SPONTANEOUS GENERATION IN THE 1870S: VICTORIAN SCIENTIFIC NATURALISM AND ITS RELATIONSHIP TO MEDICINE

by

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Abstract

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In the 1870s a debate over the spontaneous generation of micro-organisms took place in Britain. Much opposition to the doctrine of spontaneous generation came from the Victorian scientific naturalists, especially John Tyndall, Professor of Natural Philosophy at the Royal Institution, London.

This thesis provides an understanding of and explanations for the beliefs surrounding the spontaneous generation debate, particularly with regard to Victorian scientific naturalism and its relationship to medicine. Spontaneous generation threatened some of the fundamental tenets of naturalism. Furthermore, Tyndall clearly related his opposition to spontaneous generation to his support for the germ theory which he used as a vehicle for advocating a scientific approach to medicine.

The thesis concludes that Tyndall's campaign for scientific medicine was part of the scientific naturalists' campaign to spread the naturalistic world-view and to gain cultural leadership. The spontaneous generation debate is examined in detail. The shift in experimental paradigm away from physical conditions towards a bacteriological approach is described. Chapter 5 examines the threats an acceptance of spontaneous generation posed to naturalism in terms of evolution, protoplasm and naturalistic explanations of disease. The effects of Tyndall's campaign for the germ theory on the medical profession are described.

In order to understand how scientific knowledge was introduced into medicine, Chapter 6 examines the work of key medical scientists in the field of pathology with reference to their involvement in the spontaneous generation debate and in particular the reasons for their acceptance or rejection of the germ theory. Chapter 7 shows how the spontaneous generation debate impacted the domain of public health from the 1870s-1890s by means of a detailed examination of handbooks of sanitation and hygiene. The gradual introduction of results from the spontaneous generation debate into these works demonstrates the importance of the spontaneous generation debate in forming a bridge from the medical knowledge of the 1860s to the new bacteriology of the 1880s.

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CHAPTER 1

INTRODUCTION

This thesis seeks to gain an understanding of and provide an explanation for the beliefs surrounding the debate over spontaneous generation which took place in Britain in the 1870s, particularly with regard to Victorian scientific naturalism and its relationship to scientific medicine. It will be argued that the spontaneous generation controversy may fruitfully be treated as a case study in Victorian scientific naturalism, as the subject matter of the debate involved, and even threatened, some of the fundamental tenets of the naturalistic world view. These issues included evolution and the origin of life, the fundamental units of life, the principle of continuity, questions of naturalistic explanation and the narrow dividing line between materialism and naturalism. In itself, a threat to the basis of scientific naturalism is enough to explain the involvement of the scientific naturalists in such a debate. However, it is argued that there was a further, and equally important reason for their involvement. In particular, John Tyndall the main actor in the debate from the ranks of the naturalists, clearly related his opposition to spontaneous generation to his support for the germ theory. It will be argued that support for the germ theory was a vehicle for advocating a more scientific approach to medicine.

It will be shown that support for scientific medicine through the germ theory and an attack on spontaneous generation were part of the naturalists' programme to gain cultural leadership and to spread the naturalistic world view. This means that an understanding of the debate requires more than a detailed description of experimental issues and polemical exchanges between the two main protagonists, John Tyndall and Henry Charlton Bastian; it also involves an explanation of the broader implications of the debate, particularly with regard to the development of scientific medicine. In understanding the development of scientific medicine the thesis explores factors affecting the absorption of scientific ideas into medicine and how these ideas were received by certain sections of the medical profession. Finally the gradual change in theories of disease, as the germ theory was disseminated, is explored.

<u>The Context of the Spontaneous Generation Debate</u>

A number of contextual factors are important to an understanding of the spontaneous generation debate of the 1870s. Firstly as this case study concerns itself with the aspirations of a particular group within the scientific community, namely the Victorian scientific naturalists, it is important to understand the organization of the scientific community and within it, the changing role of the scientist. Secondly, discussion of a debate which relates to evolutionary issues must take heed of the contemporary scientific world view, particularly with regard to the debate on "Man's place in nature". The final important contextual element is the long and varied history which spontaneous generation had enjoyed, particularly with regard to earlier debates on the subject.

It is clear that the role of the scientist had grown and altered substantially from the end of the eighteenth century and through into the middle of the nineteenth century, towards that of a professional teacher and researcher in a recognised institution. But this change was a slow and hard fought battle. Professional opportunities and salaries remained poor at least until towards the end of the nineteenth century. Whilst a scientific professor in one of the ancient universities was well provided for in terms of both status and remuneration, the opposite was true for the majority of workers in science. Chapter 2 argues that it was certain scientists of relatively more marginal status who sought to promote the acceptance of their scientific view of the world and thereby attain a degree of status for the "man of science" relative to more traditional leaders of society, particularly the clergy.

But cultural hegemony for the scientist was impossible without an acceptance of the scientific world view that went along with it. There was indeed, in the nineteenth century, a gradually increasing acceptance of scientific interpretations in place of biblical explanations of the natural world. In particular, the style of scientific explanation and subject matter had by mid-century moved away from the idea of glorifying God through studying his works in the natural world i.e. a natural theology, and was gradually leaving out the idea of a deity from its descriptions and theories of the world, becoming more naturalistic in outlook and thereby more receptive towards the accommodation of evolutionary views of the world. Young has shown that it is misleading to talk of a conflict between science and religion or even between scientific naturalism and the established church in the second half of the nineteenth century. [1] It is more fruitful to understand the conditions which opened up the debate on "Man's place in nature". For scientists and other intellectuals, the removal of a deity as a central issue to explain final causes and origins freed the scientific imagination and offered extended scope for scientific enquiries. In particular, enquiries into origins and final causes were becoming the subject matter of scientific explanation, whereas before, the invocation of a deity meant that these sorts of questions were not proper subjects for any enquiry, let alone scientific enquiry. Not all of the scientific community enthusiastically welcomed this new spirit of questioning Nature. Many scientists, particularly those following William Whewell's epistemology, were anxious to show that scientific work was compatible with traditional theology. [2] Yet a spirit of enquiry into origins also meant that the nature and origin of life itself became a subject for scientific exploration.

Arising out of the question of origins and nature of life, the connection between the two entities, mind or spirit and matter, became a definite concern for scientists such as the scientific naturalists, who were in the forefront of the new spirit of scientific enquiry. [3] Darwin's The Origin of Species, in implying an origin of life by a process of spontaneous generation from inorganic matter, meant that the problem of the transition and boundary between the living and the not living could no longer be completely ignored. In France and Germany these implications were quickly recognised and taken up. However the empirical style of British science helped to deflect interest away from the origin of life. In Britain, despite interest in evolution and the origin of species, the question of fundamental origins was still largely ignored.

The spontaneous generation debate which arose in the 1870s was perhaps the only really major manifestation of a concern with these matters in Britain. For a time, in the early 1870s, this debate aroused considerable interest amongst both medical and scientific circles, although the number of actors who became actively involved remained relatively small. It was as if all the overt scientific energy and interest in the question of origins within the British scientific community had gone into the spontaneous generation question. An additional manifestation of this fairly low key and rather indirect interest in origins was the fact that much of the debate was conducted in terms of furnishing experimental proofs rather than in terms of more fundamental philosophical issues.

Hence the question of spontaneous generation was not a matter purely for the more philosophically minded scientist. Whereas most problems of origins and the transition between living and non living could be conveniently ignored, if necessary, the problem of spontaneous generation also had immediate and potentially serious implications for medicine. An acceptance of the possibility of spontaneous generation could be used to support an acceptance of the possibility of the *de novo* origin of disease under certain circumstances; circumstances which were themselves open to debate. So the debate itself was laden with implications at least for medical theory if not medical practice.

The final important contextual element concerns earlier debates on spontaneous generation. From ancient times, and up to the nineteenth century, the doctrine of spontaneous generation had enjoyed varying amounts of popularity. The older Aristotelian and Galenic philosophies lent credence to the idea that flies and insects could arise spontaneously from rotten meat and even that small animals such as mice could appear from the earth. However by the late seventeenth century microscopical findings began to display the complexity of life and attempts were made to discover the method of sexual generation. The development of a belief in eggs or ova as the mechanism of the beginning of life and a dislike of chance as working against the purposes of God and natural law tended to diminish support for spontaneous generation. However the appearance of parasitic worms in intestines posed a problem as they could not apparently be explained by sexual reproduction. Thus it was the case of parasitic worms which kept the possibility of spontaneous generation alive into the eighteenth century.

In Germany, in the eighteenth century, spontaneous generation was linked with the speculative and metaphysical *Naturphilosophie* which held that there was no fundamental distinction between living and non-living matter and that a "vital force" was necessary to hold together unstable living organisms. When materialism arose in late eighteenth century Germany, the materialists were initially resistant to the concept of spontaneous generation espoused by *Naturphilosophie*, but gradually the doctrine became associated with materialism, atheism and political radicalism. As Farley has shown the impact of materialism on spontaneous generation was indirect, but spontaneous generation became definitely linked to materialism and atheism so that attacks upon spontaneous generation became a tenet of the Christian faith. [4]

In France too, spontaneous generation was linked to materialism, in particular the materialistic views of the ideologues who were said to have contributed to the vicissitudes of the French Revolution. When the life-cycle of the parasitic tape worm was explained, micro-organisms remained as the only possible contenders for spontaneous generation. In the debate between Louis Pasteur and Felix Pouchet which took place in France in the late 1850s and early 1860s and the later British debate, controversy centred round the appearance of microscopic organisms in liquids. The actors in the British debate looked to the earlier French debate and Pasteur's work in particular; much effort was expended in disproving or confirming Pasteur's position.

Histories of Spontaneous Generation

In looking at previous frameworks which have discussed material relating to the subject matter of this thesis, the most important area relates to work on the history of spontaneous generation. As an historical topic, spontaneous generation has been subject to a degree of interest by historians, but the Pasteur-Pouchet debate has generally received more attention than the British debate. As the thesis examines spontaneous generation from the point of view of scientific naturalism and looks to the impact of such work on scientific medicine, historical works which explore these two domains are also relevant.

The only major work on the history of spontaneous generation from the Scientific Revolution to the twentieth century is Farley's The Spontaneous Generation Controversy from Descartes to Oparin. [5] This is an extremely valuable reference work in both temporal and geographic terms, because it discusses, in considerable detail, the progress of spontaneous generation over a lengthy period and covers France and Germany as well as Britain. The great value of Farley's approach is in its symmetrical attitude to proponents on both sides of the debate i.e. both sides are given equal weight; he does not emphasise the work which eventually "turned out right". A symmetry of explanation, such as this, is seen to be crucial in describing a scientific issue which aroused considerable controversy in the past, particularly when modern times have issued a decision on the issue and it is no longer a topic for controversy. There is clearly a temptation to take the side of those who were eventually "proved right" and demarcate between science and pseudo-science. This is particularly true of spontaneous generation where experimental issues were presented in black or white terms. Evidence was positive or negative, depending on the theoretical beliefs of the investigator. Spontaneous generation was either proved or disproved - there were no half measures. Farley, in accordance with other recent work in the history of science, attempts to treat equally the causes of true belief and erroneous belief. However in looking back over the historiography of spontaneous generation, clearly it has not been treated in this way by all authors and even Farley's tacit low key acceptance of a symmetrical approach has itself been subject to criticism.

In a review of Farley's book, Roll-Hansen, whilst appreciating the contribution that Farley's new style of analysis makes over the older style of accounts, criticises Farley's concentration on external social factors. He says: "But I think that Farley is overdoing the job. His concern with the influence of external social factors in scientific developments and his sympathy with the underdogs sometimes obscures the scientific issues." [6] He further suggests that Farley could have used a recognition of the shift in the theoretical problem to make "...Pouchet appear considerably less scientifically rational relative to Pasteur..." in the earlier French debate. [7] He is hinting at a "rational reconstruction of history" in the tradition of Lakatos and Laudan. [8]

Although Roll-Hansen clearly objects to Farley's symmetrical approach, he is wrong to suggest that this approach coupled with an emphasis on social factors distorts the theoretical shifts in the subject. Farley's presentation of the subject describes a continuous thread through the history of biology which rarely looks outside the biological world except to examine certain philosophical and medical influences. It is hard to see how the change in theory can be understood without such a symmetrical approach as Farley's. Roll-Hansen's emphasis on "external social factors" also seems to be a red herring as Farley's book is somewhat short on such factors, but understandably so given its impressive chronological breadth. And unfortunately an emphasis on breadth of timescale rather than context does detract from the possibility of providing causal explanations of beliefs in some circumstances. At least for Britain, in the nineteenth century, Farley's book provides little discussion of the nature of the scientific community and the result is that the various actors in the debate seem rather isolated; generally it is not possible to explain why the various actors should become involved. It is as if Farley begins the job of symmetrical treatment of scientific beliefs but is unable to carry it through to explanation and causal descriptions because of the breadth of the topic. A particularly pertinent example for the present work, is his admitted inability to explain Tyndall's opposition to spontaneous generation. [9]

Roll-Hansen has also criticised Farley and Geison's 1974 paper on the Pasteur-Pouchet debate. His interest lies in the claims of a rationalist position over a relativist position and in particular the need to understand the theoretical issues of a scientific controversy before attempting to explain its development. [10] However it seems as though Roll-Hansen, writing in 1983, has a somewhat old-fashioned view of the relativist position in terms of internal scientific factors and external social factors. It is not clear if it is possible somehow to separate out the theoretical issues of a scientific controversy for examination. In criticising the Farley and Geison paper where they were persuaded that external factors influenced Pasteur more than they did Pouchet, Roll-Hansen is concerned to show that Pasteur did not break the rules of the experimental method as claimed by Farley and Geison. [11] His concern seems to mirror the concern that some historians have expressed, and gone to great lengths to prove, over Darwin's scientific purity. [12] Fundamentally it is claims for scientific knowledge as a product of social construction that he finds unreasonable particularly when they result in theoretical misunderstandings of a scientific debate. But his own view threatens to distort the theoretical shifts in the long history of the subject. He suggests that Pasteur's work of 1859-1864 was really the breakthrough in the debate about spontaneous generation and "it took another ten to twenty years before the details had been straightened out and the opposition had given up." [13] It will be argued that this explanation is a totally misleading account of the British debate of the 1870s.

Most other works on the history of spontaneous generation are in the form of shorter articles on specific issues or chapters in books. In the latter category, there is a chapter which describes the French and British controversies in Bulloch's The History of Bacteriology. [14] For many years this was the standard work describing the history of bacteriology and debates on spontaneous generation. In Conant's Harvard Case Histories in Experimental Science there is a chapter describing Tyndall's and Pasteur's contributions to the spontaneous generation controversy. [15]

The style of these and other early papers on the subject tend to convey the attitude that Tyndall's discontinuous heating experiment of 1877 (described in Chapter 4) was the "crucial experiment" which ended the debate and that the debate itself can be seen in terms of the triumph of experimental technique over metaphysical speculation. [16]

The latter implication has been substantially revised by the work of Farley, Geison and Crellin. Farley's earlier papers deal with particular issues - the spontaneous generation controversy in the years 1700-1860 and the origin of parasitic worms and the spontaneous generation controversy in the years 1859-1880; this is followed by the paper with Geison on the Pasteur-Pouchet debate. [17] Farley's later book expands these topics. Crellin's work on heat resistance and the germ theory provides valuable reference material, but the subject is approached as a relatively narrow issue and he fails to take up the question of wider philosophical issues. These papers are based on Crellin's study of Crace Calvert's work. This provides a good account of British interest in spontaneous generation in the 1860s and the early 1870s, but fails to discuss the later part of the debate. [18]

Friday's paper "A Microscopical Incident in a Monumental Struggle: Huxley and Antibiosis in 1875" [19] is the earliest work to take up the question of the explanation of Tyndall's involvement in the spontaneous generation controversy. He suggests that it is the struggle between materialism and idealism rather than the medical context which can provide an explanation of Tyndall's involvement. Whilst Friday's identification of the importance of larger issues is acknowledged, he fails to identify these issues adequately. If Tyndall is to be a materialist then, as Friday rightly points out, his antipathy towards spontaneous generation cannot be explained. The struggle seems to be between naturalism and views of the world which permitted non-naturalistic explanations rather than materialism and idealism. Friday identifies a plausible context but fails to say *why* Tyndall's involvement was inevitable under this view. However it must be acknowledged that this paper was published before much of the more recent work on scientific naturalism which provides a more detailed understanding of the materialism/idealism question. [20]

Vandervliet's Microbiology and the Spontaneous Generation Debate during the 1870's provides the only substantial work dedicated entirely to the British debate of the 1870s. [21] This book provides a useful chronology, bibliography and description of some of the events in the debate. However it is episodic in style and almost completely lacking in appreciation of context. There is no historical curiosity as to why this debate should have taken place and why particular actors should have become involved. There is no mention of earlier debates, the problems of origins, evolution, scientific naturalism and the relationship between scientific knowledge and medical knowledge. Written over fifteen years ago, it represents an instance of "internalist" history which has been overtaken by more recent developments in the history of science. [22] In its defence, like Friday's paper, this book appeared before Farley and Geison's work on spontaneous generation.

Work on Victorian scientific naturalism which has been emerged in recent years, particularly by Turner and Jacyna, has been important in providing a background for this thesis. [23] In particular, Turner was the first historian to describe the scientific naturalists' strategies for gaining cultural hegemony. Work on Darwin, Young's work on the Victorian debate on "Man's Place in Nature", more modern biographies such as James G. Paradis' T.H. Huxley: Man's Place in Nature and other biographical or commemorative works all provide a valuable context within which to locate the present work. [24] However there are at least two important features absent from the recent upsurge of interest in Victorian scientific naturalism. Firstly there is as yet no collective biography of the scientific naturalists. Secondly, and more importantly for this study, there are few attempts to discuss the scientific work which these men addressed *in the context of scientific naturalism*. When they are described collectively, they often tend to appear as philosophers or members of a rather intellectual dining club. [25] When their individual scientific work is described there is a tendency to lose sight of the importance of scientifc naturalism. One of the few papers which talks of the actual scientific work of one of these men in the context of scientific naturalism is Richards' work on W.K. Clifford. [26] Whilst the present study does not seek to rectify the first criticism, it does however discuss the work of some of the scientific naturalists on spontaneous generation as it related to scientific naturalism.

Within the history of medicine, there has been considerable interest in Lister's work on the germ theory and antisepsis. [27] Spontaneous generation, in the shape of the question of the *de novo* origin of disease and its relationship to the germ theory is occasionally discussed, but debates over the origin of life are not generally seen as part of the history of medicine. Histories of public health also provide useful contextual material but are less concerned with theoretical issues such as disease theories. [28] Margaret Pelling's Cholera, Fever and English Medicine, 1825-1865 provides an excellent and detailed description of the theoretical course of disease theory over a forty year period. This book also recognises the importance of spontaneous generation for theories of disease, but her study ends before 1870. [29]

Published works which discuss the relationship of science and medicine are still relatively rare. Of these French's Antivivisection in Victorian Society and Geison's Michael Foster and the Cambridge School of Physiology provide a context in which to discuss the development of scientific medicine, but both focus on particular aspects of that relationship viz. antivivisection and physiology respectively and neither of these books is concerned with spontaneous generation or the germ theory. [30] MacLeod has produced several papers covering topics such as alcoholism, antivaccination and the public relationship of science and medicine, which discuss certain facets of this relationship. [31] Youngson's The Scientific Revolution in Victorian Medicine, as it presents the germ theory and antiseptic surgery as inevitable facts which were to be accepted when the resistance to new ideas was overcome, again tends to present the view of truth triumphing over error. [32] It is Youngson's historical strategy in examining the resistance to new ideas which tends to exaggerate this overly positivistic emphasis.

Both Lawrence and Shortt have recently written about the reception and use of scientific ideas by different parts of the medical profession in the nineteenth century, especially amongst physicians. [33] This work provides a valuable model for understanding the same sort of relationship amongst other parts of the medical world, in particular for this study, public health practitioners.

Aims of the Present Study

This thesis seeks to contribute to an understanding of the spontaneous generation debate of the 1870s in a number of ways. Firstly, in terms of methodology, this study is intended as a contribution to empirical work in the historical sociology of scientific knowledge which is seen to be one of the tools at the historian's disposal. [34] This acknowledges that the old "internal/external" debates in the history of science are now sterile and that the sociology of scientific knowledge can provide theoretical direction for the history of science. When this study was commenced, although there existed increasing levels of interest in the sociology of knowledge as a theoretical domain, there was relatively little concrete historical practice which consciously applied these principles. That this has rapidly changed is shown by Shapin's important review article of 1982. [35] Shapin suggests that, "One can either debate the possibility of the sociology of scientific knowledge or one can do it." [36] This thesis aims to do it rather than debate it.

There are, however, some important ways in which the force of the theoretical aspects of the sociology of knowledge are *particularly* pertinent to the present study. As it has already been suggested spontaneous generation was a subject where actors took sides. Although some figures, notably the eminent Burdon Sanderson, appeared to maintain an agnostic stance, it is clear that the subject aroused considerable passion amongst others. Spontaneous generation was either possible or it was not; there was no middle territory. As no one now believes spontaneous generation to be possible, at least in the form posited by Bastian, it is tempting to believe that those who opposed spontaneous generation in the 1870s were more rational than those who supported the position. But such a presentist view seriously distorts the history of that period. In adhering to the germ theory which involved a belief in invisible germs and in denying Bastian's experimental results which other competent observers had verified, Tyndall can hardly be said to have been more rational than Bastian. Indeed at a time when interest in protoplasm made the basis of life appear very simple and where medical evidence supported the *de novo* origin of disease in some circumstances, if rationality is to be an issue, then Bastian was, if anything, the more rational. Therefore in demanding that the causes of both "true" and "erroneous" beliefs are candidates for sociological explanation, the beliefs of the main protagonists in the debate, Bastian and Tyndall, must both be explained.

In the last section it was suggested that older work on spontaneous generation saw the controversy as the vindication of science over metaphysical speculation and that more recent work had even been criticised for being too even-handed. It is therefore doubly important in this area not only to look at the history of the subject in a symmetrical way with respect to all types of belief, but also to provide causal explanations for these beliefs. It is suggested that such explanations are only possible when the subject is studied this way.

Both Farley and Friday attempted to find reasons for Tyndall's beliefs on spontaneous generation. Farley admits he finds Tyndall's beliefs on materialism and spontaneous generation contradictory. While Friday recognises that Tyndall's beliefs require consideration within a wider context he is unable to provide that explanation because his identification of the wider context is not sufficiently detailed. It is suggested that these attempts failed because both authors did not relate Tyndall's beliefs to Victorian scientific naturalism and the question of scientific medicine. The present analysis of Tyndall's beliefs offers an explanation in terms of these factors. Farley suggests that, "It is difficult to understand why Tyndall was so opposed to the doctrine of spontaneous generation, given his being both a materialist and an evolutionist." [37] In Chapter 2 it is shown that Tyndall was not a materialist and that his beliefs on spontaneous generation were consistent with his philosophical position and beliefs on the nebular hypothesis.

Friday suggests that:

> "If the controversy over the germ theory is seen as a context in which the debates on spontaneous generation took place, and if attention is given mainly to medical implications, a picture emerges in which Tyndall's involvement is an aberration... If, on the other hand, both germ theory and spontaneous generation are seen in the context of the much longer struggle between materialism and idealism in science and in society, then Tyndall's involvement not only fits, but was almost inevitable." [38]

It is suggested that the wider context is natural vs. non-natural explanations of the world rather than the crude materialism/idealism struggle, and further that these sorts of explanations are crucial to the germ theory/medical context in which Tyndall's involvement is entirely inevitable and by no means an aberration. There is a danger of making Tyndall's beliefs actually appear illogical if they are seen against a background of the supposed materialism/idealism struggle. This is because support for spontaneous generation was consistent with materialism. Hence if Tyndall is characterised as a materialist it becomes difficult to explain his antipathy towards spontaneous generation, as Farley has rightly suggested. On the other hand, if Tyndall's antipathy for spontaneous generation is seen in terms of his support for scientific medicine which in turn was part of the scientific naturalists' general concern for the cultural status of science, then these beliefs become understandable.

A further aspect of the present work relates to the idea of a case study in scientific naturalism. As the previous section detailed, most work on scientific naturalism relates almost exclusively to philosophical concerns. This study provides an historical description of a piece of scientific work which related very strongly to the beliefs of actors involved in scientific naturalism. This case study also contributes to work which relates the disciplines of science and medicine in the latter half of the nineteenth century. But it is important to emphasise that an evenly balanced history of science and history of medicine is not what is attempted here. Because this study examines scientific naturalism and its *relationship* to medicine, it seeks to be located within a history of science tradition rather than within history of medicine. The aim is to look outwards from science towards another professional discipline rather than to attempt an even-handed approach towards the two subject areas. There is still relatively little work which examines aspects of the relationship between the two domains, hence this thesis offers spontaneous generation as one way of beginning to explore the connections. The relationship between science and medicine is emphasised, in terms of both the actors i.e. the scientific and medical professions and the knowledge produced, rather than treating science and medicine in purely abstract terms.

Finally, as Vandervliet's book is the only work to provide a detailed study of the British spontaneous generation debate and, as already identified, it treats the subject as a series of discrete episodes . with little by way of analytical framework, the present study seeks to remedy these deficiencies by providing a detailed chronological analysis of the debate, as laid out in Chapters 3 and 4.

Sources and Boundaries

Most of the sources for this thesis are in published form - periodicals, scientific and medical textbooks, books of lectures and essays and biographies. Much relevant material is to be found in the main scientific journal of the day, Nature, and the two medical journals, the Lancet and the British Medical Journal. Some of the more experimental, as opposed to purely polemical, aspects of the debate are recorded in the Proceedings and Transactions of the Royal Society. Other scientific journals containing relevant material include, the Monthly Microscopical Journal, the Popular Science Review and the Quarterly Journal of Microscopical Science. Huxley's Collected Essays and Tyndall's Fragments of Science provide a valuable insight into scientific naturalism. L. Huxley's biography of T.H. Huxley and Eve and Creasey's biography of Tyndall give useful details of the lives of these men. [39] Bastian's prolific publications, both in scientific periodicals and in the form of a series of monographs have been used to understand the complexities of his theoretical position and his experimental work. [40] Handbooks of sanitation and hygiene, as detailed in Chapter 7, have been used to gain an understanding of the impact of scientific theory on the branch of medicine relating to public health issues. Secondary material, too numerous to detail here, has been valuable in locating the present study within the context of other work in the history of science and medicine.

There is very little manuscript material of direct relevance except for the Tyndall-Pasteur correspondence, transcripts and photographs of which are housed at the Royal Institution, London. Louis Pasteur and John Tyndall corresponded at frequent intervals from early 1876 to the beginning of 1878, and thereafter only infrequently. The Pasteur side of the correspondence exists in the form of pencil transcripts (in the original French) made by Louisa Tyndall from the original letters. Almost all of these letters are listed in the catalogue of the Tyndall collection made by Friday, MacLeod and Shepherd and their cataloguing conventions have been adhered to in the present study. [41] The Tyndall side of the correspondence is in the form of photographs of the originals held at the Bibliotheque Nationale in Paris. As these letters do not appear in the catalogue, it seems likely that the Royal Institution acquired them after Friday, MacLeod and Shepherd undertook their study. These letters are quite fascinating as they reveal what was going on beneath the surface in the years 1876 and 1877, particularly how Tyndall drew Pasteur into the debate and urged him to take a hard line with Bastian. It is also here that many of Tyndall's anxieties about scientific medicine and Bastian's adverse influence on the medical profession are revealed. This correspondence does not appear to have been utilised by other historians examining the spontaneous generation debate. In the present study these letters have been especially useful in illuminating the debate which took place between Pasteur and Bastian in 1876 and 1877.

Turning now to the boundaries of the thesis, the period under consideration has been confined to the 1870s, except for the discussion of sanitary science in Chapter 7 which looks at how scientific theory gradually permeated this branch of medicine through the 1880s and 1890s. The description of scientific naturalism in Chapter 2 and the comparison of the scientific and medical professions in Chapter 6 clearly involve a larger historical period but the debate itself lay almost entirely within one decade, the 1870s. There is a short description of the earlier French debate on spontaneous generation in Chapter 3. The subject matter of the present study describes what happened in Britain, with discussion of Continental attitudes to spontaneous generation limited to comparisons where this is appropriate; these attitudes are not treated separately. This means that the thesis does not attempt to generalise from the British debate to other debates except for very particular concepts such as the association of materialism and support for spontaneous generation.

There is also no wholesale attempt to look at the effects of science on medicine in their entirety and no suggestion that the conclusions of this particular study may be extrapolated to apply to other examples of where science and medicine came into contact with each other. The relationship between the two is seen through the medium of one specific theory, the germ theory. The subjects of pathology, public health and antiseptic surgery are involved but this study deals almost entirely with pathology and public health as the reception of the germ theory into antiseptic surgery has been dealt with in some detail elsewhere. [42]

Whilst there is a considerable collection of material in the form of biographies and lectures and essays, quite apart from the more technical literature, which allows a detailed picture of the scientific naturalists to be built up, Henry Charlton Bastian, the chief spokesman for spontaneous generation, remains a more elusive figure. There is only one biography and this is not a published document. [43] He is also enigmatic in the sense that he appears to have been something of a philosophical lone wolf. Whereas, as Chapter 2 suggests, the scientific naturalists were to a large extent children of their time, Bastian the materialist is a rarity in the British scientific world of that period. This study does not examine Bastian's return to the subject of spontaneous generation in the early years of the twentieth century or his work on neurology which he undertook after the spontaneous generation controversy. Interesting though these subjects are, they come after the spontaneous generation debate and add little to an understanding of an already complex area.

Outline of the thesis

The thesis combines the production of narrative details about the spontaneous debate with discussion of the central philosophical and contextual issues. It is seen to be important that the flow of the "story" of the debate is continued whilst introducing the issues at appropriate times into the discussion.

Chapter 2 introduces Victorian scientific naturalism, the backcloth against which the case study is to be understood. Chapter 3 describes the spontaneous generation debate up to the middle of the 1870s and introduces the work of John Tyndall, T.H. Huxley, Henry Charlton Bastian and John Burdon Sanderson. Chapter 4 continues the narrative from late 1874, describes the second half of the debate and how it ended. Chapter 5 goes on to discuss specific issues which were raised by the spontaneous generation question and why these were important for the scientific naturalists' world view, in other words why it is appropriate to consider the spontaneous generation debate in the context of a case study in Victorian scientific naturalism. Chapter 6 deals with the reception of spontaneous generation and the germ theory amongst parts of the medical profession, in particular medical scientists working in pathology. Chapter 7 assesses the impact of the spontaneous generation debate and ascertains how far the germ theory, as a scientific theory, was assimilated into the domain of hygiene and sanitation.

The aim of the thesis is to reveal the complex relationship between the philosophy of scientific naturalism and medicine and to show that the debate on spontaneous generation and the germ theory provides an appropriate instrument for understanding that relationship. Just as previous authors have pointed to the complex relationship between science and technology, so must the relationship between science and medicine be seen as similarly complex. [44] In particular, the thesis avoids the suggestion that there was a wholesale titration of ideas from science into medicine with medicine becoming inevitably "scientific". The overall aim is to understand the detail of the spontaneous generation debate, why such a debate was important to scientific naturalism, in what sense it was used as a means of extending the domain of scientific explanation in a desire to make medicine more scientific and finally the gradual changes which took place in certain branches of medicine to accommodate the new findings of science.

CHAPTER 2

VICTORIAN SCIENTIFIC NATURALISM

The Background to Naturalism

The first half of the nineteenth century witnessed a gradual change in explanations about the natural world away from biblical analysis to an acceptance of scientific interpretations. In the science of geology this change manifested itself in the acceptance of the theory of Uniformitarianism over Catastrophism [1]. Ideas of progress and evolution, both political and biological, were formulated by Erasmus Darwin's work Zoonomia, Malthus in his Essay on the Principle of Population, both published at the end of the eighteenth century, and later on in the 1840s in Robert Chambers' anonymous work The Vestiges of the Natural History of Creation. [2] The latter work was instrumental in publicising an early naturalistic and evolutionary viewpoint claiming as it did that both the physical and biological sciences and the moral and social behaviour of the human race were all subject to natural law. This paved the way for more works on the theme of evolution culminating in Darwin's explanation of evolution in terms of natural selection in 1859. [3]

Gradually scientific explanation was taking over from the natural theology inherent in William Paley's Natural Theology, the Bridgewater Treatises and the work of William Whewell all published in the first forty years of the nineteenth century. [4] The need to view the natural world as a place which required the supervision and intervention of a Deity became less important around the middle of the century as the scientific world view gained cultural momentum aided by the new men of science described in this chapter. Voyages of discovery and exploration were undertaken, the visible results of science were perceived to afford the possibility of considerable social and technological ameliorations. The role of the scientist had moved away from the dilettante to the professional and out of this, one of the foremost problems of the age arose in the form of a new curiosity about the place of Humanity in the Nature which the new science was describing.

Hand in hand with these developments there grew up a wave of materialistic philosophies on the continent of Europe based on this new scientific world view, where God was no longer seen as the hub of explanations of the workings of the universe. This new philosophy took the form of Positivism in France (with a sect in Britain), materialism in Germany and scientific naturalism in Britain.

It is against this background, both historically and geographically that the doctrines of Victorian scientific naturalism which were advanced by a small group within the British scientific community, must be understood. The first part of this chapter deals with scientific naturalism itself, its principle tenets, and those scientists who can lay fair claim to be termed scientific naturalists. The last part of the chapter deals with the particular philosophical contributions of Huxley, Spencer and Tyndall towards the philosophy of scientific naturalism.

As the preceding chapter has shown, the reason why contemporary historians have failed to appreciate the reasons for Tyndall's involvement in the spontaneous generation controversy is because they see him as a materialist. Similarly Victorian critics often confused naturalism with materialism and Positivism. Yet it is clear from the writings of the scientific naturalists that they were anxious to avoid being termed either materialists or Positivists. Because of historical and contemporary confusion, I have adopted the novel strategy of making a direct comparison of these three movements. The comparison is made partly in terms of their philosophies; although there were similarities between their beliefs, there were also important differences. The second part of the comparison involves an identification of the membership of each group. In terms of membership, the three movements are sharply delineated. No other study of scientific naturalism has adopted this approach, but it is asserted that Victorian scientific naturalism cannot be properly understood unless it is located within its historical and philosophical context by a comparison such as this.

One of the worst offenders, in confusing the three philosophies, was A.J. Balfour. In Foundations of Belief he described the doctrine of scientific naturalism. "Agnosticism, Positivism, Empiricism, have all been used more or less correctly to describe this scheme of thought... the term which I shall commonly employ is Naturalism." [5] Much to Huxley's annoyance, the Archbishop of York had confused his philosophy with Positivism in 1868, which had prompted Huxley to renew his attack against Positivism in his lecture, "On the Physical Basis of Life". [6] This was not helped by the fact that Harrison, a leading Positivist claimed that Huxley's agnosticism was a preparatory stage on the way to Positivism. Most of the storm of criticism surrounding Tyndall's address to the B.A.A.S at Belfast in 1874 involved a charge of materialism. [7] In the face of contemporary and historical confusion reaching an understanding of scientific naturalism involves the strategy of both saying what it was and what it was not. Only then is the scene set for an understanding of the scientific naturalists' involvement in the topic of spontaneous generation.

The Principles of Scientific Naturalism

In its broadest form scientific naturalism was concerned with the action of natural scientific laws in the world and Humanity's place within this system. Nothing could be said about what lay outside or the "supernatural" and as such scientific explanation could not allow for divine intervention in the mechanism of the natural world. The concept of naturalistic explanation was important. In all the areas of scientific controversy with which the scientific naturalists involved themselves the issue of explanation can be seen as fundamental. In the spontaneous generation controversy this manifested itself as a denial that a living form could be evolved out of non-living matter as such a phenomenon was not amenable to naturalistic scientific explanation. Similarly the scientific naturalists disliked the concept of "vital force". There was no way science could explain how such a force entered a living body and how it left when the living thing died - such a concept violated the conservation of energy. Life was, at least potentially, to be described in terms of chemical and physical forces.

As Tyndall put it:-

> "... in the eye of science *the animal body* is just as much the product of molecular force as the stalk and ear of corn, or as the crystal of salt or sugar... And unless the existence of law in these matters be denied and the element of caprice introduced, we must conclude that, given the relation of any molecule of the body to its environment, its position in the body might be determined mathematically. [8]

The difficulty was seen to lie in the complexity rather than the type of problem. But the naturalists drew back from asserting that everything could be explained by molecular action for that was the path to materialism. Tyndall suggested that even if it were possible to determine with accuracy the molecular actions of the brain, mechanics could not solve the problem of the mechanism connecting the brain and consciousness. All the materialist could say was that the growth of the body and the action of the molecules within the mind was mechanical...

> "... but I do not think, in the present condition of the human mind, that he can pass beyond this position. I do not think he is entitled to say that his molecular groupings, and his molecular motions *explain* everything... The problem of the connection of body and soul is as insoluble in its modern form, as it was in the pre scientific ages." [9]

The tenets of scientific naturalism have traditionally been described in terms of the following set of scientific laws or principles:- evolution, the principle of continuity, the conservation of force or energy and the atomic theory. [10] However, it is clear that there was a considerable variation in what the individual scientific naturalists believed within these principles.

Although all the naturalists actively supported Darwin's theory, it is clear that at least Tyndall and Spencer held a much broader conception of evolution. Spencer's concept of evolution covered everything from the physical evolution of astronomical bodies to the evolution of organic life. [11] In fact, as Spencer believed that ancestral experience could shape characteristics to be inherited by future generations, his concept of evolution was Lamarckian and not strictly compatible with natural selection. In Tyndall's presidential address to the B.A.A.S. in 1874, he quoted both Darwin's natural selection and Spencer's hereditary experience theory within a few pages of each other. [12] Like Spencer, Tyndall was enthusiastic about the nebular hypothesis which described astronomical evolution. But the great virtue of evolution was that it provided naturalism with a possible historical account of the world and the place of the human race within it, which removed the necessity for a divine artificer. [13] The point about the variation in belief on evolution and the fact that individuals could hold contradictory beliefs serves to show that, as a group, they were prepared to tolerate such inconsistencies as long as the overriding principle of naturalistic explanation was adhered to.

The principle of continuity provided a description of the history of the earth in terms of a uniform, gradual process with no abrupt changes. The laws of science at work in the past were the same as those in operation at the present day. As with evolution, there were inconsistencies at work in their application of this principle. As Chapter 5 shows, in the spontaneous generation debate, Bastian pointed out that the scientific naturalists were violating the principle of continuity in their denial of spontaneous generation. However the denial of spontaneous generation was part of their more general belief in naturalistic explanation which was more important than possible minor violations of continuity.

The conservation of force or energy gave an explanation of the natural world which allowed for at least the possibility that science could eventually describe all actions in terms of matter, force and energy. While the conservation laws helped to shut out the notion of divine external interference in the fabrication of the world in terms of miracles or supernatural events, their universal application also denied the possibility of a belief in a special vital force. [14]

Finally, the atomic theory described the matter of which the universe was composed in terms of hard, round impenetrable solid balls whose kinetic action and chemical combinations could be described in purely mathematical terms and were subject to the conservation laws. This simplistic view, much of which derived in spirit from the ancient principles of Democritus, provided a plausible account at a time when little was known of sub-atomic structure. [15]

In choosing to hold these theories and principles the naturalists were attempting to extend the realm of scientific explanation. They were also redrawing the boundary, largely when and where they chose, between what was scientifically explicable and what was not. John Tyndall defended himself, by declaring which subjects were to fall under the rubric of scientific enquiry when, as a result of his notorious address to the B.A.A.S. at Belfast in 1874, he was accused of "quitting the domain of science and making an unjustifiable raid into the domain of theology". [16] But scientific naturalism was more than just a set of philosophical tenets, it was a radical world view. Although, as has been already suggested, the intellectual climate by the 1860s and 1870s was much more favourable towards naturalism, it did nevertheless present a tangible challenge to authority. The scientific naturalists did not generally see themselves as radical in an overtly political sense, although naturalism in a broad sense had become the idiom of political radicalism and was used to challenge the established church and more importantly the landed aristocracy. [17] The scientific naturalists were more involved in putting the new science to use in challenging traditional theological explanations of the world and replacing these by explanations which rested on scientific law. In this way they were challenging the status of the clergy as purveyors of true knowledge about the world - this role was now to be assumed by the new man of science.

Although not generally involved in party politics (except when Tyndall left the Liberal party and considered standing for Parliament as a Conservative candidate in 1885 in opposition to Gladstone's Irish Home Rule Bill) the scientific naturalists were not indifferent to the political implications of their philosophy. [18] By and large, as the next section shows, they were self-made men, most of whom came from relatively poor backgrounds, who had worked hard to educate themselves, to earn a living and to make names for themselves in the scientific world. Although the scientific naturalists respected most of the traditional institutions of society, they did not feel that birth or wealth should confer any privilege on an individual; talent alone was, in an ideal world, to be the only true measure of an individual's worth.

The Scientific Naturalists

The group of scientists which can be identified with scientific naturalism included in its ranks Thomas Henry Huxley who was Professor of Natural History and Palaeontology at the Royal School of Mines; John Tyndall, the Professor of Physics and later Superintendant of the Royal Institution and Herbert Spencer, a prolific writer and author of the widely read Synthetic Philosophy which covered a vast range of physical, biological and social sciences, who supported himself without the benefit of an academic post. These three were the most vocal exponents of naturalism and are the main subjects of this discussion of the philosophy.

Also associated with this group were Sir John Lubbock, entomologist and anthropologist, and the only one of them to come from a wealthy background; Sir Joseph Dalton Hooker, botanist and plant collector, who took over from his father as Director of Kew Gardens in 1865; Thomas Archer Hirst, Professor of Mathematical Physics then Pure Mathematics at University College, London and subsequently Director of Naval Studies at the Royal Naval College, Greenwich; Edward Frankland, chemist and water analyst, who became Professor of Chemistry at the Royal College of Chemistry in 1865.

Other scientists include:- Francis Galton, the eugenicist and statistician; George Henry Busk, originally trained in medicine but who turned to biological research, advised the Home Office on vivisection and became active in the administration of the Royal Society; Edwin Ray Lankester, Professor of Zoology at University College, London from 1874-1891 before going on to become Professor of Comparative Anatomy at Oxford then Director of Natural History departments at the British Museum; and William Kingdon Clifford, elected Professor of Applied Mathematics at University College in 1868. These men were scientific naturalists, but they were more peripheral to the main group and are not discussed in detail here. In fact scientific naturalism can loosely be identified with the membership of the X-Club, formed as a dining club in 1864, where the affairs of science were discussed at its monthly meetings. The X-Club had been formed explicitly to keep these busy men in touch with the activities of each other; it rapidly became influential in the affairs of both the Royal Society and the British Association. [19]

The core group of scientific naturalists were an extraordinarily close knit group, having met at fairly early stages in their struggle to seek employment as scientists, they remained friends to the end of their lives. They advised each other in choice of career, sometimes studied together and holidayed together, even lent each other money and supported each other in the many scientific controversies in which they became involved.

Scientific naturalism was associated with a relatively small subset of the larger scientific community but it is notable that there was a certain commonality in background and experience of the careers of the most active members of the group. It is suggested that an examination of these experiences can provide a tool to analyse and shed light on the reasons why scientific naturalism held such an appeal for these men especially as the significance of their backgrounds has not generally been understood. In particular, the cases of Tyndall, Huxley, Frankland and Spencer are most informative as these were the most prominent members of the group and were, in that order, most closely involved with the spontaneous generation debate.

Lubbock came from a wealthy background. He was educated at Eton before joining the family bank. His father had been a mathematician and was also an F.R.S. His own studies were self-directed and he was an active educator and publicist for Darwinism. Of the scientific naturalists, Lubbock was nearest to the old-style amateur. Hooker had also come from a scientific background and had studied medicine at the University of Glasgow. He made several journeys of exploration, both to observe the geographical distribution of plant species and to collect plants for Kew where he spent most of his working life.

Both Lubbock and Hooker came from scientific families and both came from backgrounds where the financial situation was such that they did not have to support themselves by apprenticeships unlike most of the other members of this group. The other scientific naturalists all originated from less wealthy circumstances and all had earned their own livings from an early age before turning to science as a career. Hirst was the son of a Yorkshire wool-stapler and in 1844, at the age of fourteen he became a railway surveyor in Halifax. [20] It was at Halifax, as surveyors that Hirst and Tyndall first met and formed their life-long friendship. Hirst taught with Tyndall at Queenwood before following him to Marburg to study under Bunsen.

Tyndall, the son of a small landowner from County Carlow in Ireland became a surveyor and then a railway engineer before teaching at Queenwood and going on to the University of Marburg in 1848. Frankland was the illegitimate son of a calico printer. He was apprenticed to a druggist for five years before becoming an assistant in Playfair's laboratory at the Government Museum of Economic Geology. He too taught science at Queenwood and studied with Bunsen and later Liebig, after which he came back to Britain to a succession of teaching posts before settling at the Royal College of Chemistry. Spencer's father was a teacher and a small landowner and he too had been launched into life as a railway engineer and was entirely self educated. Huxley was the son of an assistant schoolmaster from Ealing and had been apprenticed to his brother-in-law, a surgeon, which stimulated his interest in human anatomy and physiology. In 1845 he graduated from London University, having held a scholarship in Charing Cross Hospital and was appointed assistant surgeon to H.M.S. Rattlesnake in 1846.

Characteristic of the early careers of Huxley, Tyndall, Spencer, Frankland and Hirst was the fact that none of them enjoyed a traditional education and were largely self taught. It is unsurprising that three of these men began their careers on the railways - the railway boom of the 1840s provided attractive opportunities for young men whose families could not afford to launch them into traditional professions. They often had a long wait to gain their scientific education - witness Frankland's dreary and wearying five years as an apprentice to a pharmacist. [21] Lack of money was, of course, a particular factor in this. Tyndall's father had once written to him, "The only thing that prevented you from going through the degrees of college was my poverty..." [22] Tyndall financed his time at Marburg with the few hundred pounds he had managed to save from his work on the railways and small loans from his friend, Hirst. It was an austere life indeed, rising at an early hour in the morning and working late into the night to achieve in two years the work normally completed in three. Aside from perhaps their own more genteel brand of poverty these men had come face to face with poverty of a more desperate kind; Huxley in his medical work in London and Tyndall as an engineer in Preston witnessing the riots of 1842.

Tyndall, Hirst and Frankland had all studied with Bunsen at Marburg in the late 1840s and early 1850s. For these three this was their only period of formal higher education. Judging by the number of British chemists who studied in Germany, the superior opportunities for scientific education, and research had become well known in Britain. [23] The German university system offered the opportunity to study for the research degree of Doctor of Philosophy which was not available in Britain. During their studies in Germany, these three were able to see what was possible with an organised system of scientific education, properly funded by the state and with successful research schools such as Liebig's at Geissen. Some of this achievement was accounted for by organisation rather than purely by money alone. If a professor was not overburdened by teaching duties and did not have to supplement a meagre salary by many publications then there was obviously more time for research even with the minimum of scientific equipment. Tyndall's description of Bunsen's laboratory shows what it was possible to achieve with a modestly endowed laboratory under the supervision of a brilliant man. [24]

It was also at about this time that scientific materialism was at its height in Germany. The three British scientists could see that in this climate scientific education, research and knowledge could thrive free from theological dogma. Small wonder that they brought back to Britain many of the values of the German system of education and research which makes Passmore's view of scientific naturalism as "German materialism in English clothing" all the more credible. [25] None of these three set up a research school along German lines, but their period of German education fuelled their discontent when reasonably paid scientific posts were hard to come by upon their return to Britain. The scientific naturalists concern with the state of scientific education and research in Britain must be understood in the light of the fact that three members of the group had first-hand experience of working in a country where the situation was very different.

The similarity of experience of the central core of the scientific naturalists is striking, coming from that section of the middle classes which was respectable but impoverished, having an early sense of intellectual disenfranchisement, inferiority and uncertainty resulting in a hard struggle to obtain both an education and a scientific post with enough remuneration on which to live. They were the entrepreneurs of the scientific world struggling to rise up the social scale as surely as their counterparts in the realm of commerce.

All the group chose to live in or near London. Scientific naturalism was very much a metropolitan phenomenon. There seems to have been a very real desire on the part of the core group of scientific naturalists to stay in close contact with each other and with institutions such as the Royal Society, and London was seen as the centre of scientific activity. Tyndall's application for posts in Ireland and his joint application with Huxley for posts at Toronto and the new University of Sydney, New South Wales, had all been unsuccessful. [26] For Huxley and Tyndall it became a necessity to be at the centre of things in London to take up the cause of public education and the dissemination of scientific knowledge. In 1853 Tyndall, still teaching at Queenwood College, wrote to Huxley whom he had met and befriended at the Ipswich meeting of the B.A.A.S. two years earlier, to urge him for advice on his career. There was the possibility of a post at the London Institution; the Royal Institution was more to his taste but he was wary of having to give lectures in chemistry.

Huxley said of the Royal Institution:-

> "This is where, as I told you, you ought to be - looking to Faraday's place. Have no scruple about your chemical knowledge; you won't be required to train a college of students in abstruse analyses....What they want, and what you have, are *clear powers of exposition* - so clear that people may think they understand even if they don't. That is the secret of Faraday's success, for not a tithe of the people who go to hear him really understand him... It is of great importance to look to this point in London - to be unshackled by anything that may prevent you taking the highest places, and it was only my fear on this head that made me advise you to hesitate about the London Institution. More consideration leads me to say, take that, if it will bring you up to London at once, so that you may hammer your reputation while it is hot." [27]

Discouraged by the lack of interest which the Trustees of Owens College in Manchester displayed towards science, Frankland left the chair of chemistry there to take an ostensibly less prestigious post as lecturer in chemistry at St Bartholomew's Hospital, London. [28] When the Chair of Natural Philosophy fell vacant at Edinburgh University in 1859, and as the Chair was worth about £1250 a year, Tyndall was tempted. But he decided not to apply as such an appointment would have taken him away from the heart of metropolitan scientific activity. He wrote to Playfair declining to stand for election on the basis that in London, "I am here close to the heart of England and in the midst of my personal friends." [29]

The challenge which the proponents of scientific naturalism were presenting to orthodox culture was two-fold. On the one hand they were marshalling the tangible success of science in a spirit of education and popularisation in an attempt to wrest cultural hegemony from traditional religious and academic circles, but further to this they offered a challenge to more traditional scientists too. These were the new men of science, the culturally marginal, anxious to carve out for themselves a new role of the scientific professional, gaining cultural leadership through hard work and ability alone rather than by a fortunate birth. They accepted the present structure of society but felt that individuals should rise and fall within that structure according to ability and education; their analogy was to be found in the "self made man" of the bourgeoisie - these were the "self made men" of science.

Amongst scientists their largest body of critics was perhaps the group of Cambridge physicists who contained within their numbers some of the best known names in natural philosophy from the middle to the end of the nineteenth century. The members of this group included J. Clerk Maxwell, P.G. Tait, Balfour Stewart and later J.J. Thomson and Oliver Lodge, who had in common the benefit of an education in mathematics and natural philosophy at the University of Cambridge. [30] They held posts at Oxford, Cambridge, the ancient Scottish Universities and the newer Victorian universities such as Liverpool and Owens College, Manchester. Metaphysical in style, this group looked towards the ether as a fundamental constituent of matter; the emphasis of their science was on the continuous rather than the atomic in matter. Their philosophy derived from Scottish Common Sense philosophy which emphasised the harmony between God and Nature. [31] They tended to have an open if not positive attitude to scientific investigation of the psychic and supernatural which stands in marked contrast to the attitudes of Huxley and Tyndall which varied from dismissal to ridicule. [32] The Society for Psychical Research is closely associated with many members of this group. [33]

There was no need for these scientists to challenge religious culture as their own positions were secure with respect to the traditional institutions of both church and universities; rather they chose to incorporate a scientific view of the supernatural and to defend this, where necessary from the attacks of the scientific naturalists. One such defence can be seen in the work The Unseen Universe published anonymously by P.G. Tait and Balfour Stewart in 1874, largely in response to Tyndall's notorious Belfast Address. [34] In this the authors claimed that the principle of continuity and conservation of energy applied to both the visible and unseen realms, that energy transfers were possible between the two and this was how miracles could potentially be explained by science. Such a view, by involving knowledge of an unobservable world, was anathema to the scientfic naturalists.

Naturalism and Positivism

In its faith in science to improve human existence, Victorian scientific naturalism was very much a philosophy of its age. It was certainly not universally accepted in scientific circles nor was it the only popular philosophy in Britain to rest on the foundations of scientific optimism.

