men that the possibility of a degeneration and permanent rest does not suggest itself. It is exceedingly probable that a community which aimed at degeneration would end in annihilation.[12]

Marx was sufficiently interested in Lankester's views on this issue to write to his Russian friend Danielson, asking if his friend Lankester's work on degeneration had been translated into Russian.[13] That the essay was indeed translated is confirmed by the final surviving letter of Lankester to Marx, which was a reply to a request by Marx for medical attention for the Russian revolutionary Leo Hartmann.

[10] Quoted in Ronald L. Meek, ed., *Darwin and Marx on Malthus* (London, 1953), p.170.

[11] See Lewis S. Feuer, 'Is the Darwin-Marx Correspondence Authentic?', *Annals of Science*, 32 (1975): 1-12, and Margaret A. Fay, 'Did Marx Offer to Dedicate a Volume of Capital to Darwin?', *Journal of the History of Ideas*, 39 (1978): 133-46.

[12] Lankester, notes on degeneration, perhaps for a lecture (Lankester family papers).

[13] Marx to Danielson, from Karl Marx and Friedrich Engels, *Selected Correspondence* (Moscow, 1956), p. 409.

> I have no direct connection with the University Hospital, but could give Mr. Hartmann a letter to the physicians there. Better would be for him to see my friend Dr. Donkin of 60, Uppet Berkeley Street, Portman Square, on any morning before twelve. Dr. Donkin is a very able physician, graduate of Oxford, and would be glad to give his services to Mr. Hartmann, since like myself he sympathises with those who try to move onwards. I am sorry to hear that Mrs. Marx is no better. It surprises me and pleases me also that my little work should have been translated into Russian.[14]

Donkin treated not only Hartmann, but also Marx's wife and Marx himself. It was on his advice that Marx travelled abroad in 1881-82, to the south of France, Algiers and Switzerland. Mrs. Marx died in December 1881, and the loss of his wife was a cruel blow. Marx did not long survive the death of his daughter Jenny in January 1883. He died on the fourteenth of March. Only people who had been close to the family were invited to the funeral, and it is significant that Lankester was included. The only other scientist invited was Karl Schorlemmer, professor of organic chemistry at Owen's College, Manchester.

The papers preserved by Lankester's family contain no references to Marx, and he seems to have been reticent to admit to their association in later life. It was not until 1926 that he admitted to his close friend H. G. Wells that he had known Marx long before. In a P.S. to a letter in that year he added 'Did I tell you that I used to know Karl Marx and his wife and daughters intimately, and with him Engels – a rather rough specimen'.[15] Wells's *The World of William Clissold*, published the same year, contains a furious diatribe against Marx. Feuer suggests that Lankester may have been the source of the personal information about Marx used in this attack,[16] but there is no evidence for this. Lankester's original view of Marx was favourable, and although he became less supportive of Marxist doctrines in later years, there is nothing to suggest that he changed his views on Marx as an individual.

A fictional account of Lankester's relationship with Marx may throw some light on the affair, since it is obviously written from some personal acquaintance. Robert Briffault, better-known as the author of the anthropological classic *The Mothers*, published his novel *Europa* in 1937.

[14] Lankester to Marx, 17 February 1881 (see note 9).

[15] Lankester to Wells, 9 September 1926 (Wells papers, University of Illinois).

[16] See the articles by Feuer and Paul cited in note 7.

This expresses profound left-wing sympathies and contains a brilliant portrait of Lankester. There is a fictional account of the meeting at which Carnegie's *Diplodocus* cast was presented to the Natural History Museum in 1905. The hero, Julian, is introduced to Lankester, who is described as only recently having had his encounter with the police in Piccadilly Circus (actually it was ten years earlier). Lankester, it is claimed, 'had taken the occasion to deliver an eloquent and fiery address in court, denouncing the hypocrisy and brutality of the regulations on prostitution, and incidentally the infamy and inconsistency of marriage prostitution morality'. Lankester notes the 'jealousies [and] underhand hostilities' that are making his life at the Museum impossible, and then launches into an attack on capitalism, accusing it of corrupting thought and holding back science. He concludes:

> Intellectual graft! that is the inevitable result of the social and economic conditions under which we live. An honest and intelligent humanity will be impossible so long as these conditions remain as they are ... When I was young, it was my good fortune to make the acquaintance of an old German Jew who was dying, here in London, from the effects of long hardship and privation, of overwork and poverty. I did what I could to save, to prolong his life. I got him sent to Algeria, to the south of France, and got the most brilliant young physician in Harley Street to look after him. But it was too late. In the short time I knew him, he taught me more than all other teachers, dead or living. He saw more clearly than any other man the disease which is killing the world. His name was Karl Marx. From his grave in Highgare Cemetery, he may yet shake the world.[17]

Briffault's portrait is just a little larger than life, but is it any guide to Lankester's opinions? This is certainly not the only fictional account to use him as an example of a prominent figure of the left. A much earlier novel, written from the opposite viewpoint, presents a Lankester-like figure as an ogre who threatens civilisation. This is *When it was Dark*, by 'Guy Thorne', an alias of Ranger Gull. The novel appeared in 1905 and was widely praised from the pulpit. It depicts a temporary eclipse of Christianity caused by the discovery of a historical inscription forged by a villainous professor of archaeology named Robert Llewellin. The professor certainly has Lankester's physical characteristics, and it was whispered at Oxford at

[17] R. Briffault, *Europa* (London, 1937), pp. 255-58.

the time that he was the model.[18] There is talk of an unspeakable half-life behind the scenes of the eminent man of the world. Clearly Lankester was perceived as a threat by those whose opinions he challenged.

But was he really the radical ogre or hero portrayed in these fictional accounts? H. M. Hyndman, the socialist leader, told Archibald Robertson that Lankester was sympathetic to socialism.[19] In his controversy with Hyndman in 1911, Lankester objected to the latter's assumption that the current policies were producing a biological degeneration of the human race, but nevertheless wrote of 'that socialism which is to conquer'.[20] He counted some socialists among his personal friends. John Burns visited the Museum and was given a copy of *Extinct Animals* for his son.[21] Robert Blatchford of *Merry England* and *Clarion* fame was given a copy of an *Easy Chair* volume and responded with a long and enthusiastic review.[22] Lankester also knew and admired William Morris.

Neverthless, Lankester was less than certain about the ability of the lower classes to take charge of their own fate. In some notes of 1880 he wrote of the 'wonderful examples of unreasoning altruism of the poorer classes' and as late as 1917 claimed that there was more superstition among the well-to-do than among the working masses.[23] But he had little belief in mass action, or in the capacity of democracy to further the aims of science.

> Germany did not acquire its admirable educational system by popular demand – nor does England owe such institutions as the College of Chemistry, the School of Mines, the Royal College of Science, and the national art schools to political agitators ... The crowd cannot guide irself, cannot help itself in its blind impotence.[24]

[18] Personal communication to J. Lester by Archibald Robertson, who was an Oxford student at the time.

[19] Personal communication to J. Lester.

[20] Lankester in *The Times*, 27 January 1908 (on the conrroversy with Hyndman see chapter 15 above).

[21] There is a card from Burns acknowledging receipt of the book in rhe Lankester family papers.

[22] Blatchford's review is in the *Clarion* for 31 January and 11 April 1913.

[23] 1880 nores in the Lankester family papers, and Lankester, 'Is rhere a Revival of Supersition?' *Rationalist Annual* (1917): 3-10.

[24] Lankester, review of Karl Pearson, 'National Life from rhe Srandpoinr of Science', *Nature*, 63 (1900-01), supplemenr, p. 3.

