Publications of the
Wellcome Institute of the History of Medicine

General Editor: F. N. L. POYNTER, Ph.D., F.L.A., Hon.M.D. (Kiel)

New Series, Volume XVI

Medicine and Science
in the 1860s

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Medicine and Science in the 1860s

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Proceedings of the
Sixth British Congress on the History of Medicine
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Edited by F. N. L. Poynter

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Preface

READERS WHO are familiar with the earlier volumes in this series will know that the congresses at which these papers were first presented have promoted some useful studies of the history of medicine in Britain. Four of the five preceding volumes have been concerned with general historical surveys of particular topics—medical practice, pharmacy, hospitals, and medical education. For the sixth congress, which met in the University of Sussex in September 1967, a departure was made in that a single decade, the 1860s, was taken as the general theme and a number of aspects of medicine and science in that decade provided the subjects of papers. As at the other meetings, contributors were asked to concentrate on developments in Britain, so that even when a study is focussed on an idea or discovery which originated elsewhere it is its application and influence in Britain which is discussed, as in Professor McMenemey's paper on cellular pathology.

Within the space of a comparatively short meeting it was impossible to include everything of importance; any theoretical programme had to be adapted to the interests of those attending, and the time-limit necessarily imposed on speakers no doubt left much to be said on some of the larger themes. Nevertheless, with all its limitations, we believe that this collection of studies on some aspects of medicine and science in the 1860s—historically one of the most important decades in the development of modern science—will be welcomed as stimulating and informative prologues to the more substantial studies which we hope will develop from them.

The sixth British Congress was the first to be organized and sponsored by the British Society for the History of Medicine. The Faculty of the History of Medicine of the Society of Apothecaries of London, which was responsible for the first five conferences, has made a positive contribution towards national co-operation in the development of this subject by inviting three other bodies concerned with it—the Royal Society of Medicine, the Osler Club of London, and the Scottish Society of the History of Medicine—to join with it in founding the first British Society. The new society has its own official quarterly journal, *Medical History*, published by the Wellcome Institute, but does not itself compete with the lecture programmes of any of its constituent bodies, its main task being the organization of the biennial congress, the seventh of which will be held in Churchill College, Cambridge, in September 1969.

On the occasion of the Sixth Congress, the Society's first President, Dr. Douglas Guthrie, presented it with the handsome presidential badge reproduced on the wrapper of this volume. Designed and made in gold and enamel by Garrards of London, it includes elements from the coats-of-arms of the four founding bodies and will be worn by each president of the society during his two-year term of office.

F. N. L. Poynter

Introductory Remarks by the President

AT THIS Sixth British Congress on the History of Medicine, the President is expected to make a few remarks of a general nature, by way of introduction. One's first duty in such circumstances must surely be to thank the University of Sussex for their great hospitality in enabling us to meet in such charming surroundings, and this I do with very great pleasure. I also feel it a duty to welcome all the participants in this Congress, both speakers and audience. It is a further pleasure, which everyone present will share with me, to congratulate those who were responsible for the choice of so important a theme as *Medicine and Science in the 1860s.*

Ancient History, as we all know, has had ample time to become settled and standardized, but Modern History seems to be more uncertain in its content, and more difficult to understand the nearer we approach to the present time, while Contemporary History is even more obscure, so that it can hardly be regarded as history at all.

The facts are there, but their significance is still obscure and a century may elapse before truth is reached and the fallacies are discarded. In viewing the modern scene, medical historians need not go to the opposite extreme by becoming mere medical antiquarians. They will be well advised to study modern medical history, however uncertain present-day history may be. It has been most wisely decided that the present Congress shall deal with the medical history of a century ago, and with the impact of science upon the medicine of that time.

In the 1860s medicine and surgery were deriving great profit from scientific discoveries, and medical empiricism was becoming replaced by medical science. Great discoveries were in progress; one of the most important being Lister's antiseptic principle. His work, first published in the *Lancet* in 1867, following his discovery of 1865, overshadowed other classic works of the period, such as Paget's *Surgical Pathology*, and John Hilton's *Rest and Pain*, both so well worth reading, even today. Virchow's *Cellular Pathology* had appeared in 1858, and Darwin's *Origin of Species* in 1859.

I mention those two books, as they are each to be the subject of a paper at this Congress, by Dr. Towers and by Professor McMenemey. Claude Bernard's masterpiece on *Experimental Medicine*, dated 1865, linked physiology with medicine, while the studies of Mendel on heredity and of Helmholtz on optics, both in 1866, brought science into closer touch with medicine, and foreshadowed modern practice. Specialism was just beginning at that time: all manner of new branches were budding off from the parent stem. The days of the 'general' physician (and general surgeon) were numbered, although the general practitioner still remained 'general'.

Some of the new specialties owed their appearance to the discovery of new instruments of precision. Laryngology, for example, dated from the casual discovery of the laryngoscope by a singing-master, Manuel Garcia, and many other medical and surgical specialties owed their appearance to the essential instruments.

Pediatrics was a part of the general physician's work until the foundation of the Children's Hospital in Great Ormond Street, London, in 1852, and of a similar institution at Edinburgh in 1860.

Neurology also stemmed from the work of the general physician, receiving a powerful impetus from the establishment of what was then called 'The National Hospital for the Paralysed & Epileptic' in 1859. Dermatology, one of the first specialties to be recognized, was already well established in the 1860s.

The coming of State Medicine was predicted at that time by Sir John Simon, of whom we shall hear more from Mr. Macleod and other speakers. The programme is in your hands, and, as you will note, it includes Medical Journalism, Health in the Navy, the Dental Profession, Animal Diseases, and a variety of other topics.

It is perhaps surprising that no one is to submit a paper on the part played by women in the medicine of the 1860s. It was in 1865 that Elizabeth Garrett Anderson obtained the Diploma of the Society of Apothecaries; the first woman to qualify as a doctor. As for the profession of nursing, Florence Nightingale opened the first School for nurses at St. Thomas's Hospital in 1860.

In conclusion, it may be appropriate to mention that amid all the changing attitudes and opinions of the 1860 decade, only one has remained unaffected by science and apparently unchanged. I refer to what has been appropriately called the Doctor-Patient relationship, which has, from the earliest times been a central pivot of the healing art.

Medicine has derived great profit from a closer approach to Science; nevertheless Science cannot supply all the answers to medical problems. No scientific advance can ever supplant the need for a philosophic attitude. Thought and reason must always keep pace with observation and experiment. Only by the study of medical history can we hope to recapture the primitive, yet essential, idea of the art of healing. Only thus will we be enabled to supply to our patients the comfort and human sympathy which always was, and ever will remain, quite as effective and essential as any other means of treatment.

It is possible that we may be nearing the end of the Era of Specialism in Medicine. What is to be the next step? Medical education will certainly undergo great changes within the next hundred years. It is hoped that in any event the lessons of medical history will be remembered and applied, and, in the meantime, this Congress may serve to emphasize the importance of looking backwards as well as looking forwards. Medical History is in no sense a specialty, and we may be comforted by the fact that it uses a vocabulary which is intelligible to everyone.

DOUGLAS GUTHRIE

President, British Society of the History of Medicine

[illegible]

[illegible] other speakers. The programme is in your hands, and, as you will note, it includes Medical Journalism, Health in the Navy, the Dental Profession, Animal Diseases, and a variety of other topics.

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Clinical Medicine in the 1860s

by

KENNETH D. KEELE

THE DECADE 1860–70 holds particular interest for us today because it was the critical period during which medicine of the past ushered in medicine of the present. It was during this decade that the language of medicine became modernized into terms we use today, so that we can still refer to its clinical experience usefully. Before this decade medical speculations were largely unintelligible to the twentieth-century doctor; the pathological theories of Galen, Boerhaave, even the Brunonian theory, fashionable at the beginning of the nineteenth century, are all incomprehensible to us today, caged as they are in technical jargons now almost wholly abandoned.

From the beginning of the nineteenth century, however, a changing light had been slowly illuminating medicine with what we call the scientific outlook, whereby observation, experiment, and inference were as mental processes rated more highly than untrammelled speculation. This outlook had been infused into medicine in the field of morbid anatomy by Morgagni, who compared the patient's symptoms with visceral changes after death. It was systematically applied to clinical medicine by the Parisian School; by such men as Corvisart (who revived Auenbrugger's method of percussion in the process) and Laënnec, who introduced mediate auscultation.

It is not easy for us today to appreciate how momentous an advance was made when it became practicable to reveal structural changes in the patient's organs whilst he was still alive, and not only after he was dead. At once physical examination of the patient became more obviously meaningful. It was in this way that these great French physicians changed the traditional observation of a patient into the new reality of a clinical examination. This consisted of a systematic history of the patient's symptoms, and an examination of his body by inspection, palpation, percussion and auscultation, the last two of which were new nineteenth-century techniques. Inspection and palpation were ancient methods dating back at least to Hippocrates, and the examination of discharges and excreta was particularly stressed by Galen in his *De Locis Affectis* in the second century A.D.

The relative importance of both the ancient and the then modern methods of clinical examination is well illustrated by the procedure which Peter Latham taught his students at St. Bartholomew's Hospital from 1836 onwards.[1] He writes:

> The patient being placed before me, I ask him no question until I have learned everything worthy of remark which my own eyes can inform me of. His physiognomy, his complexion, whether florid, pale or dusky; the general bulk of his body, whether large, spare, or wasted; the condition of particular regions, whether swelled or attenuated; and of the surface, whether there be any eruptions or sores upon it; and lastly the powers of locomotion. All these are most important particulars. There can be no doubt concerning them; they are objects of our own observation, and come to us authenticated by the testimony of our own senses. From what we see on the exterior we obtain a clue for directing our inquiry to the seat and centre of the disease within. If locomotion be hindered we look well to the brain and spinal marrow; if there be the livid lip and dusky skin, we scrutinize particularly the condition of the heart and lungs; if the whole body or some of its parts be attenuated, we examine all the organs of nutrition. Having thus learnt all I can with my own eyes, and felt the pulse and seen the tongue, I next proceed in taking the case to that further inquiry in which the patient takes a part . . .

Latham then outlines his method of questioning the patient for his symptoms. Following this he examines the chest and abdomen by palpation. Then, says Latham, 'if there is any evidence of disease in the chest the patient must be submitted to auscultation and percussion.' He emphasizes the use of both methods, 'since the results of one perpetually correct or confirm the other.'

Auscultation and percussion, the new methods of examination, were unique in that they obtained evidence by using the ear as a diagnostic instrument, thus raising it to the same level of importance as the eye or the sense of touch, an unprecedented situation in clinical medicine, and one which lasted throughout the nineteenth century. Initial interpretation of the many signs obtained by these new auditory methods was inevitably confused. At first Laënnec and his colleagues very naturally tried to associate the auditory phenomena directly with morbid anatomical changes. Pectoriloquy, for instance, was linked with the presence of cavities in the lung; and since such cavities were most commonly a manifestation of pulmonary tuberculosis pectoriloquy came to be considered as a diagnostic sign of that disease. Skoda, seeing the source of this kind of error, tried to show that alterations in these physical signs depended only on the physical changes which underlay them and not on the nature of the morbid anatomical change as such. His work, correct in principle, contained unfortunately many errors arising from his relatively poor powers of observation and from the fact that knowledge of the basic physics of sound was still too weak to answer the questions raised. Such basic knowledge appeared with the work of Helmholtz after 1863.

During this decade Guttmann was writing his *Handbook of Physical Diagnosis*, published in 1872, and widely accepted throughout Europe. Guttmann's comment on the situation was;[2] 'Skoda has submitted Laënnec's teaching to most searching criticism, and by tracing each of the auscultatory phenomena to its physical cause has worked as great a reformation in the department of physical diagnosis as in the science of percussion. He has not only simplified matters by setting aside many of the points which found a place in Laënnec's system, but has also established, on physical principles, a classification of the phenomena of auscultation which has found universal acceptance up to the present time.'

Though, as previously remarked, this 'acceptance' was premature, the principles underlying it were valid, and Guttmann himself well represents the new German point of view on clinical methods which was to make its influence felt throughout the second half of the nineteenth century. In his *Handbook* he vividly demonstrates the introduction of quantitative measurement and applied physiology into clinical examination, summarizing the position thus;[3]

> In the examination of the thoracic and abdominal organs the methods employed are almost exclusively of a physical character. Strictly speaking the term physical methods of examination, is applied only to the practice of Auscultation and Percussion; in the wider sense, however, it includes also Inspection and Palpation, as these latter frequently give as direct and valuable information as the two former methods, particularly with regard to certain of the physical properties of the internal

organs, such as their consistence, increase in size, and the presence within them of air, fluid, etc. With equal propriety the term may be made to embrace the estimation of the temperature of the body by means of the thermometer, and the measurement of the shape and respiratory movements of the thorax, of the vital capacity of the lungs (Spirometry), of the respiratory pressure (Pneumatometry) and of the arterial pulse (Sphygmography), all these proceedings, the most important of which is Thermometry, being merely delicate aids to Inspection and Palpation, yielding precisely the same kind of information, but with much greater, even mathematical exactness. If to these methods of investigation be added the examination of the secretions and excretions of the body, and in some circumstances also that of the blood, we shall have before us all the means which are usually employed in the exploration of the thoracic and abdominal organs.

Of physical measurement applied to clinical examination that of thermometry deserved first place, but it took over two hundred years for it to graduate from the tentative trials of Sanctorius to final acceptance; and this in spite of Boerhaave's use of Fahrenheit's mercury thermometer about 1710, and Currie's excellent reports on the effects of cold baths on the body temperature in typhus in 1797. Not until Ludwig Traube adopted the method about 1850 and conveyed his interest to Carl Wunderlich, who proceeded to make observations on some 25,000 patients, completing his task in 1868, did the clinical thermometer become established. After this fevers were classified according to their time-patterns. For example the pattern typical of typhus, and even one demonstrating the anti-pyretic effect of digitalis, were produced by Wunderlich himself. Even Guttmann still describes as 'a second sign of fever, acceleration of the pulse.'[4]

Though he makes prominent mention of spirometry and pneumatometry in the passage quoted above, Guttmann in fact disposes of these methods rather summarily in his text, saying 'Like every method of examination which involves the use of large or unwieldy instruments spirometry has never come to be regularly employed'.

Three other methods of investigation, not so far mentioned, did come into routine use in the decade 1860–70; these were chemical tests, microscopy and endoscopy; all three of them visual methods which at once competed with and eventually dominated the then recently introduced auditory methods.

Chemistry made its first great impact on clinical medicine with the work of Richard Bright and his team of chemical experts Bostock, Babington and Rees, who between them in their investigation of Bright's Disease detected albumen in the urine, reduced plasma albumen, a raised blood urea, and the presence of urea in the cerebro-spinal fluid. That such investigations were possible at all was primarily due to Lavoisier, whose introduction of gravimetric methods and reorganization of chemical nomenclature brought chemistry to a language comprehensible to us today. The almost explosive advance of chemistry at the beginning of the nineteenth century did not confine itself to the field of inorganic substances. Chemical analyses of urine were performed by Berzelius; and Prout in 1821 produced a routine for the chemical testing of urine which included measurement of its daily quantity, colour, reaction to litmus, specific gravity, a boiling test for albumen, and a tasting test for sugar. Nevertheless the chemical methods used during the next thirty years remained too cumbersome for routine clinical use. From 1850 onwards however progress was rapid. Tests for sugar and acetone and many other substances appeared, and volumetric methods replaced the cumbersome gravimetric methods. The decade 1855–65 in fact saw a flood of works on the chemical analysis of urine appear in Germany and England. By 1860, for example, Neubauer and Vogel could produce a book entitled, *The Qualitative and Quantitative Analysis of the Urine, designed especially for the use of medical men*. In their introduction they assure the doctor that, 'armed with the simplest, newest methods of analysis he is able in a short time at the bed-side to test the urine of the patient, and thereby to discover the presence of abnormal constituents.'[5] Their tests detected seventeen normal and seventeen abnormal urinary substances.

Alongside this rapid refinement of urine-testing there developed the method of examination by the microscope. Once more there was the long lag of over two hundred years between Galileo's demonstration of the possible uses of this instrument and its clinical application. Only after the difficulties caused by spherical and chromatic aberration had been finally overcome by Joseph Jackson Lister in 1829 did the microscope really come into its own as an instrument of clinical investigation. Alexandre Donné in Paris examined blood, noting in one case a great increase of pale white cells, and so describing 'leucocythaemia' (leukaemia); and in vaginal discharges he found the trichomonas vaginalis. By 1844 Donné was using the newly-discovered process of photography for making photomicrographs of his specimens. By 1854 Lionel Beale was running courses on clinical microscopy for students in London. In this year he produced a textbook called *The Microscope in Medicine*, which he hoped would 'afford some assistance to practitioners and students in medicine who employ the microscope in clinical investigation and pathological inquiries.'[6] Beale combined his microscopy with chemistry, expressing the opinion that;[6] 'The laboratory is a very necessary adjunct to the post-mortem room and the clinical wards of our hospitals; and he who desires to apply all the means at present at our disposal to unravel the mysteries of disease to help him to form a correct diagnosis, or enable him to recommend the right course of treatment, will do well to make this particular branch of chemistry with microscopical examination an essential part of his study.' Beale substantiates his case with illustrations of cholesterol crystals, yeasts, penicillium glaucum, and bacteria, though strangely enough, even as late as 1872 he denied a bacterial cause of disease. William Osler was sufficiently impressed by Beale's example to imitate him in producing a lecture course delivered at McGill University in 1874, also entitled, 'The Microscope in Medicine'.

The third method of clinical examination to become established in this memorable decade, 1860–70, was endoscopy. The earliest efforts to examine the dark and inward cavities of the body were made by specula introduced into the ears, nose and vagina. Such examinations were restricted by the lack of artificial illumination of the target organ. There is something rather pathetic about the efforts of Bozzini as late as 1804 trying to examine the urethra and bladder with the aid of the best artificial light available to him, candle-light. And one's sympathy extends to the patient when one realizes that there were no anaesthetics at that time. It was not until after 1880, when anaesthetics had come into use, and Edison had invented the electric light bulb, that von Dittel found it possible to perform efficient cystoscopy.

But the first really successful examination of an internal cavity of the body was achieved by Helmholtz with the ophthalmoscope in

1851, a feat which directly derived from his mastery of the physics of light. Though Hughlings Jackson recognized the clinical value of this examination in the 1860s it did not become standard practice for more than forty years.

Though the origins of examination of the vocal cords by laryngoscopy, are disputed, the enthusiastic teacher of singing, Manuel Garcia is usually credited as the first to see the interior of the larynx reflected from a mirror placed on the uvula in 1855. Türck and Czermak in 1858 introduced medical laryngoscopy using light from gas or paraffin lamps reflected from a concave mirror on to the laryngeal mirror. This method of examination, in contrast to ophthalmoscopy, became popular at once, and detailed descriptions of the findings are described by Guttmann in 1872.

So far I have stressed the similarities between clinical examination a century ago and today. The picture is more clearly defined by taking note of methods commonly used today which were unavailable then. For example the blood pressure was not taken, endoscopy of bronchus, oesophagus, stomach and colon was impossible; there was no bacteriology; no X-rays; blood chemistry was quite untouched; and haematology, as we know it, still embryonic.

In these circumstances it is not surprising to find that the word 'diagnosis' meant something very different from its meaning today. For example 'Fever' was still accepted as a diagnosis, classified by the thermometer according to its time-picture into acute, intermittent, or continuous varieties, and further subdivided according to accompanying clinical features such as rashes, e.g. scarlet fever, rheumatic fever, etc. In Britain the 'continued fevers' were divided into three main sub-groups, typhus, typhoid, and 'relapsing fever,' this last being identified by some as intermittent biliary fever. Typhoid had been recently separated by its clinical features from typhus, an achievement in Britain unanimously attributed to Sir William Jenner, though there can be little doubt that he knew of Gerhard's part in its definition. Inflammation, in both acute and chronic forms, was receiving intense study with regard to its morbid anatomy and microscopic appearances in all organs of the body. In particular the microscope was beginning to define the differences between tumours of granulomatous origin, e.g. tuberculous or syphilitic, and true neoplasms. And Virchow's terms, thrombosis, embolism and pyaemia were entering the language of pathology. 'Dropsy' was still the diagnostic word for generalized oedema. However since Bright's day renal, cardiac, and nutritional forms of dropsy were being separated out.

The mid-nineteenth century was a period of description of many new clinical entities; Addison had presented his cases of pigmentation associated with adrenal disease, as well as those with anaemia, both eponymously named. Raynaud in 1862 described the peripheral circulatory stasis named Raynaud's Disease. Bright's Disease of the kidney was now in the process of being subdivided into acute and chronic forms, and extended to embrace syphilitic and 'gouty' kidney. Leukaemia, locomotor ataxy, trichinosis, cerebro-spinal fever with meningitis, and amyloid disease, were all recent entrants into the list of recognized diagnoses. Thus diagnosis can be seen to be rapidly stepping up towards modernity in this 1860–70 decade.

There is an inevitable time-lag between advances in the knowledge of pathology and the treatment of disease. This gap is felt the more keenly when progress in pathology has been unusually rapid, as it was around this decade. Disillusionment regarding treatment was therefore particularly acute during these years. Lauder Brunton reflects this in 1868, when he opens his account of his work on digitalis with the words; 'As we review the rapid progress made within recent years by physiology, pathology and other departments of medical science, and compare it with the slow advance of therapeutics, we experience a growing dissatisfaction with our present empirical methods of treatment which, consisting in the mere tentative administration of drugs without a definite knowledge of their action must necessarily retard progress . . . the panacea of one generation being discarded by the next, only to be again resorted to and trusted by a third.'[7] In 1869 Sir William Jenner endorsed this sentiment in an address to the British Medical Association, saying: 'The spirits of many have been damped by the idea that modern advances in the science of Medicine have led to scepticism in regard to the remedial powers of medicine as an art; and especially to the remedial power of drugs.'[8]

One need only take a glance at the treatment of one of the commonest of medical problems of this decade, 'continuous fever', to feel sympathy with this dissatisfaction. Bleeding and purging were still standard procedures; and the list of febrifuge drugs (which did not yet include any of the coal-tar derivatives) consisted of a formidable number of substances, most of which were supposed to act according to the Brunonian Theory of fever, as 'contro-stimulants', so decreasing the 'irritability' thought to underly all fever. Of these, antimony and quinine were commonly used, but Brown-Séquard[9] gives a list which includes opium, hydrocyanic acid, hyoscyamus, digitalis, belladonna, tobacco, euphorbium, camphor, acetic, oxalic, sulphuric, nitric, and hydrochloric acids. Moreover Brown and Todd had introduced alcohol, the most controversial drug of all at this time.

The tragic seriousness of this therapeutic confusion and destitution can be gauged when one learns that in the first six months of 1862, 1107 cases of true typhus were treated in the London Fever Hospital alone; of these 232 died, a mortality of 20.95 per cent. Another 343 cases were treated in six general hospitals in London, of which 80 died.[10] All were treated with various permutations and combinations of the drugs mentioned above. In Glasgow alcohol was widely used, the average consumption per patient being 46½ oz. of wine and 18 oz. of whisky, but benefit from this treatment was considered to be very doubtful.[11]

Though potent drugs like digitalis and quinine were known, their therapeutic uses were still not appreciated. This indeed is why Lauder Brunton embarked on his study of digitalis which was still being used for its so-called febrifuge action, a result thought to arise from diminution of the heart rate from its action as 'the opium of the heart'. And quinine, clearly successful when an 'intermittent fever' was due to malaria, depended for its successful rational use on a diagnostic accuracy which was then unattainable.

One of the real advances in therapeutic technique which appeared during this period passed almost unnoticed. The use of the hypodermic syringe for subcutaneous administration of a drug acting systemically was reported by Dr. Alexander Wood in 1855. The advance appeared to be the result of rather muddled thinking, since Wood intended to use the syringe and needle for, 'bringing some narcotic to bear more directly than I had hitherto been able to accomplish on the affected nerve in neuralgia'.[12] When he came across a case of cervico-brachial neuralgia he injected into the affected area, 'twenty drops of muriate of morphia'. Though this injection of morphine was given as a local anaesthetic there is no doubt that Wood appreciated its systemic effect, since he performed the injection in the evening, 'in order to give the opiate the benefit of the night', and he writes of his appreciation that 'the effects of the narcotic reach the brain through the venous circulation, and there produce their remote effects.' Thus Wood used his hypodermic syringe for the same purposes as we use it today.

The discovery of general anaesthesia by Wells, Morton and Simpson between 1844 and 1847 might reasonably be looked upon as an unmitigated boon to mankind. But listen to what Erichsen, Surgeon to University College Hospital had to say about it:[13] 'The introduction of anaesthetics led at once to an enormous increase in the number of operations performed. Here hospital wards became more crowded than formerly with severe operative cases, and in the absence of any efficient antiseptic treatment the causes of septic diseases became much more rife, erysipelas, septicaemia, pyaemia, and hospital gangrene more frequent, and the mortality proportionately increased'. Evidently this new boon of surgery without pain was won at the cost of a great increase in surgical deaths. Lister, with his gift of antiseptics, arrived on the scene in 1867—it was not a moment too soon.

I would like to conclude, this brief survey with the words of a contemporary practising physician and essayist, John Brown of Edinburgh, portraying the medical outlook of this period. He writes:

> Let us avail ourselves of the unmatched advantages of modern science . . . let us convey into our heads as much as we safely can of new knowledge of chemistry, statistics, the microscope, the stethoscope, and all new helps and methods; but let us go on with the old serious diligence, the experientia as well as the experimenta . . . Young men have now almost the whole field to themselves. Chemistry and Physiology have become to all men above forty, impossible sciences. The young man teaches and talks, the old learns and is mute. In this intensely scientific age, we need some wise heads to tell us what not to learn, or to unlearn, fully as much as what we learn.[14]

Today I don't think I could put my own outlook more clearly.

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Cellular Pathology, with Special Reference to the Influence of Virchow's Teachings on Medical Thought and Practice

by

W. H. McMENEMEY

'. . . . we must appear in the arena, stand question and give answer, and seek for agreements and counter agreements in experience; . . .'

Virchow, *Arch. path. Anat. klin. Med.*, 1855, **8**, 1.

INTRODUCTION

RUDOLF VIRCHOW proclaimed his cellular theory of life and disease to a Europe still preoccupied with the prolonged phase of slow recovery that began with the final downfall of Napoleon I. The successful harnessing of steam power and need for the furtherance of trade and culture with consequent greater prosperity facilitated interchange across frontiers. There was more than a hope that a truly international spirit would evolve. In this liberal age Virchow's theory caused a revolution in medical thought but, as with Harvey's great discovery, its final proof was forthcoming only later when technical procedures had been devised adequate for the factual demonstration of his hypothesis.

A century ago, while European eyes watched the Second Empire in its heyday with even more suspicion than the rapid and ominous rise to power of Prussia, Virchow's views had already gained substantial support. The American civil war was over, the treaties of Prague and Vienna had been signed and Europe was once more at peace. French physicians, proud of their heritage, initiated the first International Congress of Medicine in Paris to coincide with the Exhibition of 1867. At the congress banquet Virchow, the acknowledged guest of honour, sat on the right hand of the president, Professor Bouillaud. In England the merit of Virchow was formally recognized two years later by his election to honorary membership of the Pathological Society of London, along with Claude Bernard, Billroth, Helmholz and Ludwig. In the following year the peace of Europe was shattered and the French army was defeated at Sedan. This review of the rise of the concept of Cellular Pathology deals particularly with the period from 1847 until that time.

THE EDITORIAL PRONOUNCEMENT IN 1855

Virchow was thirty-four years old when, in 1855, he formally expounded[1] his theory. The events of the abortive liberal uprising of 1848, in which he had been implicated and had courted the displeasure of his superiors, belonged to the past and since November 1849 he had been enjoying the busy and fruitful life of a university professor at 'about 2000 thalers [£300] a year' in the beautiful city of Würzburg, where Kölliker, a life-long friend, had settled two years before him. His own pupils included G. E. Rindfleisch, E. Krebs, W. His senior, V. Hensen, O. Deiters, A. Kussmaul, N. Friedreich and C. Gerhardt. The editorial columns of the *Archiv für pathologische Anatomie und Physiologie und klinische Medizin* which, with Reinhardt, he had been far-sighted enough to institute in 1847, provided a convenient medium through which he voiced periodic pronouncements on pathological topics: of these editorials the most important and far-reaching was this one which appeared in 1855.

Three years earlier he had shaken the world of humoral pathologists and had stimulated microscopists, professional and amateur, by claiming[2,4] that pus cells were the product of continual tissue development and did not originate, as was the then current teaching, in an inflammatory exudate: they were in fact the end result of a process rather than its beginning. In a paper published[3] in that same year of 1852 he had asserted that those tiny elements, the cells, were the loci of life and also of disease. To his readers therefore, and to all who were privileged to hear him teach, his theory was no novelty and the dictum not unexpected. As will be seen later, others had been ventilating the same idea.

This editorial began as a progress report wherein the editor claimed that the journal in the eight years of its existence had played its part in bringing about a more scientific outlook in medicine. He prophesied that the microscope, in spite of gloomy prognostications from certain men in practice, would make for more precision in diagnosis and, in time, render possible classification of disease: it would, in fact, be an instrument of reform. He was convinced that 'all pathological formations' must be either degenerations, transformations or repetitions of physiological structures. The unit of life was the cell so that therefore it was impossible for a patient to harbour within him any community of cells that had not evolved from his own. The eternal merit of Schwann, he said, lay not so much in his cell theory but in his description of the various tissues, which meant in effect that every mature cell could be traced back to a predecessor. Virchow was in fact attempting to carry Schwann's theory from physiology into pathology, as many subsequent reviewers noted. He expressed it thus: '. . . Now if pathology is nothing but physiology with obstacles, and diseased life nothing but healthy life interfered with by all manner of external and internal influences, then pathology too must be referred back to the cell'.

It might not, he said, seem a difficult task to establish this point; in fact in itself it appeared extremely clear and simple, but it had achieved recognition only with the greatest difficulty. He complained that he had been equally accused of combating the current teachings in humoral pathology and of resuscitating those of the old solidary pathology but what he wished to see was the humoral and the solidary theories united in an empirically-based cellular pathology and this he confidently predicted would be the pathology of the future. It was then that he proclaimed:[1] 'I formulate the doctrine of pathological generation, and of neoplasia in the cellular pathological sense, in simple terms: *omnis cellula a cellula.*'

DEVELOPMENTS IN HISTOLOGY 1830–55

This editorial has to be read against the background of the time, and in particular, in relation to the then rather rapid advances in clinical microscopy with their impact on medical thought and practice. Virchow himself described[1] the eight years before 1855 as follows: '1847 were days of great scientific degeneration in medicine. The method of orderly investigation had been almost completely lost. The great upheavals that microscopy, chemistry and pathological anatomy had brought about were at first accompanied by the most dismal consequences. People found themselves helpless in the ruins as the old system collapsed; filled with exaggerated expectations they seized on any fragment which a bold speculator might choose to cast out.' For these reasons had Virchow demanded the complete emancipation of pathology and therapy from 'the oppression of the ancillary services'.

The confusion into which these 'ancillary services' (by which he appears to have meant embryology, biochemistry and microscopical histology) had brought the medical scientific world of the eighteen-forties needs here but brief elucidation. Most scientists in the middle of the nineteenth century worked alone or in relative isolation with their pupils without necessarily knowing what others were doing or thinking. Medical journals were few and sometimes inaccessible, editors were seldom critical in their attitude to proferred papers and authors inexperienced in presenting their data. Simultaneous discoveries and retrospective claims for priority were not infrequent in the pioneer days of microscopy while the borrowing and re-publication of illustrations was seemingly a widespread practice.

In the eighteen-twenties medical scientists had been mostly occupied with physiology and chemistry but Bichat had shown the importance of the study of individual tissues and organs and there was still much interest in this original form of histology.

(*A*) *The Rise of Microscopical Histology, 1830–47**

The importance of cells seems[6,7,60] to have been fully realized by C. F. Wolff[8] (1759) who, noticing the presence of little cavities in tissues, believed that they could develop into other cavities if they remained round, or into blood vessels if they became elongated. It was in fact a cell theory to challenge the then current concept that the essential unit of life was the fibre so readily identifiable in tissues, whether vegetable or animal. Ackerknecht[4] in fact, lists several early nineteenth-century writers who believed that the cell was the basic unit of life, amongst them Lorenz Oken (1805)[9] of Jena, and Raspail who, we are reminded, actually prefaced a treatise on embryology (1825) with the motto *omnis cellula e cellula*. John Hunter is thought[10]

* For a full account of the origin of the cell theory see Cameron (1952).[5]

to have believed in the principles of the cell theory because he repeatedly asserted that each individual part of an animal is endowed with a separate life and that blood is a living tissue. Robert Brown is generally held to have been the first to appreciate (1831) the prime importance of the nucleus although others had already noted its existence.

In the eighteen-thirties the race began, thanks to the invention of the compound achromatic microscope, and anatomists and physiologists throughout Europe—including Johannes Müller,[12,13] the talented teacher of Schwann, Remak, Kölliker and Virchow—studied tissues of all kinds and their constituent cells. In 1834 Purkinje and Valentin[10a] described ciliary movement for the first time, while the former at the Naturforscher-Congress held in Prague in 1837 gave an account of nerve cells and dendrites, including the large cells in the cerebellum which now bear his name, and also myelinated fibres. In the same year Henle[10b] gave the first account of epithelial cells but Remak who, in 1836 had identified the axon which Purkinje subsequently named 'cylinder axis', is said to have been the first to recognize cell division[11] (1841). Many of the illustrations published by the early workers are difficult to identify but Remak (1838)[14] illustrates 'nucleated globules' in the brain and also nerve 'funiculi' that are convincing. It was still an age when illustrations, carefully drawn with very fine detail under a strong hand lens* were giving place to those made with the aid of the new microscope. The famous cell theory propounded by Theodore Schwann in 1839[15] was built up on his own researches and on the expressed views of others, more especially Dutrochet[15a] (1824)** and of Schleiden[16] (1839), the botanist. Schwann claimed that all animal and vegetable tissues were composed of cells and that cells could develop out of cells: but he also believed that the 'elementary cells

* As an example of this former we may instance the illustrations of the structure of the optic chiasm by Müller (1826)[12].

** The importance of J. H. Dutrochet's *Recherches anatomiques et physiologiques sur la Structure intime des Animaux er des Végétaux et sur leur Mobilité*, Paris, 1824, has been stressed by A. R. Rich[15b] (1926). Dutrochet clearly recognized the universality of cells in the animal and vegetable kingdoms and the role they played in growth and metabolism.

of tissues' originated in an amorphous blastema or nutritive fluid[17].*

Meanwhile Ferdinand Jahn had turned his attention to a study of the nature of disease and in the year 1843 published[20a] a concept of cellular pathology. The importance of his teachings and the influence of this naturalist on Virchow have been evaluated by W. Pagel (1945),[20b] who shows how the latter's thinking could have been determined by Jahn's ideas in such matters as the identity of physiological and pathological processes, the local origins of disease rather than the humoral outlook, and its essentially parasitic nature.**

Schwann's work was not translated[21] into English until 1847 but it had already provided an important impetus to microscopy to such an extent that in Edinburgh Hughes Bennett began to instruct his pupils in the use of the instrument, a systematic course in histology having been inaugurated in the winter of 1841–42.†[22] John Goodsir, demonstrator of anatomy in that university in 1842 suggested that in developing animals there were nutritional centres composed of cells in the various organs and tissues, which was the quintessence of what, ten years later, was to be a claim of Virchow. In 1845, when Goodsir published[24] his views, he was not prepared to recognize this form of cell development as being universal in the animal economy because he wrote '. . . I am inclined to believe in the general existence of such centres, for a certain period at least'.

* Although Schwann and Schleiden are commonly credited with having been firm believers in the blastema theory, Kölliker (1853)[18] asserted that the former 'was disposed to believe everything without exception developed from cells' and in this opinion was supported by Moxon, while Turner (1863) maintained that Schleiden 'fully realised that the new cells of plants were never found except within those already existing'. According to Rich, however, the contribution of Schleiden has been overrated, because it is seen to contain a 'profusion of errors of observation'. The scientific world was only waiting for someone formerly to pronounce the cell theory and in history this credit has gone to Schleiden as well as to Schwann.

** I am grateful to Dr. W. Pagel for drawing my attention to the conclusion of Jahn's paper which is similar to the passage already quoted from Virchow (1855). Jahn wrote (and I copy from Pagel): 'Everything in the body consists of cells. There is no dynamical disturbance of organisation without material changes. Hence any morbid change must be referred to a change in those elementary constituents'.

† Dr. William Sharpey who lectured in Edinburgh from 1832 to 1836 was said to have 'systematically used the instrument for the purpose of illustrating his anatomical course'.[23]

Like most biologists of this time he believed firmly in the existence of a cytogenetic fluid. John Goodsir, although an anatomist, was said to have been 'exceedingly partial to pathology'.[25] In 1842, for instance, he described the structure and 'some of the pathological changes' of the kidney and liver.[26]

In 1843, reports 'on morbid structures, which the dead house almost daily affords' were already accumulating in the 'Microscopical Department' of Guy's Hospital but Williams[27] believed that it was too early to put forward any form of systematic classification of disease. He held with Carpenter, whom he quotes, that research to date—and of British authors he named Martin Barry, Goodsir and Bowman—had made it abundantly clear that 'all the changes in which organic life essentially consists are performed by cells'. Nevertheless he found it quite untenable that elaborated pus could pre-exist in the blood. Pus corpuscles he believed were unquestionably heterologous. Barry (1841)[28] however had already claimed to have followed the transition of 'blood corpuscles' into 'pus globules'. Williams knew well that some pathologists regarded the findings of morbid histologists as being 'the refined delusions of a complex optical mechanism'.

By the time Virchow decided with Reinhardt to found a journal, many important and classical papers on microscopical histology had been published in the United Kingdom, as well as the two-volume work of Todd and Bowman (1845–47)[29] in which are illustrated—among many other features—capsule cells surrounding the neurons in the Gasserian ganglion and also what appear to be glial nuclei.

(*B*) *Morbid Microscopical Histology 1847–1855*

Morbid anatomists were not slow to apply the microscope to diseased tissues and by 1847—when according to Virchow it was difficult for the interested reader to sort out the wheat from the chaff—these instruments were becoming more reliable and more available. The earliest workers in this field, according to Williams (1843)[27] were Müller, Gruby, Raspail and Rokitansky. Virchow referred especially to Lebert.[30] The workers however were handicapped because technical procedures for the preparation of material for microscopy lagged behind optical developments in the instrument. Most examinations were made on fresh material, often teased, and stains were not yet in use. Nevertheless experienced microscopists did what they could to amass information and further knowledge.* For instance on 5 April 1847 Dr. Richard Quain** presented[31] to the Pathological Society of London the case of a tumour of the tentorium cerebelli of about the size of a walnut, sent to him by Dr. Hughes Bennett. The report reads as follows:

> Being examined microscopically the structure was found to consist of round or small oval bodies varying in size from1 /100 mm. to 1 /20 mm. in diameter, apparently solid, transparent, with dark abrupt edges, shadowed gradually towards the centre (transmitted light). Each was enclosed in a sheath of cellular tissue (1 /20 mm. to 1 /80 mm). Pressure caused them to crack, generally in a radiated manner, from centre to circumference. The addition of ether and acetic acid caused no change, except the latter made the material more transparent and the nuclei proper to this tissue more distinct. The tumour was embedded in a simple granular matter.

Quain decided that this tumour was composed of 'fibroplastic cells' and today we would probably call it a fibroblastic variety of meningioma. A report such as this might today be regarded as quaint and a little unconventional. It might suggest too that the microscopist had not had a very clear view of his field, at least until he acidified the medium. Nevertheless, the report is adequate and surprisingly good having regard to the technical difficulties of the time and the relative lack of knowledge. Göppert and Cohn (1849) are quoted[32] as having been early, although possibly not the first, users of a carmine wash,† but stains, except for injection purposes are not mentioned by Beale (1854)[33] in his technical manual. The first satisfactory stain of a section is generally credited to Gerlach in[34,35] that same year.

The first edition of Gerlach's *Handbuch der Gewebelehre*[36] appeared

* British names mentioned by Williams (1843)[27] in his article *On the Pathology of Cells* are Gulliver, Bowman, Addison, Dalrymple, Smee, Wharton Jones, Barry and Goodsir.

** This was Richard Quain (1816–98), the cardiologist who became a physician at the Brompton Hospital, editor of the *Dictionary of Medicine*, Harveian Orator in 1885 and a baronet in 1891. At the age of 31 he was elected to the Council of the Pathological Society of London at its inception and served as Secretary from 1852–6 and as President in 1869. He was a cousin of Jones Quain the celebrated anatomist and of Richard Quain (1800–87) who succeeded Charles Bell in the chair of descriptive anatomy at University College.

† Sir John Hill (1770) is believed to have made use of cochineal and boiling green sealing wax as stains, while Leeuwenhoek (1719) is said to have employed saffron in a study of muscle.

in 1848 and in the following year the important and beautifully illustrated work by Hassall, *The Microscopic Anatomy of the Human Body in Health and Disease.*[37] Histological illustrations were first used in the *Transactions of the Pathological Society of London* in 1850. In the following year there was published the lovely anatomical work[38] of J. Lockhart Clarke, wherein this general practitioner of Pimlico illustrates the little 'vesicles' in the intermediolateral column of the thoracic portion of the spinal cord, which now often bears his name.

In the four years between the appearance of Clarke's first and most famous paper and the Virchow editorial of 1855 there were several publications in the field of medical microscopy, the most important perhaps for British readers of that time being Kölliker's *Manual of Human Histology*,[39] translated by Busk and Huxley (1853) and also the translation[40] by Swaine, Sieveking, Moore and Day of the four volumes of Rokitansky's *Manual of Pathological Anatomy* (1849–1854). Kölliker, who had high hopes that embryology would solve many problems in histogenesis, claimed that medicine had reached a point at which microscopical anatomy appeared to constitute its foundation. But, he said, it possessed not a single law and moreover the materials on which any such laws would have to be based were relatively scanty.

At this time Kölliker was a firm believer in the blastema theory although he wrote as if he would have liked to have been convinced that cells or their derivatives were the only elements of animals. He classified the interstitial substance or matrix into three types, the formative fluid or Schleiden's cytoblastema (vesicle germ substance), the nutritive fluid and the connecting substance. He believed that cells could be formed from (mother) cells but also that they could originate in a cytoblastema, and by this term be included chyle or lymph corpuscles, spermatic cells, ova, the closed follicles of lymph glands, splenic corpuscles and pulp, thymus, corpora lutea, the medulla of foetal bones and 'soft ossifying blastema'. Yolk, milk globules and pus were other examples of the matrix in which it was believed that cells could develop. The teaching was that the nucleolus was first precipitated out of the cytoblastema which next attracted unto itself the nutriment necessary for the formation of the nucleus.

The precipitated material was known as 'Henle's elementary bodies'.* Next appeared the nuclear membrane and finally the protoplasm of the perikaryon. The process was in fact likened to crystal formation but with the biological facility of membrane permeability.[19]

The fact that Kölliker could have been so wrong at a time when Virchow was already seeing the light—and sometime, it would seem, after Remak had seen it—reminds one of the obscurities which confronted microscopists of the fifties. Today the theory of blastema sounds as improbable and outdated as that of phlogiston but at that time it was firmly entrenched in the minds of men and, as Kölliker reminds us,[39] only a few were thinking in terms of microscopy to which most were entirely opposed.** Busk (1855)[42] speaks of 'the almost blind obedience paid to the doctrines of Schwann and Schleiden' which had apparently 'acted for some time, as a damper upon original thought'. But Kölliker was the professor of anatomy and physiology so it is understandable that his line of thinking differed somewhat from that of his colleague, Virchow, the clinician and pathologist who stated that nothing had 'penetrated less deeply in the minds of all than the cell theory in its intimate connexion with pathology'. T. H. Huxley (1853) spoke[6] as a biologist when he reviewed the work of the various cell theorists but he made no mention of pathology. William Rutherford Sanders[43] too, at a conversazione in Edinburgh in 1856 was more concerned with the developmental and theological aspects of the Schwann-Schleiden theory than with any possible application to medicine. Rokitansky however was a pathologist, the doyen of European pathologists in fact and rector of his university and in the third edition (1855) of his *Lehrbuch der pathologische Anatomie* he abandoned his early humoral views although he was still a firm believer in the concept of a cytoblastema. In the year 1855 Parisian 'micrographists' in the Academy of Medicine discussed the reliability of microscopy in the diagnosis of cancer.[44] Even the conservative Velpeau seems to have accepted the fact that the microscope when used in biopsy could serve as an additional diagnostic eye.

* Freidrich Gustav Jacob Henle (1809–85), who worked in Zürich, Heidelberg and finally in Göttingen, was an opponent of Virchow and his theory.

** It is not surprising that the microscope was still mistrusted because we read that it had 'maintained its position in the toyshop' and had always been popular with mountebanks.[41]

Although Virchow made the formal announcement of his theory in 1855, he had been formulating it for some time. Undoubtedly too he had been studying the views of others. As we have seen[2,3,4] he had three years earlier pronounced his opinion that pus cells arose from precursor cells and that disease was the result of healthy cells being exposed to abnormal stimuli. In a lecture delivered[45] in 1845 he had excluded any possibility of spontaneous generation, claiming that life in essence was cell activity, while in 1846 he had criticized Rokitansky[46] for believing that all disease was the result of dyscrasias of the blood. In 1852 he had written on the development of cell colonies.[4] Nevertheless others too had been thinking of the possible application of Schwann's theory to pathology; these included Henle (1841) who held that disease was the result of 'lawful reactions' of matter to abnormal stimuli[47] and Remak the neurologist who, in 1852, believed that pathological tissues were but variants of normal embryonic development.[48] Ackerknecht in fact speaks of the Remak-Virchow cell theory, but this man, who eleven years earlier had noted cell division and had objected to Schwann's concept of a cytoblastema, did not in the opinion of Oertel[49] (1927) cross the threshold of the Cell Theory of disease even though his foot was on it. Others (Kölliker, 1859; W. Pagel, 1945; Kisch, 1954) appear not to have shared this view, giving Remak undoubted priority for propounding the thesis of the universality of cell division. This last author has shown that from 1855 onwards, when on a few occasions these two men were rival candidates for office, Virchow kept quiet about the achievements of Remak, although in 1852 and in 1854 he had given full credit to him for enunciating the theory of cell pathology.

Another who deserves to be remembered in this connection is William Addison who in 1849 wrote:[50] ' . . . if a microscopical analysis of the healthy tissues shows the physiological conclusions . . . to require revision and amendment, then it follows necessarily that pathological conclusions derived from the same source must undergo a similar revision'.

Like Virchow this Malvern practitioner could be described as one who did not 'borrow the spectacles of others':[51] he was merely summarizing the results of twenty years' work on the colourless globules of the blood, the part they played in inflammation and the nature of the lung lesions in consumption. That he was thinking much on the same lines as Virchow is indicated by his likening the difference between the acute pneumonic lung and the phthisical lung to that between acute inflammation and a chronic abscess.[52] He had this to say about inflammation:[53] 'It is not necessary to determine, whether or not it be appropriate to apply the term inflammation to these supervening changes; it is sufficient our investigations prove that the phenomena are governed by the universal law of growth;—cell-organisms and protoplasma accumulate;—new textures, and secretions are formed;—ulcerations discharging pus occur in the mucous; and granulations degenerating into tubercles, spring up in the serous textures'. He asked himself what could be the aetiology of tuberculosis? An infant was born into the world healthy but with its very first breath it was exposed to an environment which at some time and for some reason did not suit it.[54] He concluded that the hurtful element must be a failure of nutrition.

It is not clear how much attention was paid to Virchow's theory in these early days although it will have been noted that in the editorial of 1855 he speaks of recognition having been achieved. There appear to have been no printed commentaries in the United Kingdom prior to the publication of *Die Cellularpathologie* in 1858,[55] and in fact Virchow seems to have been little known at this time. He was reported[56] however as having been present in 1855 with Kölliker and their teacher Johannes Müller, and also Liebig, to 'make microscopical and galvanic observations' on the body of Gensler, the poisoner, immediately after he had suffered judicial decapitation at the atlas with a sword.* Nevertheless a reviewer in 1856 commenting on the fact that the cell doctrine had been 'gradually advancing its footing in physiology for twenty years, observed that it was also gaining a stronghold in pathology.[58] A year later a reviewer[59] on Virchow's *Gesammelte Abhandlung zur wissenschaftlichen Medizin* (1856) referred to his original mind and to the logical way in which he drew his inferences.

* Referring to an execution in Würzburg in the year 1850 at which Kölliker was present with Virchow, the former commented[57] on the fact that such opportunities were 'becoming more and more rare'. On this earlier occasion the body was placed on the dissecting table within 35 minutes of dispatch but the findings were mostly negative. They could find no ciliary movement in the ventricles of the brain. However we read that 'the skin under galvanism rewarded us with some excellent results'.

Among the writings of this time should be mentioned the little volume of William Addison entitled *Cell Therapeutics*[60] (1856). Accepting the cell theory at any rate in principle, because he appears at this time to have been a believer in the blastema viewpoint, he advocated that therapeutic changes in the qualities of the blood must be shown either to be in harmony with the theory or 'capable of a rational explanation without it'. If one accorded to physiological cell growth and 'nuclei' the property of selective attraction or absorption then, he said, one should endeavour to get at pathological cells on the assumption that they too were subject to the same physical and biological principles.

'DIE CELLULARPATHOLOGIE' 1858

Apparently little notice was taken of the editorial in Germany because after Virchow's translation to Berlin in 1856 and the publication of his *Gesammelte Abhandlung zur Wissenschaftlichen Medizin* in the same year he was persuaded to give that systematic course of lectures which formed the basis of *Die Cellularpathologie*. His audience consisted of practising doctors who wished to learn about his new concept of disease but who, according to one of the reviewers, had no time to follow 'the microscopical debates of the day'.

Perusal of a nineteen-page review[51] dated October 1859 serves to remind us how Virchow's revolutionary theory must have appeared to British observers. The theory favoured by Schleiden and by Schwann that cells originated in an amorphous blastema had now been challenged, said the reviewer, as it had already been, by Remak. Virchow was actually claiming that every cell, including even the pus cell, evolved from some previous cell. Although Professor C. O. Weber of Bonn had already produced evidence which appeared to substantiate Virchow's theory so far as pus cells were concerned it was all very different from the then current teaching that these cells with their tiny fragments of nuclei arose spontaneously in inflammatory fluid. If, under conditions of inflammation, cells behaved differently from their wont, it was, stated Virchow, because of their power of reaction to abnormal stimuli. The reviewer wondered what was the nature of these stimuli which could have a differential effect on cells? Could one so readily dismiss the blastema concept? How did Virchow explain the phenomenon of rapid pleurisy or pericarditis with pints of fluid and masses of fibrin? But what troubled this reviewer most of all was Virchow's readiness to dismiss the concept of dyscrasia by asserting that every seeming dyscrasia was in fact a state of the body in which there was a permanent supply of hurtful ingredients somewhere within it. Did tuberculosis then start as a local disease, as Virchow would have them believe, asked the writer? Where was the local condition in smallpox, a disease that could kill before a vesicle appeared? Virchow, he said, although he looked at facts 'with so much of the instinct and fire of genius' would not, like Joshua, lead them into the land of milk and honey: his work was one-sided and there was still a lot to be explained. This critic said that a perusal of Virchow's writings often reminded him of 'ideas scattered in the pages of acute but speculative writers some half century ago'.

Another reviewer[61] welcomed the attempt to put an end to all 'the one-sided theories' then in vogue and substitute for them a knowledge of 'the fine organic processes of cell-life' as the only rational doctrine of disease but Dr. Francis Bond who addressed[62] the Birmingham and Midland Counties Branch of the British Medical Association in April 1860 was very critical. If Virchow was correct then the increase of 'fibrine' in the blood and the degree of fever in rheumatism should be commensurate with the severity of the local lesion, but this was by no means the case. Everyone knew of instances of fever which only after some time appeared to concentrate in one spot and was it not a fact that a general fever often settled when localisation occurred? Finally how did Virchow explain the fact that the red corpuscles in the blood diminished when the 'fibrine' increased? There appeared to be only one explanation of this fact, with which Virchow was not in agreement, namely, that 'fibrine' was formed from destroyed red corpuscles.

In the preface to a second edition (1859) of his book Virchow admits to having found 'many friends and vigorous opponents'. Two further papers[63,64] were published for the benefit of the critics, who seem to have been mostly Germans.* One French

* Such authorities as Spiess, Wunderlich and Griesinger seem to have feared a likely swing-back in outlook from that of the dynamic subject of physiology to that of old-world anatomy if Virchow were to succeed in having his way with the new Cell Theory of Disease.

editor, however, Charles Lasègue, in a review of two aspects of the Cell Theory, Addison on therapeutics,[65]* and Virchow on pathology,[66] accused the latter of having drawn hasty conclusions on insufficient evidence, likening him to a chemist whose analyses were inaccurate because his reagents were impure. The Berlin Professor was fully credited with a rare gift of experimentation and induction but was held to be more scholastic than Baconian, being in the habit of beginning always not with the known facts but rather with the law he sought to establish. Lasègue remarked as others did, that Virchow was exceptionally persuasive and so could readily captivate an audience. He spoke of 'ce dogmatisme impérieux et scolastique' and said that his deductions were 'inexorable'. Virchow's reply to this criticism[64] drew from Lasègue an open letter[67] in which the former was reprimanded for bitterly attacking his critic instead of defending a scientific thesis. He reminded him of a case in the Charité Hospital in which both during life and at the necropsy Virchow's diagnosis seemed to have reflected more his ability as an experimental pathologist than as a clinician. The advice from Lasègue was clear: as a mere anatomist he should not deduce too much concerning disease.

Those who were new to Virchow's views must have had real difficulty in accepting the evidence adduced from the microscope. One can imagine a clinician brought up on the humoral theory of disease trying to understand the nature, let alone the function and significance of the *Bindergewebskörper* ('areolar tissue corpuscle') which in the early days of the Cell Theory was much to the fore. In later years we read less about these structures for their importance seems to have been provisional in the evolution of the theory.

Dr. Baumgarten of St. Louis, a former pupil of Virchow reviewing[68] the second edition of *Die Cellularpathologie*, saw in his teacher's theory a means of settling the contest between the humoral pathologists and the 'neuro-pathologists': the former were mostly practitioners of medicine and clinical teachers while the latter, who believed in the overall control of body functions by the nervous system, were mostly those with 'a speculative mind'. Both of these

* Lasègue, like some other writers of the time, mistook the Malvern physician for Thomas Addison of Guy's Hospital.

systems, he said, were false only in their exclusiveness; supporters of the doctrines tended to forget that in the body there were structures other than blood, vessels, nerves and their centres. It was not to be denied that the ubiquitous blood vessels made for uniformity in the body but, he said, 'all actions derived from the nervous system lead us to no unity, except in our own consciousness'. Virchow's theory, in that it laid emphasis on the cell, allowed of a synthesis between the two apparently opposing schools of thought. This in fact is what Virchow had himself claimed.

Baumgarten's review is largely factual but he recognized the importance of local changes in blood supply and in capillary permeability, admitting the existence of affinities between certain tissues and certain substances, which he attributed to chemical properties. He seems to have no real doubts about the validity of the theory even when it came to explaining such a phenomenon as sudden death occasioned by terror or by certain poisons. In these conditions the 'neuro-pathologists' would, he said, assign the failure of life to exhaustion of the nervous system, whereas the cellular pathologist would say it was due to a cessation of irritation and irritability of the nervous system. Baumgarten justified the assumption of such abstract ideas as irritation and irritability, since organic life was based on nutrition and chemical exchanges. Irritation was that property of life which was for ever disturbing chemical equilibrium: it was in fact synonymous with the 'vital force' of the 'vitalists'.

DR. FRANK CHANCE AND HIS TRANSLATION, 1860

An Englishman who happened to arrive in Virchow's Institute towards the end of the course of lectures in 1856–7 was so impressed with their quality and originality that he determined then and there that they should be made available for the English-speaking world. He was Frank Chance, a member of the glassmaking family of Birmingham of that name and an accomplished linguist who had spent two years in his father's factory and three in the study of chemistry at King's College London, Paris and Berlin before commencing medicine at Cambridge. He later went to St. Bartholomew's Hospital and qualified in 1856.* Chance received every encouragement and much help from Virchow and the volume, based on the second edition (1859) and gracefully dedicated to John Goodsir, was published[69] by John Churchill in 1860.**

English readers must have been impressed with the easy style and independent yet modest approach of the Berlin professor. With due humility he described the development through which he—and he believed medical science also—had progressed in the previous fifteen years. In his presentation of the new outlook in medicine he would try to couple histology with pathology and allow pathological anatomy to share in the benefits of the recent developments in general anatomy gained from greater and more expert use of the microscope. Schwann he both praised and criticized. He had, he said, presented a valuable theory but for his examples had depended too much upon material from the vegetable kingdom. The animal cell differed from the vegetable cell in important respects as subsequent studies had shown but his general theory, if wrong in some details, still held. The fact that it had taken so long to recognize the important implications of Schwann's theory as applied to pathology was because it had taken time to learn the new lessons of the microscope.

He then proceeded to explain his ideas on 'cell-territories'. It was a question of being able to delineate the margin of the protoplasm of each cell and recognize the area of influence as it were, of each nucleus. He firmly rejected—one might well say he outlawed—the theory of free cell formation in a blastema and commented on

* Dr. Chance returned to London and practised from 51 Wimpole Street. He obtained his M.R.C.P. in 1859 and the F.R.C.P. two years later. Although he was physician to the Blenheim Free Dispensary he seems to have devoted most of his time to the study of languages and especially Hebrew; in fact, while at Cambridge he had won the Tyrwhitt Hebrew Scholarship. In 1864 he edited Bernard's *Commentary on the Book of Job* (reissued in 1884 with an appendix) and seven years later retired to Burleigh House on Sydenham Hill when he continued his studies. He was a member of the Old Testament Revision Society and served on the Committee for the Revision of the Old Testament. He was a widower and died at Nice in 1897.

** One wonders if this fitting dedication was Virchow's idea or if it had been suggested to him by Chance, because of his apparent neglect of the important contributions of Goodsir. Certainly Turner[70] was puzzled about Virchow's seeming reluctance to acknowledge the work of Goodsir especially in view of this dedication. In this connection it is interesting that Semon[71] recalled the fact that Virchow always held Goodsir up to his students as a model of keen and accurate observation.

the fact that he had found the nucleolus to be missing in many young cells. He considered next the specialized nature of some cells, those that produced secretions, pigment, nails, and even the crystalline lens, and the important group that were concerned in a supporting function, cartilage, bone, connective tissue, muscle and blood vessels. The nature of 'fibrine', and colourless corpuscles of the blood led him to a study of leucocytosis, pyaemia, thrombosis, phlebitis (these last two being according to one reviewer 'the missiles' of Virchow's emboli), blood pigments, fatty and amyloid degeneration, inflammation, exudations, granulations, tumours and ulceration.

Much of what he related had of course been described by contemporary or earlier pathologists whose work was duly and sometimes enthusiastically recognized: such names as Henle, Haller, Reinhardt, Reichert, Lebert, Remak, Gerlach, Bowman, Huxley, Cruveilhier, C. Vogt, Bernard, Kölliker and of course Müller are to be found prominently in text and footnotes. Some facts and indeed many were his own but always he wove them into his thesis and showed how everything fitted into place—a sort of jig-saw—if one accepted the cell theory in its logical totality and applied it equally in health and disease. The miliary tubercle was presented as having its origin in a growth of connective tissue cells which however instead of being allowed to develop naturally had by reason of an adverse influence, divided by a process of degenerative proliferation. Because of this excessive and pathological cell growth, the blood supply had become inadequate with the result that the cell masses had shrunk and disintegrated into a cheesy substance. It was noted of course that the ubiquitous character of fibrous connective tissue accounted for the fact that the same pathological feature—the tubercle for instance—could arise in structures and organs apparently so dissimilar.

REACTIONS TO THE VIRCHOW THEORY, 1860–62

Following the publication of the lectures in the English translation there were some favourable reviews.[72 73,74,75,76] However a writer in the *Dublin Quarterly Journal* (1861), while appreciative of the efforts of Virchow and other reputable workers and welcoming this particular 'gem in the casket of science', was concerned[74] about the number of 'shallow pretenders' who were exploiting microscopy. The microscopic mania, he said, was raging like some fierce epidemic and he supported the suggestion advanced by one distinguished physician that to all important charitable institutions concerned with medicine a 'regular microscopical pathologist' should be appointed to act as accredited *censor morum* and so safeguard the proper growth of a potentially important branch of medical science. One reviewer[75] thought that the theory was ahead of the facts. Another[72] saw the need for really reliable microscopical studies to substantiate the claims of Virchow, especially so far as the connective tissue cells were concerned. The theory that blood was not a 'permanent' tissue but depended upon contributions from other organs fitted in, he said, with the most recent advances in therapeutics: general bleeding, for instance, was now out of fashion. The cell was the offspring of a cell, just as an animal is the offspring of an animal. But gratitude to Dr. Chance for having brought these important lectures to their attention was tempered by the criticism that in his translation he had taxed the reader by allowing too many German 'proliferations' to creep in. Also, he regretted that a summary view of Virchow's ideas had not been provided as a foreword to the text.

A two-part review in the *British Medical Journal*[77] was far from favourable: in it the distinguished Prussian pathologist was accused of dogmatism and plagiarism. He had borrowed, said the writer, from Goodsir, Bennett, Zimmermann and Gulliver, and his theory was neither new nor true. He continued: '. . . the whole doctrine he seeks to develop is made up only of confident plausibilities, utterly irreconcilable with the cautious and exact spirit of research so necessary for establishing any just and true theory'. His theory certainly did not hold so far as yolk was concerned. This critic claimed that he himself had never seen pus cells within epithelial or connective tissue cells, neither had he seen tubercle, and still less cancer develop in fibre cells, yet these were common diseases. One could not get away from the blastema of Schwann. The reviewer continued: '. . . the system of pathology built up by Virchow abounds in the greatest contradictions and assumptions of novelties which, on being closely regarded are only well-known facts or processes with new names, or old names applied in a new sense, giving rise to the greatest confusion . . . the ambitious dreams and hopes that stimulated his researches are not only illusory, but in too many instances, contradictory, unreasonable and even ridiculous'. This writer was prepared to admit however that in the course of his 'wildgoose chase' Virchow had stumbled on a few real discoveries, such as haematoidine crystals, the black pigment in certain blood corpuscles and the presence of cellulose and starch in tissues. He had added facts and views to science which together with his studies on embolism would keep his name alive long after the time when cellular pathology was forgotten.

The charge of plagiarism was surprising, especially as another writer had been impressed with Virchow's originality of outlook. Virchow was unmoved by the review in the *British Medical Journal*, if indeed it ever came to his notice. He was too big a man and too busy to resent it and probably had not questioned the reiterated view of the *Edinburgh Medical Journal*[78] that the priority for reporting a case of leukaemia belonged to Bennett.

So severe a censure naturally led to the belief that the reviewer was personally implicated and the *Medical Times and Gazette*[79] went so far as to suggest that it was 'the Northern Professor' who had been permitted to act as 'judge, jury and executioner'. A month later they named[80] Hughes Bennett as the probable author and said that they had twice pressed the editor of the *British Medical Journal* for an answer. They reminded their readers that it was in October 1845 that Bennett had published his case of hypertrophy of the liver and spleen with suppuration of the blood, five or six weeks before the appearance of Virchow's case of 'leukaemia'. In it they said that Bennett, who disparaged the researches of others, had put forward doctrines which were utterly untenable, whereas Virchow's view of the disease was now established. Two years after this controversy the *Lancet*, referring[81] to Bennett's discovery thought that the dispute as to who had priority, should be settled in 'the sobriety of history'. In reporting an address by Professor Gulliver concerning the pioneer work of William Hewson on the role of the lymphatic glands and thymus, the *Lancet*[81] deprecated the attempt of any foreigner to steal the credit from so distinguished a pupil of the Hunter School. The German Professor, they said, had made no reference to Hewson.

Samuel Wilks (1862) shared[82] many of Virchow's opinions to a certain extent and in fact claimed to have expounded on them at Guy's. He was not however prepared to dispense with the blastema theory or to deny the existence of simple exudations, which he said were so often to be seen on serous surfaces and also in the kidneys in the form of lymph casts. There were also lardaceous exudations to be explained.

Some critics who were not prepared to accept the suggestion that pus cells had their origin in cells were nevertheless ready to believe that tumours could develop from misguided or perverted cells; but in the early sixties there was as yet no clear distinction between a true neoplasm and what we now call a granuloma. The picture was confused because both sorts of 'tumour' were still credited with an associated dyscrasia. Wilks in fact went so far as to say that surgical removal of a cancer could not be regarded as wholly remediable if it was a constitutional rather than a local disease.

Maurice Henry Collis of Dublin, on much the same theme, spoke[83] of the perplexity of surgeons when faced with a plethora of discoveries that could affect treatment. Diseased action, he said, was becoming as diverse as man himself. The varieties of morbid growths had so multiplied that the shades of difference between them had become less and less broadly marked, and the numerous peculiarities pointed to a common origin modified by individual influences. He regarded Virchow's theory as being of importance to surgeons, because if cancer had its origin outside human cells then they must despair of any remedy but the knife or other agents of destruction, whereas if the cancer cell could be regarded as a perverted lymph cell then there would be no rest until remedies were found which would influence it in the direction of more healthy growth. This was a theme already advanced by William Addison in 1856. Collis said that microscopy and the rise of medical journalism had both contributed to the increasing perplexity facing the surgeon.

William Turner[20] in a lecture at the Royal College of Surgeons of Edinburgh conceded the fact that the beginning of all the work on cellular pathology must be credited to Müller and his pupils Schwann and Henle but he affirmed that John Goodsir had made important contributions to knowledge and had as early as 1842 advanced proofs of the part played by cells and their nuclei in disease. He could not agree with the charge that the current views on the doctrine of cell pathology were founded on insufficient data. Most authorities, he said, had abandoned the blastema concept in biology although some believed in it so far as pathological conditions were concerned.

An American reviewer[76] expressed gratitude to the translator for his free but faithful rendering of a work which, if it contained no new facts, was nevertheless important and helpful to those who wanted to keep up to date.* Virchow, he said, was remarkable among men of his time, because of the reverence with which he regarded past workers. His contributions to knowledge in a space of ten years had been considerable and no living pathologist had acquired so high a reputation for honesty of purpose and keenness of observation. This reviewer was not prepared to offer detailed criticism but urged his readers to read carefully and think.