A comparison between naturalism, Positivism and materialism can be made at two different levels, because, as it has already been suggested, naturalism can only be understood in terms of both the philosophy itself and the group who subscribed to that philosophy. On the one hand a purely philosophical identification can be made of the epistemologies of these three. However further to this, an essential and more original part of the comparison involves a sociological perspective including not only identifying the groups of actors involved in each movement or philosophy, but also where possible examining the forces at work which produced common interests within a group and how these in turn affected the kinds of problems the group's members chose to address. Turning to Positivism, the views of this group in regard to science and its relationship to religion and also how the subject matter of controversies and debates with which they chose to become involved was influenced will now be examined. Scientific naturalism has received this two-fold treatment above and now Positivism is discussed in the same way.

Positivism was epistemologically similar to naturalism in its shared faith and optimism in science but differed in that it was taken explicitly by its followers as a tool for social reform and a substitute for religion. Positivistic philosophy had been largely derived from the works of the French philosopher Auguste Comte (1798-1857) who had initially been influenced by Saint-Simon's projects for reform but split from him in later years.

Comte's awareness of the need for order and classification led him to the Law of Three States which split the development of knowledge into theological, metaphysical, and positive stages. [35] The sciences were to form a natural order according to their stage of development;

sociology was to be the final element. Comte's religion of humanity recognised that certain institutions were fundamental to social order and so he was anxious to retain the rituals of Catholicism to satisfy humanity's basic needs for religious institutions. [36] Positivism held that scientific facts are the only type of knowledge that is valid and that it was the task of philosophy to find the basis of all science so that these principles could be used in practical utilitarian projects within social organisations and also so that metaphysics could be finally banished. Positivism rejected ontological speculation and thereby strict materialism. Nothing could be known of the ultimate nature of reality.

Although Positivism implied an optimistic faith in the natural progress of humanity it did not emphasise evolution as Comte thought that evolution was inconsistent with the ideal of permanent classification. He emphasised that knowledge should be capable of empirical verification. This meant that although the importance of the scientific "fact" was recognised, the Positivists were much less willing to accept many of the scientific laws and principles so central to the doctrines of naturalism. For instance Comte argued against attempts to introduce mathematics into chemistry and refused to accept Dalton's atomic theory as an actual description of reality. [37] Adherants of the positivistic philosophy would only accept the atomic theory as a useful artificial construct or generalisation. In nineteenth century Britain rejection or scepticism of atomic theory sometimes resulted in this sort of "operationalism" which stands in contrast to the ontological claims of naturalism as represented in Tyndall's thought. [38]

> "Many chemists of the present day refuse to speak of atoms and molecules as real things. Their caution leads them to stop short of the clear, sharp, mechanically intelligible atomic theory ...... and to make the doctrine of multiple proportions their intellectual bourne. I respect their caution, though I think it here misplaced." [39]

Whilst Comte had a considerable following in France, the British movement, which had centred round a small group in London, never grew to large proportions. It was founded by Richard Congreve (1818-1899) who originally held a fellowship at Wadham College, Oxford. He was greatly influenced by his meeting with Comte in 1848 and after several years planning he resigned his Oxford post and went to London to set up a Positivist community, founding the London Positivist Society in 1867 and the Positivist School in 1870. [40] Within this group were three men, taught by Congreve in Oxford, who formed the nucleus of the Positivist movement; E. S. Beesly, the Professor of History at University College, London; John Henry Bridges (1832-1906), factory and medical inspector and an active lecturer; and Frederic Harrison (1831-1923) who at various times lectured at the Working Men's College, served on several Royal Commissions and was a prolific writer. [41]

In 1878 the British Positivist movement split, ostensibly because Harrison, Beesly and Bridges supported the authority of Comte's literary executor, Lafitte, against the claims of Congreve to lead the Positivist Community, but also because of the long standing differences between Harrison and Congreve which were due in part to Congreve's dislike of the public controversy brought about by Harrison's polemic with Huxley. As a result of this Harrison opened a new meeting place in 1881 and went on to found the Positivist Review in 1893; meanwhile Congreve began to introduce the more ritualised style which he favoured into his brand of Positivism.

Huxley's first public attack on Positivism was occasioned in 1868 in his address "On the Physical Basis of Life" in Edinburgh. Having had his "new philosophy" confused with the Positivist Philosophy of Comte in the address "On the Limits of Physical Enquiry" given the preceding day by the Archbishop of York, he chose this forum to attack Comte's views and coined the famous phrase "Catholicism *minus* Christianity" to describe the philosophy of the Positivists. [42] Over the next twenty-five years there were intermittent bouts of controversy between Harrison and Huxley, much of which was conducted in meetings of the Metaphysical Society. Founded in 1869, the Metaphysical Society provided a unique common forum for the discussion and debate of clerics, scientists and philosophers. [43] Harrison claimed that Huxley's agnosticism was shallow and negative and offered nothing in place of the ancient institutions it attacked. His suggestion that agnosticism was a preparatory stage for and would give way to Positivism was a source of considerable annoyance to Huxley.

Huxley's attack on Positivism can be understood on three fronts.

a) Catholicism
b) Marginality
c) Hero Worship

a) The Catholicism and hence the ritualised aspect of Positivism was one of the major reasons contributing to Huxley's dislike of the philosophy. Although he was an agnostic, Huxley presented no radical challenge to the moral and ethical beliefs with which he was brought up; he found that "a deep sense of religion was compatible with the entire absence of theology". [44] Hence, to his way of thinking, Positivism represented a system which was all theology and no religion. Further to this, a good part of the scientific naturalists' polemic against religious orthodoxy was directed specifically to the Catholic Church's control over education and culture. In particular, Tyndall spoke out against the Catholic University of Ireland and Catholicism in Germany in 1874 claiming that no Irish Catholics were associated with the advance of the physical and natural sciences due to "the pressure exercised for centuries by the Jesuitical system, which has crushed out of Catholics every tendency to free mental productiveness." [45]

b) The marginality of Positivism with respect to the science of the day must be understood in two ways; they were marginal to both the emerging body of professional scientists and marginal to actual scientific doctrine. Although many of those at the forefront of the Positivist movement were practising lecturers and teachers, in contrast to the naturalists, none were active scientists. It is commonplace within emerging professional groups, such as the scientific profession, to edge out marginal members on the grounds of lack of professional competence. [46] Huxley felt that the writings of the leader of the movement, Comte, displayed an ignorance of actual scientific laws and principles and that his classification of the sciences did not hold true. According to Huxley, Comte had been an undistinguished mathematics teacher with only an amateur's understanding of the sciences. [47] Similarly Harrison lacked a suitable understanding of the physical sciences and philosphy which would enable him to understand agnosticism. [48] Huxley probably felt that the hybrid nature of Positivism did nothing to further the cause of science and probably hindered it by dragging in the worst bits of religion. The Positivist religion was, for him, no more than an "incongruous mixture of bad science with eviscerated papistry." [49]

There were occasions when the Positivists adopted a point of view which to the scientific naturalists must have seemed as if they were deliberately standing in the way of scientific progress. A case in point was the attitude of Bridges and Congreve towards vivisection. They had spoken out against experiments on living animals on the basis that anatomical research or clinical observation would yield the same conclusions. [50] As Chapter 7 describes, the scientific naturalists supported the cause of vivisection because they saw such experiments as fundamental to the progress of the science of physiology.

Unlike the naturalists, the Positivists did not find it necessary to place an emphasis on the fundamental laws of science. Comte's scepticism over the actual existence of atoms can be seen as an intellectual forerunner of the operationalism and empiricism of Mach and later the Vienna Circle. Although the agnosticism, supposed dislike of metaphysics and insistence on the observable found within the thought of the naturalists, might have implied a similar scepticism, there were in fact a number of circumstances where the one or other of the scientific naturalists, and especially Tyndall, professed a belief in an entity which was quite clearly beyond the realm of the observable. The atomic theory was a case in point; further examples are to be found in his treatment of the nebular hypothesis, Spencer's "Unknowable", the germ theory and even aspects of evolution. It would be fair to say that there were contradictions in their attitudes.

However whereas Positivism could offer its audience the spiritual comforts of traditional religious institutions and perhaps did not have to worry too much about the quality of science it offered, scientific naturalism had neither the attractions of the rituals of religion nor the offer of a supernatural world; only a strong faith in the laws, principles and facts of science as real objects served to cushion it from a cold, bare utilitarianism. With their own problems of cultural and perhaps intellectual marginality the naturalists were trying to convince themselves as well as their audience. Even Spencer's highly general account of evolution was tolerated by the other scientific naturalists when it came up against the Conservation of Energy and the Second Law of Thermodynamics as it was in the spirit of what the naturalists were trying to achieve. An appeal to faith in science, and especially physical science was possible before the certainty of scientific laws was called into question by the discovery of relativity and sub-atomic particles.

(c) Finally, there was a third reason for Huxley's polemic. He had already attacked Comte's views as early as 1854 in "On the Educational Value of the Natural History of Sciences", but further to this his intensified attack in 1868 to be found in "On the Physical Basis of Life" resulted not only from his irritation at the Archbishop of York's confusion but also from his adverse view of hero worship awakened by the Eyre Controversy of 1866. [51] By then, Huxley was aware of the totalitarian nature of the order and classification within Comte's teaching which could result in despotic authority. In rejecting the concept of hero worship he found Comte's worship of humanity "little more than a variation of the idea of a deity and a new form of anthropomorphism and hero worship". [52]

However, it would not be correct to say that the attitude of scientific naturalism to hero worship was one of united opposition. In particular, Tyndall, greatly influenced by Thomas Carlyle's work, took an opposing view to Huxley in the Eyre controversy, but of course this did not mean that Tyndall viewed Positivism more favourably than did Huxley. Furthermore the attempts of this group to gain cultural leadership can be seen as an attempt to establish a new type of scientific "hero" leading humanity from the darkness of theology and metaphysics to the illumination of the New Science. But this view of the "hero" was not based on authoritarianism, for the new heroes were to be those who gained their place in society through hard work, application and talent and not by birth or wealth. In his essay, "Administrative Nihilism" , Huxley argued that people should be allowed to fall or rise through the social ranks according to their natural ability which was to be encouraged by a good education for everyone. [53]

> "We have all known noble lords who would have been coachmen, or gamekeepers, or billiard-markers, if they had not been kept afloat by our social corks; we have all known men among the lower ranks, of whom every one has said, "What might not that man have become, if he only had a little education ?" " [54]

In statements like this the naturalists obviously meant to include their own situations.

German Scientific Materialism

In contrast to the Positivist movement which had its origins largely in France and gained a following across the channel, the more materialistic philosophy also associated with science in the mid-nineteenth century had its roots in Germany.

Ludwig Buchner (1824-1899), a medical doctor, and the physiologists, Karl Vogt (1817-1895) and Jacob Moleschott (1822-1893) are associated with the popularisation of the German scientific materialism of the 1850s. These men were contemporaries of the British scientific naturalists but the main flowering of German materialism was in the late 1840s and 1850s. Buchner's Kraft und Stoff (1855), the "Bible of the materialists", underwent 12 German editions in 17 years and was translated into several European languages. [55] Haeckel, although not associated with this group, was also known as a German materialist. He was greatly influenced by Darwinian evolution, much more so than the earlier materialists; his work on protoplasm was well known to the British naturalists and his later book The Riddle of the Universe was widely read in Britain. [56]

As the shift of scientific focus began turning towards Germany in the 1840s, scientists there moved away from the old metaphysical *Naturphilosophie* towards the new scientific materialism which arose in response to this "official" idealistic philosophy. Materialism was a metaphysical position characterised by its denial of any mode of existence independent of material entities. The tone of the materialist message was one of optimism and a faith in science amongst the pessimism of post-revolutionary Germany and their atheism and criticism of authority were founded in the results of science rather than in philosophy or theology. In basing its philosophy on science, like British naturalism, German materialism fell victim to similar criticisms from philosophers, theologians, other natural scientists and lay writers.

As a central part of its doctrine, materialism held in common with naturalism a refusal to acknowledge capricious forces directing processes from outside nature and hence a dislike of the concept of "vital force". Although Buchner believed that force, like matter, was indestructible, the concept of the conservation of energy, so central to scientific naturalism, was not taken up unilaterally by the German materialists. And because the materialists refused to consider organic matter as in any way special, they also favoured the idea that life had been produced from inorganic matter. Vogt, unsure about spontaneous generation from organic materials, believed in the possibility of spontaneous generation from inorganic matter. Buchner held a more general belief in the possibility of spontaneous generation.

When Darwin's The Origin of Species was published in 1859 it failed to create the same sensation in Germany as it had in Britain partly because the scientific materialists had already paved the way for potentially radical ideas. However, the materialists tended to incorporate Darwin into their own systems of thought in one shape or another. Moleschott, in particular, thought that Darwin's book had completed the work which Lamarck had begun. As the materialists had a strong commitment to progress, the Lamarckian belief in the inheritance of acquired characteristics probably held a stronger appeal than a concept of natural selection which was more difficult to reconcile with the idea of progress. As Gregory suggests, "A great deal of the materialists' attraction to Darwin was not so much the uniqueness and power of Darwin's ideas as the omission of reference to a personal creator in Darwin's work. After Darwin the idea of creation, so bothersome to non-religious minds, was provided with a respectable alternative." [57] A more profound influence from Darwin's work was exerted on Haeckel, the later evolutionary philosopher.

Positivism, Naturalism and Materialism - Some Conclusions

The previous section addressed some of the more specific reasons for the mutual dissent of Positivism and scientific naturalism: this section attempts a more general comparison of the three philosophies, Positivism, naturalism and materialism.

It is fair to say that all three had historically similar roots; all three systems of thought arose at a time when the explanation of the natural world was beginning to be encompassed by the domain of the scientist rather than the theologian. The image of the scientist was becoming that of the professional educator and researcher, although there were still difficulties in Britain in obtaining a state funded post, and also science could be linked to the results of better health and the rise of industrialisation. [58] All three movements centred broadly round the aim of promoting this new science and were criticised by the same types of people - theologians, lay writers, other scientists and academics. All three movements, and more especially naturalism and materialism, were radical. They challenged established authority by implying that the traditional order of society was not necessarily a natural order and that individuals, such as themselves, could rise within the ranks of society on the basis of ability rather than birth.

At this point these philosophies diverge. Positivism's "religion of science", naturalism's agnosticism and the atheism of the materialists were three substantially different responses to the problem of incorporating an attitude to established religion with a faith in science. Whilst the creed of Positivism gained a small following in Britain, such a ritualised philosophy would never have served the scientific naturalists who, in their attempt to gain cultural hegemony were trying to replace the old order of theology with the new image of science. On the other hand the outright atheism of the German materialists would have been unthinkable in an evangelical country like Britain; were the scientific naturalists to adopt such a position they could hardly expect the support of the educated middle classes who turned out in their numbers to hear their public lectures and who purchased their books of essays. The agnosticism of the naturalists was very much the British response to a question of that age. On occasion Tyndall sailed rather close to the wind and incurred charges of materialism and atheism which although they brought him a certain notoriety were clearly fundamentally damaging to his scientific reputation. [59]

As well as the differing response to religious questions, these three philosophies had different attachments to actual scientific laws and principles. As already noted, Positivism, further removed from actual scientific practice than the other two, hinged less on a strict belief in scientific laws than did materialism and to a larger extent naturalism. Evolution, the lynchpin of naturalism, was less important for materialism and actually antithetical to Positivism. To a degree for naturalism, evolution replaced the requirement for a strong feeling of progress. This was often an important concept for new and radical ideologies which permitted them both to shake off the old order of theology and to offer their followers the hope that society could change for the better. The concept of progress was evident in scientific materialism. But whereas Positivism offered the definite hope of social progress this was largely absent in naturalism. Evolution was taken to imply gradual changes in species to better fit their environment where the concept of struggle and survival and its counterpart in social terms in the doctrine of "laissez faire" showed that while naturalism could offer the benefits of science as short term expedients to the human race it had difficulty in finding any longer term purpose.

Similarly whilst atomism was an important tenet of naturalism which was taken to have a definite ontological significance it was seen as little more than a useful if artificial construct for Positivism.

A belief in spontaneous generation is often linked to materialism as this philosophy saw no fundamental difference between organic and inorganic matter. By contrast the scientific naturalists unequivocally rejected the possibility of spontaneous generation. Their rejection (examined in detail in Chapters 3 and 4) was bound to a number of issues. The main problem was that spontaneous generation just could not be explained in naturalistic terms. Furthermore there was the threat such a doctrine implied to the germ theory of disease and ultimately, as Tyndall saw it, the acceptance of scientific medicine.

As a final point to the comparison of the three philosophies the different national and social origins of those associated with each philosophy should be emphasised. It has already been noted that this had a distinct bearing on how each group acted. Scientific materialism was very much a German phenomenon, at its zenith in the 1850s, a decade or more before scientific naturalism became popular in Britain; Positivism arose in France and spread to a small group in Britain though not, in the main, to practising scientists. Whereas there were scientific naturalists and Positivists in Britain, few British scientists would have cared to be identified as materialists in anything like the German sense, rather there were a few individuals who had leanings in that direction. Henry Charlton Bastian, Professor of Pathological Anatomy at University College, London, and the main proponent of spontaneous generation was perhaps as close to a materialist as it was possible to be in Britain. But Bastian never called himself a materialist, he preferred the term "evolutionist" to describe his commitment to the belief that living organisms could evolve from non-living matter.

The Variations of Naturalism

Although united in broad utilitarian aims to educate and improve, it would be wrong to suggest that naturalism was an explicit creed; there was a considerable amount of individual variation within what the naturalists chose to believe and emphasise and their contributions to the overall body of thought. This section briefly examines the beliefs of Huxley, Spencer and Tyndall who are seen to be most central to the doctrine. All three were involved to a greater or lesser degree with the spontaneous generation debate.

T.H. Huxley, the most well known figure of this movement and champion of Darwinian evolution, was an eloquent and accomplished lecturer. He saw himself as an intellectual descendant of Hume and Kant and followed the latter in believing that the role of philosophy was to delimit rather than enlarge knowledge, to prevent error rather than discover truth. [60] The agnosticism which was broadly characteristic of naturalism sprang mostly from the mind of Huxley. He described how he coined the term to describe his philosophy as a result of attending meetings of the Metaphysical Society.

> "Every variety of philosophical and theological opinion was represented there, and expressed itself with entire openness; most of my colleagues were *-ists* of one sort or another; ... I, the man without a rag of a label to cover himself with, could not fail to have some ... uneasy feelings... So I took thought and invented what I conceived to be the appropriate title of "agnostic." It came into my head as suggestively antithetic to the "gnostic" of church history, who professed to know so much about the very things of which I was ignorant; and I took the earliest opportunity of parading it at our Society... To my great satisfaction, the term took..." [61]

Agnosticism was not a negative creed, nor even a creed at all, rather it was a statement of principle. For Huxley, agnosticism was :-

> "..in the application of a single principle, which is the fundamental axiom of modern science. Positively, this principle may be thus expressed: in matters of the intellect, follow your reason as far as it will take you, without regard to any other consideration. And negatively: in matters of the intellect, do not pretend that conclusions are certain which are not demonstrated or demonstrable." [62]

Huxley believed that it was wrong for a man to say that he was certain of the truth of any proposition unless he could provide evidence to justify that certainty. But the extent of that region of uncertainty depended on the individual so he did not care for Spencer's concept of a formal "Unknowable". [63]

Huxley's statement of agnosticism was the expression of a prescriptive norm rather than a description of how agnosticism was actually to be applied. The naturalists tended to redraw the boundaries between science and speculation if not where it suited them, at least to accommodate certain beliefs where scientific proof was lacking. They were not agnostic with respect to unproven doctrines such as the atomic theory and the germ theory. Their habit of choosing the boundaries of their subject often made it difficult to criticise their position. However at least one critic felt that the agnostic principle was an attempt at intellectual honesty to steer a middle course between the misconceptions of both idealism and materialism. Of agnosticism and naturalism A.R. Wallace said:

> "Its faults... spring from a creditable motive. It is the desire to be honest, to say only what you can prove, to require thorough continuity and consistency in the whole realm of accepted truths. Naturalism was a reaction to the follies of supernaturalism." [64]

However not every critic was as charitable as Wallace. Part of the long standing dispute between Huxley and Harrison was due to the latter's dismissal of agnosticism as a negative stage in the evolution of religion; one that would pass away on the road to Positivism. [65] The most perceptive criticism of agnosticism and naturalism was made by James Ward in his Gifford Lectures of 1896-1898. [66] Ward understood that scientific naturalism was not the same as materialism. Although scientific naturalists used materialistic terminology, agnosticism was a break with the old materialism and therefore offered a distinct advance. [67] But naturalism had to commit itself to what was real in the universe and this it could not do. The agnostic said it was futile to separate reality from appearance because nothing could be known of this distinction.

Herbert Spencer set out to produce a complete system of Synthetic Philosophy to cover all knowledge from physical to social phenomena of which his First Principles was the initial volume. [68] In this he attempted to bring together all scientific knowledge through the medium of evolutionary theory. Fundamental to his evolutionary view was the doctrine of the "Unknowable". The existence of two worlds was postulated - a phenomenal world dealing with the experiences of the human being with reality, and the Unknowable world which is part of reality but of which nothing can be said. So it is not known whether God exists or does not but it cannot be said that God does not exist. The "Unknowable", a concept he may have derived from Kant, was Spencer's brand of agnosticism where he subsumed the "not knowing" into a more formal conception than Huxley had done. Amongst the knowable were the physical laws and facts such as the indestructability of matter and the "Persistance of Force" - a term which Tyndall wanted him to change to "Conservation of Energy". [69]

The most fundamental part of his philosophy was based on evolution - but his view of evolution was of a much more general form than the biological mechanism and it was to apply everywhere in nature. His definition of evolution became famous.

> "Evolution is an integration of matter and concomitant dissipation of motion; during which the matter passes from an indefinite, incoherent homogeneity to a definite, coherent heterogeneity; and during which the retained motion undergoes a parallel transformation." [70]

Evolution was held to range in scope from the simple evolution of two chemicals to form a compound, to the astronomical evolution of the solar system (according to the nebular hypothesis) and to the higher forms of biological evolution of animals and the human race. Although the second law of thermodynamics, in proposing that order within the universe is gradually dissipated, forced him to modify some of his assertions, the general spirit of his philosophy remained unchanged. Furthermore, in its initial conceptions, his work did not derive from Darwin's evolution although it arose in response to similar problems. Spencer's view of biological evolution was more Lamarckian in tone, holding as he did, that the accumulation of ancestral experiences in the human race gives rise to an *a priori* knowledge of cause and effect which in turn leads to an *a priori* knowledge of the persistance of force. [71] Spencer's views of evolution were essentially optimistic. Progress was possible; humanity could improve itself and pass on improvements to posterity. This stood in contrast to Darwinian evolution where a concept of progress did not exist. Posterity only inherited random variations which better fitted the organism to the environment.

Of the scientific naturalists, John Tyndall was the most outspoken controversialist; less successful than Huxley in steering a middle course of agnosticism between materialism and idealism, it was his lot to be misunderstood on frequent occasions. The breadth of his scientific work was diverse and he had a knack of drawing controversy to himself; in particular he engaged in more than one bout of controversy with P.G. Tait, the Professor of Natural Philosophy at Edinburgh University and a leading member of the "Cambridge physicists"; the tone of their letters becoming so vituperative and personal in their priority dispute over glacier theory, that Norman Lockyer declined to publish further correspondence on the matter in his journal, Nature. [72]

Like Huxley, Tyndall had been charged with materialism, but these charges were incurred as a result of misunderstanding his statements on the power of science. Clearly Tyndall wanted to believe that one day science would explain everything but had to draw back and concede that it could not, thus displaying the ambivalence the naturalists felt on the potency of science and their inability to come to terms with any explanations of final causes. These rather ambiguous statements were taken as statements of materialism rather than attempts to meet with doubts which is how they were intended.

Tyndall's Presidential Address to the B.A.A.S. at Belfast in 1874 probably represented the most controversial episode of his career. He upset his audience and angered religious circles by his exposition of what was seen to be a materialistic viewpoint. As a result of this he was publicly branded an atheist, a charge which he never succeeded in refuting. However, his speech can be seen as a grand statement of faith in science through its natural laws - evolution, atomic theory and the conservation of energy which he set in an historical perspective - in fact it was an embodiment of the naturalistic ethos. The disturbing nature of his words lay both in the challenge to religious culture and his presentation of the highest achievements of humanity as scientific achievements contributing to the formation of naturalistic laws.

In his address he stated that the problem of primordial life was only to be solved by the conception of a creative act or by a radical alteration in the concept of matter. The following paragraph is quoted by Tyndall's biographers as particularly upsetting for his audience especially as he was at the time involved with the fight against spontaneous generation.

> "Believing as I do, in the continuity of Nature, I cannot stop abruptly where our microscopes cease to be of use. Here the vision of the mind supplements the vision of the eye. By an intellectual necessity I cross the boundary of experimental evidence, and discern in that Matter, which we, in our ignorance of its latent powers, and not withstanding our professed reverance for its Creator, have hitherto covered with opprobrium, the promise and potency of all terrestrial life." [73]

He did not mean this passage to be a refutation of his ideas on spontaneous generation (particularly as he went on to disclaim the generation of life without an antecedant in the next few paragraphs) although it is easy to understand why he was misunderstood. These words were rather a reference to his pantheistic beliefs. It has been suggested that his predilection for superficial metaphysical speculation "led him into a sort of pantheistic belief that all of the material universe was infused with life." [74] Tyndall's biographers claim that his "materialism" was misunderstood as "he endowed matter with the potency of feelings of Awe, Reverance, Morals and Religion which showed he attributed to matter what most people attributed to God." [75] This unusual twist to the meaning of pantheism was Tyndall's variation on the theme of Huxley's agnosticism and Spencer's "Unknowable". But in Tyndall's case the pragmatic agnosticism of Huxley is absent; he had to let these matters rest on belief rather than proof.

Tyndall's pantheism was an essential part of his belief in the doctrine of the nebular hypothesis which had originated with Laplace and had enjoyed varying degrees of popularity during the nineteenth century. Spencer saw the nebular hypothesis as a part of evolution - namely the evolution of nebular matter into the sun and planets of the solar system and for Tyndall it provided the useful idea that the basis of life had existed in the primordial nebula from which life began to form when the earth condensed and became habitable. On the one hand the naturalists did not wish to appeal to a special creative act to start life on earth but on the other hand they were anxious that spontaneous generation should not be used as an explanation for the initial appearance of life. As Chapter 5 explores in more detail, for Tyndall at least, the nebular hypothesis and pantheism took the question of creation one step further back.

Scientific Naturalism and Spontaneous Generation

Scientific naturalism was therefore very much a British response to the new problems of explanations of the world in the wake of a more questioning attitude towards the older style of natural theology. As this study has shown, it is not to be confused with either materialism or Positivism.

The subject matter of scientific research which the scientific naturalists chose to undertake obviously reflected their own particular specialisms and interests. It is the scientific controversies with which they became involved which are often more revealing with regard to their underlying beliefs. One such controversy surrounded the subject of spontaneous generation. As Chapter 1 suggested, the debate over spontaneous generation posed a threat to the tenets of scientific naturalism in a number of ways and in particular threatened Tyndall's desire to see the introduction of a scientific medicine. It therefore becomes important to see Tyndall's involvement in the spontaneous generation debate against this context of scientific naturalism, rather than in terms of a form of materialism, as only in that way can his efforts to discredit spontaneous generation be explained and understood. Chapter 3 goes on to discuss how the spontaneous generation debate began in Britain.

CHAPTER 3

SPONTANEOUS GENERATION - THE EARLY 1870s

Introduction

After the life-cycle of the parasitic tape worm had been explained in the 1840s, micro-organisms remained as the only possible contenders for spontaneous generation. In the early years of the nineteenth century it was known that both putrefaction and fermentation were somehow related and both had something to do with living materials but it was not clear what the relationship was i.e. whether the organisms involved were the cause or the result of the fermentation; chemical explanations of fermentation tended to be favoured. The most widely known and accepted chemical theory of fermentation was developed by Liebig. [1]

British scientists were traditionally less interested in questions of the beginnings of life than were their continental counterparts. However their attention was drawn to the possibility of a spontaneous origin of life as implied by Darwin's The Origin of Species, which was published in 1859. Around this time, the interest generated in the fundamental units of life in terms of cells, protoplasm or molecules, due to the work of Schwann and Schulze and later Virchow, Haeckel, Huxley and Bennett, also helped the doctrine of spontaneous generation, if not to gain credibility at least to be seen as worthy of consideration. The debate on spontaneous generation which took place in France in the 1860s was an additional factor in bringing the subject to the attention of the British. However it was the concern over the cholera epidemics in the 1860s, Lister's pioneering work on antisepsis, Bastian's campaign for spontaneous generation and the ensuing controversy coupled with Tyndall's work to link the refutation of spontaneous generation with the germ theory of disease which brought the matter once and for all to the attention of the British scientific community.

This chapter describes the French debate briefly, before discussing British work on spontaneous generation in the 1860s. Tyndall's experiments on light beams and dust and Huxley's address to the B.A.A.S. in 1870 ensured that spontaneous generation became one of the most popular topics for discussion amongst the medical and scientific communities. Bastian's entry into the debate in the same year and his criticisms of Pasteur are then described. Over the next three years several experimenters were drawn into the debate and discussion centred round the physical conditions of the experiments rather than the properties of the organisms involved. The most important element was the involvement of the eminent Burdon Sanderson who unintentionally lent Bastian's position support by agreeing that his results were correct, even though he did not agree with his interpretation.

The concentration of the experimenters on the detailed physical conditions of the experiments emphasised the belief that the resistance of organisms was to be understood in terms of environmental conditions rather than in terms of the properties of the organisms themselves. However several of the participants in the debate began to suspect that micro-organisms might be more resistant to heat than had previously been believed. Although by the middle of the decade it was still impossible to resolve the debate, studies on life-cycles of microscopic life were beginning to throw new light on the resistance of these bodies to heat.

Spontaneous Generation - Definitions

In both the French debate and the later British debate the question under consideration was whether or not microscopic organisms could be generated spontaneously without the presence of antecedent parent organisms of the same type within specially prepared experimental solutions. Such solutions were usually boiled, ostensibly to kill any micro-organisms which might have already been present, and then the experimental vessels were hermetically sealed to prevent new contamination from the air. Although this constitutes a broad definition of what was understood by the term spontaneous generation, it is important to recognise the sub-categories of spontaneous generation for the purposes of understanding the British debate. Amongst contemporary historians, Vandervliet fails to understand the terminology used in the debate, Rang and Crellin use Bastian's definitions of spontaneous generation, while Farley defines the terms "heterogenesis" and "abiogenesis". [2] However as Farley's definitions were not those used in the British debate (which causes him a problem in understanding Bastian's explanations) it would only be confusing to reproduce them here.

Bastian had his own definitions and these were somewhat more precise than the definitions of his contemporaries. He did not like the term "spontaneous generation" and avoided using it. Neither did he use the term "abiogenesis" preferring instead "archebiosis". Archebiosis was the generation of living beings either from inorganic material or organic material which was no longer alive i.e. from any kind of matter which was not alive. For him, heterogenesis was the generation of living organisms from matter which *was* alive. Heterogenesis covered (a) the metamorphosis of one living organism into another, (b) the metamorphosis and fusion of many minute organisms and (c) the generation of a different form of life from a portion of living matter of a pre-existing organism before or after its death, as long as the matter itself was still alive. [3] It is important to understand that Bastian believed that when an organism died its tissues only died gradually and while this process was going on it was still possible for heterogenesis to take place. Heterogenesis could not take place in boiled experimental fluids, as any living matter was killed and disorganised by the boiling process; only archebiosis was possible under such circumstances. All Bastian's experiments with boiled liquids, whether on saline solutions or on animal or vegetable infusions, were for him experiments on archebiosis. Only his first set of experiments, described in the next section, were pure heterogenesis experiments.

Of contemporary historians, only Crellin has really appreciated that Bastian's definitions of spontaneous generation were peculiar to him. [4] Rang, in his unpublished biography of Bastian, does understand Bastian's terminology, but appears unaware that this terminology differs from that of Bastian's contemporaries. [5] Farley does not appreciate Bastian's definitions to the extent that he finds Bastian's explanation of the relationship of heterogenesis to abiogenesis puzzling. [6] However the reason Farley fails to understand this explanation is that what *he* means by heterogenesis and abiogenesis, Bastian thought of as one phenomenon, namely archebiosis. For Bastian, as there was essentially no difference between organic and inorganic matter, the genesis of organisms from a boiled solution of inorganic salts was the same class of phenomenon as the generation of life from a boiled infusion of turnip. This is an instance of where Farley's broad study fails to capture and explain the detail of a particular debate.

The term, abiogenesis, may well have originated with Huxley, who coined it in his address to the B.A.A.S. in 1870. [7] Huxley used the term abiogenesis to cover virtually everything that was usually meant by spontaneous generation. Abiogenesis was to be contrasted with "biogenesis" or the appearance of life from parents of the same type. Abiogenesis was not a term which was widely used in the British debate.

However, most commentators in the British spontaneous generation debate used the terms "spontaneous generation", "heterogenesis" or even "heterogeny" in a quite general way. Heterogenesis was an old term which Pouchet had made popular in his debate with Pasteur. Pouchet understood heterogenesis to be the generation of life from *organic* (i.e. once living) materials, which was rather different from Bastian's meaning, but the British used the term much less fastidiously. Even Bastian often wrote of the evolution of life or the origin of life *de novo* rather than using his own defined terms. Tyndall made nothing of these categories; for him they were all examples of spontaneous generation and therefore all alike impossible. The distinctions did not always matter as it was usually obvious what was at stake in a particular set of experiments. Where they are important, however, is in understanding that Bastian was describing two quite distinct processes in his experiments and also in understanding Bastian's defence of his theoretical position.

The Pasteur-Pouchet Debate

A decade before the British debate, from 1858 to 1864, Louis Pasteur was involved in a debate over heterogenesis with Felix Pouchet, a respected Rouen naturalist. Farley and Geison have explained this debate in terms of its political aspects as they suggest it was not resolvable in experimental terms. [8] In France the work of Lamarck and St Hilaire had tended to associate transformism with spontaneous generation, however the considerable influence of Cuvier did much to discredit these doctrines. Church and state united against republicanism, materialism and atheism and saw Darwinian evolution as a political threat and the spontaneous generation it seemed to imply as inimical to the idea of a creative force. Hence the Pasteur-Pouchet debate was laden with important political implications.

In 1859, Pouchet brought out his book, Hétérogenie, ou traité de la génération spontanée in which he dissociated his brand of spontaneous generation from atheism and argued for a vital force which God had used in the process of creation. [9] As opposed to earlier beliefs in spontaneous generation he linked heterogenesis with successive creations rather than transformations and suggested it was the eggs rather than the adult forms of micro-organisms which were created this way. In holding these views, in common with most vitalists, he repudiated the possibility of spontaneous generation from inorganic materials.

Pasteur's work on fermentation had already led him to the belief that all fermentation was the result of the action of micro-organisms. This view opposed the chemical conception of fermentation which held that living things were a by-product rather than the cause of fermentation. However, despite his fight against heterogenesis, Pasteur actually held a secret belief in the possibility of spontaneous generation from certain types of non-living material. [10] His earlier work on crystallography had connected organic molecules with asymmetry and optical activity and he found that molecular asymmetry could modify physiological chemical reactions. Hence he drew a line between living and non-living matter only in terms of the molecular asymmetry of natural organic material or an asymmetric force which he felt was not beyond the bounds of experimental enquiry. In the 1850s he conducted experiments with powerful magnets acting upon the crystallisation process to determine whether magnetic forces would reverse the direction of natural symmetry in organic substances.

The debate between Pasteur and Pouchet was largely decided by the two Commissions set up in the 1860s by the Academie des Sciences. In contrast to Britain, the formal centralised structure of French science meant that the opinion of the Academie des Sciences was likely to be decisive in a scientific controversy in that country. In 1862 the Academie proposed to offer a prize to the experimenter who threw new light on the controversy but the Commission appointed to decide who should be awarded the prize consisted of five people who were all Catholics and who were all unsympathetic to the doctrine of spontaneous generation with its overtones of atheism and political radicalism from the start. Pouchet withdrew from the competition when some members of the Commission announced their decision before examining the entries and so Pasteur won the prize uncontested on the strength of his Memoire sur les corpuscules organisés qui existent dans l'atmosphère, published in 1862. [11]

Pouchet annoyed members of the Academie by continuing to conduct experiments with flasks of hay infusion in the Pyrenees during the next year. However a second Commission was appointed in 1864 consisting of several members of the old Commission plus two new members who were both active supporters of Pasteur and in the face of such biased and hostile opinion, Pouchet again withdrew. Pouchet's experiments in the Pyrenees were never refuted and the second Commission saw the virtual end of controversy over spontaneous generation in France.

It was, of course, possible to ignore Darwinism but those French scientists who actually attempted to discredit Darwinism relied on Pasteur's refutation of spontaneous generation. M.J.P. Flourens, who had sat on both French Commissions, published Examen du livre de M. Darwin sur l'origine des espèces in 1864 in which the main thesis was that Darwinism had been refuted because it rested on spontaneous generation which had itself been refuted by Pasteur. [12] In Britain, it was very much in the interests of the scientific naturalists to dissociate the two doctrines. They did not wish to see the refutation of spontaneous generation damaging the cause of evolution. Their success in achieving this separation is demonstrated by the fact that the eventual downfall of spontaneous generation in that country did nothing to discredit Darwin's doctrine.

Towards the end of his life in 1870, Pouchet modified his views to agree that spontaneous generation could be used to support Darwinism although he had always thought that the two views were incompatible before. However, he himself continued to believe in successive creations.

As Chapter 4 shows, the later French Commission between Pasteur and Bastian could easily have yielded the latter a triumph, if it had actually taken place! There are parallels to be drawn with the French debate. The Pasteur-Pouchet debate might well have ended differently had Pasteur repeated Pouchet's experiments or had Pouchet kept his nerve and not withdrawn from the competition, given that a death point of 100°C. for micro-organisms in solutions was widely accepted at that time, and that Pouchet's experiments probably contained heat resistant spores. [13]

The British Background

While Pasteur and Pouchet debated the spontaneous generation question in France, the British remained fairly quiet about the subject. In France, the acceptability of evolution had been directly linked to the question of spontaneous generation, hence for many French scientists the pronouncements of the two Commissions dealt a serious blow to the fate of evolution.

Whereas, in the next decade, Bastian linked his beliefs on spontaneous generation with evolution, to the extent of terming himself an "evolutionist" and the scientific naturalists clearly understood the possible implications stemming from the relationship of evolution and an origin of life due to spontaneous generation, there was generally less interest in the origin of life within the British scientific community. [14] Certainly the acceptability of evolutionary doctrines was not linked so definitively to the acceptability of spontaneous generation as it had been in France, and the scientific naturalists did everything they could to dissociate the two.

Farley suggests two reasons for the British lack of interest in origins. Firstly the naturalistic slant of British geology saw the fossil record as so complex as to be impossible to explain by natural means, let alone by spontaneous generation, so it was best left undiscussed. Secondly, the strong empirical/Baconian tradition of British science did not speculate on such matters. [15] On the first point it was certainly true that both Darwin and Huxley wished to avoid discussion of the origin of life with regard to the theory of evolution. [16] This avoidance had much to do with their desire to dissociate spontaneous generation and evolution.

On Farley's second reason, however, it is possible to overemphasise the Baconian nature of British science. It is true that the rhetoric of the scientific enterprise portrayed science as Baconian and non-metaphysical or speculative, but in fact the very scientists who publicised and welcomed this image, such as Huxley or Tyndall, allowed themselves the luxury of scientific speculation when it suited their purposes. In the spirit of his famous address Tyndall felt very strongly the need to pursue "the scientific use of the imagination" with reference to a number of theoretical concepts which were far from universally accepted such as the germ theory, the atomic theory and the nebular hypothesis. [17] Given that the linking of spontaneous generation with evolution had been so negative towards the cause of evolution in France, the scientific naturalists obviously wished the two theories to be kept separate. The best strategy was to declare spontaneous generation unscientific whilst steering discussion on evolution towards demonstrating its essential naturalness and scientific character.

Although Pasteur was one of the germ theory's best known advocates and had undertaken valuable work on silkworm diseases based on that theory, the French did not make a strong link between spontaneous generation and its medical implications during the Pasteur-Pouchet debate. Yet in Britain the question of spontaneous generation was linked more closely to practical medical issues and theories of infectious disease and wound infection. The last great cholera epidemic hit Britain in 1865 and served as a reminder of the helplessness of the medical profession in curbing such diseases despite William Farr's attempt during that period to further the cause of contagionist theories. Mortality rates after surgery due to the onset of septic infections in surgical wounds remained high, despite the skill and dexterity of surgeons. These two factors served to strengthen the links made between the germ theory and spontaneous generation and made the latter subject one which definitely fell within the domain of interest of the medical profession.

Farley is at pains to point out that most British contagionists of the middle to late nineteenth century believed that contagia were non-organismic particles and hence that disease could arise either from contact with the contagion or *de novo*. He suggests further that a belief in the *de novo* origin of disease under such circumstances does not imply any support for the doctrine of the spontaneous generation of *organisms*. [18] Logically this is true, and it is also true that it was possible to believe that contagia were the normal means of transfer of disease but that disease could arise *de novo* in extreme circumstances, or what might be termed a form of "contingent contagionism". The problem seems to lie, rather, in Farley's suggestion that contagionists of the late nineteenth century believed that contagia were not organisms. As Chapter 7 shows, in the 1870s, beliefs that organismic particles were related to disease (either cause or result) were becoming fairly widespread and certainly by the 1890s it would have been somewhat unusual to be a contagionist and not believe that the contagia were organisms. Some medical practitioners believed that organisms were the result but not the cause of disease.

Under a "contagia as organisms" view both the contagious *and* the *de novo* origin of disease were compatible with spontaneous generation, and the latter was very definitely a case of Bastian's heterogenesis.

Only the old view of disease contagia as "poisons" with no involvement of micro-organisms at all can support Farley's assertion of the independence of the two views of, respectively, belief in the *de novo* origin of disease and belief in spontaneous generation. The fact of the matter was that medical men who asserted the occasional *de novo* origin of disease under certain circumstances were concerned with more practical issues than spontaneous generation and so rarely made the link. It was rather individuals such as Bastian who clearly made the connection between the two views in order that his belief in spontaneous generation was lent credence by the pronouncements of respected medical men on the nature of disease.

Despite the French debate on spontaneous generation in the 1860s which was widely reported in the medical press, Lister's work and the cholera epidemic of 1865-1866, discussion and experimentation on the subject was relatively rare in Britain in this decade. [19] Notably, Gilbert Child, lecturer in botany at St. George's Hospital, undertook a series of experiments in 1864 and 1865 with the assistance of Lionel Beale, Professor of Physiology and General and Morbid Anatomy (later Professor of Pathological Anatomy) at King's College, London, who drew the organisms found in the experiments. [20] Animal substances in solution were boiled in glass vessels which were then sealed. After a suitable period bacteria were found in eight out of thirteen vessels. Child pointed out that this disagreed with Pasteur's results as Pasteur's vessels all remained clear - even when precautions had been taken which Pasteur would have called exaggerated. Child concluded that either these organisms could withstand boiling or they were spontaneously generated. [21] Perceptively he commented that Pasteur and Pouchet were unlikely to solve their dispute if it was the case that the germs could withstand boiling. [22] Child also alluded to the problem of microscopy which was indeed a matter for concern in these experiments even though it was rarely commented on by experimenters. Pasteur had used a magnifying power of 350 diameters to examine his substances but Child found that it was impossible to see the organisms involved even at twice that power (unless the experimenter had first examined them at a power of 1500-1700 diameters). [23] He suggested that improvements in microscopy offered the only way of increasing knowledge of the production of bacteria. [24]

Child has traditionally been seen as a supporter of spontaneous generation. [25] However evidence shows that he continued to remain ambivalent towards spontaneous generation through the 1860s. Child was not convinced that even his own experiments furnished any proof of spontaneous generation. His concern, which was somewhat rare in Britain, lay rather in the fact that he believed that "heterogeny", as he termed spontaneous generation, was an integral part of the theory of evolution and hence there was a dilemma between the philosophical necessity of spontaneous generation and the inconclusive nature of current experimental enquiries. [26] In 1870 at the beginning of the British debate he wrote, "theoretical consideration(s) is in favour of the actual existence of heterogeny as a real mode of origin of living beings, yet... the authority of M. Pasteur's famous researches inclines the balance of experimental evidence heavily the other way." [27]

John Hughes Bennett, Professor of Medicine at the University of Edinburgh, developed a theory of molecular physiology which was widely reported in the medical press of the mid-1860s. [28] Bennett's theory was not explicitly concerned with the process of spontaneous generation but its focus on the molecular level, in drawing attention to the fundamental units of life, paved the way for more theoretical considerations of spontaneous generation. By the end of the decade Bennett had come to believe in heterogenesis or the generation of life from organic materials. He also attacked Pasteur's work and the germ theory, believing that the production of organisms in experiments depended on physical conditions rather than on any living organisms which were already in the experimental fluids. [29] Much of Bastian's earlier theoretical stance was based on Bennett's molecular theory, as is described in a later section of this chapter.

As Crellin has shown there was a significant degree of interest in analyses of airborne particles in Manchester, in the 1860s, particularly by the chemist, Robert Angus Smith. [30] Of the Manchester workers only Frederick Crace Calvert contributed to the spontaneous generation debate. His involvement in industrial chemistry and particularly in the manufacture of phenol (carbolic acid) for use in the dyestuffs industry, coupled with an interest in public health issues led him to promote phenol's use as a disinfectant. [31] From there he became an advocate of the germ theory and he investigated both the effects of chemical disinfectants and heat on micro-organisms.

In 1868, Huxley's old adversary over the theory of evolution, Richard Owen, the eminent comparative anatomist, declared his support for spontaneous generation. [32] Owen subsequently undertook no experiments and took no part in the ensuing debate but his opinion was very influential. He too criticised Pasteur's experiments, finding Pouchet's work much more satisfactory and conclusive. [33]

Owen's weighty opinion was brought to the attention of the medical world in a series of articles on the origin of life published in the <u>British Medical Journal</u> in 1869. [34] These articles tended to suggest that, in the light of Owen's pronouncements the weight of opinion was in favour of spontaneous generation.

By the end of the 1860s, despite the general indifference of the British scientific world to the origin of life and despite Lister's promotion of Pasteur's doctrines through his work on antisepsis, there had been some telling criticisms of Pasteur's work from a number of British scientists, including some very influential men. All this pointed to the fact that, in Britain, Pasteur's experiments were not anything like as acceptable as the deciding factor in the controversy over spontaneous generation, as they had been in France. This in turn hinted at the possibility that spontaneous generation was by no means a dead subject and was worthy of further discussion. The scene was set for a debate over spontaneous generation in Britain.

<u>Tyndall's Early Experiments and Huxley's B.A.A.S. Address</u>

Tyndall's experiments on luminous beams were described in his well known address, "Dust and Disease" which was delivered at the Royal

Institution in early 1870. [35] Much of the address was directed at praising Pasteur and Lister in their work on the germ theory but he also detailed his own experiments on dusty air.

These experiments on floating matter in beams of light, undertaken towards the end of 1868, brought to bear a novel experimental technique on the problem of the purity of the air. Whilst experimenting on the decomposition of vapours by light, in an attempt to remove atmospheric dust, in order that there might be nothing capable of scattering the beam of light in his experiment, he discovered that the floating matter was burned by the flame of a spirit lamp and from this he concluded that the material was organic in nature.

> "I tried to intercept this floating matter in various ways; and on the day just mentioned *(Oct. 5th, 1868)*, prior to sending the air through the drying apparatus, I carefully permitted it to pass over the tip of a spirit-lamp flame. The floating matter no longer appeared, having been burnt up by the flame. It was therefore of *organic origin*. I was by no means prepared for this result; for I had thought that the dust of our air was, in great part, inorganic and non-combustible." [36]

Tyndall's discovery was by no means new; Pasteur had undertaken a similar demonstration of dust particles in the air in a public lecture delivered in 1864. [37] Furthermore it was fairly well known that the air contained organic material and it is odd that Tyndall should have found this fact surprising. [38] However, he quickly saw the implications of his accidental discovery and described the practical aspects of his luminous beam experiments which could be used to investigate the "optical purity" of the air. He suggested that the test could have been put to use in Pasteur's and Pouchet's recent controversy.

> "The method of inquiry pursued in this discourse will, I think, help to clear the field of discussion. The experimenters do not seem to have been by any means fully aware of the character of the atmosphere in which they worked; for if this had been the case, some of the experiments recorded would never have been made." [39]

With this in mind he criticised Pouchet for believing he had destroyed all the atmospheric germs in his experiments. Had he passed a luminous beam through the water he used, he would have seen that it was laden with particles. This was a clear attempt on Tyndall's part to rescue Pasteur's reputation and with it the germ theory from recent criticisms.

Tyndall made an immediate connection between the illuminated dust of his experiments and the postulated disease germs present in the atmosphere and thus began his advocacy of the germ theory in the form of a very energetic publicity campaign. Pasteur's researches into silkworm diseases and the "light and guidance" provided to surgical science in the form of Lister's antiseptic system were evidenced as positive benefits of applying this theory. [40] Tyndall also took the unusual step of bringing the germ theory and Lister's work to the attention of the public in the pages of The Times in April, 1870. [41] This resulted in a brief and somewhat hostile exchange between him and Bastian in the pages of that newspaper. [42] In particular Bastian felt that Tyndall was trying to tell the medical profession what it knew already. The significance of his germ theory campaign and the criticisms it excited, particularly from the medical profession, are detailed in Chapter 6.

It was the custom of the President of the British Association for the Advancement of Science to review some currently important area of science in the presidential address each year. In 1870, Huxley, as president, chose to discuss the topic of spontaneous generation or what he termed "abiogenesis" in his address "Biogenesis and Abiogenesis". In this way he brought a subject which was already attracting some interest, firmly within the central view of the British scientific community. [43] Spontaneous generation could no longer be ignored.

After detailing the history of the subject he discussed Tyndall's and Pasteur's experiments in support of the germ theory and emphasised that Tyndall's experiments showed the germs or particles to be ultramicroscopical. On one side of the argument, Huxley found no explanation for the development of life in boiled fluids other than the conclusion that the air contains germs that give rise to bacteria. But on the other hand, it was incontrovertible that hermetically sealed fluids, after exposure to a considerable amount of heat, sometimes contained living organisms when they were opened. In common with Child, Huxley felt it was more logical to conclude that germs could withstand greater heat than had previously been supposed than to conclude that spontaneous generation had taken place.

However he was mindful that the doctrine of evolution implied an origin of life through spontaneous generation from non-living material and so was careful not to suggest that abiogenesis had never taken place in the earth's history.

> "... if it were given me to look beyond the abyss of geologically recorded time to the still more remote period when the earth was passing through physical and chemical conditions, which it can no more see again than a man can recall his infancy, I should expect to be a witness of the evolution of living protoplasm from not living matter... I have no right to call my opinion anything but an act of philosophical faith." [44]

Having re-opened the subject of spontaneous generation, it became the most discussed topic in that year's B.A.A.S. meeting. [45]

<u>Bastian's Early Work</u>

The year 1870 was something of a watershed for the debate on spontaneous generation in Britain, for it was in this year that Tyndall's well known "Dust and Disease" address was delivered, that Huxley addressed the B.A.A.S. on the subject and that Bastian began to publish the results of his experiments on heterogenesis and archebiosis. Henry Charlton Bastian had been appointed Professor of Pathological Anatomy at University College, London in 1867; he also practised clinical medicine and was promoted from assistant physician to physician at University College Hospital in 1878. In a long article in <u>Nature</u>, "Facts and Reasonings Concerning the Heterogeneous Evolution of Living Things", he described his first experimental findings. [46]

Bastian's article raised a number of important issues. First of all, he was critical of Pasteur's work of the previous decade; in the light of what was known of death points, Pasteur had simply ignored the fact that his experiments could be interpreted in another way. Coupled with Bennett's and Child's criticisms and the recent series of articles on the origin of life in the British Medical Journal, the conclusiveness of Pasteur's researches had by now, received a serious challenge in Britain. Secondly, Bastian had explicitly drawn upon the writings and work of four of the scientific naturalists in this paper, to support his position. He quoted from Tyndall and Huxley, writing on crystalline forces and the constituents of protoplasm respectively to support his analogy between the formation of crystals and the formation of life, detailed below. [47] He pressed Spencer's view of evolution into service in support of his views on the origin of life. [48] Finally, he had been assisted in some of his archebiosis experiments by Frankland who had heated the infusions involved to a temperature of 150°C. by means of a digester. [49] Frankland himself, did not agree with Bastian's interpretation when organisms subsequently appeared, but his very involvement tended to give Bastian support to the extent that the former felt obliged to repeat these experiments in the presence of Huxley and Busk who agreed that not the slightest sign of life was generated. [50]

Bastian outlined the physical arguments in support of his beliefs in the *de novo* origin of living organisms from organic solutions and in particular he introduced his favourite analogy with crystal formation. His argument was as follows. He suggested that in experiments on the evolution of life, germs of monads and bacteria and spores of fungi could be seen gradually appearing under the microscope from an experimental solution which previously contained no living organisms. Bastian argued that physicists and chemists see crystals appearing from solutions in the same way yet they do not suggest that there has to be an invisible crystal "germ" before a crystal arises out of solution. Similarly there is no more reason to believe that there has to be a living germ before life will appear in a suitable solution. [51] On the one hand no one is asked to believe in pre-existing crystal germs yet we are asked to believe in pre-existing germs of bacteria.