In his *Kingdom of Man*, he wrote that democracy does not act because it does not understand what is going on.[25]

Lankester's attitude toward the rich was conditioned by individual views about science. He liked Carnegie, with whom he stayed on several occasions at Kibbo Castle. He respected Iveagh, who gave magnificently for scientific reseach. But on the other hand, he described with distaste the wealthy man whom he showed around the Museum and who asked what use it all was if it brought in no money. He told of an American businessman who exclaimed to a scientist, 'Who the hell cares if there's sodium in the sun?'

Some of his remarks show that he had penetrated the secret of the economic order, and had not forgotten his talks with Marx. After the Titanic disaster, he wrote to *The Times* that commercial enterprises inevitably sought profit, and that companies were mere impersonal mechanisms 'driven by the laws of supply and demand'. He called for legislation to intervene against business associarions, 'necessarily, by their nature, devoid of conscience'.[26] The rough notes for his 'Nature and Man' talks of 1905 gives his forthright opinions on the ruling classes:

> The capitalist wants cheap labour, and he would rather see the English people poor and ready to do his work for him, than better off.
>
> The country is bloodsucked and absolutely ruled; first by the Church, then by the King, then by the 'governing class', and now by this new terror the capitalist ... Asses like the King and his ministers, whether Conservative or Liberal, must hate science, because it inevitably will abolish them and their likes.[27]

On rhe ourbreak of the World War he wrote an unpublished 'Easy Chair' article summing up his critique of the social scene. He wrote of

> The fools paradise of inefficiency, laziness, luxury and self-indulgence of the more fortunate classes of the population, accompanied by the disconrent of the poorer working classes, and the consequent indifference of them to the narional cause ... We retain in Britain, in spite of all our revolutions and reforms, the structure of a conquered country ruled by the members of a privileged class.[28]

[25] Lankester, *The Kingdom of Man* (London, 1907), p. 47.

[26] Lankesrer, letter to *The Times*, 19 April 1912.

[27] *Nature and Man* notes (Lankester family papers).

[28] Draft of 'Easy Chair' article (Lankester family papers).

There can be little doubt of Lankester's progressive sympathies during the most active part of his life. But it would be wrong to see him as a socialist in any doctrinal or party sense – one can hardly see Lankester toeing any party line. He was an intellectual aristocrat, hardly the kind to keep closely in touch with the masses. He would not have followed Alfred Russel Wallace, for instance, in his rather eccentric support for socialism and other social panaceas. He did not believe that women should have the vote, and indeed thought that the fewer people who could vote the better.[29] After the death of Marx he lost touch with the Marx family, and was not attracted to the personality of Engels. Lankester's socialism is to be sought in his views on the nature and influence of science and his militant humanism.

Lankester hailed the Russian revolution of March 1917 with enthusiasm. In the overthrow of Czarist autocracy and the entry of the United States into the war, he saw the guarantee of victory and democracy. 'This is indeed a new 'spring of time'' he wrote, 'The world will forever remember the glorious Easter of 1917'.[30] In the end, it was the November revolution that the world remembered. Lankester could not understand what had happened, and wrote to Mrs Wells asking who the Bolsheviks were.[31] His later impressions were largely shaped by those of Wells. In 1920 Wells went to Russia, a trip described in his book *Russia in the Shadows*. While recognizing that the Soviets were firmly in power, he saw them purely as an emergency government. Lenin was for him the 'dreamer in the Kremlin'. His negative views were almost certainly transmitted to Lankester. The latter's opinions are summed up in a comment written on the back of one of the Museum's illustrated greetings cards and sent to Wells in 1926. The card shows the fearsome carnivorous reptile *Inostrantsieva* from the Permian rocks of Russia, and is described by Lankester as 'an ancestor of the Bolsheviks'.[32] His position is perhaps surprising in view of the reports brought back by other biologists such as William Bateson and D'Arcy Wentworth Thompson, who confirmed that the Soviets were in fact putting into practice the kind of policies for science that Lankester had always advocated.[33]

[29] Lankester to Mrs Wells, from Murren, 8 July (1914?) (Wells papers, University of Illinois).

[30] Lankester, 'This New Spring of Time', *The Times*, 19 April 1917.

[31] Lankester to Mrs Wells, 24 January 1914 (Wells papers, University of Illinois).

[32] Card dated 22 December 1926 (Wells papers, University of Illinois).

[33] See Beatrice Bateson, *William Bateson, Naturalist* (Cambridge, 1928), pp. 153-4, and Ruth Thompson, *D'Arcy Wentworth Thompson: The Scholar Naturalist* (Oxford, 1958), pp. 193-4.

In 1925 Lankester received a letter signed by D. Riazanov, Director of the newly-formed Marx-Engels Institute in Moscow:

> We know from Friedrich Engels' record that you were one of the choice friends who accompanied Karl Marx to his grave, and we should be most happy to collect at the source valuable reminscences about your intercourse with the deceased scholars Marx and Engels.[34]

According to his housekeeper, Lankester replied that he had no letters, and proferred no reminiscences. Moscow was insistent and wrote again, but Lankester answered very curtly.[35]

[34] D. Riazanov to Lankester, 2 January 1925 (Lankester family papers).

[35] Reminiscences of Lankester's housekeeper recorded by J. Lester.

- 17 -
Notable Friendships in Science, Literature, and Art

It was only rarely that Lankester failed to visit the Continent at least once a year, especially Paris, a city he loved only a little less than London.[1] It was not pleasure alone that drew him. Lankester's warm personality gave him the gift of friendship with distinguished foreign men of science, a talent further enhanced by his linguistic abilities. At home, of course, he knew many prominent scientists, but he was able to build up close ties with eminent people in many walks of life, including the author H. G. Wells and the dancer Anna Pavlova.

Lankester's visits to the Continent were appreciated by many eminent scientists. At first, his contacts were mainly in Germany, where he had studied as a young man, but from the late 1880s he began to build up closer links with French biologists. In 1887 he visited Pasteur's laboratory in the Rue d'Ulm and was introduced to the great man by Dr Lauder Brunton, a member of the committee appointed to enquire into the efficacy of Pasteur's treatment for rabies. He found Pasteur a kindly, gentle and modest man, but a fierce disputant. Lankester made many visits on subsequent occasions, when Pasteur had moved to the fine new Institut Pasteur in the Rue Dutot, the gift of a grateful world. He was soon on terms of friendship with Pasteur's assistants, Giard, Chamberland, Émile Roux and Elie Metchnikoff. In a letter home in 1889 he notes: 'Yesterday I had lunch with Giard, and tomorrow with Roux and Metchnikoff'. The latter introduced him to the criminologist Bertillon, who had the three friends photographed on the standard criminal record cards.[2]

We have already seen (chapter 12) how Lankester became involved in the effort to transfer Pasteur's techniques to Britain. It was Lankester who created the initiative which led the Lord Mayor of London to set up a subscription in support of Pasteur's work. In 1892, Lankester travelled to Paris in the company of Roscoe and Lister as the representatives of the

[1] Some impression of Lankester's summer travels can be gained from the range of hotel notepapers used for his letters to staff at the Natural History Museum (Natural History Museum archives).

[2] Letters and photographs in the Lankester family papers.