'DIE KRANKHAFEN GESCHWULSTE' AND ITS RECEPTION

Virchow's *Die Krankhaften Geschwülste* was published in Berlin, the first volume in 1863, and the second in 1864–5.** They comprised a series of thirty lectures given at the Institute in the winter of 1862–3 and were generally regarded as being the second part of his *Cellularpathologie*. As to whether a tumour was a local or a general disease, there was evidence, said Virchow, in favour of both views but he held that in the contemporary state of knowledge the origin of many tumours had to be attributed to a peculiar state of the blood.[84] He instanced syphilitic tumours, which again serves to remind us that at that time there was no clear distinction between neoplasm and granuloma. A so-called dyscrasia, said Virchow, could be due to absorption of matter from a focus of disease but the ability of this diseased substance to cause irritation must depend upon the part irritated, for how else could syphilis cause at one time an exotosis and at other times a gummy tumour? He stressed the role of trauma in the initiation of tumours and instanced the importance of soot as a local irritant.

The accounts of the various tumours were deemed by one reviewer[85] to be masterly, but the proposed changes in meaning of

* Ackerknecht (page 97 footnote) refers to an American translation in 1858 by A. H. Smith which was not accepted for publication.

** Volume III appeared in 1867.

pathological terms, the resuscitation of old ones and the coining of new ones evidently gave rise to much confusion: for instance, the German term 'struma' was equivalent to the English term 'bronchocoele' but in France the equivalent word was 'goitre'. An American reviewer, 'E.T.C.', devoted[86,87,88,89] much space to this work of a man he clearly revered. He speaks of the astuteness of the professor, the logical simplicity of his deductions, his remarkable erudition and the fact that he never failed to keep conflicting views out of sight. He adds:[86] '. . . Those of us who have enjoyed the opportunities of listening to his silvery utterances will not fail to hear them ringing in our ears . . . We can well imagine the hundreds of crayon sketches which were dashed off upon the blackboard with the readiness and skill of artistic genius.'

THE LATER COMMENTARIES ON VIRCHOW'S THEORY, 1865–70

By 1865 Virchow's theory had gained wide acceptance but there were still objectors, notably Bennett, who said[90] that he had repeatedly failed to demonstrate pus cells within pre-existing cells after he and his pupils had passed setons through the skin and muscles of animals. Moreover he had been able to study the process of suppuration in the eyeball and in the lungs, from which it was quite clear to him that the pus cells originated in a coagulated molecular exudate. A correspondent from Berlin was able to report—not surprisingly perhaps in the *Edinburgh Medical Journal* (abstracted in the *British Medical Journal*[91] and also in the *Boston Medical and Surgical Journal*[92])—that Virchow himself had changed his mind after Recklinghausen's discovery that the corneal cells were without walls but instead lay in an intercellular substance. This writer claimed that the professor now no longer regarded the cell wall as essential which was of course what Goodsir and Beale had all along said.

Furneaux Jordan, reviewing[93] the theory from the point of view of the surgeon, supported Virchow at all points. He sized up the position in 1865 as follows: 'To the genius of Goodsir is due the increased recognition of the inherent action of the cell, but to Virchow belongs the merit of showing that all specific action in pathology belongs to the tissue elements, and that no specific action is exercised by the vessels or nerves'. Paget, said Jordan,* was one who regarded the fact that disease was so often symmetrically disposed in the body as a bulwark of humoral pathology but might not blood be a medium of conveyance, as it clearly was in the spread of cancer? He believed that blood in itself could do nothing and originate nothing, because it was entirely dependent upon the solid organs and on the character of the ingesta. Only local disease or persistently injurious food could give rise to a diathetic taint. The fact that repaired structures were invariably composed of connective tissue was, he thought, strong confirmation of Virchow's theory, but he could not agree that epithelium could originate in connective tissue—this was one of Virchow's early mistakes. From his experience as a surgeon he believed it was marginal in origin and this accounted for the well-known difficulty in central healing. In the same way Jordan believed that blood clotting originated not within the blood vessels but in adjacent tissues. Although treatment remained empirical he had learned that all wounds of bone and of soft parts should be treated alike. Neglecting to bring parts together because of the fallacy of different modes of union, had protracted many a weary convalescence and aggravated many a hideous deformity. Jordan belonged to that age ignorant of bacteria and antisepsis which had to accept sloughs, bone caries and spreading cellulitis as part of the lot of the surgeon and his miserable patient.

Dr. William Stokes (1865)[94] did not think that Virchow's theory had influenced the healing art: no form of therapy had as yet reached the cell but if the theory proved to be true he did not discount the possibility. He thought the demonstration of embolic diseases and the part played by the colourless globules of Bennett and Virchow priceless additions to knowledge. No longer would physicians be overlooking embolism of the pulmonary artery and attributing the symptoms to asthma or hydrothorax, nor would they be ascribing embolus of the central artery of the retina to 'brain disease'.

In the late sixties we find that acceptance of Virchow's theory had become fairly general. The debate at the first International Congress of Medicine in 1867 on the structure and the nature of the tubercle,

* There seems to have been a fatuous disagreement between these two as to whether inflammation was more likely to affect the closed eye of the microscopist because of disuse, or the open eye because of strain.

in which incidentally there was no British participant, serves to show that the speakers were aware of Virchow's work* and there were several references to it.[95] A general disease was now regarded as an extension of an original focus. William Gull[96] (1868) wrote: 'But how rarely do we meet with a case of acute inflammation of the membranes of the brain, or of the peritoneum, which we cannot refer to some chronic disease, or to some distinct cachexia'. He was another to comment on the importance of Virchow's work on embolism.

A reviewer in the *Dublin Quarterly Journal* in 1869[97] summarized the work of the man who had brought great lustre upon the University of Berlin, the man who was 'a pathologist as well as a microscopist', a man who was known for 'his willingness to estimate justly the labour of his predecessors'. Definitions were now more precise; the word 'scirrhus' for instance which had formerly meant anything from simple induration to a cancer, was now employed only as an adjective. A classification of disease was now possible, which had not been so in Bichat's day and had not been rendered feasible by the embryological work of Haller and of Hunter. Virchow's capital discovery was, he said, the connective tissues and their equivalents. There was now renewed hope that the chemists would make important discoveries and already researchers were looking for a 'carcinomatine'.

Walter Moxon writing[19] in 1870 complained of the time it had taken for Virchow's theory to be accepted, but, he said, everybody then believed it. Harvey's enunciation of *Omne vivum ex ovo* had been extended by Virchow to the cell, but there remained plenty of problems for biologists. What was the combining influence that made cells coalesce and interlink by means of their processes? What were the laws of combination and what controlled cell behaviour?

THE UNITY OF PURPOSE IN VIRCHOW'S LIFE

Tributes to Virchow and appraisals of his manifold achievements are too numerous to review: nor is this the occasion to do so. It is relevant to our purpose however to discuss how far, if at all, his

* Hérard, speaking in the debate paid tribute to the writing of Villemin (1862) who had done so much to make known to French physicians the views of Reinhardt and Virchow on tuberculosis.

political activities prejudiced his scientific career and its recognition. His threefold life as pathologist, anthropologist and statesman have been thought of as parallel manifestations of a basic urge to bring order into chaos, to study and compare the evolution of life, of disease and maybe of society. It has been suggested that his interest in pathology was subservient to that in anthropology. Perhaps the underlying motif of his life was to further knowledge of the physiology and pathology of human society. That he admired the well-ordered life of a community of healthy cells is clear and he must often have wondered how this co-ordination of interests could be translated into terms of life in a healthy community of human beings. Did he, a lifelong liberal at heart, think of a cell community in terms of a democracy or of an autocracy? Was he looking in human society for the equivalent of neoplasia and is this why he stood up to Bismarck and—as seems to have been the case—was successful in delaying some of his more aggressive policies?* His modified support of Bismarck rather than outright opposition to him may have been dictated by motives of expediency but it may well have been deliberate policy in the belief that as an apparent supporter he was then in a better position to apply the brake. If so it was apt to be misunderstood. The *British and Foreign Medico-Chirurgical Review* in 1864 made this comment:[84] 'Many of us who during the past 2 years have occasionally turned our thoughts from the more serious contemplation of pathology to the proceedings of the Prussian Parliament must have noticed with mixed feelings of regret and admiration the name of Virchow among the leaders of the Fortschrittspartei; regret that he should be drawn from the great work which he more than any living man has furthered.'

The *Medical Times and Gazette*[99] was critical of his behaviour at the Paris Congress in 1867 when he was reported to have exhibited more the character of the Prussian politician than that of the savant, repelling all advances and abstaining from taking part in the proceedings. This was said to have been because of pique at the almost entire absence at the congress of Parisian doctors of note but, said the writer, this was no excuse for certain disparaging remarks he was said to have made concerning the members of the German

* An obituary notice in the *British Medical Journal*[98] recalls how he succeeded in defeating his Government on a motion to create a navy.

Medical Society of Paris who had honoured him by electing him into 'the Academy' and by personal attentions. This was not in keeping with the character of a man who was by most accounts kindly by nature and unassuming, but in German society his high 'geheimrat' standing would have demanded rather more respect than he evidently thought he was getting. That he was not unaware of the criticism levelled at him for forsaking his wards and laboratories for the Reichstag is shown by what he wrote in 1865:[100] ' . . . I fulfilled my duties as professor even on the same days when important debate took place in the chamber of Deputies. I am able to add, in order to tranquillize my friends, that the silent toil of the man of science, often so little thought of, taxes the strength and powers of application by far more than the noisy and consequently less slighted activity of the man of politics, which often seems to me a positive relaxation'.

CONCLUSION

Virchow was the foremost pathologist of his time. Clear-headed, far-sighted and bold, he refused to be tied down by tradition. When only 25 years old he had challenged the authority of Rokitansky: years later he dared to oppose the Iron Chancellor by whom he was challenged to a duel. In England his death, at the ripe age of 80, was likened to 'the shock of the falling of a great tower.'[98]

We may find it hard to understand why pathologists lagged behind biologists in defining and accepting the implications of 'the Schleiden-Schwann theory' of cells: it was because practitioners of medicine, whether they treated patients or practised morbid anatomy, were still tied to the humoral way of thought. But Virchow decided that the cell theory must apply to pathological lesions as well as to a healthy cell community: he not only extended the Schwann concept, he also simplified it. Oertel[7] (1927) speaks of 'the compelling completeness of Virchow's approach' because 'he made disease into altered normal life and not something different'. Virchow owed much to Schwann and perhaps even more to their teacher Müller who believed in an essential relationship between a normal tissue and its diseased counterpart:[101] he was unquestionably indebted to Remak, his senior in Müller's department by six years. If, as some hold not without justification, others have an earlier claim to priority in the matter of cell pathology, no one will doubt that it was Virchow who, by his teachings and writings, enlightened and convinced the world as to its importance. The fact that his achievements were greatly facilitated by his early and successful professorial career and his privileged position as an editor, does not detract from his high standing in relation to them.

But if Virchow was a visionary, so too was Kölliker, his friend, concerning whom the late Sir Roy Cameron spoke so charmingly just a few years ago.* In 1853—that is two years before Virchow published his editorial—he reviewed the development of his subject from the time when Malpighi and van Leeuwenhoek preached the doctrine of elementary structure in biology. Schwann, he said, had provided the basis of fact for Bichat, and then he continued[102] in this prophetic vein—

> . . . histology will last as long as no essential advance is made towards penetrating more deeply into organic structure and becoming acquainted with those elements of which that which we at present hold to be simple is composed. If it be possible that the molecules which constitute cell membranes, muscular fibrils, axile fibres of nerves should be discovered, and the laws of their apposition and of the alterations which they undergo in the course of the origin, the growth, and the activity of the present so-called elementary parts, should be made out, then a new era will commence for histology, and the discoverer of the law of cell genesis, or, of a molecular theory, will be as much or more celebrated than the originator of the doctrine of the composition of all animal tissues out of cells.

* At the meeting of the Section of History of Medicine, Royal Society of Medicine on 6 April 1955.

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The Impact of Darwin's *Origin of Species* on Medicine and Biology

by

BERNARD TOWERS

THE LAST ten years have seen published a greater volume of 'Darwiniana' than any decade in history, and there is as yet no sign of abatement.[1] The recent impetus came, of course, from the centennial celebrations in 1958 of the publication of the Darwin-Wallace papers and, in 1959, that of Darwin's greatest single book, *On the Origin of Species by Means of Natural Selection; or the Preservation of Favoured Races in the Struggle for Life*. I give it its full title, because it represents an exact summary of Darwin's particular creative insight into one very important aspect of the evolutionary process. The origin and evolution of species had been much discussed during the previous hundred years, but never before in these specific terms. It was this personal creative act, a 'bisociation' typical of advances in knowledge,[2] that led Darwin initially to speak of 'my' theory. This expression he subsequently modified or dropped, when he realized (a) that none of his ideas were in fact unique to him, and (b) that, as he had known all along, his own theory of evolution involved a great deal more than 'natural selection, or the preservation of favoured races in the struggle for life'.

Another reason for giving in full, and for emphasizing, the title of a book usually referred to as the *Origin of Species* or simply as the *Origin*—the single-word title being a mark of extreme distinction—

[1] Donald Fleming in 'The centenary of *The Origin of Species* (Review Article)', *J. Hist. Ideas*, 1959, **20**, 437–46, lists many of the books published about that time. Other books, and articles in specialist journals, have appeared at regular intervals since then. Professional historians are displacing the amateurs. Any volume of the *Journal of the History of Ideas* is liable to carry two or three full-length articles on Darwinism, and the subject is included in a Special Period Course for Part II of the Cambridge Historical Tripos 1968.

[2] Arthur Koestler, *The Act of Creation*, London, Hutchinson, 1964.

is that, in the result, it was precisely the *narrow* view (as I call it, though Professor C. D. Darlington would doubtless call it the *hard* view[3]) of evolution that, in the hands of Darwinists and neo-Darwinists, received most of the publicity and hence captured the market. This is the view that the process of evolution results solely from the natural selection of biological variations that came to be thought of (though not by Darwin) as wholly random in nature and fortuitous in onset. This doctrine added one of its strongest planks to the platform of 'traditional' British empiricism. By Sir Arthur Keith it was elevated still further: his 1925 Conway lecture[4] was entitled *The Religion of a Darwinist*, a religion where the two dominant forces were chance and struggle. Darwin might have been disappointed had he known in the 1860s that this one part of his extraordinarily complex theory, of which the *Origin* represented only what he called an 'abstract of an abstract', would become so powerful that to question its all-embracing efficacy (as did W. R. Thompson in his Introduction to the current Everyman edition of the *Origin*,[5] or as Sir Alister Hardy recently did in his Gifford lectures[6]) would be to court that curiously emotional contempt reserved by some modern biologists for anyone who hesitates about adopting a neo-Darwinian philosophy which in fact leads logically to the emptiness and meaninglessness of certain forms of modern existentialism, and finally to the abandoning of science as a really significant human activity. Darwin himself must of course take some responsibility for the restrictions from which his thought has suffered: if an author incorporates an important and true hypothesis into both the title and the subtitle of his book, he cannot really complain if disciples choose to emphasize this one more than others (of which in fact there are a great many in the *Origin*). My thesis is that what emerged out of the conflict in the 1860s, namely the philosophy of Darwinism, was a αἵρεσις, a choosing, for non-scientific reasons, of one part of a complex whole. In other words, what we know as Darwinism is, so far as Darwin and the *Origin* are concerned,

[3] C. D. Darlington, *Darwin's Place in History*, Oxford, Blackwell, 1959.

[4] Sir Arthur Keith, *The Religion of a Darwinist*, London; Watts (*Rationalist Series*), 1925.

[5] W. R. Thompson, Introduction to *The Origin of Species* (Everyman Edition), London, Dent, 1956.

[6] Sir Alister Hardy, *The Living Stream: Evolution and Man*, London, Collins, 1966.

a 'heresy' in the literal meaning of that much-abused word.

It is clear that the *Origin* created what T. S. Kuhn[7] calls a paradigmatic situation. A paradigm is a framework of ideas, a system which, amongst other functions, determines what will and what will not be admissible in future as scientific evidence. There is currently some debate amongst philosophers of science as to the precise status of paradigms.[8] Whether they turn out to be primarily sociological forces, or metaphysical insights, or theological attitudes, one thing seems fairly certain: they are not 'scientific statements' in the inductive sense of Bacon. They are, then, at any rate, 'philosophical' in the broad sense. Darwin elaborated a very complicated paradigm. Biological scientists and medical scientists appear to have handled it in ways that illustrate both advantages and drawbacks to what one might call the 'relatively harder' and the 'relatively softer' sciences. It is possible to be hard and rigorous only about what is relatively simple. The more complex the situation, in other words the greater the number of variables involved, the larger will be the inevitable percentage-error, and the softer the result of what can only be an imperfect analysis. Nothing is to be gained from making value-judgments about such different experimental situations. Now it is clear that medicine is a 'softer' science than most. If medical scientists had been more involved in the public debate on Darwinism in the 1860s, the paradigm might not have suffered the reductions it did. Though there were more significant journals extant than the *Lancet*[9] it is of interest that the first reference to Darwin in that

[7] T. S. Kuhn, *The Structure of Scientific Revolutions*, University of Chicago Press, 1962.

[8] Margaret Masterman, in *Theoria to Theory*, 1967, **1**, 345–50.

[9] Ellegård, in the preface to his comprehensive study (Ellegård, Alvar, 'Darwin and the general reader: the reception of Darwin's theory of Evolution in the British periodical press 1859–1872', *Acta Univ. Gothoburg*. 1958, **64**, 1–394), in referring to 'general' as compared with 'purely scientific' journals says, 'In the latter, scientists concentrated on details; in the former, where they addressed a wider public, they treated the problems from a more general point of view.' He adds a footnote, of interest to the historian of medicine, 'It is significant that such a purely scientific journal as the *London, Edinburgh and Dublin Philosophical Magazine* contained no single article during the whole of the period 1859–1872 where the Darwinian theory was discussed. On the other hand, the *British and Foreign Medico-Chirurgical* (*sic*) reviewed fully the *Origin of Species*, as well as the *Descent of Man* and other books of Darwinian import.' Ellegård possibly didn't realize that the reviewer was Carpenter (a very competent scientist) who was also editor of the journal, nor that he had been, as some might put it, 'nobbled' by Darwin even before the publication of the *Origin* (see below).

journal appears to have been as late as 1866:[10] the reviewer of the new edition of Todd and Bowman's *Physiological Anatomy and Physiology of Man*[11] noted that 'a slight sketch of Darwin's theory of the origins of species is introduced, with some very sound strictures upon its general applicability'. What looks possibly like medical isolation from the general stream of science did not result from want of trying. In 1863 a *Lancet* leader[12] had said, with reference to the main forum of scientific debate at the time:

> The British Association for the Advancement of Science avoids two things—Medicine and Morals. It is so intensively and exclusively fond of the physical and the demonstrable, of what can be measured, or seen, or weighed, or put into a crucible, or converted into a fossil, that anything minus these qualities has but a poor chance of being noticed by it. Accordingly it gives the go-by to Medicine and Morals. We think this a matter for regret on two grounds. Firstly, that the subjects ignored by the British Association suffer from its neglect; and secondly that the British Association suffers by neglecting them.

Of course, then as now Medicine was constantly under attack for being unscientific. Huxley, in his address in 1866 at St. Mary's Hospital,[13] told the tale of the fight between Nature and Disease, with a blindfolded doctor hitting out with a stick, sometimes catching the one and sometimes the other. But he added a neat twist by saying that of course the doctor really was quite astute and therefore, being unable to see very much, he would normally, as a prudent man, abstain from doing anything.

I want now to refer to some of the medical men of the period who did react to Darwin, and who reacted on him. They were important, to my mind, in preventing Darwin from becoming a Darwinist. They constantly reminded him that the simple formula of 'chance followed by blind necessity' was unlikely to prove adequate to handle all the phenomena that the doctor sees in practice. I limit my remarks to medical scientists not only because of the nature of this Conference, but also because recent publications have covered fairly well the reactions of non-medical scientists and others, whereas

[10] Anon., *Lancet*, 1866 (ii), 185.

[11] R. B. Todd, W. Bowman, and L. S. Beale, *The Physiological Anatomy and Physiology of Man*, Part I, London, Longmans, 1866.

[12] Anon., *Lancet*, 1863, ii, 368.

[13] Thomas Henry Huxley, 'The Relationship of Physical Science to Medical Science and Education', reported in the *Lancet*, 1866, i, 521.

there is a curious dearth of recent comment on medical men like Bastian, Carpenter, Beale, Humphry, Virchow, Maudsley, Gairdner, Lawson Tait and Paget, all of whom were certainly involved in the private debate, and sometimes in its public manifestations. Furthermore, there was Blackley, whose classic work on hay-fever[14] owed much to Darwin. And Ross's long-forgotten book *The Graft Theory of Disease*[15] contains, amongst much nonsense, an excellent account of the development of vaccination-reactions, both local and systemic, and includes an attempt to explain the latter on the basis of Darwin's 'gemmules', those microscopic carriers of biological information, as we would now say, which were an integral part of his evolutionary theory. There is much to be said (and I hope to say it some day) about this neglected part of Darwin's theory. For the moment it might just be noted that this idea, for confirmation of which he searched his presentation copy of Virchow's book in 1860,[16] is something like the modern biochemical interpretation of immunity and of the auto-immune diseases. For the time, gemmules were masked, or overwhelmed, by germs, more popular by far as the cause of disease and of its spread. In the matter of survival of scientific ideas a period of hibernation is rarely fatal. A temporarily-eclipsed hypothesis may find itself eventually rediscovered in a safe ecological niche, or even, as with those of Mendel, elevated to the heights. Darwin's gemmules have many counterparts in current biological theory. He had used the concept privately since about 1840[17] in his search for the explanation of biological variation. He always preferred, like any self-respecting scientist who does not prejudge and preclude analysis because of personal philosophical

[14] Charles H. Blackley, *Experimental Researches on the Causes and Nature of Catarrhus Aestivus* (*Hay-Fever or Hay-asthma*), London, Bailliere, Tindall & Cox, 1873. The copy in the Darwin Library in Cambridge is inscribed 'To Chas. Darwin Esq., M.A., F.R.S. etc. etc. with the author's compliments.' It was closely read and annotated by the recipient.

[15] James Ross, *The Graft Theory of Disease, being an Application of Mr. Darwin's Hypothesis of Pangenesis to the Explanation of the Phenomena of the Zymotic Diseases*, London, Churchill, 1872. The Darwin Library copy is inscribed 'from the author.'

[16] Rudolph Virchow, *Cellular Pathology as based upon Physiological and Pathological Histology*, 2nd ed., trans. by F. Chance, London, Churchill, 1860. The Darwin Library copy is inscribed 'Charles Darwin, Esq., F.R.S., with the compliments of the Author and of the Translator.'

[17] *The Life and Letters of Charles Darwin* (hereafter LLD) ed. Francis Darwin, 3 vols., London, Murray, 1887. Vol. iii, p. 72, letter of 22 August 1867 to Lyell.

predilections, to look for the causes of things and their ordering, rather than throw in the sponge, as so many have done on the question of variation, and ascribe it merely to chance.

Darwin had many close connections with medical men. There is no indication that his regard for his father's profession was, in general, ever less than profound. He had studied in detail Lawrence's famous 1819 lectures on Man,[18] where noble blows were struck for scientific freedom against the metaphysicians and theologians. Lawrence also said (wisely it might now be thought), with regard to the speculation that 'man and monkey, or at least the orang-utang, belong to the same species' (as propounded by Monboddo and Rousseau), that it was a notion 'as false, philosophically, as the moral and political consequences, to which it would lead, are shocking and detestable'. One thinks of Huxley's irresponsible boast about his working-class audiences two generations later, 'by next Friday evening they will all be convinced that they are monkeys'.[19] Darwin must not, of course, be held to account for all or even many of his 'general agent's' sparkling witticisms, the frequent asperity of which caused the *Lancet* to say in 1862, that 'the fling and the sneer, however smart, will only recoil upon himself'.[20] But Darwin was not averse to applauding fisticuffs from afar, as when he wrote to congratulate Huxley on his 1879 preface to Haeckel's new book, 'It is capital, and I enjoyed the tremendous rap on the knuckles which you gave Virchow at the close. What a pleasure it must be to write as you do'.[21] Debunking is an old method of satisfying aggressive instincts, and Huxley was a past master at it.

The distinguished physician Sir Henry Holland was a friend of Darwin's. Somewhat to the latter's surprise, he became a supporter (with reservations) of the evolutionary hypothesis. When Darwin was soliciting reviews of the *Origin* prior to publication he wrote to W. B. Carpenter, an important physiologist who did much to

[18] William Lawrence, *Lectures on Physiology, Zoology and the Natural History of Man*, London, 1819 (1st ed.), subsequently reprinted many times although the author was forced to withdraw his sanction (see Darlington op. cit.).

[19] *Life and Letters of Thomas Henry Huxley*, by Leonard Huxley. 3 vols., London, Macmillan, 1908. Vol. 1. p. 276, letter of 22 March 1861 to his wife.

[20] Anon., *Lancet*, 1862, ii, 487.

[21] *More Letters of Charles Darwin* (hereafter MLD), ed. Francis Darwin and A. C. Seward, 2 vols., London, Murray, 1903, Vol. 1, p. 383, letter No. 294, 19 April 1879 to T. H. Huxley.

popularize the general theory of evolution within the medical profession. Darwin warned him,[22] 'You will have a tough job even to shake in the slightest degree Sir H. Holland'. But in 1868 he could write to Hooker,[23] concerning his 'beloved child' pangenesis, which he feared might be stillborn, 'Old Sir H. Holland says he has read it twice, and thinks it very tough; but believes that sooner or later "some view akin to it" will be accepted'. This judgment coincided with Darwin's own, not only in 1868 but throughout the subsequent long years of neglect of pangenesis. Perhaps it is only now, a century later, that it will be vindicated by the molecular biologists.

Towards the end of the decade Darwin read and annotated the books of Maudsley,[24] the neurologist. This fact, together with the presence of other annotated medical works in the Darwin Library at Cambridge, show how closely he followed the development of scientific medicine. In keeping with his distaste, by then, for philosophy, is his neglect of the section in Maudsley's 1870 book entitled *The Limits of Philosophical Enquiry*. This was a well-argued protest at the manipulation of science to serve the ends of the positivist philosophy of Comte, a protest to which Huxley also contributed[25] because of the charges of positivism which were threatening the Darwinian camp. It is a pity that Darwin failed to read Maudsley on the dangers of 'identifying the character of an epoch of thought with the doctrines of some eminent man who has lived and laboured and taken the lead in it.' Not that Darwin could have stopped the process where he himself was concerned: long before the end of the decade the matter was well out of his hands and into those of his followers.

One of the first scientists to give an appreciative but critical comment in public on the *Origin* was the medical botanist, Daubeny of Oxford. It is well known that a meeting was held in Daubeny's rooms after the notorious Huxley-Wilberforce clash at the British

[22] LLD, vol. ii, p. 222, letter of 18 November 1859 to W. B. Carpenter.

[23] LLD, vol. iii, p. 78, letter of 23 February 1868 to Hooker.

[24] Henry Maudsley, (1) *The Physiology and Pathology of Mind*, London, Macmillan, 1867. (2) *Body and Mind: an Enquiry into their Connection and Mutual Influence, specially in Reference to Mental Disorders*, with appendix, London, Macmillan, 1870.

[25] Thomas Henry Huxley, 'The scientific aspects of positivism', *The Fortnightly Review*, 1869, reprinted in *Lay Sermons, Addresses and Reviews*, London, Macmillan, 1870.

Association meeting in 1860. Because of this it has sometimes been assumed (e.g. by Himmelfarb[26]) that Daubeny was one of the early band of disciples. But his own paper[27] at that meeting was a careful criticism on purely scientific grounds, with insistence that theologians should keep out of the discussion. He was concerned about Darwin's problems over the origin of variation and the mechanism of its inheritance. This paper shows that Sir Gavin de Beer, in his recent book on Darwin,[28] is plainly wrong in saying that Darwin anticipated classical genetics. De Beer says 'without the benefit of this modern knowledge, Darwin had put his finger on the importance of variation and the fact that it results from sexual reproduction.' But in fact it was Daubeny who countered Darwin by saying about plants that 'whilst in seeds variation is the rule, in buds it is the exception'. Darwin was not put off, and after his book on *Variation* was published he reiterated in a letter in 1868,[29] 'By the way, let me add that I discussed bud-variation chiefly from a belief which is common to several persons, that all variability is related to sexual generation; I wished to show clearly that this was an error.' If the occupational disease of historians is a kind of diplopia, that leads one to look to the future when one thinks one is looking to the past, it may be that the disorder is more severe for one who has been concerned, like de Beer, with actually influencing future attitudes. Of course, this author takes us only to what has been called the classical or first stage of genetics. According to Michie[30] we are already in the third stage. Who will turn out to be right when we enter the fourth stage

[26] Gertrude Himmelfarb, in *Darwin and the Darwinian Revolution*, London, Chatto and Windus, 1959.

[27] Charles Daubeny, '*Remarks on the final causes of the sexuality of plants, with particular reference to Mr. Darwin's work on the origin of species. Being the substance of a paper read before the Natural History Section of the British Association for the Advancement of Science, at the meeting held at Oxford in 1860*, Oxford, Parker, 1860. Reprinted in *Miscellanies*, a two-volumed collection of Daubeny's essays published by Parker 1867. This collection (vol. 2) includes a judicious summary, in an 1865 address, of the views for and against Darwinism: on p. 196 he says, 'Still, looking at the Darwinian theory, as alone it ought to be regarded, simply with reference to its scientific merits, there is much to induce us to suspend our judgment until further evidence be afforded.'

[28] Sir Gavin de Beer, *Charles Darwin: Evolution by Natural Selection*, London, Nelson, 1963, p. 86.

[29] LLD, vol. iii, p. 86, letter of 23 June 1868 to G. Bentham.

[30] Donald Michie, 'The third stage in genetics', in *A Century of Darwin*, ed. S. A. Barnett, London, Heinemann, 1958.

is anybody's guess. But the story points to the dangers of all rigid orthodoxies in science.

Darwin corresponded with Lawson Tait, the Birmingham gynaecologist who sent him the results of experiments on the adaptive value of the mouse's tail,[31] and with Sir James Paget, whose 1868 letter of thanks for the *Variation* volume is worth quoting for its comment on medicine in that decade: 'I expect to be made even more than I am now ashamed of my ignorance (and I fear I may add that of my profession too) on the influence of inheritance on the variations and mixtures of diseases. But I hope that my deeper shame may be the beginning of deeper knowledge.'[32]

Three of Paget's former pupils were professors of anatomy during this period: Rolleston at Oxford, who supported both Darwin and Huxley publicly (though again with reservations) throughout the decade; Turner at Edinburgh, who corresponded with Darwin and gets a mention in the published letters—though the important ones were unknown until 1919, when Turner's biography was published; and Humphry at Cambridge, about whom, for all his eminence in medicine and science, in medical education and in the 1870s campaign (with Darwin and Huxley) about Vivisection, there is a curious silence in most of the literature. Humphry is a worthy subject for a medical historian. In this paper I can quote from only one of his publications. His 1866 presidential address[33] to the Section of Physiology of the British Association was used to inaugurate, in 1867, the highly-respected *Journal of Anatomy and Physiology* of which he was one of the founder editors. With regard to the physical changes that would be necessary to transform, by evolution, the foot, brain and larynx of ape to man, he says, 'it is possible that such changes might be effected. One would fancy it probable; but we have at present too little right to assume it.' No one today, of course, would be prepared to argue the particular case that he was challenging. Later in the address he says, 'Neither do I think that much direct assistance has been given by the theory of Natural Selection based upon the Struggle for Existence, ably propounded

[31] MLD, vol. 1, p. 358.

[32] *Memoirs and Letters of Sir James Paget*, ed. Stephen Paget. 3rd ed. London, Longmans Green, 1903, p. 414, letter to Darwin dated 29 January 1868.

[33] G. M. Humphry, 'Address in Physiology, delivered at the Meeting of the British Association at Nottingham', *J. Anat. Physiol. Lond.*, 1867, **1**, 1–14.

after long and careful research and ably defended as it has been. It has dispersed some of the fallacies and false objections which beset the idea of transmutation of species and has placed the question in a fairer position for discussion; but it reminds us forcibly of some of the real difficulties and objections.'

This was the sort of criticism that Darwin had rightly feared. Huxley's polemical role was essential in shouting it down, and in persuading the public over the heads of scientists. Whether the success of the Darwinism that emerged was really in the interests of science is very debatable. Darwin had originally hoped and expected that his theory would act as a great incentive to research; so it did, but because of its final emasculated form of 'chance and blind necessity', the research tended to be sterile, because purely descriptive. Picken showed in 1956[34] that the work of the great Wilhelm His was gravely hampered by orthodox Darwinists, who saw nothing to investigate in his science of experimental embryology; and the absurd lengths to which unscientific speculation on Mimicry was taken, formed one of the grounds for Thompson's criticisms in the same year.[35] A paradigm which excludes genuine investigation and includes only what is either merely descriptive or phoney is always a danger to the advancement of science.

As a last example, though, of a medical man whose influence assisted Darwin in the 1860s we might take Sir William Gairdner. He was Professor of Medicine in Glasgow alongside Lister. This was also when the erroneous computation, by another Glasgow colleague, the future Lord Kelvin, of the physical age of the earth, put the whole Darwinian theory temporarily into jeopardy at the end of the decade. Gairdner's published addresses given to medical students in 1855, 1866, and 1882[36] show his admiration for the natural sciences and for Darwin's work in particular. But, like other medical scientists, he always placed the narrow view of the Darwinists within the larger paradigm that modern research is busy constructing. Gairdner's eulogy of Darwin in his 1888 address[37] as

[34] Laurence Picken, 'The fate of Wilhelm His', *Nature, Lond.*, 1956, **178**, 1162–65.

[35] W. R. Thompson, op. cit.

[36] Sir William Tennant Gairdner, *Medical Education, Character and Conduct*, Glasgow, Maclehose, 1883.

[37] *Idem.*, 'The physician as naturalist', *Brit. med. J.*, 1888, **ii**, 275–84.

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President of the British Medical Association is one of the most profound assessments of the status of the 'Origin' of the century. When historians eventually reach a conclusion about Darwin's position, it will surely be on one of those very high peaks that are reserved for men of both genius and integrity.

The Dawn of the Germ Theory: Particles, Infection and Biology

by

J. K. CRELLIN

THE GERM theory of infectious diseases was firmly established in the 1870s and 1880s through studies such as those by Koch and Pasteur which indisputably linked a specific organism with a specific disease.[1] But the 1860s provided an extremely important—probably a necessary—prelude to this. I will refer later to some of the evidence published in support of the germ theory during the 1860s but I wish, in this paper, to concentrate on conflicting theories of infectious diseases involving particles, and on the subject of spontaneous generation.

I will also be stressing work in Britain; much has been written on the nineteenth-century development of biology (and, in particular, physiology) in France and Germany as a result of growing attempts to explain physiological processes in physical and chemical terms, and this set me wondering about the contributions of British workers in the decade or so following the impetus of Darwin's *Origin of Species*.[2] It also seemed appropriate to examine the British scene in view of the numerous statements made at the time, suggesting a new scientific awareness in medical research. To give but one example, *The Quarterly Journal of Science* strikingly wrote in 1864:

> If we were asked to state what it is that more especially characterizes the scientific Practitioner of Medicine of our own day, we should

[1] This was anthrax. For a useful account of this and related work see H. A. Lechevalier and M. Solotorovsky, *Three Centuries of Microbiology*, New York, 1965.

[2] An outline of the scene in France and Germany, with references to recent publications, can be found in E. Mendelsohn's 'Physical models and physiological concepts: explanation in nineteenth-century biology', *Brit. J. Hist. Sci.*, 1965, **2**, 201–19.

> state it to be the strong desire whereby he is actuated to investigate the conditions which lead to the production of disease . . . The modern physician does not waste his energies or burn the midnight lamp in anxious strivings after the philosopher's stone.[3]

While the 1860s saw increasing interest in the germ theory, relatively little was accomplished in undermining the conflicting theories of disease which were based on infectious particles of matter and which reflected current biological thinking. Even so, these theories were not entirely inimical to the germ theory, for not only were they largely theories of infectious particles—thus creating a favourable climate for considering whether the particles could be micro-organisms or not—but they also helped to demolish the widely-held idea that infectious diseases could arise spontaneously through miasmata such as emanated from filth.[4]

PARTICLES AND THE BIOLOGICAL BACKGROUND

I do not think it would be inaccurate to label the medical and biological world of the 1860s, 'the era of the particle': one reads, for example, of biads, bioplasts, gemmules, germs, grafts, globules, oleo-albuminous molecules, morbific ferments, physiological units, and viruses. While some of these particles were essentially concerned with theories on the development of tissues, on heredity and on the spontaneous generation of micro-organisms, they nevertheless provided biological background which was not infrequently called upon in the discussions on infection and spontaneous generation. I feel, therefore, that it will be useful to sketch in, albeit briefly, *some* of this background if only to give point to my belief that general interest in particles promoted consideration of infectious particles and the question of spontaneous generation.

It is not altogether surprising that the 1860s show a preoccupation with particles even though the subject was by no means new—for

[3] *Quart. J. Sci.*, 1864, **1**, 163.

[4] The idea of miasmata from filth was held, for example, by the reforming sanitarians of the day. A helpful résumé of the influence of miasmata can be found in E. H. Ackerknecht's 'Anticontagionism between 1821 and 1867', *Bull. Hist. Med.*, 1948, **22**, 562–93.

example, a number of writers traced their views back to Buffon's eighteenth-century idea of organic living molecules.[5]

But a tremendous stimulus was given, in 1839, by Schwann's theory that the cell was the primary, fundamental unit of matter. One aspect of this theory was that cells could form spontaneously in a nutrient material or 'cytoblastema' by new molecules being deposited around a 'nucleus', and the outer ones ultimately consolidating into a membrane.[6] Mendelsohn believes that Schwann's ideas on cell formation were the most fruitful aspect of his theory[7] and it certainly influenced John Hughes Bennett, Professor of Medicine at Edinburgh. Bennett clearly believed that the molecules referred to by Schwann were in fact the basic morphological units of the organism and not the cells: 'The ultimate parts of the organization [he wrote] are not cells nor nuclei, but the minute molecules from which these are formed.'[8] Bennett formulated his views as the 'molecular theory of organization'[9] and, what is important for the present story, believed that the molecules could develop spontaneously into micro-organisms (see p. 70).

But the cell theory was principally undermined in the 1860s through the growing belief that protoplasm, not the cell, was the essential living 'unit', a belief that was coupled with growing scepticism of the importance of the cell wall. Interest in protoplasm played a significant part in the particle story for it allowed the theoretical possibility that microscopic particles could possess the properties of living matter. This idea, in the hands of men like Beale (to whom

[5] In connection with the spontaneous generation of micro-organisms, E. Parfitt, for example, wrote (*Mon. Microscop. J.*, 1869, **2**, 254): 'we have [in the development of life] something similar to that which Buffon alludes when speaking of "wandering molecules". For when the portions of the organisms have become disintegrated or broken down, the molecules and cells of which they were formed are set free, and it is to the study of these in their separate and also in their aggregate forms that I desire to direct attention'.

[6] See T. Schwann, *Microscopical Researches into the Accordance in the Structure and Growth of Animals and Plants*, Eng. trans. by Henry Smith, London, 1847, p. 193.

[7] See p. 421 of E. Mendelsohn's 'Cell theory and the development of general physiology', *Arch. Int. Hist. Sci.*, 1963, **16**, 419–29.

[8] J. H. Bennett, *Clinical Lectures on the Principles and Practice of Medicine*, 5th ed., Edinburgh, 1868, p. 118.

[9] Bennett expounded this theory in many papers but a convenient account occurs in the 5th ed. of his *Clinical Lectures* (footnote 8). It should perhaps be added that he had fully developed his theory by 1861: see his 'On the molecular theory of organization', *Proc. R. Soc. Edin.*, 1857–62, **4**, 436–46.

I shall refer later), was relevant to the problems of infection and of spontaneous generation.

Also important in stimulating interest in particles was the work of Herbert Spencer and of Charles Darwin. Spencer's enormously popular *Principles of Biology* (London, 1864–67) endeavoured to rationalize biological phenomena in physico-chemical terms. One of his most important concepts—by which he explained, for example, tissue repair—was the idea of 'physiological units' which were formed from combinations of chemical units.[10] It is not appropriate to consider here whether it was generally accepted that these particles adequately explained phenomena in physico-chemical terms,[11] but it is relevant that Henry Charlton Bastian believed Spencer's physiological units bolstered arguments in support of spontaneous generation.[12]

These physiological units were not unrelated to Darwin's particles (gemmules) which were the basis of his 'hypothesis of pangenesis', an hypothesis which implied 'that the whole [organism, that is], every separate atom or unit reproduces itself'.[13] According to Darwin this was accomplished by each cell throwing off minute granules or atoms (i.e. gemmules), an idea not unrelated to Beale's particles of living protoplasm (to be discussed below).[14] It is not easy to say what influence Darwin's theory had outside its relevance to heredity but it certainly contributed to Ross' theory of disease (see p. 66). Furthermore Darwin's discussions on the 'independent life of each minute element of the body' was strongly suggestive for the general importance of particles.[15]

[10] *The Principles of Biology*, London, 1864, vol. 1, p. 183.

[11] Mention need only be made here of an Appendix generally bound with volume 1 of *The Principles of Biology:* 'On Alleged "Spontaneous Generation", and on the Hypothesis of Physiological Units', in which Spencer answers his critics and elaborates on the idea of physiological units.

[12] H. C. Bastian, *The Beginnings of Life*, London, 1872, vol. 2, pp. 22–23.

[13] *The Variation of Animals and Plants under Domestication*, London, 1868, vol. 2, p. 358.

[14] In the draft of his theory of pangenesis, written during the first half of 1865, Darwin expressly states that the superabundant atoms or gemmules were formed from protoplasm (see pp. 258–59 of R. C. Olby, 'Charles Darwin's manuscript of pangenesis', *Brit. J. Hist. Sci.* 1963, **1**, 251–63). But Darwin was not so explicit in his account in *The Variation of Animals and Plants under Domestication*, p. 374. In 1868 Darwin, not surprisingly, was well aware of Beale's work, ibid., p. 370.

[15] *The Variation of Animals and Plants under Domestication*, London, 1868, vol. 2, pp. 369–71.

THEORIES OF INFECTION

The first theory of infectious diseases which I wish to discuss is not connected directly with any of the background I have just outlined but, nevertheless, it is equally a child of the times, reflecting the considerable interest in the application of chemistry to physiological problems. The theory was that of zymosis which, in its simplest form, likened diseases to fermentations. This analogy was an old idea, but it came into prominence largely through Justus Liebig's physico-chemical theory of fermentation which he published shortly after Latour, Kützing and Schwann had proposed, in the 1830s, that fermentation was due to a living organism, yeast.[16] Liebig believed that fermentation resulted from molecular excitation of decomposing nitrogenous matter and held that the progress of all infectious diseases consisted of two stages: one in which decomposition was set up in the blood, and two, in which the 'ferment' itself multiplied.[17]

There is, I think, little doubt that the most far-reaching feature of this theory was the importance attached to self-reproducing particles of *organic* matter for it was this feature that was developed.[18] William Farr, for example, the famed medical statistician who coined the term 'zymosis',[19] quickly gave the theory greater precision by emphasizing that the multiplying particles—of which he admitted the chemical composition was unknown—were specific for each disease; he called them *zymes*, listing twenty-four different ones which included, varioline, vaccinine, and equinine.[20]

Remembering the contemporary interest in physico-chemical explanation and the stature and influence of Liebig, his widespread influence on the subject comes as no surprise. It is interesting to see the hold Liebig's views had on one of his students, the Scotsman

[16] A useful general account of the history of fermentation can be found in W. Bulloch's *The History of Bacteriology*, London, 1938, pp. 41–63.

[17] See Liebig's *Organic Chemistry in its Applications to Agriculture and Physiology*, London, 1840, p. 373.

[18] When organic matter is referred to, the context usually allows the inference of particles (e.g., analogy with yeast fermentation which was stressed by Liebig, cf. footnote 17). But Liebig himself left the question open by also mentioning 'gaseous contagious matter' (op. cit., p. 375).

[19] The term 'zymotic' diseases became popular for infectious diseases although they were sometimes referred to as catalytic diseases. The latter term, however, also embraced such ideas as Richardson's Glandular Theory (see below).

[20] Appendix to the *Fourth Annual Report of the Registrar-General of Births, Deaths and Marriages*, London, 1842, pp. 199–205.

Robert Angus Smith, a first-class chemist and first inspector under the Alkali Act of 1863. Smith accepted Pasteur's germ theory of fermentation and putrefaction, but in 1872 was, justifiably I think, reluctant about the germ theory of disease. He somehow felt that the correct answer would incorporate the work of both Pasteur and Liebig.[21]

But it is important to remember that the theory of zymosis had a conspicuously fundamental weak point: what was the nature of the reproducing particle? While many unquestionably accepted that it was a poison, Benjamin Ward Richardson, for example, pointed out in 1859 that 'no evidence whatsoever has been adduced to indicate the actual presence of a cell or other mark of organic growth in the poisons of disease, as in the virus of small-pox.'[22] Richardson compromised by suggesting that a particle catalysed an alteration in the normal animal chemistry which in turn produced chemical poisons giving rise to the disease symptoms.

Richardson, in fact, extended this chemical approach so far that he developed virtually an independent theory.[23] This theory, the second one opposing the germ theory which I wish to mention, became known as the glandular theory. Richardson put all the blame on organic, poisonous secretions which were derived ultimately from 'albumen'.[24] These poisons catalysed, in the infected subject, the formation of similar substances which caused the disease symptoms.

[21] See especially Smith's *Disinfectants and Disinfection*, Edinburgh, 1869, pp. 17–29, and his *Air and Rain*, London, 1872, pp. 478–504. It is not without interest to quote the dedication of *Air and Rain:* 'To Justus von Liebig this volume is inscribed, not merely as a proof that I still admire him, as I did when young and listening to his teaching, but also to remind my countrymen how much we owe to his genius and labours'.

[22] See p. 23 of 'On the theory of zymosis', *Trans. epidem. Soc.*, 1860, **1**, 20–30.

[23] I do not think it is quite accurate to call Richardson's theory a 'theory of zymosis' see A. S. MacNalty, *A Biography of Sir Benjamin Ward Richardson*, London, 1950, p. 53. Richardson himself commented that there was no 'foundation for the theory of zymosis in the common acceptation of the term', (*Lancet*, 1865, **ii**, 166).

[24] See *On the Poisons of the Spreading Diseases: Their Nature and Mode of Distribution*, London, 1867, p. 16. For other discussions of the theory see *Lancet*, 1865, **ii**, 165–66; *Trans. epidem. Soc.*, 1867, **2**, 424–34. *Nature, Lond.*, 1877, **16**, 480–86. Richardson continued to uphold this theory until his death in 1896. See his *Vita Medica; Chapters of Medical Life and Work*, London, 1897, pp. 449–52.

Richardson's views were part of a fairly widespread belief that chemical substances ('poisons') caused disease. Bulloch, in his *The History of Bacteriology*, London, 1938, pp. 129–35, has touched on the subject but ignores the interest in it of British workers which included such notable figures as J. L. W. Thudichum.

It is interesting that Richardson once pointed out that he was only modifying John Snow's celebrated views that cholera was spread by a 'cholera cell'. He said Snow was handicapped by a firm belief in the idea that every cell must be derived from an existing cell for he (Snow) had lacked knowledge of 'those advances in physical medicine that have developed since he died'.[25] In other words, Richardson was referring to the rising interest in protoplasm and scepticism of the cell, but he himself was principally influenced by developments in chemistry and found additional support in the work on alkaloids in human tissues[26] by Henry Bence Jones.[27]

One can well sympathize with Richardson's chemical approach—which was held in one form or another by other workers[28] —for he believed he could successfully isolate the 'poison' of hospital fever in a solid form, and hence his theory seemed to solve or at least point to a solution of the problem of the precise nature of the 'ferment' of the zymotic theory.[29] It should he added, however, that Richardson still allowed the possibility of the transmission of the poison in a 'vaporous form'[30] though he explicitly denied the commonly held belief that smells from sewage, etc. caused disease.[31]

The problem of the nature of the particle was resolved in a different way by the third theory I wish to consider. This theory was put forward by the prominent microscopist of King's College, London, Lionel S. Beale, who believed that 'disease germs' were *living*

[25] *Trans. epidem. Soc.*, 1867, **2**, 431.

[26] Ibid.

[27] Bence Jones was an ardent and influential physiological chemist and one-time pupil of Liebig. He accepted Liebig's theory of fermentation and its application to infectious disease, but his chemical approach is best exemplified in his book *Lectures on Some of the Applications of Chemistry and Mechanics to Pathology and Therapeutics*, London, 1867, where he classified the zymotic diseases as diseases of peroxidation.

[28] Burdon Sanderson, for example, using an analogy of enzyme action commented that the 'faculty of causing chemical changes in other contiguous matter is all that is implied when the process of infection is compared to a zymosis' see p. 231 of 'Introductory Report on the Intimate Pathology of Contagion', Appendix to *Twelfth Report of the Medical Officer of the Privy Council, 1869*, London, 1870.

[29] *On the Poisons of the Spreading Diseases: Their Nature and Mode of Distribution*, London, 1867, pp. 10–11.

[30] See, for example, ibid., p. 11.

[31] Ibid., pp. 8–9.

particles of degraded bioplasm (protoplasm)[32] which were derived from normal bioplasm. The characteristics of the germs, which could be as small as 1/100,000 inch in diameter,[33] arose from their property of 'vital' movement ('quite distinct', said Beale, from 'every other kind of movement known').[34] The germs—not, he emphasized, to be confused with living micro-organisms—were specific for each disease, were transmitted through the air, and could readily pass into the blood where they initiated the disease.[35] The most far-reaching feature of Beale's theory was that for the first time there was a clear claim that *recognizable* particles—not, for instance, the *postulated* zymosis-producing particles of albumen—caused disease (though it is true that they could not be visibly distinguished from similar particles which were non-infectious).[36] Beale, in fact, supported his views with illustrations of particles, notably in his two books: *Disease Germs; their Supposed Nature* (London, 1870), and *Disease Germs; their Real Nature* (London, 1870). But his views had become well known before this, especially through his report 'Microscopical Researches on the Cattle Plague' (1866)[37] where he claimed that the cattle plague contagium consisted of 'minute particles of matter in a living state, each capable of growing and multiplying rapidly when placed under favourable conditions'.[38]

It is of interest that in another report on the cattle plague William Crookes put forward similar views to those of Beale, though he allowed the possibility that the infectious matter might be a living cell, the view held earlier by John Snow (p. 63).[39]

An accurate assessment of the general influence of Beale's theory is not easy for one wonders how his special 'vital' force, which he

[32] Beale always used the term 'bioplasm' refusing to employ the word 'protoplasm' because he maintained that it had been misapplied by others to, for example, the contractile tissue of muscle, and the axis cylinder of nerve fibres. (See Beale's *Bioplasm: an Introduction to the Study of Physiology and Medicine*, London, 1872, pp. 8–9.)

[33] Beale, *Disease Germs: their Real Nature*, London, 1870, p. 44.

[34] E.g. ibid., pp. 15–16.

[35] Ibid., pp. 80–92.

[36] Ibid., pp. 74–76.

[37] Appendix to the *Third Report of the Commissioners Appointed to Inquire into the Origin and Nature of the Cattle Plague*, London, 1866, pp. 129–54.

[38] Ibid., p. 152.

[39] p. 187 of 'On the application of Disinfectants in arresting the Spread of the Cattle Plague', Appendix to the *Third Report of the Commissioners appointed to Inquire into the Origin and Nature of the Cattle Plague*, London, 1866, pp. 187–201.

believed characterized life, was generally accepted.[40] Nevertheless there is little doubt that his theory—probably along with Antoine Béchamp's theory of particles or microzymas[41]—stimulated John Burdon Sanderson, one of the leading research workers of his day. Burdon Sanderson produced *experimental* results which, for the first time, seemed to prove that the particles were the infectious factors in disease; he repeated pioneering experiments by Chauveau and separated, by diffusion, the particles in vaccine lymph. He then showed that solutions without the particles were non-infectious but that solutions containing them were active.[42] Such particles were, he wrote, 'spheroidal, transparent, of gelatinous consistence, of density nearly equal to that of the animal liquids in which they float, and that they are mainly, but perhaps not exclusively composed of albuminous matter'.[43]

Beale's influence is also seen in the small pamphlet by James Morris: *Germinal Matter and the Contact Theory*, London, 1867. This, essentially a restatement of Beale's views, stressed that the 'minute portions of organic matter . . . constantly thrown off by animals and men . . . are either living or contain living matter [and have] the power of exciting living action similar to its own in suitable material.'[44]

(It is relevant to add that during the 1860s there was considerable interest in searching for airborne germs and though this provided little support for the germ theory it did reveal the presence of a large

[40] There was undoubtedly much sympathy with his views, see for example a review of Beale's *Life Theories: their Influences upon Religious Thought* in *Quart. J. Sci.*, 1872. **2**, 93–96, which opens with the statement that 'The physical theory of life, despite its many advocates, and its present vigorous propagation, seems destined to decline at no distant period'.

[41] Béchamp believed that life resulted from the activity of microsopic granules which he called microzymas. These appear to be related to the molecules of Schwann for Béchamp thought that protoplasm was composed of them. Microzymas in a degraded state were held to cause disease.

[42] 'Introductory Report on the Intimate Pathology of Contagion', Appendix to the *Twelfth Report on the Medical Officer of the Privy Council, 1869*, London, 1870, pp. 229–56. Burdon Sanderson found that in 19 children vaccinated with 57 samples of diffused lymph (i.e. lymph, which had been in contact with water for 24 hours) only 5 reactions occurred. However 57 samples of ordinary lymph produced 47 positive results.

[43] Ibid., p. 255.

[44] *Germinal Matter and the Contact Theory*, pp. 7 and 11.

amount of organic matter in the air thus lending support not only to Beale but also to the general theory of zymosis.[45])

Germs of bioplasm also influenced James Ross, who wrote *The Graft Theory of Disease being an Application of Mr. Darwin's Hypothesis of Pangensis to the Explanation of the Phenomena of the Zymotic Diseases*, London, 1872. Ross, like Beale, believed that infectious particles were living, but, bearing in mind Darwin's theory of gemmules, he thought that they affected the body in an analogous way to that of a grafted scion on a stock—hence the 'graft theory'. Ross explained the subsequent disease symptoms as the result of the activity of gemmules produced, for example, from cells in the vesicles of smallpox.[46] The theory was attractive. William Roberts of Manchester, who did so much to overcome the idea of spontaneous generation, wrote in 1877 that contagium 'can only be one of two things—either it is an independent organism (a parasite) multiplying within the body or on its surface, or it is a morbid cell or mass of protoplasm detached from the diseased body and *engrafted* in the healthy body.'[47]

SPONTANEOUS GENERATION

I wish now to consider a fourth theory which opposed the germ theory, that of spontaneous generation. Beale's influence is again relevant in that he contributed greatly to the rising interest in protoplasm—'the physical basis of life' as Huxley called it in 1868.[48] It was protoplasm, as an adjunct to Darwin's theory of evolution and developments in protozoology, that stimulated much argument and confusion on the question of the alleged everyday spontaneous

[45] I have outlined some of the work on the search for germs in 'Airborne particles and the germ theory: 1860–1880', *Ann. Sci.*, 1966, **22**, 49–60.

It should also be added that some of this organic matter was believed to be volatile thus lending support to the 'filth theory of disease'. (See, for example, G. Robinson, 'On the Nature and Varieties of Organic Effluvia', *Report of the British Association for the Advancement of Science*, 1863, London, 1864, pp. 120–22.)

[46] *The Graft Theory of Disease*, p. 112.

[47] See *On Spontaneous Generation and the Doctrine of the Contagium Vivum* London, n.d. [c. 1877], p. 19 (italics added). Roberts also stated 'the graft theory —which has been so ably developed by my friend Dr. Ross, I will only say that it has not, as yet, emerged from the region of pure speculation'.

[48] Huxley's lecture 'On the Physical Basis of Life' which was widely published gave a great impetus to the study of protoplasm. C. S. Blinderman (in 'Thomas Henry Huxley', *Scientific Monthly*, 1957, **84**, 180) notes that Huxley earned the sobriquet 'Huxley the Moleculite' following the address.

generation of micro-organisms and of the origin of life. W. S. Gilbert reminds us of this when he makes Pooh Bah, in the Mikado, say: 'I can trace my ancestry back to a protoplasmal primordial atomic globule'.[49]

Before looking at the argument and confusion I want to say briefly something about the relevance of spontaneous generation to the subject under discussion. Until the second half of the 1870s support for spontaneous generation of micro-organisms remained a severe handicap to establishing the germ theory, for one of the significant features of the germ theory was that it denied altogether the spontaneous generation of organisms and hence of disease. While the theories I have mentioned restricted the possibility of the spontaneous origin of infectious diseases they did not deny it entirely: disease through fermentation, as a result of internally produced particles of 'ferment', was considered to be a possibility.[50] Richardson thought that cholera poison could spring up *de novo*,[51] and Beale believed some infectious diseases 'sometimes originate in an isolated population living under certain conditions adverse to health'.[52] This was in addition to the common belief in the spontaneous origin of disease through miasmata, and in 1864 William Budd felt constrained to say that a 'very large, and by far the most influential school in this country—a school which probably embraces the great majority of medical practitioners and the whole of the "sanitary" public,—holds . . . and teaches that sundry (disease) poisons are constantly being generated *de novo* by the material conditions which surround us'.[53]

The spontaneous generation of micro-organisms versus the germ theory controversy was essentially a practical problem concerning such questions as the thermal resistance of micro-organisms and their dissemination in air and water, problems which were only

[49] Quoted in A. Hughes, *A History of Cytology*, London and New York, 1959, p. 50.

[50] See, for example pp. 6–7 of 'On Fevers' by H. Bence Jones, an article reprinted in *The Retrospect of Medicine* (ed. by W. &. J. Braithwaite), 1866, 53, pp. 1–7.

[51] *On the Poisons of the Spreading Diseases: Their Nature and Mode of Distribution*, London, 1867, p. 19.

[52] *Disease Germs: their Real Nature*, London, 1870, p. 173.

[53] *Brit. med. J.*, 1864, ii, 356.

resolved in the 1870s.[54] Nevertheless in Britain in the 1860s, although spontaneous generation of micro-organisms failed to create the tremendous interest it did in France,[55] interest was not lacking, particularly on theoretical considerations. As Gilbert Child of Oxford wrote in 1868: 'In default . . . of direct [experimental] evidence, we must have recourse to certain general considerations in order to judge of the comparative probability of the two theories.'[56]

One such general consideration was, as I have already indicated, the theory of evolution which, in the 1870s, was most persistently and persuasively used in support of spontaneous generation by its leading British proponent, the University College Professor of Pathological Anatomy, H. C. Bastian,[57] although Child had already used similar arguments in the late 1860s.[58] Bastian held that according to the well-known principle of the uniformity of nature, it was inconceivable that spontaneous generation, admitted by many to have occurred 'in the Beginning', had not been repeated. He further argued that if simple organisms such as amoeba had only originated once they would surely have evolved into more complex organisms.[59]

These arguments reinforced others endeavouring to show that the line between the living and the non-living was very finely drawn. One of the most persuasive arguments—based on physico-chemical thinking—which supported this was the analogy of the origin of life

[54] I have outlined aspects of these problems in 'The problem of heat resistance of micro-organisms in the British spontaneous generation controversies of 1860–1880', *Med. Hist.*, 1966, **10**, 50–59; and 'Airborne particles and the germ theory, 1860–1880', *Ann. Sci.*, 1966, **22**, 49–60.

[55] An excellent bibliographical summary of the French controversies can be found in G. Pennetier, *Un Débat scientifique, Pouchet & Pasteur, 1858–1868*, Rouen, 1907, pp. 43–55.

[56] *Essays on Physiological Subjects*, (2nd ed.), London, 1869, p. 140.

[57] The subject was sketchily dealt with by Bastian in a number of publications until 1874, when he published a long article 'The evolution hypothesis, and the origin of life', *The Contemporary Review*, 1873–74, **23**, 528–44, 705–20. (Reprinted with slight alterations in *Evolution and the Origin of Life*, London, 1874).

[58] *Essays on Physiological Subjects*, London, 1868; a second, enlarged edition was published in 1869.

[59] For instance, he wrote, 'The existence of such lowest and simplest organisms as the microscope everywhere reveals is quite irreconcilable with the position that life-evolution has not occurred [since life first evolved]' (*Evolution and the Origin of Life*, London, 1874, p. 38.)

with the appearance and the growth of crystals in solutions and Bastian made great play on this.[60]

Apparent confirmation for such arguments was found in the discovery of minute formless, protoplasmic-like 'protamoebas' which were the subject of a great deal of work by the German, Ernst Haeckel, and which seemed to be on the borderline of the living and the non-living.[61] The distinction between living and non-living was further blurred and confused by the fact that protoplasm, which had achieved considerable scientific respectability, was on occasions confused with albumen,[62] so it is no wonder Thiselton Dyer could write: 'Anyone who is thoroughly impressed with the probability of the truth of evolution, finds little difficulty in deductively bridging the interval between a living thing so elemental in its characters as Haeckel's lowest Moner *Protamoeba*, and a lifeless proteinaceous substance.'[63]

But Thiselton Dyer was quick to point out this is not the same as the 'leaping powers of the so-called heterogenist [a believer in spontaneous generation—see below] who boldly widens the gap and passes easily from ammonium tartrate to a *Penicillium*, or from a solution of smelling salts to a fungoid mycelium.'[64] He also stated firmly that 'continuity as much forbids us to suppose that living matter has not been evolved from lifeless matter, as to suppose that lifeless matter has ever *per saltum* flashed into life.'[65]

Though the evolution/protoplasm arguments created much

[60] This analogy was commonly discussed, and it is not without interest that Schwann believed that the laws of crystallization gave him a basis for his theory of cell formation. (This has been stressed in E. Mendelsohn's 'Schwann's mistake', *Proc. 10th Int. Congr. Hist. Sci.*, 1962, pp. 967–70.)

[61] Haeckel's work was well known in Britain. For instance his *Monographie der Moneren* was translated in full in the *Quarterly Journal of Microscopical Science* for 1869. Huxley took great interest in Haeckel's work and in 1868 named what he thought to be an amoeba-like organism, *Bathybius Haeckelii*. It was, however, an artefact. (See L. Huxley, *Life and Letters of Thomas Henry Huxley*, New York, 1901, vol. 1, p. 317.)

[62] This was commented upon by J. Drysdale, *The Protoplasmic Theory of Life*, London, 1874, p. 256.

[63] See p. 354 of W. T. Thiselton Dyer's 'On spontaneous generation and evolution', *Quart. J. Microscop. Sci.*, 1870, **10**, 333–54.

[64] Ibid.

[65] Ibid., p. 335.

interest in connection with spontaneous generation[66] there were other relevant theoretical issues. For example John Hughes Bennett's interpretation of his experimental work was probably highly coloured by his own theoretical views. In 1868 he pointed out that his molecular theory of tissue organization (see p. 59) supported his observation that particles—oleo-albuminous molecules, as he called them—which formed the pellicle upon decomposing organic infusions enlarged and coalesced to form micro-organisms.[67]

So far as I understand Bennett, he did not believe in a special vital force as did the French supporter of spontaneous generation, F. A. Pouchet.[68] Pouchet, whose long controversy with Pasteur over spontaneous generation is well known, similarly held that organisms developed in the pellicle of a putrefying infusion but believed that a vital force—a legacy from pre-existing living matter—was necessary for such spontaneous generation which was known as heterogenesis. This is not to be confused with archebiosis or life from *non-living* matter—as was to be later expounded by Bastian.[69]

What was the relationship, if any, between Pouchet's heterogenesis and Beale's germs of living matter? On the face of it there was none. Beale's germs were not living organisms, indeed he was a strong opponent of the spontaneous generation of micro-organisms,[70] but nevertheless I cannot help thinking that the similarity of the ideas (if nothing else, they were both theories of vitalism) reinforced each other.

Certainly in 1864 the *Lancet* pointed out that there was nothing in

[66] It is relevant to add the opinion of John Tyndall on the impact of Bastian's opinions: 'considerable scientific uncertainty has been produced in reference to [spontaneous generation], both in England and America, by the writings of Dr. Bastian. These writings consisted in part of theoretic considerations and reflections, not new, but sometimes very ably stated, based on the doctrine of Evolution' (*Phil. Trans.*, 1877, **167**, 150).

[67] See p. 830 of 'The atmospheric germ theory', *Edin. med. J.*, 1867–68, **13**, 810–34. Most of this paper also appeared under the title 'On the molecular origin of infusoria', *Pop. Sci. Rev.*, 1869, **8**, 51–66, which seems to have been more widely read.

[68] F. A. Pouchet's work is most conveniently studied from his two books: *Hétérogenie ou Traité de la Génération spontanée*, Paris, 1859, and *Nouvelles Expériences sur la Génération spontanéé et la Résistance vitale*, Paris, 1864.

[69] Bastian also accepted heterogensis and considered it important in certain diseases. See Bastian's 'Remarks on heterogenesis in its relation to certain parasitic diseases', *Brit. med. J.*, 1872, **i**, 201–3, 258–60, 308–10, 417–19.

[70] See, for example, *Disease Germs; their Supposed Origin*, London, 1870.

the 'new theory of heterogenesis' contrary to sound science,[71] while more significantly Huxley—the staunchest of opponents of spontaneous generation—tentatively accepted a form of heterogenesis. In 1870 he stated that 'if there was a kind of diseased structure, the histological elements of which were capable of maintaining a separate existence out of the body, it seems to me that the shadowy boundary between morbid growth and Xenogenesis [i.e. the production of offspring totally different from the parents] would be effaced.'[72] Huxley went on to state his belief that the boundary was, in fact, almost effaced as a result of Burdon Sanderson's experimental studies on the infectious nature of particles in vaccine lymph (see p. 65); however Burdon Sanderson's subsequent work mostly supported the germ theory.[73] At the same time as this work—at the end of the sixties—I believe it was becoming quite clear that theoretical arguments alone could not solve the question of spontaneous generation and that what was needed were basic studies on the biology of micro-organisms. For example, in 1869, H. J. Slack wrote significantly: 'Controversies about "spontaneous generation" ought in these days to be replaced by inquiries into the conditions under which organisms of a low character can exist, or be developed.'[74]

THE PROGRESS OF THE GERM THEORY

I now wish to consider the progress of the germ theory against this background of anti-germ theories. I have tried to show that such theories, as diffusely expressed as they sometimes were, created a suitable climate for consideration of the germ theory. They emphasized the idea of infectious particles and drew attention to the need for studying them and—what was probably more important—the study of the general biology of micro-organisms.