On the surface of (unheated) organic solutions a "proligerous pellicle" or jelly like layer of monads and bacteria could form. From these pellicles, Bastian witnessed the heterogeneous evolution of unicellular organisms.

> "... formless and apparently homogeneous or merely granular living matter, resolves itself more or less rapidly into a number of individualised segments, which are capable of existing as independent living things." [52]

These individualised segments could contain from four to eight altered bacteria and monads in their interior and would gradually evolve into other organisms such as amoebae. [53] Similar processes had first been described by Pouchet and Bennett. When Bennett first developed his theory of molecular organisation he denied that it supported spontaneous generation, although it was clearly compatible with a form of spontaneous generation and in the late 1860s he came to accept the possibility of heterogenesis, in Bastian's sense of the term, and similarly to deny the validity of the germ theory. [54]

Bennett held that it was to the molecular level that we should look to understand physical and vital action. He stated that there were two types of molecule - *histogenic* which were precipitated in fluids and *histolytic* which were formed from disintegrated tissues; the two types of molecule could change place. [55] The molecules were governed by molecular forces which caused them to combine in definite ways; these forces were independent of the cell, nucleus or other form of structure.

Bastian, who was in agreement with Bennett's focus on the molecular level, suggested in particular, that colloidal rather than crystalline aggregations were important in the heterogeneous evolution of life.

> "The molecular constitution of these two kinds of matter may be closely allied, and wherever Life-giving changes occur, we are entitled to look upon these as actions resulting from the influence of physical forces upon material collocations whose molecular constitution is of such a nature as to render them prone to undergo current rearrangements. A series of actions and reactions occur between such material collocations and their environment, and as a result Living things appear and grow. This

> tendency to undergo change is inherent in colloidal substances. As Prof. Graham told us:-"Their existence is a continual metastatis... The colloidal is, in fact, a dynamical state of matter, the crystalloid being the statical condition. The colloid possesses ENERGIA. *It may be looked upon as the probable primary source of the force appearing in the phenomena of vitality.*" [56]

Bennett's descriptions of the action of his molecules in forming vibriones bear such a striking similarity to Bastian's description that it is clear that Bastian derived these ideas from Bennett. In particular he borrowed the term "energia" which had originated with Thomas Graham, the chemist.

In his earlier work, Bastian seemed to take on much of Bennett's theory wholesale, including "energia" which was a distinctly vitalistic term with undertones of the concept of a vital force. Yet, unlike Bennett who accepted the possibility of heterogenesis but not archebiosis, Bastian was very definitely not a vitalist; he was a materialist who believed rather that there was no fundamental difference between living and non-living matter. It was those who opposed the possibility of spontaneous generation and supported the germ theory, including Tyndall, who were adhering to metaphysical vitalist theories, in his eyes. [57] Not only did he constantly emphasise the essential similarity between living and non-living matter but he also evinced the conservation of energy and the correlation of vital and physical forces to support his belief that life,

> "... is as much the essential and inseparable attribute of the particular molecular collocation which displays it, as the properties of the crystal are essential to the kinds and modes of aggregation of the molecules which enter into its composition." [58]

It was, however, possible to believe, as the scientific naturalists did, that special vital forces did not exist on the grounds of possible violations of the principle of the conservation of energy and yet still believe that living matter was sufficiently complex for spontaneous generation to be impossible.

Most observers would have found the process of archebiosis harder to accept than heterogenesis as it involved the rearrangement of dead materials some of which were just solutions of salts containing no organic material whatsoever; at least with heterogenesis living organic material was already there in the experiment. These considerations did not worry Bastian unduly. As well as his heterogenesis experiments on unboiled fluids, where organisms appeared from the "proligerous pellicle", he also undertook several experiments on the process of archebiosis. In fact, the rest of the debate was conducted in terms of experiments on what Bastian would have termed archebiosis. In these experiments infusions or solutions of various substances were boiled in order that any life contained therein was destroyed, and then hermetically sealed to prevent contamination from any organisms from the air. This implied that organisms which developed after the experiments, had arisen *de novo* by a process of archebiosis; heterogenesis was impossible as boiling destroyed any living material in the experimental vessels. Bastian felt that a temperature of 100°C. was much more injurious to organic materials than to saline solutions as the heat was very destructive in breaking up, "those very complex organic products, whose molecular instability is looked upon as one of the conditions essential to the evolutional changes which are supposed to take place." [59]

A set of experiments was undertaken with the help of Frankland. The materials used were 1) a turnip solution which had been boiled and kept hermetically sealed for twelve days, 2) solutions of ammonic tartrate and sodic (sodium) phosphate and 3) ammonic carbonate and sodic phosphate respectively. These solutions were kept in a vacuum and boiled for four hours at a temperature of 150°C and hermetically sealed. In all three experiments organisms were found after several days. The saline solutions had been carefully examined for visible organisms or spores before the experiments. Bastian concluded that the organisms found in the solutions afterwards were either evolved from the solutions or were some of the panspermists' invisible fungus germs.

"Those who believe in a special "vital principle" may naturally enough cling to the notion of a pre-existing germ, which may be the direct recipient of this peculiar power from some pre-existing organism; whilst those who are believers, rather, in the physical doctrines of Life will, I think, gradually find themselves contented with the pre-existence of potential "germs" in the form of colloidal molecules." [60]

Bastian's long first article in Nature on heterogenesis produced a response from some of his critics. One of the most perceptive criticisms was made by W.T. Thiselton Dyer, Professor of Botany at the Royal College of Science, Dublin. Like Child, Thiselton Dyer's main concern was with the relationship of spontaneous generation and evolution but he made some telling remarks as to Bastian's emphasis on the colloid in the origin of life. The energy of a colloid could not be peculiarly associated with vitality any more than crystalloid energy because that energy was one and the same thing. [61] A believer in spontaneous generation was not a true evolutionist, "but is only a vitalist minus the supernatural; the special creation which the one assumes is replaced by the fortuitous concourse of atoms of the other." [62]

Some months later Huxley delivered his address on spontaneous generation to the B.A.A.S which not surprisingly Bastian replied to, at great length, in the pages of Nature. [63] For Bastian, the difficulty with Huxley's position related to the latter's belief in "invisible germs" and the fact that no matter how an adult organism evolved, whether *de novo* or from an invisible germ, a microsocope, even under the most competent observer could not distinguish which mode of origin had occurred. The fact that the atmosphere *might* contain germs did nothing to prove or disprove the possibility of the *de novo* origin of life. Believers in the germ theory were closing their minds to the possibility that life could originate in any other way than by means of pre-existing life. Bastian's reply to Huxley elicited a somewhat hostile response.

"Any time these six months Dr. Bastian has known perfectly well that I believe the organisms which he has got out of his tubes are exactly those which he has put into them... when the first of these wonderful experiments was put under my microscope, I told him at once that it was nothing but a fragment of the leaf of the common Bog Moss (sphagnum)." [64]

Turnip-Cheese Experiments

Given the material published in both the medical and scientific press, the subject of spontaneous generation was coming to the forefront of scientific investigation in the early 1870s. Bastian continued to experiment and publish throughout the early years of the decade. His book Modes of Origin, largely a response to the criticisms of Tyndall and Huxley, was published in 1871. In 1872 he published The Beginnings of Life which contained detailed descriptions of his experiments. This book attracted interest from a number of scientists including Edwin Ray Lankester, the zoologist; William Roberts, a Manchester physician; Professor Dirk Huizinga of Groningen and most notably, the eminent physiologist John Burdon Sanderson.

The interest focused round a series of experiments which Bastian undertook on infusions of hay and on infusions of turnip and cheese. The notable point about these experiments and the ensuing controversy surrounding them is that they were undertaken and interpreted, in large part, to understand the thermal death point of bacteria in solution and under what conditions bacteria or their germs might withstand heat. Questions of acidity or alkalinity were raised and whether the existence of particulate matter i.e. visible particles of cheese could harbour germs which had been preserved from the destructive action of heat. The debate over Bastian's experiments was carried out in terms of a discussion of the physical conditions of the experiments, with the precise details of the experiments under scrutiny. These details included exact temperatures and whether these temperatures could indeed be measured exactly in boiling liquids; specific gravities of the materials used; whether the rind or the inner part of the turnip was used; how much cheese was added to the turnip solution and whether it was pounded first. Experimenters suggested modifications and improvements to the experiments so that errors might be eliminated.

The experimenters on both sides of the debate sometimes obtained experimental results which were contradictory and the continuing opposing interpretations are interesting. Despite the contradictory results, whatever way the experiments were carried out and however much the experimenters took precautions to ensure accuracy, the point was that the experiments could always be interpreted in more than one way. It was always possible to claim that one's opponent had made experimental errors and thus deny the validity of their results.

The most important feature of the controversy which Bastian aroused at this stage in the debate was probably the involvement of Burdon Sanderson. Although individuals such as Child, Bennett and Huizinga seemed sympathetic to the possibility of forms of spontaneous generation, Bastian was still very much of a loner in this debate and so it was in his interest to claim the support of an eminent physiologist. Even though Burdon Sanderson never declared any support for spontaneous generation, the very fact that he took part in some experiments, published the results and agreed that organisms would actually appear under the circumstances Bastian had laid down, lent a great deal of credibility to the subject which might not otherwise have accrued to it. In fact, he was only giving Bastian a fair hearing on the question of spontaneous generation.

As he did not believe in spontaneous generation, Burdon Sanderson was careful not to declare support for Bastian's theoretical position, whilst maintaining that he wanted to take no part in the controversy - a rather odd assertion in the face of his detailed observations and subsequent publication. The fact that a medical scientist as well known and respected as Burdon Sanderson took part in this debate without actually condemning spontaneous generation outright, as Tyndall had done, unintentionally gave Bastian's experiments credibility. Under these circumstances, it was not difficult for Bastian to make it appear that Burdon Sanderson agreed that spontaneous generation had taken place in his experiments.

In 1872 Burdon Sanderson observed a series of experiments on infusions of turnip and hay respectively; these were prepared by a friend according to the instructions given in Bastian's work The Beginnings of Life. [65] Burdon Sanderson's avowed reasons for looking at this problem experimentally were as follows:-

> "In every experimental science it is of great importance that the methods by which leading facts can be best demonstrated, should be as clearly defined and as widely known as possible. This is particularly true as regards physiology, a science of which the experimental basis is as yet imperfect. All experiments by which a certainty can be shown to exist where there was before a doubt, serve as foundation stones. It is well worth taking some pains to lay them properly." [66]

As Bastian suggested that two of the three liquids employed by Burdon Sanderson were not suitable for the experiments, he, himself, prepared a further three series of experiments in the presence of the physiologist. In the first series two infusions were prepared - one of turnip in which both the rind and central part were used and one of hay. The turnip infusion was acid; half of it was neutralised with liquor potassae. Four retorts were prepared, two with neutral infusion, two with unneutralised infusion. To one of each of the pair, a small quantity of pounded cheese was added. A fifth retort was prepared with unneutralised turnip infusion diluted with water. All five vessels were carefully boiled for five minutes and hermetically sealed.

Three retorts were filled with the hay infusion which was already neutral - one of the vessels contained a diluted version of the infusion. These vessels were boiled and sealed as with the turnip infusion. All eight vessels were kept in a water-bath at a temperature of 30°C.

Burdon Sanderson and Bastian met three days later to examine the flasks. Bastian expressed his anticipation that the turnip-cheese infusions would contain bacteria, that the other two turnip infusions would show changes, that the hay infusions would be less advanced and that the dilute infusions would show no change. In fact the neutralised turnip-cheese infusion and one of the undiluted hay infusions contained definite active bacteria and leptothrix filaments (threads of bacteria) and the second undiluted hay infusion contained some bacteria. The diluted hay infusion retort had been accidentally cracked; all other vessels remained unchanged.

A second series of experiments was undertaken where the turnip rind was excluded as Bastian felt that the irregularities in the previous results may have been due to the fact that the material used consisted partly of rind. Four retorts were prepared - two contained unneutralised turnip infusion with cheese, one contained neutral infusion without cheese and the fourth unneutralised infusion without cheese - all four contained bacteria, and one of these contained bacteria and leptothrix in a sticky mass, within a few days.

In the third series of experiments Burdon Sanderson heated two of the retorts to 250°C. for thirty minutes before the experiment to ascertain whether the condition of the internal surface of the glass vessels exercised any influence on the results. All three vessels in this experiment contained bacteria, leptothrix and a pellicle within a few days.

Sanderson's conclusions were as follows:-

> "The accuracy of Dr Bastian's statements of fact, with reference to the particular experiments now under consideration, has been publicly questioned. I myself doubted it, and expressed my doubts, if not publicly, at least in conversation. I am content to have established - at all events to my own satisfaction - that, by following Dr. Bastian's directions, infusions can be prepared which are not deprived, by an ebullition of from five to ten minutes, of the faculty of undergoing those chemical changes which are characterised by the presence of swarms of Bacteria, and that the development of these organisms can proceed with the greatest activity in hermetically-sealed glass vessels, from which almost the whole of the air has been expelled by boiling." [67]

The fact that the debate was conducted mainly in terms of the mechanical conditions of the experiments is shown by the queries and criticisms which the described experiments excited. E. Ray Lankester assisted by C.C. Pode, Demonstrator to the Regius Professor of Medicine at Oxford, obtained results where no live bacteria appeared in experiments conducted under the same conditions as Bastian's. [68] Ray Lankester wrote a letter to Nature asking exactly what kind of cheese was used in Bastian's experiments, how much was added, how far it was pounded, were particles of cheese visible to the naked eye and was the infusion strained to remove lumps. [69] Lankester was hinting at the suggestion that lumps of cheese harboured bacteria germs which were preserved from the destructive action of heat during ebullition.

Similarly Roberts suggested that there was a possibility that atmospheric germs were introduced at the moment the vessels were sealed because of an accidental reflux of air just as boiling ceased, and that in these experiments Bastian had not ensured that the entire contents of the flask were exposed to boiling heat; this latter point, Roberts felt, explained why some animal and vegetable substances can be preserved unchanged after ebullition for five to ten minutes while other substances such as milk, solutions containing cheese or alkaline albuminous solutions invariably produce bacteria after being treated in the same way. His explanation was that with these more complex organic mixtures every particle of the mixture does not attain a boiling heat; during ebullition, the liquid froths and spurts and particles can adhere to the glass walls of the vessel and therefore escape the full effects of the heat. If the two conditions were satisfied, Roberts claimed that the flasks would remain barren. [70]

Ray Lankester published what he thought might be some sources of error in Bastian's experiments. [71] First of all the liquids had to be examined before the experiment to ascertain whether, in fact, a change had occurred in the fluid after the experiment because all sorts of particulate debris appeared in freshly boiled infusions. Then there was the question of whether or not all the liquid in the tube was kept at boiling point and finally the preservative effect of "lumps". He and his colleague kept several turnip-cheese infusions, prepared according to Bastian's instructions, free from bacteria for many days when Bastian had asserted that such turnip infusions invariably became turbid in one or two days owing to the presence of myriads of bacteria.

Ray Lankester insisted that Bastian was wrong to say that lower forms of life would appear in hermetically sealed flasks containing certain organic infusions which had been kept at boiling point for one to two hours.

"On the contrary, no organic nor inorganic infusion has been contrived by Dr. Bastian nor by anyone else which will develop Bacteria, still less Torulae, after exposure for one hour (or even less) to 212°F." [72]

He argued that turnip infusions of the sort Bastian described did *not* invariably become turbid after a day or two - he had kept one clear for six months. Yet he suspected that experimental errors could not explain every facet of these experiments.

"... failure in manipulation, contamination in unsuspected ways, such as that due to the preservative influence of lumps, and, again, the mistaking of particles in an infusion which have been there from the first for organisms originated *de novo*, does not exhaust the list of conceivable explanations of phenomena attributed to spontaneous generation. When the knowledge of the natural history of *Bacteria* has advanced somewhat further, there will be a possibility of such explanations presenting themselves in ways at this moment unsuspected." [73]

In the face of Ray Lankester's opposition Bastian cleverly managed to make it seem as if Burdon Sanderson was on his side.

"... the controversy... between Pasteur and Pouchet was as to the present occurrence or non-occurrence of heterogenesis. This is what they understood, and what the majority of people at the present day still understand, as "Spontaneous Generation." And as to the reality of this process, Dr. Sanderson has been convinced. He admits that Bacteria may appear in flasks, and other situations, where we are warranted in believing that no bacterial matter pre-existed - which is exactly equivalent to a belief in "Spontaneous Generation," in the sense implied by Pasteur and others. In support of this statement I have only to make the following quotations from his papers and repeated speeches of the last two years..." [74]

In particular Bastian referred to an experiment where Burdon Sanderson had injected dilute ammonia under the skin of a guinea-pig and where an inflammation was produced where the liquid exuded was found to be charged with bacteria. Burdon Sanderson stated that this process, "can be... produced by chemical agents *under conditions which preclude the possibility of the introduction of any infecting matter from without.*" [75] This was a very important result for Bastian as it was apparently a vindication of his views on the chemical nature of fermentation and putrefaction.

Vital Resistance

The problem of spontaneous generation raised a number of fundamental and abstract philosophical issues, not least of which was the relationship of living and non-living matter, yet as has been detailed above, the actual debate was conducted at least in its earlier stages, for the most part in terms of the physical conditions of the experiments which were undertaken. There was, as yet, no discussion of the different physical properties which different organisms, at various stages of their life cycles, might possess. The most important and most discussed physical condition related to the question of the vital resistance to heat of bacteria and their germs in solution and in particular the establishment of a temperature where all vital action ceased, namely a "death-point" of all organisms in experimental solutions.

The arguments on the two sides of the debate can be characterised as follows. For proponents of spontaneous generation, if in an experiment, a suitable infusion in a vessel was heated to a temperature at or above the agreed death point for a short period and sealed in such a way as to exclude contamination from atmospheric germs and then subsequently organisms appeared in solution, then this was an example of spontaneous generation (or archebiosis for Bastian). Solutions heated to high temperatures for prolonged periods remained sterile Bastian often argued, because the generative powers of the infusion, particularly if it was organic, were destroyed by prolonged heating. For critics of spontaneous generation, the appearance of micro-organisms in such experiments indicated that these organisms had been present in the solution at the beginning of the experiment, had survived the high temperatures and had continued to live and reproduce in the experimental solutions. It was not, however, only the case that different observers interpreted the *same* results in different ways; clearly competent observers actually obtained quite contradictory results when working with the same medium under similar conditions. On some occasions infusions remained sterile, but in other experiments organisms appeared.

It did therefore appear very much in Bastian's interest that an agreed death-point be established. He seemed to look at the problem as though the death-point, which was usually taken to be 100°C., was a rule of the debate, and that experimenters who seemed to him to be arbitrarily breaking this rule by suggesting higher death-points without sufficient evidence were somehow not playing the game by the rules. But as both sides of the debate could always interpret a given experimental result according to the arguments set out above, even if Bastian could have established an agreed death-point, it seems unlikely that he would have convinced his opponents of his overall philosophy.

Burdon Sanderson undertook a series of his own experiments from March to May of 1873. In these he carried out the experiments himself rather than acting as an observer and he made some slight modifications to counteract Roberts' criticisms. [76] In the first series he effectively repeated some of the experiments of late 1872; in the second set he increased the pressure within the vessels by use of a digester (or pressure cooker) such that the temperature was raised rather above 100°C. He found that the turnip-cheese infusions which were boiled for longer periods of from thirty minutes to an hour were less likely to develop bacteria than those boiled for fifteen minutes. Also, vessels boiled under pressure i.e. heated to more than 101°C., were much less likely to develop bacteria. Liquids heated under pressures of more than 1" mercury all remained barren although half were only subjected to the temperature greater than 100°C. for only fifteen minutes.

Burdon Sanderson concluded that:

> "... although all the flasks heated above 101°C. remained sterile, this fact affords no ground for concluding that any definite relation exists between that precise temperature and the destruction of the germinating power of the liquid in question. All that has been shown is that the *chance* that such a liquid will breed Bacteria is diminished either by slightly increasing the temperature to which it is heated, or increasing the duration of the heating. Thus it appears to me quite probable that if a sufficiently large number of flasks were heated even to 102°C., some of them would still be found to be pregnant." [77]

He was suggesting that the concept of a definable death-point for bacteria in solution was probably erroneous i.e that there was probably no single temperature where an experimenter could be sure that all bacteria and their germs were killed. Bastian was careful to claim that Burdon Sanderson's experiments confirmed his own as he too had concluded that in vessels exposed to heat for a long time or to high temperatures, the process of fermentation or generation of life was delayed and modified in intensity. [78] Of course Bastian's explanation was different. For Burdon Sanderson the longer the solution was heated the more chance there was that all the organisms already contained in it were killed, therefore the longer the boiling, the less chance that anything survived. However Bastian believed that all existing micro-organisms would have been killed after exposure to that temperature for a few seconds but the germinative and nutritive powers of the experimental infusion could have been damaged by prolonged boiling or exposure to excessively high temperatures.

In experiments on organic solutions Bastian had maintained that the action of heat destroyed certain of the complex and mobile organic products whose molecular instability supplied one of the essential conditions for the production of evolutional changes. In this view he was criticised by Walter Noel Hartley, Demonstrator in Chemistry at King's College, London for his omission to say exactly what the complex organic products were in his infusions and exactly what kind of molecular instability existed. Hartley claimed that as he could detect no visible change in the contents of his test tubes before and after experiments high temperatures could not be held to be a disruptive influence in the manner which Bastian suggested.

> "Dr. Bastian records the development of organisms in a liquid heated as high as 153°C.; yet the assumed "disruptive agency of heat" is supposed to have influenced the results of Schwann and Pasteur at a temperature of 100°C.! His experience is contradictory to his own theory, and at the same time to the experiments of others, to which his theory raises objection." [79]

To some extent Hartley's criticisms were fair - Bastian did not spell out the nature of the supposed complex organic products. In his experiments at temperatures of over 150°C. Bastian had used both an infusion of turnip and saline solutions and all had developed organisms. Bastian noted differences between several different types of solutions - inorganic and organic solutions, solutions of milk and of urine, acid and alkaline solutions - but although he furnished some sort of explanation as to why there should be differences in suitability for the production of organisms, his explanations were extremely simple and clearly inadequate. For instance he had explained Pasteur's negative experimental results with urine as due to the unsuitability of the medium "...to undergo evolutional changes of a high order ...or even to produce low organisms in great abundance." The positive results Pasteur obtained with milk were explained by Bastian as due to the fact that milk is a "highly nutritive and complex fluid" and not as Pasteur had supposed that pre-existing germs had been capable of withstanding a boiling temperature in milk. [80]

Bastian concurred with Pasteur's findings that boiled acid solutions were almost always unproductive of organisms and that it was much easier to obtain organisms in experiments with neutral or alkaline solutions. This observation was to prove important in Bastian's controversy with Pasteur in 1876 (See Chapter 4). Bastian felt that alkaline and neutral solutions were naturally better suited to the evolution and growth of organisms than acids and that differences between acid and alkaline solutions could be exaggerated due to the effects of heat. For instance, metals dissolve better in hot acids than in the same acids at room temperature.

> "Just as the acid seems to exercise a certain noxious influence even at ordinary temperatures, it may be conceived that this influence, whatever its nature, may be increased in intensity with the rise of temperature, and with the consequent greater facility for the display of chemical affinities." [81]

Much of Bastian's work in the 1870s involved attempts to establish a death-point for all forms of microscopic life in solution, based on the widespread belief in the early 1870s that all bacterial life in solution, whether in adult or germinal form, was killed by exposure to a temperature of 100°C. for a few minutes. This belief had been confirmed by Pasteur's original set of experiments in the early 1860s in his debate with Pouchet, except for experiments with milk where he found that a temperature of 112°C. was necessary for sterilization. [82]

In his writings Bastian constantly adduced the accepted temperature of 100°C. as being sufficient to kill all bacterial life. [83] However, he found that much lower temperatures destroyed adult bacteria which were deliberately added to solutions. He undertook two major series of experiments. The first set of experiments were reported in his book The Modes of Origin of Lowest Organisms, published in 1871. These experiments were different from his heterogenesis and archebiosis experiments as here the experimental vessels were deliberately inoculated with micro-organisms and observed before and after heating. Bastian always claimed that the atmosphere did not contain germs of bacteria in any great number and hence that the experimental vessels were largely free from contamination in his archebiosis experiments.

Neutral saline solutions in flasks inoculated with living bacteria, vibriones and torulae (i.e. rod-shaped organisms, spiral-shaped organisms and ring-shaped yeast organisms respectively) were heated to temperatures of between 131°-167°F. (55°-74°C.). The fluids in the flasks heated to temperatures of 140°F. (60°C.) and above showed no trace of turbidity (i.e. of bacteria multiplying) after being observed for 12-14 days. [84] Furthermore Bastian found that by prolonging the length of time the vessels were heated he could actually lower the death point temperature by up to 18 F.° (10 C.°). [85] For Bastian this was conclusive evidence of an actual death point of 140°F. for bacteria in neutral saline solutions. Bastian made two important assumptions in these experiments. The first assumption was that all micro-organisms responded similarly to heat; the second assumption was that germs or spores were no more resistant than adult organisms. In other words organisms displayed the same degree of heat resistance no matter which stage they were at in their life-cycle. Bastian's results suggest that he was managing to kill the adult organisms in his solutions before they reached a heat resistant spore stage. He had, in fact, established the death-point of *adult* bacteria and saw no reasons why germs or spores might display a greater degreee of resistance.

> "Such experiments would seem to be most important and crucial in their nature. They may be considered to settle the question as to the vital resistance of these particular *Bacteria*, whilst other evidence points conclusively in the direction that all *Bacteria*, whencesoever they have been derived, possess essentially similar vital endowments." [86]

By 1873 the situation with regards the death-point of microbial life was even less certain than it had been when Pasteur performed his original set of experiments. The observations of both Cohn and Dallinger and Drysdale confirmed Bastian's findings that adult bacteria in solution were actually killed by temperatures much less than 100°C. [87] Bastian thought that all bacterial life was killed in his experiments; he saw no reason why the germs of bacteria should behave differently with respect to the action of heat than adult forms. [88] Clearly contradictory arguments co-existed within the debate; able experimenters found that bacterial life in solution was killed at temperatures well under boiling, yet opponents of spontaneous generation believed that organisms could survive boiling temperatures. These results could not apparently be explained.

A further complication with regard to the death-point was the observed fact that different solutions clearly behaved differently with regard to their tendency to remain sterile in an experiment. Solutions of turnip and cheese and also of hay were particularly hard to sterilize. Bastian agreed with Pasteur that neutral and alkaline solutions were harder to sterilize than acid solutions, although Pasteur would hardly have agreed with the explanations furnished by Bastian. Again the explanation of these experiments rested very much on the physical and chemical conditions of the experiment - type and acidity/alkalinity of the solutions and the temperatures employed. Bastian, although listing the types of organisms present in the vital resistance experiments - bacteria, vibriones, torulae - made nothing of the fact that different organisms were present in different infusions and that different organisms could withstand heat differentially, believing rather that all bacteria had essentially similar vital endowments. There was no suggestion, as yet, that the difficult to sterilize solutions of turnip-cheese and hay might contain particularly resistant organisms. The influence of the life-cycles of these organisms does not appear in discussions of Bastian's experiments; only the "germ" and adult bacteria stage seems to have been recognised by opponents and exponents of spontaneous generation alike and there was no direct evidence to suppose that these two phases might behave differently.

The Importance of Life-Cycles

However some observers had suggested that either there was no single temperature which constituted a death-point even for a single type of solution (even acknowledging the clear differences between types of solution or infusion) or that the actual temperature at which all life ceased was actually far above the commonly accepted 100°C. One or other of these arguments were made Crace Calvert, Huxley, Child, Beale and Burdon Sanderson. Crace Calvert's experiments had suggested a death-point of 150°C-200°C [89] It has already been shown that Burdon Sanderson seemed doubtful that it was possible to establish a death point for bacteria and in his lecture "Biogenesis and Abiogenesis" Huxley had suggested that it was more logical to conclude that germs could withstand a higher temperature than was commonly expected than to believe in spontaneous generation. [90] As early as 1865 Child had suggested that it was possible that the germs of bacteria could withstand a boiling temperature. [91]

Some commentators had begun to realise that much more needed to be known about the lowest forms of life before the bewildering array of experimental results could be explained. Ray Lankester had already hinted that not everything which was observed in spontaneous generation experiments could be explained by experimental error. Beale had suggested that Bastian's experiments on vital resistance were insufficient evidence to prove his case for spontaneous generation because not enough was known about the organisms appearing in these experiments.

> "We have yet very much to learn concerning the influence both of high and low temperatures upon the minute particles of bioplasm constituting the germs of the lowest forms of life. And there is

> no doubt that the effect of the same degree of temperature would be different at different phases of the life of each species of fungus or low organism, and at different periods of the year. The effect would also vary according as the organisms were exposed to sudden great alterations of temperature, or submitted to intense cold or heat by slow and gradual changes..." [92]

The most sophisticated criticisms and the understanding that more required to be discovered about the nature of these micro-organisms tended to come from those scientists who were most skilled in the use of the microscope, such as Beale who was one of the most able microscopists of the day. In particular there were two problems with regard to the nature of the micro-organisms in the experimental vessels which most observers were failing to address.

First of all there was the influence of different types of organism to be found in different substances and whether or not this could in any way explain the resistance or alternatively the fecundity of substances such as turnip-cheese infusion. Secondly there was the question of whether the germs of bacteria were more resistant than the adult form. As these germs were generally held to be ultramicroscopic organisms, it was very difficult to establish whether their resistance was in fact different from the adult form.

Very little was made of these factors at this stage in the controversy. Experimenters did often describe the organisms present but in a general way using terms such as bacteria, torulae and vibriones. Rarely were individual organisms identified and there was no way that experimenters could ensure that a pure culture of a particular organism was obtained. In his experiments, Huizinga *did* identify individual organisms according to Cohn's classificatory scheme and at one point Ray Lankester had alluded to Pasteur's findings that the butyric form of bacteria could resist temperatures of 100°C. or even 105°C. but these points were just not taken up in the surrounding controversy. [93]

A series of experiments was undertaken by William Roberts and reported in 1874 to investigate the heat sterilization of various organic liquids and mixtures. Roberts discovered a considerable degree of variation in the duration of boiling required to sterilize different types of liquid. [94] Although Roberts was inclined to attribute his results to the resistance of atmospheric germs he was aware of the difficulty of actually demonstrating that the air contained germinal particles. Like Tyndall and Pasteur he agreed that the air must contain multitudes of particles capable of provoking generation, yet he admitted the difficulty of identifying these bodies.

> "It may be assumed that they *(germinal particles)* consist partly of true spores and partly of organisms themselves, floating amid the dust of the atmosphere or mingled with the molecular matter always present in water. But it cannot be said that they have ever been actually seen and identified. The ingenious attempts of PASTEUR and others to demonstrate germs in the air are manifestly illusory. Like them I have repeatedly collected air-dust and found abundance of molecules, circles, spheres and particles of various kinds under the microscope; but these could not be identified as true spores, nor distinguished from particles of inert dust. Indeed the objects sought after are so minute and so wanting in characteristic forms, that such a search, with our present instruments, appears well-nigh hopeless." [95]

Roberts found that torulae, or yeast organisms, and their germs seemed more susceptible to heat than bacteria and their germs. [96] He suggested that not only did the vital resistance of bacteria and their germs depend on the medium in which they existed but also that different species of bacteria, or different phases of their development are capable of very different degrees of resistance to heat. [97]

These results were to prove extremely important to the controversy in Britain, but their significance was not immediately obvious and Roberts was not the only observer to come to these conclusions as to the heat resistance of micro-organisms. He had, in fact reached this conclusion not only on the basis of his own extensive series of experiments but also on the investigations of Cohn in Germany and some extremely detailed work which had already been undertaken by Dallinger and Drysdale in England.

Ferdinand Cohn was Extraordinary then Ordinary Professor of Botany at the University of Breslau. In 1870 he founded the journal Beitrage zur Biologie der Pflanzen and began a study of bacteria. Although

Cohn's work was not generally available in English translations, several of his results were published in British scientific journals and were well known to the British scientific community. [97] Cohn did much to identify and classify the different species of bacteria. Geison suggests that he "tried to bring order out of the chaos caused by the use (especially by Pasteur) of vague and arbitrary names for bacteria and by the frequent introduction of new terms" [98] In his work, Bacteria, The Smallest Of Living Organisms, published in 1872 he offered a classificatory scheme, described the bacteria which caused fermentation and made a clear distinction beween the bacteria of contagion and the bacteria of putrefaction. [99] Cohn also confirmed Bastian's result that bacteria (i.e. adult bacteria) in solution could be killed by a temperature as low as 60°C. [100]

The Heat Resistance of Spores - Dallinger's and Drysdale's Experiments

The earliest experimental results to be published in Britain describing the life-cycle of a micro-organism and its resistance to heat in detail was the work of W.H. Dallinger and J. Drysdale. Their findings were published over a period of two years from 1873-1875 in The Monthly Microscopical Journal. [101] William Dallinger was a Wesleyan minister and biologist and also an able and precise microscopist. These experimenters appreciated the contradictions inherent in experiments on the *de novo* origin of life and called for a more detailed study of the life-cycles of the organisms involved.

By continuous observation of a particular monad, a type of septic organism allied to bacteria but larger and therefore more easily observed, present in a drop of infusion of macerated cod's head, for a period of between eight and fourteen days they were able to work out the life cycle of the organism. This included both a period of multiplication by fission and a phase where sporules burst forth from a distended cyst and gradually acquired adult form. [102]

For this monad, a dry heat of 121°C. was found to destroy adult forms but not all the sporules. In experiments on the organism in solution it was found that,

> "...a temperature of 66°C., given to the infusion, destroys all adult forms; but we have found *young monads appear and develop* in an infusion which has been raised to 127°C., suggesting that the sporule is uninjured in a temperature considerably above that which is wholly destructive of the adult." [103]

Dallinger and Drysdale then went on to question Bastian's assertion that invisible gemmules possessed no higher power of resisting the destructive influence of heat than the parent organism. [104] This was the first time that the belief that spores or germs of bacteria were no more resistant than the adult organism was definitively challenged in the British debate.

Over the next year these experimenters worked out the morphological history of altogether three forms of monad which had not previously been described; two of the forms exhibited a sporule phase in their life-cycles, while the third gave birth to minute living organisms. Not only were there individual differences in the ability to resist heat amongst the three organisms, but there was a particularly marked difference between the organisms which emitted sporules and the one which did not.

> "... the two forms which emitted sporules were able to survive, *by means of their sporules*, a temperature of 148.88 C., whereas the form which gave birth to minute *living* forms only feebly survived a temperature of 82.22 C; while again the form whose sporules were too minute to be seen appears to have slightly the advantage in the contest with heat." [105]

These two observers endeavoured to explain the cause of this not in terms of some exceptional power which the sporules might possess but in terms rather of some protecting envelope or medium - the nature of which was as yet unknown. [106]

Conclusions

The situation with regard to the spontaneous generation controversy in the middle of the decade presents an interesting if complex picture. At the beginning of the 1870s an upsurge of interest in infectious disease, Lister's work on antisepsis and a number of challenges to Pasteur's earlier work coupled with Owen's support for spontaneous generation had brought the subjects of the germ theory and spontaneous generation to the attention of the scientific and medical communities. Anxiety about the positive light in which spontaneous generation was presented, prompted both Huxley and Tyndall to enter the fray to declare their support for the germ theory and their opposition to spontaneous generation; Huxley by means of his Presidential Address to the B.A.A.S. in 1870; Tyndall in his "Dust and Disease" lecture.

Bastian as the chief supporter of heterogenesis and archebiosis undertook extensive series of experiments and had published widely on the subject. It was largely Bastian's efforts which made the debate so lively throughout the decade. Several well known scientists and medical men became drawn into the debate at one time or another. Bastian was criticised by Ray Lankester, in particular, for lack of care in his experiments. At this stage of the debate the focus was on the physical conditions of the experiments and in particular the concept of vital resistance was seen to be important. However by agreeing that organisms did in fact appear under the conditions Bastian stipulated, Burdon Sanderson lent Bastian's position a good deal of credibility.

The inconclusive results obtained made most of these observers begin to suspect that Bastian's results could not just be explained away by experimental error and also that a single death-point for all organisms did not exist. The suspicions of Ray Lankester, Burdon Sanderson, Child and Beale that much more needed to be known about the organisms themselves were confirmed by the more precise studies of bacterial life made by experimenters such as Roberts, Dallinger and Drysdale in Britain and Cohn in Germany. Only after these experiments did the conditions become more favourable for the production of an explanation. However the debate was still far from its conclusion. Bastian, of course, never accepted these results; Tyndall when he re-entered the discussion took some time to understand the significance of this work and apply it to his own experimental studies. Chapter 4 shows how such results were incorporated into the work which was to prove decisive in the latter half of the decade.

CHAPTER 4

THE END OF THE DEBATE

Introduction

Tyndall was characteristically busy in the first half of the 1870s with a visit to America, a priority dispute with P.G. Tait, the Ayrton-Hooker affair, the reverberations of the Belfast Address and the publication of several books, to the extent that he did not return to the subject of spontaneous generation experimentally until towards the end of 1875. [1]

If he had hoped that interest in spontaneous generation would have quickly died away or that Bastian's experiments could have been refuted by his own earlier arguments or the subsequent experimental work of other researchers with the eventual triumph of the germ theory, he was to be disappointed. It was true that Bastian was probably the only figure who gave whole-hearted support to all forms of spontaneous generation, but a considerable spectrum of opinion existed especially within the medical world; there was sympathy, in medical circles, for what could be interpreted as a form of spontaneous generation in the shape of the *de novo* origin of disease. The medical and scientific press continued to discuss the germ theory and spontaneous generation, and several investigators, both in Britain and abroad, had turned their attention to the subject.

Tyndall had condemned Bastian's experiments and his interpretation of them. It was true that the other British experimenters criticised Bastian's results and even when there was agreement with his experimental results, had refused to admit that these were instances of spontaneous generation. However by taking up the subject and engaging in debate with Bastian they did much to keep the topic alive. And what was possibly most important to Bastian, the eminent Burdon Sanderson had agreed with him in print, that solutions could be prepared according to Bastian's directions which would not be deprived of life after five minutes' boiling.

The problem for Tyndall was that those who opposed Bastian did not necessarily offer support for his own position. Had the germ theory received more favourable reviews in the medical journals, Tyndall might not have been tempted to re-involve himself in the debate, but it was clear that the germ theory was not making the headway he desired to see and at least some of the reason he believed was due to the continued discussion surrounding spontaneous generation.

This chapter outlines the reasons for Tyndall's return to the spontaneous generation debate in terms of continuing criticisms of the germ theory. Tyndall's first set of experiments are described; the results of these completely opposed Bastian's findings. Tyndall managed to draw Pasteur into the debate whereupon the French savant disputed Bastian's results in connection with the fermentability of urine.

Meanwhile Tyndall performed a second series of experiments where many of his experimental infusions succumbed to putrefaction after boiling. These results opposed the conclusions of his first set of experiments. Having at last been made aware of the importance of heat resistant spores by Cohn's work, Tyndall was able to explain the results of his experiments through the presence of such organisms. Following on from this, he developed a method of discontinuous boiling which killed all spores in solution and was therefore able to sterilize any experimental infusion.

Pasteur challenged Bastian's results on urine and a Commission of the French Academy of Sciences was appointed to judge between their experiments. Confusion over the conditions under which the Commission was to operate meant that it never met. Long afterwards a colleague of Pasteur's was able to explain why Pasteur's and Bastian's results differed. In Britain a final exchange between Tyndall and Bastian in The Nineteenth Century in 1878 marked the end of the debate.

Tyndall Returns to the Debate

Two specific events precipitated Tyndall's return to the debate. Firstly, he wrote a letter to The Times in November 1874 on the subject of typhoid fever, the criticisms of which made it abundantly clear that if he wished to convince the medical profession of the veracity of the germ theory there was still much work to be done. [2] Secondly, in a widely reported debate at the Pathological Society in 1875, Bastian made a lengthy attack on the germ theory. [3] Hence Tyndall's re-involvement in the debate can be seen not only as an attempt to vanquish Bastian once and for all but also as an attempt to rescue the germ theory from the doldrums.

Tyndall's letter to The Times, printed on November 9 1874, requested space to bring to public attention Dr Budd's recently published book, Typhoid Fever with its striking analysis of the mode of communication of that disease. [4] By writing to The Times, Tyndall was essentially using the same means of publicity as he had already done in 1870. Although it would have been unusual for a scientist to publish original results in The Times, it was not unusual for the newspaper to receive many letters on that recurrent social problem, epidemic and contagious disease.

William Budd was lecturer in medicine and physician at the Bristol Royal Infirmary. [5] Along with John Snow, William Farr and John Simon, he can be viewed as one of the new breed of "scientific" public health reformers who were ardent contagionists and one way or another came to accept the germ theory in favour of older, and to Tyndall, less scientific theories of disease.

For Tyndall, the appeal of Budd's work was that it was a practical example of "scientific medicine" at its best. Budd had applied reasoning based on the germ theory and the belief that disease germs always "breed true" in a convincing explanation of the communication of typhoid fever.

But the response of the medical profession was somewhat negative. [6] Budd's views were not accepted universally in medical circles; in particular it was still widely believed that typhoid or enteric fever could arise if drinking water were contaminated with sewage without the introduction of a typhoid germ. [7] These critics were also aware that as he was not a medical practitioner, Tyndall could hardly be expected to know about such things. Even if he gave the "sanction of his scientific authority" to Budd's solution to the problem of typhoid, the weight of that authority was not enough to convince those who had to deal with such diseases at a practical level. [8]

The Pathological Society Debate

The widely reported debate on the germ theory, led by Bastian, under the auspices of the Pathological Society brought a more weighty blow against the fortunes of that theory. [9] The debate was a lengthy one - it was conducted over three separate meetings of the society and included such distinguished speakers as Burdon Sanderson and Charles Murchison; the latter had recently published his Treatise on Continued Fevers. [10] The tenor of the debate is interesting - those who took part in it were eminent, scientifically inclined medical men, most of whom had conducted microscopic investigations, rather than grass-roots practitioners such as general practitioners, Medical Officers of Health and others involved in public health matters. Tyndall may have expected that support for the germ theory would come from the more theoretically-minded medical men who were, at least, acquainted with scientific investigations. However if the participants of the Pathological Society debate can be taken as representative of the more scientifically inclined end of the profession, it can be seen that although they gave a great deal of detailed consideration to the germ theory, their findings largely pointed to the inconclusive nature of the role of germs in disease and so they were unwilling to offer wholehearted support for the germ theory.

A number of important issues emerged from the debate. The main reason that these medical men were unwilling to commit themselves at this stage was that not enough was known about the aetiology of contagious disease and the microscopic particles which appeared to accompany some but not necessarily all such diseases. Hence the idea of a single comprehensive germ theory was impossible at this stage. There was no one germ theory, rather several variations and even the most avid supporters such as Lister had to fall back on complicated explanatory devices to account for the phenomena he observed. [11] Even a natural ally like Burdon Sanderson was characteristically uncertain and unwilling to commit himself to one particular version of the theory.

Ultimately, it was an improved knowledge of micro-organisms which was to be decisive in the spontaneous generation debate, and the leaders of the medical profession were well aware of the importance of that knowledge for the germ theory. However the main supporter of the germ theory in the British debate, namely Tyndall, was, as yet, unaware of the importance of focusing on the organism rather than on physical experimental conditions, despite the fact that several British scientists had published results which showed the importance of this change of emphasis.

Tyndall's Boiled Infusion Experiments

Tyndall began his experiments in the autumn of 1875 and involved Huxley in the microscopic examination of the solutions he employed in his experiments. [12] The results of these experiments were described in a lengthy paper delivered before the Royal Society in January 1876, published in the Transactions as "The Optical Deportment of the Atmosphere in Relation to Putrefaction and Infection". [13]

In this paper, Tyndall reminded his audience of his original light beam experiments on the dust of the air and how this floating matter would settle in a suitably closed chamber, rendering the air optically pure. [14] He also evinced the similar result, backed up by Lister, that air which has passed through the lungs has lost its power of causing putrefaction. [15] Tyndall tested this result by the fact that expired air contained nothing which would scatter light ie. total darkness resulted.

Tyndall gave his reasons for turning his attention again to such questions. These were (a) because this method of examination of air had not been utilised by other scientists, (b) because he wanted to free both his mind and others "from the uncertainty and confusion which now beset the doctrine of spontaneous generation" and (c) because recent overwhelming criticisms of Pasteur had influenced medical opinion to the extent that the medical profession was still undecided as to whether, ".. disease germs are always produced from like bodies previously existing, or whether they do not, under certain conditions, spring into existence *de novo*". [16]

An ingenious experimental chamber was constructed (see Appendix A). In the top of the chamber a pipette (p) was held in place in an air-tight sheet of india rubber so that the lower end of the pipette could move freely within the chamber. Tubes a and b were bent so as to intercept and retain any particles carried between inner and outer air. Twelve test-tubes which were to contain the experimental liquid were fixed in air-tight holes. A searching beam was arranged to pass through the side windows of the case.

When Tyndall began his experiments in September 1875, a concentrated beam showed the air in the case to be loaded with floating matter - after three days the absence of a track of light in the case showed that the floating matter had been deposited in the interior surfaces where it was held with glycerine. [17]

His first experiments were on fresh urine. Eight tubes in the case were half filled and boiled for 5 minutes. A second series of eight tubes was similarly filled and boiled but left on a stand exposed to the air of the laboratory. After four days all the <u>protected</u> tubes remained clear while all the <u>exposed</u> tubes were turbid. [18] Subsequent microscopic examination of the turbidity revealed swarms of bacteria in the exposed tubes. [19]

A similar experiment was constructed with mutton infusion, beef, haddock, turnip and hay infusions. Turnip infusion was particularly important as Bastian had found it especially potent of life. [20] In all cases (except where a test-tube was accidentally cracked when the case was moved) the protected tubes remained perfectly clear for months, while the exposed tubes swarmed with bacterial life after a few days. This was a very important result and one that was completely the opposite to Bastian's findings. Tyndall claimed on this basis that an organic infusion which was boiled for five minutes and protected from the contamination of the atmosphere would not succumb to microscopic life.

Some of the cases were opened and the clear infusion examined under a microscope. To his astonishment he found in five of the tubes that there were live bacteria and this was despite the fact that these tubes had hitherto remained clear. Eventually he pinned his "error" down to the fact that the pipette he used had retained a tiny drop of liquid on its end due to capillary attraction. [21]

He was anxious to refute arguments against atmospheric germs based upon their being beyond the reach of the microscope. Both his own experiments and those of Brucke, Stokes and Rayleigh amongst others had shown that there are particles which can scatter light but are ultra-microscopic. [22] These germs are not hypothetical because the microscope fails to reveal them, he claimed:

> "... in the concentrated beam we possess what is virtually a new instrument, exceeding the microscope indefinitely in power. Directing it upon media which refuse to give the coarser instrument any information as to what they hold in suspension, these media declare themselves to be crowded with particles - not hypothetical, not potential but actual and myriadfold in number - showing the microscopist that there is a world far beyond his range." [23]

Tyndall affirmed that these ultra-microscopic germs were as potent in infection as the adult bacteria and were prevalent everywhere in the atmosphere. But he was anxious to point out that complete bacteria and the atmospheric matter from which they came were in general different things. He and others had never found adult bacteria in the atmosphere. [24] Tyndall alluded to the importance of the relationship of the germ theory to spontaneous generation and how all the medical world was actively discussing bacteria and contagia. He referred to the importance of the germ theory debate which had taken place amongst the members of the Pathological Society.

"The Conference was attended by many distinguished medical men, some of whom were profoundly influenced by the arguments, and none of whom disputed the facts brought forward against the theory on that occasion. The leader of the debate, and the most prominent speaker, was Dr. Bastian, to whom it also fell the task of replying on all questions raised. The coexistence of *Bacteria* and contagious disease was admitted; but, instead of considering these organisms as 'probably the essence, or an inseparable part of the essence' of the contagium, Dr. Bastian contended that they were 'pathological products,' spontaneously generated in the body after it had been rendered diseased by the real contagium." [25]

In repeating Bastian's experiments Tyndall did everything he could to meet the requirements laid down by Bastian for the production of life, even to the extent of hanging some of the warm tubes in the Turkish bath in Jermyn Street where they were subjected to a temperature of between 101-112°F. (38-44°C.) for several days, as Bastian had emphasised that the experimental tubes should be kept at a suitably warm temperature after boiling. [26] His attempts to display the diffusion of atmospheric germs, at times became somewhat over-zealous. As well as distributing tubes of infusion to many of his friends, he distributed nearly 1000 exposed tubes all over the Royal Institution, from the roof to the kitchen in the basement, all of which, in time, were smitten with putrefaction and minute organisms. The Royal Institution cannot have been an entirely wholesome place to visit while these experiments were being conducted!

Finally, in an attempt to explain the operation of epidemics in terms of the germ theory, Tyndall reported his efforts to "map" the distribution of atmospheric germs. He filled a large shallow tray with suitable organic infusions.

"Into it the germs would drop; and could the resulting organisms be confined to the locality where the germs fell, we should have a floating life of the atmosphere mapped, so to speak, in the infusion." [27]

The conclusion of these experiments was that the germs in the air were not uniformly distributed as regards either quantity *or* quality. Bacteria might take possession of one tube, while another would succumb to mould. In many tubes there was a struggle between bacteria and penicillium. In some tubes the bacteria moved rapidly, in others they were languid; some contained few bacteria, others contained swarms of them. Tyndall explained these results in an analogous way to the spread of an epidemic.

> "It becomes obvious from these experiments that of two individuals of the same population exposed to a contagious atmosphere, the one may be severely, the other lightly attacked, though, as regards susceptibility, the two individuals may be as identical as two samples of one and the same mutton-infusion." [28]

The fact that in Pasteur's work of 1862, some flasks had succumbed to bacterial life when opened and others had not was to be explained by the fact that sometimes flasks were opened in the midst of a bacterial cloud and so engendered life; sometimes they were opened in the space between two bacterial clouds and no life was obtained. [29] Tyndall tested this hypothesis by opening thirty one identical flasks; thirteen of these produced life.

> "Instead of our tubes, let us suppose thirty-one wounds to be opened in the same ward of a hospital; plainly what has occurred with the tubes may occur with these wounds - some may receive the germs and putrefy, others may escape. Helped by the conception not only of germs, but of germ-clouds, the different behaviour of wounds subjected apparently to precisely the same conditions will cease to be an inscrutable mystery to the surgeon." [30]

In the published reports of these experiments, which took place in the latter half of 1875, several issues are important. Firstly Tyndall claimed that all the protected tubes in his experiments remained sterile with the the only exceptions being those where definite experimental errors had occurred eg. a cracked tube, contaminated pipette etc. This implied that not only Bastian, but all those who had obtained bacteria in infusions which had been boiled for five minutes and hermetically sealed, were similarly in error, as far as Tyndall was concerned. Among this number were William Roberts and Burdon Sanderson.

> "The evidence furnished by this mass of experiments, that Dr. Bastian must have permitted errors either of preparation to invade his work is, it is submitted, very strong." [31]

In 1874, William Roberts had published the results of his "plugged bulb" experiments which showed that organisms would sometimes appear in protected hay infusions after boiling. Many organic infusions including fresh urine and fresh unneutralised hay infusion were relatively easy to sterilize, yet alkalised hay infusion was extremely resistant to heat. Roberts had attributed this result to the possible resistance of some of the organisms involved. [32] However Tyndall suggested instead, that these results were due to errors in Roberts' work. The pipette in his experiments was plugged with cotton wool before hermetic sealing and the bulb of the pipette was dipped into boiling water or hot oil for the requisite time; the end of the pipette was filed off and the vessel was then set aside.