Royal Society at the Pasteur jubilee celebration. He was present at the famous ceremony of December 27th, when Lister embraced the man whose researches had stimulated his own work in surgery.[3]

It was Elie Metchnikoff who was to become one of Lankester's closest friends.[4] A translation of Metchnikoff's work on 'phagocytosis', the scavenging activity of the white blood cells, had appeared in the *Quarterly Journal of Microscopical Science* in 1884. Lankester now fell under the spell of Metchnikoff's earnest and emotional personality. He became a warm suporter of his friend's theories when medical opinion tended to sneer at them as 'Metchnikoffism'. Lankester saw in these theories an application of Darwinism, with natural selection endowing the blood with extraordinary protective powers. Inflammation could now be seen as an advantageous mechanism, pouring phagocytes into the affected tissues. Lankester wrote of his friend:

> Metchnikoff was a single-minded character, absolutely and tenaciously devoted to a high purpose – the improvement of human life ... he was, in a way which was traceable to his human [*i.e.* Jewish] origin, a practical idealist concentrating his whole strength and reason on the realisation of what he held to be the highest good.[5]

Metchnikoff eventually became sub-Director and then Director of the Institut Pasteur. Lankester visited him repeatedly both there and at his home at Sèvres. He developed a great respect for his friend's wife, Olga, a cultured woman who spoke and wrote good English. Here Lankester noted that Metchnikoff was a pioneer in food hygiene, insisting on the individual wrapping of his bread rolls in paper. A letter from Lord Kitchener, at that time Commander in Chief in India, shows that Lankester had been trying to persuade him to introduce similar methods into the Indian Army.[6] In 1906 he wrote an introduction to Metchnikoff's Harben Lectures, published under the title *The New Hygiene*.[7]

[3] The ceremony is described in R. Vallery-Radot, *The Life of Pasteur* (New York, n.d.), pp. 447-51. Information also supplied to J. Lester by the Institut Pasteur.

[4] On Metchnikoff's work see Alfred I. Traubner and Leon Chernyak, *Metchnikoff and the Origins of Immunology* (New York, 1991).

[5] Lankester's review of Metchnikoff on inflammation, *Nature*, 45 (1892), p. 505, also his obituary of Metchnikoff, *ibid.*, 97 (1916), pp. 443-4.

[6] Lord Kitchener to Lankester, 4 January 1908 (Lankester family papers).

[7] E. Metchnikoff, *The New Hygiene: Three Lectures on Infectious Diseases*, preface by Lankester (London, 1906).

When Metchnikoff's *Nature and Man* appeared in 1903, Lankester pencilled in into a notebook some shtewd ctiticisms of his friend's more speculative conceptions. Metchnikoff was working on the idea thar the large intestine was a potent source of auto-intoxicarion and consequent ill-health. Lankester wrote: 'M. is a bit cracked abour the large inrestine. I don't think it is a general source of poisoning.'[8] He later came to believe there might be something in it, although medical opinion would now suport his earlier reaction.

During one of Metchnikoff's visirs to London, Lankester provided an elaborate meal for him and a select company of friends.[9] Afterwards, they all partook of a dish of milk, soured with the 'Bulgarian bacillus' which Metchnikoff saw as the correct method of controlling the bacterial flora of the intestine. Metchnikoff said: "My dear Ray, if you think that a few spoonfulls of this will be effective after what you have given us to eat and drink – well, you are wrong!" Edmund Gosse was present at the dinner and wrote on the back of Lankester's invitarion his impressions of the Russian: '... long curled grey hair, soft full grey beard, poinred nose, simple straightforward look out of the eyes, extremely natural, candid, kind, wirh no cultivared graces'.[10]

In 1913, when Lankester was staying in Bournemouth, Metchnikoff wrote that he was as usual full of work, but well enough consideting his sixty-eight years.[11] In 1916 came news of Metchnikoff's illness and partial recovery, which Lankester reported in *Nature.* Then he heard the news of his friend's dearh, and was heartbroken. To Mrs Wells he wrote:

> I have just this morning recieved a telegram from Paris to say that my dearest friend, perhaps the one I admired and loved most of any is dead ... He was only two years older than I am ... I am on the point of bursting into tears. I wish we could all go on for ever.[12]

Mary Duclaux, the English wife of Pasreur's immediate successor at the Institut, subsequenrly wrote to him about Olga Metchnikoff, stressing her loneliness and asking Lankester to write to her:

[8] Notebook in Lankester family papers.

[9] Described by P. Chalmers Mitchell, *My Fill of Days* (London, 1937), p. 186.

[10] Gosse on the reverse of the lerrer of invitarion from Lankester (Brotherton Collection, Leeds University).

[11] Metchnikoff to Lankester, 18 Januaty 1913 (Lankesrer family papers).

[12] Lankester to Mrs Wells, 6 July 1916 (Wells Papers, University of Illinois).

> She has finished her book [the biography of Metchnikoff] in Russian and I think almost in French. If it is not yet printed, it is because of the great expense of paper and printing ... I have read two of the early chapters. They are really charming, something naive and primitive and sweet. Ah, if you could induce a publisher, French or English to try the book, and if you could write a preface to introduce it to the public, there would be a way of helping this dear lady, cheering her life immensely, as well as advantaging her fortunes, and bringing fresh interest into her dull and widowed existence. Would that be possible, dear Sir Ray?[13]

Lankester did write to Olga Metchnikoff, and a portion of her reply has survived:

> ... puisque je suis seule, et j'ai des besoins très modestes. La seule chose qui me préoccupe – c'est de realiser le publication de la biographie que je regarde comme un devoir envers la mémoire d'Elie. J'espère pouvoir l'accomplir. Je vous remercie infiniment de votre offre si bon de me guider dans la publication anglais, et de la fidèle amitié, que vous nous gardez.[14]

The English translation of the biography appeared in 1921, and Lankester wrote the preface containing the moving tribute to his friend quoted above.

Lankester's devotion to science was paralleled by an almost equally intense love of art in all its forms. During his travels in Europe he never failed to study carefully the great collections. One of his surviving notebooks contains a list of London galleries where watercolours could be seen. In 1896 he wrote a detailed sketch for a tourist book on architecture. After seeing Leonardo da Vinci's 'Last Supper' in 1874 he thought he understood why moderns attached so much value to the great works of art: 'It is because we are utterly incapable of doing such things nowadays, which is rather a terrible reflection'. It would be horrible to think that that the human mind had passed its zenith; but perhaps it was only a temporary cessation, and people were organising their energies afresh.

[13] Mary Duclaux to Lankester, 26 December (1917?) (Lankester family papers).

[14] Portion of letrer from Olga Metchnikoff, January (1918?) (Lankester family papers).

> Science is being made the subject of their attention, and a fair distribution of work amongst the overcrowding masses of human beings the object of their efforts. When this is accomplished, there may be again a period of that rest and quiescence which seems to be as necessary for mental fecundity as for corporeal, and then God knows what works of art men may not produce.[15]

The day would surely come when the masses would enjoy the delights of art and literature, but the stern task of conquering nature came first. Only on this account did he fear art, lest it encroach on the basic task of taking charge of the material world. In his notebook he wrote:

> Speak with warmest love of art, its seduction so great as to be a danger, but none the less a great, perhaps the greatest joy, the highest contentment of the human mind. Science was primary, for art could only proceed on the basis of science: it assimilated new knowledge and made use of it. New knowledge must in time give new forms of art. In some arts the neglect of new means was more obvious than others. The architecture of the day was mere antiquarian copying, But our needs and our materials are new, and a true architectural aesthetic would give us beautiful forms fitted to modern requirements, and using iron and glass to accomplish new forms undreamed of by the men of antiquity.[16]

Lankester knew Holman Hunt, Rossetti and Watts personally, and discussed theories of art warmly with them and others, especially the views of the pre-Raphaelites. In 1909 Metchnikoff took a group that included both Lankester and the son of Leo Tolstoy to visit the sculptor Rodin. Lankester jotted down his recollections of the interview:

> Rodin said he remembered York Powell – man of great and varied knowledge. He advised Tolstoy not to study the antique, but living model. He disapproved of present art education and 'formulas.' An artist must develop his own quality, his observation especially. He declared that he always copied nature, not changing or combining ... Declared that he did not exaggerate size for expression. (He would nor rise to my saying that he did so in the 'baiser'.)