This suitable climate was aided by other factors such as critical

[71] *Lancet*, 1864, ii, 666.

[72] See 'Biogenesis and Abiogenesis' (Presidential Address to the British Association for the Advancement of Science 1870) in T. H. Huxley, *Critiques and Addresses*, London, 1883, p. 241.

[73] See Burdon Sanderson's next report, 'The Origin and Distribution of Microzymes (Bacteria) in Water, and the Circumstances which determine their Existence in the Tissues and Liquids of the Living Body' Appendix to *13th Report of the Medical Officer of Health of the Privy Council, 1870*, London, 1871, pp. 48–69. Though this denied airborne micro-organisms it did a valuable service to the germ theory by drawing attention to water contamination.

[74] *The Student and Intellectual Observer*, 1869, **2**, 372–80.

comments on the total reliance on sanitary measures to exterminate infectious diseases.[75] Perhaps also significant was the fact that the germ theory had an attractive simplicity in comparison with other theories: Bastian remarked that like: 'homeopathy and phrenology, this [germ] theory carried with it a kind of simplicity and attractiveness which insured its acceptability to the minds of many.'[76]

But in addition to this favourable climate there were more positive factors promoting the germ theory. One was the increasing information on fungal diseases in animals and man which provided suggestive, analogous arguments for the germ theory of infectious disease. There was also the stimulus given by Pasteur in the late 1850s and early 1860s. His celebrated work showing that fermentation was caused by living organisms, which were not spontaneously generated, readily and provocatively dove-tailed into the existing theory of zymosis, a fact which made John Simon comment in 1863 that reserve is needed on the subject of the nature of the contagia of the zymotic diseases.[77]

If then, as I suggest, the scientific climate of the 1860s was ripe for consideration of the germ theory—which, after all, was not a new one—why was progress so slow? I think first that we must not overestimate the numbers of those, at least in Britain, interested in practical scientific work. Babington, as President of the Epidemiological Society, made a remark in 1864 which helps to put such quotations as the one from the *Quarterly Journal of Science* (p. 57) in perspective: 'The great majority of medical practitioners are so fully engaged in [practical medicine] that they have little time for

[75] A stir was created by Robert Christison's views on this which he delivered to a meeting of the Association for the Promotion of Social Science held in 1863. See *Lancet*, 1863, ii, 471–76, 501–3. For a discussion on this by the *Lancet* see ibid., 541–43.

[76] *The Beginnings of Life*, London, 1872, vol. 2, appendix E. p. cxx.

[77] *Sixth Report of the Medical Officer of the Privy Council 1863*, London, 1864, pp. 53–54. The passage from which this quotation is taken is worth quoting in full: 'A few years ago it might have seemed permissible to describe without reserve the contagia of the zymotic diseases, as but some changing organic material of the first affected body. At present, however, reserve on that point is necessary. That the power of contagiousness is associated with some changing organic material is certain;—but whether the power be *proper to the material*, or be only *contingently* its attribute, seems to require further investigation. The recent very interesting experiments of Professor Schröder in Germany, and of M. Pasteur in France . . . aim at proving, most extensively, an essential dependence of specific fermentatory and putrefactive changes on the presence, in each case respectively, of some characteristic molecular living thing.'

the more searching and scientific inquiries which we profess to undertake.'[78]

Even among the inquiries which were undertaken it seems that the need for experimental studies was partly hidden by the stress placed on the acquisition of statistics in the hope that the cause of the disease would, in the Baconian tradition, become apparent.[79]

But the prime reason for slow progress was the lack of technical 'know-how' in dealing with micro-organisms, especially bacteria, which had to await the important work of Cohn and Koch in the 1870s and early 1880s. However the 1860s were not devoid of practical studies which, by throwing light on the biology of the organisms, provided ground-work for the germ theory. There were many studies by Continental workers; for instance, Davaine's observations on the 'stick-shaped corpuscles' of anthrax,[80] and studies by Hallier suggesting that contagious particles were probably stages in the life cycle of micro-organisms.[81] But what can we say about British studies, as they contributed little to this biological line of approach?

If we were considering the 1870s much could be said of the valuable contributions to progress in practical microbiology largely as a result of the spontaneous generation controversies. The 1860s, however, provide nothing so outstanding except for Lister's antiseptic surgery which was so conspicuously based on the theory of living micro-organisms. This undoubtedly promoted a growing realization that experimental technique was all important. Lister in 1869 wrote: 'a belief in the [germ] theory is almost essential in order

[78] *Trans. epidem. Soc.*, 1867, **2**, 164.

Another pertinent comment came from Huxley in a letter (20 May 1867) to Haeckel about the latter's *General Morphologie*. 'I do not believe that in the British Islands there are fifty people who are competent to read the book, and of the fifty, five and twenty have read it or will read it in German'. L. Huxley, *Life and Letters of Thomas Henry Huxley*, New York, 1901, vol. 1, p. 310.

[79] This is suggested by a number of statements such as William Budd's view (*Brit. med. J.*, 1863, **ii**, 141) that statistics leave the vital question almost untouched. An example of less direct evidence which seems to bear this out is in *Trans. epidem. Soc.*, where, up to 1880, there was little interest in the germ theory.

[80] For a summary of Davaine's work see J. Théodoridés, 'Casimir Davaine (1812–1882): a precursor of Pasteur', *Med. Hist.*, 1966, **10**, 155–65.

[81] This work was widely discussed in England; some insight into its influence can be seen in a review of Hallier and Zurn's *Zeitschrift für Parasitenkunde* in *The Academy*, 1870, **1**, 259–60.

that the experimenter may be sufficiently keenly alive to the subtle sources of failure.'[82]

Lister's work also drew attention to the action of disinfectants on micro-organisms and this soon provided evidence for the germ theory.[83] Backing this were arguments in support of the germ theory by such workers as Spencer Wells[84] and William Budd,[85] while others were strong opponents of spontaneous generation. I think it was a significant feature of the 1860s (but which only became conspicuous in the 1870s) that much of this opposition to spontaneous generation came from a small band of professional scientists which included the terrible twins of Victorian science, Tyndall and Huxley.

The intense enthusiasm of these men must have done much to inculcate a critical attitude among British scientific workers. Though their influence on British medical science and education remains to be told, I think there is little doubt that it was considerable, albeit, often in an indirect way. For instance, even though Bastian deserves great credit for initiating the British spontaneous generation controversies, many had (justifiably for much of his work) little faith in his experimental technique. Huxley, in 1870, publicly pointed out that Bastian mistook a piece of moss for a spontaneously generated organism,[86] and privately wrote to J. D. Hooker that: 'The wonderful and significant facts about Bastian's *Sphagnum* leaves is not that these were in his tubes, but that he had not sufficient histological knowledge to be led to suspect their real nature . . . I have not the

[82] *Introductory Lecture delivered in the University of Edinburgh, 8 November, 1869*, Edinburgh, 1869, p. 8 (footnote).

[83] My current studies on disinfectants will, when published, provide full documentation for this statement.

[84] 'Some causes of excessive mortality of surgical operations', *Brit. med. J.*, 1864, ii, 384–88 was essentially a review of studies by Pasteur and Davaine, but J. A. Shepherd has indicated (*Spencer Wells*, Edinburgh and London, 1965, pp. 76–77) that it was probably not without influence.

[85] 'Variola ovina, sheep's small-pox; or the laws of contagious epidemics illustrated by an experimental type', *Brit. med. J.*, 1863, ii, 141–50. This address, based on his observation of sheep's smallpox, strongly opposed the idea of spontaneous origin of the disease. Budd did not talk in outright terms of micro-organisms causing the disease but Child (*Essays on Physiological Subjects*, London, 1869, pp. 149–50) stated that Budd spoke of germs of minute organisms. E. W. Goodall, (*William Budd*, Bristol, 1936, p. 91) points out that Budd never lost an early faith in the *contagium vivum*, but that he was usually very cautious in expressing his views upon its exact nature.

[86] *Nature, Lond.*, 1870, 2, 473.

slightest faith in Bastian's work. He is a clumsy experimenter and an uncritical reasoner.'[87]

At the beginning of this paper I said that I was interested in throwing light on British contributions to the development of biology. While I do not think that in the area of debate I have outlined conspicuous strides were made (Lister's contributions apart) it seems to me that the fact that many different ideas were aired and exchanged was singularly important. We have seen that both the vitalist and mechanist outlooks made significant contributions. While there is no question of the outright triumph of one over the other, the edge can perhaps be given to Beale's theory of living bioplasts which was more fruitful because it allowed the possibility of testing the theory—by studying the particles—a ready method of testing which the physico-chemical approach denied.[88]

Beale contributed greatly to the growing realization of the need for experiment and accurate observation, which I believe developed out of the conglomeration of ideas that were put forward, and deserves fuller study. Though he was a convinced vitalist his views were 'deduced from facts of observation';[89] we have in him perhaps something of Claude Bernard who, though accepting the experimental techniques of the physicist and the chemist in biology, was not lulled into thinking that physiological processes could be explained solely in physical and chemical terms.[90] This fruitful symbiosis of mechanist and vitalist ideas was given much impetus in the decade under review.

The British scene of the 1860s is perhaps not very startling—after all the number of those actively studying the problem was small—but the debates and interchange of ideas (and there were others I have not had time to mention) were extremely important in that they created a favourable climate for considering and examining

[87] Letter dated 10 August 1870. I am grateful to the Imperial College of Science and Technology for permission to quote this letter.

[88] Supporting evidence for this statement is exemplified in 'On contagion: what do we know regarding it', *Brit. for. med. chir. Rev.*, 1877, **60**, 391–407.

[89] L. S. Beale, *The Mystery of Life*, London, 1871, p. 7. In the same work (p. 20) Beale emphasized the importance of studying micro-organisms (cf. Slack's views, p. 71.)

[90] I am not suggesting that Beale carried out anything like the intensive experimental work as did Bernard. Mendelsohn (op. cit., footnote 2) has given an admirable summary of Bernard's views.

the germ theory by highlighting infectious particles and the need to study them. This helped to create what Kuhn has called a paradigm, that is an open-ended area of research.[91] It was the detailed elaboration of this area in the subsequent decades which was so fruitful to microbiology and, in turn, medicine.

[91] T. S. Kuhn, *The Structure of Scientific Revolutions*, Chicago and London, 1962.

Antiseptic Surgery

by

F. F. CARTWRIGHT

THE HISTORY of antiseptic surgery until 1847 is the history of attempts to prevent or to treat infection of the accidental wound, more particularly of the wound received in battle. The planned wound of surgical operation was not an urgent problem for the simple reason that the inevitable pain limited open surgery to essential, life-saving measures or to the relief of disabilities which made life unbearable. But the rapid, entire, and clean healing of a wound was then, as now, the primary aim of every surgeon.

Many early surgeons tried to force healing of the wound. In the seventh century Paul of Aegina protested against such attempts and pointed out that healing is a natural process. Six hundred years later Roger of Palermo advised that the wound should not even be closed by primary suture, in order to allow free drainage of the pus which he believed to be an essential stage in healing. The truth of this belief was challenged in the thirteenth century by Hugh of Lucca, Theodoric and, later, by de Mondeville, who all attempted to avoid suppuration by a near-aseptic technique which depended upon strict cleanliness and simple dry dressing. At the end of the century came a reversal in favour of unguents designed to promote wound healing; among these was the early antiseptic dressing, lint soaked in turpentine, advocated by Giovanni de Vigo. De Vigo also enunciated the dogma that gunshot wounds are poisoned wounds and must be cauterized.

It is one of the better known facts of medical history that Ambroise Paré exhausted his supply of boiling oil and successfully treated gunshot wounds with bland ointments in place of cauterization. His success depended upon free drainage and the topical application of alcohol. Paré adopted ideas originally put forward by Paracelsus, who had advocated a policy of non-interference with the wound; the natural process of healing should be stimulated, not by drugs applied to the wound, but by strengthening the body as a whole. Paracelsus and Paré both regarded pure air as innocuous or, if anything, rather beneficial to healing; but they condemned any suggestion that a wound should be allowed to lie open to the air of the sickroom. In 1573 Jean André Delacroix followed Paré's teaching with excellent results, using ethereal oils and alcohol as a wound dressing. Meanwhile, in 1516, Cesare Magati had emphasized the dangers of impure aire—or miasma—and had advocated infrequent dressing. Magati also anticipated John Hilton's work by insisting upon absolute rest of the wounded part, and introduced or popularized water dressing.

In 1692 Richard Wiseman stated that 'blood is the natural glue and hence great care must be taken to see that it is good'. He adopted Paré's teaching that the body as a whole must be encouraged to promote healing, rather than that topical stimulants should be applied to the wound itself. Wiseman dressed with astringent lotions and turpentine. Eight years later, in 1704, came the rather mysterious work of John Colbatch, which might have revolutionized wound treatment had he not kept the nature of his dressing a secret. Colbatch used a powder, now thought to have been salicylic acid or a salicylate. According to William Watson Cheyne, his results approached those of Lister. Colbatch also drew attention to the fact that there is a similarity between corruption and fermentation.

This process of corruption or fermentation engaged the attention of Boerhaave. In 1720 he pointed out that internal abscesses often resulted from or followed a superficial wound. Boerhaave attributed these multiple abscesses, pyaemia, to corruption or putrefaction of the normal pus; he thus differentiated between normal suppuration of the wound and lethal putrefaction. Free drainage of laudable pus came to be regarded as an essential of wound treatment; about 1750 the German surgeon Lorenz Heister introduced the method of making a counter-opening. Heister also held air to be harmful and did everything possible to exclude it. Free drainage was the principle underlying P. J. Desault's method; about 1790 he coined the term 'debridement' which, although it has now come to mean the excision of injured tissue, is derived from the French word for 'bridles' and implies 'unbridling', the removal of any barrier to free drainage of pus.

In 1750-2 John Pringle read to the Royal Society a series of short papers on *Experiments upon Septic and Antiseptic Substances*[1] and, a few years later, introduced into the pharmacopoeia a class of drugs which he named antiseptics. In origin these were not wound dressings but medicaments designed for external application in putrid fevers, particularly the type of sickness that ravaged troops or crews of ships in a tropical climate. By 1800 the antiseptics had entered surgical practice for treatment of gangrene or mortification. The large majority of these drugs were used topically, but Peruvian bark, wines, and acids were given by mouth. A surgical pharmacopoeia[2] of 1800 lists the following antiseptics: Peruvian bark, rue, chamomile, turpentine, camphor, myrrh, alum, sulphate of iron, acetate of lead, galls, cold water, snow, ice, spirits of wine, wine, citron juice, vinegar, sulphuric acid, nitric acid, hydrochloric acid, spirit of turpentine, sal ammoniac, carbon dioxide.

Antiseptics were only used when mortification had already developed. Johann Ulric Bilguer, one of Frederick the Great's surgeons, employed a complicated 'antiseptic' technique which may be regarded as typical. He incised the mortified part widely, squeezed out the corrupt humours, and poured in a mixture of frankincense, mastic, sarcocolla, myrrh, balsam of Peru, oil of cloves, and balsam of Fioraventi. He then dressed the wound with dry lint sprinkled with finely powdered myrrh, sal ammoniac, camphor, and nitre, and placed over all a fomentation of lime water mixed with spirits of wine and sal ammoniac. Bilguer claimed that, of 6,618 badly wounded soldiers, he had cured no less than 5,557; of the remainder 653 died and 408 recovered after amputation of a limb.

Not least of the many contributions of John Hunter to surgery is his insistence that healing of the wound is a natural process and that the scab is nature's own wound dressing. Hunter taught that wounds must be dressed so as to encourage formation of a scab; he used chalk or calamine for the purpose, and his views were supported by John Bell among others. But Hunter objected to the belief that air is responsible for putrefaction. It is probable that Hunter's teaching directly suggested the more simple forms of wound dressing that now became popular in Britain.

In 1819 and again in 1827 Astley Cooper published a method which was really a return to Wiseman's dictum that blood is the natural glue; Cooper used lint soaked in the patient's blood; this was strapped on and covered with lint dipped in a spiritous solution of lead acetate. Another reversion to an older and simpler practice was by Vincenz von Kern in 1809, who reintroduced the water dressing, probably first suggested by Magati and advocated by Michel Ange Blondus in the sixteenth century. Von Kern advised cold water for arrest of haemorrhage, lint soaked in warm water as a dressing, absolute rest, and artificial heat to the wounded part. His method was followed by Robert Liston and remained the most popular form of wound treatment in Britain until the eighteen sixties.

In France air was still regarded as the most potent cause of putrefaction. Here Jules Guèrin (1839) and Edouard Chassaignac (1844) independently introduced complete exclusion of air by gold beater's skin or guttapercha. Some French surgeons applied a continuous vacuum to the wound surface by means of an air-pump, or squeezed out any retained air by inflatable cuffs over the dressing. The method, although widely practised, was unsuccessful; Guèrin lost all but one of his amputations at the siege of Paris between September 1870 and February 1871.

In Germany exclusion of air was also regarded as the essential of wound treatment, but here the technique, probably derived from that of von Kern, took the form of warm water baths, first introduced by Bernhard von Langenbeck in 1849. The wounded limb was immersed in a vessel of water as hot as could be borne, temperature being maintained by applied heat or, more commonly, by a continuous stream of warm water. This led naturally to the method of wound irrigation, favoured by Esmarch who introduced an apparatus for the irrigation of wounds with water or a solution of a weak antiseptic such as Condy's fluid. Both forms of water treatment remained popular in Germany until after the Franco-Prussian War.

In this brief and incomplete survey[3] of methods of wound treatment before 1865, it may be seen that three enduring problems exercised the minds of surgeons. First, should the wound be permitted to heal naturally or should treatment aim at actively encouraging the healing process? Second, was pus, benign suppuration as opposed to malignant putrefaction, an essential part of healing?

Third, was air the cause of putrefaction? By 1860 opinion still differed upon the first two problems but, although exclusion methods were not popular in Britain, it had been almost universally accepted that air, or something in the air, was the cause of the group of infections which had, by now, become known as hospital diseases.

For the introduction of anaesthesia at the end of 1846 changed the face of surgery and brought the danger of infection to the urgent notice of the civilian surgeon. Freedom from pain made operative surgery possible. In the 1830s and 1840s the very fact that an operation had been performed was often thought of sufficient interest to merit a short paragraph in the medical journals. A typical example is to be found in the *Lancet* of 1 June 1833, when it is reported that at St. George's Hospital two operations were performed on 25 April and three more on 2 May.[4] None of these was a new procedure or presented any outstanding feature.

A very different state of affairs is revealed if we glance through the journals of the late 1850s, when anaesthesia has been in use for a decade and open operation is undertaken more frequently. In the *Lancet* of 3 April 1858, are references to the successful repair of ruptured perineum and prolapsus uteri, five by Baker Brown of St. Mary's Hospital, eight or nine by Fergusson of King's, single operations by Erichsen of University College Hospital and Adams at the London.[5] Here is a series of fifteen cases in place of a single operation. In the same year the editor of the *Lancet* wrote that 'the radical cure of reducible hernia is attracting considerable attention among surgeons' and described several methods of plugging or oversewing.[6] Conservative surgery, the excision of a diseased joint instead of amputation of the limb, first advised by James Syme in 1831, has become commonplace and there are many reports of successes. Again, the tendency is now to discuss a series; one paper details no less than twelve cases of Syme's amputation of the heel.[7]

This widening of the field of surgery, this increase in the number of operations, stimulated surgeons to examine the causes of mortality. So long as the pain of surgery prevented operation except as a life-saving measure or for the relief of intolerable discomfort, the fact that the patient often died was merely an additional reason to avoid operation whenever possible. After the introduction of anaesthesia more operations were performed and therefore more patients died.

For this reason it is sometimes stated that anaesthesia, by increasing the frequency of operation, raised the death rate of surgery.[8]

In fact, this statement is untrue. Examination of the King's College Hospital case notes shows that, before the introduction of anaesthesia, 27 per cent of patients admitted to the surgical wards underwent open operation, the remaining 73 per cent being treated by other means or judged unsuitable for surgery. The operative mortality was 11.8 per cent and the mortality of those treated by other means, or untreated, just under 12 per cent. The effect of adding the two figures together is to give a total surgical death rate of 11.9 per cent. After the introduction of anaesthesia the proportion of patients treated by open operation rose steeply until a figure of 84 per cent is reached in the eighteen sixties, only 16 per cent of patients being treated by other means or judged unsuitable. The operative mortality was 9 per cent and the mortality of those treated without operation 12 per cent. The effect of adding the two figures together is to give a total surgical death rate of just under 12 per cent.

Thus the introduction of anaesthesia made only a fractional difference to the total surgical death rate, and it is this total death rate which is of importance, for a patient is just as dead whether he dies as the result of operation or because operation is withheld. But a more striking fact emerges when we consider the operative mortality. Despite a three-fold rise in the number of operations performed, the death rate fell from 11.8 per cent in the eighteen forties to 9 per cent in the eighteen sixties.

The commonest cause of death was sepsis, putrefaction, or generalized infection. This is true of both the eighteen forties and the eighteen sixties. In the absence of temperature charts, it is a little difficult to assess the proportion of deaths in the earlier period but the picture has become clear by eighteen sixty. Almost exactly two-thirds of the 9 per cent of deaths mentioned above were due to sepsis; that is to say, 6 per cent of all operation cases died of generalized infection.

In 1859 Thomas Bryant published an interesting survey based upon the results of three hundred amputations performed at Guy's Hospital.[9] The total of deaths was 76 out of 300, or 25.3 per cent. The causes of these 76 deaths were: secondary haemorrhage (5 cases), concurrent injury (5), complications such as pneumonia, diarrhoea, or recurrence of cancer (13), hectic fever (2). 24 deaths or 33 per cent were from exhaustion and 30 deaths or 42 per cent from pyaemia.* The highest total of deaths from exhaustion, 50 per cent of deaths, occurred after secondary amputation for injury. As secondary amputation was usually undertaken because attempts to save a limb had failed to prevent putrefaction, it may be assumed that death was due to exhaustion from infection rather than from the shock of injury or operation. It is therefore likely that nearly 75 per cent of these 76 deaths was due to sepsis. Even if this assumption be not allowed, the commonest cause of death was still from the one form of hospital disease, pyaemia, for pyaemia was the cause in 42 per cent of deaths and killed 10 per cent of Bryant's 300 patients. And Bryant's mortality is not high; Lister stated that at Glasgow, in the six years before antisepsis, between 40 per cent and 45 per cent of his cases of amputation and excision of joints died from one cause or another.[10]

It is notable that, in Bryant's series, amputation of necessity carried a far higher mortality (43.4 per cent) than did operation of expediency** (30.3 per cent). The danger of accident surgery is emphasized by a short series of fifteen cases of amputation through the hip joint, published in the *Lancet* in 1865.[11] Nine of the fifteen patients died, a mortality expressed as 60 per cent. Eleven amputations were operations of expediency; five of these died, a mortality of 40 per cent. In only two cases (18 per cent) was death attributable to sepsis. The remaining four cases were amputations of necessity for compound fracture of the femur. All four died from sepsis or shock. The figures not only underline the grave danger of sepsis, but serve to remind us that we must differentiate between the planned operation and emergency or accident surgery when considering pre-Listerian mortality figures. It was not so much the dirty surgeon as the filthy dung-strewn streets which caused the appalling mortality of accident surgery.

Such was the problem which faced the pre-Listerian surgeon. He could count on a basic operative mortality of about 9 per cent, two-thirds of the deaths being due to sepsis, and death from sepsis

* In three cases, death is ascribed to more than one cause.

** The nineteenth-century term 'expediency' is here used in preference to the more recent 'election'.

might occur after the most trivial operation. A typical example, taken from the King's College Hospital case notes, is that of a man who suffered from a simple synovitis of the knee joint which was aspirated in the out-patient department. An abscess formed at the site of aspiration; he was admitted and the abscess was opened. His condition deteriorated and the surgeon decided to amputate his leg; ten days later he died of pyaemia. This basic 9 per cent mortality could rise to as high as 45 per cent or more when the surgeon dealt with a compound fracture and, despite the high death rate, amputation remained the most hopeful method of saving life in serious injury.

Pyaemia was only one of the group of infections classed as hospital diseases. Pyaemia was endemic, but erysipelas, septicaemia, and hospital gangrene occurred in an epidemic form. A hospital might go for a few months or a year without a single case of septicaemia; then the surgical wards would be ravaged by a virulent outbreak. On 14 July 1863, a patient was admitted into the accident ward of St. George's Hospital with a slight wound of the leg; a week later he developed hospital gangrene. Within six days four out-patients undergoing treatment had been admitted and one in-patient had been attacked. During the next month 92 cases of gangrene occurred, 73 among out-patients and 19 among in-patients. There had been no case of gangrene for four months before the outbreak, and the epidemic subsided within a few weeks.[12]

There is a modern tendency to believe that the pre-Listerian surgeon accepted his problem without question and made little attempt to solve it. Such a view is not only false but grossly unjust. The surgeon had theories and he did his best to translate those theories into active treatment. The fact that his theories were wrong and that, as a result, his treatment was virtually useless cannot be held against him. A few practitioners considered hospital disease to be a contagion, passed directly from patient to patient, but the majority held it to be an infection, spread by means of the air. It was not air itself, but something in the air, a noxious gas or miasma.

The popular concept of miasma was that of a foul-smelling emanation which, as Ambroise Paré had pointed out, poisoned the air of the sick-room with its stench. Opinion as to the nature and origin of miasma varied and provided scope for solutions more ingenious than useful. Thus in 1794 Samuel Latham Mitchill of New York propounded the astonishing theory that nitrous oxide and miasma were one and the same. His totally fallacious evidence contained a point of interest. He claimed that a sword hung in the tent of a fever-stricken soldier became tarnished and that a fragment of bright metal suspended in a vessel of nitrous oxide tarnished quickly.

Tarnishing of polished metal suggested a cause for wound infection and a logical means of prevention. The air of a sick-room housing patients who suffered from pyaemia or gangrene had an unpleasant odour. The air arising from a cesspit or sewer had an unpleasant and similar odour. Disease of all kinds was more common in the evil-smelling tenements of the very poor than in the comparatively sweet-smelling houses of the rich. Thus the cause of hospital disease was a foul-smelling miasma, originating as an emanation from sewers or cesspits, which entered the sick-room and contaminated the wound. The contaminated wound in turn generated miasma, as evidenced by its evil smell, and the miasma travelled on air currents to other wounds. So the epidemic developed. In an endeavour to prevent initial entry of miasma it was a common practice to forbid the opening of ward windows until the atmosphere became quite unbearable.

A polished silver spoon, left uncovered to the emanation of a sewer, tarnished quickly. A tarnished spoon had an unpleasant smell, somewhat similar to the smell of a foul sewer or an infected wound, another point in favour of the view that tarnishing and infection were due to the same cause. But if a silver spoon, left uncovered to the air, were to be washed at frequent intervals with cold water, tarnishing was delayed. Thus it might be possible to prevent miasma affecting the wound by the use of plain cold water.

The British surgeon already largely relied upon simple water dressing. Here, then, was an extension of, and not a revolution in, his technique. During the late eighteen fifties there rapidly developed a school of practitioners who used vast quantities of the purest possible cold water, that is water that had been boiled and cooled, to cleanse their instruments, to irrigate the site of operation, to dress the wound, and to wash away any evidence of malignant infection that might later appear. This is the cleanliness-and-cold-water school, a name probably coined by Conan Doyle, and we should remember that Lister—himself a 'pre-Listerian' surgeon for fifteen years—adopted the method from about 1860 until 1865. Basins of cold boiled water and piles of towels appeared in his Glasgow wards and he made strict rules to enforce cleanliness. When sepsis developed he turned to kettles of boiled water, often coloured with Condy's fluid, 'with which he tried to wash away the putrefying discharges'.[13]

Men like Thomas Spencer Wells achieved quite good results by means of this clean technique which, given a large element of luck, could be successful in operations of expediency. Even here there could be no certainty of success, for the unrecognized danger of contamination by dressings, instruments, the surgeon's hands, and droplet infection was ever present. Nurses, working long hours in the unhealthy atmosphere of surgical wards, were particularly prone to 'hospital throat', the streptococcal infection which was both the cause and the result of septicaemia and erysipelas.

Cleanliness and cold water were of no avail in the operation of necessity, for accidental wounds were infected before ever they reached hospital and, however superficial that infection might be, there was no reliable means of preventing spread to the deeper tissues and into the blood stream. The surgeon did his best with escharotic drugs, acetate of lead, chloride of zinc or other metallic salts, but there was little hope for the patient once generalized infection had occurred. In this connection we must remind ourselves that raising of the standard of social hygiene and the disappearance of the horse from the streets has reduced the risk of infection in accident surgery to an extent probably equal to the reduction effected by improved methods of wound treatment.

Among other preventive measures were oral administration of sulphite of potash before operation, advocated by Polli of Milan and used by Lister, routine disinfection of the wards with a slow stream of chlorine gas, first recommended by Guyton in 1795, and cleansing with Condy's fluid or chloride of lime. But, when disease developed, the surgeon was almost helpless. He excised as much tissue as possible from a gangrenous wound or amputated the limb, supported the patient's strength with vast quantities of alcohol and strong beef tea, and applied topical medication according to the prevalent fashion.

In the latter years, some attempt was made to deal with outbreaks of hospital disease by counteracting the supposed spread of miasma. Floors and bedding were soaked with chloride of lime and Condy's fluid, sheets soaked in chloride of lime hung over the windows, chlorine gas passed into the wards, and huge fires kept burning even in the heat of summer to draw off the miasma by way of the chimney. In 1864 carbolic acid was used empirically, sometimes as a wound dressing as suggested by Lemaire, often in place of chloride of lime on the bedding and floors. The wise surgeon stopped operating in these times of epidemic, discharged as many patients as possible, and closed his wards which were then fumigated with sulphur candles or a mixture of saltpetre and sulphuric acid.

The war against gangrene and septicaemia may be likened to a series of fierce and losing battles. But pyaemia was endemic, although outbreaks sometimes reached epidemic proportions; the war against pyaemia was a continuous, smouldering engagement. So long as the creamy staphylococcal pus drained freely from the wound, all was well; hence the term 'laudable pus' which has become the shibboleth of the pre-Listerian surgeon. But if the pathway of drainage closed too soon and a loculated abscess formed, then there might speedily follow the 'fearful mischief of putrefaction of the blood' and death from the multiple abscesses of pyaemia. For this reason the surgeon tried to prevent healing by first intention and students were taught that healing of amputation wounds should be actively delayed. Most surgeons thought that hot poulticing helped to prevent pyaemia. The widely used bread poultice is now regarded as little better than a joke, but we should remember that wholemeal bread, unadulterated by preservatives, quickly grows a penicillium mould. The bread poultice may have helped to inhibit staphylococcal infection.

The thread ligature, being an unabsorbable foreign body, was a potent cause of sepsis. It seems that many surgeons suspected this, but they clung to the use of the ligature because it acted as a drain. In the hospital case notes we frequently find a date with the one mysterious word 'Through'. This simply meant that pus, draining through the dressings, had appeared at the ends of the long, hanging ligatures. For this reason most surgeons refused to accept James Young Simpson's suggestion that thread should be replaced by tight fastening of cut vessel ends to the skin with metal pins, a method which Simpson called 'acupressure'.[14]

By 1865 cleanliness and cold water had done something to reduce mortality in surgery and, on the rare occasions when it was used, had done much to reduce the mortality of the terribly prevalent puerperal fever. Charles White of Manchester, Ignaz Semmelweis of Vienna, and Oliver Wendell Holmes of Boston had shown that strict cleanliness and chloride of lime could effect a very great improvement. Little attention was paid to their advice, but their good results probably provided the stimulus for the work of Simpson, himself an obstetrician.

Simpson not only attacked the thread ligature but condemned the surgical ward. He produced statistics which suggested that the incidence of hospital diseases was associated with the size of the hospital. He showed that the mortality of amputation rose steeply from 10.7 per cent in the home to 40.9 per cent in a hospital of 300 beds.[15] Simpson advocated the building of small temporary wards, made of iron which might be flamed, or of wood which might be burned to the ground after an epidemic and cheaply rebuilt. A similar idea lay behind the proposal of Miss Florence Nightingale, who believed that hospital diseases could be ended by surrounding the ward on all sides with clean air.[16] A few of her 'pavilions' were built in Germany, but raised insoluble problems of catering, nursing, and supervision. The modified pavilion, a series of free ward blocks connected by a corridor, became the standard plan of hospital architecture, But, as evidenced by experience at the new St. Thomas' Hospital, hospital design had little effect upon the incidence of hospital disease.

Until April 1864 any attempt to deal with hospital diseases was empirical for no man knew the cause and, until the cause was known, there could be no rational method of treatment. The cause was found as the result of the work done by Louis Pasteur between 1860 and 1863. Pasteur did not himself apply his findings to disease but, on 7 April 1864, $\boxed{\text{he enunciated the Germ Theory.}}$ Telling of a flask of boiled milk which had for some time been kept sealed, he said 'And I wait, I watch, I question it, begging it to recommence for me the beautiful spectacle of the first creation. But it is dumb, dumb since these experiments were begun several years ago; it is dumb because

I have kept from it the only thing Man cannot produce, from the germs which float in the air, from Life, for Life is a germ and a germ is Life.'

Pasteur believed that he had discovered not only the cause of fermentation or souring, but the source of life itself. This fact largely accounts for the bitter antagonism to the Germ Theory of Disease which developed some ten years later. As is suggested by his words quoted above, all Pasteur's early experiments tended to show that his living micro-organisms 'float in the air'. At a later date he underlined this belief with the well-known dictum 'Every speck of dust carries at least one germ'. Thus Pasteur's teaching, when its usefulness came to be appreciated, concentrated attention upon the danger of air-borne infection.

The first man to understand that Pasteur's findings might be applied to surgical problems was Thomas Spencer Wells of the Samaritan Hospital. In the course of an address to the British Medical Association Annual Meeting at Cambridge on 3 August 1864, he stated the case very clearly:[17] 'Applying the knowledge for which we are indebted to Pasteur of the presence in the atmosphere of organic germs which will grow, develope, and multiply, under favourable conditions, it is easy to understand that some germs find their most appropriate nutriment in the secretions from wounds, or pus, and that they so modify it as to convert it into a poison when absorbed—or that the germs after development, multiplication, and death, may form a putrid infecting matter—or that they may enter the blood and develope themselves, effecting in the process deadly changes in the circulating fluid.'

But Wells did not change his method of operating. It is probable that he had not the type of mind which could translate Pasteur's theory into practice. Further, he was obtaining results which were good by the standard of those days using simple cleanliness and cold boiled water; his mortality in the dangerous operation of ovariotomy—so dangerous as to be forbidden in many hospitals—was at that time 33.96 per cent.[18]

On an unknown date between March and August 1865 Joseph Lister, professor of surgery at the University of Glasgow, was walking with Thomas Anderson, professor of chemistry. Their conversation turned to wound infection; Anderson suggested that Pasteur's work might throw some light upon the problem. This well-authenticated story is of great interest when we recall that, only a few months before, Wells had drawn attention to Pasteur's findings and that his remarks had been published fully in the *British Medical Journal*. Pasteur's monographs were also readily available. Yet Lister seems to have read nothing of the subject; his knowledge of Pasteur's work came by pure chance; the lucky chance of a colleague's suggestion.

But Lister was one of the very few surgeons of his time who possessed the type of mind which would at once appreciate the importance of the germ theory and accept it without question. For his father was a wine merchant who kept large stores of liquor and who brewed his beer in the immense cellars of Upton House. His father was a self-taught scientist, particularly interested in the microscope, who built his own instruments, ground his own lenses and, in 1832, had been elected into the Royal Society for his work upon the achromatic system. Further, Joseph Jackson Lister was an intelligent, liberal-minded parent, never so happy as when actively taking part in his children's education. Thus Joseph Lister was familiar from his earliest youth with the process of fermentation, and he knew without doubt that there existed a whole world of structures and of organisms which lay beyond the range of the naked eye.

His earlier papers bear witness to this fact. For it was the changes analogous to fermentation which interested him, the early stages of inflammation, coagulation of the blood. And it was the miniscule objects, only readily visible by magnification, of which he wrote, the ciliary muscles of the eye, the erectile tissue of the skin, blood corpuscles in the web of a frog's foot. There is a story, not very well authenticated, that Lister at one time discarded the theory of miasma in favour of disease dust, a microscopic body resembling pollen.

Thus Lister, trained in the use of the microscope at a time when the microscope was not a familiar instrument to the surgeon, appreciated the supreme importance of Pasteur's findings and applied them to surgery. But he believed infection to be entirely air-borne and, at the beginning, his sole endeavour was to protect the wound from organisms carried in the air. He had already unsuccessfully tried the simple covering of wounds with impermeable dressings, metal or guttapercha. He now turned to nature's own dressing, the scab, but he understood that the scab must contain some substance capable of destroying Pasteur's germs before they had the opportunity of invading the underlying tissues.

Crude carbolic acid had been first proposed as a wound dressing by Jules Lemaire of Paris in 1863, but Lister did not hear of this suggestion until some years later. In 1864 cattle, pastured on a sewage farm near Carlisle, became heavily infected with entozoa; at the same time there was an increase in the number of cases of typhoid and diphtheria in the city. When the sewage was treated with carbolic, both the cattle sickness and the epidemics diminished. Lister knew of this; it is probably because of the Carlisle experiment that he tried carbolic as a dressing in March 1865 with entire failure. His monograph on excision of the wrist joint in the *Lancet* of 25 March[19] does not mention carbolic or Pasteur's work; it is therefore likely that this first trial took place before he had read of the latter.

But he now turned once more to carbolic. In the first case of this series, that of the eleven-year-old boy James Greenlees, on 12 August 1865, he simply used a piece of lint dipped in crude carbolic acid, but he altered his technique in the next and far more serious injury of 19 May 1866. He squeezed out as much blood clot as possible and thoroughly swabbed the wound with carbolic. He then applied a piece of lint, soaked in carbolic, to the wound surface with an overlap of half an inch on every side. The lint was then covered with a thin sheet of malleable tin and the whole dressing strapped to the leg with adhesive tape. Subsequent dressing consisted of lifting the metal protection and painting the lint surface with carbolic.

Lister had obtained his desired result by means of the blood and carbolic-soaked lint, an antiseptic-impregnated coagulum or scab, through which Pasteur's germs could not pass. On 16 March 1867, he published an account of his method in the *Lancet*. His results depended upon only eleven cases, all compound fractures. Only one of these eleven patients had died, and that from haemorrhage due to puncture of an artery by the fractured bone. Eight of the eleven had made uneventful recoveries, but two had been attacked by hospital disease. Both recovered, one with conservative treatment and the other after amputation of the affected leg. Success such as this was quite unknown in the treatment of compound fractures.[20]

But it is often overlooked that lucky chance had once again come to Lister's aid. An antiseptic can only kill by direct contact; Lister's sponging of the wound crevices and his covering of the surface with carbolic can only have destroyed superficial bacteria. Of his eleven compound fractures, no less than eight and possibly nine must have reached him before infection had invaded the deeper tissues. For Lister did not as yet know that the organisms of hospital disease were to be found in the dirt of the streets, on his own hands, on his instruments.

In October 1866 Lister started to use carbolic during the incision of abscesses and in the following April he performed an operation under carbolic cover for the first time. The case was that of a large tumour, deeply embedded in the upper arm; the patient, a very sick old man, made an uneventful recovery. In these early cases, Lister employed his 'antiseptic curtain', a piece of carbolic-soaked lint held over the site of operation by an assistant. So far as possible incisions were made 'blind', the knife being slid beneath the antiseptic curtain. When entirely necessary, the curtain was lifted for as short a time as possible, and the bare wound freely syringed with carbolic lotion. By now he had adopted the dressing which became well known as 'carbolic putty', a stiff paste of carbolic, linseed oil, and carbonate of lime.

When, in March 1867, Lister published his first paper, his technique was a method of wound dressing and nothing else. Indeed, if we forget the underlying principle, it was not even an entirely new method, but a modification or extension of one already suggested by Lemaire. The title of this first publication is *On a New Method of Treating Compound Fractures, Abscesses etc., with Observations on the Conditions of Suppuration;* it was not until five months later, 9 August, that he spoke of *The Antiseptic Principle in the Practice of Surgery.*[21] Thus, at the start, Lister's technique was known as 'The Carbolic Treatment'; the drug carbolic assumed a greater importance than the principle of antisepsis. This mistaken approach to Lister's work persisted for many years; indeed there are some today who have not altogether freed their minds from the error.

In the years 1868–9 the *Lancet* carried out a survey of 'the carbolic treatment' as used in the London teaching hospitals and some other centres. Sir Rickman Godlee, by lifting isolated sentences out of context, used the *Lancet* reports to give an erroneous and unjust picture of immediate antagonism.[22] Naturally opinion was divided for there will always be some opposition to a new idea. but it is only by considering the survey as a whole that we can gain a true picture of London's initial reaction to Lister's technique.

The Resident Surgical Officer at University College Hospital was almost enthusiastic. 'It so happened that during the early months of the year, pyaemia was exceedingly rife in this hospital. Patient after patient perished by it; and indeed recovery after amputation of a limb was almost an exceptional rarity. This endemic of pyaemia continued up to the date at which carbolic dressings began to be used. Not one of those treated antiseptically died of pyaemia or indeed ever had a pyaemic symptom.' But, he added, carbolic dressings had not proved successful in the out-patient department, where chloride of zinc gave better results.[23]

At St. George's Hospital Mr. Rouse 'occasionally sponged the wound in the operating theatre . . . but not having found any advantage from it, he had discontinued the practice', Rouse had been 'much struck with the results in one case of compound fracture which was treated by Lister's method'.[24] This is of interest for two reasons. First it shows that carbolic was already being used, however ineffectively, in London operating theatres. Secondly, this provides one of Godlee's most glaring distortions. He quotes Rouse's sponging of the wound with the contemptuous comment 'such rubbish', but studiously neglects to add that Rouse had made successful trial of Lister's method in a case of compound fracture.

Callender of St. Bartholomew's Hospital, Gay of the Great Northern, Barnard Holt of the Westminster, all reported more or less favourably. Barnwell of Charing Cross condemned Lister's 'multiform plasters' but commended a lotion of carbolic in glycerine and water or the carbolic putty of linseed oil and whitening.[25] Maunder of the London Hospital had experienced good results by using Lister's technique in large abscesses, acute abscesses of bone, removal of mammary tumours, the repair of hernia, and the ligation of arteries.[26] Bryant of Guy's reported that he 'is giving Lister's plan of the treatment of wounds a good trial . . . upon the whole the results have been good, and he is disposed to attribute them in a great measure to the use of carbolic acid'. But his colleague Birkett 'has not had much experience of carbolic acid. He finds his cases do very well on the usual plan'.[27] At the Middlesex Hospital Thomas Nunn regularly dressed operation wounds with one in forty carbolic lotion, but Campbell de Morgan preferred zinc chloride.[28] Of particular interest is the typically honest opinion of James Paget 'in the few cases in which he has tried it, it has been useless; but this may have been, Mr. Paget thinks, from some error in the mode of application'. Another St. Bartholomew's surgeon, Holmes Coote, did not at all approve of Lister's method 'which he considers meddlesome'.[29]

It is clear that Lister's technique received a fairly extensive trial in the years before the Franco-Prussian War. But this trial was of little value, for the majority of surgeons did not appreciate that here was not just a new method but an entirely new principle. Lister made efforts to emphasize that no magic lay in carbolic. 'In using the expression "dressed antiseptically" ' he wrote in 1869 'I do not mean merely "dressed with an antiseptic" but "dressed so as to ensure absence of putrefaction".'[30]

Six months later, at the August 1869 meeting of the British Medical Association, Thomas Nunnelly of Leeds launched a violent attack upon antisepsis. This very intelligent, research-minded surgeon claimed that he and his colleagues at Leeds had given the antiseptic system a fair trial but with no good results and had therefore abandoned it, a statement later denied by his said colleagues. But Nunnelly was the first to attack the underlying principle. He questioned the existence of living micro-organisms and he emphatically denied that micro-organisms, even if they did exist, could be the cause of disease.[31] This was the opening shot in the Battle of the Germ Theory.

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The most difficult task in history is to clear our minds of truths accepted today and to share the thoughts of our predecessors. On the known facts, opposition to the germ theory in the 1870s was reasonable. Differentiation between moulds, yeasts and bacteria had barely begun; there was no knowledge that both pathogenic and non-pathogenic bacteria existed. Lister probably believed that there was *a* germ of hospital disease, or that *all* germs caused hospital diseases. Pasteur believed that these germs were endowed with life, but there was no overwhelming evidence in support. At a meeting of the Pathological Society of London in 1875,[32] Goodhart raised the obvious objection. He had examined many specimens, but in no case had he observed anything in the nature of a 'living particle'. 'There were only these rounded spheroids, chains, or aggregated masses:' he said 'we never saw them moving.'

Nor had anyone as yet demonstrated an undoubted relationship between micro-organisms and disease. Lionel Smith Beale suggested a perfectly reasonable alternative 'a disease germ is probably a particle of living matter derived by direct descent from the living matter of man's organism'.[33] The objection that micro-organisms are more likely to be the product rather than the cause of disease was rational. Further, the Germ Theory had been extended into the Theory of Spontaneous Generation, and the age-old concept of the Creation was being attacked on other grounds. A number of seriously-minded men, including scientists and members of the medical profession, found themselves quite unable to accept theories which undermined the essential foundations of their belief. A man like Beale stultified the greater part of his work by attempting to correlate his results with a rigid adheren ce to the story told in Genesis; he went so far as to invent a structure to which he gave the name of 'bioplasm' in order to differentiate between living and dead tissue. Thus, when it became understood that Lister's method depended upon the germ theory, there developed a hostility not only to the principle but to the method.

Logically as well against Genesis.

By 1869 Lister had evolved an antiseptic technique which could be applied to the planned operation. His antiseptic curtain, suitable only for a blind incision, had been replaced, first, by massive and continuous syringing with carbolic and then by the carbolic spray. It was not Lister but Thomas Nunn of the Middlesex Hospital who first used the spray.[34] Nunn modified an ordinary Richardson's spray for the purpose, and this was the first of Lister's models. In 1871 Lister replaced Richardson's spray by a much larger apparatus, standing on a tripod and worked by a hand lever. This proved too bulky and cumbersome; nor did it produce an efficient spray but rather a jet of carbolic lotion. In the following year, 1872, he introduced the well-known steam-operated spray, which became a standard article of operating theatre equipment. The original purpose of the spray was solely to 'clean the air' but, as Lister began to suspect that infection could be transmitted by other means, he evolved a technique which ensured that the surgeon's hands, his instruments, and all dressings should be submitted to the spray before touching the wound.

By 1870 Lister had also come to understand that antisepsis cannot be effective in accident surgery unless applied very shortly after injury. He first made this finding public in that paper which was destined to exert so profound an influence upon the development of world surgery *A Method of Antiseptic Treatment applicable to Wounded Soldiers in the Present War*. 'The earlier the case comes under treatment, the greater will be the prospect of success', wrote Lister, 'but even after the lapse of thirty-six hours it need not be altogether despaired of.'[35] But he had no knowledge of warfare. The treatment which he advocated in this paper of September 1870 was hopelessly impracticable, requiring a quantity of dressings and carbolic that no field surgeon could possibly carry. The French made no attempt whatsoever to put Lister's ideas into practice.[36] But the Prussians, although unable to carry out Lister's full plan, did try something in the nature of a carbolic treatment; the results were by no means good but better than when no carbolic was used.[37] Lister had not published his paper until three months after the outbreak of war; thus the German surgeon was able to make a comparison. He ended the war with a conviction that antisepsis had a place in surgery.

So it came about that Germany, alone among nations, accepted Lister's teaching wholeheartedly. There was some opposition to the germ theory; Billroth, for instance, held it possible to follow Lister's method while preserving an open mind on the underlying principle. But the majority of German surgeons accepted both the method and the principle, and it is for this reason that the German-speaking nations led the way in the great forward thrust of surgery during the two decades after the Franco-Prussian War. Such men as Theodor Billroth, Richard von Volkmann, Friedrich von Esmarch, Karl Thiersch, Ernst von Bergmann, Johann von Nussbaum and many others showed that Lister had not only made surgery reasonably safe but, by making it safe, had immensely widened the field.

It is understandable that defeated France should develop a suspicion of foreign methods, particularly if these were adopted by victorious Germany. There was a distinct tendency to look upon antisepsis as a theft of French ideas. Pasteur had introduced the germ theory, Lemaire carbolic, Chassaignac the rubber drainage tube. The French surgeon Perrin declared that France had also invented the catgut ligature, and that Lister's sole contribution was the carbolic spray.[38] There is, of course, some basis of truth in this contention, and it should serve to remind us that increased ease of communication has played as great a part in the development of medical science as have the discoveries of individuals. But Lister had his followers in France. Lucas-Championniére published the first text book of antiseptic surgery in 1876; Louis Ollier accepted Lister's teaching, albeit somewhat half-heartedly, and Jules Péan was a strong supporter of antisepsis.

For over a century American medicine had depended largely upon postgraduate study in Europe. National bankruptcy and pre-occupation with internal dissensions led to a withdrawal from Europe during the 1860s and 1870s. Antisepsis made little impact upon American practice until after 1876, when Lister attended the International Congress of Medicine at Philadelphia as president of the surgical section. After the congress, he toured the surgical centres but was received with enthusiasm only in Boston and, to a lesser degree, in New York. At the June 1882 meeting of the American Surgical Association, the majority of speakers were still opposed to antisepsis.[39] The first American book on antiseptic surgery, by Arpad Gerster, was not published until 1888, when the modified German technique had come into wide use. It is probably for this reason that the American writer, Cecilia Mettler, in her very detailed *History of Medicine*, published in 1947, virtually disregarded Lister and antisepsis.[40]

After ten years of argument and trial, opinion in Britain was still very mixed. Opposition was not confined to London; indeed there is evidence of strong opposition in the very centres where Lister worked. In 1870 James Morton, who had practised Lister's method at the Glasgow Royal Infirmary in 1867, attacked Lister and stated that the antiseptic principle was founded on false premises.[41] In the same year Lamond, secretary of the Glasgow Royal Infirmary, publicly questioned the accuracy of Lister's figures.[42] Less than six months after Lister left Glasgow, his technique had been abandoned by all but two of the surgeons. Macleod, his successor in the chair, used a modified Listerian method with unsatisfactory results and Eben Watson dressed with carbolic lotion; neither accepted the germ theory.[43] Five years later Professor Spence, Lister's colleague at Edinburgh, is reported as having 'had most enviable results in spite of persistently rejecting the antiseptic plan of treatment'.[44]

London was not enthusiastic, but a report of a meeting of the Clinical Society in 1875[45] shows that a Listerian technique was in fairly wide use. G. W. Callender of St. Bartholomew's, Thomas Pickering Pick and Timothy Holmes of St. George's had regularly employed antisepsis and spoke in favour, although Holmes denied the truth of the germ theory. Thomas Smith of St. Bartholomew's thought there to be no evidence either for the method or against, but he made a plea that all the details of Lister's teaching should be followed if his teaching was followed at all 'we should accept his method in toto or reject it altogether'. He was replying to Barwell of Charing Cross who said that antisepsis was invaluable in the deliberate wound of a planned operation but that 'Mr. Lister has mixed up with his method many details which are very repulsive to surgeons, and has thus done himself and his method an injury'. The same criticism was made by Christopher Heath of University College Hospital, who especially attacked the spray. Operating in a cloud of carbolic mist was, to say the least, unpleasant; every time he had used the spray he had 'almost resolved' never to do so again.

Lister had many fervent admirers in Britain; notable are Bickersteth of Liverpool, Lund of Manchester, Pemberton of Birmingham. His father-in-law James Syme was one of the first to adopt antisepsis; his brother-in-law Marcus Beck followed his teaching at University College Hospital; a fellow student, Cadge of Norwich, introduced antisepsis into East Anglia. In London Howse of Guy's, Croft and MacCormac of St. Thomas' must also be mentioned.

His teaching was spreading; a growing number of students and house surgeons demonstrated the success of his method. In 1874 Glasgow became a stronghold of antisepsis with the appointment of Hector Cameron who was joined three years later by William Macewen, perhaps the greatest of Lister's pupils. But we must remember that Lister worked until 1877 in a distant corner of the British Isles. His technique, although basically simple, demanded attention to a number of small details. Unless surgeons were interested enough to visit him, he could neither demonstrate nor explain the minutiae upon which success depended. Many surgeons came to him; parties from Germany and, later, from France; Saxtorph of Copenhagen, Kocher of Berne, Bassini of Padua among others. James Paget sent a house-surgeon. Christopher Heath and Croft made personal visits.

Two men demand special mention in this tale of disciples and opponents. First Thomas Spencer Wells, one of the more influential surgeons in London, who is sometimes represented as an obstinate opponent of antisepsis. Wells was one of those who awaited clear proof that antisepsis gave better results than simple cleanliness. He did not adopt the antiseptic method until 1878 when the improvement in his results, he wrote, was 'startling to himself'.[46] His ovariotomy figures show this to be true. Analysing one thousand cases, he experienced a mortality of 25.4 per cent in his first five hundred and of 25.6 per cent in his next three hundred. During the course of his next hundred operations he adopted antisepsis and his mortality fell to 17 per cent. Of the final hundred, all treated with full antiseptic precautions, only eleven died.[47]

John Wood of King's College Hospital is a lesser-known figure in the history of surgery but a better known character in the popular biographies of Lister. This rough, tough little Yorkshireman, with his sinister face and his limping gait, is the very stuff of a villain. As such he appears in the drama of Lister, the figure who represents the bitter antagonism of London. The legend seems to have appeared during Wood's lifetime for, at a meeting of the South London Division of the British Medical Association in 1879, he almost plaintively recalled that he had practised some form of Listerian technique since the earliest days. His case books bear witness to this. The notes show that he started dressing with carbolized lint in 1867 and with carbolic putty in 1868. He adopted the carbolic spray in 1872. He was not even deterred by a death from carbolic poisoning in April 1871, but experimented with the double carbolate of zinc and sulpho-carbolate of iron. Wood gave full credit to Lister; thus we find notes 'the operation was performed under the carbolic spray and the wound was dressed with carbolic on Lister's plan'; 'Mr. Wood dressed the wound yesterday again on Lister's principle'; 'Patient discharged cured, Lister's treatment being in this instance thoroughly successful.' Here is no picture of opposition; it depicts the attitude of John Wood and probably the attitude of London more accurately than does the accepted legend.

But Wells and Wood and London, and not only London, were waiting for clear proof that antisepsis was an improvement. Again and again the *Lancet* suggested that one ward should be set aside in a teaching hospital, where antisepsis might be exclusively used and a comparison made. Again and again the *Lancet* urged Lister to publish his figures and confute his opponents. Neither of these sensible suggestions were adopted; Lister refused on the grounds that statistics can be made to prove anything or nothing. The proof of his method came in October 1877, less than a month after Lister had been appointed to King's College Hospital.

The case of Francis Smith, Lister's first wiring of the fractured patella, is probably the most important single operation in the history of surgery. Lister proposed to infringe the most sacred rule of the pre-Listerian surgeon, to break the protecting skin over the site of a fracture. A simple fracture carried no risk to life; a compound fracture could often prove fatal; Lister was deliberately planning to convert a simple into a compound fracture. The fierce criticism aroused by his proposal centred attention upon his operation; when, in January 1878, Francis Smith walked out of King's College Hospital on both legs, the usefulness of antisepsis had been proved.

Lister's personal example and teaching drove the lesson home. By 1880, fifteen years after his first use of carbolic dressing, his antiseptic technique had been generally accepted. The effect upon surgical mortality may be seen in his own figures. Before 1865 the total surgical death rate was just under 12 per cent and the operative mortality 9 per cent, two-thirds of the latter being due to sepsis. At the end of 1869, in his only essay in statistics, Lister claimed that antisepsis had reduced his mortality in amputations from between 40 per cent and 45 per cent to 15 per cent, a reduction of two-thirds.[48] Lister's total mortality at King's College Hospital was 4.2 per cent, again a reduction of roughly two-thirds of the total death rate before 1865.[49] In the case books of William Watson Cheyne, who succeeded Lister at King's, we find a total surgical mortality of 3½ per cent, despite a greatly widened field; by 1910 abdominal surgery accounts for 16.5 per cent of his operations. The figures suggest that Lister's technique almost entirely wiped out deaths from sepsis, and reduced the total surgical mortality by two-thirds of the previous figure.

The work of Robert Koch, among others, and of Elie Metchnikoff showed that few pathogenic organisms are carried in clean air and that phagocytes can deal with the very small invasion likely to occur by air-borne infection. The great disadvantages of carbolic were patent to everyone, not least to Lister himself. Attempts to replace carbolic with other antiseptics resulted in failure; the Germans, notably Merke, Koch, Wolffhügel and von Bergmann, turned to Pasteur's original finding that micro-organisms can be destroyed by heat. In 1886 Ernst von Bergmann of Berlin, Gustav Neuber of Kiel, and C. B. Lockwood of St. Bartholomew's Hospital were using a mixed aseptic-antiseptic technique. Better methods of steam sterilization, introduced by Ludwig Lautenschlager of Freiburg in 1890, enabled von Bergmann to practise an almost purely aseptic technique from 1891; this was followed in Britain from about 1897 by Cuthbert Wallace of St. Thomas' Hospital and William Arbuthnot Lane of Guy's. The Boer War, which produced as its predominant injury a clean penetrating wound sustained in an abnormally sterile terrain, confirmed the Listerian surgeon in his belief that antisepsis had solved the problem of wound infection. He had overlooked the fact that antisepsis is useless against deep seated infection; this oversight became apparent during the early months of the First World War, which presented the common injury by high explosive, an extensive and deep wound contaminated at the time of infliction by organisms derived from a highly-cultivated terrain. Attempts to use an antiseptic upon these massively and deeply infected wounds failed utterly. By the end of the war the majority of surgeons had lost faith in antisepsis, although a few old-guard Listerians continued to use a modified antiseptic technique until the late nineteen thirties.

Those who so readily dismissed antisepsis because of its failure in the First World War forgot that the method, properly applied, had never failed in the planned wound of a surgical operation. This is no occasion for controversy; we will content ourselves with saying that the post-war generation of surgeons erected a complicated, cumbersome and potentially dangerous structure in place of Lister's simple and safe technique, and that much of the time and effort of their successors has been employed in patching up the innumerable cracks which have appeared in that structure. But, though we speak of modern asepsis and look upon antisepsis as obsolete, there is only one basic difference between the two methods and none in the principle underlying them. The sole difference is that Lister achieved an aseptic operation site by means of an antiseptic drug. His principle demanded and demands that no micro-organism shall ever come in contact with the wound.

Presumably this principle will remain unchallenged so long as surgical operations are performed or wounds are dressed. Herein lies Joseph Lister's unique and lasting contribution to surgery.

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Lister and the Development of Abdominal Surgery

by

JOHN SHEPHERD

THE LAST QUARTER of the nineteenth century saw a rapid and spectacular advance in the field of abdominal surgery. At the centenary of the publication of Lister's first paper on antiseptic principles it is of interest to survey Lister's own work in the abdominal field and to assess the influence of his discoveries on the expansion of abdominal surgery.

In considering the contemporary state of abdominal surgery the date 1865 is convenient because in this year the operation of ovariotomy, that is a planned excision of an ovarian cyst, had, after much strife, been accepted as a justifiable major surgical procedure. Despite successes by men such as McDowell of Kentucky in 1809, Lizars of Edinburgh in 1825, Jeaffreson of Suffolk in 1835 and Clay of Manchester from 1841 onwards, ovariotomy in Britain had been condemned, mainly by the influential London clinicians, who in most cases had never seen the operation. Between 1850 and 1857 scarcely any surgeon expecting to retain his reputation dared attempt ovariotomy. Only Clay of Manchester practised the operation consistently during this period but for various reasons, despite his remarkable successes, his influence was negligible and his statements were mistrusted. In 1857 Spencer Wells attacked the problem afresh and by 1865 he made ovariotomy generally acceptable. With a general technique which was crudely aseptic rather than antiseptic and with exteriorization of the ovarian pedicle he reduced a high mortality rate to a level which compared favourably with that accepted for major amputations at the time (Shepherd 1965).

Abdominal surgery up to 1865 was indeed almost synonymous with the surgery of ovarian cysts. If one consults any surgical text book of the time, for example that of Sir William Fergusson, there is little space devoted to abdominal surgery. Ovariotomy receives only brief mention. There is discussion of the drainage of abdominal abscesses, often in the right iliac fossa but seldom associated with disease of the appendix despite descriptions by many writers before 1865 proving that such an association existed. For large bowel obstructions lumbar colostomy was practised with occasional success. Hernia in the acute strangulated form was occasionally operated upon without opening the sac or, more dangerously, by the conversion of a strangulated gangrenous loop of intestine into a rather haphazard enterostomy at the hernia site.

Astley Cooper in 1816 (Brock 1952) had traversed the abdomen to ligate the abdominal aorta for aneurysm and there had been others bold, reckless or sometimes brilliant who defied convention to attack (without anaesthesia before 1846) conditions such as small bowel obstruction or penetrating wounds of the abdomen, the latter particularly on the battlefield. But these were isolated achievements and in 1865 they found no place in the accepted armamentarium of the surgeon.

It is quite clear that there were strong influences discouraging the invasion of the abdominal cavity by the surgeon. There was a teaching from the pathologists that the entry of air or the spilling of blood or other effusion into any body cavity would inevitably be fatal. If a spot of inflammation appeared on the surface of the peritoneum this would inevitably spread. While this was a recognition of the risk of infection understood only after Lister's application of the work of Pasteur, it is surprising that the risk was thought so much greater for abdominal explorations than for major amputations or major excisions accepted at the time as justifiable procedures.

If we accept that abdominal surgery stemmed largely from the work of the ovariotomists we may wonder why it took so long for these men to expand their repertoire. Lizars when he published his four cases of abdominal exploration (Lizars 1825) which include his one successful ovariotomy, forecast 'that operation should be resorted to more often for volvulus, intussusception, internal hernia, calculus and other obscure disease of the abdominal viscera'. This was the first suggestion that laparotomy should be used as a diagnostic technique. But criticism was too much for him and he made no further attempts at abdominal operations. Clay, whose experience and whose success up to 1865 equalled those of Spencer Wells, showed no ambition beyond the ovaries and although like any other gynaecologist, he occasionally made a mistake and diagnosed a uterine tumour as ovarian and was not afraid to remove it, he did not attempt other abdominal surgery. He may be credited with one of the first attempts at hysterectomy and this was in 1842. Spencer Wells operated for intussusception in an infant in 1863 and in the same year he did a splenectomy. Such boldness was not regarded with favour. After 1865, with the establishment of ovariotomy, it might have been thought that Lister's work would initiate an immediate and rapid expansion of abdominal techniques; in fact, real advances did not appear for more than ten years.

We can estimate with some accuracy Lister's knowledge and experience of abdominal surgery up to 1865. In his student training he probably heard ovariotomy condemned by his London teachers who in the late 1840s were conservative and perhaps bigoted on the subject. When Lister went to Edinburgh he immediately came under the influence of Syme. Since 1831 when Lizars had defeated Syme in the election for the Chair of surgery in the College of Surgeons Syme had waged a campaign against Lizars. He condemned Lizars' attempt at abdominal surgery, defamed his character and finally ruined his career by a vicious litigation. We must assume that surgeons in Edinburgh in Lister's early period were timorous as far as abdominal surgery was concerned and that Lister's experience was limited to the management of advanced abdominal abscesses and of strangulated herniae.

Lister's operation lists in the Glasgow period are extant but are not indexed. It appears that he showed at this time no particular interest in abdominal surgery. It certainly does not seem that he was caught up in the wave of enthusiasm for ovariotomy which developed between 1860 and 1870. (Glasgow did not, between these years, possess an enthusiastic or successful ovariotomist.)

Lister introduced the antiseptic method by describing in the *Lancet* of 1867 his series of compound fractures (Lister 1867a). Later in the same year in a paper at the Dublin meeting of the British Medical Association he described a case of strangulated inguinal hernia 'in which it was necessary to take away half a pound of omentum' (Lister 1867b). He obtained rapid healing by applying 1:20 carbolic to the wound and he used his dressings with antiseptic paste thereafter. Although this was scarcely a major abdominal procedure it is the first record of the use of the antiseptic method in abdominal surgery. Much more important is Lister's statement in the same paper that 'when the antiseptic treatment is efficiently conducted, ligatures may be safely cut short and left to be disposed of by absorption or otherwise . . . when the knot is applied on the antiseptic principle we may calculate, as securely as if it were absent, on the occurrence of healing without deep seated suppuration'. At this time Lister was talking of thread or silk ligatures and it is not certain that initially he prepared these antiseptically by soaking them in carbolic. He depended rather on the local effect of carbolic applied while he dealt with the potentially infected tissues at the time of the operation and on the protection of the ligated vessel stumps from infection by his antiseptic dressings. Lister indicated, in 1867, how it was in the power of the surgeon to discard the long ligature—so often the cause of fatal secondary bleeding after amputation, of wound abscess or of chronic sinus.

In 1868, while still in Glasgow, Lister published in the *British Medical Journal* a further important paper on the antiseptic system. (Lister 1868). This includes the dramatic case of a young butcher who having 'thrown a dirty bladder at his workmate had in return received a knife in his chest'. The thoracic and abdominal cavities were penetrated. Extruded omentum was excised and the wounds were soaked and dressed with carbolic. The pleural cavity was cleaned and plugged with lint soaked in carbolic. This seems to have been the first occasion on which the antiseptic method was applied to a thoraco-abdominal injury with success. It gave Lister's house-surgeon, Mr. Cameron, great pleasure seven weeks later to see the butcher driving a herd of unruly cattle to the market and 'his lusty language testified to the soundness of his lungs'.

In the same paper Lister described the antiseptic preparation of silk ligature material. Later in 1868 he began his classical experiments on catgut and succeeded in preparing a sterile and manageable material for all purposes. Spencer Wells' clumsy method of exteriorizing the pedicle could at last be discarded. Simpson's much abused acupressure technique became unnecessary. The surgeon could now tie a major visceral vessel in the abdomen, cut the ends of the ligature short and return the stump with a reasonable certainty that he had not implanted infected material.