> "The arrangement is beautiful, but it has one weak point. Cotton-wool free from germs is not to be found, and the plug employed by Dr. Roberts infallibly contained them. In the gentle movement of the air to and fro, as the temperature changed, or by any shock, jar, or motion to which the pipette might be subjected, we have certainly a cause sufficient to detach a germ now and then from the cotton-wool, which, falling into the infusion would produce its effect." [33]

Tyndall claimed that in repeating Bastian's experiments he had followed his instructions to the letter as regards the temperature which the tubes were held at after boiling and as to the concentration of the infusions, as Bastian felt both these factors were very important, but still he obtained negative results. In this he clearly set himself at odds with Burdon Sanderson's findings.

> "... I have worked with infusions of precisely the same specific gravity as those employed by Dr. Bastian. This I was specially careful to do in relation to the experiments described and vouched for, I fear incautiously, by Dr. Burdon Sanderson in vol. vii. p. 180 of 'Nature'. It will be seen that, though failure attended some of his efforts, Dr. Bastian did satisfy Dr. Sanderson that in boiled and hermetically-sealed flasks *Bacteria* sometimes appear in swarms. With purely liquid infusions I have failed to reproduce this result." [34]

The other pertinent feature of Tyndall's experimental reports is the way in which he managed to establish a crude analogy between them and the germ theory, in claiming that the behaviour of thirty one experimental tubes was similar to the behaviour of thirty one wounds in a hospital ward. This showed his remarkable insensitivity to current medical knowledge and the crudeness of his theoretical stance. To many members of the medical profession, Tyndall's ideas must have seemed almost deliberately calculated to annoy, with his suggestion that it was possible to extrapolate from a light beam and the behaviour of some tubes of organic infusion to the aetiology of infectious diseases.

There was certainly some experimental support for the belief that the particles which his light beam experiments detected were indeed ultra-microscopic, but this still did not prove that they were germs or the carriers of germs. Yet on the basis of this, Tyndall claimed that the light beam was a more sensitive instrument than the microscope. It was almost as if he were issuing a challenge to microscopists. As the next section shows, Lionel Beale, one of the most able microscopists of the day was not slow to take up the challenge.

Tyndall played down the influence of the "soil" or the state of the individual to which the germ carried disease. Although he certainly believed that some media were more nutritive than others, he did not believe that the medium had an important influence on the action of the supposed germ. This was contrary to standard medical opinion, whether germ theorist or otherwise. Every medical practitioner knew that the state of the individual was very important in deciding whether a disease would take hold and what its progress would be. Lister had stated that the soil played a very particular part in disease. [35] Perhaps the reason why Tyndall eschewed such ideas was that they were very ancient notions, arising well before ideas of the germ theory, and belonging originally to the miasmatic theory of disease with the idea of "epidemic influences". Perhaps for Tyndall such ideas were too inexact and depended too much on an element of caprice in the way nature acted, too much like vague influences, or too close to Bastian's view of heterogenesis where one organism could change into another and cause a different disease depending on its "soil". Science was powerless to offer an explanation of such insubstantial influences, but science, in the form of Tyndall's germ theory, *could* offer an explanation in terms of "germ clouds" to explain why one individual might succumb to infection and another not.

At this stage in the debate, Tyndall failed to see the significance of Roberts' results, attributing them to experimental error rather than to the possibility that micro-organisms in some particular state might, in fact, be resistant to boiling. Tyndall saw such "failures" as due to contamination from without. The organisms causing putrefaction were understood to come from the air, as yet Tyndall did not suspect that some the organisms appearing in these experiments could, under certain circumstances, already be contained in the experimental substances.

Tyndall's experimental results, presented to the Royal Society on February 13th, 1876 were also reported to a distinguished audience at the Royal Institution in the next week's Friday evening discourse. This guaranteed the maximum publicity for Tyndall's views, for not only was there the Royal Institution audience, but the wider audience who would read the reports of the discourse in the medical press or in Nature. The British Medical Journal and the Lancet both commented on the popularity of Tyndall's lecture where the audience filled the lecture theatre and overflowed into the passages. [36]

Whilst the Lancet, on this occasion, took a fairly neutral stance on this address and Bastian's opposition, the British Medical Journal clearly favoured Tyndall's point of view.

> "At present, the honours of war are generally held to be with those who hold the views so brilliantly enunciated and defended by Professor Tyndall. In any case, Professor Tyndall's experiments are of immense force in confirming the value and importance of such precautions as are involved in Professor Lister's antiseptic practice of surgery." [37]

Discussion and Criticisms

Bastian swiftly responded in a letter to The Times and a reply to the two medical journals and Nature. [38] He claimed that Tyndall's results were well known. It was quite easy to obtain organic infusions which would remain free from putrefaction. He used his old argument that the power to undergo evolutionary changes in an organic solution was damaged by boiling. [39] It was also suggested that Tyndall had not kept his infusions at a high enough temperature after boiling and there was no evidence that the infusions had been prepared at a suitable strength.

He set out a long list of observers or groups of observers who had obtained evidence of putrefaction in suitable fluids. This list stretched from Schwann in 1837 to a number of more recent observers including himself, Pasteur, Child, Bennett, Burdon Sanderson, Huizinga, Ray Lankester, Pode and Roberts. The fact that this list contained observers who had obtained organisms at some time in their experiments was uncontestable. But clearly Bastian meant the list to lend support to his theoretical position and in that sense it was misleading as it contained several scientists, including, Schwann, Pasteur, Burdon Sanderson, Ray Lankester and Roberts who clearly opposed spontaneous generation.

Tyndall was irritated by Bastian's response to his work and also by the way that he had cited Pasteur's work of the early 1860s in support of his position. Bastian often cited the work of other scientists in support of his when he could, even when they obtained similar results without in any way agreeing with his theoretical stance. Bastian seemingly could not conceive that an observer who obtained results similar to his could at the same time disagree with his interpretation. Tyndall believed that in drawing support from Pasteur's work, Bastian was profaning the high priest of germ theory. Tyndall wrote immediately to Louis Pasteur, including a copy of Bastian's <u>British Medical Journal</u> article, to show Pasteur to what use this young upstart was putting his work, and to warn him, for the first of many times, of the effect Bastian was having on medical circles.

> "You will see that Dr. Bastian takes the liberty of citing you as a supporter of his results. I wish you would send me two lines stating whether you consider him justified in thus citing you.
>
> It was high time to put a stop to Dr. Bastian. He was doing incredible mischief among the medical men of England and America". [40]

Tyndall replied to Bastian in the next issues of the two medical journals. [41] A letter of his which had appeared in the Times for February 3rd, 1876 was reprinted. Some further comments on the accuracy of Bastian's experiments were intended to remind him that Tyndall himself had a "discipline of six-and-twenty years in experimental inquiries of no easy kind." [42] Tyndall declared his intention of avoiding a "paper war" with Bastian and closed his reply by indulging in a certain amount of self congratulation as to the now more favourable tone the medical profession was adopting towards his enquiries.

> "On one point I have reason to congratulate myself, and that is the liberal tone which the medical press, with few exceptions, has observed towards me. Our science, theoretic and practical, is an organism every part of which shares the life of the whole. Isolate any portion of it from the general circulation, and that portion is doomed to atrophy and death. That the physician and physicist are mutually helpful units in this organism, will become more and more evident as time moves on." [43]

An immediate rejoinder from Bastian, again in both medical journals, brought Tyndall to task for ignoring the now well established fact that several experimenters had obtained the same results as he had, and thereby misrepresenting the state of the debate. [44]

At the same time the response to Bastian which Pasteur had sent to Tyndall appeared in Nature and the Lancet. [45] In this letter Pasteur expressed his pleasure that Tyndall was bringing the great authority of his philosophical spirit and experimental rigour to bear on the question of spontaneous generation. He cleverly turned round Bastian's implication that Pasteur's results supported his own to suggest instead that surely now Bastian had accepted his results of 1862 and also his later published results on blood, urine and grape juice. He was obviously not offended by Bastian citing him as a supporter, rather he suggested Bastian had accepted his results because to accept these results and yet retain a belief in heterogenesis involved believing that mere dust particles had more power to generate life than micro-organisms in the atmosphere. [46]

Beale joined in the debate in the British Medical Journal with a characteristically scathing attack on Tyndall's observations published in two parts on Feb 19th, 1876 and Feb 26th. [47] Beale was no supporter of spontaneous generation, but he clearly saw the assumptions inherent in Tyndall's work.

To start with, Tyndall was wrong in assuming that his infusions were perfectly sterile as it was quite possible for clear and pellucid fluids to contain hundreds or thousands of living bacteria.

But Beale's main criticism was of Tyndall's style in dealing with the press, public and his opponents. If Tyndall concluded that the press and all the world were behind him, it was probably because few dissenters dared express their views in public. [48] Beale also criticised Tyndall's use of a so-called "searching beam". Although Tyndall claimed to detect ultra-microscopic particles by means of his beam, Beale maintained that he could not know by this method whether he was able to detect bacteria in a fluid within the microscopic limit whether he could tell inorganic from organic particles, or whether dead and live bacteria could be distinguished. As the microscope could show all these things, it was no surprise that his method had "not been much turned to account". [49]

All the searching beams in the world could not reveal a bacterium. Tyndall seemed to think that just pronouncing spontaneous generation "a chimera" was acceptable instead of proving it to be the case. Beale also took issue with Tyndall about his views of the medical profession and his surprise that his views on the extirpation of disease were not accepted by them. If bacteria were the active agents in contagious disease, Beale asserted that it would never be possible to eradicate them. [50]

Having already elicited a letter from Pasteur for publication, Tyndall wrote again to Pasteur in the middle of February, 1876 to explain in more detail who Bastian was. He told Pasteur of the books and articles Bastian had written and his confident tones had made a considerable impression upon both the English and American publics.

However it was not the *public* but the medical profession that Tyndall was worried about.

> "The point of greatest practical importance, however, is the influence which his writings has exercised upon the medical profession. He has attacked your labours with great vivacity and although he has produced but little impression upon those who are intimately acquainted with your writings, he has produced a very great, and I would add, a very mischievous impression upon others." [51]

When Tyndall began his experiments his intention was not only to do service to science but also to do justice to Pasteur and so he had gone over much of Bastian's work and had refuted many of the errors which had misled the public.

> "The change which has occurred in the tone of the medical journals of England is very remarkable, and I am inclined to believe that the public faith generally in Dr. Bastian's accuracy has been considerably shaken." [52]

Tyndall intended to pursue his researches until every doubt regarding the unassailable accuracy of Pasteur's position was removed.

> "For the first time in history we have reason to entertain the sure and certain hope that, as regards epidemic disease, medicine will soon be rescued from empiricism and placed upon real scientific foundations; when that day comes, humanity, in my opinion, will acknowledge that their largest share of gratitude is due to you." [53]

<u>The Physico-Chemical Theory of Fermentation</u>

In May 1876, Bastian presented the first results from his experiments on boiled urine. In these experiments two new chemical agents were used, liquor potassae (a standard solution of potassium hydroxide) and oxygen, both of which were known to be stimulants if not active promoters of many fermentive processes. [54]

As Chapter 3 has described, it had already been noted by several observers, including Bastian and Pasteur, that neutral or slightly alkaline fluids were more prone to undergo fermentation than slightly acid infusions. The addition of a few drops of liquor potassae to a slightly acid infusion will cause fermentation to appear earlier and make more rapid progress. It seemed obvious to Bastian that the changes taking place in *boiled* acid and neutral solutions should also vary considerably.

In the autumn of 1875 he had begun a series of experiments to ascertain whether the fermentability of boiled urine, naturally a slightly acid liquid, could be increased by mixing it with a quantity of liquor potassae sufficient for neutralisation. Bastian's experiments were affirmative and so he undertook further experiments on boiled urine and boiled liquor potassae to ascertain whether the increased fermentability was due to the survival of germs or "to the chemical influence of potash in initiating or helping to initiate the molecular changes leading to fermentation in a fluid devoid of germs or other living matter." [55]

The first set of experiments was undertaken with flasks plugged with cotton wool, apparatus similar to that used by Roberts in his hay infusion experiments. Bastian developed a better technique where an amount of liquor potassae just sufficient to neutralise the urine was boiled in a small glass tube. The end of the small tube was drawn out to be sharp and brittle before the tube was sealed. This little tube was put into a retort of urine, the urine boiled and the drawn out neck of the retort sealed before ebullition ceased. (See Appendix A.) The whole retort was immersed in boiling water for a further 15 minutes. The liquor potassae was released by a sharp shake of the retort which broke the capillary end of the enclosed tube.

Bastian found that untreated boiled urine remained barren while boiled urine treated with liquor potassae would swarm with organisms within a few days; the latter effect was enhanced under the influence of oxygen and increasing the resting temperature from between 25°C.-30°C. to 50°C. [56]

Having previously evinced Burdon Sanderson's and even Pasteur's results in support of his own, Bastian was now able to use Tyndall's recent results to his advantage. Recalling that Professor Tyndall had recently strongly reinforced the belief that boiling for a few minutes killed all bacteria and their germs, Bastian concluded that the only explanation for the results of his experiments was that fermentation had been initiated without the aid of living germs. By insisting that all living organisms were destroyed by five minutes' boiling Tyndall was unwittingly lending support to Bastian's experiments where organisms were found after boiling. Bastian dismissed the hypothesis that the liquor potassae contained living germs as he had already found that the substance would only act as a fertilising agent when added in certain proportions, but if it contained living germs then, he argued, any amount would do, even a few drops: similarly the idea that the liquor potassae somehow revived the germs presumed killed in the boiled acid urine was untenable because liquor potassae in excess definitely prevented the origination of living matter but would not prevent the mere development and growth of bacteria germs. For Bastian the only feasible explanation was that the fertilising agent acted by helping to initiate chemical changes of a fermentative character in a fluid devoid of living organisms or living germs.

> "As a result of the fermentative changes taking place in boiled urine or other complex organic solutions, many new chemical compounds are produced: gases are given off, or these with other soluble products mix imperceptibly with the changing and quickening mother liquid, in all parts of which certain insoluble products also make their appearance. Such insoluble products reveal themselves to us as specks of protoplasm, that is of 'living' matter; they gradually emerge into the region of the visible and speedily assume the well-known forms of one or other variety of *Bacteria*." [57]

Pasteur's Experiments

In early July, Pasteur and his co-worker, Joubert communicated their results on the fermentation of urine to Comptes Rendus. [58] In this paper they pointed out that although urine was normally acid, when it fermented it became alkaline and ammonium carbonate was produced. Instead of a chemical hypothesis of fermentation, Pasteur assigned this action to a living organism, "le petit ferment organisé". [59] Normal urine would stay acid indefinitely when kept from contamination by the germ of this ferment. This had obvious implications in the medical world as it was clear that it was necessary to prevent the introduction of this organism into the bladder to prevent the production of irritating alkaline substances.

A week later Bastian reported his own experiments on urine to the Academie des Sciences. [60] This paper was an abstract of the work already printed in the Proceedings of the Royal Society. Bastian quite clearly aligned his results with the physico-chemical theory of fermentation and denied that the fermentation of urine was anything to do with germs in the air. [61]

Pasteur responded at a meeting of the Academy a fortnight later. [62] A copy of this reply was given to Tyndall for publication in the British scientific press and it duly appeared in the British Medical Journal shortly afterwards. [63]

> "For twenty years I have sought, without finding it, life without apparent pre-existing life. The consequences of such a discovery would be incalculable. The natural sciences in general, and medicine and philosophy in particular, would receive from it an impulse which no one can foresee. As soon, therefore, as I learned that I had been outstripped, I hastened after the fortunate investigator, ready to test his assertions. It is true that I approached him full of distrust. I had so many times found that, in the difficult art of experimentation, the most skilful stumble at every step, and that the interpretation of the facts is no less dangerous." [64]

Pasteur confirmed that Bastian's experiments were very exact in most cases. Bastian had emphasised that it was necessary to keep the vessels used in these experiments at a temperature of 50°C., but Pasteur felt this condition to be unnecessary. Pasteur agreed with Bastian that under the conditions described the experimental vessels would indeed become charged with bacteria. He suggested that if Tyndall thought otherwise it was "simply an act of forgetfulness on his part." [65] Pasteur was letting Tyndall of the hook, but it was clearly a difference between them, a difference which their opponents could use against them as Tyndall had positively asserted that boiled organic liquids would *not* ferment. The real point of disagreement between Bastian and Pasteur was in the interpretation of the experimental results; Pasteur did not believe that Bastian's experiments had proved that spontaneous generation had taken place.

> "...they only show that certain germs of low organisms resist a temperature of 100 cent. (212 Fahr.) in neutral or slightly alkaline media; doubtless because their coverings are not, in these conditions, penetrated by the water, while they are so, on the contrary, if the medium in which they are heated be slightly acid." [66]

Pasteur insisted that were Bastian to use solid potash heated to 110°C. or even his usual aqueous solution heated to 110°C. rather than 100°C., then he would have sterility in all cases. If he followed these instructions he could even omit the preliminary boiling of the urine.

Bastian's reply to the French Academy (also printed in the British Medical Journal) claimed that he had refuted Pasteur's hypothesis as to the survival of bacteria germs in boiled liquor potassae by his own experiments because urine only fermented when the exact amount of liquor potassae to neutralise the urine was added. [67] If the solution of potash contained germs, then a very minute quantity of it would be capable of acting upon an indefinite quantity of urine; this was an argument Bastian was to return to again and again. To emphasise the importance of keeping the experimental vessels at a higher temperature after boiling, a point Pasteur had dismissed, Bastian reported that he had persuaded boiled fresh and therefore slightly acid urine and indeed other boiled acid organic fluids to ferment after they had been exposed to a temperature of 50°C. when these infusions remained sterile at a temperature of 25°C. Bastian did not have an explicit theoretical explanation for the importance of temperature; Pasteur did not think it important and Tyndall explicitly denied that the effects which Bastian described with regard to temperature on acid infusions, actually took place. [68]

Pasteur could not let Bastian make these assertions without challenging them. If it was true that alkaline urine would produce bacteria, without already containing the germs of those bacteria, then what did Bastian's experimental conditions matter, such as whether the potash were liquid or solid or whether the urine was straight from the bladder or not? [69] But the point was, Pasteur claimed, he had already shown that urine fresh from the bladder, and boiled urine made alkaline with solid potash heated until it was red hot, would not ferment.

But Bastian claimed Pasteur achieved his negative results precisely because he added too much potash to the urine as he must have gone beyond the point of neutralisation. [70] Bastian had found solution of potash heated to 110°C. just as effective as that heated to only 100°C., *as long as it was added in the correct proportions to neutralise the urine*. This meant that boiled potash would not act as a ferment when either just a few drops were added or an excessive amount was added. Furthermore, Pasteur had missed the point about keeping the temperature of the urine at 50°C. Pasteur's liquids remained sterile because they were kept only between 25°C. and 35°C. All this evidence, claimed Bastian, favoured his interpretation.

Bastian reported his dispute with Pasteur to the British scientific community through the pages of Nature. [71] It was a particular trait of Bastian to try to pin down the theoretical issues under dispute to one experimental point. The issues here were the "exclusive germ theory vis-a-vis the broader physico-chemical theory which allowed for living matter to originate *de novo*" and these issues depended on the following question.

> "Can Bacteria or their germs live in liquor potassae (Pharm. Brit.) when it is raised to the boiling point (212°F.)? Such is now the simple issue to which certain great controversies have been reduced. If Bacteria germs cannot resist such an exposure, then, by M. Pasteur's own implicit admission, his exclusive germ-theory of fermentation must be considered to be overthrown by the broader physico-chemical theory." [72]

Towards the end of August, Tyndall wrote to Pasteur to send him Bastian's articles from the medical journals, this latest contribution to Nature and the abstract of the recent Royal Society paper. When Pasteur had Bastian's articles translated he suggested to Tyndall that the reason for Bastian's results was that the bacteria in these experiments, which he recognised as the same forms he had come across in 1862, he now realised would grow in neutralised urine but would not grow in acid urine. [73] This did seem to be an important result although it was not the whole explanation of what was happening in these experiments. Furthermore, Pasteur seemed unable to make sufficient capital out of this result in his dispute with Bastian. He felt sure Bastian would be convinced by the force of his results alone. Pasteur, at least at first, seemed more kindly disposed towards Bastian than Tyndall was. He emphasised to the latter that although Bastian was in error he made his mistake in good faith. The new set of experiments which he was planning, he expected would convince Bastian. [74]

Dallinger's Criticisms

As Chapter 3 describes, the Rev. Dallinger in collaboration with William Drysdale, had already made important discoveries with regard to the life-cycles of monads and the resistance of their spores to heat between the years 1873 and 1875. [75] These discoveries had yet to impact the debate significantly, due perhaps to the fact that Tyndall was still overly concerned with the idea that Bastian's experiments were in error to understand the impact of studies which showed that organisms could withstand boiling. Nevertheless Dallinger continued his work and offered searching criticisms of Bastian's conclusions with regard to archebiosis and heterogenesis. [76] These criticisms revolved round Bastian's ignorance of the life-cycles of the organisms he was studying, a subject in which Dallinger was an expert. In studying *monads*, which are larger but allied septic organisms to bacteria, Dallinger and Drysdale had already discovered that these organisms multiply not only by fission but by producing spores which were very resistant to heat. There was every reason to believe that bacteria acted analogously, despite the fact that they were so small the microscope could not reveal their spores.

> "I am ... convinced that the death-point of bacteria germs hovers very near the boiling point of water - a conviction amply sustained by fact. This being so, the survival, as germs, of some few, amidst incalculable myriads, by some accidental protection, is surely possible. So that, indeed, all true work now should be a study of the *germ* and its properties, and a discovery by patient research of the life-history of the organism.
>
> The valueless nature of mere temperature experiments on such organisms, as tests of their ability to survive, without a knowledge of their life-history, Dr. Bastian, *without knowing it*, has made sufficiently plain." [77]

One of the monads Bastian found in his experiments, Dallinger was able to identify as one of the six the life-history of which he and Drysdale had already worked out.

> "The evidence is as full as it may be; the monad Dr. Bastian saw was one whose life-history was fully worked out. As usual, it multiplies by fission, but the fission is multiple. It then passes to a sac-like condition, resulting from the uniting together or fusion of two individuals. This sac becomes still and bursts ... pouring out spore that taxed our highest powers and closest watching. The spore of only two of the monads studied survived after exposure at a temperature of 300°F. *This is one of them*". [78]

In the October number of the Popular Science Review Dallinger made a further attack on Bastian's work, this time against Bastian's conception of heterogenesis. [79] Although most of the British debate revolved round archebiosis experiments on boiled infusions, Bastian's work on heterogenesis was an important part of what he understood as spontaneous generation. It was also an important part of the concept of specificity in disease; if heterogenesis could take place then disease germs would not necessarily "breed true". Dallinger's work was the first comprehensive explanation of Bastian's heterogenesis results to be published.

Bastian emphasised that heterogenesis was the evolution of a living being from living matter or a living unit which was totally different. Tyndall took exception to this idea in his denial that a germ of a disease could give rise to different forms of disease under different circumstances, in other words he believed that disease germs always "breed true". Dallinger's objection, in harmony with his own naturalistic views, was that Bastian's idea introduced an assertion of caprice in biological laws and was ultimately as ridiculous as the idea that a gorilla could be born from a kangaroo! [80]

Dallinger accused Bastian's instances of "transformation" as being due to a "looseness of method, and a disregard of detail, minutiae, and above all continuity of research." [81] The important thing in these observations was continuity of observation; discontinuous or interrupted observation was worse than useless; the same individual organism had to have its history observed from beginning to end as he, himself, had done. This is why Bastian by returning to his microscope the next day found that his bacteria had apparently changed to monads - it was necessary to observe the *whole* life cycle. Dallinger knew of several cases which to the careless observer could seem to be cases of heterogenetic transformations. [82]

Bastian claimed that bacteria were constantly transformed into monads in the "proligerous pellicle" or scum which forms on the top of infusions. Dallinger pointed out that this scum was composed mainly of bacteria but other forms could be contained in the jelly-like layer. Millions of minute germs of diverse lowly life-forms might be interspersed in the pellicle. Monad spores are extremely small and their earliest development could not be detected. The supposed "transformations" were the slightly altered conditions of the pellicle resulting from the natural growth of interspersed monad germs. [83] Dallinger castigated Bastian for not observing the complete life-cycle of the monad continuously.

> "Thus what Dr. Bastian supposed was the "transformation" by "heterogenesis" of one vital form into another, was in fact only a series of stages in the metamorphosis through which a monad with an ascertainable history was passing." [84]

Dallinger's criticisms were interesting and perceptive. He was able to go much of the way to explain Bastian's results through his emphasis on a knowledge of life-history and heat resistant spores. The significance of his work was that it pointed to the fact that the events observed in spontaneous generation experiments, whether supposedly due to archebiosis or heterogenesis, could only be explained by looking at the life-cycles of the organisms themselves rather than the physical conditions of the experiments and the possible errors in experimental procedure. The error Bastian made in his work was not to observe the organisms involved over a long enough continuous period of time. But strangely Dallinger's work seemed to have made little impact on the debate.

Bastian made no reference to Dallinger's criticisms. Although Bastian, by the nature of his profession, was an experimental microscopist, he continued to emphasise the physical aspects of his experiments. This did not mean he did not employ a microscope to good use; he made many detailed observations and drawings of microscopical results but he clearly did not feel it necessary to make the sort of lengthy detailed observations which Dallinger called for.

Why did Tyndall not use Dallinger's criticisms to better effect? Clearly he knew of his work and the previous researches on the life history of monads and whilst he acknowledged Dallinger's work (he referred to Dallinger's heterogenesis paper in his Glasgow address on fermentation) he did not make the capital that he could out of it. [85] Tyndall could have used Dallinger's descriptions of life-histories and discoveries of heat resistant spores to lend support, on the one hand to his adherence to the notion of specificity of disease and on the other to explain why Bastian obtained organisms in his boiled infusions. On the latter point he could have extricated himself from the difficulty of actually agreeing with Bastian that organisms were killed by boiling.

Yet Tyndall was not a biological scientist. As a physicist it is perhaps hardly surprising that he failed to see the significance of detailed studies of organisms and that he continued to emphasise the physical conditions of the experiments at this stage in the debate. Neither was he a microscopist; he relied on Huxley for detailed microscopic examination of his infusions. With his advocacy of the "searching beam" as a more powerful instrument than the microscope, he may have either been unwilling to concede the role that the microscope had to play in the debate or simply have failed to understand its importance. Basically he believed in his own technique, a technique drawn from the physical sciences. He was in any case in no position to repeat Dallinger's and Drysdale's careful life-history studies.

It was not the case that Tyndall's physical technique in the form of a searching beam was somehow more convincing than a biological technique in the form of microscopy, as few scientists thought his searching beam practically useful, and it was never used by anyone other than Tyndall. As well as this, his own experiments had resulted in sterility every time and so he was not yet convinced that some organisms were resistant to heat and did not yet appreciate that this factor was important, preferring still to attribute Bastian's results to errors. Had his first set of experiments contained some failures then he would, of course, have had to look at reasons other than error to explain Bastian's experiments. Tyndall was also very much bound up with the idea that the appearance of life in the experiments was caused from external contamination, from the air rather than from the survival of organisms already contained in a particular material. In this belief he followed Lister's and Pasteur's conception of the germ theory. Essentially Dallinger's work was located in a different and novel paradigm for understanding the appearance, development and resistance to heat of low forms of life. It belonged to the paradigm of the new bacteriology which held only limited appeal while the significance of the life-cycle of organisms was not appreciated and while the main investigators in the spontaneous generation debate were still caught up in boiling experiments and searching beams i.e. physical conditions as opposed to detailed studies of the organisms involved.

Not long afterwards, however, Tyndall did learn of the significance of heat resistant spores from another source, from the German botanist, Ferdinand Cohn. Cohn's work showed that particular types of organism, prevalent in certain substances such as cheese, held the means to explain why infusions of certain substances were so difficult to sterilise.

Cohn had been working on the resistance of spores of micro-organisms to heat for some time. Nature reported his researches on turnip-cheese infusion and his discovery of a *Bacillus* or rod-like organism whose spores were very likely to be responsible for the results Bastian obtained in his turnip-cheese experiments.

> "The rennet contains a liquid ferment which causes coagulation of the milk; also ferment-organisms (Bacillus) which probably bring on butyric-acid fermentation, and cause the slow maturing of the cheese. It is their resting-spores that, enclosed by the dry cheese substance, resist boiling heat for a long time, and, in a suitable nutritive liquid, may afterwards develop to bacillus rods. (One of Dr. Bastian's results is thus explained.)" [86]

It cannot be said, however, that Cohn's work constituted a "crucial" experiment. The report of the cheese bacillus, indeed, made no impact on the debate at this stage. Bastian's Royal Society paper was published in abstract in Nature a week later and he was by now, firmly embroiled with Pasteur in the debate over the fermentability of urine. [87] Much of the progress of the British spontaneous generation debate followed this pattern. Rather than the universally acknowledged resolution of an issue, interest moved on to other areas. Essentially this was what happened with the turnip-cheese experiments, Tyndall's discontinuous boiling experiments and the controversy between Pasteur and Bastian. Work of a supposedly crucial nature was performed, but it was not necessarily accepted as such at the time. Certainly, the fact that Bastian maintained his original stance and continually denied the validity of his opponents' theoretical position did much to dilute the force of their arguments.

The meeting which took place between Cohn and Tyndall in the autumn of 1876 when Tyndall returned from his annual visit to Switzerland, was a significant event. [88] At this meeting Cohn gave Tyndall a copy of his journal, Beitrage zur Biologie der Pflanzen for July 1876 in which he reported his experiments on hay infusion in which organisms were found even after two hours' boiling. [89] The adult organisms or rod-like bacteria were termed *Bacillus subtilis* or hay bacillus by Cohn. [90] In the journal an essay by Robert Koch was published; this described the splenic fever or anthrax bacillus. The behaviour of the hay bacillus mirrored that of the anthrax bacillus in that both bacilli form spores highly resistant to heat and dryness. [91]

Tyndall's Second Series of Experiments

On October 19th 1876, Tyndall delivered an address to the Glasgow Science Lectures Association, entitled "Fermentation and Its Bearings on Surgery and Medicine." [92] In this address Tyndall discussed the analogy between fermentation and the production of alcohol and putrefaction and contagious disease. He also discussed Koch's work on the anthrax or splenic fever bacillus, *Bacillus anthracis*. [93] This was probably the first time Koch's work was reported in Britain. [94] It was clear that Tyndall had by now, become aware of the significance of spore formation in bacteria.

Tyndall referred to Burdon Sanderson's work which was the most recent British account of splenic fever. The contagium had been proved to persist for years in localities where it had once prevailed and this seemed to suggest that the rod-like organisms which, it had been established, were definitely connected with the disease, could not themselves be the actual contagium as their infective power vanished within a few weeks. Sanderson concluded that the contagium existed in two distinct forms. One form was "fugitive" and visible as transparent rods; the other form was permanent but "latent" and not visible to present day microscopes. [95]

Koch observed the life-cycle of the rod-like organisms and was able to see the gradual formation of spores. He dried infected blood containing only the adult organisms and this remained infectious for five weeks at most. But dried blood containing fully developed spores could retain its power of infection for years. This was the explanation of Burdon Sanderson's latent form of infection and this also held the clue as to how some organisms could resist a boiling temperature.

Through the latter part of the year Tyndall and Pasteur continued to correspond and to work on their respective urine experiments. Pasteur sent Tyndall a detailed description of his experimental technique. [96] Tyndall kept Pasteur up to date with Koch's and Burdon Sanderson's work and Bastian's publications. [97] Pasteur's own work revealed, as he had already predicted, that the bacteria which form in alkaline or neutral urine at 50°C. cannot multiply at all in acid urine, thus furnishing an explanation for Bastian's results, he suggested to Tyndall. [98] Both scientists agreed to delay publication of their experiments so that differences between them would not be misinterpreted and they could present a united front against Bastian. [99]

Tyndall's hopes of holding off from publication were frustrated when he received a paper sent from William Roberts, criticising Bastian's experiments, with a view to having it presented to the Royal Society. Tyndall told Pasteur that he could not send it in without a note to say that he had also made experiments on the subject and that as regards the heating of the potash to a temperature above that of boiling water, Pasteur had anticipated both Roberts and himself. [100]

Tyndall communicated Roberts' paper to the Royal Society in the middle of December, 1876. [101] In this paper Roberts pointed out that Bastian's results only confirmed the general rule which he had previously observed that slightly alkaline liquids were always more difficult to sterilize by heat than slightly acid liquids. The evidence for this was especially strong in his experiments with hay infusion where acid infusion would remain barren after only a few minutes' boiling while neutralised infusion invariably became fertile after a similar boiling. The question was, could the change of reaction enable pre-existing germs to survive the ebullition or was it true that the addition of the alkali exerted a positive influence in the *de novo* generation of organisms?

Roberts devised an experiment where liquor potassae was heated in a tube with a capillary portion to a temperature of 280°F. (138°C.) in an oil bath. When sealed this tube was introduced into a flask of urine, the flask boiled, and at the end of a fortnight shaken so that the capillary point of the little tube was broken and the urine neutralised. After three days in an incubator all the tubes remained sterile.

Tyndall's "Note on the Deportment of Alkalized Urine" was published along with Roberts' paper. [102] He was able to confirm Roberts' results and also pointed out that Pasteur had obtained similar results earlier in the year by raising the potash in the experiments to 110°C.

Hay Infusion and Discontinuous Boiling

Tyndall continued to experiment on different substances, having turned his attention, in particular, to hay infusion after he was alerted to the difficulty of sterilizing such infusions by Roberts' and Cohn's work. Tyndall submitted a paper to the Royal Society in January, 1877. [103] His results could only be described as remarkable as they were completely contrary to those of a year earlier when five minutes' boiling had sufficed to sterilize all the organic infusions he employed. These results now confirmed some of Roberts' and Cohn's results.

He now found that whereas some alkalized hay infusions were completely sterilized by five minutes' boiling, other cases withstood the boiling temperature for a much longer period. Using his closed wooden chambers as before, and cotton-wool plugged vessels as in Roberts' experiments, he obtained failure after failure even using precautions far greater than a year previously.

> "I tried to reproduce the results with animal infusions obtained with such ease and certainty a year ago. Some of these old infusions, highly concentrated by evaporation, remain with me to the present hour; they are as clear as distilled water. But in my recent experiments, where the care bestowed far exceeded that found necessary in my last inquiry, the animal infusions, like the vegetable ones, fell, for the most part, into putrefaction." [104]

The difference between these results and those of a year ago, Tyndall explained with reference to the hay infusion experiments. Where alkalized hay infusion was sterilized by five minutes' boiling, the hay had been mown in 1876. However in almost every case of greater resistance to sterilization the hay was mown in 1875 or earlier. The hay which was most difficult to sterilize was five years' old. "To the drying and hardening of the germs of the old hay by time I ascribe this singular result." [105]

The samples of hay used in these investigations made the atmosphere of the Royal Institution so infective that all manner of infusions succumbed to putrefaction and precautions which had been sufficient a year ago were found to be absolutely ineffectual. Thanks to Sir Joseph Hooker, Tyndall was able to set up a series of experiments in the new Jodrell Laboratory in Kew Gardens where the difficulties found in London disappeared and results were obtained in accordance with the earlier investigations. Tyndall also took the opportunity to report these results to the public in one of his Royal Institution Friday evening discourses with the dramatic title, "A Lecture On a Combat with an Infective Atmosphere". [106]

A month later Tyndall was ready to describe the method he had discovered to sterilize the most obstinate infusions, by simple means and at temperature lower than that of boiling water. [107]

> "The secret of success here is an open one. I have already referred to the period of latency which precedes the clouding of infusions with visible *Bacteria*. During this period the germs are being prepared for their emergence into the finished organism. They reach the end of this period of preparation successively - the period of latency of any germ depending on its condition as regards dryness and induration. This, then, is my mode of proceeding:- Before the latent period of any of the germs has been completed (say a few hours after the preparation of the infusion), I subject it for a brief interval to a temperature which may be under that of boiling water. Such softened and vivified germs as are on the point of passing into active life are thereby killed; others not yet softened remain intact. I repeat this process well within the interval necessary for the most advanced of those others to finish their period of latency. The number of undestroyed germs is further diminished by this second heating. After a number of repetitions, which varies with the character of the germs, the infusion, however obstinate, is completely sterilized." [108]

If anything should have been a crucial experiment in the British spontaneous generation debate, it was this. Although there were still many other details to be explained with regard to the resistance of different organisms to acidity and the importance of oxygen, Tyndall had now understood the most important factor, namely heat resistant spores, and he had devised a brilliant experimental technique for sterilizing experimental infusions, based on this knowledge. As Chapter 7 shows, in the long term, the value of this technique was understood by the appropriate parts of the medical profession as descriptions of it gradually filtered into handbooks of hygiene and sanitation. However, in the short term many members of Tyndall's audience remained confused. Much of their confusion was due to his obtaining such contradictory results within a short period of time.

On May 17th, Tyndall reported the final part of his researches to the Royal Society. [109] In essence this paper was a summary of the work he had performed over the last two years. He discussed the protective action of cheese in experiments with turnip-cheese infusion. He reported the remarkable difference between his first set of experiments, which had all yielded negative results, and the later experiments where many samples were far more resistant to sterilization. The removal of the experiments to Kew and the infected atmosphere of the Royal Institution were discussed. Finally he described his method of discontinuous heating to achieve sterilization and he postulated that some infusions could be sterilized by a short boiling if deprived of air. Tyndall was also beginning to realise the importance of oxygen for the survival of some types of organisms.

The fact that Tyndall's later results differed so markedly from his earlier experiments did not pass without criticism. An article by the anonymous "Inquirer" in the April number of Contemporary Review was reviewed in the Lancet. [110] The article pointed out not only the difference between Tyndall's two sets of experiments, but also the fact that Tyndall's first set of experiments did not agree with Pasteur, who always maintained that neutral solutions could putrefy after boiling.

> "...for neutral solutions, which, on the authority of PASTEUR, should have putrefied, remained barren. We would suggest that before attacking opponents, leaders so closely allied should attempt to explain their own differences. The army of the germ-theorists is divided; and it is generally believed that a divided army is worse than useless - it is dangerous to its own cause." [111]

This was exactly the position Tyndall wished to avoid. He realised that opponents of the germ theory would pick on any differences between himself and the great Pasteur to whose authority he looked for support. But the point was that Tyndall's first set of experiments did not tally with the position Pasteur had maintained since 1862. Furthermore, it was indeed strange that Tyndall obtained not one single failure in these experiments when Bastian, Burdon Sanderson, Roberts, Pasteur and others had obtained organisms in certain solutions raised to boiling point. Perhaps it was stretching his scientific credibility that he should produce such a *volte face* in his experimental results within the space of a year. Tyndall may have been able to produce good explanations, based on the work of Roberts and Cohn, for his new results, but if he supposed that his discontinuous heating technique was to prove the decisive result to end the debate he was wrong on several counts.

There were a number of reasons why the debate was not abruptly decided over Tyndall's later set of experiments. Even if Bastian had accepted Tyndall's experimental results he would never have accepted his theoretical interpretation. As well as this, Bastian's dispute with Pasteur had still not been resolved. As the next section shows, Bastian had by now agreed to have his work judged by a Commission of the French Academy, but there was no knowing in which candidate's favour the Commission would decide. Then there was Tyndall's additional worry that insufficient care on the part of Pasteur, with regard to heat resistant spores, might yield Bastian an undeserved triumph.

Secondly there was the nagging problem of Burdon Sanderson. He was no supporter of spontaneous generation, but his work of 1873 had lent indirect support to Bastian, which the latter had been quick to seize on. Furthermore, Burdon Sanderson remained persistently lukewarm over the germ theory, or at least Tyndall's version of it. As Chapter 6 describes, the physiologist had spent many years researching the subject of contagious disease and maintained that microbial life had only been found to be a definite factor in a relatively small number of diseases; there were many diseases where such forms of life had not been found. Under these circumstances it is not surprising that he was unwilling to offer wholehearted support to a crude view of the germ theory which rested not only on the idea of invisible germs but also on much else that was unproven. Tyndall was never very concerned with how germs of disease actually *caused* the disease, but for Burdon Sanderson this was the very essence of his researches.

Finally there was the fact that the medical profession was still unconvinced. Tyndall had received a certain amount of support from the medical world over the years of the debate, but it is by no means clear how many new converts he won over to the germ theory, despite the fact that he occasionally declared that the germ theory was becoming more widely accepted. He had certainly done much to bring the germ theory to the attention of medical men but many of these, judging by comments in the medical press, did not believe that Tyndall's experiments, interesting though they might have been, had anything practical to say about the spread of disease. Tyndall's last series of experiments spread confusion rather than enlightenment.

In particular, this confusion is shown by the Lancet's response to a Friday evening discourse, where Tyndall had described his means of combatting the effects of the infected atmosphere of the Royal Institution by discontinuous boiling. [112]

Why, Tyndall asked, did hay infusions resist fifteen minutes' boiling in the laboratory of the Royal Institution while thirty feet away, on the roof of the institution, infusions could be sterilized by five minutes' boiling? He answered that on the roof the air was pure, while in the laboratory the air had become infected.

> "He argues that there is not a phenomenon in such cases which does not find its parallel in the spread of contagious diseases. We confess that we cannot answer his question in the present state of knowledge, and also that we cannot follow his argument. We do not think that, had Dr. Tyndall a practical acquaintance with the difficulties which beset physicians bent on tracing the spread of contagious diseases, and with the uncertainty which in real life surrounds the whole question of infection, he would hesitate to draw such parallels as the above." [113]

The Lancet reviewer did not accept that the dust which Tyndall caused to rise from his bundles of desiccated hay was the contagion which caused putrefaction, and that the dust contained bodies so minute as to be invisible under the microscope and that these bodies could withstand five hours' boiling. The reviewer failed to understand the distinction Tyndall made between the adult bacteria and the resistant germ and that in the discontinuous boiling experiments it was the adult form which was killed when brought to maturity and not the offspring as it was the latter bodies which proved so resistant to heat. Clearly the medical profession remained to be convinced. But although medical opinion was not very positive towards Tyndall's researches at this stage, there is no doubt that his work was one step on the road towards the new science of bacteriology. It was the more orthodox researches of individuals such as Cohn, Koch and Burdon Sanderson which were to prove more convincing to the medical profession in its gradual acceptance of the germ theory in the 1880s.

Pasteur's Challenge

Meanwhile, early in 1877, Pasteur communicated his response to Bastian's findings to the French Academy. [114] The British scientific and medical communities were able to follow the progress of the debate, as the Comptes Rendus papers were translated into English in the pages of Nature. [115]

Pasteur informed the Academy that he and his colleague, Joubert, had made new experiments where they were careful to ensure that the urine was exactly neutralised; as long as the potash (solid or in solution) was raised to a temperature of 110°C. beforehand the solutions remained sterile.

But Pasteur wished to avoid dragging in profound theoretical questions and to avoid setting one opposing doctrine against another. It was purely a question of fact - whether or not boiled urine or urine straight from the bladder would ferment when neutralised by potash raised to 110°C. Pasteur wrote to Bastian to beg him to see that by continuing with his researches he was damaging the progress of scientific enquiry. [116] Pasteur assured him that he thought him an able experimenter and suggested that he come to the French Academy to discuss their differences. Yet Bastian persisted and on January 20th 1877, he wrote to Pasteur advising him that he could readily reproduce the results he had announced to the Royal Society and that Pasteur's experiments must have introduced some difference in experimental technique. [117]

Almost every letter from Tyndall to Pasteur contained a tirade against Bastian's methods and motives. Pasteur's letters to Tyndall reveal a more fair and at least at first, an almost kindly attitude to Bastian in his desire to point out to the young scientist the error of his ways, but now Pasteur's patience began to run out. Perhaps the French savant felt that Bastian should bow to his greater experience and authority especially when in a personal letter he had asked him to give up his hopeless struggle. He suggested to Tyndall that if Bastian persisted he would have to ask the Royal Society to appoint a commission to decide the question. [118]

Tyndall's patience with Pasteur was also running out. He had read the correspondence between Pasteur and Bastian which had been forwarded to him by the former. Not only was he annoyed that Pasteur had praised Bastian's ability but he felt that Pasteur had actually helped to enhance Bastian's credibility by taking his work seriously.

> "You seem now inclined to take my view of Dr. Bastian. I entertain the fear that you have damaged more than you imagine that scientific truth which both of us have at heart by stamping Dr. Bastian's work with your approbation. I should not think it likely that the Royal Society would appoint a commission to decide between you and Bastian. And here let me in all frankness say that you are likely to err in attaching to the labours of Dr. Bastian too much importance.
>
> He is steadily losing credit in this country, and a year or two hence his authority will be zero. You, I regret to say have helped unwittingly to lengthen the period of that authority by the praise which you have bestowed on his experimental work." [119]

Bastian repeated his experiments with liquor potassae heated to 110°C. for sixty minutes and then with liquor potassae heated to 110°C. for twenty hours but the urine, when neutralised, still swarmed with bacteria within twenty four to forty eight hours. [120]

Tyndall realised that the French Commission could easily result in triumph for Bastian and failure for Pasteur if the latter did not take account of heat resistant spores and with this in mind he issued a warning.

> "... my anxiety is that no loop-hole shall be allowed him through which he can escape. I would therefore most earnestly caution you against relying too much upon the alleged fact that in acid solutions germs are killed by boiling in a few minutes. There are germs of a special kind and in a special condition, that will withstand the boiling temperature for a large multiple of the time that you have found sufficient to destroy them." [121]

Tyndall intended to infect urine with old hay germs and he confidently expected that the urine thus infected would not be sterilized even after boiling for a considerable time.

> "If this should prove to be the case, your attention must not be directed to your potash alone. It might yield Bastian a triumph were you to assume that the acid urine is invariably sterilized by boiling. Of course if you operate with urine direct from the bladder you avoid this danger." [122]

The position between Pasteur and Bastian was a stalemate; neither could repeat the other's results and each maintained the correctness of his own experiments. Pasteur issued Bastian a challenge - he was to obtain his results with pure liquor potassae on the sole condition that it was first heated to 110°C. for twenty minutes or to 130°C. for five minutes. [123] Bastian accepted the challenge, Pasteur called on the Academy of Sciences to appoint a commission to judge between their experiments and on February 19th, 1877, a commission was appointed consisting of Milne Edwards, Boussingault and Dumas. [124] It would, however, be some months before the Commission met to decide between Pasteur's and Bastian's experiments.

<u>The Pasteur-Bastian Commission</u>

In the middle of July, 1877, the Commission of the Academie des Sciences met in Paris to decide between the experiments of Pasteur and Bastian. Bastian described the incredible sequence of events surrounding the Commission in the pages of <u>Nature</u> a fortnight later. [125] This revealed to the British scientific community the farcical nature of the Commission, the fact that nothing had been decided, and that Bastian had been treated rather badly by the French scientists as he had travelled to Paris believing that the conditions under which the Commission was to operate had been agreed, only to find when he arrived that the members of the Commission refused to accept these terms.

Bastian was, of course, extremely anxious that the terms under which the Commission was to operate should be agreed in detail in advance. Part of the reason for this anxiety was that he was able to spend only a few days in Paris and therefore could not accommodate modifications to his experiments, but he may also have been concerned that two members of the Commssion, namely Dumas and Milne Edwards, had served on the second Commission appointed to judge between the experiments of Pouchet and Pasteur in 1864 and had found in favour of Pasteur. [126] Milne Edwards was known to be an opponent of spontaneous generation and Dumas was an old friend and ally of Pasteur. [127] Pasteur also desired the Commission to enquire only into the question of fact at issue between Bastian and himself. He had already written to Tyndall to say that he would oppose any suggestion that the Commission should break into new and uncertain territory. [128]

On February 27th, 1877, Bastian wrote to Dumas to ask him the precise terms under which the Commission was to operate. [129] The reply which Dumas sent appears to have been lost in the post as Bastian wrote again to Dumas on May 8th, asking for a duplicate of the original reply and informing Dumas that he would be unable to go to Paris until the third week of July. [130]

However Bastian was not satisfied by the duplicate letter, as to the exact way in which the Commission would conduct its enquiry. He wanted the enquiry to be limited to the one fact under discussion between Pasteur and himself, namely:

> "*Whether previously boiled urine, protected from contamination, can or cannot be made to ferment and swarm with certain organisms by the addition of some quantity of liquor potassae which has been heated to 110°C., for twenty minutes at least.*" [131]

Bastian expressed himself happy to submit to the Commission's decision if it limited itself to this question of fact. However were the Commission to express an opinion as to the "Germ Theory of

Fermentation" or "Spontaneous Generation" then Bastian would decline to take part in the wider enquiry. Furthermore as he could visit Paris for only three or four days, he could not take part in any new experiments that the Commission might demand as this could prolong the debate indefinitely. [132] Dumas' letter of 12th July,1877, agreed that the Commission would examine only the point under discussion between Pasteur and himself. Satisfied with this response, Bastian set off for Paris.

On 15th July he met the Commission, by arrangement, in Pasteur's laboratory in the Ecole Normale Superieure. The French Commission was represented by Dumas and Milne Edwards, Boussingault having withdrawn because of a domestic problem. Milne Edwards immediately announced his objection to Bastian's sole condition, that the enquiry should be restricted to the one question of fact and he refused to take part in a commission which did not have full power to vary the experiments at its discretion. Bastian naturally felt aggrieved because not only was this contrary to the challenge that Pasteur had originally made and he had accepted but it was also not the agreement he had made with Dumas by letter. Bastian offered a compromise to Dumas.

> "The proposition was that on the present occasion we should have "the first element" of the inquiry as defined by M. Dumas in his letter of April 25; viz., that the opportunity should be given to M. Pasteur and myself of repeating (without variation) the actual experiments upon which we based our respective opinions; that I should then return to London, and after the Commission had expressed its opinion to M. Pasteur and to myself as to any variations in the experimental conditions which they might desire to institute, that I should return to Paris to witness and to perform such modified experiments." [133]

Meanwhile van Tieghem had been appointed to the Commission in place of Boussingault. This was an additional worry to Bastian.

> "This gentleman being a former pupil and present colleague of M. Pasteur, the Commission was left without a single member who could be considered as representing my views, or even as holding a neutral position between me and my scientific opponent." [134]

On July 18th, Pasteur and Bastian arrived at the laboratory at 8 a.m. having received a summons from van Teighem the previous day. Van Teighem was already there and soon after Milne Edwards arrived. The latter had had no communication with Dumas in the last few days and when told of Bastian's proposition as to the compromise conditions, voiced his disapproval and left the laboratory followed by van Tieghem. Van Teighem returned after an hour to say that after waiting in vain for M. Dumas, Milne Edwards had gone.

While Bastian conversed with van Teighem in an upper room, Dumas arrived and upon hearing of Milne Edwards' departure announced to Pasteur that the Commission was at an end without communicating with either van Teighem or Bastian.

> "Thus began and ended the proceedings of this remarkable Commission of the French Academy." [135]

Pasteur wrote to Tyndall at the beginning of August to tell him of the Commission. [136] It may have been the case that Pasteur was unaware of Dumas' original agreement with Bastian that the Commission would limit itself to a question of fact and would not try to interpret the results or ask for new experiments. On the other hand, he may now have felt such a request to have been unreasonable anyway despite the fact he had originally issued his challenge to Bastian on the one question of fact regarding their experiments. He seemed to have quietly forgotten that he issued his challenge purely on one experimental fact and that he too had stated his wish that the Commission should not explore new territory. Whatever the reason, he informed Tyndall that Bastian had left without permitting the Commission to fulfil its mission.

When Bastian arrived in the laboratory, Pasteur explained, he suddenly expressed the most strange wish which would have deprived the Commission of all liberty in making a judgement. Bastian wanted the Commission to judge the one question of fact alone, the question of fact on which Bastian had accepted the original challenge. But to Pasteur it all seemed very simple. The first function of the Commission was to see both scientists perform their experiments; but the second function of the Commission was to explain how two competent observers could obtain contradictory results and in doing this they could point out sources of error and ask for experiments to be repeated. It was to this second function, the question of interpretation and possible new experiments that Bastian objected. He insisted that the Commission was to be bound by its first function alone.

Pasteur told Tyndall that Dumas and Milne Edwards could not agree to this. Afterwards Bastian and Pasteur talked for some time and Pasteur suggested that germs clinging to the surface of Bastian's vessels could be a possible source of error. After this Bastian and Pasteur parted on the best of terms.

Tyndall's reply to Pasteur in the middle of August was the last in their correspondence on the controversy with Bastian. He praised Pasteur's work on "charbon" (anthrax) and emphasised its importance in convincing medical men of the existence of living contagia. Somewhat unkindly, Tyndall described Bastian's account of the Commission, published in Nature, as "very amusing." "I am extremely glad that the commission ended without giving him an opportunity of turning its proceedings to an improper account." [137]

The controversy between Pasteur and Bastian was now at an end. There was little point in reiterating the old ground in the scientific journals and with the end of the Commission there was no way to compare Pasteur's and Bastian's results directly and a complete explanation for the disparity in their results was not immediately forthcoming. Pasteur did arrive at a partial explanation in that he discovered that the organisms causing Bastian's results would not develop in acid urine, but there was clearly more to the question than this one fact. Bastian may have felt ill-used by the French scientists as the conditions under which the Commission was to be held were not those to which he agreed by letter. Dumas seems to have failed to commmunicate this agreement to his colleagues. But on the other hand, although the members of the Commission held views which were not likely to be sympathetic to Bastian from the start, it is hardly surprising that they refused to be bound by a decision on one fact alone when the issues involved were so important.