[15] 1874 notebook (Lankester family papers).

[16] Preparatory notes for the Romanes lecture (Lankester family papers).

The party went to Rodin's 'great glass hall' at Meudon. Lankester's account continues:

> In his 'great glass hall' were some of his earliest things, clay busts of pretty girls, clever but trivial. Lots of studies of hands and feet. Very great variety struck me, and great difference in the apparent value and weight of the work.
>
> Latest things are spirit-like floating figures on solid background, very graceful and pretty, beautiful line but vague. On the other hand a female head, 'Le douleur,' is wonderful, strangely like a Carrière painting. He agreed with me that Carrière was really a sculptor rather than a painter. He has built a wonderful Roman-Greek facade for a hall to contain his antiques.
>
> Very pleasant, affable, unaffected old fellow. He seems to me to search for and strain after beautiful flowing line, and to abound in other things in doing so. Probably the graceful little things he is doing in such number are not great effort and rather 'por-boilers' and amusement to him. There is no doubt that when he is roused to a big effort, he is equal to the occasion. I was especially struck with his dismissal of classical education in art. He agreed that a young artist must teach himself, cannot be made by a teacher ...

Lankester was present at the banquet given in Rodin's honour in London in 1902, and the artist wrote to thank him for 'the honour you have given me by your presence at the banquet given for me in London', describing it as 'this unforgettable celebration'.[17] It must be said that for the modern revolutionary schools of art, Lankester had little sympathy, cubism in particular moving him to explosive wrath.

Lankester celebrated the new century by forming a notable and fruitful friendship with H. G. Wells, who was then delighting thousands with his science stories. In his simple and direct way, Lankester wrote:

> Will you come some day to the Natural History Museum when you happen to be in town, and make acquaintance with me? I shall be on duty, more or less, till the end of August, and a line from you would fix me for any date you choose. You are to me very dear, and a real friend, as you must be to a vast number of those who read your books. But I feel as though I had a special bond to you. That Normal College or Royal College of Science is

[17] Rodin to Lankester, 31 December 1902 (Lankester family papers).

very familiar to me, and in your last book, you tell of the search for the ciliated funnel of the earthworm's nephridium – a little game for which I am responsible. I should so much like to talk with you, and look round some of the fine things in the Museum with you, and then let us go and have a chop together in the Majolica Restaurant of your old haunt.

I was at Sandgate for a few days at Easter, bur did nor then know that you lived there. Lady Jeune and Chalmers Mirchell have both promised me to invire me to meet you, but I venture ro take the course of writing myself. I dare say that you will not care for my opinion much, but I take the keenest delight in your 'time and space' stories, because I know them to be not only products of a fine imagination, but really determined by adequate and large knowledge of science. But the 'science' is only 'thrown in', as you show in the 'Romance upon Wheels', and 'Mr. Lewisham'.[18]

Wells answered promptly:

I scarcely come to London nowadays, but if I do so before August, I will certainly accept your invitation to come and see you. Your letter came two days ago, but I still regard it with something of incredulity as it lies before me. You see, I have read you so sedulously, and associated you so intimately with things like the science of biology, the British Encyclopaedia and London University – you examined me four times, for the pass and honours of both stages of the B.Sc. – that to find you among my readers is something of a shock. I really am exceedingly proud of yout approval of my work; I only wish I had earnt it more thoroughly. There is much in the spirit of scientific investigation that is as easy to caricature, as it is difficult to grasp and interpret, and which I feel very strongly is in need of explicit interpretation. I am a little hampered in my movements just at present, or I would make a special occasion and come to London to see you, but I hope if I fail to accept your invitation before the vacation, it will still be open to me to come ro see you next autumn.[19]

[18] Lankester to Wells, 12 July 1900 (Wells Papers, University of Illinois). A useful biography is Norman and Jeanne MacKenzie, *The Time Traveller: The Life of H. G. Wells* (London, 1974).

[19] Wells ro Lankester 12th or 14th July 1900 (Lankesrer family papers).

The friendship developed quickly, Lankester's letters progressing from 'My Dear Sir', and 'Dear Mr. Wells' to 'Dear Wells' and finally 'Dear H. G.' He visited Wells at Sandgate on a number of occasions, where the two friends 'settled the affairs of heaven and eatth'. At Lankester's instigation, Wells was invited to the annual dinner of the Royal Society as a representative of literature. Lankester was introduced by his friend to the circle of the Omar Khayyam Club at Frascati's – 'a lot of writers and their friends, very good company, Anthony Hope in the Chair'.[20] Wells also paid several visits to the Museum.

In 1902 Lankester wrote a long review for *Nature* of Wells's book, *Anticipations of the Reaction of Scientiifc and Mechanical Progress on Human Life and Thought.*[21] This was the first appreciation of Wells by an established scientist, and it made an impressive tribute. Wells was described as

> a mind naturally gifted with extraordinary vivid imagination, and the aptitude for true literary art. The wonderful range of knowledge of his stories, and their scientific accuracy, and the absolute restraint of the weird histories he recounts ... lend a specific charm to Mr. Wells' writings wanting in all other masters of this kind of literary craft from Swift to Jules Verne.

Lankester saw Wells's book as an inspired indictment of existing government, society, religion, education and morality. Yet it contained hope for the future, not leasr because Wells had thoroughly grasped the teachings of modern science.

There is no doubt of Wells's respect for Lankester. He was careful to send him a copy of each new work, and each was greered with praise and admiration. Lankester saw the reflection of his own views in Wells's books, expressed with the surety and clarity of genius. Wells was now in his socialist phase, and Lankester took his *Modern Utopia* with him to Paris in 1905, where he prepared his Romanes lecture. When Wells had finished baiting the Fabians, he turned to the congenial task of shocking the respectable, and wrote *Ann Veronica,* the story of a young woman who livied in sin with a married man and did not come to a bad end. Wells was attacked as a perverter of youth, but a group of friends including Lankester stood by him and protected him from any social boycott.[22]

[20] Lankester to Wells, 5 November 1901 (Wells Papers, University of Illinois).

[21] *Nature,* special supplement 13 March 1902.

[22] H. G. Wells, *An Experiment in Autobiography: Discoveries and Conclusions of a Very Ordinary Brain (since 1866)* (London, 1934, 2 vols,), vol. II, pp. 471-2.