When Lister returned to Edinburgh in 1870 ovariotomy was being done with increasing frequency and safety. To his colleague Thomas Keith he was glad to offer any suitable cases for the operation. It was in Lister's character that he should seek the help of the acknowledged expert, while a lesser man would not have done so. Keith at this time, without antiseptic precautions, was achieving results surpassing those of Spencer Wells. Much of his success may have been due to the fact that although he was appointed 'extra surgeon for ovariotomy' to the Edinburgh Royal Infirmary he always operated in a small private hospital away from the risks of infection in the crowded general ward. By 1870 Keith had the astonishing record of a hundred ovariotomies with only nineteen deaths. Those who were against Lister's work held up in evidence the success of ovariotomists like Wells and Keith without antisepsis. Lister recognized that the peritoneum had a remarkable resistance to infection and he attributed this to the vigorous production of lymph. Keith was for a while dissuaded by Lister from using carbolic solutions in the peritoneal cavity. Lister considered that the chemical effect would interfere with the normal protective mechanisms. This and the risk of excessive absorption of carbolic would, he believed, outweigh any advantages of bacterial control. He often commented that the successful ovariotomists achieved their very good results by observing at least some antiseptic principles. In due course both Keith and Spencer Wells adopted Lister's methods, preparing their ligatures, instruments and sponges antiseptically, wielding the spray and using carbolic dressings. Both surgeons halved their mortality rates after they adopted the antiseptic system completely but at the same time both only recently had discarded clumsy methods of dealing with the ovarian pedicle for the simple sterile ligature cut short. It is very likely that the latter change was as important, if not more important, than the carbolic acid.

To return to Lister's experience of abdominal surgery. In his second Edinburgh period he operated for strangulated ventral hernia on at least two occasions, freeing adherent intestine and as he worked, soaking all the tissues with antiseptic cloths and repairing the defect with catgut. But few other abdominal operations are recorded. William Dobie, one of the four assistants who went with Lister from Edinburgh to London in 1874 made careful notes of Lister's lectures and operations during this period and these records have been recently studied (Shepherd 1967). In Lister's last four months in Edinburgh, Dobie records no abdominal operations. In the first six months at King's College there is the record of an attempt to resect gangrenous intestine following strangulation in a hernia. Although this operation was not successful it is noteworthy that Lister did a resection and anastomosis in 1874 and that the procedure was not generally established until well after 1880. Dobie's record of the case is as follows:

Case of Strangulated Inguinal Hernia.
John Richards—Aet 65.

Patient was admitted about 12 o'clock on Sunday 4 Nov. (1877) . . . On Thursday last, 1 Nov., he was seized with violent pain in the abdomen and then first noticed the existence of the tumour—since then he has had stercoreous vomiting for two days . . . On admission the patient was in a completely collapsed condition . . . the hernia was found to be tense and incompressible . . . Mr. Lister examined the case about 2 o'clock and, having tried taxis under chloroform without effect, at once proceeded to operate. Cutting down upon the sac he prolonged the incision upwards . . . the hernia being still irreducible, he opened the sac and discovered a thickened portion forming a constriction which he divided . . . and gently pulled up the protruded gut from the scrotum . . . it was almost black in colour . . . Two courses were now open to him: either to return the gut as it was and trust to the formation of an artificial anus: or to adopt a mode of procedure which he had long thought of might be practicable, viz: to cut away altogether the mortified portion of intestine and then carefully stitch the sound ends together. On this latter course he decided, as giving the patient the best of two very poor chances of recovery.

He accordingly put a temporary ligation of tape on the sound intestine above and below the mortified portion, the contents of which he drew off with the trochar and canula. He then passed another tape ligation round the mortified part above and below; in order to guard against the possibility of any faecal matter getting into the abdominal cavity . . . when the gut should be divided.

He next strangulated the attached mesentery in three parts by ligatures of catgut, and cut away the mortified piece of bowel . . . Having made free use of the carbolic lotion to obviate if possible the risk of putrefaction, the next procedure was to stitch accurately together the divided ends of intestine—this was done by passing interrupted stitches of fine catgut at intervals round the circumference and filling in the spaces with continuous sutures (Glovers' stitch) the edges being slightly inverted all round—the tape ligatures were then removed, the bowel returned into the abdominal cavity and the peritoneum and deeper textures stitched with catgut, the integuments with silk—a drain of horsehair was introduced, and the wound dressed antiseptically in the ordinary way. The operation took about 2½ hours.

Patient never recovered from his collapsed condition and died about 8.30 p.m.

6 Nov. a P.M. exam. was made—there were no signs of effusion. The gut was found in the same condition in which it was left. The piece of gut with the suture was removed and subjected to the test of water run into it from a tap, a pressure a hundred-fold greater than any which would have existed in the abdominal cavity the contents of which are self-supporting—the water escaped in a very fine stream at one point only.

This meticulous case report by Dobie is given almost in full because it illustrates Lister's courage in adopting a procedure which he knew to be right even although at the time it might well be open to criticism. The steps of the operation were carefully planned and executed and could scarcely be bettered today. Undoubtedly the patient died of shock associated with the gross disturbance of electrolyte and fluid balance which we today have the knowledge to correct. We know also that if chloroform was continued in such circumstances for two and a half hours the effects might well have been dangerous. We must admire Lister's determination to find out by autopsy what error might have been committed and his insistence on testing his anastomosis. This report demonstrates very clearly how Lister tackled each surgical problem presented to him and one must recognize that even if he had not been the one to develop the antiseptic system his place would have been assured as an outstanding surgeon of the nineteenth century.

At King's College Hospital between 1877 and 1887 Lister tackled such varied problems as imperforate anus in the infant (for which colostomy was performed), removal of abdominal cysts thought to be hydatid in origin, cholecystostomy for obstructive jaundice (in 1884), gastrostomy for oesophageal obstruction and appendix abscess.

In an address in 1890 in reference to abdominal surgery Lister said modestly 'This is a department of surgery in which I have had but little personal experience'. His biographers have been rather misleading, perhaps in consequence of Lister's own remarks, in suggesting that there was a lack of interest in abdominal surgery. Godlee (1917) does not even mention abdominal surgery in listing Lister's contributions to almost every other branch of surgery at the end of his biography. Although Lister did not become involved in the ovariotomy problem as did so many of his contemporaries it is clear that he applied his mind to many other aspects of abdominal surgery and his experience was far from negligible.

It is difficult to assess the direct influence of the introduction of the antiseptic method on the expansion of abdominal surgery. Lister's reluctance to recommend it wholesale for abdominal work has been mentioned although he must at least have approved of dressing abdominal wounds antiseptically. He feared, and his fears were in part realized, that the more stupid surgeons of the time, obsessed with the idea that it was the carbolic acid and not the principles which mattered, would swill out the peritoneal cavity with this noxious substance and some would perhaps stake all on what to them was the mystical action of the spray.

In Great Britain the surgeon who really capitalized on the experience of the ovariotomists to expand abdominal surgery was Lawson Tait of Birmingham. This man of undoubted genius, recognized with some justification by many as the father of abdominal surgery, was antagonistic to Lister. This was just one more example of the jealousies and quarrels of the age. Tait was a disciple of Simpson, who had quarrelled with Lister's father-in-law Syme. Syme had attacked Simpson, in particular concerning his acupressure technique. Simpson had criticized Lister's work rather obstinately by concentrating on the fact that others had used carbolic acid before Lister. That Tait was prepared to perpetuate such quarrels is clear even from the title of one of his articles in which he enumerates his successes in ovariotomy 'without any Listerian details' (Tait 1882). Tait must, however, have been influenced by Lister's work. As Lister said, the successful ovariotomists did in reality adopt many of the basic techniques of his system.

Some have suggested that Tait introduced aseptic surgery. This is an exaggeration as his technique was an extraordinary mixture of antisepsis and general cleanliness. McKay (1922), Tait's rather partisan biographer, gives an interesting comparison of the two surgeons at work. Both prepared the skin of the operation site, Lister with carbolic, Tait with soap and water. Lister soaked his instruments and sponges in carbolic: Tait at first cleaned his instruments in tap water and later boiled them, but his sponges were treated in carbolic. Lister washed his hands in carbolic: Tait used soap and water. Lister used towels soaked in carbolic and applied them to the tissues at operation: Tait, if he wanted to wash out a wound or the peritoneal cavity used warm water which had been boiled (although at first he used ordinary tap water). Lister favoured carbolized catgut as his ligature material: Tait used boiled silk. Tait was ahead of Lister in boiling his instruments and ligatures. Lister, on the other hand, had a scientific reason for every detail of his technique and the full aseptic ritual would never have evolved without Lister's logical development of the antiseptic system.

Tait, with exceptional boldness and technical ability, in the short period between 1879 and 1885, having established himself first as a master of ovariotomy and of operations on the uterus was then the first in England to do cholecystostomy (in 1879), first to diagnose and remove an acutely inflamed appendix (in 1880) (Shepherd 1956), first to operate successfully for ruptured ectopic gestation (in 1884). These were only some of his achievements. No surgeon in the world can be said to have advanced abdominal surgery more than did Tait in such a short period. Despite his denial of the importance of Lister's work we can see a direct effect of the introduction of the antiseptic system. More important, Lister's work had cut down dramatically the dreadful morbidity and mortality from infection in hospitals and the surgeon dared to operate more ambitiously in the large hospitals.

The two major impacts on surgery in the nineteenth century, the introduction of anaesthesia and the introduction of antisepsis, were both followed by a slowing up in the progress of abdominal surgery. In the ten years after the introduction of ether and chloroform (in 1846 and 1847) there were fewer successful ovariotomies performed in Great Britain than in the preceding ten years. In fact Clay contributed almost all the successes between 1847 and 1857. During the latter period there were no advances in abdominal surgery. After Lister introduced his methods in 1867, although by this time Spencer Wells had established ovariotomy as an acceptable abdominal operation, it was ten years until a rapid expansion of abdominal surgery was initiated, particularly by Lawson Tait.

These time lags may be interpreted in different ways. Some may say that they represent a conservative caution which is beneficial to medicine or surgery. Others may suggest that they represent a period of abuse or misunderstanding of a new method. Perhaps our successors will point to a similar period of abuse or misunderstanding of two notable discoveries of the twentieth century; those of the antibiotics and the steroids. We have not shown a very high degree of intelligence in our use of these remarkable additions to our armamentarium. Perhaps as doctors we are not very good at accepting new discoveries!

There is a short postscript relevant to the history of abdominal surgery in Great Britain. When Edward VII, just prior to the planned date of his coronation, developed appendicitis, Lord Lister, retired and rather out of touch with clinical work, was summoned. By the time he joined Sir Frederick Treves in consultation the august patient had suffered many days of severe pain. The first surgeon to see him had probably talked of the hypothetical entity of typhlitis rather than of appendicitis. Treves had remained on the whole convinced that the interval operation was the correct procedure for appendicitis and had seldom removed the acutely inflamed appendix. Lister had never, as far as we are aware, removed an acutely inflamed appendix. By the time Lister saw the king there was no problem but that of deciding when the scalpel should be plunged into the large right iliac fossa abscess. In 1902 Lister was just as able to support the diagnosis and advise treatment as he would have done for a similar case in Syme's wards in Edinburgh in 1855. Fortunately the king recovered but it is a sad reflection that the highest in the land could not command more up-to-date surgical skill. Despite Tait's pioneer work (and despite the clear lead on this subject from long before 1900 by the Americans) British surgeons had been slow to recognize the necessity for operating for acute appendicitis (Shepherd 1954).

Edward VII met Lister later and said 'Lord Lister, I know that if it had not been for you and your work I would not have been here today' (Guthrie 1949). In fact the king owed little to surgical science but nature had, as often, localized an abscess and little was asked from the surgeon except to let out the abscess. But let us allow the royal tribute as more than justified as a generalization!

ACKNOWLEDGEMENTS

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Medical Chemistry and Chemical Medicine in the Nineteenth Century

by

D. N. RAINE

'What a zig-zag path, how unlike a straight line is man's progress in search of Truth.' Michael Foster.

IN THE field of chemistry, a subject which, particularly in its biological application, is considered to have flourished only in recent times, it is a constant surprise to discover the antiquity surrounding observations and procedures in common use today. The use of *lignum nephriticum* and later syrup of violets to distinguish between acids and alkalis,[1] the excretion in urine of certain pigments of dietary origin,[2] the decomposition of food by the fermentive action of saliva and pancreatic juice[3] and the saccharine nature of diabetic urine[4] were all known before 1700. Urea, the 'native salt of urine', was isolated[5] before 1750 and the importance of uric acid in the formation of calculi, as well as the characteristic purple colour this substance gives with nitric acid, the basis of the murexide test, were all known before 1800.

It is by no means uncommon to find however that the full and proper utilization of knowledge has been delayed not for years but for decades and it is the purpose of the present discussion to examine the extent and some of the reasons for the delay in exploiting the chemical resources available to the ninteenth-century medical practitioner.

CHEMISTRY IN 1800

At the start of the century Dalton had not yet revealed his *New System of Chemical Philosophy* and Humphry Davy,[6] who was to be one of his more outspoken antagonists, grouped the simple bodies, those which had so far remained undecomposed, into two gases that support combustion, seven inflammable bodies and thirty-eight metals—a total of forty-seven. The two gases were oxygen and chlorine and the remaining elements included almost all that were of importance in biology. Carbon, hydrogen, nitrogen, phosphorus, and sulphur were recognized, as were calcium, magnesium, and manganese. By the middle of the century iodine had been added with the other halogens[7] and the whole of the century saw the discovery of only twenty new elements, most of which were metals.

Organic chemistry was still very descriptive and although dry distillation had declined as a form of chemical analysis it had only been replaced by a few empirical tests and in no sense could it be described as systematic. This situation persisted into the third and fourth decades of the century and William Prout[8] looked forward to the day 'when chemistry shall be brought more under the control of laws of quantity' and he saw it only as a 'science of observation'. In the preface to his Bridgewater Treatise, Prout[9] declared that 'the phenomena of chemistry can neither be represented by figures, nor adequately described to the inexperienced by words'—but the turning point had been reached, for in Germany the 'inexperienced' were about to be addressed on this very subject by Justus von Liebig[10] in a series of letters to his local newspaper, the *Augsburg Allgemeine Zeitung*.

ANIMAL CHEMISTRY IN 1800

A review of the state of biochemistry at the opening of the century is greatly facilitated by the publication of a little-known three-volume work, *History of the Progress and Present State of Animal Chemistry* by W. B. Johnson,[11] a medical man who addresses himself from Derby.

Animal chemistry was quite as descriptive as pure chemistry at this time and some pages of Johnson's book are devoted to speculation, often very pertinent, on how the meagre knowledge available might be applied for the good of mankind. Most biological tissues and fluids were examined with equal assiduity. Apart from blood and urine, milk, gastric juice, bile, saliva, tears, sweat, nasal mucus, pus, synovial fluid, semen and liquor amnii were analysed to the best of contemporary ability; so were muscle, liver, brain, skin, tendon, bone, and hair of man as well as animals. Faeces too were not eschewed and the discovery that the excreta of a boa constrictor, the subject of a wayside exhibition in the Strand, consisted almost exclusively of uric acid led William Prout[12] to suggest that the animal's captivity had rendered it diseased. But in the true spirit of research he invited John Davy, then resident in Ceylon, to repeat his observations on the animal unconfined, and so established one of the important differences between mammals and reptiles and birds, the form in which nitrogen is excreted.

MEDICAL CHEMISTRY IN 1800

The search for useful applications of chemistry to medicine at about this time is almost vain and such that can be found are more related to the understanding of disease than to its amelioration. Scheele, by showing that bladder calculi contained uric acid, a substance known to be of animal origin, dispelled the belief, still held in spite of significant observations by Stephen Hales and others to the contrary, that these were stoney in origin. Attempts to dissolve calculi *in vivo* were uniformly disappointing; however it may be fairly stated that by the time Wollaston had described the rare cystine calculus[13] in 1810, knowledge of the composition of calculi of all types was almost as complete as it is today.

Gall-stones too had been scrutinized closely and like renal calculi had been classified according to size and shape. But this was of little value to Poulletier or Fourcroy who, by their alcoholic extraction of stones from several different classes,[14] clearly obtained the same substance, cholesterol, in pure and crystalline form, although this major constituent had to be rediscovered by Tiedemann and Gmelin more than half a century later.[15] The physiological significance of cholesterol was not to be discovered within the century, indeed it was not even known whether it was produced within the animal body or ingested with material of vegetable origin.

DEVELOPMENTS DURING THE NINETEENTH CENTURY

The evolution of medical chemistry through the century can be traced with the help of a selection of the monographs published by the more notable contributors. These found a wider audience than the scientific and medical journals of the day and if some appeared to go through more editions than their specialized nature would warrant, this at least allowed extensive revision and the final version sometimes bore little resemblance to the work at its first appearance.

Such was the case with Prout's *An Inquiry into the Nature and Treatment of Diabetes, Calculus, and the Affections of the Urinary Organs*, which first appeared in 1821. Prout lists the simple apparatus required for testing urine for acidity using litmus, protein by heat coagulation, bile by the yellow staining of linen, and the slide test for discriminating between mucus and pus. Sugar was still recognized by taste and by observing the sticky patches where urine had spotted the clothing of diabetic patients. He also used an interesting test for urea based on the speed with which crystals formed when nitric acid was added to the urine in a watch glass. With the exception of the last test, which he claimed allowed him to recognize the prediabetic state, no new diagnostic procedures were described. Even in 1834 Prout's Bridgewater Treatise on *Chemistry, Meteorology and the Function of Digestion* was more concerned with gross chemical and physiological organization than the need to differentiate the several causes of diseases having similar manifestations.

In 1844 an Assistant Physician at Guy's Hospital, Golding Bird, published his *Urinary Deposits, Their Diagnosis, Pathology and Therapeutical Indications.* Notice that the diagnosis and pathology refer to the deposits, not to the patients from whom they were obtained, but now we have 'Therapeutical Indications'. Although this work was published after Liebig's two important works *Organic Chemistry in its Applications to Agriculture and Physiology* and *Animal Chemistry or Organic Chemistry in its Applications to Physiology and Pathology*, it was too soon for it to be affected by them, but for the first time we see chemistry being related to organs rather than to the totality of animate nature, and in a large folding table Bird lists the elements of blood, elements eliminated by the liver and elements eliminated by the kidneys together with the chemical modifications of these substances by which they were recognized. Even so the information was of more interest than utility.

In 1863 two further books on urine were published, one by A. H. Hassall[16] of the Royal Free Hospital, and a further more important work translated a few years after its first publication in Germany by

Carl Neubauer and Julius Vogel.[17] Both works discuss a much wider range of chemicals and now speak of their changes in specific disorders such as typhus, meningitis, osteomalacia, rickets, scrophula, articular rheumatism and a number of less easily recognizable conditions. This represents a complete change in approach and it was one of which Dr. Vogel at least was conscious, for in speaking of the value of specific gravity determinations in typhus and other fevers he comments, 'The idea of our being able to distinguish diseases by one single phenomenon, and one which in comparison with other symptoms appears very unimportant, we must ascribe to the now happily admitted ontological method of comprehending diseases. By this method of division and classification of diseased processes, just as in the division of animals and plants in genera and species, the external appearances alone, with their thousand accidents are seized upon, instead of the essential character of the phenomena, their causes and connections, and dependence being kept in view.' At last an objective approach—but there was still a long way to go, for it was necessary to retrace now the path from these 'external appearances' back to 'the essential character of the phenomena' but now by a strictly logical process. The need for new knowledge was now apparent and new knowledge came thick and fast. The steady tread of this path has been followed from that time to the present day and will continue for some time to come. What then were the changes that brought it about?

THE INTELLECTUAL REVOLUTION

It may be presumed from the remark of Dr. Vogel that the change hung on a new look at disease; there is no doubt that this was important. But a matter of equal importance lies in a remark of Sir Eric Ashby,[18] 'At the threshold of the nineteenth century, 113 years after the publication of Newton's *Principia*, institutions for higher education in Britain were still making no contribution to scientific thought.' He goes on to say, 'Oxford was weighed down by every imaginable device for inertia' and 'In Cambridge, where the barriers to the introduction of science were less formidable, it could not be said, even as late as 1852, that scientific work had taken root there.'

Dr. Charles Newman,[19] after a search of the early clinical records in Guy's Hospital discovered that the first note concerning a physical sign occurred late in the year of 1824. Except in the hands of one reporter ten years later, such examinations remained rare and as late as 1859 the value of 'habitual observation' began to be officially urged, in the pocket book issued to clinical reporters, because, 'It is now universally admitted that this must enrich and impress the mind more easily and more permanently than any other means hitherto employed.'

In what way then had the English universities been slow to break new ground? The biological sciences had, like astronomy and geology, to pass through a phase dominated by description and classification. Thanks to the efforts of Linnaeus and Buffon in one area and of Sydenham in another, this was largely complete at the end of the eighteenth century and the divisions established by these systematists were beginning to hamper progress.

The barriers first began to fall in Germany, perhaps under the pressure of the essentially scientific philosophy of Descartes and Leibnitz. Early in the nineteenth century Goethe[20] created the term 'morphology' to refer to studies of the features that were common to organized beings, both animate and inanimate and in so doing he made an important attempt to break across the divisions between the several natural sciences. A similar phenomenon occurred in France at the hand of Xavier Bichat[21] whose two monographs, one on membranes and tissues and the other on the physiology of life and death, again stepped lightly over the divisions created by the naturalists. This new philosophy is cogently expressed in Bichat's definition of life as the totality of those functions which resist death. What better basis for the subsequent development of chemical pathology?

It was against this background that Professor C. F. Nasse cultivated a new approach to medicine and in his clinic at Bonn percussion and auscultation became established methods of examination nearly forty years before the same could be said in London.[22]

THE HYPOTHESIS TESTED

If, then, the delayed development of scientific medicine in this country is substantially due to the unadventurous attitudes in the English universities at the beginning of the century, we may have the means to put this hypothesis to the test, for to quote Sir Eric

Ashby[23] again 'the Scottish universities were more sensitive to the spirit of the age' and 'Edinburgh and Glasgow had flourishing medical schools, and the scientific subjects pre-requisite to these were taught, under the aegis of medicine and usually by medical men, to large audiences'. It would be of some interest therefore to discover when physical examination of the inmates of the teaching hospitals associated with these universities became established and to compare this with Dr. Newman's findings.

Meanwhile other evidence in support of this thesis may be obtained from the activities of the alumni of the Scottish medical schools. In 1802 a *System of Chemistry*, a popular text for many years to come, was published by Thomas Thomson, who graduated M.D. from Edinburgh in the last year of the eighteenth century. Thomson had been trained by Joseph Black who, although unsuccessful in finding a solvent for the urinary stone, made important contributions to chemistry from the combined Chair of Chemistry and Medicine in Glasgow and later from Edinburgh. In 1819 Sir Robert Christison graduated from Edinburgh after he had been examined not only on the anatomy, physiology and diseases of the stomach but also on the *chemistry* of some of the remedies he had proposed.[24]

The two most notable contributors to medical chemistry in the early years of the century, Alexander Marcet and Richard Bright, both spent their formative years in Scotland and graduated M.D. at Edinburgh within twelve years of each other. In 1831 John Thomson, the Scottish surgeon and author of another textbook of chemistry, became the first Professor of Pathology in Edinburgh and in the present context, at a time when it was necessary to explore the borderlands that divided specialized knowledge, it is interesting to learn that Thomson did not even acknowledge the separation of medicine and surgery.[25]

If more evidence is required for the advanced outlook of the Scottish universities it may be noted that the first of the two great works of Liebig was translated in 1840 by 'that intelligent young chemist' Dr. Lyon Playfair of St. Andrews and in 1842 the *Animal Chemistry* by William Gregory, Professor of Medicine and Chemistry at Aberdeen. Three years later another important work on Animal Chemistry by J. F. Simon was translated by G. E. Davy a year or so before he was appointed to the Chair of Medicine at St. Andrews.

It has not been the purpose of the present discussion to review the important contributors to clinical chemistry; many names, some well known, others less so, have been omitted. Instead an epistemological problem has been examined and although this has been done against the background of the nineteenth century, it is a problem that is always present. In Michael Foster's words the search for Truth indeed follows a zig-zag path and if the examination of history serves no other purpose it may reveal, in a now more relevant context, observations that have long been forgotten.

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Medical Attendance on Royalty
The Diaries of Dr. Edward Sieveking

by

NEVILLE M. GOODMAN

THIS PAPER is based on the contents of a black tin box. We all know the japanned black box which turns up in the attic or the cellar, or at the solicitors' or the bank, containing a time-bomb in the shape of a birth- or a marriage-certificate—or their absence; but in this case the box contained, with a few other papers and notebooks, the two locked diaries of Dr. Edward Sieveking's attendances on Their Royal Highnesses Prince Edward of Wales and Princess Alexandra, from 1863 to 1869. Originally there was a trunk-full of papers but these were destroyed by a descendant. Fortunately, through the kindness of a great-neice, Miss Louisa Sieveking, all that remained—the black tin box—was given to the Harveian Library of the Royal College of Physicians in 1959.

The Diaries reveal no intimate medical or other secrets about Royalty or other public characters; but they do outline a great personal drama in the life of their author; and they do throw considerable light on medical practice and social customs in Royal circles in the sixties.

First, the personal drama. Edward Henry Sieveking was born in the year after Waterloo, the eldest son of a Hamburg merchant, who had settled in London seven years before Edward's birth. His mother also came of Hamburg merchant stock and there were Danish connections in his ancestry. Possibly because his mother rejoiced in the Christian names of Emerentia Louisa, he married a Miss Jane Ray of Finchley, when he was 33, and had a numerous family. His aunt, Emilia Sieveking, was a remarkable woman. She volunteered to nurse in the Hamburg Cholera Hospital in 1831—an unprecedented step for a lady at that period. Later she founded in Hamburg the first Protestant Sisterhood of Mercy in all Germany. In 1837 she was asked to become Head Nurse at Kaiserswerth, near Dusseldorf, founded three years after the Hamburg Sisterhood; but refused. Florence Nightingale visited and trained there in 1848.

Sieveking studied medicine at University College, Edinburgh, Berlin, Paris and Bonn, obtained his M.D. Edinburgh in 1841, when he was 28, his F.R.C.P. London in 1852 and was elected a full Physician on the staff of St. Mary's in 1866, after no less than fifteen years as Physician to Out-patients and Lecturer on Materia Medica. He edited the *British and Foreign Medico-Chirurgical Review* from 1855 to 1860, and published a *Manual of Pathological Anatomy* with Dr. Handfield Jones in 1854, and a book on epilepsy.

Yet, by all accounts—and the late Sir Henry Tidy's father knew him well and Tidy discussed his character with me—though a hard-working, thorough, even meticulous man, he was definitely not quite of the first flight. Compared to other physicians originally appointed with him to St. Mary's in 1851—Sibson, Handfield Jones and Broadbent—his teaching was uninspired and his manner reserved and cold.

In 1863, came Edward Sieveking's great opportunity.

The Queen and the Prince Consort had agreed, before the latter's death, that the time had come to find a wife for the Prince of Wales. Vicky, the eldest and favourite daughter, who was then Crown Princess of Prussia, had been empowered to inspect the six eligible Protestant German Princesses and had found them all quite impossible. Almost by chance there was then brought to her notice Princess Alix, eldest daughter of Prince Christian, the heir to the throne of Denmark, whose beauty was only equalled by her charm. Vicky became her enthusiastic advocate; Edward fell genuinely in love when he met her; and even the Queen was roused temporarily from her monumental mourning to give her consent.

Now as a married man with Establishments of his own at Marlborough House and Sandringham, the Prince would need his own Medical Household. What more natural than that a Consultant Physician, of Danish extraction and speaking German and French, should be chosen to attend the new Danish Princess?

At any rate the Diary opens on 29 January 1863 with a copy of a letter from Sir James Clark,[1] the Queen's Physician and all-powerful with her, though generally still unpopular because of his share in the Lady Flora Hastings scandal of 1839, for which he was wrongly blamed. The letter stated that the Prince of Wales wished to appoint Dr. Sieveking as one of his Physicians-in-Ordinary. He spent a weekend at Bagshot Park with Sir James, who told him that Drs. Jenner[2] and Parkes[3]—both from University College Hospital—had helped Sir James in his choice. Dr. William Jenner, appointed Physician-in-Ordinary to the Queen in the previous year and to the Prince of Wales at the same time as Sieveking, gave him hints on his behaviour and told him that no one influenced the Queen in medical matters save Sir James Clark.

After visiting Marlborough House and inspecting the drains—which were abominable—the ventilation and the fire escapes, he was received at Windsor by Queen Victoria and the Prince of Wales, and by the Prince and Princess of Wales at Buckingham Palace on 24 March, a fortnight after their marriage. All went well and, on Friday 8 May, he paid the first of his professional weekly visits to Marlborough House, alternating with Dr. Jenner, who attended on Tuesdays. His only patient on that occasion was a smelly sink. A week later he saw the Prince and Princess professionally and successfully prescribed for their colds. In August he was summoned to Abergeldie Castle where they often stayed when the Queen was at Balmoral, two miles away.

Princess Alexandra was by now pregnant and the Queen discussed the management of the pregnancy with him. In October, he submitted a 'Note on the selection of a head nurse for the Prince of Wales's Establishment. Salary fifty pounds a year', which resulted in his own family's nurse being eventually accepted. A team of two Accoucheurs,

[1] Clark, Sir James (1788–1870). Med. educ. Edinburgh. Practised in Rome where he attended the poet Keats. The Prince Consort met him when visiting European Spas and appointed him his Physician and later persuaded the Duchess of Kent to do the same. Cr. Bart. 1837 and later given Bagshot Park by the Queen.

[2] Jenner, later Sir William (1815–1898). Professor of Pathol. Anatomy at University College Hospital. Physician in Ordinary to the Queen in 1862 and to the Prince of Wales in 1863. Cr. Bart. 1868. P.R.C.P. 1881–88. Credited with distinguishing between typhus and typhoid.

[3] Parkes, Edmund Alexander (1819–1876). Professor of Clinical Medicine at University College Hospital. Superintended the large Civil Hospital at the Dardanelles in the Crimean War. 'The founder of modern hygiene in Gt. Britain'.

Dr. Arthur Farre[4] and Dr. George Gream,[5] who would be in attendance with Jenner and Sieveking, was assembled after some wrangling, but the Princess stole a march on them by being delivered prematurely of a seven-month boy at Frogmore on 8 January 1864, with no one present except Lady Macclesfield, a Woman of the Bedchamber, who had fortunately had twelve children herself, and a local practitioner from Windsor, Dr. Henry Brown,[6] who arrived at a gallop at the last moment. The child was Albert Victor, later the Duke of Clarence.

Nothing further of importance occurred during the rest of 1864 (except a squabble between the wet-nurse and the other nurses) until, in September, Sieveking accompanied the Wales to Denmark on the Royal Yacht and brought the baby back alone in a warship to his grandmother at Balmoral. The Queen expressed her great displeasure at the baby's having been kept so long in Denmark, though she exonerated Sieveking from blame. He did not see the baby after his return until five months later, by which time the Princess was pregnant again. On 3 June 1865, Sieveking and Farre were called to the Princess, who was delivered easily of a second child, later to become George V. The following month, the nurse, which he had supplied from his own family, was given notice because she had offended the housekeeper and quarrelled with the wet-nurse and the monthly nurse. He continued to visit occasionally, but only when summoned, during 1866, and in February 1867 the Princess had an attack of rheumatic fever, in the middle of which she was confined of a girl. She was seriously ill and in severe pain from inflammation of the joints; and there was great public emotion. By March she was improving but it was becoming clear that the Accoucheur, Farre, and

[4] Farre, Arthur (1811–1887). Professor of Obstetric Medicine at St. Bartholomew's, 1841. Physician Extraordinary to Queen Victoria and Physician-Accoucheur to the Princess of Wales. Founder and Pres., Royal Microscopical Society. Bequeathed a library of over 1,000 rare obstetrical and other books to the Royal College of Physicians.

[5] Gream, George Thompson (1811–1888). St.George's. Physician-Accoucheur to Queen Charlotte, the Princess of Wales and the Empress Frederica. Wrote against chloroform in labour.

[6] Brown, Henry, M.R.C.S., L.S.A. (1802–1868). Educ. the London Hospital. General practitioner at Windsor and Surgeon to Queen Victoria (1833–66), Prince Albert and the Duchess of Kent and their Households at Windsor. Erroneously stated by Sir Philip Magnus and others to have received a Knighthood after delivering Princess Alexandra of her first child.

the Surgeon, Paget,[7] had to some extent elbowed-out the physicians—or at any rate Sieveking. Farre announced that the Prince wanted no further family consultations 'as the Princess disliked a posse of doctors coming in together'.

Nevertheless, Sieveking continued to attend occasionally and was summoned to the Princess's fourth confinement in July 1868, and her fifth in November 1869, both of which passed off without trouble. We find diary entries at both these times: 'The Princess very gracious and chatty'. 'The Princess very jolly and the Prince very chatty'.

But here the diary entries cease. In spite of the chattiness, from that time, Dr. Sieveking was never called in again either to the Prince—even during his desperate attack of typhoid two years later at Sandringham—or to the Princess or to the Household, though he continued to hold his post as Physician-in-Ordinary.

Under the date of 31 May 1873, two-and-a-half years later, is a copy of a Memorandum to the Prince of Wales complaining that he had been neglected; of a reply from Sir William Knollys, the Prince's Equerry; and a final note by himself. This reads:

> Sir. Wm. Knollys' letter entirely begs the whole question and though intended as an apology, in no way rectifies my position to the public. I was, excepting Sir W. Jenner, the only Physician-in-Ordinary to the Prince and an unmerited slight has been put upon the office by my not only not being called in but by my being superseded by a man, who at the time held no appointment in the Prince's household.

The reference is almost certainly to Sir William Gull.[8]

None of these documents suggests any reason for his supersession. In his Memorandum to the Prince, in 1873, he himself writes: 'For reasons I have never been able to fathom, my attendance at Marlborough House has not been commanded since December 1869'. I have not been able to find any other sources which shed light on his fall from favour: indeed, I am hoping that perhaps someone at this

[7] Paget, Sir James (1814–1890). Surgeon, St. Bartholomew's, 1861. Surgeon Extraordinary to Queen Victoria, 1861: Sergeant-surgeon, 1867. Cr. Bart. 1871. Wrote on surgical pathology. F.R.S.; P.R.C.S. 1875. Became personal friend of Prince and Princess of Wales.

[8] Gull, Sir William Withey, Bart. (1816–1890). Ass. Physician at Guys, 1851 and Physician, 1858: resigned 1866 because of his vast practice. Invited by Sir William Jenner to attend the Prince of Wales in 1871 when ill with typhoid and appointed his Physician-in-Ordinary in 1873. A 'holist', he deplored the unnecessary use of drugs. Left £344,000.

Congress may have come across, or be able to suggest, some further line of inquiry.

One can, of course, speculate: and in my view, the basic cause of Dr. Sieveking's downfall was the Queen's constant attempts to interfere, through him, with her son and daughter-in-law and their children. This inevitably led them to regard him as her agent and perhaps her spy. The Diaries constantly refer to this interference and record the Wales's resentment of it.

For example, we find:

—The choice of Osborne for the Princess's convalescence after her first confinement 'is not acceptable (*à cause de la Reine*)'.

—The Prince annoyed at the Queen's command for a memorandum on the 'sanitary management' of the Princess to be drawn up by her physicians.

—Interview with the Queen and the King of the Belgians ('Uncle Leopold'). They both urged Sieveking to 'exert his authority to keep the young people in order' and actually told him to write direct to Queen Victoria.

—Summoned to the Queen. The Prince told me 'not to say too much to the Queen as otherwise I should be quoted as an authority for anything the Queen wished done'.

The Queen sent for me at Windsor to ask me to speak to the Princess about warmer clothes for baby at night.

I learnt that the Princess intended to nurse her second baby herself for a short time but this must be kept secret because the Queen would not hear of it.

And so on. If ever a mother-in-law needed to be told to **Keep Out** it was Victoria; but this was far from easy when the son 'had been kept in a state of terrorism at home'—Sieveking's words—and when the mother-in-law was not only a most formidable woman with a passionate interest in humdrum detail, but Queen of England as well!

But there is a footnote to be added. Enclosed with the Diaries is a letter from the Hon. Horatia Stopford,[9] one of the Queen's Ladies-in-Waiting. It is dated: Osborne, 26 January, 1886—thirteen years later—and is worth quoting in full:—

[9] Stopford, the Hon. Horatia Charlotte, d. of Col. the Hon. Edward Stopford, 2nd s. of 3rd Earl of Courtown and Viscount Stopford. Woman of the Bedchamber and Maid of Honour to the Queen since 1857 and a patient of Dr. Sieveking.

Private and very
Confidential

Dr. Sieveking
17 Manchester Square
London

Dear Dr. Sieveking,
I have at last had an opportunity of again speaking to The Queen about you, wh: you will remember I told you I intended doing as soon as I could! I spoke *very plainly* and in short told H.M. the exact state of affairs, *as* I had learnt them from you. H.M. was most kind in all she said, and expressed Herself as being greatly distressed at the way you have been treated by the *Wales* Family, and said had *She* been on the spot at the time of The Pce. of Wales's illness, She should not have allowed things to have been done that were done! The Queen is going to speak to The Prince of Wales, and will tell him a good deal of what I have told *Her*, and I think you will ere long find that something will come of my conversation with The Queen this day.
I dare not say more, and I would beg of you not to say a word of all this to anyone but y. wife! I do not mean to let the matter drop, now that I have made the first plunge! *Nothing* could have been kinder and nicer than The Queen was about you, and she said She never could get to know how and why it was, that The Pss. of Wales had behaved in this way to you. It seems that it is *her*, not the *Prince*. The Queen is very much annoyed about it! I write in tremendous haste wh: please forgive! Send me a line to say this has reached you safely!

Yours very sincerely,
HORATIA STOPFORD

What came of it was that later that year, 1886, he received a Knighthood. When King Edward came to the throne in 1901, his appointment as Physician-in-Ordinary was continued, but he was never called in again. He died in 1904.

So much for the personal story. What of the picture of medical practice in Royal circles, which is drawn by the Diaries? First, its mechanics.

Fees. Sieveking charged 2 guineas a visit to Marlborough House, 15 guineas a visit to Frogmore, and 10 guineas a day when away in attendance, e.g. at Abergeldie or in Denmark. In 1864, there was a discussion with Sir James Clark on whether Sieveking should receive a salary of £1,500 a year in lieu of fees, but it came to nothing. His accounts were paid promptly, unlike one of his predecessors, the surgeon Robert Keate, who attended four sovereigns, and said he was owed 9,000 guineas by one of them and that his attendance on Royalty had been his ruin (Clarke, J. F., 1874).

Summonses from a distance came by telegraph. We may feel that the telephone is a major plague of the doctor today but think of being summoned to Frogmore from London by telegram, with no indication of urgency or possibility of making further inquiries. Sieveking might leave a waiting-room full of patients, go by brougham to Paddington station, take a train to Windsor where he was hardly ever met and seldom found a cab, so that he faced a long walk across the Park, to find a baby with, say, green stools. He prescribes, and returns by the same laborious methods. Next day he cannot ring up to inquire but must repeat this performance only to find the baby quite recovered and normal. After this has happened several times, a hint is dropped that he is visiting too often!

Homeopathy seems to have been a constant menace. Robert Keate relates how Queen Adelaide consulted a homeopath and William IV insisted on his 'overhauling' the prescription to see if it was safe! Sieveking notes that the Prince 'talked like a liberal about politics and like a sensible man about homeopathy'. He also notes that neither the Duke of Cambridge nor Princess May 'showed any homeopathic tendencies'.

But above all, what strikes one is the almost complete absence of even the simplest aids to diagnosis and treatment. Temperature, as measured by the clinical thermometer and recorded, is not once mentioned in the Diaries, although Sieveking himself wrote a treatise on it in 1877. Neither is an examination of urine for albumen or sugar. Even the pulse could not always be timed as a Lady-in-Waiting told the doctors that 'taking out watches to count the pulse a little alarmed the Princess'. Babies were not weighed, even at birth, and antenatal examinations were of course not made, though trouble was given by a lady styled 'Purveyor of Wet-Nurses to the Royal Family'.

Quinine was the sovereign remedy, being exhibited for practically everything from orbital neuralgia to 'a strengthening medicine after sea-sickness'.* The only drugs used which had real effects, were opium

* Students of Lytton Strachey will remember that Mr. Gilbert Scott, the architect, found it necessary to recruit for two months at Scarborough 'with a course of quinine', after the rejection by Lord Palmerston, for the second time, of his plans for the new Foreign Office.

and aperients. One day, at Abergeldie, for example, the Duke of Cambridge consulted Dr. Sieveking for a severe attack of gouty colic—whatever that was. 'I recommended his keeping to brandy and water and to take some Gregory's Powders'. He also prescribed for Princess May, who was poorly after a journey, a Seidlitz Powder followed by a draught of Spiritus ammon. co. and tincture of orange. The same day, he treated a young man 'to whom I recommended marriage', and gave one of the men-servants a dose of calomel, finishing with a dose of calomel and rhubarb to the pet bitch of Teesdale, the Equerry.

What then remained, if medical diagnosis and treatment had hardly advanced since Harvey—or indeed Hippocrates—and yet the mumbo-jumbo which had upheld the physician's prestige had largely disappeared? The answer, I think, was the detailed management of the patient; and my impression is that the mid-nineteenth century was the golden age of patient-management.

For instance, after a normal if premature confinement the Princess was restricted to beef tea with arrowroot or vermicelli and marmalade water, 'she having asked for oranges which we objected to'. On the second day she was allowed 'a whiting (fried) besides beef tea and one glass of Hock'.

The minutiae of diet, clothing and habits were all important. The rules laid down for the Princess to prevent another premature birth, for instance, allowed quadrilles but not waltzing.

Today the pendulum may have swung too far in the other direction. May I quote from that remarkable man Sir Henry Holland, who died in 1873, having set up an all-time record by attending no less than six Prime Ministers of England, not to speak of two English Queens, a Prince who became the Emperor Napoleon III, as well as four leading poets—and did all this without really being interested in medicine, as his Memoirs make abundantly clear. Writing in 1872, he says: 'The prescription of the physician, however learned in its Latin and pharmacy, is but a slender part of his professional duty. Of far greater import generally to the patient is his watchfulness over the economy of the sick chamber—its temperature, ventilation, cleanliness, and quiet—the various appliances, even of changes of posture, fitted to relieve pain and procure rest—all too that the *lenis sermo* and *hilaris vultus* (I willingly quote from Celsus) can justifiably do in soothing and giving hope. A dry and technical rule of practice ill compensates for the absence of these simpler and happier ministrations'.

Now I suspect that Sieveking, who was 'peculiarly reserved' and appeared cold and unsympathetic on the surface, according to obituary notices, was lacking in these 'simpler and happier ministrations'. Not for him the 'kindly word' and the 'cheerful countenance' of Celsus. Nor can one imagine him writing, as Sir Henry Holland does, that 'he had seen more than one case defying medicine cured by a ticket for Almack's.' His coldness would not have appealed to the exuberant and extrovert Edward, nor his Teutonic rigidity and parenthood to the habitually unpunctual and passionately anti-German Alexandra. Had he possessed the worldly skill and insight of a Sir Henry Holland into the management of patients, he might perhaps have overcome his unfortunate handicap caused by the antagonism between the Queen and her son and daughter-in-law.

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Medical Mycology 1841–1870*

by

FRANCES M. KEDDIE

IN 1841 David Gruby presented his first report on medical mycology to the French Academy of Sciences. This 'Mémoire sur une végétation qui constitue la vraie teigne' contains these words: 'to recognize the true tinea one has only to put it under the microscope . . . For this purpose take a small bit of the crust spread out in a drop of water; put it between two slides of glass and examine it at a linear enlargement of 300 times. Quantities of round or oblong corpuscles and small branched filaments will be seen. The form of these filaments puts their vegetable character beyond doubt; they belong to the group of mycoderms according to Brongniart. As we have not yet found a molecule of true tinea that was not filled with a great number of these mycoderms, these constitute a true and essential character of this malady.'

David Gruby (1810–1898) had studied medicine in Vienna where he was graduated doctor of medicine and ophthalmology in 1839 and was demonstrator in microscopic anatomy. Later, after some months of travel in England, he settled in Paris where he established his own laboratory for studies and demonstrations in microscopic pathology and photography. In the wards of l'Hôpital des Enfants Trouvés he began to study the ringworm diseases of the skin and the thrush disease of mucous membranes. In 1842 Gruby described the yeast of thrush and the ectothrix fungus of *Trichophyton* ringworm of the beard (mentagra); in 1843, the small-spore fungus of ringworm of the scalp in children (porrigo decalvans of Willan) which he named *Microsporum audouini*, and the following year, the large-spore fungus of the scalp later named *Trichophyton tonsurans* (Malmsten 1845).

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In Gruby's opinion these five infections resulted from the growth of five specific vegetable organisms. Not only was each fungus in a specific anatomical location, i.e. in the crusts in favus, in the mucosa in thrush, in the sheath of the hair in mentagra, on the surface of the hair in porrigo decalvans, and in the hair bulb and shaft in tinea tonsurans; but also the fungi in each location differed in the size of their filaments and spores. Thus Gruby differentiated the ringworm diseases by their anatomical characteristics and their species of fungus.

The idea that specific symptoms and anatomical characteristics were diagnostic for skin diseases was established by Robert Willan and Thomas Bateman in illustrated fasciculi published between 1798 and 1817. In addition, Bateman, with Willan's co-operation, wrote a 'Synopsis' of diseases of the skin based on Willan's classification, and in the preface to the edition of illustrations (1817) Bateman stated that the 'Synopsis' had already been published in four large impressions, republished in America, and translated into French and German.

Not long thereafter (1835) Agostino Bassi found a fungus in the muscardine disease of the silkworm, and by demonstrating it to be the cause of the disease, proved for the first time that vegetable bodies were capable of involving and injuring living tissues. As Raymond Sabouraud wrote in 1910, 'many foresaw that the muscardine disease was not the only malady in the world caused by *une moisissure*, and on all sides, in Germany, Sweden and France, everyone looked for the parasitic fungi. Thus the mycotic nature of favus, *la vraie teigne*, was confirmed at almost the same time in three countries. In 1837, and I believe before anyone else, Remak observed the favi to be constituted of an aggregation of filaments of yeasts which sufficed to distinguish them from other crusts. But it did not occur to him that this disease was caused by the yeasts.' In 1839 Schönlein demonstrated the vegetable nature of the so-called 'dry pustules' of favus, the porrigo lupinosa of Willan. Nevertheless the modern era of the history of fungus diseases can be dated from the six reports of Gruby, 1841–1844.

Unfortunately for medical mycology the fungi observed were not readily distinguished from each other except by experienced microscopists such as Gruby. Their growth in human tissues consisted only of filaments of various sizes and collections of rounded spores but not of organs of fructification and conjugation by which botanists could classify them. Techniques designed to culture a single fungus on artificial media were inaugurated by Brefeld in 1868, but the fungi of human cutaneous diseases were not systematically studied in pure culture by such investigators as Sabouraud until the 1890s.

Gruby's reports soon led to the study of mycotic diseases of the skin at l'Hôpital St. Louis, particularly by Bazin (1858). In 1847 Charles Robin published *Les Végétaux qui croissent sur l'Homme et sur les Animaux vivants* in which he reviewed the work of Gruby; and in the second edition of 1858 Robin included an account of the recent discovery by Eichstedt (1846) of the fungus of tinea versicolor. Robin named this fungus *microsporon furfur* and remarked that it deserved special study inasmuch as the relationship of the two forms in the skin, the yeast cells and the filaments, had not hitherto been accurately described.

The laboratory procedures for observing fungi in crusts, scales and hair consisted of viewing the material at magnifications of 300 to 600 times after the animal tissue had been cleared away by means of soaking in ether and chloroform and treatment with a solution of potash. The vegetable structures remained unchanged and thus could be examined in some detail. Blister tops produced by the application of a vesicant such as cantharides were excised, sometimes stained with carmine, and examined whole (Gudden 1853).

Most physicians were thus able to find the fungi in the diseased skin and hair if they took sufficient pains with their preparations; but, perhaps for lack of experience with microscopy they often observed and considered the fungi to be much alike, so that their pertinent structures were not found. Of tinea versicolor William W. Gull at Guy's Hospital reported (1856); 'I have always found the sporules, but until recently failed to detect the ramifying branches of the mycelium. This failure I now know to have arisen from want of care in manipulation.'

In 1842 John Hughes Bennett, Professor of the Institutes of Medicine and Senior Professor of Clinical Medicine at the University of Edinburgh, affirmed Gruby's discovery of a fungus in favus and reported favus in the skin of the mouse. Bennett noted that hairs infected by favus contained channels of air. He wrote (1858) that it astonished him that the vegetable nature of these structures could be doubted for a moment by anyone who had personally examined them, especially under magnifications of 600 to 800 diameters. Nevertheless Bennett considered the fungous infections to be 'essentially a form of abnormal nutrition with exudation of matter analagous to, if not identical with, that of tubercle, which constitutes a soil for germination of the cryptogamic plants, the presence of which is pathognomonic of the disease. The growth of the parasitic fungus on the surface of the skin has now been observed under a variety of circumstances, and constitutes in man, three forms of skin diseases, which I believe to be essentially the same, viz.—tenia [*sic*] favosa, a certain form of pityriasis of the scalp, and of mentagra. The latter is very rare in this country, and I have never seen a case of it. All these disorders, however, may be classified under the head of favus, under which I shall consider them.' Although he failed to produce favus by inoculation of his patients and himself in 1842, he did succeed in 1845.

The idea that the fungi of different diseases were of one species was based partly on the fact that common moulds grew in the media in which hair and crusts were put for culture. John Lowe, a botanist of Edinburgh, reported 'On the identity of *Achorion schonleinii* and other vegetable parasites with *Aspergillus glaucus*' (1858) and wrote as follows: 'One of the chief reasons of their having been so long considered as distinct species, appears to be that imperfect and various stages of their development have been observed, and thus a merely initial stage of the mycelium, which is capable of assuming an almost endless change of form, has in each instance, been figured as a distinct species, provided the seat and forms of the diseases differed; the observers apparently ignoring the fact, in order to define a species accurately, all parts should be present, or at any rate, those which are most important and above all, the fructification.' Lowe planted material from a crust of favus in (1) a solution of coarse brown sugar, (2) a piece of cheese, and (3) pure glycerine on 12 February 1857. On 20 March there was fructification which he recognized as *Aspergillus glaucus* in the cells in the saccharine solution. He observed 'I shall therefore merely attempt to show, in the succeeding remarks, that there is a probability of numerous forms now ranked as distinct species, being nothing more than mere variations of one or other species belonging to the two genera [*Penicillium* and *Aspergillus*] and that the same characters are common to each—and there are no legitimate grounds for their being considered as specifically, much less generically, distinct.' He thought the fungus of thrush was *P. glaucus*, that of tinea tonsurans the sporular form of *Achorion schoenleinii*, pityriasis to be an imperfect *Penicillium* or *Aspergillus*, *Microsporon audouini* of porrigo decalvans to be sporules of favus, and that *Microsporon mentagrophytes* (Ch. Robin), the fungus of mentagra, differed from the others in having large spores and filaments which were situated in the hair follicle, between the hair and the follicular walls, and not in the substance of the hair as in *Trichophyton tonsurans* nor around the aerial part of the hair as the last name variety [*M. audouini*]. This difference in situation accounted for the very slight degree of variation between these so-called species.

In 1857 Friedrich Küchenmeister's *Animal and Vegetable Parasites of the Human Body* was translated into English. His descriptions of the fungous parasites were derived from David Gruby, Charles Robin, and other physicians of the time, together with his own observations, and constituted in effect a summary of current factual knowledge. Küchenmeister appears to have accepted the fungi as separate and distinct species.

In 1859 William Tilbury Fox, of London, published a series of articles in the *Lancet* and in 1863 a separate volume on the fungous diseases of man, in which he developed the thesis that there was but one essential fungus which attacked the human surface, and that the varieties of the diseases were owing to the stage of development of the fungus and the conditions of its growth. In part Fox was refuting Jabez Hogg's concept that the fungi were accidental (1859, 1860, 1866) and in part elaborating on the research of Lowe who regarded the fungi as one or another species of *Aspergillus* or *Penicillium*—although Fox wrote that he had arrived at his own conclusions before Lowe's work was known to him. Fox had additional support from the increasing knowledge of the fungous diseases of plants, for it had been recently established that the same fungus might appear in several forms, not only as regards the vegetative but also the fructifying organs, as in the fungi which caused disease in cereals.

Fox explained the fluctuations of the parasite on the skin in terms of fluctuations of the fungus and the soil in which it grew. Thus the plant barely able to live and do damage resulted in tinea (porrigo decalvans); the fungus in a more favourable soil produced tinea tonsurans, chloasma, and tinea circinatus; in a higher degree of development and in an even more favourable soil, tinea tonsurans and sycosis; and in the best possible soil the fungus flourished luxuriantly and produced favus.

Fox had also to explain the fact that one variety of disease, by inoculation and contagion, gave rise to the same variety of disease. To do this he assumed that the stability of the species was provided by fissiparous division which would reproduce any existing peculiarities of the organism, whereas if conjugations of different parents took place, the original or some modified phase of it might result.

Fox's book of 1863 included descriptions of some thirteen different types of fungous infections and named the species of fungus connected with each disease. These descriptions, as Fox wrote, were largely condensed from the English translation of Küchenmeister's book that had appeared in 1857.

The next year, 1864, Erasmus Wilson reiterated his own rather different views on the nature of fungous infections: 'more than 20 years since, after a careful microscopic investigation of the pathological elements we came to the conclusion that they resulted from an aberration of cell-formation. We called this morbid alteration of the primary granules of the epithelium or hair-tissues "granular degeneration" and subsequent experience has not altered our opinion . . . According to our view, favous matter and the mucedinales of the phytodermata are organic matter arrested in development at the lowest degree of life, the function of reproduction; the sporules are growing organic substance, aborted epidermic granules; the filamentary portion fully-formed organic substance, beyond which there is no further growth, the highest and perfected form of development.' Fox (1864) criticized Wilson's views in the following words: 'the data which form the basis upon which the vegetable nature of parasites rests, are not refuted—not examined. The whole thing has been examined by an anatomical eye and anatomical ideas, without any reference to the botanical view of the case.' For support of the botanical view he claimed that the cellulose of the fungus stained with iodine whereas animal tissue did not stain. Furthermore, ether, chloroform, and spirits of wine rendered epithelial tissue transparent and dissolved the fatty substances, while the vegetable parasite remained undamaged. Jabez Hogg, an expert microscopist, remarked (1860) that if Wilson had submitted 'those specimens which he had at his disposal' to a more complete examination, by employing the microscope with polarized light, he would have acknowledged his error. Hogg, on the other hand, was apt at finding fungal growth in such diseases as psoriasis, eczema, and ichthyosis, and he wrote that such was the similarity and form of the fungi that he failed to make out any identity between the parasite and the disease (1859). For him (1866) as for Fox there existed but one essential fungus.

There were some, however, who from careful observation of the fungous diseases and the fungi found in them continued to support the view of Gruby and Bazin. In 1866 M'Call Anderson produced a work entitled *On the non-identity of the parasites met with in favus, tinea tonsurans, and pityriasis versicolor, including proofs derived from the occurrence of these diseases amongst the lower animals, and their transmission from them to man*. Anderson was lecturer on the practice of medicine in Anderson's University and physician to the Dispensary for Skin Diseases, Glasgow. After reviewing the variety of opinion that prevailed among the scientific men as regards the many points relating to the so-called vegetable parasitic infections of the skin, he concluded from the experiences of others and from his own observations on 1300 cases of fungus infections observed at the Dispensary, that *Achorion*, *Trichophyton*, and *M. furfur* were considerably different and that fungus infections were indeed specific. John S. Bristow, of St. Thomas's Hospital, London, was of the same opinion (1870).

In Germany Heinrich Köbner of Breslau tried (1864) without success to cultivate the fungus of tinea versicolor but did have success in transmitting it to his own skin by inoculation. Köbner showed experimentally that favus had a herpetic first state that could easily be confused with tinea tonsurans. He recommended white rabbits as recipients of inoculation with favus since their white skins made observations easier.

In France Maurice Chausit noted (1863) that the influence of micrographic research of the study of diseases of the skin offered to become nothing less than the study of parasitism. Chausit believed that the parasites observed, 'inasmuch as they were really there' could exist accidentally and they could not be considered as the essential cause of any form of disease of the skin. Chausit declared that his botanical consultants could see no vegetable growths in the tissues and that stains for cellulose failed to indicate any vegetable matter therein.

Alphée Cazenave, Professor of the Faculty of Medicine of Paris, admitted (1850) the contagion of the so-called fungous infections but denied the presence of fungi since he could not find them under the microscope. Marie-G.-A. Devergie thought (1854), as did Erasmus Wilson, that the fungi were the expression of the already morbid state of the skin.

Nevertheless treatment of tinea was essentially the local application of fungicidal medications containing sulphur, copper or mercury. In France the brothers Mahon had a kind of monopoly in treatment which they exercised by means of their secret medications. From 1807 they had had charge of treatment of the tineas of the scalp at the infirmary of l'Hôpital St. Louis, as well as the tinea patients of other towns in France. In 1854 Ernest Bazin requested permission to treat the tineas at St. Louis on a more rational basis founded upon the recent discoveries of Gruby, and in 1865 a similar request to put the treatment of tineas in the hands of physicians rather than to leave it to the care of the itinerant practitioners was presented to the governing body of the hospital in Lille.

An early experimental approach towards evaluation of the various medications for the tineas was reported by Devergie (1854), a project in which infected prisoners were used as subjects. Sixteen children, for some years in various prisons, had been treated by various medications without success. These children were divided into four categories and for each of the groups a different treatment was applied. Three of the four patients treated with a pomade of carbonate of copper were cured. The other three types of treatment proved to be too irritating and none of them was successful.

The theses of the medical students in France generally reflected the ideas of the masters under whom they worked. Eight were presented dealing with fungous diseases between 1855 and 1866. J.-A.-L. Giraud dedicated his thesis *Du pityriasis* (1855) to Devergie, and consequently considered the fungi to be accidental. Emile Diéder dedicated his, *Des principaux parasites de l'homme* (1853) to Ch. Robin, and thus reflected Robin's book of the same year. Antoine Mutin, *Du pityriasis* (1860) drew largely from the works of Bazin; R.-J.-U. Sempé (1862) attended the clinics of Bazin and although citing the opinions of Jabez Hogg, Cazenave and Devergie, followed mycology as taught by Bazin. J.-Ernest Gratiot (1862) discussed only *Microsporon furfur*, as did Oscar Wencélius (1863) and Edouard Audiguier (1866). In his thesis *Considerations sur quelques maladies de la peau a parasites végétaux observée chez l'homme et les animaux domestiques* (1865) M.-F.-Alphonse Sempé described the transmission of ringworm from a young bull terrier to cats, and to a small girl and the girl's father. He also cited the thesis of Malherbe of Nantes, *Études cliniques sur l'herpès tonsurans*, in which Malherbe stated that ringworm was common in bovines and was communicated to man, and that it was astonishing that this point had not been remarked by dermatologists.

Fungous diseases of domesticated animals were reported in the United States by J. H. Salisbury, professor of histology, physiology, and cell pathology in Charity Hospital Medical College, Cleveland, Ohio, who declared that fungous infections in kittens and young dogs were readily transmitted to children who played with them. Although the diseases in the animals were much alike, Salisbury designated them by two distinct names, *trichosis felinis*, in which the fungus was both sporulating and filamentous, and *trichosis caninis*, in which the fungus appeared more luxuriant, large, and confined to the filamentous stage of development. In his opinion these diseases could have been produced by the same specific cause.

Salisbury first noticed trichosis (ringworm) in the summer of 1864 while treating it in an orphan asylum where some thirty small boys were affected. In July and August of 1866 he commenced studying the ringworm with a view to tracing its source. Having noticed that where it prevailed the children were playing with kittens that had diseased faces, he compared the fungous growth in the kittens and the children and found that they were apparently identical in the shape of the spores and the arrangement of the epidermic cells. He then distributed diseased kittens to families where there were no cats and the children healthy. In every instance, in from five to ten days after the children began playing with the diseased kittens, they began breaking out with ringworm. Salisbury then inoculated himself with spores of the fungus from the cat and with spores of the patches of eruption on one of the children, and at each area of inoculation the fungus formed filaments and produced circular patches of eruption.

With the exception of Madura foot, mentioned by Küchenmeister and further described by Vandyke Carter (1874) as the fungus-foot of India and as mycetoma, the deep mycotic infections were not yet a subject of study.

But fungi also affected man and animals in other ways. J. H. Salisbury (1862) developed catarrhal symptoms and a measly eruption of his skin after handling mouldy straw. Having discovered the mould in the straw, he inoculated himself and his wife with a suspension of the spores of the mould and developed symptoms much like those of measles. Charles Darwin (1868) recorded cases proving that in animals and plants differences in colour were correlated with constitutional differences, as shown by greater or less immunity from certain diseases, from the attacks of parasitic plants and animals, from burning by the sun, and from the action of certain poisons. Photosensitivity was recorded in reports of white and white-spotted horses being injured by eating mildewed and honeydewed vetches; every spot of skin bearing white hairs becoming inflamed and gangrenous.

Willan and Bateman's methodical classification of skin diseases (which contained the contagious ringworm diseases of the skin) was quickly put to use by practising dermatologists of the early nineteenth century in England, France, and Germany and was likely familiar to Gruby when he succeeded in showing the fungi in four of the contagious diseases of the scalp. In effect the skin was a 'solid' medium for almost pure culture, and from these cultures animals and man were experimentally infected by inoculation and contagion. Much of this was accomplished before the 1860s, and before the pathogenesis of bacteria had yet been experimentally established.

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Occupational Medicine

by

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INTRODUCTION

OCCUPATIONAL medicine is concerned, amongst other things, with the influence of working conditions on the health of workpeople and with the measures taken to regulate these conditions, frequently by legislation. The evolution of these measures from the emergence of a problem, through a period of investigation and discussion, until legislative action is taken generally takes many years. Three examples may illustrate this process.

Flinn,[1] examining the roots of the idea which lead to Chadwick's classic *Report on the Sanitary Condition of the Labouring Population*, went back to the end of the eighteenth century. That Report resulted in the Public Health Act of 1848. The time span, therefore, from the emergence of the problem to legislation was about forty or fifty years. The pressures of agitation for improvement of conditions in coal mines at the beginning of the nineteenth century[2] resulted in the first Coal Mines Act[3] of 1842, a time span of about forty years. In the present century, Wignall[4] first reported on cases of bladder cancer from a defined population of chemical workers in England* at the British Medical Association meeting in Manchester in 1929. Regulations designed to control the occurrence of bladder cancer in the chemical industry[5] have just been issued, again, a time span of about forty years. It is not claimed that this is always the time span, sometimes it is longer, frequently shorter. The point is, that this process from the emergence of a problem to legislative control generally takes several decades.

* The condition had been described abroad as occurring among certain chemical workers at the end of the nineteenth century.

If, then, occupational medicine is studied over a single decade, the 1860s, we shall find, not so much a stage in the evolution of a discipline as a series of processes in different stages of development in diverse fields: industry, mining, agriculture and factories and medical politics.

MINING

The decade started with new Coal Mines Act[6] of 1860. From the viewpoint of occupational medicine this was a rather unexciting piece of legislation. It prohibited the employment of boys under twelve in coal mines unless they produced a certificate of education.[7] In passing, it is worth noting this, as an example of Victorian legislation on the employment of children, being directed as much at ensuring their education as with the protection of their health. The minimum age for steam enginemen, who generally operated the colliery winding gear, was raised to eighteen years.[8] In addition, there were a number of other regulations about safety. Although inspectors could 'enter, inspect and examine any colliery at all reasonable times of day or night'[9] they rarely did so for in 1865 there were twelve inspectors responsible for about 3217 coal mines.[10]

Apart from the good which stemmed immediately from this Act there were two further developments based upon it. First, was the appointment of a commission to enquire into the health and safety of persons employed in those mines not covered by the Act. This meant, in effect, the workers in metalliferous mines. Second, under the leadership of Alexander McDonald, the leader of the National Miners Union, the miners agitated by protest meetings and petitions for more effective protection[11] and they succeeded in getting the Government to set up an inquiry into the operation of the Coal Mines Act.

The report on the health and safety of persons employed in metalliferous mines published in 1864, is of considerable interest for it dealt, in detail, with many topics including the state of the health and the diseases of miners, ventilation in mines, provision in case of sickness or accident, medical services and accidents.[12] The Commissioners devoted most of their report to the mines in the West Country and to the lead mines in the north of England but they also looked at the lead and gold mines in Wales and Shropshire and the ironstone mines of Whitehaven and Furness.

The vital statistics on the mortality of miners were prepared by William Farr and show the fruitful results of the inclusion of particulars of occupation in the census of 1851.[13] 'It gave us [said Farr] an opportunity for the first time of determining the influence of occupations on sound principles, that it, by a comparison of the living at each age and the deaths at each age'.

He demonstrated first the excess of deaths, from all causes, among Cornish metal miners compared with other males of similar age from the same area (Table I). These figures were calculated from the 1861 census returns. Comparison with figures calculated from the 1851 census (Table II), showed that this excess of mortality had been present at that time. To demonstrate that this excess mortality was not due merely to working underground, in conditions of dark, damp, bad ventilation etc., Farr compared the mortality, again at different ages, with another group of underground workers, northern coal miners (Table III). Farr went on to show that the high death rate of Cornish miners compared with other males in the area appeared to be due, in a large part, to pulmonary diseases (Table IV). In a similar way he showed that the death rates among lead miners in the north of England (Table V) and lead miners in North Wales (Table VI) were also increased but to a lesser extent. Again the increase appeared to be due to pulmonary diseases (Tables VII and VIII).

Miners, at the present day, have a high mortality and it is interesting to compare these figures of Farr's with figures calculated in a similar manner from the 1951 census. As the numbers employed in metalliferous mining have fallen considerably over the past hundred years it has been necessary to use the figures for coal mine workers[14] (Tables IX and X).