The letters exchanged between Tyndall and Pasteur reveal that the former was largely responsible for drawing the French savant into the debate in the first place. Tyndall knew the stamp of scientific authority that Pasteur could give to the debate on spontaneous generation in his attempts to vanquish Bastian. It seems unlikely that Pasteur would have become involved if not for Tyndall, simply because of the extent which Tyndall furnished him with the details of what was happening in Britain including all the relevant articles from the British scientific and medical press. Similarly Tyndall egged Pasteur on; almost every letter reiterated the profound effect that Bastian was having on the medical world and how he fought for victory rather than truth. At first Pasteur seemed kindly disposed to Bastian, believing he was making an error in good faith. But later he became irritated by Bastian's persistence and issued the challenge which resulted in the Commission. Surely Pasteur must have been embarrassed at the outcome of this affair.

Many traditional accounts of spontaneous generation would lead us to believe that Pasteur's involvement was limited to his controversy with Pouchet and his victory in the 1860s. It is easy to see how events become written out of the history of science. In Pasteur's biography, written by his son-in-law in the 1880s and therefore during the former's life-time, there is no mention of the controversy with Bastian and the abortive Commission. [138] The chapter on spontaneous generation relates only to the controversy with Pouchet. The only mention of Bastian is a reference to a meeting between him and Pasteur at the International Medical Congress in London in 1881. [139] Tyndall too must have been aware of this ommission as he wrote the introduction to the English version which was translated by his mother-in-law, Lady Claud Hamilton. [140] It is easy to see why Pasteur should wish to leave this episode out of his biography. For the French, spontaneous generation was no longer a live issue by the end of the 1860s; how convenient it was to leave out the manner in which this question reappeared over a decade later and with such inconclusive results.

However several biographies of Pasteur, written after his death, discuss his debate with Bastian. [141] A colleague, Emile Duclaux, took over as the Director of the Pasteur Institute after Pasteur's death. Duclaux's biography of Pasteur describes how the latter and his co-workers gradually came to understand what was taking place in the debate over neutralised urine; the importance of Bastian's work was also acknowledged. [142]

> "All our present technique has arisen from the objections made by Bastian to the work of Pasteur on spontaneous generations. It was Bastian who made us see that this work which had been so vaunted, abounded in false interpretations..." [143]

Duclaux pointed out that it was Bastian's work which made Pasteur realise that acid urine could contain living germs which although still alive would not develop until the urine was neutralised. It was also Bastian who made the French scientists realise that both neutralised urine *and* the presence of air was required for the development of the germs. In hay infusions, the organism *Bacillus subtilis* will not develop without the presence of air. Like Tyndall, Pasteur had believed that air caused the introduction of germs, when in fact it was necessary for the development of aerobic organisms. In his urine experiments, Bastian had also realised that air was an important factor in the development of some organisms, although he regarded it as a chemical rather than a biological factor. Dramatically Duclaux declared:

> "Bastian rendered a service to science; he lashed it on its weak side, but he compelled it to advance." [144]

The Final Exchange

In The Nineteenth Century for January, 1878, Tyndall published an article entitled "Spontaneous Generation". [145] He reminded his readers, in considerable detail, of the history of the subject including the debt humanity owed to Pasteur who first formulated the modern germ theory. Lister's antiseptic system and the extension of our knowledge of lower organisms by Cohn was not only valuable to the process of antiseptic surgery but also in understanding contagious disease where the idea of contagia "breeding true" i.e. a specific organism giving rise to a specific disease, Tyndall confidently asserted was daily gaining converts.

After summarising his experimental enquiries Tyndall pointed to the changes in surgery occasioned by the germ theory; these changes were cultural changes. "Surgery was once a noble art; it is now, as well, a noble science." [146] Similarly the germ theory had been brought to good effect on infectious disease, as Koch and Pasteur had shown that splenic fever or anthrax was due to the organism, *Bacillus anthracis*. [147]

In the next issue Bastian replied. [148] He too detailed how he had become involved in the subject and how he had failed to be convinced by Pasteur's arguments in regard to the germ theory. He pointed out that his experiments had from the first met with opposition and denial although on several occasions observers who repudiated his results came to acknowledge their correctness. He still insisted, and correctly so, that Tyndall had failed to prove that the air contained germs of bacteria. Most importantly, Bastian would not accept Tyndall's assertions as to the death-point of bacteria germs.

> "On the all-important subject of the death-point of living matter, therefore, and on the degree to which a power of resisting prolonged and high temperatures is conferred upon bacteria or their germs by virtue of their previous desiccation, I am quite unable to accept Professor Tyndall's assumptions." [149]

In the March issue of The Nineteenth Century Tyndall produced his last word on the subject. [150] As neither Tyndall nor Bastian published anything more on the debate the controversy was effectively over.

Conclusions

Bastian's results with regard to turnip-cheese infusion have already been explained with reference to Cohn's discovery of the resistant cheese bacillus. Bastian's results with neutralised urine were largely explained by Duclaux.

Tyndall attributed the considerable difference between his two sets of experiments to the introduction of highly resistant old hay germs into the atmosphere of the Royal Institution during the second series of experiments. Although this explains much of what he observed, other factors are involved. It seems likely that Tyndall accidentally avoided using substances where spores would develop into adult organisms in his first series of experiments.

It was already known that urine, alkalised hay infusion and turnip-cheese infusion were amongst the most difficult fluids to sterilize. Naturally acid hay infusion and urine are seemingly far easier to sterilize - this was the result observed by Pasteur that spores would remain dormant in acid infusions. Tyndall used the acid versions of these two substances in his first experiments while he used alkalized hay infusion in his second set of experiments. The fact that most of his experiments were conducted with his wooden chamber where there was a free flow of air may also have helped the aerobic *Bacillus subtilis* to develop in the second series. Furthermore Tyndall used turnip as opposed to turnip-cheese infusion in his first experiments and this may have meant that he avoided the resistant cheese bacillus.

There are several ways in which a scientific controversy can end. One side may produce an argument or crucial experiment which convinces the other side or an idea may just die out with its originator as new generations find the idea old-fashioned and outdated. More commonly the opponents never give up their beliefs and the gradual shift in opinion within the scientific community may mean that the "losing" side gradually drops out of favour and becomes marginalised. It may become increasingly difficult to have publications accepted and, in more modern times, to gain research students and research funds. It was the latter fate which threatened Bastian now. Clearly he and Tyndall would never agree. Even though the medical profession was ambivalent towards the germ theory, or at least Tyndall's version, Bastian was becoming increasingly isolated in his views. As new researches in bacteriology were undertaken linking more infectious diseases with microscopic organisms, spontaneous generation began to be less interesting to the medical world.

Apart fom the fact that the combatants simply stopped debating the subject, the most salient reason for Bastian leaving spontaneous generation was that the pursuit of the subject was probably threatening to damage his career. Bastian was much younger than Tyndall and Pasteur and had, as yet, few scientific laurels on which he might rest. It is interesting to note that 1878 was the year in which Bastian received his appointment as Physician to University College Hospital. Whether he felt he had to drop spontaneous generation to gain the appointment or whether he was warned off the subject can only be speculation. However he was involved in two discussions on antisepsis and germs in the early 1880s. [151] In the second of these, at the International Medical Congress in 1881, Bastian reiterated his old views and was attacked by Pasteur and Roberts. [152] It was clear from this discussion that he had made some further experiments on spontaneous generation although he did not publish these.

Bastian now turned to more orthodox researches in neurology, where he had already undertaken research, and spent the next twenty five years working in this field. [153] But he never gave up his belief in spontaneous generation and returned to these researches upon his retiral from University College, London at the beginning of the twentieth century. A discussion of the links betwen Bastian's neurological work and his work on spontaneous generation is beyond the scope of this thesis. However, it may already have been too late for Bastian to save his reputation. Despite much original work, particularly on aphasia, he is almost always remembered only for his work on spontaneous generation.

The 1870s had proved to be a crucial decade in the history of bacteriology. At the beginning of the decade Pasteur's earlier work had received serious criticisms and scientific and medical opinion was fairly well disposed to the possibility of spontaneous generation. Over the next seven or eight years the detailed and complex progress of the debate shows that the focus moved away from the physical conditions of the experiments towards an appreciation of the importance of studying the organisms themselves and their life-cycles. In this way many of the results in the debate could be explained.

The value of this new approach had already been vindicated by Koch's work on anthrax and similar work would quickly follow in the 1880s. Even if Tyndall's crude version of the germ theory was not acceptable to the medical world, his and Bastian's involvement in the spontaneous generation debate, coupled with the parallel developments in Lister's antiseptic techniques, paved the way for the introduction of the new science of bacteriology in the 1880s.

CHAPTER 5

SPONTANEOUS GENERATION - THE ISSUES

Introduction

Having described the earlier history of the debate on spontaneous generation, the background and the progress of the British debate in the first part of the 1870s, this chapter turns to the important scientific issues surrounding the subject of spontaneous generation.

In particular, it is seen to be important to understand the implications of these issues both for scientific naturalism and for contemporary medicine in relation to the germ theory. These philosophical issues are dealt with under the headings of evolution and the nebular hypothesis, protoplasm and vital force and finally the material causes of disease.

Evolution, the Nebular Hypothesis and the Origin of Life

It is quite hard to talk about scientific naturalism without mentioning evolution; similarly the subject of spontaneous generation demands, in turn, a discussion of evolution; this is especially true with regard to the way the fates of spontaneous generation and evolution were linked in France. Although evolution has already been considered in relation to the philosophy of scientific naturalism, the issue at this stage is to clarify the nature of the relationship between spontaneous generation and evolution with regard to the origin of life and to expose the contradictions and problematic nature of this relationship for the scientific naturalists in the context of the British debate. Alternative views of the origin of life articulated in response to beliefs in spontaneous generation and furnished by the nebular hypothesis are also explored.

In Chapters 2 and 3 it was suggested that both Darwin and the scientific naturalists had been aware, for years, that the doctrine of evolution apparently required an origin of life on Earth due to spontaneous generation from inorganic materials. It was, of course, one thing to admit the possibility of the doctrine, in the abstract at the commencement of life on earth, but quite a different matter to aggree that Bastian's experiments constituted instances of that process in the present day. At any rate Huxley had always felt that Darwin should have faced the problem of spontaneous generation more squarely. In 1863 he had written to Charles Kingsley:

> "Against the doctrine of spontaneous generation in the abstract I have nothing to say. Indeed it is a necessary corollary from Darwin's views if legitimately carried out, and I think Owen smites him (Darwin) fairly for taking refuge in "Pentateuchal" phraseology when he ought to have done one of two things- (a) give up the problem, (b) admit the necessity of spontaneous generation." [1]

One of the scientific principles forming the bedrock of scientific naturalism was the principle of continuity or uniformity which held that the forces and scientific laws in operation in the world today were the same as those which had operated throughout the Earth's history. This was clearly an important tenet for scientific naturalism as the acceptance of such a principle greatly helped to pave the way for the adoption of naturalistic explanations of events taking place in the world. The fundamental appeal of the principle lay in the fact that scientific explanation could be divorced from any suggestion of divine fiat.

To accept the necessity of a spontaneously generated origin of life on Earth yet to deny the possibility of spontaneous generation in the present day implied a violation of this principle. Huxley's response was to affirm that spontaneous generation must have taken place when life began but to deny that he had ever seen an example of the process in the present. Tyndall took the problem of life one step further back into the mists of time by means of the nebular hypothesis.

As well as the potential contradiction there was the further problem of forming too strong a link between the fates of spontaneous generation and evolution. As Chapter 3 outlined, where the two had been linked in France, the apparent overthrow of the doctrine of spontaneous generation had been heralded as a refutation of evolution. Quite clearly the last thing that the scientific naturalists wanted was to see their efforts to discredit Bastian's experiments as instances of spontaneous generation damaging the reputation of the theory of evolution. They felt in any case that the generally scientific character of the theory was the factor that made it acceptable. As Tyndall put it:

> "The strength of Evolution consists, not in an experimental demonstration (for the subject is hardly accessible to this mode of proof), but in its general harmony with scientific thought." [2]

Huxley could admit the possibility that spontaneous generation could conceivably have taken place at some time in the past (Tyndall never publicly accepted this); and like Huxley, Tyndall denied that Bastian's experiments were satisfactory experimental proof of spontaneous generation. [3]

However there was more than one way out of the potential impasse implied by the refutation of current day spontaneous generation experiments whilst accepting the possibility of the abiogenetic origin of life. One way was simply to ignore the problem - this route was chosen by practically minded scientists and medical men whose day to day work hardly involved consideration of such fundamentals. For medically inclined scientists especially, the practicalities of dealing with infectious diseases and epidemics were more pressing problems than abstract philosophy. However, a second alternative was to posit an origin to life which came from some extra-terrestrial source. Two important variations on this alternative were chosen by the physicists, Sir William Thomson, and John Tyndall respectively. Both their explanations were based on the nebular hypothesis.

The nebular hypothesis, originally articulated by Kant and Laplace, explained the origin of the solar system in terms of gradual evolution from a gaseous nebula. The hypothesis was well accepted in the 1870s; it "reigned almost unchallenged as the cosmogeny of Victorian science." [4] Recent research has demonstrated the importance of the nebular hypothesis in the nineteenth century. In particular it has been suggested that through the nebular hypothesis the idea of evolution was imported from astronomy to biology. [5]

Tyndall held the further belief that the nebula itself possessed a potency for life which evolved (but was not spontaneously generated) when conditions on the Earth became appropriate. This belief was a form of pantheism. It was, of course, possible, as is the case with Spencer and Thomson, to hold a belief in the nebular hypothesis without adhering to pantheistic beliefs about the origin of life.

Tyndall described the nebular hypothesis as follows:

> "From the examination of the solar system, Kant and Laplace came to the conclusion that its various bodies once formed part of the same undislocated mass; that matter in a nebulous form preceded matter in a dense form; that as the ages rolled away, heat was wasted, condensation followed, planets were detached, and that finally the chief portion of the fiery cloud reached, by self-compression, the magnitude and density of our sun." [6]

For Tyndall there were two possible views as to how life arose on earth:-

> "Life was present potentially in matter when in the nebulous form, and was unfolded from it by way of natural development, or it is a principle inserted into matter at a later date." [7]

Clearly there were strong reasons for believing that no life existed on the Earth in its nebulous or molten state but did this mean that somehow creative energy paused until the Earth was in a suitable state or was the alternative of what he termed "Natural Evolution" preferable, namely that all forms of life from the humblest to the highest "were once latent in a fiery cloud"? [8]

The beauty of Tyndall's hypothesis was not only that it did not violate the principle of continuity but that it also left untouched the ultimate mystery of the universe, pushing it back beyond the realms which science had to explain.

> "For granting the nebula and its potential life, the question, whence came they? would still remain to baffle and bewilder us. At bottom, the hypothesis does nothing more than 'transport the conception of life's origin to an indefinitely distant past.'" [9]

Tyndall's level of enthusiasm for the nebular theory was perhaps somewhat unusual amongst his contemporaries but, like so many of the strands of his philosophy which seem eccentric or dogmatic on first consideration, not only was there an underlying coherency to his ideas in that they were quite consistent with his mechanistic and naturalistic stance but also his beliefs shaded off into other beliefs held by his contemporaries. The idea of matter holding the potency for life is on one hand, closely akin to Bastian's materialism or even Spencer's conception of evolution where all forms of matter have a tendency to differentiate and organise; on the other hand the potential for life in all matter is not far removed from the idea of the universe itself as a living organism, such as Burdach believed, and which is in turn a form of vitalism. [10] On the other hand again, Tyndall's views were in accord with Huxley's conception of protoplasm as the physical basis of life, where life was seen as the *product* rather than the *cause* of organisation.

Although he presented his ideas on the nebular hypothesis quite explicitly in his address, "On the Scientific Use of the Imagination", they cannot be said to have held the same level of appeal for most of his scientific contemporaries as an alternative explanation for the origin of life. Faced with spontaneous generation on one side and Tyndall's view of the nebular hypothesis on the other most scientists remained agnostic.

For Tyndall, advocacy of the nebular hypothesis brought together several fundamental principles harmoniously. First of all it was no longer necessary to postulate that life had originated by a process of spontaneous generation and hence attempts to discredit spontaneous generation did not then automatically pose a threat to the acceptance of the doctrine of evolution. The principle of continuity was saved on two counts. Firstly, it was unnecessary to suggest that spontaneous generation had taken place in the past where there was apparently no experimental proof of its taking place in the present; secondly, the very considerable problems in imagining the transition from non-living to living matter were avoided. A further important element in Tyndall's acceptance of the nebular hypothesis was the fact that it made unnecessary the introduction of a divine creative act to explain the introduction of life on Earth yet retained potentially religious ideas of the ultimate mystery of life at a suitable distance. This fitted in harmoniously to naturalism's central tenet that it was possible to describe the world in a naturalistic way without resorting to capricious forces.

> "How were they (living things) introduced? Was life implicated in the nebula - as part, it may be, of a vaster and wholly Unfathomable Life; or is it the work of a Being standing outside the nebula, who fashioned and vitalised it, but whose origin and ways are equally past finding out? ... The assumption of such a power to account for special phenomena, though often made, has always proved a failure. It is opposed to the very spirit of science..." [11]

Tyndall's form of nebular hypothesis necessitated the acceptance of a form of pantheism - although in his case it was promise of life rather than God which was to be found everywhere. Interestingly the famous statement on the potency of matter in his Belfast Address which aroused such indignation at the time was really a statement of pantheism rather than materialism.

Yet Tyndall was by no means alone amongst his contemporaries in resorting to an "astronomical" solution to the problem of explaining the origin of life on earth. While he extended the nebular hypothesis in metaphysical ways with allusions to pantheism as part of his campaign for the acceptance of a consistent and natural evolution of life, Sir William Thomson had, in the 1860s, employed an argument based on the nebular hypothesis, in a devastating attack against evolution and uniformitarianism. Thomson had used the nebular hypothesis which was based on the bedrock of Newtonian physics and gravitational astronomy, coupled with the recently discovered principles of thermodynamics, to argue that the sun and the earth were much younger than the timescale required by natural selection and strict uniformitarianism. Huxley took issue over Thomson's arguments in 1869 but was unable to refute them. [12] As a physicist Tyndall was in a better position to combat a mathematical argument, yet he remained silent.

One of the most interesting features of this episode is the sheer weight which Thomson's arguments carried and that almost single-handedly one physicist could cause such an upheaval in the sciences of geology and biology. Burchfield explains the influence of Thomson's arguments in terms of (a) his personal prestige (b) the authority of physics and (c) the accordance of Thomson's ideas with Victorian cosmology in terms of the coherence of the nebular hypothesis, natural causes and arguments from design. [13] Although Thomson's arguments were full of assumptions, the authority of the physical and mathematical sciences was almost unchallengeable.

In 1870, Tyndall discussed the possibility that life was inherent in the nebula. Although he was silent in the "Age of the Earth" debate, he was in a position to witness Thomson's success in pressing the popular nebular hypothesis into service against evolution in the late 1860s. By his advocacy of the nebular hypothesis in the early 1870s, Tyndall was not only avoiding origins due to spontaneous generation but was also trying to turn the nebular hypothesis *towards* the service of evolution and *away* from Thomson's employment of the theory against evolution. The difference was of course, that Tyndall's arguments were metaphysical while Thomson's were mathematical and therefore carried more weight and authority with the scientific community. Thomson's arguments were also easier to understand than Tyndall's. The metaphysical nature of Tyndall's conception of panthesism and the nebular hypothesis made it difficult for his audience to see what he was driving at.

In his presidential address to the BAAS in 1871, to avoid essentially the same problem of invoking spontaneous generation to which he too was opposed, and as an explanation for the origin of life on earth, Thomson put forward the hypothesis that life on earth could have originated from seeds on a meteoric fragment. [14] Although he was opposed to natural selection as he believed that it did not take into account the evidence of design and benevolent intelligence acting in the world, he was anxious to avoid unnecessary invocations of a Creative Power if a probable natural solution could be found. It was not impossible, he argued, that a planet like the Earth, if it came into collision with another large body, could break into fragments which could carry seeds and living plants and animals. One such fragment falling on the earth before it was inhabitated by life could lead to it becoming covered in vegetation.

> "The hypothesis that [some] life [has actually] originated on this Earth through moss-grown fragments from the ruins of another world may seem wild and visionary; all I maintain is that it is not unscientific, [and cannot rightly be said to be improbable]." [15]

Although Thomson's arguments against natural selection and his alternative explanations of the origin of life made little direct impact on the spontaneous generation debate, features of his arguments are important in a contextual sense when it comes to understanding the authority of scientific naturalism in scientific circles and the authority of scientific arguments, in particular arguments based on the physical sciences, in relation to contemporary medicine.

It is in the authority of physics and how such arguments impinged on the concepts of scientific naturalism that Thomson's arguments are to be understood. In Chapter 2 the opposition of the "Cambridge physicists" to scientific naturalism was discussed. In essence Thomson's attack on natural selection and uniformitarianism can be seen as one strand in the Cambridge physicists challenge to scientific naturalism. On top of the social authority which the Cambridge physicists enjoyed, they had the advantage of a subject which held the highest status and authority amongst the sciences. The scientific naturalists were at once a part yet not a part of this. As scientists they were part of the social structure which accorded the physical sciences such status and authority, indeed their own cosmology rested very heavily on the use of physical arguments. This can be seen in the way they proclaimed the identity of vital and physical forces. This can explain why they were able to mount only a poor defence against Thomson's arguments; his arguments were all too convincing. But on the other hand they wished to preserve the inherent rightness and naturalness of evolution and natural selection despite the fact that such theories were not amenable to either experimental or mathematical proof.

It was clear that much of Thomson's invective was directed against the tendency of the geological and biological sciences to ignore the principles of physics.

> "...the very root of the evil to which I object is that so many geologists are contented to regard the general principles of natural philosophy, and their application to terrestrial physics, as matters quite foreign to their ordinary pursuits." [16]

Thomson saw the battle against spontaneous generation as an attempt by right minded biologists to progress biological science away from the primitive stage and to emulate the physical sciences.

> "The earnest naturalists of the present day are, however, not appalled or paralysed by them (difficulties of living up to the ideals of science), and are struggling boldly and laboriously to pass out of the mere "Natural History stage" of their study, and bring zoology within the range of Natural Philosophy." [17]

If the scientific naturalists were in any way ambivalent in their attitude towards Thomson's arguments because of their own support for the explanatory power of the physical sciences there were others who displayed no such ambivalence. In particular, it is possible to discern an understandable resentment from the ranks of biological and medical scientists towards the physical sciences, particularly the arrogance of physicists, which is demonstrated by the two quotes from Thomson above. These criticisms run through the whole of the origin of life/ spontaneous generation/ germ theory debate. The spokesman for medical and microscopical science was Lionel Beale, Professor of Pathological Anatomy at King's College, London, vitalist, expert microscopist and ardent opponent of the mechanistic theories of life. For him, there was no essential difference between the positions of Tyndall and Thomson, at least in terms of their methods, for both were guilty of crossing the boundary from the physical sciences and either, in Tyndall's case, making pronouncements about medical matters of which he had no practical experience or, in Thomson's case, making fanciful speculations about the origin of life. As is shown below it was against protoplasmic theory and the germ theory and hence against the school of Tyndall and Huxley that most of Beale's invective was directed, but he was also critical of Thomson's views on the origin of life. In 1875 he wrote,

> "One great authority, dissatisfied with every suggestion, and being evidently convinced that no physical explanation of the origin of life upon our globe would ever be discovered, despairingly submits to us the proposition that life did not begin here at all, and that our earth was first peopled by the offspring of germs brought to us upon a fragment broken off from some distant orb that teemed with life. Whether even the simplest forms would have survived after such a ride through space unfortunately had not been determined by experiment, so the idea of our fauna and flora being derived from those of another world found little favour..." [18]

Like Tyndall and also Spencer, Huxley was anxious that the theory of evolution should not stand or fall on the question of ultimate origins. He wished to separate the origin of life from the origin of species. The best method was to limit the study of evolution to life forms which had existed or were already existing and not to speculate as to ultimate origins as all the available choices - a form of spontaneous generation (or what Huxley termed "abiogenesis"), extra-terrestrial explanations or the act of a Creator were all unappealing. He expressed the opinion that Darwin was perfectly right in choosing to limit his enquiry as he pleased, especially as Darwin had regretted pandering to public opinion in the original edition of his work by allowing for the possibility of a creative act to explain the origin of life. Although Huxley felt that Darwin could have dealt with this problem more effectively, he was irritated by the fact that The Origin of Species was so often attacked on Darwin's failure to deal with the origin of living beings. He felt that scientists should be at liberty to decide on their own boundaries.

> "This, you will observe, is a perfectly legitimate proposition; every person has a right to define the limits of the inquiry which he sets before himself; and yet it is a most singular thing that in all the multifarious, and, not infrequently ignorant attacks which have been made upon the "Origin of Species," there is nothing that has been more speciously criticised than this particular limitation." [19]

Spencer was another active supporter of the nebular theory although he did not draw out the extensive implications for the origin of life from the theory as did Tyndall. For Spencer acceptance of the theory was more of an astronomical necessity than a philosophical conjecture - it was part of his wide ranging view of evolution which included both the inorganic and organic worlds. Tyndall certainly viewed his ideas on the nebula as part of the theory of evolution, but Spencer took these ideas further. In the first volume of his system of Philosophy, First Principles, he laid out his conception of the theory of evolution as the fundamental cosmic law which necessitated a discussion of what he saw as astronomical issues, drawing on authorities such as Laplace, John Herschel and Airy, in terms of the origin of the solar system. [20] There is no mention of the relationship of the nebular hypothesis to pantheism and the origin of life; the addition of the latter ingredients were due to Tyndall.

Spencer described the universal application of evolution:

> "Evolution, then, under its primary aspect, is a change from a less coherent form to a more coherent form, consequent on the dissipation of motion and integration of matter." [21]

He saw evolution in terms of energy and the motion of matter. Evolution was a universal process which applied to every material system from the solar system and planets to the growth of organisms and organs to society as well.

Bastian's treatment of evolution derives much of its force from an analysis which is close to Spencer's. One reason why Bastian was readily allowed a platform to publish his work in scientific and medical journals, was that the roots of his beliefs were quite consistent with many of the prevalent theoretical stances of the day, such as Bennett's work on molecular physiology, as outlined in Chapter 3, Spencer's evolution and Huxley's conception of the protoplasmic theory.

Of all the scientific naturalists, Spencer's conception of evolution was probably the most compatible with a materialistic viewpoint, because it offered a complete system covering living and non-living matter, though he himself fought shy of any overt materialism, by subsuming religious or spiritual ideals in terms of the "Unknowable". Understanding evolution in terms of dissipation of motion and integration of matter could lend itself to the idea that the evolution of microscopic organisms through such a process was easily possible. Spencer, understandably, did not take such a view. However Bastian saw the "evolution of life" from non-living matter in very similar terms - molecular motion and the integration of matter and hence he declared himself an "evolutionist". [22]

Although it is fair to say that all the scientific naturalists viewed evolution, in its most general sense, as describing both the organic and inorganic realms, Spencer carried this idea furthest which is why Bastian's views on evolution are of the Spencerian variety - Bastian needed a theory of evolution which encompassed inorganic nature to make his views on spontaneous generation plausible and to narrow the dividing line between living and non-living.

It would be wrong to conclude, as Bastian did, that Spencer believed that any form of spontaneous generation was possible, whether archebiosis or heterogenesis. Although he was not involved in the British debate to the extent that Huxley and more especially Tyndall were, his criticisms were unusually sophisticated. In fact Spencer had been forced to take a stand against spontaneous generation in 1868 in response to an American review of his Principles of Biology. It was this review which had brought the problem of ultimate origins, and their relation to evolution, once and for all, to the attention of the English speaking world. Francis Ellingwood Abbot, a Unitarian minister, had suggested that the theory of evolution was in difficulty because Spencer had tacitly repudiated spontaneous generation in his work and that this logically entailed a repudiation of the development theory. [23]

Spencer denied repudiating spontaneous generation and denied linking the two doctrines. He accepted that if he had agreed that life was created by spontaneous generation then he would have been guilty of Abbot's criticism, but the point was that he did not accept such a theory of spontaneous generation. [24] Spencer believed that even the lowest forms of life were so complex it was inconceivable that they could appear from non-living matter in the course of a few hours.

> "That creatures having *quite specific structures* are evolved in the course of a few hours, without antecedants calculated to determine their specific forms, is to me incredible." [25]

Spencer's view of the origin of life took into account the idea that organic matter was gradually formed when the Earth was cooling.

> "...I conceive that the moulding of such organic matter into the simplest types, must have commenced with portions of protoplasm more minute, more indefinite, and more inconstant in their characters, than the lowest Rhizopods... The evolution of specific shapes must, like all other organic evolution, have resulted from the actions and reactions between such incipient types and their environments, and the continued survival of those which happened to have specialities best fitted to the specialities of their environments. To reach by this process the comparatively well-specialized forms of ordinary *Infusoria*, must, I conceive, have taken an enormous period of time." [26]

> "...(We) are enabled to conceive how organic compounds were evolved, and how, by a continuance of the process, the nascent life displayed in these became gradually more pronounced. And this it is which has to be explained, and which the alleged cases of "spontaneous generation" would not, were they substantiated, help us in the least to explain." [27]

Although Bastian's ideas on evolution are close to those of Spencer, he never accepted the difficulties of the transition from the non-living to the living worlds and the complexities of even the simplest organisms which were so problematic for Spencer. Like Spencer, Beale thought that the idea of non-living matter continually and easily changing to living matter was extremely improbable. Yet this view led him to a conclusion opposite to that of Spencer's because his belief in vitalism led him to believe that there was such a distinct difference between living and non-living matter that he could not conceive of any transitional state.

> "...facts and arguments render it much more probable that the passage from the non-living to the living is *sudden and abrupt, than that there is a gradual transition or scarcely perceptible gradation from one state to the other.*" [28]

With regard to the origin of life on the surface of the planet, Bastian suggested:

> "...it is only reasonable to suppose that the particular combinations giving rise to living matter, when the right time came, would have occurred in multitudinous regions over the earth's surface, and would have recurred again and again as long as the conditions remained favourable." [29]

Bastian objected to the fact that Spencer, Darwin, Huxley and Tyndall in accepting the possibility of an archebiotic origin to life yet denying the existence of the process in the present day had been prepared to "promulgate a notion which seems to involve a quite arbitrary infringement of the Uniformity of Nature." [30]

Whilst Darwin had appealed to a Creator to breathe life into a few or perhaps only one form of life, Bastian suggested that Spencer had avoided resorting to this device.

> "Herbert Spencer, of course, made no sort of appeal to a Creative Hypothesis. He distinctly taught that living matter must have been the gradual product or outcome of antecedent material combinations...
> ...But, on the question of whether the process of Archebiosis is likely to have occurred once only, as Darwin seemed to hint, or in multitudinous centres scattered over the earth's surface, Herbert Spencer made no definite statement. The latter belief would, however, be entirely in accordance with his general doctrine; and we seem all the more entitled to infer that he inclined to the notion of a multiple occurrence of Archebiosis, both in space and in time, since he did not reject the possibility of its occurrence in our own day." [31]

Bastian had claimed that Spencer's original ideas on evolution supported his own stance on archebiosis, despite the fact that the philosopher had explicitly denied the possibility of spontaneous generation some forty years before his claim.

Like the German materialist Haeckel, Bastian adopted a view of life as a transmutable descent from an archebiotic origin. Indeed archebiosis was seen as a necessity for evolution in his eyes. Bastian adopted a Lamarckian view in claiming that archebiosis was the means of replenishing the lower rungs of the evolutionary ladder; for him there could be no other explanation as to why very simple organisms were currently in existence in the present day; without archebiosis, he suggested, these would all have evolved into higher organisms. [32]

Protoplasm and the Physical Basis of Life

One reason which added to the potential acceptability of the spontaneous generation of life in the late 1860s and the 1870s is to be found in the interest which was generated amongst members of the scientific community in protoplasm as the fundamental basis of life. Hitherto, until about the end of the 1850s, the cell theory, encapsulated by Virchow's famous dictum *Omnis cellula e celluli* had been the dominant paradigm for explanations of life. The point was that in the shift away from the idea of the cell towards protoplasm as the basis of life, with the accompanying view of protoplasm as essentially a simple substance, the potential gulf between the living and non-living worlds was narrowed considerably. This was the argument of the German materialist biologist, Ernst Haeckel, who believed that the lowest forms of life, *Cytoden*, were no more than naked lumps of protoplasm, far simpler than a simple cell. [33] For Haeckel the gap between the organic and inorganic worlds was bridgable and archebiosis became acceptable. Additionally a theory which denied the special nature of cells as the seat of life dealt a serious blow to the concept of vital force.

It would be wrong to think that a belief that the simplest organisms were no more than protoplasm was confined to the German materialists or to those who thought like them, such as Bastian. Protoplasmic theory became popular in Britain aided by the publicity of Huxley's address "On the Physical Basis of Life" in 1868 and his discovery of *Bathybius Haeckelii*. [34] The theory of protoplasm represents an instance of the similarity in views between the scientific naturalists and Bastian although for somewhat different ends; for the scientific naturalists protoplasmic theory was a vehicle for the exclusion of vital forces and any suggestion of a divine artificer that these forces could imply and was also important in bringing the life sciences firmly within the remit of naturalistic physical and chemical explanations. It was argued that the description of living processes rested on chemical and physical forces acting at a molecular level rather than any special vital force. For Bastian, who also repudiated the notion of vital force, it was rather a question of bridging the living and non-living worlds so that his views on spontaneous generation could become more scientifically acceptable.

Huxley's mechanistic view of life was but a different expression in less metaphysical terms of Tyndall's pantheism. For Huxley vital phenomena were not necessarily preceded by organisation: he believed instead, "that the faculty of manifesting them resides in the matter of which living bodies are composed." [35] In other words he believed that life was possible without cells and that the appearance of cells is but a manifestation of molecular forces inherent in matter. So for both Tyndall and Huxley, matter was the seat of life. There was therefore no need to postulate abrupt change or special difference between dead protoplasm and a living cell such as Beale advocated; the naturalists' view of vitality was directly opposed to Beale's vitalism. For Beale, vital properties were in some way superadded to matter temporarily and were not permanent endowments of matter.

> "I regard "*vitality*" as a power of a peculiar kind, exhibiting no analogy whatever to any known forces. It cannot be a *property* of matter, because it is in all respects essentially different in its action from all acknowledged properties of matter. The vital property belongs to a different category altogether." [36]

In constructing a thoroughly naturalistic argument as to the basis of life it was small wonder that Huxley should be drawn to the concept of protoplasm as the basic material of life. Present day commentators disagree over whether Huxley's earlier beliefs aligned him with mechanists or vitalists, but certainly by the late 1860s he had abandoned vitalism, if he ever had believed in it and his address "On the Physical Basis of Life" clearly aligned vital forces with chemical forces. [37] This was an important point for the scientific naturalists. Vital forces were chemical or physical forces displayed by the molecules of the living body and as such were *potentially* describable by science. Not only did special vital forces imply a violation of the law of conservation of energy, as a vital force, like any other force had to come from somewhere, but also in introducing the idea of spontaneity, caprice and ultimately a divine artificer they were clearly outside the domain of scientific description. The aim of the scientific naturalists was to bring the matter of life under the umbrella of science, to make it a proper subject for scientific enquiry. But they fought shy of any explanation of ultimate causes, preferring a more positivistic stance instead which, at least in terms of its rhetoric, emphasised description over explanation. The scientific naturalists were careful to point out that science could not *explain* life merely *describe* its manifestations.

It was the task of science to provide description rather than explanation of the phenomena of life which led Huxley to concentrate on the observable phenomena of motion over the more abstract concept of force so that he preferred the term "vital motion" to "vital force" with its more metaphysical connotations. [38]

In Huxley's "On the Physical Basis of Life", he described protoplasm as the common structural unit of all forms of life, composed of carbon, hydrogen, oxygen and nitrogen. To obviate the possibility of invoking a vital force to explain why the phenomena of protoplasm were so different from its constituent compounds he drew an analogy with the properties of water which are very different from the properties of its constituents, hydrogen and oxygen.

It is unnecessary to introduce something called "aquosity" when hydrogen and oxygen disappear and water takes their place. Huxley argued:

> "Is the case in any way changed when carbonic acid, water, and nitrogenous salts disappear, and in their place, under the influence of pre-existing living protoplasm, an equivalent weight of the matter of life makes its appearance?" [39]

He concluded that the concept of "vitality" had no better philosophical status than "aquosity". Finally he pointed out that as the properties of water result from the nature and disposition of its molecules there was similarly no reason to refuse to say that the properties of protoplasm result from the nature and disposition of its molecules. Hence it could be said that all vital action is the result of the molecular forces of the protoplasm which displays it. [40]

These rather theoretical considerations were given credence by a dramatic empirical finding. In the late 1860s Haeckel reported the existence of a group of extremely primitive organisms, the *Monera*. [41] He held that these organisms consisted of undifferentiated protoplasm and lacked nuclei. For Haeckel thess organisms were so primitive as to make archebiosis a real possibility to account for their existence. Huxley's discovery of *Bathybius Hackelii*, in 1868, from the preserved mud samples dredged from the 1857 expedition of the "Cyclops" off N.W. Ireland, supported Haeckel's assertions as Huxley identified the organism as consisting of protoplasm in a primitive state of organisation. [42] Haeckel seized on the discovery and declared that *Bathybius* was produced continuously by a process of spontaneous generation from inorganic materials. [43]

Ultimately, during the 1872 "Challenger" expedition, *Bathybius* was shown to be no more than calcium sulphate in an amorphous colloidal state which only appeared when alcohol was added to the mud samples. Huxley wanted to drop *Bathybius* although Haeckel was reluctant to let go and the *Monera* only gradually and quietly disappeared from the literature.

Rupke describes the history of the discovery, acceptance and ultimately silent exit of *Bathybius* as structured by the "psychological factor of confidence in the heuristic value of evolutionary theory." [44] This analysis is undisputable but it overlooks another dimension to the episode. Huxley "saw" *Bathybius* at the height of his enthusiasm and support for the protoplasmic theory of life. So its discovery was made when theoretical considerations were optimal for interpretation of the mud samples as an organism made up only of protoplasm. In a sense biological scientists such as Haeckel and Huxley were looking for such organisms to support their theoretical standpoints.

There are certain parallels to be drawn with accounts of other scientific discoveries such as the controversy over the parallel roads of Glen Roy. Darwin explained the "roads" by analogy with the raised sea-beaches of South America while Agassiz explained the same phenomena by analogy with glaciated areas of the Alps. [45] Similarly Huxley's perception of the mud samples formed an analogy with other lower forms of life, namely Hackel's *Monera*. Huxley, himself claimed that the discovery was simply a statement of fact and was anxious to disconnect it from the theory of evolution by insisting that it neither proved nor disproved that theory. [46] As it was, he publicly verified his error and "ate the leek", as he termed it, at the B.A.A.S. annual meeting in 1879. [47] Huxley must have been quite glad to relinquish *Bathybius* in the end as Haeckel's enthusiastic linkage of the organism with abiogenesis put him in an embarassing position. As has already been shown, Huxley did not like the way that critics made so much of the question of origins and especially what he meant by abiogenesis with respect to the theory of evolution. In the hands of a German scientist and enthusiastic evolutionist such as Haeckel it was inevitable that the two theories should be linked. If Huxley was in any way ambivalent about abiogenesis in 1870 in his B.A.A.S. address "Biogenesis and Abiogenesis", his own and more especially Tyndall's attempts to discredit spontaneous generation had resulted in widespread abandonment of the doctrine by the scientific community towards the late 1870s.

Beale was highly critical of the protoplasmic theory and an active opponent of scientific naturalism. Writing in 1870, he deplored the fact that the idea of a special or vital force was regarded as such an absurdity as to require no refutation, while at the same time, the idea that living beings were machines governed by physical forces was accepted purely on the authority of "influential persons" rather than on any evidence that vital force was only a form of ordinary motion. [48]

There were a number of issues in Huxley's description of the protoplasmic theory which Beale singled out for criticism. In particular the conflation of many different substances in very different states under the term "protoplasm" was unacceptable to him.

> "In order to convince people that the actions of living beings are not due to any mysterious vitality or vital force or power, but are in fact physical and chemical in their nature, Prof. Huxley gives to matter which is alive, to matter which is dead, and to matter which is completely changed by roasting or boiling, the very same name." [49]

Not only did Huxley simplify the description of the cell until it appeared to be merely a mass of protoplasm, but he also regarded any tissue to be found in nature as composed of only one type of matter; for Beale one of the most problematic aspects of Huxley's protoplasm was that no distinction was made between matter which has the power of growth and matter which has been formed and is destitute of the means of growth. [50] The idea that all the different organisms in nature were composed of the same material was absurd.

Beale's histological approach emphasised the differences between tissues and the difference between organisms, whilst Huxley's approach was rather to unify different types of living matter, to bring such matter under the protoplasmic theory, to emphasise simplicity and to pave the way for scientific explanation. Critics of protoplasmic theory emphasised difference, not only the difference betweeen tissues of different organs and organisms but also, especially for Beale with his vitalistic beliefs, the difference between *germinal* matter which could continue to grow and *formed* matter which could not. Exponents of the physical theories of life, on the other hand, emphasised unity and identity of material in order that all life could be reduced to the basis of protoplasm.

Beale was a medical microscopist and a vitalist; he believed that the vital principle was independent of matter and was inserted separately. Not surprisingly he was violently opposed to scientific naturalism and materialism. It almost seems as if Beale criticised everyone involved in the debate. He felt that Tyndall was ignorant of the state of medicine and was recommending a worthless technique in his "searching beam". He saw spontaneous generation as impossible, the germ theory as erroneous and the protoplasmic theory simplistic. But for Beale these were all part of a more general mechanistic view of life to which both Bastian and the scientific naturalists subscribed. Much of Beale's opposition to naturalism centred around its tendency to reduce everything in the universe including living beings into chemical and physical forces. His opposition was not just against physical theories of life as favoured by the scientific naturalists but against the physical sciences themselves when they were employed by "physicists" (and in this he included Thomson's arguments about the meteoric origin of life) in explanations within the life sciences and in particular when explanations based on microscopical investigations were ignored or criticised. When Beale wrote about physicists he clearly aimed his attack mainly against Tyndall and his pronouncements on both physical theories of life and the germ theory.

But his hostility was directed more widely towards any scientific individuals who sought to employ physical explanations in this way; not only were they ignorant of the methods of the life sciences but they sought to sweep away the complexities in living beings which the former methods demonstrated. Beale was quite justified in his suggestion that statements made often and publicly enough by influential people did nonetheless add nothing to the actual verity of those statements. It was certainly true that much of the scientific naturalists' enthusiasm for physical explanation was pure rhetoric. Physical and chemical techniques had offered little by way of practical benefits in the medical and life sciences and so called physical and chemical explanations of the basis of life seemed to be confined more to the *potential* to offer explanations rather than to any actuality. Even in the avowed intention of the naturalists to confine themselves to description, physics and chemistry seemed to offer little with regard to living organisms over techniques employing the microscope such as the ability to determine structure and organisation in tissues.

Furthermore Beale's pointed reaction to the arrogance of physical scientists is understandable. It is unusual to class together the arguments of scientists such as Sir W. Thomson and John Tyndall but in Beale's eyes there was a common attitude displayed, on the one hand, by Thomson's assumption that geology and biology should accept the authority of physical explanation, and on the other hand, by Tyndall's assumption that his light beam experiments i.e. a purely *physical* technique, could prove the existence of disease germs in the atmosphere. Small wonder that Beale was equally scathing of Kelvin's speculations on the meteoric origin of life and Tyndall's views on disease germs. Of course Thomson's and Tyndall's views quickly parted company as Thomson's threat to geological and biological science can be seen, at least in part, as an attempt to reintroduce the idea of design into the mechanism of evolution. For Beale, Tyndall was the real enemy in his attempts to undermine religious beliefs by his advocacy of physicalist materialistic assertions. Huxley was tarred with the same brush as Tyndall in his view of protoplasm and in his discovery of the fanciful organism *Bathybius*. [51]

Essentially Beale's hostility to physical explanations of life and those who promulgated them can be understood in relation to the different scientific paradigm within which he operated. Beale worked within a very different culture to the scientific naturalists. He was a religious man, who was concerned about the influence of scientific theories, particularly those relating to life, on religious thought. [52] He worked within the discipline of histology which emphasised the detailed and expert use of the microscope on the structure of tissues. Most importantly he emphasised the existence of a special vital force. He deplored the tendency of believers in physical theories to talk of subjects of which they knew nothing and to deny the value of his investigations.

> "Physicists and chemists have disparaged microscopical inquiry, the remarks they have themselves made proving distinctly enough that they knew nothing of the question upon which they express confident opinions." [53]

In common with the scientific naturalists, Bastian was a supporter of the protoplasmic theory of life. Like Huxley he believed that form and organisation i.e. the cell, were not necessary for the occurrence of life and that life was a function of living *matter* rather than living *form*. In a review of the Scottish philosopher, James Hutchison Stirling's criticism of Huxley's essay on protoplasm, Bastian defended Huxley's position. The cell, he suggested, had gone as a vital unit; vitality was transferred to mere living matter and the discovery of Haeckel's *Monera*, some of which were just bits of protoplasm, was proof of this assertion. [54]

> "In place of the old morphological vital unit - the cell - with its definite characters, we are reduced to a mere naked, non-nucleated bit of protoplasm, as the simplest material substratum adequate to display all those vital manifestations, previously considered to be the essential attributes of the formed elements (cells) above mentioned. The power of displaying vital manifestations has, in fact, been transferred from definitely formed morphological units, to utterly indefinite and formless masses of Protoplasm. Instead, therefore, of an obvious *form* of Life, we are reduced to a matter of Life, presenting no appreciable morphological characters." [55]

Furthermore Bastian subscribed to Haeckel's view that the existence of low organisms, consisting only of protoplasm, made it impossible to adhere to the old cellular theory of life; like Haeckel he too looked to the existence of such organisms to support his views on the evolution of life.

A further element in the scientific naturalists' arguments with regard to the simplicity of protoplasm and absence of an external artificer in the construction of living structures involved an analogy with crystal formation. Where Huxley had used an analogy with water and "aquosity" to attack the notion of "vitality" in living matter, Tyndall in his address to the Mathematical and Physical Section of the B.A.A.S. in 1868, employed an analogy with crystal formation to the same ends. [56] Bastian was fond of quoting Tyndall's analogy, although he declared Tyndall's position illogical as he refused to take his argument to its logical conclusion which was a belief in the possibility of heterogenesis. [57]

When salt crystallises out of solution, Tyndall argued, crystals begin to deposit themselves in definite shapes - but there is no question of invoking the action of an external power as it is purely the question of physical forces. How should we describe the formation of the structure of a living grain of corn?

> "But if in the case of crystals you have rejected this notion of an external architect, I think you are bound to reject it now, and to conclude that the molecules of corn are self-posited by the forces with which they act upon each other." [58]

Tyndall went on to affirm that,

> "... in the eye of science *the animal body* is just as much the product of molecular force as the stalk and ear of corn, or as the crystal of salt or sugar." [59]

He suggested that it was theoretically feasible to determine the position of every molecule in the body - the difficulty was with the complexity of the problem rather than the quality of the problem. Consciousness could be associated with definite molecular motion set up in the brain but science could only *describe* these empirical associations; it was impossible to *explain* consciousness by empirical associations. [60]

In describing the simplicity of the basic unit of life and the essential similarity between crystalline and living matter Huxley and Tyndall were in danger of being hoist by their own petard, for Bastian was able to make use of both such arguments in support of his case for spontaneous generation. As Chapter 3 suggests Bastian relied on the analogy between the formation of crystals and the formation of germs of monads and bacteria to give his description of heterogenesis credibility. His argument was that the respective substances i.e. crystals and germs appeared gradually under the microscope in solutions which before the experiment contained germs of neither types of substance, yet we do not assume that there are "germs" of crystals present in the one case, so what better reason is there to assume that there are germs of bacteria present in the other case. [61]

As a materialist, Bastian wished to exclude the concept of vital force. His views naturally supported the exclusion of a vital force as not only was a possible violation of the principle of the conservation of energy involved i.e. an argument in purely physical terms, but there was also the problem of from where the vital force has come when micro-organisms evolved from solutions. Bastian's materialism was not sympathetic to the idea of an external agent to explain vital force. He believed that there was no fundamental difference between living and non-living matter and hence no such thing as a vital force. Once again this is an example of where Bastian held a similar view to Huxley and Tyndall but for different ends. Yet Tyndall and Huxley wished to stop at the level of scientific description, believing that there were aspects of life which science could not explain, and all the while employing a materialistic terminology whilst at the same time denying a materialist position. Bastian went further in his belief that the phenomena of life were not only to be described but fully explained in terms of physical and chemical forces.

Prayer and the Material Causes of Disease

Tyndall was concerned with the furtherance of scientific medicine through his advocacy of the germ theory. The germ theory represented a means of describing the material causes of infectious disease and ultimately, he believed, the means of controlling such diseases. If the causes of disease were material then they were part of the natural world and were potentially to be discovered and controlled by scientific research. If these causes were not purely material, as Tyndall felt was implied by an acceptance of spontaneous generation, then it was hard to see how disease could be described scientifically, let alone controlled scientifically. Such a view could do serious damage to the emergence of scientific medicine. The possibility of spontaneous generation espoused by Bastian, and considered by many of the medical profession in terms of the *de novo* origin of disease strongly suggested non-natural causes which were anathema to the scientific naturalists. This was an added reason why Tyndall fought against spontaneous generation.

The spontaneous development of disease did not, of course, imply that there were no means at all of controlling disease. Traditional sanitarian measures were promoted to effect the control and eradication of disease through cleanliness. Tyndall viewed this position as getting the right answer for the wrong reasons. [62] However in its often moralising tone the sanitarian view could be pessimistic and even passive in its tendency to suggest that disease could be causd by moral excesses and intemperance. Descriptions of causes of disease in terms of epidemic or meteorological influences were hard to reconcile with the aims and methods of scientific theory, both in terms of the physical and physiological sciences, which looked for observable material causes. Tyndall's view in bringing this domain under the rubric of science was one where intervention and control over nature would allow scientific experiment and theory to triumph over the germs of disease.

There was a second important sense in which the causes of disease could be considered as non-material and this concerned religion. An ancient belief that epidemic diseases were sent by God as a punishment for sin and were to be endured, still lingered on in the middle years of the nineteenth century. In the extreme, the sanitarian view shaded off into such a belief. Beliefs in such doctrines were hardly helpful to scientific studies of disease.

It was not uncommon in the middle of the century for Anglican clergy to offer prayers to avert natural disasters such as epidemics and adverse meteorological conditions which threatened harvests. Not surprisingly, Tyndall was in the thick of debates on the question of appointing national days of prayer to avert these disasters. His basic objection was that to pray for a disruption of natural law was in effect to ask for a miracle and miracles violated the Law of Conservation of Energy. Turner has viewed such concerns not purely as a matter of physics or theology but rather as a question of cultural leadership.

> "Prayers on special occasions represented a concrete form of superstition whereby clergy with the approval of the state could hinder the dispersion of scientific explanation of natural phenomena or claim credit for the eradication of natural problems that were solved by the methods of science or that passed away in the course of nature." [63]

Although special prayers for the relief of cholera had been approved by the Privy Council in 1831, 1833 and 1849, the practice was actively discouraged in the 1850s by Palmerston and liberal churchmen. However the subject of prayer and disease was forcibly thrust into public attention again in 1871 when prayers were offered for the recovery of the Prince of Wales from typhoid. The fact that he subsequently recovered was taken by officialdom and religious circles alike as a vindication of the power of prayer. It was as if,

> "...he had been brought back from the very threshold of death, not by some abstract 'Law of Health', not merely by human skill and tenderness, but by the mercy of God who hears and answers prayers." [64]

To crown it all, some 1500 clergy attended the thanksgiving service in St Paul's while only 12 invitations were sent to medical men. So it was against a background of a sudden and surprising blow dealt to scientific medicine from the power of prayer and hence the power of the clergy that old reactionary ideas on disease were reinforced and against which Tyndall struggled with his campaign for scientific medicine through the germ theory.