In 1911 Lankester received the manuscript of Wells's *Marriage*. He was delighted with this story of a young scientist, and thought that Wells had dealt sympathetically with the problems of the scientific researcher. He was even more delighted to find that he himself appeared in the novel as 'Sir Roderick Dover,' and wrote to Wells:

> ... I like his views and personality, and have no objection whatsoever to the suggestion that he is in some measure a reflection of me. But I wanted to hear more of him.[23]

A brief extract from the novel gives an impression of Wells's view of Lankester:

> Sir Roderick was fat – nobody who came within a hundred yards of him could be under any illusion about that – and ... he drank a good deal, ate with a cosmic spaciousness, loved a cigar, and talked and laughed with a freedom that sometimes drove delicate-minded new members into the corners ... there was a joy in Dover's laugh and voice, a beaury in his face (that was somehow mixed up with his healthy corpulence), and a breadth, a charity, a leonine courage in his mind (that was somehow mixed up with his careless freedom of speech) that made him altogether a satisfactory person.[24]

Lankester often visited the Wells family in their French home near the Seine, and the family governess remembered him as a cheerful, corpulenr figure in white flannel, holding a sunshade over his head, or rowing ro an island in the river. On one occasion he drew a picture for Wells's sons and signed it by drawing a fish (a ray) as he had done in his youth. He related stories of the famous men he had known, and the curious experiences that had befallen him.[25] Then the war came, and Wells, a fierce hater of the Kaiser's government, vanished into a semi-governmental haze. Lankester continued to write regularly to Wells and to his wife. He was not sure about his friend's excursion into religion in 1917 in the book *God, the Invisible King*. The masses would never understand that Wells used the term 'God' to denote the developing moral sense of humanity – but he

[23] Lankester ro Wells, 8 June 1911 (Wells Papers, University of Illinois). Lankester did, however, ask Wells to replace the term 'gross' with 'unabashed' or 'outspoken'.

[24] H. G. Wells, *Marriage* (London, 1912), p. 241.

[25] Mathilde M. Meyer, *H. G. Wells and his Family, as I have known them* (Edinburgh, 1956), p.85 (Meyer was the Wells's family governess).

would stand up for him against the crowd of ignorant blasphemers.[26] We have already seen (chapter 15) how Lankester helped Wells with his *Outline of History*. He wrote regularly, and continued to receive his friend's works through to 1928. The last letter is dated 27th September 1928.

In 1910 Anna Pavlova came to a London that did not know the Russian ballet. Soon she was the talk of the town, and 'Have you seen Pavlova?' became the universal question. Lankester came, saw, and was conquered. He had never seen such exquisite art, and felt that here was one person he must make his friend. It was not as difficult as he feared: Pavlova admired men of knowledge, talent and energy, and those who held service to humanity as their ideal. In a short time, Lankester was numbered among her closest personal friends. He visited regularly at Pavlova's English home, Ivy House in Hampstead, formerly the home of the painter Turner. Sometimes he was privileged to watch the dancing class. Pavlova turned to him for advice on various matters, as illustrated by the following letters.

> J'ai un grand désir de faire une collection des anciens livres sur l'art de danse. Voulez-vous peut-être, être si bon de m'aider a les trouver? Je vous prie de ne pas chercher dans les grands magazins, mais dans les petits boutiques, où vous pourriez par hazard trouver ce que vous voulez. Les graveurs anciennes m'intéressent aussi beaucoup, mais comme vous savez, j'en ai déjà beaucoup, donc il faudrait qu'ils m'envoient pour regarder avant d'en acheter.
>
> [Then in Pavlova's own hand]
>
> Je suis rrès heureuse que votre rhume est parti; je vous embrasse, votre Anne Pavlova.[27]

A few weeks later Pavlova wrote again:

> J'ai décidé lorsque je suis à Nottingham, de faite visite à Londres pour arranger mes affaires. Après mon tour, je pars tout de suite pour Berlin. J'aimerais bien vous rencontrer encore une fois avant de partir. Si vous pourriez peur-être prendre le déjeuner avec moi au Bellini Restaurant, cela me ferait grand plaisir. Pouvez-vous, s'il vous plaît cher Monsieur, m'envoyer un livre

[26] Lankester to Mrs Wells, 12 September 1917 (Wells Papers, University of Illinois).
[27] Pavlova to Lankester, 5 November 1912 (Lankester family papers).

contenant les copies des peintures de Botticelli, aussi une histoire de sa vie, comme c'est nécessaire pour mon art que j'apprenne quelque chose de lui. Si vous pourriez trouver un livre bientôt et me l'envoyer pendant que je suis ici, vous seriez bien aimable, car je veux ce livre avant mon départ.

[Again in her own hand]

J'espère aller à Londres dimanche, alors j'irais vous voir chez vous. Je vous embrasse.[28]

Sometimes Pavlova lunched at Lankester's house at Thurloe Place, and on one occasion he showed her round the Zoological Gardens. On 18th May 1913 Pavlova presided at a dinner of the Srage Society, and as she knew little English, requested Lankester to speak on her behalf. His address ascended to heights of eloquence:

> She is the greatest artist now on the stage. Her art is not only the oldest and most beautiful of all the arts which use the stage, but is the mother of all art. Dancing gave birth to music and the drama and has allied herself with her children to join her in delighting mankind. From the living presentation of moving form and colour in the dance, the painter and the sculptor have learnt their special modes of art. Before the cavemen carved ivory and painted figures on the walls of their subterranean haunts, they had danced, and given expression to noble emotions and to poetic thought in rhythmic movement, in gesture and in changing play of of the features. It is the Elemental Art – the first and most deeply-seated expression of noble and entrancing thoughts ...
>
> This is what the great dancer gives us. That is what our divinely-gifted President Anna Pavlova, gives us. At one moment she charms us with the naive emotions of the 'Fille mal gardée', and a few minutes later appears – alone and silent – as 'Night', telling us by her movmenrs and facial expression, as no words can, of its majesty, its all-embracing restfulness, its gentle gifts of refreshment ... Anna Pavlova ... is not merely the most graceful of dancers, the most accomplished in the technique of her art, but beyond and above this, expresses when she dances, as only a supreme artist can, the creative thought and conception of a mind of the rarest beauty and of the highest poetic qualitity.[29]

[28] Pavlova to Lankester, 25 November 1912 (Lankester family papers).

[29] Printed copy of the address (Lankester family papers).

As soon as he had finished speaking, Pavlova (as he recalled later to Berta Ruck), rushed forward, put her hands on his shoulders, and kissed him on both cheeks. In the October of 1913, Pavlova gave her farewell performance at the Opera House, and on rhe eighth of the month Lankester recieved a last telegram from her.

He was not able to see her again until 1920, but on his table he always kept a signed photograph of her. When she returned to England after the war, she did pay a visit to her old friend, now at Oakley Street. Lankester wrote to the novelist Berta Ruck that he feared he would not see her again on the stage.[30] When he jotted down some notes for the autobiography he never wrote, Lankester stated: 'The two most remarkable women I ever met were George Eliot and Pavlova'.

30 Berta Ruck, *A Story Teller Tells the Truth: Reminiscences and Notes* (London, 1935), p. 308.

- 18 -
The War and Last Years

The Great War was a disaster for Lankester. His *Daily Telegraph* articles stopped and his income dropped by £950 a year; even his pension was cut. The thought of the war paralysed his faculties: '[It] is too much for me. It has laid me flat. I am too old for it. I shall never recover.'[1] His life became more solitary. His friends were either away or no longer went to the clubs, although Poulton occasionally visited him at his house in Thurloe Place.

Lankesrer's radicalism now began to erode, partly because of old age, and partly due to an increased sense of patriotism. He had no doubt that the Germans were responsible for the war, although he opposed efforts to withdraw honours conferred on learned Germans as intrinsically absurd and dishonourable. He wrote an article for the *Strand Magazine* denouncing the warlike tendencies of German culture, while paying tribute to the eminent Germans he had known as a young man.[2] In a *Times* letter he opposed the view that it was cultivation of science that had made the Germans militaristic.[3] He wrote an 'Easy Chair' piece exposing the social and governmental flaws which had led to the neglect of science, but wrote on the proof sheet: 'Unpublished, and will probably remain so.'[4]

He managed to write some articles, notably one on Christianity for the *Rationalist Annual*, and a brief contribution to a symposium on American neutrality. 'America may hurt her eyes looking too long at her money pots,' he wrote.[5] His scientific work in the war period was chiefly connected with the controversial flint tools, and in 1917 he introduced an important memoir by Reid Moir to the Royal Society. The previous year he had given a lecture on blood with the title 'A Wonderful Fluid' to the Royal Artillety Institution, which was reprinted as a (now very rare)

[1] Lankester to H. G. Wells, 29 June 1915 (Wells Papers, University of Illinois).