The Commission on Metalliferous Mines was disappointed at the lack of clinical and pathological knowledge about the 'pulmonary diseases' affecting the miners. They regretted that doctors connected with the mines had not seriously investigated the causes of miners' asthma:[15]

TABLE I

Average Annual Deaths per 1,000 1860–62
All Causes—Cornwall

Age (years)	*Males excl miners*	*Metal Miners*	*Ratio*
15–25	7.50	9.44	126
25–35	8.32	9.57	115
35–45	10.08	15.12	150
45–55	12.50	29.74	238
55–65	19.96	63.21	317
65–75	53.31	110.51	217

TABLE II

Average Annual Deaths per 1,000 1849–53
All Causes—Cornwall

Age (years)	*Males excl miners*	*Metal Miners*	*Ratio*
15–25	7.12	8.90	125
25–35	8.84	8.96	101
35–45	9.99	14.30	143
45–55	14.76	33.51	227
55–65	24.12	63.17	263
65–75	58.61	111.23	189

TABLE III

Average Annual Deaths per 1,000 1849–53
All Causes—Cornish and Northern

Age (years)	*Northern Coal Miners*	*Cornish Metal Miners*	*Ratio*
15–25	8.50	8.90	105
25–35	8.49	8.96	106
35–45	10.13	14.30	141
45–55	16.81	33.51	199
55–65	24.43	63.17	258
65–75	65.16	111.23	171

TABLE IV

Average Annual Deaths per 1,000 1860–62
Pulmonary Diseases—Cornwall

Age (years)	*Males excl. miners*	*Metal Miners*	*Ratio*
15–25	3.30	3.77	114
25–35	3.83	4.15	108
35–45	4.24	7.89	186
45–55	4.34	19.75	455
55–65	5.19	43.29	834
65–75	10.48	45.04	430

TABLE V

Average Annual Deaths per 1,000 (in the North of England) 1860–62
All Causes

Age (years)	*Males excl miners*	*Metal Miners*	*Ratio*
15–25	7.57	9.53	126
25–35	9.19	12.38	135
35–45	10.13	17.64	174
45–55	16.18	33.11	205
55–65	29.38	78.34	267
65–75	66.10	127.52	193

TABLE VI

Average Annual Deaths per 1,000 1860–62
All Causes—North Wales

Age (years)	*Males excl Lead Miners*	*Lead Miners*	*Ratio*
15–25	7.46	6.04	81
25–35	10.52	15.72	149
35–45	12.57	18.05	144
45–55	15.19	25.74	169
55–65	28.11	55.19	196
65–75	75.78	86.96	115

TABLE VII

Average Annual Deaths per 1,000 (in the North of England) 1860–62
Pulmonary Diseases

Age (years)	Males excl miners	Metal Miners	Ratio
15–25	3.97	3.40	88
25–35	5.15	6.40	124
35–45	3.52	11.76	334
45–55	5.21	23.18	445
55–65	7.22	41.47	574
65–75	17.44	53.69	308

TABLE VIII

Average Annual Deaths per 1,000 1860–62
Pulmonary Diseases—North Wales

Age (years)	Males excl Lead Miners	Lead Miners	Ratio
15–25	3.39	3.02	89
25–35	5.79	4.19	72
35–45	5.41	10.62	196
45–55	7.06	14.71	208
55–65	12.21	35.32	289
65–75	16.96	48.31	285

TABLE IX

Average Annual Deaths per 1,000 1949–53
All Causes—England and Wales

Age (years)	*Males excl Miners*	*Coal Mine Workers*	*Ratio*
16–24	1.23	1.47	120
25–34	1.58	1.86	117
35–44	2.85	3.45	121
45–54	8.18	9.12	116
55–64	22.82	26.41	116
65–74	54.12	65.27	121

TABLE X

Average Annual Deaths per 1,000 1949–53
Pulmonary Diseases—England and Wales[14]

Age (years)	*Males excl Miners*	*Coal Mine Workers*	*Ratio*
16–24	0.18	0.20	108
25–34	0.39	0.36	91
35–44	0.71	0.75	105
45–54	2.24	2.71	121
55–64	5.78	8.51	147
65–74	9.79	14.45	148

> From want of sufficient investigation hasty and in many instances erroneous returns of the cause of death have been made to the district registrars. From the disease peculiar to miners being commonly registered as consumption, it has been by many supposed to be identical with phthisis or tubercular pulmonary consumption; but it is proven by the evidence of the medical witnesses that though some miners, and especially those whose families are predisposed to the affection, do die of consumption, by far the largest amount of mortality is due to other forms of lung disease of a bronchitic or asthmatic character.

Evidence collected from local medical practitioners showed that the pulmonary disease from which many of the miners died, although commonly registered as consumption, was clinically different from phthisis and was of a bronchitic character, known locally as miners' asthma. The clinical descriptions varied. Mr. John Sparman, Medical Officer to the Union ('the Union' being a group of parishes) mentioned a disease which was clearly lobar pneumonia:[16]

> In the early stages how does a man suffer? He complains of a tight pain in the chest and a slight cough, and it runs through his form; a rusty expectoration sets in, which in a few days changes to white, and they recover in two or three weeks.
>
> Have they a cold?—They complain of cold but I am aware that it is pneumonia.

This contrasts with a description of a disease of slower onset described by Dr. N. J. Haydon and which is more suggestive of pneumoconiosis.[17]

> How does it [miners' asthma] begin? With slight difficulty of breathing and cough depending on bronchial irritation; the amount of difficulty of breathing and cough necessarily differs in different individuals; in some cases the cough is nearly always present, dry and harsh.
>
> How does it proceed and what are the sounds on percussion and auscultation? The circulation becomes affected and you have increased frequency of the pulse with more or less palpitation of the heart; a very harassing cough. You have a loud respiratory sound, but a dullness on percussion; this after a time is followed by a state of extreme exhaustion; the sufferer appears to waste away; his pulse becomes much slower and in this state of sinking, or almost living death, he may and often does go on for several years.

This history is suggestive of pneumoconiosis, which was sometimes complicated by phthisis. More recently, in 1946, a description of pneumoconiosis amongst Cornish tin miners[18] also describes cases with slow onset and some with acute onset.

At the west country mines Dr. Peacock, a consultant physician at St. Thomas's Hospital, and Mr. Bankart, the medical assessors to the Commission, examined a large number of miners and found that the prevalent chest diseases were 'asthmatic affections resulting from bronchitis, pneumonia and diseases of the heart and more *rarely* tubercular consumption'. The symptoms of this miners' asthma were shortness of breath, weakness, anorexia, productive cough and sometimes haemoptysis.[19]

Among the lead miners of Yorkshire and the northern counties Dr. Peacock found that by far the largest number of cases of disease were cases of miners' asthma.[20]

An important contrast was made between the Cornish miners with a pale sallow appearance[21] and the miners from the north of England who were far more robust and healthy.[22] Describing the Cornish miners, Dr. Peacock reported,[23]

> I have already alluded to the delicate state of many of the miners and to the frequency with which they complained of dyspeptic symptoms and of rheumatic pains. Generally, however, these affections are not so severe as to prevent their working, but the men are constantly ailing, and on the occurrence of some slight exciting cause, a trivial accident or an ordinary cold, have attacks of severe illness and are laid by for a long time before they are capable of resuming their work underground.

He considered that the north-country miners had better health because they worked shorter hours and did not have the extensive ladder climbing which occurred in Cornish mines.[24] Due to the geological configuration of the lead strata, the lead mines in the north country were adit mines driven straight into the hills from the valleys[25] whereas the Cornish miners sometimes had to climb about 1700 feet to reach the surface after finishing work.[26]

Was the inferior health of the Cornish miners due to the longer hours worked (and, as would nowadays be thought, the consequent larger dose of dust)? Certainly it would be hard to accept Dr. Peacock's other suggestion that extensive ladder climbing was the cause. It is perhaps worth considering whether these men suffered also from some chronic underlying debilitating disease. As we have seen the medical assessors had remarked on the pale sallow appearance of these miners. Dr. John Iago, physician at the infirmary at Truro, giving evidence to the Commission, stated,[27]

> Looking at the anaemic and pasty colour of the miner, which is very peculiar and the other facts as to his breathing and I have seen them come to me excessively weak, when I have been unable to detect any special cause to account for their want of breath and general weakness excepting the mere absence of the red colour of their features . . .

This was before the days of routine measurement of haemoglobin. In 1903 Boycott and Haldane[28] found the average haemoglobin of 57 miners at Dolcoath mine in Cornwall was 43.1 per cent. The lowest was 17 per cent. They produced clear evidence that this severe anaemia was due to an infestation of the mine with ankylostoma. It may be relevant that in his report to the 1864 Commission Peacock remarked:[29]

> In some cases also the dyspeptic symptoms are very severe. The miner has little appetite; the food which he eats causes flatulency, pain or sickness and vomiting. His bowels are in an uncertain state, often absolutely confined, in other cases relaxed, or passing from one condition to the other. Symptoms of hepatic disorder are not uncommon. Several of the men stated that their illnesses of different kinds commenced with attacks of jaundice, and the ailing men have frequently a more or less deep bilious tinge of the eyes and surface generally. They suffer also from piles and pass blood by stool, sometimes to such an extent as materially to affect their strength.

This clinical description fits well with ankylostomiasis.[30] However, there are a number of points to be considered before presuming that the debility noticed among the Cornish miners in 1864 was due to anaemia from ankylostomiasis. First, Boycott and Haldane stated that the outbreak they described was only of relatively recent origin.

> The outbreak at Dolcoath appears to have begun about eight years ago. Some of the affected men sought treatment at the West Cornwall Miners' Hospital at Redruth, about three miles away; and the following record of cases admitted for anaemia since 1893 affords some idea of the course of the outbreak (Table XI). Of the cases recorded in the table 61 per cent were of miners directly from Dolcoath. Only one death occurred.

Second, for completion of the life cycle of the ankylostoma worm, warm damp conditions of the soil are required. Boycott and Haldane[31] found that the temperature at the bottom level of Dolcoath mine was about 79°F. The temperature measurements for the same mine reported by the 1864 Commission[32] were 76°F to 84°F. However, although the necessary temperature conditions for an outbreak of

TABLE XI

Anaemia

West Cornwall Miners' Hospital—Redruth

Year	*Cases Admitted*
1893	1
1894	3
1895	9
1896	13
1897	29
1898	23
1899	12
1900	11
1901	7
1902 (till December)	8

ankylostoma infestation were present at Dolcoath in 1864 this does not mean that there was infestation at that time. Furthermore, it would have to be shown, if the disease were to be general throughout the Cornish mines, that similar temperature conditions obtained throughout the majority of them. Third, in their clinical description of the cases, Boycott and Haldane remarked[33] that the miners complained a great deal of certain skin affections. These affections consisted of furuncles and urticaria. No mention is made of such skin affections in the clinical descriptions of the Cornish miners given to the 1864 Commission.

Therefore the suggestion that the health of the Cornish miners was worse than their northern counterparts, because of a chronic underlying anaemia due to ankylostomiasis, must be regarded as 'not proven'. It is unlikely that lead was the cause of the anaemia for two reasons. First, the Commission contrasted the pale appearance of the Cornish metal miners with the robust and healthy appearance of the Northern and Welsh metal miners, yet it was these latter and not the Cornish metal miners who were exposed to lead ore. Second, the anaemia of lead poisoning is seldom severe, the haemoglobin level rarely falling below 60 per cent.[34]

The question still remains, what was the cause of miners' asthma? Or perhaps it is more pertinent to ask, what was considered, at that time, to be the cause of miners' asthma? The conclusions of the Commission were that the great excess of sickness and mortality amongst the metalliferous miners was mainly attributable to the imperfect ventilation of the mines[35] [*sic*]. They pointed out that mortality (accidents excepted) of coal miners was considerably less than of metalliferous miners and they attributed this to 'the great attention which has been given to the ventilation of [coal] mines . . . on account of the dangerous gases'. They continued, 'the main object to be kept in view in ventilating a mine is to conduct a sufficient supply of pure air through the mine in order to displace the vitiated air where the men are at work'. The Commission considered several other possible causes, both local and general, which they thought largely contributed to impair the health of the miner;[36] namely, exposure to cold and wet, and to sudden alterations of temperature; wearing wet clothes; inhalation of gritty particles; and the exertion of climbing ladders from great depths. The subsequent discussion of these possible causes dwelt at length on sudden alteration of temperature and on the exertion of climbing ladders. But the inhalation of 'gritty particles' received no further mention.

It is difficult to understand the reason why dust was ignored as a cause of miners' asthma, because Dr. Greenhow, who was a member of this Commission had himself, as a member of Sir John Simon's team, investigated many industries with a dust hazard and had even discussed control of the hazard.[37]

Defective ventilation was clearly considered the villain of the piece. From the end of the eighteenth century, the findings of Lavoisier, who had studied the composition of air, of expired air and of occupied rooms, had dominated thought in this field. He did not hesitate to attribute to carbonic acid the malaise often experienced in crowded assembly halls, the malaise generally attributed to warmth alone.[38] For the next hundred years excess of carbon dioxide was popularly held to account for injurious physiological effects, although during the latter part of the nineteenth century, perhaps following the opinions of Pettenkofer,[39] the theory was modified to the extent that carbon dioxide was not important of itself, but only as an index of the amount of those other noxious substances.

The changing concepts of the importance of inspired gas show in the evidence to the Commission of Dr. Alfred Taylor, Professor of Chemistry at Guy's Hospital. He had found that the oxygen content of the air in the mines fell to 18 per cent in some places but thought that oxygen deficiency itself was not deleterious, unless the oxygen content fell to 17 per cent or below.[40]

Eventually in 1905, Haldane and Priestley[41] showed that the oxygen content of inspired air could be reduced to 13 per cent without sensation or change in depth or frequency of respiration and that the carbon dioxide content of the inspired air may be raised up to 3 per cent, but the concentration in the alveolar air remains virtually the same as when normal air is breathed and hence the CO_2 tension in the blood was maintained constant. Probably the last surviving relic of this theory is the injunction of the first-aider to the gathering crowd to 'stand back and give him air'. It would be an interesting study to trace the influence of this false theory during and after its reign of a hundred years.

One last point on ventilation illustrates how facts, which to us are clearly explained by dust, were used in support of the false theory. It is now well recognized that coal-face workers are liable to coal workers' pneumoconiosis from exposure to coal dust. Men who work building the roadways which follow up behind the advancing coal face frequently have to rip down other strata and if these contain silica the 'rippers' become liable to silicosis, a disease quite different clinically and pathologically from coal workers' pneumoconiosis. This distinction had already been observed as early as 1833, although Dewar the Scottish physician who made it[42] attributed the higher mortality of the 'stoneworkers' in coal mines to the smoke and imperfect ventilation. The 1864 Commissioners noted that in the west country mines there were two groups of underground workers; the 'tutworkmen' who drove in the levels and cut their way through rock and the 'tributers' who got out the ore.[43]

'. . . it is obvious that the occupation of the tutworkman is more unhealthy, as he is more frequently employed where the air is noxious. He has to cut new ways through the rock and he often labours at such a distance from any shaft or winze* that the air coming down these channels does not reach him'. It is a pity that comparative statistics between the two groups were not presented.

Provision for Accident or Illness. This was similar throughout the country but with slight regional differences. In the west country,[44] where mining accidents were more numerous than elsewhere the men paid about 6*d.* to 9*d.* per head per week into a club and received benefit for 'visible hurts'.

> The fund thus contributed is considered to be the property not of the men but of the adventurers (i.e. the speculators in the mine), and a separate account is not always kept. Should the mine be 'knocked' or abandoned, the fund is added to the assets and divided among the adventurers; the money thus apportioned has in some instances amounted to a considerable sum. This appropriation is made on the ground that any deficiency in the club money would be supplied from the mine account, and that a just apportionment amongst the contributors would be impossible. The want of some means of supplying the men, when off work in consequence of sickness, with suitable food and wine is severely felt, and much complained of by the medical men.

Besides the 'visible hurt' club, the miner could also subscribe the same amount to a fund for providing medical attendance for themselves and in some cases for their families.[45]

> In some instances the miners choose their own doctors, but generally they are appointed by the adventurers or shareholders, and an impression prevails among the men that other reasons than those of professional qualifications may influence the selection . . . The system often gives great dissatisfaction, and though some of the witnesses consider the election of a doctor by the men to be objectionable, there does not appear to be any sufficient reason why the miners should not be at liberty to apply for medical aid to any qualified practitioner residing in the district who may be willing to attend them . . .

In neighbouring Devonshire the selection of the mine doctor was in some cases left to the men and the scheme was considered to work well.[46] In the north country the 'visible hurt' clubs were not so general as in the south. The system of medical attendance varied

* Vertical shaft extending downwards.

considerably in different districts.[47] In several instances the miners elected the doctor; in others each miner selected his own medical attendant who was then paid out of the club money; while in some, medical attendance was provided by the proprietors without any charge to the men.

The direction of evolution of these medical schemes is shown in a comment about mines in Wales and Shropshire,[48] where the men subscribed as in other mines, but, with very few exceptions, chose their own medical attendants, the system of selection by the 'adventurers' having been abandoned.

None of these medical services was what we would call today an occupational health service, they provided general medical care for the man and sometimes for his family through the occupational group as frequently happens today in eastern Europe and in the developing countries. This form of general medical care centred on the place of work appeared early in the industrial revolution, for example at Styal Mill near Manchester[49] and has now virtually disappeared. This contrasts with the system of care based on the neighbourhood area, which started with the Poor Law medical service and has evolved into the National Health Service. However, the element of personal contribution which was, of course, absent

TABLE XII

Returns of Friendly Societies

For Miners and Colliers 1846–1850

Age	*Mortality per cent*	*Days sickness absence per year to 100 living*
15–25	0.934	954
25–35	0.991	981
35–45	1.123	1224
45–55	1.689	1946
55–65	4.016	2697
65–75	5.792	4940

from the Poor Law system has been inherited from these early industrial sick clubs and from some of the friendly societies, based on the neighbourhood area.

In the evidence presented to the Commission on these industrial friendly societies there is one curious table which must be one of the earliest records of sickness absence statistics amongst an occupational group.[50] It is said to refer to 'returns of friendly societies for this class in the 5 years ending 31 December 1850, the last authentic returns on a large scale' (Table XII). As metalliferous mining has become a much smaller industry in the ensuing century, it is not possible to compare these figures with the sickness absence figures for a similar group at the present time. Table XIII is drawn up in a

TABLE XIII

Days of Incapacity for Work per 100 Men[51]
5 June 1961—2 June 1962

	GREAT BRITAIN		*S.W. REGION*	
Age (years)	*All Occupations*	*Coal Miners*	*All Occupations*	*Coal Miners**
ALL	885	2028	792	1290
Up to 25	552	999	562	
25–34	584	1631	528	
35–44	711	1732	605	
45–54	984	2313	837	
55–63	1769	3386	1554	

* only 92 in sample

similar fashion[51] and shows the sickness absence figures for coal miners in the early part of the 1960s.

This report marks a step forward in industrial medicine. Hitherto the Factories Acts and the Mines Acts had been largely concerned with protecting the very young from hard work and making some provision for their education; those Acts had also limited hours of employment. This Commission takes us forward into a further stage in the development of occupational medicine. There was an epidemiological investigation of mortality and morbidity, a careful investigation of the working environment and a clinical study of the workpeople. These remain today the cornerstones of any investigation of an occupational hazard.

AGRICULTURE

Towards the end of the eighteenth century the rapid increase in population, together with the concentration of industry into factories and urban districts, added impetus to changes which were already occurring in rural life. The enlargement of the corn supply became one of the first national necessities and the continuation, in the best corn-growing area of the Midlands and East Anglia, of the older system of open field cultivation could no longer be accepted. Enclosure was applied to these areas and the 'improving landlords' practising scientific agriculture became a feature of those parts. The landscape in the Midlands and East Anglia took on the appearance of a chequered pattern of fields 'enclosed' by hedges, ditches and stone walls which today we regard as typical.[52]

These changes in farming methods, particularly in East Anglia and the east Midlands had serious social changes. The disappearance from the village of crafts such as cloth weaving, wagon building, milling and brewing took away the apprentice system and with it the older educational forces. The rural outlook was narrowed and the villagers' outlook and independence was lowered. These changes, coupled with the new Poor Law which was financed by rates on inhabitated buildings in the parish, led to a new system of agricultural employment which was soon abused.[53] This development was well described in the Sixth Report, in 1867, of the Children's Employment Commission:[54]

> The public gang system does not appear to be anywhere in the eastern counties (where alone it is found) more than 60 years old; in some localities it has not existed more than 30 years. It is the direct result, as has been described, of two causes; first, of the pulling down of cottages in what are termed 'close' parishes to avoid poor rates, and thereby driving the agricultural population off the land into distant villages and towns; secondly, of the formation, by the enclosure of wild land or otherwise, of large farms without providing an adequate

number of cottages to contain the workpeople required to supply the demand of labour for the farm.

The system of public gangs which had thus grown up operated almost entirely in the six eastern counties. Gangs usually of about twenty persons but sometimes of up to forty persons, consisted of women, young persons of both sexes, and children. They were collected together by a 'gangmaster' who contracted with farmers to undertake specific tasks (weeding, seed planting, harvesting) for which the 'gangmaster' was paid and it was he who set the terms of employment and controlled his gang. The first indication of evils resulting from this system appeared in 1843 in the report of an Assistant Commissioner of the Poor Law Board.[55]

However, the matter was forcibly brought to notice by Sir John Simon in 1864 in the Sixth Report of the Medical Officer of the Privy Council.[56] Infant mortality was known to be high in the unhealthy manufacturing towns, in Manchester it was 26,000 per 100,000. In some of the better districts it was about 9,000 but, surprisingly, in the rural town of Wisbech in Cambridgeshire it was 25,000. Other towns in East Anglia showed as sorry a record. An investigation carried out for the Privy Council by Dr. Henry Hunter showed that: 'The monstrous infantile death rate of the examined agricultural districts depends only on the fact that there has been introduced into those districts the influence which has already been recognized as enormously fatal to the infants of manufacturing populations—the influence of the employment of adult women'.

He continued by stating that this led to three evils; incorrect feeding by the foster mothers, starvation and drugging with opium.

The interest aroused by these findings widened to an awareness of other possible consequences of gang labour and of the need for action. This led to a curious episode recounted by Tremenheere. Tremenheere[57] reported to Lord Shaftesbury, who was himself a landowner, a conversation overheard at the Reform Club in which Mr. Bright had said 'that Lord Shaftesbury had now been very busy for a long time in having the motes pulled out of other people's eyes and seemed to have no inclination to pull the beam out of his own. See the way in which he and his fellow landowners had caused the Government to treat the Poor Law Commissioners about the Agricultural Gangs'. Tremenheere reminded Lord Shaftesbury of the way the Government had dragged its feet over the earlier report on the gang system by the Poor Law Commissioners and he concluded by telling Lord Shaftesbury that, 'I felt quite sure from the tone of Bright and his friends, that he would probably move very soon in the matter; if he did he would certainly say many things that would not be pleasing either to himself or his fellow landlords to hear. Jumping up from his seat he said, "I'll go down to the House and move an Address to the Queen at once. Many thanks. Goodbye".'

Tremenheere recounts that conversation as taking place on 12 May 1865. That same day the Earl of Shaftesbury moved such a petition.[58] He cited evils of the gang systems as the abuse of child labour, the physical harm to young girls from working long hours in the fields in all weathers and finally, 'Gangs of one sex, having no proper superintendence over them, were bad; but when the two sexes were combined in the same gang the vice and immorality which resulted were fearful to contemplate'. Perhaps such thoughts were fearful to contemplate in the House of Lords but Sir John Simon came to the point more simply,[59] 'The system, moreover, conduces to a vast quantity of reckless fornication'.

Lord Shaftesbury concluded his address by drawing the teeth of Mr. Bright's projected attack.[60]

> It is interesting to note also that the Factory Acts were already the yardstick against which to measure any working conditions.
> Such an enquiry was due to the manufacturers, whose employment of children and young persons the legislature had subjected to regulation and investigation. They are accused of having been exceedingly sharp in looking after the abuses of factory labour, while they had sheltered those connected with the agricultural industry. An opportunity was now presented to them of showing the reverse to be the fact.

The Report on Organised Gangs formed the Sixth Report of the Children's Employment Commission and was published in 1867. In charge of the gangs[61] were 'men whom the farmers are not willing to have in their regular employ; . . . in most cases men of indolent and drinking habits and in some cases men of notorious depravity, as a rule unfit for the office they undertake.' Under such leaders the evils of gang labour were confirmed. They included the employment of young children of six and upwards working sometimes from 8 a.m. to 6 p.m. and at others from 5 a.m. to 7 p.m.; these periods included the time spent walking to and from work, maybe, 5, 6 or 7 miles away from home. For this they might be paid 7*d*. a day and would of course have to find their own food.[62] The lack of education under such a system was obvious.

As with the abuses of child labour in factories in the early part of the century it was not simply the innocent child being driven by a ruthless master. The child had sometimes to be protected from the ignorance and apathy of its own parents.[63]

> Where the pressure of the demand for children's labour, or the selfishness, or the indifference, or the presumed necessities of the parents, conspire to urge the sacrifice of the child, there is no reason why the same legislative protection should not be thrown around the child employed in one of these public agricultural gangs as has been accorded with most beneficial results, now universally acknowledged, to the children engaged in the trades and manufactures of a great part of the kingdom, and proposed to be extended to the whole of them by the Bills[64] now before the House of Commons.

The Commissioners also drew attention to the consequences of young girls going into wet corn fields at the start of a day's weeding and getting their clothes wet through[65] and compared the provisions of the Factory Act which prohibited the employment of women and young persons on wet spinning processes in textile factories unless means were taken to prevent them from being wetted.[66]

Evidence of the bad moral results from the intermixture of the sexes was provided by Dr. Morris, for many years the medical officer in the Spalding Union.[67] He testified that 'many girls of 14, and even 13, to 17 years of age have been brought to be confined in the infirmary [the Poor Law Hospital] whose ruin has taken place in going and returning from gang work'. More concrete evidence was found in the returns of births for the Spalding registration district for the four years 1862–65. These showed[68] that the proportion of illegitimate to legitimate births in each of those years is about one to seven, being a rate of illegitimacy twice as high as the average rate of the kingdom.

It should be stressed that apart from the consequences recounted above, there was nothing harmful in the employment to attract the attention of medical men,[69] a point to which we shall return later. In fact, as in the factories half a century earlier, the most noticeable ill effects were social abuses, sometimes showing medical consequences, rather than the appearance of any occupational disease.

The Agricultural Gangs Act[70] which resulted from this report is still in force with some modifications today. The Act, as passed in 1867, regulated the employment, by gangmasters, of children, young persons and women (children were less than thirteen years of age; young persons were aged from thirteen to eighteen). No child of less than eight years was to be employed in an agricultural gang and females and males could not be employed in the same gang. If females were employed under a male gangmaster there had also to be a female gangmaster. Gangmasters were to be licensed by two justices. Another provision of the Act prohibited keepers of public houses from being gangmasters. Early factory legislation had failed due to lack of adequate provision for enforcement and it is interesting to note that, apart from the licensing of gangmasters, there was no provision for enforcement of the Act.

Although the Agricultural Gangs Act was concerned with abuses which were confined to the eastern counties, a start had been made. The idea of sparing children from long hours of factory work, to make them available for education, long established in the industrial areas, had now been accepted in some rural areas. A follow up is found in a report[71] of a meeting of the Warwickshire Chamber of Agriculture in 1868. The meeting was called, 'To ascertain its opinion as to what extent and with what modifications the principles of the Factories Act can be adapted for the regulation of such employments, and especially with a view to the better education of such children'. It was decided that, except for a short time in the spring and winter, no girls of under eleven years should be employed and that farmers would not be put to any inconvenience if they employed no boys under ten years of age.

Although there may be a tendency to regard these as isolated developments in occupational health legislation, the Victorian reformers regarded them as parts of the whole movement of social reform. Commenting on agricultural gangs in the House of Lords, the Earl of Shaftesbury spoke of the principles of the Factory Acts being applied to the children of the agricultural districts.[72] In view of this, it is interesting to see how the Factory Acts and Mines Acts have moved and changed with the passage of time, but protective legislation for agricultural workers remained stationary. Apart from two rather ineffectual Acts relating to Threshing Machines[73] and Chaff-cutting Machines[74] nothing more happened until the middle of the twentieth century.

FACTORIES

Throughout this paper the influence of factory legislation on the development of similar legislation for mining and agriculture has been stressed. But the 1860s also saw great developments in the development of factory legislation itself, prompted, no doubt, to a considerable extent, by the results of the investigations of the Medical Department of the Privy Council[75] and of the reports of the Children's Employment Commission.[76] An early result of these investigations was the 1864 Factory Act Extension Act[77] which brought six dangerous trades* under some degree of supervision and so, for the first time, extended factory legislation beyond the textile trade. It also included the first effective requirement for the provision of exhaust ventilation for the removal of harmful dust and within three years this provision was extended to all factories and workshops and still exists today, in much the same form, as our main legal safeguard against industrial lung disease. This important development was taking place in factory hygiene, almost certainly as a result of Headlam Greenhow's work indicting dust in factories as a cause of industrial lung diseases. Yet, as observed earlier, he was at this time, as a member of the Commission on Metalliferous Mines, passing over the influence of 'gritty particles' and directing attention to ventilation and the composition of gases.

The further reports of the Children's Employment Commission led to two further developments in 1867. First was the extension of factory legislation to any premises in which fifty or more persons were employed in any manufacturing process.[78] Second was the Workshop Regulation Act,[79] applying to any establishment in which fewer than fifty persons were employed, and which was to be enforced by local authorities. With a few notable exceptions[80] local administration was not sufficiently organised to cope with the task,

* The six dangerous trades were: The Manufacture of Earthenware, The Manufacture of Lucifer Matches, The Manufacture of Percussion Caps, The Manufacture of Cartridges, The Employment of Paper Staining, The Employment of Fustian Cutting.

and four years later control was transferred to the factory inspectorate. Both the Factory and the Workshop Acts of 1867 took over the important provision of the ill-fated Sanitary Act of 1866 requiring the employer to provide a fan or other mechanical means to carry off injurious dust.[81]

CERTIFYING SURGEONS

This sudden and considerable extension of factory legislation led, among other things, to a great increase in the work of the Certifying Factory Surgeons and to a reaction against them by some of the employers. This development, in turn, resulted in the doctors grouping together to form the Association of Certifying Surgeons in 1868.

The Certifying Surgeons at first had the sole duty of issuing certificates stating that a child was 'of the ordinary strength and appearance' of a child of the stated age. This duty had started, effectively, with the Factory Act[82] of 1833 four years before the introduction of the registration of births. The Factory Act[83] of 1844 had regularized the position of the Certifying Surgeons.[84]

According to a correspondent to the *Lancet* in 1868, the many new appointments to be made under the extended Factory Acts, offered the opportunity to the doctors to act with effect.[85] He claimed that he had, since appointment, firmly declined to grant certificates under a fee of 5*s*. He found the appointment superior to a Poor Law post and the work easier, pleasanter and better paid. A short note appearing in the *Lancet* a few weeks later[86] read: 'The adoption of the Factory Act in Glasgow, according to our correspondent, will render the appointment of Certifying Surgeons, of which there are three in that city, very valuable. The salary will be about £3,000 a year each. An agitation has commenced, having in view the subdivision of the work, and consequently of the emoluments of the office'.

We may set this in contrast with some other incomes of the period:

Inspectors of Factories, Baker & Redgrave,[87] ..	£1,150 p.a. each.
The House-Surgeon at the Royal Free Hospital[88]	£109:4:0*d*. p.a.
Agricultural Labourer in Norfolk[89]	10*s*. to 12*s*. per week.

The Factory Inspectors had the statutory duty to ensure that children and young persons were medically examined and were caught in the dilemma. On one side were the employers who saw little gain to them in this costly process and on the other side were the doctors pressing for high fees. Mr. Redgrave issued a circular suggesting a scale of 2*s*. 6*d*. for a visit and one certificate, and sixpence for each additional certificate. When the works were more than a mile and a half from the residence of the surgeon an extra sixpence per half mile could be charged. The *Lancet* immediately came into the attack claiming that there should be one uniform fee for all certificates granted under similar conditions of distance, etc. The explosive language reads strongly to those accustomed to the modern leaders of our medical weekly journals.[90] 'That it is required of members of our profession not only that they certify that young persons under the age of sixteen employed in factories are free from organic disease, but that they are adapted by physique and temperament for the work about to be allotted them, and this for a sum which, as a correspondent expresses it, "is too small for a cabman, and displays an ingenuity of insult which could hardly be surpassed even by a board of guardians" '.

The fees for the certificates issued by the Certifying Surgeons were paid by the employers who argued that the certificates were both costly and useless, since by 1867 certificates of birth were available and were a much more reliable evidence of age than medical examination. The *Lancet* replied with a spirited defence of the certifying surgeons.[91] 'The manufacturing interest is so well known to resent all interference with its supposed vested rights in human life and health, that we are not surprised to find a Birmingham writer kick against the pricks and vote all medical interference a bore and a sham, particularly since the manufacturer has to pay for the very inspection of which he complains'.

One wonders if the medical profession could be said also to have a vested interest for, after all, they stood to gain financially from the very inspection which they were defending. But criticism came from another quarter[92] in the Report of the Committee of Council on Education 1866–7:

There is also another impediment to which I allude with some hesitation, inasmuch as it involves a serious charge. But one can not go about very much in the manufacturing towns without becoming acquainted with many cases in which the intention of the Factory Act with regard to the ages of children is violated. It happens thus. Children are not allowed to work 'full time' until they are thirteen years old; the mills are short of 'hands'; mothers anxious for the increase of their children's wages volunteer to assert that boys or girls (actually only eleven to twelve years old) have passed the required ages; overlookers do not make too many enquiries. The doctor has only to certify that a child *looks* thirteen, and the thing is done.

The *British Medical Journal* bitterly resisted the suggestion that the certificates of fitness were no longer necessary.[93] In the warmth of their arguments the medical profession were themselves perhaps guilty of certain weaknesses. The certificates had been introduced to ensure that a child was of the 'ordinary strength and appearance of a child of the stated age'. This phraseology had been employed to avoid asking medical practitioners to certify the age of a child—something which is notoriously difficult, if not impossible, from physical examination alone. The opportunity was taken to turn this phrase to advantage now that age could be determined accurately from birth certificates and so the spokesmen for the medical profession chose to regard these as certificates of fitness for work as well as of age. But this technique is a two-edged weapon. Certificates of fitness for work, as a social measure required by law, require more careful consideration than appearing merely to perpetuate jobs for doctors. A problem posed by the claim that these certificates should be regarded as certificates of fitness for work would be the lack of purpose. Would it have been possible for a child rejected by the doctor as unfit to work in a factory to have sought and obtained employment which was not regulated by the Factories Act? What would be the procedure for obtaining treatment for a child so found at routine medical examination? The lack of attention by the medical profession to such questions arising from the course of action they proposed must make one question whether they were so disinterested and philanthropic as they appeared to be. Perhaps there is a lesson here for the present time. It is a proper function for certain bodies to look after the interests of the profession, but we should be careful not to let that function appear to become confused with our responsibilities in the development of medical care.

Was there any evidence on the proportion of children being rejected because of physical defect and disease? Mr. Redgrave thought not, although the *British Medical Journal* disagreed with him. The fact that there was practically no information on the subject, a shortcoming which one of the leaders of the Certifying Surgeons, Dr. Arlidge, admitted and suggested should be put right.[94] In a period of careful and comprehensive government reports, usually backed up by statistical enquiry it was unwise of the medical profession to propose a course of action with little but enthusiasm and opinion to support it.

The enthusiasm of the medical profession sometimes led to propositions which, to us, appear somewhat curious. A leading article in the *Lancet*[95] in 1867, referring to the House of Commons debate on Agricultural Gangs, suggested the appointment of doctors as inspectors: 'And not only must inspectors be appointed to ensure such observance, but they should be drawn from that profession which alone can exercise the kind of inspections required. How can a layman pronounce with intelligence on the sanitary effects of any given employment upon its notaries?'

It is hard to believe that the writer of that leader had even read the report on the Agricultural Gangs system, with its emphasis on long hours by young children and the accusations of immorality. In fact, the medical evidence to the Committee was that agricultural work was a healthy occupation. Furthermore, a few months later the *Lancet*[96] again took the opportunity given by publication of the new far reaching Factory Acts[78,79] to return to the theme: 'With the extended jurisdiction of factory legislation, an increased number of inspectors and sub-inspectors will be needed to ensure the proper carrying out of its provisions. The measure being almost purely sanitary in its tenor, it follows that the officers appointed to superintend its application should be drawn from that profession which has most practical acquaintance with the laws of health'.

This somewhat partisan view of factory legislation was taken by Dr. Arlidge in his presidential address[97] inaugurating the Association of Certifying Surgeons. He passed over the other requirements of factory legislation; overtime, the fencing of machinery, the proper keeping of books and such like as very subordinate, though necessary, matters in these enactments.

We find a clue to this attitude in one of the *Lancet* leaders already quoted,[96] for it goes on to remark that out of the over-stocked ranks of the medical profession inspectors may be drawn at the shortest notice. It is well that the government of the day paid little heed to this special pleading, for the medical profession has been spared a lot of routine inspectorial duties for which it is not trained and may now instead confine itself to advice on the standards which others will enforce and in providing certain monitoring functions to ensure the maintenance of these standards. It is well to read this lesson from the past for the distinction is not always clearly made at the present day.

That time of expansion and adversity brought the doctors working in industry together to form the Association of Certifying Surgeons, the first president was Dr. Arlidge, a physician at the North Staffordshire Infirmary. The objects of this association were:[93] (1) The observation and collection of facts tending to promote the advance of sanitary science and the relief and prevention of disease incident to the various processes of manufacture. (2) The consolidation and improvement of the position of the Certifying Surgeons, in relation to the Government and the public.

It may be argued that this is an unhappy combination and that the collection and assessment of such information should be pursued irrespective of the ends to which it would ultimately be put, particularly if the ends enhance the state of persons collecting the information. It would be an interesting exercise to assess critically the success of different medical bodies in trying to fill these two roles.

CONCLUSION

As observed in the introduction, this study has been a series of snapshots rather than a continuous story. It has been seen that, one hundred years ago, many of the fascinating facets of industrial medicine: environmental studies, clinical investigation, epidemiological studies and the problems of integrating different aspects of medical care were exercising our predecessors as they exercise us today.

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Social Medicine and the Growth of Statistical Information

by

RUTH G. HODGKINSON

AS THE title of this paper may be somewhat misleading in terms of modern nomenclature, I should like to make it clear that I shall not be covering the development of mathematical juggling with figures, which is a separate story requiring expert knowledge. In the 1860s, when this scientific discipline barely existed and the whole concept of social medicine was in its infancy, the primary need was for the collection and dissemination of information. So I shall use the term 'statistics' very loosely and in its widest connotation.

Although I shall be restricting myself to the health field, it is extremely important to bear in mind the close connection between social medicine and social welfare. The wider aspect would have proved more interesting and compelling. I take but one sample field, but similar treatment could have been given to education, housing, or the factory movement. There were huge, closely connected general problems throughout the greater part of the nineteenth century, and the early reform movements were interdependent. Investigation into the causes of evils and their amelioration inevitably and necessarily demanded the use of similar techniques and frequently the same information.

We must remember also the significance to our theme of the great ferment in the scientific world, the vast exploration in biology, medicine, anthropology, meteorology—to name but a very few subjects. There was an overall surge towards enlightenment. Closely allied to this was the outburst of general intellectual curiosity in the political, religious, and philosophical world. Accumulation and rationalization of knowledge characterized this period. There was, further, a new practical humanitarianism, and this was dependent on accurate information. Finally, it was also the age of nascent specialization, when not only medicine and the sciences were becoming subdivided, but also the realm of social welfare. Here, too, much more information was both needed and collected. Turning then to the new field of social medicine, I have divided this simplified résumé into three parts:

1. Why was there a need at this time for a great quantity of accurate information?
2. What became available?
3. What had to be done?

(1) The answer to the first question is well known. There was by the 1860s a crystallization of the earlier vague awareness of the health problems posed by rapid industrialization, by the sudden growth of population and its migration, by the poor quality and quantity of food, by inadequate housing, sewerage and water supplies, by poverty and insufficient medical care. In sum, the problem of the new slum town and the old impoverished village.

And by the mid-century there were no longer just the *few* dedicated pioneers of reform but *many* from all walks of society who were coming to recognize that medical and public health provisions were a vast social and economic problem to be investigated and solved by national means rather than left to individual humanitarianism or private charity. The rulers also whilst paying lip-service to 'laissez-faire', the sanctity of individual effort and the deep-rooted rights of local government, after the middle of the century, came to realize that these were national problems requiring solution on a national scale. But how?—There was no expert civil service, not even a large body of men with the right education, little mass information and fewer statistics.

As this dilemma became obvious there emerged a new generation of reformers, practical and hard-headed but still interested in humanity, who saw problems realistically and sought efficient and accurate means of pursuing their aims. Figures stark and pertinent were to provide part of the answer, and the training of experts to collect and interpret them. Chadwick, Rumsey and many others constantly demanded both, plus the means of disseminating information in a comprehensible form among the masses as well as among the educated. These new ideas were put into practice before the 'sixties but they only became widely used and recognizable then. This was particularly apparent in the labours of a corps of amateurs with a necessary professional skill—namely the doctors. Some were in hospital service, a few in private practice, but above all the State doctors, the Medical Officers of Health, of whom there were 1,500, and the Poor Law Medical Officers, of whom there were 3,500.

Their information was forceful and acceptable, because it came from an educated, respected group from the largest body of professional men with any scientific training.

(2) What then became available in facts and figures at this time? This was the dawn of our modern age of quantity investigation and the publicizing of findings. I would like to call this the *Early Age of Purposeful Inquiry*. Mathematical precision might have helped, but few of our modern techniques existed and where they did were little used. Not even Farr was a good mathematician. People worked with figures using totals and bare averages. Producing the raw materials was sufficient to obtain results.

There were two types of investigation for collecting facts and figures to promote the advance of State medical care and the Public Health Services. One, that undertaken by the individual or a society and often covering only a small field or a narrow interest, the other, the inquiry sponsored by a government body, generally on a national level, although sometimes for a special area or a specific problem. Because governments were loath to interfere or moved slowly and cumbrously, an individual or an organization frequently acted first, hoping to prod the legislature to embark on further and fuller investigations afterwards. When we reach the 'sixties the government was undertaking a large number of official inquiries—far more than we realize today. (An index makes extraordinary informative reading).

In the private sector there immediately springs to mind the many Public Health reformers who undertook so much independent research and published papers and articles apart from the reports they made to the Medical Department of the Privy Council. There were men like Gavin and Greenhow. And there was Guy with his wide range of interests and his extensive statistical material, delving into such problems as 'The influence of the seasons and the weather on sickness and mortality', into medical education, into occupational disease. There were also the lesser known individuals like Dr. Stallard with his detailed comparative study of pauperism and medical relief among Jews and Christians; and John Arlidge who inquired into Lunacy and the care of the insane. There was Henry Mayhew, who with his four volumes on *London Labour and the London Poor* (1851–62) provided a precedent for Charles Booth's monumental Survey forty years later. His tabulated statistical information was compiled from authentic records and he claimed his studies to be the first 'blue book' ever published in twopenny numbers. There were the novelists who spent a long time investigating for themselves and who probably popularized more information than the learned societies. I can allude only to Dickens—an example being his description of workhouse infirmaries and nursing—and to Charles Kingsley, whose private inquiries were so thorough that he not only provided convincing propaganda through many of his novels, but was called to give lengthy evidence on several aspects of Poor Law Medical Relief to Parliamentary Select Committees of Inquiry.

I have deliberately chosen a strange collection of individual investigators in order to give a cross-section. The greatest figure of all working independently was Dr. Henry Wyldbore Rumsey. For forty years he carried on a campaign for a wide State medical service combining the medical and Public Health fields. He probed into every problem, and his many, often lengthy publications merit re-reading today.* Rumsey, even more than other prominent reformers, preached that success could only come through united action, by organizations acting on accurate information so comprehensive that it could not be ignored.

The trend towards forming societies for collecting information, which could also act as pressure groups, was evident after the middle of the century. Often they were short-lived; of these two were outstanding, namely the Society for Improving London Workhouses, a society for collecting information, and the Workhouse Visiting Society, with its influential, public-spirited ladies. Both played a

* Particularly his *Essays on State Medicine*, London, 1856, which is over 400 pages in length and *Essays and Papers on some Fallacies of Statistics*, London, 1875.

significant role in getting workhouses improved and a nursing system begun—an important fact when over 80 per cent of the country's hospital beds were in workhouse wards or infirmaries. Florence Nightingale was so impressed by the revelations of these societies that she used her great influence on their behalf in government circles and administrative headquarters. The sum total of the individual efforts of the small societies, which included local health groups and medical associations, was significant, and this again was a new development in our history.

Many of the large societies prospered and are still with us. The Epidemiological Society was founded in 1850 through the findings of Chadwick's and Southwood Smith's Health of Towns Association (1844), the figures of the Registrar General, the inadequacy of the 1848 Public Health Act and the cholera. (The epidemic of 1848 had killed about 60,000 people). The medical profession wanted to *know* more before it could *do* more. Babington, in his opening address as President, stated that the objects of the Society were 'to collect facts upon which scientific research may be securely fixed, to remove errors which impede their progress, and suggest means of prevention and combating them'. A questionnaire on the extension of vaccination was immediately sent to 2,000 doctors. (The questionnaire was a fairly new technique used also by the Poor Law Medical Officers' Association and the Provincial Medical and Surgical Association in the late thirties). The House of Commons and the House of Lords found the Society's painstaking investigation worthy of publication. The interests and papers of the Epidemiological Society ranged over the whole world and were not devoted to purely medical science. Appearing in the *Transactions* were for example Snow's 'Comparative mortality of large towns and rural districts' (1853, Gavin), Milroy's 'Suggestions for utilising the statistics of diseases among the poor' (1859), Clifford Allbutt 'On the prevention of disease by the reconstruction of the dwellings of the poor (1866), and Frederick Mouat's 'Medical statistics, with especial reference to cholera and syphilis' (1869).

Another great fact-finding body still surviving but with changed interests and scope is the Statistical Society. It was formed in 1834 as an offshoot of the British Association when it was realised that social and economic conditions in the country had altered so materially and permanently as to warrant statistical elucidation and separate investigation. (The Manchester Statistical Society had been founded a year earlier and throughout all our period published reports and papers by distinguished reformers).

The real statisticians supported the Statistical Society for they had begun to feel the need for collaboration among themselves and realized that the trained intellect had much to gain from the amateur. The earliest list of 200 members of the Society contains an incongruous but significant group of people: Chadwick, Gladstone, Malthus, Nassau Senior and two members of the Poor Law administration. At the opening session the desirability for co-operation of private individuals with the government on statistics was stressed and that 'accurate knowledge of the actual condition and prospects of Society is an object of great national importance not to be obtained without a careful collection and classification of Statistical facts'.

The aims of the Society were to arrange for a good set of inquiries, to collect fresh statistical information and to condense and publish much that already existed. There were also great schemes for international co-operation and co-operation with local societies. Although questionnaires were to be carefully drafted and there were many willing investigators, no one really knew how to embark on these ventures. At the outset the Medical Committee ran into difficulties because of the 'scanty character' of the 'numerical' information available. The Society tried to encourage members who worked in public institutions, hospitals and charities and in the Poor Law medical service to give annual information and small grants were given to some investigators. Every year new facts and figures were published, which make important reading for the research worker today. Chadwick, Farr and Guy were among the chief contributors to the Journal. Many doctors forwarded statistics of epidemic diseases, birth and death rates and accidents. (Accidents were beginning to arouse some interest at this time.) The Society was not interested in the development and refinement of statistics as a mathematical science. It was claimed that it was a workshop rather than a laboratory and gave help to all organizations connected with social welfare. It was also believed that the 'samples of misery' did have a 'powerful effect on the political notabilities associated with the society, who carried the influence and knowledge of the Society into the Councils of the Nation'.

In 1869 the President of the Society surveyed 'what had been achieved in Statistics and what remained to be done'. He presented a graduated list of eight fields in which the greatest success had been obtained. First came Vital Statistics—births, marriages, deaths, diseases and epidemics. This was due to Farr, Chadwick and the strong medical representation in the Society. Second came census statistics including occupations, ages, social conditions and distribution of people. Third came pauperism, police and crime figures. In these three fields, it was maintained, Britain was in advance of all other countries because: 'we have attained scientific completeness and precision on (1) The extent and variety of the observations made. (2) In their authentic character. (3) In the uniform methods of their collection. (4) In the natural and lucid order of their exhibition. (5) In the rigid manner in which the deductions are kept close to the facts'.

The Society did however admit that periodical statistics of hospitals in London and other large towns still needed urgent attention 'with a view to a comparison of efficiency and the cost of relief afforded to each'. In this field Florence Nightingale, who was a member of the Statistical Society for fifty years, made the most notable contribution. With her, statistics were almost a passion and it was her dream to make 'statistics a fundamental discipline in the education of statesmen'. Regarding hospital statistics she read a paper to the International Statistical Congress in 1860 submitting a scheme for uniform numerical data and even international coverage. She had earlier worked out her Model Forms and a few of the large London voluntary hospitals had adopted her scheme experimentally. She studied her results with Farr and demonstrated how large a field for statistical analysis and inquiry would be opened by the general adoption of her Forms. The experiment was shortlived but she continued to press for her scheme for many years.

Although hospitals and sickness were discussed by the Statistical Society with the co-operation of Lister, Farr, Guy and Southwood Smith, inquiry into this vast province fell more naturally to the National Association for the Promotion of Social Science. The Statistical Society greatly aided the foundation of the Social Science

Association in 1856, and in 1864 the union of the two societies was proposed but rejected. The Social Science Association *also* had a very strong medical representation and many doctors belonged to both organizations. This was important for the inter-dependence of reform activity as well as for the spread of information and interest. At one time or another everybody connected with the wide field of social welfare read papers to this vigorous and influential association, and it was foremost in advocating improvements in the Public Health and Poor Law Medical services. Among the objects of the Association were listed . . . 'it collects statistical evidence of the relative healthiness of different localities, of different industrial occupations and generally the influence of external circumstances in the production of health and disease'. It was to this Association that Rumsey offered some of his most important and lengthy studies, and Timothy Holmes, the surgeon, presented a paper on 'the necessity for Public Inquiry into the Hospital System'. He and Bristowe had undertaken the famous public investigation into the Hospitals of the United Kingdom in 1864.

On the purely medical side State doctors both in the Poor Law and Public Health fields formed associations to provide facts and figures not only for professional reasons but also to improve the services for which they worked. The information they produced was important for the government and for the public. One of the primary aims in the founding of the British Medical Association was the setting up of Investigating Committees to inquire into the Poor Law Medical Service. The reports were often incomplete and often prejudiced but the overall picture was true and had its effect.

The role played by the medical journals in this period was also impressive. They published the facts—and even collected their own through investigating committees. In the pressure for government action on health problems the part of the Press must be appreciated. I have time to mention only the *Lancet* which was a mine of knowledge on every aspect of social medicine besides being the mouthpiece for the reformers. Of paramount significance in the sixties were the *Lancet's* very long and detailed investigations into workhouse infirmaries. These prepared the ground and forced the Poor Law authorities to undertake their own Inquiries which directly resulted in the Metropolitan Poor Act 1867—the real beginning of our public hospital system.

So far we have covered what I have termed the private sector. We have seen how large and small organizations piled up information which could not be neglected. The biased facts and figures of the outraged individual, the less dramatized and more accurate data of the learned bodies—all added up. But, as the Statistical Society soon discovered, the obtaining of facts on a massive scale was beyond its capabilities and had better be left to government scope and compulsion. In addition there were the informative petitions and memorials to Members of Parliament, the Poor Law Board, the Home Office and the Privy Council. The legislature could not ignore the demand from so many quarters and was forced to undertake investigations of its own. Chadwick's spirit was behind the inevitable trend and the condemnation that he made of the Poor Law Board in the mid-fifties 'never to act until you are obliged, and then to do as little as you can' became progressively less tenable. So there was an increase in official inquiries, which resulted in an increase in legislation, or an increase in activity, within the framework of the existing Acts of Parliament.

The Reports of the Royal Commissions and of the Parliamentary Select Committees, presenting a more comprehensive and accurate picture, became the greatest weapon for reformers in and out of Parliament. Those giving evidence and even some of the Reports included the most lurid and glaring abuses just as in the earlier days, but the increasing quantity of figures lent an air of undeniable authenticity and authority to the investigations. Most famous of the inquiries is perhaps that of the Royal Sanitary Commission (1868–71) with its far reaching findings and recommendations, which resulted in three Acts of Parliament immediately and affected health policy until the turn of the century. The three Parliamentary Inquiries of 1861–4, 1866 and 1867 into the operation of the Poor Law were also of the greater importance because of their information on the medical services and the subsequent legislation.

There was another source of government information. I can again allude only briefly to the significance of the data accumulated in the Annual Reports of the Medical Officer of the Privy Council regarding Public Health, industrial medicine, nutrition, and to Simon's great work in instituting inquiries and publishing the results. The Annual Reports of the Poor Law Board are less well known because historians have concentrated on the Public Health field and have neglected the actual medical care that was publicly provided. These Annual Poor Law Board Reports presented a wealth of information and statistics obtained from District Medical Officers, workhouse officers and Guardians. Of great value also were the separate inquiries undertaken by the Assistant Poor Law Commissioners into types of treatment available, diseases, infirmary accommodation, nursing and diets, and frequently regions were compared. Specialists also were drawn in for Poor Law investigation. Most notable was the Cubic Space Committee (1865)—the first Committee of Experts (eminent physicians)—and this was, incidentally, influential in paving the way for improvement in general hospital construction.

Parallel with the many public inquiries and private investigations ran one continuous source of information and statistics. It was used by all for comparative purposes or for obtaining authoritative official figures. I refer to the Reports of the Registrar General. They alone could show that one quarter of the entire annual mortality was preventable. The growth of this office, making improvements in the collection of vital statistics, in its scope and coverage, and in its great influence on the development of social medicine, warrants a detailed study of its own. And the role played by the Registrar General's Department in this period was of very special significance through the work of William Farr. The publication of vital statistics assisted the reformers so much because of the inspiring reports which Farr appended to Graham's official documents. His extensive and penetrating interpretations popularized Public Health questions on a high level and they have a unique place in the history of social medicine. And in his constant demand for more exact definitions of diseases and therefore their diagnosis, Farr inevitably furthered progress in medical science also.

In contrast to our contemporary, purely mathematical, presentation of vital statistics, Farr used the statistical and literary method combined, and it has been said that his literary accounts would make a modern civil servant shudder!* But as I have already tried to demonstrate, mathematics were not of predominant importance in

* M. Greenwood, *Some British Pioneers of Social Medicine*, London, 1948.

those days. Farr did not exaggerate the value of sums, he never calculated a single 'probable error' and often used averages based on small numbers. Yet his work earned him the title of 'The Father of Vital Statistics'. His extraordinarily simple mathematical techniques were sufficient for his purpose in providing the raw material for reformers. For example, he presented special Reports on the cholera in 1848–9, 1853–4 and 1865–8 and these included his theories regarding polluted water in the cause and spread of the disease. He undertook his own investigation, even exposing himself, and thereby played a part in the improvement of London water supplies. In 1869 he drew attention to the excessive number of deaths in certain workhouses and hospitals and exclaimed: 'can no further progress be made?'—He referred to reforming the provision in workhouses for the care of the sick and aged and also pointed out that prisons offered model sanitary conditions compared with what was available to impoverished honest labourers. His Reports are a mine of information on such problems as occupational disease, children's diseases, the early prevention of sickness, the care of lunatics and idiots, town improvement, vaccination and syphilis. He often wrestled with the problem, so important at the time, of the classification of epidemic, endemic and contagious diseases, and his proposals on nosology contained in a paper to the Statistical Society were later accepted by the College of Physicians and agreed to by the Registrar General.

Turning to the medical profession he proposed to improve the health of mankind by improving medical institutions and the status of the practitioners. This was a question, he concluded, to which no statesman had ever paid any attention although it was a problem which affected the entire population.

Regarding the human story and its close connection with the medical profession he asked for thought on his calculations that there were over two years of severe sickness on an average to every death. And he was not content in his reports to leave uncommented the fact that the annual mortality in healthy districts was 4 per cent compared with 14 per cent in Liverpool and other industrial towns: 'In Liverpool the death of children is so frequent and dreadful that a special system of insurance has been devised to provide . . .coffins and burial ceremonies. The mother when she looks at her baby is asked to think of its death, and to provide by insurance not for its clothes but for its shroud and other cerements.'

I leave Farr showing his appeal to the human emotions. It was a deliberate technique with the statistician/social reformer. One can gather this from Chadwick also if one reads between the high content of calculations and tables in his reports. Once, in his Presidential Address to a section of the British Association, he referred to the high mortality of soldiers through sickness and insufficient medical care: 'Every unit of such statistical figure involves a case of pain in the being . . . stricken down, of mental suffering in survivors, and of a diminished estimation of life on the parts of those who witnessed it going on to the end . . . let him do his separate duty by coming forward and pourtraying [*sic*] it, and exhorting governors . . . to do their duties of not letting ill alone . . . of not for the sake of selfish ease, violating their moral duties to investigate and forward the means of prevention'.

Chadwick and Farr were closely associated professionally. Chadwick insisted on the mean age at death as a health index and used this constantly. Farr dwelt primarily on correlating death-rates with density of population. Closely connected with both and offering much criticism or advice was Rumsey.

It is impossible to discuss here all Rumsey's lengthy research on the subject of Vital Statistics. He produced papers for all the interested societies, not only pointing out the fallacies in the data presented but offering elaborate reforms regarding their collection. Most of these have since been accepted by local and central departments, such as the revision of districts and the reorganization of information required. Rumsey insisted particularly on the greater use of medical men, of more comprehensive forms, and on a more accurate system of certification and the keeping of records of diseases throughout the entire country. In 1860 he sat on an extremely important Committee of the Social Science Association which included Southwood Smith, Farr, Simon, Greenhow and Chadwick. This was responsible for twelve recommendations on Registration—all initially Rumsey's, and which the British Medical Association endorsed at its London Congress of 1862.

Rumsey's overriding theme throughout his life-long campaign for reform in the Public Health and Poor Law medical departments was the unity of all health services and therefore strong centralization and the use of the expert. He constantly fought against the chaos of authorities in all fields and the blunderings and ineptitude of the lay administrators. Had his ideas been accepted and put into practice the dilemma which dogged the reformers and troubles the modern research worker would have been mitigated. This meets my final question—What remained to be done? Or more accurately—what are the criticisms of what existed in the 1860s and what had still to be done in the future?

(3) First, the collection of facts and figures had still to be made comprehensive on a wider national uniform basis. The blatant shortcomings in the government statistical and information service were due to complacency and ignorance of the problems, and to the retention of multitudes of authorities, jealous of their power. Both precluded the institution of a central repository or a central retrieving agency, so that much valuable material which did exist was filed away or kept in individual departments just when the information would have been useful to the interdependent development in the social welfare services. This continued to apply after the Poor Law Board and Medical Department of the Privy Council came under the Local Government Board in 1871. Still needed also was the greater use of the trained expert (including the doctor) to provide a scientific method for the collecting of data and their analysis and presentation on both local and central levels. But this is a long term view and depended very much on external factors such as a revolution in British thought on State interference and State spending, as well as on progress in education, medical science and a special knowledge in higher mathematics.

The second problem which still needed attention and one warranting more immediate criticism—was posed by the inaccuracies and anomalies and omissions which existed in the statistical information which *was* available. One thinks of the hospitals of all types, the mental institutions, the dispensaries and the sick clubs. Even in the census itself there was insufficient accurate definition and the legal requirements for giving information had large loopholes. And to give but a brief illustration from the Poor Law Medical Service,*

* Ruth G. Hodgkinson, *The Origins of the National Health Service*, London, 1967, passim.

some Unions made no returns on some questions, some defined short term totals as monthly, weekly or simply the period of treatment, with long cases of sickness frequently being enumerated several times. The classification of workhouse inmates regarding the sick varied greatly and even the Poor Law Medical Officers' reports were presented with every variation on the form provided. In fact, Rumsey once despaired that so irregular and so inaccurate were the returns on every aspect of Poor Law medical care and Public Health that they could not be applied to any general purpose of science.

Yet he was only partially correct, because a tremendous amount of information was obtained, and because of its total impact, an impetus to reform—and to medical research—was provided. In the field of social medicine more facts were collected than in any other department of social welfare, and the quantity and speed of reform depended then, as now, on the collection and dissemination of information.

I would therefore like to conclude by re-stating my belief. With all its inaccuracies and omissions, the mass of information, statistical and literary which was produced by private or state investigation was significant. And it was to gather momentum after the 1860s. When problems were so diverse, so complex and so new, to arouse wide interest for broad general reforms was of paramount importance. To the educated public, figures of arithmetic had to be added to the figures of speech, it was the beginning of our modern scientific trend. Accurate or inaccurate, the facts were slowly driven home and made it imperative on the government to take action.

In my battle for brevity I have deliberately omitted long statistical illustrations. I have given only allusions and references, but sufficient I hope to demonstrate that modern historians have over-simplified their query of the existence and utility of information available during the period under review.

In the 1860s, the necessity for more rapid advances in social medicine and the necessity for more information were understood, so too was their close relationship. It was the beginning of a new age, and both progressed together.

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The Anatomy of State Medicine: Concept and Application

by

ROY M. MACLEOD

INTRODUCTION

'IN MODERN democratic society,' T. H. Marshall has written, 'the State and the professions are gradually being assimilated to one another'.[1] The genesis of State Medicine in Victorian England illustrates this process of assimilation particularly well. Since the work of Royston Lambert and Jeanne Brand,[2] social historians of medicine have sensed the importance of the 1860s to an understanding of the process by which this assimilation has taken place through the institutional development of State Medicine. As yet, however, there have been relatively few attempts to study the evolution of the medical profession in relation to the legal, administrative, educational development of the modern State.[3] Accord-

[1] T. H. Marshall, 'The Recent History of Professionalisation in Relation to Social Structure and Social Policy', in *Sociology at the Crossroads and other Essays*, London, 1963, p. 162.

[2] See R. J. Lambert, *Sir John Simon, 1816–1904, and English Social Administration*, London, 1963, and J. Brand, *Doctors and the State*, Baltimore, 1966.

[3] Although there have been many histories of preventive medicine, notably Arthur Newsholme's *The Evolution of Preventive Medicine*, 1927, George Newman's *The Rise of Preventive Medicine*, 1932, and P. Triscă, *Aperçu sur l'Histoire de la Médecine préventive*, 1923, they, almost without exception, are not concerned with professional development. By the same token, existing institutional histories such as those of the British Medical Association and the Royal College of Physicians are often either out of date, or do not succeed in treating broader social issues. Finally, existing histories of public health which discuss the sequence of legislative and administrative acts are usually not concerned with the development of the administrative process as such, nor with the relation of this process to the development of professional institutions. There seems a need for research in the interstices between the classical divisions of historical medical writing, and especially into the social, legal, ethical, and administrative relations of medical institutions.

ingly, the following pages endeavour to outline the manner in which the academic ideal of State Medicine was applied to the practical problems of public health in the 1860s. Because the subject is still in a very early stage of scholarly analysis, no exhaustive discussion of the problem is possible. Instead, the present essay attempts only to suggest a number of general questions worthy of study, and to suggest the importance of institutional history to a full appreciation of Victorian health policy.

I. THE CONCEPT OF STATE MEDICINE

A. Origins: The Concept of Medical Police

Through the years, the term 'State Medicine' has had both a general and a specific usage. Generally speaking, it has meant the steps taken by a State, through the agency of law, to protect and preserve the health of its inhabitants. This has customarily included governmental control of the extent and quality of medical care, and some control of substances such as food and drink. During the nineteenth century, the concept of State Medicine, influenced by the advance of new scientific knowledge, social needs and changing interest groups, acquired a more specific usage, which became embodied in the institutional forms we recognize today. To grasp the significance of this development, the context of 'State Medicine' must be seen in historical perspective.

Scholars have traced the origins of State concern for the health of the body politic to the medical schemes of ancient Egypt and Greece.[4] Then, State physicians, supervised by a Council of Physicians, were appointed to serve the public. For hundreds of years, the Church, in Europe, performed the task of providing medical care for the general mass of mankind. It was not until the late seventeenth century, with the firm establishment of secular State authority, that the concepts of the 'State physician' gained wide currency in Europe.[5] This concept coincided with the development of 'mercan-

[4] See Hugh Clegg, *State Medicine in Ancient Egypt and Ancient Greece*, Osler Lecture, 1963.

[5] All work in this field must acknowledge a clear debt to the early researches of Professor George Rosen in the history of German public medicine. The present essay is no exception. See especially Rosen, 'What is social medicine?', *Bull. Hist. Med.*, 1947, **21**, 675–734; 'Cameralism and the concept of medical police', *Bull. Hist. Med.*, 1953, **27**, 21–42; and 'The fate of the concept of medical police, 1780–1890', *Centaurus*, 1957, **5**, 97–113. For general reading, Rosen's *History of Public Health*, New York, 1958, is particularly recommended.

tilism', or 'cameralism' in its specifically German form, which embodied the political, economic, and military philosophy of absolute monarchies. In mercantilist terms, power was the first preoccupation of the State. Because a large and healthy population was a vital component of power, it followed that the State was necessarily concerned with the enumeration, protection and health of its people from the cradle to the grave.

The role of the physician in this system was described by Wolfgang Thomas Rau in his *Gedanken von dem Nutzen und der Nothwendigkeit einer medicinischen Polizeyordnung in einem Staat*, published in 1764. It is to Rau that we probably owe the first discussion of the term *Medicinischen Polizeÿ* or 'Medical Police'. His argument was founded upon the logic of economics: the State requires healthy subjects if it is to excel in peace and war; the public health, being a government resource, must be safeguarded by the State through the medical profession; to ensure effective supervision, the State must enact ordnances to test the competence of its doctors and regulate their education; to suppress quackery and to develop administrative and institutional provision for their co-ordinated public activities. In retrospect, Rau represented the first eloquent demonstration of the public health philosophy, which was later to emerge fully in Johann Peter Frank's classic, *System einer vollständigen medicinischen Polizey*.[6]

The role of medical practice in mercantilism was predicated upon the existence of a patriarchal form of Government, and a systematic method of quantitative enquiry, both of which developed on the continent during the eighteenth century. In Stuart England, the need for accurate statistics was recognized in the political arithmetic of Sir William Petty and the London Bills of Mortality, but the disruption of the Civil War, and the rise of a more powerful Parliament, made the concept of a unified, centrally administered system of medical police inapplicable to England. From the end of the seventeenth century to the reign of George IV, England lacked a well co-ordinated system of local administration upon which a state medical system could be built.

Partly in consequence of this fact, the term 'Medical Police' did

[6] See Rosen, *supra*, especially his 'Cameralism and the concept of medical police', *Bull. Hist. Med.*, 1953, **27**, 21–42.

not appear in Britain until about the end of the eighteenth century. Even then, the term was first seen in Scotland, which historically enjoyed close intellectual ties with the continent, at a time when Edinburgh, reflecting the intellectual glory of Adam Smith, the brilliant advances in rational medicine, and the literary genius of early contributors to the *Edinburgh Review*, laid claim to being the 'Athens of the North'. The concept of medical police was used by Andrew Duncan, Professor of the Institutes of Medicine in Edinburgh, who acknowledged his debt to Johann Frank in his lectures on the subject of medical jurisprudence, which began about 1795. Duncan's course was devoted to 'the Medical precepts which may be of use to the legislature or to the magistracy, relating not only to the welfare of individuals, but the property and security of nations'.[7] The course covered midwifery, sanitation and control of communicable diseases, occupational hygiene, and the administration of hospitals.