Conclusions

In the relationship of evolution to spontaneous generation and their advocacy of physicalist theories of life, the scientific naturalists came up against criticisms from the life sciences and the medical profession. In particular, Lionel Beale was critical of the protoplasmic theory and the materialist position which he believed that such a theory proclaimed. It was the whole methodology of physical explanation applied to the life sciences which he felt denied the complexity of life and the role of a special vital force. Yet Tyndall's position was one where, in both the physical and medical sciences, observable, material causes were to be sought. For him, both the concept of spontaneous generation and the idea that prayer could avert epidemics implied non-material causes of disease. However, as the next chapter discusses, in his insistent campaign for the germ theory as provider of the only possible explanation for disease processes, Tyndall was unable to claim an easy victory over the medical profession.

The complex nature of the impact of the debate over spontaneous generation on the medical world raises the question of how the germ theory was assimilated as part of medical knowledge. Chapter 6 explores this question by examining the nature of the criticisms of

Tyndall's exposition of the germ theory produced by the medical profession. More specifically the work of William Roberts, John Burdon Sanderson and Henry Charlton Bastian, who were the key medical scientists involved in the spontaneous generation debate, reveals how their respective researches in the domain of pathology led each into an involvement in the debate. It then becomes possible to discern their individual reasons for acceptance or rejection of the germ theory. Chapter 6 also discusses the role of the spontaneous generation debate in antiseptic surgery and this leads on to an examination of the impact of the debate on medical knowledge in the domain of sanitation and hygiene in Chapter 7.

CHAPTER 6

SPONTANEOUS GENERATION AND MEDICAL SCIENCE

Introduction

In the last few years of the 1860s, the medical press reported widely on contagious disease, germs and the germ theory and Lister's researches on carbolic acid and antisepsis. [1] Although the fates of the germ theory and the theory of spontaneous generation were not fundamentally and irrevocably linked, it is clear that the chief actors in the British debate, Tyndall and Bastian made a very definite link between the two, although there is evidence to suggest that Tyndall regretted that the germ theory had become tangled up with spontaneous generation. [2] Tyndall was a passionate publicist for the germ theory and equally passionately believed that Bastian's experiments did not constitute a proof of the occurrence of spontaneous generation. This was very much the old argument of Pasteur who was the first to make a definite connection between the two theories. For Tyndall, Pasteur's famous researches on silkworm diseases provided a vindication of the germ theory and now Lister's work was providing practical medical applications based on the theory.

This chapter explores links between the debate over spontaneous generation and the reception of the germ theory of disease in medicine. It begins by briefly considering general reactions and then moves on to the work of medical scientists involved in the spontaneous generation debate. Firstly, the chapter discusses the work of the chemist, Edward Frankland, on water purity and Tyndall's intellectual debt to Frankland is established. Secondly, the not inconsiderable resistance to Tyndall's pronouncements displayed by members of the medical profession is discussed. It is argued that this resistance was the overt manifestation of more subtle concerns emerging from pathological research. Finally, the work of the key medical scientists involved in the debate, Bastian, Roberts and Burdon Sanderson, is examined to reveal the context of their involvement in spontaneous generation and their position on the germ theory through the 1860s and the 1870s. These three were by no means the only scientifically minded medical men to comment on spontaneous generation and the germ theory, but they were the only ones to take an active role in the British spontaneous generation debate in the 1870s. Their work reveals a coherent thread of pathological research in the 1860s and 1870s where concern over spontaneous generation and fundamental doubts over the germ theory were raised. Through this work it also becomes possible to view the involvement of these actors in the controversy in a new light. This is particularly clear in the case of Burdon Sanderson who in the 1870s was regarded as the major exponent of the germ theory despite his obvious agnosticism. The chapter closes by examining the impact of the spontaneous generation debate on Lister's work on antiseptic surgery.

Infectious Disease and the Germ Theory - Medical Opinion

The germ theory of the 1860s and 1870s was based on the idea that the air is full of (microscopically invisible) "germs" of bacteria which cause disease by entering the body and producing illness or death in the host i.e. the germs are themselves the contagious material which cause disease. Exponents of the germ theory in this period had no theoretical explanation as to how the mechanism of disease actually operated except by analogy with putrefaction or fermentation. However it was held that it was these germs which caused the spread of diseases such as typhoid, cholera, scarlet fever etc. Furthermore germs entering wounds caused the hospital diseases of puerperal fever, erysipelas and hospital gangrene. Lister's work on antiseptic surgery assumed that air-borne germs entering the wounds caused by surgery were killed by means of the carbolic spray thus ensuring that the wound would heal normally without succumbing to infection.

There were many problems with the germ theory of this period. The main objection was that it demanded a belief in "invisible" germs, hardly a concept to be taken on board lightly by the empiricist mid-Victorian medical profession. The general climate of opinion amongst the medical profession of the late 1860s and early 1870s was that the germ theory was not to be given much credence. Many of these doubts were engendered by the work of influential medical scientists such as Burdon Sanderson, as the next section reveals. Several medical men supported the concept of the *de novo* origin of disease, under some circumstances, as there appeared to be no other explanation of the outbreak of some diseases where contact with previously infected cases seemed impossible. As Chapter 7 discusses, such a view did not necessarily imply support for spontaneous generation but it was often employed, especially by Bastian, in support of such a position. Only if microscopic organisms were viewed as the active agents of disease was there a clear link between spontaneous generation and the *de novo* origin of disease. During the 1870s the gradual acceptance that there was a definite connection between minute organisms and disease meant that spontaneous generation could serve as a half-way house between older views and the germ theory. Hence in the early 1870s medical opinion towards spontaneous generation was by no means entirely unfavourable. Such views were clearly contrary to the germ theory and although these ideas could be compatible with heterogenesis most medical practitioners who supported the *de novo* origin of disease did not however, lend their support to the concept of heterogenesis as such. As Chapter 7 shows, support for the germ theory gradually grew in the 1880s after the end of the spontaneous generation debate.

The germ theory was rather abruptly brought to the attention of the British scientific and medical communities in the latter half of the 1860s when the last cholera epidemic reached East London in 1866. Luckin has suggested that general opinion is that the inaction of the East London Water Co. did much to exacerbate the ferocity of the disease. [3] Despite William Farr's efforts at the Registrar General's Office, it was impossible to convince medical men and public health officials that cholera was a water borne disease. [4] The situation was aggravated by the fact that chemical analysis failed to reveal the causative agent. Official opinion prevailed in favouring miasmatic or epidemic influence and the general view was the traditional sanitarian one; bad air, bad drainage, overcrowding and dirty habits had caused the attack. [5] Both the British Medical Journal and the Lancet later attacked the East London Water Co. for taking advantage of the scientific difficulties of identifying the cholera poison.

J.N. Radcliffe prepared an official report on the matter for the Privy Council. Luckin suggests that he was,

> "... aware of the need to refute miasmatically based doctrine in order to dramatize the potential dangers of water and river pollution, to rebuke the water companies, and to weaken the appeal of what appeared to him to be an over-exclusively 'sociological' and hence non-scientific account of the mode of transmission of infectious disease." [6]

His only allies in this view were William Farr and the chemist Edward Frankland. At that time, from about the mid 1860s until the early 1880s when bacteriological analyses based on the germ theory came more into favour, the actual analysis of water fell within the domain of the chemist. Edward Frankland, London's Official Water Analyst from 1865, was converted to the germ theory largely as a result of his work during the cholera epidemic of 1866. [7] Frankland was a distinguished chemist, holding posts as Professor of Chemistry at the Royal Institution and Royal School of Mines as well as his post as the government analyst of the Registrar General's Office. Frankland was, of course, one of Tyndall's closest friends and colleagues.

Traditional accounts of the spontaneous generation controversy, and even Tyndall's published accounts in this domain say little about a possible intellectual debt to Frankland but it is clear that Frankland was one of the earliest British converts to the germ theory and that Tyndall, who literally worked alongside him, was well acquainted with his work. In a sense Frankland was one of the first to present the germ theory as a *scientific* theory as his chemical analyses of water were based on the germ theory. It was Frankland who made the germ theory a scientific theory based on what he saw as naturalistic principles of explanation. To the scientific naturalists, concepts such as miasma were vague and unscientific but according to the germ theory contamination was due to the presence or absence of definite particles which were at least potentially detectable by scientific instruments.

The novelty of Frankland's work lay in its application of a combination of chemistry and the germ theory to the practical problems of the spread of disease and the purity of water. It was his work on water supplies during the cholera epidemic of 1866 and the fact that traditional chemical analysis failed to show the agent responsible for the cholera epidemic that forced him to change his mind over the traditional techniques of water analysis.

> "By early 1868 he had carried out a 'complete revolution' in the methods and interpretive principles of potable water analysis, a revolution based on a tentative, though remarkably modern, germ theory and on the dictum that, since the nature of the morbid elements of disease was unknown, the public welfare was best served by taking no chances with the use of purified, as opposed to pure water supplies." [8]

Not surprisingly Frankland came into conflict with chemists of a more traditional persuasion. In the Royal Commission on Water Supply, set up in 1868 in the wake of the cholera epidemic, he explained his view that tainted water which revealed no organic matter on chemical analysis could nevertheless harbour resistant germs which could multiply. This went against the traditional view that organic "poisons" if diluted became harmless and hence that nature effectively purified water supplies over time. Based on his idea of the germ theory, Frankland developed a new system of analysis where combustion methods showed the amounts of carbon and nitrogen in the water's organic matter and indicated whether or not the water had ever been polluted by sewage. [9] In other words Frankland was suggesting that once water had been in contact with sewage it could never in future be considered entirely safe. Here was an example of the scientific application of the germ theory for the benefit of humanity.

Therefore, when Tyndall began his work on spontaneous generation in the late 1860s, chemical techniques of water analysis based on the germ theory were becoming established by his friend and colleague, Edward Frankland. It is important to understand that Tyndall's luminous beam experiments were attempting to achieve somewhat similar ends to Frankland's chemical tests, but in this case to apply physical techniques as opposed to chemical techniques to establish the purity of air rather than water. [10] Tyndall was extending the use of techniques from the physical sciences into the battle against epidemic disease. Ultimately this approach was relatively unproductive as bacteriological methods of analysis based on the germ theory which he had helped to establish, took over in the 1880s. The interesting point, however, is not the apparent lack of success of chemical or physical techniques, but rather the way in which such experimental techniques were developed and deployed, assuming the truth of the germ theory which in turn paved the way for the new techniques of the science of bacteriology.

In his light beam experiments Tyndall had made the immediate intellectual leap of connecting the organic dust found in the air, with the germ theory of disease. [11] This work marked the beginning of his campaign for the germ theory of disease in which his talents as a publicist were ably demonstrated. Versions of his "Dust and Disease" discourse were published in Nature, The British Medical Journal, Proceedings of the Royal Society, Fraser's Magazine and Proceedings of the Royal Institution as well as being reprinted in Fragments of Science and Essays on the Floating Matter of the Air [12]

Tyndall praised Pasteur for discovering the nature of fermentation i.e. as caused by living organisms and commended Lister for applying Pasteur's principle to surgery in the form of the antiseptic system. He clearly meant the scientific and medical men of the day to follow him in his "scientific use of the imagination" as far as the germ theory was concerned and permitted himself a flow of the sort of rhetoric for which he was famous.

> "If the germ theory be proved true, it will give a definiteness to our efforts to stamp out disease which they could not previously possess. And it is only by definite effort under its guidance that its truth or falsehood can be established... (Dr Budd) may occasionally take a flight beyond his facts; but without this dynamic heat of heart, the stolid inertia of the free-born Briton cannot be overcome. And as long as the heat is employed to warm up the truth without singeing it over-much... so long am I disposed to give it a fair field to work in, and to wish it God speed." [13]

Tyndall's first public engagement in controversy with Bastian took place on the pages of The Times in April 1870. He took the rather unusual step of writing to the editor on the subject of his light beam experiments and their importance for the germ theory of disease. This was an obvious attempt to bring to public attention the strides forward he felt science was making in the field of medicine, particularly with regard to the chemical techniques of analysis, established by Frankland and his own new luminous beam analysis method.

> "The theory of disease was never discussed with more earnestness, or with greater precision, than at the present time. The exact methods pursued in physics and chymistry, both as regards reasoning and experiment, are making their influence felt in medicine and surgery." [14]

This letter elicited a response from Bastian who made the usual criticism of Tyndall's inferences from his discovery of organic dust to the supposition that this amounted to proof of the germ theory. Tyndall of course responded and Bastian's final contribution to the correspondence expressed his surprise at the fact that Tyndall had suddenly and apparently without provocation chosen to address the public on the subject of germ in the pages of The Times.

Bastian pointed out that Lister's results on antiseptic surgery had been known to the medical profession for some twelve months.

> "... does he, in the most disinterested way, merely express his opinion of them to the public because he thinks that the medical profession generally does not set a proper value upon them, or that some of its individual members have failed in their duty by not proclaiming the truth of these doctrines in the columns of *The Times*?" [15]

Tyndall's final attempt to publicise the germ theory at the expense of spontaneous generation in the early 1870s, was a version of his "Dust and Disease" address published in the British Medical Journal in 1871. He emphasised his views on specificity and the germ theory.

"Let me state in two sentences the grounds on which the supporters of the theory rely. From their respective viruses you may plant typhoid fever, scarlatina, or small-pox. What is the crop that arises from this husbandry? As surely as a thistle rises from a thistle-seed, as surely as the fig comes from the fig, the grape from the grape, the thorn from the thorn, so surely does the typhoid virus increase and multiply into typhoid fever, the scarlatina virus into scarlatina, the small-pox virus into small-pox. What is the conclusion that suggests itself here? It is this:- That the thing which we vaguely call a virus is to all intents and purposes a *seed*... There is, therefore, no hypothesis to account for the phenomena but that which refers them to parasitic life. [16]

Were spontaneous generation to be discredited it would be seen that epidemics do not arise *de novo* but rather arise from ancestral stock which actually lives on the human body.

"It is not on bad air or foul drains that the attention of the physician will primarily be fixed, but upon the disease-germs which no bad air our foul drains can create, but which may be pushed by foul air into virulent energy of reproduction. You may think that I am treading on dangerous ground, that I am putting forth views that may interfere with sanitary practice. No such thing." [17]

For Tyndall the point was that as medicine was at the time powerless to do anything about a disease when it established itself in the body, it was of the utmost importance to prevent its access to the body - hence his advocacy of the use of such mechanisms as cotton-wool respirators in infectious places. [18]

Bastian had criticised Tyndall for thrusting his opinions on the public and the medical world and, in particular, telling doctors what they already knew. But Bastian was by no means Tyndall's only critic from medical and scientific ranks; Tyndall's assertions stirred up much interest and discussion but little direct agreement. It is interesting to examine not only the different varieties of criticism but also from which circles the criticisms were made.

The logical problems with Tyndall's disease germs were quickly spotted by the scientific community. In a leader in Nature, Tyndall's linking of atmospheric dust with disease germs was criticised as he had undertaken no microscopical analysis - what he said was pure theory and had no bearing on the facts. [19] A week later, a further leader in Nature again criticised the germ theory or "panspermist" doctrine as it was sometimes termed, as a "flight of fancy" lacking in evidence to support it. [20]

Tyndall's views sustained severe criticism from medical as well scientific ranks. If anything the medical world was much harder on Tyndall than his scientific colleagues had been. It is not difficult to see why. There was little agreement on the nature of contagious diseases. Lister's work on antisepsis had appeared on the scene, but the theory it rested upon was still contested. Work on contagious disease, whilst suggesting that the active element of disease was particulate in nature, was otherwise far from conclusive. The medical profession looked towards men such as Burdon Sanderson for theoretical guidance but he remained ambivalent with regard to the germ theory. In addition, the experience of most medical men in the practicalities of combatting disease offered similarly equivocal evidence for the truth of the theory. There was no overt conflict between science and experience, but the situation with regard to the agents of contagious disease remained inconclusive.

Tyndall, in adopting the eloquent style of scientific publicist in his "Dust and Disease" lecture was using a mode of address which might be suitable for the general public and might be acceptable for the scientific world but was not acceptable for pragmatic medical practitioners. On the one hand he was claiming, as established truths, concepts which were far from accepted and which the medical profession had been chewing over for years; on the other hand he was telling the medical profession things that it already knew. [21]

> "But, after proving to us what we know, Professor Tyndall takes a leap, and assumes precisely those conclusions which we are desirous of his aid in testing. All these facts are as much accordant with the doctrines of Liebig and the experiments of Bastian, as with the doctrines of Schwann and the experiments of Pasteur. Granted that air-borne particles are prime agents in initiating putrefactive and fermentative change, is this by a development of pre-existent living germs, a growth of deposited ova, or by a communicated molecular motion of dead matter in a state of change? Is it from germs, or from fermentative organic particles?" [22]

Tyndall's further assumption of the permanency and immutable individuality of contagious disease, in other words the specificity of disease, was not accepted by the whole medical profession. There was still a great number of problems associated with theories of contagious disease which could not be disposed of as simply as Tyndall assumed.

> "We entirely concur in his opinion that, as a physicist, he has a great power of usefulness in this field of investigation; and if we refer him to the work of Gull, Baly, Cunningham and Lewes, Farr and Murchison, it is because we are desirous that he should not be content to win easy triumphs with audiences uninstructed in the questions he discusses, or with the partisans of the theory he has adopted, but that he should enter into the heart of the question and face its real difficulties. It would be infinitely satisfactory if we could all arrive at as simple a sole theory of disease as that which Professor Tyndall accepts entire, symmetrical, and rotund, from the supporters of the germ-theory; but we fear the solution is not yet in hand." [23]

Tyndall's work might have been received more wholeheartedly by the medical profession if the examples he chose had not been so controversial. Whilst the medical profession agreed that Lister's results were remarkable, precisely because the germ theory was not universally accepted, they did not agree on the reasons why Lister's practices were effective. George Elliott, Physician to the Hull Infirmary suggested that there were three opinions in the medical profession; 1) antiseptics destroy germs; 2) antiseptics prevent air from entering wounds; 3) antiseptics are not necessarily useful anyway. [24] Elliott felt that the theory of invisible germs which Tyndall advocated, obscured the progress that medicine was making in using and trying antiseptic methods, and was actually damaging the case for antiseptics

> "For one important reason, I believe it would be well if the theory of their existence were abandoned; and that is, I think the antiseptic treatment would be more extensively used and impartially put upon its trial, were it not for the distrust engendered by its being directed against what is naturally looked upon by many in the light of a hypothetical enemy." [25]

If medical attitudes were not very positive at the beginning of the decade, as Chapter 4 shows, the Pathological Society debate on the germ theory in 1875 demonstrated that the medical profession was still ambivalent and called for more research on the minute organisms involved in disease. Attitudes were changing gradually but the medical profession ultimately looked more towards the researches of individuals such as Burdon Sanderson, Koch and Pasteur rather than to Tyndall's experiments which were, when they produced seemingly contradictory results as in 1877, calculated to produce confusion rather than enlightenment.

By far the most scathing criticism of Tyndall's assertions engendered within medical ranks, came, not surprisingly, from Lionel Beale. As Chapter 5 shows, Beale had opposed the intrusions of the supporters of physical science into the life sciences with the protoplasmic theory. He objected to the idea that force could build organisms, the dismissal of microscopic advances and the suggestion that only physical science could describe nature even to the extent that it could predict future states of nature. Beale singled out Tyndall for particular criticism and condemned his light beam experiments and his views on the germ theory. To start with, anyone who had used a microscope was aware that the air contained organic debris, so Tyndall had made no startling discovery here. [26]

> "By the physical method of examination, particles of wool and cotton and hair, scales and other particles from insects, and starch and soot, and all the other constituents of dust, alive and dead, organic and inorganic, are illuminated so as to form one confused ray, which can be seen at a great distance; but, it need scarcely be said, the brightest light the physicist can cause to beat upon them fails to reveal the nature of the several dust particles, or enable anyone to distinguish the living particles from the lifeless *debris*; or the virulent disease germs, should there be any, from the harmless dust." [27]

Not only were Tyndall's theoretical assertions fanciful, but his suggestions as to how his physical techniques might be brought to bear on the problem of disease, Beale found ridiculous. Tyndall suggested that "Alpine air" could be brought into the invalid's room by filtering dusty air with cotton wool. These views were based not only on his own enthusiasm for Alpine exploration but also the contemporary use of Alpine resorts for tuberculosis victims as aerial sewage was held to be lower at high altitudes. Such ideas were incredible to a medical man, acquainted, not only with the foul and foggy air of the city but also with the vicissitudes of disease amongst London's urban poor.

> "Even Dr. Tyndall will scarcely be inclined to deny, after a little quiet reflection, that the promise to bring Alpine air into the London sick rooms, may appear to unromantic people who actually attend upon invalids more like the result of emotional excitement than a conclusion deduced from any exact methods of observation or experiment." [28]

In an anonymous review of Tyndall's Essays on the Use and Limit of Imagination, published in the British Medical Journal, Beale was at his most scathing towards Tyndall. He was clearly offended by Tyndall's description of him as a "microscopist ignorant alike of philosophy and biology" and a "professor in a London College famous for its orthodoxy". [29] Much of Beale's venom was directed against Tyndall's arrogance and of course Beale was not the first person to take exception Tyndall's style as a controversialist. [30]

Hence Tyndall's advocacy of the germ theory of disease engendered much criticism from within medical ranks. Characteristically it was not just *what* Tyndall said but also the way he said it which was intolerable to the medical world. But Tyndall was no stranger to debate and knew that one way to air a subject, to have it discussed, was to generate controversy about it. Tyndall wanted to convince the scientific *and* medical worlds of the truth of the germ theory of disease and the error of spontaneous generation. His methods included a burst of polemic directed against Bastian, in the very public pages of The Times, and a well publicised address "Dust and Disease" which supported the positions of Pasteur and Lister. But Bastian continued to publish on spontaneous generation; his work was detailed and scientific and was taken seriously. The medical profession remained unconvinced by Tyndall's assertions. Tyndall had not reckoned with the fact that the medical world was not the same as the scientifc world and would not readily adopt a theory of disease without practical backup.

The impact of the spontaneous generation debate, with the concomitant discussion of the germ theory, on the medical world involves a complex set of parameters. The above discussion of medical reactions towards Tyndall's work gives a broad brush view of the ambivalent attitudes involved. However it is possible to discern a finer structure. In particular it is in the science of pathology where we can discern the beginnings of more detailed bacteriological enquiries.

Pathology

In studying the reaction of the medical world, and in particular the "medical scientists", it was in the development of theoretical notions of disease and hence within pathology where the greatest potential impact of the spontaneous generation debate was to be felt. The influence of physiology facilitated a shift of interest from pathological anatomy, with its focus on disturbed structure, towards the study of disturbed function and a concern with producing etiological models of the disease process. The experimental production of disease under controlled conditions became one of the most important techniques in the new scientifically inclined pathology from the late 1860s.

Amongst those medical scientists involved in the spontaneous generation controversy, Roberts, Burdon Sanderson and Bastian were all active researchers in this area. At least over the period of the debate all three viewed their main research activities as firmly located in the domain of pathology. Therefore it becomes important to examine the work of these men in the context of pathology in order to gain a better understanding not only of the relationship between the spontaneous generation debate and scientific medicine but more specifically of the development of pathology in the 1870s and the emergence of bacteriology. Furthermore, in viewing their work from this perspective, it becomes possible to see the involvement of actors such as Roberts and Burdon Sanderson in the spontaneous generation debate not just as isolated episodes but rather as an important and consistent element of their other work. In particular Burdon Sanderson's apparent agnosticism with regards to the germ theory in the 1870s, given that he was widely taken to be one of its main advocates, only becomes understandable when viewed against his other researches.

William Roberts

William Roberts came from a medical family, trained in medicine at University College, London, became house surgeon at the Manchester Royal Infirmary in 1854 and was appointed full Physician at the early age of twenty five. [31] Much of his medical career was spent in Manchester; indeed he was widely recognised as the leading Manchester physician of the day. In 1889 he moved to London to devote his time more fully to research. At this time he also became an active committee member of the Brown Institution.

Even when he commenced his medical career in 1855, Roberts was skilled in the application of scientific techniques to medicine. Leech, his biographer, suggests that he brought to his Manchester medical work, "... advanced views concerning the methods of investigation, and brought into active use in his daily work the test-tube and the microscope..." [32]

Roberts' chief interests lay within pathology and he applied scientific techniques to his research. For around ten years from the mid 1850s he worked on urinary and renal diseases, applying chemical and microscopical techniques to problems such as estimating the amount of sugar present in urine by fermentation and the solution of uric acid calculi. [33]

By 1865 he had built up a large consulting practice and although occupied with clinical work he became interested in biological investigations. In particular he directed his attention to spontaneous generation when controversy erupted in 1870. As Chapters 3 and 4 have shown, Roberts played a small but important part in the controversy itself, for although he eschewed the polemics of the debate to a large extent, his work was instrumental in providing an early demonstration of the variability in heat resistance of different organisms and the same organism at different stages of its life cycle. [34] Although Roberts' paper, "Studies in Biogenesis" was published in 1874, it was the outcome of four careful years of research on bacteria and torulae, undertaken in his spare time in his work-room in Mosley Street, Manchester. In particular, his experiments were designed to decide the question of spontaneous generation by finding out the conditions under which he could prevent the growth of organisms in infusions where they usually appeared. "His results ... led him to the conclusion that normal tissues and juices have no inherent power to originate organisms, and that when organisms appear therein, their development is due to germs imported from without." [35]

Initially Roberts was aware that not all of his experiments could be explained completely on this view and was cautious about the complete dismissal of the doctrine of abiogenesis at that time. However as he continued his work over the next four years, Cohn's discovery of heat-resistant spores coupled with his own observations were enough to explain his earlier results. At the British Medical Association's annual meeting in Manchester in 1877 he announced his complete agreement with the germ theory. [36] Although he continued to research into micro-organisms, from 1877 he began a new series of investigations on digestive ferments and the value of pre-digested foods for invalids. In 1897, towards the end of his life, Roberts delivered the Harveian Oration, "Science and Modern Culture" in which he once more displayed his unflagging belief in the civilising force of science and the striking ameliorations which science had brought to society. Nowhere was this more ably displayed than in medicine.

> "Physiology and practical medicine have profited immensely by the general advance of the sister sciences, and by the adoption of scientific methods in the prosecution of research... The microscope also, in conjunction with chemistry, founded the new science of bacteriology. Bacteriology has inspired the benificent practice of antiseptic surgery; it has also discovered to us the parasitic nature of zymotic diseases - and opened out a fair prospect of deliverance from their ravages. [37]

William Roberts was one of a new breed of medical scientists who had been educated at University College, London and who had absorbed its scientific spirit. As Professor of Anatomy and Physiology at that institution from 1831 to 1874, William Sharpey's influence extended to a generation of medical scientists including Roberts, Foster, Lister, Schafer and Bastian all of whom had been pupils of his and Burdon Sanderson with whom he had collaborated in research. Thus it can be seen that many of the medical scientists who worked on the germ theory and spontaneous generation had been educated or had worked in the medical school of University College and had come within the pupillage of Sharpey. In fact the only notable exception was Lionel Beale, an inveterate King's College man, whose opposition to the "physical" theories inspired by the new type of experimental research in medicine at University College has already been noted.

The strong association of medical scientists with University College, London is not accidental. As Butler has shown, University College alongside the universities of Edinburgh and Cambridge all developed successful physiological rsearch schools. Sharpey pioneered practical laboratory classes in physiology. His style of teaching was carried on by Foster, Sanderson and Schafer. [38] In the 1870s Sanderson did much to enhance University College's reputation as an important research school while in the 1880s Schafer and Horsley continued this tradition, the latter both at University College and the Brown Institution. Victor Horsley, "an energetic exponent of the experimental method in physiology and pathology", had trained under Sanderson and Bastian and as a medical student had acted as a clerk to the latter. [39] Although he published a paper on neurology with Bastian these early associations do not appear to have persisted and the two medical scientists never again collaborated on neurological work. [40]

Clearly William Roberts carried with him to Manchester the values of practical laboratory research and hence a scientific approach to his work on pathology. In 1877, Roberts' address "On Spontaneous Generation and the Doctrine of Contagium Vivum" not only summed up his experimental results on the question but also discussed the application of these results to pathological problems - the action and infectiveness of disease. [41] Not only was he able to mount a sophisticated attack on abiogenesis from evolution and a detailed knowledge of the properties of micro-organisms, but he betrayed considerable sensitivity towards pathological questions, particularly those where the emerging science of bacteriology was only just beginning to provide explanations. In this respect Roberts' approach to the role of the germ theory/spontaneous generation debate mirrored that of Beale and more especially Burdon Sanderson. The two main actors in the debate, namely Tyndall and Bastian, were far less concerned over details of pathology. This lack of concern might be understandable on the part of Tyndall but was rather more surprising in Bastian's case given his training in medical research and his work on microscopy and neurology.

Saphrophytes was the term Roberts used to encompass the large class of organisms, including yeast and bacteria, which were associated with putrefaction and the decomposition and decay of organic matter. [42] To counter the claim that (a) these organisms could arise spontaneously by abiogenesis and (b) they were secondary accompaniments rather than the actual agents of decomposition, Roberts described his own experimental evidence. [43] In particular, he claimed that the evidence which proved that bacteria are the operational agents in decomposition also proved that the organisms did not arise spontaneously. He undertook a number of filtration experiments where infusions where bacteria were removed by a filter remained clear and undisturbed for months.

Addressing the B.M.A. in 1877, Roberts admitted that in his publications some three years earlier, he had been unable to explain the apparent contradictions in some of his experiments. This was the familiar problem that while bacteria themselves appeared to be killed by temperatures as low as 140 °F., there seemed no doubt that their germinal particles could survive boiling in some circumstances. Of course Cohn's work, which distinguished between the vital endurance of organisms and their spores, coupled with Dallinger's and Drysdale's work had by now, provided an explanation for these results.

Was Cohn's discovery and observation of the stages of development of *Bacillus Subtilis* in hay infusion the witnessing of an act of abiogenesis? Roberts argued that it was impossible to bridge the gap between dead and living in a mere seventy hours to produce a specifically distinct organism. [44]

In the second part of his address, Roberts turned to the subject of pathology and the question of contagium vivum or disease organisms as independent organisms or parasites. [45] In order to establish the truth of the doctrine, Roberts discussed septicaemia, relapsing fever and splenic fever.

The poison produced by decomposing animal substances, named pyrogen by Burdon Sanderson, when injected into animal bodies produced symptoms of septicaemia. But decomposition cannot take place without bacteria, hence pyrogen is the product of a special fermentation involving bacteria. When the dead tissues of a wound become infected with septic organisms, decomposition follows with the production of the septic poison or pyrogen; the poison is absorbed into the blood and septicaemia ensues. With regard to Lister's antiseptic method, Roberts felt there would be less disagreement if the principle were viewed not as an attempt to protect the wound from septic organisms but rather to defend the patient against the poison.

> "Defined in this way, I believe that every successful method of treating wounds will be found to conform to the antiseptic principle, and that herein lies the secret of the favourable results of modes of treatment which at first sight appear to be in contradiction to the antiseptic principle." [46]

It was of fundamental importance to the pathology of septicaemia that ordinary septic bacteria were only parasitic on dead or morbid tissue and would not attack healthy living tissue otherwise all animal life would perish. Infection probably occurred, he suggested, when a modification took place in the vital endowments of the septic organism whereby it acquired a degree of parasitic habit enabling it to breed in tissues of superior vitality - in this way the infective endemic pyaemia, sometimes found in the wards of large hospitals, could be produced. [47]

Relapsing fever, where the victim succumbs and recovers from successive paroxysms, Roberts associated with the vanishing and reappearance of the spirilla of relapsing fever during the paroxysm without new infection, indicating that when the spirillae disappear they leave behind them germs or spores from which new spirillae are bred. [48]

With regard to splenic fever, Roberts alluded to Burdon Sanderson's work which established that the organisms of the disease existed in a fugitive or latent form. Cohn's and Koch's researches had, by this time, established that the fugitive form were the perishable bacteria while the latent or more permanent form were the seeds or spores which were capable of surviving for an indefinite period. In his researches, Koch had observed not only the entire life-cycle of the splenic fever organism, *Bacillus Anthracis*, including its rod and spore stages, but also had tested the pathogenic activity of the organisms by injecting them into the skin of a mouse.

On the subject of the origin of contagia, Roberts recognised the importance of Cohn's discovery of the similarity in form and development between *B. Subtilis*, a harmless saphrophyte, and *B. Anthracis*, a deadly contagion. He suggested that it appeared to be the case that the infective agent in contagious septicaemia was the common bacterium of putrefaction, but modified so as to have become endowed with a parasitic habit. [49] The explanation, he proposed, was to be found in the capacity of such organisms for variation or "sporting" which was an essential tenet of the theory of evolution.

> "I see no more difficulty in believing that the *B. Anthracis* is a sport from the *B. Subtilis* than in believing, as all botanists tell us, that the bitter almond is a sport from the sweet almond - the one a bland inocuous fruit, and the other containing the elements of a deadly poison.
>
> The laws of variation seem to apply in a curiously exact manner to many of the phenomena of contagious diseases. One of these laws is the tendency to variation, once produced, to become permanent, and to be transmitted ever after with perfect exactness from parent to offspring; another and controlling law is the tendency of a variation, after persisting a certain time, to revert once more (under altered conditions) to the original type." [50]

As an example he cited an outbreak of scarlet fever in a large school where one of the masters developed diphtheria pustules on his throat and six days later his mother was attacked by diphtheria, despite the fact that there were no cases of diphtheria at the time in the school or village; only scarlet fever was present. [51]

In India, cholera breaks out and spreads over half the globe, then after three or four years the epidemic dies back and ceases; again it breaks out a few years later then disappears. This suggests that the cholera virus is an occasional sport from some Indian saphrophyte which acquires a parasitic habit, then eventually dies back or reverts to its original type. Similarly typhoid fever could be a variation of a common saphrophyte of stagnant pools or sewers which under certain conditions acquires a parasitic habit and becomes a contagious virus.

Therefore an important element of Roberts' theoretical stance on the nature of contagia lay in the concept of variations or "sporting". As this was an idea drawn from the theory of evolution it was well within the body of theory acceptable to scientific naturalism to provide explanations of phenomena. A concern with evolution became a common theme in pathological literature in the 1880s as the shift of interest towards disease processes highlighted the central issue of the origin of diseases. [52] There was, however, an ambivalent aspect to Roberts' conception of "sporting" because it was not far removed from Bastian's heterogenesis or the belief that, in the present day, one organism could metamorphose into or in some other sense give rise to a different type of organism. Clearly many of Bastian's examples of heterogenesis were incredible to Roberts eg. the growth of monads from a bacterium pellicle, or the apparent fusion of a number of monads to form an amoeba. Yet to Bastian the idea that *B. Anthracis* could be bred from *B. Subtilis* was precisely an instance of heterogenesis, as he believed that a parent organism could give rise to a totally different off-spring. In this case the idea of evolutionary variations seemed indistinguishable from heterogenesis.

Although Tyndall was an ardent supporter of the theory of evolution he was nevertheless very committed to the concept of specificity or the notion that organisms of contagium always bred true. This means that Roberts' application of the evolutionary concept of chance variations to disease organisms would have been very hard for him to accept. This was a further example of Tyndall's adherance to a very mechanistic view of the germ theory where the presence and detection of disease germs was somehow seen to be more important than how they might act on their hosts. But Tyndall's stance made it very difficult to explain some of the commonly observed phenomena of contagious disease, for example, why sometimes a disease arose without a pre-existing case as in Roberts' diphtheria example. There was also the question of why epidemics waxed and waned and why the virulence of one type of organism could die away apparently without explanation. As Chapter 4 has shown, the only explanation Tyndall could offer for such phenomena was the idea of "germ clouds". As a pathologist and clinician, Roberts' whole orientation was different. He was seeking explanations of the observed phenomena of disease and was trying out different hypotheses. Only gradually did he arrive at a firm belief in the germ theory by the time of his 1877 address to the B.M.A.

John Burdon Sanderson

Burdon Sanderson's involvement in the spontaneous generation debate in many ways mirrored that of Roberts. Sanderson had a minor yet important role in the controversy. Behind his modest contribution lay considerable experience in medical practice, public health work and pathological research.

Although Sanderson is chiefly remembered for his work in physiology, the earlier part of his career as MOH for Paddington and his work for John Simon and the Brown Institution was located in the developing realm of pathology. Only towards the end of the 1870s did he move towards purely physiological researches. Despite this he continued his interest in pathology but in a more pedagogical vein as he lectured and wrote on the nature of disease long after he ceased to be actively involved in researching the subject. This means that the spontaneous generation debate took place at a time when Burdon Sanderson was actively involved in medical work and the study of disease.

John Burdon Sanderson had originally trained in medicine at Edinburgh University and had also studied under Bernard in Paris. In 1856 he became MOH for Paddington, a post he relinquished in 1867. It is during this period that his interest in pathology was kindled. From 1857-1858 he investigated diphtheria and from 1860-1867 undertook investigations into vaccination for John Simon, Medical Officer to the Privy Council. [53] In 1865 he was appointed to the Royal Commission investigating cattle plague alongside Charles Murchison, Lionel Beale, Angus Smith and Sir William Crookes. [54] His ten or more years of public work researching disease culminated in 1869 in his report, "On the Intimate Pathology of Contagion" for Simon at the Privy Council,in which he postulated that contagious disease is spread by organic particles. [55] Sanderson resigned his post as MOH in 1867 to devote more time to pathological and physiological research. He made an arrangement with Sharpey to work in his laboratory at University College and he was soon able to relinquish both private practice and his hospital posts. Having moved away from clinical practice, Sanderson became active in pathological and physiological research in the 1870s. He hired a room for his laboratory work and undertook his experiments there with Lauder Brunton, Ferrier and Klein all of whose careers he helped by finding suitable posts.

At the onset of the debate over spontaneous generation, Burdon Sanderson had spent over a decade embroiled in the complexities of infectious diseases. If a picture emerges from his involvement in the controversy of a cautious and careful experimenter not given to the adoption of uncertain doctrines, this is reinforced by examining his work in pathology in the 1860s. He agreed that it it was incontrovertible that organisms would appear under the conditions stipulated in Bastian's experiments, yet he was unwilling to side with Tyndall's view of the germ theory until more was known as to the nature and action of pathogenic organisms .

The period from the mid 1860s was highly productive for Sanderson and allowed him to develop his ideas on contagion. Working on the Cattle Plague Commission in 1863 he discovered that the infective agent did not diffuse through parchment paper. [56] Coupled with this, Beale, working with a microscopic magnification of 2600 diameters, failed to find any definite formed objects in the blood.

> "The former fact led Sanderson to the conception of infective agents as particular bodies and not substances in solution, and the latter, there is reason for thinking, made him for long entertain the possibility of some infective agents being ultra-microscopic - a position which is now generally accepted." [57]

Although his Cattle Plague Commission work led him to conclude that some particulate agent caused a morbid state of the blood in cattle plague, he formed his views on the nature of this agent only slowly over a period of years and was unwilling to arrive at a hasty acceptance either of the germ theory or any alternative theory offering an explanation of infectious disease. In 1868 he confirmed that animals could be infected with tuberculous material derived from man but he put forward no views as to the nature of the infective agent at that time. [58]

<u>On the Intimate Pathology of Contagion</u>

In 1869, in his report to John Simon, Medical Officer of the Privy Council, entitled "On the Intimate Pathology of Contagion" Sanderson first outlined his conclusions as to the nature of disease organisms. His work of this period was regarded as highly innovative. Simon remarked,

> "We believe that at last it has become possible, with the assistance of the microscope, to make direct studies of the intimate nature and natural history of the contagia; and so far as this is true, the scientific and practical interest which must attach to such studies is transcendently great." [59]

Although this work was of an introductory nature, it came to the following conclusions as to the ultimate constitution of contagia. Firstly, each contagium existed in the form of extremely minute separate particles and secondly the particles of each specific contagium are living self-multiplying organic forms. [60] His other major conclusion was that particles of contagium are capable of rapidly reproducing themselves within the infected individuals and that these particles could survive for a long time and under extreme conditions, outside the body. [61]

Writing in 1911, Lauder Brunton cited Sanderson's work for the Privy Council as of utmost importance for the development of pathological research.

> "The great advance which pathological research has made in the last twenty years in this country is greatly due, I may say mainly due, to the impulse which Sir John Burdon Sanderson gave to it... We are so familiar now with various kinds of disease germs, microbes of all kinds, bacilli, bacteria, and cocci, that we hardly remember that when Burdon Sanderson sent in his Report in 1869... the question of whether contagia consisted of definite particles or not had not been settled." [62]

Of Sanderson's work in his Howland Street laboratory which included not only the latter study but further researches with Ferrier and his work on spontaneous generation, Lauder Brunton remarked,

> "The results obtained by Sanderson's work at Howland Street may not seem to be imposing, but they were some of the foundation stones upon which the present structure of pathology is raised, and so thoroughly and accurately did Sanderson lay them, that they remain as firm now as they did forty years ago." [63]

Sanderson's work of 1869 was one of the first studies to consider the physical nature of contagium - in other words physical qualities such as fluidity, volatility, density and solubility. The second part of Sanderson's study concerned the organic development of contagium particles.

He had undertaken a number of experiments which showed that "contagium" is neither soluble in water nor capable of assuming the form of a vapour. [64] These conclusions were of some importance if they proved to be true for all contagia, given that some contagious substances did appear to be soluble while others were apparently volatile. When a liquid is contaminated by contagium, the contamination is so universal as to produce contamination in the smallest quantity of the liquid. Furthermore if contagia were particles they were extremely minute, of the same specific gravity as the organic fluids in which they were found and virtually transparent. Given these findings Sanderson suggested that it was not possible to admit that contagium is an insoluble solid or liquid without admitting that it consists of separate particles. [65]

Sanderson repeated and perfected Chaveau's experiments on vaccine lymph which showed that diffused lymph containing only the soluble constituents of vaccine were incapable of producing cow-pox vesicles after vaccination. [66] He turned his attention to small-pox and sheep-pox which showed the same results experimentally. These provided strong evidence that the contagium existed as particles as the local effect, in terms of the size of pustules produced by the liquid was always the same no matter what the level of dilution. [67] Even if a drop were diluted ten thousand times, each of the ten thousand drops was equally infective; this could not be explained if the contagium were soluble. Roberts' experiments on ferments in the 1870s were to arrive at the same conclusion - the infective part of a decomposing liquid could be removed by filtration showing that it was insoluble and particulate in nature.

The second part of Sanderson's enquiry concerned the existence of organic development in infective particles and the relationship to the infective process. [68] The question was usually stated as follows. If contagium was alive it could either be a part of the living body or a living being inhabiting the diseased body. The mode of action of a non-living contagium must be chemical. Yet Sanderson felt that this distinction was unhelpful as no vital function was performed without some chemical change. The only character separating the living from the non-living was that of organic development so the question lay instead between whether the particles of contagium were living organisms or whether their infective properties were due to their chemical composition. The second theory was the same as assuming that each species of contagium contains some sort of immediate principle to which its specificity is due and this "principle" could only be an insoluble compound.

There were, he suggested, two obvious objections to a chemical explanation of contagia. Firstly, the multiplication of contagium in the body has no parallel in the chemical realm independently of organic development. The second point is that all contagia possess their latent virulence for long periods often under extremely harsh conditions. These phenomena were hard to explain on chemical grounds. The increase of contagium in the infected body was as rapid as many known cases of organic reproduction and the explosion of infective action occuring when particles of contagium came in contact with a living substance was paralleled in a vast number of organic processes. Cautiously he suggested:

> "The practical result of these considerations is, that chemical investigation of the nature of contagia affords but little prospect of direct result; for even if specific principles could be discovered, it is difficult to see how the phenomena could be accounted for by their chemical properties; whereas there is good and scientific ground for anticipating that the solution of the problem will some day be attained, by the investigation of the morphological phenomena which attend the infective process." [69]

He emphasised that the success of this study was strongly dependent on experimental evidence and it was vital to prove what was actually contagious material as opposed to material of contagious origin. The researches of the botanist Hallier of Jena provided a starting point. Hallier believed that the organic forms in contagious fluids were minute single celled fungi which reproduce with extreme rapidity in substances undergoing putrefaction or fermentation; the ferment of common yeast, *torula*, was an example. The organisms involved in putrefaction were much smaller than those involved in fermentation and tended to develop into rod-like bodies often termed *bacteria*. Sanderson preferred the general term microzyme to *bacteria* or *schizomycete*.

Although it was not possible to talk about the origin of microzymes without giving some consideration to the question of spontaneous generation, the question of how microzymes reproduced was far more important to the question of specificity. Some botanists believed that like microzymes always sprang from like. [70] Others like Hallier felt that colonies of microzymes could originate from the reproductive filaments of a fungus of higher organisation and that such microzymes could develop into this higher form. Hallier believed that the microzymes of two different diseases could appear the same but that the higher forms to which they developed would be specifically distinct.

> "If it could be made to appear that each microzyme may be regarded as a germ in which a specific form is wrapped up, then we should have in the inference not only an expression of the intimate relation between the disease and the organism, but an explanation of its essential nature. And if it could be further shown, that given the mature plant, the morbific germ can be produced from it, the practical bearing of such a discovery on the prevention of disease would be still more direct. If the contagium could be cultivated, it no doubt could also be exterminated by corresponding methods... We still find as we proceed that... this is far from being the case at present." [71]

Sanderson discussed these investigations with regard to cholera and sheep-pox. Hallier's investigations had led him to the proposition that cholera was spread by a mycelium which was parasitic on rice. But Sanderson criticised Hallier's neglect of details in his experiments. "... nothing is said as to the use of any means for protecting the grains from impregnation with microzymes of other than choleraic origin, so that we are bound to assume that such microzymes were present." [72] Similarly in experiments on the development of micrococci from cysts on cereals Hallier had made no comparative experiments which cultivated non-specific microzymes.

He was of the opinion that Hallier's researches were inconclusive.

> "... if it is true that our common cereals are infected with an endophyte which requires only certain very easily combined conditions of soil and temperature in order to produce nests of microzymes, and if such nests are, as Hallier states, to be found in all contagious liquids, the fact can hardly fail to have a certain significance in its bearing on the etiology of infective diseases. At present there is no ground for stating either the one or the other. The former is denied by all botanists, the latter by all pathologists." [73]

Hallier's evidence in support of the specificity of microzymes was inconclusive but this was not sufficient reason for abandoning this work entirely. There was other research suggesting that the investigation of the properties of microzymes was the path to the solution to the problem of contagion. Sanderson alluded to the two major theories as to the origin of microzymes: (a) they naturally existed as particles of living tissue and thus took part not only in morbid processes but in the performance of normal functions: the other theory suggested that the particles were in fact originally morbid and were imported from outwith the body, either (b) deriving from the tissues or organs of other infected individuals or (c) produced by the transformation of the contents of the reproductive cells of the parasitic fungi inhabiting the higher plants. [74]

Although Sanderson subsumed these ideas under two theories there were really three distinct theoretical stances involved. Theory (b) was basically the familiar germ theory. Although Sanderson described it as part of the same theory, Hallier's ideas or theory (c) were distinct from the germ theory in the important sense that they suggested a method by which an infectious disease could originate independently of communication from a sick to a healthy person. In theory (a) Sanderson was referring to the work of Lionel Beale who was vehemently opposed to the germ theory of disease. These three were the most commonly held diseases theories in the late 1860s and early 1870s. Sanderson regarded them as equal contenders for consideration and was unwilling to accept or reject any of them before further investigation of the most important issues.

These issues were stated as follows. Did the destructive parasites which inhabit the tissues of many common plants produce microzymes by a normal process of development and were these microzymes endowed with distinctive morbific properties? The part which microzymes played in normal chemical functions in the body was also important. Finally there was the question of whether or not these organisms could arise *de novo* in living tissues in consequence of impaired activity of nutrition. [75]

<u>Botany and Pathology</u>

One element omitted from Sanderson's list of important theoretical issues in the study of micro-organisms related to the bearing of botanical research on pathology. As a botanist, Hallier held that it was of primary importance to determine by experimental cultivation, the botanical specificity of microzymes.

> "This investigation, to which Professor Hallier has devoted several years of laborious research, we do not, at least for the present, purpose to follow, for although it is of all absorbing interest to the morphologist, yet from its very nature, it cannot yield practical results in pathology. Fungi of any one species, although vegetating under similar conditions, may show such differences of form that even the skilled mycologist cannot recognise their identity without watching their development, and observing their relation to the organisms they affect. This being the case, it is easy to see how little certainty the pathologist would at present gain even if he were to succeed by cultivation in discovering fungic parents of contagious microzymes; and at present the doubt and difficulty of the problem would certainly be rather increased than diminished by its removal from the field of pathological experiment into that of morphology." [76]

Sanderson admitted that although the whole question was in a certain sense botanical, yet the nature of the question brought it almost entirely within the sphere of experimental pathology. There was clearly a certain amount of criticism implied of the methods of the botanist when they were applied to pathological questions. On the one hand he felt that Hallier's botanical researches, detailed as they were, tended to obscure the more important pathological issues. On the other hand he made more direct criticisms of Hallier's methods when he pointed to his lack of suitable precautions in preventing contamination in his experiments on cholera. Similarly Hallier had presented findings linking sheep-pox and *pleospora*, an organism which developed in darnel grass; these results were also deficient in Sanderson's view.

> "... the evidence adduced by Hallier in support of the etiological relation between sheep-pox and blighted darnel is quite as inconclusive as that bearing on cholera. The question naturally suggests itself why has the theory not been tested by experiment? for nothing would appear to be easier than to ascertain whether sheep-pox can be produced or not ... by introducing them in any other way into the living body. On this subject Professor Hallier, as a botanist, does not profess to enter; he leaves it to the pathologist." [77]

Clearly he felt that despite the detailed nature of Hallier's researches he had not conducted them sufficiently rigorously to be of real value to pathology. Despite the fact that he was, by this time, convinced that disease particles were living self-multiplying organic forms, which was probably the most important conclusion of his Privy Council work, and despite the fact that he had also dismissed a purely chemical explanation of the disease process, he remained unconvinced either by a fully-blown germ theory or by Hallier's fungus theory. His concern lay rather with laying the experimental foundations upon which more exact studies of the particles of disease could be developed.

There were certainly tensions between the study of microzymes as a botanical question and alternatively as a subject belonging to the realm of pathology. In viewing Hallier's researches as purely of interest to the morphologist and of limited value to pathology Sanderson was as yet unaware of the importance of detailed studies of the life-history of micro-organisms, studies which were to become increasingly important to the spontaneous generation debate some years later. In fact by the mid 1870s it was becoming clear that the actual resolution of the spontaneous generation/ germ theory question was to be found in the botanical realm in terms of the study of the morphology of micro-organisms.

Writing over fifteen years later the botanist H. Marshall Ward described the current state of knowledge of *Schizomycetes*, a term used to cover all the minute organisms known as bacteria, microphytes, microbes and so on. [78] He described Cohn's work in the 1870s which had proved invaluable in the classification of these organisms. But Cohn had assumed the constancy of the forms he had described as species or genera. Ideas on the constancy of the forms of micro-organisms had changed in the 1870s; in particular Ward alluded to Lankester's work. Interestingly Lankester did not advance these particular researches to explain the observed phenomena in his part in the spontaneous generation controversy. As Chapters 3 and 4 describe, Lankester's concern over Bastian's experiments emphasised the physical conditions of the experiments rather than the detailed study of the organisms produced.

> "The supposed constancy of forms in Cohn's species and genera received a violent shock when Lankester in 1873 pointed out that his *Bacterium rubescens*... passes through conditions which would have been described by most observers influenced by the current doctrine as so many separate "species" or even "genera" - that in fact forms known as *Bacterium*, *Micrococcus*, *Bacillus*, *Leptothrix*, &c., occur as phases in one life-history.
> ... From that time to the present the discussion as to the limit of "species" among the Schizomycetes has been maintained; much extravagance has resulted, as well as valuable additions to our knowledge of the forms." [79]

These morphological studies were to be ranked alongside studies showing the relationship of schizomycetes to the processes of fermentation, disease and supposed spontaneous generation and suggested further that the demonstration of the relationship of these organisms to fermentation and disease had contributed to a gradual acceptance of the germ theory of disease.

Although in the ten or more years preceding Ward's article the importance of such morphological studies was increasingly recognised in the study of disease, there was still doubt as to the extent of the influence of the environment in the action of schizomycetes. There was speculation as to whether it was possible to "educate" such organisms to be parasitic. For instance *B. anthracis* and *B. subtilis*, the organism involved in Tyndall's hay infusion experiments, were very difficult to distinguish morphologically but the former was parasitic while the latter was harmless. It had been suggested that the anthrax bacillus could be converted into the harmless organism implying that the differences which botanists detected between them were due to the environment alone. Despite the importance of such assertions they could not yet be regarded as proved. [80]

In his article Ward went on to discuss the by then generally accepted causal relationship between schizomycetes and disease. However many studies had led to the conclusion that the mere presence of a schizomycete in an organ or tissue was not sufficient proof of its causal relationship to disease. He cited the necessity of satisfying Koch's postulates before a causal relationship could be ascertained. These conditions required the detection of the organism, its cultivation for several generations and the inoculation of a small amount of pure culture with the subsequent detection of the same micro-organism in the now diseased subject.