[2] Lankester, 'Culture and German Culture', *Strand Magazine* (January 1915): 31-39.

[3] Lankester, 'On German Claims in Science', *The Times*, 26 December 1914.

[4] Proof sheet in Lankester family papers.

[5] Lankesrer, 'Will Orthodox Christianity Survive the War?' *Rationalist Annual*, 1917. The arricle on America appeared in *Everybody's Magazine* for January 1916.

pamphlet.[6] He now felt that his few wretched articles came out like water from a neatly dried up pump. Producing the *Quarterly Journal of Microscopical Science* had now become difficult; he was struggling with lithographers and printers. To Mrs Wells he wrote:

> I have just sent to the press a beautiful number of the Journal, full of delightful studies and nice plates. It seems to me almost unpatriotic to think of such things. And yet it is the only thing to avoid going mad with suspense and anxiety as to the War.[7]

On the outbreak of the war, the Asquith government closed some museums and proposed to close the Natural History Museum as well. Lankester was a member of the delegation which saw the Prime Minister to protest against this. Asquith was impressed, chiefly by Lankester's arguments, and agreed to keep the bulk of the Museum open.[8]

He now wished to get rid of his Putney house, but eventually the authorities commandeered it. In 1916 he had a short stay as a guest of Mrs Wells, and then returned to London. By 1917 he felt very unwell, suffering from a cold and from the dulness of memory he considered the fearful result of old age. He spent his birthday with Edward Herron-Allen at Selsey, and argued with him about Foraminifera. In June he had ten days at Bournemouth, strolling along the seashore and picking up the curious little oyster-like shells of *Anomia* which he had never found before.

The air raids in London upset him terribly, and the Wells family offered him hospitality at Easton for a fortnight. At the end of the year he went again to Bournemouth and soon took a house at Chine Head. Here he had his beloved pine woods and sea shore, with a few visitors. His sister Minnie and her husband stayed for a few months, while Wells made a brief visit. It was as well that he had some visitors and distractions, because Bournemouth society aroused his most radical instincts. It was:

> the dullest place in the British Isles. Rank respectability, pasty faced and expressionless rolls along here in countless numbers. Should you contrive to get into conversation with a specimen ... you find it is brainless and barely articulate. It never smiles, never

[6] Lankester, 'A Wonderful Fluid', reprinted from *The Journal of the Royal Artillery*, December 1916 (offprint in Lankester family papers).

[7] Lankester to Mrs Wells, 2 February 1917 (Wells Papers, University of Illinois).

[8] Reid Moir, *Prehistoric Archaeology and Sir Ray Lankester* (Ipswich, 1935), p. 145.

> expresses intetest or emotion. It is hatmless, except fot the fact that it is hideous, and makes one feel nausea ...[9]

It was, he considered, a sort of forty days in the wilderness. When he returned, he would be sensitive to the mildest forms of gaiety. He looked forward eagerly to receiving each new work by Wells, although he had teservations about *God, the Invisible King* (see above, chapter 17). He did not return to live in London until March 1919.

When Leonard Huxley's biography of Sir Joseph Hooker appeared in 1918, Lankester reviewed it for *Nature*.[10] He recalled the Homeric struggle of Darwinism, when he was excited afresh every week by some tremendous contest or new discovery. His friends from those days were now all dead and forgotten. He himself had kept up a respectable level of activity during the war years, despite his gloom, working on the *Quarterly Journal* and promoting the work of the Committee on the Neglect of Science. He had even had a fine new 'Easy Chair' volume printed, despite the paper shortage. It was mainly his loneliness that distressed him. As he wrote to Wells:

> The failure of a man to secure some company for his last years when he can't run about ... is a tragic phenomenon and well worth painting ... I used to find a fair number of people who could talk about matters that interested me ... Flint implements and Darwinian topics seemed to be worth attention, and kept one excited and occupied. But the War knocked me off my perch.[11]

He was astonished that the Kaiser was allowed to escape after the war, and wrote furiously to *The Times* about the perpetuation of the privilege accorded to one another by royal criminals, however great their responsibility for useless bloodshed.[12]

On 2nd May 1920 Lankester was awarded the Gold medal of the Linnean Society. This was his second award from the society, the first being the Darwin-Wallace medal of 1908. Many of his friends regarded it as a scandal that he was never offered the Presidency of the Royal Society. After his death, Herron-Allen called this a stain on the history of the society and recalled the 'helpless amazement' of his foreign colleagues.[13]

[9] Lankester to Wells, 24 January 1918 (Wells Papers, University of Illinois).

[10] Reviewed in *Nature*, 101 (1918): 481-2.

[11] Lankester to Wells, 4 November 1918 (Wells Papers, University of Illinois).

[12] Lankester, letters to *The Times*, 13 November and 11 December 1918.

[13] E. Herron-Allen, Obituary of Lankester, *Journal of the Royal Microscopical Society*, 3rd series, 49 (1929): 359-65, see p. 360.

In 1921 Lankester began to take stock of his life. He jotted down some notes on his contributions to evolution theory and pasted some letters in a book.[14] He thought earnestly on the problem of war, and sketched an article in which he predicted that future wars would be more terrible because of aerial bombardment and both chemical and germ warfare. He wanted a 'religion of peace' based on teaching children the atrocious nature of war. Politicians could not prevent the build-up of armaments, and international agreement would be needed to suppress preparations for war.

> [This] must be effected by drastic measures of inspection of factories carried out by international police, and such suppression must be enforced by penalties such as exclusion from commercial intercourse. It must be made clear that there are among us certain groups of individuals who find their advantage in the manufacturing and financial profits and the political power given to them by war. They must be kept out of power, disqualified, recognized as a constant threat, and their existence as a danger.[15]

Lankester rather resented growing old. He often said that he did not feel his age until he was seventy, although in 1910 giddyness and fatigue had forced him to stop a game of golf at Dornock. In 1919 he had a bad attack of the influenza that was causing a global pandemic which laid him up for six weeks. He had to give up his articles for the *Illustrated London News* in September 1921 because the strain of writing exhausted him. Doctors, including his brother Owen, examined him and pronounced him sound, although with a very slow pulse and extremely low blood pressure. He became impatient with them and thought they could do more for him.[16] He had always enjoyed the good things of life, but from the age of sixty he submitted himself to a moderate regimen, avoiding alcohol and cutting his smoking to a minimum, although he still enjoyed a good cigar. The only thing of which he could not have enough was fresh air and sunshine. It was not only his physical symptoms that distressed him. He felt lonely, and thought that he read too much, but reflected that this was probably better than being worried by stupid people and the banalities of

[14] Preserved in the Lankester family papers.

[15] Draft of article in Lankester family papers.

[16] Lankester, statement on his health written for the use of his doctors in 1921 (Lankester family papers).