Duncan's work was later crowned by the establishment of a Chair of Medical Jurisprudence and Medical Police, in the Faculty of Law at Edinburgh. Despite its institutional connection with law, the Scottish authors chose to cast the concept of medical police in a much broader, almost social framework of references. This emphasis is particularly clear in John Robertson's *Medical Police: or the Causes of Disease with the Means of Prevention*, first published in 1809, and in Gordon Smith's *Principles of Forensic Medicine* published in 1821, where 'medical police' is defined as 'the application of medical knowledge to the benefit of man in his social state'.[8]

By the 1830s, however, the new industrial order in Britain had generated the need for health measures which were immediately relevant to the demands of rapidly increased urban populations, for pure air and water, decent habitations, protection against disease, and relief in sickness and poverty. The new Poor Law was the first legislative embodiment of these 'social services'. And, within its context, the medical responsibilities of the continental and Scottish models of the 'medical police' passed into the vocabulary of Poor

[7] Cited by F. Crew, 'Social Medicine as an Academic Discipline', in A. Massey (ed.), *Modern Trends in Public Health*, New York, 1949, p. 48.

[8] See H. Littlejohn (M.O.H. for Edinburgh) on the Burgh Police and Health Bill of 1888, Address to the Public Medicine Section of the British Medical Association, Glasgow, *Brit. med. J.*, 1888, **ii**, 51.

Law relief. By the late 1830s, the legal and sanitary functions of the medical police were absorbed into the Benthamite administrative rubric, as expressed in Chadwick's 'sanitary idea'. As early as 1844, James Black's lectures on public hygiene and medical police suggest that the term 'medical police' was already being confined to the regulation of unsanitary conditions, food, drink and water supply.[8] During the debates preceding the Public Health Act in 1848, some reformers strenuously resisted this tendency to divide medical and sanitary duties between two parallel sets of officers. Henry Rumsey,[9] for example, published an essay in 1846 revealingly entitled, *The Health and Sickness of Town Populations, considered with reference to Proposed Sanitary Legislation and the Establishment of a Comprehensive System of Medical Police and District Dispensaries*, which supported a unified medical service. But the Act of 1848 made the distinction explicit for England, and while the term 'medical police' remained in Scottish parlance until nearly the turn of the century,[10] it eventually lost its original meaning throughout Britain. By the mid-nineteenth century the earlier conception of a unified, centrally-controlled medical police was replaced by the dualism of the existing Poor Law Medical Service,[11] and 'sanitary police' of the Boards of Health. These twin services were the administrative pillars upon which modern British medical policy pursued its own characteristic development.

B. The Concept of State Medicine

On the continent, the term 'State Medicine' had a long history. The term itself was probably German in origin. As far as it is known, it was first employed by the pathologist Christian Friedrich Daniel of Halle in his *Bibliothek der Staatsarztneikunst*, first published in

[9] See also W. Strange, 'On the formation of a system of national medical police and public hygiene', *Lond. med. Gaz.*, 1846, 452–7.

[10] Henry Wyldbore Rumsey (1809–1876), private practitioner of Bristol and Cheltenham and early advocate of medical administration, who began his public career in 1838 by giving evidence to the Poor Law Commission. Between 1835 and 1875, he published over twenty books and treatises concerned with medical relief, vital statistics and sanitary legislation. He was nominated to the General Medical Council 1863, and elected F.R.S. in 1874. Little is known about the factors which influenced his decision to enter public affairs. A brief biography has been written by his grandson, H. St. John Rumsey, 'Henry Wyldbore Rumsey, 1809–1876: a pioneer of state medicine', in *Practitioner*, 1954, **172**, 570–2.

[11] See R. Hodgkinson, *The Origins of the National Health Service. The Medical Services of the New Poor Law, 1834–1871*, London, 1967.

1784.[12] The term 'State Medicine' does not appear commonly in textbooks until the second quarter of the nineteenth century. By this time, however, every German state appointed physicians to act as sanitary and judicial medical officers[13] under the supervision of a 'Government College of Physicians' or a Medical Council.[14]

The terminology applied to these officers varied. In Nassau, for instance, the state physician was called the *Medicinal Beamter* (Medical Officer); in Prussia, he was called the *Kreis Physicus* (District Physician). The *Amt* in Nassau signified responsibility for about 24,000 inhabitants. In Prussia, there were 287 *Kreise* in 1852, each with an average population of 56,000. Subordinate to the *Medicinal Beamter*, official assistants were appointed. In Prussia, these individuals, each responsible for a district of about 8,000 inhabitants, were called *District Arzt* (District Doctors). These physicians[15] worked within the context of the *Staatsrecht* (the right of the State *vis à vis* the individual), hence *Staatsarztei* or 'State Medicine'.[16] The term 'State Medicine' was used generically to describe the conceptual administrative framework within which these State officers performed their duties. These duties were largely forensic in character, although the physicians were allowed, and sometimes encouraged, to engage in clinical practice.

The introduction of the term into England can be dated with certainty at least as early as the publication of Rumsey's essay of 1846. There, in passing, he observed that where 'the preservation of

[12] Christian Friedrich Daniel (1753–1798), specialist in forensic medicine. No mention of Daniel appears in Garrison, Castiglioni or other standard English or American texts; although he receives a brief notice in J. C. Poggendorff, *Biographisch-literarischen Handwörterbuch für Geschichte der Exacten Naturwissenschaften*, Leipzig, 1858, Bd. I.

[13] See J. H. Schürmayer, *Handbuch der Medicinischen Polizei. Nach den Grundsätzen der Rechtsstaates zu academischen Vorlesungen und zum Selbstunterrichte für Aerzte und Juristen*, Erlangen, 1848.

[14] Prussia, in 1848, was among the first states to set examinations which could be taken only before the Scientific Deputation for the Practice of Medicine in Prussia, at Berlin. These examinations, which had the reputation of being extremely severe, were made even more stringent by royal command in 1850. See Theodor Billroth, *Lehren und Lernen der Medicinischen Wissenschaften*, first published in 1876, translated as *The Medical Sciences in the German Universities*, New York, 1924, pp. 122–3.

[15] There were also district surgeons appointed by the *Kreis Chirurgus* who performed legal examinations ordered by the *Kreis Physicus*. See H. W. Rumsey, 'Observations on state medicine in Northern Germany', *Brit. med. J.*, 1867, **i**, 593.

[16] See 'State medicine in Prussia, France and England', *Brit. for. med.-chir. Rev.*, 1856, **18**, 366–87.

the health of the people is increasingly regarded as a national obligation, "State Medicine" is in the ascendant'.[17] But the events of Chadwick's tempestuous tenure at the General Board of Health suppressed any hope for its permanent acceptance by the English public. Vested pride in local self-government effectively resisted the efforts of administrative Benthamites, and would have resisted, even more strongly, the imposition of a foreign-sounding vestige of absolutism.

In 1856, two years after the abrupt end of Chadwick's career and the collapse of the General Board of Health, when the public, in the memorable phrase of *The Times*, refused to be 'bullied into health', Henry Rumsey published his compendious *Essays on State Medicine*. Rumsey proposed an agenda of concrete recommendations, based on his analysis of the systems of Germany and France. A reviewer in the *British and Foreign Medico-Chirurgical Review* placed it at the end of a long list of titles appearing from continental authors on the legal relations of medicine and the State. But it was unique in being the first English work of its kind in the nineteenth century. Rumsey ordered his suggestions under three headings:

1. Those subjects concerning into what the State should direct investigation—whether (a) statistical, (b) topographical, or (c) jurisprudential.
2. Practical arrangements for the personal safety and health of the people, requiring for their enforcement either direct legislative enactments, or local institutions and regulations, including (a) preventive and (b) palliative measures.
3. The establishment of an organised machinery for carrying into effect the aforesaid enquiries, for deliberation and advice on special arrangements and emergencies, and for the administration of existing laws. This would comprehend (a) the education of medical men and the qualification of other technical, scientific or administrative agents, and (b) the institutions of official authorities—boards and offices—for central and local superintendance and action.

Rumsey's recommendations were predicated upon the division of the country into public health districts, coterminous with registration districts, and the appointment of public medical officers, whose duties would include forensic medicine and public hygiene, who would be debarred from clinical practice and who would com-

[17] H. Rumsey, *The Health and Sickness of Town Populations*, London, 1846, p. 2.

municate with a central department of health through a system of inspectors.

The *Essays* arrived at a propitious time in British health policy, when the whole institutional structure of sanitary administration was under review. It could have exerted a profound influence on health administration. But, coming so soon after Chadwick's official demise, and the effective end of the powerful health lobby associations of the 1840s, there was no machinery to advance Rumsey's objects.[18] These objects were, moreover, so close to the heat of controversy, that only a powerful lobby would have achieved them. However, at a time when the medical profession was disunited and preoccupied with internal reform, Rumsey's administrative recommendations, with their far-reaching political implications, at first did not receive such tangible support. As events of the 1860s soon demonstrated, however, Rumsey's work offered a conceptual framework for the creation of a whole new field of medicine.

II. THE APPLICATION OF STATE MEDICINE IN THE 1860s

A. Central Health Administration

'The period 1858–1871 in State Medicine,' Professor Brockington has written, 'had both the charm and brilliance of academic life. It was a chance interlude of quiet study in a backwater of the mainstream of Government'.[19] Although this glowing testimony gives perhaps a misleading interpretation of the sanitary chaos in which State Medicine existed during the 1860s, it is fair to say that, after the fall of Chadwick, central health administration could have lapsed into obscurity but for the efforts of John Simon. The famous Medical Officer institutionalized the efforts of health reformers, and created empirically the central foundations of British State Medicine.

Following the upheaval of 1858, Simon entered, as one contemporary called it, 'a new era in matters medical and hygenical'.[20] Rumsey, for one, was hopeful of the outcome. 'We may rest satisfied', he told the S.S.A. in 1859, 'that all which central administration

[18] The book did coincide, however, with the founding of the Metropolitan Association of Medical Officers of Health, and with the creation of the first lectureships in Public Health in England at St. Thomas's Hospital.

[19] C. Fraser Brockington, *Public Health in the Nineteenth Century*, London, 1965, p. 192.

[20] *Trans. Social Science Association*, 1859, p. 121.

can effect in this country will be effected under the able direction of Mr. Simon'.[21] The State's growing acceptance of responsibility for public health had resulted in the unsystematic development of health policy within several central departments, including the Privy Council, the Poor Law Board, and the Board of Trade. Throughout the 1860s, these central departments remained largely out of touch with the local working of the sanitary system. They often had no precise idea of what the Guardians, Local Boards and Vestries were doing. Statistics from the Poor Law Board were limited to financial and vaccination returns, and even the Registrar-General's statistics were often delayed. The *British Medical Journal* sadly observed that 'laws had been passed, but no one knew how they worked, or even if they worked at all'.[22]

To this situation, Simon brought an inimitable combination of personal talents. He embodied the skills of a scientific clinician and practical epidemiologist with the dedication of a responsible civil servant and the skill of a reforming politician. Moreover, he entered central government at a time when circumstances were conducive to quiet but profound reforms.[23] Simon's major accomplishments during the 1860s included the creation of a Medical Department of seventeen brilliant Inspectors who opened up the field of health administration; and the promotion of fundamental research, directed, unlike the Agriculture Office or the Board of Trade, to immediate objectives, but to the study of unknown causes of disease and physical behaviour. He also succeeded in mobilizing professional medical opinion, through the Epidemiological Society and the British Medical Association, in support of over fifteen public statutes; and in beginning an eloquent series of *Annual Reports* which investigated both immediate and far-reaching questions of public health policy. Finally, Simon succeeded in establishing, during the same decade, a context in which he, as a specialist officer, enjoyed direct access to the Minister—a position long sought by his successors, but not achieved again in substance for over two generations.

Simon's work at the Medical Department was undoubtedly

[21] Ibid., p. xxviii.
[22] *Brit. med. J.*, 1869, i, 578.
[23] See R. J. Lambert's definitive *Sir John Simon, 1816–1904, and English Social Administration*, London, 1963.

assisted by the circumstances of his appointment. He was fortunate to be under the semi-autonomous environment of the Privy Council, 'the potting shed for new administrative plants',[24] which gave him a certain administrative and financial latitude[25] and a convenient access to politicians outside his own department. Moreover, he enjoyed close personal friendships with Arthur Helps and Robert Lowe, respectively the Clerk and Vice-President of the Council. Most important, however, was Simon's own extraordinary capacity to make irrefragible sanitary sense. His almost unique position in the civil and professional world gave him a kind of expertise which prevailed time and again over the Treasury. As Ralph Lingen, Permanent Secretary to the Treasury once admitted when confronted by a proposal of Simon's, 'I do not know who is to check the assertions of experts when the Government has once undertaken a class of duties which none but such persons understand'.[26]

With some justice, the Medical Department was denounced by one irate observer—Florence Nightingale in spirit if not in fact—as 'the tightest little berth in Christendom'.[27] But one of its most important accomplishments was the close personal connection Simon forged between Government efforts in practical health administration and fundamental research, and the public activities of the medical profession. As Royston Lambert has written, where the Epidemiological Society and the Medical Officers of Health Association began, and the Privy Council Medical Department ended, was never quite clear.[28] This relationship was at the core of Simon's conception of State Medicine, which was really 'Preventive Medicine', involving professional men working within the context of governmental supervision, guidance and initiative. His was not the eighteenth century's scheme of medical police, nor Chadwick's scheme of sanitary police, nor was it Rumsey's scheme with its legal connotations. It was instead, a pragmatic, British alternative.

Although Simon's accomplishments for State Medicine during the

[24] K. B. Smellie, *One Hundred Years of English Government*, London, 1937, p. 90.

[25] Cf. the description of the use Simon made of his Inspectors' annual appointments under Clause 4 of the 1858 Act which enabled him to expand his department in amoeboid fashion for over seventeen years. Lambert, op. cit., pp. 361–3.

[26] T.1/7129A/18953/1871, Lambert to Clerk, 1 June 1871.

[27] Lambert, op. cit., p. 314.

[28] Ibid., p. 315.

1860s were significant in principle, they must not be overrated as achievements in administrative theory or practice. For instance, he was unsure himself how best to achieve a rational, locally-based health service for the nation. The great strides of public health legislation between 1864–1868, which covered the ground of his Reports were not systematic attempts to mould a new system. Simon seized chance opportunities; indeed, he may have seized too many, as the resistance which grew against the Contagious Diseases and the Vaccination Acts later demonstrated. The point is emphasized in what was perhaps the chief legislative success of the Department, and the best illustration of Simon's political ability, the Sanitary Act of 1866. In that year, cholera, the time-honoured harbinger of reform, again invaded Britain. Simon's response was to focus all his efforts upon an Act designed to extend the sanitary provisions of the Act of 1848 to all health authorities, to include almost any insalubrious house or building or workshop under the definition of nuisance, and to compel all nuisance authorities to inspect their districts and exercise their legal powers of constraint. The Act, in Simon's classic phrase, endowed sanitary legislation with the 'novel virtue of an imperative mood',[29] and set a milestone in health administration. But, like Simon's achievements generally, it was opportunistic. It soon became apparent that the gain was in principle, not in practice. As the decade neared its close, much still remained to be done to create the 'health consciousness' which Simon had identified, and the administrative machinery which Rumsey had so long desired.

B. *Professional Developments*

The Act of 1866 was greeted by Rumsey as '. . . the commencement of a new era in sanitary legislation'.[30] In 1867, Rumsey himself set out to inaugurate a professional contribution to this 'new era', in an address on State Medicine to the British Medical Association at its annual meeting in Dublin.

It was logical for a meeting on State Medicine to be held under the auspices of the British Medical Association. Since the first annual meeting of the Provincial Medical and Surgical Association

[29] John Simon, *English Sanitary Institutions*, London, 1890, p. 300.
[30] *Journal of Social Science*, 1865–6, p. 661.

in 1833, Sir Charles Hastings had stressed the importance of 'public medicine' which, by definition, began with inquiry into the causation and prevention of diseases, and included the registration of births and deaths, vaccination, medical attendance on the sick poor, and medical reform. As early as 1837, the British Medical Association had resolved to take a role in politically promoting what T. W. Grimshaw called 'preventive medicine', including all questions of public health coming before Parliament, and the subject of health administration had been constantly, if erratically, renewed from time to time ever since.[31]

Thus, in 1840, the British Medical Association, with the Epidemiological Society, played a significant role in abolishing innoculation by Act of Parliament, and later in introducing compulsory vaccination. The Medical Act of 1858 placed the profession as a whole in a new relation with the State. In 1860, Dr. John Symonds moved a prophetic resolution asking for a Committee to consider the 'present position of medical practitioners in respect of medico-legal investigations and to confer on the expediency of impressing upon the legislature the appointment of State physicians, whose duties might embrace both medico-legal inquiries and the care of the public health'.[32] During the same decade, the British Medical Association was beginning to test its growing strength as a professional pressure group. As Ernest Hart reminded the 3,000 members of the British Medical Association in 1867, 'the political importance of the Association cannot be over-estimated, if its political action be exerted with vigour, with disinterestedness and with discretion'.[33]

It was also fitting for Rumsey's inaugural remarks on State Medicine to be made in Dublin. There was, firstly, a long-standing tradition of medical interest in preventive medicine[34] in Ireland. In 1841, the first chair of hygiene in the British Isles was established

[31] See Alfred Carpenter, 'The early work of the Association in Preventive Medicine', *Brit. med. J.*, 1882, **ii**, 265.

[32] *Brit. med. J.*, 1879, **ii**, 245.

[33] *Brit. med. J.*, 1867, **i**, 199.

[34] See K. Dewhurst, 'The Genesis of State Medicine in Ireland', *Ir. J. med. Sci.*, 6th series, 1956, 365–84.

at the Royal College of Science at Dublin.[35] Its first Professor, and a leading advocate of a socially-conscious medical profession, was Dr. Henry Maunsell, author of the influential essay on *Political Medicine*,[36] and one of the first Irish representatives to attend the Provincial Medical and Surgical Association. Moreover, Rumsey had close personal ties with Dublin through his friendship with William Stokes, Professor of Medicine at Trinity College, Dublin, who had been Crown Representative for Ireland to the General Medical Council since 1858, and who was actively concerned about the public role of the medical profession.[37] Finally, Ireland provided a philosophically congenial meeting place for State Medicine discussions because the centralized administrative structure of Ireland was potentially much more conducive to the introduction of systematic government medical schemes than was the decentralized local government of England. Through the dispensary system, Irish medical reformers had the experience of conducting administrative experiments in what W. L. Burns later called Britain's 'social laboratory'[38]—experiments which were sometimes later translated into the administrative framework of England and Wales.

It was against this background that Rumsey opened his address: 'Not many years ago, State Medicine in England was treated by well-informed people as a mere idea, a speculative theory, or at best, a German innovation. Any formal view of the subject was liable to be met by taunt and ridicule. Able critics told us that we "might as well have a State Astronomer, or a State chemist'[39] . . .

[35] Henry Maunsell filled the chair from 1841–6. The post fell vacant between 1847–1863, but was subsequently held by Edward Mapother (1864–8) and Charles (later Sir Charles) Cameron (1868–1921). See J. Fleetwood, *The History of Medicine in Ireland*, Dublin, 1951, p. 97.

[36] H. Maunsell, *Political Medicine, being the substance of a Discourse lately delivered before the Royal College of Surgeons in Ireland on Medicine considered in its Relations to Government and Legislation*, Dublin, 1839.

[37] William Stokes (1804–1878), President of the Royal Irish Academy; LL.D. Cambridge, 1874; the third Irishman to receive the Prussian *Order pour le Mérite;* second cousin of George G. Stokes, President of the Royal Society. See W. Stokes, *William Stokes: His Life and his Work*, London, 1898.

[38] Thus 'the most conventional of Englishmen were willing to experiment in Ireland on lines which they were not prepared to contemplate or tolerate at home'. W. L. Burn, 'Free Trade in Land: An Aspect of the Irish Question', *Trans. R. hist. Soc.*, 4th series, **31**, 68.

[39] H. Rumsey, 'Remarks on State Medicine in Great Britain', *Brit. med. J.*, 1867, **ii**, 197.

Over the previous ten years, he said, by a combined process of internal autonomous developments and external social pressure following in the wake of cholera, the fragmentary system of medical advisers to Local Authorities and Courts of Law had rapidly expanded. At the same time, the machinery of sanitary law had become steadily more difficult to operate. The areas and jurisdiction of Local Authorities still overlapped confusingly. Britain was still without records of national sickness, or any current account of local death rates. At the centre, the registration of mortality was still kept administratively separate from the prevention of disease. Even more disheartening was the fact that mortality rates had failed to improve significantly between 1848–1867. This seemed to suggest that early Chadwickian reformers had been too optimistic about the effect of sanitary measures upon the community health. More attention had yet to be given to the larger questions of housing, nutrition, food and drugs, and to such diseases as diphtheria and tuberculosis. Now, Rumsey told the British Medical Association, the 'demand for the aid of physicians and surgeons in various departments of civil and legal duty has so greatly increased, the force of circumstances, in fact, has been so irresistible—that the public, as well as the Government and the medical profession itself, were compelled to inquire into the principles and methods on which this growing department of medicine is to be most effectively worked . . .'[40] As one reviewer added, 'It is not reform that is needed, but a new organisation altogether, one founded on well-established principles, and as much as possible on exact science'.[41]

The content of Rumsey's 1867 address was influenced by his appreciation of the need to improve the collection of national statistics. Rumsey himself wrote that he acted in response to a request from Arthur Ransome, who had asked him to sponsor a resolution supporting some of the improvements in death certification urged by William Farr's 27th Annual Registrar General's Report. A principal feature of Farr's plan was the appointment of a specially qualified 'Registration Medical Officer' for every registration district of the kingdom, to rationalize the diffuse and unsystematic mortality statistics into a comprehensive and uniform

[40] Ibid.
[41] Ibid., p. 137.

scheme. This plan coincided with Rumsey's plan of 1856 to build up from the local officer of health a complete system of sickness registration, accurate death certificates, medico-legal investigation, and scientific advice. These objectives, Rumsey said, are 'comprehended in, and constitute, the main elements of what we call State Medicine'.[42]

The medical press welcomed Rumsey's remarks, which seemed to encapsulate the sentiments of a growing Victorian 'sense of fact'. In the same year, Edwin Lankester, the stormy petrel of popular science, published his *Notes for a History of Sanitary Legislation* which described the slow, *ad hoc* manner in which sanitary administration had developed,[43] and A. P. Stewart and E. Jenkins together published the first edition of their *Medical and Legal Aspects of Sanitary Reform* which bitterly criticized the existing state of affairs. As one medical journalist wrote: 'In relation to sanitary science, a new era seems to be opening. Never has there been a time when the public attention has been more directed to all that concerns the health of the people, and never has the Government exerted itself more to meet the existing wants'.[44]

In part, Rumsey's organizational scheme complemented the efforts which, since 1858, Simon had been discovering empirically. It was not open to Simon to direct officially the development of the health profession from his offices in Richmond Terrace; but it was not necessary. Rumsey and the British Medical Association assumed the task of organizing a powerful professional campaign. Unlike the public health movement of the 1840s, the major organizational impulse for the State Medicine movement of the 1860s and 1870s was generated by the medical profession itself.

This movement was well under way by 1868, when, following the resolutions in favour of Farr's scheme at the Dublin meeting, a British Medical Association Committee of Council on State Medicine was formed, under the chairmanship of Professor Henry W. Acland, Regius Professor of Medicine in Oxford. It began immediately to study the possible amendment of the sanitary laws, to gain the co-operation of the Social Science Association, and to

[42] Ibid., p. 197.
[43] See the brilliant review in 'Histories of medicine', *Brit. for. med.-chir. Rev.*, 1872, **50**, 430–7.
[44] 'Papers on Sanitary Science', *Brit. for. med.-chir. Rev.*, 1867, **40**, 60.

invite representatives from local medical societies. The committee of fifteen included Edwin Ray Lankester, Rumsey, John Symonds, Edward Mapother and Alexander Stewart.[45] The same group or 'network' formed and reformed time and again under different labels, until it became virtually a pressure lobby in itself.

In May 1868, it formed a joint Committee with the S.S.A. 'to promote a better administration of the Laws relating to Registration, Medico-Legal Inquiries, and the Improvement of Public Health'.[46] This joint Committee sent a Memorial to the Privy Council, urging steps to improve the registration system and to rationalize local health administration. This Memorial conveyed a growing sense of urgency. Outside of London, there were only ninety-two officers of health, and these were either overworked, under-employed, underpaid or powerless. Moreover, there was no guarantee whatsoever of their competence, as the Joint Committee observed, 'the amount actually disbursed under the present disjointed and very inefficient system would go far to maintain a sufficient staff of specially trained and highly qualified district scientific officers with inspectorial functions. Without such officers, it is vain to expect any material improvement in this important department of public service.'[47]

The Committee requested a Royal Commission to inquire and report on the existing manner of death registration, the operation of sanitary laws, and the place of health officers in their administration. In May 1868, a deputation from the Joint Committee visited the Duke of Marlborough, the Lord President of the Council; the Earl of Devon, President of the Poor Law Board; and the Right Hon. Gathorne-Hardy (later Lord Cranbrook), Home Secretary. Its

[45] (a) John Addington Symonds (1807–1871), M.D.Edinb., 1828; F.R.C.P., 1857; F.R.S.Edinb.; practised privately in Bristol 1831–69; lectured in forensic medicine, Bristol Medical School, 1832–6; lectured on the practice of medicine, 1831–45. (F. Boase, *Modern English Biography*, Truro, 1897, vol. III, col. 858).

(b) Edward Mapother (1835–1908), M.D.Dublin, 1857; F.R.C.S.I., 1862; first Medical Officer of Health of Dublin; professor of hygiene and anatomy in Royal College of Surgeons of Ireland; settled in London, 1888. (See J. S. Crone, *A Concise Dictionary of Irish Biography*, London, 1928, p. 150).

(c) Alexander Patrick Stewart (1813–1883), M.D.Glasgow, 1838; practised privately in London, 1839–83; lectured on materia medica, Middlesex Hospital; retired 1866; author of *Sanitary Economics, or, Our Medical Charities as they are and as they ought to be* (London, 1849). (F. Boase, op. cit., col. 748).

[46] *Brit. med. J.*, 1868, i, 489.

[47] Ibid.

particular proposal for a medical service aroused some comment and criticism among the profession. W. T. Gairdner, Medical Officer of Health for Glasgow, objected to the emergence of full-time non-clinical health officers who might unalterably divide the medical profession into two estates. On philosophical grounds, Gairdner saw 'the idea of State Medicine as one more proper to Dr. Rumsey than to the deputation'. But Rumsey, with the support of Ernest Hart and the Committee, prevailed. Some months after this interview, a Royal Commission, later to become famous as the Royal Sanitary Commission, was formed.

In the meantime, in June 1868, a Committee on State Medicine was appointed by the General Medical Council to report on 'the steps proper to grant Diplomas or Certificates of Proficiency in State Medicine, and for recording same in the Medical Register'.[48] Under the guidance of Henry Acland, the Committee included the ubiquitous Rumsey, Robert Christison (the brilliant Scottish authority on medical jurisprudence), George Paget, Edmund A. Parkes, and William Stokes, and others of the 'State Medicine' network. Their research was conducted by questionnaire and correspondence with twenty-nine British and six Continental authorities. Five questions were asked, concerning the character, timing and sequence of a recommended course. From the respondents, including Guy, Farr, Lankester, Letheby and Maudesley, it was clear that State Medicine, academically speaking, was still in a very primitive state.

British respondents could point to few texts suitable for new students. Most recommended Tardieu's *Dictionnaire d'Hygiène publique et de Salubrité*, Michel Levy's *Traité d'Hygiène publique et privée*, Taylor's *Medical Jurisprudence*, and Baker's *Laws of Health*. Maudesley felt that John Stuart Mill's *System of Logic* would prepare the candidates' faculties of reason. But most agreed with Taylor, that 'the work has still to be written which should be made a textbook for students of State Medicine'.[49] It is significant that neither eighteenth-century treatises on medical police, nor contemporary German works were recommended. If State Medicine

[48] *Resolutions of the General Medical Council adopted 9 and 12 July, 1869, and Second Report of the Committee on State Medicine of the General Medical Council*, 1869, p. 1.
[49] Ibid., p. xxi.

had been hitherto half French and half German, the Committee seemed determined to make its new counterpart fully British.

They felt they had ample justification for acting in this manner. For, as Rumsey found, 'the practical results of German State Medicine, do not appear to be always commensurate with the professional completeness of the organization'.[50] German district physicians, combining sanitary and forensic work with medical practice, constantly found themselves in a false position whenever their private cases became subjects of official inquiry. In evidence before the General Medical Council Committee, Professor Varrentrapp of Frankfurt a.M. admitted that while the *KreisPhysicus* and *Districtarzt* whom Rumsey admired were good legal officers, 'public health is in Germany very much neglected—till now, comparatively to forensic medicine, more neglected than in France, and by far more than in England'.[51] Despite the long-established tradition of State Medicine in Germany, it was only in 1867, at its 41st Annual Meeting, that the Congress of German Naturalists and Physicians formed a section devoted to public health. Several German periodicals of forensic medicine were in existence, but only a fraction of their total space was devoted to public health. Public Health as an independent subject was taught in only three Bavarian universities —Munich, Würzburg and Erlangen. The Prussian universities had thus far ignored it. In consequence Dr. L. Pappenheim of Westphalia said, 'Many sanitary officers are unacquainted with the principles of national economy, with *Staatsrecht* and with *Polizeiwissenschaft*, and a great many theoretical schemes of preventing disease or promoting health, clash with important rights or interests.'[52]

[50] Ibid., p. 96. The facts warranted the cautious words of one medical writer in 1856: ' . . . it is not always to be supposed . . . that because we find laws promulgated they are always executed. There are many continental towns enjoying the advantage of very wise and hygienic laws, but where the want, as Mr. Rumsey observes, of English capital and energy have prevented the application of engineering skill to the execution of great works of purification'. *Brit. for. med.-chir. Rev.*, 1856, **18**, 383.

[51] *Report of the General Medical Council Committee*, p. 54. The forensic emphasis of German state medicine is reflected in the format of the *Vierteljahrsschrift für Gerichtliche und Öffentliche Medizin*, which was transformed from the earlier *Wochenschrift für die gesamte Heilkunde* by Johann Ludwig Casper of Berlin in 1852. The journal altered its title from 'offentliche medicin' to 'öffentliche sanitätswesen' in 1872. It is now the *Deutsche Zeitschrift für die gesamte Gerichtliche Medizin*.

[52] *Report of the G.M.C. Committee*, pp. 58–9.

Moreover, Varrentrapp told his British colleagues, German experience had shown 'public health must be kept separate from Forensic Medicine'. 'Government alone cannot forward public health', he said. 'Local Authorities, medical corporations or societies must do the greatest part'. Pettenkofer shared this sentiment, and expressed envy that the British practical work in preventive medicine under Simon's leadership was so fully 'in accordance with the spirit of the age. I am anxious that England should so far outstrip my German Fatherland in this matter.'[53]

On the basis of this evidence, the Committee recommended that a special certificate should be awarded for State Medicine; that State Medicine should be considered to include legal medicine (medical jurisprudence) and preventive medicine (or public hygiene); that it not be compulsory on all practitioners and that no practitioner should take a State Medicine qualification without taking a medical qualification first. The Committee further decided to insert the State Medicine qualification into any future Medical Bill to go before Parliament. The Committee followed Rumsey's recommendations in declining to empower the Colleges of Physicians and Surgeons to institute exams and grant diplomas. Unlike the new disciplines of engineering, the new discipline of State Medicine was to be university bred, thus strengthening the institutional connection between the State and the universities that the Civil Service Commissioners had already begun.

The second major outcome of the Committee was the formation of the Royal Sanitary Commission, in November 1868. The Sanitary Commission was not everything that the Committee had wished. Disraeli's Government excluded the coroner and registration systems from its terms of reference, and ironically, in view of the Committee's own history, excluded Scotland and Ireland from its investigations. The first Commission included Rumsey, but he was excluded from the Commission when it was re-appointed in April 1869, by H. A. Bruce (later Lord Aberdare), the Liberal Home Secretary. The new Commission included two peers, seven M.P.s, two engineers, three lawyers and five doctors. Of the five doctors, three (Acland, Christison and Stokes) were in the reforming party, but the result was disappointing.

[53] Ibid., p. 66.

Both Rumsey and Simon gave evidence before the Commission. Rumsey reiterated his desire for a national system of medical officers based on registration areas. Simon, however, gave little help to Rumsey by declining to make precise suggestions about the organization of local health. Under the circumstances, the local administration upon which State Medicine depended was left without fundamental change. It remained in an invidious position, partly enjoined with the poor law administration, and partly competing with it for powers and resources.

That the Commission's Report provided the basis for British health legislation and administration for the following half-century is well known; what is less known, but equally important, is the fact that it shaped the future of the public health profession. The Commission in its Report of 1871, endorsed the need for special courses to equip medical officers in the practice of State Medicine. But, in defining State Medicine as 'the application of the physical and medical sciences to the preservation of the health of the community',[54] they turned away from Rumsey's legislative and administrative definition, and moved more towards Simon's empirical concept of 'Preventive Medicine'.

By the early 1870s, the concept of State Medicine had been greatly altered in both form and substance. There was still a strong emphasis on vital statistics, but the continental emphasis on jurisprudence and administrative law, which had been adapted and modified by the Chadwickians, had been replaced by a characteristically British emphasis on hygiene, and a clear separation had begun to develop between the duties of the health officer and the practice of curative medicine.

III. THE INSTITUTIONALIZATION OF STATE MEDICINE, 1870–1900

During the 1870s, the State Medicine movement generated in the 1860s crossed several major watersheds and the last quarter of the century were years of consolidation and elaboration. Legislatively, the Public Health Act of 1875 consolidated the measures recommended by the Royal Sanitary Commission, and left the field to further specialist and consolidating legislation. The attention of Parliament turned away from public health administration and pro-

[54] *Second Report of the Royal Sanitary Commission*, 1871, p. 61.

ceeded to measures which required no such controversial ministerial arrangements. Administratively, the resignation of Simon in 1876 left the extension of his health policies to his successors, George Buchanan, Richard Thorne Thorne and William Power. As recent research has shown, the traditions and administrative needs of the growing Whitehall establishment of the 1870s and 1880s did not allow for an entrepreneurial 'superintendent-general of health'.[55] Amid competing claims on public expenditure, central 'State Medicine' fared badly, until the revelations of the Boer War again focused attention on the lamentable state of the nation's health.

Partly owing to the legislative and administrative circumstances, the initiative in State Medicine during this period largely passed from central government to new institutional groupings within the profession. Following the efforts of Rumsey and Acland, a permanent section of the British Medical Association was established for 'Public Medicine' in 1871. Alexander Stewart was its first President. The name of the section itself revealed the character that professional State Medicine had assumed. It had become, not a State service, devoted to the relief of the sick poor, but a 'public service' concerned primarily with environmental health and vital statistics.[56] Although some participants rightly held that the poor law District Medical Officers had an equal claim to a place in any national system of State Medicine,[57] most intended that the professional service should be dissociated completely from the taint of the Poor Law Medical Service. In general terms, therefore, the Section did not initially extend itself to questions of medical care. Instead, through the Joint Committee on State Medicine, it looked to the continuing reform of sanitary law and to the eventual establishment of what Sir Arthur MacNalty has called 'the apotheosis of State Medicine'—a Ministry of Health.[58] Secondly, through its own social auspices, it strove to

[55] See R. MacLeod, 'The frustration of State Medicine, 1880–1899', *Med. Hist.*, 1967, **11**, 15–40.

[56] This definition became accepted usage in the United States. See Harry S. Pearse, 'State Medicine', *Med. Rec., N.Y.*, 2 July, 1898, pp. 13–15.

[57] For a description of one Victorian attitude towards State Medicine when it implied curative services, see Milton Terris, 'Hermann Biggs' contribution to the modern concept of the Health Center', *Bull. Hist. Med.*, 1946, **20**, 397.

[58] Sir Arthur MacNalty, 'The history of State Medicine in England', Lecture 4, *Jl. R. Inst. publ. Hlth. Hyg*, 1948, **11**, 64.

achieve the professional and educational recognition which it felt belonged to the practice of public health.

In 1872 William Stokes as President of the Section attempted to define the role of the new 'speciality'. 'State Medicine', Stokes said, comprised legal and 'preventive medicine', and 'preventive medicine' in turn embraced virtually the whole of sanitary science, the influence of sex, age, birth, life, sickness and death.[59] The section quickly realized the significance of this definition. Under the Public Health Act of 1872, all Local Authorities were permitted, and all Borough Health Authorities were required to appoint 'qualified' Medical Officers of Health. Edward Seaton later claimed that the Act, which, 'despite much criticism gave such an important part in sanitary administration to scientific experts', accomplished 'a peaceful revolution . . . which has done more for the greatest happiness and welfare of the greatest number of this country than all the political subversions recorded in our annals.'[60]

At the time the Act was passed, however, it was clear that scientific experts of the necessary kind did not exist in sufficient quantity. Rumsey told the British Medical Association in 1873, that 'Local Authorities were quite incapable of themselves to judge of the comparative fitness of candidates for office',[61] and W. H. Michael and A. P. Stewart described the folly of entrusting 'powers of such vital importance, requiring great knowledge, experience and tact, to persons in no way qualified either for organisation or execution, possessing no special aptitude or training and aided by no intelligent or consistent guidance in carrying on the sanitary work of the country.'[62]

There were, moreover, many lingering doubts about the limits of State Medicine. As de Chaumont observed

> State Medicine has been written about, talked about and quarrelled about, but it has rarely been explicitly defined, and to many it conveys no very distinct idea. It has been confounded with Public Health, and

[59] W. Stokes, 'On State Medicine', *Brit. med. J.*, 1872, **i**, 385.
[60] 'The evolution of local sanitary administration', *Brit. med. J.*, 1891, **ii**, 285.
[61] *Brit. med. J.*, 1871, **ii**, 233–5. The Joint Committee, which by 1871 numbered 31, comprised an elite of health officers, including W. Budd, David Davies, W. T. Gairdner, E. Hart, A. P. Stewart, E. Lankester, Mapother of Dublin, A. Ransom, W. Strange, and virtually every name familiar to the historian of Victorian social medicine.
[62] *Brit. med. J.*, 1873, **ii**, 227.

generally much misunderstood, the part being frequently taken for the whole, and the wider scope of its action but little apprehended. It induces the questions of public health and hygiene, general, special and individual, but its own appropriate province is such general control as will determine the several specialities in the directions most fitted for the well-being of the community. In fact, we succinctly define State Medicine to be, in quasi-legal phraseology, 'the office of the Sanitarian promoted by the State'.[63]

To provide the body of 'Sanitarians' needed by the State, the Section began efforts to educate them.

In April 1870, Stokes urged the General Medical Council to register the new qualification in State Medicine, and, in August, a Committee on Qualification in State Medicine was appointed under Dalrymple, Hastings, Parkes, Ransome and Hart.[64] In 1875, Stewart and Carpenter reported that they had designed a diploma course, and hoped that within five years qualified men in sufficient numbers would be available for official appointments. The course required 'an adequate knowledge of *Legal Medicine* and Medical Jurisprudence, and of Preventive Medicine, or Public Hygiene, comprehending *Medical Police* and the management of medical institutions supported by national or local taxation.'[65] In the meantime, Stokes had established the kingdom's first diploma course in State Medicine in Dublin in 1870. Stoke's course comprised papers on sanitary law, engineering, vital statistics, meteorology, pathology, chemistry and medical jurisprudence,[66] and was open to doctors of medicine of the universities of Oxford, Cambridge and Dublin who wished to study 'the application of Medical Science to the health

[63] F. S. B. de Chaumont, *Lectures on State Medicine*, London, 1875, pp. 5–6. In the first edition of the *Index Medicus* in 1879, State Medicine was defined as comprising medical ethics, medical education, hygiene and public hygiene, military and naval hygiene, medical jurisprudence and toxicology, and veterinary medicine. By 1895, the *Index Medicus* extended its definition to include hospitals, occupations, epidemics, schools, food and drugs and

[64] 'The G.M.C. on education and registration', *Brit. med. J.*, 1873, **i**, 407; *Brit. med. J.*, 1873, **ii**, 198, 472.

[65] 'Report of the Committee of Council upon the subject of "State Medicine Qualification" ', *Brit. med. J.*, 1875, **ii**, 244–5.

[66] 'But not, surprisingly, a course in the use of the microscope', *Nature, Lond.*, 22 June 1871, p. 138.

and well-being of the masses'.[67] In the same year, Dr. William Guy was appointed the first lecturer in Hygiene in King's College, London. In his lectures, Guy oriented his course in State Medicine towards the practice of 'preventive medicine'. Indeed, he felt that this was the area where the British contribution had been greatest, and where its pursuit was, for the medical practitioner, most relevant.[68]

This emphasis upon hygiene, as reflected in the definitions given by Stokes and Guy was also embodied in the new programme for State Medicine begun by G. E. Paget and the Board of Medical Studies at the University of Cambridge in January 1870. The manner in which the University Syndicate expressed its policy was indicative of the way in which the institutionalization of the new profession had proceeded:

> The scientific knowledge of these subjects (Preventive Medicine and other branches of State Medicine) has of late been largely extended, and they have excited and are excited and are exciting more and more public interest. Most of them have been the subjects of Parliamentary legislation, and have become the business of Government departments. In connection with them, some important offices have long existed. Many others, chiefly connected with Preventive Medicine, have been recently created, and the number of these will probably be largely increased.
>
> It is therefore of importance to the Community that some steps should be taken for providing persons more thoroughly qualified for the performance of these duties.
>
> It seems also desirable that means should be provided for testing their fitness and certifying it, so that the Government, or some other authority, may have some guide to the choice of the fittest persons.[69]

The first examination under the programme at Cambridge was held in October 1875. Twenty-six candidates presented themselves and twenty were approved for the Diploma of Sanitary Science, Cam-

[67] 'Memorandum in reference to the Establishment of Qualification of State Medicine in the University of Dublin, January, 1870', as quoted in H. W. Acland, *National Health*, London, 1871, Appendix B, p. 119. W. Stokes, 'On State Medicine', *Brit. med. J.*, 1872, i, 385. There was, however, no prescribed curriculum, and no fees were charged for the exams, as it was considered that enough had been paid in obtaining the ordinary degrees in medicine and arts. 'The G.M.C. on education and registration', *Brit. med. J.*, 1873, i, 407. By 1874, the Royal Dublin Society had begun a Public Health lecture series. *Brit. for. med.-chir. Rev.*, 1875, **54**, 1689.

[68] W. A. Guy, *Public Health*, London, 1876, p. 216.

[69] 'Report of the Board of Medical Studies', *Cambridge Reporter*, 26 January 1857, pp. 186–7.

bridge. The first graduates included names which have become classic in the field of Public Health practice, including Alfred Carpenter, G. S. Fosbroke, and A. S. Underhill. Because Cambridge became, and was for years, the largest producer of new Medical Officers of Health, averaging ten to fifteen per year until well into the present century, it played a major role in staffing the 1800-odd sanitary authorities in England and Wales.

The contents of the Cambridge course give an instructive insight into the way in which 'professional' State Medicine took academic form. Papers were set in two parts, the first dealing with chemistry and analysis, fields in which most early Medical Officers of Health were sadly ill-equipped. Part II consisted of two parts, one dealing with sanitary statistics, the other with areas of practical importance, including disinfection, vaccination, epidemic diseases, the construction of hospitals, and the inspection of factories, mines, workshops and common lodging houses. The course involved fieldwork exercises in which candidates were sent to inspect and report on different areas of Cambridge. The set reading was considerable and very up to date; almost all the texts had, in fact, been written within the preceding ten years. The list included Parkes' *Manual of Public Hygiene*, George Wilson's *Handbook of Hygiene*, Hart's *Manual of Public Health*, the *Army Medical Reports*, the Registrar-General's Reports and the *Medical Reports* of the Privy Council and the Local Government Board. In addition students were required to read Hassell's work on atmospheric pollution, and Florence Nightingale on hospitals.

It is significant that in courses ostensibly devoted to State Medicine, the textbooks recommended by Rumsey, particularly the German textbooks, were conspicuously absent. Developing slowly at Cambridge, and later to develop at the Victoria University (Manchester), Durham, Oxford, Birmingham, Liverpool, Leeds, Sheffield, Bristol and London,[70] were courses in what came to be known as 'Public Health and Hygiene' suitably recognized by the characteristic qualification 'D.P.H.'. This diploma, rather than a certificate in State Medicine, became the educational hallmark of

[70] In 1875, the London diploma was put on an equal footing in the university syllabus with medicine and surgery, through the efforts of Dr. George Buchanan and Dr. Parkes. In addition the ordinary M.B. examination had papers in medical jurisprudence and hygiene, *Lancet*, 1875, i, 131.

British, and later American and Commonwealth public medicine.

The remaining decades of the century saw the further development of specialist instruction for Sanitary Inspectors, the establishment of professional registration and the development of specialized professional institutions. In 1886, after a number of abortive attempts,[71] State Medicine was introduced into the new Medical Act, through the exertions of the new Public Health Medical Society and the efforts of Lyon Playfair.[72] Once the registration of Health Officers was accomplished, specialists multiplied and the new non-curative branch of the medical profession emerged. In 1886 it numbered 263; by 1900, nearly 700. The new discipline was by no means uniform. Some health officers were full-time, others were part-time; some held posts with several Local Authorities, others with only one. But all had in common the experience of moving away from clinical medical practice and establishing themselves in a new scientific discipline.

In 1888 this trend was emphasized in the establishment of the Society of Medical Officers of Health, and the inauguration of the College of State Medicine, to train men, in lectures and laboratories, 'to guard the public health or to fill any of those offices which require sanitary knowledge'.[73] Developments in the early 1890s testified to the increasingly technical character of the new profession. In 1891, the Congress of International Hygiene was held in London, during which the Public Health Society, meeting in the State Medicine

[71] See, e.g., the *Bill to enable Legally Qualified Medical Practitioners to hold Public Medical Appointments and to Amend the Medical Act*, 1876, (81). v. 37.

[72] See the Medical Act 1886 (49–60 Vict.c.48), Part II, ch. 21: 'Every medical practitioner to whom a diploma for proficiency in sanitary science, public health or state medicine, has after special examination been granted by any college or faculty of physicians or surgeons, or university in the United Kingdom, or by any such bodies acting in combination, shall, if such diploma appears to the Privy Council, or to the General Council, to deserve recognition, in the Medical Register, be entitled, on payment of such fee as the General Council may appoint, to have such diplomas entered in the said register, in addition to any other diploma or diplomas in respect of which he is registered'. The debate surrounding the introduction of State Medical qualification by the 1886 Act is curiously missing from Charles Newman, *The Evolution of Medical Education in the Nineteenth Century*, London, 1957.

[73] M.H. 25/27. James Cantlie, Hon. Sec., College of State Medicine, to President, Local Government Board, 17 October 1887. For the history of the College, see the MS entitled, 'College of State Medicine, 2 May 1888', preserved in the archives of the Lister Institute of Preventive Medicine; see also *The Times*, 3 May 1888, and R. Brudenell Carter, 'The aims and objects of State Medicine', *Publ. Hlth, Lond.*, 1888, **1**, 109–11.

Department at King's College, London, reconstituted itself as the British Institute of Public Health.[74] In 1892, the journal *Public Health* and the *Journal of State Medicine* were begun and a new set of professional goals materialized. The broad view of public service they represented was fittingly described by the latter in its definition of State Medicine as 'that department of medical science which deals with subjects upon which a medical man may be consulted by the executive or the legislative Government of the country'.[75] Perhaps one of the most significant reminders of the new professionalism came in 1897, when the British Medical Association changed the title of its 'Public Medicine' section to 'State Medicine' and began actively to include poor law medical questions in its agenda.[76]

These trends towards professionalism inevitably involved the danger of overspecialization. On the one hand, the clinical side of medicine was being neglected by the new 'State doctors' who were in turn alienated from the majority of the medical profession. As early as 1878, the *British Medical Journal* tried to explain the poor attendance at the Public Medicine Section at its annual meeting in Bath, by saying that: 'Discussions on such subjects as the construction of infectious hospitals, the condemnation of unsound meat, and the registration of disease, however interesting they may be to Medical Officers of Health, can scarcely vie for attractiveness to the great body of men with the more burning questions which were offered for debate in the programmes of other sections.[77] By the end of the century, Sir Walter Foster was expressing the gathering fear that medicine was in jeopardy of dividing itself into two nations, one concerned with the cure of disease, the other with the preservation of health. And George Wilson, Medical Officer of

[74] *J. St. Med.*, 1892, **1**, 4–6. The B.I.P.H. became the Royal Institute of Public Health in 1905 and the Royal Institute of Public Health and Hygiene in 1937. The B.I.P.H. in Russell Square became the first central institution in London for 'the training of medical practitioners, desirous of obtaining the D.P.H.' It should not be confused with the Royal Sanitary Institute (now the Royal Society for the Promotion of Health), which performed the parallel function of training non-medically qualified sanitary inspectors.

[75] *Journal of State Medicine*, I (1892), 36–9. The journal was edited by successive lecturers in Hygiene and Public Health at King's College, London, beginning with Professor William Smith.

[76] W. H. Michael, the barrister from Swansea, anticipated this trend in 1874, when he defined public medicine as 'medical service paid out of funds raised by taxation or rates'. Section of Public Medicine, Norwich, *Brit. med. J.*, 1874, **ii**, 225.

[77] *Brit. med. J.*, 1878, **ii**, 295.

Health for Warwick, told the British Medical Association in 1899 that 'the bacteriologists so dominate the public press that we almost seem to live in a bacillus-stricken world.'[78]

The tremendous social and political repercussions of this sentiment were demonstrated in the Anti-Vaccination League Movement and the early resistance to the Notification of Infectious Diseases in the 1890s.[79] Such agitations were, in part, exacerbated by the much broader anti-scientific reaction which swept through the British public between 1870–1914, and which expressed itself as opposition to animal experimentation, fear of the new 'sciences', and distrust of the 'materialistic' medical profession.[80] Before a 'State system in the fullest sense', as Arthur Newsholme wished,[81] could ultimately succeed, the public required a confidence in their health officers which formal 'police powers' embodied in a system of State Medicine, could not command. In retrospect, the need to reconcile systematic supervision of the public health with the privileges of individual social and economic liberty remained one of the most difficult legacies of the age of Rumsey and Simon.

CONCLUSION

In the period outlined in this paper, we can discern five phases in the professional development of State Medicine in Britain. The first, from about 1810 to about 1830, witnessed the assimilation of the term 'medical police' into academic usage in Scotland; the second, from about 1830 to the late 1840s, saw the modification of the theoretical police concept to the sanitary needs of the United Kingdom; the third, from about 1848 to 1860, witnessed informal efforts by individuals or small networks of medical reformers to give legislative substance to the concept of State Medicine; and the fourth from 1860 to 1875, essentially the 'heroic age' of State Medicine, saw Simon's efforts to develop State Medicine empirically, and the parallel efforts of the network of medical reformers and the British Medical Association to establish State Medicine on a sound

[78] *J. St. Med.*, 1899, 7, 499.

[79] See R. MacLeod, 'Medico-legal issues in Victorian medical care', *Med. Hist.*, 1966, **10**, 44–9. R. MacLeod, 'Law, medicine and public opinion; The resistance to compulsory health legislation, 1870–1907', *Public Law*, Part I (Summer, 1967), 107–28. Part II (Autumn, 1967), 189–211.

[80] See, e.g., Arthur Shadwell, in the *Contemporary Review*, 1900, 580, J. P. Warbasse, *Medical Sociology*, New York, 1909, pp. xi, 23–30.

[81] A. Newsholme, *Medicine and the State*, London, 1932, pp. 21–2.

professional and educational basis. The heroic age came to a close with the establishment of the Local Government Board and the deaths of many of the early reformers. John Symonds, for example, died in 1871, Edmund Parkes died in 1876, Rumsey died in 1876 and William Stokes in 1878. Alexander Stewart and William Farr died soon after in 1883, and de Chaumont in 1888. Only a few survived until the end of the century. Acland died in 1900, Simon in 1904 and Mapother in 1908. The fifth phase, between 1875 and 1900, witnessed the development of institutional forms, the development of scientifically-based traditions of experimental and preventive medicine, and the formulations of distinct professional goals and academic status.

Over the whole period, progress in State Medicine was almost inevitably piecemeal and pragmatic. But, as the German professors acknowledged, a centralized administrative system was not a panacea for all public health problems. It is a paradox of British public administrative history that, in spite of the relatively weak organizational structure of its State Medicine, the British public health system was, by 1870, one of the most effective systems in the world. Medical reformers in Britain accepted the institutional legacy of German State Medicine, but altered its substance and spirit. In managerial terms, British reformers, over a period of about forty years, seized upon a conceptual innovation, copied it, and proceeded to improve upon it. This process, occurring at a time when British institutions were extremely sensitive to continental example was reflected time and again in such areas as technical education, scientific research and industrial development. In this case, the process of institutionalization stimulated, and was stimulated in turn by the development of Government policy.

By the century's end, Rumsey's ideal system had not been achieved in quite the way he intended, but State Medicine, in a sense, had come of age. Already, however, the mid-Victorian concept of State Medicine was giving way to a broader concept of 'social medicine'. Edward Seaton called it a revolution. As Beveridge later wrote in a strikingly similar context, it was 'in some ways a revolution, but in more important ways, it was a natural development from the past. It was in fact, an English revolution'.[82]

[82] *The Beveridge Report*, 1942.

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The Beveridge Report, 1942.

The Causes of Death and Morbidity in the Royal Navy in the 1860s

by

F. P. ELLIS

MY TASK today is relatively simple in some respects. The *Annual Statistical Report of the Health of the Navy* was first published in 1840 to cover the seven-year period 1830–36 and separate reports are available for each of the years which are the concern of this conference. The most reliable figures are for deaths. It was customary by 1860 to report all deaths to the Admiralty by letter and the three main measures of morbidity—the number of invalidings, the days lost by sickness and the numbers of cases on the sick list, were derived from detailed Nosological Returns prepared by medical officers in charge of ships or establishments.

These returns had been a statutory requirement since 1822—prior to which little of statistical value is available concerning the health of the Navy. Their quality improved steadily, particularly during and after the Crimean War, and great importance was attached to the information thus provided on the medical causes of manpower wastage and the subsequent impact of disease and injury on operational efficiency. Throughout the 1860s the *Reports*, edited first by Alexander Bryson and then by Alexander Mackay, attained a high standard and they are invaluable as sources of reference for students of global medicine. Separate chapters are devoted to the Home Station, the Mediterranean, the North American and West Indies, the South-East Coast of America, the Pacific, the West Coast of Africa, the Cape of Good Hope, the East India and China and the Australian Stations, to the Irregular Force and to the Total Force which in the year 1863 comprised 258 ships and shore establishments, with some 552 medical officers. Time only permits me to introduce this wealth of information by reference to some of the more striking figures for the Total Force.

DEATHS

The population of England alone in the 1860s was about half the population in the 1960s. The average strength of the Royal Navy and of the Royal Marines combined was rather more than half the strength today. The death-rate from all causes for the male population of England was consistently greater than that for the Navy, usually twice and sometimes three times as great.

TABLE I

Death Rates per 1,000 from all causes for the Royal Navy, Civilian Males in England aged 15–44 years and for Merchant Seamen, 1860–1867.

Year	*Royal Navy*	*Males in England*			*Merchant Seamen*
		15–24	25–34	35–44	
1860	14.7	7.1	9.0	12.7	21.9
1861	15.0	7.3	9.2	12.7	20.8
1862	15.3	7.3	9.4	12.8	20.8
1863	11.4	7.4	9.7	13.3	18.3
1864	14.0	7.8	10.8	14.9	19.9
1865	11.3	7.8	11.2	14.8	23.3
1866	10.2	7.9	11.7	15.5	25.8
1867	11.5	7.3	10.8	14.3	26.9

However, if one looks only at the rates for men in civilian life between the ages of 15 and 44 (Table I), although the naval death rates were on the whole greater than those for civilian men of a similar age distribution in the early years, during this decade the naval death rate tended to fall and the civilian rate tended to rise.

The death rate in the Merchant Service was always considerably greater than that for the Navy and was increasing towards the end of the period. These comparisons must be offset by the high invaliding rates which enabled the Navy to return many unfit men to civilian life and it was probably even easier for the Merchant Service to eliminate the unfit. Records of ill-health were also possibly less reliable in the Merchant Service.

TABLE II

Death Rates per 1,000 from all causes for the Royal Navy and for the Civilian Male Population of England and Wales aged 15–44 years, 1961–1964.

Year	*Royal Navy*	*Males—England and Wales*	
		15–24	25–44
1961	1.2	1.01	1.8
1962	1.3	1.00	1.75
1963	1.1	0.99	1.8
1964	1.0	1.03	1.81

When the death rates are compared for the Total Naval Force during the period 1961 to 1964 and for men in similar age groups resident in England and Wales at that time (Table II) there is seen to be little difference between the contemporary civilian and naval death rates but the overall rates for both groups are only a fraction, about one-tenth, of the rates in the 1860s.

The most important causes of death in the 1860s were drowning, phthisis and haemoptysis, continued and remittent fever—which must have included such infectious diseases as typhus, typhoid, and malaria—wounds and injuries, dysentery, yellow fever, inflammation of the lungs and pleura, heart disease and cholera—a depressing picture dominated by infectious diseases, drowning and injuries (Table III).

TABLE III

Main Causes of Death in the Total Force 1860–1864.
Numbers of Deaths

	1860	1861	1862	1863	1864	1860–1864
Total Force	64,025	62,485	58,870	54,090	53,000	
Drowning	117	104	96	103	186	606
Phthisis and haemoptysis	142	133	122	85	105	587
Continued and remittent fever	64	108	102	56	47	477
Wounds and injuries	85	101	100	80	78	444
Dysentery	125	55	81	45	49	355
Yellow fever	81	141	57	5	40	324
Inflammation of the lungs and pleura	63	62	31	42	34	232
Functional and organic disease of the heart	44	39	33	27	36	179
Cholera	8	3	90	26	9	136
Total deaths—all causes	938	940	902	619	742	4141

The main causes of death were similar to those in the Merchant Service (Table IV) but the incidence was rather different and this explains the high death rate for merchant seamen throughout this period. For example, during the year 1869 the Returns from the Registrar-General for British Seamen showed that there were 4,832 deaths, ten times the total number of deaths in the Royal Navy that year—the rates were 24.7 per 1,000 for merchant seamen and 9.9 per 1,000 for the Navy, the latter figure being consistent with the trend towards improvement shown in Table I. The following accounted for five-sixths of the deaths in the Merchant Service: 'drowned by wreck', 'drowned by accident other than wreck', 'other accidental deaths not drowning', fever, consumption, yellow fever, dysentery and cholera. Deaths by drowning caused a much higher proportion of deaths from all causes in the Merchant Service than in the Royal Navy possibly because merchant seamen by the nature of their service in peace time spent more time on the high seas in all weathers and were less carefully selected, trained and disciplined. The death rates from infectious diseases and consumption (or phthisis) on the other hand were more closely related. Scurvy, the scourge of seamen

TABLE IV

Causes of Death in the Royal Navy (Total Force) and for Merchant Seamen in 1869.
Numbers of Deaths

	Merchant Seamen	*Royal Navy*
Drowned by wreck	1770	75
Drowned by accident other than wreck	1069	
Other accidental deaths not drowning	277	39
Fever	291	8
Consumption	165	51
Yellow Fever	142	57
Dysentery	157	17
Cholera	137	5
Scurvy	9	0
Total deaths	4832	484

in the eighteenth century, caused only nine deaths of merchant seamen and none in the Royal Navy in this year. Nevertheless, this sorry story is underlined by the rarity of drowning and of deaths due to infectious diseases in the Navy today. Only 42 men were drowned during the first five years of the present decade and only 8 deaths due to infectious diseases were reported—2 from malaria, 2 from poliomyelitis, 1 from tuberculosis, 1 from leptospirosis, 1 from meningitis (type unspecified) and 1 from amoebic abscess.

This dramatic change in pattern is given further emphasis by the fact that neoplastic diseases do not even appear as a cause of death in the Annual Reports for the 1860s (Table V).* In the first five years

TABLE V

Main Causes of Death in the Royal Navy 1960–1964. Numbers of Deaths

	1960	1961	1962	1963	1964	1960–1964
Total Force	93,890	91,950	91,230	92,705	94,583	
Malignant neoplasms	8	7	7	3	6	31
Leukaemia and aleukaemia	4	2	1	1	2	10
Lymphosarcoma etc.	4	2	3	–	–	9
Benign neoplasms etc.	3	–	1	–	1	5
Total deaths due to neoplasms	19	11	12	4	9	55
Arteriosclerotic and degenerative heart disease	24	10	5	9	6	54
Total deaths due to disease	60	33	28	20	33	174
Total deaths due to injury	100	71	75	74	65	385
Total deaths—all causes	177	111	117	100	104	609

* The nomenclature for the 1960s which is used in Tables V and IX and elsewhere in the text is that of the 'A' List of 150 Main Groups of Diseases or Injuries from the World Health Organization's *International Statistical Classification of Diseases, Injuries and Causes of Death* (Geneva, 1955), published in 1957 which, together with the even more abbreviated 'C' List, is usually used for the construction of the tables in the recent Annual Statistical Reports on the Health of the Navy and also in similar reports of the Army and the Royal Air Force. Whilst this is a convenient method for showing the impact of certain broad groups of disease, it has the drawback epidemiologically that the incidence of important individual diseases which do not appear in the 'A' List may be obscured—one such example is coronary disease, which caused about 75 per cent of the deaths from arteriosclerotic and degenerative heart disease but is not shown separately at present in the Naval reports. From the year 1958 onwards, information is available, therefore, at the Ministry of Defence on the more detailed causes of sickness in tabulations derived from the Holerith cards for each case according to the W.H.O. Tabular List, of over 1,000 diagnostic headings and their 4-digit Sub-Categories, which can be consulted for research purposes or to answer administrative enquiries.

of the present decade if deaths due to malignant neoplasms, the leukaemias, lymphosarcoma, other malignant disorders of the lymphatic system and benign neoplasms are considered together, this group becomes the most prominent cause of death from disease in the modern Navy. Neoplastic disease and arteriosclerotic and degenerative heart disease—largely coronary disease—caused about

TABLE VI

Main Causes of Invaliding for the Total Force 1860–1864
Rates per 1,000

	1860	1861	1862	1863	1864
Phthisis and haemoptysis	10.0	6.9	5.4	6.3	5.1
Rheumatism	4.8	3.3	2.9	3.5	4.1
Functional and organic diseases of heart	2.4	3.4	3.1	3.0	2.9
Wounds and injuries	2.8	2.5	2.7	2.4	2.4
Dysentery	3.3	1.5	1.2	1.7	1.4
Epilepsy	2.2	1.6	1.7	1.6	1.7
Dyspepsia and debility	2.0	1.7	1.1	1.7	2.2
Syphilis	1.2	1.2	2.3	1.8	1.9
Hernia	2.1	1.9	1.3	1.5	1.3
Ulcer	1.3	1.5	1.5	1.3	1.4
Inflammation of lungs and pleura	1.2	1.3	1.1	0.9	0.9
Diseases of bones and joints	1.0	1.1	0.7	0.6	1.0
Total invalidings—all causes	44.4	38.2	33.0	35.1	35.4

two-thirds of all the deaths from disease, whilst injuries, many of which were caused by road accidents, accounted for more deaths than all the diseases combined. But the average annual mortality rate was only 1.3 per 1,000 whereas in the corresponding period of the 1860s it was 14.2 per 1,000.

INVALIDINGS

During the first five years of the 1860s 33 to 44 per 1,000 of the

Total Force were invalided each year (Table VI). The most common disorder resulting in invaliding was phthisis and haemoptysis, followed by rheumatism, heart disease, wounds and injuries, dysentery, epilepsy, dyspepsia and debility, syphilis, hernia, ulcer, inflammation of the lungs and pleura, and diseases of the bones and joints.

The rates for epilepsy of 1–2 per 1,000 may be compared with the averaged rates for ratings in the post-war Navy, which still lay between 0.4 and 0.7 per 1,000:

1953–54	1955–57	1958–60	1961–63
0.6	0.7	0.5	0.4

Despite the careful screening of new entries and the great reduction in disorders likely to give rise to epileptiform convulsions, epilepsy continued to figure quite prominently in naval-hospital practice. The rates for officers were nearly always much lower.

The invaliding rates for all causes were three to four times as high as the rates for officers and ratings of the Total Force for the years 1960–64 which were very consistent at about 11 per 1,000 per year, the most prominent disabilities being psychoneuroses—which were not included in any of the tables in the 1860s—wounds and injuries and diseases of the stomach and duodenum, many cases of which were probably included earlier under dyspepsia and debility.

DAYS LOST DUE TO SICKNESS

The average number of men who were sick daily from all causes ranged from 50 to 60 per 1,000 of the Total Force in the 1860s (Table VII). Wounds and injuries caused more time lost on the Sick List than any disease group, closely followed by syphilis—before the days of dark-background microscopy and the Wasserman test, however, and almost certainly including many cases of chancroid. Next in prominence came 'phlegmon [boils] and abscess', ulcer, rheumatism, influenza and catarrh, and remittent fever, diarrhoea, inflammation of the lungs and pleura, dyspepsia and debility, phthisis and haemoptysis, gonorrhoea—diagnosed clinically not by microscope, orchitis, bubo—usually due to venereal infection—and diseases of the heart.

The average rate for officers and ratings sick daily in the first five years of the 1960s was only about 12 per 1,000 of the Total Force for all causes and 3 per 1,000 for injuries. Injuries still accounted for more time lost on the Sick List than any other group of disorders and the next most prominent cause of wastage was acute respiratory disease—probably the same type of illness reported as influenza and

TABLE VII

Average 'Sick Daily' in the Total Force 1860–1864

Rates per 1,000

	1860	1861	1862	1863	1864
Wounds and injuries	7.6	8.1	8.4	8.0	7.5
Syphilis	6.4	7.9	7.5	7.8	8.6
Phlegmon and abscess	6.0	6.8	6.6	6.2	5.9
Ulcer	5.7	6.1	6.3	5.1	5.0
Rheumatism	3.6	4.4	3.9	3.7	4.0
Influenza and catarrh	3.6	3.2	2.7	2.3	2.1
Continued and remittent fever	2.1	2.7	3.0	2.1	2.0
Diarrhoea	2.1	1.9	1.9	1.5	1.3
Inflammation of lungs and pleura	1.8	1.7	1.3	1.2	1.2
Dyspepsia and debility	1.4	1.3	1.3	1.2	1.4
Phthisis and haemoptysis	1.2	1.6	1.0	1.4	1.2
Gonorrhoea	0.9	1.3	1.0	1.3	1.1
Orchitis	0.9	1.5	1.0	1.0	1.1
Bubo	1.0	1.1	0.9	1.1	1.2
Diseases of heart	0.6	0.7	0.6	0.5	0.7
Total "sick daily"—all causes	53.6	59.0	57.1	54.3	54.0

catarrh in the 1860s—followed by psychoneuroses, skin diseases, duodenal ulcer and gastro-enteritis and colitis which, no doubt, included many of the cases formerly diagnosed as diarrhoea; but the daily rates were halved for injuries and reduced five or six times for diseases as a whole.