> "The satisfying of all these requirements is difficult, and the necessity of overcoming the difficulties has led to what may almost be termed a special branch of medical art. At the same time the majority of the principles which are here becoming recognized have long been known to biologists, and especially to botanists, and there are still numerous indications of a want of botanical training on the part of writers on these subjects. It is impossible here to even mention all the methods devised for staining, preparing, and examining tissues, &c... or for cultivating these minute organisms..." [81]

Effectively Ward was making a case for the power of the "new botany", the new scientific, laboratory based botany of the last quarter of the nineteenth century. Harry Marshall Ward was one of the small group of botanists which Thiselton Dyer had helped to establish during his time as Assistant Director then Director at Kew.

Although the spontaneous generation debate was conducted mainly in terms of its implications for human and animal diseases there were clear analogies to be drawn with plant diseases. Thomason has suggested that the debate over spontaneous generation coupled with Burdon Sanderson's work on pathology and Lister's work on surgical antisepsis helped to develop a more favourable climate of opinion towards the idea of a *contagium vivum*. This in turn aided the generally favourable reception of Marshall Ward's work on coffee leaf disease in Ceylon by the scientific community in the early 1880s. [82]

It is interesting to note that more than fifteen years after Sanderson had called a botanist to task for undertaking studies of micro-organisms which appeared to be of limited relevance to the study of disease, Ward was able to point to the lack of appropriate botanical training amongst medical practitioners. But while Ward's analysis was thoroughly up to date, in the same edition of the Encyclopaedia Britannica, Charles Creighton who had himself undertaken pathological research at the Brown Institution, was by no means as postive with regard to the germ theory. He described as "doctrinaire" the school of pathologists who refused to accept the occasional *de novo* origin of typhoid and emphasised the environmental or "natural-history" aspects of disease in terms of history, geography and ethnology to be considered alongside the morbid anatomy and clinical history of each disease. [83] Whilst not directly opposing the germ theory, he believed that it was still premature to describe as pathogenic the various forms of micro-organisms to be found in the body after death from disease. [84] Although Creighton could be regarded as somewhat old-fashioned in 1886, as Chapter 7 shows the germ theory was accepted at very different rates by different authors. In emphasising environmental factors Creighton's views were consistent with contemporary sanitarian feeling.

Burdon Sanderson's Contributions to Pathology

In the early 1870s Sanderson began to study the processes of wound infection. [85] Having dismissed the chemical theory of contagion, he began to examine in more detail the lower forms of plant life and he showed that Hallier's views were erroneous. Bacteria were to be freely found in external nature but living tissue was usually bacteria free. [86] He then showed that in both septicaemia and pyaemia bacteria were present yet he remained sceptical with regard to the germ theory. Writing in 1878, although cautiously accepting that bacteria were the causal agents in septicaemia, he was still unwilling to commit himself to the germ theory. [87]

In the late 1870s he worked on pleuro-pneumonia and anthrax at the Brown Institution. Sanderson's biographers assert that this work anticipated later results on immunity and did not receive the attention it deserved. [88] After this period his contributions to pathology were mainly in the form of critical articles and lectures and his research interests turned to physiology. His biographers suggest that it is difficult to assess Burdon Sanderson's contributions to pathology - the dangers of jumping to conclusions often led him to err on the side of caution.

> "His outlook was almost too wide. It was not enough for him that certain bacteria were found associated with certain disease manifestations: he must needs know the vital capacities of such organisms and how they worked before assigning to them an etiological relationship to the conditions in which they occurred. Thus in 1877 he refused to range himself as a supporter of the germ theory of disease, and he then freely stated that knowledge was yet too limited to permit of theories being entertained; it was the duty of investigators to go on accumulating facts. [89]

Despite his hesitancy, Sanderson was usually regarded, alongside Lister, as the chief supporter of the germ theory and his work on septicaemia and pyaemia was instrumental in convincing the medical community of the truth of the germ theory. Indeed it is understandable that Burdon Sanderson's cautious and detailed studies of disease and infection should have held such authority with the medical world in contrast to Tyndall's more flamboyant claims. Lister in several of his papers expressed his indebtedness to Sanderson's work in the development of his own views. [90]

As Chapters 3 and 4 have discussed, Tyndall was critical of the support which Sanderson apparently lent Bastian's position by confirming that organisms developed in his experiments after boiling. However Sanderson was always careful to assert that these experiments did not constitute a proof of spontaneous generation. The real bone of contention for Tyndall was that the eminent Burdon Sanderson, one of the leading medical scientists of the day, still in 1877 refused to accept the truth of the germ theory.

In the summer of 1877, after Tyndall had announced his results on discontinuous boiling, Burdon Sanderson delivered a lecture to the Association of Medical Officers of Health. [91] Tyndall took exception to the following remarks of Sanderson on the structure of germs.

"The ground which the orthodox biologist holds now, as against the heterodox, is not that every *Bacterium* must have been born of *another Bacterium*, but that every *Bacterium* must have been born of something which emanated from another *Bacterium*, that something not being assumed to be endowed with structure in the morphological or anatomical sense, but only in the molecular or chemical sense. It is admitted by all, even by Professor Tyndall, that, so far as structure is concerned, the germinal or life-producing matter out of which *Bacteria* originate exhibits no characters which can be appreciated by the microscope... Germs have given place to things which are ultramicroscopical - to molecular aggregates - of which all we can say is that they occupy the border-land between living and non-living things." [92]

Tyndall was in complete agreement that it was not possible to appreciate structural characteristics in *Bacteria* by the use of a microscope, yet that did not mean that they had no structure.

"A little consideration will make it plain that the microscope can have no voice in the question of ultimate germ-structure. What is it that causes water to contract at 39° F., and to expand until it freezes? It is a structural process of which the microscope can take no note, nor is it likely to do so by any conceivable extension of its powers... It cannot be too distinctly borne in mind that between the microscopic limit and the true molecular limit there is room for infinite permutations and combinations." [93]

Tyndall asserted that if a particle, when sown in suitable soil, produces a plant then this is proof that the particle is the germ of the plant. It was not possible that the germ which emerged into *Bacillus anthracis* had no structural difference from the germ which develops into a harmless *Bacterium* and if they possess structural differences then they must possess structure itself.

Sanderson felt that he and Tyndall were in general agreement over the question of abiogenesis but there were differences between them in the sense in which the term "structure" was used. Furthermore much less was known of the structure and attributes of the germinal particles of *Bacteria* than Tyndall supposed. [94]

A germ was thought to possess structure in the molecular but not the anatomical sense. There was no evidence that "germs" of bacteria were endowed with the particular texture which concerned histologists and there was no way that the biologist could have recognized ultramicroscopical structure or organisation except from observing similar organisms which actually possess visible structure. Embryology showed that although it was possible to infer the existence of structure in embryonal organs, it was not possible to carry this back to the ovum itself. For instance, the ovum of the mouse and the elephant are similar but the potential difference between the two of them was understood to be dependent on an actual difference of molecular structure. The sense in which Tyndall used the term "structure" to refer to germs could only meaningfully refer to molecular structure otherwise his arguments were irrelevant. [95] Apparently Tyndall had overlooked the distinction that Burdon Sanderson had made between anatomical organisation and molecular structure.

He agreed that germinal particles were produced from parent organisms but there was much more to the process than Tyndall's simplistic representation.

> "If, for the sake of clearness, we call the particle *a* and the organism to which it gives rise A, then what is known about the matter amounts to no more than this, that the existence of A was preceded by the existence of *a*. With respect to A, we know, by direct observation, that it is an organic structure; but inasmuch as we know absolutely nothing as to the size and form of *a*, we cannot even state that it is transformed into A, much less can we say anything as to the process of transformation." [96]

The question of the molecular structure of living material was of fundamental importance to biology - each form of living matter must possess a molecular structure peculiar to itself but, he suggested, we are far from knowing what these molecular structures are. It had certainly not been proved that atmospheric dust contained organised particles endowed with anatomical structure. Furthermore these germinal particles were not necessarily the germs of disease as experiment had shown ordinary bacteria could be introduced into the blood of healthy animals without producing any disturbance of health. [97]

<u>Henry Charlton Bastian</u>

Bastian began his researches on the lower organisms in 1868 not long after his appointment to the chair of pathological anatomy at

University College, London. His earliest scientific work, published in the mid 1860s, involved the study of guinea worms and other nematoids but he gave up this research when he developed an allergy to these organisms. [98] He then took up simultaneously the study of clinical neurology and heterogenesis. The former subject occupied him for the whole of his career while, as Chapter 4 describes, he abandoned the study of micro-organisms in the late 1870s only to return to this work after his retirement in the early years of the twentieth century. Unlike Burdon Sanderson, Bastian was involved in practising clinical medicine throughout his career yet his studies of micro-organisms, although originating from clinical research as is shown below, betray much less of a practical concern in the processes of disease and his interests in the origins of life led him away from the pathology of disease processes.

At first sight his early work on nematoid worms seems to bear little relationship to his experiments on heterogenesis and abiogenesis which followed some three or four years later. His work on worms involved an extensive natural history study of these creatures where their anatomy and physiology was described in some detail and their zoological position with repect to other similar creatures was discussed. [99]

Although such a detailed natural history study might have seemed rather unusual for an individual involved in clinical medicine, there are definite relationships between Bastian's earlier studies and his work on spontaneous generation and it is possible to discern in this earlier work both the development of some of the interests and also the necessary skills to be found in his later work on spontaneous generation.

To start with, his work on nematoids provided him with a thorough grounding in the art of microscopy, one of the most important tools of the medical scientist and one which was to prove instrumental in the resolution of the spontaneous generation debate. On the one hand Tyndall, the physicist, simply underestimated the value of microscopy, "When... the contents of the cell are described as 'absolutely structureless,' because the microscope fails to distinguish any structure... then I think the microscope begins to play a mischievous part." [100] To Beale, on the other hand it was the most important piece of apparatus in the scientific investigation of disease.

Secondly, nematoids were involved in parasitic diseases both in the plant and animal kingdom, ranging from the guinea worm which was said to cause "Egyptian chlorosis" to *Tylenchus tritici* which produced a disease in wheat. [101] Hence these investigations were Bastian's first studies of minute organisms, some of which were involved in disease. Furthermore Bastian had traced the life-history of some of these organisms. [102] He realised that the apparent discovery of so many free nematoids suggested that these might represent only certain stages in the life-history of parasitic forms previously unknown and therefore perhaps had no claim to be considered as distinct and independent species. [103]

The third important aspect of Bastian's work on nematoids related to their powers of surviving heat and desiccation or their tenacity of life. Many nematoids displayed a remarkable power of recovery after complete desiccation. Bastian suggested that these organisms probably had a definite span of active existence in which to go through their various stages but that this could be prolonged by the interpolation of a number of periods of dormancy under the varying conditions of their environment. The active life of the species *Tylenchus tritici* is about nine or ten months but individuals had been known to retain their life for a period of twenty seven years. [104] Davaine's experiments had shown that although these organisms could survive desiccation they lost their vitality after exposure to a dry heat of 160°F. [105] Davaine had discovered and Bastian confirmed that the young organism was much better able to withstand desiccation and heat than the adult organism. [106]

Bastian's first work in pathology, published in 1868, involved a study of tubercle. [107] He felt it was important to be sure of the microscopical characters both of tubercle and histologically allied products which could possibly be confused with tubercle on microscopical examination, particularly at a time when the inoculability of tubercle was attracting so much attention. He showed that the histological elements met with in tubercular grey granulation were indistinguishable from those met with in the early stages of fibroid degeneration and suggested that the growth of tubercle occupied an intermediate position between inflammation on the one hand and degeneration on the other.

His first experiments on microscopic organisms which were also published in 1868, arose from a study of the blood of patients suffering from infectious fevers; in particular he was trying to detect the minute particles said to cause disease. [108] Having made detailed studies of nematoids, he now examined more minute organisms involved in human infectious disease. His first experiments were on the passage of red and white blood corpuscles through the walls of capillaries either during mechanical congestion or inflammation. For both types of corpuscle, Bastian described the process as one of some sort of inherent amoeboid activity. [109]

His next piece of pathological research described the plugging of vessels in the brain by embolic material apparently made up of an agglomeration of white blood corpuscles and causing delirium in the victim. [110] For about a year from the summer of 1868 he had frequently examined the blood of patients suffering from rheumatic fever, typhoid fever and pneumonia:

> "... in some of these, in addition to finding a most marked increase in the number of white corpuscles, I was much struck by the existence of large masses of protoplasmic material in every way similar in composition to the white corpuscles themselves..." [111]

He suggested that congestions caused by these corpuscular aggregations could produce the symptoms of diseases such as typhoid.

His first description of the possible *de novo* origin of bacteria also arose out of the same series of observations on moving particles in the blood of patients. [112] He referred to the work of three researchers - Davaine on malignant pustule, an American researcher, Salisbury on fevers and typhoid and Hallier's research into measles. All these men suggested that the presence of these diseases was invariably accompanied by bacteria or spores of fungi. In the past year Bastian had examined many specimens of blood with high microscopic powers.

> "All these specimens have been submited to a prolonged and careful examination, and yet in none of them have I been able to find any of the spores in question, whether of fungi or of algae. But I have, not infrequently, found in these specimens of blood - examined, perhaps, three or four hours after mounting - a variable number of mere moving molecules, or short, irregularly rod-shaped particles, these being highly refractive, and varying in size from 1/30000" to 1/10000" in diameter." [113]

Bastian offered three possible explanations as to the origin of these particles. In illnesses such as anaemia the red corpuscles undergo alterations and may be mistaken by an inexperienced observer for bacteria. Secondly, some of the smaller particles could be Bennet's *hystolitic* particles derived from white corpuscles. The third theory, and the one which Bastian favoured, was that certain moving particles could be formed *de novo* through molecular changes in the blood plasma itself in individuals where their vital power was reduced to a low ebb by disease. Bastian had discovered bacteria in the brain of a patient who had died of rheumatic fever where the brain itself had been kept free from external contamination. Furthermore he had found bacteria in specimens of blood after twenty four to forty eight hours, where careful examination had established that these did not exist when the blood was first mounted. [114] He rejected the idea that diseases such as scarlet fever and enteric fever were specific diseases which originated from germs.

> "He... regarded them as blood diseases, modified by circumstances in various cases and being in relation to each other much in the same way as certain diseases of the nervous system which are known to be interchangeable". [115]

Although Bastian's interest in the *de novo* origin of micro-organisms had been aroused by the study of blood in clinical cases he gradually moved away from pathological research to what appears to have been a purer interest in the origin of these organisms. After the mid 1870s he conducted no further clinical research in this area. But as Burdon Sanderson had retained an interest in pathology when he was no longer actively involved in research in that area, so too did Bastian retain an interest in the clinical aspects of spontaneous generation long after he had ceased to be involved in clinical research. Bastian was part of the same intellectual tradition as Roberts and Burdon Sanderson. His medical training had been undertaken at University College, London and he too saw himself as a medical scientist applying scientific methods to the study of medicine. By the late 1870s Bastian was probably the only prominent researcher in Britain to support the concepts of archebiosis and heterogenesis wholeheartedly but a decade earlier his views as to the etiology of disease were quite consistent with his contemporaries. During the earlier period he treated the germ theory with scepticism but so did Burdon Sanderson, Beale, Roberts and many other medical men. In the late 1860s he would have agreed with Burdon Sanderson that living particles were in some way involved in the processes of disease but from that date their views diverged. While Sanderson and Roberts came round to a gradual acceptance of the germ theory, Bastian never changed his views and clung steadfastly to his beliefs on spontaneous generation which whilst they were credible in 1870 were becoming increasingly unacceptable towards the end of the decade.

There were a number of reasons why Bastian never relinquished his views on spontaneous generation. To start with, he was quite justified in believing that he had never received a fair hearing from his critics in the medical and scientific worlds and many of his later writings were directed at vindicating his position against, what were by then, long dead opponents. His later results were largely ignored. Even after his death, his son challenged bacteriologists to disprove his father's work without an apparently satisfactory conclusion. [116]

Secondly, had Bastian retained an interest in the study of infectious diseases he may ultimately have been forced to reconsider his views on spontaneous generation and the germ theory. However his growing clinical specialisation in neurology led him to concentrate on that class of disease at the expense of infectious diseases. At an early stage in his work on spontaneous generation he moved away from what he saw to be narrower questions in pathology and the biomedical sciences towards broader biological issues with the result that his beliefs on spontaneous generation were untouched by many developments in pathology and bacteriology. However he was able to claim that some bacteriological research actually supported aspects of his earlier work. In his later works Bastian maintained that his beliefs on heterogenesis where one organism could metamorphose into another had been vindicated by bacteriological research. In 1913, he wrote:

> "There is, for instance, the well known mutability of Bacteria both in form and in function; the fact that their mere "discontinuous growth" puts heredity out of court; and therefore at once tends to show that the passage from one form to another under the gradual influence of changing media and other conditions is not only possible, but of actual occurrence. No abrupt line of demarcation separates the pathogenic from the non-pathogenic forms, for, as Lehmann and Neuman say in their *Principles of Bacteriology* (1901, p. 118), "We can understand and know the pathogenic varieties only if we study simultaneously the non-pathogenic, from which the former have once originated and will always originate" The admission of this truth must soon become more general, and, as a consequence, there will be a demonstration of the untenability of ultra-contagionist doctrines in reference to the very many communicable diseases in which Bacteria play a prominent part." [117]

His views on neurology were consistent with the materialist position he had developed in his work on spontaneous generation. Having seen much of his work in the latter area discredited or ignored, from the 1880s onwards Bastian developed his ideas on evolution through the medium of his neurological work instead. He opposed the conception of "mind" as a separate entity, believed that there was no real distinction between conscious and unconscious acts and maintanied that the difference between sensation and perception was only one of degree. [118] It was no more possible to dissever feeling and consciousness from the physical conditions on which they depended than it was to dissever magnetism or heat from their physical conditions. As heat was a mode of motion so was consciousness a result of a concert of molecular motions. [119]

> "For the Evolutionist" the Metaphysical conception of Mind as an entity should disappear, and with it all forms of "spiritualism." He who believes in Archebiosis, either once or repeated, if consistent, can believe only in mental phenomena as resulting from the action of nervous systems, and as having no existence apart therefrom." [120]

Antiseptic Surgery

The investigation of the processes of disease was an important part of the domain of pathology and as the first part of this chapter has shown for Burdon Sanderson and Roberts at least, an understanding of the problems involved in establishing the etiology of disease and the nature of contagia delayed an acceptance of the germ theory at least until the middle of the 1870s for Roberts and in Sanderson's case possibly later. Yet for Tyndall who was in no position to appreciate the finer points of pathological questions it was the practical benefits accruing from the application of the germ theory which were important. The two areas where the germ theory was to advance the cause of scientific naturalism were, in Tyndall's eyes, in the control and prevention of infectious disease, and in the reduction of mortality rates due to the infection of surgical wounds, i.e. public health and antiseptic surgery respectively.

The inventor of antiseptic surgery was the surgeon Joseph Lister. In 1883, Tyndall expressed his admiration for,

> "the labours of a man who combines the penetration of the true theorist with the skill and conscientiousness of the true experimenter, and whose practice is one continued demonstration of the theory that the putrefaction of wounds is to be averted by the destruction of germs of bacteria." [121]

For Tyndall, Lister was the ideal exponent of scientific medicine. On the one hand not only had Lister adopted the germ theory while on the other he based his practical medical techniques on this theory. This was an important area where the germ theory had led to immediate practical results.

Lister's early research had been into the process of inflammation but it was in 1865, when he read Pasteur's work, that his interest in the germ theory was kindled. [122] He applied Pasteur's researches to his own work, by a process of analogy, and began to see that what Pasteur did to prevent germs entering a flask and causing putrefaction, could be applied to the entry of germs into wounds. His first paper enunciating the antiseptic principle was published in 1867. [123] The principle emphasised the necessity of dressing wounds with some material capable of killing septic organisms, yet which should not prove to be too caustic to surrounding tissue. Carbolic acid was chosen as a suitable substance as it had already been used for the disinfection of sewage. [124]

Lister's therapeutic practice changed considerably over the years as he tried different sorts of dressings and ligatures and he soon applied the principle to surgical wounds as well as wounds from compound fractures. In 1871 he introduced the carbolic acid spray. [125] The introduction of the spray was based on his belief that the air contained a multitude of the germs which caused infection. The concept that the air itself was the main carrier of disease germs was a central tenet of the germ theory initially described by Pasteur and reinforced by Tyndall. The carbolic spray was, therefore, a therapeutic technique directly based on this theoretical position.

As Lister was aware that the germ theory had not achieved universal acceptance, throughout the 1870s he stressed that it was not necessary to believe in the germ theory. It was only necessary to believe that the septic agents, whatever they were, came from outside the body and they could be destroyed by means of chemical agents.

> "I do not ask you to believe that the septic particles are organisms. That they are self-propagating, like living beings, and that their energy is extinguished by precisely the same agencies as extinguish vitality, such as heat and the various chemical substances to which I have referred, is certain, and is of the utmost practical importance." [126]

Lister adopted the strategy of separating theory and practice in the hope that the antiseptic system could be accepted on its own merits as an effective treatment and thereby could be disseminated as widely as possible amongst non-believers. However there were problems with this approach. In the 1870s much uncertainty surrounded the application of antiseptic techniques. Some surgeons used a modified antiseptic technique and there was a general emphasis on hygienic hospital surroundings which made the adoption of his technique seem less compelling. But although Lister had emphasised that it was not necessary to adopt the theory itself, without that belief it is hard to see why surgeons should have adopted the extreme care that Lister advocated. Accidents could easily occur. [127]

It is difficult to assess how far the argument over the germ theory and spontaneous generation, conducted by Tyndall and Bastian, advanced or impeded the cause of antiseptic surgery. Youngson suggests that ultimately the final triumph of antiseptic surgery depended on acceptance of the theory which lay behind it and that attention was increasingly focused on the germ theory as the 1870s wore on. [128] Although he describes the debate between Tyndall and Bastian in some detail, including Burdon Sanderson's agnostic stance, he is unable to form any distinct conclusion as to its effects on Lister's antiseptic techniques.

> "Amid such confusion of expert opinion it was inevitable that non-experts clung to some simple explanation. And that simple explanation was some variant of the idea of spontaneous generation, or what the *Lancet* called 'occult atmospheric influences tending to the production of erysipelas and pyaemia.'" [129]

Youngson is really suggesting that the spontaneous generation/germ theory debate confused medical opinion to the extent that medical men clung to old miasmatic theories. But this oversimplifies the relationship of the debate to theories of infection. The idea, as Burdon Sanderson himself put it, that there was indeed some sort of relationship between microscopic organisms and infection was gaining ground even if many medical practitioners were unwilling to commit themselves to a full blown germ theory. Moreover the miasmatic theory of disease was becoming old-fashioned by the 1870s and was not, strictly speaking, a variant of spontaneous generation, in the sense that the term was employed in that period.

The main impact of the spontaneous generation debate was rather that it gave the germ theory a considerable amount of publicity. In his letters to The Times, public lectures and publications on the subject, Tyndall was not only attempting to vanquish Bastian, he was mounting a publicity campaign for the acceptance of the germ theory, both amongst the medical profession and the public at large, and his campaign was at its height in the 1870s, a crucial decade for the acceptance of the germ theory and the emergence of bacteriology. In the domain of public health and sanitary science Tyndall became a participant as he was able to suggest a few simple techniques which could easily be accommodated to existing sanitary practice. With the application of the germ theory to antiseptic surgery, Tyndall was an enthusiastic spectator, only able to offer a general endorsement of Lister's techniques. Similarly Tyndall's work had little to offer the increasingly important techniques of bacteriology such as staining and the production of pure cultures.

Apart from the complexities of Lister's techniques, the fact that he kept changing them and the possibilities of failure if the rationale were not appreciated, there was a problem that the techniques were in a sense too theoretical for the pragmatic English surgeon. They depended a great deal on speculation about what was happening in wounds. These new techniques brought changes to the practice of surgery which could be perceived as a threat to the attainments and social status of the older type of surgeon. No longer was surgery a matter of heroic skill and manual dexterity; careful surgery and aftercare were emphasised as more important skills.

Conclusions

The role of the spontaneous generation/ germ theory debate in shaping medical opinion in the 1860s, 1870s and later is complex; three separate strands of scientific research were involved.

As the spokesman from the physical sciences, Tyndall's bold assertions regarding the truth of the germ theory were largely unpalatable to the medical community. On the one hand, although he emphasised the complexity of miscoscopic organisms in order to deny the possibility of spontaneous generation, he failed to appreciate the complexities of pathological processes and his attempts to subsume such processes under a single mechanistic germ theory were antithetical to the empiricism of contemporary medicine with its stress on the apparent individuality of disease cases, an emphasis on clinical observation and a dislike of theory. Tyndall's observations were drawn from his research on light beams, where a purely physical phenomenon was employed in recording the amount of dust in the air. Furthermore, not only did he make the theoretical assumption that the quantity of this dust was an exact measure of the amount of contagious material in the atmosphere, he also denied the effectiveness of the most important tool of the medical scientist, namely the microscope, in favour of his searching beam.

Beale's and Bastian's work can be seen in terms of their contributions to biological sciences. Beale emphasised the complexity of living material in his denial of the physical theories of life within which he included both protoplasm and spontaneous generation. For him, the microscope was of fundamental importance in the study of minute organisms and the structure of living material and he strenuously denied the power of Tyndall's light beam technique. While Beale asserted the complexity of living material, Bastian's beliefs were based on the apparent simplicity of the lower organisms. He never felt it necessary to undertake careful life-history studies as he had done for worms, advocated by botanists such as Marshall Ward, which were so important in the eventual resolution of the spontaneous generation debate. Although engaged in clinical research, Bastian saw both his work on spontaneous generation and neurology as a contribution to wider theoretical issues relating to the theory of evolution.

As pathologists Burdon Sanderson and Roberts stressed both the complexity of the lower organisms in their denial of spontaneous generation and the complexity of pathological processes in their gradual acceptance of the germ theory. Both saw a need for a combination of clinical observation and scientific experiment in understanding and explaining the processes of disease.

In the field of antiseptic surgery the spontaneous generation debate allowed an additional airing of the germ theory at an important time when Lister was trying to convince his peers of the value of antisepsis. However as Chapter 7 argues, in the domain of sanitation and hygiene the impact of the spontaneous generation debate and discussions of the germ theory was far more direct. It is in this area of medicine where it becomes possible to discern not only a gradual acceptance of Tyndall's techniques but also an acceptance of the germ theory as the new science of bacteriology began to offer more concrete examples of the role of micro-organisms in disease and as medical opinion began to look towards explanations of disease in terms of microscopic organisms. In many ways the retrospective significance of the spontaneous generation debate was greater than its actual significance in the 1870s. In the 1880s spontaneous generation was cited as being vital in the 1870s. In addition, the writing of triumphant histories of figures such as Pasteur further served to make the debate appear far more decisive in retrospect than the historical evidence suggests. [130]

CHAPTER 7

THEORIES OF DISEASE AND SANITARY SCIENCE

Introduction

Although the climate of medical opinion was not inherently unfavourable to Tyndall's campaign for scientific medicine in the 1870s, the style of his campaign and in particular his apparent ignorance of true medical knowledge excited criticism from medical ranks. This criticism was made not only by the older type of medical practitioner, whose medical education had been undertaken long before curricular reforms introduced science classes, but also by figures such as the influential Lionel Beale, who as an expert microscopist was well placed to understand the weaknesses of Tyndall's arguments on the germ theory. Even figures such as John Burdon Sanderson and William Roberts were reluctant to espouse the germ theory until a fuller picture began to emerge of the role of microscopic organisms in disease processes.

This chapter goes on to examine how the results of the spontaneous generation debate impacted one particular branch of medical knowledge in the 1870s and afterwards. The areas of medicine where the subjects of spontaneous generation and the germ theory were potentially of most importance included pathology, in the detailed study of the action of disease, in antiseptic surgery in the study and prevention of wound infection and hygiene and sanitation, in the control of epidemic disease and sterilization techniques. Chapter 6 has discussed the first two areas and it is the latter subject area which is discussed in detail here.

The chapter begins by looking at the state of scientific medicine in Britain particularly with reference to physiology and pathology. Contemporary theories of disease are discussed in order to understand the changes in disease theory taking place in the 1870s and 1880s; the changes themselves are described later in the chapter. Before introducing works on sanitation and hygiene, the individuals working in that area, namely the "sanitarians", are described. In particular the view that sanitarian belief was generally hostile to science is criticised. That this criticism is justified is demonstrated by the gradual accommodation of the germ theory in works of sanitation and hygiene through the 1870s, 1880s and 1890s. A consideration of the impact of the spontaneous generation debate on these handbooks follows, in terms of sterilization techniques based on Tyndall's experiments, changes in disease theory, the acceptance of the germ theory and the move towards bacteriology.

Scientific Medicine in Britain

When Tyndall talked of making medicine scientific he meant something more than "experimental medicine" which was the more commonly used term in the 1860s and 1870s. He wished to see a new form of medicine where not only was experimental laboratory research performed but medical knowledge was also informed by scientific theories such as the germ theory. Therefore, for Tyndall at least, scientific medicine was to incorporate both an experimental and a theoretical component. However, appeals for reform of medicine were often made in terms of experimental medicine. This did not mean that scientific theory was unimportant, rather that the value of scientific experiment provided a much more visible and tangible rhetoric. In particular it was to Continental models of physiology and pathology that the scientific naturalists looked for the experimental components of the new scientific medicine.

In France, Claude Bernard viewed the achievement of nineteenth century physiology as the introduction of "the experimental method in the science of vital phenomena". [1] In Germany, in 1847, Hermann Helmholtz, Emil DuBois Reymond, Ernst Brucke and Carl Ludwig issued a pledge to reconstitute physiology by explaining vital phenomena in terms of physical and chemical principles. [2]

Also in 1847, Rudolf Virchow, one of the foremost contemporary writers on scientific medicine, called for medicine to be built on the foundations of experiment.

> "Experiment is the final and highest court of pathological physiology, for experiment alone is equally accessible to the entire world of medicine, and experiment alone shows the specific phenomenon in its dependency on specific conditions..." [3]

But despite the successes of Continental programmes of physiology in the middle of the century, these were slow to percolate across the English Channel. For both pathology and physiology there was a lack of appropriate professional opportunities and a general indifference to the resources required for basic research. As Beale suggested, the British public did not seem to understand that a medical man could be a scientist as well. Many people did not believe that there was any connection between the scientific investigation of disease and the treatment of the sick because there appeared to be no immediate benefit. Furthermore there was a feeling that many medical men were not capable of undertaking research because scientific work unfitted a man for a practical calling and the public often had no confidence in any but practical as opposed to scientific doctors. [4] Yet a scientific training could only enhance the skills of a medical practitioner.

In pathology, professional opportunities were slow to emerge and there were few positions until at least the second quarter of the nineteenth century. The earliest opportunities were posts as museum curators or demonstrators in morbid anatomy, preparing pathological specimens for teaching purposes. Although a number of lecturing posts and chairs in pathology were created from the fourth decade of the century onwards, Foster suggests that these were generally looked upon as stepping stones to honorary appointments as physicians or surgeons. [5] Burdon Sanderson, Roberts and Bastian all either began their careers or held early appointments in pathology and all follwed career paths where their pathological appointments were relinquished in favour of chairs in medicine or physiology.

These research interests and career moves suggest that as a medical discipline pathology was not as prestigious as other branches of medicine. Part of the reason may have been that pathology was founded upon morbid anatomy and histology and, certainly in Britain, the prevalence of the older style of anatomy had done much to stifle the growth of scientific research within medicine, particularly in the field of physiology. [6] Additionally, despite advances in pathology between 1830 and 1880, little demand was created for pathological specialists; pathological techniques were such that most medical practitioners could tackle them themselves. [7] Certainly when Beale called for funding for pathological research in 1878 his demands were modest. In only one London hospital was there suitable means for conducting scientific investigations of disease. [8] A mere £5000 spent in grants and fellowships would give definite benefits in a short space of years. Laboratories and work rooms for microscopical work, such as those found on the continent could easily be established in the better off London hospitals.

Even as late as 1885 Burdon Sanderson bemoaned the lack of professional opportunities in pathology. [9] It was still necessary to travel to the continent to study pathology and in Britain there were few research scholarships and permanent positions in pathology. He expressed the hope that further endowments would come into being as the medical profession began to recognise the value of pathological research. One way or another there was still nothing like a career in England for the scientific pathologist.

One very important factor which hindered the development of scientific medicine in Britain lay in the strength of the antivivisectionist movement. [10] Although evolutionary and naturalistic trends in other branches of science had by now done much to expunge the old natural theology from other branches of science, it still exerted a powerful grip in the domain of physiology. [11] In itself, this was an important reason for the scientific naturalists to promote a more modern and scientifically based form of medicine. The older style of physiology with its moralistic and theological overtones, demonstrating the glory of God through the wonder of the animal or human body or older notions of disease as punishment for the sins of intemperance were much more suited to the antivivisectionist movement's credo than was the new experimental style of medicine. This is demonstrated by the contrast in style between W.B. Carpenter's <u>A Manual of Physiology</u> which had been the standard textbook for many years, and the new <u>Handbook for the</u>

Physiological Laboratory edited by John Burdon Sanderson, Emanuel Klein, Michael Foster and T. Lauder Brunton which provided a focus for antivivisectionist attack. [12]

It is not hard to see why antivivisectionists should find this work offensive. The Handbook was published in 1873, only eight years after the last edition of Carpenter's manual, which had hitherto stood as the standard physiological textbook for many courses on physiology. The two books are very different in style. There is no mention of performing experiments on live animals in the earlier book, while there are descriptions of experiments on living animals all the way through the later work including diagrams of animals undergoing experiments in special holders. [13] To crown it all, although there are occasional references to anaesthetising animals in experiments, it is by no means clear, even to the well educated lay person, whether the animals in all the experiments described in the handbook were actually anaesthetised anyway. If the handbook was the bible of the new experimental medicine it was also very easy to read as a "Handbook for the Vivisection Laboratory".

Such a view was reinforced by the fact that in 1875, in his evidence to the Royal Commission on Vivisection, Klein stated that he never bothered with anaesthetics on animals unless for the convenience of avoiding bites and scratches. [14] One way or another the book represented a complete shift in the paradigm of physiology towards a modern scientific discipline. But science meant experiment, and in the case of both physiology and pathology it meant experiment on animals. Small wonder that the critics of vivisection should see themselves also as the critics of science itself; a science which apparently took no heed of animal suffering.

Of course the scientific naturalists saw that experiments on animals were necessary if medicine was to become a scientific discipline. Huxley had been an important part of the scientists' lobby to protect experimental medicine in the mid 1870s and to promote legislation on vivisection. This lobby also involved figures such as Burdon Sanderson, Simon, Sharpey and Foster. [15] Huxley was also the scientific representative on the Royal Commission on Vivisection in

1876. [16] Neither Frankland nor Tyndall were involved in the political aspects of the vivisection lobby but both were convinced of its importance. Frankland wrote to Playfair in 1875 on the presentation of the latter's bill to Parliament to express his indignation at the state of physiology in Britain in comparison to Germany and his concern that few physiologists would risk prosecution if the bill became law. [17] Huxley fought for scientific medicine through the lobby for vivisection; Tyndall fought for scientific medicine through the germ theory but he also realised the importance of animal experimentation. Speaking in 1876, he directly linked support for experimental pathology to the search for the causes of contagious disease:

> "... the actions of the various ferments upon the organs and tissues of the living body must be studied; the habitat of each special organism concerned in the production of each specific disease must be determined, and the mode by which its germs are spread abroad by further infection. It is only by such rigidly accurate inquiries that we can obtain final and complete mastery over these destroyers. Hence, while abhorring cruelty of all kinds, while shrinking sympathetically from all animal suffering - suffering which my own pursuits never call upon me to inflict, an unbiassed survey of the field of research now opening out before the physiologist causes me to conclude, that no greater calamity could befall the human race than the stoppage of experimental inquiry in this direction." [18]

Tyndall's support for the germ theory can be seen as one of the early steps along the road to the nascent sciences of bacteriology and immunology, the success of which was to form the ultimate crucial test for the antivivisectionists as the discoveries involved were undoubtedly based on animal experimentation. [19]

However the relationship between the medical profession and its attitudes towards scientific values began to change. Shortt suggests that only when the medical world began to see science as representing a form of expert knowledge which could be used to enhance status and emphasise expertise did it become important. [20] An example is the use of the rhetoric of science by the consultant elite who began to take on board scientific values after mid-century. [21] The reason for this was partly to gain autonomy from lay control, partly with the rising middle class concern with health and partly in a desire to demonstrate expert knowledge which the layman could not understand. Lawrence provides further insights into the employment of the rhetoric of science by the medical profession. He suggests that in the late nineteenth century physicians employed a vocabulary which invoked science as the foundation of medicine yet prescribed for it only a limited role in clinical practice. [22] Clinical skills were part of the gentlemanly attributes of a physician and the implication that medicine could be reduced to the rules of an applied science threatened their status as cultured gentlemen. Butler has analysed the introduction of experimental teaching into the medical curriculum. [23] Laboratory teaching was introduced in the 1860s and 1870s precisely as it was seen to be of value in teaching scientific observation and reasoning. These studies are in broad agreement with the results of the present study with regard to the take up of scientific theories, such as the germ theory, into branches of medicine, in this case in hygiene and sanitation. In other words it was indeed the methodology of experimental science which had begun to appear valuable to the medical profession.

Theories of Disease

In order to understand how the germ theory was disseminated and how theoretical explanations of disease changed through the 1870s and 1880s within the domains of sanitation and hygiene it is important to to examine briefly how concepts of disease evolved during the second half of the century within the area of pathology and bacteriology. The germ theory has been strongly linked to scientific medicine. It is therefore important to examine the appeal of different disease theories.

One popular theory in the first half of the nineteenth century was the idea of the active material of disease as a type of poison. Pelling suggests that the analogy between disease and the actions of poisons enjoyed a long history.

> "Since it was always an obvious analogy on empirical grounds alone, its terms could be used familiarly without implying very much. In the nineteenth century 'morbid poison' was used like an algebraic expression in some arguments, just as 'epidemic influence' or 'epidemic atmosphere', was in others." [24]

This was the sense in which Beale had used the term "poisons of disease" in his book, Disease Germs in the early 1870s. [25] He used it as a general term without implying that his readers should actually believe that the causes of disease were poisons. It was a short-hand term, used in much the same way as late nineteenth century physicists used the term "ether". A particular reason why Beale liked this term was because it avoided introducing germs and as it will be emphasised below, Beale did not believe in the germ theory. But this does not mean that the term had always been purely pragmatic; much useful work was achieved in the first half of the century on the pathology of disease based on the poison analogy. [26]

Another important analogy became popular towards the middle of the century, this time between fermentation and disease. In particular, Liebig's chemical theory of fermentation offered more scope for research into disease processes than the older poison theory. [27] Both William Farr and later Bastian were attracted to the theory; based on the analogy, Farr had coined the term "zymotic" or fermentative disase. [28]

Liebig held that some chemical substances were so unstable that they could enter into new forms under the contact of different bodies or catalysts. The large size of organic molecules made them unstable and prone to decomposition by this process of fermentation, putrefaction or decay. The theory was couched very much in terms of molecular actions or motions. [29] He believed that in the disease process a class of substance was developed during decomposition which acted on the body as a deadly poison and that this poison reproduced itself in the manner that yeast did when added to liquids containing gluten. [30] It is easy to see why Liebig's theory should appeal to Bastian whose ideas depended on physical and chemical rather than biological analogies to narrow the gap between living and non-living phenomena.

Liebig's was one of the first theories of disease which based itself on scientific theory and as its concentration was on the molecular level it was unlikely to attract the interest of those who wrote the sanitary handbooks as their interests lay much more in macroscopic phenomena such as ventilation and drainage. There is no mention of Liebig's work in any of the sanitarian works described later in the chapter. By the time Bastian declared his allegiance to Liebig's theory in the 1870s it was already almost thirty years old and becoming out of date in the face of mounting evidence for the connection of organisms with the processes of disease. The germ theory was also based on the analogy between fermentation and putrefaction but the analogy involved biology rather than chemistry, in that Pasteur had seen fermentation in terms of the reproduction of organisms rather than in terms of the reproduction of a chemical substance.

Bastian believed that disease was caused by a pathological change in state in some part of the body. Molecular changes could then bring about the generation of disease organisms in the body's tissues. The process of disease was, therefore, one of heterogenesis where the organisms were generated from the pathogenic matter of the living body. Under this view, bacteria were related to disease but they were the *effect* rather than the cause of disease. [31] Bastian did not deny that disease could be contagious but he thought that this manner of disease causation was far over-estimated by the germ theorists.

Chapter 5 described Beale's opposition to physicalist views of life in terms of his adherance to vitalism. In Chapter 6, his opposition to Tyndall's views were described. In fact, Beale seemed to disagree with almost everyone involved in the spontaneous generation debate as he opposed not only all forms of spontaneous generation, although he and Bastian did not engage in debate, but also what he termed the "vegetable germ theory of disease". [32] Although Beale offered much criticism to his contemporaries, on the positive side, he had developed his own theory of disease.

His view of the germ theory rested on his objection to the idea that the contagious particles of disease were germs of bacteria or fungi, in other words plant or vegetable organisms, which originated from outside the human body. He agreed that the active matter of disease was living but the disease particles derived from the body itself and were degraded forms of living human bioplasm. [33] "Bioplasm" was the term Beale used for living protoplasm or germinal matter which was capable of growth. Such disease organisms, once generated, acted as contagia and could infect others. Under his view, Beale suggested, there was more hope of eradicating disease than if the germ theory proved true. However his measures for combatting disease in terms of disinfection, preventing germs from entering the body and proper administration of food and liquid to invalids was quite traditional and compatible with the sanitary view.

The tendency of proponents of often very different disease theories to advocate the same measures of prevention is striking. Preventative measures were almost always seen in terms of traditional sanitarian techniques. This is clear in the examination of the sanitary handbooks. Actual preventative measures changed very little with the change in theoretical viewpoint. Both Beale and Bastian felt that their respective theories offered more hope for control of disease than if the germ theory were proved true. In particular Bastian felt that an emphasis on the germ theory might actually curb investigation of the true causes of disease.

> "But owing to its influence, in combination with the more generally received doctrines concerning the origin of life, there has gradually grown up an unwillingness in the minds of many to believe that these contagious diseases can arise *de novo*. And, this being one of those beliefs which tends to curb inquiry, and to check the possible growth of sanitary knowledge in certain highly important directions, it seems to me necessary to look with scrutinising care to its foundations - not only with a view to the advancement of medical science, but with the direct object of removing all checks which may exist to the growth of sanitary precautions against the origin of these most pestilential affections." [34]

There was a clear moral undertone in Beale's view of disease which again was very much in line with traditional sanitarian sentiment. Man in his ignorance of the proper laws of health was responsible for the production of disease germs. [35] It was as if the sins of the fathers were visited on succeeding generations and only when correct sanitary measures were implemented was there hope of eradicating disease.

For Beale it was an essential part of his theory that disease germs were not of a vegetable nature, but were of animal origin. He also emphasised the contagious nature of disease organisms much more than did Bastian. But apart from these differences there is a degree of similarity between Bastian's and Beale's views as both believed that disease organisms were originally generated in the body under the influence of exciting causes in the form of bad sanitary habits. Under Bastian's meaning of the term, Beale's view was none other than a form of heterogenesis although Beale would have strenuously denied that his theory had anything to do with spontaneous generation. Bastian's theory of disease does not appear in the sanitarian handbooks. Such was the influence of Beale that, although his theory did not achieve a great deal of popularity, it appeared as a serious contender for the germ theory in these publications in the late 1870s and early 1880s, as will be discussed below.

One of the most popular views of disease from at least the early nineteenth century onwards was the equation of dirt and disease, or as it came to be known, the pythogenic theory of disease. This theory held particular appeal to public health practitioners because it emphasised correct sanitary measures and the importance of public health programmes.

> "According to this theory, which was a dominant one both in medical circles and among the general public down into the 1880's, diseases arose spontaneously from the miasma, or effluvia, or noxious gases emanated by accumulated organic matter. Put simply, bad air from putrefying matter vitiated health and produced disease. [36]

This view was a more up-to-date version of the old "miasmatic" theory which had been popular in the first half of the century.

Pelling asserts that most medical men of the middle years of the century adhered to a form of "contingent contagionism" where diseases were understood to be caused by several different factors. [37] Recognising that such a term covers a broad spectrum of opinion, the results of the present study show that a form of contingent contagionism was popular from at least the end of the 1860s for nearly thirty years. By the 1870s the view of a large part of the medical profession was that although diseases usually spread by means of contagion, it was possible under some circumstances for diseases to arise *de novo*. This belief arose, quite naturally, through the observation that it was sometimes impossible to trace a prior case of infection. The emphasis was usually on the extremity of conditions which could cause a disease to arise; few medical men of the 1870s and 1880s believed that diseases were normally transmitted in this way. However to believe in the *de novo* origin of disease was not necessarily to believe in the possibility of spontaneous generation. Only if the active material of disease was regarded as living did this view support spontaneous generation.

The interest in the *de novo* origin of disease lies, for the present discussion, in its relationship to spontaneous generation. Both Tyndall and Bastian directly linked the two theories. Tyndall thought that such spontaneous origins of disease were completely antithetical to the germ theory. So when he chose to fight the cause of scientific medicine through an attack on spontaneous generation he was directly attacking the views of Bastian, but he was also indirectly attacking a weight of medical opinion, in particular the traditional sanitary view with its support for the pythogenic theory and forms of contingent contagionism. This presents a very good reason why the medical profession criticised Tyndall's views. His conception of the germ theory was insensitive to the many factors at work in the spread of disease.

The germ theory was purely a contagionist theory, although not the only one possible, as in the 1870s not every medical practitioner believed that contagia were living organisms, although by the 1890s it would have unusual not to have held such a belief. Tyndall explained disease only in terms of the germs of disease; other factors were not seen to be significant. The tendency of germ theorists to offer monocausal explanations of disease did much to make the germ theory unpalatable to the medical profession who were aware that disease processes were of a complex nature even if they could not be fully explained. Tyndall was reluctant to admit to predisposing causes within individuals which meant that he could not offer a satisfactory explanation as to why some individuals might succumb to epidemic disease and others not, other than in terms of "germ clouds". [38] Although Lister understood that the environment played a part in shaping and determining how micro-organisms would act, his adherance to the germ theory led him to underestimate the ability of healthy tissue to fight off infectious germs and so he overemphasised the importance of killing organisms, by means of the carbolic acid spray, before they entered wounds. [39]

The Sanitarians

The sanitarianism of the 1860s to 1890s, which is described in this chapter, had grown from earlier roots in the efforts of figures such as Chadwick and Southwood Smith in the 1830s and 1840s for sanitary reform. As Secretary to the Poor Law Commission, Chadwick's enquiries of the late 1830s into the living conditions of the poor had resulted in his Report on the Sanitary Condition of the Labouring Population of Great Britain in 1842. [40]

In 1858 the Privy Council became responsible for Public Health and Simon was appointed as Medical Officer. His style was very different from Chadwick's who had stressed sanitary engineering over medical knowledge. As a trained surgeon and pathologist, Simon recognised the need for systematic and scientific investigation in the public health domain. One of the most important achievements of his work was the annual reports he presented to the Privy Council from 1858-1871. [41]

In viewing his role as one where sanitary law was to be put on a scientific basis, Simon did much to advance the germ theory and to establish the link between cleanliness, ventilation and health. [42] After the cholera epidemic of 1866, the Royal Sanitary Commission of 1869-1871 followed by the Public Health Act of 1875 were the crowning achievements of Simon's career. [43] The act prescribed the sanitary measures which were to be employed by local authorities and also what the relationship between local and central government should be in these matters.

Simon presents an interesting figure in the world of sanitary reform in that he clearly saw himself as a scientist as much as a sanitarian. He supported the germ theory and was sympathetic to the causes of experimental medicine through vivisection. [44] Although he was the most eminent of the sanitary reformers of his day he did not adopt the moralising sanitarian style of many of his contemporaries, preferring instead to advance the cause of reform through science. The work of a man such as Simon, active in sanitary reform yet attuned to the values of scientific research, did much to smooth the path for the introduction of scientific theories such as the germ theory into the public health area. So although Tyndall was campaigning from outside the medical world, sanitarians already had an internal role model, in the shape of Simon, for scientific research in the service of public health.

When the younger generation of sanitary reformers became active in the 1860s, 1870s and 1880s, they were left the legacy of important public health legislation which now had to be enforced. Progress was slow; the overall mortality rate was the same in 1875 as it had been in 1850. [45]

The sanitarians, as a whole, were active from about the 1820s and 1830s until towards the end of the century. It is difficult to put a date to the start of the movement as such; earlier interest in disease and health reform could reasonably be included under the broad umbrella of sanitarianism. The zenith of activity and public visibility of the newer generation of sanitarian thinkers lay roughly from 1870 to 1890, just as the new science of bacteriology began to take over in explanations of disease. They were often Medical Officers of Health (M.O.sH.) or ex M.O.sH., other medical practitioners involved in public health at grass roots level, or teachers of public health, working in the 1860s, 1870s and 1880s and they tended to be active in sanitary organisations. The particular figures discussed in this study are Benjamin Ward Richardson, George Wilson, Arthur Newsholme and Edmund Parkes. Although Simon was something of an in-between figure, the sanitarians in general can be contrasted with the medical scientists and their allies of the same period, who were researchers and experimenters as well as teachers, such as Burdon Sanderson, Beale, Bastian, Roberts and Foster.

Lloyd Stevenson's "Science Down the Drain", although published over thirty years ago, is still the most comprehensive attempt to describe the sanitarian view. [46] He has characterised the "sanitarian syndrome" as one which rejected animal experimentation, the germ theory, compulsory vaccination and the Contagious Diseases Act and has suggested that sanitarians tended to be religious in temperament and in their zeal for cleanliness and moral purity emphasised the natural over the artificial. [47]

However there are flaws in Stevenson's arguments which the present study demonstrates. To start with, although it is often feasible to characterise an historical movement by examining only a few individuals in detail, Stevenson makes it appear that his three sanitarians, Benjamin Ward Richardson, George Wilson and William Job Collins, were representative of a wider body which displayed facets of the "sanitarian syndrome", which is not the case. Of his sanitary trio, only Collins, by far the youngest of them, displayed virtually all the aspects of the sanitary character.

My study has revealed that neither Richardson nor Wilson fit Stevenson's definitions well. The main problem is that Stevenson paints a picture of sanitarian hostility towards scientific innovations and, in particular, the germ theory and it is clear that neither Richardson nor Wilson, in common with their other sanitary colleagues, was hostile to science. This is apparent in the examination of the sanitary handbooks which follows. Like their colleagues in other parts of the medical profession, sanitarian practitioners would not take on board new theories without proper consideration but it is clear that the germ theory was discussed and assimilated over the 1880s and 1890s as the theory itself was perceived to increase in heuristic power.

Richardson, although he never accepted the germ theory, was willing to accommodate other scientific measures into the subject of sanitation. Wilson readily accepted the germ theory, even though he was critical of some of the failures of the new bacteriology.

Stevenson has painted a static picture of the relationship of a particular branch of medicine viz. sanitarianism to science. As this study shows with respect to Richardson, Wilson and the other sanitary figures, Parkes and Newsholme, there is a gradual accommodation of new scientific theories into existing practice. Sanitarianism can only be defined loosely. It is true that many individuals involved in sanitary work adopted a zealous style, emphasised natural over artificial solutions to health problems and proclaimed themselves to be more concerned with practice than theory, but like their colleagues in other branches of the medical profession, they gradually accepted scientific theories and values into their work.

Benjamin Ward Richardson

Benjamin Ward Richardson did not write textbooks on sanitation and hygiene but his views are nonetheless important as he was a self-styled spokesman for sanitarianism. Stevenson suggests that Richardson had one of the most fully developed cases of the sanitarian syndrome. [48] However Richardson will not fit readily into Stevenson's definitions. Firstly, he had the benefit of a scientific training which was relatively unusual amongst sanitarians, and secondly the breadth and sheer energy of his endeavours and writings made him much more of a public figure than other sanitarians, whose work can be discovered largely by the sanitarian textbooks which they wrote and by their activity in sanitary organisations. Richardson held as an important aim, the desire to bring to public attention the subject of public health and sanitation, and at least in terms of publicity, he achieved a considerable measure of success. He was a well known and respected figure in both the science and medicine as well as in the public domain. Therefore much can be learned about the sanitarian world view from an examination of his writings.