'sociery.' 'What I require,' he insisted, 'is cheerful, non-worrying society, good-natured laughing people ... but I cannot lay in a stock of that kind of thing.'[17]

In April 1920 he wrote to Goodrich that he would have to give up his editorship of the *Quarterly Journal of Microscopical Science.* Goodrich replied:

> I am sorry you wish to give up editing the Q.J.M.S., but I suppose it is a considerable worry, especially now with all these new difficulties about prices. Of course we must not allow such a useful and venerable journal to die out – rhat I think is out of the question. Bourne did say something to me about ir. He is away now, but when he returns, we must try and sertle what musr be done about it.[18]

Eventually the journal was sold for £500, Goodrich becoming chief editor. Lankester's connection was emphasized by the title of Honorary Editor, appearing on the title page of each issue. The fiftieth year of the journal had been celebrated by a special historical article by G. C. Bourne, who paid a fine tribute to his old teacher:

> To each successive generation of aspirants ro the honour of having their early and often immature work published in the Journal, he has been a guide, philosopher and friend, appreciative, fruitful in suggestion, and full of encouragemenr towards further efforts.[19]

On his return from Bournemouth in March 1919, Lankester had moved into a more commodious house at 44 Oakley Streer, Chelsea. Here his comfort was jealously guarded by Miss Pearson, a very efficient housekeeper, and by Miss Mills, the maid. Lankester was not always an easy man to serve: if a scientific paper went astray, there was no resr for anyone until it was found. He disliked small rooms and had the dividing walls removed between the principal ones. Upstairs was his study (two rooms made into one), in the back part the bed, surrounded by hundreds

17 *Ibid.*

18 E. S. Goodrich to Lankester, 13 April 1920 (Lankester family papers).

19 G. C. Bourne, 'Fifty Years of the Q.J.M.S.', *Quarterly Journal of Microscopical Science*, n.s., 253 (1919).

of books and his various treasures. In the study was his large, glass-topped desk, bearing his magnifying lens and his favourite paperweight – a cast of a gorilla's foot. On the study mantelpiece there stood the large signed photograph of Pavlova, and on the wall hung a portrait of Huxley.

Apart from members of his family, he had many visitors. His old friend Sir Theodore Cook and his wife lived a few doors away. Sir David Bruce, Sir Bryan and Lady Donkin, Sir Philip Burne Jones, Sir Edmund Gosse, Lance Sieveking, Julian Huxley, E. B. Poulton, Lady Chalmers Mitchell and Sir Henry Wood (of Promenade Concert fame) all visited 44 Oakley Street. Compton Mackenzie came and introduced him to the gramophone, which became a great source of pleasure and solace. Another visitor was the young Peter Scott. Lankester knew of R. F. Scott's wish that his son should become interested in science, and Peter Scott's memories of his visits illustrate one of Lankester's most human charactersitics, his liking for and understanding of children.

> There was always some kind of specimen to show me – often for me to take home – an insect, a fossil, a feather, a vertebra – and the specimen led to pictures in books (often his own) and a dissertation. Once it was the eatbone of a sperm whale, another time a series of snail shells, and on another, a fly in amber ... I see the room as rather dark, with a pool of light over the desk which was covered with books and papers and specimens. I think that it was in this room that I first looked through a mictoscope.[20]

Like Darwin, Lankestet was a great devourer of novels, a pastime that grew on him as he became more and more incapable of muscular exercise. The detective story bored him, and on the whole the modern novel irritated him:

> The rubbish and self-satisfied bosh which pours out now in magazines and novels is astonishing. The authors are so set upon being 'clever,' 'analytical', and 'up-to-date', and are really mere prattling infants.[21]

In his later yeats he had little taste for novels taising heavy issues. He liked good story-telling which would take him out of himself for a while. He had a great liking for Charles Reade, and his housekeeper read *The Cloister and The Hearth* to him more than once. It was this liking for good

[20] Peter Scott's reminiscences of Lankester, personal communication to J. Lester, November 1959.

[21] Lankester to Wells, 31 March 1919 (Wells papers, University of Illinois).

story-telling which led him to make the acquaintance of the novelist Berta Ruck, writing to request a meeting as he had with Wells years before. A notable friendship developed between the ageing naturalist and the writer, who did much to cheer his declining years.

In 1923 Lankester fell ill and had to have a nurse. The next year his sister Fay died after an operation, and the blow was a cruel one. 1925 was the Huxley centenary year, for which Lankester contributed some memories to *Nature*[22] and wrote an interesting letter to Poulton, who published it in the *Literary Guide*.[23] These contributions brought an appreciative letter from Huxley's son and biographer, Leonard, who considered Lankester's recollections to be fuller and more inspired than those of anyone else. He begged Lankester to write his memories out in full.[24]

Lankester may have complied with this request had he been able to carry out his plan to write the story of his life. In July 1924 Chapman and Hall had written offering to publish his memoirs, if and when he decided to write them. He decided to call the book 'Memories and Adventures of Sixty Years', and got as far as jotting down a few headings and random notes. But the task of writing was now beyond him. Some time later he tried to dictate his thoughts to a secretary, but found it impossible. Had he been able to complete his task, his memoirs would have ranked along with the 'lives and letters' of the great Victorian naturalists such as Darwin, Lyell and Huxley.

Lankester did not approve of what was happening in the world of biology. Morphology no longer dominated the field, and Lankester detected a lack of binding theory and purpose. To Wells he complained:

> A rather painful thing is the present ignorance and neglect of morphology. The present biological activities in universities etc., is reduced to rather feeble laboratory notebooks with curves and mathematical swagger about rates of movement or of chemical change, leading to nothing! No binding theory or purpose inspiring it ... The notion of 'what will pay' and what will appeal to those asking for practical results, now dominates and blocks the former road of pure speculation and observation resulting therefrom. Moreover, mere fragmentary notes are published as important 'original' research.[25]

22 Lankester, 'Huxley', *Nature*, 115 supplement (1925): 702-04.

23 Letter published in the *Literary Guide*, July 1925.

24 Leonard Huxley to Lankester, 11 May 1925 (Lankester family papers).

25 Lankester to Wells, 10 April 1924 (Wells papers, University of Illinois).

Nevertheless, many of the younger scientists came to pay their respects. Goodrich's pupil Gavin De Beer saw him once in 1922 and realized that he was in the presence of a forceful personality. De Beer noted that he was interested in the haemoglobin of the *Planorbis* snails living in an aquarium on the table.[26] Of these younger men, Lankester had a high regard for J. B. S. Haldane, and wrote enthusiastically to Wells about him.[27]

In 1926 Wells sent Lankester a copy of his *The World of William Clissold*. Lankester thought it a curious book, 'something of Casanova and of the fat boy of Pickwick', but he was pleased with Wells's second attempt to portray him in print under the name 'Sir Rupert Yorke', although he could not understand why he was disguised by a pseudonym when Edmund Gosse and his father appeared in their own right.[28]

As he became older, Lankester became liable to outbursts over fancied slights, outbursts he always regretted later. When Heron-Allen brought out a work on the barnacle goose legend, Lankester thought he had been plagiarized and refused a proferred dedication in a virulent letter. Later he wrote penitently to his friend excusing his temper and blaming it on senile decay.[29]

Lankester disliked many postwar things, and thought that modern inventions such as radio and the cinema had fallen into the hands of knaves and fools. The scientific potential of the cinema attracted him, however. He wrote an article on the subject for a special cinema issue of the *Times Literary Supplement*.[30] He had known the pioneer Muybridge, who had obtained a series of accurately timed photographs of a galloping horse during the 1890s. In 1911 he saw some films of the Pathé organisation made by the French pioneer Comandon, showing amoeba and other microorganisms. His article argued that every biological laboratory of university rank would soon have its own photographic studio.