CASES OF SICKNESS

The rates for the cases of sickness for the Total Force provide the most impressive numerical index of all (Table VIII). 'Phlegmon and abscess' caused the largest number of cases each year in the 1860s. The next most common causes were wounds and injuries, influenza and catarrh, ulcer, diarrhoea, rheumatism, syphilis, fever, and dyspepsia and debility. An average of 1,441 cases of sickness per 1,000 of the Total Force were reported annually. This compares unfavourably with the average figure of 1,333 cases for the years 1830 to 1836 and, together with the deaths and invalidings for those years, suggests that ill-health in the Navy had actually increased during this thirty-year period. This was possibly a result of the more far-reaching activities of the Fleet during the first half of the Victorian era; the Total Force had also doubled its numbers, or perhaps

TABLE VIII

Main Diseases and Injuries in the Total Force 1860–1864. Cases. Rates per 1,000

	1860	1861	1862	1863	1864
Phlegmon and abscess	234.6	263.0	262.3	253.7	239.7
Wounds and injuries	232.3	246.5	256.7	247.1	238.9
Influenza and catarrh	179.9	152.2	150.9	137.2	124.4
Ulcer	89.6	96.3	108.1	92.7	87.2
Diarrhoea	104.9	93.1	99.9	95.0	79.2
Rheumatism	82.4	87.8	81.9	78.0	85.1
Syphilis	70.6	80.0	76.7	80.7	83.5
Continued and remittent fever	57.4	63.1	78.4	65.0	61.2
Dyspepsia and debility	46.3	48.4	51.3	58.3	53.8
Total cases—all causes	1411	1461	1506	1454	1374

it was an artefact due to better reporting and improvements in the statistical machinery.

When the dramatically high figures in Table VIII—almost one and a half sicknesses per man per year—are compared with the total annual case rates during the early 1960s (Table IX) which were usually rather higher than 300 per 1,000 for the Total Force—or one-third of a sickness per man per year—a further measure is obtained, suggesting a four to five-fold improvement. Such a comparison might be partially misleading as the figures for the 1960s,

TABLE IX

Main Diseases and Injuries in the Total Force 1960–1964. Cases. Rates per 1,000

	1960	1961	1962	1963	1964
Acute upper respiratory infections	58.9	63.8	72.1	64.2	53.9
All injuries	54.7	52.7	48.3	51.0	50.1
'Other' diseases of genito-urinary system	28.9	25.1	24.6	16.3	15.1
All other diseases classified as infective and parasitic	17.8	19.9	21.0	20.1	19.4
Gastro-enteritis and colitis	20.1	18.1	16.5	18.5	17.5
Gonococcal infection	20.0	18.4	17.2	16.0	13.3
Influenza	12.3	26.4	18.2	21.9	9.3
Ill-defined and unknown causes	16.0	16.1	15.3	17.9	16.8
Diseases of the skin including boils, abscess, cellulitis and other infections	13.2	13.1	13.8	14.3	12.4
Total cases—all causes	328.8	338.5	325.9	326.8	292.2

with certain exceptions,* excluded cases who were sick for less than 48 hours. However, the figures for deaths and invalidings, to neither of which the '48-hour' qualification applied, suggest that the overall

* The exceptions were that all cases of venereal disease and all cases terminating in death or invaliding were shown as cases even though they might not have been sick for 48 hours.

improvement was at least of this order. It is also probable that many men shown on the Sick List today would not have been considered ill enough to be excused duty in the 1860s before the Welfare State had conditioned the population from which the Navy draws its recruits to more generous medical care and to more exacting standards of fitness for work than prevailed in the past.

Acute upper respiratory tract infections and wounds and injuries were the most frequent causes of sickness but the incidence of both was greatly reduced. The rates for diarrhoea (shown under gastro-enteritis and colitis) and for influenza were also greatly reduced and so were the rates for skin diseases including boils (or 'phlegmon') and abcess.

'Other diseases of the genito-urinary system'—largely cases of non-gonococcal urethritis—and gonococcal infections still account for much sickness amongst ratings in the Navy today, though rarely reported for officers, whilst 'all other diseases classified as infective and parasitic' also includes illnesses of venereal origin such as chancroid and 'other unspecified venereal disease' for which the venereal disease rate ranked only second to that for gonorrhoea in the early 1960s. The rates for gonorrhoea alone were rather less than, but were still of the same order as, the average annual rate of 23.6 per 1,000 for the first five years of the 1860s and focus attention on these preventible infections as one area where preventive measures could have been intensified. On the other hand, the annual rates for syphilis, by this time diagnosed with certainty, never exceeded 1 per 1,000 for any of these years, in vivid contrast with an average rate of 78 per 1,000 in the early 1860s, when the diagnosis frequently must have been doubtful to say the least.

Inability to make a microscopical diagnosis and the lack of other laboratory tests to confirm the clinical diagnosis obscured the true picture of these infections in the 1860s as it obscured that of the infectious fevers. No doubt many of these sad cases had multiple infections. Even today, the infective agent which causes non-gonoccocal urethritis has still to be identified. This brief summary of achievement thus ends with an example of failure of preventive measures to keep pace with the phenomenal advances in the therapy of these diseases, particularly during the past thirty years. But it is in the study of such failures that the road towards improvement lies, in the identification of the causes of morbidity and their true incidence with some degree of accuracy. This sociological problem is still with us and it is not confined to the Navy, as several papers and leaders have pointed out recently, nor, of course, was it in the 1860s.

UNREPORTED SICKNESS

One can only guess at the size of the submerged portion of the iceberg—unreported sickness. Most men in the Navy do not report sick without good reason. It is possible that a hundred years ago the volume of unreported disease was greater than it is today but this is only conjecture. The broad picture can be sketched from the admirable tables and commentaries which Bryson and Mackay provided. The more vivid contrasts are plain for all to see. The student must, however, complete the picture for himself.

SUMMARY

In the 1860s the most prominent causes of deaths in the Royal Navy were infectious diseases, particularly phthisis, and the fevers, many more of which were of 'unknown origin' than is the case today. The next most common cause was drowning and the third wounds and injuries. The most prominent causes of invaliding were phthisis, rheumatism, functional and organic disease of the heart and wounds and injuries. The most prominent causes of working time lost by men on the Sick List were wounds and injuries, 'syphilis', 'phlegmon and abcess' and ulcer. The greatest numbers of cases of sickness reported annually were due to 'phlegmon and abscess', wounds and injuries, and influenza and catarrh.

Although the aetiological agents of the fevers and infections had still to be identified, nosological tables based on clinical diagnoses produced a surprisingly clear-cut picture of the medical problems in the Fleet a hundred years ago and the epidemiological approach to their ultimate control is epitomized in the Annual Health Reports which reveal a state of affairs which compared not unfavourably with conditions on shore.

A century of progress leaves us with wounds and injuries as a major cause of deaths and morbidity and acute respiratory tract infections, influenza, 'gastro-enteritis and colitis' and venereal diseases

as major causes of morbidity, although their incidence is greatly reduced and a new set of diseases—the psychoneuroses, which our forefathers did not consider of sufficient importance to merit inclusion, is of increasing prominence in the statistical tables.

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Animal Disease and Public Health

by

J. W. BARBER-LOMAX

To a veterinarian concerned with the history of this period the outstanding figure was John Gamgee and the outstanding event was the calamitous outbreak of cattle plague—rinderpest—which was introduced into this country from Russia in June 1865—appearing in the London market on the 14th of that month—at almost the centre point of the decade with which we are concerned.

Because of the importance of this outbreak of disease among our cattle a century ago and the fact that it led to the establishment of a Veterinary Department of the Privy Council—the forerunner of the present Animal Health Division of the Ministry of Agriculture, Fisheries and Food—much has been written about and spoken of the subject during the past two or three years. However, little attention seems to have been paid to the wholesale consumption in this country during the period in question of diseased meat, i.e. meat from animals which had died of, or had been slaughtered because of, disease, and meat from apparently healthy animals slaughtered for food although not fit for human consumption, and of the general disease situation of the flocks and herds of the country.

A brief account of the history of epidemic disease among farm livestock in the United Kingdom will serve to establish the position at the beginning of the decade we are considering.

As far as is known, the first outbreak of disease of an epidemic nature was in July 1714 when cattle began dying in Islington of a condition which turned out to be rinderpest—not previously encountered in the United Kingdom but known as the much-dreaded cattle plague which periodically moved westward into Europe from the Russian steppes. Thanks to the activities of a

Commission of Justices of the Peace of the County of Middlesex appointed by the Lord Chancellor, and the wise counsel of Thomas Bates, surgeon to George I, the outbreak was under control in three months and eliminated from the country by the end of the year. All infected cattle were destroyed and burned, buildings where sick animals had been housed were disinfected and kept empty for three months, the sale of sick animals was prohibited and compensation was paid to the owners of beasts which had been compulsorily slaughtered. The whole exercise was a masterly one.

In 1745, rinderpest again appeared near London and spread rapidly. The previous lesson had not been learned and owing to the necessary measures not being conscientiously applied the outbreak was not brought under control and the disease eliminated until 1757—twelve years later.

Another outbreak of rinderpest occurred in the autumn of 1769 but thanks to a rigid application of a slaughter policy it was soon brought under control. An Act was passed in February 1770 'for indemnifying all persons with respect to advising and carrying into execution His Majesty's Orders in Council made for preventing the spreading of a contagious distemper among the horned cattle', and those in authority made certain that cattle dealers and farmers observed the conditions laid down for the control and eradication of the epidemic.

In 1839 foot-and-mouth disease first appeared in Britain, a large dairy herd at Islington being first attacked. This outbreak occurred despite the fact that the importation of susceptible animals—all cloven-hoofed animals—was at that time prohibited and it is not known how the disease was introduced, though it is probable that the ban on import of animals was not strictly observed. During the next thirty years, waves of epidemics of foot-and-mouth disease occurred, interspersed with periods of comparative freedom from the disease.

Contagious bovine pleuro-pneumonia—the lung disease—was the next epidemic disease to appear, in 1842, and it ranged unchecked for nearly thirty years causing heavy losses of up to 90 per cent in some outbreaks.

In 1842, in reply to the agriculturists' demand for free trade, the Government had thrown open Britain's ports to foreign cattle and sheep, and in fact to foreign disease, on payment of duty. What was worse, in 1846, the duty was removed and the way was wide open for the introduction of any disease that happened to be prevalent on the continent.

Sheep-pox was the next epidemic disease to appear, and was first diagnosed near Windsor in the autumn of 1847. The disease spread to many parts of Britain causing heavy losses with a mortality on occasion as high as 50 per cent. This disease did not disappear until 1850 and then only temporarily.

We now arrive at the middle of the century with past experience of epidemics of rinderpest (which however had not been seen since 1770), with foot-and-mouth disease, and pleuro-pneumonia causing sporadic but sometimes heavy losses and with sheep-pox temporarily absent.

Apart from these imported diseases the flocks and herds of the country were affected by less spectacular endemic diseases little understood by the farming community such as sheep scab, liver rot, anthrax and rabies.

At this moment, in 1852, John Gamgee, second son of an English veterinarian living and working in Florence, finished his training at the Royal Veterinary College, London, and received his diploma from the Royal College of Veterinary Surgeons. He had been educated for the medical profession but after his elder brother, Joseph Sampson—later to become a noted surgeon—who had also qualified as a veterinarian, turned to medicine, John forsook his medical training and entered the veterinary field, though continuing to attend classes at University College Hospital Medical School.

In the 1850s and 1860s John Gamgee led the attack against the importation of contagious animal diseases and the prevalent practice of human consumption of diseased meat. His campaign was at first largely unsuccessful and he was ridiculed rather than ignored: he was described at the end of his all too short life as 'a living monument of departmental incapacity and natural ingratitude'.

Two statements of Gamgee's illustrate the twin themes of my review. At the First International Veterinary Congress in Hamburg in 1863, which he had been instrumental in arranging, he was asked whether, and if not, why, in England, no measures had been taken against pleuro-pneumonia if, according to his account, the country suffered so much from the disease. Gamgee replied that hitherto no preventive measures had been taken and that diseased animals were slaughtered and sold as food.

In that same year, in a lecture delivered before the London Metropolitan Association of Medical Officers of Health on The Diseases of Animals in Relation to Public Health and Prosperity, Gamgee said 'I was led [first taught] to believe that the destructive epizootics of the bovine tribe in this country were non-contagious and originating spontaneously here. I now know that we may as soon expect the spontaneous outbreak of pleuro-pneumonia and epizootic aphtha [foot-and-mouth disease] in the British Isles as the spontaneous generation of wolves in Epping Forest.'

The average annual mortality among cattle due to disease in Great Britain prior to 1842, the year in which pleuro-pneumonia appeared, has been estimated to be at the most two and a half per cent, but by 1860 the loss is believed to have been nearly double that figure. If we take the estimated total of cattle in the United Kingdom in 1860 to be seven and a half million we find that about 375,000 head died of disease in that year, and it is probable that more than half the loss was due to pleuro-pneumonia. Investigation shows that the number of cattle estimated to have died of disease in 1860 was roughly three and a half times the number imported from the continent.

Of the sheep in these islands it has been estimated that at the beginning of the 1860s one and a half million died annually from disease and, of the pigs, about one hundred and thirty thousand.

From these figures and the calculated values of the stock, the deaths of food-producing animals at this period probably represented an annual loss of more than six million pounds sterling.

To continue the loss-from-disease picture to the middle of the decade—when rinderpest was re-introduced in 1865 and until the country was once more free of the disease, just over two years after the epidemic had started—over four hundred thousand cattle had died from this disease alone.

Why was the animal disease situation so alarming at this time and why was John Gamgee ceaselessly campaigning for measures to be introduced for the control of imports of stock?

Few there were, either in the medical or veterinary professions, who had a clear understanding of the cause of epidemic disease and few also who realized with Gamgee and his Continental colleagues that it was necessary to contain an outbreak of imported epidemic disease in an island community by employing methods of slaughter of diseased stock, control of movements of animals, disinfection and, necessarily, of providing compensation for owners whose animals had been compulsorily slaughtered.

In the case of some epidemic diseases such as pleuro-pneumonia and sheep-pox, where methods of inoculation were applicable with advantage, our Continental colleagues were able to reduce the ravages of epidemics, but it was evident to Gamgee and those of his colleagues who believed in the contagious nature of such diseases, that the way to prevent their introduction to this country was to provide a proper system of inspection at the ports, coupled with the provision of facilities for quarantine.

It was not until 1848, the year after sheep-pox was introduced into Britain, that an Act was passed to enable the inspection of imported stock at ports and the destruction of those affected with disease. The Committee of the Privy Council for Trade had realized that sheep-pox was a serious matter and was causing considerable loss to sheep farmers. It was known by the Committee that the City of London had powers to seize and destroy infected animals if exposed for sale but there was no precise information available about the powers enjoyed by local authorities throughout the country although it was known that there were many local bye-laws and regulations concerned with the seizure of diseased meat and fish.

The enquiry to all market managers throughout the country put about by the Privy Council showed that although many towns had regulations for the seizure and destruction of unsound meat and fish, nowhere outside Smithfield Market in the City of London did there exist any powers covering the seizure and destruction of live diseased animals. It emerged from this enquiry that even those local authorities who had established such regulations found it difficult to enforce them. This investigation led directly to the two 1848 Acts, one the Sheep and Cattle (Contagious Diseases Prevention) Act, the other the Sheep and Cattle (Prohibition of Importation) Act, being placed on the Statute Book.

It was not, however, until early in 1866 that Parliament provided sufficient powers by means of the Cattle Diseases Prevention Act to ensure that all local authorities were enabled to deal with outbreaks of disease in such a way that they were brought to an early end, and it was not until 1869 that regulations were introduced to control the importation of stock in such a manner that diseased animals could be slaughtered on entry or even prohibited from landing. Even so, this latter control was left in the hands of the Customs authorities until 1871, when the responsibility for the inspection at landing places and the appointment of inspectors was transferred to the Privy Council.

Turning now from animal disease to public health matters, it will no doubt surprise you to learn that comprehensive legislation in England and Wales empowering Medical Officers of Health and Inspectors of Nuisances to seize unsound meat and unsound animals dates only from the Public Health Act of 1875. Until the second quarter of the nineteenth century, although severe losses from disease were occasionally sustained, these were experienced at rare intervals and in special districts, and the universal practice was to bury the carcases of animals which had died from disease. It is true that occasionally such carcases would be used for human food but such occasions would be rare and only in remote districts.

With the increase in population and the growth of towns the numbers of animals reared not only did not keep pace with this increase but actually declined. This situation led understandably to the increase in the price of meat and animal products, and led also to the temptation for farmers and butchers to sell to the public as human food every animal dying from or slaughtered on account of disease. With the ever-increasing scarcity of animal food due to the ravages of epidemic diseases, more and more diseased meat found its way to the table and by the end of 1862 it was estimated that in the large towns one quarter of the animal products used for human food was from diseased animals. It was frequently claimed that diseased meat, sold cheaply, provided food for the poor who otherwise would not have been able to afford meat, but this was far from the case as can be calculated from the prices paid by the meat dealers and butchers for diseased and dying animals.

Although many otherwise well informed people discounted the dangers to the consumer of meat from diseased animals we can find many reports of illness and even death following the consumption of unwholesome meat. John Gamgee, and his brother Joseph Sampson, were constantly urging the need for efficient inspection of live cattle, and meat exposed for sale for human consumption, and the power to order confiscation and destruction to protect the consumer. The Gamgees wrote many letters and articles in the professional and daily press and John on numerous occasions lectured to groups of Medical Officers of Health in an attempt to bring home to those concerned with food supplies that the produce of diseased animals was unwholesome as human food. Time after time examples were given of human illness and even death after eating meat which should have been condemned, and it is difficult for us to realize that it was necessary to agitate repeatedly in this way for the prevention of abuses which would not today be tolerated.

The principle difficulties to overcome when discussing this period in the light of this particular subject are (1) the contagious nature of disease was not properly understood and (2) the effect of damaged or diseased animal flesh on the health of man was imperfectly appreciated. It has been said that 'he who does not think bacteriologically and act according to its principles runs the risk of committing the grossest errors in the elementary rules of meat inspection'. In the 1860s such a statement as this could not have been made.

In face of the limited understanding in the 1860s of the problems inherent in animal disease and public health, it is useful to examine the lecture given by John Gamgee in April 1862 in London to the Metropolitan Association of the Medical Officers of Health. In this talk he defined five categories which he used to 'classify the impurities known to us as pervading animal food'.

Let us deal with these categories in turn:

1. '*Cadaveric venom and animal poisons of undetermined nature, developed spontaneously in health or disease*'.

Herein were included the flesh of animals slaughtered when exhausted by overdriving or stress from other causes, those which had died following protracted labour and metritis, and those dying following normal parturition but affected with generalized post-parturient infection. Today we recognize that such carcases should be condemned for human food.

2. '*Animal poisons well known from their effects in creating specific contagious diseases*'.

Although Gamgee recognized that the flesh of cattle infected with rinderpest could be eaten by man with impunity, he realized that the danger of traffic in such meat was on account of the spread of the disease which would lead to further decimation of stock.

The same was his opinion regarding cattle affected with pleuro-pneumonia, but he was also quite satisfied that their flesh and that of cattle affected with rinderpest was of inferior quality and should not be sold as good meat. There was also the danger that such cattle were often slaughtered in a moribund condition when the disease picture was further complicated and the flesh almost certainly toxic.

Foot-and-mouth disease concerned him principally on account of the injurious nature of the milk. Not only did milk from affected cattle produce the disease in man but it was also dangerous as the udder was frequently affected with mastitis and the milk was thus further contaminated.

In the case of anthrax, Gamgee was on surer ground. Not only were many cases recorded of infection in man following the handling of anthrax carcases, but the literature of the period contains cases of infection following the eating of meat from infected animals.

Today in this country legislation effectively prevents meat of the nature described above from reaching our tables.

3. '*Organic Poisons the result of Decomposition*'.

Under this heading was included the 'sausage poison' and the 'cheese poison'. In this category today we would put meat poisoning of man illustrated by toxic gastro-enteritis and botulism.

4. '*Mineral and vegetable poisons absorbed into the systems of animals*'.

The heroic dosing of cattle by unqualified and, even at times, qualified practitioners with salts of arsenic and antimony, with jalap, cantharides, calomel and aloes was thought to give rise to ill effects in man consuming the flesh of such beasts. Fortunately the trade in diseased animals by encouraging their slaughter rather than their medical treatment to a certain extent precluded the trade of drugged carcases and poisoning of this kind was more likely to result from the contamination of food due to the careless handling of such medicaments by those administering them. Today the viscera and organs of animals which have been treated medicinally before slaughter are rejected for human consumption even if the flesh is fit.

5. Gamgee's last category—'*Parasitic animals inducing disease in man and animals*'—was particularly relevant.

In 1861 Leuckart had established the relationship between the cysticercus of cattle and taenia saginata, and in 1869 Oliver showed that taenia saginata was produced in man by eating bovine flesh infected with the cysticercus. The cysticercus of swine when ingested leads to the production of taenia solium in man and more dangerously also to the possible formation of cysticerci in man. As far as the latter is concerned, during the period 1856–1866 Von Graefe in Berlin found ninety cases of cysticerci in the human eye out of 88,000 cases of ocular disease.

As regards the former danger—that of taenia solium infection—Gamgee related to his audience the following experience he had had in a butcher's shop in Edinburgh. 'I called into a butcher's shop and was talking to the clerk for some time. I observed this individual pick up bits of fat pork and put them in his mouth raw. I cautioned him and told him that he would be sure to suffer from tapeworm. He exclaimed "I have never been able to git rid of that and wish you could give me a cure". Meanwhile he passed his hand up the leg of his trousers and seized in his finger an active joint of a tapeworm that he had felt climbing down his leg all the time I had been speaking to him'.

The third dangerous parasite was trichinella spiralis. An English surgeon, Hilton, in 1832 had found calcified muscle trichinae in the human cadaver but did not detect the worm. This was first discovered by James Paget in 1835 and was named by Richard Owen. The importance of trichinae as regards meat inspection was established in 1860 through the discovery of trichinosis by Zenker. The danger of trichinosis is shown by the fact that in outbreaks of this disease in Germany in 1863 and 1865, in which 500 people were infected, no less than 129 died.

In my paper I have concentrated almost exclusively on the activities of John Gamgee with some reference to the backing of his campaigns for the control of contagious diseases of food animals and for the abolition of the diseased meat trade by his brother Joseph Sampson, the surgeon, and for this I make no apology. Gamgee was the outstanding figure of the period in this field, but I must also give credit to the country veterinary practitioner. Out of a total of just over 1,000 qualified veterinarians in the United Kingdom at this period, three hundred were supplying disease statistics from their areas of practice to John Gamgee each year and from their returns he was compiling country-wide statistics to add weight to his campaigns. These men were good observers and in spite of the deficiencies of their college training, whether in London or in Edinburgh, were satisfied that the diseases with which we have been concerned were not spontaneously generated but imported from the continent of Europe and were spread by means of contagion throughout the herds and flocks in their areas of practice.

Gamgee was their spokesman, but we must not forget them, and it was thanks to their vigilance and experience that, once legislation to control animal disease and to prevent the exposure for sale of diseased meat was enacted, conditions gradually improved.

(This review of animal disease and public health in the 1860s has been compiled from the journals of the period, principally the *Edinburgh Veterinary Review* (a journal published by John Gamgee himself), the *Veterinarian*, the *Lancet* and the *British Medical Journal*: further reference can be made to the columns of the daily newspapers, particularly *The Times* wherein much correspondence concerning these two topics appeared.)

Notes on West African Public Health

by

RALPH SCHRAM

VERY FEW people in Britain, and even more regrettably, in Nigeria, Ghana, Sierra Leone and Gambia, realize that the medical history of the West Coast extends back as far as it does. It is still perfectly true that the state of health of most ports and towns of West Africa is far from adequate, but nonetheless a considerable effort has been made by many different agencies, foreign and national over several centuries.

As long ago as 1504 a hospital was built by Portuguese traders on the island of St. Thomas (São Tomé) to the south of Nigeria (Ryder, 1961). From the fifteenth century these traders brought goods from the offshore islands to the mainland over the stretch of coastline from the Canary Islands to the Congo river. They seem to have survived through their knowledge of cinchona bark, which in the latter part of their travels they brought from South America.

Doctors were known to accompany Dutch trading ships in the sixteenth and seventeenth centuries, and by this time Britain, Denmark, and a few other European and later American countries brought medical care with their traders and missionaries. By the time of the eighteen sixties there were therefore present on the Coast some elements of practically all the agencies which brought medical care to this part of Africa, whether military, naval, commercial, missionary, scientific or governmental.

THE ARMY AND NAVY

In the eighteen sixties Britain, in attempting to crush the slave trade was perforce extending her Protectorates farther and farther along the Coast towards the Congo, from early beginnings in the Freed Slave Settlement of Sierra Leone. From medical officers of that time we can gain a fair picture of the state of health of the troops and sailors. Dr. Albert Gore published his book *A Contribution to the Medical History of our West African Campaigns* in 1876 but he went back over the records for one hundred years, noting that doctors had served as early as 1765, and he named fifty-eight doctors between the years 1841 and 1858 'of whom a large number lie buried in Africa'.

By 1864 there were thirty-five doctors in the Army, but following the 1869 cholera epidemic in Bathurst (the only reference I have discovered to this disease in West Africa) there were 1,700 deaths amongst Africans, and a number of deaths in the British army as well. The troops were then reduced in number and by the end of the decade there were only seven doctors in the West African armies. These increased shortly afterwards with the onset of the Ashanti wars, to seventy-three, including ten African surgeons.

All medical service for doctors on the West Coast was voluntary, and even in the Medical Directory for 1889 there is a note that each year in West Africa was reckoned double towards retirement, and if the doctor survived twelve months he had a full year's leave. Double pay was issued after the year 1873 while serving on the West Coast.

Other notes on the West African armies do not relate directly to the 1860 period, but can not have been much dissimilar. Daniell (1845) referring to 1822–1830, in a series of articles in the *London Medical Gazette* noted the deaths of 1,298 white troops in West Africa out of 1,658. The rest were invalided home and only thirty-three remained fit! The cause of most of this mortality was the African fever, and the confusion and chaos amongst medical men in 1860 has to be read to be believed. Daniell attributed all this ill-health to the 'extensive surfaces of mud and stagnant water exposed to . . . a torrid sun (which) generated those miasmatous exhalations', but he did notice the mosquitoes and sandflies: 'In vain the wearied seamen seeks for repose, his winged tormentors multiply as the night advances, and ever on the alert, incessantly hover around him . . . until he hastens upon deck, there to await, in no happy mood, the break of morn'.

He seemed to have failed to suspect them as a direct cause of fever, and his treatment was bloodletting, saline purges and calomel, along with his contemporaries, but he did recognize that 'this system of treatment . . . possessed no power of controlling the progress of the remittent fevers of inter-tropical Africa'.

For sixty years the Royal Navy established a squadron of ships engaged in patrolling the coast in an attempt to capture slave ships, free the prisoners and drive the evil trade out of existence. The health of the sailors was often in as great peril as that of the slaves, who were at least immune to most of the fever. Surgeon Commander Padwick wrote (1922): 'This coast is certainly the father and mother of all fevers; its history is practically the history of malaria and yellow fever'.

Nine years after he was appointed naval surgeon to H.M.S. *Cyrene* James Boyle recorded his observations in a book published in 1831 entitled *A Practical Medico-Historical Account of the Western Coast of Africa*. Referring to Ghana he wrote that there was: ' . . . scarcely an instance of a European arriving here who is not attacked with the endemic fever, either immediately on his arrival or within four months', and he regarded the Niger Delta as worse than Ghana, and the Bight of Benin as the worst of all the West African squadron stations (Boyle, 1831).

In 1869 the *Lancet* published an obituary of Dr. Alexander Bryson. It alleged that he was more concerned with statistics than men but this was unfair, and probably only occurred because he was a controversalist who in 1848 had quarrelled over the nature of the contagion of yellow fever with Sir William Pym, inspector general of quarantine. Bryson's *Report on the Climate and the Principal Diseases of the African Station* (1847) had highlighted the severe mortality of naval personnel in West Africa, the worst station in the world, and was the result of an inquiry instigated by the Chief Medical Officer, Sir William Burnett. Although neither Bryson nor Pym were wholly right or wrong as to the contagious character of yellow fever, Bryson's reforms following the inquiry were of considerable value. He recommended the cessation of bleeding and mercury in the treatment of fever, the staying out at sea of naval ships, the provision of cleaner clothing, and more emptying of ships bilges. Yet disregarding both Bryson and Pym in 1852 the General Board of Health in Britain published a Second Report on Quarantine for yellow fever in which it was considered that malaria and yellow fever were identical, the disease was not communicable from ship to ship, or ship to shore, and that no quarantine was needed! (Bryson, 1849; Carter, 1931; Lloyd, 1949; Lloyd and Coulter, 1963).

As though the West Coast had not sufficient disease of its own, in the eighteen sixties there are several records of yellow fever being transmitted from South America and the West Indies. Scott (1939) mentioned specific epidemics in 1862 at Calabar and Benin, and in 1864 in Lagos, and Carter (1931) attributed a yellow fever epidemic on Fernando Po to the ship *Rosa del Turio* which sailed from Havana in 1866.

TRADING EXPEDITIONS

Some of the great commercial firms of West Africa were born in the decade under consideration. John Holt started as a young oil trader in 1869 and survived the fever in his little thatched hut storerooms, or 'factories' as they were known. Many traders lived in the river estuaries on old East Indiamen which were moored or grounded in the river: the trapped water in the bilges breeding mosquitoes in great numbers (Thorp, 1956).

Three important men connected with the commercial ventures of West Africa died in the eighteen sixties: Macgregor Laird in 1861, Dr. James Ormiston McWilliam in 1862, and Dr. William Balfour Baikie in 1864. Laird was a shipbuilder of Birkenhead, and sponsored the first steamship expedition up the Niger in 1832, and the African Steamship Company which was the predecessor of the Elder Dempster Shipping Line of Liverpool. The majority of the crew died of fever but he persisted until annual shipping voyages up the Niger were established. Dr. McWilliam, the chief medical officer of the second Niger Expedition (sponsored not by Laird but by the Government under T. F. Buxton, one of the greatest anti-slavery leaders in British history) was a Scot from the Orkneys. He put tremendous efforts into preserving the health of the crews of his three steamers, including an air-conditioner designed by Dr. Reid, who had just designed the air-conditioning of the House of Commons. Both Laird and McWilliam nearly died themselves but their crews did not die from neglect but from misunderstanding of the cause of the fever.

McWilliam had his ships steam rapidly through the forest zone to avoid the miasmata, and confined all his men below decks, even adding chlorine to the atmosphere from the 'medicator'. The decks had canvas screens erected around them to keep off the early morning mists, and even respirators were issued. The atmosphere below decks was so impossible in the end that they reversed the pumps so that the air was evacuated by the medicator instead of being pumped in, and holes had to be cut in the iron decks. It was all to no avail. Yet on board McWilliam had quinine, but he used it only during treatment and not prophylactically (McWilliam, 1843; Allen and Thomson, 1848; Willcox, 1948; D. N. B., 1909; Laird and Oldfield, 1837).

Dr. Baikie, an explorer and linguist and missionary as well as surgeon, published an account of the Third (1854) Niger Expedition in 1856, in which there was no loss of life. He had taken David Livingstone's advice and used quinine prophylactically on board the steamer *Pleaid.* Gradually settlers, missionaries and government personnel began to survive as the value of quinine became clear, although it was not really until 1943 that malarial prophylaxis, then with mepacrine, was used systematically by white troops, airmen and seamen in West Africa (Crowther, 1855; Pedraza, 1960; Baikie, 1856; Rexford-Welch, 1958).

But even though quinine began to be used preventively appreciation of the aetiology of fever remained singularly confused. It was still a long time before the work of Walter Reed or Ronald Ross and their teams, indeed the microbial theory of disease itself was still in its infancy. Dr. Thomas Winterbottom wrote as follows in 1807 from Sierra Leone. In 1867 there was virtually no change in his view:

> Though the most common cause of fever in hot climates be the air which blows over marshes, yet when the fever is once introduced among a number of people, it is very apt to become infectious. The signs of an unhealthy country are great swarms of flies, mosquitoes, etc., thick fogs lying on the ground for sometime after sunrise, heavy dews, cold nights, preceding very hot days. Healthy situations are those when the soil is dry and elevated, as upon the sides of a summit of a hill, at a distance from marshes and stagnating waters. In general, places open to the sea, bordered by a sandy or gravelly beach, in fleets lying at anchor off a swampy shore, that the ships nearest to it have proved very unhealthy, while the crews of vessels more distant from it a mile or more remained in perfect health.

From the long-term point of view the most important individual on board the Niger Expeditions was not McWilliam, Laird or Baikie but a young Nigerian clergyman, Samuel Adjai Crowther, who was getting his first impressions of the strategic value of the great Niger river basin. We shall return to him later. Thus in the eighteen sixties it would be impossible to divide the medical work done by different agencies at all clearly. For example the 1841 expedition was sponsored by the Government, yet it carried naval officers, scientists and missionaries and African ministers, and its object was to set up commercial enterprises. Again, the earliest C.M.S. missionary doctors were drawn from the Naval squadrons, and others entered the Colonial Medical Service in Freetown from previous service in the Navy.

GOVERNMENT SERVICES

A true government medical and sanitary service did not begin until well after the eighteen sixties for most of West Africa, except in Freetown and Bathurst. Even in Freetown a Board of Health was not set up until 1867, and this may well be attributed at least in part to the efforts of a great sanitary reformer who was a Nigerian doctor in the British Gold Coast Army: Dr. James Africanus Beale-Horton. Horton published in that year his *Physical and Medical Climate and Meteorology of the West Coast of Africa,* in which, quoting Florence Nightingale in his support, he urged better sanitation in the cities and the creation of a public health inspectorate: 'There is nothing so necessary for the healthy growth of a community as the drainage and sewerage of the towns they inhabit'.

Like Crowther, Horton was brought up in Freetown yet a Nigerian by birth. Together with his friend, a Yoruba, Dr. William Broughton Davies, he also entered Fourah Bay College, and the two had closely parallel careers. They both studied medicine in London, together with a Dr. Manley, both were sponsored by the C.M.S., both qualified M.R.C.S. in London. They both took their M.D.s in 1859, Davies at St. Andrews and Horton at Edinburgh. They both joined as medical officers in the British Army in Ghana, and both retired as Lt. Colonels, in 1881 and 1880 respectively. Horton is the best remembered, for he wrote and spoke in so forthright a manner on politics, medicine, and public health. He even left his house in Freetown in his Will as a university building (Fyfe, 1962; Easmon, 1956).

The state of the public health of West African towns in the eighteen sixties was appalling, and the gradual growth of trade led only to further overcrowding and slum conditions. Before 1877 there was not even a single Inspector of Nuisances for Lagos, and the Sanitary Service did not come into being as such before 1897. A great fire swept through Lagos in 1877, but real improvements did not start until drainage of the swamps under the direction of the medically qualified governor, Sir William MacGregor, acting on the advice of Sir Ronald Ross in 1899; the widening of the streets to take a steam tram in the early years of the twentieth century; the lighting of the streets in 1872, at first by oil, and then in 1898 by electricity. The setting up of the Lagos Executive Development Board and its slum clearance programme did not occur until the plague epidemic of 1924, but even back in the eighteen sixties something had begun: Acting Governor William McCoskry began to reclaim and build the Marina, and Governor Glover acquired the neighbouring swampy island of Ikoyi as a government residential district in 1865. This proved the first of the new, planned estates of Lagos, followed in the 1930s by Yaba, and in the 1950s and 1960s by Apapa and Surulere (Thorp, 1956). The sight of dead bodies of captives impaled on stakes in the lagoon, slave barracoons, and human sacrifice, seen by missionaries in 1860, had gone for ever.

MEDICAL MISSIONARIES

Several missionary societies of the modern missionary movement were well established by the eighteen sixties, commencing with the Sierra Leone Company, which had grown out of the abolitionist movement led in Britain by William Wilberforce and Fowell Buxton and strongly supported by the Society of Friends, the early Methodist Missionary Society and the Clapham sect of Low Churchmen. Catholic missions of the earlier era had all died out, but recommenced with the arrival of nuns on Goree Island off Dakar in 1819. By 1823 Anne Marie Javouhey was invited by the Governor of Gambia to supervise the nursing of Bathurst Hospital and later to reform Freetown Hospital as well. She caught yellow fever herself but fortunately recovered. Nuns did not reach Nigeria until 1886 (Bane, 1956).

A brilliant Catholic leader from India, Bishop Melchior Joseph de Marion Bresillac started the Society of African Missions by landing at Freetown in March 1859 with six Fathers, at the height of a yellow fever epidemic. He refused to stay away and died, together with five of the six fathers, in a few weeks. It was exactly 400 years since Father Bolano, the first Catholic missionary to West Africa, had arrived, and, with the return to France of Father Planque, the survivor, not a single Catholic missionary remained in West Africa. They did not return until Father Borghero, an Italian, landed in Dahomey in 1861 to begin dispensary and orphanage work. In 1862 he moved to Lagos.

Protestant missions on the West Coast were well established by the eighteen sixties, even as far east as Nigeria. There in 1845 the Church Missionary Society came under the Rev. David Hinderer and Henry Townsend, the Methodist Missionary Society under the Rev. T. B. Freeman who landed first in Ghana in 1838, the American Baptist Mission under the Rev. T. Bowen in 1850, and the Presbyterian Mission under the Rev. Hope Waddell. These were the men and women who established the first permanent medical work in Nigeria and Ghana: they rescued orphans and widows from the inter-city and slave trade wars, but they did not then build real hospitals. However the first true hospital in Nigeria, Sacred Heart at Abeokuta, was built by a Catholic priest, not fully medically qualified, the Rev. Father J. Coquard, in the year 1895.

Disease virtually wiped out the early Protestant missions also, killing or invaliding home all the Baptist missionaries by 1868. The American Civil War cut their small funds and for seven years no missionary came out. Their houses were destroyed, their grave-stones broken up and used for grinding pepper (Maddry, 1939). The Rev. Tom Bowen their leader, held miasmatic ideas of a most bizarre variety making Dr. Winterbottom's views in 1807 reasonable in the extreme:

> The healthiest places near a sickly river are immediately on its banks . . . the reason appears to be that malaria near the water is attracted by it

> and absorbed. The immediate banks of our Southern rivers are healthier than the higher places at a considerable distance.
>
> The leeward side of a town, other things being equal, is healthier than the windward, because the effluvium of the city is less deleterious than the malaria of the forest which it arrests.
>
> The house should stand in an airy situation. It has been found that the malaria is carried away from such places by the wind, so that they are healthy though the poison is generated on the spot.
>
> The sides and the tops of hills should be avoided for two reasons (1) The winds carry malaria to the highlands where it is retained, perhaps by the attraction of the ground, and (2) it travels with fogs, which are naturally drawn towards the hills.
>
> Leaves of trees, like water, appear to absorb malaria . . . should be planted near the house.
>
> Avoid the night air. Closely shut up in our rooms . . . If we must go out, it is said that breathing through a silk handkerchief thrown over the face is a protection. (Bowen, 1857)

He did however use quinine for fevers, and admit that bloodletting was seldom admissible. But his theory that hilltops were dangerous and low-lying riverbanks preferable, led to the ill-siting of many mission institutions. By the 1920s however Baptist hospitals began in earnest and some of the finest medical work by voluntary agencies is done by them today.

Bowen was invalided back to the United States by the eighteen sixties, but the Baptist Mission, and other similar missions particularly when they employed American negroes, West Africans or those of mixed descent, did better and stayed to work a lifetime on the Coast. The Methodist Mission sent Thomas Birch Freeman whose father was a West African in Britain and his mother English: the Presbyterians sent Dr. Archibald Hewan, a West Indian doctor in 1855. Freeman stayed a long lifetime, doing dispensary work where he could in Ghana, Dahomey and Nigeria and was an outstanding leader and peacemaker. Dr. Hewan opened medical work in Calabar, with the Rev. Hope Waddell and other famous Scottish missionaries in that historic old town. He stayed ten years before returning to work in Britain, outliving therefore all the C.M.S. medical missionaries who died in rapid succession in a few years of dysentery and fever. Thus as the first African doctor in Nigeria he was at work there ahead of colonial days. He returned to the

U.K. in 1864 to take his M.D. at Edinburgh, and sadly, did not return (Ajayi, 1965; Findlay and Holdsworth, 1922).

The Rev. Zerub Baillie, who had studied medicine at one time, opened a clinic in Duke Town, Calabar, while his colleagues Waddell and Edgerly opened similar ones in Old Calabar and Creek Town (MacDonald, 1960). After opening a second dispensary at Ikorofiong, Baillie worked until he was invalided home in 1865, worn out, having lost wife and child. He reached Liverpool but died before he returned to his native Scotland.

But the most significant of all the missions in the eighteen sixties was almost certainly the Anglican. Their first doctors were already dead by the eighteen sixties, Dr. J. N. Ashwood in Freetown in 1850, Dr. E. C. van Cooten in 1851, Dr. W. Hensman in 1853, and Dr. Irving in 1855 in Nigeria. The latter two had worked with naval expeditions prior to missionary work. Irving died of dysentery in Lagos after a year or two at Ibadan, Hensman appears to have committed suicide from an overdose of laudanum taken while depressed after fever, even though he had survived the tragic 1841 expedition. When one recalls that some treatment of fever then consisted of the application of sixty leeches in one day and native cupping, which was recorded for Mr. Townsend, senior C.M.S. missionary to Lagos, the suicide is not so surprising (Thorp, 1956). Irving and Hensman both tended a C.M.S. couple who have the rare tribute of nothing but praise from Nigerian historians: Anna and David Hinderer. During the Ijaiye war and for eighteen years they served Western Nigerian churches and schools, and cared for a large number of orphans in Ibadan. On their centenary, hundreds of Nigerian women wore dresses with the portraits and names of the Hinderers printed on the cotton, and they are still remembered (Hone, 1873).

Such long service was exceptional, and early C.M.S. missionaries were universally anxious to train Nigerian colleagues quickly, for which they had the full support of the Rev. Henry Venn in London at Salisbury Square. The very first recorded West African doctor to qualify, Dr. John Macaulay Wilson (Easmon, 1956) was sent to Britain from the Sierra Leone Freed Slave settlement in 1799. By the eighteen sixties three doctors came back to West Africa from Britain (Horton, Davies and Manley), whom we have seen already, sponsored by the C.M.S. In 1861 Dr. A. A. Harrison, Dr. Irving's successor, taught anatomy in Lagos to a few pupils so successfully that at least two of them went abroad to qualify medically, Dr. Nathaniel King and Dr. Obadiah Johnson, later joint author with his brother of the *History of the Yorubas*. It was in 1864 that the Rev. Samuel Crowther was consecrated Bishop of 'Western Equatorial Africa beyond the Queen's Dominions' in Canterbury Cathedral. His work from that year until 1879 in the valley of the Niger River led to the growth of a strong church and extensive preventive and curative services of hospitals, health centres, maternity homes, leprosy settlements and dispensaries of today. In the eighteen sixties it was an uphill struggle against war, superstition and problems of language and financial scarcity. His mission stations were often attacked, and during the Nupe Civil War of 1867 he was captured and held to ransom. He was helped to escape by a British government official, William Fell, who died afterwards from a poisoned arrow and thus gave his life to save one of the most remarkable, able, and noble men in the history of West Africa (Warren, 1960).

Thus many agencies began medical and health work in West Africa one hundred years ago. At that time perhaps the greatest contribution came from the voluntary societies, now still at work and on a larger scale than ever, but naturally as a much smaller proportion of the total medical work (Schram, 1967). Their work has been neglected, and largely forgotten. It is to be hoped that in the coming official history of medicine in West Africa, supported by The Wellcome Foundation, and so much needed by the profession in that part of the world, much of this wonderful work will be given the attention it deserves.

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The Dental Profession in the 1860s

by

N. DAVID RICHARDS

THIS PAPER reviews the developments within the dental profession and the evolution of dental services during the period 1860–1870. Its aim is primarily to present a detailed historical account of the non-clinical aspects of dentistry during these years; the principal focus is placed upon studying sociological and socio-historical aspects of dentistry, and, as a result of the author's lack of qualification to write about developments and improvements in methods of treatment and in clinical techniques, little is said about the clinical side of dental practice in these years. Oscar Wilde, in *The Importance of Being Earnest*, has written that 'it is very vulgar to talk like a dentist when one isn't a dentist. It produces a false impression'. It is then the author's intention, in considering dentistry a hundred years ago, to talk not *like* the dentist but *about* the dentist.

Before studying the events of the 1860s it is necessary to fill in some background information about the position accorded to dentistry and about the stage of professional development in the years immediately preceding those of the period of this present essay. Developments within the dental profession in the 1860s cannot objectively be seen in a vacuum, and it soon became clear that many of the prevailing factors influencing dental conditions in the sixties have their antecedents to be found in the earlier history of dentistry and surgery and that these should be considered against a background of the social and economic forces at work during this period. It is above all important to remember that the organization of dentistry as a separate professional entity was a recent development—the first signs of and claims for a professionalization for dentistry, in this country at least, had not evolved until the first decade of the Victorian era. During these formulative years certain progressivelyminded dental practitioners began to try to organize themselves into societies and associations, and to develop dental surgery as a professional specialism.[1] Ignorance, empiricism and lack of organization characterized the dental scene, and, despite the efforts of such visionaries as George Waite, J. L. Levison and James Robinson,[2] it was not until the mid-nineteenth century that dentistry began to organize itself on any firm footing. Before this period dentists were basically divided into two quite separate sections—on the one hand there was a small group of educated men who held medical and/or surgical qualifications, and on the other hand a large group of uneducated and unqualified persons—in both factions bitter jealousies abounded keeping men apart. Dentistry was at this stage relatively undeveloped as a clinical specialism; practitioners guarded their secrets of practice and technique, text-books were few, and formal teaching of the dental art was singularly lacking. Alfred Hill has summed up the picture well: 'without a common centre or bond of union, the dentists of that time practised exclusively and primarily for their own interests'.[3] He continued: 'Whilst the dentists of this country were thus slumbering, or at any rate inactive, their brethren in America had been organising Dental Colleges'.[4] Following unsuccessful attempts to found dental departments within medical faculties, dentists in North America launched themselves down an independent line and founded separate dental colleges—events which were to have repercussions on the progress of the dental art in this country.[5]

[1] For a detailed account of the dental scene during this period see N. D. Richards, 'Dentistry in England in the 1840s: the first indications of a movement towards professionalization', *Med. Hist.*, 1968, **12**, 137–52.

[2] Pleas for the creation of a Faculty of Dental Surgeons and of an organized dental profession, and for the reform of dental practice are found in George Waite's *Appeal to Parliament, the Medical Profession and the Public on the Present State of Dental Surgery*, London, 1841; in J. L. Levison's letter in the *Lancet*, 1840–1, **i**, 598; and in the editorial comments of James Robinson in the *British Quarterly Journal of Dental Science* (1843—two editions only), and *Forceps* (1844–5).

[3] Alfred Hill, *The History of the Reform Movement in the Dental Profession in Great Britain*, London, Trubner, 1877, p. 32. This most useful publication provides an excellent background source to dental developments during the years 1850–1870.

[4] Hill, op. cit., p. 36.

[5] William Gies, 'Dental Education in the United States and Canada', The Carnegie Foundation for the Advancement of Teaching, Bulletin No. 13, New York, 1926, p. 38–40.

A. N. Whitehead has written—'each profession makes progress but it is progress in its own groove'.[6] The dental profession in addition to making progress in the 1860s was to some extent also finding its own groove—a track separate from surgery. The organization of dental societies, the inauguration of securely-backed journals, and the founding of dental hospitals and schools became the three prongs which were to lift dentistry from an itinerant and poorly recognized craft to semi-professional and at length professional status. Significant developments in all three of these aspects of dentistry took place in the latter half of the 1850s, and, although this paper is directed at tracing developments within the sixties, it is necessary to recount some of the earlier events, since these were profoundly to affect the course of dentistry in the period of our present study.

The first indications that movements were under way to reform the prevailing chaos in dental matters followed the publication, in the *Lancet* of 25 August 1855, of a letter from Samuel Lee Rymer, who urged the necessity of founding a College of Dental Surgery. A series of open public meetings of interested practitioners followed at the London Tavern in Bishopsgate. Meanwhile, unbeknown to these would-be reformers, a number of eminent practitioners had been meeting together in November 1855, and, on 11 December 1855, had forwarded to the Royal College of Surgeons a memorial petitioning for the institution of an examination and for a department of dental surgery. The adoption of such a course of action, they felt sure, would prove a great boon to practitioners and also secure a manifest advantage to the public. The sending-off of this petition appears to have been a closely-guarded secret, and was not made known until October 1856, during which times the public meetings organized by Rymer were being held. The publication of news of this memorial created a sense of irritation, and criticism of the secret actions of these high-minded practitioners followed, largely as a protest at the way reform was initiated, not by democratic processes, but by the actions of a small coterie of practitioners. No reply was immediately received from the Royal College; whilst the memorial was however lying at the College, the memorialists, as this small group became known, agreed to constitute themselves into a

[6] A. N. Whitehead, *Science and the Modern World*, Cambridge, 1926, p. 275.

scientific society to be called the Odontological Society of London. The date chosen to make this event known was a curious one, falling as it did the day immediately prior to that—11 November 1856—on which Rymer had called a further meeting to propose the founding of a society of dentists and a movement to reform dental affairs. Feelings ran high, and there thus began a bitter feud between Rymer and his associates, who subsequently formed themselves into the College of Dentists of England, and the memorialists—a feud which was to continue some seven years. Quite suddenly then, from a state of disorganization and estrangement, two important societies of dentists had been organized, in antagonism one to the other. The issue was not, was reform necessary, but rather how should dental surgery be reformed—as a branch of surgery or as an independent profession? As we have learned, the dental profession in North America, after several rebuffs from the medical and surgical fraternity, chose to follow the independent line—in this country, for the time being, at least, development along the two diametrically opposed poles was proposed.

The second important prong, which, we have suggested, marked the emergence of professionalism, was the beginning of dental journalism. Largely following the efforts of James Robinson, attempts had been made, during the years 1843–5, to initiate two dental periodicals—these publications were however short-lived. The years 1856–7 saw the emergence of a more secure dental journalism. Two rival journals appeared—the *British Journal of Dental Science*, which, although offered to Rymer and his supporters, subsequently passed into the hands of those sympathetic to the Odontological Society, and the *Quarterly Journal of Dental Science* (in 1859 to become the *Dental Review*, a monthly publication) which espoused the cause of the College of Dentists. These rival journals provided outlets for vehement criticisms, which in due time were met with further criticisms, and no quarter was given between the two parties. Nevertheless despite such asseverations, these journals also paved the way for raising the professional status of dentistry, and afforded a means of communication and interchange of ideas.

Rivalry ensued also between the London School of Dental Surgery, founded together with the Dental Hospital of London by the supporters of the Odontological Society, and the Metropolitan School of Dental Science, together with the National Dental Hospital, established in 1861, which were instituted by the College of Dentists. Both factions played their role then in developing a system of organized dental education and institutions, where a body of patients could be seen by the student.

Although they were separated by a wide gulf, it should not be thought that an amalgamation of the two professional societies was a total impossibility. Largely through the initial instrumentality of Robert Reid of Edinburgh an informal meeting was held in August 1857 between John Tomes, representing the Odontological Society, and Thomas Underwood of the College of Dentists. Terms for an amalgamation were drawn up—the proposed united dental society to be known as 'The Institute of British Dentists'. For a time the rancour between the two rivals died down, and invitations to meetings and dinners were exchanged. With only small numbers voting and by the slender majority of 34 to 27, the College of Dentists however decided to turn down the merger, believing as they did that the Royal College of Surgeons would never grant the dental diploma, for which the memorialists had petitioned. Anyway, they argued, why should we be examined by doctors of medicine and surgeons, when we know a great deal more about teeth than they do? The die was cast, and further dissensions and bitter controversies followed—these hampered much of the good work, which, it is fair to say, emanated from both sides.

It was not only in London that moves were made to institute some recognized form of dental qualification. John Smith made representations to the Royal College of Surgeons of Edinburgh requesting steps to be taken to institute a diploma in dental surgery; these proved unsuccessful, so he set himself the task of establishing a dental hospital and school in Edinburgh—a development, which, as we shall see, was to be mirrored in other parts of the country in the late fifties and early sixties.

The whole course of dental education was however changed by the incorporation of a clause in the 1858 Medical Act, which gave the Royal College of Surgeons of England the power to institute examinations and grant certificates of fitness to practise dental surgery. This very important development followed upon a statement from the Secretary of the Royal College of Surgeons, Edmond Belfour, who, in reply to a memorial dated 4 April 1857, from the Odontological Society, suggested that they should petition Parliament and have a clause inserted into a Medical Bill. Discussions followed, and on 6 July 1858, at the Committee Stage of the Medical Practitioners Bill, Mr. A. J. Beresford Hope, M.P. for Maidstone, moved the following clause: 'It shall notwithstanding anything herein contained be lawful for Her Majesty, by charter, to grant to the Royal College of Surgeons of England power to institute and hold examinations for the purpose of testing the fitness of persons to practise as Dentists, who may be desirous of being so examined, and to grant certificates of such fitness'.[7] The clause was agreed to without a single dissentient vote and the complete bill received the Royal Assent on 2 August 1858, and so became law. Thus was the first formal recognition given to dentistry; henceforth a dental qualification was to be recognized, and an attempt made to solve the problem revealed in the *Quarterly Journal of Dental Science*, which had observed: 'it is monstrous that the public should have no means of knowing who is the *dentist* and who the *empiric*'.[8] The inception of the dental diploma was indeed of great significance—as Hill puts it: 'over all the confusion and strife of party feeling which had characterized the year, this fact stood forth prominently as that which would most unmistakably affect the future of the dental profession. It was the most conspicuous event of the time, and it only remained to take the necessarily consequent steps thereupon, and await the manifestation of professional feeling on the subject, which time only could develop and declare'.[9]

Under the 1858 Medical Act, a register of medical practitioners was started under the control of the General Council of Medical Education and Registration of the United Kingdom. This new controlling authority was a means of bringing unity to the medical profession; it is important then in understanding developments within the dental profession, evolving as it did as a specialism or branch of surgery, to notice that it was only in these years that medicine itself was becoming united, and, in many eyes, respectable.

[7] Hansard's Parliamentary Debates, vol. 151: 1000. 21 and 22 Victoriae 1857–8.

[8] *Quart. J. dent. Sci.*, 1857–8, **1**, p. 2.

[9] Hill, op. cit., p. 142.

In the years between 1830 and 1858 the structure of a reorganized system of medical education was emerging. Changes in the system of medical education, the corpus of medical knowledge, the social structure of the profession, and also of the wider society, made possible the reorganization of the medical and surgical profession along planned lines.[10]

Although the Charter granted to the Royal College of Surgeons on 8 September 1859 certainly established the Licence in Dental Surgery, it also provided that the diploma should not confer on its holder the right to have his name inserted in the Medical Register. This was then, as Sir Wilfred Fish has remarked, 'only a beginning to the establishment of an organised profession'.[11] This refusal to register the L.D.S. was, as we shall see, an important factor in the starting of a campaign in 1870 by Charles Fox for compulsory education and registration—a movement which was to culminate in the election of the Dental Reform Committee in 1875 and the passing of the Dentists' Act of 1878.

The years 1856–9 saw then the emergence of claims for the recognition of professional status for dentistry—these were however early days and much remained to be done. A letter-writer to the *Quarterly Journal of Dental Science* summed it up in the following words: 'this is a critical and important period to the profession and may be compared to a stage in life bordering between youth and manhood'.[12] Despite the noble plans for the development of dental education, and for the professional recognition of dentistry, the internecine struggle initiated in the 1850s was to act as a barrier to immediate progress along the road to professional respectability.

A very lucid account of dentistry at this time is presented by the comments of an American dentist who visited the country in 1859. In particular he remarked upon the contrast between the manner in which the public ranked the medical and dental professions. Speaking of dentistry, he commented: 'a dentist was assessed as an icicle by the entire public, and accepted solely as a doubtful expedient in the distant hope of relief from a very indifferent complaint: in fact calculated to produce injury rather than benefit.

[10] S. W. F. Holloway, 'Medical education in England: 1830–1858—a sociological analysis', *History*, 1964, **49**, 299–324.

[11] Sir Wilfred Fish, 'A profession in the making', *Brit. dent. J.*, 1959, **107**, 19.

[12] Letter from James Karran in *Quart. J. dent. Sci.*, 1857–8, **1**, 57.

The dental profession was popularly regarded as discreditable in the extreme.'[13]

The French philosopher, Jean Jacques Rousseau, has written: 'it matters not what sort of trials we have provided they produce their proper effect'. Trials there were as we have seen—but what was the effect of all these trials and tribulations upon the future development of dentistry? It was in need of a vast improvement in both its organization and in the appreciation and respect accorded it by the public. Progress was in hand, and the later years of the 1850s offered the prospects of better things to come. As Hill says, 'the year 1860 opened under circumstances which fostered the highest expectations of that portion of the profession who either from the first, or gradually, and afterwards, approved of a connection with the Royal College of Surgeons'.[14] Dentistry, prior to the introduction of a formal curriculum and course of study, was regarded as a trade, which might be acquired during an apprenticeship, or undertaken without training and learned through unguided experience. With apprenticeship the curriculum had necessarily to be elastic, and the social status of the practitioner was lowered since his training tended to be associated with that of a tradesman. The plans for the L.D.S. examination offered dentistry a road along which to progress from trade to profession.

The news of the proposed plans for the introduction of the dental diploma at the Royal College of Surgeons caused some secession from the ranks of the College of Dentists, and exposed a palpable and painful condition of weakness in that body. Efforts at organizing a dental qualification quite separate from the aegis of the surgical college were however redoubled by those who remained in office at the College of Dentists, and a plea for a charter was submitted to Her Majesty from George Waite, President of the College, and five other signatories of the College. They noted that 'owing to want of recognised qualification the profession of dental surgery in England does not hold that position which its growing importance demands',[15] and they asked for a charter of incorporation for a Royal College of Dentists of England, and the right to grant diplomas. Whilst the

[13] *Am. J. dent. Sci.*, 1859, n.s. **9**, 350.
[14] Hill, op. cit., p. 167.
[15] Hill, op. cit., pp. 397–8.

Royal College of Surgeons were preparing a curriculum for their L.D.S. qualification so then were the College of Dentists also active in the preparation of their qualification, to be known as the M.C.D.E. The profession therefore had laid before it, from which it might choose, the two proposed qualifications—the one already legally recognized, the other not.

Although many of the town and country practitioners were on their side, the hope of the College of Dentists for an independent existence for the dental specialism was dimmed, if not eclipsed by the powerful forces, weight of authority and sense of dignity at work in support of the Royal College of Surgeons, on whose Council Board the dentists would however be totally unrepresented. The independent principle had sufficient force and merit in it to influence a considerable number of dentists. Indeed the zeal with which the supporters of the College of Dentists strove to meet their goals, went not unnoticed by the other party, who brought forth a public protest against the independents. Under the heading of 'Diplomas in Dental Surgery'. there appeared the following notice in *The Times* of 28 February 1860: 'We the undersigned [94 names] practitioners in dental surgery, deem it our duty publicly to protest against the proceedings of this so-called College of Dentists of England, as being wholly unsanctioned by law, unwarranted by precedent or professional usage, and opposed to the opinion and feeling of the great majority of the leading practitioners in dental surgery. And we more especially protest against the issuing of diplomas without legal authority, believing that such diplomas are calculated to mislead the public, by whom they may be mistaken for the legally-authorized dental diplomas to be granted by the Royal College of Surgeons of England through its dental department'.[16]

Thus was the gauntlet thrown down—the challenge was speedily taken up, and a counter-protest, sent out by the independents and signed by a large number of members of the medical profession, was inserted in *The Times* of 8 March 1860. They contended that, in taking the title of College of Dentists, they had infringed no legal right whatsoever, and their diploma was suggested before the certificate to be issued by the College of Surgeons—it was therefore

[16] *Transactions of the College of Dentists of England*, 1860, p. 43.

a wicked fabrication to say they were trying to confuse the public. On the contrary they wished their certificate to stand out as an independent and honourable distinction, indicating the one principle, that the organization and status of the dental profession could be better provided for by an institution of its own than by its becoming an appendage of the College of Surgeons.

The College of Dentists were however presented with a *fait accompli* when it was announced that the first examination for the L.D.S. would take place on 13 March. So many—43—candidates presented themselves, amongst them some distinguished names, that two further days had to be allotted for the examination. The granting of these certificates was noted in the *Lancet* by a further measure of support for the Odontological Society: 'it is sufficiently obvious that the diplomas of a self-constituted college without any charter or other recognised legal standing, will labour under a great disadvantage as compared with those of the College of Surgeons. Any attempt at simulation would so degrade the College of Dentists as to produce its immediate dissolution'.[17] But simulate they did, and they proceeded to confer a diploma upon their members, not simply on account of their membership but also as a guarantee of the holders' fitness to practise dentistry. Despite the prophecy of the leader writer of the *Lancet*, the dissolution of the College of Dentists was not immediate. The *Medical Times and Gazette* and the *Medical Circular* supported the independent principle—believing as they did that the progress of specialities in the College of Surgeons was going unchecked and that diplomas were now being awarded to men who had not complied with the educational regulations of the college.[18] The *British Medical Journal* went one further in supporting the independents—'they disdain to creep into the medical profession surreptitiously and disgracefully. They are determined to prove that the worth of their profession is not to be increased by their buying the privilege of becoming appendages of a body in which they would be regarded as intruders'.[19]

So the battle continued—with the L.D.S. instituted, and despite a further protest on behalf of the independents signed by three

[17] *Lancet*, 1860, **i**, 253.

[18] *Med. Times Gaz.*, 25 February and 10 March 1860, and *Medical Circular*, 29 February and 21 March 1860.

[19] *Brit. med. J.*, 10 March 1860.

hundred medical practitioners, the writing seemed to be on the wall, and henceforward the star of the College of Dentists was on the wane. As Samuel Cartwright rightly remarked at the opening of the London School of Dental Surgery on 30 April 1860, 'all we require now is union, not schism, and a desire to do the best for our common weal, and the dental profession of England, as a whole, will command respect and position'.[20]

As we have already noted, the licentiates in dental surgery were not to be listed on the Medical Register. A leading article in the *British Journal of Dental Science* considered the registration of dentists to be a step in the right direction: 'it remains only to protect both the profession and the public. The machinery for this purpose is already at hand. Let the Medical Registration Act be extended to dentists and the thing is done'[21]—words which were later in the decade to be re-echoed with cries for the compulsory education and registration of the dentist. As for the freedom of anyone to adopt the title of surgeon-dentist, a decision of the Court of Queen's Bench, in the case of Ladd versus Gould, on 21 January 1860, had demonstrated that anyone was free to adopt the title of surgeon-dentist—many charlatans there were, allied to neither educational movement, who were ready to take the title. Despite the presence of the two opportunities of advancing the dental art, it is clear that many who practised dentistry allied themselves to neither camp and remained apathetic to educational reform and to improvements in professional status.

The rival bodies continued to oppose each other in earnest; one real bone of contention was that of the practice of dentists who advertised their services to the public. Was dentistry to be regarded as a trade or as a profession?—this was the question to be asked. Following a trenchant article in the *Lancet* on the unprofessional conduct, by means of advertising, of members of the independent college, George Waite, their president, was moved to apply the 'tu quoque' argument back in return at several prominent members of the Odontological Society.

For the independent party the event of 1861 was the inauguration, on 11 November, of the National Dental Hospital, Great Portland

[20] *Brit. J. dent. Sci.*, 1860, 3, 302.
[21] Ibid., p. 285.

Street. Thus now both parties were supporting a dental school and a hospital. As Hill says: 'each side congratulated itself upon its career, and was determined to pursue its course with unabated vigour. Conflict, it was evident, was not to cease and the hopes of union, and consequent peace between opposing parties, were still faint and dim'.[22] Towards the latter end of 1862, however, faint whispers of peace were heard. Samuel Rymer, a pillar of the independent movement, had felt that the institution of the L.D.S. had destroyed all hopes of establishing his ideals. The expectation of obtaining a charter for an independent college appeared in vain—the College of Dentists had done much to improve dental education, but one mighty desideratum remained—the possession of a charter. The prospects of this happening looked very unlikely. Consequently Rymer could not see the independent movement being able to establish on any firm basis either itself or its examinations and diploma. The early ardour and emotion had given way, in the face of reality, to an understanding that the interests of the profession would be well served by an amalgamation of the two rival bodies. Following informal discussions between Mr. Rymer and Mr. Vasey, an influential supporter of the Odontologicals, terms of amalgamation were proposed. Since the earlier amalgamation plan had been rejected by the College of Dentists, they were now to take the initiative. Accordingly a new society, the Odontological Society of Great Britain, was to come into being; both parties, voting separately, resoundingly agreed to the union. The College of Dentists was dissolved on 20 March 1863, and the first meeting of the new society held on 4 May. As John Tomes indicated at the special general meeting of the Odontological Society called to consider the amalgamation, what was needed was a sense of brotherhood amongst dentists and a common desire, by abandoning unprofessional practice and advertising, to advance the prestige of the profession. That dentists away from the metropolis were to be respected was evidenced by his statement that 'there are many practitioners unknown to us who hold as high a position in town and country as those who frequent this room. We are too apt to class those we

[22] Hill, op. cit., p. 188.

do not know with the inferior men with whom we happen to be brought into contact'.[23]

Thus was the union, to which many had looked forward for seven long years, achieved. The independent approach had set in motion agencies and ideas from which the profession had benefited; it failed, Hill asserts, simply because it was surpassed.[24] The birth of the new society introduced a new era, and 'it was not only the formal cessation of hostilities between parties who never should have been in opposition, but also the blending of hitherto hostile forces in a common pact for the common good'.[25] Now, it seemed, was the winter of the dentists' discontent made glorious summer.