Richardson was active in setting up the Sanitary Review and Journal of Public Health in 1855 (which became the Social Science Review in

1862) and he presided over the Health Section of the Social Science Association in 1875. It was on this occasion that he delivered his most famous and almost legendary address "Hygeia: a Model City of Health". His declared intention was to bring the dry subject of sanitation to popular attention and in this he clearly succeeded as the address was printed as a leading article in The Times and was widely reviewed. [49] "Hygeia" painted a picture of ideal urban organisation guaranteed to eliminate almost all disease. In this respect it adhered to an ideal of progress through civilization without specific recourse to scientific intervention. Under this view it was only imperfect civilization which bred disease and ill-health.

For Richardson, the sanitary view encompassed a whole way of life. The preventative outlook of sanitary science tended towards a holistic system rather than the individualistic interventionist scientific method which treated the individual disease rather than the whole spiritual and physical person.

Richardson on Vivisection and Physiology

Even though he felt that progress was possible without scientific intervention, Richardson's background in scientific research made him not unsympathetic towards vivisection. Although he tended towards antivivisectionist sentiments in later years, he never completely rejected the value of animal experiment.

He was not, of course, sympathetic to painful experiments on animals, for their own sake, but his own experimental background, rare as it was amongst sanitarians, qualified him to understand the expediency of certain experiments especially with regard to anaesthetic trials. [50] He expressed regret that painful experimentation had tarnished the whole science of physiology, even though many physiologists never undertook such research. This was because painful experimentation had diverted the minds of physiologists from other types of more natural research. Writing in 1896 near the end of his life, he said:

"In the past fifty years physiology has become so narrowed in its progress, so bent on making discovery by vital experiment, and that alone, the world has ceased to think of its work by any other name than vivisection, and of its professors by any other name than vivisectors." [51]

Richardson called for a more humane physiology with "nature as the experimentalist and man as the observer and chronicler". [52] This illustrates what was the sometimes passive nature of the sanitarian view; the style of science advocated here was quite inimical to modern scientific work. He felt that there was almost unlimited scope for observing the external conditions of life and uniting this with anatomical investigations. Observation was emphasised over experiment. The universe was the laboratory and the physiologist was only cramped upon entering a laboratory of bricks and mortar. Nature inflicted pain, and it was the job of science to understand the workings of Nature and bring relief to that pain, rather than adding to it.

Essentially Richardson was not expressing a hostile attitude to science. He was expressing regret that physiology had spoiled its image and he was proposing an alternative, more humane and, by 1896, very old-fashioned paradigm for scientific research.

Richardson on Disease, Vaccination and the Germ Theory

In 1876, Richardson published Diseases of Modern Life, an exposition of his views on the causes and means of controlling disease. [53] He described communicable diseases as spreading by particles of organic poison which were always specific in nature i.e. each specific poison produced the same disease each time. This view of the materials of specific disease as organic poisons was well within the bounds of orthodoxy at the time, and was quite compatible with the beliefs of other sanitary scientists of that period. His views on the specific nature of disease contagia were also up to date. It was a central part of the germ theory that diseases were caused by specific organisms and that the organisms of one disease could not metamorphose into the organisms of another for that would be tantamount to heterogenesis. Richardson believed these diseases were controllable by vaccination, in the case of smallpox, and by destroying infected material.

> "The poisons of the communicable diseases are controllable. This is proved convincingly by one striking example, the control of smallpox by the process of vaccination. It is proved again by the success that has attended the attempts to stamp out the infectious disorders by isolation of the infected, and by prompt disinfection or destruction of articles of clothing which have been charged with the poisonous particles. But we have to wait for science to point out to us the precise nature of the poisonous particles, and how many are the varieties of them, before the triumph of control can be considered complete." [54]

Here was an example of the rhetoric of science providing the foundation of the subject but clinical practice remaining firmly within the domain of the experience of the medical practitioner. Science, therefore, was to provide the underlying theory as to the nature of disease particles and so Richardson saw sanitary science as resting on the basis of scientific medicine. But the actual practices to control the spread of infectious disease were the traditional ones of smallpox vaccination, isolation and disinfection, in the application of which public health practitioners were experts.

Despite the fact that he looked to science to provide the ultimate explanation for the nature of disease particles he never accepted the germ theory. In 1876, he claimed that no one had ever seen a germ and so there was no way of defining the difference between germs of specific diseases; furthermore there was no explanation as to why these germs did not always kill their victim once they started to propagate in the body. [55] These were common criticisms of the germ theory at that time, from all quarters, which pointed to quite serious problems with the theory and its relationship to observables which its exponents were seldom able to counter. Bastian adopted a very similar position in his criticism of the theory.

Yet twenty years on, at a time when most sanitarians and the medical profession in general had accepted the germ theory, Richardson still complained that he had never seen a germ and that he did not see how germs could grow into bacteria - his organic particles of twenty years earlier. He also criticised the analogy of plant or animal growth which he saw as the basis of the theory. [56] Such views were out of date when he expressed them at the end of the century.

But Richardson's late denial of the germ theory was not typical of sanitarian feeling. As the next section shows, sanitarians happily accepted the germ theory as evidence for it was forthcoming over the years. However, it was suggested earlier that Richardson was the self-appointed publicist for sanitarianism. If his views on the germ theory are taken as representative, which they were not, there is a danger of seeing the sanitarian position as more extreme than it actually was.

Handbooks of Hygiene and Sanitation - An Introduction

Richardson's views have been discussed with regard to his popular writings, but it is handbooks of hygiene and sanitation which can offer a more detailed appreciation of the changes in theoretical views of the sanitarians over the years, particularly with regard to theories of disease. A close examination of widely read works in this domain, especially editions of the same work spanning a number of years, shows these changes very clearly. Thus it can be seen how medicine became "scientific" in adopting the germ theory of disease, in the domain of sanitary science and public health. The other important part of medicine where theories of infection had an important impact was antiseptic surgery and this will be discussed at the end of the chapter.

There are two means of examining how far scientific theory was incorporated into sanitation and hygiene; firstly by describing and understanding changes in sanitary theory *per se* and how far and where the germ theory and new bacteriological research were incorporated into theories of disease; secondly by showing how far work in science of both a theoretical and experimental nature impacted the sanitary writings, and particularly with reference to Bastian's and Tyndall's work purely in the practical measure of "purification of water".

These two aims were by no means mutually exclusive. Clearly Tyndall's and Bastian's experiments had implications for both the narrow concerns of water purification *and* the nature of disease theories.

The works under consideration are, Arthur Newsholme's Hygiene (1892 and 1902 editions), George Wilson's A Handbook of Hygiene (1st edition in 1873, then 1879, 1883, 1892 and 1898 editions) and E.A. Parkes' A Manual of Practical Hygiene (1st edition in 1864, then 1878, 1883, 1891 editions). [57] All three were textbooks and may be taken as popular representatives of the state of sanitary knowledge over the period under discussion, i.e. about 1870-1895. Newsholme's book was a later work which appeared after the germ theory was established, although interestingly he was the least up to date of the three writers. The other two works had "pre" and "post" germ theory editions.

Newsholme's work was designed for medical students and candidates for diplomas in public health and sanitary science. [58] Stevenson describes Wilson's book as "one of the pioneer handbooks of hygiene in England, for many years the standard manual for candidates for certificates or diplomas in public health at English universities. [59] Both Newsholme and Wilson were Medical Officers of Health for a number of years and both held executive posts on the council of the Sanitary Institute. Stevenson suggests that Wilson displayed many of the symptoms of the "sanitarian syndrome". [60]

E.A. Parkes was the Professor of Military Hygiene at the Army Medical School and Emeritus Profesor of Clinical Medicine at University College. The fifth, sixth and seventh editions (covering the late 1870s and the 1880s) were edited by F.S.B.F. de Chaumont, Parkes' successor to the chair of Military Hygiene while the eighth and subsequent editions (from 1891) were edited by J. Lane Notter, the subsequent incumbent of the chair. This work was aimed at the military aspects of the subject although there is much in common with the other works. A Royal Commission had been formed in 1857 to enquire into the sanitary condition of the army in England. The new regulations which resulted gave the Army medical officer an official position with respect to sanitation and hygiene and hence the work was produced as an attempt to carry out the wishes of the commission in regard to sanitary science and so it was designed for those attending the Army Medical School. [61] All three works can be regarded as textbooks for intending M.O.sH. and diploma students, military or civilian.

Tyndall's and Bastian's Experiments - Purification of Water

Tyndall's and Bastian's experiments on spontaneous generation implied relatively few immediate practical connotations except for the obvious one of the possibility of purifying or sterilizing water by boiling. The sanitary handbooks reveal how far science penetrated into medical practice in this respect. The first edition of Parkes' manual, in 1864, contains a single page on boiling water to remove organic and inorganic impurities, but this is expanded very substantially to nine or ten pages in subsequent editions. [62] The 1878 and 1883 editions discuss the uncertainty of whether bacteria are killed by boiling and this is augmented by a later discussion on death points in the disinfection chapter. [63] A footnote in the purification by boiling section of the 1878 edition discusses the apparent resistance of bacteria or germs in terms of a "*spheroidal*" condition and refers to Burdon Sanderson's experiments. [64] In the 1883 edition Burdon Sanderson remains in the footnote and the "spheroidal" discussion is replaced by a description of Tyndall's discontinuous boiling experiments which appear in the main body of the text. [65] Finally, in the 1891 edition, the section on the uncertainty of the boiling process disappears, the Burdon Sanderson footnote disappears and Tyndall's experiments become the central argument of the section on purification of water by boiling. [66] This is a very good example of the incorporation of new knowledge. The possible resistance of bacteria germs is at first uncertain and over a period of thirty years Tyndall's experiments gradually become the central argument while uncertainty over the resistance of bacteria disappears. But the practical measures of boiling water to remove organic impurities do not change substantially. A theory becomes available to explain why boiling occasionally results in failure and to offer a modified practice to remove these failures.

Tyndall's work is referred to in three other places in Parkes' manual. His light beam test for germs in water is referred to in the 1878 and 1883 editions though not subsequently. [67] As Beale had predicted, Tyndall's light beam test did not become popular. It was a new practice, based on the germ theory, which offered no benefit over other established techniques particularly as microscopical techniques improved. There was therefore no reason for the practically minded sanitarians to adopt it. The 1878 edition asserts that the development of germs or oval cells into bacteria was not definitely proved, while the 1883 and subsequent editions adduce Tyndall's cultivation experiments as evidence of the spores of bacteria in the air. [68] Finally reference is made to Tyndall's boiling experiments again in the chapter on disinfection, where the main discussion of disease theory appears in the work.

Newsholme's 1892 edition alludes to Burdon Sanderson's and Dallinger's experiments on the death-point of bacteria while noting that spores might be more resistant than adult forms. [69] It is only in the 1902 edition that a short description of Tyndall's work on discontinuous boiling appears, displaying a lag of well over 20 years since the original experiments. [70] In the light of what Parkes' manual had to say about Tyndall's experiments and sterilization in the 1880s, Newsholme was surprisingly slow in accommodating this work. Wilson's handbook makes no mention of Tyndall's boiling experiments.

Speculation about the *de novo* origin of bacteria - Tyndall and Bastian

In Parkes' manual, from 1878 onwards a chapter on disinfection discussed the possible different views of the living nature of contagia in terms of (a) animal bodies originating and growing in the body, in other words, Beale's theory; (b) fungoid particles; (c) bacteria from outside the body, or the germ theory. [71] The problems of observing bacteria, the fact that they were known not to be the cause of some diseases and their very universality were evidenced against the view that they constituted the specific contagia. [72] Watkins suggests that Parkes, himself, originally believed that disease was spread through specific poisons which were chemical substances but by the 1870s he began to recognise the role played by bacteria although he always believed that bacteria were the carriers of contagia rather than contagia in themselves. [73]

The 1878 edition of Parkes' work contains an interesting speculative section, absent in similar works, on the bearing of the spontaneous generation controversy on the question of disinfection and the nature of contagia. This is worth examining in detail because it gives an insight into the perception of the sanitary establishment as to the importance of Tyndall's and Bastian' work in a more global and theoretical sense as opposed to the pure experimental sense detailed in the section above.

> "The belief in the part played by Bacteridia has led also to much interest being taken in the discussion on ferments, and in the question of spontaneous generation, as it is imagined that a clue might thus be found to the origin, *de novo*, of the contagia. Mr Darwin's doctrine of Pangenesis has even been pressed into the discussion, though it rather makes the darkness greater than before. It is curious to find so practical a matter as that of disinfection brought into relation with some of the most subtle and controverted questions of the day; but the important bearing which the acceptance of one or other of these views would have on the practice of disinfection is evident." [74]

As a footnote, the discrepancy between Tyndall's and Bastian's experiments was commented upon. [75] In the 1883 edition the discussion of their work is moved to the main text. [76] In the 1883 and 1891 editions the discussion of pathogenic organisms and their relationship to disease is extended, with reference to Koch's demonstration of the link between phthisis and *bacillus tuberculosis*. [77] The speculative section on spontaneous generation entirely disappears in the 1891 edition. From a situation where three theories compete and there is speculation over spontaneous generation and contradictory experiments, one theory emerges as dominant reinforced by new bacteriological work. Speculation on the Tyndall-Bastian controversy disappeared because debate had moved away from spontaneous generation by 1890 and the resolution of that controversy was no longer of interest.

In the 1891 edition, Tyndall's work is firmly lodged in the Disinfection chapter at the begining of the section: "Effects of Heat as a Disinfectant".

> "Tyndall was the first to point out that, whilst boiling failed to sterilize an infusion, successive heatings for a short time, even below the boiling-point, were successful. The explanation proposed is, that during the period of latency the spores are in a hard state capable of resisting high temperature, but just before the period of active germination, they become softened, and therefore amenable to the influence of heat. As, however, spores in various stages may exist in the same fluid, successive heatings are necessary so as to arrest each group at the proper time, but by repeating the heatings sufficiently often an infusion may be sterilized at a point below the boiling-point of water. This method of intermittent heating is now in general use for sterilizing cultivating fluids. Important in all ways, this question of the nature of contagia is especially so in a practical sense, viz., that of the easy or difficult destruction of these agents." [78]

Quite remarkably Tyndall's work had furnished not only an accepted explanation but also the means to suggest new techniques in public health/bacteriological work. Some of his suggestions, such as the use of respirators and more especially his light beam test never became practical techniques. But his method of discontinuous boiling, which was a modification of an existing technique became the dominant method of sterilizing water. Although the editors of Parkes' manual were perhaps unusually up to date in their reference to Tyndall's work in 1878, it was not until the early 1890s that Tyndall's work was accepted as one of the main theoretical and practical sources in this area of sterilization. This is not unexpected, partly because it takes a finite time for new discoveries to be incorporated into an existing body of knowledge, but more especially in this area because a full appreciation of Tyndall's work was only possible within the context of a more advanced knowledge of the life-cycle of bacteria and their relation to the causation of disease.

In Newsholme's 1892 edition he indicated that there were strong reasons for regarding contagia as microscopic organisms, although he adhered to rather outmoded terms "malarious poison" and "the poisons of typhus fever". These terms are absent in the 1902 edition where a definite statement that the contagia of the infectious diseases are bacteria appeared. [79] Again, Newsholme was apparently not as up-to-date as the editors of Parkes' manual.

Newsholme, Epidemic Constitution and the Germ Theory

However, it would be wrong to conclude from Newsholme's work that his theoretical position was much more old-fashioned than that of his contemporaries, particularly as he was younger than Wilson and Parkes and had received his medical education in the 1870s.

Looking back, in 1935, over his long career in public health, Newsholme reviewed the progress of the germ theory. He suggested that the concept of "epidemic constitution" had, for some time, materially biased his own views and those of modern sanitarians. [80]

> "When in the years rapidly following 1875 the germ theory of disease became established, involving not only the notion of contagia but also that of a probable specific contagium for each infectious disease, the early impulse was to reject altogether the conception of "epidemic constitution". But this did not happen; for the germ theory failed to explain why at long intervals diseases like smallpox or influenza swept round the world." [81]

The monocausal explanation of disease furnished by Tyndall's view of the germ theory was not borne out by the practical experience of public health practitioners. Newsholme had, in fact, undertaken a laborious set of investigations on rainfall and the diseases of rheumatic fever and diptheria and had demonstrated an inverse relationship between rainfall and disease. He suggested that medical opinion had gradually come round to a more acceptable intermediate position.

> "But in order to realise the gradual process of enlightenment we must recall the intermediate stage between the blind acceptance of the unknown influences included in "epidemic constitution" and the - perhaps too limited - bending of the knee to the acceptance of specific infectivity, untempered by consideration of environmental factors other than infection." [82]

Despite the fact that Newsholme was slow to introduce the germ theory into his hygiene handbook, his description of his student years, in the 1870s, reveals that the germ theory was the subject of much discussion and he, himself, kept abreast of the developments in the spontaneous generation debate and their importance for the acceptance of the germ theory. In 1878, as a medical student at St. Thomas's Hospital, when, as he described, the germ theory was already gaining ground, he read a paper to the Students' Society entitled, "The Origin *de novo* of Zymotic Diseases". [83]

> "The two theories of the genesis of epidemics then prevalent were the germ theory, and the physico-chemical theory, and these were intermingled with the controversy then being pursued as to the possibility of "spontaneous generation" of living matter. Evidently the proof or disproof of Bastian's theory of abiogenesis had direct bearing on the medical problem; and Tyndall's investigations on dust and Pasteur's demolition of Bastian's views were reviewed in my paper with zest. In 1878 the opposing views were regarded as still debatable." [84]

In his paper Newsholme rejected numerous hypotheses, deciding instead in favour of the germ theory which "was supported by numerous facts and analyses." [85]

<u>Changes in Disease Theory - Wilson's Handbook as an example</u>

Wilson's handbook made very little reference to Tyndall's experiments. For the present discussion its interest lies in what it reveals about the sanitarian view of moral purity, vaccination and a gradual acceptance of the germ theory. Furthermore it provides an interesting and revealing account of the change in disease terminology over a period of about thirty years from the mid 1860s.

In the first edition of his book (1864), Wilson discussed the influence of hereditary factors on disease. In itself this was quite orthodox if seen in terms of the traditional separation of causes of disease into predisposing (often hereditary) causes and exciting causes. He betrayed an enthusiasm for Galton's work in this respect, but used this work in terms of "deterioration" of the race and to issue a powerful invective enumerating the social causes of deterioration and disease. [86]

> "Intemperance, immorality, injudicious marriages, excesses of every description, overwork, idleness, depressing passions, may be enumerated as the most disastrous. All of them tend to impair the constitution of the individual, or the well-being of the offspring, and in proportion to their prevalence they lower the standard of public health." [87]

This is sanitarian moralising at its best. Although later editions contain a lengthy historical introduction on public health, the explicit hereditarian overtones and extremes of moralising fervour of the first edition were absent.

It is in Wilson's work where the gradual modification and accommodation of new views was best displayed. Often only single words or phrases were changed rather than a paragraph or section revised. This is shown in his description of the disease agents of enteric fever. The 1873 edition contained the sentence, "The sewer-air, laden with specific poison, readily finds its way into houses...". [88] In the 1879 edition, "specific poison" became "morbific ferments", which evolved into "morbific ferments or contagia" in 1883, "morbific ferments or microbes" in 1892 and finally "pathogenic organisms" in 1898, with no change in the surrounding sentence and very little change to the adjacent text over the intervening years. [89] It is as if perceptions of the nature of disease changed with no change in the underlying mechanisms of disease. Wilson's use of terminology mirrored very clearly the changes in the general terminology of disease from poison to ferments, with the relationship of contagious diseases to the processes of putrefaction and then finally on to the idea of some sort of organic particle in terms such as "microbes" and "organisms", as the germ theory is gradually accepted. The underlying cause of the disease, namely the entry of sewer-air into houses is not changed and neither is the implied practice, namely to prevent this substance from entering dwellings, in the first place. Once again, practice does not change but gradually accommodates new theoretical advances. There is no revolutionary shift in theory, rather the gradual assimilation of new concepts.

Wilson added the term "microbes" to his morbific ferments in 1892 suggesting that by that stage he had accepted the possibility that the agents of disease were independent organisms. His "pathogenic organisms" with no reference to either the fermentation or poison analogy in 1898 shows that his conversion to the germ theory was by then complete.

Wilson's spirit of accommodation and gradual acceptance is apparent all the way through the different editions of his book. Earlier editions of the work betray an enthusiasm for Pettenkofer's theory on the relation between soil humidity and the spread of disease, but of course by the 1898 edition he had accepted the significance of Koch's work. However the old theory was not abandoned; it was retained in the guise of predisposing causes and thereby the new theory modified and was accommodated into existing practice. The existing sanitary concern with damp houses still had to be dealt with to check the spread of tuberculosis. In other words, the bacillus was seen as a necessary but not sufficient cause of the disease.

> "While there can be no doubt that soil dampness and damp dwellings still play an important part as predisposing causes in the prevalence of phthisis, the discovery of Koch's tubercle bacillus, and the now generally entertained belief that the disease is essentially specific, and is spread by infection or the ingestion of the milk or flesh of tuberculous animals..." [90]

<u>Wilson and the Germ Theory - a Sanitarian is Converted</u>

In common with the other sanitarian handbooks, it is the chapters on disease theory that changed most over the years, and in Wilson's case revealed in detail his conversion to the germ theory. In 1873 he suggested that despite the mystery surrounding the nature and origin of infectious diseases, their communicability was undoubtedly due to sanitary defects and could be greatly controlled by proper precautions. [91] By 1879 Wilson was prepared to discuss in more detail the aetiology of disease and, in particular his scepticism toward the germ theory.

> "According to the germ or parasitic theory of infectious diseases, the origin *de novo*, of a fever poison is as impossible as the spontaneous generation of plants or animals; the inference being that enteric fever, for example, can only be developed from the specific contagium of the fever... Now, to this it may be replied that the poisons of all the acute specific diseases must have originated at one time or another independently of pre-existing cases, and there is no reason to believe, therefore, that the causes which led to the development of the first cases should not be in operation at the present day... it is a matter of almost daily observation that pyaemia and puerpural fever are not only generated *de novo*, but the researches, more especially of Dr. Sanderson, show that they can be generated at will, and when so generated they become, under certain circumstances, eminently infectious. It is no argument, therefore, that because a disease is infectious it cannot be generated *de novo*." [92]

A number of points require further discussion in the above paragraph. Firstly there is the question of the same causes which brought about the first diseases still being in operation, in other words an argument from the principle of continuity. This was an argument of which Bastian too was fond and it was undoubtedly a problem for scientific naturalists and other evolutionary thinkers. Much of the new evolutionary outlook on science was based on such a principle, both in the biological and geological worlds and hence there was an obvious contradiction for the scientific naturalists, a contradiction which afforded the opponents of the germ theory considerable mileage. The second point is that, in common with many other members of his profession, Wilson declared his commitment to contingent contagionism. Such a belief was reinforced, not only by practical experience, but also by the results of a series of experiments undertaken by Burdon Sanderson in 1872. [93] In these experiments Burdon Sanderson had injected a small quantity of an apparently germ-free chemical irritant into the subcutaneous tissue of a rabbit. This experiment, and others with different chemicals, produced a form of septicaemia known as "Pasteur's septicaemia". A plausible explanation of this experiment was that non septic organisms had become pathogenic under the influence of the inflammatory process, in other words this was potentially a form of heterogenesis.

To the same section in the 1883 edition, Wilson added a discussion of competing disease theories and covered exactly the same three theories that Parkes' manual had discussed - disease germs originating from within the body, fungoid particles and bacteria from outwith the body. Like Parkes he argued that the universality of the various micro-organisms militated against the view that they were the contagious particles of disease. [94] Wilson's use of the same arguments as the editor of Parkes' manual at the same period gives a picture of the early 1880s as a time when medical theory was at last coming to grips with the idea of disease contagia as organic and indeed living particles. There was no real revolution in the disease theory over the whole period in question, but if there is any sense of a revolution at all, it must have taken place around the early 1880s. The "normal science" of the fermentation/poison paradigm had begun to break down due to the "anomalies" uncovered by new bacteriological work and better microscopical techniques. Three theories emerged as contenders out of the germ theory camp and eventually the bacteria from outside the body theory triumphed as the theory offering most promise for future work. [95]

In the 1892 edition of Wilson's handbook, the germ theory is finally accepted.

> "...'the germ theory of disease',... in the light of recent researches, may now be said to have reached the stage of positive demonstration." [96]

Raymond has suggested that given that public health work was poorly paid and lacking in status, M.O.sH. who used the germ theory and bacteriology in their practice were trying to raise their status and extend their power. She also disagrees with Stevenson's analysis of the relationship of sanitarians and science. "..they used the rhetoric of science in the 1860s and 1870s to support a progressive expert image and an interpretation of germ theory which gave a more compelling rationale for existing practice." [97] The results of the present study are broadly in agreement with Raymond's analysis, except they suggest that acceptance of the germ theory and the values of science were much more apparent in the 1880s and later, rather than in the 1860s and 1870s. Like other parts of the medical profession, sanitarian practitioners began to see the value of science as providing a means of improving autonomy and status. One striking indication of the favourable light in which sanitarians were beginning to see science is indicated by the change of title of Wilson's handbook. It was originally entitled, Handbook of Hygiene, but in its 5th edition, published in 1883, the title had become Handbook of Hygiene and Sanitary Science. [98]

Conclusions

This chapter has examined the relationship of scientific knowledge and medical knowledge through the impact of the spontaneous generation debate and the germ theory on hygiene and sanitation. Stevenson's view that sanitarian feeling was inimical to science has been shown to be an exaggeration. Like their counterparts in other branches of the medical profession, sanitarians appreciated that science gave them access to expert knowledge which could be used to enhance the status of their work even though their clinical practice remained much the same.

The sanitarian handbooks reveal that the germ theory and sterilization techniques based on Tyndall's work were well accepted by the middle of the 1890s and in some cases well before the 1890s. Older theories were replaced gradually. In the period from the early 1870s to the late 1890s there is little change in overall structure of all the works examined and much of the text remained the same. A word is changed in a sentence and a sentence is added or removed rather than any large scale revisions. There is no feeling of radical opposition or unreasoned resistance to the new scientific theories.

More than one area of medicine felt the impact of the spontaneous generation debate. In the field of antiseptic surgery the impact was less direct, but both in pathology and in the domain of sanitation and hygiene, the impact of the spontaneous generation debate and discussions of the germ theory were far more obvious. In assisting at the birth of scientific medicine the spontaneous generation debate acted as the midwife to the new science of bacteriology.

CONCLUSION

This thesis has examined the debate over spontaneous generation which took place in Britain in the 1870s. The main claim is that support for scientific medicine through the germ theory and an attack on spontaneous generation were part of the scientific naturalists' programme to spread the naturalistic world view and to gain cultural leadership. As part of their aims to spread scientific naturalism, it has been shown that the scientific naturalists fought spontaneous generation because it posed a threat to a number of fundamental tenets of the naturalistic world view including evolution and the origin of life, fundamental units of life, naturalistic explanations and the narrow dividing line between naturalism and materialism. Further to this, this thesis has demonstrated that the spontaneous generation debate in promoting discussion of the germ theory and the scientific investigation of disease organisms, played an important part in the introduction of scientific concepts into medicine in the 1880s.

Chapter 2 showed how the scientific naturalists' claims for the power of science can be seen as an attempt to gain cultural hegemony at the expense of traditionally powerful groups, particularly the clergy. The replacement of theological explanation by naturalistic scientific explanation was part of the argument that the promulgators of this knowledge should be the new cultural leaders.

Chapters 3 and 4, in describing the detailed progress of the debate, have shown how the involvement of the scientific naturalists, in particular Tyndall, can be seen in terms of his campaign for the germ theory and his concern for the state of scientific medicine. The debate itself was carried out mainly in terms of the physical conditions of the experiments. In particular, much effort was expended in establishing a death-point for all micro-organisms in solution. Some observers began to suspect that the contradictory results obtained in these experiments could not be explained away purely by errors and there gradually arose a feeling that more needed to be known about micro-organisms themselves. This was confirmed by the work of Roberts, Cohn, Dallinger and Drysdale. After the middle of the decade, the experimental paradigm of the spontaneous generation debate began to move more towards a bacteriological approach, focusing on the organisms themselves and their life-cycles. Tyndall was able to apply this new approach in his discontinuous boiling experiment. Bastian, of course, never accepted the validity of this work. By the end of the decade, although the germ theory had a long way to go before becoming universally accepted, spontaneous generation had gained little support. The focus of interest in the medical world had moved onwards towards bacteriological researches and Bastian's style of work could not be accommodated within the new bacteriological paradigm.

Chapter 5 demonstrated the threats to the naturalistic world view which the subject of spontaneous generation had posed. The debate brought into focus the question of the origin of life. Darwin's theory implied that life must have originated by a process of spontaneous generation. On the one hand the naturalists believed that the concept of spontaneous generation could not be explained in naturalistic terms, but on the other hand, to accept that spontaneous generation took place in the past but did not take place in the present involved a violation of the principle of continuity, one of the central tenets of scientific naturalism. Tyndall tried to avoid the problem by his belief in the nebular hypothesis and a form of pantheism which saw matter as suffused with the potential for life. Huxley admitted that some form of spontaneous generation must have taken place in the past but denied that Bastian's experiments were present day instances of such a process. Spencer's conception of evolution, as it applied to the whole spectrum of inorganic and organic matter, lent itself to a consideration of spontaneous generation. But he, himself, denied the possibility of such a process on the grounds that even the simplest organisms are so complex that it was incredible that they could evolve from lifeless matter in a few hours.

The interest in protoplasm as the fundamental basis of life, which Huxley had done so much to promote, had also proved instrumental in paving the way for consideration of spontaneous generation. By reducing life to mere protoplasm and vital forces to chemical and physical forces, by emphasising the similarity between living and crystalline matter, the scientific naturalists were not only excluding an element of caprice from the natural world and thereby emphasising the importance of scientific explanation, but also they were also unwittingly lending Bastian's materialistic view support by implying that as living matter was essentially simple, the gap between living and non-living matter was potentially bridgeable.

The scientific naturalists did not want to see their fight against spontaneous generation damage the cause of evolution, particularly as Pasteur's refutation of spontaneous generation had been viewed as a refutation of evolution by several French scientists. Therefore they wished to dissociate the fates of the two doctrines and in this aim they were largely successful. The fact that medical issues were more prominent in the British debate than in the French debate helped deflect interest away from evolution. When the spontaneous generation controversy erupted in the early 1870s, the recent cholera epidemic made it clear that whatever other benefits science had brought to humanity, it was still almost powerless in the face of such an epidemic disease. Furthermore, contemporary medical explanations of disease in terms of epidemic or atmospheric influences or a predisposition of the individual and the belief that diseases could spring up *de novo* under certain circumstances were antithetical to the alternative beliefs of the scientific naturalists where, as in other areas of application, they offered explanations in terms of phenomena obeying natural scientific laws. The germ theory was just such a scientific theory which explained the spread of disease by means of prior cases of disease, in terms of the transmission of germs of disease-causing bacteria. The application of this theory in Lister's antiseptic system was seen as a vindication of the power of science.

In Chapter 5 it was shown that Tyndall fought against non-scientific explanations in medicine. By eradicating explanations in medicine which involved vague influences, an element of chance or caprice in the spread of disease, and causes which could not be explained scientifically, medicine would at last become a science rather than an art. The enhanced effectiveness which would accrue from scientific medicine would be testimony to the power of science and thereby enhance the status of the purveyors of scientific knowledge. Yet, as Chapter 6 described, Tyndall's campaign for scientific medicine through the germ theory excited much criticism from the ranks of the medical profession. Part of this criticism came from some of the older members of the profession who felt that Tyndall's view of the germ theory was based on ignorance of true medical knowledge. Lionel Beale made many criticisms of Tyndall's work. As a vitalist, he was opposed both to scientific naturalism and materialism. He opposed mechanistic views of life including Huxley's conception of protoplasm and Bastian's spontaneous generation. He was especially scathing towards Tyndall's "searching beam" particularly as the latter had implied that such a technique was more powerful than the microscope. Basically it was the whole approach of so-called "physicists" in reducing life to mere protoplasm, reducing vital forces to chemical physical forces and promoting physical techniques over microscopical techniques to which Beale objected.

Much of the criticism which Tyndall's advocacy of the germ theory excited was based on more specific concerns in the domain of pathology. The work of William Roberts and John Burdon Sanderson in this field shows that both were unwilling to accept the germ theory until well into the 1870s when more evidence for its validity was available. In particular Burdon Sanderson maintained his agnosticism with regard to the germ theory until the end of the decade despite the fact that, some ten years previously, he had concluded that the active agent of disease consisted of living particles. Chapter 6 described the research of these two medical scientists and also Bastian's early work in pathology. All three arrived at an involvement in the spontaneous generation controversy through their interests in this field of medical science.

The success of Tyndall's campaign for scientific medicine through his opposition to spontaneous generation and his advocacy of the germ theory is measured in terms of how far the medical profession was persuaded to take on board new theories and practices suggested by developments in physiology and pathology and the new science of bacteriology. Chapter 7 explored these questions with reference to the domain of public health. This chapter demonstrated that scientific theories, and especially the germ theory, were gradually adopted into the domain of public health in the form of new explanations while existing practice stayed essentially the same. Effectively a change in theory took place without a corresponding change in practice. As sanitarian and public health handbooks show, changes in disease theory took the form of a gradual accommodation with little sense of a revolution in explanation. The change took place largely in the 1880s in the decade after the spontaneous generation debate.

If there were few perceived ameliorations to be offered by science, what then were the reasons why medical practitioners accommodated scientific explanations into medical theory, particularly in the domain of public health? The answer lies in two directions. Firstly, as was shown in Chapter 7, it was possible to claim that scientific knowledge offered a form of expert knowledge and expert knowledge offered power. It was, at least partially, testimony to the success of the scientific naturalists' efforts to increase the status of scientific knowledge, that scientific knowledge was increasingly seen as true, exact and expert knowledge. Clinical practice could now be put on a theoretical foundation in terms of scientific explanation of tried and trusted methods. This was something more than employing the rhetoric of science; science could be used increasingly to explain why clinical practice was effective and thereby to enhance the authority of its practitioners. The practice itself, in remaining the same, could still be seen as an art, only to be learned through years of experience. Public health practitioners were responding to the offer of expert knowledge which science promised. In this respect their response was akin to other branches of the medical profession. In the domain of public health, new bacteriological work which was undertaken in the decade after the spontaneous generation debate increasingly offered new and scientific explanations of disease.

Secondly, this is not to suggest that older theories of disease, in terms of influences or poisons, were not of themselves plausible explanations of disease. The change taking place can be seen as the process of adopting a new scientific paradigm. One factor in choosing between competing paradigms is the question of which paradigm offers most scope for future research. The new science of bacteriology offered the possibility of isolating and identifying the different disease organisms and the production of effective vaccines. In adopting these new areas of expert knowledge the medical profession could extend its domain and improve its status.

The final question is whether or not the spontaneous generation debate and Tyndall's campaign for a scientific medicine did in fact significantly affect the growth of scientific medicine in Britain. In a sense it is always impossible to tell how events would have occurred had some historical variable not been present. Advances in pathology and physiology due to figures such as Foster and Sanderson, and the nascent science of bacteriology with the discoveries of Pasteur, Koch and Cohn would all have influenced the medical world without Tyndall. However it is fair to say that he did much to bring the work of the last three to the attention of the British scientific and medical communities and his campaign for scientific medicine can be seen as very timely. However Tyndall's work and the spontaneous generation debate can be seen as a much more important factor than purely a publicity campaign.

The spontaneous generation debate took place in a pivotal decade between the medical knowledge of the 1860s and the very different style of medical knowledge in the 1880s. In the 1860s, disease theory was bound up with the idea of poisons, epidemic and meteorological influences and theories of disease such as the pythogenic theory and the physico-chemical theory. By and large, except for the work of individuals such as Simon and Budd, disease was not generally a subject for scientific investigation. In the 1870s the debate over spontaneous generation and the germ theory provided a focus for the scientific investigation of micro-organisms and implied that the questions formulated during the debate could be seen to apply, by analogy, to infectious diseases. This was particularly important when the debate itself shifted from a physical approach towards a bacteriological approach which concentrated on studying the properties of the organisms themselves. In providing a forum for the scientific investigation of micro-organisms, the spontaneous generation debate of the 1870s formed an important bridge from the medical knowledge of the 1860s to the new bacteriology of the 1880s.

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APPENDIX A

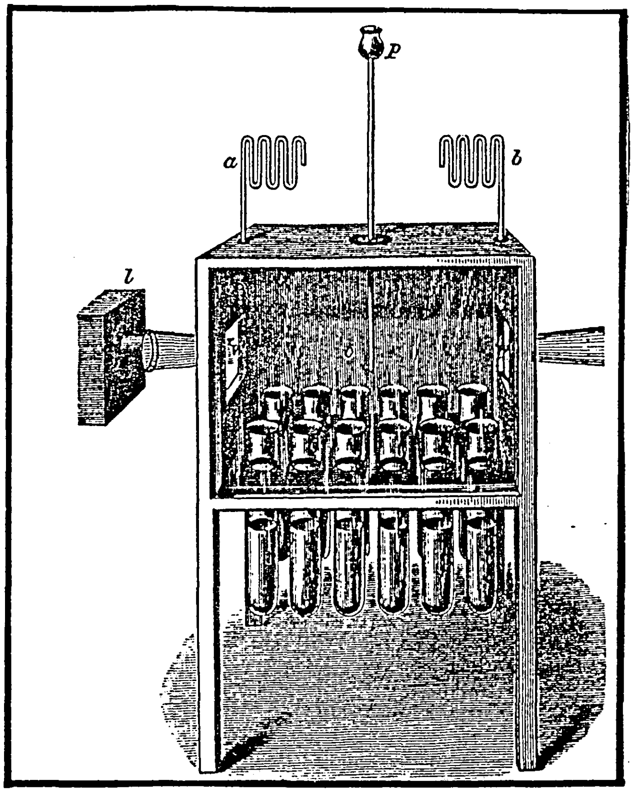

FIGURE 1

Tyndall's Experimental Apparatus (from Tyndall, 1883, 50)

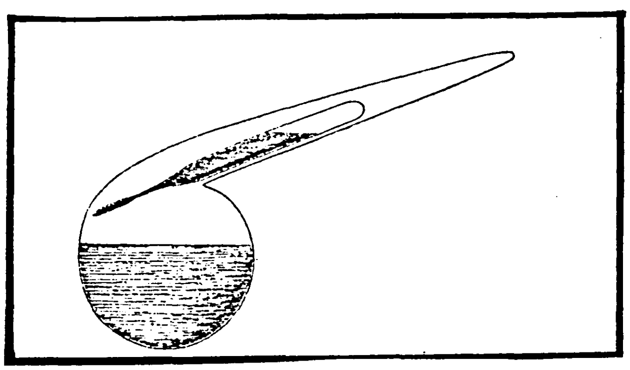

FIGURE 2

Bastian's Experimental Apparatus for Experiments on Neutralised Urine (from Bastian, 1876f, 152)

The following individuals participated in the Pathological Society debate on the germ theory in 1875. (See Bastian, 1875)

H.C. Bastian, Professor of Pathological Anatomy, University College, London.

J. Burdon Sanderson, Professor of Physiology, University College, London.

Dr. Maclagan

Dr. J. Dougall

Dr. E. Crisp, M.D., Chelsea

Mr. J. Hutchinson, Surgeon to the London Hospital and Royal London Opthalmic Hospital.

Mr. Knowsley Thornton, M.B.

Dr. C. Murchison, Physician to and Lecturer in Medicine, St. Thomas's Hospital, Physician, London Fever Hospital.

Mr. W. Wagstaffe, Assistant Surgeon, St. Thomas's Hospital.

Dr. Payne, Assistant Surgeon, St. Thomas's Hospital.

Mr. J. Hogg, Surgeon, Westminster Opthalmic Hospital.

APPENDIX C

Brief Biographies

Scientists

Under this heading appear scientific naturalists and other scientists prominent in the spontaneous generation debate or mentioned in the thesis.

Charles Babbage (1792-1871)
He was the son of affluent parents and studied at Cambridge. In 1827 he became Lucasian Professor of Mathematics at Cambridge. (D.S.B.)

George Henry Busk (1807-1886)
He was the son of a merchant and had orginally trained in medicine but turned to biology and teaching in 1854. He was an active administrator and served as Home Office Inspector under the Cruelty to Animals Act. (D.S.B.)

William Kingdon Clifford (1845-1879)
In 1867, he was Second Wrangler and Smith's Prizeman at Trinity College, Cambridge. In 1868 he was appointed Professor of Applied Mathematics, University College, London. (D.S.B.)

Ferdinand Cohn (1828-1896)
In 1850 he was appointed Privatdozent in the Department of Botany, University of Breslau; he was appointed Extraordinary then Ordinary Professor in 1859 and 1872 respectively. He undertook much original work in bacteriology. (D.S.B.)

Frederick Crace Calvert (1819-1873)
Crace Calvert settled in Manchester after spending eleven years studying chemistry and working in a chemical factory. He worked in dyeing and printing and made many chemical researches especially into the uses of phenol. He was Honorary Professor at Manchester Royal Infirmary. (D.N.B.)

William Henry Dallinger (1842-1909)
Dallinger was a Wesleyan minister from 1861-1880 and then governor and president of Wesley College, Sheffield from 1880-1888. He undertook pioneering studies of monads. (D.N.B.)

Charles Robert Darwin (1809-1892)
He was the son of a physician and studied at Edinburgh and Cambridge Universities. As naturalist on the voyage of H.M.S. Beagle he began to formulate his ideas on evolution. (D.S.B.)

Michael Faraday (1791-1867)
His father was a blacksmith and he had no formal education. Appointed assistant at the Royal Institution in 1813, he later became its Superintendent. (D.S.B.)

William Henry Flower (1831-1899)
Following training in medicine, he worked as a surgeon. In 1861 he became Conservator of the Hunterian Museum of the Royal College of Surgeons. From 1884-1898 he was Superintendent of the Natural History Departments of the British Museum. (D.S.B.)

Edward Frankland (1825-1899)
He was the illegitimate son of a daughter of a calico printer. Initially apprenticed to a druggist, he worked in Playfair's laboratory, taught with Tyndall at Queenwood, studied in Germany and was appointed Professor of Chemistry at Owens College in 1851. After lecturing in chemistry at St. Bartholomew's Hospital from 1857-1864, he became Professor of Chemistry at the Royal Institution from 1863-1869. He was Professor of Chemistry at the Royal College of Chemistry from 1865-1885. (D.S.B.)

Francis Galton (1822-1911)
Having entered Trinity College, Cambridge in 1840, he travelled widely and became an F.R.S. in 1856. Influenced by his cousin Charles Darwin's On the Origin of Species, he investigated the heritability of genius, published extensively and founded the science of "eugenics". (D.N.B.)

Thomas Archer Hirst (1830-1892)
He was the son of a wool-stapler and apprenticed as a land surveyor, in Halifax, where he first met Tyndall. After studying at Marburg he taught at Queenwood. He was appointed Professor of Mathematical Physics at University College, London in 1865; in 1867 he became Professor of Pure Mathematics. (Obituary, Nature, 45, (1892),339-340)

Joseph Dalton Hooker (1817-1911)
The son of Sir William Jackson Hooker, he graduated M.D. from Glasgow University in 1839. His passion for botany and travelling however led to widespread research and publication in this area, resulting in his appointment as assistant director at Kew in 1855. He succeeded his father as director in 1865, continuing his active work until his retirement in 1885. An intimate of Charles Darwin, he collaborated with him in work on evolution. (D.N.B.)

William Jackson Hooker (1785-1865)
The son of a merchant's clerk, he was educated at Norwich Grammar School, Glasgow University (L.L.D. 1820) and Oxford (D.C.L. 1840). As Regius Professor of Botany at Glasgow (1820-1841) he improved the botanic gardens there and began the campaign to save Kew, becoming its first Director in 1841. (D.S.B.)

Thomas Henry Huxley (1825-1895)
As the son of an assistant headmaster, he had minimal education, being apprenticed to his brother-in-law, a surgeon, in 1841. Two years later a scholarship led to Charing Cross Hospital, then an M.D. from London University in 1845. He was ship's surgeon on H.M.S. Rattlesnake. In 1854 he was appointed lecturer in natural history at the Government School of Mines later becoming Professor at the Royal College of Science. From 1862-1884 he served on ten Royal Commissions. (D.S.B.)

John Lubbock (Lord Avebury) (1834-1913)
The son of Sir John William Lubbock, he spent three years at Eton before entering the family bank. His education was self-directed with an emphasis on natural history. Friendly with Darwin, Tyndall, Huxley, Hooker and others, he became an F.R.S. in 1858. (D.S.B.)

James Clerk Maxwell (1831-1879)

His father was a small landowner. Educated at Edinburgh Academy and Edinburgh University (1847-1850), he entered Peterhouse College, Cambridge, becoming a fellow in 1855. He was Professor of Natural Philosophy, Marischal College, Aberdeen from 1856 to 1860 then Professor at King's College London where he did his most renowned work on electromagnetism and light. He was appointed Professor at Cambridge in 1868. (D.S.B.)

William Odling (1829-1921)

Born into a medical family, he was educated at Nesbit's Chemical Academy and Agricultural College. Afer attending Guy's Hospital and London University he gained an M.D. in 1851. After a few years teaching at Guy's he became M.O.H. for Lambeth from 1856-62. After further teaching at St. Barts. he was appointed Fullerian Professor of Chemistry at the Royal Institution in 1867, on Faraday's death. (D.S.B.)

Richard Owen (1804-1892)

Originally trained in medicine, he became assistant, then conservator to the Hunterian collection of the Royal College of Surgeons. From 1856 to 1884 he was Superintendent of the natural history departments of the British museum. (D.S.B)

Edwin Ray Lankester (1847-1929)

The son of Edwin Lankester, he studied at Christ Church, Oxford, worked on marine zoology at Naples (1871-1872), becoming Professor of Zoology at University College, London from 1874-1891. Further professorial posts followed at Oxford (Comparative Anatomy, 1891-98) and the Royal Institution (Physiology, 1898-1900) followed. He was also Keeper of Zoology at the British Museum from 1898-1907. (D.N.B.)

Herbert Spencer (1820-1903)

Spencer worked as a railway engineer from 1837-1841 and began to write. He was sub-editor of the Economist from 1848-1853. He published extensively, his major work being the Synthetic Philosophy which covered all aspects of evolution and sociology, biology and psychology. (D.N.B.)

Balfour Stewart (1827-1887)
Stewart was the son of a merchant and embarked on a mercantile career himself after studying at Edinburgh University. He became interested in science and in 1859 beacame Director of the Kew Observatory. In 1870 he was appointed Professor of Natural Philosophy at Owens College, Manchester. He was interested in Psychical Research. (D.S.B)

Peter Guthrie Tait (1831-1901)
Tait was educated at Edinburgh University and Peterhouse, Cambridge where he was Senior Wrangler in 1852. He was Professor of Mathematics at Queen's College, Belfast from 1854-1860 and Professor of Natural Philosophy at Edinburgh University from 1861-1901. (D.S.B.)

William Turner Thiselton-Dyer (1843-1928)
His father was a physician and his mother was a botanist. He was educated at King's College, London and Oxford where he read mathematics and chemistry and gained 1st class honours in the Natural Science School. He was Professor of Natural History at the Royal Agricultural College, Cirencester in 1868 and then Professor of Botany successively at the Royal College of Science, Dublin and the Royal Hunterian Society. He was Huxley's Demonstrator. In 1885 he became Director of the Royal Botanic Gardens at Kew. (D.S.B.)

Charles Wyville Thomson (1830-1882)
He was the son of a surgeon and embarked on a medical education at Edinburgh University but gave it up due to ill health. He held a number of chairs including the Chair of Botany at the Royal College of Science Dublin and in 1870 he was appointed Regius Professor of Natural History at the University of Edinburgh. (D.S.B.)

William Thomson (Lord Kelvin) (1824-1907)
His father was Professor of Mathematics at Glasgow University. He was Second Wrangler at Cambridge University in 1845 and was Professor of Natural Philosophy at Glasgow University from 1846-1909. (D.N.B.)

John Tyndall (1820-1893)
He was the son of an Orangeman who was at different times a small landowner, shoemaker, leather worker and member of the Irish Constabulary. He worked as a surveyor and railway engineer. He taught

at Queenwood with Hirst and Frankland in 1847 and in 1848 went with Frankland to study at Marburg. In 1853 he was appointed Professor of Natural Philosophy at the Royal Institution, London and in 1867 suceeded Faraday as Superintendent. (D.S.B.)

Alfred Russel Wallace (1823-1913)
He was educated at Hertford Grammar School and was a schoolmaster. He embarked on a collecting trip in 1848 and independently discovered the principle of natural selection. He returned to England in 1862 and spent the remainder of his life lecturing and publishing. (D.N.B.)

William Whewell (1794-1866)
Whewell was the son of a master carpenter and was educated at Trinity College, Cambridge. He was ordained deacon in 1825 and priest in 1826. From 1841 he was Master of Trinity College, a post he held until his death. He was a member of the group of scientific reformers which included Herschel, Babbage and Peacock.

Medical Scientists

Henry Charlton Bastian (1837-1915)
Bastian Gained his M.D. from University College, London in 1862. In 1867 he was appointed Professor of Pathological Anatomy at University College, London. In 1878 he became Physician to University College, Hospital. From 1887-1898 he held the Chair of the Principles and Practice of Medicine. He also held an appointment at the National Hospital from 1868-1902. (D.S.B.)

Lionel Smith Beale (1828-1906)
Beale was educated in medicine at King's College, London. At King's College he was Professor of Anatomy 1853-1869, of Pathological Anatomy 1869-1876 and of Medicine from 1876-1896. (D.S.B)

William Budd (1811-1880)
Budd studied in London, Edinburgh and Paris. In 1842 he was appointed Physician to the Bristol Royal Infirmary. He undertook important research into typhoid. (D.N.B.)

John Scott Burdon Sanderson (1828-1905)

Burdon Sanderson studied medicine at Edinburgh University and Paris. He was M.O.H. for Paddington from 1856-1867. He was appointd Jodrell Professor of Physiology at University College, London in 1874. He was Waynflete Professor of Physiology at Oxford from 1882-1895 and then Regius Professor of Medicine from 1895-1903. (D.N.B.)

William Benjamin Carpenter (1813-1885)

He was the son of a Unitarian minister and schoolmaster. He was apprenticed to a general practitioner and studied at University College, London and Edinburgh University. He practised medicine for a time but gave it up for research. In 1845 he became Fullerian Professor of Physiology at the Royal Institution and Professor of Forensic Medicine at University College, London. (D.S.B)

William Farr (1807-1883)

Farr studied in Paris and London. In 1838 he became Compiler of Abstracts at The Registrar General's Office and held this post for forty years. (Eyler, 1979)

Michael Foster (1836-1907)

He studied at University College, London gaining his M.D. in 1859. He was apponted Professor of Practical Physiology at University College in 1869, Praelector of Physiology at Trinity College, Cambridge in 1870 and Professor of Physiology in the University from 1883. (D.N.B.)

Joseph Lister (1827-1912)

He studied at University College, London and qualified in medicine. In 1854 he became Assistant Surgeon at the Royal Infirmary, Edinburgh. In 1860 he became Regius Professor of Surgery at the University of Glasgow. In 1866 he was appointed to the Chair of Surgery at University College, London and in 1869 he became Professor of Clinical Surgery at Edinburgh. In 1876 he was appointed to the newly created Chair of Clinical Surgery at King's College, London. (D.S.B)

Benjamin Ward Richardson (1828-1896)

He studied at Anderson's College and trained in medicine. He contributed to the development of scientific pharmacology and was an active reformer.

William Roberts (1830-1899)

He received his M.D. from the University of London in 1854 and was a member of staff on the Manchester Royal Infirmary from 1855-1883. (D.N.B.)

Edward Sharpey-Schafer (1850-1935)

He qualified in medicine after an education at University College. He was Assistant Professor of Physiology at University College from 1874-1883, Jodrell Professor from 1883-1899 and Professor of Physiology at Edinburgh University from 1899-1933. (D.N.B.)

John Simon (1816-1904)

Simon received his M.R.C.S. in 1838. He was Senior Assistant Surgeon at King's College Hospital from 1840-1847 and was appointed lecturer in pathology in 1847. He was subsequently Surgeon at St Thomas's Hospital. He became the first M.O.H. for the City of London in 1848, Medical Officer for the general Board of Health 1855-1858 and of the Privy Council from 1858-1871. He was Chief Medical Officer for the Local Government Board from 1871-1876.