Lankester still had enough strength to fight intellectual battles. In 1921 he attacked the Reverend A. H. T. Clarke, a critic of evolutionism, in the

[26] Gavin De Beer, 'Glimpses at Some Historic Figures in Modern Zoology', in E. Ashworth Underwood, ed., *Science, Medicine and History: Essays in the Evolution of Scientific Thought and Medical Practice written in Honour of Charles Singer* (Oxford, 1952, 2 vols.), vol. 2, pp. 233-42, see p. 235.

[27] Lankester to Wells, 28 October 1925 (Wells papers, University of Illinois).

[28] Lankester to Wells, 3 September 1926 (Wells Papers).

[29] Quoted in Heron Allen's obituary of Lankester, *Journal of the Royal Microscopical Society*, 49 (1929): 363.

[30] 'Science and the Film', reprinted in Lankester, *Great and Small Things* (London, 1923), pp. 16-28.

Nineteenth Century.[31] In 1922 he joined Donkin in criticizing J. A. Thompson for allowing Sir Oliver Lodge to contribute a section on 'psychic science' to the popular work *The Outline of Science*.[32] A Max Beerbohm cartoon shows a tubby Lankester confronting a frail and slender Lodge. Lankester mutters, 'Odd, that with such a brow ...' while Lodge murmurs, 'Strange that a man who looks so very credulous ...'[33] Whenever he met Lodge, Lankester would poke him in the ribs and exclaim, 'Well, Lodge, you old charlatan, how are the spooks?' Lodge would take it all in good part and reply 'Ah, Lankester, not the charlatan you think'.[34] In 1925, again supported by Donkin, he assailed Arthur Balfour for asserting that telepathy was an accepted fact.[35] The following year he wrote a critique of Conan Doyle's *History of Spiritualism* for the *Sunday Times*.[36] He thought that he had dealt leniently with Doyle, but had had to restrain his language. In general he was passionately opposed to the postwar enthusiasm for spiritualism and superstition.

The controversy which excited him most occurred in 1923 and centred on a photograph of the cast of a gorilla's foot published by the American explorer Carl Akeley. The gorilla had long been seen as a close relative of the human species, and the photograph showed the great toe resting alongside the others in a human fashion. In his *Great and Small Things* Lankester protested that this was not true to life. Akeley was defended by W. K. Gregory, although even he was forced to admit the inadequacy of the original photograph. To Lankester's satisfaction, Henry Fairfield Osborn, Director of the American Museum of Natural History, eventually prevented the exhibition of the misleading cast in the Roosevelt Hall of the Museum.[37]

[31] Lankester's attack on Clarke appeared in the *Nineteenth Century*, 2 May and 6 August 1925.

[32] Donkin in the *Saturday Review* (20 May 1922): 520; Lankester and Donkin, *ibid.*, 2 July 1922, p. 142.

[33] The cartoon appeared in the *Sketch*, 8 June 1921.

[34] Amy Roberta Ruck, *A Storyteller Tells the Truth: Reminiscences and Notes* (London, 1935), p. 310.

[35] Lankesrer's and Donkin's letters appeared in *The Times* on 21 April and 4 May 1925, Balfour replied to the first on 30 April.

[36] *Sunday Times*, 15 August 1926.

[37] Akeley's photograph was published in *The World's Work*, Ocrober 1922 and later in his *In Brightest Africa*; for Lankesrer's original critique see his *Great and Small Things* (London, 1923), p.7, footnote. See W. K. Gregory's and Lankesrer's letters on 'The Gorilla's Foot' in *Nature*, 112 (1923), pp. 758-9; Gregory's reply, 112 (1924), pp. 421-3 and Lankester again, *ibid.* pp. 457-8. Also letter of Gregory to Lankester, 19 February 1924 (Lankester family papers).

In 1925 rhe world was asronished by the 'Monkey ttial' of John Thomas Scopes in Dayton, Tennessee. Lankestet shared the tevulsion of many scientists against the religious fundamentalists' efforts to block the reaching of evolution in the schools. He contributed an article to the special supplement on the affair published by *Nature*, arguing that the whole thing was a 'silly American kick-up' arising from a failure ro use words such as 'fact,' 'theory,' and 'science' properly.[38] The educational enterprise flourished best when free from state prejudice, state ignorance, and state jobbery. He also wrote to *The Times*, using the opportunity to state his views on the nature of knowledge.

> In my judgement, it is much to be regretted that in the attempt to arrive ar such knowledge and understanding of the world around us as may be possible, we should prejudice the resulr of our enquiries by labelling one another as 'scientist' and 'religionist' ... We are all limited in our power of investigation. The one thing we may rightly demand of each other ... is veracity.
>
> We must steadfastly refuse to believe anything upon insufficient evidence. We must never stifle a doubt. We must never pretend that what is only probable is a certainry. Our knowledge, far-reaching as it is, is only the balance of probabilities ... Only those who are thus trained to distinguish truthful from untruthful statements, and to hold fast to that which has been tested and found true, can be accorded the honourable title of 'free man'; they will do that which is right because they know that which is true. That is my belief; it is a religious belief, and is indeed my religion.[39]

In 1927 Lankester fell ill and everyone thought it was the end. He recovered, but never had the full use of his legs and had to be taken out in a wheelchair by his friend Mr Dixon. To Wells he wrote, 'I cannot read very much, but am cheered by the gramophone and the truly remarkable performances I get from it', although he also complained about the difficulty of getting good records.[40] He complained: 'I am alright in my

[38] 'Evolution and Intellectual Freedom', special supplement to *Nature*, 116, 11 July 1925; Lankester's contriburion is pp. 71-2.

[39] Lankester in *The Times*, 22 July 1925.

[40] Lankesrer to Wells, 25 Aprit 1928, and for the complaint abour good records, 29 March 1928 (Wells papers, University of Illinois).

armchair, and hope to see some friends here now and then, but it is no joke being shut indoors as I am. One forgets even the most interesting things for want of hearing them spoken of'.[41]

On Christmas Eve 1928 his friend Thiselton-Dyer passed away. Writing to Lankester, his widow recalled that he had been one of the two friends her husband had really cared for. 'Looking back over my life', she wrote, 'I really think the pioneers had a very good time'.[42]

Early in 1929 Sir William Orpen painted a portrait of Lankester that attracted much attention at the Academy exhibition. Wearing a brown tweed jacket, Lankester holds an open book and reclines on a couch. There is great knowledge and great weariness in the lined face. On May 15th he celebrated his eighty-second birthday, but his friends and relatives were saddened by the knowledge that the end could not be far off. He still received visitors. Only a few weeks before the end S. J. Hickson came, and although in pain and weak he talked for neatly an hour about freshwater medusa. H. G. Cannon and the microscopist Robert Chambers visited him and talked for half an hour, much longer than the nurse had ordered. Two days before the end he asked to have played on the gramophone the song 'Absent yet Present', followed by 'Le cygne'. He died on August 15th. The funeral service was held at St Martin's in the Field, with representatives of the scientific societies and many personal friends attending. The congregation sang 'Lead Kindly Light' and 'Abide with me', and the service ended with Chopin's Marche Funèbre in B flat minor, opus 35.[43] He was cremated at Golders Green Crematorium.

[41] Lankester to Wells, 22 September 1928 (Wells papers).

[42] Harriet Thiselton-Dyer to Lankester, 26 December 1928 and 6 February 1929 (Lankester family papers).

[43] There is a copy of the order of service for the funeral in the archives of the Natural History Museum.

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