Thus far we have noted the effect of the organization of dental societies, and of an established dental press in raising the status of dentistry. There remained other further and important development—an improvement, through the institution of dental hospitals and schools, in standards of education. The first half of the nineteenth century had seen a large growth in the numbers of hospitals—a growth made possible by money given by the public. According to Abel-Smith, however, 'the major impetus in channelling charitable bequests and donations came increasingly from doctors. They wanted hospitals for teaching and research'[26]—so too on a smaller degree did the dentists. The years of the 1850s saw the creation of special hospitals, for example, the children's hospitals at Liverpool (1851) and Great Ormond Street (1852)—in the wake of these followed a number of dental hospitals. The first two efforts, both destined to be short-lived, to establish dental hospitals or dispensaries were made in London. About 1840 there was founded at Windmill Street, off Tottenham Court Road, the London Institution for the Diseases of the Teeth,[27] and in 1855 C. J. Fox established the London Dental Dispensary in Clarence Gardens, Regent's Park. These were very small-scale institutions, but they were the forefathers of the two dental hospitals in London—the Dental Hospital of London and the National Dental Hospital—to which reference

[23] Hill, op. cit., p. 199.
[24] Hill, op. cit., p. 201.
[25] Ibid.
[26] Brian Abel-Smith, *The Hospitals 1800–1948*, London, Heinemann, 1964, p. 16.
[27] This institution was known to have a number of pupils, see *Forceps*, 27 July 1844.

has already been made. It should not however be thought that the provincial dentists were lacking in their appreciation of the benefits to be gained, by both the public and profession, from the establishment of dental hospitals. These years also saw the founding of dental hospitals and dispensaries at Birmingham (1858), Liverpool (1860), Edinburgh (1860), and Plymouth (1861).

These institutions, besides affording dental services to the poor, provided a ready source of patients on whom the student could learn to develop his dental expertise, and to whom the benefits of conservation, in addition to the instant relief of pain through extraction, could be demonstrated. The initiative for establishing such institutions, supported by charity, came from dentists themselves, the broad vision and charitableness of whom was manifested at a period when communal service, let alone public or social service, was not a prominent factor. Henceforward dental service would be made available to a section of the community which hitherto had, for the sake of economy, been driven to seek relief from cheap and inexperienced practitioners, and various quacks. A steady growth in the work of these dental hospitals can be noted—for example the number of patients at the Dental Hospital of London increased from 2,116 in 1859 to 12,978 in 1864 and 17,926 in 1869. This was by far the largest dental hospital but all the institutions showed increases in the numbers of their patients. The work undertaken at these hospitals showed up the magnitude of the dental problem—a leading article in the *Dental Review* reminded its readers that 'numerous as are the ills which flesh is heir to, few are so universal as diseases of the teeth'.[28] It continued: 'those who gain their bread by the sweat of their brows seldom pay attention to their teeth until they become a source of suffering or annoyance . . . in consequence of the want of funds and the absence of a proper organization the treatment of the teeth has still, with rare exceptions, been limited to the operation of extraction'.[29] The need was then for dental hospitals and dispensaries to afford treatment to those who could not pay—in due course various abuses of these charities were reported and it was felt that they were being used by numbers of patients who could pay part if not all of the

[28] *Dent. Rev.*, 1862, **4**, 107.
[29] Ibid, pp. 108–9.

treatment costs. A further leading article feared that 'the very poor neither understand nor appreciate any operation in dentistry save extraction, and are undoubtedly the last persons to avail themselves of the real advantages of these institutions'.[30] Important though the establishment of special dental hospitals was, there was also the need for an improvement in the dental services available at the general hospitals—as one commentator remarked: 'it seems then to me to be essential for the progress of dentistry that in all the large towns where there is a county or town hospital a competent staff of professed dentists should be connected with the institution'.[31] Above all the dental treatment should be effected by dentists and not by surgeons practising as dentists engaged primarily in extractions.

Some measure of the public lack of appreciation for sound dental care was provided in the Preliminary Prospectus of the Committee of Management of the Dental Hospital of London (1859)—'statistical records show that the teeth of the lower orders are very defective, the average of decayed permanent teeth in young persons, between the ages of six and fifteen, ranging as high as 24–25 per cent, while in adults the percentage is very much greater; and it is not too much to affirm that in the majority of instances, such teeth are lost to the poor, after much suffering for want of timely remedial treatment'.[32]

The ignorance of the layman made it possible for the individual practitioner to exploit the market, and so lead dentistry into disrepute despite the noble endeavours of certain educationally-minded practitioners. One writer noted that the expert practitioner was adjudged as he was the best and quickest at extraction,[33] whilst another warned his readers not to conclude that all inferior dental work belonged to the past, because there were still fogies, old and young, who failed to keep pace with progress.[34] The status accorded to the dentist was lowly, and one dentist stated that 'no gentleman of moderately good means would think of making his son a dentist unless he had a lucrative practice to leave him'.[35] Commenting upon

[30] *Dent. Rev.*, 1864, **6**, 59.
[31] B. J. Richardson, *Archives of Dentistry*, 1864, **1**, 109.
[32] Quoted in the *Dent. Rev.*, 1864, **6**, 56.
[33] B. J. Richardson, *Archives of Dentistry*, 1864, **1**, p. 103.
[34] L. J. Platt, *The Domestic Guide to a Good Set of Teeth*, 1862.
[35] *Brit. J. dent. Sci.*, 1866, **9**, 62.

the state of the profession in 1860, the *Dental Review* suggested that 'the number of dentists in England is very difficult to determine. Those in pure practice are perhaps within twelve hundred, but there is a large body of men who combine, more or less, the business of the Dentist with that of the Chemist and Druggist. Of dentists, in the simple sense of the word, there are in London alone about four hundred'.[36] According to the 1861 Census returns there were 1549 persons in England who listed themselves as dentists,[37] that is there was one dentist to every 12,236 persons in the country. If not in the eyes of the public, dentists were, at least for Census purposes, regarded as a profession.

To return to professional developments after the amalgamation of the two rival dental parties—one of the strongest evidences of the fusion was provided by the submission of several leading members of the independent faction in offering themselves for examination for the L.D.S. A further development was the establishment of a Dental Students' Society at the Dental Hospital of London—this was to be a sort of junior Odontological Society. Amid an increased sense of co-operation and fraternity the profession was slowly developing, and as the *Dental Review* noted, 'for the first time in the history of our profession in this country, there exists amongst us a feeling of animosity and kindred fellowship, which is as remarkable as it is hopeful'.[38] There were still however questions left unanswered —what and how was the dental student to be taught? Despite the recognition of hospitals and dental hospitals as providing bases for the completion of the course for the L.D.S. examination, it became increasingly aware that the L.D.S. was shunned by many—a willing public was calling for their services and, even without this pro-

[36] *Dent. Rev.*, 1860, **2**, 2.

[37] Calculated from the occupational tables of the 1861 Census reports, in which dentists were listed as order 3 in the Professional Class I, which consisted of Persons Engaged in the Learned Professions or Engaged in Literature, Art or Science. 653 of those who styled themselves as dentists lived in London—the highest representations being in Pancras 94, Marylebone 90 and Kensington 53. In the provinces Manchester and Salford had 78 dentists, Liverpool 50, Hull 39, Bristol 36, Birmingham 32 and Leeds and Nottingham 26. According to these returns 27 of the 1549 dentists were aged between 10 and 15, and 205 between 15 and 20. In addition to those whose full-time occupational activity was dentistry, large numbers of medical and surgical practitioners, together with chemists and druggists, the numbers of whom are hard to calculate, practised some dental service at this time.

[38] *Dent. Rev.*, 1864, **6**, 47.

fessional appendage, a ready and often lucrative living was available to them.

One of the stipulations of the proposal that the Royal College of Surgeons grant certificates of proficiency to practise dental surgery was that these certificates would be available to all who had been in practice in 1859 without their passing through the prescribed curriculum for the examination. In September 1863 this term of grace expired—a fact which was responsible for a lowering of the numbers who offered themselves for examination, and which was later in view of the small numbers of practitioners who bothered to take the L.D.S. to raise further doubts in the minds of those who wished to see the progression from trade to profession. In order to increase the demands made for professional status it became clear that the numbers of properly-certified men should be increased as much as possible—an increase, which the *British Journal of Dental Science* advocated in its editorial columns. By 1865 only 280 dentists —136 of them London practitioners—had bothered to take and be awarded the L.D.S. It was noted that of 40 dentists in Liverpool there was only one L.D.S., and only seven L.D.S. amongst 51 dentists in Manchester.[39] One argument, put forward by J. H. Parsons of Halifax,[40] was that the L.D.S. movement was a failure since so few provincial practitioners, by virtue of the paucity of local provincial dental schools, and the subsequent need to study the curriculum in London, were able to afford either the time or the money to take the qualification—to this suggestion the *British Journal of Dental Science*, in a pointed editorial, retorted—did not the lawyers, the surgeons and the clergymen have to forsake the country for the town to undertake their prescribed courses of study? Could not dentistry expect its students to do likewise?

The fact remained however that the vast majority of practising dentists refused to submit themselves to examination—they were conducting lucrative practices and realized that the possession of a diploma, with its consequent ethical enforcements would curb their activities, particularly advertising. The need as certain leading practitioners saw it, was for a curb on advertising since, as Hill notes, 'nothing is at all likely to correct and eradicate the obnoxious

[39] *Brit. J. dent. Sci.*, 1865, **8**, 564.
[40] Ibid., p. 421.

system of advertising but a higher and proper admission of what a profession demands by those whose proclivities at present lean strongly towards public announcements'.[41]

One way towards professional status was by the inclusion of those with the L.D.S. in the Medical List. In the spring of 1865, when a bill to amend the 1858 Medical Act was being brought forward, a memorial, signed first by the three examiners on the dental board, Thomas Bell, John Tomes, and Samuel Cartwright, and then also by a number of licentiates in dental surgery, all of whom had full medical qualifications, was presented to the General Council of Medical Education and Registration. Their request for registration was refused on the grounds that all persons on the register should be fully qualified, and that the introduction of specialists would be confusing to the public—one of the very things the 1858 Medical Act had sought to avoid. It was suggested that after due application to Parliament a separate register of dental licentiates should be composed. Since the Medical Directory also did not list qualified dentists, Alfred Hill set himself the task of compiling a list of the dental licentiates[42]—action which was swiftly to lead to the Medical Directory following suit and listing qualified dentists. 'Outside still' however remained the cry of those who thought dentists should be on the Medical Register; for the moment they decided not to press the matter but to content themselves with the gain of certain small privileges such as exemption from jury service.

The calls for reform within the profession became more general—it was however realized how differently dentists organized themselves away from the capital. One suggestion, made by W. H. Waite,[43] was for the institution of branches of the Odontological Society, so that the privileges of membership might be more readily evidenced away from London; a warm reception was given to Waite's idea but again no immediate action followed. Although a separate organization, the Odonto-Chirurgical Society, founded in Edinburgh in 1867, was modelled on the Odontological Society and adopted the same laws.

[41] Hill, op. cit., p. 205.
[42] Alfred Hill, *The Dental Licentiates Directory and Local List; being an Alphabetical List of the Names of those Gentlemen who hold the Degree of Licentiate in Dental Surgery of the Royal College of Surgeons of England*, London, John Churchill, 1865.
[43] W. H. Waite, 'Local societies', *Brit. J. dent. Sci.*, 1866, **9**, 333.

A correspondent in the *British Journal of Dental Science* asked 'are we rising or sinking? Is the Dental Profession becoming elevated in the Social Scale, or is it rapidly sinking?'[44] A further leading article commented: 'notwithstanding all that has hitherto been done, quackery and humbugs are more rampant than ever'.[45] It was suggested that the dental diploma attached few privileges and little or no status on the holder, and that the Odontological Society did nothing to remedy the evils associated with dental quackery. What was slowly evolving was that the Odontological Society was becoming less and less politically-conscious and more and more of a scientific body devoted to the furtherance of clinical issues. A movement to produce political action was however at hand and feeling began to mount that some form of restriction of practice would be advantageous to the professional progress of dentistry—Henry Sewill in 1868 was among the first to suggest a restriction of the title 'dentist'.[46] The idea of registration appeared to be the paramount measure in the minds of some; the expectation of its accomplishment could not however be seriously entertained whilst the number of licentiates was so small.

Following suggestions made by C. J. Fox, dentists were urged to unite and send a prayer to the Royal College of Surgeons to enable those who had been in practice prior to 1859 to submit to examination without having undergone the prescribed curriculum. Fox's blueprint for the future organization of the dental profession was given in a paper to the Odontological Society—the cure for the evils under which the profession was labouring lay in registration and compulsory education.[47] He proposed that after a certain target date (1875 or 1880), by act of Parliament, registration of dentists be compulsory—all those who were practising before this date, and those who subsequently gained the L.D.S. or medical qualifications should be registered. He also advocated a crusade against advertising dentists—'show us a real light—drain this swamp—arrest this inundation of the rude and the illiterate'.[48] It is interesting to note that

[44] *Brit. J. dent. Sci.*, 1867, **10**, 361.
[45] Ibid., p. 408.
[46] *Brit. J. dent. Sci.*, 1868, **11**, 512.
[47] C. J. Fox, 'The position of dental surgery as a profession', *Trans. odont. Soc. Gt. Br.*, 1870–1, 3 (New series), pages 11–30, and *Brit. J. dent. Sci.*, 1870. **13**, 569–80.
[48] C. J. Fox, op. cit., p. 23.

the Odontological Society had recently proclaimed itself a scientific society, over and above politics; Fox's paper being of a political and radical nature, no discussion of it was permitted at the meeting.

The plea for a sifting of the wheat from the chaff was to produce much comment, although, despite the election of a Dental Diploma Committee to promote Fox's ideas, five more years were to flow by before definite action, in the shape of the creation of the Dental Reform Committee, was to follow. This, together with the passing of the 1878 Dentists Act, which led to the partial restriction of practice and the institution of the dental register, and the establishment of the British Dental Association in 1880, belong however to a later period than that of this present paper. The seeds for the fertile growth of the moves towards registration and a closing of the ranks were however to be found in the later years of the sixties. State control of medicine after 1858 enabled the public to distinguish between the qualified and unqualified; what the qualified dentists needed was the introduction of a similar measure to help the public distinguish between qualified and unqualified dental practice. Many people however only sought the dentist's services at moments of pain, for extractive treatment. The nearest and cheapest practitioner was often the man to whom they had recourse—indeed the extraction of aching teeth was all that dentistry comprised in the minds of the public at large. A growing population offered ample opportunity for the spread of quackery: the opportunity was not wasted and was dilligently used. The population movement away from the rural areas to the new industrial and urban areas concentrated the clientele of the dentist in a smaller area, and therefore made possible the development of a fixed surgery rather than an itinerant practice.

Dental health education at this time was primitive, and, although there is some evidence to suggest that the upper and middle classes were becoming more health-conscious, the vast majority of the population placed very little value upon the care and preservation of their teeth. One significant barrier to the recognition of dentistry as a profession was the failure on the part of the public to respect dentists and the need for dental attention. The isolation of disease and the protection of the public, as part of the public health and sanitary movement, were accomplished by compulsion, that is by the exercise by the state of a police power. The ravages of dental disease, affecting as they did the individual rather than the public at large, were not the subject of state action. The enfranchisement of the middle- and working-class urban population in 1867, together with the effects of the 1870 Education Act, which made possible the appointment of school-boards elected by rate-payers and with the development of a system of local government by county boroughs and counties paved the way for the development of local health services, and in particular of local school health services, and for a gradual improvement in dental public health. It was, however, and for that matter still is, a long and winding path to progress.

On the optimistic note of Fox's reforms this present study of the dental profession is concluded. During the years 1864–1869 only twenty-nine persons had qualified as L.D.S.—an average of only about five per year. Before dentistry could expect major reformation, particularly by Parliamentary Act, the numbers of practitioners, to whom the legal conduct of dental practice would be restricted, would have to be increased. To open the portals to all for a certain period, and then to close the ranks did not appeal to everybody—like a dose of medicine it might subsequently be good for the constitution, but it was not nice to take at the time.

Although this paper is not devoted to clinical developments in the sixties, one technical innovation must be mentioned—the development and usage of nitrous oxide as an anaesthetic agent. Following stringent tests at the Dental Hospital of London, this gas was administered to large numbers of patients at the hospital. A subscription-list from dentists was initiated, and in this way the boons of painless dentistry were first demonstrated to the public in this country during the closing years of the 1860s.

The effect of all the proposed reforms was to inculcate some deeper feeling of professional status and to remind the profession of Dickens' statement—'it is well for a man to respect his own vocation, whatever it is, and to think himself, bound to uphold it and to claim for it the respect it deserves'. Clearly the level of professional respect, let alone the public respect, was not high; the years 1858–1870 had however seen significant progress along the road from trade to profession. Developments in the educational process, the consolidation and diffusion of a body of knowledge, the standard of qualification, the emergence of set standards of conduct and the recognition of a scientific status, at least by fellow professionals, could be noted. It was then during the years of our study that the first dental schools and hospitals were instituted, the first exclusively dental qualification was proposed and introduced, and the first united dental society was founded. On these three pillars dentistry was in time to build—progress was slow, but as the *Quarterly Journal of Dental Science* noted, 'great reforms are seldom accomplished at once, and this is peculiarly the case when the elements to be reformed are wholly disorganized'.[49] Comparing its position in 1870 with that twenty years before, the dental profession might well remember Walt Whitman's lines from his *Song of the Open Road*: 'I am larger, better than I thought, I did not know I held so much goodness'.

The cosy dreams of the reformers were not totally beyond realization; progress along scientific and political lines was being effected by the dentists, and increasing signs of professionalization were to be noticed. The last ten years had seen the establishment of the L.D.S., and the authentic recognition of dentistry as a legitimate branch of surgery. On the debit side, however, the continuing assumption of the title of dentist by the unqualified was going unchecked, and to many this was the fly in the ointment. Great numbers of the public were little aware that dentistry was becoming a scientific profession; in their view dentistry consisted solely of what was looked upon as mechanical work—the extraction of decayed teeth and the insertion of artificial substitutes. Much still remained to be achieved, and this was then the period of infancy for the profession which really could not be said to have cut all its own teeth before the incorporation of the British Dental Association in 1880. For the dental profession the 1860s were a period full of teething troubles, and dentists were learning the value of Longfellow's lines from *A Psalm of Life*:

> Let us, then, be up and doing
> With a heart for any fate;
> Still achieving, still pursuing,
> Learn to labour and to wait.

Learning to labour and wait for registration the dentists certainly were.

[49] *Quart. J. dent. Sci.*, 1858–9, **2**, 3.

Medical Literature

by

E. GASKELL

TO MAKE a survey of medical literature in any period of history is a delicate task. How is one to select, out of the vast mass of material which was already appearing a century ago, not only those books and articles which typified their period but also those which deeply influenced it? Fortunately preliminary guidelines can be found in the stimulating and far-seeing address[1] delivered by John Shaw Billings to the Seventh International Medical Congress held in London in 1881. Entitled ambiguously 'Our medical literature' (Billings believed fervently in the universality of medicine) this paper amounted to a public dissection, or more exactly vivisection, of the *corpus litterarum medicarum* as it then was. From his analysis of the periodicals and books pouring from the world's medical presses at an ever-increasing rate, Billings drew certain conclusions about the relative strengths and weaknesses of the advanced countries. It is true to say that no one was better qualified than he to make these assessments for he headed a library in Washington which, by every means in his power, he was pushing into world leadership. The Billings statistics are not, of course, unchallengeable; but they have enough substance to justify consideration at the present symposium since the period to which they relate is a mere decade removed from 1870.

The tabulated figures gathered by Billings—dividing the world's output of books and articles into fourteen subjects and showing the contribution of each nation—indicated if nothing else that a serious recording and storage problem had already posed itself which could only be alleviated, if not solved, by a new approach to bibliography and librarianship. From the evidence of his statistics he was led to four main conclusions which bear repeating here; (1) the French[2] were producing more books and articles than any other nation; (2) the Germans were more productive on the basis of sheer bulk, that is to say number of pages actually printed; (3) new journals were sprouting and dying in great numbers, nowhere more so than in the U.S.A. and France, but least of all in the 'most stable of all' countries, Great Britain; (4) the Germans were vastly superior in the biological departments: 'As regards scientific medicine we are at present going to school in Germany'.

In order to discover whether Billings' last tenet was as true of the 1860s as he claimed it to be of 1881, we made a simple comparison of the German and British periodical articles recorded in the first section of the Garrison-Morton[3] list of classic medical texts. These cover the basic pre-clinical sciences. One might argue, but with difficulty, that the ratio of 5:35 in Germany's favour there revealed does not necessarily imply a general overall superiority in the laboratory work of that country. Of far greater significance in the writer's view is the fact that these 35[4] outstanding contributions by German anatomists, physiologists, etc., are contained in the pages of nineteen journals, whereas the five[5] British articles come from four journals only one with a medical connection. If nothing else this reflects a certain vigour within German medical journalism; but it also presupposes a strong and dispersed community of research-scientists.

Nevertheless about 10 per cent of the reviews in the *Medical Times* and the *Lancet* were of books on anatomy and physiology. For the most part these were manuals and dissecting guides rather than reports of original research, a fact which drew unfavourable comments from both the *Medical Times*[6] ('many original works on anatomy [have] not of late years issued from the English Press'). The chief treatises of the time were by W. B. Carpenter[7] ('It is hardly an exaggeration to say, that the majority of medical practitioners . . . owe all that they know of physiological science to some one or other of Dr. Carpenter's treatises'[8]), W. S. Kirkes[9], R. B. Todd and W. Bowman,[10] but excellent though they were of their kind they still failed to evoke—at least for the above reviewer,—a picture of vitality in British physiology. Britain, he regretted, was importing her physiologists 'as she does her corn or her cotton'; well-endowed chairs were conspicuous by their absence; whilst, impeding progress on all sides, was the traditional British distaste for any research which had no immediate practical application.[11] By the end of the decade, however, there were already signs of long-held prejudices breaking down under the weight of constant criticism from a few enlightened quarters, and an undoubted sign of this was the founding in 1868 of the *Journal of Anatomy*, to be followed three years later by the Chair of Physiology[12] at University College, London. Even so, one can point during this decade to the brilliant writings of T. H. Huxley[13] and R. Owen,[14] both of whom demonstrated an extraordinary capacity for producing results in a none too encouraging intellectual environment. Deficiencies existed too in other departments of pre-clinical literature; these were partly remedied during the decade by the efforts of G. V. Ellis, Professor of Anatomy at University College, London, and his artist-assistant G. H. Ford.[15] One reviewer who welcomed their book (*Lancet*, 1863, **i**, 299) seized the occasion to deplore the dearth of iconographic literature; this, he said, was all the more regrettable in view of the difficulties one had in obtaining dissection material.

But the real strengths of British medicine lay elsewhere, in the clinical departments. Such indeed were their attractions for the English medic that pathologists came to use the Pathological Society 'as an arena for the discussion of questions of clinical rather than of pathological interest', exhausting 'the patience of their hearers with the clinical history before scarcely a word is said about the specimen itself' (*Lancet*, 1867, **ii**, 613). This bias shows itself quite clearly in the pages of Garrison-Morton,[16] though it must be emphasized that this evidence is here offered as confirmatory rather than incontestable in its own right. The decisive factor in all this was the steady stream of interesting case-material provided by the numerous London hospitals. Young pathologists and clinicians made wise use of it, producing improved descriptions of many conditions.[17] The young Samuel Wilks (36 years old in 1860) was writing clear and classic descriptions of skin,[18] bone,[19] and venereal diseases;[20] whilst the even younger Fox[21] (b. 1836) and Nettleship[22] (b. 1845) were throwing valuable light on impetigo and urticaria. An optimistic view of dermatology was taken by the reviewer of Neligan's *Treatise of Skin Diseases*, 2nd ed., 1866; a book which he classed as 'one of the numerous modern works . . . which have rescued the treatment of these maladies from the quacks' (*Med. Times*, 1866, **1**, 673). And yet, healthy though these signs and portents were, the literature of the new specialty exhibited enough unevenness to prompt the remark 'that we can scarcely point to any one book in the English language where a serious and competent writer has attempted to deal with [the] great question of classification' (*Brit. for. med.-chir. Rev.*, 1869, **43**, 396). One thing in particular hampered the dermatologists. They had no special hospital of their own, apart from a small one in Blackfriars, and the Western Dispensary in Charlotte Street. Compared with the situation in the continental capitals dermatology was labouring under extremely difficult circumstances made worse by the generally unsympathetic attitude to specialism taken by the *Lancet*. The whole situation changed for the better in 1863 when St. John's Hospital for Diseases of the Skin was opened.[23]

The ophthalmologists, on the other hand, were fortunate in having a long established hospital (Moorfields), and a journal,[24] in which to concentrate their efforts and thus intensify progress in their specialty. Moreover they had men of real eminence whose reputations on the continent were highly respected (Sir W. Bowman, Th. Windsor, J. Laurence, Sir J. Hutchinson). Even so, as was pointed out by one observer, much of the fundamental scientific work in the subject had been accomplished on the continent,[25] especially in the laboratory of the Anglophile Donders.[26] Through his friendship with this man, Bowman was able to acquire the manuscript of his classic work on refraction,[27] bring it to England, and have it published in English by the New Sydenham Society in anticipation of its appearance in the original Dutch!

Also indebted to continental achievements were the laryngologists, the leading personalities amongst whom (Morell Mackenzie,[28] Gibb,[29] Johnson[30] and Walker[31]) had all met and been influenced by Czermak[32] on his visit to London in 1863. Like the ophthalmologists their powers of diagnosis had been immeasurably improved by advances in technology. In contrast we have a picture of otology (*Med. Times*, 1866, **ii**, 46) as an 'obscure because little cultivated corner of the medical field' with writers often offering contradictory opinions on fundamental tenets of their science. In Toynbee,[33] however, they had one personality whose works virtually transformed their specialty.

Nothing more dramatically illustrates the impact of new hospitals on the quality of medical literature than Charles West's *Lectures on the Diseases of Infancy and Childhood*, already recognized as a classic text well before the 1860s. The first edition of 1847 was based on 600 cases and 180 post-mortems seen at the Children's Dispensary in Lambeth, and represented lectures delivered to pupils of Middlesex Hospital. By 1865 the book had run through five editions, and had been translated into several languages. It had also grown in authority, for during the intervening years West had been able to examine nearly 40,000 children, most of them at Great Ormond Street. From this vast number he selected 1,200 interesting cases and 400 post-mortems around which to build his fifth edition ('the establishment of the Children's Hospital brought me readier means of more careful observation'). But West was not entirely on his own. 'Students of children's diseases', remarked the *British Medical Journal* (1870, **ii**, 89) in a review of Hillier's *Clinical Treatise*, 'cannot complain of any paucity of literature on the subject'. Nor could they, for apart from periodical literature several books (E. Smith,[34] E. Ellis,[35] T. H. Tanner,[36] T. Holmes[37]) won the general approval of reviewers. 'It is satisfactory', said the *Brit. for. med.-chir. Rev.* (1870, **46**, 387–400) in a collective review of British and foreign paediatric texts, 'to find that the increased attention being paid to this subject is already bearing fruit in many excellent works and monographs, in the production of which our own countrymen have not been behind-hand'. That this was so reflects great credit on English paediatricians; their opportunities for making clinical and pathological examinations of children were still much fewer than those available to doctors in Paris, Berlin and Vienna, as was forcibly pointed out at the time.[38]

In a less enviable position, from the point of view of the treatment available to them, were the older sections of the population. In 1864 when Maclachlan[39] published his large tome on the diseases of old age, English geriatric literature amounted to a few articles, and books by B. van Oven,[40] G. E. Day[41] and Sir A. Carlisle.[42] 'We are almost wholly indebted to continental authors . . . for the information we possess' said Maclachlan, though this criticism of his countrymen was modified a little when he expressed admiration for their 'practical views and sound sense' as against the 'recondite reasoning' of the Germans and the 'great industry' of the French. These reservations about foreign literature did not, however, prevent him from making frequent allusions to it in his text which was justly described by the *Medical Times* as a landmark in British geriatric literature.

Whilst geriatrics can be dismissed almost in one sentence it would take several paragraphs adequately to describe the wealth of literature published during this one decade on the various branches of surgery, a department in which British achievements were highly esteemed on the continent.[43] One of the most impressive texts to appear was the three-volume work edited by T. Holmes[44] in which Lister (who contributed chapters on anaesthesia and amputation) was 'introduce[d] to a number of English readers who have hitherto known him only as the able co-operator in Mr. Symes' most brilliant feats of surgery, and the main author of our present theory of inflammation' (*Lancet*, 1863, **i**, 68). Acclamation for this multi-author work was general; the *Lancet*, for instance, called it 'no ordinary book' and commended it for containing in its 1,000 pages 'a store of information such as no other surgical work in the language can pretend to offer'. Somewhat less unanimous was the praise for another textbook[45] from the hand of a revered but rather old-fashioned surgeon, Sir William Lawrence, F.R.S. Where praise was given it tended to be double-edged, as for instance in the *Medical Times* review which commended Lawrence's book as 'a handsome volume', 'a charming recreation', 'an admirable book, and one adapted to the wants of all ages, ranks and classes in the profession', and one 'not so much criticized as to be reviewed'. The effect is to leave one wondering whether the reviewer's criticisms are toned down out of a genuine attachment to the past or exaggerated respect for Lawrence. 'Mr. Lawrence', he went on, 'is a surgeon of the old school, but when it is recollected for how long a period the views he re-announces upon these general topics were held, and for how long they influenced practice, it is well that we should . . . ask ourselves whether . . . in a few cases at least, we may not, in avoiding Scylla, have fallen upon Charybdis' (*Med. Times*, 1863, **i**, 68). The *Lancet* reviewer was less kind, attacking Lawrence as a purveyor of 'old platitudes . . . reprinted from surgical authors of twenty years ago'. The Lectures were no more, and no less, than 'loose rambling sketches of disease' (1863, **i**, 119). The trenchant and scornful language reminds us once more of the deep cleavage between old and new ideas so characteristic of the 1860s.

Obstetrics and gynaecology, for which there was an abundance of case-material and in the practice of which monetary rewards were often high, had a great attraction for the British practitioner. 'Woman', opined the *Medical Times*, 'is in the ascendant.[46] Gynaecology is the result. Simpson popularizes the speculum, Bennet finds a mare's nest for the public. Spencer Wells[47] turns folk craft into high art . . . Obstetricians in general look up, form societies, read reports, assume a scientific status, and of course multiply and increase' (1865, **ii**, 77). This was no over-statement of the truth. Review columns bulged with reports of new books and pamphlets, edition superseding edition in much the same way and almost as rapidly as they do today. But occasionally surprising gaps in the literature revealed themselves, as for instance when W. S. Playfair's book on operative obstetrics[48] was hailed as the first of its kind since 1825 (*Med. Times*, 1865, **i**, 265).

The great unevenness, variability and sometimes sheer tendentiousness of the special literature during the 1860s are reminders of the cold and unwelcoming climate in which the immature specialties were then struggling. The *Lancet*, as usual to the forefront in controversy, was particularly ferocious in its opposition to what it considered a disastrous erosion of general medicine. But so far as the standards of general medical textbooks were concerned there was really little to fear, for their sections on special diseases only stood to gain from any improvements in the specialties themselves. Students of the 1860s had several excellent texts from which to choose—Graves,[49] Todd,[50] Trousseau,[51] Watson,[52] Aitkin and Bennett[53]—the most widely admired being Watson's *Principles*, which by 1857 had reached a fourth edition. This stylistic classic—admired by the *Medical Times* reviewer to the extent that he looked upon a new fully revised edition in 1871 almost as an act of profanity —was urbane, readable, witty and above all, humane. In the revised edition Watson openly changed many of his views in the light of recent discoveries, showing a generosity which earned him general approval, though it is true that some objected to his advocacy of purgations in the early stages of cholera. Two other medical texts also deserve mention. W. Aitkin's *Science and Practice of Medicine* was extremely successful and went through second and third editions in 1865, but perhaps even more important in the long term was the work of John Hughes Bennett, physician and physiologist. The *Lancet* had no doubts on this; not only would it secure a lasting reputation for the author, it would also mark an era in the progress of medicine.

A passion for clinical medicine did not necessarily imply 'the ability to cure disease'—as was pointed out in a long mordant essay reviewing eleven books on therapeutics and materia medica (*Brit. for. med.-chir. Rev.*, 1869, **44**, 1–26). 'That the whole subject of therapeutics is in a most unsatisfactory state every one seems ready to admit', it began; not only was the nature of disease poorly understood, pathologists seemed incapable of agreeing on a common rationale for their subject. The scepticism which was diagnosed in this review was even more plainly stated in Rogers' *Present State of Therapeutics*, 1870 ('we have really no principles of therapeutics'). Dr. Ainstie was one who advocated more and better 'clinical observation and experiment' (*Med. Times*, 1868, **ii**, 661); so, too, did the *Brit. for. med.-chir. Rev.* (1869, **44**, 1) for whom Bence Jones (*Lectures on some of the Applications of Chemistry and Mechanics to Pathology and Therapeutics*, 1867) most perfectly epitomized the ideal of a practical experimentalist. The most popular students' textbook was Garrod's *Essentials of Materia Medica and Therapeutics*, 3rd ed., 1868, though considerable success was also enjoyed by Scoresby-Jackson's *Notebook*, 1868 and Royle's *Manual*, 5th ed., 1868.

But despite the widespread nihilistic attitude towards therapeutics, the appearance of the new *British Pharmacopoeia* in 1864 led to a whole harvest of books, nor was there much less of a flood in the somewhat less scientific—but seemingly more promising—field of climatology. It was more a matter of placing therapeutics on the right road than of losing faith in it: 'During the next fifty years probably more advances will be made in the knowledge of the therapeutical value of mineral springs than has been made for the last 300 years . . . Everywhere information is pouring in. Pamphlets and octavo volumes are being published . . . The tendency of the age is more and more "to throw physic to the dogs" ' (*Med. Times*, 1865, **ii**, 376). Fortunately for the future of therapeutics there were those who realized that the best way to combat this tendency was to study the etiology of disease on more scientific lines.

Up to this point we have been concerned with medical literature as the almost exclusive product of the medical profession. But Victorian 'medicine', interpreted in its broadest sense, involved a far wider public than the doctors and experimentalists. To take only three examples—public health, vaccination and lunacy—these were, and are, legitimate objects of enquiry on grounds other than purely medical ones. Lay comment[54] on these matters is thus an important aspect of medical literature during the 1860s, and it is to be sought in a variety of sources such as the press, the review journals and in the somewhat less objective pamphlets written in large numbers by writers committed to causes (e.g. the antivaccination movement). On the purely medical aspects of vaccination remarkably little was written in book-form in spite of the disastrous smallpox epidemic of 1863–4. This had the effect of reviving the somewhat flagging debate on the principle of compulsion, and most of the literature concerning it appeared in periodicals or ephemeral pamphlets. But, had we nothing else to inform us of the tragic events which precipitated the demand for compulsory vaccination, we should still have reason for suspecting their occurrence from the appearance of a facsimile[55] of Jenner's *On the Origin of Vaccine Inoculation* in 1864. Reviewed with enthusiasm in the *Brit. for. med.-chir. Rev.* (1864, **33**, 427) it was hailed as a timely reminder of Britain's slackness—'the worst vaccinated of all nations'—and as a 'too well merited rebuke, for the want of care that we, his successors, have given to the performance of vaccination'.

Public health, too, had a relatively sparse literature if one ignores the government and local authority reports noticed extensively in the *Brit. for. med.-chir. Rev.* and the papers delivered at the meetings of the Epidemiological and Statistical Societies and the Social Science Association. How, indeed, could one reasonably expect more than a handful of books in a subject so young which was still struggling to find a rationale and a terminology? 'The science of hygiene', it was remarked, 'may almost be said to have come into existence during the last twenty years' (*Brit. for. med.-chir. Rev.*, 1869, **44**, 177). The leading protagonists were Chadwick,[56] Simon,[57] Gairdner[58] and Rumsey,[59] the former an old hand who had not yet lost his touch, the latter a young man of great vision who used statistics freely to strengthen his case for state medicine.

Another topic which unfailingly aroused debate both inside and outside the medical profession was the incidence of lunacy in Britain. Year by year the *Brit. for. med.-chir. Rev.* analysed the returns of the Lunacy Commissioners and scrutinized the annual reports of asylums, steadfastly refusing to be panicked by the 'unusual (i.e. high) returns of the state of lunacy' (1869, **43**, 383). Applying cold common sense instead of the hysterically induced reactions all too common in other more prejudiced quarters it pointed out how statistics were merely a reflection of society's increased sense of responsibility for the mentally sick. C. L. Robertson was another commentator who endeavoured to inject an element of reason into a situation which had become dangerously inflamed (*Med. Times*, 1869, **i**, 364). Nevertheless the public was convinced 'that the number of lunatics augments with terrible rapidity' (*Brit. for. med.-chir. Rev.*, 1869, **43**, 383), whilst from time to time statements hardly likely to reduce the impression of impending disaster were regrettably made by people who should have known better.[60]

SOCIETIES AND SPECIALIST JOURNALS

If British medical literature suffered from certain defects during the 1860s there was no shortage of people ready to point them out. In some cases the critics would go a stage further and attempt to remedy an unsatisfactory situation by writing books, establishing personal reputations as research-workers, or by grouping together in new societies. Not surprisingly many of these innovators were young men impatient with old ideas, but this was not always so. One brilliant exception was Sir Thomas Watson, first president of the Clinical Society[61] (founded in 1868 when he was 76), who pinned his and the Society's faith in 'more exactness of knowledge, and therefore more direct and intelligent purpose'. Practical effect was given to these ideals through the pages of its *Transactions* and in the hospitals where its members were employed. But allowing for exceptions nothing more dramatically illustrates the clash between two generations of doctors than the circumstances out of which the New Sydenham Society[62] emerged, phoenix-like, in 1857.

For years the original Sydenham Society had been treading its leisurely path. Its membership, spread over the whole country, was still numerous enough in 1857 not to give grounds for pessimism. But there were mutterings from the provinces about cliquishness and careless housekeeping in the Society's affairs. Many members regretted, and even resented, the somewhat faded and old-world image of their recent publishing programme, and attacked the editors for the irregular appearance of volumes. The *London Medical Review* took up the cry, berating the Society for 'its spiritless tendency to reproduce works from the ancient authors'. And so the Society's affairs were wound up at a meeting during which its president, Sir John Forbes, was provoked by his critics into challenging their leader to act constructively. 'If some young men', he said, 'thought the Society's work was not finished . . . they had better form a new one for themselves'. The New Society[63] into which these young men regrouped themselves endured for over forty years, sustaining throughout that time a progressive publishing programme in which translations figured prominently—Hebra on skin diseases, Casper on forensic medicine, Trousseau on clinical medicine, Frerichs on liver diseases, Kramer on ear diseases, Griesinger on mental pathology, and Donders on refraction.[64] In addition, for the whole of the 1860 decade, and half way through the next, a *Yearbook* or conspectus of medical literature regularly appeared, though from 1865 it came out biennially.

Other societies[65] flourished too. The Medical Society of London, almost a century old, continued to publish articles of a high standard in its *Transactions*, whilst at the Royal Medico-Chirurgical Society the tendency was increasingly for members to question the judgment, and even integrity, of their own editor for preferring to publish lengthy clinical disquisitions rather than papers delivered at society meetings. (*Med. Times*, 1868, **i**, 317) But this was a sign of the times, for statistics—with which these same articles were usually brimming—had assumed for many people the status of a god whose twin shrines were the Statistical and Epidemiological[66] Societies. Obstetricians,[67] pathologists,[68] odontologists,[69] not to mention ethnologists, and anthropologists,[70] formed together both for the good of their specialties and the advancement of personal ambitions knowing that the surest way to further their careers, whilst at the same time increasing the sum of knowledge, was to publish in the pages of their societies' respective journals.

Not all specialist journals were issued by societies, nor were they all equally stable. On the contrary, their death-rate was high enough to discourage all but the most tenacious of editors. The *Medical Critic and Psychological Journal*[71] lasted only three short years and was not reborn until 1875—in spite of having replaced a journal of thirteen years' standing (*Journal of Psychological Medicine*). The dermatologists were no more fortunate: their *Journal of Cutaneous Medicine*[72] expired after four volumes. Often, too, things were made no better by the element of personal aggrandisement which seemed in one way or another to insinuate itself into the fabric of these journals. *The Stethoscope*,[73] for instance, which was hardly allowed to get off the ground was scathingly attacked on all sides for its lack of quality articles, but more particularly (*London Medical Review*, 1863, **3**, 494), for being 'the offspring of some small coterie'. An even more blatant example of personal rivalries was the rapid rise and fall of first the *Journal of British Ophthalmology*,[74] edited by Jabez Hogg and then the *Ophthalmic Review* edited by John Laurence. Fortunately for this specialty, however, it had the added resource of a hospital regularly publishing a highly esteemed journal.

The fashion of published hospital reports had been set by Guy's as early as 1836, and by 1860 there were few doubts about the benefits which would accrue to medicine from an increase in the number of such journals. In 1864 the London Hospital[75] followed Guy's example, St. Bartholomew's doing the same in 1865 and St. George's the year after. Nevertheless certain sections of the medical press expressed reservations, as for instance the *British Medical Journal* (1865, **ii**, 370) when it commented cynically: 'if a young man yearns for success in life, he publishes a few cases in the medical journals and then turns them into a pamphlet at sixpence or a shilling'. One can also trace a whiff of disapproval in the *Medical Times*' reaction to books such as Abercrombie's *Medicine*, Budd's *Diseases of the Liver*, and Habershon's *Diseases of the Abdomen*: 'there is a certain class of books on practical medicine which is invariably successful . . . They consist less of exhaustive treatises than a string of clinical reports' (1868, **i**, 317). On the other hand the same journal was glad to note the 'liberality of the day' by which medical reporters were allowed free access to wards and operating theatres in order to describe interesting cases, even before they could be written up for hospital journals.

RECORDING THE LITERATURE

As periodical literature expanded so it became increasingly apparent that only radically improved methods of recording it would prevent chaos from ensuing. This basic fact was grasped, and acted upon, by J. S. Billings who, by publishing a two-volume catalogue[76] of his library in Washington, launched medical bibliography into a completely new era. Ten years later he was to make an equally significant step forward by issuing the first volume of his *Index Medicus*, an annual guide to periodical literature which has continued to appear, with only brief interruptions, until the present day.

But however much one prizes the achievements of Billings it would be wrong to imply that all around him lay a bibliographical wasteland. Already in 1860 Britain possessed two well established abstracting journals, each appearing twice yearly, the one to all intents complementing the other. William Braithwaite's *Retrospect of Medicine*, which appeared continuously from 1846 until 1901, contained lengthy résumés of articles in a few leading clinical journals, chosen with the object of banishing 'unfounded theory and opinion, taking facts as [one's] guide', and, so far as one can tell, in an entirely unprejudiced manner. One recalls, for instance, the generous amount of space given to Lister's *Lancet* articles, and the editor's admiring comments on them, whilst elsewhere he takes a neutral stand between Lister the innovator in his claim for priority, and Simpson the somewhat peevish objector. In retrospect Braithwaite is to be congratulated for having drawn attention to the work of Semmelweis[77] and, in his occasional discursive surveys of new work in obstetrics, for having repeatedly urged upon doctors the need for utter cleanliness in proceeding from post-mortems to the wards.[78] Rivalling the *Retrospect* was the *Half-yearly Abstract of the Medical Sciences*, published by Churchill and edited by W. H. Ranking and C. B. Radcliffe.[79] Though its abstracts were far shorter than Braithwaite's, its coverage of journals was much wider and more catholic, taking in 43 foreign titles amongst which was a handful from the pre-clinical sciences. On the odd occasion, too, a separate section was devoted to a survey of physiology.

This field of quasi-bibliography was also entered by the New Sydenham Society, ever ready to broadcast the latest advances in continental medicine. Its *Yearbook*[80] was unashamedly based on German and French models and indeed was only made possible by the unselfish co-operation of the leading German abstracting journal, Schmidt's *Jahrbuch*. The revise-sheets supplied from this source were, for the first volume at least, hastily translated and arranged into five sections. This led to inaccuracies which were duly noted and deprecated by reviewers. In all other respects, however, the reception given to it was encouraging, and later volumes—appearing biennially from 1865 until publication finally ceased in 1875—earned fairly general approval amongst reviewers. One criticism which was voiced concerned the large number of 'sensational' articles abstracted in the section on forensic medicine;[81] these came mostly from German and French sources and regrettably typified the periodical forensic literature of those countries. That the Society considered it necessary to devote a whole section (and later two) to toxicology and the forensic sciences may be a result of an exaggerated deference to continental achievements in medicine. It may also reflect the preoccupation of mid-Victorian England with poisoning[82] as a social hazard; what is certain is that the evidence of doctors was being increasingly sought in the courts of law.

Two other periodical publications of a slightly different nature deserve consideration at this point. The *British and Foreign Medico-Chirurgical Review*, often quoted in the present essay, belonged to a literary genre which had flourished for half a century. Its lengthy reviews, each one based on a group of books united by a common theme, give a clear picture of the directions in which progressive medical thought was then moving. To the historian of comparative medicine they are invaluable as a kind of running commentary on the interplay of ideas between the leading nations of Europe and America. Without detailed research it would be impossible to make even an approximate valuation of the journal's influence, but one cannot fail to be impressed by its clear and steady support for the scientific principle in medicine even at the risk of offending national sensitivities.

Horace Dobell's *Reports on the Progress of Practical and Scientific Medicine in Different Parts of the World* are in a quite different category. More a repository of curious information than a systematic abstracting journal they were castigated both for unoriginality and for lacking comprehensiveness. Only two volumes appeared, consisting of articles from correspondents in Paraguay, Iceland, Denmark, U.S.A., Italy and numerous other countries. At a time when the tendency in medical literature was to specialize rather than to spread one's talents thinly over a wide area, Dobell's *Reports* had little chance of succeeding. However, they retain a certain interest for the historian in that they reveal odd facts and preoccupations within medicine at the end of the decade. We learn, for instance, that Listerism had already been adopted in distant Carmarthen. We hear from Dobell, in the most optimistic terms, about the frenetic building of new hospitals—'if properly handled and supplemented by the parochial system of England [the new hospitals] would place our poor, sick and aged on a footing of comfort unsurpassed by any country in the world'—whilst, in counterpoint, we have Percy Leslie's indictment of charities and a foreshadowing of state medicine—'Government will be induced to take some steps for the efficient supervision, if not the entire control, of the heterogeneous collection of private bounties, posthumous bequests, and other charities, now often squandered or but half-developed, through inefficient control or ignorant management'.

Whereas the 1860s seemed to be a particularly receptive period for review and abstracting journals the following decade was to be the deathbed of at least three—Dobell's *Reports*, the *British and Foreign Medico-Chirurgical Review*, and the New Sydenham Society's *Yearbook*.[83] Competition from the growing number of journals[84] had proved too intense. These journals were forming their own abstracting and review sections, sometimes going to considerable trouble to track down new material. The *Ophthalmic Review*, for instance, produced its own 'periscope or digest of the principal British and foreign ophthalmic production' out of an 'organized system of exchange . . . arranged with all the leading ophthalmic periodicals thru'out Europe'. In a sense medical literature was organizing itself to cope with a new scientific age and, in the process, was transforming herself out of all recognition.

REFERENCES

1. *Brit. med. J.*, 1871, **ii**, 262–8.
   Lancet, 1871, **ii**, 265–70.
   Trans. 7th Intern. Medical Congress. Vol. 1, pp. 54–70, London, 1881. Reprinted in *Selected Papers of John Shaw Billings*, ed. F. B. Rogers, Medical Library Association, 1965, pp. 116–38.
2. 'Periodical literature has had a striking influence upon the progress of medical science in France. It is carried on with unceasing energy and employs the time and powers of some of the most intellectual men in Europe. No less than forty journals appear at stated intervals . . . Scarcely is there a person interested in the cultivation of the healing art that does not see at least two periodicals weekly', (*Med. Times*, 1863, **i**, 300).
3. Garrison, F. H. and Morton, L., *Medical Bibliography*, 2nd ed., London, Grafton, 1954.
4. Three sample titles are: (i) *Abhand. naturforsch. Gesell. Halle;* (ii) *Berichte k. sächs. Gesell. Wissens.*, Leipzig; (iii) *Jahresber. schles. Gesell. vaterländ. Cultur.*
5. (i) *Proc. R. Soc.*; (ii) *Phil. Trans. R. Soc.*; (iii) *Trans. Linn. Soc. Lond.*; (iv) *J. Anat. Physiol., Lond.*
6. *Med. Times*, 1863, **i**, 195.
7. *Principles of Human Physiology*, 6th ed., London, 1864.
8. *Brit. for. med.-chir. Rev.*, 1865, **36**, 51.
9. *Manual of Physiology*, 5th ed., London, 1863.
10. *Physiological Anatomy and Physiology of Man*, London, 1866.
11. J. Z. Laurence and T. Windsor, editors of the *Ophthalmic Review*, were well aware of this: 'The policy of the rulers of the profession . . . had long been consistently in favour of a humble standard of general education among medical students . . . Among them, it was a common ambition to become a practical man. The ideal practical man was one who despised, and resisted the acquirement of, any knowledge that did not, to his own immediate perception, visibly and directly lead him to the cure of disease'. (*Ophthal. Rev.*, 1869, **1**, 89).
12. Filled by Sir John S. Burdon-Sanderson (1828–1905) after Michael Foster had vacated a lectureship in the same subject.
13. *Lectures on the Elements of Comparative Anatomy*, London, 1864.
14. *On the Anatomy of Vertebrates*, 3 vols., London, 1866–8.
15. *Illustrations of Dissections in a Series of original coloured Plates*, London, 1867. See the obituary of Ellis in *Brit. med. J.*, 1900, **i**, 1132.
16. Garrison-Morton lists 47 British periodical references in the clinical section, which compares well with the 49 German contributions.
17. 'It is a remarkable feature in the Pathological Society that it enlists the junior and working members of the profession, so that although "original members" are now rarely to be seen at its meetings, the stream of scientific investigators never flags, and the younger members come forward as their elders are drawn away by other and, probably, more lucrative engagements' (*Lancet*, 1867, **ii**, 613).
18. *Guy's Hosp. Rep.*, 1861, **7**, 297; 1862, **8**, 263.
19. *Trans. path. Soc. Lond.*, 1868–9, **20**, 273.
20. *Guy's Hosp. Rep.*, 1863, **24**, 1.
21. *Brit. med. J.*, 1864, **i**, 78, 467, 495, 553, 607.
22. *Brit. med. J.*, 1869, **ii**, 323.

23. Russell, B., *St. John's Hospital for Diseases of the Skin, 1863–1963*, Edinburgh, Livingstone, 1963. The *Brit. for. med.-chir. Rev.* (1867, **40**, 74–94) thought that England was 'entitled to claim the largest share in the advancement, during the last fifty years, of the study of perhaps the most neglected of all departments of medicine', but it was also concerned at the decline in standards since the days of Willan and Bateman. Hebra's clinic in Vienna, and the St. Louis Hospital in Paris, were now, in its view, setting the pace.

24. In 1857 appeared the first volume of the *Ophthalmic Hospital Reports and Journal of the Royal Ophthalmic Hospital*. A chapter on the early years of this journal can be read in E. T. Collins, *The History and Traditions of the Moorfields Eye Hospital*, London, Lewis, 1929. Further useful information on the personalities in British ophthalmology during the 1860s can be found in A. Sorsby, *The Royal Eye Hospital, 1857–1957*, London, Royal Eye Hospital, 1957, and his 'Defunct London eye-hospitals', *Brit. J. Ophthal.*, 1936, **20**, 77–98.

25. References to continental influences occur frequently in the reviews of the period. Laurence and Windsor, for instance, attributed most of the progress made in ophthalmology to the 'application of the higher mathematics to the solutions of the problems in physiology', in which work 'our countrymen took little share'. (*Ophthal. Rev.*, 1865, **1**, 89). A reviewer of Laurence's *Optical Defects* reminded his readers that 'until a comparatively recent period, most ophthalmic surgeons were content to commit the treatment of such cases to the "Honble Comp. of Spectacle Makers". Of late years the progress made by continental ophthalmologists has attracted attention in this country'. (*Med. Times*, 1865, **ii**, 665). Another reviewer was struck by the 'stream of ophthalmic publications, by home and foreign authors'. (*Lancet*, 1866, **i**, 402).

26. See the obituary of Donders by Bowman in *Proc. R. Soc.*, 1891, **49**, vii–xxiv.

27. *On the Anomalies of Accommodation and Refraction of the Eye*, trans. W. D. Moore, London, New Sydenham Society, 1864.

28. *The Use of the Laryngoscope in Diseases of the Throat*, 2nd ed., London, 1866.

29. (i) *The Laryngoscope*, London, 1863; (ii) *On Diseases of the Throat and Windpipe as Reflected by the Laryngoscope*, London, 1864.

30. *The Laryngoscope*, London, 1864.

31. *On the Laryngoscope and Its Clinical Application*, London, 1864.

32. Czermak's *On the Laryngoscope and its Employment in Physiology and Medicine* was available in English through the translation made by G. D. Gibb (New Sydenham Society, 1861). In a long review of books on laryngoscopy Czermak's English disciples are described as the 'new corps of laryngoscopists . . . some of whom with the ardour of recruits, magnify their office, and would impress upon outsiders the unequalled or unsurpassed importance of their craft' (*Brit. for. med.-chir. Rev.*, 1865, **36**, 310–29).

33. *Diseases of the Ear*, London, 1860.

34. *On the Wasting Disease of Infants and Children*, London, 1868.

35. *A Practical Manual of the Diseases of Children*, London, 1869.

36. *A Practical Treatise on the Diseases of Infancy and Childhood*, London, 1870.

37. *The Surgical Treatment of the Diseases of Infancy and Childhood*, London, 1868.

38. 'As it is, we have a number of small hospitals, some containing only from ten to twenty beds, whereby we lose the splendid opportunities for clinical study and instruction which the larger children's hospitals in Paris so abundantly afford'. (*Brit. for. med.-chir. Rev.*, 1870, **46**, 387).

39. *A Practical Treatise on the Diseases and Infirmities of Advanced Life*, London, 1868.

40. *On the Decline of Life in Health and Disease*, London, 1853.

41. *A Practical Treatise on the Domestic Management and most important Diseases of Advanced Life*, London, 1851.

42. *An Essay on the Disorders of Old Age, and on the Means for Prolonging human Life*, London, 1817; 2nd ed., London, 1818.

43. M. COURTY, 'A surgical visit to England', (*Med. Times*, 1863, **ii**, 229–31): 'It is here especially that we see revealed the general spirit of the nation, that feeling which impels it not towards more change—for few are so constant as the English, whether in their customs of their surgery (as is seen by their old-standing predilection for union by the first intention) but towards progress, or rather towards action. English Surgery, indeed, appears to me eminently active, and, consequently enterprising and bold, if not in all ways, at least in certain directions. Patients and Surgeons alike participate in this impulse, and certainly we find in France on the part of the former a resistance which is not met with in England'.

44. *A System of Surgery*, 4 vols., London, 1860–4.

45. *Lectures on Surgery*, London, 1863.

46. Though his language is somewhat facetious the reviewer (of C. G. Ritchie's *Contributions to assist the Study of Ovarian Physiology and Pathology*) makes a valid point in referring to the effect of a prolific Royal Family on the practice of obstetrics—'A new order of things begins with a new reign. There are royal marriages, births of princes and princesses, marriages again of princes and princesses of another generation, and again more births'. But, Royal Family apart, it is also true that women's diseases were receiving enormous attention and publicity. We get an inkling of one reason for this typically Victorian *quasi* hysteria in the review of Thomas's *Diseases of Women*, New York, 1868: 'Girls of tender age are required to master studies which are too difficult and to tax their intellect . . . The results are . . . great feebleness of the muscular system, and marked tendency to disease in the generative organs'. (*Med. Times*, 1868, **ii**, 163).

47. Spencer Wells [1818–97] earned fame primarily as an ovariotomist, only secondarily as the editor of the *Medical Times*, a post which he vacated in 1863 five years after his first successful ovariotomy (see J. A. SHEPHERD, *Spencer Wells*, London, Livingstone, 1965).

48. *A Handbook of Obstetric Operations*, London, 1865.

49. (i) *A System of Clinical Medicine*, Dublin, 1843. (ii) *Clinical Lectures on the Practice of Medicine*, Dublin, 1864.

50. *Clinical Lectures*. 2nd ed., London, 1861.

51. *Lectures on Clinical Medicine*, London, New Sydenham Society, 1868.

52. *Lectures on the Principles and Practice of Physic*, London, 1843; 4th ed., 1857.

53. *Clinical Lectures on the Principles and Practice of Medicine*, 4th ed., 1865.

54. The complete contents of five lay periodicals are conveniently laid out in vol. 1 of the *Wellesley Index to Victorian Periodicals*, London, Routledge & Kegan Paul, 1966. The chronological arrangement of this index helps us speedily to pick out the surprisingly large number of articles written on medical or para-medical topics. The *Cornhill Magazine*, frequent contributors to which were G. H. Lewes, James Hinton (surgeon, philosopher and editor of the New Sydenham Society's *Yearbook*) and F. E. Anstie (lecturer and physician at the Westminster Hospital, also editor of the *Practitioner*), often campaigned on behalf of the insane, the incurables, the nurses, charities and the deaf and dumb. In the pages of *Blackwood's* can be traced the public's passion for dietary matters (e.g. Aytoun on dyspepsia, and Banting) whilst the *Quarterly Review* had a predilection for social reform in all its aspects, e.g. sanitation, science teaching in schools or nursing (cf. Harriet Martineau on Florence Nightingale). *Macmillan's Magazine* contains a perceptive article by Henry Whitehead (John Snow's churchman friend) on the role of water in spreading cholera; it also deserves to be remembered in medical history for having published Galton's first important paper on heredity ('Hereditary talent and character', 1865, **12**, 157–66, 318–27).

55. Edited by J. Brendon Curgenven.

56. *Address to the general meeting of the National Association for the Promotion of the Social Sciences*, London, 1851.

57. The critical importance of the 1860s in the history of public health is clearly conveyed by R. Lambert in his *Sir John Simon, 1816–1904, and English social Administration*, London, MacGibbon & Kee, 1963. These years also encompass the last great cholera epidemic in England, 1866, out of which appeared a flurry of new books, reviewed in the *Brit. for. med.-chir. Rev.*, 1866, **38**, 129–61, 417–28.

58. *Public Health in Relation to Air and Water*, Edinburgh, 1867.

59. *On State Medicine in Great Britain and Ireland*, London, 1867.

60. The *Lancet* (1861, **i**, 624) quoted the following, without comment, from the *Dundee Advertiser:* 'The remarkable and suggestive statement was made on Tuesday, in the Annual Report of the Medical Superintendent of the Montrose Lunatic Asylum, that there are ten lunatics in that institution whose madness "originated in the excitement of attending 'revival meetings' " '.

61. The *Brit. for. med.-chir. Rev.'s* attitude to the Society was equivocal. It questioned whether 'the institution of a new society in London for the promotion of medical and surgical science may . . . appear uncalled for, and on a par with the establishment of new hospitals'; it suggested that what was needed was a much more thoroughgoing pursuit of the 'therapeutical line of observation and research'. On the other hand the *Review* was impressed by the youthful nature of the Society's membership: 'It may be said to be the representative society of young physic! The names of the provisional committee . . . are those of the young, or comparatively young, physicians, and surgeons of the metropolitan hospitals, known for their active prosecution of medical research' (1869, **43**, 153).

62. The *Lancet* poured scorn on the Society for having pursued a slack and misdirected publishing programme, but in language which was much too severe and not altogether in tune with the facts. The Society's publications included many translations of good modern works. In the *Lancet's* view the Society had expired 'by a suicidal act, the natural climax of a fourteen years' career of exclusive misgovernment and disregard of the opinions of the profession and the subscribers' (1857, **ii**, 535).

63. There is no historical account of the Society apart from Sir Jonathan Hutchinson's short *Retrospective Memoranda*, London, New Sydenham Society, 1911. One might be excused for deploring this incompleteness in our knowledge of a vital section of Victorian medical history.

64. In Hutchinson's opinion the two most important publications of the New Society were Trousseau's *Clinical Medicine*, 1868, and Hirsch's *Geographical Pathology*, 1883–6.

65. Scotland, Ireland and the provinces were also very much involved in these developments and for reasons of space we have chosen to do no more than note the fact. Details of journals issued in Edinburgh, Dublin, Liverpool, Newcastle, etc. can be found in W. R. Lefanu's 'British periodicals of medicine; a chronological list', *Bull. Inst. Hist. Med. Johns Hopkins Univ.*, 1937, **5**, 735–61, 827–55; 1938, **6**, 614–48, to which we are heavily indebted throughout this essay.

66. Founded in 1850; its *Transactions* were first issued five years later (see the *Commemoration Volume* published by the Society in 1901).

67. Obstetrical Society of London, founded in 1858; its *Transactions* began in the following year. It should be noted that the obstetrical societies in Dublin and Edinburgh (founded in 1838 and 1840) had thus far published their transactions in general medical journals based in these cities. (J. M. MUNRO KERR, *et al.*: *Historical Review of British Obstetrics and Gynaecology, 1800–1950*, Edinburgh, Livingstone, 1954.)

68. Pathological Society of London, founded in 1847; its *Transactions* had appeared continuously since that date (J. H. DIBLE, 'History of the Pathological Society of Great Britain and Ireland', *J. Path. Bact.*, 1957, **73**, supp., 35 pp.)

69. Founded in 1856, since which date the *Transactions* had been published.

70. The *British Medical Journal* had great praise for the Anthropological Society's translations of Waitz, Vogt, Broca, Pouchet and Blumenbach, and warmly welcomed the appearance of a new *Ethnological Journal*. But with this praise went an element of regret at the new journal's policy of ignoring the achievements of the Ethnological Society whose first president had been a medical man, J. C. Prichard (1865, **ii**, 119).

71. From 1861 to 1863.

72. Edited by Erasmus Wilson.

73. This failed to survive one year, 1864.

74. 1864–1867.

75. The *Medical Times* 'regretted the burial in hospital case-books of much that is valuable in professional experience' and greeted the London's *Reports* as following 'the plan so successfully pursued by Guy's' (1865, **i**, 96).

76. Billings published a *Catalogue of the Surgeon-General's Library* in 1864 and 1865.

77. *Retrospect*, 1862, **46**, 205.

78. 'Medical practitioners in general are not yet sufficiently alive to the danger of going from surgical operations, sloughing sores, and post-mortem examinations, to attend women in labour. If they will read Mr. Nunneley's "On Erysipelas", they will find this subject handled with great ability' (*Retrospect*, 1861, **44**, li).

79. See the obituary of this interesting personality in the *Lancet*, 1889, **i**, 1331–32.

80. From 1865 until the end of its career in 1875 the *Yearbook* was entitled *Biennial Retrospect of Medicine, Surgery and their allied Sciences*.

81. The section on toxicology and forensic medicine was at first edited by Dr. Odling. The leading English book on the former subject was Taylor's *Manual of medical Jurisprudence*, 8th ed., London, 1866. On the state of English toxicology it is worth quoting the *Brit. for. med.-chir. Rev.* (1867, **40**, 345): 'It must, in fact, be confessed that at the present moment we have no English work which represents the advanced state of toxicology, and we, alas! can hardly hope for a new edition of that best of works on the subject—Christison "On Poisons" '.

82. Palmer had been hanged for poisoning in 1856. The Arsenic Act was passed in 1851. Wilkie Collins published the *Woman in White* in 1861, and the popular novelist, Miss Braddon, was producing 'sensational novels' which, in the words of the *Annual Register* for 1864, held 'considerable though not indisputed sway'.

83. 'Our Society', said Hutchinson, 'could not compete as regards promptitude of publication with an autocratic editor [i.e. Ernest Hart, editor of the *Medical Record*] . . . Meantime our medical journals were devoting more and more attention to this class of literature'.

84. Amongst these journals was the *Medical Mirror* (1864–1870), begun because 'with the single exception of a monthly periodical published in Scotland, there is no paper in Great Britain which serves to fill up the wide space of distinction between the medical weekly journals and quarterly reviews'. For its news it cast around the daily papers, popular journals (e.g. Dickens' *All the Year Round*) and serious journals of a non-medical character (e.g. *Social Science Review* edited by Benjamin Ward Richardson). As well as reviews it published abstracts, hospital reports, pass lists and job vacancies. It deserves a minor place in medical history for having published Albert Napper's essay on village hospitals (1864, **1**, 20–24, 94–96) before its separate appearance as a pamphlet.

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82. Rabies had been [illegible] in 1848. The [illegible] Act was passed in 1851 (Wilkie Collins published the *Woman in White* in 1860), and the [illegible] novelist Miss Braddon was producing 'sensational novels' which, to [illegible] of the Journal [illegible] 1864, had considerable [illegible].

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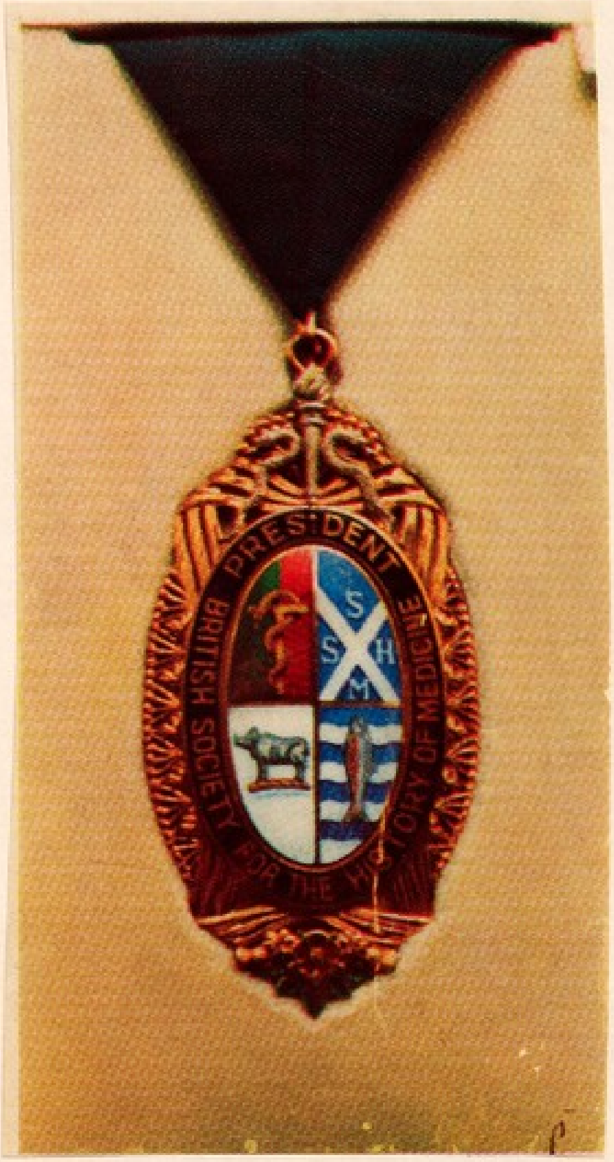
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